Darwin and the Dilemma of Geological Time

By Joe D. Burchfield*

I

"HE WHO CAN READ Sir Charles Lyell's grand work on the Principles of Geology, which the future historian will recognize as having produced a revolution in natural science, yet does not admit how incomprehensibly vast have been the past periods of time, may at once close this volume." This challenge appeared in every edition of On the Origin of Species. Yet even before the first edition appeared, Charles Darwin had begun to doubt the wisdom of his treatment of geological time; and long before the final edition was completed, it had become one of his most perplexing problems. Scientific arguments limiting the earth's age appeared soon after the publication of the Origin, and in the hands of Darwin's critics they presented a formidable obstacle to natural selection. The obstacle was all the more disturbing, moreover, because it was based upon arguments that Darwin felt incapable of answering.

The references to geological time in the Origin and in his preserved correspondence leave no doubt about the importance Darwin assigned to it. At one point he referred to the insufficiency of time as "probably one of the greatest objections yet advanced" against the theory. But the record is very sparse. With his limited reservoir of physical strength, he understandably devoted his best efforts to those objections that he believed he could answer, which is perhaps one reason why his dilemma over geological time has received so little attention from Darwinian scholars. The aim of the present essay, therefore, is to piece together the scattered fragments of the record in order to determine whether the problem was actually as formidable as Darwin's remarks seem.

Received Feb. 1973; revised/accepted May 1973.

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Geological map of the southeast of England and part of France, exhibiting the denudation of the Weald.

to imply, and if so, what effect it had on the subsequent development of his views on natural selection. In this regard it is important to note that he faced two entirely different sets of objections to his reliance on time, the first arising from his own calculation of the time required for the denudation of the Weald and the second from Lord Kelvin’s physical arguments limiting the age of the earth. His reaction to the second, more important challenge was influenced at least in part by the aftermath of the first, and both were reflected in successive editions of the *Origin*. Consequently, it has proven valuable for the purpose of this essay not only to examine the nature of the objections raised by the arguments on geological time and the reactions of Darwin and his supporters but to put them into a chronological frame.

II

One could almost say that Darwin’s problem arose originally from overconfidence. Lyell had taught him that the time available for geological and biological change was virtually limitless, and his own observations had strengthened this conviction to the point of certainty. As early as the essay of 1842 he accepted the extreme imperfection of the geological record and the reality of long ages before the Silurian as matters of course, and he spoke confidently of *immense* ages having elapsed during each geological period. Although he confessed several times to the belief that his reliance upon the imperfection of the geological record was the weakest part of his argument, it was a weakness stemming from the inherent difficulties of arguing from negative evidence rather than from any doubts about the significance of the gaps in the record itself. In preparing the *Origin*, therefore, he presented a meticulous account of his reasons for believing the record to be imperfect, but he abandoned this caution when he turned to a discussion of the magnitude of that record expressed in years. Rather than attempting to demonstrate the lapse of years through his customary compilation of evidence, he chose merely to illustrate his case with a single example drawn from a familiar source.

Darwin’s choice of the denudation of the Weald—the great valley stretching between the North and South Downs in the south of England—for his one illustration of the quantitative magnitude of time was perhaps an obvious one. Lyell had devoted two chapters of his *Principles* to the denudation of the Weald, and its geological character was thoroughly familiar to Darwin and most of his contemporaries. Furthermore, Darwin was firmly committed to marine action as a principal agent in the shaping of the earth’s crust. It was therefore only natural that he would choose marine denudation, a process that he believed he thoroughly understood, to illustrate the rate of geological change. The calculation itself, however, was hasty and crude. The width of the Weald valley, he declared, is *about* 22 miles, and the average thickness of its formations is 1,100 feet. If the valley were formed by marine erosion—a point which,

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6 Perhaps the most notable example of Darwin’s affinity for explaining geological phenomena in terms of marine denudation was his prolonged defense of his hypothesis concerning the so-called parallel roads of Glen Roy. See “Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber in Scotland, with an attempt to prove that they are of marine origin,” *Philosophical Transactions of the Royal Society*, 1839, pp. 39–81. I am grateful to Dr. M. J. S. Rudwick for pointing out this example to me.
at the time, he thought was beyond question—a measure of the rate at which the sea wears away a line of cliffs should permit one to calculate the time necessary for its formation. He had no measure of this rate, but he assumed that a rate of 1 inch per century for a cliff 500 feet high would be accurate enough. "At that rate," he concluded, "on the above data, the denudation of the Weald must have required 306,662,400 years; or say three hundred million years."

Darwin's calculation was indeed simple. In fact it was appallingly naïve. As his critics were to point out, he had uncritically assumed that a cliff 1,000 feet high would be eroded only half as fast as one 500 feet high; he had neglected undercutting, differing hardness of strata, and the fact that erosion would occur on both sides of the valley at once; and he had begun with a gross assumption of the rate of erosion in the first place. Admittedly, his declared intention was merely to give "some crude notion" of time. But results stated as 306,662,400 years and the confident assertion that "in all probability a far longer period than 300 million years has elapsed since the latter part of the secondary period" both seemed to imply an unwarranted degree of precision. Certainly his critics regarded it as such.

There can be little doubt that Darwin quickly repented of his overly casual quantitative treatment of time. The second edition of the *Origin*, appearing only a month after the first, already carried a disclaimer suggesting that the estimate of the time required for the denudation of the Weald might need to be reduced by a factor of two or three. The disclaimer was inserted, moreover, despite his repeated protestations that he "must make only actually necessary corrections" in the edition. It is not so easy to determine, however, why he felt such a correction was necessary. As Francis Darwin, Bert James Loewenberg, and others have pointed out, there are serious gaps in the existing records of Darwin's correspondence before 1862, and one of the more serious of these gaps includes the correspondence he received concerning the first and second editions of the *Origin*. From the few letters that have survived, however, it seems highly probable that Lyell himself was responsible for the first word of warning. Lyell, after all, had carefully avoided any temptation to publish a quantitative estimate of geological time, just as he had avoided the use of the term *infinite* that some of his followers so carelessly bandied about. And he alone read the *Origin* in proof before it appeared. Only one, the first, of the letters he wrote after he finished reading the *Origin* on October 3, 1859, has survived; but from Darwin's replies and additional comments to Hooker and Wallace, we know that between October 3 and November 22 he wrote several "volume-like" letters full of discussion and comment. Exactly what

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14 *Ibid.*, p. 171, letter to Joseph Hooker, Oct. 15, 1859. Other references are found in replies from Darwin to Lyell on pp. 208–214 (Oct. 11, 1859), pp. 173–174 (Oct. 20, 1859), and p. 228 (Nov. 23, 1859). The surviving letter, which is confined to the opening chapters of the *Origin*, is reproduced on pp. 205–208. A copy of a second letter (Oct. 28, 1859) and an extract from a third (Nov. 21, 1859) are included in Lyell's journals, but both deal exclusively with biological questions, and neither answers to Darwin's description of being "volume-like." See Leonard G. Wilson, ed., *Sir Charles Lyell's Scientific Journals*
these criticisms involved, we can for the most part only guess. Such references as we have indicate that they ranged over the whole of Darwin’s argument and indeed provided the basis for nearly all of the changes made in the second edition of the *Origin*. Darwin was quite specific on this point in a letter written just after the new edition appeared: “It is perfectly true that I owe nearly all the corrections to you, and several verbal ones to you and others.” But more to the point, this sentence concludes with the remark, “as yet only two things have annoyed me; those confounded millions of years (not that I think it is probably wrong), and my not having by inadvertence mentioned Wallace towards the close of the book in the summary.” In the absence of any other evidence, it seems highly probable that one of Lyell’s corrections included the calculation of the denudation of the Weald.

