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Chapter 16

Small-r realism[1]

1. Introduction

The debate between scientific realism and antirealism has held a central position in the philosophy of science since the middle of the twentieth century. It is driven by two competing intuitions. Realists are guided by a triumphalist sentiment that our best science has enjoyed extraordinary success in discerning contingent truths of the world, where those truths extend from the prosaic to the profound. Antirealists regard such triumphalism as overreaching and counsel a more skeptical attitude to science's putative successes.

Something like the scientific realist intuition is correct, in my view. The long-standing problem has been to find a precise and sustainable formulation of it. This chapter seeks to establish that small-e empiricism solves the problem. It is already the strongest formulation of scientific realism that is admissible within the bounds of the fallibility of inductive inference. Since it concedes this fallibility to antirealists, it amounts to a view that successfully mediates between the extremes of an excessively optimistic realism and excessively pessimistic antirealism.

What will sustain this mediating view, I shall argue in Section 2 below, is that there are two parts to the scientific realists' intuition. The first is that we have the epistemic means to learn core, contingent propositions in a science. The second is that individual sciences have successfully employed these means. This first part only is amenable to a formulation as a general doctrine of scientific realism. Determining the truth of the second part lies beyond what a general doctrine can achieve. It must be determined in a case-by-case analysis. The enduring failure of formulations of scientific realism lies in unsuccessful attempts to find a general formulation that implements this second part.

[1] My thanks for Howard Sankey for helpful comments on this chapter.

Section 3 reports that small-e empiricism already embodies a formulation of scientific realism that conforms with this first part. The formulation, here called "small-r realism," identifies inductive inference as the uniquely available epistemic means. Crucially, small-r realism makes no assertion that any particular science has attained a final truth. Rather it asserts that sciences have the means to do this through inductive inferences from experience. The extent to which any particular science has succeeded in applying the means is left to the individual science. The complications of the fallibility of inductive inference are thereby relocated to where they belong: in the case-by-case analysis of the inductive support of the individual sciences. This particularism extends to how strongly supported propositions are to be interpreted. Whether such a proposition should be interpreted literally, as some versions of scientific realism require, should also be left to a case-by-case analysis. The dangers of assuming otherwise are illustrated by the notorious complications of interpreting the meaning of entropy.

The resulting small-r realism is essentially coincident with small-e empiricism. It adds only a reminder of the fallibility of inductive inference. Thus, as Section 4 reviews, small-e empiricism/small-r realism moderates the extremes of empiricism and realism into a single, mediating position that preserves the best of both views.

Section 5 offers a short review of the bewildering proliferation of views in the present literature that carry the label "realism." Some 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. Small-r realism offers such a repair. It implements, in a compact and narrow form, the original intuition underlying scientific realism.

Section 6 introduces the project of the remainder of the chapter. It is to assess how well the realism and antirealism literature accommodates the core concept of small-e empiricism/small-r realism: inductive inference. In sum, antirealist views attempt to amplify inductive fallibility into inductive skepticism; and 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 both problems.

Antirealism of most forms, it is urged in Section 7, relies on a misappraisal of inductive inference. It comes in many forms. The so-called underdetermination thesis is recovered from a naïve form of hypothetico-deductive confirmation. Van Fraassen's failed attempt to impugn the

inductive support for science employs a correct but misdirected concern that explanatory prowess need not indicate truth. A simpler "all or nothing" argument depends on a false dilemma: either inductive support is inerrant or a complete failure. The most enduring argument for antirealism is the pessimistic meta-induction. It tries to establish that present and future science must be forever poorly grounded since there have been notable past failures. On closer examination, the pessimistic meta-induction itself fails in every way that inductive inferences can be assessed. Most importantly, it pre-emptively discounts the strength of present evidence for all sciences without even considering that evidence.

Section 8 continues the analysis of antirealism by reviewing two views that carry the label "realism" but prove, on closer inspection, to be forms of antirealism: perspectival realism and Chang's "realistic realism."

The review of optimistic attempts by scientific realists to diminish the import of inductive fallibilism begins in Section 9. 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 10 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 by discounting the import of inductive inference for those parts of a theory not selected; and exceeding 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 11 contains a short, summary conclusion.

2. How to Formulate Scientific Realism: Escaping Fallibilism

2.1 A Division of Labor

How can we take the triumphalist intuition that science has successfully discerned deep truths of nature and give it a precise formulation? A primary concern is that the formulation should accommodate the fallibility of inductive inference. My sense is that the literature in scientific realism has met only with limited success in this task. Common formulations tend to skirt around a successful formulation without getting it precisely right.

The key to a successful formulation is to recognize that the intuition underlying scientific realism has two parts:

Core Intuition of Scientific Realism

- Our epistemic resources are powerful enough to inform us of deeper truths of science.
- Our present sciences have used these resources effectively to learn these truths.

The first part only is amenable to a formulation of scientific realism as a general philosophical doctrine. For it has to express something general and robust through time that will apply across all the sciences. The second part depends on the specific state of individual sciences at specific times. Whether a particular science at a specific time meets the conditions of this second part must be determined on a case-by-case basis. It is not within the scope of a general philosophical doctrine. The case-by-case analyses must be carried out in the individual sciences themselves or in the history of science.

2.2 Inductive Support of Experience as the Means

Small-e empiricism supplies a single, unique means as the epistemic resource of the first part. It is inductive inference; and small-e empiricism affirms that this resource does have the capacity to inform us of these deeper truths. That inductive inference is fallible does not undo this capacity. Rather, it just means that the support might err, but that the chance of an error can be made very small, when the inductive support is very strong.

Whether the inductive support for some contingent proposition in science has failed is not something that can be determined by general principles. In the case of very strong inductive

support, such failure is very unlikely, but can happen. If we have very many contingent propositions, all enjoying strong support, then we can still expect that one among the very many will fail. That is as much as can be said at the level of general principles and as much as can be incorporated into a general formulation of scientific realism.

