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Chapter 19

The Proliferation of Scientific Realisms

1. Introduction

The original motivating intuition of scientific realism was narrow. It was to seek a precise formulation of the optimistic view that our science has enjoyed an unparalleled success. A good starting point might just assert that the central propositions of our best sciences are true or likely so. Unfortunately, the literature that developed out of the renewed mid-twentieth century interest in scientific realism has not maintained a similar narrowness. Formulations of scientific realism have rapidly multiplied into the expanding disarray of competing senses now filling the present literature. This proliferation all but guarantees an enduring and unresolvable debate. We cannot agree on a doctrine, whether it is right or wrong, if we cannot first decide precisely what it is.

With this problem in view, small-r realism has been formulated austerely to capture as precisely as possible what is correct and what is viable in the original intuition, while still accommodating the fallibility of inductive inference. It is offered as a corrective to the present surplus of competing formulations. It reflects the need to contract the definition of scientific realism to something narrow and to abandon the permissiveness that allows everyone to have their own proprietary version of scientific realism. That is not to deny everyone a place to state their views. It is to deny a blanket license that allows the term “scientific realism” to have any meaning anyone may choose. It is to insist that the term “scientific realism” should have a simple, univocal meaning. If we are to identify as such a formulation of scientific realism, it is, I maintain, small-r realism.

Section 2 offers a short review of the bewildering proliferation of views in the present literature that carry the label “realism.” Their great variety makes it almost impossible to give a simple, univocal answer to the question of what is scientific realism.

Section 3 introduces the project of the remainder of the chapter. It is to assess how well the realism literature accommodates the core concept of small-e empiricism/small-r realism: inductive inference. Strong forms of realism attempt to discount the import of inductive fallibility. This investigation reaffirms small-r realism as the formulation of realism that escapes this problem.

The review of optimistic attempts by scientific realists to diminish the import of inductive fallibilism begins in Section 4. One strategy employs the notion of "approximate truth." It fails through its opacity. To declare a theory approximately true tells us nothing about where the theory fails. Similarly, we derive only scant protection from inductive fallibilism by declaring exceptions as inconceivable monsters, since what is inconceivable today may become conceivable tomorrow, even if that happens only very rarely.

Section 5 reviews three versions of selective realism: structural, entity and explanationist (indispensabilist) realisms. Each asserts that a realistic construal should be reserved for selected elements of a theory. They risk improperly breaching the limits of inductive inference in two ways. First, they discount the import of inductive inference for those parts of a theory not selected; and second they exceed what inductive inference can provide with assurances of truth for those parts selected. In so far as these selective realisms breach the limits of inductive inference, they are unsustainable. In so far as they do not breach them, they add nothing of principle to small-r realism.

The further difficulty of selective realism lies in the details. There are multiple proposals for which parts of a theory should be selected and some disagreement among the proposals. Structural and entity realists directly conflict. A still further problem is that the identification of the selected elements can be vague. There are many—too many—proposals for structure. Although the idea that some elements of a theory are indispensable to the theory's empirical success, there seems to be no way to identify which they are, when the theory is proposed. It can even be troublesome retrospectively, as the example of Carnot's 1824 memoir on heat engines shows.

Section 6 contains a short, summary conclusion.

2. Scientific Realisms: Expansion, Fragmentation and Drift

This section offers a brief survey of the development of conceptions of scientific realism. It illustrates how its conception has drifted, expanded and fragmented. Some versions are expansive and seek to combine many elements into a single synoptic view. Others merely identify one element as realism in a literature that has become increasingly fragmented. This is a literature urgently in need of repair. To repeat the main claim of this chapter, small-r realism offers such a repair. It implements, in a compact and narrow form, the original intuition underlying scientific realism.

2.1 Aims and Beliefs

The proliferation of differing senses began almost immediately. van Fraassen's early definition of scientific realism provoked a flood of responses and provided a focus for the growing realism debate. He gave it as (1980, p.8, his emphasis):

> *Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.* This is the correct statement of scientific realism.

It differs in important ways from the original intuition. Since the avowed purpose of van Fraassen's text was to refute scientific realism, the broad acceptance of van Fraassen's formulation strikes me as an enduring error on the part of scientific realists. How wise is it for scientific realists to allow their opponent to define their view? Such a definition will surely be tailored to serve the purposes of the opponent. Accordingly, van Fraassen included two notions that are not present in the original intuition of scientific realism and should not have been admitted. They are an assertion of the aim of science and an assertion about mental states, that is, beliefs. They were not new ideas in this literature, but van Fraassen's formulation gave them prominence as part of a sharp and short definition of scientific realism.

These additions diffused and diluted the definition of scientific realism. There are no aims without aim seekers and no beliefs without believers. Neither are required by the motivating intuition of scientific realism. They confound the definition with superfluous encumbrances. That science *aims*, or more accurately, scientists aim for something is no assertion of its success. We all seek things that, alas, we never achieve. We may even lack the means to realize our aims. That scientists *believe* something is a similar distraction. That someone happens to believe something is no assertion of its truth or even likelihood of truth. To incorporate the goals and mental states of scientists into the definition of scientific realism yields a defective definition whose presence persists in the literature to its detriment.

2.2 Proliferation

Early work on scientific realism was advanced by a conference on scientific realism held at the University of North Carolina, Greensboro, on March 26-28, 1982. A subsequent conference volume, edited by the conference organizer, Jarrett Leplin, identified ten versions of scientific realism in the literature of the time. They were: (Leplin, 1984, pp. 1-2)

1. The best current scientific theories are at least approximately true.
2. The central terms of the best current theories are genuinely referential.
3. The approximate truth of a scientific theory is sufficient explanation of its predictive success.
4. The (approximate) truth of a scientific theory is the only possible explanation of its predictive success.
5. A scientific theory may be approximately true even if referentially unsuccessful.
6. The history of at least the mature sciences shows progressive approximation to a true account of the physical world.
7. The theoretical claims of scientific theories are to be read literally, and so read are definitively true or false.
8. Scientific theories make genuine, existential claims.
9. The predictive success of a theory is evidence for the referential success of its central terms.
10. Science aims at a literally true account of the physical world, and its success is to be reckoned by its progress toward achieving this aim.

This list is reproduced here not because any of these formulations is especially successful. Rather, it is to illustrate just how scattered and fragmented were formulations of scientific realism, even in this early time. Leplin remarked "…scientific realism is a majority position whose advocates are so divided as to appear a minority."

Several factors encouraged continuation of this proliferation of notions of scientific realism. First is a disagreement on precisely what the doctrine should assert. Does it assert the truth of claims in a science? Their approximate truth? That their terms are referentially successful? That science aims for truth? That successive stages of science progress towards the truth? And so on.

Next, claims about the means through which these possibilities are realized are also included in the definitions. Is the truth of propositions secured through their explanatory success? Through their predictive prowess?

Next, the characterizations of scientific realism includes negations of alternative positions. To assert that propositions of a science can be true or false would surely be superfluous, were it not that instrumentalists might assert otherwise. Similarly, had instrumentalists not asserted otherwise, there would be little point in including in a definition of realism that theoretical terms do more than merely correlate observables.

