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THE RISE OF THE DILUVIAL THEORY IN BRITISH GEOLOGICAL THOUGHT

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PREFACE

The purpose of this study of the rise of the diluvial theory in British geological thought in the early nineteenth century is to examine the scientific evidence presented on its behalf, the criticism it received, both scientific and religious, and the reasons for its eventual modification. The study is limited to British geologists during roughly the period 1813-1831, except for some discussion of the views of the German, Abraham Werner, and the Frenchman, Georges Cuvier, both of whom had a significant influence on British geological thought. Only the newer diluvial theory of Cuvier and William Buckland is treated here, for the diluvial theories of the eighteenth century, which attempted to explain most stratified rocks as the result of the Biblical flood, had been discredited among geologists by this time.

The specific theory of Cuvier and Buckland should be distinguished from the general catastrophist-diluvialist climate of the time. Most geologists accepted Buckland's contention that the evidence of valleys and the diluvium with its fossil contents suggested diluvial action. They did not, however, necessarily agree with him that this evidence was the result of a single, recent, universal, transient deluge, identical with the Biblical flood.

The impression one gets as he reads the geological writings of the early nineteenth century in Great Britain is that the published surveys of the history of British geology in this period have too simplified an interpretation. A common misconception is to label every geologist either a Wernerian or a Huttonian, a Neptunist or a Plutonist, a diluvialist or an antidiluvialist. What one finds, of course, is that most geologists were not blind disciples of one theory or the other but were quite eclectic in their attitude toward geological theories, many being sceptical of all of them.

This work was done in partial fulfillment of the requirement for the degree of doctor of philosophy at the University of Oklahoma. I wish to thank Professors Thomas M. Smith and Duane H. D. Roller for reading and commenting on portions of the manuscript and for their helpful advice and encouragement. Almost all of my research was done using the resources of the History of Science Collections of the University of Oklahoma Library. I am grateful to the curator, Professor Roller, for obtaining certain works and materials needed in this study and to the librarian, Mrs. Marcia Goodman, for her aid. The manuscript materials utilized in this work were consulted at the library of the American Philosophical Society, Philadelphia, Pennsylvania, whose manuscript librarian, Mr. Murphy Smith, was of great help to me. I consulted a number of works in other libraries in Philadelphia, including those of the University of Pennsylvania, the Library Company, and the Academy of Natural Sciences. Mrs. Esther R. Houghton, of the Wellesley Index to Victorian Periodicals,

1824-1900, Wellesley College Library, Wellesley, Massachusetts, was very helpful in verifying the authors of anonymous review articles of the period. During my work on this dissertation I have held a National Science Foundation Graduate Fellowship and have been employed as an Instructor in the History of Science by the University of Oklahoma. I wish also to thank those graduate students in the History of Science at the University of Oklahoma who have assisted me by commenting on my work. Lastly, I wish to thank my wife, Mary Ellen, for reading and correcting parts of the manuscript and for her encouragement and understanding during the course of this work.

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CHAPTER I

GEOLOGICAL THEORY IN GREAT BRITAIN PRIOR TO 1822

The first comprehensive description of the geology of England: Outlines of the Geology of England and Wales, by W. D. Conybeare and William Phillips, was published in 1822.[1] Conybeare,[2] its principle author, and his close friend William Buckland,[3] were probably the most renowned geologists in England at that time. They were among the most influential members of the Geological Society of London, the leading organization in Great Britain devoted to geology.[4]

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[1]W. D. Conybeare and William Phillips, Outlines of the Geology of England and Wales, with an Introductory Compendium of the General Principles of That Science, and Comparative Views of the Structure of Foreign Countries (London: William Phillips, 1822). This work described the English strata from the uppermost to the coal formation. A proposed second part, treating the formations below the coal, was never published.

[2]The Rev. William Daniel Conybeare (1787-1857), Christ Church, Oxford, B.A. 1808, was rector of Sully in Glamorganshire, 1823-36; vicar of Axminster, Devon, 1836-44; and dean of Llandaff, 1845-57.

[3]The Rev. William Buckland (1784-1856), Christ Church, Oxford, B.A. 1804, was reader in mineralogy, Oxford, 1814; reader in geology, 1819-56; and dean of Westminster, 1845-56.

[4]Horace B. Woodward, The History of the Geological Society of London (London: Geological Society, 1907) contains much information about the early history of the society.

Conybeare and Buckland were among the first in Great Britain to apply the new paleontological methods developed by Georges Cuvier, the illustrious founder of vertebrate paleontology, and one of the most celebrated scientists of his time.[5] Cuvier's geological ideas, developed during the course of his work on the geology and paleontology of the region around Paris,[6] were put forth in their most complete form in the preliminary discourse to his work on fossil bones.[7] This discourse, translated into English in 1813 under the title, Theory of the Earth,[8] with notes by Robert Jameson,[9] was the most popular geological work in Great Britain in the decade succeeding its publication.[10]

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[5]Georges Cuvier (1769-1832), professor of natural history at the College de France, professor of anatomy at the Jardin des Plantes, a Councillor of State, a baron, and a peer of France.

[6]Georges Cuvier and Alexandre Brongniart, Essai sur la géographie minéralogique des environs de Paris, avec une carte géognostique, et des coupes de terrain (Paris: Baudouin, Imprimeur de l'Institut Imperial de France, 1811).

[7]Georges Cuvier, Recherches sur les ossemens fossiles de quadrupèdes, ou l'on rétablit les caractères de plusieurs espèces d'animaux que les révolutions du globe paroissent avoir détruites (4 vols.; Paris: Deterville, 1812). The "discours preliminaire" is in Vol. I, pp. 1-116.

[8]Georges Cuvier, Essay on the Theory of the Earth, with Mineralogical Notes, and an Account of Cuvier's Geological Discoveries, by Professor Jameson, trans. Robert Kerr (Edinburgh: William Blackwood, 1813).

[9]Robert Jameson (1774-1854), mineralogist and geologist; regius professor of natural history and keeper of the museum at the University of Edinburgh, 1804-54.

[10]The work went through five editions in Edinburgh: 1813, 1815, 1817, 1822, and 1827, and one in New York: 1818. Another English translation was published in London in 1829.

Cuvier's editor, Jameson, was the leading disciple in Great Britain of Abraham Werner, who had developed the most widely accepted geological system of the time.[11] Werner had devised a scheme for classifying minerals by their external characteristics, which he applied to the classification of rocks, as a basis for investigating the structure of the earth. It is not surprising that Jameson edited Cuvier's book, for Cuvier made considerable use of Wernerian ideas, although he had not been a student of Werner's as Jameson had.

It had long been known to students of the subject that the earth's crust is stratified, and almost all believed that the layers or strata into which it was divided had been formed or deposited in water. This idea was supported by the presence in many of the strata of what appeared to be the remains of the shells of organisms similar to existing marine animals.[12] It seemed obvious to those who accepted this idea that each individual stratum must have been formed or solidified at a later time than had the stratum immediately beneath it. The time between successive formations need not have been very long, however, for it was possible to conceive of many strata being formed

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[11]Abraham Gottlob Werner (1749-1817), professor at the Mining Academy in Freiberg, Saxony, 1775-1817. Werner's ideas were spread mainly by his students, among whom were Alexander von Humboldt, Leopold von Buch, and Jean Francois d'Aubuisson de Voisins, three of the foremost geologists of Europe.

[12]Frank Dawson Adams, The Birth and Development of the Geological Sciences (New York: Dover Publications, Inc., 1954), contains a lengthy account of the history of early geological speculation.

almost simultaneously by some process such as crystallization from solution in water.

The basic division in the Wernerian classification of the strata was the formation, defined by Jameson as "a determinate assemblage of similar or dissimilar rock-masses, which are characterized by external and internal relations as an independent unit."[13] A common but more hypothetical definition was: "a series of rocks supposed to have been formed in the same manner and at the same period."[14] How did one recognize a formation? Sometimes it was by the similarity in appearance of its successive beds or members. A series of alternating strata, however, such as a number of thin sandstone layers interspersed with shale, could constitute a formation, as could a series of strata, each of which graded into the stratum succeeding it.[15] A formation was a series of strata that could be regarded as a unit because they were presumably formed under the same conditions at the same time.

Werner divided the strata in Saxony into a number of formations. As others had before him, he recognized a definite order in the succession: from granite and other crystalline formations, which were lowest in the sequence, up through formations, such as the sandstones, that

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[13]Robert Jameson, System of Mineralogy, Comprehending Oryctognosy, Geognosy, Mineralogical Chemistry, Mineralogical Geography, and Economical Mineralogy, III (Edinburgh: William Blackwood, 1808), 59.

[14][Thomas Thomson], Review of A Critical Examination of the First Principles of Geology; in a Series of Essays, by G. B. Greenough, Annals of Philosophy, XIV (1819), 458.

[15]Ibid.

appeared to be cemented sand and gravel, to the unconsolidated alluvial formations of sand, gravel, and clay at the top. Before Werner the lowest formations, such as granite, gneiss, and schist, had been called primary or primitive rocks. Above these were the secondary rocks, so-called because they were believed to have been formed after the creation of life and to have been derived ultimately from the primary rocks. For example, sandstones appeared to have been formed from fragments broken off from earlier rocks by the violent action of ocean waves. Werner, observing that there was no sharp boundary between the primary and the secondary rocks, created a new transition class, made up of those formations that appeared to be intermediate in character between the primary and the secondary rocks. This class, supposed to have been formed during the transtion of the earth from its chaotic to its habitable state, consisted of the oldest secondary rocks, which have a compact, crystalline texture and contain few or no organic remains.[16]

The Wernerian classification thus had three major divisions:

1. The primitive rocks, which lay under all others, were completely crystalline and therefore were believed to have been precipitated from a state of chemical solution.[17] These rocks had been

> deposited from the original chaotic fluid, in a certain determinate order. In them no detritus, or anything like organized nature, was to be observed; and . . . every rock remained exactly in the same state, in which it was at the period when it first acquired solidity.[18]

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[16]Jameson, pp. 97-98, 145.

[17]Ibid., p. 67.

[18]Thomas Allan, "Remarks on the Transition Rocks of Werner,"

Granite, the lowest rock of this class, was believed to be the oldest known rock.[19]

2. The transition rocks were above the primitive rocks; and the lowest member of the series, the limestone, contained fossils.[20] The class was principally crystalline, but included a formation (grey-wacke, a coarse sandstone) in which there were "fragments, which must have existed previously in a different state: hence . . . these rocks were formed at a subsequent period."[21]

3. The floetz rocks were so-called because they were flat-lying or horizontal, in contrast to the highly inclined transition strata. Therefore, they were "never found conformable with the transition rocks."[22] In these rocks, said Jameson,

> mechanical deposits occur in great quantity, and the proportion of chemical precipitates decreases. The principal rocks are Limestone and Sandstone: to these may be added, Gypsum, Salt and . . . Coal.[23]

To these Werner added two more classes of less important superficial rocks: the alluvial rocks, the product of local causes, which in most places cover the "regular" strata and are almost entirely composed of unconsolidated mechanical deposits, and the volcanic rocks,

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Transactions of the Royal Society of Edinburgh, VII (1815), 111. This paper was read February 17, 1812.

[19] Jameson, p. 68.

[20] Ibid., p. 146.

[21] Allan, p. 112.

[22] Ibid.

[23] Jameson, p. 68.

the least important of the classes, made up entirely of the products of volcanoes.[24]

Jameson said that Werner and his pupils had shown that the primitive and the transition formations and, contrary to previous opinion, most of the floetz formations were universal--that is, they were originally deposited, with some interruptions, universally around the earth. Their present extent might be much different from what it was originally.[25] Partial formations, which included all of the alluvial and volcanic formations, and a few of the floetz, were deposited "only here and there," in scattered patches.[26] The Wernerians believed that the universal formations had been laid down by the ocean, which was originally higher than the present mountain tops and had gradually declined to its present level. It had first precipitated granite in great thickness with a surface whose relief was greater than that of the surface of the present earth, although negligible with respect to the thickness of the granite. The other strata had been successively laid down on this surface, filling in the irregularities, the newer formations becoming progressively more horizontal, so that the floetz formations were essentially flat. Except for local subsidence, therefore, the present inclinations of the strata were due to the conditions of their deposition, principally the inclination of the surface upon which they were deposited.[27]

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[24] Ibid.

[25] Ibid., pp. 63-64, 153.

[26] Ibid., p. 63.

[27] Ibid., pp. 55-56, 69-70.

The evidence for the theory of the universal ocean was strong. The remains of marine animals were widespread in the transition and floetz strata at both high and low elevations. Jameson offered two generalized observations that, he said, substantiated "in a satisfactory manner the universal diminution of the water from the surface of the earth." These generalizations, which he attributed to Werner, were:

> 1st, That the outgoings of the newer strata are generally lower than the outgoings of the older, from granite downwards to the alluvial depositions, and this not in particular spots, but around the whole globe. 2nd, That the primitive part of the earth is entirely composed of chemical precipitations, and that mechanical depositions do not appear until a later period, that is, in the Transition class; and that from this point they continue increasing, through all the succeeding classes of rocks, to the newest or the alluvial, which are almost entirely mechanical deposits.[28]

Werner made an exception to his first generalization by creating a subclass of formations in the upper part of the floetz called "overlying formations." The most important of these was the newest floetz-trap formation, a widely distributed but very discontinuous formation, often found at high elevations. It included basalt; a black, crystalline rock that Werner insisted was chemically precipitated from solution in the universal ocean. Werner assumed that the ocean had risen at the

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[28] Ibid., pp. 78-79. The first generalization is, in other words, that the highest outcrop of each succeeding formation is always lower in elevation than that of the formation below it. This is often the case for a single mountain range, where granite forms the core, and the newer formations, dipping away from the granite crest, outcrop at successively lower elevations. Werner, however, maintained that it was true universally. Conybeare devoted several pages to refuting him on this point: see his Outlines . . ., pp. xviii-xxi. The second generalization supported the Wernerian contention that the universal formations, where found, occur always in the same determinate order.

time of the deposition of basalt and the other members of the newest floetz-trap formation because they were "unconformable and overlying": that is, they "differ not only in direction, but lie over the ends of the strata" upon which they were deposited. According to Jameson:

> It is evident from the nature and position of these rocks, that they have been formed by a vast deluge. The water appears to have risen rapidly; again to have become more calm; and, during the period of its settling, to have deposited the different rocks of this formation; and, lastly, to have retired to its former level with considerable rapidity.

To the rapid rising of the water, Jameson attributed "the heaps of trees, the beds of gravel, sand and clay, and their more frequent occurrence in low than high situations, their constant occurrence in the lower parts of the formation." To the rapid retiring of the water, he attributed the broken and discontinuous stratification of the formation.[29] This rising of the ocean, he said, "may be termed a Deluge, as it took place when the surface of the earth was covered with animals and vegetables, and consequently at a period when much dry land existed."[30]

Werner held also that there had been changes in the fossil organic world corresponding to the successive changes in the character of the formations deposited. In Jameson's words:

> It is evident that, during the period when the earth was still covered to a great height with water, neither plants nor animals had been created. When the water diminished in height, and the dry land began to appear, marine plants, and the lowest and most imperfect animals, were created. As the water diminished, it appears

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[29]Jameson, pp. 65, 83-85. See also Adams, pp. 218-25.

[30]Ibid., p. 349.

> to have become gradually more fitted for the support of animals and vegetables, as we find them increasing in number, variety and perfection, and approaching more to the nature of those in the present seas.
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> . . . . . .
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> In respect to the nature of these remains, we may remark, that those which occur in the earliest periods, belong to the lowest and most imperfect class of animals, the zoophytes. In the newer and newer formations, we meet with quantities of shells and fish, and these are accompanied by a variety of marine plants. But these organic remains are completely different from any of the animals or vegetables of the present state of the earth. The organic creation during that period appears to have had a totally different aspect from what it assumed in the succeeding. In the newer formations, we find the remains of known genera, and in the newest of all the remains of organic species, resembling those found in the present seas. Land plants appear later, and land animals still later.[31]

Its adherents believed that the Wernerian system was far superior to any previous geological theory. Jameson wrote that it was utterly different from the idle speculations of the past:

> That illustrious mineralogist, to whom we owe almost every thing that is truly valuable in this important branch of knowledge, after the most arduous and long-continued investigation, conducted with the most consummate address, discovered the general structure of the crust of the globe, and pointed out the true mode of examining and ascertaining those great relations, which it is one of the principal objects of Geognosy to investigate. . . . We should form a very false conception of the Wernerian Geognosy, were we to believe it to have any resemblance to those <u>monstrosities</u> known under the name of <u>Theories of the Earth</u>. Almost all the compositions of this kind are idle speculations, contrived in the closet, and having no kind of resemblance to any thing in nature. Armed with all the <u>facts</u> and inferences contained in these visionary fabrics, what account would we be able to give of the mineralogy of a country, if required of us, or of the general relations of the great masses of which the globe is composed? Place one of these speculators in such a situation, and you will immediately discover the nature of his information, and he himself will find that he knows nothing; that he has been wandering in the mazes of error; and that, however <u>easily</u> he may have been able to explain

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[31] <u>Ibid</u>., pp. 80-82.

> the formation of this globe, and of the whole universe, he cannot give a rational or satisfactory account of a single mountain.[32]

Geognosy, which was the term preferred by the Wernerians as the name of their science, was defined by Jameson in 1804 as the study of "the structure, relative position and mode of formation of the mineral masses of which the crust of the earth is composed." He added that "by geology, Werner understands idle and imaginary speculation respecting the formation of the earth."[33] Apparently he received some criticism for this slur on geology, for in 1808 he contended that it was geogony that meant "abstract speculations," whereas geology signified "the whole science; . . . and therefore Geognosy is only a branch of Geology."[34]

Cuvier also had great praise for Werner's contributions to Geology. Geology, he said,

> has taken its place among those departments of knowledge that are positive.
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> . . . . . .
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> Two celebrated men, Pallas and Saussure, had prepared the way for this happy reform,--a third has accomplished it,--I mean Werner. With him, the most remarkable epoch of the science of the Earth commences; and we may even say, that he alone has filled that epoch. For he has had the good fortune to see those ideas, which were so novel, and those views, which, before his time, were so unknown to naturalists, universally prevalent during his own

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[32]Ibid., pp. 41-42. See Adams, pp. 212-14, and Sir Archibald Geikie, The Founders of Geology (2d ed.; London: Macmillan and Co., Limited, 1905), pp. 207-09, for accounts of the devotion of Werner's students to him.

[33]Robert Jameson, System of Mineralogy, Comprehending Oryctognosie, Geognosie, Mineralogical Chemistry, Mineralogical Geography, and Oeconomical Mineralogy, I (Edinburgh: Archibald Constable and Co., 1804), xx.

[34]Jameson, System . . ., III, 343.

life.[35]

. . . . . .

If we except his opinions respecting volcanic countries, . . . all the rest of his ideas have only met with a temporary opposition.[36]

Werner's ideas may have been triumphant in France in 1818, when Cuvier gave his address, but they were far from being dominant in Great Britain. There, after the Wernerian theories had been publicized by Jameson in 1808, they came under increasingly severe attack. The Wernerian doctrines received greater opposition in Great Britain than in France for at least three reasons: the traditional Baconian-Newtonian scepticism of hypotheses, the insularity of the British and their traditional dislike of foreign things, and the less centralized nature of British science and scientific publications. These reasons, coupled with the extravagances present in the Wernerian doctrines and the irritating self-assurance of Werner and his followers, were enough to insure a thorough and critical examination in Great Britain of all aspects of the Wernerian geognosy.

A theory that was considered by many to be the opposite of Werner's, but which aroused as much or more opposition on both geological and religious grounds, had been proposed by James Hutton, a Scotsman, in 1785.[37] It was explained and supported by evidence in

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[35]Georges Cuvier, "Historical Eloge of Abraham Gottlob Werner, Read at a Sitting of the Royal Institute of France," Edinburgh Philosophical Journal, IV (1821), pp. 1-2.

[36]Ibid., p. 10.

[37]James Hutton, "Theory of the Earth; or an Investigation of the Laws Observable in the Composition, Dissolution, and Restoration

his Theory of the Earth, published in 1795.[38] The theory was popularized in 1802 by Hutton's friend, John Playfair, in his Illustrations of the Huttonian Theory, a work that won wide acclaim as a literary masterpiece.[39]

The earth, according to Hutton, is a self-winding, self-regulating machine, showing evidence of infinite wisdom in its design. It is the business of geology, he said, to examine this machine, to investigate its structure, and, more important, its cycle of operations:

> If we believe that there is almighty power, and supreme wisdom employed for sustaining that beautiful system of plants and animals which is so interesting to us, we must certainly conclude, that the earth, on which this system of living things depends, has been constructed on principles that are adequate to the end proposed, and procure it a perfection which it is our business to explore. Therefore, a proper system of the earth should lead us to see that wise construction, by which this earth is made to answer the purpose of its intention, and to preserve itself from every accident by which the design of this living world might be frustrated. For, as this world is an active scene, or a material machine moving in all its parts, we must see how this machine is

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of Land upon the Globe," Transactions of the Royal Society of Edinburgh, I (1788), Part II-1 (Papers of the Physical Class), 209-304. This paper was read March 7 and April 4, 1785. James Hutton (1726-97), of Edinburgh, was a versatile scientist, who produced works in geology, chemistry, physics, metaphysics, meteorology, and agriculture.

[38]James Hutton, Theory of the Earth, with Proofs and Illustrations. In Four Parts (2 vols.; Edinburgh: William Creech, 1795). A portion left in manuscript was found and published in 1899 as Theory of the Earth, with Proofs and Illustrations, in Four Parts, III, ed. Sir Archibald Geikie (London: Geological Society, Burlington House, 1899).

[39]John Playfair, Illustrations of the Huttonian Theory of the Earth (Edinburgh: William Creech, 1802). John Playfair (1748-1819), mathematician and geologist; joint-professor of mathematics at the University of Edinburgh, 1785-1805; professor of natural philosophy, 1805-19.

