

Essay Review

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WALLACE REDUX?

Ross A. Slotten, *The Heretic at Darwin's Court: The Life of Alfred Russel Wallace* (New York: Columbia University Press, 2004), vii + 602, pp., ISBN 0-231-13010-4

Martin Fichman, *An Elusive Victorian: The Evolution of Alfred Russel Wallace* (Chicago: University of Chicago Press, 2004), x + 382, pp., ISBN 0-226-24613-2

For much of the twentieth century, Alfred Russel Wallace seemed almost forgotten. Amabel Williams-Ellis's biography was one of the few, and its title – *Darwin's Moon* – rather neatly encapsulated poor old Alfred Russel: a mere satellite of Darwin, only visible in the great man's reflected light.¹ Practically the only relevant fact that most could recall was, that in sending his paper to Darwin, describing his independent discovery of natural selection, Wallace prompted Darwin to abandon his 'big species book' and produce the slim (by Victorian standards) 'abstract' of his theory that became *On the Origin of Species*. Historians interested in Darwinism also knew that Wallace went off the evolutionary rails in his later years, embracing spiritualism and denying that natural selection alone could have created anything as extraordinary as the human mind. Appalled by his co-discoverer's change of heart, Darwin wrote to him, 'I hope you have not murdered too completely your own & my child'.²

During the late 1990s, Wallace began, in biographical terms, to emerge from Darwin's shadow, becoming the subject of a modest flurry of interest. Beginning with Peter Raby's *Alfred Russel*

¹ Amabel Williams Ellis, *Darwin's Moon: A Biography of Alfred Russel Wallace* (London, Blackie, 1966).

² Charles Darwin to A.R. Wallace, 27 March [1869], in Francis Darwin and A.C. Seward (eds.), *More Letters of Charles Darwin* (London: John Murray, 1903), vol. 1, 312.

Wallace: A Life, published in 2001, Wallace became a minor 'industry' in 2002, with the publication of Andrew Berry's *Infinite Tropics: An Alfred Russel Wallace Anthology*; Jane Camerini's *The Alfred Russel Wallace Reader*; and Michael Shermer's *In Darwin's Shadow: The Life and Science of Alfred Russel Wallace*.³ The following year saw William Bryant's *Naturalist in the River: The Life and Early Writings of Alfred Russel Wallace*, and these are now joined by Ross Slotten's *The Heretic at Darwin's Court* and Martin Fichman's *An Elusive Victorian*. One would think that we are now in a position to understand Wallace's life, his work, his ideas, and his lasting importance to the history of science.

Despite these new books, for reasons that are important, Wallace seems as elusive as ever. Two problems confront any Wallace biographer. The first is that – by comparison with Darwin – the sources are thin, a fact that leads most writers to rely too heavily upon Wallace's autobiography, *My Life* (1905). Nonetheless, the Wallace archive is substantial. Britain's National Register of Archives lists eighteen collections of Wallace papers, in eight different repositories; and Charles H. Smith's superb Wallace website lists many more, scattered across the globe – from the American Philosophical Society to the National Library of Australia.⁴ James Moore and John van Wyhe are currently producing a comprehensive calendar of his correspondence (including hundreds of newly discovered letters), which should help make these scattered resources accessible to scholars. In the meantime, there are the latest books by Slotten and Fichman.

A WALLACE-EYE VIEW

One immediate contrast between these works lies in the fact that Slotten has consulted about twice as many archives as Fichman, who has concentrated instead on Wallace's published works. This has important implications. From the geographical distribution of animals to the redistribution of land, from understanding insect mimicry to communicating with the dead, no subject seems to have been foreign to Wallace. Modern scholars, constrained by their disciplines, find his dizzying range hard to comprehend, much less to

³ See Jim Endersby, 'Escaping Darwin's Shadow', *Journal of the History of Biology*, 36 (2), (2003), 385–403.

⁴ See <http://www.wku.edu/~smithch/index1.htm>, accessed 18 May 2005.

integrate. This is a problem whose traditional solution, as Fichman notes, has been simply to ignore Wallace's 'non-scientific' interests – from socialism to spiritualism. However, this approach can lead to anachronistic classifications of interests that are too conveniently labelled 'non-' or (worse still) 'pseudo-' scientific. Fichman tries to avoid such traps by taking all of Wallace's enthusiasms seriously, and by constructing a coherent picture of their relationship to one another. However, such success as he enjoys comes from relying rather too heavily on Wallace's own (and retrospective) sense of how his ideas were connected.

