



Review symposium: a reply

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Roderick D. Buchanan, *Playing with Fire: The Controversial Career of Hans J. Eysenck* (Oxford: Oxford University Press, 2010), ISBN 978-0-19-856688-5 (hbk), 475 pp., 21 illustrations, £34.95/US\$65.

Playing with Fire is a new biography of Hans J. Eysenck. As author, I was honored to have my book made the subject of a review symposium in this journal (Brock, 2011; Hall, 2011). I am grateful for the opportunity to add to this symposium here. As Hall (ibid.) commented, the project *was* rather unusual in the way it was organized, with an Australian historian working at a Dutch university engaged to write a biography of a German-born British psychologist. I have Trudy Dehue and Maarten Derksen at the University of Groningen to thank for setting up the project, and for giving me complete autonomy to pursue it. The strengths and weaknesses of the book are therefore my responsibility alone.

At the front of *Playing with Fire* is an unusual statement from Hans Eysenck's widow, Sybil Eysenck. It reads: 'the views expressed in this book are not shared by me'. This disclaimer has generated considerable comment and speculation (e.g. Rushton, 2010; Brock, 2011; Winston, 2011). Although some reviewers have interpreted the disclaimer as vitiating the book's contents, it was *not* included as part of any agreement or legal requirement. I had a series of interviews with Sybil Eysenck and subsequently offered her the opportunity to read the manuscript before publication. While she did not dispute any specifics, she did not care for the approach I had taken and asked to have the disclaimer inserted. I felt a bit saddened by this. In contrast, my publishers saw this as a back-handed compliment to my scholarship. Including the disclaimer would also assure readers that this was not authorized hagiography, which would help promote the book. But it could well

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have been left out, for there was no other basis for bargaining. There were no family-controlled personal archives to negotiate over and the book was otherwise thoroughly legally vetted. What one makes of the disclaimer depends on what authority you give its author as a critic of a serious work of history that happens to centre on her late husband.

I have little to add to Hall's (2011) glowing assessment beyond a gracious 'thank you'. Conversely, Brock (2011) appeared to read *Playing with Fire* in a manner at odds with its content as a work of history. First, Brock interpreted the Strong Programme as central to the way I dealt with my polarizing biographical subject. However, in assessing the book in these terms Brock appeared to conflate the programme's symmetry principle with what David Bloor termed the impartiality principle (which Brock linked to the notion of 'neutrality'). While the principles of symmetry and impartiality are not unrelated, they are different. Here is the explanation Brock gave:

What is he [Buchanan] going to offer as an alternative to these hagiographies and hatchet jobs? The principle of symmetry as outlined by David Bloor (1976) in his strong programme for the sociology of science. According to this principle, the author should stay neutral with respect to scientific controversies so that the views of both sides can be understood and explained. (Brock, 2011: 109)

Brock's version of symmetry resembled an approximate definition of Bloor's impartiality principle. Bloor stipulated that one must remain impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies require explanation. The impartial historian's task is not to judge but to explain the basis of scientists' judgements (Bloor, 1976: 7). However, in the example Brock later gave he appeared to suggest this concept entails an airing of 'both sides' of a debate: either, without *any* kind of evaluations from anyone, including the scientists; or, with an equal airing of scientists' evaluations pro and con., with the historian presumably staying 'neutral'. The example Brock provided of the book's lack of impartiality (wherein he suggests the book is not symmetrical) was:

As for the principle of symmetry, it seems to have been forgotten somewhere along the way since the book is quite literally peppered with evaluations of Eysenck and his work and these are usually of a critical nature. We are told, for example, that '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' ... (Brock, 2011: 109)

This quote from of *Playing with Fire* (289–90) amounted to one complete sentence, and a portion of the next. It was part of a lengthy passage representing how scientists reacted to Eysenck's race and IQ book. By itself this quote implied nothing in relation to Bloor's symmetry principle, which stipulated that truth and falsity, rationality or irrationality, success or failure, should be explained in the same terms (Bloor, 1976: 7). However, this quote did offer a good example of impartiality in action. The 'no logical connection' criticism being discussed was an attributed viewpoint, not mine – the preceding phrase

