

**Extending the Argument from Unconceived Alternatives:
Observations, Models, Predictions, Explanations, Methods, Instruments,
Experiments, and Values**

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Stanford's argument against scientific realism focuses on theories, just as many earlier arguments from inconceivability have. However, there are possible arguments against scientific realism involving unconceived (or inconceivable) entities of different types: observations, models, predictions, explanations, methods, instruments, experiments, and values. This paper charts such arguments. In combination, they present the strongest challenge yet to scientific realism.

1. Introduction – The Significance of Unconceived Theories

This primary aim of this paper is to consider how unconceived alternatives of a non-theoretical variety bear on the scientific realism debate. (This debate is multi-faceted. It concerns the epistemic status of theoretical discourse in contemporary science, what counts as scientific progress (or 'the aim of science'¹), the extent to which continuities in the content of science should be expected over time, and so forth. I am not going to explore the facets in detail here. I target explicit aspects at various points.) I begin, however, by presenting what I take to be the strongest version of the argument from unconceived alternative theories. I do this partly because some of the new arguments I

¹ I'd prefer not to use this phrase, but include it because others still do. See Rowbottom (2014a).

consider subsequently have similar targets; for example, several concern how values (or estimated values) for prior probabilities can change unexpectedly and unpredictably. However, I also think that this ‘strongest version’ has independent interest, since it avoids (a dubious) appeal to induction.²

Let’s get to business. The significance of unconceived alternative theories may be illustrated by appeal to confirmation theory. Assume, for the sake of illustration, that the confirmation of a hypothesis, h , is equal to its conditional probability given some evidence, e , in the presence of some background information (or ‘knowledge’), b .³ (This assumption is *not* necessary.⁴) Then, the confirmation value can be calculated by Bayes’s theorem (in the form used by Salmon 1990a):

$$P(h, eb) = P(h, b)P(e, hb) / P(e, b)$$

This involves $P(e, b)$, which decomposes into $P(h, b)P(e, hb) + P(\sim h, b)P(e, \sim hb)$. And $P(\sim h, b)P(e, \sim hb)$ in turn decomposes into $P(h_1, b)P(e, h_1 b) + \dots + P(h_n, b)P(e, h_n b)$, where the set of possible alternatives to h is $\{h_1, \dots, h_n\}$.

² The historical cases studied by Laudan (1981) may also be understood simply to cast doubt on the putative (probabilistic) connection between empirical success and successful reference of central theoretical terms (and/or approximate truth). No appeal to induction is then needed.

³ I prefer ‘background information’ to ‘background knowledge’ for reasons explained in Rowbottom (2014b). See also Williamson (Forthcoming) for an alternative view of b (and e).

⁴ The important requirement, which will become evident in the discussion that follows, is that the confirmation (or corroboration) value depends on $P(e, \sim hb)$. This holds for all standard confirmation (or corroboration) functions, such as those championed by Popper (1983), Milne (1996), and Huber (2008).

Contemporary theories are only highly confirmed provided that $P(\sim h,b)P(e,\sim hb)$ is low. Let's now think in terms of the subjective interpretation of probability, which is the most popular among contemporary confirmation theorists (and Bayesians in particular), for illustrative purposes.⁵ If we want scientific experiments to be capable of highly confirming theories that are strongly doubted, beforehand, we should not stipulate that $P(h,b)$ must be high. (The discovery of the Arago-Poisson bright spot was a case where $P(h,b)$ was low, on the subjective view. See Worrall (1989) and Rowbottom (2011).) So it follows – since $P(\sim h,b)=1-P(h,b)$ – that we should not stipulate that $P(\sim h,b)$ must be low. We should look to $P(e,\sim h)$, which is typically known as *the catchall*.

Naturally, it *is* possible for one's subjective probability in the catchall to be low, and for $P(\sim h,b)P(e,\sim hb)$ to be low as a result. However, said probability would become dramatically higher, if a new *serious* alternative predicting e (or predicting e to an appropriately similar extent to h) became apparent. (A 'serious' alternative in this subjective context means an alternative that the individual would take seriously, hence its prior would be reasonably high.) And then the confirmation value of h would become considerably lower. Thus, confirmation values may lower considerably, *as a result of a newly conceived alternative theory (with the correct properties)*.

