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# BOOK REVIEWS

## Trout, J. D. (2016), *Wondrous Truths: The Improbable Triumph of Modern Science*, New York: Oxford University Press, 264pp, ISBN 978-0199385072.

Modern science, one of the greatest, if not the greatest, achievements of humanity, emerged in Europe in the 17th century. Why just there, at that particular time, when there were plenty of other times and places when and where it might have arisen? J. D. Trout's answer is that it was sheer luck. Robert Boyle and Isaac Newton just happened, by sheer luck, to hit upon an explanatory theory that just happened to be close enough to the truth to make subsequent science successful. This explanatory theory was their version of the corpuscular hypothesis. Subsequent science made progress by means of the inductive procedure of *inference to the best explanation*. An explanation is the "accurate description of causal factors" that bring about an effect (p. 8). But this inductive procedure only delivers success if based on background or prior theories that are at least approximately correct. Scientific method cannot explain the birth of modern science. There were many other times and places besides Europe in the 17th century when and where the experimental method of science was put into practice, but rapid progress in scientific knowledge did not result. It happened in the time of Boyle and Newton because they had the good fortune to hit upon an explanatory "hunch" that was close enough to the truth.

This account of the birth and nature of modern science is by far the most interesting and important part of *Wondrous Truths*. It is however confined to the final three chapters of the book—apart from preliminary hints at the beginning. The first half of the book is taken up with issues that have to do with scientific explanation and understanding.

Chapter Two argues that scientists tend to accept explanatory theories that seem to enhance their understanding of the phenomena in question, even though understanding is an unreliable guide to truth. It goes on to consider psychological, social and educational factors involved in influencing scientists in their judgements concerning comprehensibility. Feelings of pleasure, fluency, familiarity, the perceived simplicity and beauty of a theory, tend to be associated with understanding, and the acceptance of explanatory theories. Explanations that unify may provoke an "'oceanic feeling', a sensation of […] being connected with the most fundamental structures of the external world" (p. 32). Even though the feeling of understanding is not in itself a reliable guide to truth, nevertheless it may in scientific practice be some kind of guide, as scientists are trained to accept, comprehend and appreciate the explanatory patterns of empirically successful, already accepted theories.

Chapter Three further elaborates the point that feelings of insight and understanding are not, in themselves, reliable guides to truth. This chapter considers some of the neurological processes that underpin the desire to understand.

Chapter 4 sets out to expound and defend inference to the best explanation. Science, we are told, is based on induction, and all inductive inferences are explanatory in character. Such inferences are fallible, but not circular. If inferences to the best explanation draw upon background theories that are near enough correct, there is a chance the inferences will meet with success. If, on the other hand, all these background theories are bad, there will be little chance of success. Everything depends on how good or bad prior theories are. Method alone cannot explain the success of science. There is a sense in which an empirically successful theory is itself explained by the phenomena it explains. "Evolution explains the diversity of species, and the diversity of species also explains evolution" (pp. 92–93). It seems circular but, we are assured, it is not. The charge of circularity arises also in connection with arguments for scientific realism which, critics hold, presuppose just that which is at issue. But here, too, the charge of circularity is misconceived.

What is to be made of the account of the birth and nature of modern science, scientific method, and scientific explanation, with which I began? There is, in my view, one central nugget of truth in it—but it is surrounded by claims that seem to me to be at best seriously incomplete, at worst misleading or downright false. The nugget of truth is that acceptance of a metaphysical view of nature played a crucial role in the birth of modern science. Everything else is problematic. Let me take components of the above story one by one.

For Trout, the key step that made modern science possible is the one taken by Sennert, Boyle and Newton in moving from alchemy to the corpuscular hypothesis and modern physics and chemistry. This strikes me as a very restricted view of the intellectual developments associated with the birth of modern science. As important, indeed even more important, is the work to be associated with Copernicus, Kepler and Galileo, leading up to Newton's *Principia*—the three laws of motion, the law of gravitation, and the successful prediction, by their means, of the motions of the planets, comets, tides, and other phenomena. There is scarcely a mention of this all-important sequence of discoveries, surely of central significance in the emergence of modern science. It is doubtful that Newton's speculations about corpuscles, set out in the main in his *Opticks*, would have had the influence that they did have were it not for the impact of the astonishing achievements of the *Principia*.

For Trout, all-important for the birth of modern science is the transition from the metaphysics of alchemy to that of Newton's version of the corpuscular hypothesis. Again, even more important, I would have thought, is the transition from Aristotelianism to Galileo's view that the book of nature is written in the language of mathematics—the metaphysical thesis that the universe is such that both terrestrial and astronomical motion is governed by laws capable of being formulated in simple mathematical terms. This key metaphysical revolution, from Aristotelianism to the idea that simple mathematical laws govern natural phenomena, is ignored by Trout, even though it plays, if anything, an even more important role in the birth of modern science than the transition from alchemy to the corpuscular hypothesis.

