

Discussion on ‘A. R. Wallace in the light of historical method’ by John van Wyhe

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Summary

The recent article by historian John van Wyhe purports to identify seventeen ‘myths’ concerning the life and work of naturalist Alfred Russel Wallace. Here we briefly describe what we feel is wrong with them, and refer to published literature that extend these arguments. Our objections do not extend to the ‘historical method’ van Wyhe adopts, but instead to the way he has ignored the criticisms of peers to the extent of not even acknowledging their scholarly articles.

Keywords: Alfred Russel Wallace, historical method, history of science, biography

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INTRODUCTION

In John van Wyhe’s (2020) article devoted to Alfred Russel Wallace (1823–1913), ‘in light of the historical method,’ we question his conclusions although not the ‘historical method’. We are perplexed that van Wyhe has ignored the many scholarly studies, which have carefully examined his published arguments and commonly found them seriously flawed.

Van Wyhe treats what he believes to be seventeen misunderstandings concerning Wallace that he says he has refuted in his previous articles and books. We here examine each in turn, if briefly, with reference to subsequent studies that have assessed van Wyhe’s arguments.

DISCUSSION OF THE ‘MISUNDERSTANDINGS’

1 & 2: ‘Wallace was [not] working-class or from the opposite side of the social spectrum than Charles Darwin.’ and ‘As a youth Wallace was [not] forced to leave school early at age 14 because the family money ran out.’

Van Wyhe’s almost total devotion to Wallace’s work in the Amazon and Malay Archipelago before he returned to England in 1862 leads him to ignore or misunderstand that most ‘working-class hero’ referrals by Wallace researchers relate to Wallace later becoming a hero to the working-class due to his extraordinary attention to their various unfair treatments. Whereas it is true that Wallace could probably claim a higher social standing than the then average man, all evidence points to his

family’s inclusion among the economically ‘common.’ When Wallace was five, they abandoned their country cottage near Usk, Wales—the one van Wyhe refers to as the ‘grandest in the area’—to move to a series of modest rentals in Hertford. Things did not get better, either, as he ‘finally left school at Christmas, 1836’ (likely, sometime before 18 March 1837, per Raby 2001; Wallace 1905, i, p. 79) to join his brother John in London. Wallace is not clear as to the exact reason for the latter move, but earlier in his account, after describing the family’s financial trials and tribulations while in Hertford, he states: ‘It will thus be seen that we were all of us very much thrown on our own resources to make our way in life’ (Wallace 1905, i, p. 15). He may or may not have been ‘removed’ from school before completing his term, but it is clear enough that the family underwent financial strain. On the school-leaving age matter, Williams (2020) notes that, contrary to van Wyhe’s words, ‘the raising of the school-leaving age to 14 did not happen until 1918,’ and ‘Wallace’s employment as a teacher [at Leicester] would have been completely unrelated to the age [he] left school’ [at Hertford]. Furthermore, the Hertford institution was aimed toward folks of modest means, and Wallace’s employment there was to help pay the relatively low fee for his schooling.

3 & 4: ‘Wallace went to the Amazon as a specimen collector [not to investigate the causes of biological evolution].’

That Wallace intended to support his field studies as a collector is not an issue, but contrary to Van Wyhe’s statement, there is sufficient evidence that he was also interested in investigating the ‘transmutation question’,—which van Wyhe dismisses without a mention. He does cite the well-known 1847 letter to Bates, but misrepresents it. Wallace (1847) wrote: ‘I begin to feel rather dissatisfied with a mere local collection—little is to be learnt by it. I should like to take some one family,

to study thoroughly—principally with a view to the theory of the origin of species’. There is no implication here he is speaking solely of insects, or of Great Britain alone, as attention to entire families would require study of overseas species (the British fauna is depauperate, even with respect to mainland Europe). What more clear statement of intent is possible? Then there is Wallace’s ‘Sarawak Law’ paper, containing the famous words: ‘The great increase of our knowledge within the last twenty years, both of the present and past history of the organic world, has accumulated a body of facts which should afford a sufficient foundation for a comprehensive law embracing and explaining them all, and giving a direction to new researches. It is about ten years since the idea of such a law suggested itself to the writer of this paper, and he has since taken every opportunity of testing it by all the newly ascertained facts with which he has become acquainted, or has been able to observe himself’ (Wallace 1855, p. 185). For discussion of the many additional lines of evidence originating during 1848–1858 that bear on this matter and clearly demonstrate that van Wyhe’s statement is spurious, see Costa (2013a, b; 2014a, b; 2019a, b), Costa & Beccaloni (2014), Beccaloni (2014a, b).