Now many scientific realists no doubt hold that confirmation rests on more than psychology (even of a mob variety); they prefer a non-subjective account of

⁵ I prefer a group level interpretation, as suggested by Gillies (2000) and Rowbottom (2013).

confirmation, based on a logical, objective Bayesian, or perhaps even frequency or propensity view of probability. But even if one adopts such an account, on which the *actual* confirmation values never fluctuate, one should nevertheless concede that our *estimates* of those values may fluctuate as a result of our changing information about the alternatives to *h*. For as Salmon (1990b, p. 329) puts it:

What is the likelihood of any given piece of evidence with respect to the catchall? This question strikes me as utterly intractable; to answer it we would have to predict the future course of the history of science.⁶

Salmon's solution to the problem is to consider only the (positively) *conceived* alternatives to *h*.⁷ And on the basis of these, we can calculate the confirmation of *h* *relative* to the conceived alternatives. However, relative confirmation has no obvious connection to truth-likeness, even on the assumption that absolute confirmation (in some non-subjective sense) does indicate truth-likeness (or probable truth-likeness, or whatever surrogate one prefers). Hence, there are no grounds for thinking that *h* is truth-like unless there are grounds for thinking that there are no serious unconceived alternatives to *h*.⁸

⁶ Salmon adopts a frequency-based view, but might have done better, given the problems identified by Hájek (1999, 2007), to adopt a long-run propensity view. See Gillies (2000) and Rowbottom (2015).

⁷ The form of conceivability under discussion here is 'positive' in the sense discussed by Chalmers (2002, p. 153), although he primarily discusses situations: 'to positively conceive of a situation is to imagine (in some sense) a specific configuration of objects and properties'.

⁸ Here, I grant the dubious assumption that the possible is a subset of the conceivable. Some arguments for the underdetermination of theories by evidence instead rely on inconceivable theories.

Now grant that there have been many serious unconceived alternative theories in the past, as Stanford (2006) argues. The significance of this, for the tenability of scientific realism, does not depend on any inductive inference from the past to the present (and future), although Stanford (2006) does make such an inference.⁹ Rather, it poses a challenge for the realist who claims that contemporary theories are typically approximately true, provided that they are well-confirmed.

Why be confident that the confirmation value of any given theory (on a subjective view), or the estimate thereof (on an objective view), would not change drastically if all the unconceived alternatives were appreciated? What licenses inferring absolute confirmation values from relative confirmation values? If the realist cannot answer satisfactorily, it is reasonable to deny realism.¹⁰ And as we will see in what follows, the force of this challenge may be strengthened by appeal to unconceived observations. We will also see how even our estimates of *relative* confirmation can be unstable and/or incorrect, for independent reasons to do with unconceived models, experiments, and the like.

We will now begin to consider these different kinds of unconceived entities, many of which are connected in interesting and subtle ways. The findings in several of the

⁹ Stanford (2001, p. S9) writes: ‘the history of scientific inquiry offers a straightforward inductive rationale for thinking that there typically are alternatives to our best theories equally well-confirmed by the evidence, even when we are unable to conceive of them at the time’.

¹⁰ The problem of unconceived alternatives, so construed, also presents a challenge to *some* possible forms of anti-realism. For example, it throws doubt on the view that contemporary theories save the phenomena, or some proper subset thereof, in the most elegant (or more generally, virtuous) way possible. For unconceived alternatives may have much higher priors than their conceived counterparts.

different sections may also be connected; for example, prediction and explanation are two sides of the same coin, if Hempel's (1965) symmetry thesis holds.

2. Unconceived Observations

Put on hold the idea that some experiments – types or tokens – might not be conceived of, despite being conceivable, at any given stage in science. Why else might observations – and related observation statements – fail to be conceived of? One possible scenario is as follows. The observations in question are theory-laden, and the necessary theory (or set of theories) to conceive of them is *itself* unconceived.