> so contrived, as either to have those parts to move without wearing and decay, or to have those parts, which are wasting and decaying, again repaired.[40]

This world-machine, being perfect, exhibits no traces of the manner in which it was created. The creation belonged to a different order of things, about which it is useless to speculate:

> In examining things which actually exist, and which have proceeded in a certain order, it is natural to look for that which had been first; man desires to know what had been the beginning of those things which now appear. But when, in forming a theory of the earth, a geologist shall indulge his fancy in framing, without evidence, that which had preceded the present order of things, he then either misleads himself, or writes a fable for the amusement of his reader. A theory of the earth, which has for its object truth, can have no retrospect to that which had preceded the present order of this world; for, this order alone is what we have to reason upon; and to reason without data is nothing but delusion. A theory, therefore, which is limited to the actual constitution of this earth, cannot be allowed to proceed one step beyond the present order of things.[41]

If we examine nature, Hutton said, we find evidence of "a succession of worlds." According to Playfair, the "fundamental proposition" of the Huttonian theory was

> That in all the strata we discover proofs of the materials having existed as elements of bodies, which must have been destroyed before the formation of those of which these materials now actually make a part.[42]

We thus infer, said Playfair, that the earth has had "many great revolutions." In one of these revolutions, or cycles of change, the

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[40] Hutton, Theory . . ., I, 275-76.

[41] Ibid., pp. 280-81. The Newtonian influence on Hutton is here apparent. Hutton seems to have believed that he could do for geology what Newton had done for astronomy. In not speculating about a beginning, Hutton was in agreement with most Wernerians, who refused to discuss the state of the earth before the universal ocean.

[42] Playfair, p. 5.

strata, composed of material eroded from the land, are consolidated on the floor of the ocean by subterranean heat, acting under pressure. The strata are then elevated by the expansive force of that heat in order to form new lands to replace the old, which are eventually eroded away.[43]

The ultimate purpose of this succession of cycles being to maintain and perpetuate life, Hutton said, nature does not operate in violation of that purpose: Therefore general deluges cannot have happened:

> But, surely, general deluges form no part of the theory of the earth; for, the purpose of this earth is evidently to maintain vegetable and animal life, and not to destroy them.[44]

Neither are violent catastrophes a part of the operations of nature:

> We are not to suppose, that there is any violent exertion of power, such as is required in order to produce a great event in little time; in nature, we find no deficiency in respect of time, nor any limitation with regard to power. But time is not made to flow in vain; nor does there ever appear the exertion of superfluous power, or the manifestation of design, not calculated in wisdom to effect some general end.[45]

The "unscientific" nature of these arguments was apparent to Hutton's contemporaries, and the Huttonians were not able to furnish any better arguments against catastrophes.

Consistent with his belief in the perfection of the earth and in the cyclic nature of its operations, Hutton held that life in the past had not differed appreciably from that at present:

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[43] Ibid., pp. 4-56.

[44] Hutton, Theory . . ., I, 273.

[45] Ibid., p. 182.

> There are, indeed, varieties in those species, compared with the present animals which we examine, but no greater varieties than may perhaps be found among the same species in the different quarters of the globe. Therefore, the system of animal life, which had been maintained in the ancient sea, had not been different from that which now subsists, and of which it belongs to naturalists to know the history.[46]

Playfair, however, faced with the recent discovery in many parts of the earth of the bones of large extinct quadrupeds, admitted that

> The inhabitants of the globe, then, like all the other parts of it, are subject to change: It is not only the individual that perishes, but whole species, and even perhaps genera, are extinguished. It is not unnatural to consider some part of this change as the operation of man. . . .
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> But besides this, a change in the animal kingdom seems to be a part of the order of nature, and is visible in instances to which human power cannot have extended. If we look to the most ancient inhabitants of the globe, of which the remains are preserved in the strata themselves, we find in the shells and corals of a former world hardly any that resemble exactly those which exist in the present. The species, except in a few instances, are the same, but subject to great varieties.[47]

Hutton's point of view was, in a sense, non-historical, the important thing being an understanding of the operations of the world-machine. Playfair, on the other hand, tended to emphasize the gaining of this understanding, not as an end in itself, but in order to interpret the past:

> To trace the series of these revolutions, to explain their causes, and thus to connect together all the indications of change that are found in the mineral kingdom, is the proper object of a Theory of the Earth.[48]

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[46] Ibid., p. 176.

[47] Playfair, pp. 469-70. The idea that the large land quadrupeds found in the recent strata were extinguished by man was revived by John Fleming in the 1820's.

[48] Ibid., p. 2.

In both of them, however, the emphasis was on the philosophical understanding of the operations of the earth rather than upon the investigation of its strata and structure.

Hutton's uniformitarian philosophy, that is the view that the same causes have been in operation throughout the history of the earth, was summed up by him in the following words:

> Not only are no powers to be employed that are not natural to the globe, no action to be admitted of except those of which we know the principle, and no extraordinary events to be alledged in order to explain a common appearance, the powers of nature are not to be employed in order to destroy the very object of those powers; we are not to make nature act in violation to that order which we actually observe, and in subversion of that end which is to be perceived in the system of created things. In whatever manner, therefore, we are to employ the great agents, fire and water, for producing those things which appear, it ought to be in such a way as is consistent with the propagation of plants and life of animals upon the surface of the earth. Chaos and confusion are not to be introduced into the order of nature, because certain things appear to our partial views as being in some disorder. Nor are we to proceed in feigning causes, when those seem insufficient which occur in our experience.[49]

The Newtonian influence on Hutton, for example in the warning against "feigning causes," is in this passage quite evident. Playfair compared the Huttonian system of geology with the Newtonian system of astronomy as modified by Laplace:

> In both, we perceive continual vicissitude and change, but confined within certain limits, and never departing far from a certain mean condition, which is such, that, in the lapse of time, the deviations from it on the one side, must become just equal to the deviations from it on the other. In both, a provision is made for duration of unlimited extent, and the lapse of time has no effect to wear out or destroy a machine, constructed with so much wisdom. Where the movements are all so perfect, their beginning and end must be alike invisible.[50]

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[49]Hutton, Theory . . ., II, 547.

[50]Playfair, p. 440.

Hutton, in the closing paragraph of his paper of 1785, made a similar statement:

> For having, in the natural history of this earth, seen a succession of worlds, we may from this conclude that there is a system in nature; in like manner as, from seeing revolutions of the planets, it is concluded, that there is a system by which they are intended to continue those revolutions. But if the succession of worlds is established in the system of nature, it is in vain to look for any thing higher in the origin of the earth. The result, therefore, of our present enquiry is, that we find no vestige of a beginning,--no prospect of an end.[51]

The Huttonian theory immediately was attacked on both religious and scientific grounds. Perhaps the first to attack it was John Williams, in his Natural History of the Mineral Kingdom, published in 1789.[52] He had many objections to the theory on geological grounds, and his religious objections were typical of the attitude of many Christians towards Hutton's work. "In short," he said,

> few of our author's conclusions are defensible, and no wonder, when he warps and strains every thing to support an unaccountable system, viz. the eternity of the world; which strange notion is the furthest of all from being defensible.
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> . . . . . .
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> The wild and unnatural notion of the eternity of the world leads first to scepticism, and at last to downright infidelity and atheism. If once we entertain a firm persuasion that the world is eternal, and can go on of itself in the reproduction and progressive vicissitude of things, we may then suppose that there is no use for the interposition of a governing power.
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> . . . . . .
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> Thus, our modern philosophers labour hard to confirm their favourite scepticism, &c. by all possible means; or, in other words, they labour hard to rob us of our best inheritance, both here and hereafter,--to sap the foundations of our belief in revelation, and of the superintending care and love, and of the over-ruling

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[51]Hutton, Transactions of the Royal Society of Edinburgh, I (1788), Part II-1, p. 304.

[52]John Williams, The Natural History of the Mineral Kingdom, in Three Parts (2 vols.; Edinburgh: the Author, 1789).

> providence of the all-benevolent, all-powerful God, our Saviour, who cares for us, and upholds us through all the stages of our existence,--and like actual robbers, these philosophers give as nothing in exchange for our natural inheritance. If they say that we are poor mistaken ignorants, and that they wish to convince us of our error,--this is worse than nothing. If we err in charity, let us live and die in this error.[53]

Among the other prominent early opponents of the Huttonian theory were Jean André De Luc, Richard Kirwan, John Murray, William Richardson, and Robert Jameson.[54] As time went by, much of Hutton's work was more favorably viewed by geologists. After it had been

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[53] Ibid., I, lvii-lxi.

[54] De Luc's criticisms were contained in four letters "to Dr. James Hutton, F.R.S. Edinburgh, on his theory of the earth," Monthly Review, II (1790), 206-27; 582-601; III (1791), 573-86; V (1791), 564-85; in a review of Hutton's Theory of the Earth, British Critic, VIII (1796), 337-52, 466-80, 598-606; and in three later works: An Elementary Treatise on Geology, Determining Fundamental Points in That Science, and Containing an Examination of Some Modern Geological Systems, and Particularly of the Huttonian Theory of the Earth, tr. Henry De La Fite (London: F. C. and J. Rivington, 1809); Geological Travels (3 vols.; London: F. C. and J. Rivington, 1810-11); and Geological Travels in Some Parts of France, Switzerland, and Germany (2 vols.; London: F. C. and J. Rivington, 1813). Kirwan's criticisms were contained in "An Examination of the Supposed Igneous Origin of Stony Substances," Transactions of the Royal Irish Academy, V (1793), 51-82; Geological Essays (London: D. Bremner, Successor to Mr. Elmsly, Strand, 1799), pp. 433-99; "Observations on the Proofs of the Huttonian Theory of the Earth, Adduced by Sir James Hall," Transactions of the Royal Irish Academy, VIII (1802), 3-28; and "A Reply to Mr. Playfairs' Reflections on Mr. Kirwan's Refutation of the Huttonian Theory of the Earth," Philosophical Magazine, XIV (1802), 3-13. Murray's criticisms were published anonymously in his A Comparative View of the Huttonian and Neptunian Systems of Geology: in Answer to the Illustrations of the Huttonian Theory of the Earth, by Professor Playfair (Edinburgh: Ross and Blackwood, 1802). Richardson's criticisms were in his "Inquiry into the Consistency of Dr. Hutton's Theory of the Earth, with the Arrangement of the Strata, and Other Phaenomena on the Basaltic Coast of Antrim," Transactions of the Royal Irish Academy, IX (1803), 429-87. Jameson's criticisms were in his System . . ., III, 344-48, 355-67.

generally accepted that basalt and granite were of igneous origin, many geologists decided that he had been essentially correct in his emphasis upon the role of heat in the formation of strata, even if he had attributed too much to it. The rest of his theory, however, was still considered to be excessively and dangerously speculative, and ill-founded. A typical criticism was that of Conybeare, who said:

> Hutton had the merit of first directing the attention of geologists to the important phaenomena of the veins issuing from granite rocks, and traversing the incumbent strata, and of bringing forward in a striking point of view the circumstances which seem to corroborate the igneous origin of trap rocks: the wildness of many of his theoretical views, however, went far to counterbalance the utility of the additional facts which he collected from observation. He who could perceive in the phaenomena of geology nothing but the ordinary operation of actual causes, carried on in the same manner through infinite ages, without the trace of a beginning or the prospect of an end, must have surveyed them through the medium of a preconceived hypothesis alone.[55]

Neither the Huttonian nor the Wernerian theories were highly regarded by William Smith and his pupil, John Farey, who before 1815 were probably better informed than any others about the geological structure of England.[56] They were opposed to the adoption of any hypothesis or any nomenclature, such as that of Werner, that was not based strictly on the geological phenomena of England.[57]

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[55]Conybeare and Phillips, p. xliv.

[56]William Smith (1769-1839), geologist, engineer, and canal surveyor; called "the father of English geology." John Farey (1766-1826), geologist and consulting surveyor, was Smith's most prolific and outspoken literary supporter and defender. During the period, 1806-23, he wrote a great many letters, some anonymous, on geology and in support of Smith's claims to recognition for his discoveries, which were published in a number of journals, particularly in the Philosophical Magazine. He had a vigorous style and was often free with his criticism.

[57]See almost any of the numerous letters that Farey sent to

Smith had, about 1791, developed the hypothesis that each particular formation was characterized by its own unique assemblage of fossil species; and, using this hypothesis, he was able to identify the same formations in many parts of England. He failed to get the assistance of the Geological Society in publishing a geological map of England, in part because of his refusal to adopt the Wernerian system of formations.[58] He eventually succeeded in getting the map published in 1815.[59] The Geological Society sponsored a competing map, compiled by its most influential member, George Greenough, with assistance from Buckland and Conybeare.[60] This map, published in

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the editor of the Philosophical Magazine between 1806 and 1823, especially "Short Notices of Geological Observations Made in the Summer of 1814, in the South of Yorkshire, and in North Wales, and of Some Inferences Therefrom, as to the Structure of England and Wales," Philosophical Magazine, XLV (1815), 161-77, and "Observations on the Priority of Mr. Smith's Investigations of the Strata of England; on the Very Unhandsome Conduct of Certain Persons in Detracting from his Merit therein; and the Endeavours of Others to Supplant Him in the Sale of His Maps;--with a Reply to Mr. W. H. Gilby's Letter in the Last Number," Philosophical Magazine, XLV (1815), 333-44.

[58]Farey, Philosophical Magazine, XLV (1815), 337.

[59]William Smith, A Delineation of the Strata of England and Wales, with Part of Scotland; Exhibiting the Collieries and Mines, the Marshes and Fen Lands Originally Overflowed by the Sea, and the Varieties of Soil According to the Variations in the Substrata, Illustrated by the Most Descriptive Names (London: J. Cary, 1815), accompanied by A Memoir to the Map and Delineation of the Strata of England and Wales, with Part of Scotland (London: John Cary, 1815).

[60]John Farey, "Free Remarks on Mr. Greenough's Geological Map, Lately Published under the Direction of the Geological Society of London," Philosophical Magazine, LV (1820), 379-83. See Elizabeth Oke (Buckland) Gordon, The Life and Correspondence of William Buckland, D.D., F.R.S., Sometime Dean of Westminster, Twice President of the British Association (New York: D. Appleton and Company, 1894),

1820, was used by Conybeare and Phillips in their Outlines.[61]

Smith's priority to his hypothesis was disputed by some, who credited it instead to Werner. For example, Thomas Thomson in 1816 remarked concerning Smith: "His opinions are precisely the same as those of Werner; though I am not sure that he is aware of the coincidence, and I have no doubt that they originated with himself."[62] William Fitton, in 1817, also claimed that Smith's views were identical to those of Werner:

> It has been proved, (and Werner was the first to make the observation), that the masses or strata that constitute the surface of the globe, present themselves in groups or assemblages, the members of which are generally associated, wherever they occur, and are so connected as to exhibit a certain unity of character. To such assemblages Werner gave the name of Formations; and his doctrine . . . is--that the exterior of the earth of the earth consists of a series of these formations, laid over each other in a certain determinate order. Not that the whole series is anywhere complete; but that the relative place of its several members is never departed from. . . . The succession is never violated, nor the order inverted.
> . . . . . .
> The opinions of Mr. Smith . . . so nearly coincide with the doctrine of Formations which we have just stated, that it would be difficult to express them in any other terms.[63]

14-15, for evidence regarding the association of Buckland and Conybeare with the map.

[61] Conybeare and Phillips, fifth (unnumbered) page of the "Preliminary Notice."

[62] Thomas Thomson, "Account of the Improvements in Physical Science during the Year 1815," Annals of Philosophy, VII (1816), 64-65. Thomas Thomson (1773-1852), chemist; regius professor of chemistry at Glasgow University, 1818-52; editor of Annals of Philosophy, 1813-27.

[63] [William Fitton], Review of Transactions of the Geological Society, Vol. III, Edinburgh Review, XXIX (1817), 71. William Henry Fitton (1780-1861), geologist and physician; studied under Jameson; M.D. Cambridge, 1816; president of the Geological Society, 1827-29.

This alleged identity of the ideas of Smith and Werner was denied by Smith's supporters. Thomas Tredgold, in answer to Fitton, compared the principles of Werner with those of Smith:

> Werner's law of succession, which he pretended was universal, evidently flowed from his hypothesis of the formation of the earth; an hypothesis which sets both reason and experience at defiance. The progress of inquiry would, however, have very soon shown its fallacy in the hands of any other person than Werner. But he saw that his law was not the law of Nature,--various strata were found to succeed one another in a different order from what he had assigned them in his hypothesis: this however was easily remedied by creating a distinction without a difference; and the formation was termed a newer, or an older formation, as the case required. Thus, we have new granite and old granite, and the same of other substances:--besides, in the class of formations which Werner calls transition, there appears to be no regular order of succession whatever. . . . But even the classes of Werner do not always succeed one another in the order which Werner assigned them; granite being sometimes found above strata which contain petrifactions.
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> Also, there is nothing more evident than that the Wernerians are without any fixt principles of tracing the structure of the earth; for they are always in doubt and difficulty--even in those places where they constantly reside, and where the tracing the strata presents no difficulty whatever: they write as mineralogists, but certainly not as geologists;--they say a formation occurs in this or that country, (seldom describing it as a continued stratum), and that it is probably of the primitive, transition, or floetz class of formations--almost always as if the rock occurred in detached patches,--seldom pointing out the place with the least precision; and, instead of attempting to show the structure of the country (on which this far-famed hypothesis is founded) by maps and sections, the Wernerians content themselves with giving a string of technical terms connected by expressions which are scarcely to be understood.
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> How different is the course which Mr. Smith has pursued in his attempt to develop the structure of his native country! His principles have arisen wholly out of his own observations on the strata of England; and I am not aware that he has attempted to found any general system of geology upon his discoveries. He has ascertained that certain shells are peculiar to certain strata; and, with the help of this and some other principles equally original, he has succeeded in tracing the principal features of the structure of England; and by selecting a series of strata (many of them in other respects insignificant) he has been able to lay down on his map the principal outlines of the geology of England and Wales.

> If the results of Werner's researches had borne the least analogy to those of Mr. Smith, we might have supposed them to have been conducted on similar principles; but it is too evident that the Wernerians search only for evidence to support a favourite hypothesis, while Mr. Smith attempts to describe the real state of the earth's surface.[64]

The similarity between Werner's and Smith's theories is only superficial. Werner, who investigated primarily the older formations, in which fossils are few or absent, defined formations in terms of all of their characteristics. His theory of deposition and his interest in mineralogy led him to stress the mineral character of the formation as the most important diagnostic aid; but he recognized that other characteristics, such as the fossil content, could, as indicated by his theory, also be useful tools for identifying the formations. Smith, on the other hand, relied almost exclusively on the character of the fossils as a means of identification, a technique that was well adapted to the highly fossiliferous English formations of relatively late age that he described. The Wernerian formations and the Smithian formations were different, those of Werner being based primarily upon mineral characteristics, while those of Smith were founded almost exclusively upon their fossil contents. This difference was pointed out by Farey while criticizing Greenough in 1819:

> Mr. Greenough, in evident allusion to . . . how far Mr. W. Smith is to be considered as a discoverer of the connection now so well proved to exist, between particular Beds or laminae of the

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[64]Thomas Tredgold, "Remarks on the Geological Principles of Werner, and Those of Mr. Smith," Philosophical Magazine, LI (1818), 37-38. The Wernerian at whom most of this criticism was aimed was Jameson. Thomas Tredgold (1788-1829), a self-educated engineer, wrote works on carpentry and the steam engine.

> Strata, and particular species . . . therein, and as the first who actually used and taught this mode of identifying, mapping and tracing the Strata, remarks as follows: viz. "An opinion has for some time past been entertained in this country, that every Rock has its own Fossils."
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> Before I proceed to remark on the Extracts . . . which follow in Mr. G's work, and by which he wishes to appear to prove his position above quoted, I will remark on the loose manner in which the two material parts thereof are defined, that are marked with italics: leaving thus his proposition open to the showing, as in some of the following extracts is attempted to be done, viz. that each "different stone," that is each mineral species of Stone (without regard to its place in the series of Strata), "yield quite different sorts or species of Shells," and that the supposed relation subsists, between mineral and animal Species, instead of the relation which Mr. Smith and myself contend for, viz. between the successive periods or eras of deposition of the particular Beds, and the particular species or varieties of Animals, which, at or immediately prior thereto, existed in the water, on the bottom of which the Beds in question were formed.[65]

While Smith urged the usefulness of fossils as a means of identifying strata, Cuvier stressed their importance for interpreting the history of the earth. Although his geological thought was heavily influenced by Wernerian doctrines, Cuvier differed from Werner in advocating paleontology rather than mineralogy as the most useful tool for understanding the past. He also asserted, contrary to Werner, that strata had originally been laid down in a horizontal position and that the present inclined position of many strata, especially of the older formations, was caused by violent earth movements subsequent to their deposition.[66]

Cuvier adopted the Wernerian position that there was a regular succession in the nature of the beds laid down by the sea, due to

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[65]John Farey, "Free Remarks on the Geological Work of Mr. Greenough," Philosophical Magazine, LIV (1819), 127-28.

[66]Cuvier, Essay . . ., pp. 10-11.

changes in the composition of the waters of the universal ocean. He believed that in response to this varying composition of the marine environment there occurred a corresponding succession of marine life.[67] It was by the study of the successive forms of life on the earth, Cuvier claimed, that a historical sense could be given to geology. Only paleontology, he insisted, could clearly establish that a significant lapse of time, sufficient for the new fauna to grow, occurred during the deposition of the successive formations. Only paleontology could tell us about the revolutions affecting the land and the sea that have taken place on the globe.[68]

The nature of these revolutions of the globe, according to Cuvier, was most easily and clearly understood by the study of the paleontology of the large animal quadrupeds.[69] For example, he argued that in the region around Paris there had existed a rich land fauna, including large quadrupeds, now extinct. The strata containing the remains of these animals were overlaid by strata containing marine shells. Therefore, there had been a transition from a terrestrial fresh-water environment to a marine environment, the result of a violent irruption of the sea that had utterly destroyed the animal life of the region. After a period of occupation of the region by the sea, the land had again risen above the waves, and a new land fauna

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[67] Ibid., p. 13.

[68] Ibid., pp. 54-55.

[69] Ibid., pp. 57-60.

had appeared. These perhaps had migrated from other lands not overrun by the sea.[70]

The paleontological evidence was only the most convincing of the many kinds of evidence for catastrophes in the past, Cuvier said. The faulted, folded, and up-turned strata and widespread deposits of conglomerate testified to the violence of earth operations in the past.[71] These revolutions could not possibly have been produced by the action of existing geological causes, for there is a sharp distinction between the past and the present order in the history of the earth:

> It has been long considered possible to explain the more ancient revolutions on its surface by means of these still existing causes. . . . But . . . unfortunately this is not the case in physical history:--the thread of operation is here broken, the march of nature is changed, and none of the agents that she now employs were sufficient for the production of her ancient works.[72]

He considered the existing agents of change, such as the erosional activity of water and the depositional activity of volcanoes and mineral springs, to have been of negligible importance as causes in the production of the immense changes that had taken place in the past.[73]

Cuvier emphatically dissociated his geological doctrines from the earlier "theories of the earth," among which he mentioned the Huttonian theory, although not that of Werner. These previous speculations

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[70] Ibid., pp. 107-11.

