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Crossing the 'explanatory divide': a bridge to nowhere?

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At the very beginning of his book Tabery explains the key phrase from the title and also briefly describes his view on the nature–nurture controversy:

> We have moved beyond versus. Whether it is medical traits like clinical depression, behavioral traits like criminality, or cognitive traits like intelligence, it is now widely recognized that 'nature versus nurture' does not apply. There are no genes for depression such that having the gene ensures the development of depression and lacking the gene ensures resilience to depression. Likewise, there are no environments for depression such that all differences in depression can be explained by pointing to those differences in environment.[1]

The claim is that 'we have moved beyond versus' and that 'it is now widely recognized that "nature versus nurture" does not apply'. But what exactly is this fact that, allegedly, is now widely recognized but of which scholars were not aware earlier (in the age of 'versus')? Answer: that there are no genes for depression such that having the gene ensures the development of depression and lacking the gene ensures resilience to depression.

This does not seem to do justice to participants in the 'versus' debate: those who discussed the 'nature versus nurture' question were actually well aware that complex psychological characteristics (like depression) were probably not causally dependent on a single gene and also that the explanatory role of environmental factors should not be ignored. Surely they did not claim that the presence or absence of one gene was sufficient to 'ensure' anything. Tabery presents their position as being extremely simplistic and implausible even by standards of their own time. Could lucid thinkers like Galton, Fisher, Jensen or Eysenck have gotten things so wrong, and for such a long time? Hardly.

I will try to show that the 'versus' position was much more sophisticated and more plausible than Tabery makes it appear.

Tabery's book is divided into three parts that deal with three aspects of the nature-nurture debate: historical, philosophical and bioethical. My commentary will be restricted to the first two parts, which deal with history and philosophy.

History

One of the key episodes in the nature-nurture debate was about race, genetics and IQ: is it reasonable to hypothesize that the observed difference between the average IQs of Whites and Blacks is partly genetic? Jensen said 'yes', Lewontin said 'no'.

Now, after 40 years, perhaps we should be able to make sense of that debate. Consider the view that Jensen was mainly right, and that Lewontin's basic arguments were wrongheaded and motivated largely by his political bias. Tabery rejects this view for two reasons.[1] First, he says that 'it is simply unfair to write off one side as ignorant or biased as if that position had no genuinely intellectual foundation'. Second, he argues that particular biases in the race and IQ debate cannot have played a decisive role because very similar methodological arguments were already rehearsed in a much earlier debate between Lancelot Hogben and RA Fisher about statistical interaction between genes and environment.

First, there is nothing 'unfair' about presenting one of the positions in a debate as 'having no genuinely intellectual foundation', i.e. as being wrong in its basic claims. The only question is whether this picture is supported by evidence. After all, we should not assume a priori that the truth is in the middle and that each side must have some 'genuinely intellectual foundation'. And, second, speaking about the debate between Hogben and Fisher, it also had a clear political background, so there was ample space for the operation of similar external factors there as well. (I am not implying that both debates should be explained in the same way.)

Speaking about Marxist political beliefs that make people strongly resist any idea of linking genetics and human behaviour, there is a fascinating case of John Maynard Smith who describes how it was precisely this kind of political opinion that prevented him and JBS Haldane from making a breakthrough, which easily opened itself to someone without ideological blinders (WD Hamilton):

> And neither [JBS Haldane] nor I at that stage were at all willing to entertain the notion that such behavior would be anything other than culturally determined and influenced. We were, I think, very reluctant, as Marxists would be, to admit that anything genetic might influence human behavior. And I think that we didn't say consciously to ourselves that this would be un-Marxist so we won't do it, that's not the way that the mind works; but I think it was a path that our minds were not, so to speak, prepared to go down, in quite an unconscious sense.[2]

In my opinion, the key consideration for properly evaluating the role of politics in the race and IQ controversy is the asymmetry between the two sides. It is Lewontin, Gould, Kamin and Rose who themselves used to stress that their opposition to hereditarianism was politically motivated. This sometimes went so far that they even claimed that their 'critical science' was an 'integral part' of their struggle to create 'a more socially just—socialist—society'.[3] No remotely similar statement can be discovered in the writings of scholars from the opposite camp.

Furthermore, some highly esteemed figures in contemporary biology, who never publicly entered the fray of the race and IQ debate, treat Lewontin's politicization of science as a matter of common knowledge and as very regrettable:

> Lewontin, in particular, is known to be strongly politically biased and himself admits to being scientifically unscrupulous on these issues. That is, he takes them as political ones and therefore feels justified in the use of biased arguments.[4]
>
> R. C. Lewontin is an equally distinguished Cambridge (USA) geneticist, known for the strength of his political convictions and his weakness for dragging them into science at every possible opportunity.[5]
>
> I had already some years ago called attention to Lewontin's misleading claims. I suggest Lewontin's book *The Triple Helix*. The unwary reader will not discover how totally biased his presentation is. All evidence opposed to his claims is simply omitted! And if you present the truth you are denounced as a Nazi or Fascist! The public unfortunately is all too easily deceived! Particularly when wishful thinking is involved! (from Ernst Mayr's 2003 letter to Cambridge geneticist AWF Edwards, courtesy Prof. AWF Edwards.)