Imagine the following hypothetical scenario. It's 1850, and archaeologists are exploring the remains of an ancient civilization, which spanned the Iberian peninsula. A striking feature of the civilization is the art, which involves many depictions of ducks, drawn in the same style. Murals of ducks are found in ruins of (buildings thought for independent reasons to be) temples, and pictures of ducks are found buried with the dead. The archaeologists take this to be evidence that ducks had some kind of special religious or spiritual significance in the civilization. They are somewhat surprised not to have found many remains of ducks in their archaeological work. But they suspect that the animals were treated as sacred, and allowed to roam free.

A few years later, however, there is a remarkable new find. Elsewhere in Iberia, the preserved remains of a previously unknown animal – a lagomorph – are discovered. Scientists decide to call it 'the rabbit'. (In this scenario, no-one has before

encountered rabbits because they were wiped out by a remarkably infectious virus – somewhat similar to our very own myxoma – before they spread beyond Iberia, back in ancient times.¹¹) And it is not long before a young archaeologist hypothesizes that all the aforementioned art depicts rabbits. He publishes his magnum opus on the civilization, and the centrality of this noble beast therein. He goes on to have a glittering career, as one of the leading lights of archaeology.

The moral of the story is as follows. Singular observation statements concerning ducks may now be replaced with singular observation statements concerning rabbits.¹² So from one point of view, the nature of the evidence itself is unstable.¹³ For one theory does not explain the presence of duck art, whereas the other does not explain the presence of rabbit art. From another point of view, the evidence remains the same – the pictures on the murals, and so on, are unaltered – but a different interpretation thereof is available. We do not need to decide which view is better, for present purposes.¹⁴ Either way – whether e changes to e^* on some of the serious alternatives

¹¹ Pedantic readers might think that hares are sufficiently similar for the pictures to be seen as hare-ducks. But imagine, if your will, that the whole *leporidae* family was wiped out by the virus, which affected hares as well as rabbits (unlike myxomatosis).

¹² They may also be replaced with observation statements concerning duck-rabbits, and this is potentially important from the point of view of scientific method. I will avoid discussing this possibility, however, in order to streamline the discussion.

¹³ It's possible for some evidence to remain the same, and for some to change, on this view. My example is chosen to avoid this complication.

¹⁴ Perhaps there are two different senses of 'evidence' – one subjective/intersubjective, and the other objective – employed here. That is, unless the subjective/intersubjective evidence is taken to be non-propositional. My own view is that there are *some* situations where the evidence itself changes, although this hypothetical scenario may not be one of them.

to h , or there are some serious alternatives to h that predict e because they involve unexpected interpretations thereof – we can see that there are highly unpredictable routes by which confirmation values can change.¹⁵

Here's a brief illustration. It would be odd to insist that the prior attached to 'Ducks are depicted' *should* be higher than that attached to 'Rabbits are depicted' (relative to background information that both kinds of animal were around at the time). Moreover, both theories save the (relevant) phenomena. Hence, if we let the duck theory be represented by h and the rabbit theory be represented by h^* , we may say – as a matter of fact on a non-subjective interpretation of probability, and for some reasonable people on *subjective* view of probability – either that: (1) $P(h,b) \approx P(h^*,b)$ and $P(e,hb) = P(e,h^*b)$; or (2) $P(h,b) \approx P(h^*,b)$ and $P(e,hb) = P(e^*,h^*b)$, where any suitable theory should account for *either* e or e^* (and e and e^* are mutually exclusive).

To summarize, the possibility of unconceived observations of the kind discussed above is significant in *raising* the plausibility of the claim that there may be *serious* unconceived alternative theories. We will discuss unconceived observations due to unconceived experiments, rather than unconceived theories, below.

¹⁵ This may be conceded without adopting any form of extreme relativism, or completely collapsing the distinction between fact and theory. One need not go as far as Feyerabend (1958). The point can hold even if one only holds, with Harré (1959, p. 43): 'that only some descriptive statements involve terms whose meanings depend partly on theory. In any event, realists have used theory-ladenness as an argument for the view that the line between the observable and the unobservable can shift; see, for instance, Maxwell (1962). To deny theory-ladenness is to concede much – too much, perhaps – to instrumentalists.

In closing this section, I should mention unconceived observations of a final kind. These involve new and unanticipated phenomena seen *without* theoretical changes or the aid of experiments. The appearances of newly discovered plants and animals are cases in point. (No-one conceived of the appearance of the brontosaurus, for example, until bones from the beast were discovered. And if we were to encounter a well-preserved brontosaurus, we might still be surprised by its appearance.) The order of such appearances may be contingent, and affect the direction of science. But I shall not consider this possibility in any depth here.