5: ‘There is no evidence that Wallace was searching for any mechanism or solution.’

There is abundant evidence, which is given in full in the references cited above. Wallace’s exact understanding of the concept of ‘adaptation’ is beside the point: no one has ever implied that Wallace’s search for an evolutionary model was based on his then-perception of what an ‘adaptation’ might be in an evolutionary sense (McKinney 1972; Smith 2012, 2015a). Indeed, here van Wyhe has fallen into a misappreciation: Wallace would come to recognize, as Darwin had, that the *mechanism* of evolutionary change involved a *process of adaptation*, and not just an end-result of having adaptations (i.e. that came about directly in response to some other possible causal influence such as climate). It appears that van Wyhe is suggesting that because Wallace went into the field with an incorrect interpretation of the dynamic concept of adaptation, he could not have been seeking an understanding of how species changed over time. This level of restriction of focus is not useful. Buffon, Maupertuis, Erasmus Darwin, Lamarck, and others, though slaves to various understandings and terminologies of their times, were all trying to identify how a sustained process of organic change might take place.

6: ‘Wallace [did not] write his essay on the island of Ternate...’

This perhaps cannot be proved absolutely one way or another at present, though Beccaloni (2019) and Beccaloni *et al.* (2019) provide substantial evidence that the event took place in the village of Dodinga on the island of Gilolo. Of more import, however, is van Wyhe’s resolve that Wallace’s memory is not to be trusted on these matters. Van Wyhe conceals that the memory of events has two main components: the ability to recall the qualities of an event itself, and an ability to attach such remembrances to absolute labels involving dates, and the proper names of the people and places connected to them (Bradburn *et al.* 1987; Thompson *et al.* 1997)—both

are well known among students of self-biography. In the first sense Wallace’s memory—of the qualitative content of events, places, and things—was in his own time remarked upon as being excellent (some of the reviews of his autobiography actually criticize him for including *too much* detail, and a reading of the work reveals many impressively detailed descriptive passages; Smith 2020). Conversely, his ability to recall exact dates and names was not so good, and many related errors appear in his writings (Smith 2016; 2019a, b). Nevertheless, it is one thing to complain that he mistook a year or ship’s name, and quite another to dismiss his recollection—on six different published occasions (Smith 2015b)—that he specifically sent the Ternate essay out on the next mail from that town only a few days after writing it (i.e. on 9 March 1858, not 5 April). As to his not mentioning Malthus in the Ternate essay, and therefore possibly having been unaware of him at that time, many other people he might have mentioned in that work, including Lyell, are not singled out either. Van Wyhe is technically correct in pointing out the lack of contemporary period proof of this, but as it stands we have a choice of three explanations: (1) that a person with a demonstrably excellent memory had a lapse concerning such an important matter; (2) that Wallace lied about this; and (3) that all is merely as it appears to be. Historical work is about weighing *all* the kinds of available evidence, not just ones that suit a particular agenda.

7: ‘Wallace’s original theory was in some ways similar and parallel to Darwin’s, but also quite different...’

This matter of alleged similarity is a much more complicated issue than it initially appears to be. There have been attempts to claim that what the two of them said in the joint Linnean Society presentation (Darwin & Wallace 1858) contained fundamental differences, but these attempts have largely been disposed of through closer analysis (e.g. Kottler 1985). As a result, the original appraisals (such as Darwin’s famous words ‘Even his terms now stand as heads of my chapters’) of similarity still seem largely apt. Even so, some other differences between Darwin’s and Wallace’s contributions in their ‘joint’ paper have indeed been noted, for example by Kutschera (2003), including their contrasting positions on Lamarckian effects, and Darwin’s identification of sexual selection. It may be that what Wallace *didn’t* say in his essay ultimately matters most in this context. Wallace does not mention humankind in the work, but nevertheless almost everyone has assumed, possibly incorrectly (see Smith 2004, 2008, 2019a) that his natural selection model was meant to apply to the development of our ‘higher’ attributes, just as it did to animal qualities. This should give pause, because if he was still confused on this issue and avoiding the question, his choices of direction in the years following require serious re-evaluation.

8 & 9: ‘Wallace recollected decades later that he wrote the essay ‘in order to send it to Darwin.’

