Science, progress and democracy: Critical Appraisal of J.D. Trout, *Wondrous Truths*, Oxford University Press, 2016.

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J.D. Trout's *Wondrous Truths* is a great read and a really enjoyable book. Trout provides an empirically informed answer to the question "how does science progress?" or "why is science successful?" His account draws on psychology, cognitive science and neuroscience as well as on philosophy and history of science. Trout is a naturalistic philosopher – one who believes that philosophy should be informed by, and consistent with, the findings of the sciences – and he is a physicalist. Both these perspectives inform his approach. He is also a scientific realist of a particular stripe and holds an ontic view of scientific explanation. These latter two views are also crucial in shaping his approach to explaining the success and persistence of science.

I am fully on board with Trout's naturalism and physicalism but do not have time to discuss these in full here. Unlike Trout, I am not a scientific realist but I am more closely aligned with Arthur Fine's "Natural Ontological Attitude" than with Van Fraassen's constructive empiricist anti-realism. My outlook on science is also influenced by Nancy Cartwright, John Dupré and Bill Wimsatt who each add different bits of nuance to the realism debates in philosophy of science. Further, unlike Trout, I don't strongly back one horse among the alternate theories of explanation in philosophy of science, which I think makes me an explanatory pluralist. So, I think that Trout's favored ontic view of explanation can have a role

to play in accounting for some explanatory work in science. In what follows, I first briefly outline each chapter in Trout's book and characterize the central theses of the book. In the discussion that follows will focus on the following: the scope of Trout's account and the question of what are the mature sciences; the experimental method and the success of science as a historiographical critical target; and scientific realism and the various philosophies of science.

Trout says that "The central idea of this book is that science in selected areas of Europe rose above all other regions of the globe because it hit upon successive theories that were approximately true through an awkward assortment of accident and luck, geography and personal idiosyncrasy" (5). In each chapter of the book Trout lays out his defense of the various components of this view. In Chapters 2. and 3. he makes the case for the role of psychological and biological components of scientific discovery in the success of science. The fluency heuristic – "the substitution of the deliberate and analytic determination of truth with the feeling of ease" (44) is the centerpiece Chapter 2. This explains why we hang on to various theories or perspectives on the world. We hang on to familiar world views that we have worked on and trained in largely because "when it comes to mental processing, easier is better liked, and as a result, the sense of understanding is stronger" (20). This alone will not account for the success of science and of specific scientific theories because fluency can sustain and reinforce completely mistaken worldviews. (I take it that these include religious worldviews, ideologies and so on.) Further compounding fluency is the idea, elaborated in Ch. 3, that "We are biologically disposed, so to speak, to get a good feeling out of learning" (68). Here Trout

makes use of work in the neurosciences that shows that learning feels rewarding and this is because of the role dopamine plays in the learning process. In Chapter 4. Trout articulates his realist view, which he takes to be crucial. If the psychological and biological mechanisms articulated in Chapters 2. and 3. were the sole determinants of theory choice, then we could be saddled with lots of highly misleading theories. In this chapter Trout defends Inference to the Best Explanation (IBE) against its critics in philosophy of science and also criticizes anti-realist philosophers of science. For Trout, the best explanation for the success of science is that it is approximately true. The central idea of Chapter 5. is that in science: "progress was driven by contingencies of creative talent, geographic location, social affiliation with the right people, the purposes of patronage, access to raw materials, and the needs of industry or the military" (117). This view is pitted against the idea that science results from hard work and repeated application of the scientific method. Trout argues that this latter view completely rules out the role of chance in the advancement of science. Chapter 6. examines the special case of the rise of Newtonian science in light of the account developed in the previous chapters. In his concluding chapter, Trout tackles the role of science in a "modern democracy" and argues that science guided social policy can contribute to human well-being. There is a huge amount covered in this elegant little book and I would enjoy discussing all the topics in there. Unfortunately, I only have space to pick a few.

The scope of Trout's account and the question of "what are the mature sciences?"

According to Trout, successful sciences are here to stay: "Mature sciences do not easily surrender their achievements, and regresses are uncommon" (204). Here I want to examine

