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New objections to the problem of unconceived alternatives

Novas objeções ao problema das alternativas não concebidas

Seungbae Park[1]

ABSTRACT

The problem of unconceived alternatives can be undermined, regardless of whether the possibility space of alternatives is bounded or unbounded. If it is bounded, pessimists need to justify their assumption that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that scientists have already eliminated enough false alternatives. If it is unbounded, pessimists need to justify their assumption that the probability that scientists have not yet moved from the possibility space of false alternatives to the possibility space of true alternatives is higher than the probability that scientists have already moved from the former to the latter space.

Keywords: changing spaces, Humean skepticism, scientific pessimism, scientific realism.

RESUMO

O problema das alternativas não concebidas pode ficar prejudicado, independentemente de o espaço de possibilidade das alternativas ser limitado ou ilimitado. Se é limitado, os pessimistas necessitam justificar sua suposição de que a probabilidade de os cientistas ainda não terem eliminado falsas alternativas suficientes seja maior do que a probabilidade de que os cientistas já tenham eliminado falsas alternativas suficientes. Se ele é ilimitado, os pessimistas necessitam justificar sua suposição de que a probabilidade de os cientistas ainda não terem passado do espaço de possibilidade das falsas alternativas para o espaço de possibilidade das verdadeiras alternativas seja maior do que a probabilidade de que os cientistas já tenham passado do primeiro espaço para o segundo.

Palavras-chave: mudança de espaços, ceticismo humeano, pessimismo científico, realismo científico.

[1] Professor of Philosophy at Ulsan National Institute of Science and Technology in the Republic of Korea. 50, UNIST-gil, Ulsan, South Korea. Email: nature@unist.ac.kr.

1. Introduction

P. Kyle Stanford (2006) has developed an intriguing problem, the problem of unconceived alternatives (PUA). It asserts, roughly, that since scientists in the past could not conceive of the alternatives that ousted their accepted theories, current scientists also cannot conceive of the alternatives that will oust currently accepted theories. If the PUA is correct, then present theories are false; there are unconceived alternatives to them which will replace them, and hence we should not advocate scientific realism. The PUA has elicited several critical responses from realists.

This paper raises new objections to the PUA. In Section 2, I expound the PUA. In Section 3, I argue that if the possibility space is bounded, pessimists need to justify their assumption that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that scientists have already eliminated enough false alternatives. If, on the other hand, the possibility space is unbounded, pessimists need to justify the assumption that the probability that scientists have not yet moved from the space of false alternatives to the space of true alternatives is higher than the probability that scientists have already moved from the former to the latter space. In Section 4, I respond to the possible objections that I should defend scientific realism instead of merely criticizing pessimism, that pessimism claims not that present theories are false but rather that present theories are unwarranted, and that a list of past theories nonetheless refutes scientific realism. At the end of the day, it will become clear that the PUA can be undermined, and that realism is no worse off than pessimism.

2. The problem of unconceived alternatives

The PUA asserts that "the history of scientific inquiry itself offers a straightforward rationale for thinking that there typically are alternatives to our best theories equally well-confirmed by the evidence" (Stanford, 2006, p. 20). How do pessimists justify the contention that there are alternatives? Stanford offers a list of theory-transitions that have occurred:

Stanford's List

> *from elemental to early corpuscularian chemistry to Stahl's phlogiston theory to Lavoisier's oxygen chemistry to Daltonian atomic and contemporary chemistry*
>
> *from various versions of preformationism to epigenetic theories of embryology*
>
> *from the caloric theory of heat to later and ultimately contemporary thermodynamic theories*
>
> *from effluvial theories of electricity and magnetism to theories of the electromagnetic ether and contemporary electromagnetism*
>
> *from humoral imbalance to miasmatic to contagion and ultimately germ theories of disease*
>
> *from eighteenth century corpuscular theories of light to nineteenth century wave theories to the contemporary quantum mechanical conception*
>
> *from Darwin's pangenesis theory of inheritance to Weismann's germ-plasm theory to Mendelian and then contemporary molecular genetics*
>
> *from Cuvier's theory of functionally integrated and necessarily static biological species and from Lamarck's autogenesis to Darwin's evolutionary theory (Stanford, 2006, p.19-20).*

This list provides the basis for the PUA. Without it, we have no reason for thinking that past scientists were not able to think up present theories. Because Stanford's list is longer than Larry Laudan's (1981, p. 33) list of past theories, it appears that the former is better than the latter in making a case for pessimism, the view that present theories are false.

