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WE MAXIMIZE THE GROWTH OF KNOWLEDGE WITH ONLY MINIMAL METHODOLOGY?

by

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on

Laurence Briskman's

THREE VIEWS CONCERNING THE UNITY OF SCIENCE

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Can We Maximize the Growth of Knowledge With Only Minimal Methodology?*

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Briskman's Argument: Do different subject-matters require different methods of inquiry? Impossible, says Larry Briskman.[1] Inquiry is needed in order to know how to divide up the world into appropriate subject-matter domains. So the adoption of a methodology must precede and be independent of our location in ontological space.

A similar argument would seem to show that methodology can never be criticized on the basis of experience. For, as Briskman argues, we can never learn from experience that something is amiss with our methods since learning from experience requires them! Thus methodology should depend neither on the growth of scientific knowledge nor the diversification of scientific problems.

Some Observations: The philosophical arguments sketched above sound persuasive, partly perhaps, because they remind us so much of the futility of trying to gather inductive support for inductive methods and the ancient problem of **diallelus**, or what Rescher calls the **Wheel Argument**.[2]

Yet to the naive observer of present-day science it also seems obvious that different disciplines routinely utilize different methodologies. When chemists study the transition of a particular material from the solid to liquid phase, they may make only three or four determinations of the melting point. They do not control for the sex or race of the lab technician, nor the history of the chemical substance.

However, if social scientists study the transition from childhood to puberty in a given culture, they try to use large sample sizes (N = 100) and report the spread of the results in terms of standard deviations. They may go to great lengths to use "naive" subjects and are very sensitive to the possibility of subtle impacts of the personality of the observer.

The reader is undoubtedly squirming with impatience. Surely, you are thinking, we must distinguish Big-M methodological principles from little-m methodological techniques. Of course, chemists rinse glassware three times and psychologists insert lie-detector questions into questionnaries. These little-m subject-matter methods obviously are developed in conjunction with experience, but they can only be judged using big-M philosophical principles and it is only the latter which are claimed to be invariant.

Later I will return to the question of the proper relationship between big-M and little-m methodologies but let me now move on to a different observation.

In my course on History of Methodology we discuss Aristotle's **Organon**, Bacon's **Novum Organon**, and Whewell's

Novum Organon Renovatum and I blithely tell my students that each successive theory of methodology corrects and improves on its predecessors. (Popper, of course, surpasses them all, but this discussion takes place in a History class.)

There is clearly Big-M progress which in turn influences science. But what fuels the changes in Big-M? Is it only philosophical criticism, or might the results of scientific practice have something to do with it? For example, is there not a very straight-forward sense in which the discoveries of non-Euclidean geometry and non-Newtonian physics refute Kant's methodological prescriptions?[3]

And what are we to make of innovation on what we might call the middle-sized M level? I am thinking of such things as Galileo's discovery of how to combine the use of mathematical idealizations with empiricism.[4] Or Sennert's discovery that causes are not always commensurate with effects (which would have defused Kant's **a priori** derivation of Newton's Laws).[5]

At this point the purist may reply that the extent to which scientific practice **is** relevant to the appraisal of a methodology is a direct measure of the extent to which the "methodology" is non-minimal and hence illegitimate. But I wish to argue for another conclusion, namely that it doesn't matter if methodologies contain presuppositions about the world as long as it is possible to criticize those presuppositions. And sometimes the best form of criticism uses experience gathered by means of the very methodology in question!

But isn't that what Briskman has shown is impossible? Let us see.

A Scientific Analogue: I will now digress somewhat to refute a similar claim which is often made about science, **viz.** that if a scientific theory is somehow used in the process of data collection, then that data could never count against the theory.

Here is a simple example of what I call the coherence method of testing, i.e., a case where it is necessary to use a theory in order to test it. Consider the following claim:

> C: "If any two objects are placed on the pans of a simple balance, the pan with the heavier object goes down."

This would be easy to test if we had an independent method of detecting heaviness. Suppose we don't, however, and use the following operational criterion:

> O: "A is heavier than B just if A goes down when placed on a balance opposite B."