There is nothing especially perilous in this epistemic situation. It is quite familiar from ordinary life. A 20-year-old can, on average, expect to live a further 55.63 years.[2] That is 20,319 days. As time passes, each day, that person will make an inductive inference to the conclusion that this day is not their last. The inference will, on average, yield a correct conclusion 20,318 times and an incorrect result once.

Anything further must be handled on a case-by-case basis by an examination of the particular circumstances of the contingent proposition at issue. Commonly, we find in history of science that such unlikely failures are revealed by the discovery of new evidence. As much as centuries of time may pass before this new evidence emerges.

Where existing formulations of scientific realism err is that they try to find some way of incorporating both parts of the initial intuition into their formulations. They are, in effect, engaged in a futile attempt to evade the fallibility of inductive inference. We shall review some of them below. Briefly, these formulations may attribute not truth but a nebulous "approximate truth" to well-supported contingent propositions. Or they may try to select out that part of a present theory that will endure, while being unable unambiguously to identify precisely which those parts may be. Or they may speculate on future successes. If a science has not secured the truth today, we are assured that it is on the way to the truth and that scientific progress will take us there eventually.

2.3 Feynman on Flying Saucers

That its fallibility is not fatal to science is widely recognized in the sciences, but most commonly only tacitly. Here is an instance of its explicit recognition by Richard Feynman, a Nobel laureate

[2] Statistic for 2022 from US Social Security Administration.
https://www.ssa.gov/oact/STATS/table4c6.html

and founding figure in twentieth century quantum theory. (Feynman, 1965, pp. 165-66, his emphasis)[3]

> Some years ago I had a conversation with a layman about flying saucers—because I am scientific I know all about flying saucers! I said 'I don't think there are flying saucers'. So my antagonist said, 'Is it impossible that there are flying saucers? Can you prove that it's impossible?' 'No', I said, 'I can't prove it's impossible. It's just very unlikely'. At that he said, 'You are very unscientific. If you can't prove it impossible then how can you say that it's unlikely?' But that is the way that *is* scientific. It is scientific only to say what is more likely and what less likely, and not to be proving all the time the possible and impossible. To define what I mean, I might have said to him, 'Listen, I mean that from my knowledge of the world that I see around me, I think that it is much more likely that the reports of flying saucers are the results of the known irrational characteristics of terrestrial intelligence than of the unknown rational efforts of extra-terrestrial intelligence.' It is just more likely. That is all. It is a good guess. And we always try to guess the most likely explanation, keeping in the back of the mind the fact that if it does not work we must discuss the other possibilities.

2.4 Early forms of Scientific Realism

Versions of these ideas were already present in formulations of scientific realism from the mid twentieth century, at the outset of the present debate. For example, Boyd (1973, pp.1, 3) gave a formulation marred only by its inclusion of an appeal to approximate truth:

> By scientific realism I mean the doctrine that the sort of evidence which ordinarily counts in favor of the acceptance of a scientific law or theory is, ordinarily, evidence for the (at least approximate) truth of the law or theory as an account of the causal relations obtaining between the entities quantified over in the law or theory in question.

and

[3] For video of Feynman reading this text see https://www.openculture.com/2011/08/richard_feynman_the_likelihood_of_flying_saucers.html

> … experimental evidence for a theory is evidence for the truth of even its non-observational laws and, hence, for the truth of observational predictions deduced from them, since deductive inference preserves truth.

The idea of a division of labor between philosophers and scientists is clearly articulated in J. J. C. Smart's (1963, p.27, his emphasis) monograph on scientific realism. He defends what we would now call an "entity realism" and notes:

> I shall try to uphold a realistic view of these theoretical entities, and will try to establish that the elementary particles of physics are just as respectable entities as tables and galvanometers. By 'just as respectable' here I mean '*philosophically* just as respectable'. Unicorns are philosophically just as respectable as cows: it is for the zoologist, not the philosopher, to decide that there are no unicorns. Similarly, the physicist may come to replace his present theory of elementary particles with some other theory of what goes on at the sub-atomic level. Nevertheless, he will presumably postulate some new set of theoretical entities to replace the old ones. It is not for the philosopher to decide between one physical theory and another any more than it is up to him to become a zoologist and decide for or against the existence of unicorns. My problem is concerned with the ontological status of the physicist's theoretical entities, whatever they are.

3. small-r realism

3.1 The Formulation

These last reflections delineate a version of scientific realism that lies within the existing commitments of small-e empiricism. In more compact form, it is:[4]

> *small-r realism*: the propositions of experience can provide inductive support for the truth of the contingent propositions of the sciences. This inductive support can be very strong, but can never rise to absolute certainty; and no other means can provide that certainty.

This formulation is largely a restatement of the core doctrine of small-e empiricism, as formulated in Chapter 12. The addition is an explicit acknowledgment of the fallibility of

[4] These contingent propositions include what are commonly called scientific laws, for, within small-e empiricism, they are just contingent propositions of very broad scope.

inductive inference, as explored in Chapter 15. It is merely a reminder of what is part of the definition of inductive inference, its fallibility.

Small-r realism simply affirms that inductive inferences are a sufficient means for securing, fallibly, contingent propositions of a science. The doctrine cannot and should not assert that any of our latest science has actually secured such propositions. For there are many sciences and some are in active evolution.

A case-by-case analysis of individual sciences is encouraging. Many have secured very strong evidential support for their core, contingent propositions. If we add this fact to small-r realism we arrive at the optimistic result amenable to scientific realists: the world is most likely truly as these sciences say. We have strong inductive support for the contingent proposition in cosmology of the expansion of the universe from a big bang. We infer that it is very likely that this expansion truly is the case. This optimistic result can be recovered for central propositions drawn from across the sciences. It cannot be recovered universally. Evidential support in some sciences is weak. No theory of quantum gravity enjoys evidential support comparable in strength to that of, say, ordinary chemistry. All such theories must be treated as speculative.