The more moderate phrasing of the second edition proved to be too little and too late, however, and as “those confounded millions of years” clearly indicates, Darwin had already begun to feel the sting of his lapse of caution. On December 24, 1859, only two days before the second edition appeared, the *Saturday Review* carried a highly critical review that singled out the denudation of the Weald as the focus for attack. Striking accurately at what was perhaps the weakest part of Darwin’s vast accumulation of evidence, the reviewer used it to discredit the argument as a whole. “Enough has been said to show what a pile of unsupported conjecture has been required to sustain this last and ablest attempt to penetrate the mystery of the origin of species,” he proclaimed. The reviewer’s criticism was to the point, as Darwin was forced to admit, but he was not yet daunted. On January 3 he wrote Hooker: “Some of the remarks about the lapse of years are very good, and the reviewer gives me some good and well deserved raps—confound it. I am sorry to confess the truth: but it does not at all concern the main argument.” Nonetheless, in a letter to Asa Gray three months later he mentioned again an earlier request (which is evidently not preserved) that all reference to the denudation of the Weald be deleted from the American edition of the *Origin*. He noted, however, that Joseph Beet Jukes, then the head of the Irish Geological Survey, had urged that the calculation be left in.

Despite this request, the American edition included the Wealden calculation just as it had appeared in the second British edition. But it also carried the following footnote:

I have left the forgoing passages as they stand in the second edition, but I confess that an able and justly severe article, since published in the *Saturday Review* (December 24th, 1859), shows that I have been rash. I have not sufficiently allowed for the softness of the strata underlying the chalk; the remarks made are more truly applicable to denuded areas composed of hard rocks. Nor have I allowed for the denudation going on on both sides of the ancient Weald-Bay; but the circumstances of denudation having taken place within a protected bay would prolong the process. It has long been my habit to observe the shape and state of surface of the fragments at the base of lofty retreating cliffs, and I can find no words too strong to express my conviction of the extreme slowness with which they are worn away and removed. I beg the reader to observe that I have expressly stated that we cannot know at what rate the sea wears away a line of cliff: I assumed the one inch per century in order to gain some crude idea of the lapse of years; but I always

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supposed that the reader would double or quadruple or increase in any proportion which seemed to him fair the probable rate of denudation per century. But I own that I have been rash and unguarded in the calculation.\textsuperscript{19}

This footnote is unique. It is the only one ever to appear in any edition of the \textit{Origin} and as such bears eloquent witness to Darwin's agitation and concern.

But there are other reasons why the footnote is instructive as well. First, it shows clearly that Darwin thought it necessary merely to illustrate, not prove, the vastness of geological time. Perhaps more important, it tells much about his method of presenting his arguments. He says that he expected his readers to alter his estimated parameters by any amount that seemed fair, a point apparently supported by the disclaimer that he only sought to give "some crude notion" of time; but his methods of stating his results effectively nullified the disclaimer and implied a substantial degree of precision. The sentence added in the second edition suggesting that "perhaps it would be safer to allow two or three inches per century and this would reduce the number of years to one hundred and fifty or one hundred million years"\textsuperscript{20} is perhaps more in keeping with the footnote, but there is no reason to believe that he originally felt such an allowance to be necessary. Nor is there any indication that he had any doubts about the validity or approximate accuracy of his method of approach. And finally, the coupling of an embarrassed admission of error with an attempt at justification that would temper his retreat provides a model of his subsequent method of dealing with unanswerable criticism.

The \textit{Saturday Review} was just the beginning, however. In February 1860 he was attacked on the same issue from the presidential chair of the Geological Society. The attacker was John Phillips, the Professor of Geology at Oxford and an old opponent of Darwin and Lyell. Phillips argued that a river similar to the Ganges—a choice he considered justified since the Wealden formation had been laid down under tropical conditions—could account for the denudation of the Weald in only 1.3 million years, while Darwin's marine denudation, if carried on at the most rapid rate presently observable, could do the job even faster. Phillips carefully discounted any claims to accuracy in these calculations but considered them quite sufficient to discredit Darwin's "inconceivable number of 306,662,400 years," which he denounced as an "abuse of arithmetic."\textsuperscript{21} During the next several months he expanded his critique, first in the Rede Lecture at Cambridge and then in a monograph. In this final form Phillips included the first important attempt to calculate geological time from the rate of accumulation of strata, and his result—that probably only about 95 million years have elapsed since the beginning of the Cambrian—was a mere fraction of the time Darwin believed his theory required.\textsuperscript{22}

Meanwhile, others joined the fray. During the spring of 1860 Francis Bowen, Professor of Natural Religion and Moral Philosophy at Harvard, took up several meetings of the American Academy of Arts and Sciences to denounce the speculative nature of Darwin's reliance on time, condemning it as cosmology rather than natural


history. 23 And in his subsequent review he directed his most severe criticism at Darwin’s reliance upon the imperfection of the geological record and at the calculation of the denudation of the Weald. 24 In England a similar attack was taken up by Bishop Wilberforce, not yet bruised by his upcoming encounter with Huxley. After quoting the results of the Wealden calculation, Wilberforce declared: “As these calculations concerning the general duration of formations are highly characteristic of the whole argument, it may be worth while to submit them to a somewhat closer examination.” 25 He then examined them for five pages and concluded by throwing them out entirely. Darwin, he suggested, might be compared to a magician who would shorten or prolong time at will with a wave of his magic wand, or to an ancient geographer who wrote of men whose heads grew under their shoulders. 26 Other reviewers were not so picturesque but they frequently hit at the same target—Darwin’s reliance upon time and the imperfection of the geological record, even when the Weald itself was not mentioned. 27

