Fleeming Jenkin and The Origin of Species: a reassessment

SUSAN W. MORRIS*

INTRODUCTION

Early in June of 1867, Charles Darwin turned back the cover of his copy of the respected quarterly North British Review, to find on its opening pages a lengthy essay attacking his theory of natural selection.¹ As with the vast majority of articles in the Victorian periodicals, the review was anonymous,² prompting immediate speculation in Darwin's circle as to the author's identity. It was to be about a year-and-a-half before Darwin would learn that the engineer Fleeming Jenkin had written the essay. By then, Darwin had concluded that the critique was the most valuable he had ever read on The Origin of Species.

The traditional interpretation of Jenkin's value to Darwin has been that he altered Darwin's views on two related problems: that of what were termed 'individual differences' versus 'single variations', and on a problem known as the 'swamping' of variation by 'blending inheritance'.³ In the nineteenth century, inherited traits were held to derive from the differing traits of the parents having been 'blended' together. 'Individual differences', for Darwin, were the ordinary characteristics by which we recognize any one individual from among all the others in its species. 'Single variations', by contrast, referred to unusual, discontinuous mutations, also known as saltations or sports. Jenkin was thought to have demonstrated to Darwin that single variations, being rare, would be 'swamped' out of existence by the 'blending' of their traits when they bred back into the large, general population of unvaried creatures. The persuasiveness of Jenkin's argument, scholars

* Department of the History of Science, Medicine & Technology, The Johns Hopkins University, Baltimore, Md 21218, USA.


2 W. E. Houghton estimates that '85 to 90% ' of all review articles were anonymous. See Houghton, "The Wellesley Index": uses and problems', The Victorian Periodicals Newsletter (1968), 1, 13.

believed, drove Darwin to greater reliance on Lamarckism and on mechanisms other than natural selection to account for evolution.4

In 1963, Peter Vorzimmer and a number of other historians began pointing out errors in this conventional interpretation.5 Even their work, however, unfortunately shares with the orthodox interpretation many misreadings of what Jenkin said, and how Darwin reacted. This paper reassesses Jenkin, his review, and Darwin’s response, proposing a new explanation of how and why Jenkin influenced Darwin.

Perhaps the most significant misreading lies in identifying Jenkin’s principal argument. The present paper argues that Jenkin’s main point was on the shortness of geological time (the age of the earth) and, though historians sail in troubled seas when dealing with historical influence, proposes that Darwin, too, regarded geological time as the essay’s most important argument. In contrast, most commentators have consistently asserted that Jenkin’s main point deals with variations and inheritance. This view arises, I believe, for two reasons. One is that the swamping argument was, indeed, perceived by many naturalists of the time to be a major obstacle to incremental evolution, spawning a whole school of isolation theories in response. For those theorists the essay’s swamping argument was a crucial obstacle to natural selection, and many later scholars have inferred that Darwin shared this view. Secondly, some authors have been predisposed, by their recognition of the later importance of Mendelism, to see the issue of heredity as presenting a decisive problem for evolutionary theories; they consequently award great importance to arguments, such as Jenkin’s, dealing with inheritance. Accordingly, they have also misread Darwin’s reaction to Jenkin’s essay, seeing in Darwin’s shift to Lamarckism a direct response to problems of heredity. Yet Darwin’s shift can be interpreted as an attempt to accelerate the pace of evolution in order to overcome a newly recognized shortness of time. Scholars have largely overlooked the fact that Darwin’s reading of Jenkin’s review marks the onset of Darwin’s troubles over the dilemma of geological time.

ORIGIN AND ANALYSIS OF THE TRADITIONAL INTERPRETATION

Despite the widespread belief that there is ample evidence that Jenkin influenced Darwin in the matter of variations, actual evidence has always been scanty. Indeed, the traditional interpretation of Jenkin’s influence relies almost wholly on two letters from Darwin to Alfred Russel Wallace in early 1869, in which Darwin noted, ‘Fleming Jenkyn’s [sic]

4 While Darwin’s natural selection rested primarily on the mechanism of small, continuous change, the extent of his reliance on other, larger mechanisms of change was ambiguous. P. Bowler, in ‘Darwin’s concepts of variation’, Journal of the History of Medicine (1974), 29, 196–212, discusses this ambiguity, advancing the idea that Darwin entertained, in addition to single variations and individual differences, a ‘third concept’ of variation, namely small but discontinuous changes. For Darwin’s reliance on sexual selection, see H. Cronin, The Ant and the Peacock: Altruism and Sexual Selection from Darwin to Today, Cambridge, 1991.

arguments have convinced me' and 'F. Jenkins [sic] argued... against single variations ever being perpetuated and has convinced me. It is easy to combine these two statements with changes appearing in the subsequent edition of the *Origin of Species* (fifth edition, August 1869), in which Darwin specifically mentioned Jenkin’s article, and to infer that Jenkin’s criticisms on variations and swamping decisively engendered Darwin’s changes.

Peter Vorzimmer was the first to demonstrate the misleading nature of this superficial interpretation of Jenkin’s influence on Darwin. Citing evidence from Darwin’s notebooks, from the ‘Essay of 1844’ and particularly from the *Variation of Animals and Plants under Domestication*, all written before Jenkin’s article appeared, Vorzimmer demonstrated that the changes appearing in the fifth edition of the *Origin* expressed ideas Darwin had long held. This impressive argument, supported by Olby’s 1963 publication of Darwin’s manuscript on ‘Pangenesis’, and by more recent work by Geison, demonstrated that, with respect to Darwin’s views on inheritance and variation, Jenkin’s article could not have done more than convince Darwin of the correctness of conclusions that he had already reached on his own.7

We cannot doubt, however, that Jenkin’s essay had an important effect on Darwin. For example, Darwin wrote to J. D. Hooker that, ‘Fleming Jenkyns [essay] has given me much trouble, but has been of more real use to me than any other’, and Francis Darwin observed that ‘my father ... felt [Jenkin’s criticisms] to be the most valuable ever made on his views’. 8

Scholars have assumed that these two statements refer to variations and inheritance. Indeed, they are generally adduced as part of the ‘ample evidence’ supporting the traditional interpretation of Jenkin’s role. A moment’s reflection, however, makes us realize that these statements do not link Jenkin’s essay with any specific area of Darwin’s theory. Linking them to variations and inheritance is a convention, which does not appear to be supported by the evidence.9

Vorzimmer’s paper, therefore, by the very process of demonstrating that Darwin was not notably influenced by Jenkin in the matter of variations, effectively creates a new puzzle: if Jenkin’s essay was indeed as inconsequential as Vorzimmer suggests, what could have led Darwin to conclude that the review was so valuable? The present paper suggests that Darwin had in mind Jenkin’s argument on geological time.

In the nineteenth century, there were two great problems that barred full acceptance of the emerging ideas of evolution: one was the nature of heredity; the second was a genuine doubt that the earth was sufficiently old for a slow evolutionary scheme such as Darwin’s to have worked. With the onset of the twentieth century, the introduction of Mendelism

6 Darwin to Wallace, 22 January 1869 and 2 February 1869, British Library (hereafter BL) Add 46434. See also F. Burkhardt, S. Smith et al. (eds.), *A Calendar of the Correspondence of Charles Darwin, 1821–1882, with Supplement*, Cambridge, 1994, which lists the published sources and the known archive of each letter.
7 Olby, op. cit. (5); Geison, op. cit. (5); Vorzimmer, op. cit. (5), 389.
9 Typical examples of this linkage can be seen in Willis, op. cit. (3), 5, who writes ‘the variation would soon tend to be lost by crossing. This was shown by Fleeming Jenkin in a criticism which Darwin considered as the best that was ever made of his work.’ And Eiseley, op.cit. (3), 209–10, in discussing Darwin’s views on variations, says, ‘there is ample testimony... to the effect which Jenkin’s criticism had upon him’, and then cites as that testimony the letters to Wallace as well as that to Hooker; this latter, as just noted, says nothing about variations.
greatly facilitated the understanding of heredity, hence of evolutionary theory; as a result considerable attention has been focused on the historical development of inheritance theories. By contrast, the discovery, at roughly the same time, that the earth’s heat was constantly generated by radioactive decay, restored to naturalists the ‘countless ages’ needed for gradual evolution, and thus allowed questions about the age of the earth to evaporate as an obstacle for evolutionary theorists. The interest of historians in the question appeared to have evaporated with it, until Stephen Brush and Joe Burchfield reminded scholars that the earth’s age always remained a deeply significant quandary for Darwin. Burchfield suggested, moreover, that Darwin’s inability to devise a satisfactory counter-argument allowed historians to undervalue a problem Darwin himself regarded as the ‘gravest’ objection ever raised against his theory.

In other words, when the shortness-of-time threat dissipated, Jenkin’s criticisms on this point came to appear superfluous. Consequently, with the question of geological time declining in historical importance, and interest in heredity increasing, Jenkin’s essay, and its influence on Darwin, came to be treated within the framework of Mendelian enlightenment, casting the rest of Jenkin’s review into the shadows. One has only to note how universally Jenkin’s article is described as having only two arguments – the swamping from blending inheritance, and the limits to variation – to realize how narrowly his article has been assessed. In truth, the article makes five separate arguments against natural selection: on the limits to variation, the swamping of variations by large numbers, the shortness of geological time, the problems of classification, and the logical rigour of Darwin’s theory.

The traditional interpretation of Jenkin’s essay appears to begin with St George Jackson Mivart, in his 1871 On the Genesis of Species. In advancing his own ideas in opposition to Darwin’s, Mivart made extensive use of Jenkin’s essay. His characterization of the essay introduced what became the traditional interpretation of Jenkin’s contribution: Mivart emphasizes the swamping of variations by inter-crossing; he quotes the notorious

12 For example, both Poulton, op. cit. (3), and Eiseley, op. cit. (3), embed their discussions of Jenkin’s influence on Darwin within their chapters on Mendel.
13 For example, Vorzimmer, op. cit. (5), 387, says of Jenkin’s essay: ‘the main attack can be divided into two distinct halves…one against individual differences, the other against saltations’. Hull, op. cit. (1), 346, says: ‘Jenkin raised two major objections.’ P. J. Bowler, in The Eclipse of Darwinism, Baltimore, 1983, discusses only blending inheritance before noting that Jenkin ‘also argued’ about limits to variation. Similarly, Hardin, op. cit. (3), hints at the limits argument but focuses on swamping. Eiseley, op. cit. (3), mentions only swamping. J. Moore, in The Post-Darwinian Controversies, Cambridge, 1979, and M. Ruse, in The Darwinian Revolution, Chicago and London, 1979, both briefly mention Jenkin’s criticisms relating to geological time, but focus on swamping and limits. Geison, op. cit. (5), and Burchfield, op. cit. (5), and in Lord Kelvin and the Age of the Earth, New York, 1975, both mention Jenkin’s argument about geological time, but do not see it as the essay’s centerpiece. Burchfield, for example (Kelvin, p. 73), asserts that Jenkin’s review ‘hinged’ on the swamping argument, and Geison says Jenkin’s ‘attack…was directed especially against…heredity’, op. cit. (5), 380. C. Smith and M. N. Wise, Energy and Empire: A Biographical Study of Lord Kelvin, Cambridge, 1989, focus on Jenkin’s argument on geological time. Only S. J. Gould, in ‘Fleeming Jenkin revisited’, Natural History (1985), 94, 14–20, assesses Jenkin’s full article.
‘shipwreck’ story (reviewed below), which soon became standard practice for discussions of Jenkin’s work; he cites the limits to variation as Jenkin’s second argument; and he originates the claim that Jenkin’s review provoked a change in Darwin’s thought by writing that ‘the consideration of [Jenkin’s] article has occasioned Mr. Darwin to make an important modification in his views’.14

Clearly, the close identification of the swamping argument with Jenkin’s essay seems to issue from Mivart. Jenkin, in fact, was not unique in making the swamping criticism against natural selection, but his review was one of the first to receive much attention. Appearing, as it did, in 1867, Jenkin’s work seemed to have provoked the school of isolation theories which arose almost immediately thereafter, intended to surmount the problem of swamping; Moritz Wagner’s ‘Migration Law’, published in 1868, was one such example.15 Until the twentieth century and the introduction of Mendelism, the swamping problem greatly concerned evolutionary theorists (other than Darwin), and Jenkin continued to receive the greatest credit for introducing it. In 1886, for example, George Romanes, writing on the ‘Difficulties against Natural Selection as a theory’ could state that, ‘the third and last difficulty... consists in the swamping influence... [which] was first prominently announced in an anonymous essay by the late Professor Fleeming Jenkin’. Romanes went on to describe Jenkin’s article as,

...a highly remarkable one... [presenting] a more searching and effective criticism of Mr. Darwin’s theory than any other essay with which I am acquainted. With regard to [swamping], the criticism is especially cogent and, so far as I know, is the only criticism of importance which Mr. Darwin has not expressly answered.

