with the scientific episode it describes. It raises many questions, not always sharply defined, it offers less incisive analysis than one might hope for, and it expresses some questionable judgments. Also like the polywater literature, it makes intriguing reading and here and there yields nuggets of scientific interest or amusement. One is a biological disproof of polywater attributed to the physicist Richard Feynman: "There is no such thing as polywater because if there were, there would also be an animal which didn't need to eat food. It would just drink water and excrete polywater."

Overall the scientific community can find some solace even from the polywater episode. There was no fraud, and it was demonstrated that there is enough flexibility in current science for investigators to exercise imagination and to follow a hunch. Perhaps the main problem was that our scientific system of "organized skepticism" (in the phrase of R. K. Merton) eroded at the start of the gold rush, permitting publication of a series of hasty, incomplete, and poorly thought-out papers. Once these initial papers were in print the standard for publication on polywater had been lowered, and an alarming pattern set in of communication by preliminary notes and press releases. However, it took only four years from the Lippincott-Stromberg paper that named polywater for even Deryagin to admit in print that his anomalous water was merely a solution of impurities. Gradually the self-correcting nature of scientific research took hold and put an end to the polywater gold rush.

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On the Origin of the Principle of Diversity

A Delicate Arrangement. The Strange Case of Charles Darwin and Alfred Russel Wallace. ARNOLD C. BRACKMAN. Times Books, New York, 1980. xii, 370 pp., illus. $14.95.

Charles Darwin wrote of the *Origin of Species* that it was one long argument. The same might be said of Arnold Brackman's *A Delicate Arrangement*. Unlike the *Origin*, Brackman's book fails to establish its thesis: that Darwin perpetrated a "cover-up and conspiracy" against Alfred Russel Wallace. The cover-up is Darwin's alleged delay in transmitting Wallace's famous 1858 paper to Lyell and concomitant lies to both Lyell and Hooker. The conspiracy, generated by Darwin's cover-up but executed primarily by Lyell and Hooker, consists of attempts to deprive Wallace of his priority over Darwin.

What was there to be covered up? Brackman strongly insinuates, but does not openly charge, that Darwin was guilty of unacknowledged borrowing and perhaps outright theft of the "principle of divergence" from Wallace in the years between 1855 and 1858.

Brackman's insinuations can be refuted, but to do so it is necessary to define the principle of divergence and its importance to Darwin and Wallace. Divergence need only mean that taxa can be arranged in a branched—hence diverging—scheme. Let us call this taxonomic divergence. However, once Darwin and Wallace became convinced of evolution, taxonomic divergence became charged with new significance. It became necessary to explain why evolutionary history shows a divergent pattern and to explain how divergence occurs—in other words to formulate a principle of divergence. For both men this was a problem to be solved by natural selection, and for both men implicit in the origin of divergence was the even more fundamental problem of the origin of new species. Thus deriving a unifying principle that would apply natural selection to the origin of species and divergence became a matter of prime theoretical importance. Darwin and Wallace both responded to the explanatory challenge. But, in my opinion, the chronology and the content of their responses differed strikingly.

Darwin recognized the evolutionary implications of taxonomic divergence soon after becoming a transformist in 1837. Initially he tended to explain species formation by geographic isolation. This, however, was never his exclusive explanation (see D. Kohn, "Theories to work by," *Stud. Hist. Biol.* 4, 67–170 [1980]), and ultimately he became committed to the view that new species can form without geographic isolation. This conclusion was important in setting the stage for his later explanation of divergence. In 1838 he first formulated the idea of natural selection to explain adaptation. His attempts in the 1840's to apply selection to divergence were constrained by two leading assumptions: first that variation in nature is severely limited and second that there is a fixed limit to the amount of life the globe can sustain. The first was a common, though not unchallenged, assumption among naturalists. The second, a reformulation of the idea of the balance of nature, was most forcefully argued by Charles Lyell. By the mid 1850's Darwin had revised both assumptions and was able to derive his principle of divergence. He argued that a locality can support more life if occupied by diverse forms partitioning resources. Thus specialization is advantageous to an organism. Hence natural selection, which explains all adaptation, favors the evolution of new varieties, hence of new species. From this first fork of the branching phylogeny it is a matter of reiteration to generate all of classification. Simply put, niche within niche engenders group within group. Darwin's principle, which he regarded as a "keystone" of his work, is itself a set of nested arguments comprising the idea of natural selection, the idea of speciation without isolation, and the view that the relations among organisms create new evolutionary situations.