Darwin had defenders, of course: Hooker, Huxley, and Jukes argued that the Wealden calculation should be left in the Origin; Gray opposed Bowen; and a few reviewers specifically defended his reliance upon time. 28 But it was not enough. From the beginning he had believed the imperfection of the geological record to be his weakest point, and now he was convinced that he had been guilty of an incredible blunder. His distress was acute, as this note to Lyell reveals: “Having burnt my own fingers so consumedly with the Wealden, I am fearful for you [this concerns Lyell’s proposed work on the antiquity of man] . . . for heaven’s sake take care of your fingers; to burn them as severely as I have done, is very unpleasant.” 29 His decision had already been made. A few days earlier he had written Lyell: “The confounded Wealden calculation to be struck out, and a note to be inserted to the effect that I am convinced of its inaccuracy from the Saturday Review, and from Phillips, as I see in his table of contents that he alludes to it.” 30 It is worth noting here that he had not even read Phillips’ book. As he confessed in the same letter, he had not yet had the heart to face it, nor the long, hostile review by Bowen. He had no doubt about their content, however, for he knew the gist of Phillips’ Geological Society lecture and probably had heard Bowen’s views as well. Certainly he had read enough to be all too aware of his blunder. Thus when


27 Perhaps the best example of such reviews was William Hopkins, “Physical Theories of the Phenomena of Life,” Frasier’s Magazine, June 1860, 61:739–752, and July 1860, 62:74–90. At the time Darwin considered it one of the best reviews to appear against him (LLD, Vol. II, p. 327, letter to Asa Gray, July 22, 1860).


the third edition of the *Origin* appeared in April 1861 all mention of the Weald was conspicuously missing, including the proposed footnotes, although the claims for the imperfection of the geological record remained as strong as ever.

### III

With the new edition of the *Origin* Darwin felt himself well out of a troublesome situation. He was confident that the world would prove older than Phillips had made it, but he was equally sure that his own calculation had been a mistake. The important part of his geological argument, after all, concerned the imperfection of the geological record, and upon that point he had gotten strong support from geology. He hoped, therefore, that the question of time had been laid to rest, and despite an occasional remark in a critical review, for a time that hope seemed justified. The halcyon period lasted for several years, and the fourth edition of the *Origin* appeared at the end of 1866 with no significant changes in the treatment of geological time. Then, six months later, the calm was shattered by H. C. Fleeming Jenkin’s famous review.31

Darwinian scholars, and Darwin himself, have justifiably stressed the importance of Jenkin’s argument against the probability of a single, individual variation being perpetuated in a community of normal organisms. But the review also contained another argument that in the long run was to prove almost as significant, an argument stressing the inadequacy of geological time.32 Jenkin had before him the fourth edition of the *Origin*, from which all reference to the Weald had been removed, but on turning to the question of time, he, like many other critics, resurrected the Wealden calculation and Darwin’s “confounded millions of years.” He dismissed it almost immediately, however, as an example of the kind of fuzzy reasoning that had led geologists to the “wholly erroneous” conclusion that past time was virtually limitless. On the basis of the evidence available, he asserted, Darwin’s results could be expanded or contracted by a factor of a hundred or even a million; the data were simply too meagre for judgment. Indeed, “The whole calculation savours a good deal of that known among engineers as ‘guess at the half and multiply by two.’”33 Much more significant than this derisive treatment of the already discarded Wealden calculation, however, was Jenkin’s attack upon the whole principle of geological uniformity and its implications for Darwin’s theory.

A physicist and engineer of broad learning, Jenkin brought the newly formulated laws of thermodynamics to bear on the question of geological uniformity. He pointed out that in a finite world heated by a finite sun the available store of energy must be limited, and he explained why, according to the second law of thermodynamics, every energy transformation—that is, every process of change—must dissipate a part of that energy and render it useless for further transformations. In geological terms he argued

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32 With the exception of Eiseley and Geison (see n. 3), both of whom single out the problem for special discussion, the importance of geological time in Jenkin’s attack on Darwin has been largely ignored. E.g., Peter J. Vorzimmer gives perhaps the longest critique yet to appear of Jenkin’s review without once mentioning his discussion of Kelvin and geological time. See Peter J. Vorzimmer, *Charles Darwin: The Origin of Species and Its Critics*, 1859–82 (London: University of London Press, 1972).

that these facts meant that uniformity could not be a law of nature, that the earth must be running down, and that present geological forces must be less powerful and less violent than those in the past. The present rate of geological change cannot therefore be used justifiably as a guide to the age of the world but must "yield before more accurate methods of computation," the methods of physics.\(^{34}\)

Neither Jenkin's attack on uniformitarianism nor the physical calculations of time he used to support it were original. They had in fact been fully discussed some five years earlier by his close friend and associate William Thomson, the future Lord Kelvin. The basic principles of Kelvin's arguments were straightforward and, once perceived, relatively simple. Seeking first the ultimate source of the heat radiated by the sun and the earth, he found only one source that was conceivably adequate in terms of the scientific knowledge of the time—gravitation. Gravity, according to the almost universally accepted nebular hypothesis of Kant and Laplace, was the force that had formed the earth and the sun from the scattered primordial particles of matter, and, according to the principle of the conservation of energy newly quantified by Joule, the heat thus generated eclipsed all other possible sources. Kelvin's contribution was to relate the two to the problem of geological time. In the case of the sun this involved determining the total potential energy of a nebular mass equal to that of the sun but widely distributed in space, the calculation of the amount of heat generated by this energy upon the contraction of the nebula to form the sun, and a comparison of this result with measurements of the rate at which the sun radiates its energy. It was a straightforward extrapolation of the nebular hypothesis, and the mathematics were reasonably simple.\(^{35}\)

The terrestrial problem was more complex, since it involved the distribution of heat after the earth's crust was formed rather than the initial supply of heat itself. The problem essentially was to determine how long it has taken the earth to cool from molten heat to its present condition, and the mathematics involved were much more complex than for the solar problem. A general solution to the problem of heat conduction had already been developed by J. B. Fourier, however, and Kelvin used it along with the limited data available and a judicious choice of assumptions to solve his problem. His results placed the earth's age—including the Precambrian—at 98 million years, but in order to allow for probable corrections, he bracketed his final result between 20 and 400 million years.\(^{36}\) The sun's heat gave a range of between 10 and 100 million years. Both calculations had required numerous assumptions, but Kelvin had taken pains to make them as consistent with physical knowledge as possible; the mathematics and physical principles involved seemed unassailable; and he believed that the agreement between the two results spoke for itself.

Kelvin's arguments were published in the spring of 1862, just a few months after Darwin, with a sigh of relief, felt himself finally free from the Wealden calculation with the appearance of the third edition of the *Origin*. Apparently unaware of the change, Kelvin included an attack on Darwin's calculation along with a more general polemic against the fallacies of geological uniformity. But Kelvin's arguments were entirely physical. They made slight reference to geology except for criticism, and they com-

\(^{34}\) *Ibid.*


pletely ignored geological evidence. Thus they in turn were ignored or overlooked by geologists, until Jenkin propelled them squarely into the biologists’ camp.

By 1867 Jenkin had been working closely with Kelvin for several years as colleague, business partner, and self-proclaimed admiring disciple.  

