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The Large-Scale Structure of Inductive Inference by John D. Norton
Reply by the Author

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My sincere thanks to the three commentators for their careful reading of my text and their thoughtful commentaries. I am especially grateful that the commentators are generally favorable to the overall project of the work and its claims. In their assessments, each commentator does raise challenging questions that engage well with my text. In the following, I do my best to respond to them, in so far as I have something productive to add.

Agnes Bolinska, "What, Exactly, is the Large-Scale Structure of Inductive Inference?"
The title of Agnes Bolinska's commentary asks *the* most important question that can be put to a volume that has the title "Large-Scale Structure of …" Just what is the large-scale structure of inductive inference, according to my volume? She observes quite correctly that the local and even medium-scale structure is well articulated. The large-scale structure is less clearly delineated. She struggles to find a clear overview in my narrative and weighs several options. The best I can offer is that, on the largest scale, we simply find of a massively tangled network of mutually supporting inductive relations. In the case of a mature science, the structure is quite rigid and self-supporting. If we select any proposition central to such a science, we will find it well supported inductively by the network. That for me is the distinctive characteristic of a mature science.

Bolinska quite rightly objects that this answer falls short of what we might otherwise expect in the specification of a large-scale structure. Here I agree. The volume's title was inspired by the titles of great works in physics and cosmology, such as Stephen Hawking and George Ellis,' *The Large-Scale Structure of Space-time,* and P. James Peebles,' *Large-Scale Structure of the Universe.* These great works can give a simple answer. The large-scale structure of the universe is of a roughly uniform distribution of several forms of matter, expanding uniformly from a singular past. In comparison, all I can offer is that the large-scale structure of inductive inference is a tangled network without any two parts being necessarily the same in their local structure. This answer falls short of the simple answer readers may have expected.

That the large-scale structure just is this tangled network and that not much more can be said in the largest scale really is the point; and it is an important one. Optimists about science like me believe that our mature sciences are very well secured inductively by the evidence of experience. For this reason, we are weary of the inevitable, ill-fated challenges that skeptics mount against such sciences. Because of my interest in Einstein, I am frequently sent such challenges to Einstein's special theory of relativity. Similarly, biologists must weather challenges from creationists who dispute Darwinian evolution. In response to such challenges, we would like to offer a simple and definitive demonstration of the failure of the skeptic's proposal. That proves difficult. We can argue that *this* skeptical proposal fails because it does not agree with *that* part of the established science. But then the skeptic disputes *that* part of the established science; and process continues, tediously, until one side just wears out.

We envy the geometers who can give a concise answer to circle squarers. The large-scale structure of Euclidean geometry is that it consists of all geometrical figures finitely constructible using the provisions of Euclid's five postulates. Since π is a transcendental number, it is demonstrable that no Euclidean construction can square the circle. Biologists defending evolution from creationists cannot supply a comparatively simple proof. They must insist that all the pieces of the pertinent science fit together so well that one cannot just take out one piece and expect the remainder to stand. That insistence is supported piecemeal. Youg earth creationists soon find themselves needing to dispute the physics of the radiometric methods that give us an Earth that is billions of years old; and then further they must dispute the science supporting big bang cosmology that gives us an even older cosmos.

That there is no simple formula for the large-scale structure has been a long-standing difficulty. It means that there is no simple demonstration of the inductive security of our mature sciences. The best we can do is to affirm that some particular result is well secured inductively; and then proceed with the tedious task of showing one by one that the same is true of the remaining results. Here I imagine an enormous architectural structure with vast arrays of arches, vaulted ceilings and the like. It was built by many designers, with no two parts being the same. That the structure stands can only be affirmed by visiting each part and checking its solidity.

There is no single design, implemented over and over, so that once its security is established in one place, then the security of the totality follows.

For such architectural structures and for mature sciences alike, we affirm their security by affirming it for each part individually. That is enough, since the affirmation for each part suffices for the whole.