3.2 Interpretive Autonomy of Science

An important consequence of small-r realism/small-e empiricism is the interpretive autonomy of science. The autonomy requires the failure or superfluity of any general principles of interpretation, such as a realist's blanket injunction to read the propositions of mature sciences literally or an antirealist's blanket injunction that these propositions cannot be read literally. For how such propositions are to be interpreted is itself a factual matter for the science. Small-r realism/small-e empiricism asserts that our only means of assessing all such factual matters is by the inductive support of experience.

We may conceive a general principle of interpretation that goes beyond what the inductive support of experience supplies. Since the inductive support of experience gives us all the support we can have, the principle will fail, in so far as it goes beyond what inductive inference can support. If it does not go beyond what inductive inference can support, then it is superfluous. Anything it can contribute has already been contributed by the inductive support of the propositions from experience.

Nothing fancy is intended by the term "interpretation" in the interpretive autonomy of science. It is just what we take the propositions of a science to mean in a quite ordinary sense.

For mature science, it is generally unproblematic to take the interpretation simply to be what the propositions assert literally. When astronomy tells us that our planetary system is heliocentric, it tells us just what is says, that the orbits of the planets are centered on the sun.

Although this is true generally, it is not universally true. We must always allow for the possibility that the literal interpretation is incorrect. A straightforward case arises with effective theories. An example is the theory that gases and liquids are continuous fluids whose behavior is governed by the Navier-Stokes equation. It is very well supported inductively by experience. However further experience tells us that the theory is only true of averaged behaviors, after the molecular granularity of gases and liquids has been smoothed out. The theory's assertion of continuity is not a literal truth, since a literal truth requires the infinite divisibility of the fluid. The inductively well-supported atomic theory of matter denies this infinite divisibility.

3.3 The Interpretation of Entropy

One presently intractable example is enough to illustrate the delicacy of problem. When water boils to steam at 100C and 1 atmosphere pressure (101.42 kPa), its internal energy per unit mass u changes by[5]

$$\Delta u = 2{,}087 \text{ kJ/kg};$$

and its entropy per unit mass s changes by

$$\Delta s = 6.0471 \text{ kJ/(kg.K)}.$$

Both of these quantities, u and s, are core theoretical terms in thermodynamics. The propositions asserting both changes are very strongly supported by experimental measurements on the more directly measurable properties of water and steam, such as their temperatures, pressures and densities, and by the measurable quantities of heat supplied to them. The entropy change Δs is

[5] Data from Parry *et al.* (2014) Table S-1. It gives the value of Δs directly and the enthalpy change per unit mass $\Delta h = 2{,}256.5$ kJ/kg. The internal energy change per unit mass Δu is calculated from $\Delta h = \Delta u + p\Delta v$, using the pressure $p = 101.42$ kPa and the volume change per unit mass $\Delta v = 1.6708$ m^3/kg.

computed[6] as the heat supplied in (reversibly) vaporizing the water to steam Δh divided by the absolute temperature T, $\Delta s = \Delta h / T$, where $T = 373.15$K.

If approached at a more general level, the similar propositions asserting the values of Δu and Δs would naturally both be interpreted literally. A literal interpretation of the value of Δu is strongly supported by all the evidence that has repeatedly affirmed that energy is a conserved quantity passed between materials undergoing changes. It is a commonplace of thermodynamics that the energy content of the water increases as indicated, whether the change is considered phenomenologically or statistically in light of the molecular constitution of water.

Tempting as it is, no similar literal interpretation of the value of the entropy change Δs is sustainable. The question of just how we are to think of entropy has remained troublesome since its introduction by Clausius in 1865. Is it a real physical quantity like internal energy? Is it a measure of accessible energy? Is it a merely instrumental tool for gauging the temporal direction of processes? Is it a purely effective magnitude resulting from our limited control of molecular-scale processes? Is it a parameter of the probability distribution for energy over the system's components? Is it a measure of the probability of a state or of the randomness of a state? Is it a measure of information in the spirit of Shannon's information theory? Or is it a quantity best understood by conceiving thermodynamic processes as computations? These are open issues over which debate endures.[7]

The proposition $\Delta s = 6.0471$ kJ/(kg.K) is as strongly supported by the evidence and as good a candidate for truth as any in thermodynamics. It has been an unchallengeable result since the nineteenth century. However, there is no agreement on what is its literal construal.

4. A Single Mediating Position

Small-r realism provides a view that mediates as successfully as possible between the strictest versions of scientific realism and antirealism. It preserves as much of the original intuitions behind scientific realism, while discarding those parts incompatible with the fallibility

[6] The heat of vaporization is given by the enthalpy change $\Delta h = \Delta u + p\Delta v$, where Δu is the internal energy increase of the steam and $p\Delta v$ is the energy lost to work in expanding the steam by a volume Δv against the resisting pressure p.

[7] I have written extensively in the debate. See Norton (2025) for a recent contribution.

of inductive inference. In not endorsing the blanket truth of our mature sciences, it concedes to some of reservations of antirealism, while not succumbing to a pessimistic overreaction to inductive fallibilism. That the commitments of small-r realism coincide with those of small-e empiricism is a welcome result. For it tells us that together that they provide a single view that preserves what is valuable in all of the existing doctrines of empiricism, scientific realism and antirealism.

Here is how small-r realism relates to Big-R Realism and to antirealism.

4.1 Not Big-R Realism

By a Big-R Realism, I mean a version of scientific realism that proposes, as a general result, that our best science has in some way discovered the final truths of the science. It may be a proposal of structural realism that asserts an enduring truth in the identification of structure; or it may be a version that asserts an equally enduring "approximate truth" of some contingent proposition.

Small-r realism is close in spirit to Big-R Realism in its epistemic optimism. The methods of science are fully capable of providing strong support for the truth of the core propositions of a science. Small-r realism differs from a Big-R Realism in affirming only the capacity of inductive inference from experience to secure the truth of contingent propositions central to a scientific theory. Whether a science has succeeded in securing such strong support must be determined on a case-by-case basis. Even ardent Big-R Realists must accept that some sciences are in a rudimentary state and have not secured this strong support. To sustain itself, Big-R Realism must provide a way to separate out those sciences or those parts of the sciences that have secured the final truth from those that have not. This amounts to the need for some generally applicable principle that tells us when the truth of a contingent proposition has been determined with finality.

