In September 1887 the Duke of Argyll published an article entitled 'A great lesson' denouncing Darwin's coral reef theory and alleging that it continued to be supported in the face of new evidence contradicting it because of the authority of Darwin's name and the status of his disciples in British science. His attack touched off a robust correspondence in The Nineteenth Century which soon spread to Nature, and in which T. H. Huxley was the chief defender both of Darwin's views and of scientific procedures. Partly this controversy revolved around the interpretation of coral reefs themselves, but partly also it sprang from the resentment of scientists at the Duke's charge that they were swayed in their judgements by other than objective scientific criteria and modes of argument. The Duke was, in effect, anticipating later interpretations of science as a social activity, but as a result of the exaggerated and polemical nature of his contributions this insight was lost sight of as the scientists defended both their methods and Darwin's views. On the substantive question of the origin of coral reefs the Duke and his supporters were later shown to be largely wrong and Darwin largely right, and the incident did nothing to advance the understanding of the reefs. Had this discussion not eclipsed the Duke's point about the nature of reasoning and judgement in science, he might have made a more lasting contribution to the understanding of science itself.

In 1842, Lyell greeted the new theory with 'wild excitement and sustained enthusiasm' (Judd, 1909, p. 358), and immediately abandoned his own theory, that atoll reefs grew on the rims of submerged volcanic craters: his reaction gave Darwin 'immense pleasure, or rather delight' (Darwin to Lyell, 20 September 1859: F. Darwin, ed., 1887, vol. 2, p. 166). Darwin recalled in 1876 that the theory 'was thought highly of by scientific men' and had become 'well established' (Barlow, ed., 1958, p. 98), and he derived 'high satisfaction' from 'solving the problem of coral-islands' (Barlow, ed., 1958, p. 80). Soon after its announcement the theory received field support from the work of Jukes on the Great Barrier Reef (Stoddart, 1888), and from that of Dana on the United States Exploring Expedition. It was indeed cited as one of the reasons for the award to Darwin of the Royal Society's highest honour, the Copley Medal, in 1864, and for the honorary LL.D. awarded him by Cambridge University in 1877. By the time that Darwin issued a revised second edition of The Structure and Distribution of Coral Reefs in 1874 he could well assume that his views had been generally accepted and that the problem he addressed had effectively been solved.

'A GREAT LESSON'

The impact of the Duke of Argyll's article, entitled 'A Great Lesson', was thus all the greater, particularly as it was written in highly polemical style. The Duke first gives an extended and apparently
 sympathetic account of the theory, and showed how it was more satisfactory than earlier explanations had been. In somewhat extravagant language he described how it 'took the scientific world by storm. ... The theory of the young naturalist was hailed with acclamation. It was a momentous generalisation. It was soon almost universally accepted with admiration and delight. It passed into all the popular treatises, and ever since for the space of nearly half a century it has maintained its unquestioned place as one of the great triumphs of reasoning and research. ... I have heard eminent men declare that, if he had done nothing else, his solution of the great problem of the coral islands of the Pacific would have sufficed to place him on the unapproachable peaks of science, crowned with an immortal name' (Argyll, 1887c, p. 300-301).

And the Great Lesson? 'It is that Darwin's theory is a dream. It is not only unsound, but it is in many respects directly the reverse of the truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in this matter fell into errors as profound as the abysses of the Pacific. All the acclamations with which it was received were as the shouts of an ignorant mob' (Argyll, 1887c, p. 301).

But the Duke went further. He speculated how this state of affairs had come about. 'It is well known that the plebiscites of science may be as dangerous and as hollow as those of politics. The overthrow of Darwin's speculation is only beginning to be known. It has been whispered for some time. The chare has itself been in danger of slowly going out of sight. Can it be possible that Darwin was wrong? Must we indeed give up all that we have been accepting and teaching for more than a generation? Reluctantly, almost sulphurically, and with a grudging silence as far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about. ... Darwin's theory of the coral islands must be relegated to the category of those many hypotheses which may have to be indeed helped science for a time by promoting and provoking further investigation, which in themselves have now finally kicked the beam' (Argyll, 1887c, p. 301).

And Darwin went wrong because of his adherence to 'the old doctrine of science, long ago formulated by Hutton, that the work of erosion and of denudation must be equal to the work of deposition' (Argyll, 1887c, p. 304).