When and how Darwin came to formulate the principle of divergence is the subject of intense historical research. It is certain that by September 1857 Darwin sent Asa Gray a fully articulated statement of the principle. Subsequently, Darwin wrote and rewrote extensive treatments of divergence for chapters 4 and 6 of *Natural Selection*, the long version of the *Origin*. But these versions do not differ at all in concept from the September 1857 document. However, as we have seen, the principle of divergence was not a unitary idea. Nor was its formulation a single event. Recent work, based on Darwin's abundant notes from the 1840's and 1850's, shows that Darwin repeatedly deployed versions of the principle in extended discussions of classification and embryology (D. Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838–1859*, Cambridge Univ. Press, in press), geographic distribution (J. Browne, "Darwin's botanical arithmetic and the principle of divergence, 1854–1858," *J. Hist. Biol.* **13**, 53–89 [1980]), and ecological relations (S. Schweber, "Darwin and the political economists: divergence of character," *J. Hist. Biol.* **13**, 195–289 [1980]). In the years of massive research leading up to the construction of the *Origin* the theme of divergence is ever present. The case seems to be that Darwin actually applied limited principles of divergence, tailored to the contemporary state of argument and data in a number of different disciplinary domains, before he was able to abstract all the connections present in the 1857 letter to Asa Gray. The essence of Darwin's principle was a view of nature in which evolution creates new evolutionary opportunities. Formulation of this principle was a long maturational process in which Darwin abandoned his early focus on one global limit for many local opportunities. As we shall see, this was a reversal of perspective that Wallace did not accomplish.

Wallace became an evolutionist after 1845 and published his position in his 1855 paper written in Sarawak. The Sarawak paper states the fact of divergence but does not posit an explanatory principle. Three years later he discovered natural selection, which is clearly presented in the 1858 paper that Wallace sent Darwin from Ternate. In the Ternate paper Wallace also attempted an explanation of divergence. Beyond the principle of natural selection, three assumptions critically determined his treatment. From his tropical experience, he was aware of abundant variation in nature. He seems to have experienced none of Darwin's hesitation on this account. Hence, Wallace was able to proceed directly from natural selection to divergence. Like Darwin, he assumed that new species formed without geographical isolation. However, unlike Darwin in his mature position, Wallace in 1858 assumed that there was an absolute global limit, and indeed a stringent local limit, to the amount of life that can be sustained. Hence, he argued, in a locale new varieties will occur and periodically be forced by circumstances to enter into severe competition with their parental form. As Wallace framed the conditions of this competition, only one "superior" form will survive to exploit the fixed place available: "The superior variety would then alone remain, and on a return to favourable circumstances would rapidly . . . occupy the place of the extinct species and variety. The variety would have replaced the species." From this example, Wallace proceeds to his only statement about divergence in the Ternate paper: "But this new, improved, and populous race might itself, in course of time, give rise to new varieties, exhibiting several diverging modifications. . . . Here, then, we have progression and continued divergence." Clearly, Wallace thought he had explained divergence with this argument. But, equally clearly, he has offered an explanation that is ecologically static, where a new species forms only by the extinction of its parent. There is none of the creation of new evolutionary opportunities by the subdivision of the environment that characterized Darwin's principle of divergence. In sum, Darwin and Wallace offered different explanations of divergence because they made different assumptions about the balance of nature. These assumptions reflected the relative maturation of their theories. In the first flush of discovering natural selection, Darwin in 1838 and Wallace in 1858 drew exactly the same conclusion: the balance of nature operated with such rigor that only one form could stably occupy a locale. By 1858, Darwin had sufficient time to relax this assumption and to formulate the principle of divergence. Wallace did not.