Both of these items contain so many dubious conclusions and conjectures—and dependence on ‘poor recollection’—that only a brief reply can be given here. There is no contemporary evidence that Wallace’s packet was a reply to Darwin’s letter dated 22 December

1857. That he *did* send it to Darwin is beyond question, but apparently this was largely—or *entirely*—because he knew Darwin was a friend of Charles Lyell, whose opinion he really was seeking. Concerning 'beliefs that trace back to', the only proposal that Wallace's packet was a response to Darwin's letter was made by Raby (2001) concerning a *My Life* passage: 'I [Wallace] asked him if he thought it sufficiently important to show it to Sir Charles Lyell, who had thought so highly of my former paper' (Wallace 1905, i, p. 363). Raby interpreted this as period historical evidence of Wallace knowing this only because Lyell's opinion had been conveyed in Darwin's letter; however, a more likely interpretation is that with these words Wallace of 1905 is reminding his readers of Lyell's part in the whole story, which had only been mentioned once several pages earlier and not alluded to again. Wallace's strange phraseology is characteristic of his uneven writing style (as discussed by Charles Peirce in a 1906 book review: Peirce 1906; Smith 2014a), but beyond this the words 'thought so highly of [my essay]' appear in a letter Wallace sent to his mother in October of 1858 describing the recent events (Smith 2016); Wallace likely had the letter handy fifty years later when he was writing his autobiography. Most probably, Wallace's packet was already in the March 1858 mailbag by the time he opened Darwin's letter (a day or few days later). The discussion concerning the mail steamer dates is a distraction, as we ultimately do not care what ships went where and when, but how the mail itself moved through the system. There is no way to know this—although we do know that a letter Wallace sent in March 1858 (to Henry Walter Bates's brother Frederick) reached England in early June. The packet to Darwin must have been hand-sorted several times on its way from Ternate to Down, and who can say what kinds of delays there may have been? The whole steamer-schedule business tells us little in the end, appealing only to conspiracy theorists and Darwin apologists.

10: *'Wallace's essay was published in accordance with the standards of the day.'*

We fail to see what conclusion is drawn here. Blaming Beddall (1968) for making the 'lack of consent' observation is beside the point: the work *was* published without his consent (including, as he complained later in print—rather deliberately, at least five times—without his having been offered an opportunity to examine proofs). He later recollected that he had said nothing about publishing his essay when he sent it out (remarks confirmed by Darwin's words in a letter written at the time), and whether it was published 'in accordance with the standards of the day' contributes little to the matter.

11: *'Wallace's and Darwin's contributions were communicated by Charles Lyell and Joseph Dalton Hooker to the Linnean Society on 1 July 1858 'in the order of their dates' ...'*

The evidence for this seems sketchy, and although it may well be true that the practices of the time weighed in Darwin's favour, this is somewhat like saying slavery was acceptable in the early nineteenth century. Thus, we may understand, as dispassionate witnesses, how the decisions were made, but, considering that Wallace was the initiating figure (and that he was not consulted about the decision to publish), we may still believe

that something unseemly took place. In three recent papers Partridge (2015, 2017, 2018) draws attention to the dynamics of publication for the 1858 essays. In these works he focuses on the apparently single-minded efforts by Darwin's friends to establish his priority *in print*, and that Darwin's contribution to the presentation consisted largely of outdated thoughts from his initial words in 1844 (leaving further questions as to the meaning of 'priority' in this instance).

12: *'It was seldom described as the Darwin–Wallace theory of evolution...'*

Wallace's contributions to *natural selection theory* were as wide-ranging as Darwin's, if not nearly so minutely laid out. It remains to be seen whether his ideas on evolution *in general* will eclipse Darwin's; perhaps we will find out once they are more fully explored (Smith 2012), a process that is taking longer than it should.

13: *'Wallace was [not] the greatest field biologist of the 19th century.'*

Field biology as a discipline and occupation is a twentieth century phenomenon, contrasting with typical nineteenth century efforts by enlisting a more experimental approach (as opposed to mere observation and collection). Thus we can best identify Wallace as a 'field *naturalist*,' the word 'field' being inserted to distinguish the approach of his early years from armchair observers, museum specialists, and other kinds of nature-focused workers such as astronomers (as found in the early issues of *Nature*). Van Wyhe's complaint that it is hyperbole to rate Wallace as the 'greatest field naturalist' of the nineteenth century (or perhaps, ever) requires an assessment of who else was in the running for such recognition. If such a vote had been made during the nineteenth century, the geographer Humboldt likely would have this honour, with a few votes possibly going to geologist Darwin. More recently, Wallace has risen in such stakes not only because his was a more personal effort and lasted longer, but because the 'prizes' he secured, including the theory of natural selection, were equally numerous and have ultimately proved more substantial (Beccaloni 2020). Moreover, he was one of the few naturalists who published important scholarly papers while still in the field. Van Wyhe implies that the 'greatest' label is nothing more than hagiography, but how are we to judge this?