[71] Ibid., pp. 15-17.

[72] Ibid., p. 24.

[73] Ibid., pp. 24-36.

had not been sufficiently general and had not taken into consideration the secondary strata and its fossils, he said.[74] Previous geologists were either mineralogists, with little interest in fossil remains, or they were fossil collectors, with little knowledge of the strata:

> Naturalists seem to have scarcely any idea of the propriety of investigating facts before they construct their systems. The cause of this strange procedure may be discovered, by considering that all geologists hitherto have either been mere cabinet naturalists, who had themselves hardly paid any attention to the structure of mountains, or mere mineralogists, who had not studied in sufficient detail the innumerable diversity of animals, and the almost infinite complication of their various parts and organs. The former of these have only constructed systems; while the latter have made excellent collections of observations, and have laid the foundations of true geological science, but have been unable to raise and complete the edifice.[75]

Cuvier believed that the mineralogists, such as Werner, had not "defined the species of organized extraneous fossils in each description of the strata with that accuracy which has become necessary." As for the fossil collectors:

> Considering these fossil plants and animals merely in themselves, instead of viewing them in their connection with the theory of the earth; or regarding their petrifactions and extraneous fossils as mere curiosities, rather than as historical documents; or confining themselves to partial explanations of the particular bearings of each individual specimen; they have almost always neglected to investigate the general laws affecting their position, or the relation of the extraneous fossils with the strata in which they are found.[76]

The extent of Cuvier's influence on British geology was well recognized by contemporary British geologists. For example, Roderick Murchison, in his eulogy of Cuvier, said:

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[74] Ibid., pp. 49-50.

[75] Ibid., pp. 51-52.

[76] Ibid., pp. 53-54.

> He it was who, removing from geology the incumbrance of errors and conceits heaped on it by cosmogonists, contributed more than any individual of this century to raise it to the place which it is assuming amongst the exacter sciences.[77]

Conybeare credited Cuvier with awakening geologists to a realization of the vast importance of paleontology:

> the high scientific distinction of Cuvier, and the striking and interesting nature of the facts developed in his brilliant Memoir, excited a marked sensation and commanded the general attention of men of science; for none such could peruse with indifference those masterly descriptions, which exhibited the environs of one of the great metropolitan cities of Europe as having been successively occupied by oceanic inundations and fresh-water lakes; which restored from the scattered fragments of their disjointed skeletons the forms of those animals, long extinct, whose flocks once grazed on the margins of those lakes; and which presented to our notice the case of beds of rock only a few inches in thickness, extending continuously over hundreds of square miles, and constantly distinguished by the same peculiar species of fossil shells.
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> . . . . . .
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> From this period the views of the zoological school were universally adopted by the most active and efficient labourers in the progress of English geology, and were by them from time to time greatly extended.[78]

Conybeare said that Cuvier "was the first to raise comparative anatomy to the rank of an exact science, and, . . . by his highly philo-

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[77]Roderick I. Murchison, Presidential Address to the Geological Society of London, February 15, 1833, Philosophical Magazine, ser. 3, II (1833), 469. Roderick Impey Murchison (1792-1871), geologist and geographer; learned geology in the 1820's after an army career; active in the Geological Society, the British Association for the Advancement of Science, and the Royal Geographical Society; knighted and later made a baronet. See also the Report of the British Association for the Advancement of Science (1831-32), pp. 104-05, for a highly laudatory eulogy of Cuvier by Buckland.

[78]W. D. Conybeare, "Report on the Progress, Actual State, and Ulterior Prospects of Geological Science" Report of the British Association for the Advancement of Science (1831-32), pp. 371-72.

sophical generalization of the constant coordinate relations of the animal structure, became at once the Newton of that science."[79]

Cuvier's influence is evident in Conybeare's "Introduction" to the Outlines. For example, Conybeare agreed with Cuvier and Smith in their emphasis upon the importance of paleontological evidence in characterizing and identifying strata. He stated that fossil remains

> are not irregularly dispersed throughout the whole series of these formations, but disposed as it were in families, each formation containing an association of species peculiar in many instances to itself, widely differing from those of other formations, and accompanying it throughout its whole course; so that at two distinct points on the line of the same formation, we are sure of meeting the same general assemblage of fossil remains.[80]

The farther apart stratigraphically two formations are, he said, the more unlike are their organic remains. He believed that in the younger formations the organic remains appear to approach in form to existing life: that is, the newer the formation, the greater should be the percentage of its fossil species that are the same as existing species.[81]

Conybeare adopted the position of Werner and Cuvier that conglomerates were consolidated masses of gravel derived by violence from older strata. He agreed with Cuvier that these strata were originally horizontal:

> We are sure when we find such beds, as we often do, in nearly vertical strata, that this cannot have been their original position, but is one into which they have been forced by convulsions which have dislocated them subsequently to their consolidation.[82]

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[79] Ibid., p. 403.

[80] Ibid., p. x.

[81] Ibid., pp. ix-xiii. Compare Cuvier, Essay . . ., pp. 13, 173.

[82] Conybeare and Phillips, p. xiv. Compare Cuvier, Essay . . ., pp. 10-11, 16.

As marine formations overlie two-thirds of our continents and are found in some of the highest mountains, Conybeare said, the level of the ocean was in the past much higher than it is now: "The great and fundamental problem therefore, of theoretical geology is obviously to assign adequate causes for the change of level in this ocean." It appeared to him that this was due to "violent convulsions which have either heaved up the present continents, or . . . depressed the present channel of the ocean."[83]

Conybeare, like Cuvier, distinguished between the past and the present order of things. He declared that the formation of valleys was "connected with the most recent of those causes which have modified" the surface of the earth, "previously to its passing into the state in which we now behold it, and becoming subject to the order of causes which still prevails."[84] He agreed with Cuvier that the causes of change in the present (except possibly for volcanoes and earthquakes) were incompetent to have produced any of the "revolutions" that took place in the past. He noted that these causes--rivers, rain, frost, waves, and vegetation--often produce "a balanced and compensated effect of destruction and renovation." The actions of the atmospheric agents "appear to be circumscribed within very narrow limits." The sea had "a similar tendency to impose a limit to its own ravages" by forming a barrier against itself, and

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[83]Conybeare and Phillips, pp. xv-xvi. Compare Cuvier, Essay . . ., pp. 7-9, 11.

[84]Conybeare and Phillips, p. xxi.

rivers could not have produced their own valleys.[85] He concluded:

> Historical records, and the very nature and physical possibilities of the case, alike compel us to dissent entirely from those crude and hasty speculations which would assign to the causes now in action, the power of producing any very material change in the face of things; and which would refer to these alone, acting under their present conditions, and with only their present forces, the mighty operations which have formed and modified our continents.[86]

Unlike Cuvier, Conybeare seems to have favored the vulcanist theory. Although in 1822 he treated the theory as hypothetical and worthy only of a lengthy footnote, he did not attack it; and his support for it became very strong in succeeding years.[87] The theory, according to Conybeare, assumed that "the crust of the Earth rests on a heated nucleus," which is in a fluid or viscous state. The "undulations" of this nucleus were the primary cause of the convulsions that took place in the past history of the earth. The assumption of a hot liquid nucleus would explain volcanoes and earthquakes, as well as the spheroidal shape of the earth. The effects associated with the nucleus would have been much more violent in the earliest periods, when it was hotter and the earth's crust was very thin; and the earth's climate would have been correspondingly warmer.[88]

The evidence for the vulcanist theory, he said, included the following: volcanic energy is the only agent we know that produced

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[85] Ibid., pp. xxiii, xxxi-xxxiii.

[86] Ibid., p. xxxiii.

[87] See Conybeare, Report of the British Association for the Advancement of Science (1831-32), pp. 365-414, which is strongly vulcanist in tone.

[88] Conybeare and Phillips, pp. xvii-xix.

effects analogous to the violent movements that are presumed to have affected the mountains and their surrounding strata; the large numbers of extinct volcanoes are evidence that volcanic activity has been greater in the past; the widespread occurrence of trap rocks (basalt) and granite, which appear to be of volcanic origin also indicates greater volcanic activity in the past; the temperature of the earth seems to increase with depth; and many fossils, found in northern countries, are of species whose modern analogues live in tropical seas, indicating that the climate of these countries was formerly warmer.[89] Conybeare considered the study of the effects of modern earthquakes and volcanoes to be of importance. He looked upon these agents as important links with the past, although relatively feeble in their activity compared with that of the past.[90]

Conybeare's influence on British geology was strong. Adam Sedgwick spoke of Conybeare as "his master in the science,"[91] while Murchison called the Outlines his scientific Bible and acknowledged that of the older geologists he was indebted to no one more deeply than to Conybeare. Geikie, Murchison's biographer, wrote: "From his

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[89] Ibid.

[90] Ibid., p. xxxvi.

[91] John Willis Clark and Thomas McKenny Hughes, The Life and Letters of the Reverend Adam Sedgwick, LL.D., D.C.L., F.R.S., Fellow of Trinity College, Cambridge, Prebendary of Norwich, Woodwardian Professor of Geology, 1818-1873 (2 vols.; Cambridge: At the University Press, 1890), I, 221. The Rev. Adam Sedgwick (1785-1873), president of the Geological Society, 1829-31; in the 1830's, with Murchison, investigated the "transition rocks" of Werner.

earliest geological paper onwards, the influence of that book may be traced in all his geological writings."[92]

Probably the most widely read British vulcanist in the period before 1822 was Robert Bakewell, whose Introduction to Geology was first published in 1813.[93] His work contained a considerable amount of speculation, compared to subsequent works, such as that of Conybeare and Phillips; nevertheless John Farey contended that it contained "many more of the facts concerning our planet, and fewer of the absurd and whimsical assertions and theories concerning it, than any of the numerous systematic works which have preceded it."[94] He declared that most cases of disagreement between him and Bakewell stemmed from the latter's tendency to rely too much on

> that very erroneous and dangerous dogma of the Anglo-Wernerian Theorists, viz. that the kind of stone or mineralogical characters of a substance, will, by help of "the Geognosy," determine its

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[92]Archibald Geikie, Life of Sir Roderick I. Murchison, Bart.; K.C.B., F.R.S.: Sometime Director-General of the Geological Survey of the United Kingdom; Based on His Journals and Letters, with Notices of His Scientific Contemporaries (2 vols.; London: John Murray, 1875), II, 306.

[93]Robert Bakewell, An Introduction to Geology, Illustrative of the General Structure of the Earth; Comprising the Elements of the Science, and an Outline of the Geology and Mineral Geography of England (London: J. Harding, 1813). Robert Bakewell (1762-1843), geologist and mineralogical surveyor; taught geology in London; like Smith and Farey, never admitted to the Geological Society. A second edition was published in 1815, a third in 1828, and a fourth in 1833, and a fifth in 1838.

[94]John Farey, "Notes and Observations on the Introduction and Three First Chapters, of Mr. Robert Bakewell's 'Introduction to Geology';--Embracing Incidentally, Several New Points of Geological Investigation and Theory," Philosophical Magazine, XLII (1813), 247.

> priority of formation to others, &c.; or in plainer terms, will fix its place in the series of strata; but which Smithian observers, well know to be untrue.[95]

Bakewell's criticism of Werner, however, was no more restrained than that of Farey. In connection with this he remarked:

> I have expressed my objections to the theory of Werner with less respect than many may think it entitled to; but the confident, not to say arrogant manner in which it has been supported, considering the preposterous claims which it makes on our credulity, is truly ridiculous, and will form an amusing page in the future history of science. The theory will be preserved from oblivion embalmed in its own absurdity.[96]

He was friendlier to the Huttonian theory, but not uncritical of it.[97]

Bakewell regarded the earth as having been originally in a chaotic state and having advanced by progressive changes to its present "tranquil habitable state."[98] Life also had changed in a progressive manner.[99] The earth was very old, its history having been marked by "ages of endless duration,"[100] and the periods of formation of the various strata were separated by "distant intervals of time."[101]

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[95]John Farey, "Notes and Observations on the Fourth, Fifth and Part of the Sixth Chapters of Mr. Robert Bakewell's 'Introduction to Geology';--Embracing Incidentally, Several New Points of Geological Investigation and Theory," Philosophical Magazine, XLII (1813), 357.

[96]Bakewell, p. 229. In another place he commented: "The term 'well educated geognost,' as used by some writers, denotes a perfect disciple of Werner, who has lost the use of his own eyes by constantly looking through the eyes of his master." (p. 353.)

[97]Bakewell, pp. 113, 209.

[98]Ibid., pp. 325-26.

[99]Bakewell (2d ed.), p. 16.

[100]Bakewell, p. 192.

[101]Bakewell (2d ed.), p. 18.

He believed that the history of the earth had been characterized by intervals of repose following periods of intense agitation and violence, including great convulsions and the upheaval of land.[102] There was probably greater violence in the past, when the earth's internal heat was more intense, although future periods of violence were possible.[103] He made no sharp division between the present and the past; and he believed that modern processes were closely analogous, although inferior in violence, to those of the past.[104] He appears to have accepted the hypothesis of a recent geological catastrophe unrelated to the Biblical flood.[105] He objected, however, to the assumption of universal floods in order to explain the apparent alternation of land and sea, preferring to explain the phenomena by assuming the local uplift of land.[106]

Bakewell speculated that many of the secondary strata had been formed or consolidated by the action of submarine volcanoes. He believed that basalt had flowed from such volcanoes, while sandstone and limestone may have been consolidated by deposition from water impregnated with siliceous or calcareous earth that had been emitted by

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[102] Ibid., pp. 52-54, 429.

[103] Ibid., pp. 431, 439.

[104] Ibid., p. 231.

[105] See ibid., p. 430, where he speaks of the mammoth and the mastodon having "perished in the last grand revolution of the globe before the formation of man."

[106] Ibid., p. 234.

volcanoes.[107] Farey criticized him for "Plutonic blemishes," such as his hypothesis that mountains had been uplifted by the force of subterranean heat. Farey expressed the hope that Bakewell's geological writings, "from being less Geognostical and Plutonical, will I trust further improve hereafter."[108]

Bakewell paid little attention to paleontological evidence, although he agreed that Smith's work was of importance. He regretted that Smith had not published earlier, "as the novelty and interest which would have attached to them a few years since is in some degree passed by, in consequence of similar discoveries, by the celebrated naturalist Cuvier, having been published already through Europe." He doubted

> whether the existence of organic remains is sufficient to identify strata in distant parts of the globe, as similar remains are sometimes found in rocks which have very little resemblance to each other; on which account I am inclined to believe that this position, like many others which have been advanced in geology, must be taken with certain limitations: indeed, were we to admit that any one stratum ever extended from the arctic circle to the equator, it seems more than probable that the animals which lived upon it must have been very different in different latitudes.[109]

Another British vulcanist was John Macculloch, an early member of the Geological Society, whose field work on the igneous rocks contributed much evidence for the validity of the vulcanist theory.[110]

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[107] Ibid., pp. 135, 230-32.

[108] John Farey, "Short Notices of Geological Observations Made In the Summer of 1814, in the South of Yorkshire, and in North Wales, and of Some Inferences therefrom, as to the Structure of England and Wales," Philosophical Magazine, XLV (1815), 175-76.

[109] Bakewell (2d ed.), p. 466.

[110] John Macculloch (1773-1835), geologist, mineralogist,

By the late 1820's the vulcanist theory was accepted by most British geologists, and Fitton in 1828 commented that

> nothing has been of late more remarkable, with reference to its history in this country, than the universal adoption of a modified volcanic theory, and the complete subsidence, or almost oblivion, of the Wernerian and Neptunian hypotheses.[111]

A considerable group of geologists, including many ex-Wernerians, were sceptical of geological theory in general and were inclined in particular to question the value of paleontology in interpreting the past. A prominent sceptic was George Bellas Greenough, one of the founders and first president of the Geological Society.[112] He wrote in 1819 that the utility of fossils in identifying and tracing strata had been greatly overrated and that it was absurd to believe "that every part of the earth has been peopled, at the same period, by the same animals," which was to him the implication of Smith's hypothesis.[113] He questioned the practice of reasoning about fossil species on the basis of assumed analogies between them and modern species. The older fossils are so unlike the modern species, he argued, that we can infer nothing about their habits.[114] We can't even decide, he said, whether

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chemist, and physician; M.D., University of Edinburgh, 1793; president of the Geological Society, 1815-17; a difficult personality, his relations with other geologists were much embittered in his later years.

[111]William H. Fitton, Presidential Address to the Geological Society on February 15, 1828, Philosophical Magazine, ser. 2, III (1828), 295.

[112]George Bellas Greenough (1778-1855), geologist and geographer.

[113]G. B. Greenough, A Critical Examination of the First Principles of Geology; in a Series of Essays (London: Longman, Hurst, Rees, Orme, and Brown, 1819), pp. 287-88.

[114]Ibid., pp. 296-97.

they were marine or fresh-water animals, or in what depth of water they may have lived. He therefore discounted Cuvier's evidence for inroads and retreats of the sea in the region around Paris.[115] He also regarded as unfounded Werner's idea that there had been a gradual approximation or succession of life toward that of the present.[116]

Greenough did accept the Wernerian idea that the older, more consolidated strata were largely chemical in origin. Since they were believed to have been precipitated from solution in the ocean, their formation need not have taken much time, especially as it was also believed that strata which graded into one another were deposited at the same time. Greenough went to the extreme of denying that an unconformity of stratification between beds indicated an interval between depositions. In many cases, he said, unconformity was caused "by the disturbance which one of the substances sustained from the deposition or precipitation of the other."[117] Some geologists of like mind, such as John Kidd, went so far as to assert that most or all of the older rocks were formed at the same time.[118]

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[115] Ibid., pp. 302-304.

[116] Ibid., p. 282.

[117] Ibid., pp. 271, 275-76.

[118] John Kidd, A Geological Essay on the Imperfect Evidence in Support of a Theory of the Earth, Deducible Either from Its General Structure or from the Changes Produced on Its Surface by the Operation of Existing Causes (Oxford: the Author, 1815), pp. 132, 135. This book was dedicated to Greenough. John Kidd (1775-1851), physician and chemist; M.D., Oxford, 1804; professor of chemistry; Oxford, 1803-22; regius professor of physic, 1822-51; gave lectures on mineralogy and geology and was succeeded by Buckland in 1814.

Greenough's scepticism filled some people with dismay. At least one reviewer, in the American Journal of Science, greeted Conybeare's work with enthusiasm because of its affirmative character, which contrasted with the tone of Greenough's work. Referring to Conybeare's "Introduction," he wrote:

> It contains a general view of those principles of geology, which may be regarded, apart from all hypothesis, as established; and really, after all that has been said and written of late, upon the imperfection and falsity of geological positions; after witnessing the extensive scepticism of one of the first geologists of England, in his late work; and observing the anxious doubts his writings have infused into some, and the irritation produced by them upon others, who saw a death blow given to their favourite system; after this, we are truly happy, that there are some principles of the Science, that have lived through the furnace and come forth with additional brightness. We are glad also to see other principles, springing up and flourishing, on the mouldering ruins of former systems.[119]

The supposed contrast between the two works was not so apparent to a reviewer in the British Critic, who was somewhat apprehensive about the trend that the two works indicated:

> At present, the prevailing spirit seems to be the absolute rejection of all theory; a principle which, however just in itself, may be, and often is, carried to a blameable excess; whilst the opposite fault is characterized by at least one advantage, that though an erroneous principle in itself, it is nevertheless very commonly the parent of an ardour in the pursuit, and a success in the investigations, which would not have been attained without it. Valuable facts have often been elicited in the ardent pursuit of a most visionary theory, and discoveries of real importance, case aside in the enthusiasm of following up some fanciful hypothesis, have been subsequently treasured up, and found to possess infinitely more value than the speculations which gave them birth.[120]

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[119]Anon., Review of Outlines of the Geology of England and Wales, by W. D. Conybeare and William Phillips, American Journal of Science, VII (1824), 232.

[120]Anon., Review of Outlines of the Geology of England and

The same sentiment had been expressed more strongly and eloquently by Playfair, who was paraphrased by William Brande in 1817:

> Much as has been said upon the mischief of geological theories which by some are represented as ingenious, though dangerous fictions, no one can justly deny their importance and utility, as furnishing strong incitements to the labour of observation and experiment. He that has framed a theory, is fond of searching for confirmations; and he proceeds with a zeal and enthusiasm widely distinct from the cold accuracy of the mere accumulator of insulated facts. In all physical inquiries, theory and observation should go together, like mind and body; the one guiding and directing the other. It is true, that the impartiality of an observer may often be affected by system; but upon this it has been justly remarked by Mr. Playfair, that it is a misfortune, against which, the want of theory is no security. The partialities in favour of opinions, are not more dangerous than the prejudices against them; for such is the spirit of system, and so naturally do all men's notions tend to reduce themselves into some regular form, that the very belief that there can be no theory, becomes a theory itself, and may have no inconsiderable sway over the mind of an observer. Besides, one man may have as much delight in pulling down, as another in building up, and may chuse to display his dexterity in the one occupation as well as in the other. The want of theory then, does not secure the candour of an observer, and may greatly diminish his skill. The discipline best calculated to promote both, is a thorough knowledge of the methods of inductive investigation, an acquaintance with the history of physical discovery, and the study of those sciences in which the rules of philosophizing have been most successfully applied.[121]

British geological thought by 1822 was in a state of flux, with no theory or system recognized as dominant. The disillusionment

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Wales, by W. D. Conybeare and William Phillips, British Critic, new series, XX (1823), 285.

[121]William Thomas Brande, Outlines of Geology: Being the Substance of a Course of Lectures Delivered in the Theatre of the Royal Institution in the Year 1816 (London: John Murray, 1817), pp. 27-28. Much of this passage is quoted directly from Playfair, Illustrations . . ., pp. 524-28, and the work as a whole is quite favorable to the Huttonian theory. A revised edition, Outlines of Geology (London: John Murray, 1829), was much less favorable to the Huttonian theory, contained a weaker statement on theoretical speculation, and accepted the diluvial theory of Buckland, whereas the 1817 version had not even mentioned the deluge.

caused by the realization that the Wernerian system contained defects equally as grave as those of other systems led increasingly to a sceptical attitude toward all geological speculation. The attitude of the Geological Society in this regard has been described in the following terms:

> To multiply and record observations, and patiently to await the result at some future period, was the object proposed by them, and it was their favourite maxim that the time was not yet come for a general system of geology, but that all must be content for many years to be exclusively engaged in furnishing materials for future generalizations.[122]

A new version of an old theory was, however, about to cause a revival of geological speculation. The diluvial theory--that is, the theory that many geological appearances can be explained as the result of a recent, universal, violent deluge, which may or may not have been that of Noah--was challenging the prevailing scepticism.