Argyll's new hero was Mr. John Murray, of the Challenger Expedition, 'a man whose enthusiasm for science, sagacity and candour of mind, are not inferior to those of Darwin.' The Duke referred to a paper by Murray given to the Royal Society of Edinburgh (Murray, 1880) 'supported with such a weight of facts and such a close texture of reasoning that no serious reply has ever been attempted' (Argyll, 1887c, p. 305). There was, alas, according to the Duke, but 'slow and sulky acquiescence, rather than ... that joy which every true votary of science ought to feel in the discovery of a new truth and — not less — in the exposure of a long-accepted error' (Argyll, 1887c, p. 305). He alleged that, by contrast, Darwin himself shortly before his death had been impressed with Murray's evidence, and had confessed that 'serious doubts had been awakened as to the truth of his famous theory' (Argyll, 1887c, p. 305).

And he ended by noting the 'curious power which is sometimes exercised on behalf of certain accepted opinions, or of some reputed Prophet, in establishing a sort of Reign of Terror in their own behalf.' Evidence of this he found in the fact that apparently Murray himself had been persuaded to delay publication of his own findings on account of it. And as his coup-de-grace, he threw in the Bathynius incident, in which Huxley had been centrally implicated, as evidence of the existence of both 'a ridiculous error and a ridiculous credulity' in science (Argyll, 1887c, p. 308; cf. Rupke, 1976).

This was provocative, powerful and persuasive writing in the high tradition of late Victorian polemics. In the coral reef question the Duke believed he had at last cornered Darwin, after long years of spinning over evolution theories of species and natural selection. As a convinced catastrophist he believed too that disposing of Darwin's coral reef theory would remove one of the leading examples of uniformitarian argument in geology. And it gave him a splendid opportunity to expose and reprimand his scientific opponents in high places for the conduct of their own research: if they would not listen to him on religion and agnosticism and miracles, then they could only hear the truth about their own professional lives. Almost at once the Duke's attack became a cause célèbre among scientific men, and the destruction of his views a first priority. Before examining this response, we need first to establish the context in which it was written. Why did the Duke come to write it? What was the real force of his criticisms? Why did so obviously polemical a piece so anger scientific men?

THE DUKE OF ARGYLL

George John Douglas Campbell, 8th Duke of Argyll, was then in his middle sixties. Holder of the title since 1847, he owned 170,000 acres in the Highlands, which he controlled from the family seat at Inveraray Castle. Until 1881, when he broke with Gladstone over the Irish question and resigned from the Government, he had been a prominent Liberal politician. Through his political life he had served in the cabinets of Aberdeen, Palmerston, Russell and Gladstone. Usually he held the office of Lord Privy Seal, on occasion that of Postmaster General, but from 1869 to 1874 he was Secretary of State for India. His resignation in 1881, and his subsequent personal estrangement from Gladstone, marked the end of his political career.

He also found time to play a highly visible role in science and education — Chancellor of the University of St. Andrews in 1851, Rector of Glasgow University in 1854, elected a Fellow of the Royal Society at the age of 28 in 1851, President of the British Association in 1855, President of the Royal Society of Edinburgh in 1861. He lived
through and commented volubly upon the great upheavals in science resulting from The Origin of Species, and maintained a vigorous debate in the Nineteenth Century, the Fortnightly Review and the Edinburgh Review on questions of science and religion up to the time of his death in 1900.

Yet he remains a shadowy figure, almost wholly ignored in the modern historiography of science (though see Mason, 1978). Quintessentially Victorian, he nevertheless is difficult to place: a high patrician of great wealth living in an almost feudal Highland society, a politician of ability working with the most distinguished public figures of his day, apparently one of the finest orators of the time (Russell, 1916), and also with claim to be regarded as a geologist and a natural historian. As far back as 1835 marine shells indicative of arctic conditions had been found in clays on his estate near Helensburgh (Smith, 1838), and later he discovered Tertiary leaf beds beneath basaltis on the Isle of Mull (Argyll, 1851). He entered the great geological debates of his time as a vigorous propagandist — for catastrophism against uniformitarianism (e.g. Argyll, 1863), against fluvial glacialism (e.g. Argyll, 1871) and against the erosive power of glaciers and the glacial theory generally (e.g. Argyll, 1894) (even though, ironically, it was at Inveraray that Agassiz and Buckland found the first field evidence of glaciation in Scotland: E. Agassiz, 1885, vol. 1, p. 307). And he was of course one of the foremost lay opponents of evolution and Darwin's mechanism for it. No wonder that Leonard Huxley described him (1918, vol. 1, p. 359) as 'a polemical upholder of ideas left stranded by the progress of science.'