A couple more points need to be made about the PUA. First, it ingeniously combines the problem of underdetermination and the pessimistic induction. The latter holds 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" (Stanford, 2006, p. 7). The problem of underdetermination occurs when rival theories make (more or less) the same claims about observables, but radically distinct claims about unobservables. These rival theories are not moderately but "radically distinct alternatives" (Stanford, 2006, p. 19).

Second, Stanford contends that the possibility space "appears to be indeterminate and unbounded" (2006, p. 133). To put it differently, there seem to be an infinite number of alternatives in the possibility space. Scientists cannot exhaust this unbounded possibility space because they can only remove a finite number of alternatives. The number of alternatives in the possibility space will never decrease, no matter how many alternatives scientists remove. Present scientists do not have a better ability than past scientists to exhaust the possibility space (Stanford, 2006, p. 45). Therefore, new theories will supersede accepted theories indefinitely into the future.

The PUA evoked several critical responses from realists. To summarize: Stanford's list is biased (Ruhmkorff, 2011, p. 881; Mizrahi, 2015); Current theories are better than their forerunners (Devitt, 2011, p. 292; Ruhmkorff, 2011, p. 878); Scientists' arguments for present theories are different from or better than

those for past theories (Enfield, 2008, p. 891; Roush, 2010, p. 34; Fitzpatrick, 2013, p. 145); Some theoretical elements of a past theory were retained in a present theory (Psillos, 2009; Saatsi, 2009; Ruhmkorff, 2011, p. 882); and it is dubious that infinitely many scientific theories exist (Ruhmkorff, 2011).

Samuel Ruhmkorff (2011) contends that from the fact that accepted theories have been rejected in favor of alternatives it does not follow that there are infinitely many alternatives. Thus, justification is required for the belief that the space of alternatives is unbounded. Considering Ruhmkorff's observation, I take it to be an open question whether the space of alternatives is bounded or unbounded. Thus, I will tackle the PUA in the next section both under the condition that there are finitely many alternatives and under the condition that there are infinitely many alternatives.

3. Finite or infinite

3.1. Finite

How can realists argue that present theories are true under the condition that the space of alternatives is bounded? They could say that the number of past theories is so large, as indicated by Stanford's list, that it amounts to an inductive rationale for the position that scientists have already eliminated enough false alternatives, such that they are already at the ends of the chains of alternatives, and hence that present theories are true. To simplify, suppose that there are six alternatives in the space, that only one of them is true, and that scientists have already eliminated five false alternatives. Realists can say that the list of the five false alternatives is long enough, i.e., scientists have already eliminated enough false alternatives, and hence that the present theory is true.

Under the same condition, how can pessimists argue that present theories are false? They could say that scientists have not yet eliminated enough false alternatives, that scientists need to eliminate more false alternatives before arriving at the ends of the chains of alternatives, and hence that present theories are false. In other words, Stanford's list of past theories is too short and fails to constitute an inductive rationale for thinking that scientists have eliminated enough false alternatives. To simplify, suppose that there are six alternatives, that only one of them is true, that scientists have eliminated two alternatives, and hence that the present theory is the third of the six alternatives. Pessimists can say that the list of two false alternatives is too short to show that scientists have already eliminated enough false alternatives, and that scientists need to eliminate more false alternatives, including the present theory, before reaching the true theory.

Note that pessimists claim that scientists have not yet eliminated enough false alternatives while realists claim that they have. They have reached a stalemate.[2] In such a stalemate, each side should establish that their position is more likely to be true. Pessimists should justify their assumption that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that scientists have eliminated enough false alternatives. In contrast, realists should justify the opposite assumption. To date, neither pessimists nor realists have attempted to justify these assumptions in the literature. No pessimist has shown that Stanford's list of past theories is too short; no realist has shown that it is long enough.

For the PUA to succeed in showing that present theories are false, pessimists need to break the stalemate in their favor, i.e., they need to establish that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that scientists have already eliminated enough false alternatives. They need to show, for example, that the former is 51% while the latter is 49%. Note that I am not holding the PUA to an inordinately high standard. That is, I am not requiring that pessimists offer a deductive argument proving that scientists have not yet eliminated enough false alternatives. I am only requiring that pessimists provide an inductive argument showing that scientists have not yet eliminated enough false alternatives.