37 The problem of the age of the world and its bearing on natural selection interested them both, and Jenkin was thus in a position to be fully versed in the older man’s views. He also had a talent for presenting detailed arguments in a clear, concise form. Thus in turning Kelvin’s arguments against Darwin, he explained them briefly but convincingly, making no claims for their numerical accuracy but stressing the soundness of the approach and the principles involved. He was sure that better data would force some changes in Kelvin’s results, but he was equally sure that they would still decisively show that “not only is the time limited, but it is limited to periods utterly inadequate for the production of species according to Darwin’s views.”  

38 This passage is heavily marked in Darwin’s copy of Jenkin’s review, but apparently he was not immediately disturbed by it. The other parts of the review demanded his first attention. Perhaps too, the mention of the long-discarded Wealden calculation blunted its initial impact. Certainly he was unconcerned when he wrote to Hooker: “I cannot think how you can attach so much weight to the physicists,” since they disagreed so enormously about the rate of cooling of the earth’s crust.  

39 And Hooker in turn defended biology against the attacks of Jenkin and Kelvin.

40 In February 1868, however, Kelvin once again joined the fray with the bold assertion that “It is quite certain that a great mistake has been made—that British popular geology at the present time is in direct opposition to the principles of natural philosophy.”  

41 Kelvin’s primary target was again uniformitarianism, but the implications of his arguments for natural selection could not be ignored. Once again he explained why geological uniformity was incompatible with the laws of thermodynamics and repeated his arguments for limiting the earth’s age, throwing in still a third argument for good measure. It was a vigorous polemic, concentrating upon physical laws and as usual ignoring geological evidence. But in 1868 no one in England was better qualified to speak on the laws of thermodynamics, and he backed his conclusions with all of the power of numbers and calculations. Geologists as well as physicists now felt obliged to heed his arguments, and few felt capable of withstanding them. It was only the beginning; in less than a decade, Lyell’s unlimited ages had contracted into the finite limits set by Kelvin’s physics.

Darwin could hardly help being disturbed by Kelvin’s apparently unassailable physics, but the argument that seemed to influence him most came from neither Kelvin nor Jenkin but from an entirely unexpected source. In 1868 James Croll was a


39 Darwin Reprint Collection, No. R 89, Cambridge University Library.


middle-aged, self-educated Scot who had just taken up his first semi-professional post as clerk in the office of the Geological Survey of Scotland.\textsuperscript{43} Lacking formal training in mathematics, Croll had developed a unique form of quantitative logic which he applied to the problems of physical geology, especially to a search for the cause of the glacial epochs. These studies had convinced him, among other things, that the earth’s stratigraphic crust had been deposited far more rapidly than most geologists believed possible, and he consequently became one of the first converts to Kelvin’s limited geochronology. Croll believed that the geologists had been led astray by their inability to conceive of magnitudes as large as a million years, let alone countless millions of years. Thus they were unable to appreciate the amount of change that could be accomplished in that time. Once this deficit was recognized, he felt sure that a proper interpretation of the geological evidence would show that it could fit comfortably within Kelvin’s limits.\textsuperscript{44}

Several of Croll’s papers had appeared as early as 1864, but Darwin was evidently unaware of them until the fall of 1868 when he received copies from the author.\textsuperscript{45} The reaction was immediate. With his gift for vivid analogy Croll was able to illustrate just how inconceivably vast a million years was when compared to ordinary human experience, and in the same way to make his quantitative arguments comprehensible to the unmathematical Darwin. Even more important, however, he provided Darwin with the one thing the attacks of Kelvin and Jenkin had not—a way out—a possibility that limited time and natural selection were not incompatible. A grateful Darwin wrote back: “I have never, I think, in my life been so deeply interested by any geological discussion. I now first begin to see what a million means, and I feel quite ashamed of myself at the silly way in which I have spoken of millions of years.”\textsuperscript{46}

Darwin’s views on natural selection had already undergone considerable change before he read Croll’s papers, or, for that matter, before he had read either Jenkin’s or Kelvin’s. As early as 1865 his search for the causes of variation and heredity had led him toward the hypothesis which he called pangenesis, and by late 1866 he had prepared his ideas for publication.\textsuperscript{47} Pangenesiawas intended to complement natural selection. Among other things it allowed Darwin to place a renewed emphasis upon

\textsuperscript{43} The only account of Croll’s life is James Campbell Irons, \textit{Autobiographical Sketch of James Croll with Memoir of his Life and Work} (London: Stanford, 1896).


\textsuperscript{45} Croll’s first paper related to the subject was “On the Physical Cause of the Change of Climate During Geological Epochs,” \textit{Phil. Mag.}, 1864, 28:121–137. Five others followed before 1868. It is not certain how many of these he sent to Darwin, but he definitely included the most important, “On Geological Time and the Probable Date of the Glacial and Upper Miocene Period,” loc. cit.

\textsuperscript{46} \textit{MLD}, Vol. II, p. 211, Sept. 19, 1868. The analogy that so impressed Darwin was both simple and graphically effective. Croll suggested that an actual experiment be done by hanging a narrow strip of paper 83 ft 4 in. long around a large room. The full length of the tape would then represent 1,000,000 years, while 100 years—the limit of time that he felt could actually be conceived of in human experience—would be represented by a mark only 1/10 in. from the end. In this way he believed the inconceivable vastness of 1,000,000 years, let alone Kelvin’s 100,000,000 years, could be appreciated if not fully grasped. See “On Geological Time,” 35:375.

\textsuperscript{47} Discussions on the development of Darwin’s theory of pangenesis are given by Geison (n. 3) and Peter Vorzimmer, “Charles Darwin and Blending Inheritance,” \textit{Isis}, 1963, 54:371–390. Darwin’s first published account of the theory was in \textit{Variations of Animals and Plants under Domestication}, 2 vols. (London: Murray, 1868), pp. 357–404. \textit{Variations} also shows how far he was leaning toward the Lamarckian influences of environment and use and disuse of organs.
such Lamarckian influences as the direct effect of external conditions, the use and disuse of organs, and the inheritance of acquired characteristics as causes of variations which could be acted upon by natural selection. He had developed the hypothesis of pangenesis without reference to the question of geological time, but it was evident that it could, if necessary, provide for a much more rapid rate of organic evolution than mere random variations alone. This was perhaps one of the reasons why Darwin was so little disturbed when Jenkin first introduced him to Kelvin’s arguments. On the other hand, however, it seems almost certain that both of Jenkin’s major points—the “swamping” effect that would obliterate random variations and the brevity of geological time—added to the appeal of pangenesis in Darwin’s eyes. In any case, the mechanism for hurrying natural selection was already at hand when Croll supplied the final argument needed to convince him that such an acceleration was consistent with the proper interpretation of the geological record.