Finally, the term "realism" itself has a long history in philosophy. Two usages were already well settled in the nineteenth century and persist in later writing.[1] One sense of realism asserts that genus and species (in the nineteenth century formulation), or universals (in the twentieth century formulation), have a real existence. It stands in opposition to nominalists, who assert otherwise. In another, realism asserts the veracity of our ordinary perceptions, in contrast with idealism that asserts a mind-dependency of these perceptions. Both of these ideas, we shall see, diffused into formulations of scientific realism.

2.3 Synthesis

One reaction to this proliferation of senses of scientific realism was the attempt to synthesize them into a single, all-embracing doctrine. Psillos (1999, p. xvii) sought to represent scientific realism as the conjunction of three "stances," to use an unfortunate[2] term favored by van Fraassen. They were the metaphysical, semantic and epistemic and were described as:

1 The metaphysical stance asserts that the world has a definite and mind independent natural-kind structure.

2 The semantic stance takes scientific theories at face-value, seeing them as truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. Theoretical

[1] Both senses appear in the entry for "realism" in the nineteenth century *Vocabulary of Philosophy* (Fleming, 1860, p. 422); and in the entry for "realism" in the authoritative twentieth century *Encyclopedia of Philosophy* (Edwards, 1967, pp. 77-83). The latter reports multiple versions of realism: "naïve realism," "new realism," "perspective realism," "representative realism" and "critical realism."

[2] It is unfortunate since it depicts a thesis as a mental state with some suggestion of arbitrariness. In a debate, for example, we may *choose* to adopt a combative or a conciliatory stance. We saw in Chapter 4 that this freedom of choice was explicit in van Fraassen's voluntarism.

assertions are not reducible to claims about the behaviour of observables, nor are they merely instrumental devices for establishing connections between observables. The theoretical terms featuring in theories have putative factual reference. So, if scientific theories are true, the unobservable entities they posit populate the world.

3 The epistemic stance regards mature and predictively successful scientific theories as well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, do inhabit the world.

Even though they extend well beyond the original motivating intuition of scientific realism, both of the older, nineteenth century senses of realism have been incorporated into expanded statements of scientific realism. Psillos' first statement ("…the world has a definite and mind independent natural-kind structure…") is a brief assertion of both.

At the same time, Niiniluoto (1999, p. 2) identified what he called "six different problems of realism" (p. 2). He selected among responses to them to produce the "critical scientific realism" the work was to defend. It is the conjunction of six theses, R0-R5: (p. 10)

At least part of reality is ontologically independent of human minds. (R0)

Truth is a semantical relation between language and reality. Its meaning is given by a modern (Tarskian) version of the correspondence theory, and its best indicator is given by systematic enquiry using the methods of science. (R1)

The concepts of truth and falsity are in principle applicable to all linguistic products of scientific enquiry, including observation reports, laws, and theories. In particular, claims about the existence of theoretical entities have a truth value. (R2)

Truth (together with some other epistemic utilities) is an essential aim of science. (R3)

Truth is not easily accessible or recognizable, and even our best theories can fail to be true. Nevertheless, it is possible to approach the truth, and to make rational assessments of such cognitive progress. (R4)

The best explanation for the practical success of science is the assumption that scientific theories in fact are approximately true or sufficiently close to the truth in the relevant respects. Hence, it is rational to believe that the use of the self-corrective methods of science in the long run has been, and will be, progressive in the cognitive sense. (R5)

Chakravarrty's (2007), as its title *A Metaphysics for Scientific Realism* suggests, is another attempt at synthesis he calls "semirealism." The view seeks to synthesize popular versions of scientific realism, notably entity and structural realism, along with a realism in metaphysics. Concepts in metaphysics that are to play a central role in semirealism include causation, disposition, laws and kinds.

From the perspective of small-e empiricism, there is little scope for a synthesis of scientific realism and metaphysical realism to add anything substantial to scientific realism. Whether something is real is a contingent, factual matter that can only be established by inductive inference from experience. We can attribute a distinct, factual reality to a metaphysical conception only because we have sufficient empirical evidence for it. That means that it has *already* been accommodated into empirically well-supported science. If the reality of a metaphysical conception has no such empirical support, then it is unwarranted and can add nothing factual to the realism.[3] This latter case is likely given Chakravarrty's (2024, p. 279) observation "… that metaphysical inquiry is largely a priori: metaphysical arguments proceed substantially via reflection on non-empirical considerations…"

2.4 Fragmentation

In the more recent literature, these different components are separated and variously offered individually as scientific realism. We can find just about any one of the above ideas or some combination of them identified as scientific realism. Here are some examples. They have not been collected systematically, but at least illustrate the spread of definitions.

> (Scientific) Realism. The theoretical entities of natural science do actually exist essentially as scientific theorizing characterizes them. They are real items of the world's furniture and do indeed possess substantially the descriptive constitution ascribed to them by science. (Rescher, 1987, p. xii)

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[3] Chakravarrty responds to concerns of this type in his (2024, p. 281, his emphasis): "…one of the central claims I make about scientific ontology is that all approaches to it that interpret science as yielding some sort of knowledge of the world involve metaphysics in *some* way."

I propose what I take to be a standard construal of the scientific realist position as a form of the traditional metaphysical realist doctrine that the world exists independently of the mental. The realist position is a position of epistemic optimism, which holds against the sceptic that humans are able to acquire knowledge of the world. (Sankey, 2008, p.3)

Scientific realism is the view that our scientific theories are approximately true. (Dicken, 2016, p. 1)

Scientific realism may be crudely characterised as insisting that science tells us how the world is, in both its observable and unobservable aspects… (French, 2018, p. 394)

I will understand scientific realism in what I take to be a classical sense. According to scientific realism, the aim of science is to discover the truth about the objective reality which we inhabit. Progress in science consists in advance toward this aim. (Sankey, 2018, p. 72)

Scientific realism postulates that science aims for truth in both the domains of the observable and the unobservable, and is capable of achieving this aim, at least approximately.
According to scientific realism successive theories of modern science bring us progressively closer to the truth, that is, a depiction of nature's processes and entities as they actually are. (Dieks, 2023, p. 1)

Traditionally, scientific realists have taken the perfectly sensible view that the dramatic empirical and practical achievements of our best scientific theories show that those theories must be at least probably, approximately true. (Stanford, 2018, p. 212)

> We take scientific realism to be the doctrine that most of the statements of the mature scientific theories that scientists accept are true, or approximately true, whether the statement is about observable or unobservable states of affairs. Here, "true" is to be understood in a straightforward correspondence sense, as given by classical referential semantics. (Azhar and Butterfield, 2018, p. 304)

> There are semantic and epistemic variants of scientific realism in the literature. The semantic version holds that most successful theories are (nearly) true or (approximately) so. The epistemic version holds that successful theories are warranted. Let me call the former "semantic realism" and the latter "epistemic realism" hereafter." (Park, 2022, p.1)

We can sympathize with Anjan Chakravartty who undertook the daunting task of surveying the range of present formulations of scientific realism. He wrote: (2017, Section 1)

> It is perhaps only a slight exaggeration to say that scientific realism is characterized differently by every author who discusses it, and this presents a challenge to anyone hoping to learn what it is.