My preceding objection to the PUA can be illustrated by a famous episode described by Carl Hempel (1966, p. 3-6). The Vienna General Hospital had two maternity divisions in 1846. Surprisingly, about 10% and 2% of women died of childbed fever in the first and second divisions, respectively. Ignaz Semmelweis tested six hypotheses to identify the cause of the excessively high death rate of the first one. He eliminated five hypotheses before arriving at the sixth hypothesis that cadaverous materials on medical students' hands had caused infections in the women in the first division. The first five hypotheses were useless, whereas the sixth hypothesis was useful, in lowering the inordinately high mortality rate.

What matters for this paper is not the content of the five unsuccessful hypotheses, but rather the fact that Semmelweis eliminated them before reaching the successful hypothesis. Suppose that pessimists and realists were observing Semmelweis when he was testing the sixth hypothesis. Pessimists would have predicted that the sixth hypothesis would fail the test, offering the list of five unsuccessful hypotheses and saying that the list was too short, i.e., that Semmelweis should eliminate more unsuccessful hypotheses before reaching a successful hypothesis. Realists would have predicted that the sixth hypothesis would pass the test, offering the list of five unsuccessful hypotheses and saying that the list was long enough, i.e., that Semmelweis had already eliminated enough unsuccessful hypotheses before reaching a successful hypothesis (Park, 2018b, p. 330-331).

[2] Park (2018a, p. 8) constructs a similar stalemate between those who claim that T is empirically adequate and those who claim that T is empirically inadequate.

The list of five discarded hypotheses could not adjudicate between pessimists' and realists' predictions regarding the issue of whether the sixth hypothesis would succeed. After all, their opposing predictions rested upon the same list of five unsuccessful hypotheses. It was useless for pessimists to point to the list and insist that the sixth hypothesis would fail the test, for realists could point to the same list and claim that the sixth hypothesis would pass the test. The force of the pessimists' argument was completely countervailed by that of the realists' argument. This point also applies to the cases in which Semmelweis was testing the fifth hypothesis or the fourth hypothesis. No matter how short or long the list of unsuccessful hypotheses was, it could not adjudicate between the pessimists and the realists over whether Semmelweis's next hypothesis would succeed.

Similarly, Stanford's list cannot adjudicate whether scientists have eliminated enough alternatives. Both pessimists and realists appeal to Stanford's list to justify their opposing positions, and the force of the pessimists' argument is completely nullified by that of the realists' argument. The length of the list does not affect this stalemate. No matter how long it might be, it cannot adjudicate between pessimists and realists over whether current theories are true or false. Under these circumstances, it is unclear how pessimists could persuade realists that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that they have. It follows that the PUA does not go through.

Some philosophers are racing towards a long list of past theories. Laudan (1981, p. 33), Timothy Lyons (2003, p. 898-899), and Stanford (2006, p. 19-20) provide such lists. We tend to be impressed by them and, thus, at least initially embrace the pessimistic prediction that present theories are false. On close examination, however, there is a gap between the lists and the pessimistic prediction. Even if a list of past theories were perfect, the inference from the list to the pessimistic prediction is illegitimate. Suppose that pessimists painstakingly delved into the history of science and came up with an exhaustive list of *all* past theories, which perfectly represented the population of past theories. Such a list would only prod realists to say that this complete list plainly shows scientists have eliminated enough alternatives and hence present theories must be true (Park, 2018b, p. 337).

Pessimists have presupposed all along in the literature that the longer the list of past theories is, the better it is for pessimism; realists have presupposed all along in the literature that the shorter the list is, the better it is for realism. For these reasons, pessimists have worked hard to come up with long lists of past theories, and realists (Musgrave, 1985, p. 211; Leplin, 1997; Saatsi, 2009, p. 358) have tried to prune the lists by saying that past theories on the lists, although successful, did not make novel predictions.

The present discussion indicates, however, that both pessimists' and realists' presuppositions were false. Realists can reject pessimism on the grounds that the list is long enough to show that scientists have already eliminated enough false alternatives; pessimists can reject realism on the grounds that the list is too short to show that scientists have already eliminated enough false alternatives. Thus, the longer the list of past theories is, the better for realists; the shorter it is, the better for pessimists.