Romanes then quoted, in a footnote, all of Jenkin’s shipwreck story, as a way of giving the flavour of Jenkin’s critique.16

By 1888, swamping was being described as the ‘celebrated difficulty’ of Darwinian evolution, ‘first forcibly pointed out by the late Professor Fleeming Jenkin’.17 Vernon Kellogg, in 1907, illustrated the swamping problem with a mathematical example patterned on Jenkin’s, although not mentioning him by name.18 And by the 1930s and 1940s, J. C. Willis was writing that ‘Fleeming Jenkin...pointed out that unless a great many individuals varied in the same direction over the whole of a considerable area, the improvement would promptly be lost by crossing.’19 By 1952, what had been a ‘celebrated difficulty’ had grown into ‘a most devastating criticism of many points of the Darwinian theory... Jenkin’s criticism... probably did more to weaken Darwin’s position than any other single criticism’.20 Views such as these have led to today’s narrow and prevailing interpretation of Jenkin’s work.

18 V. L. Kellogg, Darwinism To-Day, New York, 1907, 44–5.
19 Willis, op. cit. (3), 165; see also 5, 13–14, 25.
We do not know when Darwin learned that Fleeming Jenkin was the author of the article in the *North British Review*. While he had, when it was published, discussed the review in depth with friends and colleagues (see below), 22 January 1869 is the first time one sees Darwin link Jenkin’s name to the critique when, in the context of describing changes in the forthcoming (fifth) edition of the *Origin*, Darwin wrote to Wallace those well-known words, ‘I always thought individual differences more important than single variations but now I have come to the conclusion that they are of paramount importance, & in this I believe I agree with you. Fleming Jenkyn’s [sic] arguments have convinced me.’

Wallace responded on 30 January, ‘Dear Darwin, Will you tell me where are Fleming Jenkyn’s [sic] arguments on the importance of single variations. Because I at present hold most strongly the contrary opinion, that it is the individual differences or general variability of species that enables them to become modified and adapted to new conditions.’ Wallace’s letter is striking in its urgency, and odd in that Wallace had not only read Jenkin’s article but had, only months before, published a lengthy response to it. Therefore, he must either have not yet known that it was Jenkin who had written the *North British* essay, or he believed that Darwin referred to a new essay by Jenkin. Darwin, apparently himself forgetting that Wallace was already familiar with Jenkin’s review, replied on 2 February,

I must have expressed myself atrociously: I meant to say exactly the reverse of what you have understood. F. Jenkins [sic] argued in *N. Brit. R.* against single variations ever being perpetuated and has convinced me, though not in quite so broad a manner as here put. I always thought individual differences more important, but I was blind and thought that single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to a similar conclusion, and I like much to be in accord with you.

What do these letters actually say? The cautionary, ‘though not in quite so broad a manner as here put’ seems clearly intended by Darwin to deter Wallace from reading too much into the preceding phrase that Jenkin has ‘convinced’ him. We should note the intensity of Wallace’s reply, as contrasted with Darwin’s offhand approach, marked by his somewhat careless use of language in the first letter. The whole subject of variations, he implies by the word ‘merely’, is not crucial to him, he has raised the subject in the first letter because he knows it to be of immediate interest to Wallace, not necessarily to himself. The solicitous, ‘I like much to be in accord with you’ reinforces this point.

The problem of variations seems to have been of far greater import to Wallace than to Darwin, an interpretation consistent with the work of Bowler and Vorzimmer. Wallace’s views on variation were, at this time, still in ferment, whereas Darwin’s views had been

---

21 BL (Add 46434). Fleeming is pronounced ‘Flemming’, hence Darwin’s spelling. Jenkin was named after his father’s commanding officer, Admiral Fleeming.
22 DAR 106/7 (Ser. 2): 75–6.
23 Wallace’s rejoinder to Jenkin (and to the Duke of Argyll) was included in Wallace’s essay ‘Creation by law’, *Quarterly Journal of Science* (October 1867), 471–88; reprinted in Wallace’s *Contributions to the Theory of Natural Selection*, London, 1875, 264–301.
24 BL (Add 46434).
settled for some time. Wallace's later reliance on neo- or ultra-Darwinism is further evidence that individual differences held, for him, a far more incisive role in evolution than they did for Darwin. Therefore, with respect to his above-noted correspondence with Darwin, to distinguish their role from that of single variations was a compelling problem for Wallace. This interpretation concurs with Vorzimmer's demonstration that Jenkin's comments regarding variations were not significant to Darwin. Consequently, we might infer that those comments cannot have been what led Darwin to say the essay had 'been of more real use to me than any other'.

The other place in which Darwin connects Jenkin's review to the problem of variations is in the Origin itself. In the fifth and sixth editions (August 1869 and February 1872), Darwin replaced what had been a paragraph on inheritance with a discussion of variations and individual differences, which reads, in part,

I saw, also, that the preservation in a state of nature of any occasional deviation of structure, such as a monstrosity, would be a rare event; and that, if preserved, it would generally be lost by subsequent intercrossing with ordinary individuals. Nevertheless, until reading an able and valuable article in the 'North British Review' (1867), I did not appreciate how rarely single variations, whether slight or strongly-marked, could be perpetuated.

Historians usually stop the quotation at this point, raising the question of why, given Vorzimmer's proofs of 1963, Darwin would have said that Jenkin's essay had brought to his attention a point we now know he had been thinking about for twenty-five years. One reading that the scholar may give to this curious passage, however, is that Darwin's statement was simply gratuitous. There are other instances in the Origin where he falsely credits other authors with the origin of his ideas. One is his citing the Saturday Review (December 1859) for having convinced him to modify his calculations regarding the denudation of the Weald when he had, in fact, at the prodding of Sir Charles Lyell and others, already decided to change his Weald argument before the review appeared. We might infer, therefore, that Darwin used such citations as literary strategies to placate critics, as much as to acknowledge genuine influences on his thinking.

A more revealing instance of such an attribution, however, is found in Darwin's discussion of Mivart's criticisms. There, Darwin was again writing in general about ideas he had developed long before, yet he states, 'I am bound to add, that some of the facts and arguments here used by me, have been advanced for the same purpose in an able article lately published in the "Medico-Chirurgical Review". This citation is directly analogous to the one concerning the North British Review, in that Darwin used both to acknowledge published sources for material he was borrowing. In the case of Jenkin's review, however,

26 Darwin, op. cit. (11), 178.
27 In the first two editions of the Origin, Darwin included his own projection of the time that had been required for the denudation of the Weald, an area in southeastern England. His 'crude notion' of 306662400 years was immediately ridiculed, even by his friends, for the inexactness of the premises he had used, and Darwin soon regretted ever having included the discussion in his book. Several scholars discuss the changes Darwin made in his Weald argument. Burchfield discusses the role of Lyell and Hooker, in particular. See Burchfield, op. cit. (5), 303–6; see also Darwin, On the Origin of Species, London, 1964, 286–87; Darwin, op. cit. (11), 483–4.
28 Darwin, op. cit. (11), 264.
this becomes evident only if one reads beyond the point at which historians generally end
the quotation, for Darwin goes on to say,

I did not appreciate how rarely ... variations ... could be perpetuated. The author takes the case of
a pair of animals, which produce during their lifetime two hundred offspring, of which, from
various causes of destruction, only two on an average survive to procreate their kind ... He then
shows that if a single individual were born...

and so on. In other words, Darwin borrowed a mathematical example that appeared in
Jenkin's essay, and acknowledged his source with his customary display of generosity.

HENRY CHARLES FLEEMING JENKIN (1833–85), AND THE ORIGIN
OF AN ARGUMENT

Most of what we know of Jenkin has come from the Memoir of Fleeming Jenkin written
by his student and friend, Robert Louis Stevenson. Although charming, this work often
presents Jenkin more as caricature than character. As one insightful friend later noted,
"Jenkin was not altogether fortunate in his biographer ... Perhaps a narrative by a less
expert pen would have presented a larger figure." Excessive reliance on the Memoir has
resulted in scholarly repetitions of the same anecdotes, occasionally supplemented with
information from obituaries on Jenkin, and to a limited extent with correspondence
preserved in the Kelvin collections at Glasgow and Cambridge Universities. While drawing
on these standard resources, this paper also uses previously unexploited material to shed
new light on Jenkin's circle, and on how he became involved in the evolutionary debate.

Although generally described as Scottish, Jenkin was English on his father's side and
only partly Scottish on his mother's. He was born and spent his early years at his father's
family home in Kent, about 40 miles from Darwin's Down. His father, Charles, was a naval
officer, and to the manner born. The family fortune having been lost, however, it was
always clear that Fleeming would have to make his own way in the world. Fleeming's
mother was an intelligent, flamboyant woman whose passions for music, literature and
politics dominated the small family's social relations. Whether living in Paris, Genoa,
Manchester or London, the Jenkins dwelt in political and literary circles.

Fleeming's formal schooling began in Scotland, where for three years his schoolmates
included Peter Guthrie Tait and James Clerk Maxwell. An only child, he then moved with
his parents to the Continent, living for various periods in Germany, Paris, and Italy, where
he matriculated at the University of Genoa, taking his MA with first class honours in 1850,
at the age of seventeen.