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[122]Charles Lyell, Principles of Geology, Being an Attempt to Explain the Former Changes of the Earth's Surface, by Reference to Causes Now in Operation, I, (London: John Murray, 1830), 71-72.

CHAPTER II

THE DILUVIAL THEORY AND WILLIAM BUCKLAND

The word "alluvial" refers to a flood or to the material deposited by flowing water. Thus "alluvium" is defined as "a deposit of earth, sand, and other transported matter left by water flowing over land not permanently submerged."[1] Jameson described the Wernerian class of alluvial rocks as:

> those rocky substances that are formed from previously-existing rocks, of which the materials have been worn down by the agency of water and air, and afterwards deposited in nearly horizontal beds on the surface of the land, or on sea-coasts.[2]

There is in this definition no implication of a deluge. The term referred to those materials deposited by the ordinary operations of flowing water, including local floods. The Wernerian deluge was supposed to have occurred at the time of the deposition of the newest floetz-trap formation and to have had no special connection with the alluvium.

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[1] A New English Dictionary on Historical Principles: Founded Mainly on the Materials Collected by the Philological Society, ed. James A. H. Murray, I (Oxford: at the Clarendon Press, 1888).

[2] Robert Jameson, System of Mineralogy, Comprehending Oryctognosy, Geognosy, Mineralogical Chemistry, Mineralogical Geography, and Economical Mineralogy, III (Edinburgh: William Blackwood, 1808), 206.

Many British geologists, however, felt that much of the alluvium had been produced by a recent catastrophic deluge because of the following evidence: 1. the character of this alluvium, formed out of remains apparently torn from the regular strata and scattered or transported over extensive areas; and 2. the existence, in this alluvium, of the bones of extinct animals.

An extensive knowledge of the character of the alluvium in England was first acquired by William Smith. In 1806 his pupil, John Farey, defined "alluvial matters" as

> the fragments of the regular strata, more or less mixed with each other, or with extraneous matters, and rounded or worn, lying upon the regular strata (for such are rarely or probably never seen in or under the strata) and are there found deposited, apparently by the action of violent currents of water, . . . the manner of these alluvial deposits being perfectly different from, and apparently regulated by laws quite dissimilar from those which obtained when the deposition of the strata took place.

He thought that the fragmentary character of these alluvial deposits was probably related to

> the truly enormous and violent breaking up, which the strata have almost universally undergone, . . . the effects of which are well known to miners . . . by the name of faults, troubles, dykes, fissures, &c, &c.[3]

Farey complained that many geologists (presumably including the Wernerians) included under the term "alluvial" rocks that he and Smith classed as regular strata:

> With many modern writers, the terms alluvium and alluvial strata, are applied, according to their theories, to a great

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[3]John Farey, "On the Stratification of England; the Intended Thames Archways, &c," Philosophical Magazine, XXV (1806), 45-46. This letter is dated 21st May, 1806.

> part of the strata which compose the British Islands; . . . but Mr. William Smith and myself constantly confine the term Alluvia, to superficial matters.[4]

Smith in 1816 contrasted the "finely preserved" condition of the fossils of the regular strata with those of the alluvial deposits, which were "greatly rounded by attrition." These circumstances, he said, "clearly proved two distinct operations of water":

> Conceiving, therefore, the Gravel Fossils to be the most indubitable effects of a great body of water passing over the surface of the earth, with violence sufficient to tear up fragments of the Strata, round them by attrition, and drive them many miles from their regular beds to the promiscous situations which they now occupy. These have been called alluvial Fossils, and the Gravel which contains them being thus clearly distinguished from the regular Strata beneath.[5]

Hutton had denied the occurrence of violent deluges, and Playfair presented arguments against the idea that a deluge, or debacle, had any permanent effect on the earth's surface. "In Dr. Hutton's theory, he said,

> nothing whatever is ascribed to such accidental and unknown causes; and, though their existence is not absolutely denied, their effects, whatever they may have been, are alleged to be entirely obliterated, so that they can be referred to no other class but that of mere possibilities.[6]

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[4]John Farey, General View of the Agriculture and Minerals of Derbyshire; with Observations on the Means of Their Improvement. Drawn up for the Consideration of the Board of Agrigulture [sic] and Internal Improvement, I (London: G. and W. Nicol and Others, 1811), 131.

[5]William Smith, Strata Identified by Organized Fossils, Containing Prints on Colored Paper of the Most Characteristic Specimens in Each Stratum (London: The Author and Others, 1816), Introduction (second unnumbered page).

[6]John Playfair, Illustrations of the Huttonian Theory of the Earth (Edinburgh: William Creech, 1802), p. 400.

He stated what appeared to him to be the principal objections to all explanations of the formation of valleys that required the interposition of an extraordinary cause:

> The general structure of valleys among mountains, is highly unfavourable to the notion that they were produced by any single great torrent, which swept over the surface of the earth. In some instances, valleys diverge, as it were from a centre, in all directions. In others, they originate from a ridge, and proceed with equal depth and extent on both sides of it, plainly indicating, that the force which produced them was nothing, or evanescent at the summit of that ridge, and increased on both sides, as the distance from the ridge increased. The working of water collected from the rains and the snows, and seeking its way from a higher to a lower level, is the only cause we know of, which is subject to this law.
>
> Again, if we consider a valley as a space, which perhaps with many windings and irregularities, has been hollowed out of the solid rock, it is plain, that no force of water, suddenly applied, could loosen and remove the great mass of stone which has actually disappeared.[7]

Playfair thought some valleys "so particularly constructed, as to carry with them a still stronger refutation of the existence of a debacle":

> These are the longitudinal valleys, which have the openings by which the water is discharged, not at one extremity, but at the broadside.

The source that excavated such a valley, he said, "must have been nothing at the two extreme points . . . and must have increased with the distance from each":

> It can have been produced, therefore, only by the running of two streams in opposite directions, on a surface that was but slightly uneven, these streams at meeting taking a new direction, nearly at right angles to the former. A clearer proof could hardly be required than is afforded in this case, that what is now a deep valley was formerly solid rock, which the running of the waters has gradually worn away; and that the waters, when

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[7] Ibid., p. 401.

> they began to run, were on a level as high, at least, as the tops of those mountains by which the valley is bounded toward the lower side.[8]

With respect to the large erratic granite boulders frequently found around the Alps, he remarked that a debacle could never have transported these immense blocks unless the valleys intervening between them and the Alps were not there. If these valleys had not been present so that there had been a uniform declination of the land surface from the Alps to where the granite blocks were deposited, the ordinary operations of nature, he contended, would have been sufficient to transport them.[9] In describing the activities of the Alpine glaciers, he remarked:

> The immense quantity and size of the rocks thus transported, have been remarked with astonishment by every observer, and explain sufficiently how fragments of rock may be put in motion, even where there is but little declivity, and where the actual surface of the ground is considerably uneven. In this manner, before the valleys were cut out in the form they now are, and when the mountains were still more elevated, huge fragments of rock may have been carried to a great distance; and it is not wonderful, if these same masses, greatly diminished in size, and reduced to gravel or sand, have reached the shores, or even the bottom, of the ocean.[10]

"Where the strata are nearly horizontal," he said, "they afford the most distinct information concerning the direction and progress of the wasting of the land." He related that John Barrow had observed in his travels in South Africa that among the mountains there, which are formed of horizontal strata,

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[8] Ibid., pp. 402-03.

[9] Ibid., pp. 407-08.

[10] Ibid., p. 389.

> the high or steep sides look constantly down the rivers, while the sloping or inclined sides have just the opposite direction. When, in travelling northward, he passed the line of partition, where the waters from running south take their direction to the north, he found, that the gradual slope, which had hitherto been turned to the north, was now turned to the south: The abrupt aspect of the mountains, in like manner, from facing the south, was directed to the north; so that, in both cases, the hills turned their backs on the line of greatest elevation.
>
> It is evident, therefore, that the form of this land has been determined by the slow working of the streams. The causes which produced the effects here described, began their action from the line of greatest elevation, and extended it from thence on both sides, in opposite directions.[11]

Playfair stated that evidence of a particular kind, if found, would furnish support for a diluvial theory:

> If there were any where a hill, or any large mass composed of broken and shapeless stones, thrown together like rubbish, and neither worked into gravel nor disposed with any regularity, we must ascribe it to some other cause than the ordinary detritus and wasting of the land. This, however, has never yet occurred; and it seems best to wait till the phenomenon is observed, before we seek for the explanation of it.[12]

Unfortunately for Playfair's theory, such evidence was soon found by Sir James Hall.

Hall, a friend of Hutton and Playfair, gave strong support to the Huttonian hypothesis of the igneous origin of crystalline rocks by his experiments on the effect of heat upon rocks.[13] He disagreed,

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[11] Ibid., p. 411. Judging from his later support of the diluvial theory of Buckland, Barrow would probably not have agreed with Playfair's explanation of his observations.

[12] Ibid., pp. 411-12.

[13] See Sir James Hall, "Experiments on Whinstone and Lava," Transactions of the Royal Society of Edinburgh, V (1805), 43-98. Sir James Hall, Bart. (1761-1832), geologist and chemist, was called "the founder of experimental geology."

however, with Hutton's prejudice against deluges and thought that there was ample evidence that they had occurred. As early as 1798, referring to the Huttonian theory and to the problem of the removal of the strata on the upraised continents, he said:

> The whole of this system appears to me well founded, except in what regards the removal of the superincumbent mass, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. Pallas, M. de Saussure, and M. Dolomieu, and to believe that, at some period very remote with respect to our histories, though subsequent to the induration of the mineral kingdom, the surface of the globe has been swept by vast torrents, flowing with great rapidity, and so deep as to overtop the mountains; that these torrents, by removing and undermining the strata in some places, and by forming in others immense deposits, have produced the broken and motley structure, which the loose and external part of our globe every where exhibits.[14]

Hall answered Playfair's arguments by supposing that the strata, previous to the inundation, had been broken up by earth movements:

> Many of the rocks being rent in various ways, the hardest parts being in a shivered state, would easily be carried forward. The soft beds of shale or slate-clay being laid open to the attacks of the current, would be deeply abraded by its action . . . . The water would thus be loaded with a multitude of blocks of every size, shape, and quality, and with a quantity of clay, which being soon reduced to mud, through which these stones were irregularly and confusedly scattered, would flow at the bottom of the water . . . and would be deposited.[15]

The country in the neighborhood of Edinburgh presented abundant evidence to support this hypothesis; it contained large masses of unstratified materials, apparently deposited by a deluge. Hall thought that this evidence should answer Playfair's objection:

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[14] Ibid., p. 68.

[15] Sir James Hall, "On the Revolutions of the Earth's Surface," Transactions of the Royal Society of Edinburgh, VII (1815), 173. Read March 16, 1812.

> Such seems to have been the origin of that body of compact blue clay which forms a material part of our low districts, bearing every indication of having flowed as a mass into its present situation; for it is totally devoid of stratification, though frequently of great thickness. . . .
>
> The existence of assemblages of this sort, affords, by its simple testimony, a powerful argument in favour of a stream having overflowed this country, superior in magnitude to any known river; and the facts seem to meet the challenge held out by Mr. Playfair.[16]

Hall mentioned an hypothesis, described by De Luc and attributed by him to a Mr. Wrede, that explained the large granite blocks found scattered over the land in northern Germany. It assumed that the land had at one time been covered by the Baltic Sea and that the granite blocks had been transported across from their place of origin, Scandinavia, "by means of the winds, on floats of ice, and settling in their present places, had been left by the retiring waters."[17] This hypothesis was modified by Hall to suit his diluvial theory. The blocks, he said,

> may have been brought to their present place, not by a permanent and steady position of the ocean, varying by slow degrees, as has been alleged by M. Wrede, but by a sudden diluvian wave washing over some district, situated either at a sufficiently high level, or near enough to the pole to be the seat of glaciers.[18]

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[16] Ibid., pp. 173-74.

[17] Ibid., pp. 145-46. The hypothesis was apparently proposed by Erhard Georg Friedrich Wrede (1766-1826) in his Geognostische Untersuchungen über die Sudbaltischen Lander, besonders über das untere Odergebiet . . . (Berlin, 1804). See Jean André De Luc, Geological Travels, I (London: F. C. and J. Rivington, 1810), 37.

[18] Hall, Transactions of the Royal Society of Edinburgh, VII (1815), 158.

The wave, he said, "would float and carry off all the ice in the glaciers, . . . and, along with the ice, all the blocks of stone imbedded in it, or attached to it in any way." The blocks may have come from the Alps, from Scandinavia, or elsewhere.[19]

The diluvial theory of Hall had no connection with the Biblical deluge. At least he gave not the slightest hint of such connection. He regarded his theory as a logical extension of Hutton's, for a deluge would be the natural consequence of the rapid uplift of a large body of land:

> I have no hesitation in declaring my hearty concurrence in what I consider as the essence of the Huttonian Theory; I mean as to all that relates to the influence of internal heat in the formation of our rocks and mountains: But I could never help differing from Dr. Hutton, as to the particular mode in which he conceived our continents to have risen from the bottom of the sea, by a motion so gentle, as to leave no trace of the event, and so as to have had no share in producing the present state of the Earth's surface.[20]

He appears to have considered Hutton's uniformitarianism as an unnecessary and arbitrary assumption that was inconsistent with the rest of his theory.

Many of the Huttonians seem to have adopted Hall's modification of Hutton's theory. Sir George Mackenzie, for example, concluded from his observations in Iceland that the earth's internal heat had been responsible for violent earth movements.

> Beside the filling up of the ocean, and the formation of land by such repeated operations of internal heat, this powerful agent seems to have raised vast masses of rock out of the sea, to a

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[19] Ibid., pp. 158-59.

[20] Ibid., p. 140.

> great elevation. . . . It may be just observed, that the aspect of the rocks of Iceland exhibits striking evidence of violent disruption, such as seems to have been the universal cause of the present uneven appearance of the surface of the globe. To consider the slow operations of the atmosphere, or rivers, as sufficient for shaping out huge mountains, and forming stupendous precipices, which are known to defy the most violent external attacks the destructive agents of nature can make, is a poor resource, either for those philosophers who can raise and sink the waters of the ocean as fancy may prompt, or for those who have seen the effects of the earthquake and the volcano, and can appreciate the power of subterraneous heat.[21]

Even Playfair appears to have wavered in his opinions, for in 1814 he wrote:

> Nothing is more certain, than that all the changes which we discover on examining the interior of the earth, are not to be ascribed to such slow operating causes as are now at work on the surface. Of this truth we are fully convinced, though we are perhaps disposed to ascribe much more to those causes than the French naturalist is willing to allow.[22]

Playfair's words: "The changes which we discover on examining the interior of the earth," may have reference to the evidence for large-scale convulsive movements along faults; and there are statements in his Illustrations indicating his belief that considerable violence had accompanied such movements.[23] He may not have altered his opposition to the employment of violent deluges to explain the present surface configuration of the earth.

The Huttonians in Great Britain who did not accept Hall's catastrophism gave up advocating their position, at least publically.

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[21]Sir George Steuart Mackenzie, Baronet, Travels in the Island of Iceland, During the Summer of the Year MDCCCX (2d ed; Edinburgh: Archibald Constable and Company, 1812), pp. 387-88.

[22][John Playfair], Review of Essay on the Theory of the Earth, by Cuvier, Edinburgh Review, XXII (1814), 459.

[23]See especially pp. 61-62.

Leonard Horner, a Huttonian, in his publications in the Transactions of the Geological Society was not given to much speculation; but he admitted, with respect to detached hills and accumulations of conglomerate, that "some powerful cause acting on the surface" had "left them in their present insulated position."[24]

There was in general a tendency to limit speculation in geology. Of the geologists who promoted this tendency, perhaps none was more vociferous than Thomas Thomson, editor of Annals of Philosophy, who said:

> Geology does not consist in speculating about the origin or creation of the earth, it consists simply in determining the number, nature, and position of all the rocks which constitute the surface of the earth; and till it is confined to this, it can never become a useful nor correct science.[25]

This tendency had become so pronounced by 1819 that Thomson could write:

> I abstain the more willingly from entering into any discussion respecting the theory of the earth . . . because the world in general seems now sensible of the unprofitable nature of such speculations. Even Professor Jameson, whose zeal burned for so many years with such furious ardour, . . . has ventured to call in question some of the most material parts of his master's geognosy; and if he exercise his own judgment without fetters for a few years longer, I venture to predict that he will not be a Wernerian at all. Even the Huttonians, those Calvinists of the science of geology, whose theory was so complete and so beautiful, if we took its foundation for granted, and were complaisant enough to overlook its inconsistency with the phenomena of nature--even they have become a great deal more tolerant; they no longer hurl their

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[24]Leonard Horner, "Sketch of the Geology of the South-Western Part of Somersetshire," Transactions of the Geological Society, III (1817), 384. Read March 3, 1815.

[25][Thomas Thomson], Review of the Transactions of the Geological Society, Vol. II, Annals of Philosophy, V (1815), 446-47.

> anathemas and their interdicts against their antagonists; they no longer affirm that mineralogy and geology are unconnected sciences, and that we may become profound geologists without any knowledge whatever of rocks or of minerals. On the contrary, they have exercised their industry with laudable zeal, and not only favoured us with descriptions of tracts of country themselves, but encouraged others to undertake similar tasks. Geologists in general seem now satisfied that the true object of their science is to acquire an accurate knowledge of the structure of the earth; that this knowledge can be acquired only by patient observation; that at present our knowledge of that structure is very incomplete; and that till the position of all the different strata over the whole surface of the earth be accurately ascertained, it would be a waste of time to speculate upon the original formation of these strata, or the changes which they have undergone since their original creation. . . . The splendour of such speculations is too apt to have irresistible attractions for a young and generous mind just starting in the arena, and eager to attract the attention of his fellows. But the fate of the numerous list of preceding writers in this tempting career, and the fate obviously impending over even the latest and best qualified adventurers, ought, I think, to be a warning. . . . the impending fate of Hutton, and even of Werner, is obvious and irresistible. Facts are eternal, speculations are palaces of ice glittering like gold and jewels, and built apparently of the most solid materials; but melting away before the rays of the sun, without leaving even a trace behind them.[26]

Thomson was even opposed to speculation on the deluge, considering the subject to be "of second-rate importance."[27] In this, however, he was opposing a powerful trend.

The religious implications of geology had long been of concern to many people. The Mosaic account of the creation, as given in the book of Genesis, states that the earth was formed in six days; and it spedifies a definite order in which the events of creation took

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[26]Thomas Thomson, "Historical Sketch of the Improvements in the Chemical Sciences During the Year 1818," Annals of Philosophy, XIII (1819), xci-xcii.

[27][Thomas Thomson], Review of A Critical Examination of the First Principles of Geology; in a Series of Essays, by G. B. Greenough, Annals of Philosophy, XIV (1819), 373.

place. Also in Genesis is the story of a widespread flood employed by God to destroy all human beings except the family of Noah.

Most "theories of the earth" in the seventeenth and eighteenth centuries had been more or less influenced by Genesis. As many of these theories in England had attributed most or all stratification to the effects of the Biblical deluge, the newer theories that either severely restricted the geological importance of the deluge, such as that of Werner, or denied its importance altogether, such as that of Hutton, disturbed many religious people.

Geological discussion in England about the deluge appears to have been intensified by the publication in 1813 of Cuvier's Essay, which strongly implied that the Biblical deluge had been responsible for many of the phenomena associated with the alluvium, It presented a diluvial theory that rapidly displaced the older theories of the deluge.

Cuvier was one of the first to show that the animals whose bones are found in the alluvium belonged to extinct species. In a paper of 1796, in which he stated that the fossil species of elephant differed from the existing ones, he suggested that these animals of extinct species lived in another world than ours, and that they were destroyed and replaced by members of the living species:

> is est probable qu'elles ont appartenu à des êtres d'un monde antérieur au nôtre, à des etres détruits par quelques révolutions de ce globe; êtres dont ceux qui existent aujourd'hui ont rempli la place, pour se voir peut-être un jour également détruits et remplacés par d'autres.[28]

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[28]Georges Cuvier, "Mémoire sur les espèces d'éléphans vivantes

These sentiments are similar to his later idea of successive creations.

In 1806, in summarizing his work on fossil pachyderms, Cuvier reached the following geological conclusions:

> Ces différens ossemens sont enfouis presque partout dans des lits à peu pres semblables; ils y sont souvent pêle-mêle avec quelques autres animaux également assez semblables à ceux d'aujourd'hui.
>
> Ces lits sont généralement meubles, soit sablonneux, soit marneux; et toujours plus ou moins voisins de la surface.
>
> Il est donc probable que ces ossemens ont été enveloppés par la dernière ou l'une des dernières catastrophes du globe.
>
> Dans un grand nombre d'endroits, ils sont accompagnés de dépouilles d'animaux marins accumulées; mais dans quelques lieux moins nombreux, il n'y a aucune de ces dépouilles: quelquefois même le sable ou la marne qui les recouvrent ne contiennent que des coquilles d'eau douce.
>
> Aucune relation bien authentique n'atteste qu'ils soient recouverts de bancs pierreux réguliers, remplis de coquilles marines, et par conséquent que la mer ait fait sur eux un séjour long et paisible.
>
> La catastrophe qui les a recouverts étoit donc une grande inondation marine, mais passagère.
>
> Cette inondation ne s'élevoit point au-dessus des hautes montagnes; car on n'y trouve point de terrains analogues à ceux qui recouvrent les os, et les os ne s'y rencontrent point non plus, pas même dans les hautes vallées, si ce n'est dans quelques-unes de la partie chaude de l'Amérique.
>
> Les os ne sont ni roulés ni rassemblés en squelette, mais épars et en partie fracturés. Ils n'ont donc pas été amenés de loin par l'inondation, mais trouvés par elle dans les lieux où elle les a recouverts, comme ils auroient dû y être, si les animaux dont ils proviennent avoient séjourné dans ces lieux, et y étoient morts successivement.
>
> Avant cette catastrophe, ces animaux vivoient donc dans les climate où l'on déterre aujourd'hui leurs os; c'est cette catastrophe qui les y a détruits, et comme on ne les retrouve plus ailleurs, il faut bien qu'elle en ait anéanti les espèces.
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> . . . . . .

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et fossiles," Mémoires de l'Institut National des Sciences et Arts: Sciences Mathematiques et Physiques, II (an VII, i.e. 1799), 21. Lu le premier pluviose an 4 (21 January, 1796).