3. Unconceived Models and Unconceived Predictions

Models are necessary in science because theories alone often lack appropriate predictive force.¹⁶ Consider pendulum motion in classical mechanics. An early model was the simple pendulum; the mass of the rod bearing the bob is ignored, as is friction, and a small angle of swing is assumed (such that the sine of the angle is approximately equal to the angle). Moreover, the movement is taken to occur only in two dimensions. But the adequacy of classical mechanics to deal with *real* pendulum motion was unclear initially, in so far as more sophisticated models were yet to be conceived of. (It is also easy to create only slightly more complicated systems, *in terms of component parts*, which are *much* harder to deal with via classical mechanics. Consider the double pendulum.) That is to say, tractable models with

¹⁶ In what follows, I mainly discuss abstract, rather than concrete, models; on the concrete side, I mention only model organisms. However, concrete models are important more broadly, in so far as they can function, for example, as means of animating theories. Think of the antikythera mechanism – see De Solla Price (1974) – as a case in point. For more on concrete models in non-biological contexts, see Rowbottom (2009).

fewer idealisations were developed only slowly, over a period of time. And the true predictive power of classical mechanics was unclear for over a hundred years after Newton, at least. Lagrangian and Hamiltonian mechanics, for instance, were vital for some applications. Such *reformulations* of classical mechanics, which were employed in the model building process, were not readily apparent. And Butterfield (2004) argues that such reformulations may fall between the levels of ‘laws of nature’ and ‘models’.

Why does this matter for confirmation? In essence, unconceived models may be responsible for unconceived *predictions*, and the resources of a theory may fail to be apparent – and be underestimated (or even overestimated¹⁷) – as a result. A semi-formal illustration follows. (Think now in non-subjective terms, for simplicity’s sake.) Let e represent the total body of available evidence that a theory in mechanics is expected to account for. And let h and h^* represent the two available theories in mechanics (i.e., the only two conceived theories that have not been shown to predict $\sim e$ in conjunction with b). $C(h^*, e, b)$ may be much higher than $C(h, e, b)$ because $P(e, hb)$ is much lower than it should be. And it may be much lower than it should be due purely to unconceived models based on h (or unconceived reformulations of h).¹⁸

¹⁷ See the discussion of expectations concerning Newtonian mechanics and the tides, in the next section.

¹⁸ Tractability is an important issue, which is bound up with the talk of models and reformulations. Here’s an example from Cartwright (1998, p. 2):

Solution of Laplace’s tidal equations, even in seas of idealized shape, taxed mathematicians for well over a century until the advent of modern computers. Even then, some decades were

For example, h and b might entail e , whereas h^* and b might not. However, only the following might have been shown: h^* and b entail e^* , and h and b entail e^\dagger , where e^* and e^\dagger are each proper subsets of e , and e^\dagger is a proper subset of e^* .

In summary, even judgements of *comparative* confirmation depend on judgements about the predictive power of theories, and such judgements are contingent on the available, and hence *conceived*, models. So why be confident that there are no unconceived models that would affect (estimated) confirmation values? (Again, the anti-realist need not suggest that one should be confident that there *are* such unconceived models.) This is a further challenge to the realist.

In closing, I should mention that there is another sense of ‘model’, common in the biological sciences, which may also be relevant; that of ‘model organism’. Clearly no *undiscovered* organism has been conceived of as a model, in this sense. Indeed, it is even possible to discover some organism and not conceive of using it as a model, out of ignorance about some of its properties. However, it would take us too far astray to discuss models of this form (and reasoning by homology, and well as reasoning by analogy).