In everything he did he acted with aristocratic confidence. George Russell (1916, p. 74) recalled that 'he never seemed to realize that his associates were in any sense his equals. As a professor, he harangued and expounded and laid down the law. As a chieftain, he summoned one guest to his side and then in turn dismissed him to make way for another. He spoke as the Elder of the Kirk when he rebuked episcopacy; as the President of the Geological Society when he reproved Evolution; and as the hoary Whig when he preached the sanctity of property to the socialistic and inexperienced Gladstone who was fourteen years his junior.' His paper on the physical geography of Argyllshire (Argyll, 1868) was a sustained attack on Archibald Geikie's Scenery of Scotland (1865). He not surprisingly attacked The Origin of Species at the Royal Society of Edinburgh in 1864, and he returned repeatedly to the evidence for natural selection. Darwin found much of this criticism 'very clever, but not very profound' (Darwin to Hooker, 8 February 1867: F. Darwin, ed., 1887, vol. 3, p. 61; also Darwin to Lyell, 1 June 1867: F. Darwin, ed., 1887, vol. 3, p. 65; Darwin to Lyell, 22 January 1865: F. Darwin, ed., 1891, vol. 3, p. 92). Hooker, who read the Duke's 'immeasurable pamphlet', The Reign of Law (1867) with 'utter disgust and uncontrollable indignation' because of its attacks on Darwin, observed nevertheless that 'he writes extremely well and expresses himself with admirable facility — in fact he has a fatal facility for handling things he does not fully understand, and which he has not the time, and probably not the power to grasp the principles of' (Hooker to Darwin, 20 May 1868: Huxley, 1918, vol. 2, p. 114).

CONTEXT

Thus by the time of the 'Great Lesson' the Duke's views on scientific questions were widely known, as were his methods of advancing them. He had similarly strong opinions on questions of religion, morality and ethics, and pressed them also in comparable manner, often illustrating his arguments with examples from geology and biology.

His coral reef attack in fact came in the middle of a vigorous debate initiated in the Fortnightly Review in November 1886, with an attack on T. H. Huxley and others by W. S. Lilly, under the title 'Materialism and Morality': its flavour can be gauged by the fact that Lilly managed to allege directly that Positivism 'reeks of the brothel, the latrine and the torture trough' (Lilly, 1886, p. 580). Huxley replied in December, in 'Science and Morals', refuting Lilly's position on grounds 'which the proprietors do not permit me to make so emphatic as I could desire' (Huxley, 1886, p. 789). In February 1887 Huxley (1887a) returned with an attack on the ideas of catastrophe and miracle in a sermon in St. Paul's by Canon Liddon, published in the Nineteenth Century under the title 'Scientific and pseudo-scientific realism.' This called forth a response from the Duke himself, magisterially rebuking Huxley, and in his final paragraph linking the controversy directly to 'the muddy torrent of bad physics and worse metaphysics which has been rushing past us under the name of Darwinism.' In language which could only goad Huxley, the Duke referred offensively to 'the fumes of worship and of incense raised before the fetish of a Phrase ['Natural Selection']' (Argyll, 1887a, p. 338), and in direct anticipation of the 'Great Lesson' he concluded that 'It was high time indeed that some revolt should be raised against the Reign of Terror which had come to be established in the scientific world under the abuse of a great name, not because Huxley has not joined this revolt openly for a long time but indeed it is only beginning to raise its head. ... The time is coming when that revolt will be carried further.' Huxley responded in 'Science and pseudo-science' in April, alleging that the Duke 'expresses himself in such a manner that it is obvious he is unacquainted with even the rudiments of that knowledge which is necessary to the discussion into which he has rushed' (Huxley, 1887b, p. 494). As for the 'Reign of Terror', Huxley was entirely dismissive: 'Can it be ... that a quillbot is to be defined in the court-yard of Burlington House for the benefit of all anti-Darwinian Fellows of the Royal Society? ... nothing short of midsummer madness can account for the fiction that I am waiting till it is safe to join openly a revolt, hatched by some person or persons unknown, against an intellectual movement with which I am in the most entire and hearty sympathy' (Huxley,
The Duke's immediate response, in 'Science falsely so called' (Argyll, 1887b), was at least as much the culmination of this preceding debate as the initiation of a new and more scientifically technical one.