Fichman gives us a 'Wallace-eye' view of an extraordinary career. And while that is preferable to the partial and anachronistic views he is seeking to replace, the result is a little disappointing. In recent decades, the history of science has been transformed by new approaches to the study of scientific communities, in preference to isolated individuals; to contextualizing careers, by relating them to social, political, and economic history; and to examining the spread of ideas through writing, publishing, and reading. Perhaps most important, the history of theories is being complemented by a history of practices, and by the analysis of tools and techniques that have formed and disseminated concepts.

Regrettably, this rich ferment of sociologically informed cultural history seems to have escaped Fichman's notice (except as a source of perfunctory background material). What he offers instead is good old-fashioned history of ideas. Wallace emerges as a 'great man' (almost a lone genius), who has some great ideas that are holistically connected to his other, not-so-great ideas, but in interesting ways. Examining these connections allows the diligent reader to understand the grand vision that inspired him; or at least the grand vision that, with the benefit of hindsight, Wallace believed had inspired him. Wallace's world view, as Fichman notes, shared similarities with other evolutionary cosmologies, most notably that propounded (anonymously) by Robert Chambers in his *Vestiges of the Natural History of Creation*, and with the profound (or at least, profoundly verbose) works of Herbert Spencer.

Fichman attempts to combine intellectual and contextual history, but my sense is that his particular hybrid is likely to disappoint both proponents of the contextual and fans of the traditional. Even if one regards ideas as the essence of science (or of an individual's world view), one may still wish not only to compare Wallace's views with those they most clearly resemble – such as

Chambers' and Spencer's – but also to contrast them with other evolutionary narratives, such as those of Lyell and of Darwin. Fichman notes that the scientific naturalists of Wallace's day were unimpressed by his teleological vision, with its heavy freight of ethical claims and spiritual assumptions, but he does not tell us why. My suspicion is that this question is unanswerable without delving more deeply into the socio-historical contexts of competing narratives. But if an alternative, idea-centric explanation is possible, Fichman has failed to provide it.

WALLACE THE HERETIC?

Fichman's book is not, as he acknowledges, intended to be a biography in the strict sense, but rather a thematic study of Wallace's ideas and their interconnections; he recommends Raby and Shermer to those who simply want the life story. Slotten's biography appeared too late for Fichman to recommend, but I would certainly do so to anyone who wants a lucid, thoroughly researched, and readable account of Wallace's life. However, despite this recommendation, I am compelled to echo the advice the young Darwin was given by his mentor John Stevens Henslow, concerning Charles Lyell's *Principles of Geology*: read it, 'but on no account ... accept the views therein advocated'.⁵ For while Slotten's facts are generally reliable, he is rather less sure when it comes to analysis and interpretation.

Slotten's approach is built around four major themes: 'class relations, shifting paradigms, the nature of scientific research, and science versus religion' (p. 5). There certainly were, as Slotten argues, important class differences between Darwin and Wallace, which clearly influenced their careers, but this is scarcely news. More contentious is his use of Thomas Kuhn's language of scientific revolutions as a vehicle of interpretation. In Slotten's view, Wallace was a classic Kuhnian 'interloper': a young outsider who was sufficiently unfettered by the preconceptions of the prevailing paradigm to overturn it and forge a new one – hence Slotten's eye-catching decision to brand him a 'heretic'.

One problem with this approach is that *The Structure of Scientific Revolutions* is almost half a century old, and its central ideas

⁵ As recalled by Darwin, in Michael Neve and Sharon Messenger (eds.), Charles Darwin, *Autobiographies* (Harmondsworth: Penguin, 2003), 59.

were significantly revised by Kuhn during his own lifetime. Slotten seems uninterested in these revisions.⁶ Moreover, the analytical usefulness of Kuhn's concepts (even in their revised form) has been disputed so often that historians are now rather suspicious of any explanations revolving around such broad notions as 'paradigm shifts'.

Slotten's confident assertion that 'the old paradigm in nineteenth-century Western science was creationism' tends to confirm these suspicions, not least because for something to count as a paradigm (in any of Kuhn's senses), it has to be much more than a big idea shared by a lot of people. Kuhn's protean concept is far more complex, as he makes clear in his later introduction of the terms 'disciplinary matrix' and 'exemplar' to capture just two of his original meanings of 'paradigm'.⁷

It is also more than a little problematic for Slotten's argument that the term 'creationism' was rarely employed during the nineteenth century. Indeed, its first appearance in a scientific context came only in 1880, when Darwin's American champion, Asa Gray, used it in a lecture 'On Natural Science and Religion', delivered to the theological school at Yale. As Gray's usage made clear, 'creationism' (in the sense that Slotten uses it) was a term coined to discuss the religious implications of Darwin's ideas. The word became widely used in America only in the late twentieth century by Christian fundamentalists opposed to the teaching of evolution in schools as an established scientific explanation of the origin of species. To describe creationism as the prevailing paradigm of pre-Darwinian science suggests an extensive ignorance of the history of both science and religion – as well as of Kuhn's various definitions of the term. Indeed, one can no more describe something as amorphous as 'Darwinism' as a 'paradigm' in any of Kuhn's later, more sophisticated senses, than one can use the term to describe 'creationism'. Yet, Slotten seems happy to do both.

