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Author's Response

to Mousa Mohammadian, William Peden and Elay Shech
on *The Material Theory of Induction*

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Authors should hope—as I do—that their work is not the last word but provides fertile starting points for further investigations. From this perspective, each panelist's response is most welcome. Each raises substantial issues that require further, productive analysis. I have taken great pleasure in reflecting on how best to reply. My thanks to each of them. The replies below are in alphabetical order by author.

Mousa Mohammadian, "Virtues of 'values' and 'virtues': on theoretical virtues and the aim of science"

Mousa Mohammadian has written an interesting and important paper on theoretical values in science, "Theoretical Virtues and Theorizing in Physics: Against the Instrumentalist View of Simplicity." (2021). It is the basis of his main criticism of my treatment of epistemic values in Chapter 5 of *The Material Theory of Induction.* Mohammadian treats theoretical virtues as aims of science, where I treat epistemic values as means to an end, the better inductive support of theories.

Our views can be largely, if not completely, reconciled by noting that the scope of my analysis is narrower. My work is explicitly devoted to analyzing inductive inference and the bearing of evidence. My chapter addresses these values only in so far as they play a role in this analysis. The adjective "epistemic" in "epistemic values" is a crucial restriction. It indicates their role in the gaining of knowledge (episteme). Precisely when they serve this function, they are not properly called values, but are a means towards the end of gaining knowledge.

Mohammadian's opening discussion suppresses the adjective "epistemic" and announces that he will call the values or virtues at issue "Items." This renaming is not benign. It is an expansion of the values under discussion beyond the narrower scope of epistemic values. An indicator of the expansion is that, in his proposition P6, Mohammadian presents his Items as monadic properties of theories. Epistemic values serve their purpose not through a monadic attachment to a theory, but through their bearing on the relation between evidence and theory. Both accuracy and explanatory power are only well-defined when applied to the relationship between theory and evidence. A theory cannot be accurate or explanatory without specifying what it recovers accurately or explains. These attributions can vary according to the specification. A theory may accurately accommodate one body of evidence but not another. Newtonian theory accurately accommodates the slow motions of bodies, but not fast motions. A wave theory of light explains interference phenomena very well, but explains the photoelectric effect poorly.

The expansion now makes it possible for us to seek theories with certain attributes for reasons other than their epistemic potency. While I did not see such other reasons clearly articulated in Mohammadian's note, they fall readily to hand. We might seek simpler theories for purely pragmatic reasons. We find them easier to understand and apply.

Within the functions allowed by Mohammadian for his Items is this: "Impressive degrees of many Items form the epistemological justification for truth [of theories understood realistically]." When his Items perform this function, they act as epistemic values, or as I would call them, epistemic criteria. They bear on the relation of theory and evidence and they are now a means to the end of establishing the truth of theory.

In short, in so far as Mohammadian's Items function as epistemic values, I do not see that any difference between my view and his is sustainable. In so far as they have other roles, the Items lie outside my analysis and afford no basis for disagreement between us.

Mohammadian presents two further criticisms of my analysis. The first is that the end I attribute to epistemic values—"getting closer to the truth"—is incompatible with an instrumentalist understanding of theories. The criticism depends on using a sense of "truth" other than the one I use. In the twentieth century, philosophers of science developed an allergy to the term. To them, it connoted the mistaken infallibilism of earlier science that contradicted the fallibilism that had overtaken twentieth century philosophy of science. Newton had made great advances in physics, but we were mistaken to think he had found the final and absolute truth of gravity. Call this "big-T Truth."

My use of the term "truth" is otherwise. It is, I presume, unobjectionable when logicians define a deductive inference as one that preserves the truth of its premises. That is just what deductive inferences do. Call this "little-t truth" since there is no pretense of the discovery of the final and absolute Truth. Inductive inferences are those whose premises do not establish their conclusions, but merely lend support to them in differing extents. More fully, what is supported is the truth of conclusions in the little-t sense used by logicians. That is just what inductive inferences do.