According to small-e empiricism, such a principle is unattainable. The best we can have is very strong, but fallible inductive support for contingent propositions. The failure of attempts to work around this limit is assured. It is just a matter of examining these attempts to discern how they fail. Claims of approximate truth founder on the ambiguity of the adjective "approximate" when applied to "truth." Claims of enduring structure require something that has yet to be found: a precise definition of structure such that this structure of a present scientific theory can be affirmed to survive through all future discoveries.

These last remarks should not be understood as a form of skepticism. They are entirely compatible with the possibility of the contingent propositions of a science enjoying such strong inductive support that the falsity of their central propositions is well-nigh inconceivable. Such is the case, I believe, with many of our best sciences. Slender as it may be, the possibility of falsity remains as a matter of principle, even if not one of any practical import.

4.2 Not Antirealism

By antirealism I mean those positions that assert that science can secure truth only for contingent propositions that express experience directly, but not for propositions that go beyond direct experience. These latter propositions may be portrayed as merely of instrumental or pragmatic utility. The view is not just that science happens to have failed to secure the truth of the latter. Rather, it asserts that to secure it lies beyond the methods available to science as a matter of foundational principle.

Small-r realism conforms with antirealism in not automatically declaring the truth of core propositions of mature sciences. It differs from antirealism in treating alike the import of inductive support for the contingent propositions of a science. Small-r realism has to treat them alike since it has no means to distinguish them. small-r realism/small-e empiricism has abandoned the long dubious bifurcation of experience and theory, as reported in Chapter 10.

Antirealism asserts a one-size-fits-all blanket view of skepticism towards any propositions beyond direct experience. In small-r realism, all that matters is the extent of the inductive support for a contingent proposition. It does not even have a general principle that accords specific observational propositions greater support than the propositions of the deeper theory to which the observations pertain. Decisions concerning comparative strengths of support must be made on a case-by-case basis.

Commonly, individual observation reports are better supported than their theoretical generalizations. Examples are easy to find. In the 1990s, Schilling and collaborators (1993) had observed superconductivity at the highest temperature (under normal pressures) yet found, 133K, in their sample of Hg-Ba-Ca-Cu-O. Given the continuing progress in finding higher temperature superconducting material, this observational report is better supported inductively than the generalization that all superconduction arises at normal pressures below 135K.

Inversions are equally easy to find, in which the strengths of support for particular experiential claims are weaker that those of the theoretical claims that govern them. For example,

the evidential support for the truth of the conservation of energy is far stronger than for the veracity of any particular report of a perpetual motion machine. The evidential support for the standard chronology of species evolution is far stronger than for the veracity of a report of coincident fossilized human and dinosaur footprints.

5. Scientific Realisms: Expansion, Fragmentation and Drift

The original motivating intuition of scientific realism was narrow. It just asserted that the central propositions of our best science 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.

To support this analysis, this section offers a brief survey of the development of conceptions of scientific realism. It illustrates how its conception has drifted, expanded and fragmented.

5.1 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.

5.2 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.[8] 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.

5.3 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[9] 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 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.

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[8] 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."

[9] 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.

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.[10] 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…"

5.3 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)

> 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)

[10] 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."

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, 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).

5.4 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.

6. Induction in the Realism-Antirealism Debates

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 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 a misconstrual of the import of inductive fallibilism arises in both realist and antirealist literatures. Here we see a repeat of the misconstrual of inductive fallibilism in philosophy that was explored in the last chapter. Antirealists seek to amplify the fallibility from prudent, controlled doubt into imprudent skepticism. Big-R Realists seek to discount inductive fallibility, assuming that it can be controlled by careful adjustments to the formulation of scientific realism. Both, I will argue below, are incorrect.

7. Antirealism: The Pessimistic Overreaction to Inductive Fallibilism

Antirealism in philosophy of science is not a single doctrine. It is a collective term for accounts that deny scientific realism. It follows that the term is inherently vague; and it is so for two reasons. First, since there is at present no single definition of scientific realism, the particular sense of antirealism invoked will depend on which version of scientific realism is disputed. Second, each instance of antirealism arises within a viewpoint that contradicts some version of

scientific realism. Thus, each instance will in turn reflect the particular orientation of the positive viewpoint that hosts it.

This inherent vagueness makes general claims about antirealism difficult to sustain. The same is not true of the *arguments* used to support antirealism. A persistent feature of these arguments is a misappraisal of the cogency of inductive inference and inductive support. An inductive skepticism that just denies the inductive import of evidence is common. This pessimism is often grounded in a misconstrual of inductive fallibilism as a simple failure of inductive support.

7.1 Anti-inductivism and Anti-explanationism

Antirealist views commonly incorporate some version of inductive skepticism. Otherwise, the realist import of the strong inductive support of many sciences would be inescapable. A persistent strand of anti-inductivism arises in the context of the widely reported thesis of the underdetermination of theory by evidence. Elsewhere (Norton, 2008), I have argued that the thesis is little more than a hunch that some find plausible and even compelling and others, like me, find unsustainable. The thesis depends on a naïve form of hypothetico-deductive confirmation, in which two hypotheses are mistakenly judged equally confirmed if they deductively entail the same evidence. In another strand, we saw in Chapter 4 above that a broadly focused anti-inductivism became explicit in van Fraassen's developing skepticism.

One element of van Fraassen's growing anti-inductivism proved especially troublesome for scientific realists. It assailed directly what is still the most popular argument for scientific realism, the "no miracles argument." This argument depends on the notion of explanation in science. According to it, the immense success of science would be inexplicable—miraculous—if some version of scientific realism were not true. A celebrated version was given by Putnam, when contemplating the reality of electrons, DNA and the curved spacetime of general relativity. He wrote (1975-76, p. 178, his emphasis)[11]

> But if these objects don't really exist at all, then it is a *miracle* that a theory which speaks of gravitational action at a distance successfully predicts phenomena; it is a *miracle* that a theory which speaks of curved space-time successfully predicts

[11] Putnam's version is distinctive in speaking of "miracles." The same argument, without "miracle" verbiage, is already in Smart (1963, p. 47).