‘critic after critic’ should have made that clear. (According to these critics, it was this lack of logical connection that forbade extrapolation . . .) Moreover, *both* these *linked* sentences had footnotes detailing the provenance of this criticism that did not appear in Brock’s quotation. The first sentence had a footnote attributing this criticism to its most famous exponent, Richard Lewontin, with a lengthy explanation of Lewontin’s oft-quoted seed example that first appeared in his 1970 ‘Race and Intelligence’ article. The second sentence had a footnote tracing the origins of this criticism back to Lancelot Hogben in 1933. A section of this quote was emphasized because this criticism was recognized as crucial at the time, frequently cited as a stumbling block to inferring the genetic basis of between-group differences based on within-group heritability measures. I am not sure whether Brock used this quote because he thought it represented my opinion. If so, then it was simply a misunderstanding. While the wording of the second sentence might have been better, the context in which it appeared and the attributions given made its historical status unambiguous.

Conversely, Brock may have used this quote because it conveyed an evaluation, one of the many assessments of Eysenck’s work I cited. It was either the presence of these judgements per se or their allegedly unbalanced pro versus con. presentation that Brock saw as contravening Strong Programme principles. Whether any account conforms to a particular scholar’s version of the Strong Programme is always going to be contestable. Endless debate in science and technology studies (STS) attempting to reconcile the perspectives of history, sociology, philosophy and the sciences themselves has seen to that. However, what is at issue here goes beyond the Strong Programme; it goes to the heart of history as a representational discourse. I would steadfastly argue that the quote Brock highlighted was part of a faithful rendering of reactions to Eysenck’s book as they occurred, that there were in fact many negative assessments of Eysenck’s position in that particular controversy and in many others. It was one way of writing good sociologically informed history. It amounted to a sympathetic and accurately weighted representation of the actors’ voices – the interventions, arguments *and* judgements of the scientists in accordance with their role in a controversy. Conversely, an equal time organization of ‘both sides’ of what were complex, subtly shaded and/or lopsided debates would have been far more problematic. Irrespective of the dictates of the Strong Programme, balancing the account in this manner would be quite misleading, a contemporary imposition sacrificing subtlety and dynamic context while over-compensating the underdog or losing position(s). Such an approach might suit ahistorical analytic purposes but it would inhibit our understanding of science-as-it-happened.

In any case, I think focusing on whether my book conforms to the Strong Programme is something of a red herring. I had other more pressing theoretical concerns. Nonetheless, in the Introduction to *Playing with Fire* I offered a brief caution against asymmetric Whiggish accounting and the dangers of presentism. I also put a short case for why the scientists should be allowed to speak for themselves, with a caveat regarding the kind of information and perspectives that only the historian might have. These comments were directed toward audiences not familiar with basic science historiography. Contrary to the impression Brock gave, Bloor was never cited, the Strong Programme never mentioned. The book does incorporate *elements* of the Edinburgh School, but only insofar as they overlap with the general demands of good history.

To my mind, the key issue is whether the book is internally consistent with the goals set. In the Introduction to *Playing with Fire* I explicitly positioned myself between Eysenck's supporters and critics, and situated the book as a dialogical counterpoint to the hagiography of Gibson's biography and Eysenck's autobiography. For what it is worth, I think the book actually is symmetrical throughout. For example, the competing claims of Eysenck and his nemesis Leon Kamin are situated in the same kind of intellectual cum ideological explanatory framework. The book is also, for the most part, impartial – bearing in mind the caveats I foreshadowed. For *much* of the book I just let the scientists speak. My voice does come to the fore in places, especially towards the end of the book in chapter 9 and the Conclusions. However, I never set out to be 'balanced' or 'neutral' in the way Brock imagined I should be.¹ The book is indeed situated between pro and con Eysenck camps, and is set against previous biographical resources and Eysenck commentary. This is not inadvertent, as Brock implied, but entirely deliberate. Finally, Brock alludes to the thorny issues of praxis and values at the frontiers of STS generally, suggesting such issues can be largely solved with a cap-in-hand declaration of the historian's 'biases'. While this strikes me as far too simplistic, that debate might best be left for another day.

