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Derek Partridge
Charles H Smith, editor



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*Alfred Russel Wallace Notes** 10. The Impact of A. R. Wallace's Sarawak Law Paper Resurrected.

Derek Partridge ^a

^a University of Exeter, Exeter, UK. Email: d.partridge@exeter.ac.uk Accepted for publication 15 April 2020.

Summary: Six assertions that relate to the impact of Wallace's Sarawak Law paper¹ on the development of evolution theory have been proposed and analyzed by John van Wyhe.² He concluded that they were all erroneous. The analysis presented a valid criticism of some casual and over-confident pronouncements with respect to interpretations of history. More significantly, it is a misguided attempt to expose "original historical meanings," and thereby dismiss all other interpretations as necessarily incorrect. A re-analysis reveals that, contrary to van Wyhe's analysis, much of the conventional wisdom is plausibly correct, and it remains the case that "the past is a foreign country," but it is not another planet. *Key words:* Alfred Russel Wallace, John van Wyhe, evolution, natural selection, Sarawak Law paper

One person's denial of the antecedent is another's affirmation of the consequent.

Introduction

The present note addresses John van Wyhe's (2016) criticism of six assertions deriving from Wallace's (1855) Sarawak Law paper. It is wholly supportive of his objections to claims such as these being reported as straightforward facts in the popular and the scholarly literature. It does not, however, continue that support to the point of his declaration "that each of these [six assertions] is erroneous" (p. 56).³ In pure logic, which this domain does not admit,⁴ declaration that a supposed fact is erroneous is the other side of the coin from reporting it as straightforwardly true. Unlike coins, however, complex and necessarily probabilistic analyses, such as historical interpretation demands, almost never fall out at either extreme.⁵ The root cause of the unwarranted certainty as well as the fundamentally misguided analyses is erroneous reasoning. It varies from simply illogical to no more than fictitiously-exclusive plausibility. When plausible initial conclusions become surreptitiously elevated to certainties they add spurious weight to subsequent conclusions.

In the present re-assessment, the old impacts are re-established, but as no more than plausibly true. The restoration is based on a critique of the reasoning employed by van Wyhe, not on the traditional historiographic strategy of composing an alternative context for analysis. It is the analysis itself that is at fault, not the contextualization on which it is based.

His re-assessment concludes: "Wallace's Sarawak Law paper has had an unusually convoluted and confusing legacy in recent debates. Its original historical meanings have been partially lost" (p. 65). The wholly unwarranted implication being that the analysis presented has reminded us of the "original historical meanings." Such "meanings" are mental constructs particular to a person at a specific time. They are not an objective feature of events even without the added constraint of "original" ones. The meaning of the paper to Wallace in February 1855 would seem to be the absolute original meaning. But its meaning to Lyell, not attained until November 1855, is equally important, although it probably changed significantly in April 1856 when he first learned the detail of Darwin's theorizing. As for the third principal,

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Darwin, what were its meanings to him on the several occasions when he appears to have read it? And so on. The very idea of “original historical meanings,” let alone that an analysis can be known to have revealed them, is highly suspect.⁶ It cannot be the basis for blanket dismissal of alternative interpretations presented as possibilities.

In this note, the re-analysis is limited to the six assertions that van Wyhe presents and claims to have refuted. It concentrates on the possible beliefs and interpretations of the actions of the three principals – Wallace, Lyell and Darwin – as contributions to a plausible account of the likely impacts of Wallace’s Sarawak Law paper on the development of Darwin’s theorizing on evolution by means of natural selection. The plausible outcomes revealed range from “most probably not erroneous” to “probably erroneous.” This more secure refutation is attached to the first two assertions, but only because both are phrased in a brittle and deceptive way. They are brittle because a fairly secure denial is easy to construct, and deceptive because the denials do not actually impact on the central narrative in the manner, or with the force, implied. The first assertion ducks the more difficult and more significant issue of an announcement less stark than an avowal. Whereas, with respect to articulating the likely general impacts of Wallace’s paper, the second overstates the claim by the inclusion of “only.” There is also a logical coupling of the two “easy to break” words such that denial of the first assertion eviscerates the possibility of supporting the second: if Wallace’s paper is not an “avowal of evolution” in the sense that it never “laid out the theory of evolution” (in effect, a denial of the stated pre-condition for Assertion 2, p. 56), then it is logically impossible that it lacks “only” an explanation of the process by which evolution occurs. The second assertion, as phrased, is thus a spurious addition to the blunt claim that Wallace’s paper “makes no mention of evolution of any kind” (p. 57). As a final preliminary observation on the phraseology, one that echoes the primary motivation for the whole re-analysis, it should be noted that Assertion 2 refers to “the theory of evolution.” This should be “a theory of evolution” if “the theory” is not qualified and so tied to a specific version. Even if it is referring to Darwin’s outstandingly successful *Origin* version, it is still “a theory” as is every theory in science. True theories in science are almost as fanciful as original meanings in history.

The present note is not endeavoring to recover “original historical meanings [that] have been partially lost” (p. 65). By means of a re-assessment of the reasoning used by van Wyhe, it resurrects the credibility of the interpretations that van Wyhe’s paper purportedly demolished. Each re-analyzed assertion is concluded with a plausibly-true version of the original assertion, a derivation pertinent to the core narrative under scrutiny.

A Critique of Pure Reasoning

Wallace’s 1855 paper laid out nine “propositions in Organic Geography and Geology ... the main facts on which the hypothesis is founded” (p. 185), the hypothesis being that “Every species has come into existence coincident both in space and time with a pre-existing closely-allied species” (p. 186). This is known as the Sarawak Law because Wallace referred to it as a “law” and the paper was written whilst Wallace was in Sarawak, a small territory on the island of Borneo – famously, the domain of the White Rajah, the Englishman Sir James Brooke.