For too long we have treated Darwin and Wallace, since they were cofounders of natural selection, as if they were intellectual Siamese twins. The preceding comparative reconstruction, while recognizing important parallels, suggests that there were equally important contrasts in the development and logic of the two men's thought. Brackman's reconstruction is another matter. He does not distinguish between divergence as a representation of evolutionary history and the principle of divergence as an intricate argument with an equally intricate history. He does not recognize that the principle of divergence is an application of natural selection, and he does not see that Wallace and Darwin derived two fundamentally different principles. Failing to make these distinctions, Brackman treats divergence like a token—a unitary "idea" that Wallace "had" and that Darwin might have stolen. As a consequence, what Brackman finds interesting in the Darwin-Wallace relationship is not intellectual development but priority.

As Brackman tells the story, Wallace's 1855 paper forced Darwin to begin writing *Natural Selection*. There is something to this. Leonard Wilson has shown (*Sir Charles Lyell's Scientific Journals on the Species Question*, Yale Univ. Press, 1970) how deeply the paper impressed Charles Lyell. Under strong pressure from Lyell, Darwin began writing in May 1856. But Brackman claims that Wallace's 1855 paper, which states the fact of divergence, also contains the principle in seed. This he believes frightened and stimulated Darwin. But if Wallace's principle of divergence is an application of natural selection, which he did not grasp until 1858, Brackman's claim is meaningless. There was no principle of divergence in the 1855 paper, nothing to be influenced by, nothing to steal. Darwin's marginalia on the 1855 paper bear this out. Certainly he recognized Wallace as a transformist who "Uses my simile of tree," but he also noted a disqualifying religious tone: "It seems all creation with him." Darwin could not share the enthusiasm of Lyell or Blyth for the paper. He found "Nothing very new"—certainly nothing theoretical that was new to him. He probably saw Wallace as one more naturalist with heterodox but partial views on species. In so doing Darwin badly underestimated not Wallace's position but his potential.

Some three years later, in March 1858, Wallace sent Darwin his fateful Ternate paper. There Darwin learned that his long-nurtured theory of natural selection had been discovered by another man. The arrival of Wallace's paper is high drama. Nevertheless, it was an intellectual non-event. For Darwin learned nothing about the mechanisms of evolution from Wallace's paper. Indeed, as we have seen with regard to divergence, there was a great deal Darwin could have told Wallace. For Brackman, however, the arrival of the Ternate paper is crucial. He strongly implies, though he does not make the claim explicitly, that Darwin stole the principle of divergence after the arrival of Wallace's paper. To support this allegation, Brackman attempts to show that Darwin (i) lied about the date of the paper's arrival and (ii) revised sections of *Natural Selection* dealing with divergence after he received the paper. Obviously, both these claims hinge on knowing the date of arrival of Wallace's paper. Unfortunately no direct evidence on the matter exists. The manuscript of Wallace's paper and his letter to Darwin are lost. We do not know the date they were sent and we have no postmarks to indicate the date they arrived. All we have for certain is Darwin's record of the event. On 8 June 1858, he wrote to Hooker without mentioning the paper. On 18 June (the letter is dated only "18th") he sent Wallace's paper to Lyell, stating it had been received that day. Brackman is not prepared to accept Darwin's word. Most often he suggests the paper arrived a day or two after 3 June. He relies for this date on the evidence presented by Lewis McKinney (*Wallace and Natural Selection*, Yale Univ. Press, 1972). Wallace sent a letter in early March 1858 to Frederick Bates of Leicester. This letter survives and has both London and Leicester postmarks, which McKinney and Brackman read as 3 June 1858. Hence, they argue, Darwin ought to have received Wallace's paper circa 3 June, not 18 June as he claimed to Lyell. I find this an intriguing but inconclusive argument. There is no evidence that the two pieces of mail sailed on the same ship, or if they did that they received identical handling during the several changes of ship en route. If there is a discrepancy, one must be suspicious. But does one suspect Darwin's character or the vagaries of the post? To my mind Brackman's insinuation that Darwin lied is unproven.