4. Unconceived Explanations

Put aside the previous worries about models, and imagine, for the time being, that observation statements can typically be derived directly from theories (without even

to elapse before computers were large enough [sic] to represent the global ocean in sufficient detail, and techniques had improved sufficiently to give realistic results.

the need for reformulations). Assume also a syntactic view of theories. Now, for the sake of exposition, we can use Hempel's (1965) deductive-nomological account of explanation. On this view, an explanans must be true, entail the explanandum, contain a general law statement ('theory'), and have empirical content. (This is the basic picture, although some small refinements may be added. For example, it can be stipulated that no *proper* subset of the propositions in the explanans should entail the explanandum.) Hence, the explanans for 'The pen hit the ground one second after it was dropped' might once have been thought to involve Newton's law of gravitation and Newton's second law of motion, the mass of the Earth, the mass of the pen, the distance between the centre of mass of the Earth and the centre of mass of the pen, and the distance between the pen and the ground.

Because the explanans should be true, however, we should take into account the rotation of Earth, use special relativity instead of Newtonian mechanics (assuming the former is true), and so forth.¹⁹ Thus, it becomes extremely difficult, at any point in time, to distinguish between an *actual* and a *potential* explanation. So let's just discuss *potential* explanatory power, in what follows. The *potential* explanatory power of a theory (or bundle of theories) depends only on which *known* observation statements it entails (when conjoined with true statements of initial conditions). Think of it as what the theory would explain *if it were true*. The truth status of said theory (or bundle of theories) is irrelevant.

¹⁹ My own view is that explanation – like understanding – is non-factive; I am sympathetic, for example, to the views of Elgin (2007). Since this is disputed by many realists, I here treat it as factive.

Now think about how we measure potential explanatory power more carefully. As noted above, we require *true statements of initial conditions*. But even granting that we can tell that determine whether any statement of initial conditions is true, when it's considered, a problem remains. For in some cases, *we may simply fail to conceive of the initial conditions*.

Consider, for example, the history of the study of the tides. One threshold moment was Newton's treatment, in the *Principia*. But this only showed that *some* aspects of the tides could (potentially) be explained. As Cartwright (1999, p. 2) writes:

From time to time a new idea has arisen to cast fresh light on the subject. While such events have spurred some to follow up the new ideas and their implications, they have also had a negative effect by appearing superficially to solve all the outstanding problems. Newton's gravitational theory of tides... [potentially] explained so many previously misunderstood phenomena that British scientists in the 18th century saw little point in pursuing the subject further.

The *superficial* explanatory power noticed by Cartwright – an eminent oceanographer – arises, to some extent, because of the unconceived initial conditions in (and concerning) our seas and oceans, which are highly complex. So in effect, beliefs that the periods of the tides in any specific area could be (potentially) explained by Newtonian mechanics, in the eighteenth-century, were largely *on faith* (or, at the minimum, a rather dubious extrapolation from successes in some contexts to future successes in others). It was *not* just a matter of thinking that the values of variables of

known types, e.g. ocean floor topography and coastal geography, were relevant to saving the phenomena. It was, moreover, a matter of thinking that all the *relevant* types of variables had been conceived of. But they hadn't. The discovery of Kelvin waves, for example, came considerably later. And this sort of pattern has been repeated throughout the history of research into the tides, according to Cartwright (1999, p. 1):

[E]very improvement in accuracy of measurement and prediction has led to further fundamental research into previously hidden details.²⁰

In essence, the point here is that *judgements of explanatory power are liable to change considerably, just as judgements of predictive power are*, as the limits of the conceived expand. (And judgements of the relative merit of theories, on the basis of estimated explanatory power, are liable to change as a result.)

Incidentally, in using the D-N account of explanation, above, I have also advanced a further argument that judgements of predictive power may be highly error prone, *provided that there are relevant cases where explanation is symmetrical with prediction*. (This does *not* require that explanation is symmetrical with prediction in general.) I might also have considered the possibility that models can play explanatory roles (as I think they do, despite their typical falsity – again, see Elgin 2007), and therefore that the development of new models can change evaluations of the explanatory power of theories. However, I leave such speculations – about how it

²⁰ Surprisingly, Cartwright (1999, p. 4) nevertheless endorses convergent realism at one level: 'the *global* aspects of tidal science... seem to have reached a state of near-culmination'.

is best to interrelate the considerations in this section with those in the last – to the reader.

