The Joint Essay of Darwin and Wallace

Following his return from Para (now Belém, Brasil) in July 1852 Wallace rented a house in Upper Albany Street (44) close to the Zoological Gardens, Regent's Park, where over the next eighteen months he occupied himself writing up his travels on the Amazon and Rio Negro and with naming and sorting out the collections which he had sent back to his friend and agent – Samuel Stevens.

During this period he went frequently to the British Museum to make notes and sketches of the rarer and more valuable species of bird, beetle and butterfly of the various Malay Islands in preparation for his next expedition to the Malayan Archipelago. It was on one such visit in early 1854, shortly before he sailed for Singapore, that he briefly met Charles Darwin.

Wallace's next contact with Darwin was through his agent Samuel Stevens who communicated the contents of the following memorandum to him in Sarawak in February 1856.

C.D. Memorandum Dec. 1855

> "Skins. Any domestic breed or race, of Poultry, Pigeons, Rabbits, Cats and even dogs, if not too large, which has been bred for many generations in any little visited region, would be of great value, or even if recently imported from any unfrequented region. It w^{d}. be necessary to notice & select a characteristic specimen of, adult animal of any breed. In Poultry – both cock & hen & especially the cock sh^{d}. be procured. The whole humerus and femur, & as much as possible of the cranium sh^{d}. be left in the skins. – Each specimen sh^{d}. be ticketed with native name, habitat & any procurable information. Specimens not bred for many generations in domestication of no value–".

Meanwhile Wallace had reached Sarawak early in November 1854. Unfortunately it was the start of the four-month wet season when collecting was out of the question. Thus with little else to do he began to put down on paper his facts and ideas on the geographical distribution of plants and animals. This then became his first great essay entitled "On the Law which has regulated the Introduction of New Species" – in which he showed that natural classification as well as geological and biogeographical evidence, are consistent with the law –

> "Every species has come into existence coincident in time and space with a pre-existing closely allied species".

He later claimed in a letter to Bates (Amboyna: Jan 4, 1858) that

> "it was the promulgation of Forbe's theory[1] of 'polarity' which led me to write and publish, for I was annoyed to see such an ideal absurdity put forth when such a simple hypothesis will explain the facts."

1. Forbes concluded (Anniversary Address of the President of the Geological Society 1854) "That the relation between the palaeozoic and neozoic life-assemblages is one of development in opposite directions, in other words of Polarity." Darwin commented (letter to Hooker: 7 July, 1854) "It is very strange, but I think Forbes is rather fanciful, his 'polarity' makes me sick – it is like "magnetism" turning a table."

Indeed in his paper which he sent to the *Annals and Magazine of Natural History* and in which it appeared the following September (1855) he quotes Forbes's theory on four occasions while at the same time pointing out that

> "it is about ten years since the idea of such a law suggested itself."

Charles Lyell was greatly impressed by Wallace's paper and in April 1856, when visiting Down, recommended Darwin to read it while Edward Blyth (Calcutta: December 8, 1855) pointed out to Darwin that

> "Wallace has, I think, put the matter well; and according to his theory, the various domestic races of animals have been fairly developed into species. Mr. Wallace could also well support his views by reference to the *Helices* & *Bulimi* collected in the Philippines by Cuming – & also to the varieties of the Indian *Melaniae.* What do you think of the paper in question? Has it at all unsettled your ideas regarding the persistence of species, – not perhaps so much from novelty of argument, as by the lucid collation of facts & phenomena."

Darwin's views were eventually summed up by the following interleaved notes in his own copy of the *Annals and Magazine of Natural History,* in which Wallace 1855 appeared:

> "185 Wallace's paper: Laws of Geograph. Distrib. nothing very new – 186 His general summary "Every species has come into existence coincident in time & space with preexisting species." – Uses my simile of tree – It seems all creation with him – Alludes to Galapagos 189 on even adjoining species being closest – (It is all creation, but why does *[interl above* 'is'] his law hold good; he puts the facts in striking point of view – 194 Argues against our supposed geological perfect knowledge – Explains Rudimentary organs on same idea (I sh^{d}. state that put generation for creation & I quite agree)."

Wallace remained in Borneo for the next 15 months and in early 1856 visited the islands of Bali and Lombok on his way to the Celebes. From there (Ampanam) while waiting to get to Macassar he wrote to his agent Mr. Stevens on August 21, 1856 telling him that he had sent off to Singapore a case with collections containing 300 birds, 100 shells and 465 insects.