29 Darwin, op. cit. (11), 178.
30 Stevenson's Memoir of Fleeming Jenkin is the only full biography of Jenkin. It is included in Jenkin, PLS,
op. cit. (1), i, pp. xi-cliv, and was also published separately (New York, 1887) as well as in nine different editions
of Stevenson's collected works.
31 B. Matthews (ed.), 'Introduction' to Papers on Acting Vol. III: Mrs. Siddons as Lady Macbeth and as Queen
Katharine by H. C. Fleeming Jenkin, New York, 1915, on 16.
32 The Jenkin family had moved from York to Kent during the reign of Henry VIII, although Stevenson writes
that the family 'derived from Wales', thus some describe Fleeming Jenkin as Welsh. R. L. Stevenson, op. cit. (30)
(New York, 1887), 2.
Already by this age, Jenkin had developed the appetite for melodrama that would inspire a lifetime of varied interests and activities. Whether mounting the barricades in the Paris revolution of 1848, or dodging the crossfire in Genoa, whatever was theatrical and dramatic was dear to him. His passion for the drama, especially, is an aspect of his life warranting greater attention from scholars. Not content with reading plays, he also wrote and staged them. He wrote drama criticism, papers on acting, and on Greek costume. His favourite music, not surprisingly, was opera. His mature scientific work explored the sound and articulation of the human voice. This latter work has been seen as the outgrowth of his interest in electrical science, which it certainly was, but we should not fail to see it also as typical of his fascination with mechanisms of theatrical communication. Jenkin was one of the first to grasp the significance of Edison’s new phonograph as a vehicle for permanently preserving records of the spoken word, particularly for preserving the ephemeral ‘actor’s art’. Inevitably, this fascination with the tools of imaginative and dramatic representation found its way into Jenkin’s essays. The profession of engineering itself served Jenkin’s inventive and theatrical nature: building bridges, railroads, and travelling round the globe on massive cable-laying expeditions were enterprises of stagecraft, writ large. His world view looked out on a singular intersection of scientist and artist, realist and romantic.33

Jenkin and his parents returned to England in 1851 when Fleeming entered upon an engineering apprenticeship with the prestigious firm of William Fairbairn in Manchester. This location brought him into contact with Joule’s work on the mechanical equivalence of heat, and with the newly developing ideas of thermodynamics. Following his apprenticeship and a period in Switzerland working on railroads, he joined R. S. Newall & Co., the firm then manufacturing the first Atlantic telegraph cable. Although still quite young, Jenkin apparently had charge of engineering and electrical work, machine design, construction, and testing of cables and equipment.

In 1855, Jenkin began making frequent visits to the London home of Alfred and Eliza Austin who would, in 1859, become his parents-in-law, and to whom he was introduced by the Manchester novelist Elizabeth Gaskell. Alfred Austin and his two elder brothers, John and Charles, were prominent members of Britain’s legal establishment. They were and had been close friends with Jeremy Bentham, James and John Stuart Mill, Carlyle, Macaulay, and other luminaries of British literature, politics and economics.34 A family of brilliant and bold talkers, the Austins celebrated and debated the latest ideas in politics, economics, science and the arts. It seems likely to have been in this company that Jenkin first honed the skills in political economy that later enabled him to make seminal contributions to economic science.35

33 Jenkin is a striking example of nineteenth-century interactions between realism, romanticism, science and art, as described by Brush, op. cit. (10), especially 482–93, and Brush, The Temperature of History: Phases of Science and Culture in the Nineteenth Century, New York, 1978.

34 J. S. Mill’s Autobiography (Halifax, 1992, ed. A. O. J. Cockshut) is replete with allusions to the Austins; see, for example, ch. 3.

35 Jenkin is today more often cited in the economics literature than in the history of science. His contributions were made in five papers, included in PLS, op. cit. (1), and republished in 1931 by the London School of Economics as No. 9 in Series of Scarce Tracts in Economic and Political Science.
In the distinguished and argumentative society that gathered in the Austins’ drawing-room, young Jenkin had opportunities to exercise his already broad range of intellectual interests and to train his enthusiasm for verbal combat. Nearly every description of Jenkin which has come down to us attests to his relentless zeal for argument. Colvin described him as ‘too trenchant in reply and too pertinacious in discussion… the most unflinching of critics and disputants… He would eagerly watch for and pounce on your remarks, and the futile or half-sincere among them he would toss aside with a prompt and wholesome contempt, his eye twinkling the while.’ And a son of Jenkin’s, ‘used not to understand why Papa seemed so fond of contradicting everyone and of saying things exactly as he meant them, especially to people we wished to make friends with’. Moreover, of his essays in the *North British Review*, Jenkin himself confided to the editor, David Douglas, that ‘I hope for hearty abuse, which is much better than to be passed over in silence. I am confident I can crack my opponents’ heads if they will only fight.’

By 1859, Fleeming Jenkin was evidently a young man on his way up. Through his cable-work he became friends, and soon a business partner, with William Thomson, nine years his senior and, at thirty-five, already an important figure in British science. In 1861, the British Association for the Advancement of Science formed a committee to determine and fix standards for electrical measurements, appointing Jenkin to the post of secretary. Collaborating with his old schoolmate, J. C. Maxwell, and with another Scot, Balfour Stewart, Jenkin over the next few years experimented and wrote reports establishing the units in which to express the magnitudes of various electrical phenomena. By the early 1860s, then, Jenkin found himself in a circle comprising Thomson, Maxwell, Stewart and their ilk, who shared a strong opposition to Darwin’s theory. Thomson, in particular, was exercised over the uniformitarian geological theory on which natural selection was founded, and he profoundly influenced Jenkin’s views.

Historians of science have tended to see Jenkin as a kind of Dr Watson to William Thomson’s Sherlock Holmes, a view seemingly encouraged by Thomson himself. That Thomson regarded Jenkin as his own creation would not seem to be in doubt: for example, in his obituary of Jenkin, Thomson mentions himself five times in the first seven paragraphs. None the less, the ‘chilly tone of patronage’ that marked Thomson’s official

37 Charles Frewen Jenkin to R. L. Stevenson, 9 August 1887, Yale University, Beinecke Library, Robert Louis Stevenson Collection (hereafter Beinecke Collection) no. 4982.
38 F. Jenkin to David Douglas, 2 March 1868, letter no. 114, ‘Scrapbook’, Douglas Papers, National Library of Scotland (NLS). Having been introduced by P. G. Tait, Douglas wrote that Jenkin ‘was soon to become one of my most trusted allies’. (D. Douglas, ‘Scrapbook’, ch. 11, p. 143, Douglas Papers, NLS). Jenkin’s letter was occasioned by his *NBR* essay on trade unions (‘Trade-Unions: How Far Legitimate’, *NBR* (March 1868), 48 o.s., 1–34); he and Douglas were considering republishing the ‘Trade-Unions’ and the *Origin of Species* articles as a book. See Jenkin to Douglas, 12 February 1868, letter no. 113, Douglas ‘Scrapbook’, NLS. I am most grateful to Joanne Shatock for personal communication and material about Douglas and the *North British Review*.
39 Sir William Thomson was not created Lord Kelvin until 1892, well beyond the time period treated in this paper. To maintain consistency with the quotations in the paper, I refer to him throughout as Thomson, rather than Kelvin.
40 The articles appeared individually in the annual *Report of the British Association* for the years 1862–64 and 1867. They were collected and reprinted in Fleeming Jenkin (ed.), *Reports of the Committee on Electrical Standards*, London and New York, 1873.
recollections of Jenkin was startling even to the latter’s family and friends. However, in the early years of their friendship Jenkin certainly was a devoted acolyte of the dynamic genius whom Jenkin’s wife dubbed ‘Professor Apollo’.  

One of Thomson’s earliest and lifelong interests concerned the physics of the earth and the solar system. He took as his starting point Fourier’s work on heat conduction, and he immediately applied to the earth the implications of the new researches in thermodynamics in the 1830s. Thomson held that the sun was a fluid mass, gradually cooling, and that the continuation or maintenance of its heat came from gravitational contraction. This was consistent with his view that gravity was ‘the original form of all the energy in the universe.’ The sun’s heat, in other words, was the result of mechanical action.

In the early 1850s, Thomson succeeded in reconciling the apparently paradoxical conclusions from the researches of Sadi Carnot and James Joule that when work was done, heat (energy) was both lost and conserved. Thomson showed that although heat (energy) was not lost absolutely, it was, largely through friction, ‘dissipated’, or rendered unavailable to do useful work, as the bodies doing the work approached a uniform temperature. This implied that although the total amount of energy in the universe would remain forever constant, its ability to do work would gradually but continually diminish. This did not ipso facto imply a state where everything would be cold:

When all the chemical and gravitation energies of the universe have taken their final kinetic form, the result will be an arrangement of matter possessing no realizable potential energy, but uniformly hot – an undistinguishable mixture of all that is now definite and separate.  

Meanwhile, beginning in the 1830s, debate flourished between those geologists known as uniformitarians and those called catastrophists. ‘Uniformitarianism’, as William  

41 In addition to the obituary, Thomson composed a ‘Note by Sir William Thomson on Fleeming Jenkin’s contributions to electrical and engineering science’, included in the posthumous publication of Jenkin’s papers and biography (PLS, op. cit. (1)). Sidney Colvin to R. L. Stevenson, 13 September [1887]: ‘[Mrs Jenkin and I have] settled a few remaining points as to the memoir, & chiefly, to cut out altogether the two concluding paragraphs of Sir W. Thompson’s [sic] note. Left standing, their chilly tone of patronage could not but shock: without them, & headed as we have now headed it, ‘Note by Sir W.T. on F.J.’s contributions to Electrical and Engineering Science’, the thing will read all right and in its place. Mrs Jenkin, when the book out, will write to Sir W.T., throwing the responsibility of the deletion on me as editor, and saying that as his (F.J.’s) general intellectual powers & pursuits have been so fully dwelt on in the memoir, it seemed that the reference to them here might be spared.’ Beinecke Collection, no. 4390. Thomson’s obituary on Jenkin is found in ‘Obituary notices of fellows deceased’, Proceedings of the Royal Society of London (1885), 39, pp. i–iii and was reprinted in Thomson’s (Baron Kelvin’s) Mathematical and Physical Papers, 6 vols., Cambridge, 1911, vi, 335–8.

42 Smith and Wise, op. cit. (13). Part III of this work, ‘The economy of nature: the great storehouse of creation’, contains extensive discussion and explanation of Thomson’s work in thermodynamics and its application to the questions of the ages of the sun and earth. See especially chs. 15, 16 and 17, pp. 524–611.


44 Thomson and Tait, op. cit. (43), 606.

Whewell famously defined it, meant that geological activity or causes had been uniform, in kind and in degree, throughout time. It denied that geological agents, such as volcanoes, earthquakes, or erosion, had been any more extreme or rapid in the past than in the present, or that any agents had existed in the past which did not obtain in the present. In keeping with these views, uniformitarians and catastrophists tended to divide over the extent to which the world was ‘progressive’. In practical terms this meant evaluating whether the earth (or any other body in the solar system) had originated in a primitive state and was constantly evolving toward an eventual end state quite different from its original. Charles Lyell, the leading advocate of uniformitarianism, denied this was possible and believed instead in a ‘ceaseless repetition of continent-raising and continent-ending’ processes.46

Obviously, the notion of non-progression held by Lyell was incompatible with the new thermodynamic principles enunciated by Thomson. If all natural processes generated friction and consequently heat, which was thereby dissipated so that it was no longer available to do further work, then the sun and the earth and the entire solar system had finite lives.