CONSEQUENCE

The continuity of the debate is clearly signalled by the fact that Huxley's response to the Duke's coral reef attack was appended to an article in the November issue of the Nineteenth Century, entitled 'Science and the Bishops' (Huxley, 1887c). This was primarily a dissection of three sermons given during the British Association meeting in Manchester by the Bishops of Bedford, Manchester and Carlisle. Huxley gave the Duke the benefit of the doubt that his paper was 'doubtless well-meant,' and he then listed seven areas of error in it.

First, that 'no serious reply has ever been attempted' to Murray's solution theory of 1880: what of the extended discussion by Dana in the American Journal of Science for 1885, which finally rejected it in favour of subsidence? Second, that Darwin knew of the new theory before he died, 'was at least ready to entertain it, and to confess that serious doubts had been awakened as to the truth of his famous theory.' Huxley noted that Darwin had maintained his theory against Semper's criticisms in 1879 (Darwin to Semper, 2 October 1879: F. Darwin, ed., 1887, vol. 3, p. 182), and he published a letter written to him by Judd, then President of the Geological Society, who described a conversation with Darwin shortly before the latter's death, in which Darwin maintained his theory against Murray's (Judd to Huxley, 10 October 1887: Huxley, 1887c, p. 640-641). Third, Darwin's theory (according to the Duke) is a dream, the reverse of the truth. 'Really?' is Huxley's comment. Fourth, the acclamation for it no more than the shouts of an ignorant mob. No doubt the mob includes Professor Dana? Fifth, the overthrow of Darwin's theory is only beginning to be known. Huxley, in his Six-foot 'slow and sulky acquiescence' to Murray's views. On the contrary, says Huxley, 'they cannot be said to have met with general acquiescence of any sort, whether quick and cheerful, or slow and sulky'; nor can they be said to have been ignored, given their advocacy by no less a person than the Director of the Geological Survey (Geikie, 1883). Finally: Murray was dissuaded from publication. I doubt it, says Huxley, but if he had been it would have been 'sagacious and kind.' 'I cannot pretend, he concludes, 'that we are seriously disturbed by charges which every one who is acquainted with the truth of the matter knows to be ridiculous' (Huxley, 1887c, p. 637).

Huxley's response was followed by a long article in Nature for 10 November by the immediate past President of the Geological Society, Professor Bonney. Bonney, who was responsible for the third edition of Darwin's Structure and Distribution of Coral Reefs eighteen months later, not surprisingly found Darwin's theory 'capable of a more general application, and presents less serious general difficulties, than does that of Mr. Murray' (Bonney, 1887, p. 26). But on the 'peculiarly offensive' nature of the 'grave charges against the honour and good faith of men of science,' he called on the Duke either to establish or to withdraw the accusations (Bonney, 1887, p. 26). Bonney averred that he knew rank-and-file scientific men better than the Duke, 'owing to his exalted station and his high occupations of State,' and he felt 'bound to declare,' somewhat sanctimoniously, that 'he had never found men as a class less self-seeking or more earnest in their desire for truth.'

Both Huxley's and Bonney's statements were, given the nature of the Duke's article, both courteous and restrained, but undoubtedly the matter caused considerable general aggravation in the scientific camp. Hooker, writing to Asa Gray (15 November 1887: L. Huxley 1918, vol. 2, p. 342), called the Duke's paper 'a very stupid one,' and thought Bonney's 'wonderfully good'; '... what struck me most was, the Duke's not seeing that Darwin's theory was, whether right or wrong, a stroke of genius, unaided by that knowledge we now possess of land, sea, sea-bottom, chemistry and corals; whereas Murray's is a conclusion arrived at through the labours of a staff of most eminent fellow workers on the Ocean, aided with a mass of facts and data that they were collecting around him during the Challenger voyage, - considerations which were of course irrelevant to the points the Duke was making.