> "There are also some good specimens of the pigeons & generally the birds are very good specimens & I have taken great care to pack them. The domestic duck var. is for Mr. C. Darwin & he would perhaps also like the jungle cock, which is often domesticated here & is doubtless one of the originals of the domestic breed of poultry."

After two months and a half in Lombok[2] he left for Macassar and the Celebes which he reached at the beginning of September 1856 and where he remained for the next three months. On October 10, 1856 he wrote his first letter to Darwin no doubt informing him of his collections recently despatched from Singapore while at the same time discussing the whole concept of domestication. Darwin in his reply (May 1, 1857) says:

> "I have acted already in accordance with your advice of keeping domestic varieties

2 During which time he was able to determine the exact boundary between two of the primary zoological regions (= The Wallace Line).

> & those appearing in a state of nature, distinct; but I have sometimes doubted of the wisdom of this, & therefore I am glad to be backed by your opinion. – I must confess, however, I rather doubt the truth of the now very prevalent doctrine of all our domestic animals having descended from several wild stocks; though I do not doubt that it is so in some cases. – I think there is rather better evidence on the sterility of Hybrid animals than you seem to admit: & in regard to Plants the collection of carefully recorded facts by Kolreuter & Grertner, (& Herbert) is *enormous.*"

Wallace must also have raised the question of the effect of climate on variation since Darwin further replied:

> "I most certainly agree with you on the little effects of "climatal conditions", which one sees referred to ad nauseam in all Books; I suppose some very little effect must be attributed to such influences, but I fully believe that they are very slight.– It is really impossible to explain my views in the compass of a letter on the causes & means of variation in a state of nature; but I have slowly adopted a distinct & tangible idea.– Whether true or false others must judge; for the firmest conviction of the truth of a doctrine by its author, seems, alas, not to be slightest guarantee of truth.–"

However, it was the opening paragraph of Darwin's reply that must have given Wallace the greatest encouragement:

> "By your letter & even still more by your paper in Annals, a year or more ago, I can plainly see that we have thought much alike & to a certain extent have come to similar conclusions. In regard to the Paper in Annals, I agree to the truth of almost every word of your paper; & I daresay that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper; for it is lamentable how each man draws his own different conclusions from the very same fact. –
>
> This summer will make the 20th year (!) since I opened my first-note-book, on the question how & in what way do species & varieties differ from each other. – I am now preparing my work for publication, but I find the subject so very large, that though I have written many chapters, I do not suppose I shall go to press for two years.–"

After writing to Darwin, Wallace left Macassar in December 1856 for the celebrated Aru Islands with their birds of paradise. It was only on his return to Macassar in the following summer that he found Darwin's reply to his first letter awaiting him.

Wallace wrote again to Darwin almost immediately (27 September, 1857). Unfortunately only fragments of this letter remain, nevertheless as with his first letter to Darwin, we are able to deduce much of the subject matter from Darwin's subsequent (22 December, 1857) reply.

In one of the fragments that remain Wallace wrote:

> "– of May last, that my views on the order of succession of species were in accordance with your own, for I had begun to be a little disappointed that my paper had neither excited discussion nor even elicited opposition."[3]

He then goes on to tell Darwin that he intends to pursue his theory further:

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3 Stevens had written to him after his article had appeared pointing out that he had heard several naturalists express regret that Wallace was theorising instead of collecting more facts.

> "The mere statement & illustration of the theory in that paper is of course but preliminary to an attempt at a detailed proof of it, the plan of which I have arranged, & in part written, but which of course requires much <research in> libraries & collections, a labour which I look - - -"

Charles Darwin – aged 40 (1849)

Darwin in reply told him that the paper had been noticed and that his (Darwin's) attention had been specially called to it by both Lyell and Blyth. At the same time he was quick to reiterate that he had been working on the same problem and would publish on it in a year or so:

> "– My work, on which I have now been at work more or less for 20 years, will not fix or settle anything; but I hope it will aid by giving a large collection of facts with one definite end: I get on very slowly, partly from ill-health, partly from being a very slow worker. – I have got about half written; but I do not suppose I shall publish under a couple of years. I have now been three whole months on one chapter on Hybridism!"