While not yet exercised over (or perhaps aware of) the large bank of time being drawn on by the uniformitarians, Thomson in the 1850s began arguing that the idea of a cyclical and non-progressive universe with ‘no vestige of a beginning, no prospect of an end’47 contravened the physical idea (which we now recognize as fact) of the dissipation of energy. Yet even Thomson was, at the time, given to using terms such as ‘countless ages’ and ‘endless futurity’ while writing, with no apparent irony, that ‘it seems not improbable that the earth has been efficiently illuminated by the sun alone for not many times more or less than 32,000 years’ and, ‘I conclude that sunlight cannot last as at present for 300,000 years.’48 Hardly an ‘endless futurity’. And even while employing such rhetoric as ‘countless ages’, Thomson would write that ‘the end of this world as a habitation for man, or for any living creature or plant at present existing in it, is mechanically inevitable; and...purely mechanical reasoning shows a time when the earth must have been tenantless’.49

46 Cannon, op. cit. (45), 38. Ironically, considering its importance to Darwin, Lyell’s uniformitarianism was essentially opposed to organic evolution. Indeed Lyell, in the 1830s–40s, denied the possibility of ‘the successive development of animal and vegetable life, and their progressive advancement to a more perfect state’, and by 1850 had modified his views only far enough to say that ‘the popular theory of the successive development of the animal and vegetable world...rests on a very insecure foundation’. Principles of Geology, 2nd edn, 3 vols., London, 1832–33, ii, 157; 1850 quote from 8th edn, London, 144. The extent to which Darwin adopted Lyell’s uniformitarian theory is analysed in R. Hooykaas, The Principle of Uniformity in Geology, Biology and Theology, Leiden, 1963, and in M. Bartholomew, ‘The non-progress of non-progression: two responses to Lyell’s doctrine’, BJHS (1976), 9, 166–74. Added irony, given William Thomson’s opposition to Darwinism, is that Thomson’s work, to a large extent, helped ‘lay the ground for a modern, evolutionary view of the earth’ and thus made Darwinian evolution more plausible. See R. H. Dott, Jr, ‘James Hutton and the concept of a dynamic earth’, Toward a History of Geology (ed. C. J. Schnee), Cambridge, Mass. and London, 1969, 122–41, on 140.


Thomson’s projections about the ultimate age of the sun were based on the sun’s estimated mass, and the known rate at which it ‘gave away’ its energy. For the earth, Thomson based his calculations on its temperature gradient, on the assumption that the earth in its primitive state had been uniformly molten and was gradually cooling in accord with Fourier’s heat equations, and on the known melting points of rock. Importantly, he calculated that no chemical reactions in the interior of the earth could possibly account for the amount of heat actually contained in it. A corollary was that the continual cooling of both the earth and the sun, caused by the dissipation of energy, meant that geological processes could not possibly have remained constant, or uniform, over the immense periods of time envisioned by the Lyellians.

That Thomson was eager to press this idea on geologists may be surmised by the conclusion to his 1852 paper ‘On a universal tendency in nature to the dissipation of mechanical energy’:

Within a finite period of time past the earth must have been, and within a finite period of time to come the earth must again be, unfit for the habitation of man as at present constituted, unless operations have been, or are to be performed, which are impossible under the laws to which the known operations going on at present in the material world are subject.50

Over the next several years Thomson would, with more felicitous phrasing, reiterate this view time and again.

For example, in two papers, ‘Physical considerations regarding the possible age of the sun’s heat’ and ‘An examination of some points in the doctrine of the internal heat of the Globe’, delivered at the British Association meeting in Manchester in 1861, Thomson launched what was to become an implacable attack against uniformitarianism. Jenkin, apparently at Thomson’s request, helped to see Thomson’s papers onto the meeting’s agenda. Since a broken leg prevented Thomson’s attendance, Jenkin wrote to him from Manchester that,

after some writing and enquiry I found Prof. Rogers had taken charge of your papers, so I did not think you would like me to interfere further: but today the Sun paper was on the list but not read and stands first for tomorrow. I am afraid the Earth will not come on or will come on far too late. The geologists seem to have referred it to Section A and by that time Section A was very full. I am very sorry for this as I think it would have cut some of their arguments short… I am sorry I could not do more for your papers, I wish I had had them from the first.

A few days later Jenkin wrote again,

to give you some private news of the British Association. I am sorry to say the paper on the Sun being put off to Wednesday had no justice done to it: being cut short by Airey [sic] and not discussed – the paper on the Earth met with a somewhat better fate and was followed by a good discussion: but I wish you had been present.51

We can only speculate whether the discussion might have changed had Thomson indeed been present. Curiously, the official Report of that BAAS meeting makes no mention of this

50 Thomson, ‘On a universal tendency in nature to the dissipation of mechanical energy’, Philosophical Magazine, Ser. 4 (1852), 4, 304–6, on 306.

51 Both letters are undated, but from the contents we can deduce that the first letter was written on Tuesday 10 September 1861. Both are in Cambridge University Library, Kelvin Manuscript Collection: the first is no. J37, Add 7342; the second, no. J36, Add 7342.
paper on the earth, and the only printed records of it appear to have been in *The London Review* and *The Athenaeum*.52

Some have suggested that Thomson's BAAS papers of 1861 may have been provoked by his reading of Darwin's *Origin* 53 Burchfield, for example, posits that until Thomson learned of Darwin's calculation of some 300,000,000 years for the denudation of the Weald, he may not have realized what immense periods of time were actually envisioned by the uniformitarians when they spoke of 'countless ages' and of 'times incalculably remote'. Certainly at Manchester Thomson aimed directly at Darwin when asking, 'what are we to think of such geological estimates as 300,000,000 years for the "denudation of the Weald"?' 54 Yet Thomson's argument still appears to have been aimed against uniformitarianism. His paper on the sun, for example, ended with the statement that, 'it seems therefore, on the whole, most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years'. He then reasserted his 1852 conclusion, but now expressed with uncharacteristic eloquence:

> As for the future, we may say with equal certainty that inhabitants of the earth cannot continue to enjoy the light and heat essential to their life for many million years longer, unless new sources, now unknown to us, are prepared in the great storehouse of Creation.55

Following the BAAS meeting, Thomson published a flurry of articles in the lay press: first, an abstract of the 'Age of the sun's heat' in *The Athenaeum*; next, the full paper in *Macmillan's*; finally an article on 'Energy', co-written with P. G. Tait, in the Presbyterian journal *Good Words*.56 These actions readily convey the importance to Thomson of bringing the principles of this new science – the conservation and the dissipation of energy – and their implications for the earth and the sun, to the attention of a wide general audience. Certainly the time was right to 'raise the post-Darwinian debate above the level of popular controversy', as Smith and Wise suggest,56 and place it in the context of the new thermodynamics. Still, beyond the comments he had already voiced, Thomson made no further attacks on natural selection *per se*, concentrating his attention instead on the geophysics that was his forte.

Thomson delivered a revised and retitled version of his BAAS paper on the earth in April 1862 in Edinburgh, and saw it published in January 1863.57 In this technical article,
combining recent data on temperature gradients with Fourier's equations for heat conduction, Thomson conjectured that the age of the earth ranged between 20 million and 400 million years, with the most likely figure around 98000000 years. Then in 1865, in the absence of any visible concessions by the uniformitarians in the intervening years, Thomson continued his attacks on Lyell's geology with the haughty ""Doctrine of Uniformity"" in geology briefly refuted".58 In May 1866, he delivered Cambridge University's Rede Lecture, speaking on 'The Dissipation of Energy', wherein he presented new ideas on the age of the earth, and where he repeated verbatim the concluding lines of his 1852 paper (above).59

It seems probable that readers such as Darwin saw Thomson's papers as a continuation of the uniformitarian–catastrophist debate in which Thomson's former Cambridge mathematics coach, William Hopkins, a respected geologist, was prominent in leading the anti-uniformitarian forces. The difference between an argument based on primitive heat and one based on the dissipation of energy was subtle, and while Hopkins himself had attempted to awaken his fellow geologists to the importance of thermodynamics for their field, it cannot have been obvious to non-physicists how Thomson was adding anything new to the debate, beyond abstruse mathematics and a shrill tone.60

Not surprisingly, therefore, Darwin's reaction to Thomson's polemics was one of indifference. Indeed, he was almost contemptuous of the arguments levelled at him from physics and astronomy. In 1866, for example, he snapped at J. D. Hooker, 'I cannot think how you can attach so much weight to the physicists, seeing how Hopkins, Hennessey, Haughton, and Thomson have enormously disagreed.'61

Darwin's pique was justified. We have already seen that Thomson's own estimates for the age of the sun had varied between 32000 years (in 1854) and 500000000 years (in 1861). This was in part due to changes in Thomson's ideas on the origin of the sun's heat. Additionally, the available data were constantly changing, particularly in new areas of research such as the temperature gradients of earth, so that while Thomson's methods remained consistent, the solutions to his equations varied as a result of changing their initial conditions. Moreover, differences in their opinions on the rigidity of the earth and the thickness of its crust divided the conclusions of Thomson from those of Hopkins. For his part, Samuel Haughton changed from an estimate (in 1862) of 100 million years to (in 1864) 2300 million.62

None the less, Darwin's complacency was to be short-lived. It was impossible to accommodate both uniformitarianism and natural selection within the new principles of

59 The Rede Lecture was not published but an extensive account of it appeared in the Cambridge Chronicle, 26 May 1866, and that account was reprinted in S. P. Thompson, The Life of Lord Kelvin, 2 vols., 2nd edn, New York, 1976, i, 437–42.
60 Hopkins's BAAS Presidential Address presents an example of his views regarding the application of thermodynamics to geology; see 'Address by William Hopkins, Esq.', Report of the British Association 1853, pp. xii–lvi.
61 Darwin to J. D. Hooker, 28 February 1866, DAR 94: 31–2. Actually, only one of this group, Thomson, would have been considered a physicist. William Hopkins, while well known as a mathematician, was principally regarded as a geologist, as was the Rev. Samuel Haughton.
62 For a discussion of Haughton's views, see Burchfield, Kelvin, op. cit. (13), 100–3.
thermodynamics. Consequently, after the uniformitarian–catastrophist arguments of the 1830s–50s, and the development of thermodynamics in the 1850s, the 1860s were primed for a clash between major new scientific concepts. Rival theories faced an arduous struggle for existence in the scientific world and in the world of public opinion.

If we can paraphrase Carlyle’s observation that the printing press was the only pulpit, then we might say that the age-of-the-earth sermons thundering from the pen of William Thomson, relying on technical language and appearing largely in scientific journals, had little prospect of influencing a lay audience. Jenkin’s essay, in contrast, would bring the physical arguments against Darwinism directly to a public that had been largely ignorant of them, posing a vigorous new threat to the authority that natural selection was just beginning to wield in the congregation of national opinion. The thrust of Jenkin’s article would be to show the public that there were strong mathematical and scientific, as compared with religious, grounds on which to oppose natural selection. For his readers, his review would remove the Origin controversy from the domain of religion, and arm them to debate in the domain of science.

Arming their readers to debate was an important feature of the quarterly reviews. As scholars, we tend to look back on the evolution controversy as a clash between Darwinians and anti-Darwinians, neglecting to recognize the presence of an important third party: the ‘articulate classes’ then arising in every walk of British society. Education was a new consumer-good for a growing middle class. Displaying one’s knowledge of current issues was a mark of status and cultivation, as well as a means of advancing in this socially competitive world. With Britain’s inadequate formal schooling system, however, the ‘educated and would-be-educated’ depended on books and the serious periodicals for the knowledge they craved. Contributors to these periodicals were ‘entrusted with nothing less than the “momentous task of forming national opinion”’. Despite Darwin’s occasional claims of indifference to public support, the Origin was written in a style clearly designed to appeal to, and persuade, that audience. Both Darwinians and their critics, by publishing their evolutionary debate in the periodical press, aggressively sought to tip the scales of public opinion in their favour.