> Ces résultats, déjà en grande partie indiqués dans l'article de l'éléphant, me paroissent tous regoureusement déduits des faits exposés dans cette primière partie.[29]

Cuvier distinguished two kinds of alluvium or "loose beds:" the older, which contained only extinct species, and the recent, which contained only existing species. After comparing the fossil bones of the extinct Irish elk and fossil deer with those of existing species of these types, he concluded that the two kinds of bones

> appartiennent à deux ordres de terrains, et par conséquent à deux époques géologiques différentes; que les uns ont été ensevelis, et le sont encore journellement dans la période où nous vivons; tandis que les autres ont été victimes de la même révolution qui a détruit les autres fossiles des terrains meubles, tels que les mammouths, les mastodontes et tous les pachydermes, dont les genres ne vivent plus aujourd'hui que dans la zone torride.[30]

Cuvier's Essay on the Theory of the Earth was hailed by many of the religiously orthodox as a scientific confirmation of the Mosaic narrative of the creation and the deluge. Jameson, the editor, said in his preface that, although the Mosaic account was an inspired writing and therefore rested on "evidence totally independent of human observation and experience," nevertheless it was "interesting, and in many

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[29] Georges Cuvier, "Résumé général de la première partie," in Recherches sur les ossemens fossiles de quadrupèdes, ou l'on rétablit les caractères de plusieurs espèces d'animaux que les révolutions du globe paroissent avoir détruites (4 vols.; Paris: Deterville, 1812), II, Chapitre XII, pp. 2-4 (the chapters are paged separately). This part was originally published as "Résumé général de l'histoire des ossements fossiles de pachydermes, des terrains meubles, et d'alluvion," Annales du Museum d'Histoire Naturelle, VIII (1806), 420-24.

[30] Georges Cuvier, "Sur les os fossiles de ruminans trouvés dans les terrains meubles," Journal de Physique, LXVIII (1809), 377.

respects important, to know that it coincides with the various phenomena observable in the mineral kingdom." The structure of the earth, he said, and the way in which fossils are distributed, "are so many direct evidences of the truth of the scripture account of the formation of the earth; and they might be used as proofs of its author having been inspired, because the mineralogical facts discovered by modern naturalists were unknown to a sacred historian." He suggested that there were "many physical considerations" which made it probable that a day may, at the time of the creation, have been "indefinitely longer" than it is at present, so that even "the six days of Mosaic description" were "not inconsistent" with geological theory. The deluge, he said, was "equally confirmed, with regard to its extent and the period of its occurrence" by geology, as was the recent origin of the human race. Cuvier's statement of the proofs of the deluge and of the recent population of the world, as well as his evidence against the idea of transmutation of species, "cannot fail," he said, "to admonish the sceptic, and afford the highest pleasure to those who delight in illustrating the truth of the Sacred Writings, by an appeal to the facts and reasonings of natural history."[31]

Adam Sedgwick, "On the Origin of Alluvial and Diluvial Formations," Annals of Philosophy, New Series, IX (1825), 241, wrote that "since the publication of Cuvier's great work on fossil quadrupeds," the distinction between the older and the recent alluvium "has been very generally admitted."

[31]Georges Cuvier, Essay on the Theory of the Earth, with Mineralogical Notes, and an Account of Cuvier's Geological Discoveries, by Professor Jameson, trans. Robert Kerr (Edinburgh: William Blackwood, 1813), pp. v-vi, viii-ix.

Thomas Chalmers, the Scottish Presbyterian theologian and minister,[32] wrote a review of Cuvier's work that was generally favorable.[33] "The appearance of the work," he said,

> has afforded matter of triumph and satisfaction to the friends of revelation, though, in these feelings, we cannot altogether sympathize with them. It is true that his theory approximates to the information of the book of Genesis more nearly than those of many of his predecessors. . . . This leads us to anticipate the period when there will be a still closer coincidence between the theories of geologists and the Mosaical history of the creation. It is well . . . that the chronology at least of Moses begins to be more respected; that a date so recent is ascribed to the last great catastrophe of the globe, as to make it fall more closely upon the deluge of the book of Genesis.[34]

He also favored the hypothesis of successive creations, a new creation of life after each revolution in the past, at which Cuvier had only hinted. Chalmers regarded it as "an argument for the exercise of a creative power, more convincing perhaps than any that can be drawn from the slender resources of natural theism."[35]

The book would not be liked, Chalmers said, by the "antemosaical philosophers" and "geological infidels" of the day. He chided them for ignoring the historical evidence for revelation and intruding science into the field of religion:

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[32]Thomas Chalmers (1780-1847), theologican, minister; prof. moral philosophy, St. Andrews College, 1823-28; prof. divinity, Edinburgh University, 1828-43; one of the founders of the Scottish Free Church, 1843.

[33]Thomas Chalmers, "Remarks on Cuvier's Theory of the Earth; in Extracts from a Review of That Theory Which Was Contributed to The Christian Instructor in 1814," in Miscellanies; Embracing Reviews, Essays, and Addresses (4 vols.; New York: Robert Carter, 1848), I, 180-93.

[34]Ibid., p. 180.

[35]Ibid., p. 188.

> while you so readily lift the cry against the unphilosophical encroachment of foreign principles into your department, you make no conscience of elbowing your own principles into a field which does not belong to them.[36]

Chalmers, however, had some criticisms of Cuvier's book. He thought it a weakness that Cuvier

> assigns no distinct cause for the earth's revolutions, and leaves us utterly at a loss about the nature of that impelling principle, which gives rise to the sweeping and terrible movements that are thought to take place in the waters of the ocean.[37]

He objected also, with reference to the deluge, that

> whereas Cuvier represents it to be an operation of so violent a nature as to agitate and displace everything that was movable--we guess, from the history, that an olive tree was still standing, and not lying loosely on the ground, with part of its foliage.[38]

Chalmers had too much respect for the Mosaic account of the creation to accept Jameson's stretching of the days to indefinite periods of time. The consistency of the account would be grossly violated if this were done, he said.[39] He admitted, however, the force of "the unanimity of geologists in one point,--the far superior antiquity of this globe to the commonly received date of it, as taken from the writings of Moses." This unanimity, he felt, could not be ignored:

> We may feel a security as to those points in which they differ, and, confronting them with one another, may remain safe and untouched between them. But when they agree, this security fails. There is no neutralization of authority among them as to the age of the world; and Cuvier, with his catastrophes and his epochs, leaves the popular opinion nearly as far behind him, as they who

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[36] Ibid., pp. 186-87.

[37] Ibid., pp. 180-81.

[38] Ibid., p. 187.

[39] Ibid., pp. 189-90.

> trace our present continent upward through an indefinite series of ancestors, and assign many millions of years to the existence of each generation.[40]

He, therefore, proposed allowing an indefinite period of time between "the first creation of the earth and the heavens," or what is called "the beginning" in Genesis, and the first of the six days. The first verse of Genesis, he said, announces the creation "in the beginning," but the second verse "describes the state of the earth (which may already have existed for ages, and been the theatre of geological revolutions) at the point of time anterior to the detailed operations" of the six days. In this case, he said,

> Moses may be supposed to give us not a history of the first formation of things, but of the formation of the present system; and as we have already proved the necessity of direct exercises of creative power to keep up the generations of living creatures; so Moses may, for anything we know, be giving us the full history of the last great interposition, and be describing the successive steps by which the mischiefs of the last catastrophe were repaired.[41]

Chalmers' interpretation of Genesis was soon adopted by William Buckland, who was born at Axminster, Devon, in 1784 and attended Corpus Christi College, Oxford, from which he received his B. A. in 1804. After graduation, Buckland stayed at Oxford on a scholarship. He attended the lectures of John Kidd on chemistry and mineralogy, which included some geology, and of Sir Christopher Pegge on anatomy. In 1809 he was elected a fellow of Corpus Christi College and took holy orders.[42]

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[40] Ibid., p. 191.

[41] Ibid.

[42] Mrs. Elizabeth Oke (Buckland) Gordon, The Life and

Buckland made geological tours in England in 1808 and 1810, and in 1813 he went with his friend William Conybeare to Ireland. Between 1812 and 1815 he made a number of journeys with Greenough in order to collect materials for the latter's geological map of England. Conybeare and Henry De la Beche also assisted in this work.[43] A letter to Conybeare, written in April 1814, reveals Buckland's characteristic humorous enthusiasm toward life with its reference to a prospective visit by Conybeare to Paris "to see Kings and Emperors, and Cuviers and Crocodiles." Buckland, in this letter, also evinced an interest in some notes that Conybeare had begun "touching Moses and Huttonianism," a natural interest for any clergyman-geologist.[44]

In 1814 Buckland succeeded Kidd as Reader in Mineralogy, but his lectures emphasized other aspects of geology. In 1815 he published "the first comparative table of the strata of England and those of the Continent, as arranged by Werner."[45] In this table Buckland introduced

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Correspondence of William Buckland, D.D., F.R.S., Sometime Dean of Westminster, Twice President of the Geological Society, and First President of the British Association (New York: D. Appleton and Company, 1894), pp. 1-7.

[43] Ibid., pp. 11-14.

[44] Ibid., p. 14.

[45] Ibid., pp. 14, 17-18, 22. The table mentioned here was apparently an earlier version of that appended at the end of William Phillips, A Selection of Facts From the Best Authorities, Arranged so as To Form an Outline of the Geology of England and Wales. With a Map and Sections of the Strata (London: William Phillips, 1818), entitled "Order of Superposition of Strata in the British Island," by Rev. W. Buckland, B.D. F.R.S. M.G.S. Fellow of C.C.C. and Professor of Mineralogy in Oxford. On the back of this folded sheet is a short list entitled "Order of Succession of the Secondary or Floetz Forma-

the term "diluvian detritus" to refer specifically to that portion of the alluvium produced by a deluge. He defined it as

> Sand Clay and Gravel composed of fragments both of neighbouring hills and of distant rocks, containing bones of the Elephant, Rhinoceros, Ox, Deer, Hippopotamus, &c. not mineralized. Locality: Every where in vallies, often on summits and slopes of hills and on elevated plains.

This later alluvium he referred to as "post-diluvian detritus" and divided it into two parts: "blown sand" and "fluviatile detritus."[46]

In his definition Buckland differed from Cuvier, for he included in the diluvian detritus, or diluvium, deposits that contained existing species of animals; and he severely limited the post-diluvian deposits to those unmistakeably related to present-day operations. Cuvier, on the other hand, distinguished the alluvium containing extinct species from that containing existing species and referred only the former to diluvial action. The tendency to attribute almost all of the alluvium to a deluge can be seen in William Phillips' definition of the alluvium in 1818:

> Alluvies, in the Latin, signifies a land-flood. This term, however, might seem to confine the cause that has produced ruin

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tions, as Sketched in a Hasty Manuscript List, Given by the Late Herr Bergrath Werner to Professor Buckland, at Freyberg, in July 1816," This list does not include the different coal and trap formations of Werner.

[46] Ibid. Joseph Townsend had earlier (1812) divided the alluvial deposits into "ancient alluvial, which may be called Diluvian" and recent alluvial. See Joseph Townsend, Geological and Mineralogical Researches, During a Period of More than Fifty Years, in England, Scotland, Ireland, Switzerland, Holland, France, Flanders, and Spain: Wherein the Effects of the Deluge Are Traced, and the Veracity of the Mosaic Account is Established. (Bath: Samuel Bagster, 1824), 252. This was first published in 1812.

> of so great extent to the ancient agency of almost universal floods. But it is certain that there exist accumulations. . . that result from causes even now operating.[47]

In 1819 the government was persuaded, through the representations of Sir Joseph Banks, President of the Royal Society, and other prominent persons, to endow a Readership in Geology at Oxford, to which Buckland was appointed.[48] His inaugural lecture, delivered on May 15, 1819, was later published by him under the title Vindiciae Geologicae.[49]

In his lecture Buckland said that science no longer threatened religion:

> When it was attempted to explain every thing by the sole agency of second causes, without any reference whatever to the first; when nature was set up as an original source of being, distinct and independent of the Almighty; when it was taught that matter possessed an existence which he never gave it, and that the elements had differences and qualities independent of him: these surely were grounds sufficient to excite alarm in all persons who were zealous for the cause of religion, and the preservation of the best interests of mankind. But the doctrines which gave Philosophy its formidable aspect have now been almost utterly abandoned: and if we will calmly allow reason to subdue the first alarm which excessive zeal excites in good and pious minds, it will teach us, that nothing can be more unjust than the apprehension lest the study of nature, when rightly pursued, . . . should in any way be destructive of the credibility of those things, which he has disclosed to us in the revelation of his will.[50]

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[47]Phillips, p. 1.

[48]Gordon, Life of Buckland, pp. 22-23.

[49]William Buckland, Vindiciae Geologicae; or the Connexion of Geology with Religion Explained, in an Inaugural Lecture Delivered before the University of Oxford, May 15, 1819, on the Endowment of a Readership in Geology by His Royal Highness the Prince Regent (Oxford: The Author, 1820). Cited hereafter as Buckland, Vindiciae Geologicae. Buckland, although he usually styled himself and was called "Professor," actually held the position of "Reader in Mineralogy and Geology," which is the way he is styled on the title page of this book.

[50]Ibid., pp. 27-28.

> . . . . . .
> if some writers on Geology . . . have professed to see in the earth nothing but the marks of an infinite series of revolutions, without the traces of a beginning; it will be quite sufficient to answer, that such views are confined to those writers who have presumed to compose theories of the earth, in the infancy of the science, before a sufficient number of facts had been collected; and that, if possible, they are still more at variance with the conclusions of Geology, (as a science founded on observation,) than they are with those of Theology.[51]

Science, and particularly geology, he said, can assist religion. For example, geology gives us evidence of the creative power of God:

> We argue thus--it is demonstrable from Geology that there was a period when no organic beings had existence: these organic beings must therefore have had a beginning subsequently to this period; and where is that beginning to be found, but in the will and fiat of an intelligent and all-wise Creater?[52]

By means of geology

> we trace the finger of an Omnipotent Architect providing for the daily wants of its rational inhabitants, not only at the moment in which he laid the first foundations of the earth, but also through the long series of shocks and destructive convulsions which he has caused subsequently to pass over it.[53]

When we perceive

> that the secondary causes producing these convulsions have operated at successive periods, not blindly and at random, but with a direction to beneficial ends, we see at once the proofs of an overruling Intelligence continuing to superintend, direct, modify, and control the operations of the agents, which he originally ordained.[54]

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[51] Ibid., pp. 21-22.

[52] Ibid., p. 21.

[53] Ibid., p. 12.

[54] Ibid., pp. 18-19.

Geology is, Buckland maintained, "in no way inconsistent with the true spirit of the Mosaic cosmogony":[55]

> The two great points . . . of the low antiquity of the human race, and the universality of a recent deluge, are most satisfactorily confirmed by every thing that has yet been brought to light by Geological investigations; and as far as it goes, the Mosaic account is in perfect harmony with the discoveries of modern science. If Geology goes further, and shews that the present system of this planet is built on the wreck and ruins of one more ancient, there is nothing in this inconsistent with the Mosaic declaration, that the whole material universe was created in the beginning by the Almighty.[56]

There were four hypotheses, he said, that had been suggested as reconciling geological appearances with scripture. The first of these: that the strata were all laid down by the deluge, he rejected because it did not allow enough time and because the deluge "is recorded in Scripture merely as a work of destruction."[57]

The second hypothesis supposed

> that these strata have been formed at the bottom of the antediluvian ocean during the interval between the Mosaic Creation and the Deluge; and that, at the time of that deluge, portions of the globe, which had been previously elevated above the level of the sea, and formed the antediluvian continents, were suddenly submerged with their inhabitants, while the ancient bed of the ocean rose to supply their place.

However, both scripture and natural appearances were against this interpretation:

> It should rather appear from . . . Scripture, that the antediluvian continents were the same with the present: and a similar conclusion is to be derived from the universal diffusion of the bones of land animals in those superficial depositions of gravel, which seem to have resulted from the deluge. . . . As these bones are

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[55] Ibid., p. 29.

[56] Ibid., p. 24.

[57] Ibid., p. 30.

> remarkably perfect, and seldom have signs of having been much rolled, or transported from a distance, they appear to have belonged to animals that lived and died near the spots where they are now found: these places consequently must have formed parts not of the ocean of the antediluvian world, but of its continents.[58]

Buckland said that he opposed this hypothesis "with diffidence, as it has received the countenance of very high authority."[59] Presumably he meant Cuvier, although Buckland's interpretation of this hypothesis was much different from that of Cuvier.

The hypothesis as Cuvier presented it was

> that the crust of our globe has been subjected to a great and sudden revolution, . . . which cannot be dated much farther back than five or six thousand years ago; that this revolution had buried all the countries which were before inhabited by men and by the other animals that are now best known; that the same revolution had laid dry the bed of the last ocean, which now forms all the countries at present inhabited; that the small number of individuals of men and other animals that escaped from the effects of that great revolution, have since propagated and spread over the lands then newly laid dry. . . .
>
> Yet farther,--That the countries which are now inhabited, and which were laid dry by this last revolution, had been formerly inhabited at a more remote era, if not by man, at least by land animals; that, consequently, at least one previous revolution had submerged them under the waters; and that, judging from the different orders of animals of which we discover the remains in a fossil state, they had probably experienced two or three irruptions of the sea.[60]

The third hypothesis was the hypothesis of Chalmers, which supposed that the word, "beginning," in the first verse of Genesis signified "an undefined period of time which was antecedent to the deluge and the creation of the present animals and plants." Buckland

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[58] Ibid., p. 31.

[59] Ibid., p. 32.

[60] Cuvier, Essay . . . (1813), pp. 171-72. This statement remained unchanged throughout all editions of the Discours, from 1812 to 1834.

indicated a preference for this explanation, although he raised no objection to the fourth hypothesis, which was that the "days" of the Mosaic creation were periods of a much longer extent than our present days.[61]

When Cuvier's hypothesis and the second hypothesis of Buckland are compared, the only similarity between them is found in the very point to which Buckland objected: that the present land was under the sea before the deluge. Chalmers had recognized that Cuvier in his book implied that the formation of the strata took place over an interval of time much longer than that between the Mosaic creation and the deluge.[62] Cuvier may have intended his first revolution (that which destroyed the animals found in the diluvium) to coincide with the time of the creation of the present plant and animal system, which was identified by Chalmers with the six-day creation of Genesis.[63] At any rate his hypothesis avoided certain difficulties inherent in any, like that of Buckland, which assumed that the antediluvian land was the same as that at present. The absence of human remains and those of animals of existing species in the diluvium Cuvier explained by assuming that the present land, or at least a large part of it, was under the sea during the sixteen hundred years between the creation of man and the present animal and plant species and the time of the deluge. This would be consistent with the account in Genesis

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[61] Ibid., pp. 31-42.

[62] Chalmers, p. 191.

[63] Ibid.

of the preservation of all animal species, which seems to imply that the same species existed before and after the deluge.

R. Hooykaas, in his book Natural Law and Divine Miracle says:

> Cuvier's doctrine of the fixity of species and his catastrophism were not founded upon his theology, but bore an exclusively scientific character: they agreed with the facts known to him and were grounded upon only these facts, and not upon texts from Scripture. His doctrine of catastrophes was not founded on Genesis (which, moreover, says nothing of a series of geological catastrophes), nor did he ever say that after each catastrophe God created a new fauna. . . . Perhaps Cuvier may have believed that there were new creations, but he did not assert it explicitly.
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> . . . . . .
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> Cuvier was remarkably free from unfounded speculation and prejudice, and always religiously tried to remain on a factual basis and to be up-to-date in his scientific explanations. . . . It would not be right to say that there was no relation between his science and his religion, but perhaps his religion was so firmly established that it did not need confirmation from science. Like Pascal and Newton . . . he was free from restraint in his scientific work; it was for the same reason that he found it so easy methodologically to separate science and religion.[64]

Cuvier's doctrine of catastrophes may not have been founded on Genesis, but his statements about the last catastrophe were certainly influenced by Genesis. The date, the universality, the suddenness of the flood, and the destruction of all but a few animals are features of his hypothesis that seem to have been suggested by Genesis. It is true that Cuvier was cautious compared to many other writers on this subject, and his work was praised by Fitton as being the best

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[64]Reijier Hooykaas, Natural Law and Divine Miracle: A Historical-Critical Study of the Principle of Uniformity in Geology, Biology and Theology (Leiden: E. J. Brill, 1959), pp. 197-98.

of the writings on the geology of the deluge and for "the general soundness of its reasoning, and the moderation of its tone."[65] But the fact that he implied that the Biblical and the geological deluges were identical violated what many scientists, like Fitton, considered to be the proper scientific attitude.

There is evidence that, even if Cuvier himself had religious beliefs "so firmly established" that they "did not need confirmation from science," the French Protestants, of whom he was a political leader, did need such confirmation for their beliefs. Charles Lyell reported a conversation in 1830 with Jean François d'Aubuisson de Voisins, the French geologist, in which the latter said:

> 'We Catholic geologists flatter ourselves that we have kept clear of the mixing of things sacred and profane, but the three great Portestants, De Luc, Cuvier, and Buckland, have not done so; have they done good to science or to religion?--No; but some say they have to themselves by it."

D'Aubuisson proceeded to comment on Buckland:

> 'Pray, gentlemen, is it true that Oxford is a most orthodox university?' Certainly. 'Well then, I make allowances for a professor there, dividing events into ante and post-diluvian: perhaps he could get no audience by other means.'

Lyell's comment on this reveals that he considered it common knowledge that Cuvier had adapted his views to conform to the religious prejudices of his fellow-Protestants:

> This attack against Buckland convinces me that the French Institute chose Oonybeare before Buckland, because they considered the latter as trading in humbug, which I am sorry to say is

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[65] [William H. Fitton], Review of Reliquiae Diluvianae, by William Buckland, Edinburgh Review, XXXIX (1823), 206.

> notoriously true of Cuvier, but not of Buckland, for although I am convinced he does not believe his own theory now, to its full extent, yet he believed it when he first started it.[66]

It was also suspected that Cuvier dissembled his true scientific views for reasons in addition to religious pressure. Lyell, in a letter to Darwin in 1863, wrote the following:

> Constant Prévost, a pupil of Cuvier's forty years ago, told me his conviction 'that Cuvier thought species not real, but that science could not advance without assuming that they were so.'[67]

It is therefore difficult to agree with Hooykaas that Cuvier was free from restraint in his scientific work and found it easy to separate science and religion.