Elsewhere in his letter Wallace mentions his forthcoming paper on the Aru Islands [*Annals and Magazine of Natural History,* suppl. to 20, Dec. 1857] in which he subscribes to the view they once formed part of New Guinea and that subsidence now accounted for their separation. Darwin agreed:

> "I shall be quite prepared to subscribe to your doctrine of subsidence: indeed from the quite independent evidence of the Coral Reefs 1 coloured my original map in my Coral volume of the Arru Isld. as one of subsidence, but got frightened & left it uncoloured.-"

Alfred Wallace – aged 30 (1853)

On the problem of the formation of oceanic islands, however, he disagreed:

> "But I can see that you are inclined to go much further than I am in regard to the former connections of oceanic islands with continent: - - - you have my very sincere & cordial good wishes for success of all kinds; & may all your theories succeed, except that on oceanic islands, on which subject I will do battle to the death."

In December after finishing arranging and dispatching his Aru collections, Wallace embarked for Amboyna from where he wrote to his friend Bates (January 4, 1858):

> "To persons who have not thought much on the subject I fear my paper on the

> "Succession of Species" will not appear so clear as it does to you. That paper is, of course, merely the announcement of the theory, not its development. I have prepared a plan and written portions of a work embracing the whole subject, and have endeavoured to prove in detail what I have as yet only indicated. - - - I have been much gratified by a letter from Darwin, in which he says that he agreed with 'almost every word of my paper.' He is now preparing his great work on 'Species and Varieties,' for which he has been preparing materials for twenty years. He may save me the trouble of writing more on my hypothesis, by proving that there is no difference in nature between the origin of species and of varieties; or he may give me trouble by arriving at another conclusion; but, at all events, his facts will be given for me to work upon. Your collections and my own will furnish most valuable material to illustrate and prove the universal application of the hypothesis. The connection between the succession of affinities and the geographical distribution of a group, worked out species by species, has never yet been shown as we shall be able to show it."

Wallace finished the letter after his arrival at Ternate (January 25, 1858) when he enclosed a memorandum of his estimate of the number of distinct species of insects he had collected:

"Butterflies	620 species
Moths	2,000 species
Beetles	3,700 species
Bees, wasps etc.	750 species
Flies	660 species
Bugs, cicadas etc.	500 species
Locusts, etc.	160 species
Dragonflies, etc.	110 species
Earwigs, etc.	40 species
Total	8,540 species"

A few weeks later while waiting in Ternate in order to decide where he should go next on his journey, Wallace was prostrated by a sharp attack of intermittent fever which lasted several days. As with his enforced rest in Sarawak, 1854, he had nothing to do but to think over those problems particularly interesting to him, such as natural succession and descent, and speciation:

> "and one day, while lying on my bed during a cold fit, wrapped in blankets though the thermometer was at 88° Fahr., the problem again presented itself to me, and something led me to think of the "positive checks" described by Malthus in his "Essay on Population", a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks – war, disease, famine and the like – must, it occurred to me, act on animals as well as man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact there suddenly flashed upon me the idea of the survival of the fittest – that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over, I had thought out almost the whole of the theory; and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin." (see Wallace 1908; – *probably posted sometime between* 5 & 19 *March*, 1858).

Prior to their first meeting in the British Museum (1854) both Darwin and Wallace had independently arrived at the conclusion that the doctrine of the fixity of species was false. Darwin had inferred it in March 1837 (aged 28) following John Gould's identification of the Galapagos birds (particularly the mocking birds; see Sulloway 1984: 42) while Wallace adopted a transmutationist hypothesis about 1845 (aged 23) while living in Neath (see letter to Bates December 28,1845 and Picture Quiz above).

Darwin, in his autobiography, states that he opened his first note-book on transmutation in July 1837. He continues:

> "In October, 1838, that is, fifteen months after I had begun my systematic inquiry, I happened to read for amusement, "Malthus on Population," and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observations of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The results of this would be the formation of new species. Here, then, I had at last got a theory by which to work; but I was so anxious to avoid prejudice that I determined not for some time to write even the briefest sketch of it. In June, 1842, I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil, in thirty-five pages, and this was enlarged during the summer of 1844 into one of 230 pages, which I had fairly copied out and still possess."