5. Unconceived Experiments, Methods, and Instruments

This brings us to experiments. Even given a theory and models that render it predictive in a domain of interest, the possible experiments involving it, which can be performed in practice, are not made manifest. Partly, this is due to the instruments and methods that have not been conceived of. Think of the role played by the torsion balance in Cavendish's (1798) measurement of the mean density of the Earth, which did not occur until long after Newton's death. New ways to measure the gravitational constant (which may be easily calculated from the aforementioned density) have been devised even in the last decade; the most recent experiment, performed by Rosi et al. (2014), achieves an astounding reduction in experimental error.

But this is far from the whole story. To design an effective experiment, or an experiment that is possible to perform given funding constraints, may require a great deal of ingenuity. Consider blind and double blind experiments, which were possible – and arguably, possible to *positively* conceive of – for centuries. Nonetheless, the first recorded example occurred in the late eighteenth century, when King Louis XVI appointed commissioners to investigate animal magnetism.²¹

²¹ For a brief summary of the episode, and references to some of the relevant literature, see Kaptchuk (1998) and Best et al. (2003). See Kaptchuk (1998, n. 9) for a mention of some precursors.

Why does this matter for realism? Scientists' assessments of their theories depend on the evidence at their disposal. (Such evidence also affects their assessments of the attractiveness of the *research programmes* involving said theories.) And the available experiments delimit the available evidence. Hence, which theories are more confirmed/corroborated, and therefore whether progress towards truth occurs, is (sometimes) contingent on which experiments are conceived of.

The significance of unconceived experiments is greater still if novel predictions have more power to confirm than accommodations, as argued by philosophers such as Maher (1998) and Douglas and Magnus (2013).²² For the extent to which we can make novel predictions is contingent upon the new experiment types – and not merely new experiment tokens – that we can conceive of. Indeed, some theories have plausibly suffered, in comparison to their counterparts, precisely because they made no new predictions. Consider Bohm's 'interpretation' of quantum mechanics, which is a different quantum mechanical theory from those seriously considered beforehand, from a realist perspective, due to its distinctive claims about the unobservable, most notably that particles have definite positions at all points in time and that their states evolve deterministically.²³ The mere fact that Bohm's 'interpretation' appeared after the Copenhagen 'interpretation' would make it less confirmed by the evidence, given that the two theories are (apparently) empirically equivalent and the latter was used to predict some of the evidence that the former was not (and the converse does not

²² The opposing view is defended by Harker (2008). For a nice summary of the historical views on this issue, see Musgrave (1974).

²³ It is a matter of dispute as to whether the wave function need be understood as an element of physical reality. See, for example, Dürr et al. (1997).

hold).²⁴ Thus its *contingent* fate as a marginal (or ‘sidelined’) theory – as illustrated by Cushing (1994) – was appropriate, provided that its prior probability was (and remained, as background information changed) no higher than that of its rival.

But maybe there is an experiment, as yet unconceived, that would discriminate between the Copenhagen and Bohmian views? It would be the height of arrogance to be certain that there is not, in so far as the predictions we can make from the theories depend, as is evident from some of the formal representations we considered above, on *background information* (including *auxiliary hypotheses*). But why should we even think that there is *probably* not any such experiment? What is it that the realist knows about how background information will probably change, in the future, which *licenses* that inference? Again, this is a challenge. It is not a rhetorical question.

Consider also one final sense in which unconceived experiments can result in alterations of confirmation/corroborations values, on views which link such values closely to hypothesis testing. There is an intuitive sense – which might be made more precise in a variety of formal fashions – in which some tests are more severe than others. And for some philosophers of science, how strongly a theory is to be preferred is a function of how well it has been tested. But then, of course, the fates of theories depend on the experimental tests conceived of. For example, Popper (1959, p. 418) writes: ‘ $C(h, e)$ can be interpreted as degree of corroboration only if e is *a report on the severest tests we have been able to design.*’ So on one reading, which is explored in detail in Rowbottom 2008, merely designing (*qua* conceiving of) a new experiment

²⁴ As noted by Faye (2014), ‘Copenhagen interpretation’ is really ‘a label introduced... to identify... the common features behind the Bohr-Heisenberg interpretation’.

