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A PLURALISTIC VIEW OF FOUNDATIONS OF SCIENCE

Before writing this I read Bas van Fraassen's statement. I find little I can disagree with in what he says. Consequently, what I would like to do is address several clusters of concepts and issues that are often somewhat neglected in discussions of the philosophical foundations of science.

I. Epistemology of Experiments

Bas rightly emphasizes the importance of going beyond models of theories in analyzing the content of science. What I want to stress is how far the whole activity of experimentation, including its reporting, is from the standard theoretical model of science, which Bas and I substantially agree about.

The first point is that experimental reports, much more than theoretical papers, are not generally understandable. They are like detailed sports reporting, intelligible only to the initiated. There is, of course, the insistence of journals that reports be as brief as possible, and certainly there is no requirement they be generally intelligible. However, I think there is a deeper epistemological reason, which also applies to sports reporting. It is not possible to describe in ordinary language, even augmented by some technical terms, the backhand stroke of a tennis player with any accuracy. Similarly, it is not possible to describe in language the many activities of an experimentalist. This applies to his actions setting up and running experiments, but also to his perceptions in digesting the results in their first "raw" form.The written account can only hint at the main features of what is done or what is observed.

The central epistemological point of these remarks is that this radical incompleteness of descriptions of experiments is not a mark of bad science, but is an essential, unremovable feature of almost all science. The drastic descriptive limitation of what we have to say about experiments is, in my view, a fundamental limitation of our scientific knowledge, possible or actual.

Moreover, this radical incompleteness of the experimental reporting leads to more appeals to authority in experimental work than in theoretical work. It is common to hear, in every part of experimental science: "Well, we know those results are right and can be trusted because we know X and his lab." Of course, there is the answer that experiments can be repeated by others, and this is the great empirical check against being overwhelmed by authority. But it is still an important point, which can be easily amplified by various historical examples, that in many respects theoretical results can be evaluated for error much easier and more directly than experimental work. To bring this up to current science, computational experiments and simulations need to be included as well.

Part of the epistemology of experiments recognized by everyone is the presence of experimental error, but the theory of error has not crept into the philosophical foundations of science, but remains on the ground floor of actual experimental work, with only an occasional philosophical nod to its importance. Yet error is a central concept of a proper general epistemology, and, on the other hand, has a long technical history of theoretical development, at least since the early work of Simpson in the 18th century. Its conceptual place in science, however, is, in my view, still not fully recognized. Let me give just one personal example. I have spent many years working on the foundations of measurement, and I recently went to a gathering of the measurement theorists' "clan" in Kiel, Germany. It was generally agreed that a really proper inclusion and analysis of error in foundational theories of measurement is the number one general problem.

Finally, I emphasize, as I have before, the hierarchy of models used in the analysis of experimental data. Modern statistics has developed within the set-theoretical view of mathematics, as can easily be seen by perusing the pages of *The Annals of Statistics* and other leading journals. The many levels of data reduction usually needed to get to detailed statistical analysis is an epistemological problem, not a statistical problem as such, and needs more philosophical analysis, with close attention to the varying practices in different parts of science. Skepticism about explicit objectivity holding "all the way down" is certainly one of the reasons for the recent increase in acceptance of a Bayesian viewpoint in statistics, although it was already very explicit at many points in Laplace's writing. The arguments about Bayesian ideas, both by philosophers and statisticians, are highly relevant to the epistemology of experiments.

II. Theoretical Physics is not Primitive Mathematics

I continue to endorse the set-theoretical view of theories as an important foundational viewpoint, but I think there is much about theoretical science that is left out of this view. I especially have in mind the detailed comparison of the methods of theoretical physics and the methods of pure mathematics. The latter come close to satisfying the set-theoretical view of theories. The former do not. It is possible to take the view that it is the job of philosophers of physics to fill in the set-theoretical foundations of physics. (Certainly it is the kind of enterprise I have engaged in myself in the past, and it is not my point here to denigrate it.) What I want to emphasize here is that it is also philosophically important to understand the difference in the way mathematical methods are used in theoretical physics and in pure mathematics, or for that matter, standard applied mathematics – from the standpoint of the discussion here I make no real distinction between the practices of pure and applied mathematics.

Even the most superficial empirical examination of *Physics Review Letters*, the journal that perhaps more than any other publishes important new physics results, both experimental and theoretical, will confirm what I have to say, or at least so I claim. The articles, strictly limited as to length, do not read at all like articles in mathematics journals. Mathematics is used in an informal way, but that phrase is too vague. The real characterization, I think, is that theoretical physics is concerned with problem solving, in strong contrast to theorem proving. The essential skill is knowing just what assumptions or empirical facts to draw upon. Formal axioms seldom if ever play a role.

I should say at once that mathematical physics, as opposed to theoretical physics, does use axioms. Look at the heroic efforts to provide an axiomatic foundation for quantum field theory. Essentially all the sustained efforts to provide such an analysis were made by mathematicians, not theoretical physicists.