> phenomena; and the fact that the laws of the former theory are derivable "in the limit" from the laws of the latter theory has no explained methodological significance.

Van Fraassen (1980, Ch.2) denied that the success of science requires adopting scientific realism. It is not needed, he insisted, to explain the success of science. Famously, he offered an alternative, modeled on evolutionary biology: (p. 40, his emphasis)[12]

> … I claim that the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature.

Van Fraassen's analysis is routinely mentioned as the source of a rejoinder to the no miracles argument in the form of a riposte I have often heard: "What has explanation to do with truth?!" No doubt this riposte appears somewhere in print, but I could not find it. A less colorful version appears in Fine (1986, p. 115):

> … to argue for realism one must employ methods more stringent than those in ordinary scientific practice. In particular, we must not beg the question as to the significance of explanatory hypotheses by assuming that they carry truth as well as explanatory efficacy.

To begin, I do think that the anti-explanationist argument has merit. It derives from the overall weakness of the inductive argument form known as "inference to the best explanation." The cogency of the rule depends on explanations carrying inductive force. The difficulty is that notions of explanation are so varied and are even so poorly articulated that a tight connection between explanatory prowess and truth is precluded.

Consider, for example, that some processes are so complicated that only an opaque theory can adequately describe them in full, quantitative detail. The full account is intractable as an explanation. In such cases, we can still provide serviceable explanations in intentionally

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[12] Appealing as is the rhetoric of this response for its vivid imagery, its weakness has been apparent to me from the outset. It fails if the survival of a scientific hypothesis is enhanced, even only in some probability, if it latches onto truths deeper than superficial regularities. Nothing in van Fraassen's arguments precludes it, beyond his voluntary decision to deny it.

simplified narratives. An example is that changes in greatly complex weather patterns are often explained with folk-meteorological conceptions like a "cold front." That is, the explanations depend on the introduction of falsities into the narrative.

This critique of the import of explanation in the no miracles argument has merit. The mistake, however, is to stop with it. For it is common for scientists to claim support for their theories on the basis of the theory's explanatory prowess. In Norton (2021, Ch.8-9), I gave an extended analysis of canonical examples of inferences to the best explanation in science. I found that they did not depend on any special notion of explanation. Rather, they had a structure that succeeded without it. Each example consisted of a favored theory that proved adequate to the evidence. It was compared with a foil that failed, either through refutation by the evidence or by the accumulation of an unsustainable inductive debt.

The outcome is that the no miracles argument cannot be employed as a general argument for scientific realism. It proceeds at too general a level. The notion of success is too broad and the notion of explanation enjoined too vague. However, the argument can work if it is implemented in individual cases, in which the cogency of the real argument, independent of explanatory flourishes, it recoverable. My narrative in (2021, Ch. 8-9) gives many examples. Darwin, for example, invoked natural selection as the explanation for the diversity and adaptation of species. His case for natural selection can be reconstructed without needing any special notion of explanation.

7.2 The All or Nothing Argument

These last remarks catalog skepticism about forms of inductive inference. There is a simpler dismissal of inductive inference in the antirealism literature: it is just to ignore it. This response is invited by Big-R Realism seeking an unattainable goal: the assured truth of at least some parts of science. The easy response correctly reports the fallibility of inductive support: *assured* truth is unattainable. That response can then be amplified, incorrectly, into the fallacy of a false dilemma: either truth is assuredly attainable or we cannot attain truth at all. Since the first horn is precluded, we arrive at the antirealism of the second.

This false dilemma seems to be how Chang builds the case for his antirealist "realism for realistic people." (See Section 8.2 below.) He writes: (2022, p. 2)[13]

> Seeking absolute truth is not an operational ideal–there is nothing we can actually do in order to approach that ideal. According to the common picture of scientific knowledge, science should give us the true picture of the reality that exists well-formed 'out there' completely independently of our conceptions and our experiences. But such 'reality' is not accessible to us and there are no actual methods by which we could attain assured knowledge about it.

And then: (pp. 6-7, his emphasis)

> I wish to take philosophers' attention away from standard scientific realist attempts to show that the impossible is *somehow* possible, namely that empirical science can attain assured truths about what truly goes beyond experience. As I will argue in Chapter 2, it is time to accept the fact that we cannot know whether we have got the Truth about the World (and that such thoughts are perhaps not even meaningful). Scientific realists go astray by persisting in trying to find a way around this fact, while antirealists make the mistake of engaging unproductively with that realist persistence.

We pass between the horns of the dilemma by accepting that assured truth is unattainable, but that we can secure very strong inductive support for the truth of contingent propositions in science. It suffices.

7.3 Pessimistic Meta-induction

The most common argument for antirealism is the pessimistic induction or, more accurately, the pessimistic meta-induction. In its barest form, it is an overreaction to the fallibility of inductive inference. We can find many occasions in the history of science in which our favored theories failed in some way. Therefore, we are to expect our present theories to suffer the same fate. That is, we are to adopt an inductive skepticism in which we no longer have confidence in the inductive inferences supporting, for example, the truth of propositions or the reality of entities in our present science. From some failures historically of inductive support for

[13] Rescher (1987, p. 41) accuses instrumentalists of arriving at their view by a similar misappraisal of the import of what he calls the "fallibility and corrigibility of science."

some aspects of scientific theories, we are to leap imprudently to a complete lack of confidence in our present scientific theories and perhaps even to the stronger expectation of their ultimate and enduring failure. Such is the summary of the argument given by Stanford (2006), who identified the argument in the writings of Henri Poincaré: (p. 7, his emphasis)

> Contemporary philosophers of science call this argument the *pessimistic induction*: its central idea is that the scientific theories of the past have turned out to be false despite exhibiting just the same impressive sorts of virtues that present theories do, so we should expect our own successful theories to ultimately suffer the same fate.