Another misunderstanding was Brock's (2011) suggestion that the book did not give a convincing explanation of why Eysenck was so popular. It is not entirely clear what Brock meant by 'popular'. At first he seemed to equate popularity with the respect and admiration of Eysenck's scientific peers. Brock claimed Eysenck was showered with honours, citing the belated American awards Eysenck received listed in the Conclusions of my book (425). But as I also pointed out, virtually no honours came from the UK where Eysenck could not even attain honorary status in the BPS. Eysenck was definitely *not* unambiguously popular with his peers, especially in his adopted homeland. The high citation rate Brock also mentioned is an equally ambiguous index of popularity in this sense. Citation counts provide a crude measure of impact, but not necessarily of respect and admiration.² The best example of this would be Eysenck's notorious 1952 article on psychotherapy – his most cited work. It appeared to achieve this status largely by antithesis, habitually cited by an emerging research field keen to dispute what were seen as its outrageous claims. So as I spelt out in *Playing with Fire*, Eysenck clearly had his circle of admirers. They saw fit to honour him and offer him tributes. But he also had plenty of detractors, some of them very prominent figures. They distrusted and even loathed him. Attempting to explain these contradictions of reputation and effect was the crux of my book, as set out in the opening pages of the Introduction, and is the basis upon which it ought to be assessed.

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 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. It is not unusual for reviewers to paraphrase a book's content in a way that glosses over where the ideas are coming from and who is doing the talking. However, Brock appeared go further than this. Here is what Brock wrote:

My guess, and it is only a guess, is that although Eysenck was a fractious individual who often engaged in fights with other psychologists, he was a tireless warrior on behalf of psychology

itself. The series of popular books that he wrote helped to raise the profile of psychology in the wider society. There are many people with degrees in psychology, myself included, whose initial acquaintance with the subject was through these books. The furore over race and IQ went even further in raising the public profile of psychology. Eysenck wrote for popular magazines as diverse as *Reader's Digest* and *Penthouse* and was a regular feature on radio and television, both in Britain and elsewhere. For many people, he was 'Mr. Psychology' and he was often recognized by taxi drivers, shop assistants and the like. (Brock, 2011: 111–12)

And here is where very similar material appeared in my book, *Playing with Fire*:

With few competing titles, Eysenck's Pelican paperbacks came to represent much of what the British public understood about psychology in the 1950s and 1960s. Initially at least, Eysenck's peers welcomed the popular arm of his writings. While these books helped sell Eysenck and his point of view, they also helped promote psychology as a discipline. Popularization helps create cultural space for any given scientific discipline, enrolling sympathetic patrons in government and industry, as well as promoting it to a lay audience of potential consumers. Popularization helps give identity and purpose to any given research community, feeding images of what they are supposed to be doing and why. Most prosaically, it helps recruit new workers. Eysenck's popular books turned countless would-be psychologists on to the discipline generally, as well as attracting students to the IoP [Institute of Psychiatry]. Eysenck recalled how many people told him his Pelican books were their first taste of psychology, piquing their interest as youngsters in career-molding ways. Many psychologists I have met in the course of this project – some associated with Eysenck, others not – told me the same thing. (267)

But also here:

... his seemingly compulsive output, with its high to lowbrow appeal, becomes much more intelligible. 'Mr. Psychology' has to have an across-the-board strategy. ... He was a star in the new media and the old – all of which tended to intensify the demands and effects of his celebrity. (11)

And here:

... a sizeable chunk [of Eysenck's writing] was made up of replies, rejoinders, and corrections. In addition, he still found time to write for middle-brow periodicals like *Encounter* and *The Listener*, as well as the likes of *Reader's Digest* and *Penthouse*. He also wrote many newspaper articles and letters to the editor – writings that seldom appear in any Eysenck bibliography. (9)

And here:

Eysenck remained defiantly upbeat about the effects the [race and IQ] controversy had on his reputation. He saw it as legitimating his status as Britain's 'Mr. Psychology', despite the harassment he had to endure. He became, he thought, the everyman-rebel the public could identify with, an outsider they could rely on to represent the real message of his discipline. (356–7)

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 it was not used to explain Eysenck's unique influence and standing.³

More generally, reviewers have tended to respond to *Playing with Fire* according to their attitude toward biography. The genre has experienced contrasting fortunes in various fields of late. For example, the biographical approach *has* been rehabilitated in the history of science, demonstrably reconciled with context-driven history.⁴ Conversely, the genre is still rather suspect in the history of medicine, where Great Doctor narratives have been rather forcefully shown the door (Linker, 2007). However, I have yet to discern either suppression or rehabilitation in the history of psychology. As Brock (2011) hinted, biographies of psychologists have been, with a few exceptions, somewhat uncritical toward their craft and not terribly well done. There are far too many potted accounts.

It was never my intention to write a heavy-handed theoretical primer. I was keener to simply demonstrate the possibilities the biographical genre had to offer. Nevertheless, I think some reviewers have done an excellent job in situating my book in this sense (e.g. Pettit, 2011). I wanted to write as complete an account of Eysenck's professional life and times as *practically* possible while filling in some of the gaps in the annals of British psychology – especially the rise of personality research in the UK mid-century and the development of clinical psychology. But given that Eysenck covered so much ground, I had to make some unenviable choices about what to leave out. Even though Sybil Eysenck had disposed of her husband's personal papers, I still managed to locate a considerable quantity of Eysenck's memos, notes, letters and research reports in other archival collections. This left me with good source material in some areas but not others. These source issues guided most of my choices, but a range of other background considerations also came into play. For example, Eysenck's work on crime and personality was only briefly mentioned because it had been recently covered by others (e.g. Rafter, 2006). I avoided astrology and parapsychology because the pattern of Eysenck's involvement was similar to other areas I did cover (e.g. smoking and cancer). Although I did touch on the vitamins and intelligence fiasco, his work on genius and creativity was also left out. It amounted to only one book and a handful of articles after he retired – not a particularly significant topic in the context of Eysenck's whole career.

Historians of science have come to value biography as a way of exploring the cultural identity of the scientist's role, connecting the individual to a social and institutional level of analysis. Prompted by a range of scholars (e.g. Porter, 1995; Daston, 2001; Gross, 1990; and Soyland, 1994) I focused on Eysenck's enacted identity. While Eysenck advocated a strongly quantitative, de-personalized version of psychological science, he demonstrated a quite selective commitment to the standards such an approach ostensibly entailed. Instead, much of Eysenck's prodigious output turned largely and uncomfortably on the authority of his personal judgement. Talking to multiple audiences at once, he asked them all to accede to the heightened sense of trust this standpoint demanded. In Eysenck's hands, science became an individualistic, adversarial, zero-sum game. Empirical rationality was not so much a communal virtue but a proprietary tool ruthlessly deployed to win.

Focusing on Eysenck's enacted identity helped tease out a predictable pattern of intervention and disputation because it took as its primary object the doing of science.

Rather than attempting to divine psychobiographic clues to the inner man, I framed questions of motivation largely in terms of visible engagement. Such an approach made it clear why Eysenck was never hugely popular with his peers, and never wholly accepted as a public spokesperson for his discipline either. Moreover, it provided for a *nuanced* appreciation of the man and his sociopolitical context.