However, we do not need to see the Wallace-Darwin thesis as a Kuhnian revolution to assess Slotten's argument that Wallace's status as an outsider increased his willingness to embrace radical-

⁶ One can trace substantial revisions in Kuhn's thought from the first (1962) edition of *The Structure of Scientific Revolutions*, to the second edition (Chicago: University of Chicago Press, 1970). However, the essays by Kuhn collected together in James Conant and John Haugeland (eds.), *The Road Since Structure* (Chicago: University of Chicago Press, 2000), present even more thorough-going revisions to such key concepts as 'paradigm shift'.

⁷ Kuhn, *Structure*, *op. cit.* (note 6), 182.

ism. Indeed, a similar claim has been put forward by Shermer, although he relies on tendentious psycho-biography rather than Kuhn to support it.⁸ Yet, if this argument is correct – and outsiders certainly do make breakthroughs – it becomes difficult to explain how Darwin came to contribute to the ‘revolution’. This position is especially problematic for Slotten, who is anxious to portray Darwin as a scientific insider, with friends and supporters in high places to advance his claims. Hence, he argues, one of the ‘reasons for Wallace’s obscurity’ is that:

He was a field biologist, not a laboratory scientist. Although Darwin spent five years as a travelling naturalist, his reputation was built mainly on his extraordinary experimental work. For nearly forty years he rarely left home, confining himself to the laboratories he had set up in his office and extensive gardens at Down House. His painstaking and methodical approach – the accumulation of facts and the obsessive testing of his hypotheses against those facts – elevated biology into a true science (p. 6).

There emerges a beguiling contrast between Wallace, suffering in the field, and a hypothetico-deductive Darwin, labouring away in the comparative luxury of pastoral Kent. But this fantasy reveals nothing except Slotten’s ignorance of the world that Darwin and Wallace shared. Darwin had nothing that resembled a modern laboratory. What he had was a modest study, a greenhouse, children who helped him, friends who argued with him, and a worldwide network of correspondents. He certainly did experiments, but they were attacked by ‘real’ biologists, such as the German Julius Sachs, who possessed a university position, and a modern, well-equipped laboratory to go with it. Men like Sachs increasingly came to regard Darwin as a bumbling amateur, a holdover from the pre-laboratory world of natural history – a perception that helped fuel scepticism about natural selection during the later decades of the nineteenth century.⁹

In his own day, Darwin’s reputation was built on the success of his *Journal of Researches* (the *Voyage of the Beagle*, as it is now known). And he was careful to promote the image of a seasoned traveller – just as Wallace had done. In fact, the *Origin* begins with

⁸ Michael Shermer, *In Darwin’s Shadow: The Life and Science of Alfred Russel Wallace* (Oxford: Oxford University Press, 2002).

⁹ Soraya de Chadarevian, ‘Laboratory Science versus Country-House Experiments: The Controversy between Julius Sachs and Charles Darwin’, *British Journal for the History of Science*, 29 (1), (1996), 17–41; Garland E. Allen, ‘Naturalists and Experimentalists: The Genotype and the Phenotype’, *Studies in the History of Biology*, 3 (1979), 179–209.

the words: ‘When on board H.M.S. *Beagle*, as naturalist, I was much struck with certain facts’.¹⁰ There is no doubt that Darwin’s barnacle-like attachment to Down House, which kept him close to London’s centres of influence while maintaining his cherished privacy, made it possible to build a reputation unlike anything to which Wallace could aspire. And the stay-at-home Darwin had both the money and the opportunities to pursue his researches in ways that the nomadic Wallace could not.