At first, it certainly looks like adding **O** to **C** makes **C** true by definition and hence untestable. However, surprisingly this is not the case if we assume that heaviness is an intrinsic property of objects which does not change according to the situation. For example, the following research report, which uses **O**, would falsify

C:

Observation 1: X is heavier than Y. (Determined using O.)

Observation 2: Y is heavier than Z. (Determined using O.)

Prediction (using C): If X is placed opposite Z, X will go down.

Observation 3: Z goes down when placed opposite X! (So C is refuted.)

Since we believe so strongly that C is true, it may be hard to imagine the above series of observations so let me introduce a parallel example where the central claim is false:

C': "If any two people are in a relationship, the one who is more loveable gets the bigger valentine."

Let us operationalize "loveable" as follows:

O': "A is more loveable than B just if B sends A a bigger valentine than A sends B."

It should be easy to visualize the following refuting situation:

Observation 1: X is more loveable than Y (by the O' test).

Observation 2: Y is more loveable than Z (by the O` test)

Prediction (from C'): If X and Z are in a relationship, Z will send X a bigger valentine.

Observation 3: Z gets the bigger valentine!

This string of observations clearly refutes C' (as interpreted by O').

<u>How Experience Could Undermine Counter-Induction:</u>

Salmon argues (in a Briskman fashion) that there can be no inductive support for induction because (here I am being very brief) any evidence gathered which seems to favor an inductive rule would simultaneously support a counter-inductive rule.[6]

I will criticize this argument by showing how experience will cause even a stupid counter-inductivist to change his methodology.

I follow Salmon's formulation of the counter-inductive rule:

R: To argue from "Most instances of A's examined in a wide variety of conditions have **not** been B" to (probably) "The next A to be encountered will be B."

R obviously sanctions the following inference:

"In most instances of the use of R in arguments with true premises examined in a wide variety of conditions R has been **un**successful.

Hence (probably):

In the next instance to be encountered of the use of R in an argument with a true premise, R will be successful."7

Let us suppose our counter-inductivist is able to observe only a small bit of the world (say whether a flash of green light is followed by a red or green signal) and decides to "test" R through experience. He makes observations, uses R in order to make predictions, and keeps records of his successes and failures. To insure his sincerety we also ask him to bet jelly beans on his predictions. (The counter-inductivist I have in mind loves jelly beans.)

I will present two scenarios, one in which the future is simply related to the past and one in which the order is more complicated.

Case I. What happens to our counter-inductivist in a very simply-ordered world?

1. He observes green.
2. Using R he predicts red will follow and bets one jelly bean.
3. Green in fact follows and he loses one jelly bean.
4. He records "R failed when green flashed."
5. He once again observes a green light.
6. He is about to bet on red—using the same reasoning as in #2—when he suddenly realizes that he has more information, namely the fact recorded above in step #4. He now reasons on the meta-level: "Since R failed before in a green flash situation I now see (using R) that it should be successful this time." This gives a second reason for predicting red so he bets two jelly beans.
7. Green in fact follows and he loses two beans.

As we can see, our counter-inductivist is in a steep downward spiral.

Case II. What happens to our counter-inducvitist in a more complicated world?

1. He observes green.
2. Using R he predicts red.
3. Now red in fact follows and he wins one jelly bean.
4. He records "R succeeds when green light flashing."
5. He again observes green.
6. As before at first he wants to bet on red. However, analysis on the meta-level convinces him that his success last time makes it likely that R will fail this time.

   Therefore, since he wants to succeed (he is only counter-inductive, not masochistic), he bets on green.
7. Red occurs and he now loses a bean.

Once again he is in a downward spiral. A second failure at the meta-level will make him even more confident that R will be successful the **next** time at the object level, i.e., in green flashing light situations. So he bets more and more jelly beans**--and loses them.**

My conclusion is the following: The counter-inductive method does not break down on paper. However, it does **not** deliver the beans.