this notion of mature sciences. Trout's scientific realism is "understood to apply specifically to the mature sciences – certainly physics and chemistry and to selected areas of biology" (82-83). On Trout's view science carves nature at its joints and "Once theory carves those joints, it has exposed natural kinds, objects in nature that pay a taxonomic role in mature, working science" (38). Here he also says that "Natural kinds are the very stuff of science" (38). The mature sciences are either a subset of particular sciences or can be picked out via their approach. Also, Trout introduces mature theories. For example, he says that the realist claim is that an inductive practice like IBE can only safely be applied "once you have a mature theory" (90). In the discussion of realism and anti-realism, Trout inter-substitutes mature and successful when referring to theories. So, mature sciences trade in mature or successful theories. Also, Trout gives us an idea of what do not count as mature (or successful) sciences: "when philosophers of science look at our best scientific theories, they have looked for explanations of their success. After all, we don't find this success just anywhere: just look at palm reading, astrology, or the theory of humors" (101). In other places Trout contrasts Newtonian mechanics with (parts of) the alchemical world view and also says "modern chemistry is better than alchemy, and the modern physiological theory of axonal conduction is better than the early modern theory of animal energies" (110). So, failed proto-sciences on the ash heap of history are not mature sciences. Let's consider some finer grained distinctions among scientific practices along with some other ways of dividing up scientific practices, such as natural vs. social sciences, in order to put pressure on Trout's claim that scientific realism applies to the mature (or successful) sciences.

First, let's consider some finer grained distinctions between scientific practices judged via approximate truth. Trout admits that successful scientific theories can have problematic components. This is part of his notion of approximate truth. On this account, Newton can adopt the approximately true corpuscularianism of the alchemists. However, consider some different types of problematic components to scientific theories, for example, de-phlogisticated air and luminiferous ether. How does the proponent of approximate truth ascertain which of the two theoretical posits de-phogisticated air and ether are the worst or which is better or can we say that they are on a par with one another. We can make a case for Preistley's de-plogisticated air being an approximately true theoretical posit given that there is a type of air (we now know to be oxygen) that is required for combustion and respiration. In contrast, we take Maxwell's extension of Newtonian mechanics dealing with light to be approximately true despite its (we now know) completely false theoretical posit that ether permeates the universe. Is the one approximately true part of the phlogiston theory a better theoretical posit than the completely out-there component of Maxwell's otherwise legitimate theory? The contrast between alchemy and Newtonian mechanics or alchemy and (modern) chemistry is not as much of a challenge for the notion of approximate truth as debating the relative merits of de-philogisticated air and ether or comparing Preistley and Lavoisier's chemical systems. Lavoisier, the winner in the case of oxygen, still had plenty of alchemy left in his work including elan vital and caloric, both of which are principles in the alchemists' sense of the term. These considerations lead me to think that assessing a theory's maturity via the notion of approximate truth is a more delicate and detail dependent issue than it appears when we consider the coarse-grained contrast classes that Trout offers.

Here's a more recent example of an arguably mature (or successful) scientific practice: Genome Wide Association Studies (GWAS) in human behavioral genetics. I am quite skeptical about this approach but if you read any behavioral genetics and medical neuroscience you would wonder how anyone could question such a wonderfully successful approach to understanding human traits. My concern is that complex techniques for establishing correlations between various human traits and bits and pieces of the genome are being taken to be techniques for establishing the genetic basis of the relevant traits. It appears out of place to say that GWAS are bad science (in the way that astrology or alchemy are bad sciences) or that GWAS are not based in mature theory. If I understand the notion of approximate truth that Trout is using, I think that GWAS are based in approximately true theories: GWAS relies on population genetics, evolutionary biology and molecular biology. If this is the mark of mature science, then GWAS is a mature science but, I argue, it is not the best way forward in the attempt to find the genes underlying human (behavioral) traits. Can Trout's notion of approximate truth be wielded in criticizing perfectly respectable and by all accounts successful scientific practices that still have serious problems. One thought here is that Trout may want to bring his notion of explanation to bear in a case like this: GWAS may pass muster on the grounds that it is approximately true but it does not provide "an accurate description of the underlying causes that bring about an effect" (115), Trout's hallmark of a good explanation.

Now let's consider the social sciences. Is Chompskian linguistics successful because it is approximately true? Chompskian linguistics is an example of a hugely successful theory and

collection of methods but one that has many detractors (including Trout in other work than the book being considered here). Or consider nativism in cognitive science in general? Is that approximately true? The nativist position dominates developmental cognitive psychology and has done for sixty years or so. Finally, what about evolutionary psychology? By all accounts this is a fantastically successful approach in the social sciences but is it successful because it is approximately true? On one understanding of approximate truth, evolutionary psychology is clearly approximately true. Evolutionary psychologists adopt somewhat accurate notions of evolution, at least on some dimensions, and they also borrow liberally from nativist approaches in the cognitive sciences. On the other hand, evolutionary psychology is criticized from many different perspectives. On common type of criticism of evolutionary psychologists is that they don't get the evolutionary biology right.