Croll’s papers were still fresh in his mind when Darwin began the revisions for the fifth edition of the *Origin* in December 1868. The work was done hurriedly in six weeks borrowed reluctantly from the study of sexual selection, but the arguments of Croll and Kelvin had had their effect. The new edition contained the most significant changes Darwin was ever to make in his treatment of geological time. He was much more tentative and cautious in his pronouncements, and much more willing to admit that his earlier demands for time had been excessive. He was also obviously relieved at the thought that biological and geological change had probably taken place many times faster than he had previously thought possible. He consequently struck out statements that had stood unchanged, despite the retreat on the denudation of the Weald, through four editions. Gone were such remarks as

... it is highly important for us to gain some notion, however imperfect, of the lapse of time. During each year the land and water have been peopled by hosts of living forms. What an infinite number of generations, which the mind cannot grasp, must have succeeded each other in the long roll of years.

In their place the fifth edition substituted statements like this:

Mr. Croll judging from the amount of heat energy in the sun and from the date which he assigns to the last glacial epoch estimates that only sixty million years have elapsed since the deposition of the first Cambrian formation. This appears a very short period for so many and such great mutations in the forms of life, as have certainly since occurred. It is admitted that many of the elements in the calculation are more or less doubtful, and Sir W. Thomson gives a wide margin to the possible age of the habitable world. But as we have seen, we cannot comprehend what the figures 60,000,000 really imply; and during this, or perhaps a longer roll of years, the land and the waters everywhere teemed with living creatures, all exposed to the struggle for life and undergoing change.

There was more to these changes than the mere replacing of one phrase by another, reflecting more up-to-date evidence. The whole tone of the discussion had changed. Even where the phrases of the older editions remained, they were qualified and made

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more cautious. For example, "The consideration of these facts impresses the mind in almost the same manner as does the vain endeavor to grapple with the ideas of eternity" had heretofore emphatically concluded the discussion of denudation. But in the fifth edition it was followed immediately by:

Nevertheless this impression is partly false. Mr. Croll, in a most interesting paper, remarks that we do not err 'in forming too great a conception of the length of geological periods,' but in estimating them in years. When geologists look at large and complicated phenomena, and then at the figures representing several million years, the two produce a totally different effect on the mind, and the figures are at once pronounced to be too small . . . . Few of us, however, know what a million really means.

Darwin relied heavily on Croll, even to the point of repeating the analogy he had used to illustrate the problem of conceiving of vast time spans. But as he saw the objections of Phillips and Jenkin answered, another problem rose to take their place. Regardless of Croll's arguments, Darwin found that he could not escape the fear that Kelvin's time scale would compress the critical Precambrian era into a period totally inadequate for the demands of natural selection. Again he turned to Croll, clearly groping for any thread of argument that might help:

Not withstanding your excellent remarks on the work which can be effected within a few million years, I am greatly troubled at the short duration of the world according to Sir W. Thomson, for I require for my theoretical views a very long period before the Cambrian formation. If it would not trouble you, I should like to hear what you think of Lyell's remark on the magnetic force which comes from the sun to the earth: might this not penetrate the crust of the earth and then be converted to heat? This would give a somewhat longer time during which the crust might have been solid; and this is the argument on which Sir W. Thomson seems chiefly to rest. You seem to argue chiefly on the expenditure of energy of all kinds by the sun, and in this respect Lyell's remark would have no bearing.

This time, however, Croll could offer no help. And thus, when the fifth edition of the *Origin* comes to the discussion of the need for a long period of evolutionary development before the Silurian, it continues:

Here we encounter a formidable objection; for it seems doubtful whether the earth in a fit state for the habitation of living creatures has lasted long enough. Sir W. Thompson [sic] concludes that the consolidation of the crust can hardly have occurred less than 20 or more than 400 millions years ago, but probably not less than 98 or more than 200 million years. These very wide limits show how doubtful the data are; and other elements may have to be introduced into the problem. Mr. Croll estimates about 60 million years have elapsed since the Cambrian period, but this, judging from the small amount of organic change since the commencement of the Glacial epoch, seems a very short time for the many and great mutations of life, which have certainly occurred since the Cambrian formation; and the previous 140 million years can hardly be considered as sufficient for the development of the varied forms of life which certainly existed towards the close of the Cambrian period.

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62 *Origin*, 5th ed., pp. 352–353. *Variorum*, p. 485. It is in instances like this that the normally valuable *Variorum* fails to convey the full impact of the change in Darwin's presentation. The interspersal of numerous changes in the various earlier editions of the *Origin* effectively conceals the overall change in tone.
This statement, perhaps more than any other, reveals how tentative Darwin’s conclusions about time had become; but like the footnote in the American edition, it coupled apparent retreat with a last-ditch defense and an obstinate refusal to yield on fundamentals. He was certainly not willing to give up natural selection in the Precambrian, but neither was he able to refute Kelvin’s arguments, nor even to get around them as Croll had allowed him to do in the post-Cambrian. By reducing his demands for time, he had made what peace he could, but he still could not shake the nagging doubt that neither Kelvin nor Croll had allowed him time enough. Even these concessions, however, had been made at considerable cost. The new edition of the *Origin* was riddled with instances where Darwin had been “led to place somewhat more value on the definite and direct action of external conditions.” But such references had never been entirely absent from the *Origin*, and it was a price he was willing to pay.

IV

It was perhaps fortunate that Darwin had completed his revisions before Huxley entered the lists, for his champion’s retreat was even more pronounced than his own. Speaking as president of the Geological Society of London in February 1869 Huxley appointed himself attorney to defend geology from the charges Kelvin had leveled a year earlier. Huxley presented his defense with his customary elegance, but his approach was almost the reverse of Darwin’s. Instead of qualifying an apparent concession, he began by denying his opponent’s position and then co-opting it. Thus, for example, he denied Kelvin’s implication that uniformitarianism represented “British popular geology” by asserting that catastrophism still had many able adherents. But then he immediately suggested that both views should be abandoned in favor of a new school of geological thought which would incorporate the strongest features of its predecessors and eliminate their flaws. This new approach to geology would use the present as a guide to the past but would not require identity; it would include periodic catastrophes as a part of the uniformity of nature; and, if necessary, it would accommodate the slowing down of geological processes required by Kelvin’s physics. In short, Huxley’s *evolutionism* would allow for the irreversible changes in the earth’s activity that Kelvin’s arguments demanded.

He adopted much the same tactic in dealing with Kelvin’s estimates of time. First he accused Kelvin of using mathematics to give an unwarranted aura of certainty to calculations based upon the uncertain foundations of hypothesis and conjecture. He claimed that the wide margins for error that Kelvin had felt obliged to allow were clear evidence of the vagueness and uncertainty of the data. But then he immediately betrayed the point by demanding to know whether geology had ever denied that 100 million years may have been sufficient to account for the record of geological change. Whatever may have been the merits of Huxley’s first point—and certainly his criticism of Kelvin’s use of mathematics to imply certainty was justified—the second was sheer sophistry, and nothing could more clearly demonstrate his willingness to abandon the uniformitarianism of Lyell if Kelvin’s physics made it necessary.