The completed abstract was eventually shown to his two friends and confidants - Charles Lyell and Joseph Hooker. Following the completion of his geological works (1842, 1844) and the second edition of *Journal of Researches*, he spent a further eight years producing his monumental two volume book on cirripedes ("I hate a Barnacle as no man ever did before, not even a sailor in a slow sailing ship"). But from September 1854 he devoted his whole time to arranging his huge pile of notes, to observing, and to experimenting in relation to the transmutation of species. Thus in early 1855 he returned to the problem of the geographical distribution of plants and began experimenting by immersing seeds in salt water – concluding that seeds could be transported across wide spaces of ocean and still be capable of germinating. The results formed the basis for his first paper to the Linnean Society on May 6, 1856 entitled "*On the action of Sea-water on the Germination of Seeds*".[4]

On April 25, 1855 Darwin also started communicating with Asa Gray who had pointed out (in his reviews, 1840, 1846 of Zuccarini's *Flora Japonica)* that an unusual number of the plants of western North America were also to be found in eastern Asia and nowhere else (particularly the magnolias!). In his second letter to Gray on June 8, 1855, Darwin sent him a copy of his first note on his experiments with sea-water and seed germination, published in *Gardeners Chronicle*, May 26, 1855.

On receipt of the first part of Gray's *Statistics of the Flora of the United States*, 1856-7, Darwin replied (October 12, 1856):

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4 In May 1855 the Secretary (J.J. Bennett) and J.D. Hooker submitted a resolution that in future papers communicated for the Proceedings should be printed in full, given a printed cover and registered as a Periodical. The outcome was the appearence on 1 March, 1856 of a new octavo publication entitled *The Journal of the Proceedings of the Linnean Society* whose great advantage was rapid publication.

> "Nothing has surprised me more than the greater generic & specific affinity with E.Asia than with W.America. Can you tell me (& I will promise to inflict no other question) whether climate explains this greater affinity? or is it one of the many utterly inexplicable problems in Bot. Geography? Is E.Asia nearly as well known as W.America? so that does the state of knowledge allow a pretty fair comparison?"

At this stage Darwin presumably felt he needed to let Gray into his secret in order to continue receiving his valuable comments on North American botanical geography. Thus when he next wrote (20 July, 1857) he told Gray of his hypothesis on speciation and then in the following letter (5 September, 1857) because Gray seemed interested:

> "as it is an immense advantage to me to write to you & to hear **ever so briefly**, what you think, I will enclose (copied so as to save you trouble in reading) the briefest abstract of my notions on the **means** by which nature makes her species. Why I think that species have really changed depends on general facts in the affinities, embryology, rudimentary organs, geological history & geographical distributions of organic beings. In regard to my abstract you must take immensely on trust; each paragraph occupying one or two chapters in my Book. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if anyone, like the Author of the Vestiges, were to hear of them, he might easily work them in, & then I sh[d]. have to quote from a work perhaps despised by naturalists & this would greatly injure any chance of my views being received by those alone whose opinion I value.–"

The enclosed sketch, "most imperfect" was essentially that shown to Lyell and Hooker in 1844 with the addition of a statement on the principle of divergence

> "that the varying offspring of each species will try – only few will succeed – to seize as many and as diverse places in the economy of nature as possible".

Thus in September 1857 Gray entered the circle of initiates into the concept of natural selection, but with the rider not to mention it to anyone else.

Early in 1856, probably as a result of reading Wallace's 1855 paper Lyell urged Darwin to write out his views "pretty fully" and to publish. Darwin immediately began to do so

> "on a scale three or four times as extensive as that which was afterwards followed in my *Origin of Species;* yet it was only an abstract of the material I had collected."

This work was steadily continued so that by the time Wallace's MS arrived from Ternate in May/June[5] 1858, overthrowing all his plans, Darwin had completed some ten chapters (about half a projected book).

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5 Unfortunately Wallace's letter to Darwin together with its enclosure have disappeared as have the letters from Lyell and Hooker relating to it. The only documentary evidence is contained in Darwin's letter to Lyell & Hooker and Wallace's autobiography 1905, **1**: 363. As Wallace later remarked July I, 1908: ". . . and, without any apparent warning, my letter, with the enclosed Essay, came upon him, like a thunderbolt from a cloudless sky!"

To Charles Lyell June 1858

Down Bromley Kent

My dear Lyell 18th.

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 shd. be forestalled. You said this when I explained to you here very briefly my views of "Natural Selection" depending on the Struggle for existence. – I never saw a more striking coincidence. If Wallace had my M.S. sketch written out in 1842 he could not have made a better short abstract! Even his terms now stand as Heads of my Chapters.

Please return me the M.S. which he does not say he wishes me to publish; but I shall of course at once write & offer to send to any Journals. So all my originality, whatever it may amount to, will be smashed. Though my Book, if it will ever have any value, will not be deteriorated; as all the labour consists in the application of the theory.