– which can be performed in practice, and not merely in principle, perhaps – is sufficient to render current corroboration values irrelevant. That’s because one can’t have a *report* on the severest tests one has designed unless one has also performed said tests. Consider, in this regard, the remarkable experiment performed on Gravity Probe B, concerning the motion of a gyroscope orbiting the Earth. The probe was launched over forty years after Schiff (1960) proposed such a test, noting that ‘experimental difficulties... are greatly reduced if the gyroscope does not have to be supported against gravity... experiments of this type might be more easily performed in a satellite’. The final results from the experiment appeared in Everitt et al. 2011.

6. Unconceived Values (or Theoretical Virtues)

A final item, my treatment of which is somewhat more speculative, is values *qua* theoretical virtues. Consider, for example, Kuhn’s (1977, p. 321) list thereof: ‘accuracy, consistency, scope, simplicity and fruitfulness’. We have assumed the importance of some of these, in the previous discussion. For example, we’ve discussed how the limits of what we’ve conceived might adversely affect our estimates of accuracy and scope, and touched on how unconceived theories may be simpler than, despite being otherwise as virtuous as, their conceived counterparts. Indeed, one rough way to present the standard argument from unconceived alternatives is: ‘Unconceived theories may be – or are, or often are – more virtuous than those we’ve conceived of.’

Now how we *rank or weigh* the virtues, even assuming that we agree on them, will affect the values assigned to priors, such as $P(h,b)$.²⁵ For example, you and I might prefer different theories simply because I think that simplicity is more valuable than scope, whereas you think that scope is more valuable than simplicity. (This is irrespective of our individual stances on the realism debate. We may agree on what the theoretical virtues are, but disagree on whether they are pragmatic or epistemic in character.) Here, however, I'm concerned with whether there are virtues that we've not conceived of, and in whether conceptions of virtues change in interesting ways over time. From a realist perspective, for example, are there indicators of truth-likeness that we have not yet conceived of (and therefore failed to recognize)? Is there any principled way to show that the probability of such unconceived theoretical virtues is low?

Now Kuhn (1977, p. 335) concludes, on the basis of his limited sample from the history of science, that: 'If the list of relevant values is kept short . . . and if their specification is left vague, then such values as accuracy, scope and fruitfulness are permanent attributes of science'. In order to stack the argumentative deck (concerning unconceived values) in the realist's favour, let's grant this. Let's grant even that Kuhn's list of values is exhaustive, so that we do not have to invoke mysterious undiscovered values. The question still remains as to whether understanding of those values can change over time. For example, might one man's simplicity be another woman's complexity?

²⁵ For further discussion of this phenomenon, with particular attention to interpretation of probability, see Rowbottom 2011, ch. 3.

The point is not *merely* that simplicity may be sub-divided, into syntactic ('elegance') and ontological ('parsimony') varieties (among others, perhaps). Rather, the notion is that what counts as simple, even within such sub-divisions, may nevertheless be a matter of legitimate dispute. Consider elegance, in the case of astronomical models of the solar system. The findings of Kuhn (1957) support the conclusion that reasonable disputes may occur. For example, should Tusi couples be used in place of Ptolemaic equants? Let h be a theory (or model) involving the former, and h^* be a theory (or model) involving the latter. $P(h,b)$ may be higher than $P(h^*,b)$, whereas $P(h,b^*)$ may be lower than $P(h^*,b^*)$, where b and b^* represent different background assumptions.

Similar concerns arise concerning consistency (in so far as h may be consistent with other scientific theories relative to b , but not b^*) and fruitfulness (which is notoriously difficult to measure, in any event), but I will not press the point here. The nature of this kind of challenge to realism is already evident.

7. Conclusion

Grant the (highly implausible) thesis that the possible is a subset of the conceivable in practice. What's conceived is nonetheless limited, for a variety of reasons; limitations on time and material resources, contingencies about where attention is directed, and so forth. The tenability of scientific realism of a convergent variety depends on those limits being less significant, over time. And the tenability of the view that contemporary ('well-confirmed') theories are approximately true, even in what they say about the unobservable, relies on those limits being insignificant in a *remarkable number* of (rather diverse) respects.

Given the absence of effective arguments that those limits are not insignificant in these respects, scientific realism is unsupported by the available evidence. It is less prudent than anti-realist alternatives involving *agnosticism* about the truth-likeness of contemporary theories. If scientific realism is to become respectable, the challenges enumerated above must be answered.

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