If our counter-inductivist is hungry he will soon respond to this fact. Notice that reasoning about the notebook record of prediction successes and failures alone will never cause us to abandon counter-induction. But the pragmatic aspects of prediction successes or failures will. This may happen on the purely biological level--as our counter-inductivist weakens from hunger he may become too dizzy to apply R and start responding in a Skinnerian fashion.

I leave it to the reader to check the following claims:

1. An inductivist will fare well in either of the "worlds" described above.
2. If the relationship between the green and red light generators is random, induction and counter-induction both fail to provide guidance.

I conclude that experience could force a counter-inductivist to change his methodology.

The Minimalist Reply: The minimalist will surely reply that in order to evaluate their methodological rules both the inductivist and counterinductivist are actually using the same ultimate methodological principle which goes some thing like this:

U: If a method doesn't deliver the beans, try something else before you starve to death.

I agree that both people and pigeons act in accordance with some such Ur-methodology. But do we wish to say that U exhausts the logic of scientific research?

There are times when Popper waxes eloquently on the similarities between the methodology of an amoeba and an Einstein. But at the risk of being a species elitist, I must insist on the differences. After all, if people were born with a full command of the intricacies of scientific method, all of our sophisticated analyses of what is wrong with pseudo-science and exhortations against operationalism, naive inductivism, and justificationism would surely be quite superfluous.

Methodological Bootstrapping: I believe there are a continuum of methodological principles, ranging from U through Big-M, middle-M, and little-m rules. They all relate directly or indirectly to the aim of obtaining knowledge about the world but they vary enormously with respect to their domain of applicability, the specificity of their prescriptions, and the strength of their presuppositions about the world.

I believe these methods develop over time as scientists encounter new methodological problems. I see no reason why different subject-matters may not present different methodological problems--if the entities under study are propensities, we may have to learn how to interpret and test statistical laws. If our domain of inquiry consists of extremely complicated causal networks, we may have to develop path-analysis techniques. If the systems we study are to a good approximation closed like the solar system, we can use much simpler methods of analysis than if our systems are radically open as are the minds of (some) sophomores in psychology classes. One should beware of dis-unity of methods, but not be surprised if no strong unity emerges.

I suppose that it is true that in some sense all of 20th century logic rests on the principle of non-contradiction. And there's a sense in which all of 20th century methodology depends on the simple method of trial and error. But minimal logic or minimal methodology is not the whole story--and certainly not the apsect which is most directly helpful to practicing scientists. Of course, our bolder methodologies are fallible. But if we follow Quine on logic and Bartley on rationality, even our minimalist preconceptions might some day require revision.

Notes

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1Larry Briskman, "Three Views Concerning the Unity of Science," presented at the Twelfth International Conference on the Unity of Sciences, November 24-27, 1983, Chicago, IL.

2Nicholas Rescher, Methodological Pragmatism. Oxford: Basil Blackwell, 1977.

Rescher quotes Montaigne's version of the argument: "To adjudicate [between the true and false] among the appearances of things we need to have a distinguishing method (**un instrument judicatoire**); to validate this method we need to have a justifying argument; but to validate this justifying argument we need the very method at issue. And there we are, going around on the wheel." (p.17)

3Imanuel Kant, Metaphysical Foundations of Natural Science, 1786. In the Preface, Kant points out that in chemistry, unlike physics, the generalizations are merely empirical and "admit to no presentation **a priori** in intuition". Thus one might say that post-Newtonian developments only show that Kant's synthetic **a priori** methodology does not apply there--physics is more like chemistry than Kant realized. However, Kant says that all mathematical science has a "pure part" which is based upon an **a priori** cognition.

[4]Noretta Koertge, "Galileo and the Problem of Accidents," Journal of the History of Ideas, 38 (1977), 389-408.

[5]For a discussion of a more elaborate attempt to relate the symmetries of causes and effects see A. F. Chalmers, "Currie's Principle," British Journal for the Philosophy of Science, 21 (1970), 133-48.

[6]Wesley C. Salmon, The Foundations of Scientific Inference. Pittsburgh Press, 1966, pp.12-17.

[7]Wesley C. Salmon, "Should We Attempt to Justify Induction?" Philosophical Studies, 8 (1957), pp.45-47.