Van Wyhe’s reasoning that leads him to a re-assessment of the impacts of six assertions relating to Wallace’s Sarawak Law paper are re-examined, one by one, below.

“1. The paper was Wallace’s first avowal of evolution” (p. 57)

Not really. It was not an “avowal of evolution” as in a “declaration” or “affirmation,” but instead a collection of observations from which he deduced a general law characterizing the successive appearance of new species in terms of similarity to their predecessors and to their

neighbours. It was, however, a law whose systematic simplicity suggested that the phenomena it unified ought to be explainable by means of a natural process – Wallace explicitly referred to this likelihood as “some great natural law ... which has regulated the peopling of the earth with animal and vegetable life” (p. 195).

Van Wyhe argues, somewhat curiously and inconsistently accepting at face value the relevant part of a 1903 statement by Wallace, that “Wallace left evolution ‘to be inferred’ from his generalizations.” (p. 58). Wallace implied it, he did not avow it. Nevertheless, “It was,” we are also told, “as close as one could get to arguing that new species evolve from earlier ones – but at the same time never suggesting [explicitly?] that species might change” (p. 58). How is it possible that “new species evolve from earlier ones” without species change? It is not. Far from needing to say that species change, such change is a necessary consequence if “new species evolve from earlier ones.”

Although “the Sarawak Law paper makes no [explicit] mention of evolution of any kind” (p. 57), van Wyhe accepts that “Wallace left evolution ‘to be inferred’ from his generalizations” (p. 58). Whether readers in the mid-nineteenth century would make the intended inference to tie in with an anticipated unknown natural law is, in general, possibly unlikely as van Wyhe maintains. However, we are not concerned with the general reader. The relevant interpretations are those of three, differently specialized, specialists: Wallace, Lyell and Darwin.

A plausible re-statement of Assertion 1, and a version that is in accord with van Wyhe’s conclusion, is:

1a. *It was Wallace’s first presentation of a pattern of observations that implied evolution in a way that was as close as one could get without actually stating it.*

“2. Wallace laid out the theory of evolution minus only a ‘mechanism’” (p. 58)

Again, not really, although a “mechanism,” a process that could explain evolutionary change, is in fact not included in the paper. So, if the paper implies (as accepted above) the presence of a framework of organic change – one governed, as Wallace stated, by natural processes – it does so without inclusion of any means to achieve such change. The necessity for “some great natural law,” as Wallace put it (p. 195), was acknowledged but no such law was proposed.⁷ Although natural selection is generally believed to be the missing process, stating the bare process would not have been enough. In support of this claim, we might note that Darwin’s first extensive articulation of natural selection, in the context of a feedback loop in his 1844 Essay, was fatally undermined by changes in his own newly-emergent, post-1844, beliefs as well as by clashes with subsequent observations.⁸ Consequently, he embedded the natural selection mechanism in a new context by re-specifying when, where, how in detail, and to what end it operated; this he published in 1859 as *On the Origin of Species*. So Wallace was right, “a great natural law” (p. 195) was required, one that appropriately contextualized a basic mechanism of organic change. To propose the lack of “only a ‘mechanism’” is to invite a focus on the insignificant exclusivity of “only,” and so divert consideration away from the feature that Wallace stated was needed, but that he was unable to supply – “a great natural law.”

Neglecting the diversionary brittleness introduced by the word “only” in Assertion 2, the 1903 Wallace quotation that van Wyhe reproduces as evidence against “avowal” in Assertion 1 affirms the essence of the supposedly-refuted impact of Assertion 2. To re-use van Wyhe’s quoted evidence:

I myself firmly believed that it was a direct modification of the pre-existing species through the

ordinary process of generation ... but I could not as yet see any mode or process by which the change could be effected, and the characters of the new species fixed and rendered permanent by natural law, I left it to be inferred till such a law should be discovered (Wallace 1903, in van Wyhe 2016, p. 58).

“Some of the wording in this recollection,” van Wyhe reveals, “is potentially misleading” (p. 58) – unsurprising for a recollection of beliefs from nearly half a century earlier. Very conveniently, it appears that the unreliable can be identified and used to refute Assertion 2, and the reliable recollections did much the same job for Assertion 1. In particular, Wallace’s stated inability to “see any mode or process by which the change could be effected” is ruled out as support for Assertion 2 because it is “a post-*Origin of Species* (1859) manner of speaking” (p. 58). Surely the manner in which Wallace chose to describe his remembrances in 1903 is irrelevant, especially to Lyell in 1856 when he may have had this paper in mind (if not explicitly mentioned) at the time he advised Darwin to publish. It is not clear why this use of a claimed post-*Origin* manner of expression in any way undermines the essence of Wallace’s stated claim. The passage of time must be cause to treat the whole document with care, but Wallace’s 1903 claim that his paper inferred evolution was apparently acceptable as straightforward evidence to support the denial of the first assertion.