Given what we know of him, Jenkin probably relished the controversy. Certainly it does not strain credibility to imagine that in his 1860s milieu of anti-Darwinian electrical-standards men, their casual conversation turned occasionally to Darwin and natural selection.

JENKIN’S REVIEW: ‘THE ORIGIN OF SPECIES’

The existing evidence suggests that Jenkin first drafted his essay in 1862 or before, not in 1867 as has been generally assumed. In the early 1860s, his interest in evolution had been aroused by a number of events. Through his cable-laying voyages beginning in the 1850s,

64 Houghton, op. cit. (63), 7.
65 Houghton, op. cit. (63), 4.
67 Lyell’s Principles was similarly aimed at a lay audience. See M. Rudwick, op. cit. (45), pp. xi–xii.
Jenkin had become intrigued with the tiny and mysterious creatures he found clinging to cables retrieved from the sea floor.\textsuperscript{68} Then in 1861, he had become involved with Thomson's BAAS papers. Moreover, on the BAAS electrical-standards committee, he was surrounded by a circle of anti-Darwinian physicists. Thus it seems not unlikely that in this environment Jenkin attempted his own critique, aimed at Darwin specifically, rather than at uniformitarianism.

Evidence does exist for this conclusion, such as the clear statement from Stevenson that 'in the year 1863...[Jenkin and his wife] moved into a cottage at Claygate near Esher...He had begun by this time to write. His paper on Darwin...had indeed been written before this in London lodgings.'\textsuperscript{69}

Further corroboration for an early dating of the essay comes from its text, for it draws on ideas developed by William Thomson prior to 1863, but not at all on arguments and calculations made by Thomson in 1863–67. Moreover, it is certain that Jenkin reviewed either the first or the second edition of the \textit{Origin}, notwithstanding the apparent assumptions to the contrary by historians. In the fourteenth paragraph of Jenkin's essay, he quotes Darwin, giving the location of the quoted sentence as 'p. 153' of the \textit{Origin}, a location it enjoyed in the first two editions only. This perhaps justifies, or at least explains, Jenkin's discussion of Darwin's Wealden calculation, which appeared only in the \textit{Origin}'s first two (November 1859 and December 1859) printings.\textsuperscript{70}

It seems unlikely, however, that Jenkin showed his draft to any of his scientific colleagues, for surely such keen mathematicians as Thomson or, especially, Maxwell would have noticed a significant error in the mathematics of Jenkin's 'swamping' argument. More probable is that Jenkin discussed the draft with his father-in-law, Alfred Austin, whom Jenkin regarded as 'the cleverest man in London', and this probably accounts for the essay's reading like a legal brief.\textsuperscript{71} Surely the theatrics of the courtroom would have appealed to Jenkin's tastes.

Lawyers distinguish between cases based on facts and those based in law. In his essay, Jenkin quickly establishes that his case against natural selection will be based in law. He first summarizes Darwin's theory, then notes that it bases 'large conclusions' on 'small facts'. Only such specialists as Darwin can judge the facts, 'but the super-structure based

\textsuperscript{68} Anne Jenkin to Robert Louis Stevenson, 28 December [1885?], Beinecke Collection, no. 4978.

\textsuperscript{69} Stevenson, \textit{Memoir}, PLS, op. cit. (1), i, pp. lxvii–lxviii. In writing the \textit{Memoir}, Jenkin's widow provided Stevenson with access to 'two huge portmanteaux of family letters & papers', Jenkin's notebooks, and other materials (see Anne Jenkin to Stevenson, n.d. and 2 September [1886?], Beinecke Collection, nos. 4977 and 4979). Moreover, while Anne Jenkin did not actually collaborate with Stevenson in composing the \textit{Memoir}, as was originally contemplated (see Henry James to Stevenson, 8 December [1885], Beinecke Collection, no. 4926), she did review each chapter as it was completed, writing extensive comments and corrections in the margins, and in places directing Stevenson to consult specific documents. She also reviewed the final proofs before they were printed. Thus it seems unlikely that an error could have been made in something as important to Jenkin, and as easily verified, as the date when he first wrote his Darwin essay.

\textsuperscript{70} For example, Burchfield: 'Jenkin had before him the fourth edition of the \textit{Origin}, from which all reference to the Weald had been removed.' op. cit. (5), 307. Vorzimmer: 'Jenkin's review...was based essentially on the fourth edition which had appeared the previous December', in 'Blending' op. cit. (5), 386; and 'It was the appearance of the fourth \textit{Origin}...that served as the object of Fleeming Jenkin's review in the NBR the following spring. The delay between the publication of the Origin and the appearance of the review was a sign of the pains taken by the author to gather all his critical evidence', \textit{Controversy} op. cit. (5), 148.

\textsuperscript{71} Jenkin to David Douglas, 10 February 1868, letter no. 112, Douglas Scrapbook, NLS.
on those facts enters the region of pure reason, and may be discussed apart from all doubt as to the fundamental facts.\footnote{72}

This strategy gave Jenkin two advantages: (1) he could concede the facts and yet yield nothing to his opponent and, (2) he could appeal to scientific law, in which the case for physics – the science of Newton – would appear far stronger than that for geology or for evolution. By invoking this strategy, Jenkin implicitly contrasted his approach with that of Darwin, who built his theory upon the accretion of just those facts that Jenkin dismissed. In thus basing his case on (physical) law, Jenkin seized the philosophical high-ground, where physics reigned, and he undoubtedly hoped no one would notice that the actual ‘law’ on which he relied (now enshrined as the Second Law of Thermodynamics) was, at the time, little better grounded than Darwin’s.

He then raised five questions, in the same order in which he summarized the theory: (1) ‘Can natural selection choose special qualities, and so breed special varieties as man does?’ – meaning, is natural selection analogous to artificial selection? (2) Is the power to magnify the peculiarities [distinguishing] breeds from their original stock’ unlimited? – can it, in other words, create new species? Next, having already alluded to the assurances of geologists that ‘the habitable world has existed for countless ages’, a lawyer-like Jenkin asked, (3) Is there no other evidence than that of geology as to the age of the habitable earth? – and what is the value of the geological evidence?’ He then inquired, (4) whether ‘mere difficulty in classifying organized beings’ mandates our expecting ‘that they have had a common ancestor?’ Finally, he asked, (5) what value are we to attach to ‘minor facts supposed to corroborate the new theory?’\footnote{73}

These questions were not new with Jenkin. The first and last, in particular, had been treated by numerous critics, but Jenkin must have felt he had something new to add. Notably, he asserts that thus far Darwin’s critics ‘have been chiefly men having special knowledge similar to [Darwin’s] own’, thus ignoring the many religious arguments that had been levelled at natural selection, and laying the ground for a critique based entirely in science. Jenkin’s approach thereby reflects ‘the detachment and independence’\footnote{74} of his own religious views and, in this sense, the North British Review was an ideal location for the article, being a journal intent on asserting its independence from religious influence.

Having introduced his themes, Jenkin immediately mixed up his first two questions. His confusion might have arisen, in part, from his using this essay as an initial foray into a subject – population dynamics – that he would explore in detail in two later North British Review articles.\footnote{75} Jenkin consequently began his review by answering his second question – whether the range of variability is infinite – arguing for an asymptotic limit to the extent of any variability.

In opening with a question on limits, Jenkin was asking of natural selection essentially the same question as that which Sadi Carnot had asked of heat engines. In ‘Reflexions on
the motive power of fire’, a paper with which Jenkin was undoubtedly familiar, considering its importance for the thermodynamics of William Thomson, Carnot had inquired, 'can we set a limit to the improvement of the heat engine, a limit which, by the very nature of things, cannot in any way be surpassed? Or, conversely, is it possible for the process of improvement to go on indefinitely?" Clearly, Jenkin believed that, at least with respect to natural selection, it could not:

Hundreds of skilful men are yearly breeding thousands of racers. Wealth and honour await the man who can breed one horse to run one part in five thousand faster than his fellows. As a matter of experience, have our racers improved in speed by one part in a thousand during the last twenty generations?... We are thus led to believe that whatever new point in the variable beast, bird, or flower, be chosen as desirable by a fancier, this point can be rapidly approached at first, but that the rate of approach quickly diminishes, tending to a limit never to be attained.77

He then introduced his metaphor of the sphere of variation, an argument, essentially, for the stability of species. This was the point used so extensively by Mivart78 and which, post-Vorzimmer, has received the greatest attention from historians:

A given animal or plant appears to be contained, as it were, within a sphere of variation; one individual lies near one portion of the surface, another individual, of the same species, near another part of the surface; the average animal at the centre. Any individual may produce descendants varying in any direction, but is more likely to produce descendants varying towards the centre of the sphere, and the variations in that direction will be greater in amount than the variations towards the surface.79

Turning to his first question, on the creation of new species, Jenkin then made the ‘swamping’ argument that came to wield so much influence. As others did, he criticized Darwin’s theory on the grounds that it accounted for the adaptation, but not the creation, of characteristics: ‘such a process of improvement as is described [by natural selection] could certainly never give organs of sight, smell, or hearing to organisms which had never possessed them’.80

Continuing with the argument, Jenkin noted a distinction between what he called ‘common variations’ (Darwin’s individual differences) and saltations, or sports. He developed an ingenious mathematical argument, deducing that, with respect to sports, the mathematical probabilities were more than fifty to one against the survival and reproduction of any given salutation. Yet Jenkin had carelessly made an error. Where he had

77 Jenkin, NBR, op. cit. (1), 281–82. We do not know whether Jenkin, at this point, may also have had in mind the economic notion of diminishing returns. His economics writings are remarkably free of biological and evolutionary metaphors.
78 Mivart, op. cit. (14); see especially ch. 5.
79 Jenkin, NBR, op. cit. (1), 282. For all the attention it has received, no one has pointed out an apparent fallacy in this argument: Jenkin’s ‘limit in a given direction’ applies to change in one trait only, not to the whole organism, whereas evolution implies changes in a number of traits, each of which, varying in a given direction, could actually have a limit. Using Jenkin’s notion of the sphere of variation, it would seem to be specific traits that lie within the spheres, not whole beings, as he imagined.
80 Jenkin, NBR, op. cit. (1), 288.
written, ‘[of one million born]...the chances are fifty to one against the gifted individuals being one of the hundred survivors’, he had meant to write ‘one of the ten-thousand survivors’, a slip embarrassingly made public in letters to Nature in 1871.81 That modern scholars seem not to have noticed the error suggests they have not read this section of Jenkin’s essay as carefully as they might have. Still, readers then and now may have overlooked the slip because, in appealing to a general audience, Jenkin immediately converted the abstruse mathematics into a striking melodrama of a hypothetical ‘white man...wrecked on an island inhabited by negroes’.82

It seems possible that Jenkin was inspired to this frequently-quoted fable by the actual shipwreck of the P&O steamer Alma in the Red Sea in 1859, an incident involving several of Jenkin’s cable-laying colleagues, and in which his employer, R. S. Newall, played the genuine role of hero.83 Alternatively, the scenario may have come from Jenkin’s mother who, by the early 1860s, was herself a successful author of Gothic romances, and who had been raised in the West Indies. In Jenkin’s story, his ‘shipwrecked hero’ gains every advantage conceivable in the struggle for life. He becomes king, kills many blacks (negative selection), has many children (positive selection), but Jenkin asserts that countless generations ‘would not suffice…to turn his descendants white’84 because the hero’s traits would be increasingly blended in his descendants. Jenkin asks whether the population would gradually acquire the ‘energy, courage, ingenuity, patience, self-control, endurance’ of the hero, ‘those qualities, in fact, which the struggle for existence would select, if it could select anything?’85