Huxley claimed to be defending geology by “mother wit,” but he succeeded in an “artful dodge.” Despite the merits of his proposed evolutionism, and there were many, he had evaded rather than answered Kelvin’s objections, and he had concluded by seeking accommodation with the physical arguments that neither he nor anyone else at the time could refute. Darwin, too, had sought accommodation, but with reluctance and lingering doubts. Certainly he could take little comfort now in his champion’s bald assertion that

Biology takes her time from Geology. The only reason we have for believing in the slow rate of the change in living forms is the fact that they persist through a series of deposits which, geology informs us, have taken a long while to make. If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly.\(^\text{57}\)

But the geological clock was already being set in accordance with the physical clock, and if biology was to follow Huxley’s advice, some serious adjustments would be necessary.

Kelvin greeted Huxley’s attack with an immediate point-by-point refutation.\(^\text{58}\) Showing considerably more heat than usual, he charged Huxley not only with evading the issue, but with championing the very know-nothing attitude toward the principles of physics that had been geology’s chief failing. He quoted extensively from the geological literature to prove that he had not attacked a straw man, and meticulously defended the physical bases of his own calculations. The allowances for error that Huxley had attacked as vagueness, he proclaimed, were in fact demonstrable proof of the care taken to keep the physical speculations within the bounds of the evidence. Kelvin’s address lacked Huxley’s style, but there was no denying its power. Certainly it lent substantial force to the assertion that

The limitation of geological periods, imposed by physical science, cannot, of course, disprove the hypothesis of transmutation of species; but it does seem sufficient to disprove the doctrine that transmutation has taken place through “descent with modification by natural selection.”\(^\text{59}\)

Huxley would no more concede to this final statement than Darwin would, but on the general issue of time he essentially conceded defeat.\(^\text{60}\)

Darwin could hardly help being troubled by this exchange, although he had nothing but praise for Huxley’s address.\(^\text{61}\) But if he felt disappointed by his friend, he soon got an unexpected—if unintended—lift from his opponents. In July 1869 Kelvin’s friend and sometime collaborator P. G. Tait published a long article purporting to be a review of several articles relating to the age of the earth.\(^\text{62}\) It was actually a review of the exchange between Huxley and Kelvin, with a strong bias in the latter quarter. Tait, who was often arrogant in debate, outdid himself as he relegated Huxley and geology in general to a backward, “beetle-hunting,” and “crab-catching” state of scientific

\(^{57}\) Ibid., p. 331.
\(^{59}\) Ibid., pp. 89–90.
\(^{62}\) “Geological Time,” N. Brit. Rev., July 1869, 50:215–233. The review was anonymous, but the author was soon known.
development, incapable of arguing on the same plane with physics about the age of the earth. "Let us hear no more nonsense about the interference of mathematicians in matters with which they have no concern," he declared in response to Huxley’s reference to Kelvin as a passer-by, "rather let them be lauded for condescending from their proud pre-eminence to help out of a rut the too ponderous waggon of some scientific brother." Geological methods alone, Tait claimed, were totally inadequate to solve the problem of the earth’s age, but there was not the least possibility of error in extrapolating back to the beginning of the earth and sun on the basis of their thermal loss. So zealous was his defense of Kelvin that he readily questioned the cosmological speculations of the older mathematicians, including Laplace, whose results had been used by the uniformitarians; but he did not feel that this in any way weakened the physicist’s case. If Kelvin had erred at all it was in giving geology every possible benefit of the doubt. Tait himself believed that a closer look at the evidence would lower the limits of time to 10 or 15 million years or less.

Darwin fairly chortled. He saw in the review a grudging admiration for Huxley’s defense, but more important he saw reasons for doubting the omnipotence of the physicists’ mathematics. Tait’s 15 million years was impossibly brief, unless one were prepared to return to the days of Mosaic geology; and if the physicists could not agree among themselves, then there was surely hope for yet another opinion. Two letters concerning Tait’s review are worth quoting in detail because they show the height of Darwin’s optimism with regard to the physicists’ views on time. On July 24, 1869, he wrote to Hooker:

There is another article just come out in the last North British, by some great mathematician, which is admirably done . . . There are some good specimens of mathematical arrogance in the review, and incidently he often shows how astronomers have arrived at conclusions which are now seen to be mistaken; so that geologists might truly answer that we must be cautious in admitting your conclusions. Nevertheless, all uniformitarians had better at once cry ‘peccavi,’—not but what I feel a conviction that the world will be found rather older than Thomson makes it, and far older than the reviewer makes it. I am glad I have faced and admitted the difficulty in the last [fifth] edition of the origin.

And again, two weeks later, he wrote:

The article in N. British Review is well worth reading scientifically; George D. and Erasmus were delighted with it. How the author does hit! . . . You will be amused to observe that geologists have been misled by Playfair, who was misled by two of the greatest mathematicians! And there are other such cases; so we could turn round and show your reviewer how cautious geologists ought to be in trusting mathematicians.

This was to be his last spurt of optimism, however, for whatever their minor disagreements, the physicists had not been proven wrong. Their fundamental arguments appeared as unassailable as ever, and they continued inexorably to draw the limits of time tighter. Darwin also now had his son George, fresh from a mathematics degree at Cambridge and soon to become one of Kelvin’s ablest disciples, to explain just how irrefutable the physicists’ position seemed to be. Thus, as geologists in increasing numbers adopted Kelvin’s conclusions, Darwin grew more and more despondent. Hooker, as usual, tried to cheer him up:

63 Ibid., p. 217.
65 Ibid., p. 315; Aug. 7, 1869.
Grove is disgusted at your being disquieted about W. Thomson. Tell George from me not to sit on you with his mathematics. When I threatened your tropical cooling arguments with the facts of the physicists, you snubbed me and the facts sweetly, over and over again; and now because a scarecrow of $x + y$ has been raised on the selfsame facts, you boo-boo. Take another dose of Huxley's penultimate G. S. Address and send George back to college.\textsuperscript{66}

But even Hooker's optimistic support could not counteract the dejection Darwin felt upon learning that Wallace too had gone over to Kelvin's camp and had begun to adapt the theory of natural selection to his limitations.

Wallace first questioned the excesses of uniformitarian geology's demands for time in a review of the tenth edition of Lyell's \textit{Principles} in April 1869. At the time, however, he still had serious reservations about Kelvin's arguments, particularly the calculation of the age of the sun.\textsuperscript{67} Although only a small part of a lengthy review, his remarks drew Darwin's immediate attention. "Thomson's views of the age of the world have been for some time one of my sorest troubles," he wrote, "and so I have been glad to read what you say."\textsuperscript{68} Wallace apparently took the remark to heart. By October he was busy with the physicists' arguments,\textsuperscript{69} and in early December he sent Darwin an optimistic announcement: "I have written a paper on Geological Time . . . and I think I have hit upon a solution of your greatest difficulties in that matter."\textsuperscript{70} The response came by return mail: "If you throw light on the want of geological time, may honour, eternal glory, and blessings crowd thick on your head."\textsuperscript{71} But again Darwin's hopes were short-lived. Instead of a refutation of Kelvin's position or a convincing new argument for extending the limits of geological time, Wallace had chosen to follow Huxley's advice and adjust the biological clock to the new time scale. The argument he presented was ingenious, but it offered Darwin little comfort.