3.2. Infinite

Suppose now that the space of unconceived alternatives is unbounded. Under this framework, pessimists seem to have a strong case against realism, as explained in Section 2. Should we thus give up on realism? André Kukla replies that "it is not obvious that an infinite space of theories cannot be exhausted by a finite number of theoretical subtractions" (2010, p. 245). His idea is that as scientists uncover more alternatives, they might be getting steadily closer to exhausting the unbounded space. He does not specify, however, how they can eliminate an infinite number of alternatives by eliminating a finite number of them.

My response differs from Kukla's. Consider again the episode of Semmelweis. He eliminated five hypotheses before arriving at a successful hypothesis. Imagine again that pessimists watched Semmelweis as he was testing the sixth hypothesis. Since there are an infinite number of unsuccessful hypotheses, they would have predicted that he would indefinitely flounder in the space of those infinitely many unsuccessful hypotheses. Their prediction was false. It is a historical fact that he reached the successful hypothesis. He moved from the space of unsuccessful hypotheses to the space of successful hypotheses, i.e., he pulled five unsuccessful hypotheses in a row from the former space and then pulled the successful hypothesis from the latter space. Therefore, it is wrong to think that he had to exhaust the space of infinitely many unsuccessful hypotheses before obtaining a successful hypothesis.

The kinetic theory replaced the caloric theory in the nineteenth century, a transition which appears on Stanford's list. Suppose that the downfall of the caloric theory makes it likely that there is an infinity of false theories of heat. Does it follow that scientists will indefinitely eliminate false alternatives and will never arrive at the true theory of heat? Pessimists say, "Yes," whereas realists say, "No." Both, however, need to defend their answers. Pessimists have the burden of showing that kinetic theorists in the nineteenth century did not switch from the space of false alternatives to the space of true alternatives. Realists have the burden of showing that they did switch spaces.

To break this stalemate, each side should establish that their position is more likely to be true. That is, pessimists should justify their assumption that the probability that scientists have not yet moved from the space of false alternatives to the space of true alternatives is higher than the probability that they have. In contrast, realists should justify their assumption that the probability that scientists have already moved from the space of false alternatives to the space of true

alternatives is higher than the probability that they have not. To date, neither pessimists nor realists have attempted to justify these assumptions in the literature.

It would be difficult for pessimists to justify their assumption. Recall that Semmelweis actually moved from the space of unsuccessful hypotheses to the space of successful hypotheses. It is not clear what would entitle pessimists to say that kinetic theorists, such as Count Rumford, were not able to change spaces, while Semmelweis could. What is the relevant difference between Semmelweis and Rumford? Can biologists switch the spaces, but physicists cannot?

Pessimists might claim that scientists can move from the possibility space of unsuccessful alternatives to the possibility space of successful alternatives, but not from the possibility space of false alternatives to the possibility space of true alternatives. This is exactly what Stanford would say, given that he defends instrumentalism, after advancing the PUA. He says that "we might use our theories for prediction, intervention, and other pragmatic purposes without believing the theoretical descriptions they offer of the natural world" (2006, p. 197). It appears that pessimists can believe that current theories are successful, but not that they are true.

Realists would reply, however, that this suggestion involves a double standard. Why is it that scientists can move from the possibility space of unsuccessful alternatives to the possibility space of successful alternatives, but not from the possibility space of false alternatives to the possibility space of true alternatives? It would beg the question for pessimists to appeal to the pessimistic induction or to the problem of underdetermination to justify this suggestion, for these anti-realist ideas are the constituents of the PUA and the PUA is the subject of the present dispute.

It is a difficult task for a pessimist-cum-instrumentalist to justify the double standard. Elegance is a theoretical virtue for both realists and instrumentalists. For example, physicists use quantum mechanics when investigating very small objects, such as electrons and quarks. They use the general theory of relativity, however, when investigating very large objects, such as a galaxy and the universe. They are unhappy with this current status and have tried to come up with a grand unified theory to use whether they deal with small or large objects. Even instrumentalists would support the advent of such a theory. It follows that elegance dictates that we choose a single standard over a double standard on the issues of switching spaces.