However, I am not persuaded that Slotten’s contrast, in terms of ‘the nature of scientific research’, does justice either to the differences between the two men, or to their lives and work. Some sense of the limited careers open to natural scientists – and comparisons with scientific contemporaries Thomas Huxley and Joseph Hooker – could have illuminated the contrast. How was it, for example, that Huxley, coming from circumstances every bit as humble as Wallace’s, was able to turn his sojourn aboard HMS *Rattlesnake* into positions of increasing respectability and, ultimately, political power? Adrian Desmond and Paul White have given subtle and rich analyses of Huxley’s career, which provide instructive contrasts with Wallace’s. White, for example, has shown how Huxley slowly but successfully manoeuvred to make himself a spokesman for a rising generation of paid men of science. In the process, he succeeded in identifying himself with the cause of what might be called ‘scientific naturalism’ (although, as White argues, the term is problematic) and its usefulness to empire and industry. Eloquence was perhaps Huxley’s greatest asset, and his lectures and books established him as *the* exemplary man of science. By contrast, Wallace’s table-tapping and land nationalization appealed to a much narrower, less influential, audience, and were far less likely to win support from the hard-headed industrialists and educational reformers who listened whenever Huxley spoke.¹¹

Joseph Hooker had a well-connected (if not very wealthy) father to advance his career. His succession to the directorship of the Royal Botanic Gardens, Kew, in 1865 was followed by a cascade of honours, culminating in the presidency of the Royal Society.

¹⁰ Ernst Mayr (ed.), Charles Darwin, *On the Origin of Species by Means of Natural Selection: Or the Preservation of Favoured Races in the Struggle for Life* (1859; 1st facsimile, Cambridge, MA: Harvard University Press, 1964), 1. As many historians have noted, this self image came at the expense of the literal truth: Darwin was not the *Beagle*’s official naturalist.

¹¹ Paul White, *Thomas Huxley: Making the ‘Man of Science’* (Cambridge: Cambridge University Press, 2003).

His later career can give the impression that he was, as his first biographer put it, 'born in the purple'.¹² However, a close examination suggests a very different story. Like Huxley on the *Rattlesnake*, Hooker spent four painful years as an underpaid assistant surgeon on a surveying vessel, HMS *Erebus*, where he was bound by naval discipline, and where he – unlike Darwin – could not go ashore for months at a time with his personal servant to make collections. The chief strategy Hooker used to transform his travels into a career was classification, the endless and meticulous identification, categorization, and naming of the empire's plants. However, like Huxley, he would also sit on the official committees that Wallace actively avoided.

Indeed, Wallace seems to have been impervious, if not openly hostile, to imperial rhetoric and missions. He was more concerned to put his science at the service of the powerless, who inevitably make poor patrons. Darwin, Hooker, and Huxley were obviously more useful supporters, and Wallace remained on friendly terms with all three. Yet he was never one of them: a point Fichman rather over emphasizes, more or less accusing the X-Club and the professionalizers of science (two groups who are rather more distinct than I think Fichman makes them out to be) of deliberately marginalizing Wallace. However, as Paul White has pointed out, men like Hooker and Huxley continued to support Wallace's job applications.¹³ They had their doubts about him, not least because of Wallace's ill-considered decision to make a public bet with a proponent of the flat-earth theory, John Hampden. Wallace won the bet, but Hampden never accepted that he had done so, which resulted in a long, undignified public brawl. Hooker felt that such disputes threatened the dignity of science, but was nonetheless persuaded to lend his support to get Wallace a government pension. The ambivalence of some of Wallace's friends and supporters must have contributed to his marginalization, but it can hardly be characterized as a deliberate strategy on Hooker's or Huxley's part.

Such considerations highlight a central irony in the literature on Wallace – the more that biographers focus on him, the harder he becomes to see. Unless historians can make apposite comparisons between Wallace and his contemporaries – friends, rivals, and

¹² Leonard Huxley, *Life and Letters of Joseph Dalton Hooker* (London: John Murray, 1918), vol. 1, 3.

¹³ Paul White, 'An Elusive Victorian', *Isis*, 96 (1), (2005), 129–130. This is a review of Fichman.

correspondents – it becomes almost impossible to understand the man himself. Just as an undue attention to Darwin has distorted our understanding of nineteenth-century science and its cultures, so the Wallace industry seems set to create new distortions. Contextual, social history – which Fichman largely ignores and Slotten generally misunderstands – offers a great deal more than either of these books to those who want to understand the nature of Victorian science. Fichman’s is simply quaint: historians of science have largely abandoned this style of history of ideas, because this process seems unable to answer the kinds of questions that we are interested in. Unless this tradition is re-invigorated by a real engagement with the methods and concerns of contextual history, there seems little point in reviving it. And, while anyone unacquainted with Wallace will find Slotten’s work readable and engaging, this probably will not be enough to tempt those who have already read Raby’s biography.¹⁴

What the current literature largely fails to provide is a rich sense of how Wallace fitted (or failed to fit) into the diverse social, political, economic, and scientific contexts in which he moved. Until someone is ready to tackle this admittedly very difficult task, there seems little point in writing more books about him. Having been rescued from Darwin’s fan club, Wallace now seems to need protection from his latest admirers.

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¹⁴ Peter Raby, *Alfred Russel Wallace: A Life* (Princeton: Princeton University Press, 2001).

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