The paper most often credited with advancing this pessimistic argument is Laudan's (1981),[14] "A Confutation of Convergent Realism." Laudan's paper is rich in examples of failed theories. After his discussion of other failures, he gives a list of further failures: (p. 33)

> –the crystalline spheres of ancient and medieval astronomy;
> –the humoral theory of medicine;
> –the effluvial theory of static electricity;
> –"catastrophist" geology, with its commitment to a universal (Noachian) deluge;
> –the phlogiston theory of chemistry;
> –the caloric theory of heat;
> –the vibratory theory of heat;
> –the vital force theories of physiology;
> –the electromagnetic aether;
> –the optical aether;
> –the theory of circular inertia;
> –theories of spontaneous generation.

It is followed by a confident boast:

> This list, which could be extended *ad nauseam*, involves in every case a theory which was once successful and well confirmed, but which contained central terms which (we now believe) were non-referring.

[14] The term "confutation" is uncommon and means "disproof" according to the 1912 Websterian dictionary.

This historical tale of woe provided the basis for Laudan's disputing of scientific realism. He argued that this history shows that the success of a scientific theory provides no basis for scientific realism in two specific forms. A theory's success is independent from whether the theory's terms successfully refer and from whether its propositions are approximately true. He concluded: (p. 47)

> The fact that a theory's central terms refer does not entail that it will be successful; and a theory's success is no warrant for the claim that all or most of its central terms refer.
>
> The notion of approximate truth is presently too vague to permit one to judge whether a theory consisting entirely of approximately true laws would be empirically successful; what is clear is that a theory may be empirically successful even if it is not approximately true.

It is notable that Laudan's "Confutation" does not characterize his argument as an induction over the history of science. The term "pessimistic" also does not appear. Both terms appeared in contemporary characterizations of the argument. Newton-Smith (1981, p. 14) described the "pessimistic induction." Putnam (1978, p. 25) called it a "meta-induction." They were soon combined into the "pessimistic meta-induction," which became a familiar label. French (2000) mentions in passing "the so-called pessimistic meta-induction." This latter term is, in my view, the better name for it. The inference does not dispute any particular result in present science. Rather it disputes our methods for arriving at these results. Since these methods are inductive, it is a skeptical induction about induction, that is, a meta-induction.

Although Laudan does not make this meta-inductive character explicit, it is present in his assertion above: "a theory's success is no warrant for the claim that all or most of its central terms refer." The "warrant" refers to the inference from empirical success to referential success, which is, prudently, inductive. Laudan disputes the generality of this inference by collecting instances of its failure. That is, he is using an induction over these instances to dispute the further inference indicated by "warrant."

Laudan's formulation of the pessimistic meta-induction is just one of many variants in a literature that grew large. A notable variant is Stanford's "problem of unconceived alternatives." The insecurity of inductive support for our present science lies, according to the argument, in our

inability to conceive of the unrecognized alternatives that would be better supported. Stanford introduces his argument as: (2006, pp. 17-18, his emphasis)

> … any real threat from the problem of underdetermination comes not from the sorts of philosophically inspired theoretical alternatives that we can construct parasitically so as to perfectly mimic the predictive and explanatory achievements of our own theories, but instead from ordinary theoretical alternatives of the garden variety scientific sort that we have nonetheless simply not yet managed to conceive of in the first place. I will call this worry *the problem of unconceived alternatives*, and although it has historically received far less attention than either the search for empirical equivalents or the traditional pessimistic induction, I will suggest that it ultimately deserves far more.

7.4 The Inductive Failure of the Pessimistic Meta-induction

The literature on the pessimistic meta-induction is large. It contains further variant forms of the argument and many analyses of its evident shortcomings.[15] My concern here is the extensive strand of criticism that assesses the pessimistic meta-induction as an inductive inference. In short, the pessimistic meta-induction fails in every aspect in which an inductive inference can be assessed. It is one that fails badly.

Mizrahi (2013; 2016; 2020, Section 5.1) identifies numerous failings in the pessimistic meta-induction. If we are to conceive of the induction as an inference from a sample to a population, then it is elementary that the sample must representative. Here the sample fails to be representative of the population of present theories to which we are to infer. For most of the sample comes from earlier eras in science which do not resemble the present era. Further the sample must not be cherry-picked. That is, the individuals in the sample must not be selected because they manifest the characteristic sought. Developments of the pessimistic meta-induction are explicitly biased. To establish the history of science as a "graveyard"[16] of theories, they selectively report instances of failure.

To bring an induction from the history of science closer to proper inductive methods, Mizrahi (2013) assembled an inventory of past science labeled as "theory" or "law." He used a

[15] See Wray (2015) for a brief survey.

[16] A label reported by Psillos (1999, p. xxi).

random number generator to pick a sample of 40 from each. He found 15% of the theories and 12.5% of the laws to be now abandoned. That is most have not been abandoned. Mizrahi finds optimism and not pessimism in the associated success rates. In a susbsequent analysis (Mizrahi, 2016), drew a sample of 40 from an inventory of sciences in which the terms "theory," "entity" and "posit" appear. The results were similarly optimistic. Mizrahi (2013, p. 3225) concluded "…the pessimistic induction is a fallacious argument and the counterexamples to which it points miss their intended target."

Mizrahi left without sustained challenge the assumption that it is appropriate to treat the historical induction as an inference from a simple frequency to a population frequence. He gave one example as: (213, p. 3222)

15% of sampled theories are abandoned theories (i.e. considered false).

Therefore, 15 % of all theories are abandoned theories (i.e. considered false).

It is quite doubtful that this simple argument form is properly applied here. Indeed, the material theory of induction requires that it can only be applied if there are background facts, specific to the domain, that authorize it.