All this is by way of preliminary critical remarks. I have not yet touched on what seem to me to be the really serious inadequacies in the story Trout tells us about the birth of modern science. These concern serious inadequacies in what he has to say about explanation, inference to the best explanation, and the role of scientific method in the scientific revolution. As we shall see, these topics are all inter-connected.

A good explanation, we are repeatedly told, is "an accurate description of the underlying causes that bring about an effect" (see pp. 4–8, 15, 17–18, 115, 140, 204, 213). This "ontic" characterization of explanation is intended to be entirely general, but it would seem not to cover purposive explanations—(which explain by depicting actions as being conducive to the achievement of a goal in a given environment). In order to include both causal and purposive explanations we would have to say something like "an explanation renders intelligible a range of apparently diverse phenomena by revealing that there is something inherent in all these phenomena that, in some sense, determines or is responsible for what occurs".

Even if we ignore this defect in Trout's account, there is another difficulty. A good explanation is accurate, but most scientific explanations are only approximately true, as Trout recognizes, and almost all accepted theories in physics are, at best, only approximately true, again as Trout recognizes (p. 110). An approximately true theory is, strictly speaking, false. Trout's account of explanation needs a way of distinguishing a theory that, though false, is "near enough" true to provide good explanations from one that is not. At least, we require a way of explicating what it means to say of two false theories that one is "closer to the truth" than the other. This is a well-known open problem in the philosophy of science: the problem of verisimilitude. There is not a whisper of any of this in Trout's book.

But putting that difficulty on one side, it turns out that Trout's account of explanation is not really an account of explanation at all—as Trout himself acknowledges on page 131 and in note 29 on page 215. It is rather an account of what constitutes a *good* explanation. A good one is one that is, near enough, true; a bad one is one that is false. This tells us nothing about what it is for a theory to be explanatory, whether true or false. It tells us nothing about how we are to grade theories in terms of their explanatory character, irrespective of their truth or falsity. But it is this latter notion, about which Trout says nothing, that we require for the methodological rule: inference to the best explanation. Trout's notion of explanation, if inserted into this rule, reduces it to: inference to the (nearly) true description. *Inference to the best explanation* is potentially helpful because, as long as we have a good account of what it is for a theory to be explanatory, we can determine, by inspection, which of two or more theories is the best explanatory one—and if explanatoriness indicates truth, we have a procedure that enables us to arrive, fallibly, at truth. *Inference to the (nearly) true description* is, by contrast, entirely unhelpful. Confronted by a number of rival (self-consistent) descriptions we cannot tell, by mere inspection of them, which is true.

I come now to the really serious failing of Trout's book—its discussion of *inference to the best explanation*. Trout is right about one matter: theoretical physics, in particular, only ever accepts *explanatory* theories, and explanatory, here, means *unifying*. Newtonian theory, Maxwell's theory of the electromagnetic field, Einstein's theories of special and general relativity, quantum theory, quantum electrodynamics, quantum electroweak theory, quantum chromodynamics, the standard model and, potentially, string theory: all are great achievements of *unification* of previous disparate theories and laws, and phenomena. It is important to note that, given any one of these theories, endlessly many *disunified* rivals can easily be concocted to fit the phenomena even better than the accepted unified theory. What Trout entirely overlooks is that, in persistently accepting unified theories only in physics *against the evidence* in this way, physics thereby makes a substantial, highly problematic, implicit metaphysical assumption about the nature of the universe: it is such that all disunified theories are false. The universe is such that some kind of unified pattern of physical law runs through all phenomena. This profoundly problematic metaphysical thesis is a presupposition of that component of scientific method that asserts: only accept theories that are sufficiently *unified*, that is *explanatory*.

There is a further crucial point. This substantial metaphysical conjecture about the nature of the universe, made by science at any stage in its development, is almost bound to be more or less false, and thus in need of revision and improvement. Scientific method, properly conceived, is designed to help us critically assess and improve metaphysical conjectures as we proceed, in the light of which seem to be the most fruitful for science, and other factors. Sustained imaginative and critical exploration of the metaphysical presuppositions of science, undertaken in an attempt to improve them, constitutes an important part of scientific research. It is just this activity that the natural philosophers, associated with the birth of science, engaged in: Newton, Boyle, Galileo, Kepler, Descartes, Leibniz, Huygens, and others.[1] And as a result of this activity, the *metaphysics*, and the associated *methods*, of science evolve with evolving scientific knowledge. There is something like positive feedback between evolving metaphysics and methods, and evolving scientific knowledge, a feature of scientific method which helps explain the explosive growth of scientific knowledge.