To be sure, Cuvier made no explicit statement of a doctrine of successive creations, and in his Essay he even specifically denied that his statements implied it.[68] He did, however, make the following statements:

> In animal nature, therefore, there has been a succession of changes corresponding to those which have taken place in the chemical nature of the fluid.[69]

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[66]Charles Lyell, Letter to his sister (Marianne): Toulouse, July 9, 1830, Life, Letters and Journals of Sir Charles Lyell, Bart., Author of Principles of Geology &c., ed. Katherine M. Lyell (2 vols.; London: John Murray, 1881), I, 276. Cited hereafter as Lyell, Life, Letters and Journals. The French Institute had just chosen Conybeare as an honorary member.

[67]Lyell, Letter to Darwin: 53 Harley Street, March 15, 1863, Life, Letters and Journals, II, 365.

[68]Cuvier, Essay . . . (1813), pp. 125-26: "I do not pretend that a new creation was required for calling our present races of animals into existence. I only urge that they did not anciently occupy the same places, and that they must have come from some other part of the globe."

[69]Ibid., p. 13.

. . . . . .

Amidst these changes of the general fluid, it must have been impossible for the same kind of animals to live:--nor did they do so in fact.[70]

. . . . . .

Oviparous quadrupeds began to exist along with the fishes, and at the commencement of the period which produced the secondary formations; while the land-quadrupeds did not appear upon the earth till long afterwards.[71]

. . . . . .

The Mammalia, as they are the last, so they are the most perfect products of creative power.

The existence of Reptiles commenced much earlier.[72]

Such remarks were definite enough so that at least two Englishmen, Thomas Chalmers and John Fleming, attributed the idea of successive creations to Cuvier.[73]

Contrary to Hooykaas' view, Cuvier did not remain "up-to-date in his scientific explanations." His Discours remained virtually unaltered during a twenty-year period of rapid change in geologic thought, although he added some material on the historical evidences for a deluge. The book was a progressive force in geology at the

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[70]Ibid., pp. 12-13.

[71]Ibid., p. 108.

[72]"On the Osteology of Reptiles, and on the Geological Position of Their Fossil Remains," Philosophical Magazine, LXV (1825), 454. This is a translation of "Sur l'Osteologie des Reptiles, et sur la position geologique de leurs debris," in Cuvier's Recherches sur les ossemens fossiles, où l'on rétablit les caractères de plusieurs animaux dont les révolutions de globe ont détruit les espèces, V, 2d Part (2d ed.; Paris: G. Dufour et Ed. d'Ocagne, 1824), 8-9: "Les mammifères sont les derniers comme les plus parfaits produits de la puissance créatrice. Les reptiles ont commencé bien plutôt à exister.

[73]Chalmers, p. 188. John Fleming, The Philosophy of Zoology: or a General View of the Structure, Functions, and Classification of Animals (2 vols; Edinburgh: Archibald Constable & Co., 1822), I, 28.

beginning of this period, but it had a reactionary influence at the end.[74] Lyell, writing to Mantell in 1828, remarked:

> there is a cry among the publishers for an elementary work, and I much wish you would supply it. Anything from you would be useful, for what they have now is positively bad, for such is Jameson's Cuvier.[75]

Buckland, in his Vindiciae Geologicae, showed extreme deference to Cuvier, quoting him three times and referring to him one other time.[76] He called him "one of the most enlightened Philosophers, and the greatest Anatomist of this or any other age,"[77] and was hesitant about opposing his interpretation of the deluge. Cuvier was the only notable living geologist quoted by Buckland.

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[74]Cuvier's Discours was originally published as the "Discours preliminaire" to his Recherches sur les ossemens fossiles . . ., I (1812), 1-120, plus 20 p. appendix. The first three editions of the Essay were based upon it. A second edition, upon which the fourth edition of the Essay (1822) was based, was published in his Recherches . . ., I (1821), i-clxiv. The third edition, upon which the fifth edition of the Essay (1827) was based, was published separately under the title Discours sur les révolutions de la surface du globe, et sur les changemens qu'elles ont produits dans le règne animal. (3d ed.; Paris: G. Dufour et Ed. d'Ocagne, 1825). This edition, which was the most extensive revision of the work, contained large additions, principally upon the historical evidence for the deluge and the recent origin of the human race, but the changes made to the previous text were insignificant. There were a number of other editions before Cuvier's death (the 6th ed. is dated 1830), but they contained no significant changes.

[75]Lyell, Letter to Mantell: Temple, February 5, 1828, Life, Letters and Journals, I, 177.

[76]Buckland, Vindiciae Geologicae, pp. 5-6, 8, 24, 30.

[77]Ibid., p. 5.

Buckland also quoted the words of "the great master of modern science" and "that great Philosopher" Sir Francis Bacon:

> Let no man upon a weak conceit of sobriety or ill applied moderation think or maintain that a man can search too far, or be too well studied 'in the Book of God's Word,' or the 'Book of God's Works;' but rather let men endeavour an endless progress and proficiency in both: only let them beware that they apply both to charity, and not to swelling; to use, and not to ostentation; and again, that they do not unwisely mingle or confound these learnings together.[78]

He did not, however, refer to a more famous statement by Bacon, often quoted in geological works, about the folly of mixing religion and natural philosophy:

> For nothing is so mischievous as the apotheosis of error; and it is a very plague of the understanding for vanity to become the object of veneration. Yet in this vanity some of the moderns have with extreme levity indulged so far as to attempt to found a system of natural philosophy on the first chapter of Genesis, on the book of Job, and other parts of the sacred writings; seeking for the dead among the living: which also makes the inhibition and repression of it the more important, because from this unwholesome mixture of things human and divine there arises not only a fantastic philosophy but also an heretical religion. Very meet it is therefore that we be sober-minded, and give to faith that only which is faith's.[79]

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[78] Ibid., pp. 28-29. The quotation is from Bacon's Advancement of Learning, lib. 1.

[79] Francis Bacon, The Philosophical Works of Francis Bacon, Baron of Verulam, Viscount St. Albans, and Lord High Chancellor of England: Reprinted from the Texts and Translations, with the Notes and Prefaces, of Ellis and Spedding, ed. John M. Robertson (London: George Routledge and Sons Limited; 1905), p. 272 (Novum Organum, Book I, Aphorism LXV). The same passage is quoted, in Latin, in William Knight, Facts and Observations towards Forming a New Theory of the Earth (Edinburgh: Archibald Constable & Co., 1818), p. 325. Knight added: "This is a passage which cannot be too often remembered, and which, like many others in the same immortal work, is pregnant with the spirit of true wisdom. It seems, as Professor Playfair justly remarks, to have been prophetically addressed to De Luc, Kirwan, and the other cosmogonists of the present day, who

The endowment of a chair of geology at Oxford and the sentiments expressed in Buckland's inaugural lecture seem to have been the outcome of a movement among some of the clergy, principally of the evangelical, or "low church," party of the Church of England, to reconcile geological thought with revelation. It appears that they felt this would be of benefit to both the science of geology and to the Church. Geologists might then develop their science with the Church's blessing, and the Church would gain strength from the support that geology could give to some of its doctrines. The appearance of Cuvier's Essay in 1813 seems to have stimulated this movement, for here at last was a work by a prominent geologist that went far toward meeting what were felt to be reasonable conditions for a reconciliation. The idea of successive creations, implicit in Cuvier's thought, was seized upon as the key to the solution of the major difficulty between geology and revelation at this time: the question of time.

Probably the most important theological pronouncement upon this matter was contained in a work by the Rev. John Bird Sumner in 1816.[80] A leader of the evangelical party, Sumner became bishop of Chester in 1828 and archbishop of Canterbury in 1848. His position in the Church was made more secure by the fact that his brother,

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have done all in their power to degrade the Sacred Writings by the arguments which they have brought forward in their defence." (p. 326).

[80]John Bird Sumner, A Treatise on the Records of the Creation, and on the Moral Attributes of the Creator; with Particular Reference to the Jewish History, and to the Consistency of the Principle of Population with the Wisdom and Goodness of the Deity (2 vols.; London: J. Hatchard, 1816).

Charles Richard Sumner, was a favorite of the Prince Regent, who in 1820 became King George IV, and was appointed bishop of Winchester in 1827.[81] John Sumner was Buckland's major theological authority in his inaugural lecture.[82]

In an appendix to his book, entitled "That the Mosaic History is Not Inconsistent with Geological Discoveries," Sumner wrote that many of the terms in the Mosaic account of the creation and the deluge were not intended to be interpreted literally. Just as some expressions are inconsistent with modern theories of astronomy, so others may be inconsistent with geological theory. In both cases the expressions had been used because they were understandable to people at the time they were written:

> No rational naturalist would attempt to describe, either from the brief narration in Genesis or otherwise, the process by which our system was brought from confusion into a regular and habitable state. No rational theologian will direct his hostility against any theory, which, acknowledging the agency of the Creator, only attempts to point out the secondary instruments he has employed.[83]

According to the Mosaic history, Sumner said,

> we are bound to admit, that only one general destruction or revolution of the globe has taken place since the period of that creation which Moses records, and of which Adam and Eve were the

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[81]John Bird Sumner (1789-1862); Charles Richard Sumner (1790-1874). See articles on the two Sumners, Encyclopaedia Britannica (11th ed.; Cambridge, England: 1911), XXVI, 82-83.

[82]Buckland, Vindiciae Geologicae, pp. 25-27. The endowment of the chair in geology at Oxford was at the instigation of the Prince Regent, who reportedly was greatly impressed by Buckland's inaugural lecture.

[83]Sumner, I, 284.

> first inhabitants. . . . But we are not called upon to deny the possible existence of previous worlds, from the wreck of which our globe was organized, and the ruins of which are now furnishing matter to our curiosity.[84]

The idea of successive creations was thus pronounced to be consistent with Genesis. That is, there may have been creations before the last one, of which Moses speaks; and these may have involved other lands and other forms of life. This interpretation necessitated the assumption of an indefinite interval of time between the first and second verses of Genesis or included within the first verse in the word "beginning."

In return for this concession of time, which the geologists were unanimous in demanding, Sumner and Buckland wanted something from geology. Actually they, in this transaction, could appear to be magnamimous, for they were acceding to a compromise proposed by a geologist, Cuvier. The influence of Cuvier's work on Sumner is evident from the latter's reference to him as an authority.[85] Sumner stated that there were three articles in Genesis which affected geology and with which geological theory should be consistent. These were: 1. that God was the original creator; 2. that at the formation of the globe we inhabit, the whole of its materials were in a state of chaos and confusion; and 3. that at a period not exceeding five thousand years ago, the whole earth underwent a mighty catastrophe, in which it was completely inundated by the immediate agency of the Deity, and

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[84]Sumner, I, 284-85, as quoted in Buckland, Vindiciae Geologicae, 2627.

[85]Sumner, I, 280.

all its inhabitants destroyed except the remnant miraculously preserved.[86] Geologists were being asked, in effect, in return for the grant of sufficient but not unlimited time, to accept the idea of successive creations, which implied that there was a first creation, and to accept the identity of the geological and the Biblical deluges.

In the summer of 1819, after his inaugural lecture, Buckland started an active investigation into the geological evidence for the Mosaic deluge. This work resulted in a paper, which he presented to the Geological Society in December;[87] and it in 1823 resulted in a book.[88] His motives for embarking on this task seem to have been the need of fulfilling the expectations aroused by his inaugural lecture and of proving himself worthy of the position to which he had been

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[86] Ibid., p. 272.

[87] William Buckland, "Description of the Quartz Rock of the Lickey Hill in Worcestershire, and of the Strata Immediately Surrounding It; with Considerations on the Evidences of a Recent Deluge Afforded by the Gravel Beds of Warwickshire and Oxfordshire, and the Valley of the Thames from Oxford downwards to London; and an Appendix, Containing Analogous Proofs of Diluvian Action. Collected from Various Authorities." Transactions of the Geological Society, V (1821), 506-15, 516-37, 538-44. Read December 3, 1819. For a summary of the paper as read see Annals of Philosophy, XV (1820), 210-12. Further evidence of denudation by diluvial action was presented by Buckland and Conybeare in a paper, "On Coal Fields Adjacent to the Severn," Annals of Philosophy, XV, (1820), 212-15, 299-301, 450-54. This was read December 17, January 7, January 21, and March 17, 1819-20.

[88] William Buckland, Reliquiae Diluvianae; or, Observations on the Organic Remains Contained in Caves, Fissures, and Diluvial Gravel, and on Other Geological Phenomena, Attesting the Action of an Universal Deluge (2d ed.; London: John Murray, 1824). The first edition was published in 1823. The difference between the two editions is minor, the second edition containing some additional material in the footnotes. The second edition will be cited hereafter as Buckland, Reliquiae Diluvianae.

appointed. Perhaps, also, he wanted to enhance his reputation in the Church, if not among geologists, for it is doubtful that he improved his reputation among the latter group.

In his paper Buckland attributed the formation of valleys in great part to the action of the deluge. He claimed that "traces of diluvian action are most unequivocally visible in the features of every valley of the earth," although "we must not attribute the origin of them all exclusively to that action."[89] The only valleys whose formation he attributed to the erosive action of streams were "the deep and precipitous ravines which are produced by mountain torrents." The "magnitude and depth" of other valleys, he said, "bespeak the agency of a mass of waters infinitely more powerful than even the most violent water-spouts of modern times could produce." They must be attributed to the effect of the retiring waters of the deluge.[90] He admitted that valleys could have other causes than diluvial action. In some cases, he said:

> more especially in mountain districts, . . . the original form in which the strata were deposited, and the subsequent concretions to which they have been submitted, the fractures, elevations, and subsidences which have effected them, and their partial destruction at early periods by the violent actions of water, (of which the evidence is contained in the various beds of conglomerate that alternate with the secondary strata and transition rocks;) all these and perhaps many other causes may have contributed to produce vallies of various age and form upon the surface of the

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[89]Buckland, Transactions of the Geological Society, V (1821), 524 (footnote).

[90]Ibid., p. 523 (footnote).

> earth, before it was submitted to that last universal and recent deluge, which has finally modified them all.[91]

The shortness of the deluge, Buckland said, was proved by the incomplete roundness produced in the pebbles left by it. Even the softer pebbles had rarely "received that total and extreme degree of roundness . . . which is similar to what we now see produced by the long continued action of violently agitated water on fragments exposed to the waves on the sea shore."[92] Rounded pebbles found in the diluvium "received their attrition from the long continued action of violently agitated waters, during more early revolutions that have affected our planet."[93]

Among the contemporary authorities to whom Buckland referred for evidence of a recent deluge were Kidd, Conybeare, Farey, Hall, Greenough, and above all, Cuvier, who had given "the most enlarged and philosophical view of the state of the question that has even been taken."[94] These authorities, he said, "present us a mass of evidence . . . all conspiring to establish the important fact" of "a recent deluge acting universally and at the same period over the surface of the whole globe."[95]

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[91] Ibid., p. 524 (footnote). Note that he doesn't even mention ordinary stream action.

[92] Ibid., p. 529.

[93] Buckland, Reliquiae Diluvianae, p. 256.

[94] Buckland, Transactions of the Geological Society, V (1821), 534, 538-44.

[95] Ibid., p. 544.

Greenough, despite his scepticism on other matters, did not dispute the evidence for a recent deluge.[96] He interpreted the deluge as a violent universal flood, perhaps caused by a comet, that separated the present from the past order. He argued that the hypothesis of a single deluge was preferable to that of many on the scientific principle of economy.[97]

Conybeare's views on the formation of valleys by diluvial action were contained in his "Introduction" to the Outlines in 1822. The configuration of valleys, he said, "is exactly that which would necessarily be produced by the action of waters scooping out channels for their passage in draining themselves off from the face of a country." Although the original formation of valleys in mountainous districts must be attributed to "violent convulsions which appear . . . to have broken and elevated the strata,"

> there are the strongest proofs that even here also the vallies have subsequently been greatly modified by the rush of mighty currents of water through them; and in lower countries, where the horizontal and undisturbed position of the strata shew that other convulsions cannot very sensibly have affected the figure of the surface, we must refer its present inequalities almost exclusively to the excavating action of such currents.[98]

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[96]G. B. Greenough, A Critical Examination of the First Principles of Geology; in a Series of Essays (London: Longman, Hurst, Rees, Orme, and Brown, 1819), pp. 149-99.

[97]Ibid., pp. 151-52.

[98]W. D. Conybeare and William Phillips, Outlines of the Geology of England and Wales, with an Introductory Compendium of the General Principles of That Science, and Comparative Views of the Structure of Foreign Countries (London: William Phillips, 1822), pp. xxii-xxiii.

According to Conybeare, the hypothesis that valleys have been excavated by the long-continued action of the streams that now flow through them "must be abandoned at once by any one who will take the trouble of subjecting it to a rigorous application to the vallies of any extensive district." It was philosophically absurd and physically impossible:

> The advocates of this view imagine, that in a long lapse of ages the incessant action of this minute cause would be sufficient to account for the mighty effects observed; . . . yet even conceding that eternity, it is easy to shew that the phaenomena attendant on vallies are very commonly of such a nature, that to believe them to have been formed by their actual rivers, however long their action may have endured, involves the most direct physical impossibilities. . . It is indeed the more extraordinary that a cause so manifestly inadequate, should ever have been embraced, since the fundamental fact of geology, namely that the continents . . . were once covered with the ocean, . . . involves in itself the admission of a cause fully adequate; for, however that ocean may have been brought to its present level, it could never (on any view of the matter) have drained off the surface of the lands it has deserted, without experiencing violent currents in its retreat; and in those currents (the existence of which no one can on any hypothesis dispute) might have been found a force far more commensurate to the effects to be accounted for.[99]

The "proofs" of the inadequacy of the hypothesis, Conybeare said,

> are not merely the apparent disproportion between this cause and the effect to be accounted for, and the entire absence of any streams in many vallies (those of the chalky districts especially), but we find a still more decisive refutation in a phaenomenon of common occurrence,--the intersection of two series of vallies, the one extending longitudinally along the base of a chain of hills, and the other cutting transversely across that chain, under such circumstances that no stream could have risen to a sufficient height to form the transverse vallies by excavating a passage through the crest of the chain, but must have discharged its waters at a level far inferior to that required for this effect, through the longitudinal valley at its base.[100]

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[99] Ibid., pp. xxiii-xxiv.

[100] Ibid., p. xxvii.

At the end of his Vindiciae Geologicae, Buckland added an "Appendix, Containing a Brief Summary of the Proofs Afforded by Geology, of the Mosaic Deluge."[101] He had drawn up this summary of "the main reasons which confirm me in the opinion which I have always entertained" because of "an article which appeared in the Quarterly Review of May 1819, on Mr. Gisborne's Testimony of Natural Theology to Christianity."[102] This review, which actually appeared in January, 1819, was anonymous; but it is known to have been written by Thomas Dunham Whitaker.[103]

In his review Whitaker characterized Gisborne as "peculiarly unhappy and uninformed" on geology because he attributed all of the "dislocation and disruption" of the stratified rocks to the Biblical flood and objected to the hypothesis that our present earth was constructed from the materials of a former one. Gisborne contended that the supposedly extinct species found in the strata might still exist and that the stratified rocks with their fossil remains could have been produced in the "sixteen centuries and a half" that "elapsed

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[101]Buckland, Vindiciae Geologicae, pp. 35-38.

[102][Thomas Dunham Whitaker], Review of The Testimony of Natural Theology to Christianity, by Thomas Gisborne, Quarterly Review, XXI (January, 1819), 41-66.

[103]See Hill Shine and Helen Chadwick Shine, The Quarterly Review under Gifford: Identification of Contributors, 1809-1824 (Chapel Hill: University of North Carolina Press, 1949), p. 65. Thomas Dunham Whitaker (1759-1821) was vicar of Whalley, Lancashire, 1809-21, and held other benefices. He wrote a number of topographical works on various portions of England. Thomas Gisborne (1758-1846), was a minister of the Church of England and a writer on theological subjects.

between the Creation and the Deluge." Whitaker commented:

> Such is our respect for Mr. Gisborne's character, that we will not venture to pronounce this representation of the advanced state of geological knowledge designedly unfair, but we cannot forbear to say that it implies such a defect of information with respect to the latest discoveries on the subject, as must render the author, in the opinion of every well informed geologist, wholly incompetent to the task of writing or debating on the subject.[104]

Relying on Cuvier, whom he called "the last, and beyond comparison the most scientific writer on the subject," Whitaker presented the results of modern geology:

> Mr. Gisborne might have learned from every intelligent geologist of the present day, that in the formation and disposition of the principal strata of the earth, there appear none of those marks of confusion of which he so loudly complains and from which he infers so much; while, on the contrary, it is manifest that regular deposits have been made, and at successive periods evidently been superinduced upon each other; that in each of these are found, in undeviating order, the remains of different classes of animated things, beginning with the monads, the simplest of the living works of the Creator, and ascending through the scale to tribes of quadrupeds, in which the gradation closes without ever rising to man; --that between these successive deposits are indubitable vestiges of successive convulsions, equally formidable with those which dislocate and, if Mr. Gisborne will have it so, deform the present crust of the earth;--that in order to mineralize these successive deposits some chemical cause or causes must uniformly have been employed, which have had the collateral effect of destroying the animals whose nature and organs fitted them to exist upon the surface of the last deposit, and unfitted them for the next;-- and finally that these chemical causes, whatever they were, have ceased to operate, excepting in particular instances, and upon a very limited scale.[105]

What, he asked, can explain the existence of the fossil remains of so many shell fish,

> but the operation of some chemical and sudden infusion, which from that time forward rendered the medium in which they had been originally placed unfit for their further existence?"

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[104] [Whitaker], pp. 42-44.

[105] Ibid., pp. 52-53.

This, "in all probability," he said, "was the commencement of that process which reduced them from an animal to a fossil state."[106]

Whitaker believed that the Mosaic narrative of the creation was corroborated by the "wonderful coincidence betwixt that and the order in which organized animal remains are discovered in the successive strata"; but he refused to take literally the "days" in the account. Citing Linnaeus as his authority, he denied that the Biblical deluge had left any existing traces:

> To our author, probably, and to others, at the first view, this may appear a startling declaration; but let them recollect how few and of how small extent were the apertures necessary for the emission of subterraneous waters at Noah's deluge, and how little reason there is, from the account of Moses himself, for believing that the general surface of the globe underwent any material change in consequence of that catastrophe. The annihilation of the human race, with a few exceptions, was the object of God, and for that purpose an inundation, without these supposed convulsions, otherwise than as required for producing that inundation, was quite sufficient.[107]

The fact of the Biblical deluge was not the issue. It was rather the validity of science and reason itself:

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[106] Ibid., p. 48. The ideas that the animals were destroyed and the deposits containing them mineralized by some sudden chemical cause was common at this time. It was only vaguely hinted at by Cuvier, but it was accepted by William Smith, Stratigraphical System of Organized Fossils, with Reference to the Specimens of the Original Geological Collection in the British Museum: Explaining Their State of Preservation and Their Use in Identifying the British Strata (London: E. Williams, 1817), p. ix, and Greenough, pp. 274-75.