Van Wyhe endeavors to endow his denial of Assertion 2 with increased credibility by eliminating, from the choice of options, the possibility that Wallace inferred an adaptive process, such as natural selection. We are informed that Wallace “was wholeheartedly anti-adaptationist at this time in his life and theorizing” (p. 59). The basis for this claim, as derived from the Sarawak Law paper, is the discussion of rudimentary organs. Van Wyhe points out that Wallace thought they were incipient, not remnant, structures, and emergence for future use cannot be due to an adaptive law, but “the opposite of an adaptive law” (p. 59), although “non-adaptive law” is both more accurate and less restrictive.⁹

However, this “anti-adaptationist” claim is only a half truth: van Wyhe is using later perceptions to label what was not Wallace’s perception of the matter in 1855 – which was that adaptation was somehow “correlated” with evolutionary process.¹⁰ Van Wyhe is ignoring the “original-source” evidence, neglecting the very principle that he urges on historians. Wallace’s far more nuanced and open-minded 1855 view is evident in the primary sources, which is not the blunt, “wholeheartedly anti-adaptionist” stance asserted. Wallace appears at this point to believe that adaptations are not “causal” in the process, but are more after-effects (note the remarks of McKinney 1972a, p. xxi, that Wallace “thought that adaptations had occurred while species evolved”). For example, Wallace’s position is plainly stated in a paper published in 1856 when he discusses the huge canine teeth of the male orang-utan:

We conceive it to be a most erroneous, a most contracted view of the organic world, to believe that every part of an animal or of a plant exists solely for some material and physical use to the individual ... to believe, in fact, that we know the one sole end and purpose of every modification the exists in organic beings, and to refuse to recognize the possibility of there being any other. (Wallace 1856, p. 30)

We look upon the anomalies, the eccentricities, the exaggerated or diminished development of certain parts, as indications of a general system of nature, by a careful study of which we may learn much that is at present hidden from us; and we believe that the constant practice of imputing, right or wrong, some use to the individual, of every part of its structure, and even of inculcating the doctrine that every modification exists solely for some such use, is an error fatal to our complete appreciation of all the variety, the beauty, and the harmony of the organic world. (Wallace 1856, p. 31)

The primary-source evidence is that in 1855 Wallace was certainly not “wholeheartedly anti-adaptationist.” This means that there is no basis to so characterize the unknown “great natural law” that Wallace proposed, which was, presumably, the intended implication of the “anti-adaptationist” mis-characterization of Wallace.

We might also note that treatment of the oddity of vestigial organs as exceptional to the general case of organic development is, in effect, the modern understanding: being features of an organism that have no impact on survival and reproductive success, vestigial organs are features that the inexorable adaptive push of natural selection gets no purchase on. Hence, they linger on through the millennia. With respect to rudimentary organs, natural selection is still thought to be non-adaptive, but that is no bar to the belief that evolution is fundamentally an adaptive process.

To summarize: if “It [the Sarawak Law paper, implied evolution and] was as close as one could get to arguing that new species evolve from earlier ones” (p. 58) without explicitly stating it, but declared the necessity for an unknown natural law to explain it, then a plausibly true re-statement of Assertion 2, using post-*Origin* terminology, is:

2a. *Wallace laid out a strong case for inferring a theory of evolution minus a “mechanism.”*

“3. Darwin failed to see how close Wallace was approaching” (p. 59)

Oddly, the opening sentence of the supposed refutation of Assertion 3 is: “The preceding two elements lead directly to a third, that Darwin himself failed to understand Wallace’s paper and how close Wallace was to approaching the same theoretical position” (p. 59). In other words, Assertion 3 is true !

A dip into formal logic exposes this contradiction:

if each Assertion “is erroneous” (opening claim, p. 56),

then it is *not* the case that “Darwin failed to see how close Wallace was approaching” (Assertion 3).

Therefore, that Darwin *did* see how close Wallace was approaching must be the conclusion, a logical consequence that van Wyhe seeks to deny. The less straightforward logic of the argument actually offered is no better:

if “Wallace left evolution to be inferred from his generalizations” (p. 58, the conclusion drawn as part of the rejection of Assertion 1),

and “Darwin’s understanding of the paper is quite clear” (p. 59),

and Darwin’s understanding of the paper is not “in any way surprising” (p. 61),

then Darwin read Wallace’s paper as implying evolution.

Van Wyhe concludes the opposite. There is a logical conflict here: van Wyhe is caught between an initial admission that Wallace wrote his paper implying evolution, and a refusal to admit the possibility that Darwin misread it coupled with a commitment that Darwin’s reading discerned nothing of evolution in it.

An attempt to escape from this unwanted entailment is made by throwing in the contention that “Wallace hid evolution too well” (p. 61). If “evolution” was hidden “too well” for Darwin, and for Lyell (see Assertion 4), two differently-oriented evolution “specialists,” then for whom was it inferring evolution? How does this hidden “too well” fit in with the earlier assertion that Wallace’s paper “was as close as one could get to arguing that new species evolve from earlier

ones” (p. 58) without actually stating it? On what basis is this hidden “too well” claimed? It is key to the exculpation of Darwin from the charge of misreading Wallace’s paper, but is presented with no support from the original sources – a serious lapse for one who concluded his reassessment with the hope “that in the future writers will endeavour to summarize and characterize the original sources” (p. 65). However, from deductions that both Darwin and Lyell failed to perceive Wallace’s inference of evolution, it might be similarly deduced that, therefore, Wallace “hid evolution too well.” But this terminal deduction cannot then be used to support its antecedents. Such a circle of plausible deductions requires a clean break and an anchor in original sources, otherwise it holds as much promise of valid progress as a dog chasing its own tail.

Overall, the suspicion must be that the assertion declared erroneous here, Assertion 3, should have been: *Darwin saw how close Wallace was approaching.* But even this would be tricky because its negation retains the implication that Wallace was approaching Darwin’s ideas – which van Wyhe does not accept as a possibility. Two independent potential impacts have been unhelpfully conflated, and they require independent dismissals in order to enable the revelations that van Wyhe apparently wishes to promote:

1. The Sarawak Law paper inferred that an evolutionary process could account for the observed organic change, but well-hidden and not in such a way that it was a significant approach to Darwin’s theorizing, and therefore
2. Darwin, quite rightly, did not read Wallace’s approach to his evolution theory in the Sarawak Law paper or anything relating to evolution in it.