Shech (2019) examined whether suitable background facts can be identified that would warrant the inference. Those facts would have to prevail over many sciences and over a great expanse of time, from the historical instances taken as premises to the present sciences whose cogency is to be concluded. It is not so surprising that Shech's careful examination found no such warranting facts. They did not reside in a uniformity of methods used by the sciences over time and field, since there is no such uniformity. They did not reside in some uniformity of facts about the investigating scientists themselves; and again not in some uniformity of explanatory modes used. The outcome is a failure of the pessmistic meta-induction, not just in the details Mizrahi catalogued, but simply as inductive argument. There is no warrant for its use. Shech concluded: (2019, p. 928)

> I have argued that historical inductions such as the (global) PMI and the problem of unconceived alternatives dissolve if we work with the material theory of induction. The reason is that we lack the material postulates needed to license the pessimistic inference: the great heterogeneity of case studies from the history of science of conceiving, constructing, and discovering (explanatory and predictively successful) theories along with the abundant variety of context that scientists find themselves in

and traits that they exhibit make it unlikely that any commonality will be found strong enough to authorize the induction."

These problems still have not exhausted the deficiencies of the pessimistic meta-induction. What might well be the most serious failure has been identified by Psillos (2022, §2.4). The pessimistic meta-induction is supposed to establish that we cannot infer reliably to the truth of all present theories. The grave weakness is that the pessimistic meta-induction just ignores whatever may be the present evidence for the truth of our best present science. Any assessment of the truth of present science *must* take into account that present evidence. It is, by any reasonable standard, the strongest and most reliable evidence.

The observed expansion of the galaxies and the cosmic microwave background radiation provides strong evidence for big-bang cosmology. The extensive repertoire of atomic spectra provides strong evidence for quantum theory. Paleontological and embryological observations provide strong evidence for evolutionary theory. If we are to accept the conclusion of the pessimistic meta-induction, we would have to overrule the inductive support provided by all these specific experiences, tailored as they are to each specific theory. We would have to accept that the pre-emptive generality of the pessimistic meta-induction is the stronger factor.

If we contemplate the practicalities of such acceptance, its absurdity becomes apparent. Are we to press a modern astronomer to abandon confidence in the truth of planetary heliocentrism because Ptolemy was a geocentrist; or that a modern chemist should abandon the truth of the oxygen theory of combustion because the failed phlogiston theory enjoyed a brief period of celebrity? At best, that past efforts have failed can serve only in a secondary role, evidentially. It counsels caution in the assessment of the primary evidence for a present science.

There is a final irony in the pessimistic meta-induction. The whole burden of the pessimistic meta-induction is to assail inductive inference itself. That is, its goal is not just to cast doubt on *some particular* inductive inference, in science. It is to impugn the cogency of *all present* applications of inductive inference in science. If we are to identify one place where inductive inferences are done well, it is in our best science. The pessimistic induction is supposed to convince us that even here in this best possible case, we cannot rely on inductive inferences. If that is so, then we should have no confidence at all in the weaker inductive inferences of the pessimistic meta-induction induction. The extent to which it succeeds is the extent to which it fails.

8. Antirealist "Realisms"

Some views that carry the label "type-X realism" are, on closer inspection, versions of antirealism. They are offered as repairs to realism but, on closer examination, prove to be proposals that negate it. Two versions are sketched here.

8.1 Perspectival Realism

So-called "perspectival realism" is a clear case of type-X realism in which the conception has migrated sufficiently far from the original intuition that the label "realism" is a misnomer. According to it, objective truths in science are beyond the reach of scientists. It is not that scientists need to try harder. It is, in this view, an unbreachable barrier of principle The best scientists can have, in Giere's (2006) formulation, are models that provide perspectives. The models cannot be said to be true of some target system, since we cannot expect a perfect fit. Talk of "similarity," Giere (2006, p. 66) finds, is more appropriate than talk of "truth." The best scientists can have is conveyed in a quote that Giere imagines being given by scientist, enlightened by perspectival realism: (p.6)

> "According to this highly confirmed theory (or reliable instrument), the world seems to be roughly such and such." There is no way legitimately to take the further objectivist step and declare unconditionally: "This theory (or instrument) provides us with a complete and literally correct picture of the world itself."

To articulate the view further, Giere (p.16) tells us that "Color vision provides the best exemplar I know for the kind of perspectivism that characterizes modern science." The human visual system provides the perspective in the exemplar: (p.27)

> Why should a surface with a given surface spectral reflectance be called "yellow"? Without reference to the particular characteristics of the human visual system, there is no physical basis whatsoever for this identification.

Presumably we are to treat the dependence of our *perception* of color on our visual system as a good analog for how all *scientific claims* are dependent on a perspective, and ineliminably so. If that is the analogy, it is obviously a poor mapping between the exemplar and the science. An analysis of human visual experience allows us to infer from our perception of yellow to an objective property of the surface: it reflects the light in the frequencies corresponding to yellow.

The identification of this objective property by inferences is the better analog for claims in science.

This last remark is no mere quibble about a misused analogy. It reveals the mismatch between perspectival realism and the inferential practices of science. The specific measurements that manifest in different measured values will vary according to the instruments used. In that sense they are perspectival. It is routine in science to free those measurements from their perspectives, that is, from their attachment to this instrumentation by inductive inferences.

Take for example the recent triumph of LIGO's gravitational wave detector. On August 17, 2017, it detected a binary neutron star merger in the event labeled GW170817. It was a distinctive detection insofar as the detection arose in both gravitational and electromagnetic wave channels. Here is the collaborative's summary of the result: (Abbott et al., p.2)

> Here, we report on the global effort that led to the first joint detection of gravitational and electromagnetic radiation from a single source. An ~ 100 s long gravitational-wave signal (GW170817) was followed by an sGRB (GRB 170817A) and an optical transient (SSS17a/AT 2017gfo) found in the host galaxy NGC 4993. The source was detected across the electromagnetic spectrum—in the X-ray, ultraviolet, optical, infrared, and radio bands—over hours, days, and weeks. These observations support the hypothesis that GW170817 was produced by the merger of two neutron stars in NGC4993, followed by an sGRB and a kilonova powered by the radioactive decay of r-process nuclei synthesized in the ejecta."

Compare this summary with Giere's (2006, p. 92) assessment of a similar case:

> … the same object can often be observed from several different perspectives, such as a nearby galaxy observed by both optical and radio telescopes. This is indeed good evidence that there is "something" there, but that is scarcely knowledge in the objectivist sense. The knowledge we get comes from one perspective or another, not from no perspective at all. Multiplying perspectives does not eliminate perspectives."