14: *'He is [not] also the father of biogeography.'*

Van Wyhe is either being lawyerly here, or does not appreciate the interrelation of the fields of biogeography and zoogeography, or the history of their development. Most responsible sources now refer to Wallace as either the 'father' of zoogeography, or of *historical* (also known as evolutionary) biogeography alone. Until recently, most practitioners of zoogeography have been in one sense or another systematic zoologists who turned to a historical-spatial perspective to help complete their evolutionary models of particular species groups. By contrast, since the beginning of the nineteenth century, the typical 'biogeographer' has been botanically, geographically, or ecologically trained, and been concerned largely with ecological settings. These boundaries have increasingly blurred, as well they should. If there are any 'fathers' of

biogeography they are probably early nineteenth century workers such as Humboldt and the Candolles; but they had no model of evolution that provided a critical long-term time dimension. Wallace and Darwin did, and it was the former who led the way in the spatial realm. Wallace may therefore reasonably be recognized, at the least, as a father of historical biogeography alone, or alternately of zoogeography.

15: 'His book *The Malay Archipelago* has [not] never been out of print.'

In a 30 December 2015 email to van Wyhe in response to the 2015 note, CHS included:

There appear to be no new English-language printings of MA by Macmillan (or other publishers) after the 1922 one [and before the 1962 one], but this does not mean that it was 'out of print' at that point; *British Books in Print* still lists it as being in print as of its 1936 volume [also, in its 1924, 1928, and 1932 ones; the listing disappears after 1936]. When someone refers to something as still being 'in print' as of a particular date, it doesn't mean that there has been a new imprint created then, just that the last [-dated] imprint, however old, is still available for ordering through the publisher.

Meanwhile, according to the bibliographic database *FirstSearch*, imprints of the title were issued in Japanese in 1931, 1942 and 1954, and in Chinese in 1933, 1935, 1939, and 1959; each of these undoubtedly remained 'in print' for at least several years afterwards, extending all the way to the 1962 Dover edition. There may well have been other editions not recorded in a library collection. The distinction between 'in print' and 'out of print' is clear enough that for several years there was a series called *Books Out of Print* that allowed the larger publishers, at least, a chance to formally declare at some point (probably in most cases when stock ran out, or requests for the title had dropped off) that a particular title was no longer available. So, being 'in print' has to do with availability, and essentially nothing to do with the most recent imprint date. Some titles are still available for thirty years or more after their last (or only) imprint date, something CHS can personally attest to from years of experience as a science materials purchaser.

16: 'At the end of his life he was [not] the most famous scientist in the world.'

The argument has been that he was either *among* the most-famous/'greatest,' or *the* most-famous/greatest of those *still living* at the time of his death. Van Wyhe's remarks notwithstanding, there is sufficient defense for such a surmise. Smith (2014b) and Beccaloni (2013) both present data bearing on this question. Smith's examination of his name in *HathiTrust* records over the final period of his life, found that among then-living scientific figures only Lord Kelvin had significantly more mentions. However, Lord Kelvin died in 1907, leaving Wallace the highest 'scorer' for the following six years. Re-running the survey for 1900–1909 and 1910–1919, yields essentially the same results (the closest challengers were Robert Koch, Wilhelm Ostwald, Wilhelm Wundt, and John Muir). Fiske, mentioned by van Wyhe, often wrote on evolution, but was more of a philosopher and historian, and died in 1901; Lankester, Burbank, Galton and Crookes, are also mentioned but come up short, though Lodge has a similar tally. Using Google's Ngram Viewer, it can be shown that by Wallace's later years he was being cited more often than Hooker, Huxley and Owen (the last two of whom had died in the 1890s), and had drawn about even with Lyell (also deceased). An analysis of mentions of Darwin, Huxley, Lankester, and Wallace in *The Times* over 1880–1920 confirms these results (Fig. 1). Looking at the number of mentions of prominent scientists in the several hundred newspapers covered by the British Library newspaper database over 1905–1915 shows that—beyond Lord Kelvin and Lodge—Madame Curie, Lord Lister, Koch, John Muir, and Wallace were similarly frequently mentioned. Plenty of incidental evidence backs up these numbers (including that Wallace was, during the period following Lord Kelvin's death in 1907, more than three times more frequently referred to as *the*—as opposed to 'a', 'that', 'this', 'our', etc.—'Grand Old Man of Science' in the British Library online newspaper database than was any other figure). Admittedly none of this proves outright that Wallace was, absolutely, the greatest scientist of his time, at the least he should not be so summarily dismissed as one of the very top candidates. Furthermore, van Wyhe's implication that the quotes given by Smith (2014b) refer only to obituaries is incorrect.