If, however, we give Brackman's questionable arrival date of circa 3 June the benefit of the doubt, what is the evidence to support his second critical claim, that Darwin altered the section on divergence in *Natural Selection* with Wallace's paper in hand? The source of evidence is Darwin's pocket "Journal," which, in fact, belies this interpretation. He wrote: "April 14th Discussion on large genera & small & on Divergence & Ch. 6 (Moor Park) finished June 12th—& Bees Cells." It would appear that Darwin wrote both the new discussion in chapter 4 on large and small genera (which strongly reflects the principle of divergence) and the major exposition of divergence in chapter 6 either by 14 April 1858 or during his stay at Moor Park (20 April to 4 May) and that he finished the task on 12 June at Down. This is borne out by his letter of 6 May informing Hooker that he was sending the manuscript on large and small genera that day. The conceptual underpinnings of the relevant portions of chapters 4 and 6 are identical. If anything the discussion in chapter 4 is an application of chapter 6, and it was in Hooker's hands weeks before Wallace's paper arrived. By 8 June Darwin had very likely completed the discussion of divergence in chapter 6, for he offered to "have this discussion copied out" for Hooker. Thus if Wallace's paper arrived circa 3 June, there were at most six days (3 to 8 June) when Darwin *might* have had the paper while he put the finishing touches on the manuscript of chapter 6, which had been composed largely in April and May. It is on these six days that Brackman's claim rests. In support, he makes much of Darwin's 8 June letter to Hooker, where, to quote Brackman's interpretation, Darwin was "elated to report that he had at last resolved the frustrating problem of how species diverged in nature." But Brackman's reading of the letter is incorrect. What Darwin actually wrote was "I will try to leave out all allusion to genera coming in and out in this part, till when I discuss the 'principle of Divergence,' which with 'Natural Selection' is the Key-stone of my Book." Darwin was referring to the organization of his book, not announcing the discovery of a new principle.

In sum, Brackman's claim is based on inconclusive dating and incorrect reading. In fact he never puts his claim that Wallace's paper influenced Darwin to the critical test. He never compares the content of the September 1857 outline of the theory, sent to Asa Gray, with the April-to-June 1858 discussion of divergence in *Natural Selection*. Had he done so, he would have found a mass of detailed evidence and argumentation to support divergence in *Natural Selection*, but no conceptual reformulation of the principle already stated in 1857. So even if we grant Brackman's six clear days in June 1858, there is no evidence that Darwin took advantage of the time to alter his position.

If the central charge that Darwin was influenced by or perhaps stole from Wallace is fallacious, and the implication that he tampered with *Natural Selection* is contradicted by his diary, and the claim that he delayed sending Wallace's paper to Lyell rests on doubtful dating, what is left of Brackman's case? Only an atmosphere of deceit conjured up by numerous innuendos about missing, delayed, and planted letters. The historical record is inevitably imperfect. Nevertheless, several of the lacunae Brackman points to have assignable causes. First, for example, there is the lamentable paucity of extant Lyell letters in Darwin's papers in Cambridge. The bulk of these should be among the over 3900 mostly post-1862 letters to Darwin arranged in alphabetical order, scores of which show signs of damp. Worst hit was the letter L, for which only seven miscellaneous letters, 82 Lubbock letters, and three Lyell letters survive. One may infer that a packet of Lyell letters disintegrated in good uniformitarian fashion along with the bulk of the L's.

Second, Brackman accuses Darwin of delaying three months in replying to his first letter from Wallace. Wallace's first letter to Darwin is lost. But Darwin's reply of 1 May 1857 survives and acknowledges receipt of Wallace's 10 October 1856 letter—roughly 26 weeks later. Brackman takes the average one-way transit between England and the Malay Archipelago to have been about 12 weeks. Therefore, he argues, Darwin received the letter in late January, not May, 1857, and Darwin must have lied, which shows how frightened he was of Wallace's 1855 paper. But how can we depend on Brackman's average figure when we find that Wallace, who was keen for contact with Darwin, did not reply to Darwin's 1 May 1857 letter until 27 September, 21 weeks later?