In his ambiguous style of writing, Darwin had sometimes implied that small saltations would actually be reproduced throughout a population if that population was small and isolated, but Jenkin, by having his island scenario stand for just such an isolated population, effectively discredited that notion. Having thus dispensed with sports, Jenkin then turned to evolution via individual differences. Again arguing from mathematical probabilities, he showed that even a common variation would be diluted into oblivion, unless it appeared simultaneously in at least half the members of a population. Evoking his earlier shipwreck story, he concluded with the oft-quoted,

If it is impossible that any sport or accidental variation in a single individual, however favourable to life, should be preserved and transmitted by natural selection, still less can slight and imperceptible variations, occurring in single individuals, be garnered up and transmitted to continually increasing numbers; for if a very highly favoured white cannot blanch a nation of

81: ‘Studens’, ‘The genesis of species’, Nature, 2 March 1871, 347; The Author of the Article [F. Jenkin], ‘The origin of species’, ibid., 30 November 1871; A. S. Davis, ‘The North British Review and the origin of species’, ibid., 28 December 1871, 161. The corrections in Jenkin’s anonymous reply were unfortunately not appended to the essay when it was republished in 1887, leaving readers to puzzle over the calculations, as Darwin perhaps had done.
82 Jenkin, NBR, op. cit. (1), 289–90.
83 Werner Siemens, making the passage with Newall, includes a fascinating account of the shipwreck in his Inventor and Entrepreneur: Recollections of Werner von Siemens, 2nd English edn, London and Munich, 1966, 138–44. A long article also appeared in the Times: ‘The loss of the Alma’, The Times, 7 July 1859, 5. I thank Bruce Hunt for bringing this incident to my attention.
84 Jenkin, NBR, op. cit. (1), 289–90.
85 Jenkin, NBR, op. cit. (1), 290.
negroes, it will hardly be contended that a comparatively very dull mulatto has a good chance of producing a tawny tribe.  

At this point historians, believing they have now read Jenkin’s important criticism, often seem to quit the essay. However, Jenkin’s careless order in answering his first two questions, and his failure to detect his mathematical error, suggest he did not labour overlong on these two sections, and this would imply, in turn, that in his mind they were not the heart of his argument. Indeed, at this point Jenkin had just brought his locomotive up to speed. Scholars therefore abandon the essay at just the point where a discussion of Darwin’s most formidable obstacle begins, for in the ensuing section, titled ‘Lapse of time’, Jenkin investigated whether the earth was sufficiently old to have allowed natural selection to work, predictably seeking his answers not from geology, but from physics.

As noted above, the papers produced by William Thomson between 1861 and 1866 made passing references to Darwin but did not focus their arguments on refuting Darwin’s notions. When Jenkin’s article appeared, no one had yet explicitly linked thermodynamics and Thomson’s arguments on the ages of the sun and the earth to a specific criticism of natural selection.  

The essence of the Jenkin–Thomson complaint was that uniformitarian geology contravened not only the laws of thermodynamics, but even those of common sense, by positing the earth as a perpetual-motion machine. To physicists and engineers, it was an abuse of science

to suppose, as Lyell… has done, that the substances [inside the earth], combining together, may be again separated electrolytically by thermo-electric currents, due to the heat generated by their combination, and thus the chemical action and its heat continued in an endless cycle.  

Jenkin sought to demonstrate the wrong-headedness of this notion, and to replace it with a proper understanding of the conservation and the dissipation of energy.

We have experimentally proved… that the total quantity of energy in the universe is constant… At first sight, this constancy, in virtue of which no energy is ever lost, but simply transferred from mass to mass, might seem to favour the notion of a possible eternity of change, in which the earlier and later states of the universe would differ in no essential feature… To professor Sir W. Thomson of Glasgow we owe the demonstration of the fallacy of this conception, and the establishment of the contrary doctrine of a continual dissipation of energy, by which the available power to produce change… diminishes at every change of the distribution of energy.

The invocation of Thomson’s name in this argument was, of course, correct, but it served also as a clever strategy. When Jenkin’s article appeared, Thomson’s public fame was at a new height: in recognition of his achievements regarding science generally and the transatlantic telegraph cable in particular, Thomson had been knighted just eight months earlier. To have ‘Professor Sir W. Thomson of Glasgow’ as an ally strengthened the authority of an anonymous author seeking to persuade a general audience. Moreover, for

86 Jenkin, NBR, op. cit. (1), 291. Modern readers, finding Jenkin’s story racist, might consider it in the context of language common in his era. The Times story on the Alma, for example, in describing how some 200 native crewmen and 200 European passengers and crew got safely off the sinking ship, boasted that ‘the Anglo-Saxon race again proved that pre-eminence in courage and determination which have won for them a moral superiority over the whole world’. The Times, op. cit. (83).
87 Smith and Wise, op. cit. (13), 580.
88 Thomson, op. cit. (57), 3.
89 Jenkin, NBR, op. cit. (1), 297–98.
the uniformitarians reading his essay, Jenkin’s sympathetic tone (‘at first sight this constancy ... might seem to favour’ a uniformitarian interpretation), introduced an air of moderation in striking contrast to Thomson’s recent scorn.

Jenkin then embarked with his readers on an extended discussion of friction, energy conservation, and entropy, ‘providing one of the most lucid explanations of the energy doctrines given at the time’. 90 Revealing the teacher he would one day become, he carefully explained physical science for the untrained reader, patiently returning to each point time and again, with example upon example, in a spiralling argument illustrating the consequences of thermodynamics on the geological premisses of natural selection. In particular, he showed that thermodynamics applied as much to Nature as to man-made objects.

Nature no more works without friction than we can, and friction entails a loss of available power in all cases. When the rain falls, it feels the friction as much as drops from Hero’s fountain; when the tide rolls round the world it rubs upon the sea-floor... when the breeze plays among the leaves, friction occurs... No finite construction of physical materials can continue to do work for an infinite time. 91

Moreover, what applied to the earth applied equally to the solar system. Jenkin explained to his readers that while energy as a whole was conserved, its ability to do work was not. The transfer or redistribution of energy from one body (such as the sun) to another (such as the earth) necessarily meant a decay or loss in the energy available to do work.

When we find the sun raising huge masses of water daily from the sea to the skies, lifting yearly endless vegetation from the earth... performing the great bulk of the endless labour of this world and of other worlds, so that the energy of the sun is continually being given away; then we may say this continual work cannot go on for ever. 92

Jenkin appealed to the common sense of his readers, to their intuitive grasp of the world they experienced daily. The sun, it could be seen, had a finite life, determined by its supply of material and the rate at which it ‘gave away’ its energy. There were no other known or anticipated sources of energy for the sun. Jenkin acknowledged that the calculations were indeed ‘imperfect’, but no matter how generous one made one’s assumptions,

we are assured that the sun will be too cold for our or Darwin’s purposes before many million of years – a long time, but far enough from countless ages; quite similarly past countless ages are inconceivable, inasmuch as the heat required by the sun to have allowed him to cool from time immemorial, would be such as to [have turned] him into mere vapour. 93

How long might one expect the sun to last? Repeating Thomson’s most generous calculation, made in 1861, Jenkin answered that, ‘from the earth we have no very safe calculation of past time, but the sun gives five hundred million years as the time separating us from a condition inconsistent with life’. 94 This comment that the earth ‘gives no safe calculation’ differs notably from the views Thomson had developed by 1867.

90 Smith and Wise, op. cit. (13), 536.
92 Jenkin, NBR, op. cit. (1), 296.
93 Jenkin, NBR, op. cit. (1), 300.
94 Jenkin, NBR, op. cit. (1), 305. Jenkin had also stated the 500 million figure on p. 301, properly crediting it to Thomson. Ruse, op. cit. (13), 223, errs when he states that Jenkin’s essay gives the earth, ‘a span of 20 to 400 million years, with 98 million as the most probable figure (Jenkin, 1867)’. These figures are in Thomson’s ‘Secular cooling’ paper of 1863 (see note 57); they do not appear in Jenkin’s article.
Jenkin, of course, agreed with the theory that the earth had once been hot and was in a process of long-term ‘secular’ cooling, but neither primitive heat nor the nature of the earth’s crust and core were essential to his essay. Instead, his argument on the lapse of time was drawn almost entirely from Thomson’s work on the age of the sun’s heat, the paper which Jenkin regretted had ‘had no justice done to it’ at the 1861 BAAS meeting.

Returning to his lawyer-like strategy, Jenkin added, ‘this reasoning concerning the sun’s heat does not depend on any one special fact, or sets of facts, about heat, but is...the very essential condition of action’.95 In other words, it was not a hypothetical conjecture but a conclusion deduced from physical principles, from laws. Then, attending again to the mistaken beliefs of those such as Lyell, Jenkin added,

There is a kind of vague idea...that somehow chemistry or electricity, &c., may reverse all [the cooling of the earth]; but it has been explained that every one of these agencies is subject to the same law; they can never twice produce the same change in its entirety. Every change is a decay.96

Finally, in a passage particularly noticed by some Darwinians, Jenkin explained to his readers what this implied for any self-contained system (such as the solar system or, more specifically, the earth):

[When there has] been a gradual and continual dissipation of energy, there will on the whole have been a gradual decrease in the violence or rapidity of all physical changes... There are periods of greater and less activity, but the activity on the whole diminishes. Even so must it have been, and so will it be, with our earth... As the sun’s heat diminishes, so will the violence of storms; as inequalities of surface diminish, so will the variation of climate.97

This gradual running down and cooling of the earth meant that there was a finite period of time in which the earth had been, and would continue to be, hospitable to life.

Jenkin had expressed the thermodynamicists’ thesis with unsurpassed clarity, bringing it alive for his readers in a way that Thomson never accomplished. Where Thomson was arrogant, Jenkin was humorous.98 While Thomson drew his arguments from Fourier analysis with differential equations, Jenkin drew from rainfall and ball games. The lyricism of Jenkin’s writing stands in marked contrast to the wooden prose of Thomson. Jenkin’s review is distinguished by its appeal to the educated but technically untrained public – the rapidly expanding audience for the Victorian reviews.

Since the 1830s geology had enjoyed an unrivalled popularity with the public. ‘The general middle-class public purchases five copies of an expensive work on geology for one of the most popular novels of the time’,99 and most often that work on geology was Lyell’s Principles. Focusing on geological time, therefore, Jenkin’s essay was assured of a keen

95 Jenkin, NBR, op. cit. (1), 300.
96 Jenkin, NBR, op. cit. (1), 300.
97 Jenkin, NBR, op. cit. (1), 302.
98 An example of Thomson’s arrogance is displayed in his 1862 paper ‘On the rigidity of the earth’ where, after referring to G. B. Airy as a ‘naturalist’ Thomson sneered in a footnote, ‘“Naturalist: A person well versed in Natural Philosophy.”’ – Johnson’s Dictionary. Armed with this authority, chemists, electricians, astronomers, and mathematicians may surely claim to be admitted along with merely descriptive investigators of nature to the honourable and convenient title of Naturalist.’ Philosophical Transactions of the Royal Society of London (1863), 153, 573–82, on 577.
99 H. Martineau, History of the Peace, Boston, 1866, iii, 185.
Evolution demonstrates Darwin not to his Jenkin’s the geology of continents, religious expected seems because, selection and soundly attacked. His writing accumulated; this has happened. ’02 Jenkin, NBR, op. cit. (1), 313.