But, to continue with the arguments presented: Why, in van Wyhe’s view, did Darwin fail to perceive the evolutionary inferences? He claims that the surprise for the modern reader is based on attributing a modern mind-set to the paper’s original readers. Whereas, we will see that this charge is more appropriately levelled at Darwin – more precisely, at van Wyhe’s interpretation of Darwin’s reading. In addition, Darwin, it is explained, was unimpressed with Wallace’s few observations in comparison to his own vast accumulation of similar items. It was, however, Wallace’s use of the word “creation” that was the fundamental obstacle, one that the modern reader could well seem to appreciate. But in 1855, as van Wyhe pointed out, “the term creation at the time was rather vague. It could mean both divine special creation or appearing through some unknown natural causes” (p. 57). Ignoring the ambiguity of “both ... or,” we are to be persuaded that Darwin fixated on Wallace’s use of “creation” as the providential option, and that it apparently did not occur to him that “unknown natural causes” might be what Wallace meant. This is the interpretation advanced despite the fact that in the paper Wallace asks in relation to rudimentary organs: “What have they to do with the great laws of creation? Do they not teach us something of the system of Nature? ... they must be the necessary results of some great natural law ... which has peopled the earth with animal and vegetable life” (p. 195).¹¹ So, quite contrary to modern interpretation, Wallace’s use of “creation” as a process explainable by a “natural law” would seem to rule out the “divine special creation” option for the 1850s reader. But it did not do so for Darwin, we are assured.

Supposedly backing up Darwin’s stubbornly modern reading of “creation,” we have:

This interpretation exactly matches what Darwin later wrote to Wallace in late 1857: “Though agreeing with you on your conclusion’s [sic] in that paper, I believe I go much further than you.” Darwin went “much further” because he held that species actually changed and evolved one into another whereas he took the Sarawak Law paper to mean that new species were separately created one after another. (van Wyhe, 2016, p. 60)

After dismissing the exaggeration of “exactly matches,” one can see that Darwin’s sentiment

of agreement but going “much further” is also entirely consistent with another reading: that Darwin read the implication of evolution in Wallace’s paper but his own theorizing went “much further” because he had also proposed a mechanism.

Apparently, “Darwin did not know that Wallace privately believed in actual descent” (p. 60). This claim can only have relevance here in excusing Darwin’s misreading of Wallace’s paper, and van Wyhe makes no such excuses. It is also quite probable that Lyell was in a similar position of ignorance with respect to Wallace’s private beliefs, and yet he appeared to extract the evolutionary inferences (see later discussion of Lyell’s notes). Both men had long been engaged with evolutionary ideas, albeit from opposite sides, so it would be more than a little curious had Darwin totally failed to extract the implications of evolution in Wallace’s paper. Particularly so because Darwin had been explicitly exposed, although somewhat obliquely, to its evolutionary viewpoint by his correspondent Edward Blyth.¹² In addition, Lyell and Darwin had certainly communicated over this paper, but whether they discussed or even touched on its evolutionary implications is unknown. However, it would be most odd had Lyell’s recommendation of the paper to Darwin failed to include his own interest in it, which he had noted as going “far towards Lamarck’s doctrine”¹³ – the specific theory of evolution that he had attacked repeatedly.

We might also note that Darwin, even with regard to his own theorizing, had a tendency to over-state the similarity of different theories (e.g., between his 1844 Essay and Wallace’s Ternate paper¹⁴). Later, he apparently failed to see the radical metamorphosis of his own theory as anything more than embellishments and adjustments of the theory he had conceived in 1839 (e.g., a claim made by Darwin in his autobiography,¹⁵ and yet the 1844 and 1859 theories are fundamentally different, as possibly first summarized by Ospovat in 1981: “In 1844 natural selection was not a theory of organic development, but simply a theory of organic response to environmental change”¹⁶). Darwin, the great theorizer, often appears to have been surprisingly un-attuned to the similarities and differences between theories.

Darwin’s estimation of Wallace’s paper is recorded in his annotations to the paper as well as notes on a slip of paper in the back of a copy of the *Annals and Magazine of Natural History*.¹⁷ A presentist acceptance of his words, which apparently makes “Darwin’s understanding of the paper quite clear” (p.59), seems to reveal a curiously obtuse reading – although it is less unperceptive if we admit the non-providential 1850s meaning of “creation” instead of insisting that Darwin had fixed exclusively on the modern one. It is this ahistorical restriction on the reading of Darwin’s words that produces the claimed clarity to Darwin’s failure to see that Wallace had taken some first steps towards a great natural law that would explain his Sarawak Law.

However, we can re-state from a basis that does not exclude the plausible alternatives introduced above that Wallace’s paper did imply evolution as Wallace intended (a part of the denial of “avowal” in Assertion 1). By also rejecting the unsubstantiated claim that the inference of evolution was so well-hidden that even evolution “specialists” did not detect it, we might have:

3a. *It is hard to understand how Darwin failed to see the evolutionary implications in Wallace’s paper.*

“4. Lyell did see how close Wallace was approaching” (p. 61)

Quite probably. A fundamental reason why Wallace’s paper would have greatly struck Lyell is that it was almost certainly written in large part with Lyell’s anti-evolution stance in mind.¹⁸ In addition, there is original-source evidence in Lyell’s notebooks: on first reading it,

he immediately associated it with Lamarck's evolutionary doctrine, and, after learning of Darwin's theorizing, Lyell immediately returned to a consideration of Wallace's paper. Although he left no explicit indication, it seems unlikely that after having closely studied the paper Lyell would not also have noted Wallace's mention of the necessity for "some great natural law" to explain his observations. This carries the obvious implication that Wallace was likely working to discover it.