But perhaps Brackman's most far-fetched "evidence" of foul play is the insinuation that Darwin sent the September 1857 abstract to Asa Gray "as an escape hatch for the moment at which he would proclaim priority in the field." The truth is that in July 1857 Gray had very directly asked Darwin to explain his views: "It is just such sort of people as I that you have to satisfy and convince, and I am a very good subject for you to operate on, as I have no prejudice nor prepossessions in favour of any theory at all." Darwin, whom Brackman repeatedly calls "secretive," replied with his abstract. By September 1857, the "secretive" Darwin had revealed natural selection, the heart of his theory, to Hooker, Lyell, and Gray—men he knew well enough to trust. He did not reveal it to Wallace, a man with whom he had exchanged one letter.

I have tried to dispel Brackman's cloud of deception. One final insinuation goes to the heart of the matter. For the years 1856 through 1860 there are extant seven letters from Darwin to Wallace and only one from Wallace to Darwin (a fragment of 27 September 1857). To Brackman this 7:1 ratio implies malevolent destruction on Darwin's part. Part of the explanation for this discrepancy was given long ago by Francis Darwin, though Brackman discredits it. According to Francis Darwin his father systematically destroyed letters before 1862. But he kept letters that were directly useful in his research. Many of the letters kept are fragments, preserved only for their pertinence to the notes with which they are found. Thus the sole surviving early Wallace fragment was kept for its information on jaguars in a collection of notes on the laws of variation. Wallace was not the only correspondent to suffer Darwin's single-minded winnowing. For the period 1856 through 1860 there are 881 letters from and 254 letters to Darwin extant, or a ratio of approximately 3.5:1. The eight Wallace-Darwin letters form a small fraction of the letters surviving for the period. But they survive in a ratio comparable to that for other correspondents of Darwin's. Indeed, Wallace fared better than Huxley (10:1), though not as well as Hooker (5.3:1) or Asa Gray (3.35:1). Letters were an important source of scientific information to Darwin. The fact that he did destroy some is evidence of rigorous organization, not deceit.

If there was no theft, no cover-up, no pattern of tampering with the record, might not Darwin, Hooker, and Lyell have still conspired to cheat Wallace of his priority? This depends on what we are referring to. Darwin had priority in the discovery and in the writing down of both natural selection and the principle of divergence. Brackman fails to disprove that long received view of the events. Wallace had priority in composing a paper that was ready for publication. But Darwin also had an important claim with respect to composition. By June 1858 he had completed ten and a half chapters of his book, that is, over 250,000 words of well-articulated argument supported by a masterly array of facts. Darwin had virtually completed the plan that Wallace was just contemplating.

Brackman successfully shows that Darwin's friends acted to protect his interests by arranging simultaneous publication. He also shows that Darwin was sufficiently self-interested to encourage joint publication and produce both an extract of his 1844 Essay to prove the longevity of his claim to natural selection and the 1857 abstract prepared for Gray to prove the priority of his claim to the principle of divergence. But Darwin's claims were valid and the mere fact that his friends acted to defend them is not a conspiracy. Hooker and Lyell, however, did go one step further. Brackman is right when he says that they manipulated the order of submission (without Darwin's knowledge) by putting Darwin's pieces before Wallace's paper. By placing the documents in the chronological order of their composition they favored Darwin's priority over Wallace's. No doubt they colored the judgment of history. Did this act constitute a conspiracy? No, just a delicate arrangement.

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Issues of Communication

Reflections on Science and the Media. JUNE GOODFIELD. American Association for the Advancement of Science, Washington, D.C., 1981. xii, 114 pp. Paper, $9. AAAS Publication No. 81-5.