We can have small-t truth without big-T truth in inductive inferences. Inductive inferences are fallible. Newton's arguments for his law of gravity and his system of the world are to this day paradigms of inductive rationality. Yet all he had secured was his system's little-t truth. Its big-T truth was not established.

This small-t sense of truth in inductive inference is quite compatible with instrumentalism; and may even be essential for it. According to it, we dispense with Truth as an aim of science and we replace it with the aim of instrumental success or something akin to it. If our instruments are to be successful beyond compatibility with our past records, then we are making a claim that requires inductive support. We may claim, for example, that the simpler generalization is more likely to succeed with future cases. More fully, the claim concerns the small-t truth of a proposition: that the generalization will succeed with future cases. The small-t truth of that proposition is supported inductively by the success of the generalization with past cases; and the support is stronger if the generalization conforms with various criteria: that it be consistent with past cases, that it fit them more accurately, that it be simpler than its competitors and so on.

Finally, Mohammadian disputes my contention that we have considerable freedom in choosing values, as they are normally understood. To make his case, he considers values that most of us would find repugnant. I have no interest in debating these extreme cases when more benign cases are sufficient to establish my point. We each have the freedom to choose among the differing values that support omnivory, vegetarianism or veganism; and again the freedom to choose between whether we value professional success over support for our families, or vice versa.

William Peden, "Explanatory reasoning in the material theory of induction."

William Peden's discussion is most welcome. It may solve or go a long way towards solving an enduring weakness of the analysis of inference to the best explanation in Chapters 8 and 9 of *The Material Theory of Induction*. My congratulations to him for finding a new and fertile way to think about a problem that defeated me.

The popularity of inference to the best explanation in the philosophy of science literature derives from its visceral appeal. When a theory provides a good explanation of some errant facts, the sense that the theory got it right is instant and compelling. My presumption in approaching inference to the best explanation was that I could identify something inductively potent within explanation that supports this success. Then, on the model of the other chapters, I would seek a material basis for it in background facts.

The analysis did not go that way and perhaps it was naïve to expect it would. The notion of explanation itself is sufficiently scattered that our literature cannot agree on which is the right account of it. One account may fit well with one case, but fail with others. And so on for other accounts. It was more than optimistic to think that an idea so fractured might nonetheless capture that one, elusive inductively potent power. In my search for this power, I examined many standard cases of inference to the best explanation in science. The search failed. The commonality I did find is the one that Peden reports. Cases of inference to the best explanation involve a successful theory that accommodates the evidence. Competing foils fail. They are contradicted by the evidence or need further, unsustained hypotheses to preserve their evidential viability.

What results is an account of inference to the best explanation without explanation. It is adequate to all the cases I have investigated. However, it fails to capture the original visceral sense that there is something special afoot to do with explanation. That is the missing piece that I have hoped could be supplied somehow.

Here is where Peden's ingenious proposal turns the tables. What if the basis of the visceral sense simply derives from the simple model proposed? We sense that the favored hypothesis gets it right because it accommodates the facts, while the alternatives fail. That is explanatory enough for inductive purposes and no further notion of explanation is needed. Peden is cautious and does not suggest that this is the entirety of our understanding of explanation. However, he does propose that it captures enough of the applicable notion of explanation that no richer notion of explanation is needed. I find this an appealing resolution of the problem. For it says that my failure to find some further inductive power in explanation is inevitable. There is no such thing to be found.

One of my case studies in Chapter 9 fits especially well with Peden's proposal. In the early twentieth century, Newtonian theory was able to account precisely for the motions of the planet Mercury, aside from an anomalous advance of Mercury's perihelion by 43 seconds of arc per century. In 1915, Einstein found an explanation for the anomalous motion in his general theory of relativity and offered it as an important confirmation of his theory. Einstein's claim fits the model of inference to the best explanation proposed. His theory accommodates the anomalous motion in the sense that it deductively entails it. The astronomer Erwin Finlay-Freundlich, who was a contemporary of Einstein, had already canvassed four possible alternatives and found each to be refuted in various ways by the evidence.