The narrative continues with an attempt to inventory the many versions of scientific realism in the present literature:

> … most people define scientific realism in terms of the truth or approximate truth of scientific theories or certain aspects of theories. Some define it in terms of the successful reference of theoretical terms to things in the world, both observable and unobservable.
>
> Others define scientific realism not in terms of truth or reference, but in terms of belief in the ontology of scientific theories.
>
> Another way to think about scientific realism is in terms of the epistemic aims of scientific inquiry … . science aims to produce true descriptions of things in the world (or approximately true descriptions, or ones whose central terms successfully refer, and so on).

2.5 Type-X Realism

These last formulations of scientific realism are intended to represent a generic conception of scientific realism. Much of the more recent activity in the literature arises from a sense that the generic notion of scientific realism needs to be amended in ways that are no longer compatible with the plain label "scientific realism." This sense is indicated by the addition of an adjective that I label as an addition of "X" to "realism." Each instance of an X flags that the new proposal is more than a simple attempt to capture the generic idea. It is in some way correcting it. The correction may be a narrowing of the scope of scientific realism. Or it may be a migration to a neighboring conception that may not be a realism after all, but is a form of antirealism. As a group they are a vote of no confidence in the generic formulations for, they do not allow those formulations to stand.

The most common form of "type-X realism" is "selective realism." It is a class of views in which the assertion of reality is restricted to some elements within a successful scientific theory. They include "entity realism," "structural realism" and "explanationist realism." I will return to them below in Section 10. There are, however, views carrying the label of type-X realism that are antirealisms. They are reviewed in Section 8 below.

3. Induction in the Realism-Antirealism Debate

We have just seen that there is a multiplicity of different views concerning realism and its possible modes of failure. There can be no simple diagnosis for why this multiplicity has come about. The issues raised by realism or its failure cut to the heart of our understanding of science as an epistemic enterprise. They touch on many views and approaches and thus will elicit many responses. My concern in this chapter and the next is to consider the realism-antirealism debate in the context of small-e empiricism, where inductive inference plays a central role. The issue that arises repeatedly in the realism-antirealism debate is the use of inductive inferences and, in particular, the accommodation of inductive fallibilism.

The main claim of the following sections is that there is a misconstrual of the import of inductive fallibilism in the scientific realism literature. (The next chapter will argue for a misconstrual again in the antirealism literature.) Here we see a repeat of the misconstrual of inductive fallibilism in philosophy that was explored in an earlier chapter. Big-R Realists seek to discount inductive fallibility, assuming that it can be controlled by careful adjustments to the formulation of scientific realism. They are mistaken.

4. The Search for Permanent Truths

Strong versions of scientific realism underestimate the import of the fallibility of inductive inference. They treat its fallibility as an obstacle that can be surmounted by more careful formulations of the doctrine of scientific realism. These versions seek an assurance in the correctness of aspects of scientific theories that transcend the limits arising from the fallibility of inductive inference.

All these attempts fail for a reason that is foundational for small-e empiricism: our only means of ascertaining the truth of a contingent proposition is through inductive inference. As a quite general matter, attempts to secure assurances of truth that go beyond the fallibility of inductive inference necessarily fail.

This section reviews two attempts to tame the fallibility of inductive inference by adjustments to the notion of truth or of what we can accept as true. The first, the idea of approximate truth, fails because of its opacity. To declare a proposition as an approximate truth tells us only that something is right and something is wrong with it, but not which part is right and which is not. The second "monster-barring" declares certain possibilities as so extremely unlikely as to be beyond serious consideration. The difficulty with it is that what is inconceivable today, may be quite conceivable tomorrow, even if that eventuality is rare.

4.1 Approximate Truth

One of the most common attempts to evade the fallibility of inductive inference is to replace a claim of truth for some favored theory with one of approximate truth. The approach reflects the intuition that a stable and successful scientific theory can only be so if it has gotten something right. However, in an accommodation of the pessimistic meta-induction, realists are loath to translate the "something right" into a flat declaration of truth. In its place, "approximate truth" is used.

The decisive difficulty with the notion is its opacity. There is an extensive literature, founded mostly by Karl Popper, that seeks to provide a serviceable definition using the notions of "verisimilitude" or "truthlikeness." The literature provides conditions that allow us to say when one false theory is closer to the truth than another. A simple formulation compares the sets of true consequences derived from each. The one with the larger set is closer to the truth. This imperfect proposal comprises early steps in an expanding repertoire of proposals. It became enmeshed in continuing technical difficulties that seem, at least to me, remote from practical applications.[4]

The overall project of defining truthlikeness does not serve to rescue scientific realism from the fallibility of inductive inference. The informal idea is that a successful theory has some parts that are correct and some not. A viable definition of truthlikeness would tell us *what it means* for such a theory to have parts that resemble the truth. The difficulty is that such definitions do not tell us in any particular case, *which parts* of the theory resemble the truth. Cases to which the definition would be applied arise only when all applicable evidence has been applied inductively. To go beyond and, in addition, to designate which parts of the theory are truthlike and which not is to do more than can be done with inductive inferences from the evidence. But what inductive inference does exhausts what can be done.

There is a further problem of opacity in the way the notion of approximate truth is used in definitions of scientific realism. The idea of approximation requires some measure of closeness. In many cases, such measures are readily at hand. For example:

> The Washington Monument, a celebratory granite column in the Mall in Washington DC, is approximately 560 feet tall.

The notion of approximation used here is imprecisely given but easily made more precise. The "approximately" is satisfied if it is taken to mean "within 10 feet of the true height," which happens to be 554 feet 7 11/32 inches. Since examples like this can be given precise meaning, it is tempting to migrate the modifier "approximately" from the height of the column to the proposition overall:

> It is approximately true that the Washington Monument, a celebratory granite column in the Mall in Washington DC, is 560 feet tall.

As long as this reformulation is understood to assert exactly what the first version asserted, there is no problem. It is just a clumsy way of asserting the approximation in the height. If, however, we do not know how to reverse the migration of "approximately," the proposition has become opaque. Might we mean that the monument is only approximately in the Mall, but is perhaps

[4] For a survey, see Oddie and Cevolani (2022).

somewhere in larger Washington D.C.? Or might we mean that the monument is only approximately a column, since it is actually an obelisk, which has more precise properties. Or is that the monument is only partly granite? All we know that something might be wrong in the proposition, but we cannot say precisely what.

This opacity afflicts the use of approximate truth in scientific realism. An excessively cautious nineteenth century Newtonian might have wanted to characterize Newtonian theory as approximately true. But that Newtonian would scarcely have been able to reposition the modifier "approximately" correctly. The Newtonian did not then know which newer theories were to replace Newtonian theory. Recall the example of stellar aberration in an earlier chapter. A Newtonian would not know that the Newtonian rule of composition of velocities fails for scalar speeds close to that of light, but gives approximately correct results for angular shifts in the case of the aberration of starlight. These issues do not even touch the more severe problem of making sense of how a Newtonian particle is "approximately" a quantum particle.

4.2 Monster-Barring

Small-r realism assures us that our epistemic resources are powerful enough to secure contingent truths of a science with very little chance of error. We have good reason to expect that most of our mature, well-supported science will persist. However, small-r realism enjoins us not to take the next step, tempting as it might be. We should not try to argue away the small but enduring chance of error that remains even with the strongest of inductive support. Here we should proceed as in ordinary life. We do not live our lives in fear of dying by a lightning or meteor strike, but we cannot assert its absolute impossibility.

