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Book Reviews

P. KYLE STANFORD, *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives,* Oxford: Oxford University Press, 2006, xiv + 234 pp., $45.00.

Chapter 1 and 2 of the book set out the problems it deals with and the main arguments that are pertinent in that area. The main problem is scientific realism: "the position that the central claims of our best scientific theories about how things stand in nature must be at least probably and/or approximately true" (p. 6). The "miracle argument" is a standard argument for scientific realism and it states that this position is the only one that does *not* make the success of science a miracle. The "pessimistic induction" counters this argument by observing that "the scientific theories of the past have turned out to be false despite exhibiting just the same impressive sorts of virtues that present theories do, so we should expect our own successful theories to ultimately suffer the same fate" (p. 7). The other most important argument against scientific realism is the "underdetermination of theories by the evidence": every theory must have empirical equivalents, i.e. theories that have the same empirical support and the same empirical consequences as the original theory but nevertheless posit very different unobservable entities. Therefore, we cannot trust any theory's statements about unobservable entities.

However, these standard arguments for and against realism all have their problems and this is why Stanford wants to modify and thereby strengthen them. Instead of "radical" underdetermination of theories, i.e. their complete empirical equivalence, he considers "transient" underdetermination, i.e. empirical equivalence of theories with respect to only the actually existing evidence. This sort of underdetermination is called "transient" because any new data can destroy the temporary empirical equivalence of those theories. Transient underdetermination becomes a threat to scientific realism with regard to our current best confirmed theories if we consider the possibility that there might be "unconceived alternatives" to those theories that are equally well-confirmed. If such unconceived alternatives exist, they may tell us very different things about unobservable entities, thereby undermining the beliefs we gained from our current theories. And indeed, the "new induction over the history of science" renders this possibility real. Again and again, *later* theories positing very different unobservable entities were shown to be consistent with the then available empirical evidence but were unconceived at the time. The important fact at this point is that science often uses eliminative inferences in order to argue for the acceptance of a theory. By their very nature, however, *unconceived* alternatives cannot be subjected to a conscious eliminative procedure – they simply go unnoticed. Thus, eliminative inferences leading to one accepted theory are unreliable because scientists are unable to exhaust the space of all reasonable theory candidates that are well-confirmed by some set of empirical data.

This claim is made more concrete in Chapters 3 – 5. Stanford presents three examples that demonstrate that at three stages of the history of biology, serious scientific alternatives to the accepted theory existed at the time but were not considered. Neither (1) Darwin's pangenesis theory, nor (2) Galton's stirp theory, nor (3) Weismann's germ plasm theory were without theoretical alternatives. In fact, (2) was an alternative unconceived at the time to (1), (3) was an unconceived alternative to (2), and contemporary molecular genetics was an unconceived alternative to (3). Hence, all judgments about the (approximate) truth of some theory are unwarranted as long as its endorsement is based on eliminative inferences.

In the Chapters 6 and 7, Stanford reviews several of the realist responses to the challenges posed by the historical record. He concludes that none of them is really capable of resisting the challenge posed by the problem of unconceived alternatives, but for various reasons. One of Stanford's argumentative strategies is to show that many a move intended to defend realism from some attack may indeed rebut that particular attack but only at the price of diluting realism to an unacceptable degree – he calls these "Pyrrhic" victories for scientific realism.

In the final chapter, Stanford sketches his view of science without realism. He denotes his favorite form of instrumentalism as "epistemic", contrasting it with earlier "semantic" or "linguistic" approaches to instrumentalism. The basic idea of epistemic instrumentalism is that it licenses the use of a theory for predictive and explanatory purposes and for interventions into Nature's course. However, this license does not commit one to believing the theory's stories about the nature of those theoretical entities that it posits.

The book is highly recommended to anyone interested in the contemporary debate about scientific realism. It shows with particular clarity and at a remarkable level of historical scholarship how detailed historical analyses can bear on this debate.

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A.D. MORRISON-LOW, *Making Scientific Instruments in the Industrial Revolution*, Aldershot: Ashgate Publishing Ltd., 2007, xvi + 408 pp., illus., € 55.00.

A.D. Morrison-Low has provided an outstanding integrative history of instrument making that many of us perhaps secretly aspire to but seldom attain. This is a substantial achievement and raises her work far above the caliber and importance of most other writings on the history of scientific instruments and their manufacture. The focus of this work is the making of scientific instruments in England outside of London, in the urban centers of coastal Bristol and Liverpool, York and Sheffield in Yorkshire, and Birmingham in the Midlands and Manchester in the North. The author traces the inspiration for this work to a review by John Millburn of the 1985 exhibition "Science and Profit in 18th Century London" at the Whipple Museum at Cambridge. Millburn observed that by seeing the exhibition and reading the companion monograph, "one would never guess that little is in fact currently known about the detailed structure of the instrument making trade". Morrisson-Low, thus inspired, made it her quest to add substantively to our understanding of the inner workings of the scientific instrument trade. What sets this work apart from much literature on scientific instruments is that Morrison-Low places the subject within the context of a much broader body of historical literature, particularly but not exclusively economic and business history.