It is unsurprising that as a man who had long railed against evolutionary theories, Lyell appears to have extracted the inference of evolution in Wallace's paper. He could hardly have done otherwise, because Wallace consciously wrote his paper as an explicit challenge to Lyell's well-known anti-evolution arguments. For Lyell, it was a set of personal challenges to be countered. In addition, as a theory that was not obviously flawed, in contrast to both *The Vestiges* and Lamarck's proposals, perhaps it was, in Lyell's estimation, a more significant challenge, one demanding careful consideration.¹⁹ So it merited a notebook entry and associated notes probing some of Wallace's stated contentions with the hope of discovering their weaknesses.

In support of his denial of Assertion 4, van Wyhe repeats with approval Wilson's (1970) summary of the nature of the change in Lyell's notebook entries before and after he learned the details of Darwin's theorizing in April 1856. Before this meeting with Darwin, "Lyell's notes show that he was convinced that species do not evolve" (p. 62). So, "Contrary to the way Lyell's notebooks have long been described, they do not indicate that he understood Wallace's paper to be about evolution" (p. 62). This is an odd deduction, quite apart from its unwarranted certainty. Lyell's first notes on Wallace start with attempts "to explain away why new species resembled earlier ones in the same place" (p. 62) (i.e., possible non-evolutionary explanations of explicit elements of Wallace's paper), and continue on with "notes show[ing] that he [Lyell] was convinced that species do not evolve" (p. 62). These are offered as a failure to "indicate that he [Lyell] understood Wallace's paper to be about evolution" (p. 62). It would be a good deal less odd as an indication that precisely because Lyell *did* understand Wallace's paper to be about evolution, uncomfortably persuasively and personally so, his initial notes were aimed specifically at refuting elements of Wallace's paper. In the few months since opening his first "scientific notebook" with an entry about Wallace's paper, his subsequent entries reveal his increased concerns about the solidity of his arguments for species immutability, arguments he had been developing for more than twenty years within the successive editions of his *Principles of Geology*. The mistake in van Wyhe's analysis is to fail to appreciate that pointedly anti-evolution notes logically infer a consideration of evolution, not an absence of it.

As Wilson also suggests, and van Wyhe concurs, apparently seeing it as another important indicator of "original historical meaning," "Lyell's notes begin to tentatively explore evolution *after* his April 1856 conversation with Darwin" (original emphasis, p. 63). As van Wyhe interprets: Lyell's view was transformed from committed anti-evolutionist to a more open questioner of the possibility of species transmutation. He saw the possibility that Darwin's theory of descent with modification could "explain" "the regularity of successive appearances that Wallace outlined so powerfully" (p. 63) – which Lyell, we might note, then immediately recorded, explicitly in relation to Wallace, and only Wallace (Wilson 1970, p. 55). Puzzlingly, van Wyhe continues: "Lyell did not see that Wallace's paper implied evolution. If he had seen evolution as the key to Wallace's pattern, then it could not be "explained" by Darwin's evolution six months later" (p. 63). This is another odd deduction, one that appears to be founded on conflation of a general category – theories of evolution – with one of its specific instances, viz, Darwin's theory. The latter is just one particular example drawn from the set of possible explanations of evolution. Surely Lyell could have seen that Wallace's paper implied some, as

yet unknown, process of evolution, and after conversation with Darwin he saw that the specific process of natural selection was, potentially, an explanation – a specific natural means by which the inferred evolution might be explained.²⁰ It was therefore a potential explanation of Wallace's pattern; a viable candidate for Wallace's "great natural law." Without delving into relative probabilities here, it is clear that the crucial interpretations dismissed by van Wyhe are in fact quite plausible.

4a. *Lyell most probably did see that Wallace had laid out a credible framework for a theory of evolution.*

"5. Lyell urged Darwin to publish because of Wallace" (p. 63)

We cannot know for sure, but are there any other names to go into the frame? Lyell's notes on Wallace and his Sarawak Law paper in volume 1, page 31 of his "Index Books" (notebooks "in which he indexed books and articles he had read"²¹) includes the observation that Wallace's law "goes far towards Lamarck's doctrine."²² This is an explicit connection to a theory of evolution, the one that Lyell had repeatedly rejected in the various editions of his *Principles of Geology*. It is thus certainly plausible that Lyell read Wallace's paper as another emerging threat to his belief in the immutability of species. Apparently, it did strike Lyell as a notable (literally so, as he made a note of it) and coherent first step towards the possibility of a theory of evolutionary change. Wallace then would surely be a prime candidate to take the next. The use of yet another brittle and deceptive word, "urged," in the phrasing of this assertion attracts a challenge; one which van Wyhe does not shirk. But whether Darwin was "urged" or less forcefully "advised" to publish a sketch of his theorizing has no significant bearing on *why* Lyell made this request. It is the reasoning behind this suggestion that is important, regardless of the forcefulness with which it was delivered. Lyell was concerned that his friend, Darwin, might be forestalled.²³ This could have been because of his reading of Wallace's paper, or by the entrée of someone else into transmutation theorizing (someone he had failed to note explicitly), or by a general sense that such theorizing was "in the air." According to van Wyhe, Lyell's "wish you would publish" reads as "an almost offhand remark at the end of the letter" (p. 64). Yet, in the subsequent months of letters to various correspondents, Darwin repeatedly wrote of Lyell's "urging"²⁴ and "urgent advice."²⁵ Darwin apparently took Lyell's advice seriously despite the fact that he had absolutely no wish to comply. He wanted to get on with developing his theory and begin drafting chapters for his "big species book." Nevertheless, he acquiesced to Lyell's suggestion, and although he eventually abandoned the task of writing a sketch, he spent some months agonizing over how exactly he might do it. Why would Darwin treat a casual "wish" of Lyell's so seriously, especially one that he had absolutely no appetite for? If it wasn't Lyell's insistence that forced Darwin to spend months worrying about how best to accomplish a task he saw as an unnecessary diversion from his "big book" work, what was it? It is worth noting that the 1-2 May letter from Lyell containing the "offhand" publication advice appears to be a repeat request:

Some year or so ago, you recommended me to read a paper by Wallace in the Annals, which had interested you & as I was writing to him, I knew this would please him much, so I told him. He has to day sent me the enclosed & asked me to forward it to you. It seems to me well worth reading. Your words have come true with a vengeance that I sh^d. be forestalled. You said this when I explained to you here very briefly my views of "Natural Selection" depending on the Struggle for existence. (18 [June 1858], DCP2285, accessed 9th July 2019)

In footnote 4, the editors of the Darwin Correspondence Project state that the final sentence quoted above refers to Lyell's April 1856 visit with Darwin. Although Wallace is also explicitly mentioned, the relationship, if any, between him and Lyell's warning is open to inter-

pretation. Unfortunately, we are missing Lyell's initial, and possibly more directed, advice to Darwin. Nevertheless, it was, as van Wyhe notes, "quite sufficient that Darwin explained his theory and Lyell replied that such an idea could be hit on by someone else if Darwin carried on with further years of detailed research" (p. 64). The plausibility of this non-targeted explanation is boosted by omission of the evidence that Lyell immediately and explicitly linked Darwin's theory with Wallace's paper in his notebook, evidence that suggests the "someone else" was Wallace.

As van Wyhe points out, from the evidence we have of Lyell's publication advice to Darwin, it did not directly follow his reading of the paper and his Index Books entries. How could it, before Darwin had explained his theory to Lyell? This occurred some five months later, at which point Lyell immediately returned to a consideration of Wallace's paper. Lyell's "Scientific Journal" has the entry:

April 16, 1856

With Darwin: On the Formation of Species by Natural Selection

...The reason why Mr. Wallace [s] introduction of species, most allied to those immediately preceding in time, or that new species was in each geol. period akin to species of the period immediately antecedent, seems explained by the Natural Selection Theory. (Wilson, 1970, pp. 54-55)

"In other words," as van Wyhe paraphrases, "a topic that Lyell had found curious and striking is recorded as explained by what Darwin imparted during their conversation" (p. 63). Replacement of Lyell's words, "the Natural Selection Theory" with "what Darwin imparted during their conversation," subtly adds an unmerited element of plausibility to van Wyhe's reading of this note. He maintains "that it was not the role of natural selection that explained this relationship [the one he had read into Wallace's paper], it was the direct descent or derivation of new species from ancestors" (p. 63). End of discussion, until we acknowledge that this is no more than possibly true. Only then can another possible and more straightforward reading be entertained: Lyell wrote "Natural Selection Theory" so perhaps he was really referring to the action of natural selection as an explanation of the evolutionary inference that he had earlier read into Wallace's paper. Did Lyell quite possibly see "Natural Selection Theory" as a plausible candidate for the "great natural law" that Wallace said was necessary, but unknown and therefore not included in his paper? However, van Wyhe progresses his argument to the point of "no evidence" for this option: "As Lyell thought this 'explained' the Wallace observation, this shows that Lyell did not see an explanation in the Wallace paper itself. Hence we have no reason to assume ... that Lyell would see Wallace as a potential competitor or rival with Darwin. There would have been nothing to warn Darwin about as far as Wallace's paper was concerned. And indeed there is no evidence that he did" (pp. 63-64).

It is true that there is, as they say, "no smoking gun" to confirm the commonly held view, but then there very seldom is. But this is not to say that "there is no evidence" to support Assertion 5. Indeed, there is an impressive breadth of supportive evidence:

- There is evidence that Wallace's paper struck Lyell as curious and interesting: uniquely so, it can be argued from the absence of any similar notebook treatments of other evolutionary writings.
- Lyell's explicit linkage of it to "Lamarck's doctrine" is evidence suggesting that he did extract the evolutionary inferences. Although van Wyhe claims: "we know from his notebook, the paper certainly did interest Lyell, but not as an evolutionary theory" (p. 64) – what he had earlier called "a plausible contemporary reading" (p. 63) has meta-

morphosed into a declaration of truth, one that is somewhat at odds with the evidence.

- On learning of Darwin's natural selection theory, some five months after his first noted interest in Wallace's paper, Lyell immediately noted down a relationship between it and Wallace's paper (and to no one else's). This is further evidence that Wallace, and only Wallace, was called to mind by Darwin's revelations of how organic change might be accomplished by purely natural means.
- Finally, there is clear evidence that Lyell did indeed advise Darwin to publish a sketch of his theorizing precisely because he foresaw the likelihood of him being forestalled.

Combine all these evidential elements, and although it is not inevitable that Wallace or anyone in particular was the cause of Lyell's suggestion to Darwin, Wallace is, at the very least, a likely candidate with no similarly likely competitors. It is just plain wrong to claim that the possibility of a linkage between Lyell's warning to Darwin and Wallace is made "without any evidence" (p. 64). It certainly cannot be promoted as an absolute fact, but neither can it be dismissed as a conjecture "without any evidence," a groundless modern invention.