Further, we should not seek to provide a general formula for those sciences or parts of a science that have achieved permanence and are absolutely immune from correction. As long as the support of experience is inductive, there will always be some ineliminable chance of error, even if very small. No formula, no matter how artful, can eliminate it entirely.

A few accounts of scientific realism appear, at least on superficial reading, to succumb to the temptation. However, my sense is that, on closer reading, they prudently retain the qualification that the presumed stability of the science is still subject to a failure, even if it is very unlikely. They identify sciences in which the chance of error has become very small. The possibility of error remains, but it is discounted by a strategy that can be called "monster-barring."[5] They are such that the chance of their realization is so small that, *for practical purposes,* they can be ignored.

Hoefer's (2020) handles the problem deftly. His "tautological scientific realism" ("TSR," p.22) explicitly excludes present quantum theories from realism, as his title "Scientific Realism without the Quantum" suggests. He is surely correct in this exclusion. Although the successes of present quantum theory are widespread and extraordinary, the theory is beset with enduring foundational problems.[6] He describes the distinction crucial to TSR as: (2020, p. 22)

> The claim of TSR is not that our current science is so very successful that we should *infer* that it is probably true. That would be IBE [inference to the best explanation], and would be wrong. The claim of TSR is rather that certain parts of our current scientific lore are such that we can't conceive of any way that they could be seriously mistaken, other than by appealing to scenarios involving *radical* skepticism.

Hoefer offers present chemistry as a science in which we cannot now conceive of serious mistakes without this radical skepticism. He identifies three versions of this radical skepticism. Past regularities may simply fail to persist; or some "super-genius" may find a brilliant alternative chemistry; or we may find ourselves in a fanciful scenario such as the classic brains-in-vats fantasy.

My addition is to emphasize that these sorts of failures for a well-established theory are rare and it is simply a misplaced caution to take them seriously. However, we have no way to rule them out absolutely. There can be no viable, general principle that precludes the actuality of these extreme cases. The classical physicists of the nineteenth century did find their certainties overturned by the radical pathologies of quantum theory. It posits a radical non-locality of states in space and some interpretations of quantum theory ask to us to accept the reality of an ever-growing multiplicity of parallel worlds in which all possibilities are realized.

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[5] The term "monster-barring" is borrowed from Lakatos' (1976) *Proofs and Refutations.*

[6] The measurement problem has resisted all attempts to find a single, universally accepted resolution; and the need to renormalize in quantum field theory results from the failure of the theory at high energies.

Vickers (2023) shares Hoefer's and my optimism about the success of science. What he seeks is clearly stated in his title, *Identifying Future-Proof Science*. His goal is (p. 1) "identifying scientific claims we can be confident will last forever." Vickers is surely correct to dispute the inductive skepticism of antirealists. He quite properly calls into question Wray's blanket inductive skepticism on the future stability of scientific theories. Vickers (2023, p. 4) displays the sort of skepticism he wishes to dispute in his quoting of Wray's (2018, p.1) claim:

> I will argue that our current best [scientific] theories are quite likely going to be replaced in the future by theories that make significantly different ontological assumptions.

Vickers eventually introduces criteria for identifying future-proof science. He allows—as he must—that even future-proof science may err. He is eager to minimize the chance of this error. He sets it as very low through a comparison: (p. 217)

> The claim … is that we are as certain about the status of those scientific claims as we are about historical claims such as the claim that the Second World War really happened, the Hiroshima bomb really was dropped on 6 August 1945, and so on.

My sense, however, is that Vickers is a too optimistic in two ways. First, although he is right to expect most of his future-proof science to remain, he underestimates the chance of error. It is equated with the chance of an error in ordinary experience, such as in his example that he is (at the moment of writing) striking the keys of his laptop. If we are to take this possibility of error seriously, Vickers suggests a dire consequence follows: (p. 4)

> … if it is insisted that we aren't sure about scientific ideas on these grounds, then we have to accept that we are never sure about anything. In which case words such as 'fact', 'sure', and 'certain' are never applicable, and might as well be eradicated from the dictionary: 'Knowledge is impossible!'

What Vickers regards as an unacceptable outcome is, for me, just the right way to understand the fallibility of inductive inference, if the terms Vickers lists are to connote absolute certainty, as I have made clear in the last chapter.

The second problem is that Vickers provides sociological criteria for identifying future-proof science. It is that at least 95% of a large scientific community that entertains a diversity of perspectives accepts the claim as (using Vickers' quote marks) "established scientific fact" (p. 217). From the perspective of small-e empiricism, the only way that such claims can be established is by the inductive support of experience. They are not established by what any community, no matter how prestigious, is willing to assert. The best that can be said is that the endorsement of the scientific community is a strong indicator that the requisite inductive support is in place. However, that endorsement is only a strong indicator but is not constitutive of that support.

5. Selective Realisms

Invocations of approximate truth indicate that something, but not everything, is correct in a theory. Selective realisms are a family of more narrowly targeted attempts to identify which parts of the theory are true. This section recounts three leading versions. If all they seek to do is to extract as much as can be learned from the evidence of experience inductively, they are just a form of scientific realism with no special powers that conforms with small-r realism. Selective realisms fail when they try to outmaneuver the powers inductive inference and the restrictions of inductive fallibility. This can happen in two ways.

In the first, they seek more for the elements of a theory that they have selected as the real than inductive inference can supply. That is, they seek a level of certainty for them that goes beyond the limits of inductive fallibility. These efforts must fail. For, according to small-e empiricism, inductive inferences from experience are our only means of learning the truth of contingent propositions.

In the second, they seek to discount the inductive bearing of the evidence of experience. This discounting applies to the elements of the theory that are not distinguished as selected for the formulation of selective realism. They are not accorded reality even if there is evidence supporting their reality, which is usually the case. These efforts must also fail. For, once again, inductive inferences from experience are our sole means of ascertaining the truth of contingent propositions. We have no further means of deciding when their assessments are cogent and when they are to be ignored.

There is another problem, internal to the selective realism literature. Different versions of selective realism contradict each other on which elements of a theory are to be taken as real and which are not. Entity realism accords a fixed reality to some entities, but not the relations that govern them. Structural realism takes the reversed view. It is only these relations that persist, not the entities supposed to bear them. That there can be such marked differences in the selection of selective realism indicated the fragility of the selection.

5.1 Structural Realism

One of the most popular amendments to scientific realism is structural realism. The overall idea is simple and carries an initial appeal. It came to prominence in the work of John Worrall (1989), where the approach is introduced as a response to the pessimistic meta-induction. Drawing inspiration from earlier analyses by Henri Poincaré, Worrall accepted that only some parts of our current best scientific theories can be correct. The proposal is that we can distinguish those parts as the "structure." It is the factual or real component of the theory that survives changes in theory. Worrall's enduring example is of Augustin Fresnel's early nineteenth century recovery of various results in optics. Fresnel derived his results on the assumption that light propagates as waves in an elastic medium, the ether. As an illustration, I and R are the intensities of the components of the incident and reflected beams of light, polarized in the plane of the reflecting surface. i and r are the angles of incidence and reflection. These quantities are related by the equation:

$$R/I = \tan (i\text{-}r) / \tan (i+r)$$

The key point for Worrall's analysis is that this equation and others like it were preserved when Fresnel's theory was replaced by Maxwell's electrodynamics of light. He noted: (p. 117, his emphasis)

> Roughly speaking, it seems right to say that Fresnel completely misidentified the *nature* of light, but nonetheless it is no miracle that his theory enjoyed the empirical predictive success that it did; it is no miracle because Fresnel's theory, as science later saw it, attributed to light the right *structure*.

and then (p. 118, his emphasis)

> [Maxwell's] field in no clear sense approximates the ether, but disturbances in it do obey *formally* similar laws to those obeyed by elastic disturbances in a mechanical medium.

