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## Review symposium: A reply to the reply

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The first charge that Buchanan makes is that I ‘appeared to conflate’ (Buchanan, 2012: 140) the principles of symmetry and impartiality in David Bloor’s strong programme for the sociology of science (Bloor, 1976). It is as obvious to me as it must be to anyone else that these two principles are not the same and that this is why Bloor listed them separately. That said, they are two closely related aspects of the same programme and Bloor’s decision to list the principle of symmetry immediately after the principle of impartiality is based on his view that the former can be logically derived from the latter. As far as conflating the two is concerned, I do not know how Buchanan got this impression but I plead not guilty to the charge.

Although Buchanan dismisses my attempt to link his work to the strong programme as ‘something of a red herring’ (Buchanan, 2012: 141), he goes on to say, ‘For what it is worth, I think the book actually is symmetrical throughout. . . . The book is also, for the most part, impartial’ (ibid: 142). He also concedes: ‘The book does incorporate *elements* of the Edinburgh school’ (ibid: 141). This suggests to me that I was justified in making this link. He claims that I gave the impression that he cited Bloor, though as the choice of words indicates, I did not say that he cited Bloor.

It would have been much easier to deal with this reply if Buchanan had confined himself to discussing what I wrote. Through the use of phrases like ‘appeared to conflate’, ‘gave the impression’ and ‘appeared to suggest’ (ibid: 140), he attributes statements to me that I did not make. Another example of this practice is the claim that I ‘appeared to suggest’ that neutrality entails either the omission of any evaluations, including those of the scientists, or an equal airing of both sides in the debate. I do not know what I may or may not have ‘appeared to suggest’ but the fact of the matter is that I did not say either of those things.

With respect to the principle of impartiality, Buchanan makes a great deal out of my use of the term ‘neutral’, which he places in scare quotations throughout the piece. He rightly suspects that I borrowed the term from Eysenck’s son Michael who, on being told by Buchanan that he would not be taking sides in the controversies surrounding his

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father, asked incredulously, ‘How can anyone be neutral about my father?’ (Buchanan, 2010: 6). Although Buchanan wants to suggest that there is a world of difference between ‘neutrality’ and ‘impartiality’, anyone with a reasonable knowledge of the English language will recognize that they are synonyms. If anyone cares to look up the word ‘neutral’ in the *Concise Oxford Dictionary*, they will find the following: ‘not supporting either side in a conflict or dispute; impartial’ (‘neutral adj.’, *Concise English Dictionary*, 2008). It would be possible to substitute the word, ‘impartial’ for ‘neutral’ in my review without altering the meaning that I wanted to convey. I was simply referring to Buchanan’s claim that he tried to avoid taking sides in the controversies surrounding Eysenck, a point that he does not dispute.

Buchanan condescendingly attributes Michael Eysenck’s incredulity to the fact that he is a psychologist and cannot adopt the perspective of the historian: ‘Michael Eysenck might have wished for a more normative approach on my part – an understandable perspective for a psychologist’ (Buchanan, 2012: 146). He seems to have tarred me with the same brush. He says that I read the book ‘in a manner at odds with its content as a work of history’ (ibid: 140). He also says that the approach of giving both sides equal time which he falsely attributes to me ‘might suit ahistorical analytic purposes’ (ibid: 141) and constantly refers to ‘the general demands of good history’ (ibid: 141). His impartial stance was ‘an attempt to rise above the disciplinary factionalism that blighted Eysenck’s career’ (ibid: 145).

With the exception of a few basic norms, such as reading all the relevant literature, consulting primary sources, accuracy of citation and so on, it is wrong to suggest that historians are unanimous on what ‘good history’ is. A major point of disagreement is whether there can be any such thing as impartiality. Kurt Danziger (1997: 3) has written:

Human knowledge . . . is always *situated* knowledge, that is, knowledge obtained from a particular position and hence partial. And, in the recent words of Harvard historian Mario Biagioli (1996: 193), ‘being partial is no sin’. We cannot avoid being partial, we can only avoid owning up to it.