I hope you will approve of Wallace's sketch, that I may tell him what you say. My dear Lyell | Yours most truly | C. Darwin

That same day (June 18, 1858) Darwin forwarded the paper to Lyell. At the same time he wrote to Hooker telling him of Wallace's letter and its enclosure and explicitly announcing his resolve to abandon all claim to priority for his own "outline of evolution". He also admitted to having written a letter to Wallace (see letter to Wallace of April 16, 1859)

"Stating that I would not publish anything before you had published",

but that before he could post it he had received Hooker's reply reminding him that both he and Lyell had read Darwin's outline 14 years previously and that they should not withhold this knowledge of his (Darwin's) priority. Hooker then suggested a compromise: the simultaneous publication of the two, moreover if he Darwin agreed he would write to Wallace accordingly.

Darwin agreed provided Lyell also supported his claim. Darwin wrote to Lyell the following week (June 25, 1858) with a postscript the next day requesting that if Lyell supported his claim to priority the answer be sent to Hooker (Darwin also enclosed the letter from Wallace that had accompanied his MS).

As we all know, the answer was in the affirmative and so Darwin allowed Hooker and Lyell to offer a joint paper from himself and Wallace to the Linnean Society on July 1 1858: "*On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection.*"

Darwin's contribution included extracts from his 1844 MS on Species (4 pages) and the abstract he had sent to Asa Gray the previous September (3 pages). This abstract with minor emendations and changes in punctuation, was published in its entirety (it was not an abstract of a letter as suggested in our *Proceedings*, 3:45).

Both Hooker and Lyell had realised the urgency of the publication of the joint paper. As fate would have it the last meeting of the Session of the Linnean Society, normally held in the middle of June (17 June), had been adjourned as a mark of respect

following the death of its past President, Robert Brown. Consequently the Society was forced to intercalate a Special Meeting in order to elect a successor to Brown on Council (the successor was Bentham). This special meeting was scheduled for July I, 1858.

Meantime on June 29, 1858 Darwin wrote to Hooker in acute distress, he himself was ill, scarlet fever was raging in his family, one of his daughters had diphtheria and his infant son had died the previous day.

That same night though quite prostrated he wrote again to Hooker sending the necessary papers for Hooker to complete the prefatory statement.

The joint letter from Hooker and Lyell was sent to the Secretary, J.J. Bennett on 30 June together with the Darwin-Wallace papers. Hooker later emphasised that "no fourth individual [beyond Darwin, Lyell and himself] had any cognisance of our proceedings". In those days abstracts or even titles of papers to be read were not sent out in advance thus the Darwin-Wallace papers which headed the list must have come as a surprise item to almost all of the 30+ fellows present.[6] The Secretary (as was the custom) first read the Darwin-Wallace papers followed by five of six papers that had been postponed from the 17 June meeting dealing with the organisation of *Phoronis hippocrepis*, observations on *Ammocaetus*, a new genus of *Cucurbitales*, a new genus named *Hanburia*, the "Nueva Quinologia" by the late Prof. Pavon and two letters on the vegetation of Angola.

The sixth paper was to have been by Bentham but after hearing the first two papers he was so perturbed that he withdrew unread his paper on the British Flora (supporting the fixity of species).

Darwin, owing to his illness and distress, could not be present, while Wallace was on the north coast of New Guinea. However, both Lyell and Hooker were there and according to Hooker (many years later) they made a few remarks before Bennett read the propitious papers – impressing on those present the necessity of profound attention and for giving their most careful consideration to what they were about to hear and its bearing on the future of natural history.

Although the interest excited was intense there was no discussion; Bell (the President) neither made nor called for remarks, and as Bentham pointed out later, Bell would not have allowed anyone else to initiate a discussion of the startling hypothesis so unexpectedly presented and thereby prolong an already long meeting. It appears that the sheer volume of contributions virtually buried the Darwin-Wallace papers while the contained concept of natural selection went over most fellow's heads. Much later Hooker wrote:

Those in attendance included the President, Thomas Bell, Sir Charles Lyell, Dr Joseph Dalton Hooker, Mr Nathaniel B. Ward, Mr John M. Complin, Mr Robert Heward, Dr Fredric D. Dyster, Mr Daniel Oliver jun., Mr Samuel P. Pratt, Mr S. James A. Salter, Mr William Archer, Mr John Ball, Mr John Thomas Syme, Mr Fredrick Curry, Dr William Henry Fitton, Mr Samuel Stevens, Dr William Benjamin Carpenter, Dr Bethold Seeman, Mr Arthur Henfrey, Mr Benjamin W. Hawkins, Dr William John Burchell, Mr George B. Buckton, Mr William M. Buckton and the Associate Member Mr Black. Dr Dyster brought Dr Baly as his guest and Mr Ward brought Dr Melville. Thus in total there were 27 persons present.

