P. Kyle Stanford, *Exceeding Our Grasp*: *Science, History, and the Problem of Unconceived Alternatives*. Oxford: Oxford University Press, 2006. 234 pp.

In recent years, two challenges stand out against scientific realism: the argument from the underdetermination of theories by evidence (UTE) and the pessimistic induction argument (PI). In his book, Kyle Stanford accepts the gravity of these challenges, but argues that the most serious and powerful challenge to scientific realism has been neglected. The problem of unconceived alternatives (PUA), as he calls it, is introduced in chapter one and refined in chapter two. In short, PUA holds that throughout history scientists have failed to conceive alternative theories roughly equally well-confirmed to the theories of the day by the available evidence and, crucially, that such alternatives eventually were conceived and adopted by some section of the scientific community. PUA is a version of UTE, but, unlike its kin, enjoys substantial historical support. It leads to a sort of pessimistic induction that Stanford brands 'the new induction' (NI), according to which we should be doubtful about the truth claims of current theories since the historical record suggests that unconceived alternatives are typically lurking in the shadows. His proposal contains two important shifts of focus: First, there is a shift from artificially produced rival theories - of the kind typically talked about in the underdetermination debate - to actual rivals. Second, instead of focusing on empirically equivalent rivals, he urges a shift to rivals that are more or less equally well-confirmed to existing theories by the available evidence at a given point in time. Prima facie, PUA sounds like a welcome addition to the anti-realist arsenal, drawing on historical evidence to support the induction that current theories probably face genuine alternatives waiting to be conceived.

Behind the initial plausibility, lays a network of objectionable views of which I will only touch the surface. To start with, the main evidence offered for PUA and NI, namely Darwin's pangenesis theory, Galton's stirp theory and Weismann's germ-plasm theory, is too small a sample to draw inductive inferences about the history of science, let alone its future. Yet, it would be unfair to dwell on this point, as one can only analyse so much of the historical record in the confines of a single book. More case studies need to be carried out to determine if PUA and NI are borne out of the history of science, but it is certainly a bit premature to claim, as Stanford does, that "this single series of historical episodes may go a considerable distance" (p.47).

The discussion in the historical chapters (three to five) betrays an inconsistent attitude Stanford harbours towards criteria of success. Although at times he wishes to battle the realist on common ground by accepting her high, yet often vaguely formulated, standards of success (e.g. p.34, p.160 f2) - typically involving a cocktail of unified explanations, varied and novel predictions, and successful interventions - in practice he often rests content with theories that only offer explanations. Darwin's, Galton's and Weismann's theories of generation and inheritance are primarily explanatory in nature with little to no empirical support. What is more, Stanford does not attempt to vet the quality of explanations. This last point comes out early on, when he tells us that "Aristotelian mechanics was used to explain the generation of cats and the formation of human societies" and that such explanations count as Kuhnian losses (p.22).

The only potentially impressive predictive accomplishments in Stanford's discussion of the three theories of generation and inheritance boil down to Weismann's prediction of reduction division, something he brands "a previously unknown phenomenon in the history of biology" (p.130). The prediction, i.e. germ cells receive half of the germinal material of the parent cell, fails to impress when compared to the staple of novel predictions one finds in physics, where theoretically calculated values approach the observed values to many decimal places. Moreover, the phenomenon was neither previously unknown, nor clearly unexpected. As Stanford notes earlier in that chapter, Edouard van Beneden had already identified "'maturation division' (later termed 'reduction division' by Weismann)" (p.106). He also notes that Weismann "became aware of van Beneden's work and realised that… reduction division… fit his own theoretical predictions" (pp.106-7). Stanford offers no independent confirmation that this was indeed the order of events, simply taking Weismann's word for it. But let us not be unduly uncharitable. Perhaps, the phenomenon was unexpected but a welcome consequence of Weismann's theory. Since Stanford uses this example to argue against the realists' reliance on novel predictions (p.130, p.181, p.207), one anticipates an elaboration of the story behind Weismann's prediction that answers the typical realist concerns on such matters, e.g. whether reduction division was a consequence of only some features of Weismann's theory, whether these features survived into our modern account, etc. Instead, all we are told is that Weismann made this prediction "on the basis of purely theoretical considerations" (p. 136, f7).