Molly Kao, Critical Commentary on John Norton's "The Large-scale structure of inductive inference"

In her commentary, Molly Kao raises two issues, both of which merit serious reflection. In the first, she notes that, in the earlier volume on the material theory of induction, the focus was on the logic of inductive inference and not the procedures employed in scientific discovery. The present volume engages with those procedures. To establish self-supporting inductive structures, scientists posit hypotheses, temporarily without support. The material theory of induction insists that none of the commonly discussed rules of inductive inference can be applied universally, as far as the *logic* of inductive support is concerned. Might it be, Kao suggests, that these familiar rules have a role in scientific *discovery*? Might they be used by scientists to arrive at the hypotheses whose role is central in the inductive enterprise?

This is an interesting suggestion. What I have written so far takes no position on it. It seems to me that the matter cannot be decided by abstract reasoning. It concerns what scientists actually do; and that can only be decided by careful historical studies. Here is an interesting project for someone to undertake.

Kao's second remark concerns the distinction between inductive and deductive inference. Inductive inferences are warranted, according to the material theory, by background facts. Kao suggests that something similar arises in deductive inference. Her example makes clear how this comes about. It is based on the conditional proposition:

> If [*antecedent*] light is uniquely an electromagnetic wave,
> then [*consequent*] the energy that an incident beam of light transfers to
> the electrons on a metal surface will vary with its intensity.

If we can affirm the antecedent as a premise, then we can deduce the consequent.

The core of this deductive inference is the conditional proposition. It is not itself a truth of logic, but a contingent proposition whose truth depends on the factual conditions of the pertinent domain. Here I agree with Kao. In this sense, inductive inferences and deductive inferences of this type both depend on background facts in the domain.

How, then, we might ask do inductive and deductive inferences differ? The answer is that they differ the way familiar from our traditional logic texts. The conditional proposition serves to warrant an inference from the antecedent to the consequent, such that if the antecedent is true, then the consequent must also be true. If the inference were inductive, the truth of the consequent would not be assured, but only enhanced.

The value of Kao's observation is that it shows how a proposition can serve in two capacities. It can represent a factual state of affairs; and at the same time serve as a warrant for an inference. This idea is fundamental to the material theory of induction and, as Kao's example shows, it is already a familiar idea in deductive inference.

Raphael Scholl, Comment on John D. Norton, The Large-Scale Structure of Inductive Inference. In his commentary, Scholl raises three issues, each close to the core claims of my text and each bears some reflection. In the first, I am pleased that Scholl finds my account of the role of hypotheses to fit well with an historical episode on which he is an expert: Semmelweis' work on puerperal fever. I am pleased, but not surprised. My text is, as Scholl notes, a work in integrated history and philosophy of science. I have gone to great pains to assure that its claims are instantiated in real cases in history of science. I have found repeatedly that this fit persists when new historical examples are considered.

In the second issue, Scholl expresses discomfort at something that initially also troubled me. A major claim of my work is that we can find arch-like structures in relations of inductive support. We start with some body of evidence and, figuratively, an arch is erected over it as the the base. In one side of the arch, some material fact "*A*" warrants an inductive inference from the evidence to some hypothesis "*B*". In the other side of the arch, these roles are reversed. What was the hypothesis becomes the warranting fact *B* that authorizes an inductive inference from the evidence to the original *A*, now treated as an hypothesis in need of support. In the combined structure, taken as a whole, the evidence inductively supports both *A* and *B*. My text is filled with many examples and, I believe, Scholl is comfortable that they are cogent inductively.

It turns out that there are cases in the history of science in which the component inferences in the arch are not inductive but deductive. From the evidence conjoined with *A* we can *deduce B*; and from evidence conjoined with *B*, we can *deduce A*. However, the combined inference, from the evidence to the logically stronger conjunction *A* and *B*, is inductive.[1]

I will admit that, when I first found these examples, they bothered me. I shared Scholl's discomfort at the idea that purely deductive inferences can dissolves into an induction. However, as I explored more historical case studies, more examples of this structure appeared. There seemed to be no way to discount the individual examples as somehow mistaken or spurious. My text develops several examples. Some come from Newton's celebrated analysis of gravitation. Others are in relations of support among atomic spectra that form the foundation of modern quantum theory. After some reflection, as I report in *Large-Scale Structure…* p.83, the air of paradox was dispelled. If I was comfortable with an arch-like structure whose sides are inductive inferences, then I should be even more comfortable with an arch-like structures in which the less secure inductive inferences in the sides are replaced by more secure deductive inference.