One further element of van Wyhe's dismissal of Assertion 5 is based on the absence of either Darwin's or Lyell's naming of Wallace as the expected forestaller when Lyell's publication advice proved to have been prescient. It is pointed out that when Darwin wrote to Lyell he said that he had, as Lyell had predicted, been forestalled. However, he did not also add something along the lines of: it is "the very same man" (p. 64) you warned me about. If we neglect all the associated connections with, and mentions of, Wallace (and only Wallace), the entirely plausible reason offered by van Wyhe is that Wallace was not explicitly identified as the forestaller because he had never figured in Lyell's warning. The predicted priority challenge arrived and it just happened to have been written by someone called Wallace. Alternatively, if Wallace alone had been identified as the likely forestaller, then the arrival of his Ternate paper would still have been a shock for Darwin, but not the identity of its author. In this case, Darwin's failure to name Wallace explicitly as the anticipated forestaller was precisely because he was the expected threat, and therefore no surprise to him or to Lyell. From the evidence that, in a frantic exchange of letters focused on securing Darwin's priority, Wallace was not explicitly named as the forestaller, although mentioned twice in Darwin's initial letter, either explanation – Wallace as the author was unanticipated and irrelevant, or explicitly foreseen and therefore expected – is possible. But from such a flimsy and convoluted element of evidence, composed in a rush by a severely agitated man, no solid inference is warranted. Indeed, had Wallace never been explicitly linked by Lyell to his warning, he was, nevertheless, a name that had been passed between Lyell and Darwin, a name almost certainly communicated in the context of Darwin's theorizing.²⁶ A failure to explicitly name Wallace as the forestaller (although mentioning him several times) is just as much evidence that he wasn't surprised because Wallace was the anticipated forestaller, as evidence that he wasn't surprised because no one had been singled out as the likely forestaller. It might well be argued that the latter option is the more unlikely because Wallace was known to both men, primarily, if not quite solely,²⁷ in the context of evolutionary theorizing – he could hardly have been an irrelevant nobody in this situation.

Van Wyhe concludes his dismissal of this assertion with:

What we do not find in the primary sources is any evidence for the widely held belief that Lyell told Darwin about the Sarawak Law paper as a form of warning or that it had any connection with Lyell's recommendation that Darwin publish an outline of his theory before he was forestalled. (p. 64)

Again we see the absence of direct evidence confused with the absence of any evidence.

Were “smoking guns” the only form of legitimate evidence for historical interpretations, the past would not be a foreign country.²⁸ It would be a distant planet – known to have existed but little else. The discipline of history would necessarily be very thin, like our certain understanding of the meanings of day-to-day activities on Mars.

The multiplicity of interconnections among our three principals (to the exclusion of all others) that revolved around the theorizing that led to Darwin’s publication of the *Origin* make Wallace’s deduced pivotal role eminently plausible.

5a. *Lyell advised Darwin to publish quite probably because of Wallace.*

“6. Darwin wrote to Wallace to warn him off his patch” (p. 65)

Maybe. Darwin was less than open and forthright with Wallace (as any scientist is likely to be with a barely-known correspondent when in the midst of developing a major theory). Much of their correspondence prior to the *Origin* is lost, which leaves this as an unhelpfully open issue (and the least important assertion with regard to its potential impact on the development of evolution theory). Van Wyhe chose to start from an earlier plausible inference, once more shorn of probability and transformed into a fact: “as demonstrated above, Darwin had no idea that Wallace was approaching his patch” (p. 65). At a stroke, Darwin could hardly have issued a warning for something that he “had no idea” about. However, if we entertain other plausible inferences aired earlier, unsurprisingly, we find this assertion is not simply dismissible, and that Darwin’s known letters to Wallace can indeed be plausibly interpreted as carrying a mild warning.

6a. *Darwin’s letters to Wallace do contain oddities that are not encouragements.*

Conclusions

The past is not a country about which we can interpret very much with certainty. It must be, as L. P. Hartley observed, somewhat foreign. Much of what exactly went on there and especially the underlying reasons will always be open to conjecture. They provide the inexhaustible material for historical analysis, speculation, argument and surmise. What they do not provide is a “uniquely correct” historical interpretation which would, of course, close down the essence of the discipline. History is a discussion, a quest to identify more accurate and ultimately, possibly correct interpretations. But as with “original historical meanings,” the potential existence of correct interpretations is one thing, but identification as such is quite another.

As van Wyhe’s prefacing quotation from Darwin declares, it is sometimes valuable to expose errors. It is, however, even more valuable to expose possible alternative interpretations of historical events, especially if each is presented with its supporting argument. This enables the interested observer to weigh the options as well as to propose adjustments to the analysis in the light of new information. What is not valuable is to present necessarily probabilistic inferences, such as interpretations of historical events, as correct deductions of the “original historical meanings” (p. 65) which requires that all the alternative inferences are erroneous.

As a final example, consider the accusation “that Darwin borrowed, without acknowledgement, his ‘principle of divergence’” (p. 65) from Wallace’s writings. It has, we are told by van Wyhe, been “unequivocally refuted” (p. 65) by Kohn (1981) (although not so-claimed by Kohn). This assertion closes the door on the possibility of re-considering this borrowing until the impossibility of “unequivocal” argument in historical reconstruction is acknowledged. Actually, Kohn wrote only that “Brackman’s book [the very specific form of the

accusation that Kohn challenged] fails to establish its thesis.”²⁹ Kohn’s paper is a vigorous rebuttal of the plagiarism accusation couched in Brackman’s sometimes cavalier style, but it cannot be unchallengeable, not with respect to Brackman’s particular argument, and even less so with respect to the accusation in general. An unequivocal refutation it is not, and can never be.

Although it can be excessively tedious and expansive to couch every statement within an explicit admission of uncertainty, a worthwhile analysis within a domain of complex context sensitivity, such as interpretations of history, must strive to avoid even implications of unwarranted certainty. The undoubted value of identifying errors is undermined when the value of inescapable uncertainty is abandoned for supposedly clear truth or error. When the allegedly decisive outcomes are generated by a strategy in which plausible early conjectures are later re-introduced as certainties, then the lost value becomes even harder to pin down.