I made the point that it is out of a recognition that we always write from a particular position that authors like Graham Richards, in his book *Race, Racism and Psychology* (1997), begin by stating their position. Buchanan interprets this as suggesting that ‘such issues can be largely solved with a cap-in-hand declaration of the historian’s “biases”’, a view that he finds ‘simplistic’ (Buchanan, 2012: 142). I did not suggest that this practice will solve all the issues and I do not think that the writers who have engaged in this practice are so simplistic that they think it will solve all the issues either. They do it because they think that an honest statement of their position, as they understand it, is an improvement on pretending that it does not exist.

Writing history in order to gain support for a particular position within psychology is likely to result in Whig history and is not to be recommended but we are talking here about issues that go beyond inner-disciplinary concerns. As Michael Eysenck (2010) noted in his review of Buchanan’s book, his father engaged in speculation that was no less grandiose than that of Freud. Some examples are the claim that the Irish have lower IQs than the English because their best and brightest have emigrated, in many cases to England. He also suggested that African-Americans have low IQs because they were selected for dullness by slave traders.

The members of these groups find such views offensive and understandably so. As one of the small minority of working-class people who made it through the social barrier of the 11-plus, I am offended by Eysenck's suggestion that genetic differences in ability make the class system inevitable. Others might wish to challenge his view that there are 'fascists of the left' or that smoking does not cause lung cancer. These are issues with profound social implications and I consider a stance of pseudo-impartiality to be morally irresponsible. I also do not accept that taking a moral stance is incompatible with 'the general demands of good history' (Buchanan, 2012: 141).

One of the saving graces of Buchanan's book is that it is nowhere near as impartial as he likes to think. In the review by Michael Eysenck referred to above, Buchanan is described as a 'critic' (Eysenck, 2010: 736). Michael Eysenck also points out that: 'Buchanan refers to the numerous methodological shortcomings in my father's research' (ibid: 736). I quoted an example of this in my review: 'critic after critic noted that the heritability of within-group differences *could not* and *should not* be extrapolated to between-group differences. In fact, there was no logical connection between the two' (Buchanan, 2010: 289–90; original emphases). Buchanan tells us that he was not expressing his own view in the second sentence but merely reproducing the views of others and refers to a footnote in which this is allegedly shown. Curiously, he does not reproduce this footnote. I will therefore reproduce a part of it here: 'It is a logical fallacy that can be traced back to the previous century according to some writers' (ibid.: 290). The footnote does not attribute this view to anyone in particular; it is concerned with who originally made the point. When someone reproduces the views of others and goes on to say, 'In fact . . .' without attributing it to anyone else, I take it that he is expressing his own view and I am sure that every other reader would do the same. The use of the term 'logical fallacy' without scare quotation marks underlines this point. I suggest that, in spite of what he is now claiming, Buchanan was expressing his own view.

It is too easy to get bogged down in the discussion of a single quotation. My point was that Buchanan does take sides in the debates surrounding Eysenck's work. On the same page as the quotation referred to above, he discusses the heritability of IQ:

Amongst human populations, height can be cited as a similar example. This distinctly heritable attribute displays significant differences between different ethnic groups and/or regions. But are these differences genetic? Maybe, but pronounced increases in height have occurred over the past few generations, more in some groups and regions than others. These increases are probably due to environmental factors like better nutrition and less disease. (Buchanan, 2010: 290)

There is no question of this view being attributed to anyone else. It is clearly Buchanan's own. Many other examples could be given. Why else would Sybil Eysenck ask that the disclaimer 'The views expressed in this book are not shared by me' be added to the book? Presumably, she was not referring to the views of Eysenck's critics that are reproduced in the book but to the views of the author himself. Indeed, Buchanan acknowledges that he was only impartial 'for the most part' and that his own voice comes through at various points in the book. As Michael Eysenck noted in his review, that voice when it does appear is usually critical. I pointed this out in my review and suggested that the book was much stronger for it.