The key to Wallace's argument was Croll's explanation of the cause of recurring periods of glaciation—a hypothesis which he had rejected just a few months earlier, but which, perhaps swayed by Darwin's enthusiasm, he now adopted wholeheartedly.\textsuperscript{72} The hypothesis was certainly persuasive. It was based upon the fact, firmly established by observation and Newtonian dynamics, that the shape of the earth's orbit oscillates between near circularity and pronounced ellipticity. The nearly circular orbits, Croll suggested, would give rise to moderate terrestrial climates such as we presently enjoy. Pronounced orbital ellipticity, on the other hand, would produce long, harsh winters in the hemisphere experiencing winter at the apogee of the orbit, winters which because of a combination of geological and meteorological factors, would be only partially offset by the brief, moderate summers. The net result, according to Croll's hypothesis, would be a gradual build-up of ice and snow and the onset of a glacial epoch. During

\textsuperscript{66} \textit{Ibid.}, Vol. II, pp. 6–7; 1870, but exact date unknown.

\textsuperscript{67} "Geological Climates and the Origin of Species," \textit{Q. Rev.}, Apr. 1869, 126:374–379. The review was unsigned, but Wallace provided Darwin with a draft copy for comment.

\textsuperscript{68} \textit{LLD}, Vol. III, p. 115; Apr. 14, 1869.

\textsuperscript{69} Wallace appealed to Darwin's son George for help, and although the questions he asked have not been preserved, they almost certainly related to the physical arguments on time. See James Marchant, \textit{Alfred Russel Wallace, Letters and Reminiscences}, 2 vols. (London: Cassell, 1916), Vol. I, pp. 246 (letter from Wallace to Darwin, Oct. 20, 1869) and 247 (reply, Oct. 21, 1869).

\textsuperscript{70} \textit{Ibid.}, p. 248; Wallace to Darwin, Dec. 4, 1869.

\textsuperscript{71} \textit{Ibid.}, p. 249; Darwin to Wallace, Dec. 5, 1869.

\textsuperscript{72} Wallace had been skeptical of Croll's arguments in his review of Lyell in April 1869 (see n. 67), and Darwin had responded by calling attention to his own enthusiasm (n. 68).
prolonged periods of high orbital eccentricity, however, the hemisphere experiencing winter at the apogee of the orbit would alternate with the precession of the earth’s axis, and thus the formation and recession of glaciers would recur alternately in the northern and southern hemispheres every 10,500 years (half the period of precession).73

Croll had used tables of changing ellipticity to date the occurrence of the “ice ages” and by comparing post-glacial with earlier geological epochs had arrived at his estimate of geological time. Lyell, despite skepticism about much of the theory, had made a similar calculation but had chosen an earlier period of more extreme ellipticity as his base and had used the rate of change of fossil mollusks as his chronometer.74 Wallace studied both calculations and concluded that both Croll’s 60 million years and Lyell’s 240 million years were excessive. Croll, he believed, had been misled through his reliance upon geological changes that were too localized for accurate generalizations; and Lyell, by his choice of the earlier period as the date of the last ice age and his consequent overestimation of the time required for species change. By combining what he considered to be the reliable features of both the calculations, the more recent date for the ice age and a consequently accelerated rate of species change, Wallace arrived at a figure of 24 million years for the time since the beginning of the Cambrian. This estimate, he concluded happily, would fit easily within Kelvin’s limits and still leave a period three times as long for the slow operation of natural selection during the Precambrian.75

Wallace was not finished, however, for it was in the application of Croll’s hypothesis to biology that he showed the true measure of his ingenuity. Neither he nor Darwin had ever completely escaped from the Lamarckian dependence upon environment as a causal factor in species change. And now he saw in the radical changes of climate a mechanism whereby the continuously “altered physical conditions would induce variation.”76 Furthermore, in alternating from one hemisphere to the other, the successive cycles of glaciation would stimulate a constant migration of plant and animal types, thus continually bringing allied species into competition and accelerating the process of extinction. Under these circumstances, Wallace concluded,

We should have all the elements for natural selection and the struggle for life, to work upon and develop new races. High eccentricity would therefore lead to a rapid change of species, low eccentricity to a persistence of the same forms; and as we are now, and have been for 60,000 years, in a period of low eccentricity, the rate of change of species during that time may be no measure of the rate that has generally obtained in the past geological epochs.77

It was a remarkable proposal, combining elements of respected theories in biology, geology, and physics; and, as might be expected, Darwin’s reaction was cordial but reserved. He was impressed by the idea of climatic stability in the recent past and by the possibility of almost constant migrations during earlier periods. Such conditions, he readily admitted, would certainly have accelerated specific change. He seems to have missed a significant point in the argument, however, for he suggested that Wallace’s

76 Ibid., p. 454. Emphasis mine.
77 Ibid. Emphasis his.
position would be strengthened if Kelvin's views on the greater intensity of past geological activity were proved correct. Evidently he had failed to grasp the very important distinction between Wallace's reliance upon cyclic meteorological changes to stimulate natural selection and Kelvin's reliance upon secular geophysical changes. On balance, therefore, he was no more ready to accept Wallace's compromise than he was to capitulate wholly to Kelvin's demands. He made the reason for his reservations clear:

The whole subject is so new and vast, that I suppose you hardly expect anyone to be at once convinced, but that he should keep your view before his mind and let it ferment. This, I think, everyone will be forced to do. I have not as yet been able to digest the fundamental notion of the shortened age of the sun and earth.

The last sentence speaks for itself. Darwin had seen geologists in growing numbers interpret their evidence to fit within Kelvin's chronological limits. Both Wallace and Huxley had yielded before his demands, and even Lyell, however tentatively, had adopted a comparatively brief time scale. Darwin was thus stranded upon ground even more tenuous than his earlier reliance upon the imperfection of the geological record. For a time he had been encouraged by Croll's arguments about the amount of change that could take place in a limited time, but despite his admiration for Croll, he still did not believe that 140 million years would be adequate for the pre-Cambrian development of life. Now Wallace would cut that allowance in half. Darwin would no doubt have liked to be able to follow their lead, but the theory of natural selection had been born in the confidence of almost unlimited time, and compromise as he must, he could not shake the fear that too little time would strangle it.