Tabery himself acknowledges[1] that throughout the 1970s Lewontin 'mixed his science and politics', but for some reason he refuses to take seriously the possibility that such a strong political bias may have caused Lewontin to espouse anti-hereditarian arguments that failed to meet normal scientific standards.

In a way Tabery does concede tacitly and indirectly that Lewontin's objections to hereditarianism were ultimately unsuccessful when he helpfully draws a table representing several differences between the 'variation-partitioning approach' and the 'mechanism-elucidation approach'. Variation partitioning refers to the attempt to measure the strength of different contributions to phenotypic variation (genetic, environmental, G-E interaction, G-E correlation). Mechanism elucidation refers to the study of the ontogeny and operation of proximate mechanisms. Tabery regards each of these two approaches as legitimate and fruitful in its own domain of enquiry, which directly contradicts Lewontin's radical view that the former approach is entirely useless. So it seems that, after all, Tabery actually agrees that Lewontin's dismissal of the variation-partitioning approach is indeed untenable. Nevertheless he is unwilling to concede that the notorious intrusion of politics into Lewontin's thinking could account for his fundamentally misguided argument against Jensen.

There are also several specific problems with Tabery's historical account of the debate about race and IQ. For instance, he mentions that Lewontin criticized Jensen for fallaciously inferring the between-group heritability of IQ merely from within-group heritability, but he fails to mention that this particular criticism was later shown to be unjustified.[6] This was actually an important aspect of the whole controversy and therefore the complete information would have been welcome.

Tabery says that Jensen's genetic hypothesis regarding race differences found favour with a number of scholars, among whom he includes Richard Herrnstein.[7] This attribution is mistaken because, far from supporting Jensen on the race issue, in the cited article from 1971 Herrnstein in fact explicitly says that 'the case is simply not settled, given our present stage of knowledge'. What makes Tabery's mistake additionally odd is that I warned about this widespread misinterpretation of Herrnstein's early work in

Making Sense of Heritability,[6] the book that Tabery reviewed (twice).

Tabery states that Cyril Burt's data 'seem to have been largely fabricated' and supports this statement by referring to Kamin[8]—a source which presents only one perspective on the Burt affair, and a very biased one at that. Several later books[9–11] as well as a number of articles that were devoted specifically to this topic called the 'fabrication' theory into question. Without getting into detail, one noteworthy fact related to the Burt affair is that Burt's estimates of the heritability of IQ, which were supposedly based on completely fabricated data, are very similar to later, unquestionably reliable estimates. It is unclear why Tabery does not mention any sources related to Burt besides Kamin's book. Contrast this uncritical acceptance of the worst-case scenario (fabrication) with the following cautionary conclusion that is shared by many contemporary scholars: 'I do not think we will ever know whether Burt was intentionally fraudulent or unacceptably careless in his later years. The cases for and against him rest largely on circumstantial evidence'.[12]

According to Tabery, participants in the debate about race, IQ and heritability often experienced negative consequences due to their espoused views. The only illustration of this phenomenon he gives is Marcus Feldman's speculation that once his job search was stymied because he had co-authored an anti-heritability article with Lewontin. In the end Feldman did get that job. It is puzzling that out of so many much more serious episodes of scholars getting in trouble because of their research on this topic, Tabery picks out such a non-event as an illustration. Typically those on the receiving end of strong negative reactions have been scientists with a hereditarian bent. Therefore, even if—counterfactually—Feldman had failed to get a job because of his association with Lewontin, this would not have been a good illustration of the prevailing trend. Why not select, instead, some of many real cases of social scientists who have been exposed to irrelevant criticism, pressure, intimidation and character assassination for defending the nature side in the nature-nurture debate? For a catalogue of such examples see Gottfredson[13] and Cofnas.[14]

Philosophy

The central part of Tabery's book addresses the 'explanatory divide' between two research perspectives—variation partitioning and mechanism elucidation—and the need to bridge that explanatory divide. To put it briefly, variation partitioning is an attempt to estimate relative contributions of different sources of variation (genetic, environmental, etc.) to phenotypic variation. Mechanism elucidation is an attempt to describe specific causal paths that produce phenotypic outcomes.