In any event, the longer the list of past theories is, the better it is for realists; the shorter it is, the better it is for pessimists. Realists can reject pessimism on the grounds that the list is so lengthy that it shows that scientists have switched the spaces of true and false alternatives; pessimists can reject realism on the grounds that the list is too short to show that scientists have stitched the spaces of true and false alternatives. The bottom line, however, is that no matter how long the list, it cannot resolve the dispute between pessimists and realists, given that they would take the same list to be too short, or long enough, to justify their conflicting positions.

4. Objections and replies

4.1. The burdens of proof

In Section 3, I argued pessimists need to justify two assumptions. If the possibility space is bounded, they should justify the assumption that the probability that scientists have not yet eliminated enough false alternatives is higher than the probability that they have. If the possibility space is unbounded, they should justify the assumption that the probability that scientists have not yet moved from the space of false alternatives to the space of true alternatives is higher than the probability that they have. Pessimists might object that such a philosophical strategy is unimpressive. Instead of merely exposing pessimists' burdens of proof, I should present a novel argument for realism. No such argument can be found in this paper.

I make the following two comments on this objection. First, new arguments for realism can be found in Park (2011, p. 23; 2018b, Section 2; 2018c, p. 57-61; 2019a, Section 4; forthcoming, Section 3). This paper does not aim to present such arguments. It rather aims to undercut the PUA, which can be achieved merely by exposing the two assumptions that pessimists need to justify. This paper is in the same spirit as Moti Mizrahi's (2013, 2015, 2016) papers. He too criticizes the pessimistic induction without presenting any new argument for realism.

Second, one of the strongest arguments against realism is the problem of underdetermination. Realists believe that T_1 is true. Critics offer a competitor, T_2, which is empirically equivalent to, but logically incompatible with, T_1, thereby challenging realists to demonstrate the superiority of T_1 over T_2. The literature on the problem of underdetermination is voluminous. Any reader who thinks that creating a burden of proof against one's opponent is not an impressive philosophical strategy owes us an account of why philosophers have generated voluminous literature on a presumably unimpressive argument against realism.

4.2. Humean skepticism

Pessimists might also object that my interpretation of the PUA is uncharitable. Contrary to how I interpreted it in Section 2, it does not make the strong assertion that present theories are false. It rather makes the weaker assertion that they are unwarranted. Thus, the PUA involves not (P) but rather (H):

(P) Because extant theories were false, current theories are also false.

(H) Because extant theories were false, current theories are unwarranted.

Note that the premises of (P) and (H) are the same, but their conclusions differ. The conclusion of (P) is a strong

claim, whereas the conclusion of (H) is a weak claim. A strong claim is more vulnerable to criticism than a weak claim.

The PUA, pessimists might continue, trades on the insight that even if current theories are true, the unfortunate history of science provides reasons for doubting that they are true. The number of unconceived alternatives does not matter. Even if there were no unconceived alternatives that will supplant present theories, present theories would be unwarranted. Moreover, the PUA does not rely on the assumption that scientists have not yet eliminated enough false alternatives, nor on the assumption that they have not yet switched from the space of false alternatives to the space of true alternatives. It rather relies on the fact that some earlier theories were false.

Contrary to what I claimed in Section 3.1, pessimists might argue that the PUA does not claim that true theories are probably at the ends of chains of alternatives. It rather claims that they are equally likely to lie at any point along the chains, and hence present theories might be true. The issue is neither whether present theories are true nor whether scientific revolutions will occur. The issue is rather whether present theories are warranted. They are unwarranted, even if they happen to be true, and even if scientific revolutions will not occur. Stanford's list undermines the position that they are true.

What are we to make of this allegedly charitable interpretation of the PUA? There are three reasons for thinking that it is not a charitable interpretation, but rather a distortion of the PUA. First, as quoted in Section 2, Stanford states that "the history of scientific inquiry itself offers a straightforward rationale for thinking that there typically are alternatives to our best theories equally well-confirmed by the evidence" (Stanford, 2006, p. 20). This statement conflicts with the allegedly charitable interpretation that neither the number of unconceived alternatives nor the prospect of scientific revolutions matter.

Second, as noted in Section 2, the PUA combines the problem of underdetermination with the pessimistic induction. The latter asserts 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" (Stanford, 2006, p. 7). Suffering the same fate implies that present theories are false, which clashes with the allegedly charitable interpretation that the issue is not whether present theories are true. The PUA asserts not that present theories are unwarranted, but rather that they are false.