Notes

1. A. R. Wallace (1855).
2. van Wyhe (2016).
3. When the context is obvious the many references to van Wyhe’s “reassessment” and to Wallace’s Sarawak Law paper will be restricted to page numbers. As the former is paginated from 56 to 66, and Wallace’s from 184 to 196 there can be no ambiguity.
4. Karl Popper’s *The Logic of Scientific Discovery* famously made the case for the non-equivalence of proof and refutation; i.e., that denial of a proposition is possible but verification of its truth is not. However, in a domain as far divorced from formal logic as speculating on the understandings of historical figures, Popper’s analysis has little relevance.
5. The consequence is “our expulsion from Boole’s two-valued paradise” and a necessity to operate within the difficulties of “relative plausibility” or “degrees of probability,” neither of which admit solid and satisfactory formal foundations (van Deemter 2010, p. 189).
6. As with many other concepts such as “original historical meanings” potential existence is one thing, but certain discovery is quite another.
7. Although explicitly tied to the phenomenon of rudimentary organs at this point in his paper, a few sentences later Wallace explicitly broadened his “great law” to cover the regulation of all life.
8. Charles Darwin’s 1844 essay was reprinted by his son Francis (Darwin 1909, pp. 57-255). Examples of the emergent difficulties that Darwin became aware of are: his realization that beings in a state of nature were *not* subject to little variation (a shift noted in Darwin 1909, footnote 1, p. 59), and learning of islands that contained more than one endemic species of the same genus which contradicted his Essay claims based on the assumed importance of isolation in new species formation (letter to Hooker [11-12 July 1845], DCP889). These DCP numbers refer to letters in the Darwin Correspondence Project database, <https://www.darwinproject.ac.uk/letters>.
9. Not all negations are opposites, the opposite of “black” is “white,” but “non-black” may be any color other than “black.”
10. See, for example, Smith (2008).
11. Despite this prose being initially driven by Wallace’s misunderstanding of vestigial organs, as the quotation reveals, his appreciation of the necessity for an unknown “great natural law” extended to all

life on earth.

12. A letter from Edward Blyth to Darwin, 8 December 1855, DCP1792.

13. Wilson (1970), p. 66.

14. A letter from Darwin to Lyell [25 June 1858], DCP2294: "There is nothing in Wallace's sketch which is not written out much fuller in my sketch copied in 1844."

15. Barlow ed. (1958), p. 124.

16. Ospovat (1981), p. 210.

17. According to McKinney (1972b, p. 117), Darwin "read and annotated Wallace's paper shortly after it was published in 1855. Furthermore, he took notes on blue paper and pinned them in the back of his copy of the December issue of 1855."

18. Costa (2014, p. 146) pointed out Fichman's observation that Lyellian geology "permeates" (Fichman 2004, p. 34) the Sarawak Law paper, and tabulated the correspondences (Costa 2014, p. 176). Costa continued, describing it as "a fact that resonates with the lengthy critique of Lyell found early in Wallace's Species Notebook. Lyell as both inspiration and foil is in evidence in this paper," (Costa 2014, p. 146) as Wallace later confirmed in his autobiography. The immediate original-source evidence is in Wallace's "Species Notebook" where he systematically penned responses to Lyell's anti-evolution rhetoric (transcribed and annotated by Costa 2013a; and further discussed in Costa 2013b).

19. *The Vestiges of the Natural History of Creation* was published anonymously by Robert Chambers, first in 1844. It was a widely read and heavily criticized tract on evolution that nevertheless went through twelve editions spanning four decades.

20. "Evolution" and "anti-transmutation" are not simple, absolute opposites. Even prior to reading Wallace's paper, Lyell was toying with the possibility of some minor degree of "evolution" as a possible secondary agency to the divine, "First Cause" (Lyell 1853, p. 578).

21. Wilson (1970), p. 65.

22. Wilson, (1970), p. 66.

23. Lyell's concern argues for some degree in belief in the possibility of evolution because surely Lyell would not encourage his friend to publish (what he believed to be) a totally erroneous theory. Several scholars have addressed this "puzzling case of Lyell" (Mayr 1991, p. 101). Both Mayr (1991) and Recker (1990) attribute Lyell's actions to his friendship with Darwin. How is advice to publish a theory that you believe is false an act of friendship?

24. Letter from CD to W. D. Fox, 8 [June 1856], DCP1895.

25. Postscript in DCP1874, 11 May 1856 letter to Hooker, this refers to earlier urgent advice to publish on corals but the "again" clearly means new advice about publishing a sketch was also "urgent."

26. In addition, Darwin had been in correspondence with Wallace explicitly concerning their evolution theorizing, e.g., letter to Wallace, 22 December 1857, DCP2192; and Darwin's letter to Lyell 18 [June 1858], DCP2285, in which he reminded Lyell of the Wallace connection when he received Wallace's Ternate paper.

27. It appears that Darwin first contacted Wallace as a potential supplier of specimens but their correspondence at this time, insofar as it has been found, centers on evolution theorizing.

28. "The past is a foreign country: they do things differently there" is the opening of L. P. Hartley's *The*

Go-Between, published in 1953. This much-re-used metaphor points up the many uncertainties in any re-telling of history, which might well include the impossibility of identifying “original historical meanings.”

29. Brackman (1980). Although this example tends to evoke extreme reactions because of its association with conspiracy theories, that fraught domain is entirely outside the point of my objection to van Wyhe’s argument. The fact that Kohn’s argument is not an unequivocal refutation, asserts nothing about the likely truth of the accusation, except that absolute certainty either way is not an option. Incidentally, Kohn’s conclusion that “Brackman’s book fails to establish its thesis” (Kohn 1981, p. 1105) is very far from an unequivocal refutation. Indeed, it almost invites an attempt to establish the thesis!

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