Worrall called this view "structural or syntactic realism" (p. 112). He recognized that this illustration from Fresnel-Maxwell was exceptional in having Fresnel's equation carried over unchanged to Maxwell's theory. He noted: (p. 120, his emphasis)

> The much more common pattern is that the old equations reappear as *limiting cases* of the new - that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit.

An illustration is provided in Ladyman and Ross' (2007, pp. 94-95) later development of structural realism. The Galilean transformation between inertial frames of reference in relative motion at speed v is defined as[7]

$$x' = x-vt \quad y' = y \quad z' = z \quad t' = t$$

It is replaced in special relativity by the Lorentz transformation[8]

$$x' = \gamma(v)(x-vt) \quad y' = y \quad z' = z \quad t' = \gamma(v)(t-vx/c^2)$$

Whereas we may justly worry that these transformations differ greatly in structure, Ladyman and Ross reassure us that: (p. 95)

> However, when ... v/c tends to 0, the mathematical structure of the latter [Lorentz transformation] clearly increasingly approximates that of the Galilean transformations.

This original conception of structural realism was soon refined and expanded into a range of views. A major division was emphasized by Ladyman (1998) and Ladyman and Ross (2007). They identified "epistemic structural realism" in which the structure of a theory is all that we can know of the theory. It was contrasted with "ontic structural realism" characterized by Ladyman and Ross (2007, p. 130) as:

> Ontic Structural Realism (OSR) is the view that the world has an objective modal structure that is ontologically fundamental, in the sense of not supervening on the intrinsic properties of a set of individuals. According to OSR, even the identity and individuality of objects depends on the relational structure of the world. Hence, a first approximation to our metaphysics is: 'There are no things. Structure is all there is.'

Worrall's original proposal was already sufficient to provoke considerable response in the literature. The ontologically adventurous proposal of ontic structural realism accelerated the

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[7] x, y, z are Cartesian spatial coordinates and t is a time coordinate. Primes indicate coordinates in a frame of reference moving at speed v along the unprimed frame's x axis in the $+x$ direction.

[8] $\gamma(v) = 1/(1-v^2/c^2)^{1/2}$

response. For surveys, see Frigg and Votsis (2011) and Ladyman (2023). Among the many issues debated, one drew special attention. If in the ontic version "structure is all there is," what are we to make of the non-structural elements of a theory? Ladyman (2023, Section 4) identifies seven different answers. They span from "(1) … there are no individuals…" to "(7) Individual objects are constructs."

The issue I wish to raise here is the great difficulty in practice in identifying structure, in both epistemic and ontic versions. The case of the Galilean-Lorentz transformations is one of the simplest, but it already delicate. There is no simple preservation of formulae in the transition from the Galilean to the Lorentz transformation. We need to consider the limit of very small v/c to find it. In so doing, we eradicate the relativity of simultaneity, which is the distinctive novelty of special relativity and at the heart of the theory's novel effects. Recovering a continuity of structure between classical, Newtonian physics and quantum theory is even more delicate since the mathematical structure of the quantum theory differs greatly from that of classical physics.

The result is that, at least in my reading of the structural realism literature, is an enduring and bewildering vagueness in exactly what counts as structure. Where Worrall identified structure syntactically with formulae, Frigg and Votsis (2011, p.231) report the identification of structure using set theory, category theory, graph theory, information theory and group theory. Ladyman (2023, Section 1) notes "Clearly, there [are] many different notions of structure, and many different kinds of structural realism."

My view is that we can identify which structure is preserved through theory change and, if the selection is well-supported inductively, we can have some confidence that this structure will persist into future science. The fallibility of inductive inference, however, precludes an unqualified assertion that we have found with finality the structure that will persist into future science. This judgment is instantiated in a study of the evolution of theories of the electron that I co-authored with Jon Bain (Bain and Norton, 2001). This study is on a short list of illustrations of structural realism offered in Ladyman and Ross (2007, p. 94).

We sought to identify the characterization of the electron that persisted through an extraordinary era of growth in physics. It started with Thomson's (1897) identification of the particle in cathode rays, through the transformations of relativity and quantum theory and then on to the quantum field theory of the standard model of particle physics. We found that the Hamiltonian or Lagrangian for the electron remained remarkably stable throughout this era of tumultuous change. This, we argued, contradicted the claim of the pessimistic meta-induction that the history of science is a catalog of failure. Rather the persistence of this one structure gives an alternative picture of the sequence of theories accumulating and sequentially improving our understanding of the electron.

5.2 Structural Realism Contravenes Inductive Inference

Our ambitions in Bain and Norton (2001) were modest. We wrote: (p. 456)

> … the history of the electron shows us that our theories are always corrigible. Although we cannot display the structure [of some imagined final theory], we can certainly display our best candidate for that structure, recognizing that its form and content are likely to change as understanding grows. At any one time in the development of theories of the electron, we can read our best candidate from the latest theory. It is simply the smallest part of the latest theory that is able to explain the successes of earlier theories.

There are two elements in this modesty, one explicit and one implicit:

- Our *assured* identification of the structure of the electron was purely retrospective. It was the best account we could provide of the stable elements in the past evolution of theories of the electron. Inductive fallibility precludes projecting that assurance into future science.
- Our analysis did not deny that, at each stage, we had good evidence for those parts of the theory of the electron that extended beyond its structure.

A version of structural realism that conforms with these two elements, limited to this particular case, would, in my view be both serviceable and defensible. However, it would offer nothing that is not already in small-r realism.[9] In this case, it simply reports the evidence that electrons *have been* characterized stably by their Hamiltonians and that it gives inductively good, but fallible reasons for expecting the characterization to persist. The import of evidence for other parts of the various, evolving theories of the electron, is not discounted.

In so far as versions of structural realism in the literature breach either of these conditions, they are in conflict with what inductive inference does and can provide.

[9] Here I join a tradition of criticism reported in Ladyman (2023, Section 5) that a defensible form of structural realism collapses into generic scientific realism.

First, where Bain and I were careful to limit the future prospects of our results, I find no corresponding caution in the structural realism literature. There, the structure identified is given a timeless character without an obligatory qualification of its corrigibility. In so far as this amounts to a declaration that the final truth has been discovered, it contradicts the fallibility of inductive inference that precludes any such final declaration.

That a timeless character is sought for structure seems to be the original motivation for structural realism and at least a part of its continuing motivation. French and Ladyman (2003, p. 33) report:

> For Worral the motivation for adopting structural realism is just the need to respond to the pessimistic meta-induction. While we share the desire to articulate a form of realism that can account for theory-change, our understanding of structural realism is also informed by the need to provide an ontology that can dissolve some of the metaphysical conundrums of modern physics, and also the need to have a conception of how theories represent the world that is compatible with the role of models and idealisations in physics.