That Darwin's opponents shared his conviction was soon demonstrated. In St. George Mivart's On the Genesis of Species, as in Fleeming Jenkin's review on which it relied, the question of geological time played an important but secondary role. Mivart used the brevity of time to support the contention that new organic forms must have appeared suddenly rather than through the slow process of natural selection. Otherwise, he asked, why was there no evidence of the countless transitional forms which must have existed? Even granting that the geological record was imperfect, could it be as imperfect as Darwin's theory required? In order to answer the second question Mivart turned to Kelvin, and there his argument was necessarily circumstantial, since he admitted frankly that he was unable to give a knowledgable opinion of Kelvin's arguments. He believed, nonetheless, that the fact that they had never been refuted must provide a strong presumption in their favor. He felt, too, that the capitulation of Huxley and Wallace must be regarded as significant, especially since if Kelvin's conclusions were true, they must tell against natural selection. Much of Mivart's argument turned on the absence of transitional forms, and in that as well as for Kelvin's views, he relied heavily upon Jenkin, whom he did not believe Darwin had adequately answered. He went far beyond Jenkin, however—indeed, far beyond anything Darwin had dreamed of—when he suggested that natural selection would

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78 Marchant, Wallace, Vol. I., p. 251, Darwin to Wallace, Jan. 26, 1870. Although Wallace's article did not appear for another two weeks, it had been ready since December, and he sent a copy to Darwin in proof. He was "anxious to hear if it at all helps to get over your difficulties" (ibid., pp. 249-250).

require one hundred times longer for the pre-Silurian development of life than for post-Silurian. Such a conclusion would invalidate even Wallace's attempt at compromise, since his 24-million-year post-Cambrian age would still require more than 2 billion years from the beginning of life. Such figures were utterly inconceivable in terms of Kelvin's physics, but, as Mivart failed to note, they were also ridiculous in terms of Darwin's views on natural selection.

Except for this final assertion, there was nothing really new in Mivart's treatment of geological time. Thus, given Darwin's generally low regard for Genesis as a whole, it is surprising that he paid any attention to what was obviously a secondary argument. It says much about his state of mind, therefore, that he was disturbed. Like all of the other attacks built on Kelvin's physics, it was the one part of Mivart's argument that he felt incapable of answering. Certainly there is no mistaking the plaintive, almost hopeless tone of his remark when writing about it to Wallace: "I can say nothing more about missing links than what I have said. I should rely much on pre-Silurian times; but then comes Sir W. Thomson like an odious spectre." He indeed found nothing else to say. His detailed reply to Mivart formed most of an additional chapter in the final edition of the Origin, but the questions of missing links and the inadequacy of geological time were conspicuously neglected.

Darwin had made what compromises he could in the fifth edition, well aware that he had not completely answered the physicists' objections. He was also all too aware that increasing numbers of geologists were interpreting their evidence to fit within Kelvin's limitations. He still had no answer to his dilemma. There seemed to be no way to refute the physicists' arguments, but further concessions would, it seemed to him, threaten the very foundations of his theory. Thus, except for two additional sentences, the discussion of geological time remained almost unchanged in the final edition of the Origin. Each of the two sentences, however, was significant in its own way. The first was a proviso amending Darwin's earlier hesitancy about accepting 140 million years as adequate for the pre-Cambrian development of life:

It is, however, probable, as Sir William Thompson [sic] insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; and such changes would have tended to induce changes at a corresponding rate in the organisms which then existed.81

The defections of Huxley and Wallace had had their effect. For the first (and so far as I am aware, the only) time, Darwin publicly abandoned the steady-state uniformitarian geology that had guided so much of his early research. It was not a step taken lightly, and it proved to be the limit of his willingness to compromise.

As with all of Darwin's concessions, this one was made reluctantly, and the second sentence added to the discussion contained the usual partial retraction. This time, however, he reserved his final remarks for his concluding summary, and clearly he meant them as his last word on the subject:

With respect to the lapse of time not having been sufficient since our planet was consolidated for the assumed amount of organic change, and this objection, as urged by Sir William Thompson, is probably one of the gravest as yet advanced, I can only say, firstly,

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that we do not know at what rate species change as measured in years, and secondly, that many philosophers are not yet willing to admit that we know enough of the constitution of the universe and of the interior of our globe to speculate with safety on its past duration.82

As a final word, this was a far from comfortable defense, but it was certainly not surrender. It is perhaps one of the most impressive demonstrations of Darwin’s confidence in the ultimate validity of his theory that, faced with what appeared to be an insuperable obstacle, he asked for a suspension of judgment.

V

There can be little doubt that the concern for the brevity of geological time became one of Darwin’s “sorest troubles.” The despair apparent in his references to it, the compromises he felt obliged to make, and even the felicity with which he embraced the partial relief provided by Croll all bear witness to the fact. Unlike the various biological and geological objections that occupied a much greater portion of his time and effort, Kelvin’s arguments posed a threat to natural selection that Darwin was frankly unable to answer. They were based upon apparently irrefutable physical principles, and they were convincing enough to cast the Lyellian view of geological time into doubt for more than a generation. The embarrassment caused by the Wealden calculation had already made Darwin sensitive to the question of geological time long before he learned of Kelvin’s arguments, and yet despite the capitulations of geology, and indeed of many of his supporters, he refused to yield on the fundamentals of natural selection. Every compromise was accompanied by a qualification, and although this procedure led inevitably to inconsistencies in the later editions of the Origin, it was an orderly retreat.

During the last ten years of Darwin’s life, geology overwhelmingly conformed to Kelvin’s chronology, while paleontology piled up evidence in favor of the gradual evolution of new species. Darwin himself relied less and less upon natural selection alone as he concentrated more attention on the causes of variability. But the problem posed by the contrast between the brevity of time and the extreme slowness of organic change before the Cambrian remained as perplexing as ever. Nevertheless, after the appearance of the final edition of the Origin, Darwin abandoned all direct reference to the problems raised by geological time and left it to younger men to resolve the dilemma. He still had no adequate answer to the physicists’ objections, and the few references to them in his later correspondence make it clear that he still regarded the limitations imposed by the argument from the earth’s cooling to be one of the most formidable objections to his theory.83 He nonetheless remained unwilling to hurry evolution to the extent advocated by Wallace,84 and as he came to rely less on natural selection alone the threat posed by the inadequacy of time seemed less important. Somehow, he was convinced, sufficient time would be found to vindicate his theory.85

84 Ibid., p. 23, Darwin to Wallace, Nov. 8, 1880.
85 Since completing this paper I have found an interesting letter which seems to bear out several of its conclusions. George H. Darwin to Kelvin, Nov. 1, 1878: “I think what my father said about time is quite justifiable from a biological point of view; 100 or 200 million years is ‘incomprehensibly vast,’ even a million is not conceivable in the way a hundred is. I have no doubt however
It was to be more than twenty years after Darwin’s death before radioactivity would provide the answer that he hoped for and thus resolve the dilemma. But, in the meantime, the majority of biologists followed his lead, confident that however long it had taken, natural selection had played a critical roll in the successive appearance of new species.

that if my father had had to write down the period he assigned at that time, he would have written a 1 at the beginning of the line & filled the rest up with 0’s.—Now I believe that he cannot quite bring himself down to [the] period assigned by you but does not pretend to say how long may be required.” Kelvin Papers, No. D8, Cambridge University Library.