Here it helps to recall that all of these arch structures are in turn components of still further relations of inductive support. Take, for example, a Newtonian case explored in my text. In it, *A* is that planets move in elliptical orbits and *B* is that planets are attracted to the sun by an inverse square law. Each of *A* and *B* also have secured support from elsewhere. *A* is supported by Kepler's careful study of the motions of the planets; and *B* can be recovered from Kepler's harmonic ("third") law.

Scholl compounds his concern by noting that, in the Newtonian example above, the *A* and the *B* are apparently[2] in an if-and-only-if relation of deductive dependence. I take the concern is that such deductive dependence might already be the whole story. It would leave no place for inductive inference. The resolution is that the if-and-only-if biconditional by itself is just a statement of deductive logic. To yield contingent conclusions, we need to add contingent facts in the form of evidence. We can only use the biconditional to infer from elliptical orbits to the inverse square law, if we have some reason to think that planets do move in elliptical orbits.

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[1] To preclude confusion, this is not an instance of hypothetico-deductive confirmation, where the deductions are from the hypotheses to the evidence. In these cases, the deductions are in the reverse direction, from the evidence to the hypotheses.

[2] This if-and-only-if is not quite correct. For example, an inverse square law is also compatible with parabolic and hyperbolic motions.

Astronomical observations of the positions of the planets provide that crucial evidence. They tell us, for example, that the finite repertoire of observed positions of the planets is such that elliptical orbits can be fitted to them near enough. The combined inference from this evidence to the conjunction of *A* and *B* is inductive.

Scholl also draws welcome attention to my disputing of Quine's widely celebrated "web of belief." According to it, the propositions of a science form a web connected by elastic threads to experience. We are free to make arbitrary adjustments in any place since the web just deforms to accommodate it. For anyone who takes a serious interest in history of science, Quine's metaphor is woefully at variance with what actually happens in science. In mature sciences, we cannot arbitrarily adjust things without triggering a catastrophic collapse. I argue (p. 3) that a better picture replaces Quine's elastic threads with inelastic cables. Adjusting one element of the network would break connected cables and trigger a propagating collapse through the whole web. This is what happens when we have scientific revolution.

My task in the volume is to sustain this alternative picture. Quine's picture makes sense if we work with an inadequate hypothetico-deductive account of inductive support. In it, two systems of hypotheses are equally well supported by the evidence, if they both entail it. This freedom, I argue, does not arise if we employ a richer account of inductive support, such as is supplied by the material theory. It accounts for why we do not see the persistence of distinct competing sciences, each equally supported by the same evidence. Sciences are only truly competing if they differ on some factual matter that is accessible to empirical decision. This, I argue and illustrate in Chapter 4, introduces an instability in the competition. If one science does even slightly better over a disputed fact, it gains a factual advantage that, in accord with the material theory of induction, enhances its inductive reach. The process repeats in favor of the stronger science. The resulting instabilities are amplified and the ascending science overwhelms and eliminates the other.

Scholl notes correctly that my example of the tensegrity icosahedron illustrates how a system of mutually supporting relations can arise without having a hierarchical structure. Here, I mention a minor point for completeness. In the written text, I did not notice that the tensegrity structure could also illustrate my inelastic replacement for Quine's elastic web. Scholl is quite correct to observe that it can also serve this end. We need only to add the assumption that the cords in the tensegrity icosahedron are inelastic and inextendible.

In closing remarks, Scholl asks for further assurance that the circularities in the relations of inductive support in a mature science have benign termination. Here I recall what I have written in response to Agnes Bolinska’s commentary above. The assurance can only come from a full exploration of the science. It is impractical, as Scholl notes, for a single person to carry out the check. Fortunately, we do not need one person to do it. We have the collective scrutiny of all scientists. If an unsupported and thus arbitrary element were to remain in a mature science, it would be an opening for some enterprising scientists to advance a new, heterodox science. The professional reward for doing so is immense. Who would not want to be their science’s next Einstein? A mature science remains immune to such assaults over long periods of time. That stability is powerful evidence that the science has no such arbitrary, unsupported elements.