As mentioned above, judging by the fact that Tabery presents both of these perspectives as legitimate approaches it seems that, despite his desire to stay above the fray, he ends up largely rehabilitating the research project of behaviour genetics. For, in acrimonious methodological debates that reached their peak in the 1970s, behaviour geneticists were criticized precisely for conducting research that allegedly provides no knowledge of explanatory value. The accusation was that, due to a number of methodological limitations, their preferred method, analysis of variance, could not support any claims about causes of phenotypic differences. Behaviour geneticists, in turn, have never disputed the need for eventually learning more about the actual mechanisms that explain how genetic and environmental factors produce psychological variation. So the current explanatory divide, in which each of the two approaches is thought to have a valuable research goal, is actually a vindication of the variation-partitioning project which was for a long time accused (with a lot of support from philosophers) for developing 'methods of estimating useless quantities'.[15] For more details see Sesardic.[6]

Tabery says that differences in methodology and research questions 'do not isolate variation-partitioners and mechanism-elucidators in two incommensurable world views. There is an explanatory divide, but it can be bridged'. Obviously, no reasonable person thinks that those who are engaged in these two different approaches are isolated from each other in two incommensurable world views. It is clear that there will be a lot of give-and-take between the two groups, sharing information, mutual citation, occasional cooperation and so on. And yet despite the non-existence of a clear boundary, it might still be widely recognized that the two fields have their distinctiveness and that many scholars easily perceive themselves (and are perceived by others) as belonging in one of these traditions. Is such a situation undesirable? Is there anything to be gained by an attempt to change things and 'bridge the explanatory divide'?

If two recognizably distinct approaches exist, with largely different methodologies and research goals, and if scholars can combine these two approaches whenever they think this will help them solve their problem, why should they strive to 'bridge' the 'divide' between these two approaches? And what would 'bridging the divide' even mean here?

We might be tempted to assume that any bridge-building enterprise is a good thing and that it does not require any special justification. For is it not good to engage in an integrative effort, emphasize pluralism, try to cross boundaries and so on? Not necessarily.

To achieve his goal, Tabery uses 'two philosophical tools: the philosophy of mechanisms and the concept of an actual difference maker'.[1] These are two philosophical theories, each of which connects with one of the two sides of the explanatory divide (mechanism elucidation and variation partitioning). These philosophical theories are general in nature and are not focused on anything so specific as the nature-nurture controversy. And yet Tabery argues that, by relying jointly on these two philosophical theories, one can build a bridge between mechanism elucidation and variation partitioning in the context of explaining human psychology.

But it seems this project did not get far. Moreover, there are reasons to doubt that any similar enterprise would fare better. First, it is hard to see how these philosophical theories of a very wide scope, addressing highly general methodological issues, could shed light on how to achieve fruitful cooperation when mulling over very specific research questions. The more general a theory, the less likely it will be to give non-trivial guidance in finding the most promising strategy for a concrete research agenda.

Second, the example that is supposed to illustrate how the union of two philosophical approaches bridges the explanatory divide—research on brain-derived neurotrophic factor (BDNF)—fails to prove Tabery's point. The example does show a successful combination of two methodologies (variation partitioning and mechanism elucidation) but it is far from clear that the success was due to an important methodological (or philosophical) insight that is available today but was absent earlier. Perhaps, on the contrary, it is just that, with time, our empirical knowledge has become so rich and detailed that it was only thanks to this that now it became possible to make the step that scientists have always regarded as their ultimate goal: to go from what they learned about the components of variance and try to deepen their understanding by discovering how these causes produce their effects. In other words, it may be that what was holding scientists back was not their ignorance of an alleged philosophical route to bridging the explanatory divide. Rather, it was just that, until recently, empirical knowledge was not advanced enough to open the path to uncovering the details of an underlying causal story for a phenomenon under investigation.

Third, and connected with the previous point, Tabery is wrong when he criticizes behaviour geneticists because 'historically they have often ignored or black-boxed the causal mechanisms linking actual difference makers to the actual differences they made'.[1] When behaviour geneticists 'ignored' or 'black-boxed' causal mechanisms, this was not because they believed that mechanisms are unimportant. They would have loved to know more about mechanisms but they realized that the goal was not achievable at the time. Sometimes the level of knowledge in genetics and neuroscience puts limits on the search for mechanisms. What was badly needed was not philosophical theories but more empirical knowledge. Only after the recent explosion of knowledge about genes and brains could variation partitioning be complemented with a hunt for specific causal mechanisms. As soon as the newly acquired knowledge opened the path for probing deeper, scientists immediately jumped at these new and exciting research opportunities. They did not have to wait for methodological lessons or tools from philosophers. Scientists cross this kind of 'explanatory divide' in the same way that people cross the equator: without noticing that it exists.

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