[107] [Whitaker], 53-55. These arguments and many others against the validity of the supposed physical traces of the Biblical deluge are to be found in Count de Buffon, "Proofs of the Theory of the Earth: Article V," in his Natural History, General and Particular, trans. William Smellie, ed. William Wood (20 vols.: London: T. Cadell and W. Davies, and others, 1812), I, 128-32. Whitaker didn't mention Buffon, perhaps because he was considered by many to have been an infidel.

> Every Christian philosopher admits the fact, nay more, he admits it on the faith of Revelation, corroborated by that very tradition. But with respect to the auxiliary evidence adduced by Mr. Gisborne's school, evidence deduced from existing appearances in the crust or on the surface of the earth, he takes leave to hesitate, perhaps to deny the consequences, He will say to Mr. Gisborne, We are agreed as to the fact of an universal deluge, and the force of the united proof from Scripture and tradition, by which it is established,--but when you require me to believe, on pain of being called an infidel, not only that every phenomenon, in or beneath the earth's surface, is solvable on that hypothesis, but on that alone,--nay, more, when I am obliquely threatened with the penalties of unbelief, unless I renounce all the lights which modern research and modern science have thrown upon a subject even yet comparatively new; when in every stratum and every fissure of the earth, I meet with appearances, which, according to my apprehension, negative such an hypothesis, in the use of my senses and in the operations of my understanding, I will no more be intimidated by a bigot, than by an inquisitor, I cannot accept of loose declamation for irrefragable argument, nor unwarranted assertion for legitimate proof,--I am not disposed to believe, that in a world, constituted of elements like the present, subterraneous fires could not be kindled, nor steam expand, nor earthquakes rend the surface, nor volcanos burst forth from its bowels, till their several principles were put in action by the sin of man. . . .
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> . . . . . .
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> sincerely do we hope, for the sake of revealed religion itself, that Mr. Gisborne will be the last Christian writer who shall attempt to shew that the present appearances on, and immediately beneath, the surface of our earth, can only have been occasioned by the Noachian deluge. The maintainers of a contrary opinion have been, by our author, very unskilfully, and with as little distinction as charity, accused of infidelity. On this subject, it is fitting that he should be better informed. These persons then are, with Mr. Gisborne's permission, to be divided into two classes: the first, consisting of those who doubt or deny the reality of the Noachian deluge; and the second, among whom we desire to be numbered, of those who cordially accept the evidence of Moses, corroborated as it is by universal tradition, for the certainty of that astonishing event, while they descry no certain and ulterior confirmation of it, in the present appearance of the globe.[108]

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[108][Whitaker], pp. 64-66. He added: "we pretend not to deny that any of the clefts and fissures on the earth's surface, <u>may</u> have been among the causes of the flood: we neither dogmatize with Mr. Gisborne, nor deny with infidels."

Whitaker's theological position was a strong one, and reinforced as it was by the Baconian awareness of the dangers of mixing science and religion, it appealed to many geologists at this time. It must be noted that Whitaker was opposed only to the idea that there existed physical traces of the deluge. He firmly believed that the succession of organic remains in the stratified rocks was consistent with the Mosaic history of the creation and that this was proof of its inspiration.[109]

Buckland agreed with Whitaker's criticism of Gisborne's geological errors, except with respect to the evidence for the deluge:

> There is, however, one point of vital importance, on which it is sufficiently apparent, from the preceding Lecture, that I entirely differ from the writer of this Review, namely, in the belief he entertains, on the authority of Linnaeus, that Geology affords no proofs of the Mosaic Deluge.

He stressed that he was criticizing Whitaker on scientific and not theological grounds:

> this difference may be the more securely stated, as the general attachment of the Quarterly Review to the cause of Revelation is so decided; and as the very paper in question contains the strongest assertions of the truth of the Mosaic History: it is simply therefore a matter of science, on which our opinions are at variance.

How, he asked, could Whitaker prefer the opinion of Linnaeus to that of Cuvier on this question?:

> I am at a loss to conceive how any person who has evidently read the works of Cuvier with so much attention as the writer of this Review . . . could have been induced to revert to the premature opinion of so infantine a Geologist as Linnaeus, and have overlooked that most important conclusion which I have before quoted,

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[109] Ibid., pp. 53, 66.

in which Cuvier himself sums up the results of his own valuable observations.[110]

The trend in geology had been for some time to refer many phenomena, which formerly had been attributed to the deluge, back to a time previous to it. This tendency to deny the evidence for the deluge had made many theologians uneasy. Some had attacked geologists as infidels, and others had adapted their theology to the new state of affairs by minimizing the importance of the question. Buckland believed that this tendency had gone too far and that there did exist physical evidence for the deluge, which must be sharply distinguished from the evidence for previous revolutions. "It is from the want of accuracy in distinguishing between these facts," he said, "that errors have prevailed, such as those into which Linnaeus fell."[111]

Buckland's attempt to identify the geological and the Biblical deluges brought an immediate reply from John Farey, in the form of a letter to the Philosophical Magazine.[112] Farey deplored Buckland's revival of diluvial geology:

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[110]Buckland, Vindiciae Geologicae . . ., pp. 35-36.

[111]Ibid., p. 36.

[112][John Farey], "Reflections on the Noachian Deluge, and on the Attempt Lately Made at Oxford, for Connecting the Same with Present Geological Appearances," Philosophical Magazine, LVI (1820), 10-14. Dated July 6, 1820. This letter is anonymous (it is signed "A.B.C.") but it was undoubtedly written by Farey, an inveterate writer of letters, signed and unsigned, to the Phil.Mag. and other journals during this period. His style is very distinctive, and his mention of some of his previous letters and a reference to William Smith (of whom Farey was the most outspoken champion) leave no doubt as to the authorship.

> I remember having seen Mr. Bakewell commended in your Work, for having in the year 1813 abstained, from introducing the Deluge of Moses into his "Introduction to Geology," as the previous Writers had almost invariably done, to the manifest injury of Geology on the one hand, and of Religion on the other: since which, the practice has almost entirely grown into disuse, while the number of writers on Geological subjects, have been greatly on the increase; and I regret therefore to see, the new Geological Professor at Oxford, attempting now to revive the exploded notion, that any of the phaenomena at this time visible, on or within the Earth, are, with any proper regard to probability, referable to the Deluge of which Moses writes.[113]

He did not deny that violent deluges had occurred in the past, long before the Noachian deluge. The strata had undoubtedly been denuded and valleys excavated by some unknown cause. This had been followed by a succession of "gravel Floods," which he described as "most violent and over-whelming," and which apparently had occurred almost universally. These events had occurred long before the deluge of Noah, and indeed before the creation, described by Moses, of the present animals and plants and of man. They had occurred during the period when "the creative power of the Deity modified and gave immediate impulse, to such of the chemical or mechanical laws, as were

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[113] Bakewell had indeed been commended--by Farey! See John Farey, "Notes and Observations on the Introduction and Three First Chapters, of Mr. Robert Bakewell's 'Introduction to Geology;'--Embracing Incidentally, Several New Points of Geological Investigation and Theory," Philosophical Magazine, XLII (1813), 247, where Farey says: "By producing a work, which on the one hand no where shocks us by its impiety, in setting up mistaken phaenomena of the Earth and false hypotheses regarding it, against those Revelations which have obtained the assent of the largest portions of civilized men; and on the other hand, has excluded those futile and mischievous attempts at supporting Revelations and Miracles, by inapplicable natural phaenomena, by supposed present evidences of the Deluge of Noah, in particular; on these grounds I consider Mr. Bakewell as having performed most important services, to Science and to Religion, at the same time."

then in operation, in framing or changing the appearances of the Earth."[114]

The bones of extinct animals sometimes found in the gravel, "are generally so found," Farey said,

> in Valleys and low Places, amongst Gravel which has been removed, by far less and more local Floods, than the General Gravel Floods above spoken of.
>
> These lesser Floods that buried Bones, seem with great probability to have happened, in the interval between the Creation of Animals, (as related by Moses, allowing, with all sensible Commentators, that not Days, literally, but long and indefinite Periods were by him assigned, to the great and multitudinous work of creating, the progenitors of the present Animals and Plants) and the last and finishing work of the Creator, in placing Man upon the Earth; which seems to have immediately preceded the ordaining of those laws of Nature, as we call them, which have since carried on the system of the Universe.[115]

The Noachian deluge, which occurred long afterward, left no permanent traces:

> Now the mistake of Professor Buckland, and of all those who have preceded him, in referring these tumultuous events, to the Deluge happening in the days of Noah, consists, in not having carefully considered the words used by Moses in describing the Noachian Deluge, which if they had done, instead of taking on trust, the absurd interpretations of those words, or rather the fabrifications [sic] which were framed by Dr. Woodward and many other writers of the two last Centuries, the Professor must, by this examination of Moses' words, have found, that the same, throughout, refer to a quiet effusion of Water upon the surface of the Earth, for the avowed purpose and for no other, but that of drowning the degenerate race of Mankind, whose crimes and violences had filled the Earth; and that in point of fact, according to Moses, the surface of the Earth, was not torn up or moved, so as in any material degree to disturb and root up the Vegetable races!; nor did it annihilate any of the race of Fishes, not even the most torpid and helpless of the species of Shell-Fish! The vegetable earth or Mould, fit for the growth of useful plants (the evidently slow result of long periods of decomposition, and the accumulation of decayed vegetable matters) was not, according

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[114] [Farey], Philosophical Magazine, LVI (1820), 13.

[115] Ibid., p. 12.

> to Moses, either washed away, or covered, by naked and fresh-moved Rubbish, because Noah on quitting the Ark, or very soon after, planted a vineyard!
>
> Whereas, the Gravel Floods which the Professor has laboured to identify with this Noachian Deluge, must, undoubtedly, have left the entire surface of the earth, as utterly unfit for the immediate reception and support of Men, and of granivorous Animals, or even of Plants, as the Sea Beach and Sands now are, on which the Tide and Waves of the Ocean daily lash: besides which, the Bones of Men, and more especially their implements and works of art, ought to be found buried in or under the gravelly mixtures, if such had in reality been moved by the Noachian Deluge, which is described as having extinguished a full, if not a crowded population: whereas no such Remains, or any other evidences of Man's existence upon earth, prior to these Gravel Floods, are any where found.
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> If also, the Noachian and the Gravel Floods had been identic, the Animal Bones buried in the Gravel ought, in all cases to correspond exactly, with the present races of Animals, since these last, are the descendants by procreation, of the very race, out of which, according to Moses, Noah selected his pairs of Animals, for again replenishing the Earth, after the Deluge.[116]

It would appear that any notion that Buckland's theory of the deluge was the product of Bibliolatry is much too simple. The temptation for Buckland and his supporters to equate the geological and the Biblical deluges was too strong to be bothered by the many dissimilarities in detail between the two. The immediate benefits to be gained by being able to support Christian belief by the science of geology seemed to them to outweigh the possible long-run consequences should this attempt to upset the precarious truce among geologists on this subject ultimately fail.

In the summer of 1821, a cave containing the bones of extinct animals was discovered at Kirkdale, Yorkshire, by some workmen in a limestone quarry.[117] Unfortunately, between the discovery and December,

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[116] Ibid., pp. 11-12.

[117] Buckland, Reliquiae Diluvianae, p. 6.

when Buckland arrived, the cave was visited by many people, and most of the bones removed or disturbed, so that he had to examine many of the bones away from the cave, in private collections.[118] The cave was not large, being no more than about six feet wide and three feet high, with a length of about two hundred feet.[119]

Buckland had investigated other caves, but none so rewarding as this one. Here he found, as he told the Royal Society on February 21, 1822, "one of the most complete and satisfactory chains of consistent circumstantial evidence I have ever met with in the course of my geological investigations."[120] The discovery of such caves was not uncommon in England, and the Royal Society had heard reports on two other bone-caves, both at Oreston, near Plymouth, in 1817 and 1821. However, their discoverer, Joseph Whidbey, had not drawn the conclusions that Buckland had from his cave at Kirkdale.[121]

The Copley Medal of the Royal Society for 1822 was awarded to Buckland for his investigation of the Kirkdale cave. Sir Humphry

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[118] Ibid., pp. 14-18, 29-30.

[119] Ibid., Plate 2, Figure 3.

[120] William Buckland, "Account of an Assemblage of Fossil Teeth and Bones of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyaena, and Sixteen Other Animals; Discovered in a Cave at Kirkdale, Yorkshire, in the Year 1821: with a Comparative View of Five Similar Caverns in Various Parts of England, and Others on the Continent," Philosophical Transactions of the Royal Society of London (1822), p. 171. Read February 21, 1822.

[121] Sir Everard Home, "An Account of Some Fossil Remains of the Rhinoceros, Discovered by Mr. Whitby, in a Cavern Inclosed in the Lime-stone Rock, from which He Is Forming the Break-water at Plymouth," Philosophical Transactions of the Royal Society of London (1817), pp. 176-82. Joseph Whidbey, "A Farther Account of Fossil Bones Discovered

Davy, president of the Royal Society, on awarding the medal praised Buckland's work and commented on its importance in establishing that the animals whose bones were found in the cave had actually been inhabitants of the surrounding country. He argued for the hypothesis that the climate had been much warmer previous to the deluge and discussed the relation of the subject of geology "with that of the chaotic state of the globe, and with those of the successive creations of living beings, and the early revolutions of our planet, until it became at last fitted for the habitation of man." He stated that

> the scriptural account of the deluge was now completely established from geological grounds; but the science of geology, he maintained, should be studied in a manner altogether independent of the authority of the Sacred Scriptures; for that these, as Bacon had said long before, merely gave some remarkable facts in the history of the globe, and not systems of philosophy;--the latter were left to be framed by the industry of man, and by the exercise of his god-like faculty of reason, which, in its highest sphere, approximates to revelation itself.[122]

The cave at Kirkdale, Buckland concluded, had been inhabited by hyenas over a long period of time.[123] This was the only conceivable hypothesis, he felt, that could explain: 1. The large numbers of hyenas' bones.[124] 2. The great variety of animal species represented

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in Caverns Inclosed in the Lime Stone Rocks at Plymouth," Philosophical Transactions of the Royal Society of London (1821), pp. 133-35.

[122]See a "condensed epitome" of Davy's speech given in the proceedings of the Royal Society, Annals of Philosophy, new series, V (1823), 64-65. The award of the Copley medal was made on November 30, 1822.

[123]Buckland, Reliquiae Diluvianae, p. 19.

[124]Ibid., p. 17.

by the bones, which included, besides the hyena, the water rat, fox, rhinoceros, deer, elephant, tiger, bear, wolf, weasel, hippopotamus, horse, ox, hare, rabbit, mouse, raven, pigeon, lark, snipe, and duck. These bones, most of which were of extinct species, had presumably been dragged to the cave by the hyenas.[125] 3. The broken and extreme fragmentary condition of many of the bones.[126] 4. The presence of balls of what appeared to be the excrement of hyenas.[127] 5. The great preponderance of teeth, which Buckland assumed had been rejected as inedible by the hyenas.[128]

If the cave had been a hyena's den, Buckland said, it furnished undeniable evidence that "there was a long succession of years in which the elephant, rhinoceros, and hippopotamus had been the prey of hyaenas, which, like themselves, inhabited England." Since these same extinct species were also found in the diluvium, this period was immediately prior to the deluge:

> M. Cuvier has . . . ascertained that the fossil elephant, rhinoceros, hippopotamus, and hyaena, belong to species now unknown; and as there is no evidence that they have at any time, subsequent to the formation of the diluvium, existed in these regions, we may conclude that the period, at which the bones of these extinct species were introduced into the cave at Kirkdale, was antediluvian. Had these species ever re-established themselves in the northern portions of the world since the deluge, it is probable that their remains would have been found, like those of the ox, horse, deer, hog, &c. preserved in the postdiluvian

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[125] Ibid., p. 15.

[126] Ibid., pp. 10-12.

[127] Ibid., p. 20.

[128] Ibid., pp. 16-17.

> accumulations of gravel, sand, silt, mud, and peat, which are referable to causes still in operation, and which, by careful examination of their relations to the adjacent country, can be readily distinguished from those which are of diluvian origin.[129]

It followed that "they also inhabited all those other regions of the northern hemisphere in which similar bones have been found under precisely the same circumstances." That is, they ranged over a great part of northern Europe, North America, and Siberia.[130]

The bones, which covered the floor of the cave, were imbedded in a layer of mud. In many places, particularly near the walls, this mud was covered by a layer of stalagmite.[131] From the appearance of the cave, Buckland made the following chronological inferences:

1. There was a period during which the cave "existed in its present state, but was not tenanted by the hyaenas." This period, "if we may form an estimate from the small quantity of stalagmite now found on the actual floor of the cave," was "a very short one." Since most of the floor was covered by mud, he admitted that his estimate of the quantity of stalagmite was only approximate; but "it cannot be very great," he insisted.

2. The next period was that in which the cave was inhabited by the hyenas. That the stalagmite was still forming at this time was proved by the existence of bones consolidated in a matrix of stalagmite. During this period no mud was introduced, for no alternation

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[129] Ibid., pp. 41-42.

[130] Ibid., p. 43.

[131] Ibid., pp. 10-11.

of the mud with beds of bone or stalagmite was found.[132]

3. The third period was that in which the mud was introduced, the period of the deluge. The cave was abandoned by the hyenas before the deluge as no complete skeletons were found. The bones lay principally in the lower part of the mud, consistent with the idea that the mud was introduced after the bones. The mud could not have been the result of local floods, because the cave in Buckland's time remained dry even after the greatest rains and was located eighty feet above the bed of the stream below it, or far higher than the stream could possibly rise.[133]

4. The fourth period was that during which the stalagmite above the mud was deposited. The quantity of this stalagmite was much greater than that formed in periods one and two. After stating that there was no way of distinguishing the relative quantities of stalactite formed on the top or sides of the cave during the various periods, Buckland, nevertheless, argued that "the limited quantity of post-diluvian stalactite, as well as . . . the undecayed condition of the bones" showed that

> the time elapsed since the introduction of the diluvial mud has not been one of excessive length, nor at all exceeding that which M. Cuvier . . . infers to have elapsed since that great and universal inundation which has overwhelmed the earth, at a period, which, he says, . . . cannot have exceeded five or six thousand years ago.[134]

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[132] Ibid., pp. 48-49.

[133] Ibid., pp. 6, 49-50

[134] Ibid., pp. 50-51. He apparently intended to say "stalagmite," instead of "stalactite" here, for there was no way of determining what part of the stalactite was post-diluvian.

Buckland's paper in the Philosophical Transactions was reviewed in the Quarterly Review for July, 1822.[135] The name of the writer of the review is not certain, but there are several lines of evidence that point to John Barrow[136] as the author: Barrow wrote regularly for the Quarterly Review; the review announced that another cave had been discovered near Plymouth and that Buckland had gone to investigate it;[137] Buckland later wrote that Barrow had been the first to inform him about the discovery of the cave;[138] the discovery was reported to the Royal Society by the reading of a letter from Joseph Whidbey, the discoverer, to John Barrow, dated 19th August, 1822;[139] the review mentions an incident that occurred on Captain Parry's arctic expedition; which is consistent with Barrow's known interest in arctic exploration.[140]

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[135][John Barrow], Review of "Account of an Assemblage of Fossil Teeth and Bones . . .," by William Buckland, Quarterly Review, XXVII (1822), 459-76. This was not published until October, 1822 (See Shine, p. 79).

[136]John Barrow (1764-1848), second secretary of the admiralty, 1804-06, 1807-45; baronet, 1835; founder of the Royal Geographical Society; wrote works on exploration, especially in the arctic; influential in getting the navy to undertake voyages of geographical discovery and exploration.

[137][Barrow], Quarterly Review, XXVII (1822), 470-71.

[138]Buckland, Reliquiae Diluvianae, p. 68.

[139]Joseph Whidbey, "On Some Fossil Bones Discovered in Caverns in the Limestone Quarries of Oreston. In a Letter Addressed to John Barrow, Esq. F. R. S. To Which Is Added, a Description of the Bones by Mr. William Clift, Conservator of the Museum of the College of Surgeons," Philosophical Transactions of the Royal Society of London (1823), pp. 78-90. Read February 6, 1823. The letter was dated Phymouth, 19th August, 1822.

[140][Barrow], Quarterly Review, XXVII (1822), 473.

Barrow, assuming him to have written the review, stated that geology had put aside "insane and visionary 'theories'" concerned with the creation of the earth and that it no longer pretended "to penetrate into the causes that produced the various revolutions which the earth has obviously undergone." He was lavish in his praise of Werner:

> The indefatigable and accurate Werner may be considered as the father of geology. It was he who first observed the particular distribution of petrified plants and animals in particular species of rocks. . . . It was he who first affirmed . . . that the more recent the formation the nearer do they approach to the now existing species, till those found in the latest alluvial deposits, become identical with them.[141]

That his geological ideas were derived in large part from Cuvier's Essay is evident from his frequent references to the book and from the fact that his statements about Werner were almost identical with those made by Jameson in his notes to Cuvier's work.[142]

The review was quite favorable to Buckland. Barrow rejoiced to find that

> those very circumstances which the ignorant and flippant sciolists of the last age employed against the authenticity of the Sacred Writings, are those which geology has brought forward as the most splendid and incontestible proofs of their veracity.[143]

He went on to discuss the evidence for a warmer climate in the past, a question upon which Buckland had refused to express an opinion. Like Davy, Barrow thought a warmer climate very probable and attributed the

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[141] Ibid., pp. 460-61.

[142] Cuvier, Essay (1st ed.; 1813), pp. 225-27.

[143] [Barrow], Quarterly Review, XXVII (1822), 476.

change in climate to a change in the position of the earth's poles or in the inclination of its axis to the plane of its orbit, which also would explain, he said, the revolutions the earth had undergone. "The old theory of internal heat, and gradual cooling of the globe, long since exploded," he said,

> has been revived, to account for the phenomena in question; but the arguments built on a foundation so unstable would lead to conclusions so absurd and unphilosophical, that, in our opinion, they are not worth pursuing.[144]

As if he were replying to Barrow, Buckland interpolated a discussion of the climate of the past into his account of the Kirkdale cave when he revised it for his Reliquiae Diluvianae. Refusing to commit himself on the question, Buckland gave the evidence for both sides and said that all opinions must be premature.[145] His cautious attitude was very likely caused by Cuvier's opposition to the hypothesis of a hotter climate. Buckland said that his book was concerned only with establishing that there had been a recent and general inundation and that the animals whose remains were found in the diluvium were "natives of high north latitudes and not drifted to their present place from equatorial regions."[146]

Buckland was, however, by this time fairly well committed to the hypothesis of hotter climates. It fitted well with the idea that the deluge represented a radical change in the order of things.