Brock's (2011) review cited my treatment of Eysenck's politics to illustrate what he saw as the perils of the biographical genre. I certainly never took Eysenck's apolitical stance at face value, as Brock claimed. Eysenck may well have maintained that science and politics were separate, but as I discussed in the book he kept to this only when it suited him, only on his own terms. The questions I canvassed were referenced to a long and often quite speculative commentary. At risk of oversimplifying a very detailed account, I did show that Eysenck had overt political sympathies – some of which Brock cited – although they were inconsistent, hard to read, and seemed to shift over the years. Nevertheless, Eysenck's political sympathies did not *appear* to stem from some kind of hidden political agenda, certainly not from a personal involvement or direct engagement with a particular political party or cause. Across his career Eysenck had shown an impatience with bureaucracy and a disinterest in organizational politics. He was too pre-occupied with his own extensive scientific interests to be an extramural activist or 'joiner'.⁵ As he matured, the only organizations Eysenck took a great interest in were those he helped found, those created in his own image. However – and this is where nuance is important – that should not be read as some kind of blanket apologia. Eysenck might not have been quite the crypto-fascist some fingered him as, but he was still quite wilfully blind to the broader sociopolitical effects that his views had. He still allowed himself to function as an iconic, legitimating figure for a resurgent right, emboldening racist elements and encouraging a kind of retro-nativistic psychological science. Those sociopolitical effects were indeed explored, contrary to the impression Brock gave. And those Eysenck gave succour to, those who violently opposed him, they were not left in the shadows either. Nevertheless, I hardly think I will have the last word on Eysenck's politics. New evidence may well emerge to fill in the gaps and paint a somewhat different picture.

Far from leading us astray, 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 – demonstrating how, for example, his commitment to racial research was quite limited before he intervened on Jensen's behalf, and how *relatively* restrained his response was in the brouhaha that followed. Only a biographical perspective could piece together a lifetime of recurring contradictions. While the genre undoubtedly has its limitations, we should be open to the opportunities it affords.

Playing with Fire was, above all, an attempt to rise above the disciplinary factionalism that blighted Eysenck's career. The reactions my book has generated illustrate just how divisive the man still is. I see little value in reviewers citing either pro or con. Eysenck camps to demonstrate my book's fairness or otherwise. Doing so serves little other than partisan ends – a case in point being Brock's use of J. Philippe Rushton's (2010) review to support the notion that I was not 'neutral'. As well as being an Eysenck acolyte, Rushton has a vested interest in the question of racial IQ differences. Perhaps the

most notable thing about Rushton's review was that he did not quarrel with the way I represented the debate on these questions, or any other technical issue.

I appreciate all the attention *Playing with Fire* has received, even if some of it is a little puzzling. Brock claimed I had inadvertently produced something of value because I was not 'neutral'. But given his reservations, what seemed missing from Brock's review was a convincing explanation of why he thought *Playing with Fire* was a good book.

Notes

1. I have struggled to understand how this misunderstanding about 'neutrality' came about. I suspect it stemmed from a confusion of attribution. Only when Hans Eysenck's son Michael was quoted did the word 'neutral' appear in *Playing with Fire* (6) with something like the meaning Brock ascribed to me. 'How could *anyone* be neutral about my father?' was Michael Eysenck's incredulous reaction when I told him I would not be 'taking sides' but instead trying to explain the divergence of opinion about Hans. If anything, Michael Eysenck might have wished for a more normative approach on my part – an understandable perspective for a psychologist. See M. Eysenck (2010).
2. A good deal of Eysenck's high citation count stemmed from his astonishing productivity and the success of his MPI/EPI/EPQ tests as research instruments – both covered in *Playing with Fire*.
3. I have two small caveats regarding the similarities of the two texts. First, the part about taxi drivers and shop assistants in Brock's paragraph apparently derived from one of Eysenck's own accounts. Second, I never claimed Eysenck was a regular feature on TV and radio *outside* Britain as Brock appeared to claim would have to be greatly qualified to be accurate.
4. In his account of scientific biography's changing fortunes, Thomas Söderqvist noted that by the 'turn of the millennium, no serious historian of science rejected the genre – as long as it contributed to a socially and culturally informed history of science' (2007: 257).
5. Eysenck's politics were never far from his own personal scientific agenda. For example, Eysenck's public condemnation of Arthur Scargill *et al.* during the crippling miners' strike of the 1980s appeared to be motivated more by a need to vindicate his notion of left-wing authoritarianism than by a political affinity with the Tory government of the day. As I pointed out (357), Eysenck was actually quite ambivalent about Thatcher and her policies.

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