Third, as mentioned at the end of Section 2, Ruhmkorff (2011) accuses the PUA of making an unjustifiable assumption that infinitely many scientific theories exist. His interpretation of the PUA clashes with the allegedly charitable interpretation of the PUA that the number of unconceived alternatives does not matter.

Although (H) is not attributable to Stanford, pessimists might be tempted to retreat to it, once they realize that they need to justify the two assumptions that I exposed in Section 3. So (H) deserves our critical scrutiny. I make the following two critical comments on it.

First, true theories are not equally likely to exist at any point along the chains of alternatives. As seen in Section 2, one of the realist responses to the PUA is to point out that presently accepted theories are superior to their precursors. If T_1 is superior to T_2, T_1 is more probable than T_2. Given that current theories are more probable than their predecessors, future theories will also be more probable than their predecessors. It follows that true theories are more likely to fall at the ends of chains of alternatives than at any intermediate points of the chains.

My opponents would object that realists need to justify the assumption that we are sufficiently near the end of the chain, or the assumption that present theories are already in the subset of theories likely to be true. Otherwise, realism is not defended.[3]

This is an insightful objection. It is, however, compatible with everything I said earlier in this paper. I stated in Section 3 that realists need to justify the assumption that the probability that scientists have already eliminated enough false alternatives is higher than the probability that scientists have not, and that realists also need to justify the assumption that the probability that scientists have already moved from the space of false alternatives to the space of true alternatives is higher than the probability that they have not. Again, this paper does not defend realism, but rather undercuts the PUA. Moreover, a similar objection can be constructed against pessimism, i.e., pessimists need to justify the assumption that we are sufficiently far from the end of the chain, or the assumption that present theories are in the subset of theories likely to be false. Otherwise, pessimism is not defended.

Second, (H) merely exemplifies Humean skepticism about induction (Park, 2019b, Section 3). Humean skeptics observe that the sun has risen, but conclude that we ought to be skeptical about whether it will rise or not. Similarly, they observe that earlier theories were false and then conclude that current theories are unwarranted. By contrast, pessimists are committed to the reliability of induction. They observe that the sun has risen and conclude that it will rise. Analogously, they observe that extant theories were false and conclude that current theories are therefore false. Thus, if Stanford retreats to (H), he is not an inductivist but rather a Humean skeptic.

Humean skepticism clashes with instrumentalism, which Stanford (2006, p. 197) embraces, as mentioned in Section 3.2. Instrumentalists are committed to the reliability of induction. They affirm that a scientific theory which has made true predictions will continue to make true predictions. Thus, Stanford cannot embrace both Humean skepticism and instrumentalism.

[3] A reviewer of this journal made this useful comment.

Stanford might reply that just because we are Humean skeptics in some domains, it does not follow that we should also be Humean skeptics in other domains. We can be Humean skeptics in some domains, but inductivists in others. For example, we can reason that because extant theories were false, present theories are unwarranted, and yet we can also reason that since the sun has risen, it will rise.[4]

It is an admirable position that we can be Humean skeptics in some domains, but inductivists in others. But why should we be Humean skeptics on the issue of whether current theories are false or unwarranted? What reason is there for thinking that this issue belongs to the set of domains in which we should be Humean skeptics, as opposed to the set of domains in which we should be inductivists? Why should we believe that (H) is a more reasonable inference than (P)? It is one thing to say that we should be Humean skeptics in some domains, but inductivists in other domains; it is quite another to say that we should be Humean skeptics on the issue of whether current theories are false or unwarranted. Some justification is required to move from the former to the latter.

As noted earlier, Stanford embraces instrumentalism, which asserts that since a theory has been useful, it will continue to be useful. Why should we be inductivists with respect to the issue of whether a useful theory will continue to be useful or not, but Humean skeptics with respect to the issue of whether present theories are false or unwarranted? Why not be inductivists on these two issues and Humean skeptics on other issues? It is not clear how my opponents would answer these questions.