Buchanan devotes a great deal of space to my suggestion that he had failed to account for Eysenck's 'popularity' (Buchanan, 2012: 142). With hindsight I accept that the word was an unfortunate choice. It will be obvious to anyone with only a superficial knowledge of Eysenck's career that he was a divisive figure who was not universally popular. A better choice would have been 'eminence'. It seemed to me that Eysenck's substantive contributions to psychology, which centre on his three-factor theory of personality, were meagre compared to the esteem in which he was held by his fellow psychologists and that Buchanan had not provided an adequate explanation for this situation. It is certainly true that he was shunned by the British establishment but the American Psychological Association and the American Psychological Society showered him with honours and awards. He was also the third most cited psychologist in the world after Skinner and Freud, which is no small achievement, even if we allow for the fact that such statistics can be manipulated. Buchanan suggests that I attributed this situation to Eysenck's role as a popularizer:

Brock then seemed to attribute whatever esteem Eysenck had earned from his peers to his success as a popularizer. For Brock, Eysenck's unexplained 'popularity' could be sourced to (and to some extent equated with) the connection that Eysenck made with the general public on behalf of the discipline. But oddly enough, the insights Brock offered to fill in this alleged explanatory gap in *Playing with Fire* appeared to draw directly from it. (ibid: 142)

He then uses copious quotations to show that Eysenck's role as a popularizer was covered in the book and concludes triumphantly:

There is little in Brock's paragraph not present in *Playing with Fire*. Brock cannot claim such material is not in the book, nor can he claim that it was not used to explain Eysenck's unique influence and standing. (ibid: 144)

All this is based on a misunderstanding. I did not suggest that the esteem in which Eysenck was held among psychologists could be attributed to his success as a popularizer. My point was more abstract than that. I suggested that it could be explained by the fact that he was a disciplinary chauvinist who worked tirelessly on behalf of the profession of psychology and I mentioned his success as a popularizer as an example. Another example that I gave was his strident opposition to psychoanalysis throughout his career and his promotion of behaviour therapy. Psychoanalysis had been created by medical doctors and was largely the preserve of psychiatrists, while behaviour therapy was based on psychological principles and was largely practised by psychologists. This connection is not made in the book and the fact that Buchanan did not make the connection even after reading my review underlines the point.

Much of the reply is devoted to a defence of the genre of biography. Buchanan writes:

... biography provided an invaluable basis for assessing the accusations and innuendo that had swirled around Eysenck and occupied some critical observers for decades. Only a biographical perspective could assess these issues in the context of his whole career. ... Only a biographical perspective could piece together a lifetime of recurring contradictions. (ibid: 145)

This is merely stating the obvious. If we are interested in the career of an individual scientist, it would make sense to take a biographical approach. However, if we are interested in scientific practices or scientific objects, it would be appropriate to take a different approach. These are complex issues which take us beyond a review of this particular book and I believe they require a separate treatment. Suffice it to say that I have never dismissed the genre as worthless. I simply think that the interesting questions in history are to be found elsewhere.

Buchanan opens the final paragraph by suggesting that I claimed he had ‘inadvertently produced something of value’ (ibid: 146). I made no such claim. I pointed out that Buchanan is nowhere near as impartial as he thinks and that the book is much stronger for it. I made no comment on whether or not the book would have had any value if he had attained this impossible goal. Yet another misunderstanding occurs in the final sentence where Buchanan says what was missing from my review was a convincing explanation of why I thought it was a good book. Such an explanation was missing but what was also missing was the statement that I thought it was a good book. I made several positive comments in the review and this makes me wonder why it has led to such a vitriolic response. I said, for example, that the book was a valuable contribution to the literature, given that the only accounts of Eysenck’s life and work that we previously had were the different versions of his autobiography and a hagiography by Tony Gibson. I also said that it was a scholarly work and that it had been thoroughly researched. However, at no point did I say that I thought it was a good book. This is simply an inference that Buchanan has drawn from the positive comments that I made.

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