In his fourth section, Jenkin addressed his question about the extent to which the difficulty of classifying species justified a theory of a common ancestor. Evolution by continual modification implies a constant blurring of the boundaries defining species and varieties. Jenkin attempted to show that phenomena with blurry boundaries are a common feature of both the natural and man-made worlds, and imply nothing about the antecedents of the phenomena. His specific argument is not especially convincing, and was soundly attacked by Wallace a few months later, but Jenkin’s approach – challenging the philosophical validity of an aspect of Darwin’s theory – prepared the ground for Jenkin’s final assault on natural selection.

In his fifth and summary section, Jenkin echoed the complaint made by so many others, and which underlay the whole of his essay.

The chief arguments...rest on conjecture. Beasts may have varied; variations may have accumulated; they may have become permanent... We are asked to believe all these maybe’s happening on an enormous scale, in order that we may believe the final Darwinian ‘maybe,’ as to the origin of species... There is little direct evidence that any of these maybe’s actually have been.102

His scepticism shows in the ‘we are asked to believe’. Rather than opposing natural selection because it was irreligious, Jenkin’s reasoning in the essay shows that he opposed Darwin’s theory at least in part because he found it too much like religion. One was expected to receive it as one did religious dogma – by faith. Indeed, if Jenkin shared the religious misgivings of his friend Thomson and others who opposed natural selection because, for example, its randomness allowed no place for God, it is nowhere evident in his essay.103 Throughout the review, he pejoratively invoked religious language, referring to Darwin’s supporters as ‘true believers’ and ‘the faithful’. In one telling passage he complained that ‘the believer...can marshal hosts of...imaginary foes; he can call up continents, floods, and peculiar atmospheres; he can dry up oceans’104 – what was this if not making Darwin an analogue of Moses?

This passage was marked by Darwin with the words ‘good sners’, showing that Darwin himself recognized and appreciated Jenkin’s humour, a facet of the review often seemingly lost on earnest historians of science. The ubiquitous humour of the essay demonstrates its appeal to a general audience and the extent to which Jenkin was conscious of writing for that audience.

100 The point is made, for example, in D. L. Hull, ‘Certainty and circularity in evolutionary taxonomy’, *Evolution* (1967), 21, 174–89, on 175.
101 Wallace, op. cit. (23), 295.
104 Jenkin, NBR, op. cit. (1), 293.
THE DARWINIAN REACTION

Jenkin’s anonymous review generated immediate interest in Darwin’s circle. Joseph Dalton Hooker lamented that he had ‘been reading Tate’s? [sic] Review in N. British, & wish I was not so confounded lazy and I would answer it’.105 Other Darwinians sniffed that the critique was ‘pretentious’ and it was ‘a pity the man who wrote it had not studied a little zoology and botany before writing about them’.106

Darwin appears to have taken the review seriously from the start. Most noteworthy is that almost immediately upon reading it he wrote to Lyell, saying he would be in London the following week and wanted to discuss the article with him.107 Next, Darwin answered a letter from Charles Kingsley, replying that the piece ‘seems to me one of the most telling Reviews of the hostile kind’.108

In a letter to Darwin on 6 June 1867, Kingsley had called to Darwin’s notice a particular part of the essay,

I advise you to look at a wonderful article in the North British about you...Remark the argument...that geological changes were more violent, & the physical energies of the earth more intense in old times...if that be true – then changes of circumstances in plants and animals must have been more rapid.109

Clearly, Kingsley’s interest in the article was in its attack on uniformitarism and on geological time, and he recognized that Jenkin’s criticisms contained not only a statement of the problem, but also the mechanism of its solution. In his reply on 10 June, Darwin planted the review in this identical soil of geological time:

With respect to the antiquity of the world & the uniformity of its changes, I cannot implicitly believe the mathematicians, seeing what widely different results Haughton/Hopkins & Thompson [sic] have arrived at. By the way I had a note from Lyell this mg, who does not seem to value this article enough. Is there not great doubt on the bearing of the attraction of gravity with respect to the conservation of energy? The glacial period may make one doubt whether the temperature of the universe is so simple a question.110

Darwin’s protest about not believing the ‘mathematicians’ shows what had, over the years, become his almost reflexive response to the criticisms of physicists and astronomers.111 As we have seen, Darwin had disdained Thomson’s repeated attacks on geological time, and deleted his erroneous Weald argument, for example, because of complaints from geologists, showing no concern over the criticisms of physicists.

In this letter to Kingsley, however, we can see Darwin grappling, for the first time, with the physical arguments. Clearly he does not accept Jenkin’s views. Indeed, to accept them would have meant surrendering his gradualist theory. Even adopting the view urged by

105 J. D. Hooker to Darwin, 18 June 1867, DAR 102: 167–8. ‘Tate’ refers to Peter Guthrie Tait, who was known to contribute to the North British Review.
106 Wallace to Darwin, 25 September 1867, DAR 106/7; Charles Kingsley to Darwin, 6 June 1867, DAR 169.
107 Darwin to Lyell, 9 June 1867, American Philosophical Society (APS), 329.
108 Darwin to Charles Kingsley, 10 June 1867, APS 330.
109 Kingsley to Darwin, 6 June 1867, DAR 169.
110 Darwin to Kingsley, op. cit. (108).
111 Darwin undoubtedly refers not to ‘mathematicians’ per se, but to practitioners of the school of ‘mathematical geology’, of which William Hopkins had been a founder. See Smith, op. cit. (45).
Kingsley, that changes had been more rapid in the past and that evolution might thus have proceeded more quickly, meant retreating from uniformitarian geology, and that was a step Darwin resolutely refused, adamantly replying to Kingsley, ‘the crust of the earth was at this recent period as now, & the force of Nature not more energetic’. But Darwin’s bluster in this letter, and the compass of his protests, attest to a new anxiety.

In these two immediate reactions to the article, writing to Lyell and to Kingsley, it is plain that Darwin’s attention was seized neither by the nature of variations, nor by the mechanism of inheritance. He focused on Jenkin’s argument on geological time. We should note Darwin’s comment to Kingsley that Lyell does not value the article highly enough. In what context would Lyell have valued the article at all, if not in terms of the attacks Jenkin had launched against uniformitarian geology?

Darwin’s letter to Kingsley is equally revealing, however, for the light it sheds on the lingering issue of Jenkin’s influence on the subject of variations, for Darwin remarks that Jenkin’s review had helped him realize he must change some of his wording in the *Origin*. He writes that one portion of the review,

which could make me modify wording of a few passages in the *Origin* is I think about sudden sports, & these I have always thought, but now more clearly see, would generally be lost by crossing. R[eviever]. does not however notice, that any variation would be more likely to recur in crossed offspring still exposed to same conditions, as those which first caused the parent to vary…I have moreover exactly stated that I do not believe in the sudden deviation of structure, under nature such as occurs under domestication, but I weakened the insistance [sic] in deference to Harvey.113

Certainly, the phrase ‘but now more clearly see’ is susceptible to the traditional interpretation that only from reading Jenkin did Darwin come to realize that saltations would be swamped by large numbers of unvaried organisms. But the passage taken as a whole argues against this interpretation. Darwin states plainly enough that ‘I have always thought’ that saltations would be lost by swamping, while his last sentence implies that he recognizes his error in weakening his insistence against sports, because the weakening has resulted in readers, such as Jenkin, misunderstanding his views.

In other words, Darwin seems to have seen, from Jenkin’s having misunderstood him, that readers inferred that Darwinian evolution might proceed by saltations. His exasperation with the misunderstanding is vividly conveyed by his indignant ‘I have moreover exactly stated’, and his real view is clearly put forward: ‘I do not believe in the sudden deviation of structure.’ Overall, the passage supports the conclusion, originally stated by Vorzimmer, that Darwin did not form his opinions on variations and inheritance as a consequence of reading Jenkin, but did so long before.

What is it, then, that Jenkin helped Darwin ‘more clearly see’? We can only conjecture, but it may be that Darwin recognized in Jenkin’s mathematical example an explanation that would be effective in communicating to readers the very point Darwin had always

---

112 This quote is from a draft (DAR 96: 9, 32) of Darwin’s letter of 10 June. One sheet (of four) of the finished letter (op. cit. (108)) is lost, but the extant draft is complete. I thank the Darwin Project, Cambridge University, for providing a transcript of the draft. Since we do not know that the completed letter was identical to the draft, it is probably more appropriate to take the draft as revealing Darwin’s immediate reaction to Jenkin’s essay.
113 Darwin, op. cit. (112).
held. In other words, Jenkin and Darwin agreed on the problem with sports, and in arguing against them Jenkin created an argument that Darwin could borrow (which, indeed, he did) to make his own view more clear. Realizing now that he must strengthen his ‘insistence’ against saltations, Darwin goes on in his letter to Kingsley to give an example of how he would amend his wording in the *Origin*.

When speaking of the formation for instance of a new species of Bird with long beak, Instead of saying, as I have up to now incautiously done, a bird suddenly appeared with a beak being longer than that of his fellows, I would now say that of all the birds annually born, some will have a beak a shade longer, & some a shade shorter, & that under conditions or habits of life favouring a longer beak, all the individuals, with beaks a little longer would be more apt to survive than those with beaks shorter than average.¹¹⁴

Wording that had heretofore seemed to refer to saltations – ‘a bird suddenly appeared’ – would henceforth be changed to make plain that Darwin was all the time referring to individual differences. It seems clear, then, that Jenkin’s influence in the matter of variations was limited to changing the style in which Darwin expressed his ideas, not in changing the ideas themselves.

For the next few months, Darwin’s extant correspondence shows few signs of anxiety over the age of the earth. In a letter to Lyell, however, Darwin suddenly thanked Lyell ‘for your long and interesting letter ... You give me some consolation but I take the Sun much to heart.’¹¹¹ The tone and wording of Darwin’s letter suggest that the problem had weighed on his mind for awhile. ‘The Sun’ was, of course, Thomson’s argument from 1861, which Darwin had never, in the intervening years, either explicitly acknowledged or even apparently recognized as an obstacle for his theory. Yet now, we see that a major transformation had occurred in Darwin’s views.

Historians have inferred this letter of Darwin’s to have been written in early March 1868, and to have been provoked by William Thomson’s paper, ‘On Geologic Time’, read on 27 February 1868 to the Geological Society of Glasgow. It seems unlikely, however, that there was sufficient time after the delivery of Thomson’s address for Darwin to have written such a letter.¹¹²

More telling, however, is that the purpose of Thomson’s Glasgow paper was to present new arguments regarding the influence of the tides on geological time. The cooling of the earth occupied a decidedly secondary portion of the paper, and the sun’s heat an even more minor third portion. Furthermore, in these sections on the earth and on the sun, Thomson

¹¹¹ Darwin, Draft letter, op. cit. (112).
¹¹³ By its return address, scholars in the Darwin Project know that the letter was written during one of three time periods: September 1867, 1–10 March 1868, or September 1868. The March date has been privileged on the assumption that the letter was provoked by Thomson’s paper, read on the evening of 27 February 1868. This assumption seems unlikely, in that it requires that Darwin, in London, acquire an actual copy of Thomson’s paper from Glasgow, digest the contents of that lengthy, technical paper, confess his worry to Lyell, and then that Lyell develop and write a ‘long letter’ addressing Thomson’s arguments, all in the few days allowed by the March dates. Moreover, one page of Lyell’s letter was not mailed with the rest, making it incomprehensible to Darwin until he received the missing sheet. The time required for all this would seem to render improbable that Darwin’s letter, if written in March, was caused by Thomson’s address. September 1868 is unlikely for other reasons not germane to this paper.
added nothing new to the arguments he had advanced in 1861, arguments Darwin had heretofore ignored. It is difficult to understand, therefore, why or how this paper alone would suddenly have caused Darwin to reverse himself and take the sun, especially — not the tides and not the earth — ‘much to heart’.