4.3. The agnostic deduction vs. the optimistic deduction

My opponents might argue that the pessimistic induction can be understood not as an argument for pessimism, but as a critique of realism, the thesis that present theories are true. Stanford's list may fail to show that present theories are false, but it does contain successful but false theories, and such theories are counterexamples to realism. To use an analogy, a list of some white crows may fail to show that all crows are white, but some white crows are counterexamples to the view that all crows are black. Thus, Stanford's list may not lead to pessimism, the view that present theories are false, but it does lead to antirealism, the view that present theories are unwarranted.[5]

As the foregoing paragraph indicates, my opponents call this objection to realism *the pessimistic induction*. Such a nomenclature would be endorsed by Samuel Ruhmkorff, who says, "The first, the pessimistic induction (PI), is Larry Laudan's attempt to refute the no-miracles argument for scientific realism by attacking the link it alleges between success and truth in scientific theories" (Ruhmkorff, 2013, p. 410). Note that Ruhmkorff calls Larry Laudan's objection to realism "the pessimistic induction." Laudan (1981, p. 33) offers a list of past theories, not to establish pessimism, but to refute realism. Thus, Ruhmkorff would agree with my opponents above that the pessimistic induction can be understood as an objection to realism and not as an argument establishing pessimism.

In my view, however, it is inadequate to call Laudan's objection to realism the pessimistic induction. Consider that Timothy Lyons (2003, 2016, 2017, 2018) *correctly* reconstructs Laudan's objection to realism not as an inductive argument but rather as the following deductive argument:

> 1. *If (a) that realist meta-hypothesis were true, then (b) we would have no successful theories that cannot be approximately true. (If we did, each would be a "miracle," which no one of us accepts.)*
> 2. *However, (no-b) we do have successful theories that cannot be approximately true: the list (of "miracles").*
> 3. *Therefore, (no-a) the realist meta-hypothesis is false. (And the no-miracles argument put forward to justify that meta-hypothesis is unacceptable.) (Lyons, 2016, p. 566).*

This argument is not an inductive argument but rather a deductive argument, *a modus tollens*, to be more specific. No inductive argument inheres in Laudan's objection to realism. Consequently, we cannot call it the pessimistic induction.

Can we call Laudan's objection to realism *the pessimistic deduction*? My answer is "No." The word 'pessimistic' is not precise enough. I propose that we distinguish between pessimism and agnosticism. Pessimism asserts that present theories are false, whereas agnosticism asserts that they are unwarranted. These two positions significantly differ. When pessimism is under attack, philosophers tend to fall back on agnosticism. Thus, these two positions deserve distinct names. In light of this distinction, I call Laudan's objection to realism the *agnostic* deduction instead of the *pessimistic* deduction.

How do I react to the agnostic deduction? Realists can play a similar game, i.e., they can make a list of some successful true theories of the past, such as the oxygen theory,[6] and then construct a corresponding modus tollens: If it were true that all successful theories were false, there would be no successful true theories, but there were some successful true theories, so it is false that all successful theories are false. I call this modus tollens *the optimistic deduction*. Just as the agnostic deduction is only intended to falsify realism, so the optimistic deduction is only intended to falsify pessimism.

[4] I thank the reviewer for this comment.

[5] I thank the reviewer for this insightful objection.

[6] They were accepted in the 20th century, so they are past theories.

My opponents might object that the optimistic deduction is an unimpressive argument, given that it is not a positive argument for realism but merely a negative argument against pessimism.

The optimistic deduction, however, is no worse off than the agnostic deduction. The optimistic deduction is not a positive argument for realism any more than the agnostic deduction is a positive argument for pessimism. Moreover, as quoted above, Lyons (2003, 2016, 2017, 2018) has consistently advanced the agnostic deduction as an objection to realism. Consequently, any objection to the optimistic deduction applies no less to Lyons's agnostic deduction. In addition, if Lyons's agnostic deduction is worthy of attention because it delineates how realism can be attacked, the optimistic deduction is also worthy of attention because it delineates how pessimism can be attacked.

5. Conclusion

I exposed the two burdens of proof that pessimists are saddled with under the conditions that the possibility space of alternatives is bounded and unbounded. Unless these burdens are shouldered, the PUA does not succeed, and it remains simply an open issue whether current theories are true or false. It is incoherent for pessimists to retreat to Humean skepticism in response to my criticism against the PUA. In addition, if pessimists run the agnostic deduction against realism, realists would, in response, run the optimistic deduction against pessimism. Let me sum up this paper with a motto: "A list of past theories cannot adjudicate between realism and pessimism."

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