The debate which ensued in Nature was conducted on various levels. Mellard Reade (1887) produced a letter from Darwin dated 22 September 1880 in which Murray's views were termed 'far-fetched.' Bonney repeated that if Sir Wyville Thomson had indeed discouraged the reading of Murray's coral reef paper in February 1877 (in a letter which the Duke had seen) and which according to him showed that Thomson 'dreaded some injury to the scientific reputation of the body of naturalists of whom he was the chief': Argyll, 1887d, p. 53), then the reason probably had to do with Murray's own inexperience at the time and the need to exercise proper control over Challenger results. Sir Wyville had unfortunately died five years earlier, just before Darwin, and thus could not speak for himself, but one of his students pointed out that, with his well-known anti-Darwinian views, it was illogical to suppose that he would have attempted to suppress Murray's paper simply because it controverted Darwin's theory. An anonymous correspondent reported Sir Wyville's opinion that Murray's 'theory had not as yet been sufficiently investigated or sufficiently corroborated, and that therefore any immature, dogmatic publication of it would do less than little service either to science or to the author of the paper' (Anon., 1887, p. 200). In any case the allegation that Murray had been silenced was disposed of not only by his presentation to the Royal Society of Edinburgh in 1880, but more particularly when he was invited to lecture to the Royal Institution, on 16 March 1888. The lecture was published in full in the Proceedings, and Nature carried a full
abstract of it (Murray 1888c, 1889).

The Duke stood his ground, refusing to withdraw or to modify his charges. He added the accusation that a paper on the reefs of the Solomon Islands that Guppy offered on Murray's advice to the Geological Society in 1885, when Bonney was President, had been refused. The reason the Duke gave for this was that, like Murray's, Guppy's views could be considered unorthodox, even heretical, from the Darwinian point of view. Bonney promptly denied the allegation. The current President, Judd (soon himself to introduce a new edition of Darwin's three geological works, including *Coral Reefs*), denied that any paper had been offered (Judd, 1888b) whereupon Guppy (1888b) explained that it was his offer to offer a paper that had been declined: with Murray's help it had subsequently been published by the Royal Society of Edinburgh (Guppy, 1886). Guppy was even more annoyed that none of his work had been mentioned by Bonney or Judd throughout the whole controversy. As a result he resigned from the Geological Society, commenting, 'I have never been on the winning side' (Guppy, 1888b, p. 343): only the discreet intervention of Geikie caused him to withdraw his resignation (Guppy to Geikie, 10 January 1888: Edinburgh University Library, Geikie Papers, Gen 524).

A second strand in the controversy was over the alleged competency of solution to form lagoons in the manner envisaged by Murray. Many found a simple logical difficulty, beginning with Darwin himself, who found it 'astonishing that there should be rapid dissolution of carbonate of lime at great depths and near the surface, but not at intermediate depths where he places his mountain peaks [built up by pelagic deposition]' (Darwin to Reade, 22 September 1880, in Reade, 1887, p. 54). The Challenger office was active in experimental work to prove the power of solution by seawater on coral and other skeletons (Anderson, 1888-1889), and both Irvine (1888a) and Ross (1888) extrapolated from their experimental results to comment on implications for the form of surface reefs. Mellard Reade (1888b, p. 535) pointed out that through arithmetical error Ross had exaggerated the rate of solution in a hypothetical atoll lagoon by a factor of 126, with consequent major underestimation of the likely time scales involved in Murray's theory; and he again demanded to know why, if solution were so important in lagoon formation, it failed to act on the banks of pelagic carbonate deposits which Murray believed (and as Guppy had found in the case of uplifted reefs in the Solomons) were the foundations of coral reefs.

Third, there was the introduction of fresh field evidence. Guppy (1887a, 1887b) produced a further substantial memoir in the *Transactions* of the Royal Society of Edinburgh as well as a book on the geology of the Solomon Islands. In Bonney's account of Diego Garcia Atoll in the Chagos appeared in the *Proceedings* of the Royal Society in March 1888, with a substantial abstract in *Nature* in April (he dismissed both subsidence and solution) (Bourne, 1888b, 1888c). Wharton (1888) published a long account of the submerged banks of the South China Sea, and suggested that atolls could be formed without any lagoon-solution if corals simply grew up to the surface round their margins. Bourne (1888e) drew attention in *Nature* to Johannes Glaisher's monograph on the raised fringing reefs of Sinai, in which subsidence had clearly played no part.