What is missing is some account of how general relativity does not just deductively entail the anomalous motion, but does it in an especially nice way. Lipton characterized this component of inference to the best explanation as "loveliness." In Chapter 9, I propose a simple thought experiment that shows that whatever additional loveliness general relativity may have, that additional loveliness has at best weak inductive powers, if any at all. What if, I imagine, that one of the foils had succeeded empirically? What if, for example, the nineteenth century astronomers had found the planet Vulcan? Its gravitational pull would have been sufficient to account for the anomalous motion of Mercury within existing Newtonian theory. General relativity would not just have been superfluous. It would now have entailed an additional perihelion advance of that same 43 seconds of arc per century beyond the motions observed by the astronomers. Newtonian theory would have accommodated the motion of Mercury exactly and general relativity would have made a falsified prediction.

The evidence would now have spoken in favor of Newtonian theory. No doubt some analysts would have praised Newtonian theory for its explanatory loveliness, finding it in the theory's great simplicity and scope. General relativity would be judged as contradicted by the evidence. Whatever explanatory loveliness general relativity has would not have protected it from the contradiction. Rather it would more likely be criticized for the folly of its superfluous, mathematical excesses.

Elay Shech, "What powers logical inference?"

Elay Shech's most astute analysis cuts to the heart of the material theory of induction. He has delineated a vexing foundational problem that I had dimly recognized, but had avoided examining. The time for that examination has come.

According to the material theory of induction, an inductive inference is warranted by background facts of the pertinent domain; and that warrant is independent of our thoughts and knowledge. If the conclusion of the inductive inference is a truth of that domain, then it seems inevitable that there are background facts in the domain that warrant the inference so assuredly that it becomes a deduction. The threat then is that all good inductive inferences reduce to deductive inferences and the very idea of inductive investigation collapses.

The simplest of these warranting facts is the conclusion itself. Given the background fact of the truth of the conclusion, we trivially deduce the conclusion. Shech provides a less trivial example. He shows in an example that, by successive strengthening logically of the background warranting fact, we eventually arrive at a warranting fact strong enough to reduce the inference to a deduction.

Here is another example that takes the liberty of a conveniently simplified mineralogy. Imagine that our evidence is of small amount of a mineral contaminant that is highly conductive electrically, black and laminar in structure. We seek to identify it. The first background fact is:

(1) Highly electrically conductive minerals are metals or graphite.

If we add to this fact the relative abundance of various metals and graphite among mineral samples, we can warrant inductive inferences of varying strengths to the contaminant being one or other metal or graphite. A second background fact is:

(2) Laminar minerals are graphite, mica and a few other related minerals.

Again, depending on facts about the relative abundance of these minerals, this background fact warrants a corresponding inductive inference about the nature of the contaminant. If we take the conjunction of these two background facts along with facts about the electrical insulating properties of most minerals, we arrive at:

(3) The only highly electrically conductive, laminar mineral is graphite.

This background fact warrants the *deduction* that the sample is graphite.

It seems plausible that this sort of strengthening of the warranting fact can always be secured. If the conclusion is true, it would seem that all we need is a sequence of strengthening background facts that approach the conclusion itself. At some stage in the sequence, the inferences warranted becomes deductive.

Shech considers several solutions. One intriguing possibility that he does *not* endorse is that the facts of the pertinent domain are fundamentally indeterministic. It would then follow that, for at least some cases, short of the conclusion itself, there is no sequence of strengthening background facts that convert the inductive inference into a deductive inference.