To be clear, since the pessimistic meta-induction seeks to establish future failures of science, a response must concern itself with the prospects of future science.

Of course, I may be reading too much into the lack of a disclaimer that the timeless import of structural realism is intended only to be secure retrospectively but prospectively corrigible. However, if such a disclaimer is intended tacitly, then what results is a doctrine that fails to extend beyond small-r realism. For there is no *assured* way to identify *prospectively* the structure that will be preserved through all future theory change. Thomson in 1897 might well have imagined that future theorizing would alter his account of the electron. However, he would have had no basis for identifying the Hamiltonian specifically as that component that would endure. Small-r realism allows us to assert that, on our best present evidence, electrons are characterized by their Hamiltonians and, precisely because of this strong inductive support, we would expect this characterization to persist, but we can give no absolute assurance of it. More generally, small-r realism allows the expectation that something of the present theory will endure, if it does happen that future theories contradict present theory. But it can give no absolute assurance of what the preserved element will be.

The result is a diluted structural realism. It merely gives the anodyne assurance that something will remain unchanged and something will not, if a new theory happens to replace the present theory. What it cannot do is to provide any final characterization of what will assuredly remain unchanged. That is, it cannot provide an independent theory of structure with substantive content. For that would be to anticipate the specific character of all future theories, in contradiction with the fallibility of inductive inference.

The second breach with small-r realism is in the *denial* of the import of the inductive support for the non-structural parts of a theory. Epistemic structural realism denies that these non-structural parts can be known; and ontic structural realism denies that they can be real. What may tempt structural realists to this extraordinary denial is that, when present sciences replace past sciences, the denial minimizes the corrections to the inventory of what is real. Thomson may have thought that he had identified electrons as charged, classical particles. However, structural realists tell us that he was not authorized to this belief. He was only authorized to accept the structure, even though he could not then have known what it was.

This denial by structural realists is irrational in the direct sense that it contravenes the dictates of rationality as encoded in an inductive logic. As a general matter, in any well-developed science, the scientists have good evidence from experience for all parts of their theory. Thomson's (1897) "Cathode Rays" is celebrated as the definitive identification of electrons as charged particles, as opposed to Lenard and Hertz's identification of them as ethereal waves. Norton (2021, Ch.9, Section 6) reviews in some detail how Thomson's arguments in that and associated papers established his identification to the satisfaction of the then present physics community. The bearing of the evidence on the non-structural parts of a theory in such cases cannot be discounted because it is inconvenient for structural realism.

5.3 Clarifying Structural Realism[10]

These last remarks constitute my best efforts to circumscribe and assess the state of structural realism. What has made these efforts difficult is a lack of coherence in the structural realism literature itself, especially after the introduction of ontic structural realism. One manifestation is the enduring proliferation of different accounts of the nature of structure.

[10] I thank Aiden Meyer for helpful discussion that led to the writing of this subsection.

Another is an evident tension between claims made for structural realism. For example, French and Ladyman (2011, p. 32) write:

> The job of predicting what will be preserved and what abandoned by future science belongs to science itself not to philosophy, …

This is a most prudent assertion, quite compatible with small-r realism. However, the sentence continues with a "but" that undermines the prudence:

> … but our claim is that from a structuralist point of view it is possible to explicate the continuity in scientific theories that often does not hold at the level of objects and properties.

What they had in mind had already been laid out in text earlier in the paragraph. They first noted that some structure in Newtonian physics had not been preserved in the transition to special relativity. They continued: (pp. 31-32, my emphasis)

> The advocate of OSR is not claiming that the structure of our current theories will be preserved simpliciter, but rather that the well-confirmed relations between the phenomena will be preserved in at least *approximate* form and that the modal structure of the theories that underlies them, and plays the appropriate *explanatory* role, will also be preserved in *approximate* form.

Ontic structural realism, they clearly assert, predicts the preservation of something in future science. What precisely that will be is now hedged into vagueness by the terms I have highlighted. Section 4.1 above recalled the troublesome ambiguity associated with the modifier "approximate"; and the next chapter will recount the corresponding problem for "explanatory."

Ladyman, apparently, was quite comfortable with this formulation. In his later (2021, p. 246), he quotes it as an expression of his and French's joint view. The quoting of this passage is then immediately followed by an assertion of the claim foreshadowed by the title of the 2021 paper, "Structure not Selection," that ontic structural realism is not a selective realism.

> However, this does not enable any kind of selective realism because it says nothing at all about how the laws of the theory are only approximate, and so does not enable the selection of what will be retained in advance.

The passage Ladyman quotes gives a prescription, albeit ambiguous, for which parts of a science will be preserved in future science; and that is a just what a selective realism can do.

5.4 Entity Realism

Entity realism asserts that we should be cautious asserting the truth of a theory. However, it asserts, the prospects are much better when it comes to asserting the reality of entities within a theory. We find the same misalignment with inductive inference as with structural realism. On the one hand it seeks to go beyond what inductive support can provide in assuring us of the reality of certain, selected entities. On the other hand, it contravenes the import of inductive support in denying reality to anything else.

Since Hacking (1982, 1983, 1989) is the most prominent proponent of entity realism, we can explore his specific arguments in greater detail. Hacking employs Putnam's referential model of meaning to provide theoretical entities like electrons with some independence from the theories in which they appear. Hacking (1982, pp. 73-75; 1983, Ch. 6) employs the resources of Putnam's theory to argue that the many theories of the electron, posited over decades, were all referring to the same entity, while attributing different properties to it.

More is needed if we are to affirm that the entity is real. Many authors have written about the same character, Sherlock Holmes, and have given him different roles in their fictional writing. He is still not real. To establish the reality of his core example, the theoretical entity electrons, Hacking reports the experiences of experimenters who use electrons to create further effects. We, in his remarks, are to identify with the experimenters. He gives the same characterization in his two accounts. Hacking sets the entire passage in italics to emphasize its importance: (1982, p. 77; 1983, p. 265)

> *We are completely convinced of the reality of electrons when we regularly set out to build and often enough succeed in building—new kinds of device that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature*.

This is the repeated theme of Hacking's narrative. Experimenters are convinced of the reality of electrons and the experience of using electrons in experiments is convincing:

> The experimenter is convinced of the reality of entities some of whose causal properties are sufficiently well understood that they can be used to interfere *elsewhere* in nature. (1982, p. 75, his emphasis)

> The experimentalist does not believe in electrons because, in the words retrieved from medieval science by Duhem, they "save the phenomena." On the contrary, we

> believe in them because we use them to *create* new phenomena, such as the phenomenon of parity violation in weak neutral current interactions. (1982, p. 84, his emphasis)
>
> Experimenting on an entity does not commit you to believing that it exists. Only manipulating an entity, in order to experiment on something else, need do that. (1983, p. 263)

What is affirmed here, again and again, is a powerful psychological effect. Experimenters cannot be anything other than realists about electrons in order to design and carry out their experiments.[11]

What is missing from Hacking's analysis is a sustained analysis of whether this experimentalists' conviction is well-founded evidentially. Mere reports of the firmness of their conviction is an indicator that there might be such a case to be made. But the reports do not make the case.[12]

Hacking's narrative aims to privilege the reality of entities used in interventions over other realities. He wrote (1982, p. 71) "The experimentalist need only be a realist about the entities used as tools." The difficulty is that the experiences Hacking reports occur in many other contexts. When one learns special relativity, it is astonishing how often its two simple postulates generate effect after effect in physics. Particle physicists have found again and again that the standard model of particle physics accommodates a steady stream of new accelerator results. General relativists find again and again that when any competitor to general relativity can be put to the test, general relativity wins. The same is true for engineers who successfully apply Newton's mechanics to system after system. In all these cases, the theorists develop a realist attachment to the core posits of each of these theory.