Giere's perspectival realism prohibits the collaborative from affirming anything beyond the following collection of perspectival claims:

> We have detected a binary neutron star merger in the gravitational wave perspective.
>
> We have detected a binary neutron star merger in the X-ray perspective.

We have detected a binary neutron star merger in the ultraviolet wave perspective.

We have detected a binary neutron star merger in the optical wave perspective.

We have detected a binary neutron star merger in the infrared wave perspective.

We have detected a binary neutron star merger in the radio wave perspective.

The major claim of the collaborative is a breach of this perspectival prohibition. The collaborative uses the measurements in each perspective as observational support for the objective fact of a binary neutron star merger in the host galaxy NGC 4993; and properly so. This well-founded objective declaration is precisely what perspectival realism prohibits.

Massimi (2018, 2022) has developed Giere's perspectivalism by emphasizing that perspectives are culturally and historically situated: (2022, pp. 5-6, Massimi's emphasis)

> *Scientific perspective (sp)*: A scientific perspective sp is the actual—historically and culturally situated—scientific practice of a real scientific community at a given historical time. Scientific practice should here be understood to include: (i) the body of *scientific knowledge claims* advanced; (ii) the experimental, theoretical, and technological resources available to *reliably* make those scientific knowledge claims; and (iii) second-order (methodological epistemic) principles that can *justify* the *reliability* of the scientific knowledge claims so advanced.

The earlier account affirmed Giere's denial of objectivity: (Massimi, 2018, p. 165)

> There cannot be an objective, unique, true description of the way the world is as soon as we acknowledge that our scientific knowledge is always from a specific vantage point – either in the sense of (1) [historically situated] or (2) [culturally situated] or both.

Perspectivalism cannot be called a "realism," if we are to adhere to the original intuition of scientific realism. By expanding the scope of perspectives, Massimi's analysis enlarges the gap between it and this original intuition. Because of the insistence that objectivity is impossible as a matter of principle, perspectival realism is better conceived as a form of antirealism.[17]

[17] Massimi's response is to introduce a conception of "perspectival truth," which is truth contextualized to different perspectives. (2018, p. 171; 2022, p. 179)

8.2 Realistic Realism

Chang's (2022) *Realism for Realistic People* is, superficially another instance of the migration of the notion of scientific realism. "Realism" is the first word in the title. It becomes apparent almost immediately in the text that the policy it advocates for "realistic people" is simply to abandon the prevalent conception of scientific realism for a form of antirealism. On its first page (p.1), scientific realists are disparaged as those who have "succumb[ed] to an authoritarian or even autocratic impulse." The text continues to renounce as a "widespread conceit that modern science has *basically* the right answers, or at least the right methods for getting the right answers." This renunciation of scientific realism is thorough. It disallows even the methods of inductive inquiry that, in small-r realism, comprise the foundation of scientific realism. This conceit, we are told, is "maintain[ed] … like an article of faith."

After this introduction, readers can have little doubt about Chang's distain for scientific realists and that the term "realistic" in his title must be read ironically. A new sense is intended. That Chang is trying to redefine "realism" is quite explicit. He tells us so by quoting from his own, earlier remarks: (P. 7, his emphasis)

> I am trying to carry out a task that I already advocated a decade ago: 'I think *realistic* people (including most empiricists and pragmatists) should re-claim the label of "realism"!'

The direction of the redefinition is given in the work's subtitle: *A New Pragmatist Philosophy of Science*. Its details go beyond our present concerns. Very briefly, this new pragmatist realism is centered on the notion of (p.4) "operational coherence," which is "[v]ery roughly … a matter of making elements of our activities fit together harmoniously so that our aims may be achieved." And that:

> …what we should mean by something being 'real' is that it can be employed in coherent activities that rely on its existence and its basic properties. Truth and reality conceived in such a way are attributes grounded in our activities.

9. The Search for Permanent Truths

Where antirealism overestimates the import of the fallibility of inductive inference, strong versions of scientific realism underestimate it. They treat the fallibility of inductive inference 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 is right and which 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.

9.1 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.[18]

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

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 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 the preceding 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.

9.2 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."[19] 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

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

present quantum theory are widespread and extraordinary, the theory is beset with enduring foundational problems.[20] 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.

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:

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[20] 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.

> 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.

10. Selective Realism

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.

10.1 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[21]

$$x' = x\text{-}vt \quad y' = y \quad z' = z \quad t' = t$$

It is replaced in special relativity by the Lorentz transformation[22]

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

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 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

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[21] 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.

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

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.

10.2 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.[23] 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.

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

[23] 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.

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.

10.3 Clarifying Structural Realism[24]

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. 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, …

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

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 9.1 above recalled the troublesome ambiguity associated with the modifier "approximate"; and Section 7.1 recounted 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.

10.4 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.[25]

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.[26]

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.

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.

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[25] 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."

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

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.

10.5 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

10.6 Explanationism 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)[27]

> 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 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

[27] 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.

> 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.

10.7 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.

10.8 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.[28]

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.

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.

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[28] 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.

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. This impossibility 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 in order to be able to recover the limits Carnot deduced in his earlier work.

11. Conclusion

The present literature in philosophy of science depicts empiricism and scientific realism as irreconcilable opposites, largely I believe through the influence of van Fraassen's *Scientific Image*. It is an unfortunate conceptualization since empiricism and realism each have something of value. Each, however, also harbors elements that should be discarded. Big-E Empiricism endorses an indefensible inductive skepticism. Big-R Realism underestimates the intractability of the inductive fallibility.

My goal in this chapter has been to show that, if we discard these troublesome elements in each, we arrive at a welcome consonance between the two views. Small-e empiricism discards inductive skepticism. Small-r realism accepts that inductive fallibilism is inescapable. What results in small-e empiricism and small-r realism is a single, optimal, mediating position that preserves the best of both extremes and discards the worst.

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