"As seen through the medium of the popular press the scientist is apt to appear as an enemy of society inventing infernal machines, or as a curious half-crazy creature talking a jargon of his own and absorbed in the pursuit of futilities." So wrote E. E. Slosson, the first director of Science Service, a nonprofit corporation endowed in 1921 by the publishing magnate E. W. Scripps in order to provide American newspapers with accurate reports on science.

If this caricature persists, it is not because the coverage of science in the mass media remains as uniformly poor and haphazard as it undoubtedly was when Slosson surveyed the scene. Today, as William D. Carey observes in the foreword to this thoughtful essay on the subject, science and the media "depend on each other." Scientists have an interest in communicating the results of their work, if only to assure continuation of the public patronage that has become the sine qua non of large-scale research. Some scientists and technologists also play major roles in discussions of public policy. For the media, science has become a regular source of important news—or, more exactly, of what the gatekeepers of public information consider newsworthy. (Their criteria are certain to include dramatic and controversial applications of science, but not necessarily a discovery that merely alters the fundamental understanding of nature.)

This mutual dependence has brought about important changes in the coverage of science. More space is devoted to it by the print media, more time by the broadcast media. A new profession of "science writers" and "science correspondents" has acquired a perch on both branches. Popular magazines have sprung up alongside the more established and more technical journals. Television programs and series have been produced to explore the process of discovery and to investigate issues and problems connected with new technologies.

Overwhelming as the flood of information often appears, anyone with sufficient interest and preparation can keep abreast of major developments by judiciously monitoring the best of the newspapers, periodicals, and broadcasts. But what about "ordinary citizens" with only a casual interest and a limited educational background, who are less discriminating as to sources and are therefore dependent on whatever they happen to "read in the newspaper" or "see on TV"? It is June Goodfield's contention that the information about science that tends to reach this largest and most politically weighty segment of the population is too often flawed, oversimplified, imbalanced, and misleading.

As a prolific historian of science, whose other most recent book (*An Imagined World: A Story of Scientific Discovery*) is based on years of first-hand observation of scientists at work, Goodfield is equally troubled by the tendency of the media to report research findings out of context. Without some understanding of "the patterns, the limits, the nature of discovery, the balance of certainty and uncertainty," the methodology and "spirit of science," (p. 88), she points out, the data alone cannot be properly evaluated.

In seeking to explain the reasons for such shortcomings in the communication of scientific findings, Goodfield calls attention to the contrasts in mind-sets and constraints between scientists and journalists. Scientists are trained to be cautious, to publish findings only after peer review. They do not expect the most vexing quandaries to be cleared up quickly; they know that every discovery is apt to raise as many new questions as it answers old ones. They express themselves in technical languages. Some are so acutely aware of the limits of their expertise that they are reluctant to speculate about the remote implications of their work. Others inflate their expertise and take advantage of the gullibility of susceptible reporters. Human frailty makes them happy when their accomplishments are publicized but indignant when investigative efforts cast their behavior or that of their institutions in an unfavorable light.

For their part, journalists are constantly on the scent of scoops and exposés and just as constantly confronted by imminent deadlines. They cannot always take the time to investigate a development in science thoroughly or to present it with all the qualifications that may be necessary. In order to obtain information quickly, and to make it seem credible, they are tempted to rely on those Rae Goodell has called the "visible scientists," whose names are well known but who may not be the most expert sources. Even when they take pains to tell a scientific story properly, their work is at the mercy of editors who may be more concerned with the span of attention of the average reader or viewer and who may "slug" the story with a sensational headline that "sells newspapers" or raises ratings. In the wake of Vietnam and Watergate, journalists have tended to become especially skeptical, even adversarial, toward all authority. Some who view science as the last of the sacred cows to be left unmolested take particular delight in finding conflict, suppression, scandal, and petty foibles in the ranks of "pure" science.

As evidence of the shoddy treatment of scientific subjects, Goodfield cites two cases in particular. One concerns the allegation of the falsification of research data by an investigator at the Memorial Sloan-Kettering Institute who