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[11] Hacking's repeated reports do not fit with his remark: (1982, p. 71) "But experimenters are realists about the entities that they use in order to investigate other hypotheses or hypothetical entities. That is not a sociological fact."

[12] In efforts to reconstruct Hacking's argument, Miller (2016) found four possible arguments and one non-argument.

We do not need to pause here to assess whether these convictions are sustained by a viable inductive argument from the evidence. For present purposes we need only this. IF there is a viable argument form here, then there is nothing special about the entities used in interventions. Hacking has identified a quite general inductive argument form that extends well beyond entity realism. It has returned us to a generic scientific realism that authorizes a wide range of realities.

Hacking does insist that what makes the experimental intervention case special is that it depends on the appreciation of the causal powers of the entity and their use in interventions. He wrote: (1982, p. 86)

> We can, however, call something real, in the sense in which it matters to scientific realism, only when we understand quite well what its causal properties are.

And again

> Experimenting on an entity does not commit you to believing that it exists. Only *manipulating* an entity, in order to experiment on something else, need do that. (1983, p. 263, his emphasis)

I did not see how Hacking made the case for this beyond merely repeatedly asserting it. The experiences of special relativists, general relativists, particle physicists and classical physicists are just as compelling.

5.5 Entity Realism versus Structural Realism

It is a most curious aspect of the selective realism literature that two of its most prominent versions stand in direct contradiction with each other. Structural realism elevates not just the general theory, but a particular element in it, the structure. Epistemic structural realism precludes knowledge of the unobservable entities that bear the theoretical structure. Frigg and Votsis' (2011, p. 232, their emphasis) give this definition: "*Epistemic structural realism* (ESR) is the view that all we can know about the unobservable world is its structure." Ontic structural realism is more severe. Ladyman and Ross offer the memorable slogan above "There are no things. Structure is all there is." For structural realists, the entities are either opaque to us or fictional.

Hacking, in his entity realism inverts this position. The equations are variable, but the entities are fixed. They are the true realities. Hacking noted: (1982, p. 72)

> The experimenter is convinced of the existence of plenty of "inferred" and "unobservable" entities. But no one in the lab believes in the literal truth of present

> theories about those entities. Although various properties are confidently ascribed to electrons, most of these properties can be embedded in plenty of different inconsistent theories about which the experimenter is agnostic.

He adds in the later publication: (1983, p. 264)

> There are a lot of theories, models, approximations, pictures, formalisms, methods and so forth involving electrons, but there is no reason to suppose that the intersection of these is a theory at all. Nor is there any reason to think that there is such a thing as 'the most powerful non-trivial theory contained in the intersection of all the theories in which this or that member of a team has been trained to believe'. Even if there are a lot of shared beliefs, there is no reason to suppose they form anything worth calling a theory

5.6 Explanationist Realism (Indispensabilism)

A third variety of selective realism has come to be called "explanationism." Here is how it is introduced in Chakravartty's survey: (2017, Section 1.3, his emphasis)

> These species of generic realism can be viewed as falling into three families or camps: explanationist realism; entity realism; and structural realism. There is a shared principle of speciation here, in that all three approaches are attempts to identify more specifically the component parts of scientific theories that are most worthy of epistemic commitment. *Explanationism* recommends realist commitment with respect to those parts of our best theories—regarding (unobservable) entities, laws, etc. that are in some sense indispensable or otherwise important to explaining their empirical success—for instance, components of theories that are crucial in order to derive successful, novel predictions.

The label "explanationism" is misleading. The key thesis is of an *indispensability* of the components of a theory that are responsible for its success. These components, loosely speaking, explain why some theory has been successful in spite of it harboring other components that are false. Proponents of this indispensabilism are not advocating an explanation-based abductive account of inductive inference.

The core of the view is that we can identify which elements of an otherwise false theory are responsible for its successes; and that we are entitled to attribute reality to them. In this form, the view is a more relaxed version of structural realism. Structural realism identifies these real elements as the structure of a theory and seeks to give structure a more specific characterization. Indispensabilism merely asserts that there are such elements and they are to be identified by their indispensable role in generating empirical successes. In this formulation, the thesis is one of inductive inference: we are authorized to infer to these elements because of their indispensability.

Because of its close affinity with structural realism, its assessment here is similar. Indispensabilism provides no viable addition to small-r realism. This is the benign reading of indispendabilism. In its narrowest form it may merely identify its indispensable elements *retrospectively* as truths about past and present theories. It may also enjoin us to infer to the likely persistence *prospectively* of the indispensable elements in future science. As long as this inference proceeds within the limits of inductive fallibility, it conforms with small-r realism. What it adds is a detail in the specific form the inductive inference is to take as one founded in indispensability.

There is a less benign reading that may be suggested but is left somewhat open. It is the proposal of an enduring truth that the presently indispensable elements will assuredly persist in future theories. If that stronger claim is intended, it contradicts the fallibility of inductive inference and is unsustainable.

A version of this indispenabilism is found distributed through Kitcher (1993, Ch.5). He remarks, for example, "No sensible realist should ever want to assert that the *idle* parts of an individual practice, past or present, are justified by the success of the whole." (p. 142, his emphasis). A more fully elaborated and best known version of indispensabilism was provided in papers by Psillos (1994, 1996), whose essential content was reproduced and developed in his monograph (1999). He describes the view as: (1999, p. 103)[13]

> Put positively, it is enough to show that the theoretical laws and mechanisms which generated the successes of past theories have been retained in our current scientific image. I shall call this the *divide et impera* move. It is based on the claim that when

[13] In the strategy of *divide et impera* ("divide and conquer"), famously used by Julius Caesar in his subjugation of Gaul, victory is secured by fomenting and exploiting division in one's enemies. The connection to indispensabilism seems slender as does the desirability of employing a label that routinely applies to the invasion and subjugation of one's neighbors.

> a theory is abandoned, its theoretical constituents, i.e. the theoretical mechanisms and laws it posited, should not be rejected *en bloc*. Some of those theoretical constituents are inconsistent with what we now accept, and therefore they have to be rejected. But not all are. Some of them have been retained as essential constituents of subsequent theories. The *divide et impera* move suggests that if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of scientific realism can still be defended.

The upshot of this move is a response to the pessimistic meta-induction: (p. 104, his emphasis)

> The right assertion seems to be that the genuine empirical success of a theory does make it reasonable to believe that the theory has *truth-like constituent theoretical claims*.