These examples introduce different types of questions about Trout's notion of mature science. For example, does Trout have a view similar to Ian Hacking's idea that the natural sciences can be readily carved off from the social sciences and that different standards apply in assessing social science theorizing? In this case, no social science would belong in the list of mature sciences. Even if they are to be treated separately with respect to maturity, what does success amount to in the social sciences? Can it be cashed out via the notion of approximate truth or do we need different criteria of success in this domain? There are risks in all directions here. For example, when Trout is considering the faceoff between governments and scientists over issues of policy, he says "a modern democracy must recast policy making in a scientific image" (196). Much of the science relevant to policy decision making is social science and Trout

wants our government to be responsive to our best science, which in this case is our best social science. If there can be no mature social science, then this hands policy makers a distinction to justify their rejection of certain types of research. If there can be mature social science and it is to be characterized via the notion of approximate truth, then perhaps Chomskian linguistics, evolutionary psychology and nativist developmental psychology all have equal claim on our policy makers. This situation would not bode well for statistical learning theorists and cultural evolutionary theorists' place at the policy decision table, given that their approaches are antithetical to nativists.

Trout's critical target in the history of science.

Trout says that the dominant view of scientific progress is that the experimental method and incremental advancement explain progress in science. I don't agree that this is the dominant view or, if it is, where and for who it is the dominant view? Reflecting on Trout's critical target helps reveal more about this own view but also, I argue, reveals some fellow travelers that he does not acknowledge.

What Trout calls the "experimental story" is the idea that successful science results from hard work and repeated application of the scientific method. As he rightly points out, if this were the case, successful science would have arisen out of Medieval Islamic experimental work, which was highly developed four hundred years before Boyle and his contemporaries. The more general version of the "experimental story" is that science improves incrementally, largely due to this experimental work. Think of this as gradualism in the context of scientific progress.

Trout is strongly opposed to this outlook: “To spin the history of science into a story of rational development, you need an unscientific tolerance for inaccuracy, an aversion to uncertainty, and a compulsive need for narrative closure” (117-118). Here he chides proponents of the “experimental story” for leaving the vast amount of contingency impacting scientific advance aside. Promoting his positive view of the role of contingency he says: “Once we give due weight to contingency in scientific progress, we can for the first time give a more accurate account of the history of science” (119). Proponents of the “experimental story” are not only misguided for ignoring contingency, they are at fault for downplaying the role of theories and theorizing in scientific progress. Here he says “Let’s celebrate the importance of unplanned, unguided, and occasionally uninterested theorizing not as a dignified alternative to experimental reasoning, but as essential to it” (122). Ignoring theory is also bad because latching onto true theories is the key ingredient for scientific progress on Trout’s account. His emphasis on contingent factors is tempered by the idea that contingent factors have no traction “unless the [contingently arrived at] discovery promotes a theory or theoretical outlook that actually captures the causal structure of the world” (123).

If Trout showed up when Popper and Lakatos were castigating Kuhn and Feyerabend for their irrationalism with abut scientific progress, he would be hard to seat on one or other side of the table. He sounds very much like Popper when he urges us to celebrate theorizing but cites Kuhn favorably (while disagreeing with Kuhn’s social constructivism) and sounds like an irrationalist when he champions all the forms of contingency that are relevant to scientific

success. All this makes it hard to see what Trout's take on the discovery/justification divide is; perhaps he thinks that it is a false dichotomy.

Let's for now continue to focus on the opposition between the "experimental story" and the emphasis on theory. Where does Trout stand on the experimentalist movement in the history and philosophy of science that set themselves in opposition to the dominant view of history of science that they took to be charting the march of theory? Cartwright, Peter Galison, Hacking and Alan Franklin come to mind as early proponents of this approach. Now the torch is carried by the likes of Melinda Fagan, who argues, persuasively, that some success in some sciences, such as stem cell biology cannot be understood from a theoretical perspective and that the work is best understood as not to be guided by theory. Taking a more general approach, Hacking rejected the respresentationalist approach that guided what he perceived as the theory first perspective. I take none of these pro-experiment types to be proponents of the dull "experimental story" that Trout rightly criticizes. Rather, they take their work as providing a corrective to the theory first approach that they see as dominating, particularly the philosophy of science. Perhaps Trout doesn't have to convince the majority of philosophers of science to focus on theory at the expense of experiment as, if Hacking et al. are right, they already do.

Many philosophers of science ignore or downplay the role of contingency in science but philosophers of science who take the details of scientific practice very seriously cannot be accused of this. For example, Ken Waters, in his Presidential Address to the Philosophy of

Science Association, emphasized the relevance of the contingent details of scientific practice to our appraisal of scientific ideas. This idea was not lost on the previous president of the PSA, Helen Longino, who has long championed this approach. The likes of Waters and Longino also do not subscribe to an anemic version of the "experimental story." Historians of science also have been long time critics of philosophers' penchant for the "experimental story" instead of accounts of the development of science that are rich with details of the contingency that Trout urges us to take note of. Historians of science also criticized "history of ideas" approaches to the history of science and instead proposed richer histories of science. Ken Alder's work, such as *The Measure of All Things*, is history of science in this more inclusive vein but here are countless examples to choose from and there is hardly any work in the history of ideas tradition in history of science now. In response, philosophers have criticized these historians of science for losing any possibility of normative purchase in a sea of useless detail.

