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Chapter 20

Inductive Skepticism in Scientific Antirealism

1. Introduction

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. Some versions arrive at it by amplifying the fallibility of inductive inference into the assured failure of inductive inference. Examples below are the "all or nothing" argument and the pessimistic meta-induction.

Antirealists must deny this inductive import. For the inductive import of the massive compilation of evidence for our best sciences strongly supports their truth, in contradiction with antirealist claims.

This chapter explores the role and variety of anti-inductive views in scientific antirealism. Section 2 reviews the widely-noted inductive skepticism of the undetermination thesis. It depends on a naïve form of hypothetico-deductive confirmation. Section 3 recounts the anti-explanationism van Fraassen developed to defend his version of antirealism. It employs a correct but misdirected concern that explanatory prowess need not indicate truth. Section 4 recounts the simpler "all or nothing" argument for inductive skepticism. It depends on a false dilemma: either inductive support is inerrant or a failure.

Sections 5 and 6 review the most enduring argument for antirealism, the pessimistic meta-induction. It tries to establish that present and future sciences 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 7 completes 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." Perspectival realism, reviewed in Subsections 7.1 to 7.4, seeks to compartmentalize the import of evidence into separate perspectives that are divided according to the source of the evidence. This compartmentalization is inconsistent with how evidence from different sources is used routinely. Evidence from different sources can mutually support each other, so that each are seen to support the same facts. Realistic realism, discussed briefly in Section 7.5, seeks to replace a realist conception of theories with an antirealist, pragmatic conception. Section 8 is a short conclusion.

2. The Underdetermination Thesis

A persistent strand of anti-inductivism arises in the context of the widely reported thesis of the underdetermination of theory by evidence.[1] A weaker version of the thesis merely asserts that sometimes we may not have sufficient evidence available to establish a particular theory. This circumstance is common when new sciences or scientific theories are emerging. It is not a form of anti-inductivism.

The anti-inductive version of the thesis is its strong form. It asserts that no body of evidence, no matter how extensive, can determine a particular theory. By "determine," I understand that the body of evidence is to supply strong inductive support for one specific theory, such that it is preferred over all its rivals.

The thesis enjoys continuing respect outside philosophy of science. Within philosophy of science, even though some support persists, it has been widely derided. My critique in Norton

[1] See Stanford (2023) for a recent survey.

(2008) finds the strong form of the thesis to be little more than a speculation that some find plausible and even compelling and others, like me, find unsustainable.

The thesis, I have argued, depends on a naïve form of hypothetico-deductive confirmation, in which two hypotheses are mistakenly judged equally well confirmed if they deductively entail the same evidence. We saw in Chapter 15 that Bridgman was unable to separate the standard theory of the history of the universe from a fantasy theory in which the universe was created five minutes ago with all memory traces of an earlier time as part of the creation. Bridgman was correct that each entail the same evidence. However, as I noted in Chapter 15, he was wrong to conclude that this evidence cannot separate the two, for the evidence favors the standard theory strongly.

The arguments for the strong form of the underdetermination thesis routinely display theories that are empirically or observationally indistinguishable. In addition to employing the naïve form of hypothetico-deductive confirmation, these examples are self-defeating. For their indistinguishability is established by displaying a mapping between their observational consequences. The fact that such a mapping can be displayed leaves open the possibility that the theories are merely variant formulations of the same facts.

Finally, there is a striking consequence for our mature sciences. The results of these sciences have acquired an exclusive stability and security that is sustained by a rich body of evidence. Its strength of support is judged by the scientists to be overwhelming. The strong form of the underdetermination thesis requires that these scientists, in science after science, are simply mistaken in their assessment of the evidence. The more plausible alternative is that a few skeptical philosophers have been hasty is adopting an oversimplified and faulty account of inductive evidence.

3. Anti-explanationism

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)[2]

> 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 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)[3]

> … 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 in discussions: "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.

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

[3] 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.

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. This full account is intractable as an explanation. In such cases, we can still provide serviceable explanations in intentionally 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.