Where Kitcher's analysis is concerned with the referential success of terms, Psillos' emphasized that his account concerns empirical support for certain parts of a theory: (2022, Section 2.5)

> The underlying thought is that the empirical successes of a theory do not indiscriminably support all theoretical claims of the theory, but rather the empirical support is differentially distributed among the various claims of the theory according to the contribution they make to the generation of the successes.

The account is, at its core, a contribution to inductive logic.

5.7 Indispensabilist Inductive Logic

Psillos' version of indispensabilism posits that the empirical evidence can selectively favor different parts of a theory. That such favoring can be identified on a case by cases basis is plausible. It is, in general, difficult and delicate. Judgments of which parts of a theory deserve credit for its success tend to be fragile and are commonly revised when the theory is superseded. In the case of stellar aberration from the last chapter, Bradley could very plausibly have attributed the success of his aberration formula to the rule of Newtonian addition of velocities. After Einstein's development of special relativity, the success is judged as due to something narrower: that Newton's rule happens to coincide with the relativistic rule only in the direction of composed velocities for the particular case of aberration.

With this delicacy recognized, it would be an important advance for realism if an indispensabilist analysis could give a general rule for identifying such favored elements. Psillos does offer such a rule: (1999, p. 105)

> When does a theoretical constituent *H* indispensably contribute to the generation of, say, a successful prediction? Suppose that *H* together with another set of hypotheses *H′* (and some auxiliaries *A*) entail a prediction *P*. *H* indispensably contributes to the generation of *P* if *H′* and *A* alone cannot yield *P* and no other available hypothesis *H** which is consistent with *H′* and *A* can replace *H* without loss in the relevant derivation of *P*.

This rule has some initial plausibility. It is, however, precisely the sort of general inductive rule that is precluded by the material theory of induction. Within a much more extensive critical appraisal of Psillos' proposal, Lyons (2006, p. 540) points out the immediate weakness of the rule. It generally proves easy, in every case, to find many alternative hypotheses that can serve as the *H** of the rule. It follows that no *H* is indispensable. Psillos had already recognized the threat in his original narrative and had proposed a solution: (1999, p. 105)

> … if we impose some natural epistemic constraints on the potential replacement—if, for instance, we require that the replacement be independently motivated, non ad hoc, potentially explanatory, etc.—then it is not certain at all that a suitable replacement can always be found.

This is the first step in a mode of failure for proposals for universally applicable rules of inductive inference that, through my work on the material theory of induction, I have found to be quite common. Counterexamples are found to an initially plausible rule. Extra clauses are added to protect the rule from the counterexamples. Psillos' extra clauses are only vaguely indicated. Were they specified more carefully, we would find further counterexamples for the modified rule; and the process would continue indefinitely.

Psillos' rule is an example of what I have called "hypothetical induction" in my survey (Norton, 2005). Simple versions of hypothetical induction suffer the same problem as Psillos'. There are too many hypotheses that entail the evidence. An entire industry of additions to the original rule grew up in unsuccessful attempts to preserve the universal applicability of a modified version of the rule.

5.8 Indispensabilism and Sadi Carnot's *Réflexions*

That identifying the indispensable part of a theory is far from easy is illustrated by the first of Psillos' two major illustrations. Sadi Carnot's (1824) *Réflexions sur la Puissance Motrice du Feu* is one of the most extraordinary works of science of all eras. In a few short pages it lays the foundations for a new science, thermodynamics, along with novel methods of analysis of great subtlety. It succeeds in doing so while working with an incorrect theory of heat, caloric, as a conserved substance and without positing an independent second law of thermodynamics.[14]

Psillos (1994; 1999, Ch.6) seeks to show that Carnot's success did not depend on any indispensable elements that were not passed over to the subsequent formulations of thermodynamics in which heat is merely a form of energy that can be converted into work. Psillos' investigation of the history of Carnot's work is thorough and commendable. However, his logical analysis of which parts of Carnot's work were indispensable for the success of Carnot's analysis and were carried forward to later theories is mistaken. Here is how Psillos summarizes these efforts: (1996, p. 310)

> It turns out, for instance, that Carnot's explanation of the fact that maximum work is produced in a Carnot-cycle employed only the principle of the impossibility of perpetual motion and not the assumption that heat is a material substance

Psillos emphasizes that the assumption of the conservation of heat is dispensable: (1999, p. 119)

> Hence, Carnot did not appeal to any assumptions about the conservation of heat in order to establish his law.

This appraisal is incorrect on all points. The decisive element in Carnot's determination of the conditions for the recovery of the maximum work is his general depiction of a heat engine as operating by passing heat from a hot source to a cold reservoir. Without it, Carnot's analysis cannot be set up. Carnot used this conception since he assumed heat to be conserved. It followed that the heat in a heat engine had to go somewhere and it was only the motion of heat from hot to cold that facilitated the generation of motive power.

[14] For my analysis of what is extraordinary in Carnot's work, see Norton (2022). The concept of a thermodynamically reversible process is a proposal of the most profound foundational importance to thermodynamics.

This foundational posit was derived from the assumption of the conservation of heat. If we conceive of heat as transformable to work, there is no need for this posit. The obvious default is that the maximum work energy that can be recovered is just the energy of the heat itself. That amount greatly exceeds the maxima recoverable with a Carnot-style analysis. In this sense, Carnot's (false) assumption of the conservation of heat was indispensable to his analysis.

Psillos correctly notes that Carnot's argument employs the impossibility of a perpetual motion machine. However, he is mistaken in suggesting that it is indispensable for the later recovery of Carnot's results, such as in William Thomson's (1852) celebrated papers. In them, the assumption of the conservation of heat is discarded and it is allowed that heat can be converted to and from work energy. The impossibility of a perpetual motion machine does appear as what becomes the first law of thermodynamics. It appears as Thomson's (1852, p.11) Prop. I. The first law alone is an insufficient basis for deriving Carnot's results on the maximum of work recoverable from a heat engine. The second law of thermodynamics must be posited independently if we are to arrive at these results. Accordingly, Thomson (1852, pp. 12-14) needed to introduce extra posits, inspired by Carnot's analysis, as assumptions so that he could recover the limits Carnot deduced in his earlier work.

6. Conclusion

The previous chapter argued that we need to reduce the formulation of scientific realism to accommodate inductive fallibility; and that small-r realism provides a sustainable reduction that cannot be expanded. This chapter has sought to support this claim in light of the present proliferations of writing on scientific realism. It reviews the range of stronger formulations of scientific realisms in the present literature. It finds that formulation stronger than small-r realism fail when they go beyond small-r realism.

The enormity of the present literature in scientific realism and the great variety of proposals has made this a daunting task. My hope is that the survey has collected enough of the variety of formulations do that contact, even if weak, has been made with most serious proposals. I apologize in advance to those who find their favorite formulation slighted. My sense, however, is that the survey has captured the major strands of the present literature. Some views that carry the name "realism"—"perspectival realism" and "realistic realism"—will be discussed in the next chapter, since they are, in my view, versions of antirealism.

Finally, I have ample anecdotal evidence of a common reaction to small-r realism. Almost everyone who favors scientific realism has a version they find appealing. The reaction is initial disbelief that this favorite version overreaches and that we need to retract from it. This chapter may not convince everyone, but at least it conveys why I see there to be overreaches.

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