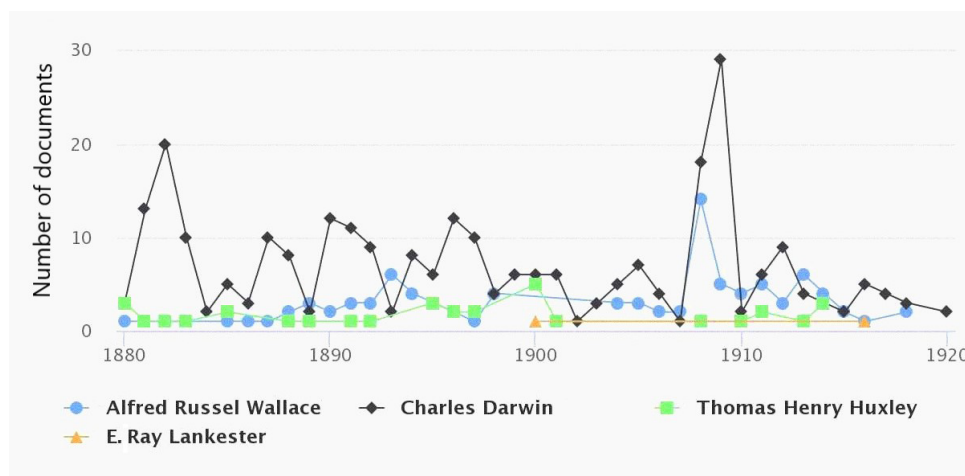


Figure 1. Number of articles per year in *The Times* mentioning four naturalists over 1880–1920.

17: 'He has [not] become strangely forgotten.'

We tend to agree with van Wyhe that this is not really the case; even if Wallace *has* been forgotten to a degree not uncommon among historical figures, a fundamental question is to *what degree does he deserve not to be forgotten*. His was once a household name, and there were good reasons for this.

SUMMARY

Our replies to van Wyhe's remarks are intended, not to prop up Wallace, but to return productively to the question of what happened during his life, and how this may be important to us. Perhaps the most troubling part of the way van Wyhe applies the 'historical method' is that it ends up being too backward-gazing. The real importance of history is its ability to cast light on the present, and the possibilities of the future, if only we could come to consensus on all the facts of the past. Unfortunately, we are a long way from achieving this, and indeed even if we eventually can, the mission of historical revelation will not have been completed: much of what took place in the past is unrecognized now not only because it was unrecorded or has been forgotten, but because it is no more understood now than it was at the time. This is especially true of intellectual history. For figures as intellectually creative as Wallace undoubtedly was, we submit that one should have especially good reasons to object to the manner of his representation, especially if at odds with the records of the time. While we applaud van Wyhe's support of this particular method of doing history, it appears to us that his choice of Wallace as whipping boy in this context was ill-chosen. We are certainly not unamenable to revisionist interpretations of Wallace's life and work, but these should incorporate more than assertions.

Wallace was by anyone's standards a remarkably inventive thinker, and differed from most other scientists in history in having secured a substantial social reputation, especially in his later years. This, together with the romance of his travels and his broad and often provocative interests, explains the current rise of a popular attraction to him. We don't believe this interest is a bad thing—as long as it is based on the truth—so the changing 'Wallace narrative' over time is neither surprising, nor to be regretted. Is there any reason to think that the opinions of Wallace in his own time were any more valid, or useful, than are any such opinions offered today? Individual historians do not get to make such decisions: their job is to record, as accurately as possible, both the facts and the opinions. This doesn't mean that we need to encourage unsupported hero worship, but historians should not push conclusions based on preconceptions and incompletely reported research. Van Wyhe frequently states that he is the first historian to subject Wallace to critical inquiry, but many past historians have taken on Wallace as a primary subject: for example, Gerald Henderson, H. Lewis McKinney, Malcom Jay Kottler, Michele Malinchak, Jane Camerini, Jean Gayon, Michael Shermer, and Martin Fichman.

Whereas van Wyhe is free to come to his own conclusions on the matters he deals with, it is not

acceptable to denigrate contrary pieces of evidence, nor is it proper to ignore the criticisms of peers by not acknowledging their scholarly articles.

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