Perhaps my favorite exhibits for my thesis about the difference are the papers of the two great magicians of 20th century physics, Dirac and Feynman. Any fundamental paper by either one of them is far from being mathematically complete, either in terms of assumptions or argument. But these are only wonderful examples. I think the difference holds for almost any of the first theoretical papers in any part of modern physics. (I say "almost any" although I believe "all", because I have not looked at a wide enough range of candidates.)

The saliency of the methodological differences between theoretical physics and mathematics is even more evident in physical papers and treatises further removed from what we all tend to regard as fundamental theory. For example, papers and books on quantum optics or solid state physics are full of mathematical equations and mathematical calculations. I say "calculations" deliberately, for it roughly characterizes the restricted kind of mathematical arguments used. One can look from one physical paper or book to the next and no trace of even an elementary reductio ad absurdum argument or of a pure exisence proof will be found. (Undoubtedly isolated counter examples can be found to what I am saying, but they will be very isolated, i.e., of very low frequency.)

So the three main characteristics of the use of mathematics in theoretical physics are these. First, they are highly constructive, mainly calculational in character – the calculus is not called the calculus by some inadvertent mistake. Second, they are fragmentary, a calculation, then a physical principle or argument, then another calculation, all weaved together into something physically plausible but far from being an exemplar of purely mathematical argument. Third, the broad organization of the argument fits a problem-solving not theorem-proving style. It is not that theoretical physicists forget to prove the theorems, or don't know how to, it is that they are doing something different, something that is in its own way just as wonderful and impressive as sustained proofs in mathematics.

It is a problem for the philosophy of science to understand the detailed nature of the methods of theoretical physics better than we now do. What I have sketched is only a superficial beginning.

III. Ancient Antinomies and Modern Invariances

Because of his view of the nature of the heavens, Aristotle famously held that the world is eternal. Almost a thousand years later the Christian philosopher Philoponus launched a major attack against Aristotle on the eternity of the world. Although Christians needed to believe in the creation, not the eternity, of the world, Aquinas wisely concluded in Question 46, Part I of the *Summa Theologica* that it must be an article of faith, not demonstration, that the world had a beginning. This long and passionate debate, which I am only sketching, formed the subject of Kant's First Antinomy in the *Critique of Pure Reason*. A modern view, so I would argue, is that a decisive demonstration of the finiteness or infinitude of space and time is out of reach. It is unlikely we can ever prove the Big Bang was the absolute beginning; we certainly seem to have no clue as to how such a proof would be constructed. Note also that we even have a revivial by Hoyle and others of the view that the universe may have been through endless cycles of creation, and the Big Bang was only the beginning of the current cycle.

Ancient arguments between Aristotelians and atomists about the continuity or discreteness of matter are well-known, and were the focus of Kant's Second Antinomy. From a mathematical standpoint continuity and discreteness are radically different, and we ordinarily think of a particular feature of a mathematical model of a theory as exemplifying one or the other. On the other hand, it is difficult to conceive of an experiment that would distinguish between a physical quantity being continuous or discrete if a sufficiently small grid of discreteness is permitted. Experimental proof could only go one way: in some cases there is discreteness of a given, established coarseness. So we expect indistinguishability at a very fine level of discreteness between it and continuity. An appropriate invariance should hold, in spite of the central role of continuity in classical mathematical analysis and many other parts of mathematics. The role of continuity, so I would argue, is not as an inaccessible ontological principle, but as a powerful assumption to support the calculations, symbolic and numerical, of mathematical physics that require continuity. They work, because they are such fine approximations. If there is discreteness, it is not too coarse. Here we can take the view that a discrete and a continuous model, one each of a given set of phenomena, must satisfy theories that are mutually contradictory when restricted to the same domains of interpretation.

This same kind of contradiction arises in connection with Kant's Third Antinomy, that between deterministic and indeterministic theories, the latter having a component of absolute spontaneity, in Kant's insightful terminology. Here we have modern support of the same kind mentioned in connection with the Second Antinomy: two kinds of theories, but indistinguishable from an observational standpoint, no matter how many observations are taken, if the accuracy of the quantitative observations is bounded, i.e., not infinitely precise. Perhaps the simplest example is the ergodic one of Sinai billiards. We have a single billiard ball moving without loss of energy and satisfying the law of reflection, but with a convex reflecting obstacle in the middle of the table. The motion of the ball is ergodic. If we finitely partition the space of the table on which the ball can move and correspondingly make time discrete, starting with the simple Newtonian model, then this discrete model cannot be distinguished by observation at the discreteness level of the elements of the partition from a finite- state discrete Markov process. (The result can also be formulated in terms of flows for continuous time.)

So what I am arguing for are new invariances in old bottles, to show how enduring these philosophical and conceptual antinomies are, ready for new ideas and new interpretation on issues central to the natural sciences.

References

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