4. The All or Nothing Argument

These last sections catalog skepticism about specific 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 if Big-R Realism is understood to seek 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 built the case for his antirealist "realism for realistic people." (See Subsection 7.5 below.) He wrote: (2022, p. 2)[4]

> 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.

[4] 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."

5. 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 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),[5] "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;

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

> –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.

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 historically affirmed 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.

This sharply formulated objection deserves an immediate response, before further responses are developed in the subsection that follows. Stanford is correct to note that we *might* be mistaken in our present assessment of the import of empirical evidence because of unconceived alternatives. That is, inductive support is fallible. It does not follow that we *should* infer that we are mistaken. Fallibility is not failure.

6. 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.[6] 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.

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

Mizrahi (2013; 2016; 2020, Section 5.1) identified 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"[7] 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 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 derived an optimism and not pessimism from 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: (2013, 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

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

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.

7. 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. An essential part of that negation are forms of inductive skepticism. Two versions are sketched here.

7.1 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."

According to perspectival realism, this assessment is conveyed in a quote that Giere imagines being given by a 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.

7.2 Perspectival Inductive Skepticism

Giere is right that our representation of some—but not all—systems come in different, limited perspectives. Each in some cases captures some elements of the system but omit or oversimplifies or misrepresents others. They cannot be combined tractably into a single, complete representation. A familiar example is maps of a city. Each city is represented by many different maps. There are street maps for road traffic; maps that highlight biking and walking paths; and maps that display public transit routes. Other maps display public utilities, such as electric power, water supply, drainage and sewage lines. Climate science provides another example closer to our science. Models of the Earth’s climate identify one aspect of the processes in the climate and idealize or simplify away other elements. These models, as reported in

Jeevanjee et al. (2017), cover atmospheric fluid, Earth's rotational, oceanic, surface, convection and radiative aspects of the climate.

Where Giere errs, however, is in generalizing what applies to some systems investigated in science to all scientific investigation. This expansion is based on a mistaken, general claim about inductive inference. It is that the import of evidence from different sources cannot be combined. Their import is always restricted narrowly to the perspective associated specifically with that form of evidence.

The arbitrariness of this perspectival compartmentalization is a form of inductive skepticism. It is contradicted by small-e empiricism and familiar inductive inferences. Take the case in which we have different experiential processes emanating from the same source. Consider, for example, an electric discharge from the clouds in stormy weather. We learn of it through the electromagnetic process of a lightning flash; through the vibratory propagations of the sound of thunder; and, if the wind blows towards us, the material propagation of pungent ozone gas. Each item of evidence combines to affirm that the others were the result of the same electric discharge. For example, the pungent smell of ozone is evidence for an ozone-producing electric discharge; and we know from electrochemistry that this same discharge produces light, which affirms that the lightning results from the same discharge.

7.3 How LIGO Merges Perspective

We can combine perspective and routinely do so. An example is 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 $\sim$ 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.

7.4 A Situated Perspectival Realism.

Massimi (2018, 2022) has developed Giere's perspectivalism by emphasizing that perspectives are also 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.

By expanding perspectives to include cultural and historical factors, this form of perspectival realism increases the compartmentalization of the import of evidence and the inductive skepticism associated with it. It would, for example, block the inference reported in Chapter 11 above from historical reports drawn from different cultures in China, Japan, the Arab world and Europe to the simple fact of the supernova of 1006AD.

Overall, perspectivalism cannot be called a "realism," if we are to adhere to the original intuition of scientific realism. Because of its insistence that objectivity is impossible as a matter of principle, perspectival realism is better conceived as a form of antirealism.[8]

7.5 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 of realists, 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)

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

> 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.

8. Conclusion

Antirealism thinking is correct to resist Big-R Realism if it is understood to include claim that science has arrives *assuredly* at final truths. Here it aligns with small-r realism/small-e empiricism Where it errs is in going beyond small-e empiricism and denying that science has had any success in attaining fallibly truths beyond experience. It is an overreaction that compels it to adopt a skeptical anti-inductivism. For otherwise, the inductive import of the extensive bodies of evidence for our mature science would contradict the antirealism.

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