Trout indicates that science textbooks give too much weight to the "experimental story" and this is right. The problem is that science textbooks contain, for the most part, very underdeveloped history of science and little or no philosophy of science. Here I agree that Trout has put his finger on a problem but my idea of the solution to this specific problem – the poor treatment of history and philosophy of science in science textbooks – comes from another direction. I would urge that university science education should be supplemented with both history and philosophy of science. This can be achieved in the context of teaching history and philosophy of science courses to all science majors. This is an approach that will take some time but perhaps not more than the time it would take to convince science textbook writers to

include more sophistication and nuance in their treatment of these topics. I certainly think that it is too optimistic of Trout to think: "If a modern physics textbook must have a one-paragraph history of science, let's make it an accurate one: it's not that there was a good method; it's that theories got better" (110). To achieve this, we would have to convince physicists that one specific account of the success of scientific theories should be included in their textbooks, which would be a tall order.

Scientific realism and the various philosophies of science.

I said at the outset that I share Trout's naturalist and physicalist views. Likely I don't share exactly the same variants of physicalism and naturalism as those he espouses but we won't have much to argue about. For example, I enthusiastically endorse Trout's diagnosis of anti-physicalists. Why on earth would anyone think that the physicalist account of consciousness, for example, would be easy to understand? I would go even further, the toy physicalist "theories" put forth by anti-physicalists in order to knock them down have nothing close to scientific content. The assumption behind these so-called theories is that the constituents of the world are going to line up like building blocks rather than involving multiple interacting complex dynamic systems. If we need some of the most complex mathematically based theory and modeling around to grasp climate, I imagine we would need some pretty complex math and theory to grasp consciousness. We don't think that the climate is not physical because it proves so hard to grasp. But what of realism?

In discussing some anti-realists, Trout entertains the idea that Van Fraassen, Fine, Larry Laudan et. al are not really skeptics about science, rather they strike that pose to ask questions about how science is working. Trout doesn't buy this and says that they are skeptics about science. I disagree, certainly in the case of Fine. It would take a lot more work to make the case for Laudan and Van Fraassen and we are not talking about them here, we are talking about Trout. Trout then moves to the "if you don't like it/trust it then don't use it" argument against the (presumed skeptic). I think that this move has a place in public discourse about science but doesn't hit as hard here, inside the philosophy of science. Radical social constructivists about scientific knowledge trust and use science and its products and are as entitled to as the rest of us. For example, Bruno Latour defends climate scientists against misguided climate change deniers. Even the Latour of *Science in Action* saw himself to be giving an explanation for why scientific knowledge is special. He was not trying to argue that science didn't work in the way a contemporary anti–vaccine person does. Trout says that his introduction of this kind of criticism has "an uncharitable tone." But so does the insinuation that our "core theoretical beliefs in physics, chemistry and, biology lack warrant" (106). I don't think this is quite the right characterization of Van Fraassen, Fine and Laudan and even Latour. My take is that someone like Van Fraassen, for example, disagrees with Trout (and Boyd et al.) about the source of warrant for theoretical beliefs in physics (and other sciences).

There are points when Trout engages in the kind of table thumping that Fine has long argued against, particularly in the context of the realism/anti-realism debates. In this part of his discussion, there is the implication that anyone who is not a realist of his particular

persuasion is intellectually dishonest. Here I plead for a little more mutual tolerance within philosophy of science. Consider this possibility: Presumably Trout is not an ontic structural realist. Those folks are realists but provide completely different explanations for the success of science than Trout's, along with different accounts of what count as the mature sciences. James Ladyman, for example, has said that he would have to bite the bullet and say that biology is not a mature science in the same sense that physics is. Is there a place for proponents of alternate realisms to defend science alongside Trout?

The place where I want to stand alongside Trout as well as anti-realists, constructivists, structural realists, pluralists about explanation, feminist philosophers of science and on and on is when we stand up and are counted in our support of his proposal that science be integrated into our modern democracy. Our current predicament in the USA is a tough one in which science, reasoning and education in those things is under attack. I am not going to stop an up and coming graduate student in philosophy from making a name for herself in our field by introducing a novel criticism of the ontic account of explanation but I am going to argue strenuously that she should close ranks with her opposite number in that debate when called to defend science and defend its key role in our democracy. On this point, Trout and I are in strong agreement.