My solution to the problem is this. The assertion that some specific inductive inference is warranted by background facts is incomplete. We need to specify which background facts are involved and only then have we specified the inductive inference and its warrant completely. In general, a single pair of premise and conclusion can appear in many inferences, each with different warranting facts. Some will be inductive and some deductive according to the logical strength of the background fact. The cogency of each inference is independent from our thoughts and beliefs and determined solely by the meanings of terms in propositions

This is, in my view, the right way to conceive of inductive inference. The complications arise in the application of the material theory. If we want to say that some figure in history of science was warranted in making some specific inductive inference, for completeness, we do need to specify just which background fact warrants it. If we are to attribute inductive rationality to the figure, there must be some sense that this background fact was accessible to the figure.

Here the situation is similar to that of deductive logic. Thanks to the work of Andrew Wiles (1995), we now know that Fermat's last theorem is deducible from suitably removed mathematical premises, but only with great difficulty. While this shows Fermat was correct in asserting his result around 1637, we do not think that Fermat had a deductive proof of it.

Shech also raises the question of the warrant of deductive inferences. He is right to suggest that the idea of validity in deductive inference faces problems comparable to those faced by inductive inference. They are, however, of a much lesser import and can often be ignored without any real cost. A distinction helps us see the problems for deductive logic. It is between deductive inferences as they appear in ordinary discourse and deductive inferences as they appear within some formal system of deductive logic. My remarks on deductive inference pertain to the first. Whether a deduction is valid is decided by the meaning of the terms in the propositions. The inference from (*A* and *B*) to *A* is valid because of the meaning of "and." As I note in the concluding sections of Chapter 2 of *The Material Theory of Induction*, the meanings of some terms can be vague enough to make determining validity difficult. The most familiar example is the material conditional "if … then …" It captures some of our ordinary uses of "if … then …" but not all; and finding a better specification of the connective has proven to be a major project in formal logic.

When a symbolic system is offered as a deductive logic, if it is to be applied usefully to ordinary discourse, it is, in effect, a formalized proposal for how we should understand the meanings of terms used in inferences in ordinary discourse. The formal rule, universal instantiation, of predicate logic tells us that if we have the formula $(\forall x)f(x)$ then we can write the formula $f(a)$. It is intended to capture the idea in ordinary discourse that, if some property holds of all, then it holds of each individually.

These sorts of applications are unproblematic. Problems arise when these methods try to capture terms in ordinary discourse whose meanings are sufficiently diffuse as to escape characterization in the small number of axioms commonly used in these systems. In my paper mentioned by Shech, "How to Make Possibility Safe for Empiricists", I show how these problems arise for the modal logical S5. The notions of possibility and necessity as really used are much more complicated than the few properties attributed to them in the axioms of S5. For example, the logic proposes a duality of possibility and necessity. Informally:

necessary = not-possibly-not

While the duality holds for some senses of possibility and necessity, it does not hold for all. In the empirical conception of possibility proposed in my paper, possibilities are those supported to any extent by the evidence and necessities are those compelled by it. The compulsion arises when we have extremely strong inductive support. The law of conservation of energy becomes necessary in this conception.

This conception violates the duality of necessity and possibility. For while energy conservation is necessary, a violation of energy conservation is also possible, even if very unlikely. We can have both that energy conservation is necessary and its failure is possible. These problems cascade. The semantics employed in S5 translates into the informal idea of possible world semantics. According to it, necessities are true in all possible worlds and thus can never fail to obtain. In the empirical conception, the necessity of energy conservation obtains overwhelmingly but not always. It can fail. The albeit remote possibility of this failure is enough, however, to undermine possible world semantics.

That a simple formal system like S5 must fail to capture the empirical conception of necessity follows from the material theory of induction. For its notions of necessity and possibility are inductive notions. They are defined by what the evidence supports no matter how weakly and what it compels inductively. Since there are no universal formal rules governing relations of inductive support, it follows that there is no universal formal system governing the empirical conception of possibility.

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