Gradually, however, the fire died away in the controversy, especially when Huxley, Bonney and Judd ceased to be active participants. The Duke himself did not retract and the professors could not budge him. Bonney and Judd busied themselves with their new editions of Darwin's book. Murray, Irvine and Anderson carried on with their experiments on the solution and precipitation of calcium carbonate in seawater (in one of which they induced a bulky precipitate by adding 750 cc of urine to 3 litres of seawater at 60-80°F.; Murray and Irvine 1890, p. 86-87), but the theoretical significance of such data was perhaps not immediately apparent.

Huxley and the Duke continued to dispute in the *Nineteenth Century* on religion and science, most notably on agnosticism throughout 1889 and on the question of the Flood in 1890-1891. Even here, however, the Duke could not leave the coral reef controversy alone. In April 1891, in 'Professor Huxley on the warpath,' he once more provided a long summary of his principle arguments from 'A Great Lesson': that new and critical facts inimical to Darwin's theory 'attracted comparatively little attention among somnolent believers in a glorious dream.' 'I attributed this,' he continued, 'to the well-known slowness of even the scientific world to confess a great delusion, and to a special indisposition in England to admit that a fatal breach had been made in any doctrine so long accepted under the authority of Charles Darwin' (Argyll 1891a, p. 687). Did Huxley really hold that 'scientific men are never susceptible of prejudice — never swayed by preconceptions — always exempt from the impositions of authority'? The point was given greater force by the Duke's discovery that Huxley had made 'the most shameless use of the phrase 'conspiracy of silence', in connection with the opposition of some members of the Institut de France to Darwinism. Huxley declined to start the whole argument off again, though pressed to do so by the Duke (Argyll, 1891b). It was a rather bitter and ungenerous exchange, and showed the wounds the controversy had caused.

**CONCLUSION**

A century later, the whole incident has fallen out of sight, the observations and interpretations of Bourne, Guppy and Wharton are largely forgotten (Steers and Stoddart, 1977). Forty years afterwards W. M. Davis (1928) pronounced for Darwin and dismissed the opposition in *The Coral Reef Problem*, and since then deep drilling on open-ocean atolls has beautifully established the reality of the main tenets of Darwin's theory of subsidence.

If the participants in the debate added
little to our understanding of coral reefs and their origin, however, there is one component of the controversy which has an increasingly familiar ring. Paradoxically it had nothing to do with the facts at issue, and paradoxically too it was the charge made by the Duke which most angered scientific men. Stripped of its hyperbole, the Duke's argument was that science is indeed a social activity, that influence, seniority and authority have great power within it, and that long-accepted views can from time to time be abandoned and replaced by new, for social rather than scientific reasons. It was not the first time that the Duke had made these suggestions: he outlined them in explicit manner at the conclusion of his first presidential address to the Geological Society in 1873, when he stated that 'the history of geology, like the history of other sciences, is the history of the prevalence of particular theories at particular times — not generally to be wholly abandoned but almost always to be greatly modified' (Argyll, 1874, p. lxxviii). He was speaking then of glacial theories in particular, and his 'Great Lesson' simply extended the analysis to coral reefs. It is not difficult to read into the Duke's suggestions the outlines of a Kuhnian interpretation of scientific activity and change (Kuhn, 1962), and there would today be few histories of science which did not bear out the significance of patterns and procedures in the socialisation of science that the Duke, however crudely, was outlining.

It was the Duke's misfortune that, as always, he overplayed his hand, and that the subsequent controversy masked the real point he was trying to make. It is somewhat remarkable that, on his death in 1900, the only substantial obituary in a scientific journal was that by Archibald Geikie in Nature (it also formed the substance of Geikie's contribution to the Dictionary of National Biography entry, and was noticeably sympathetic, though Geikie's later recollections in his own autobiography were somewhat sharper: Geikie, 1900, 1904, pp. 187-190, 1924, pp. 225-228). Geikie of course had felt the Duke's wrath over fluvial denudation in the Highlands, though he leaned more towards him on coral reefs than he did to Darwin (Geikie 1883a, 1883b). Geikie wrote that the Duke's writings 'became more and more polemical as the years went on, and though always acute and forcible, often failed to grasp the true bearing of the facts, and to realize the weight of the evidence against the views which he had espoused' (Geikie, 1901, p. 390).

Had the Duke been able to argue his case with greater tact and subtlety, and had the professors not taken such immediate umbrage at the aspersions they believed he was casting on their methods and procedures, then something of significance might have been learned from the 'Great Lesson,' even though it had nothing to teach about coral reefs themselves.

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