The three historical case studies prop up PUA, but only because its demands on genuine rivals are so meagre. PUA's sole requirement is that rivals are roughly equally well-confirmed by the available evidence. In a footnote, Stanford lowers the demands even further saying that strictly speaking alternatives should not be "*effectively ruled out* by the [available] evidence" (p.26, f10) [original emphasis]. He goes on to say that the stronger requirement, namely "roughly equally well-confirmed by the available evidence", though not necessary is defensible and "deflects any suggestion that such alternatives were ignored on evidential grounds" (ibid.). Both requirements are simply too weak. Theories or theory parts can count as genuine rivals even if there is *no evidence available whatsoever*. Indeed, that's precisely what Stanford has in mind with the aforementioned theories, for he prides himself that (at least with respect to the unconceived parts) "there simply was no available evidence" (p.134). Equally shocking is his use of this lack of evidence to thwart any objections about how easy it is "to judge that a given alternative was even roughly as well confirmed" (ibid.). But to suggest that lack of evidence can convey (a) what counts as a genuine rival and (b) what counts as more or less roughly well confirmed is to reduce PUA to a lame duck. Or, to borrow one of Stanford's expressions, PUA becomes 'a pyrrhic victory' for the anti-realist.

Chapters six and seven are by far the most philosophically rewarding. There Stanford undertakes to defeat various realist replies to PI, for he takes some of them to also threaten PUA (p.141). Quite a few of the objections he raises are not original, e.g. the deficiencies of pure descriptivist and of pure causal theories of reference, but they are rendered in an incisive manner. The most sophisticated recent defence of realism,

selective confirmation, consumes the whole of chapter seven. Against it, Stanford raises his centerpiece objection, i.e. realist criteria of success aiming to distinguish between essential and idle parts of theories "are either not prospectively applicable at all, fail to distinguish the parts of present theories realists hoped to defend from any others, or require us to exercise discriminatory abilities whose reliability is itself subject to an historical challenge" (pp.183-4).

Stanford's dismissal of some realist strategies towards selective confirmation is at times hasty and injudicious. His dismissal of the structural realist strategy, for example, reveals a lack of familiarity with the relevant literature. Contra Stanford's charge that it is unclear whether we can plausibly distinguish structure from non-structure, it can be pointed out that 'structure' denotes specifications of nature up to isomorphism while 'non-structure' denotes specifications that go beyond isomorphism (p.181). Furthermore, his charge that the structural realist "leaves us with no justifiable confidence in our ability to clearly distinguish" essential from idle features of theories (p.183) seems to be based on the erroneous assumption that the structural realist is committed to all and only fundamental theoretical structures. Yet, the structural realist is neither committed to Weismann's admittedly structural claim about germinal specificity (p.181) because it possessed no independent confirmation nor stunned by the non-fundamental status of Galton's ancestral law of inheritance in contemporary genetics (p.182) because there is no restriction to believe only fundamental theoretical structures. As for Stanford's allegation that structural realism is consequently forced to retreat to such vague and general statements as "[structure] will be recoverable in *some* way, *some*where, *some*how from future science" (ibid.) [original emphasis], the following robust proposal can be given as a response: Not all structures survive theory change, but all genuinely successful theory features survive, either intact or suitably modified (via non-trivial correspondence rules), and are structural.

This book fails to convince the reader of its main proposals, from PUA and NI to what seems to be a new brand of instrumentalism put forth in the eighth and final chapter. The book nonetheless succeeds in stimulating a rethink of the underdetermination problem. It does so by calling into question what ingredients are necessary for a strong underdetermination recipe, suggesting that something less than empirical equivalence and something more than artificially produced rivals will do just as well, if not better.

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