However, if we look again at Jenkin’s paper, we find a lapse-of-time argument based entirely on the dissipation of the sun’s heat, and which might therefore be considered a more credible cause of Darwin’s newly evinced concern. If so, then it is possible that the letter to Lyell was written as early as September 1867.

Lyell was not the only one with whom Darwin discussed his growing concern about the shortness of time. At the Norwich meeting of the British Association in August 1868, Joseph Dalton Hooker, as President, used part of his Presidential Address to review Darwin’s contributions to science. In preparing his speech, he engaged Darwin’s assistance, both in correspondence and in a personal visit shortly before Hooker went to Norwich.117 It seems safe to assume, therefore, that Hooker’s address was consistent with Darwin’s own thinking. It is significant, in this regard, that Hooker directed fully half of his remarks about Darwin’s work to defending the Origin against the criticisms regarding the age of the earth, specifically as raised by Jenkin. The astronomers’ objections to Darwin’s theories, Hooker began, ‘are strenuously urged in what is in my opinion the cleverest critique [of Darwin’s work] that I have hitherto met with’. He then went on to describe the shortness of time as the ‘most formidable argument urged by the reviewer’.118 We might see Hooker’s address, therefore, as a succinct Darwinian assessment of Jenkin’s essay. Hooker’s analysis concurs with Darwin’s later statements attesting to the essay’s value, and identifies Jenkin’s crucial argument as that regarding the age of the earth, not the arguments on variations and inheritance.

Searching for a solution to the dilemma of geological time Darwin turned, in 1868, to his son, George, a newly-minted Cambridge wrangler, and to James Croll, for assistance.119 From George, he sought help in comprehending Thomson’s work. From Croll, he hoped to gain not only comprehension, but a counter-argument.

It is curious that Darwin should have turned to Croll. Self-educated and anti-uniformitarian, Croll considered himself closer to physics than to geology. Moreover, Croll’s own estimates of the age of the earth were even more restrictive than Thomson’s. In an earlier day, Darwin would perhaps have lumped him with ‘Hopkins, Hennessey, Haughton, and Thomson’ as one of those mathematicians who should be ignored. That Darwin now looked to him for help suggests the extent of the change in Darwin’s perception of his predicament. Croll helped Darwin come to terms with the validity of the physical arguments against natural selection, and persuaded him to reject the false physical concepts to which he clung in hopes that they might mitigate the Thomsonian arguments

117 See Hooker to Darwin, 22 June, 12, 25 and 29 July and 6 August 1868; Darwin to Hooker, 6 June, 14, 17 and 28 July and 17 August. The 17 August letter notes Hooker’s visit to Down. Hooker’s letters are in DAR 102, Darwin’s in DAR 94.


119 See, for example, Charles Darwin to George Darwin, 9 December 1868, DAR 210.1.1; George Darwin to Charles Darwin, 6 February 1869 and 14 February 1869, DAR 210.2, which discuss Darwin’s concerns regarding the age of the earth.
so clearly advanced by Jenkin. In February of 1869, for example, Croll confirmed the certitude of Jenkin’s argument on dissipation,

It is a pity that Sir Charles [Lyell] should have made those remarks on the secular loss of heat... If there is one thing more than any other in physics, regarding which we have absolute certainty, it is that the solar system is losing its store of energy. We not only know this fact, but we have a means of determining the actual rate at which it is losing its power.\textsuperscript{120}

Croll supported Thomson’s estimate of 100 million years for the age of the earth; he had himself arrived at the figure of 60 million years. But, after reading Darwin’s admission that he was ‘greatly troubled at the short duration of the world’, Croll responded encouragingly that, ‘it is quite possible that we may yet be able to get considerably more than one hundred million of years, although very much beyond this we are brought to a limit by other considerations.’\textsuperscript{121} He also assured Darwin that ‘I think that you may quite fairly assume a very long period before the Cambrian formation, even according to Sir William Thomson’s theory.’\textsuperscript{122}

Darwin entertained the hope that electromagnetic phenomena might affect the energy content and, consequently, the age, of the earth, and Croll addressed this as well:

One thing is certain, that it is but an infinitesimal quantity of the forces of nature that ever assumes the electric or magnetic form... The quantity of energy in the form of electricity coming from the sun (if there be any at all) is certainly trifling compared with what comes in the form of heat.\textsuperscript{123}

While Darwin incorporated some of Croll’s ideas into the \textit{Origin}, he could do little to respond to arguments that were, essentially, unanswerable,\textsuperscript{124} except by shortening the time required for natural selection to work.

Over the succeeding months, Darwin came to believe that ‘negative selection’, the increased destruction of non-adapted creatures, would not only help overcome the problem of swamping but, more important, would accelerate the evolutionary process. Negative selection consequently became more prominent in the later editions of the \textit{Origin}. We see this change in Darwin’s thinking most clearly if we look back at Darwin’s explanation to Kingsley, in June 1867, of how he would modify his wording in describing variation and selection. Darwin had then written,

I would now say that of all the birds annually born, some will have a beak a shade longer, & some a shade shorter, & that under condition or habits of life favouring a longer beak, all the individuals, with beaks a little longer would be more apt to survive than those with beaks shorter than average.

Eighteen months later, however, for the fifth (1869) edition of the \textit{Origin}, Darwin had altered his example, from an emphasis on increased survival to one on increased destruction:

If... a bird of some kind could procure its food more easily by having its beak curved, and if one were born with its beak strongly curved, and which consequently flourished, [from this would

\textsuperscript{120} Croll to Darwin, 4 February 1869, DAR 161.
\textsuperscript{122} Croll to Darwin, 4 February 1869, DAR 161.
\textsuperscript{123} Croll to Darwin, 4 February, DAR 161.
\textsuperscript{124} See Burchfield, op. cit. (5), for an exposition of Darwin’s response.
follow] the preservation of a large number of individuals with...curved beaks, and...the
destruction of a still larger number with the straightest beaks.125

The element of increased destruction of the original creatures appeared to solve the
problem of swamping. More necessary for Darwin, however, was that it shortened the time
required for a variant organism to become successful. And by the time Darwin introduced
this change to the Origin, shortening the time had become of paramount importance. This
one idea, therefore, simultaneously helped solve two of the problems raised by Jenkin:
swamping by large numbers, and the short age of the earth. Many scholars have noted this,
but seem to have missed its full value in terms of time.

In general, the modifications that Darwin introduced to his theory – the increased
reliance on sexual selection, on the inheritance of acquired characteristics, and on other
mechanisms of evolution – which have for so long intrigued scholars, have been interpreted
by them largely as solutions to problems of swamping and blending. Scholars have
occasionally noted that these mechanisms would also accelerate the rate of evolutionary
change, without explicitly recognizing that speeding up the process was Darwin’s most
urgent need. Inheritance was no longer his crucial problem. Vorzimmer shows us that
Darwin had, to his own satisfaction, solved the inheritance problem. If we look, therefore,
through Darwin’s eyes, rather than through a lens crafted by Mendelism, the crucial issue
facing Darwin when he sat down to revise the Origin was the shortness of time. As he
wrote to Wallace in December 1869, ‘if you throw light on the want of geological time,
may honour, eternal glory and blessings crowd thick on your head’.126

When it came to grappling with the age of the earth, however, Darwin himself could
only introduce to the Origin (1869) the rueful admission that,

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...Mr. Croll estimates that about 60
million years have elapsed since the Cambrian period, but this...seems a very short time for the
many and great mutations of life.127

Darwin then took refuge in a restatement of the argument he had read in Thomson and
in Jenkin, the argument Hooker had also noted in his BAAS Address, and that Kingsley had
called to Darwin’s attention immediately after reading Jenkin’s review – the argument that
comprised the main thermodynamical objection to uniformitarianism. In the end, Darwin
made a concession, small, but a concession none the less:

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.128

James Marchant), New York and London reprint edn, 1975, 204. Darwin was replying to a letter from Wallace
in which Wallace had announced, ‘I have written a paper on Geological Time, which will appear in Nature, and
I think I have hit upon a solution of your greatest difficulties in that matter’. Wallace to Darwin, 4 December 1869,
ibid., 204.

127 Darwin, op. cit. (11), 513. Note Darwin’s expression ‘great mutations of life’ when he appears to mean
‘individual differences’ or its alternative, ‘insensible variations’, as an example of how his wording could confuse
readers.
128 Darwin, op. cit. (11), 513.
Darwin's attribution of this point to Thomson is, of course, correct, but also noteworthy. While the arguments on the shortness of time were, indeed, created by Thomson, it appears to have been Jenkin who articulated them with sufficient grace and clarity to convince Darwin of their importance. When Thomson advanced these arguments, Darwin paid no heed. Only after he read Jenkin's review – immediately after reading it – had Darwin become concerned about the problem of 'lapse-of-time'.

CONCLUSION

Historians have followed customary practice in attributing to Sir William Thomson the arguments on the shortness of geological time. This custom, however, has masked the role of Fleeming Jenkin in translating Thomson's arguments such that Darwin finally came to realize that those arguments imperilled his theory of natural selection. Jenkin's advocating them in a journal aimed at the general public further imperilled the serious influence that Darwinian theory was just beginning to command in public opinion.

The ultimate reasons for this sequence of events may lie as much in the audiences for science as in the science itself. In contrast to Thomson's formal and highly technical arguments aimed largely at other scientists, Jenkin's writings aimed at basic, thorough instruction of the rapidly growing, literate public, a public especially eager to understand and make informed judgements on the latest developments in science and technology. Both Jenkin's review and Darwin's *Origin of Species* were directed at that same audience.

Reared in four countries, travelling around the globe in cable-laying expeditions, and frequenting the drawing-rooms of novelists and politicians, Jenkin had grown adept at mediating between cultures. Writing for the readers of the Victorian reviews was for him an opportunity to dramatize issues of compelling importance to contemporary readers, and to translate between the technical and non-technical worlds, irritating and educating the inhabitants of both. His essays brought new and singular arguments to bear on previously well-discussed topics. His disputatiousness would not let him rest when an argument could be had. His innovative and theatrical imagery impressed his ideas vividly on the minds of his readers. All his essays in the *North British Review* share these essential qualities of translation, provocation, and melodrama. In the *Origin* review, his most well known, he transformed the alien language of mathematics and the new science of thermodynamics into a vernacular comprehensible to an intelligent but non-mathematical audience, one member of which turned out to be Charles Darwin.

129 F. Jenkin, 'Submarine telegraphy' NBR (December 1866); 'The origin of species' (June 1867); 'Fecundity, fertility, and sterility' (December 1867); 'Trade-unions: how far legitimate' and 'The atomic theory of Lucretius' (both in March 1868).