> "the subject was too novel and too ominous for the old school to enter the lists before armouring. It was talked over after the meeting "with bated breath". Lyell's approval, and perhaps in a small way mine, as his Lieutenant in the affair, rather overawed those Fellows who would otherwise have flown out against doctrine, and this because we had the vantage ground of being familiar with the authors and their themes."

At the end of the day Lyell and Hooker had achieved their objective – they had persuaded Darwin to publish while at the same time establishing the question of priority through his participation in a joint publication with Wallace. Finally the new octavo *Proceedings* allowed the rapid publication of the Darwin-Wallace theory which proved highly beneficial in its rapid promulgation. The joint communication was printed in our *Journal of Proceedings,* vol. 3 : pp 45-62 on the 20th August, 1858 – within two months of being submitted.

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APPENDIX

Towards the end of July 1858 Darwin began to write, at Hooker's request (see letter to Hooker: July 5, 1858), what he called an 'Abstract' of his hypothesis with the intention of presenting it to the Linnean Society for publication in the Journal of Proceedings. However, unfortunately for us, towards the end of the year he concluded that because of the multifarious facts it contained it would have to be issued as a book, independently of the Society. It was published in 1859 by John Murray under the title: *On the Origin of Species by means of Natural Selection, or the preservation of Favoured Races in the Struggle for Life.*

Charles Robert Darwin was proposed as a Fellow 20 December 1853. His membership form was signed by Thomas Bell, Sylvanous Hanley, J.D. Hooker, Edward Forbes, Robert Brown, J.S. Henslow, J.J. Bennett and Adam White. He was elected on 7 March 1854 and signed the obligation book 2 May 1854.

Alfred Russel Wallace on the other hand was only proposed as a Fellow on 2 November 1871. His membership form was signed by the President, George Bentham, by J.D. Hooker, H.L. Stainton, Alfred Newton, W.H. Flower, J.W. Dunning, G.B. Gray, Edward Sheppard, Alfred Bennett, Albert Mtiller, R. Ainsworth and his old agent

Samuel Stevens (also treasurer of the Ent. Soc.). Wallace was elected on 18 January 1872 whereupon he wrote the following letter:

> 37 Holly House, Barking
> Feb. 3rd, 1872
>
> The Secretary *of* the Linnean Socy.
> Sir,
> I beg to acknowledge the receipt of your communication of yesterday's date informing me of my election to the Fellowship of the Linnean Society . I hope to be able to attend the next meeting in order to be admitted.
>
> I take this opportunity of expressing my sense of the honour the Council have conferred upon me by my unsolicited election, and of thanking them for the resolution subsequently passed in my favour.
> Remain Sir
> Your obedient Sev^{t}.
> Alfred Wallace

Wallace's first, single authored paper to the Society was on "Malayan Papilionidae" (The Prince of the whole lepidopterous order) in March 1864, while his last in 1897 was on "The Problem of Utility" (the basic principle of natural selection). However, at the age of 85 he made a final contribution to our *Proceedings* in the form of a note entitled "Selections from Malthus's Essay on Population which suggested the idea of Natural Selection" (*The Darwin-Wallace Celebration held on 1st July* 1908 by *the Linnean Society of London:* pp 111-118). Apparently this contribution was solicited by the Secretary Benjamin Dayden Jackson to form a fitting end to the Jubilee publication, but perhaps more importantly it shows the influence Malthus continued to have on Wallace's thinking, This influence is alluded to in several of Wallace's letters of that period. Thus he draws Darwin's attention (July 9, 1881) to a book by George on *Progress and Poverty* in which

> "there is also an elaborate discussion on Malthus's 'Principles of Population' to which both you and I have acknowledged ourselves indebted."

And then in a letter of recollections to A. Newton (December 3, 1887) –

> "The most interesting coincidence in the matter, I think, is, that I, as well as Darwin, was led to a theory itself through Malthus – in my case it was his elaborate account of the action of "preventative checks" in keeping down the population of savage races to a tolerably fixed but scanty number."

Finally in his acceptance speech of 1908 when receiving the first Darwin-Wallace Medal he testified that the effect of reading Malthus

> "was analogous to that of friction upon the specially prepared match, producing that flash of insight which led us immediately to the simple but universal law of the survival of the fittest, as the long-sought effective cause of the continuous modification and adaptation of living things."

B.G..G..