Trout repeatedly asserts that scientific method has nothing to do with the birth of modern science (see, for example, pp. 9–10, 77–78, 118–119, 169, 180, 182, 184–185, 187–191). But he is wrong. Just that which he holds to be the key factor in leading to modern science—Newton's version of the metaphysical thesis of the corpuscular hypothesis—is intimately associated with scientific method in the form active in Newton's time. Furthermore, scientific method, properly conceived, can be discerned as being implicit in the kind

[1] Trout ignores throughout that those who created modern science, Galileo, Newton and the others, were not scientists, but natural philosophers for whom metaphysics, philosophy, methodology, even epistemology, were vital aspects of the enterprise.

of considerations that led Kepler, Galileo, Newton and others to develop the metaphysical conjectures that they adopted. Luck was involved too, of course. It always is in scientific discovery, as Trout stresses. But that does not mean that scientific method had no role to play. Trout holds that scientific method has no role to play in the birth of science because he takes for granted an untenable conception of scientific method, which dissociates the experimental methods of science from *metaphysics*, and the critical appraisal of metaphysical ideas in terms of their fruitfulness for science (and other factors). What made the birth of modern science possible was the adoption and implementation in practice of scientific method which brought together empirical observation and experimentation on the one hand, and a metaphysical view of nature which holds, roughly, that phenomena are governed by laws capable of being formulated in simple, precise mathematical terms.

But am I here being a bit unfair to Trout? Is the meta-methodological view I have indicated, which links together the *metaphysics* and *methods* of science, and specifies how metaphysics and methods evolve with evolving knowledge, sufficiently well known for it to be reasonable to hold that Trout should have known of this view? Trout puts forward one small element of this view, namely that metaphysics played an important role in the birth of modern science: is it fair to criticize him for not knowing that this has already been put forward as a part of a broader view that solves key problems his book fails to solve (such as problems of verisimilitude, explanation and induction)?

I must now make a confession. I am responsible for the view I am alluding to: *aim-oriented empiricism*. It was first published 44 years ago, in the leading journal in the field, *Philosophy of Science*.[2] Over the years, the view has been further developed in publication after publication, a crucial book even having the same publisher as Trout's book.[3] I leave it to the reader to decide whether Trout ought to have known of this earlier work.

I might add, however, that it is not just my previous, highly relevant work that Trout ignores. He also ignores work in the history of science that tackles the very problem that concerns him: why did modern science start when it did?

[2] Maxwell, N. (1974), 'The Rationality of Scientific Discovery, Part I: The Traditional Rationality Problem,' *Philosophy of Science*, vol. 41, pp. 123–153.

[3] Between 1974 and 2014, six books, four contributions to books, and 14 papers in academic journals: see http://discovery.ucl.ac.uk/view/people/ANMAX22.date.html. My *The Comprehensibility of the Universe* (1998, Oxford University Press), even puts forward an account of explanation as unification which is just what Trout's story requires without suffering from the defects of his own "ontic" account of explanation.

It is perhaps unfair to criticize Trout for ignoring David Wootton's brilliant *The Invention of Science*, which gives an account starkly at odds with Trout's: this book was published in 2015, one year before *Wondrous Truths*. The same cannot be said of H. Floris Cohen's excellent *How Modern Science Came into the World* (2010). And there are other works, highly relevant to Trout's theme, that receive no mention: for example, A. E. Burtt's *The Metaphysical Foundations of Modern Science* (1932); R. S. Westfall's *The Construction of Modern Science* (1977). (Koyre's *Newtonian Studies* does, however, get a mention.)

What, finally, of Trout's central contention: modern science began as a lucky accident? Almost everyone recognizes that luck plays a crucial role in scientific discovery. Scientific method is, for most, concerned with the empirical *assessment* of hypotheses; most hold that there is no method of discovery in science—all sorts of factors, coincidences, chance meetings, dreams and lucky guesses playing a role. Even those who hold, as I do, that there is a rational method of discovery in science, nevertheless hold that this method is non-mechanical and fallible: it may help, but it does not dictate. Successful discovery requires good luck.

What we require, for Trout's contention to be of interest, is that in many other times and places besides Europe in the 17th century, all that which made the birth of science possible then were also present at all these other times, and in all these other places. We require a combination of factors: intellectual factors, such as the adoption of scientific method in its appropriate form, observation and experimentation tied to Galileo's conception of the universe; and social and technological factors, such as the printing press (to disseminate results), and discovery being given great social prestige and status. Trout provides a list of many times and places where, he maintains, science might have begun if natural philosophers had been more lucky (pp. 188–189). The crucial synthesis of empiricism and appropriate metaphysics was, however, lacking. Trout and I agree. For Trout, it is just back luck. For me, there is bad luck, yes, but on top of that there is the crucial lack of scientific method in its required form—the form it took with Kepler and, above all, with Galileo.

It may be objected that I am unfair to Trout in that, in his 'Preface' he says his book is written "for a popular audience". But even a book for a popular audience should attempt to do intellectual justice to problems and themes it tackles. This is what Galileo and Darwin did. And interpreted as a popular work on the role of explanation and metaphysics in the birth of modern science, far too much space (two chapters) is taken up belabouring the obvious, namely that it is all too easy for us to think we understand when we don't, and to think something is true when it isn't.

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