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[144] Ibid., p. 474.

[145] Buckland, Reliquiae Diluvianae, pp. 44-47.

[146] Ibid., p. 47.

His fondness for the hot-climate hypothesis is revealed in the following statement;

> One thing, however, is nearly certain, viz. that if any change of climate has taken place, it took place suddenly; for how otherwise could the elephant's carcase, found entire in ice at the mouth of the Lena, have been preserved from putrefaction till it was frozen up with the waters of the then existing ocean? Nor is it less probable that this supposed change was contemporaneous with and produced by, the same cause which brought on the inundation.[147]

The idea of a sudden chill associated with the deluge is also present in a footnote referring to the discovery in Bering's Strait of a mass of ice one hundred feet high:

> An undoubted proof of this ice being primitive (i.e. not formed by any causes now in action), was afforded by the great number of bones and teeth of mammoths which make their appearance when it is melted.[148]

Buckland stressed, however, that the cause of the deluge, whether "a change in the inclination of the earth's axis, or the near approach of a comet," or any other astronomical cause, was foreign to the object of his work.[149]

After discussing other caves in England and finding the phenomena in them in general agreement with his theory, Buckland considered the caves of Germany, which, unlike those of England, were well known and had been the subject of a considerable literature.[150]

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[147] Ibid., p. 47.

[148] Ibid., p. 46

[149] Ibid., pp. 47-48.

[150] Ibid., pp. 99-104.

These caves were principally in the Harz Mountains and in Franconia, between Nürnburg and Bayreuth.[151]

In all of the German caves, with the exception of the cave of Kühloch in Franconia, Buckland found essentially the same phenomena as he had at Kirkdale: a layer of mud covered by a layer of stalagmite. Not all of the caves contained bones, those that did appeared to have been the dens of bears, as the bones of two extinct species of bears comprised at least three-quarters of the bones. The bones of the hyena, elephant, and rhinoceros were sometimes found, and Buckland assumed that either all were carried into the caves by the deluge or the elephant and rhinoceros bones were dragged in by stray hyenas.[152] Many of the caves contained a number of chambers at different levels, and in these the greatest quantity of mud and bones was usually concentrated at the lowest level, indicating that the diluvial waters had carried them downward from the higher levels. The mud often contained rounded pebbles, a further indication of the violence of the flood waters.[153]

From this evidence Buckland concluded that: 1. "The agent, by which the mud and pebbles were introduced, was the same diluvial waters, which extirpated the animals that had antecedently inhabited the cave."[154] The mud in the caves was similar to the diluvium:

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[151] Ibid., p. 104.

[152] Ibid., pp. 105-06.

[153] Ibid., pp. 136-37.

[154] Ibid., p. 121.

> its perfect agreement with the diluvial loam that abounds on the surface of the adjacent countries, added to the fact of the materials within the cave being often sorted, or drifted, as if by water into distinct deposits of loam, and sand, and pebbles; and the still more irresistible argument, arising from the almost universal presence of the pebbles themselves, renders it impossible to refer the earthy matter in question . . . to any other origin, than one violent movement of waters over the land without.[155]

With regard to the caves and fissures of Germany and England, he said, we are led to infer:

> an identity in the time and manner in which these earthy deposits were introduced; and this identity is still further confirmed by the agreement in species, of the animals whose remains we find enveloped by them, both in caves and fissures, as well as in the superficial deposits of similar loam and pebbles on the surface of the adjacent countries. . . . hence it follows, that the period at which the earth was inhabited by all the animals in question was that immediately antecedent to the formation of those superficial and almost universal deposits of loam and gravel.[156]

2. "This diluvial detritus was not introduced at different intervals by the action of rivers, or land-floods, but was by one single operation superadded to the bones already existing in the dens."[157] This was proved by the fact that the mud and pebbles were mixed evenly with the bones in the deepest recesses of the caves, indicating that the bones were stirred up by the diluvial waters and then dumped along with the mud and pebbles. Also, as at Kirkdale, the stalagmite was "never found in continuous strata alternating with other strata of mud, or pebbles, but always forming a single crust on the upper surface of the sediment."[158]

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[155] Ibid., p. 145.

[156] Ibid., pp. 145-16.

[157] Ibid., p. 121.

[158] Ibid., pp. 143-44.

3. "The period of its introduction is that from which we must begin to date the formation of the superficial crust of stalagmite."[159] This was pure assumption on Buckland's part. There was little or no evidence to indicate that the stalagmite had started forming immediately after the deluge. He was unable to find any stalagmite beneath the mud, although he thought that it was probably present.[160] As he attributed the preservation of the bones in part to the stalagmitic cover, he could perhaps argue that the deposits had not remained uncovered for long.[161]

The cave of Kühloch was not so easily explained by Buckland's diluvial theory. This cave, he said,

> is more remarkable than all the rest, as being the only one I have ever seen, excepting that of Kirkdale, in which the animal remains have escaped disturbance by diluvial action; and the only one also in which I could find the masses of black animal earth. . . . It is literally true that in this single cavern . . . there are hundreds of cart-loads of black animal dust entirely covering the whole floor.[162]

This dust, which seemed to him to be derived principally from the decayed bones of bears, still contained an abundance of broken bones and teeth. The total quantity of animal matter he estimated at not less than 5,000 cubic feet. Allowing two cubic feet of dust and bones for each individual bear, he computed that there were present in the

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[159] Ibid., p. 121.

[160] Ibid., pp. 123, 128, 137.

[161] Ibid., p. 121.

[162] Ibid., pp. 137-38.

cave the remains of at least 2,500 bears, "a number which may have been supplied in the space of 1,000 years, by a mortality at the rate of two and a half per annum."[163]

In an article published in 1794, in connection with some bones of bears found in the cave at Gailenreuth, near that of Kühloch, John Hunter had reasoned that

> if we consider the distance of time between the most perfect having been deposited, which we must suppose were the last, and the present time, we must consider it to be many thousand years; and if we calculate how long these must still remain to be as far decayed as some others are, it will require many thousand years.[164]

According to Buckland, Hunter had overrated considerably the amount of time required for the decay of the bones, as it could have been produced over a period of a few hundred years or less.[165] If the bones had been protected by the mud during the period after the deluge and if the deluge had occurred immediately after the last bones were deposited, Buckland could argue that Hunter's reasoning, based upon a comparison of the amount of decay of the latest and of the earliest bones, was invalid.

Was Buckland, in his estimate of a few hundred years in this case and of a thousand years at Kühloch, influenced by the necessity of not allowing more than sixteen hundred years or so from Adam to

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[163] Ibid., pp. 138-39.

[164] John Hunter, "Observations on the Fossil Bones Presented to the Royal Society by His Most Serene Highness the Margrave of Anspach," Philosophical Transactions of the Royal Society of London (1794), p. 409. Communicated by Everard Home. Read May 8, 1794.

[165] Buckland, Reliquiae Diluvianae, pp. 146-47.

Noah? Sir Edward Bailey suggests that he was.[166] As has been pointed out, however, Buckland had good grounds from his theory for ruling out Hunter's reasoning.

Buckland found no pebbles at Kühloch cave and only faint indications of diluvial mud. The absence of the pebbles and the presence of such an enormous quantity of animal dust indicated to him a less powerful action of the diluvial waters.[167] The evidence for the presence of diluvial sediment was not good. The upper portion of the animal earth seemed "to be mixed up with a quantity of calcareous loam, which, before it had been disturbed by digging, probably formed a bed of diluvial sediment over the animal remains." However, "a small quantity of this loam may possibly have been derived from dry dust that has fallen from the decomposition of the roof." The diluvial waters, he believed, had laid down the loam and had laid open the present entrance of the cavern, which had prior to the deluge "formed the deepest recess of an extensive range of inhabited caves." This circumstance and the fact that the present entrance to the cave sloped upward toward the interior explained the less powerful action of the deluge.[168] Buckland did not notice that these conditions were not significantly different from those in other caves that had not escaped violent diluvial activity.[169] He might have argued that

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[166]Sir Edward Bailey, Charles Lyell (Garden City, New York: Doubleday & Company, Inc., 1963), p. 52.

[167]Buckland, Reliquiae Diluvianae, p. 140.

[168]Ibid., pp. 140-41.

[169]For example, Baumans Hohle and Biels Hohle, ibid., plates 15, 16.

these caves, unlike that at Kühloch, showed evidence of a former connection to the surface by means of fissures, so that it was possible that the mud and pebbles entered by these fissures rather than by the entrance.[170] The absence of any stalagmite at Kühloch would explain the advanced state of decay of the bones there.[171]

The belief that the remains of man had not been and probably would never be found in the diluvium of Europe was widespread at this time. Buckland argued that the large numbers of wild animals that lived in Europe in antediluvian times (inferred from the numbers found in the diluvium) could not have existed in a country inhabited by man. The remains of antediluvian man, he said, would probably be found only in the Near East and Asia.[172]

Because of this belief, scientists took a very sceptical attitude toward any reported findings of the remains of man associated with bones of extinct animals, either in caves or in the diluvium. Buckland discussed several of these occurrences and dismissed them all as inconclusive. The human remains in all of them, he concluded, were postdiluvian.[173]

The most interesting of these discoveries was reported by Baron von Schlotheim in 1820.[174] The remains of man were found in

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[170] Ibid., pp. 140-41.

[171] Ibid.

[172] Ibid., pp. 169-70.

[173] Ibid., pp. 164-70.

[174] E. F. Baron von Schlotheim, Die Petrefactenkunde auf

the diluvium which occupied depressions in a gypsum formation in the valley of the Elster River, near Leipzig, Saxony. Associated with these remains were those of the extinct rhinoceros, hyena, jaguar, and deer, as well as many bones of animals of apparently existing species.[175] The human bones exhibited the same amount of alteration and decay as the animal bones, and in one case human bones were discovered at a depth of twenty-six feet from the surface, actually eight feet below the bones of the extinct rhinoceros found in the same deposit.[176] Von Schlotheim, after first reaching the conclusion that man had lived at the same time as the rhinoceros and other animals of extinct species, changed his mind and attempted to attribute the association of the bones to postdiluvian local land floods. That his arguments in this connection sometimes contradicted the evidence that he had previously presented was pointed out by Thomas Weaver, who argued that all of the bones were of the same age. The evidence of Kirkdale and of other caves in England suggested, he said, that some of the present species existed before the deluge along with other

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ihrem jetzigen Standpunkte durch die Bescheribung seiner Sammlung versteinerter und fossiler Überreste des Thier- und Pflanzenreichs der Vorwelt erläutert (Gotha: in der Becker'schen Buchhandlung, 1820), xliii-lxi; and Nachtrage zur Petrefactenkunde (Gotha: in der Becker'schen Buchhandlung, 1822), 1-16. See also Thomas Weaver, "On Fossil Human Bones, and Other Animal Remains Recently Found in Germany," Annals of Philosophy, XXI (1823), 17-34, which consists of a translation of portions of von Schlotheim's reports with comments by Weaver.

[175] Weaver, p. 32.

[176] Ibid., pp. 25, 29.

species that were destroyed by it.[177]

Buckland agreed with von Schlotheim that the bones of man and the other animals of existing species were introduced at a later date; and he further stated that the bones of the animals of existing species, including those of man, were less calcined than those of the extinct animals.[178] It is difficult to see how Buckland reconciled this with von Schlotheim's statement that "the human bones, like those of the other animals, are more or less altered, and deprived of their animal gluten."[179] It is curious that a reviewer, William Fitton, although critical of many aspects of Buckland's book, did not question his conclusion that the human bones had been introduced at a later date. Fitton added that

> a single fragment of a human bone, obtained unequivocally, and under the same circumstances with those of any extinct species of other animals, would be conclusive on this point.[180]

The most important points established by Buckland's investigation of Kirkdale cave, when viewed in connection with the other caves and deposits in Europe, were the following:

1. The extinct elephant, rhinoceros, hippopotamus, and hyena were antediluvian inhabitants of Europe and were not drifted northward by diluvian currents, indicating that much of Europe was dry land before the deluge. This cast doubt on the theory of De Luc (and Cuvier)

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[177] Ibid., pp. 30-33.

[178] Buckland, Reliquiae Diluvianae, pp. 168-69.

[179] Weaver, p. 29.

[180] [William Fitton], Review of Reliquiae Diluvianae, by William Buckland, Edinburgh Review, XXXIX (1823), p. 224.

that the sea and land had changed places and supported the hypothesis that the antediluvian climate of Europe had been warmer than that of the present.[181]

2. Animals not distinguishable from those of existing species also inhabited antediluvian Europe. This point, although not emphasized by Buckland, was stressed by Weaver.[182]

Buckland presented in the Reliquiae Diluvianae a list of nine "facts" to which, "in addition to those afforded by the interior of caves and fissures," he appealed for support of his diluvial theory. Essentially the same list had appeared in his earlier book, the Vindiciae Geologicae, and the items in it were, briefly: 1. The shape of hills and valleys, indicating the action of flowing water; 2. The termination of minor valleys in some main trunk conducting their waters to the sea; 3. the existence of outliers; 4. the existence of the diluvium; 5. the nature of the diluvium; 6. the organic remains in the diluvium; 7. the impossibility of referring any of these facts to the effect of any other cause; 8. the universality of these phenomena; and 9. the fact that changes now going on seem to have commenced at about the date of the deluge.[183]

There was much duplication in this list, but it constituted, nevertheless, a substantial body of evidence for a deluge, although the value of the arguments based on the appearances of valleys

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[181]Buckland, Reliquiae Diluvianae, pp, 162-63.

[182]Ibid., p. 36, and Weaver, p. 29.

[183]Buckland, Reliquiae Diluvianae, pp. 226-28.

obviously was dependent on showing that these appearances could not have been produced by the rivers flowing in them, for little attempt was made to show that they could indeed have been produced by a violent deluge. The argument ran like this: The appearance of valleys suggests that they were excavated in whole or in part by the action of flowing water. The streams now flowing in the valleys are incapable of producing these effects regardless of the time allowed them, therefore these effects must have been produced by a violent deluge.

There were two main kinds of evidence: the diluvium and valleys of denudation. Since these phenomena were universal, it could be argued that they were produced by a single, universal cause. It was not difficult for Buckland to show that his diluvium was universal, for it included all unconsolidated gravel that was not actually in a river bed or on the seashore.[184] He argued that valleys could not be shown to have been excavated by local floods due to "the bursting of water-spouts" or to "the bursting, at successive periods, of the barrier of some fresh-water lakes" unless we can suppose the water-spouts to have "fallen universally and contemporaneously . . . over the whole earth" or unless we can assume the existence of such lakes

> at the head of every stream, and of every valley in the world; for there are none in which the effects of similar denudation are not apparent.[185]

Furthermore, modern lakes

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[184] Ibid., pp. 211-23.

[185] Ibid., pp. 213, 257.

> have a tendency to fill up, by a gradual accumulation on their bottoms, and not to burst their barriers; and . . . whatever antediluvian lakes and inland seas may have formerly existed, the gorges and defiles by which their waters were discharged can be referred to no physical cause at present in action, but were excavated by some extraneous and more mighty power than the waters of the lakes themselves.[186]

Buckland in this case was using a uniformitarian argument to demolish a hypothetical physical cause. As he proposed no causes for his deluge, he was not open to similar criticisms. This refusal to postulate a cause resulted in a certain degree of vagueness in visualizing the effects of such a catastrophe. Buckland, for example, quoted from Sumner's "inestimable and most judicious work" as follows:

> we cannot easily assign limits to the effect of a body of waters like the ocean pouring in over the land when its level was destroyed; we are at a loss to conceive what the power of such a machine might be when once in operation.[187]

"We know," Buckland said,

> from the effect of a mountain torrent in cutting ravines and drifting gravel; from the blocks of granite which were lifted to an elevated point on the side of a mountain by the bursting of a small lake . . . in Switzerland, . . . that the force of water in rapid motion is competent both to transport such masses of gravel and granite blocks as we have been tracing over the world, and to excavate valleys which . . . still bear a due proportion to the bulk and power of the agent that produced them.[188]

Thus the theory fixed neither the cause nor the effects of the deluge.

Buckland was sometimes guilty of giving the impression that the views of an eminent authority supported his theory when in fact

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[186] Ibid., pp. 213-14.

[187] Buckland, Reliquiae Diluvianae, p. 236, quoting Sumner, II, 350.

[188] Buckland, Reliquiae Diluvianae, p. 236.

they opposed it. For example, he referred to Buffon for evidence that the form of the valleys in France was due to the excavating action of a retiring ocean without informing his reader that the latter considered the Biblical deluge to have had no part in the formation of valleys and indeed to have not left any existing traces.[189] He also cited de Saussure for evidence of a debacle in the Alps, but neglected to mention that he had held that most valleys had been formed by the action of the streams that occupy them.[190]

Cuvier had asserted that since the diluvium was confined to the lower regions of the earth, the flood waters did not cover the summits of the higher mountains. Buckland disagreed and produced evidence of bones and diluvium found at high altitudes. His religious motivation was particularly apparent here, for he stated that this evidence showed, as the Bible said, that "all the high hills and the mountains under the whole heavens were covered."[191]

A very large part of Buckland's argument for a deluge was based on the supposed inadequacy of "modern causes." He took care to minimize the effects of such causes and to confine their activity to as limited a range as possible. Thus he stated that the diluvian waters,

> (if we except the very limited and partial action of modern causes, such as of torrents in cutting ravines, of rivers in forming deltas,

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[189] Ibid., p. 211. George Louis Leclerc, Comte de Buffon (1707-88), French naturalist and writer on zoology and geology.

[190] Ibid., p. 212. Horace Benedict de Saussure (1740-99), Swiss scientist and writer on the Alps.

[191] Ibid., pp. 221-23.

> of the sea in eroding its cliffs, and of volcanos in ejecting and accumulating their exuviae,) appear to have been the last agents that have operated in any extensive degree to change the form of the earth's surface.[192]

He insisted that the valleys were present before the streams which are now in them began to flow. "It is not easy," he said,

> to imagine how valleys . . . could have been formed in any conceivable duration of years by the rivers that now flow through them, since all the component streams, and consequently the rivers themselves which are made up of their aggregate, owe their existence to the prior existence of the valleys through which they flow.[193]

This line of argument would receive a strong rebuff in 1827 from G. P. Scrope, who said,

> It is scarcely necessary to attempt a serious refutation of a species of quibble which has been too often brought forward in place of argument by the diluvian theorists; viz. that rivers are caused by the pre-existence of the basins through which they flow, and consequently these could not have owed their existence to the rivers that flow through them! It is clear that no extensive surface of the earth could at any time have been so uniformly smooth and level but that the rains falling upon it must have collected into streams as they drained off. The erosive force of these streams would necessarily by degrees excavate channels of a depth and width proportioned to the duration of the process, their magnitude and velocity, and the more or less destructible nature of the rocks over which they flow.[194]

The importance of Buckland's investigations into the geology of the deluge lies in the stimulation that his work gave to vertebrate

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[192]William Buckland, "On the Excavation of Valleys by Diluvian Action, as Illustrated by a Succession of Valleys which Intersect the South Coast of Dorset and Devon," Transactions of the Geological Society, second series, I, 96. Read April 19, 1822.

[193]Ibid., p. 97. See also Buckland, Reliquiae Diluvianae, p. 237.

[194]George Poulett Scrope, Memoir on the Geology of Central France: Including the Volcanic Formations of Auvergne, the Velay, and the Vivarais (London: Longman, Rees, Orme, Brown, and Green, 1827), p. 164.

paleontology and to the investigation of the "antediluvian" period on England. Buckland's work revealed that conditions prior to the deluge were not greatly different from those after, and subsequent investigations would go much farther in this direction. They would eventually remove all of the distinctions that Buckland assumed between the two periods, eliminating the necessity for a diluvial hypothesis to explain them.

CHAPTER III

THE DILUVIAL THEORY ATTACKED AND DEFENDED

Buckland's Reliquiae Diluvianae, which appeared in May, 1823, was in general well received.[1] It was dedicated to Shute Barrington, Lord Bishop of Durham, who had advised Buckland to begin his investigations. "I know not," Buckland said,

> to whom I can so fitly dedicate the results of an inquiry, which but for this timely encouragement I might never have undertaken. It has, already, produced conclusions, which throw new light on a period of much obscurity in the physical history of our globe; and, by affording the strongest evidence of an universal deluge, leads us to hope, that it will no longer be asserted, as it has been by high authorities, that geology supplies no proofs of an event in the reality of which the truth of the Mosaic records is so materially involved.[2]

The fact that the book's publisher, John Murray, was also the publisher of the Quarterly Review assured that its review in that journal would be a favorable one. The task of writing the review was given to John

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[1] F. J. North, "Paviland Cave, the 'Red Lady,' the Deluge, and William Buckland," Annals of Science, V (1941-47), 103, 112. Buckland, in a letter to Lady Mary Cole, in April, 1823, stated that he expected his book to be out the "1st of May" (p. 112).

[2] William Buckland, Reliquiae Diluvianae; or, Observations on the Organic Remains Contained in Caves, Fissures, and Diluvial Gravel, and on Other Geological Phenomena, Attesting the Action of an Universal Deluge (2d ed.; London: John Murray, 1824), p. 111.

Barrow, one of the Quarterly's regular contributors. Barrow's article was, however, withdrawn and replaced by one written by Edward Copleston, Provost of Oriel College, Oxford. This was done as a consequence of Buckland's desire to have his book treated as more than simply a work in geology. The editor, Gifford, discussed the situation in a note to the publisher in July, 1823:

> Buckland, I know, complains that he has been treated solely as a geological writer--but he aspires to something higher, and it was this which made me wish for a more philosophical view of the subject; and this the Provost could well have given.[3]

Copleston's review was in the number for April, 1823, which did not appear until September.[4] Buckland was well pleased with it, calling it "very flattering,"[5] and the book sold so rapidly that a second edition was published in December.[6] Buckland at this time

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[3]Murray MS, Gifford to Murray, (July, 1823), in Hill Shine and Helen Chadwick Shine, The Quarterly Review under Gifford: Identification of Contributors, 1809-1824 (Chapel Hill: University of North Carolina Press, 1949), p. 84. The Murray Register gives Copleston as the author and adds the note: "an article was written by Mr. Barrow & withdrawn in favour of this one--See Dr. Buckland's letter Feb 3/ 1823."

[4][Edward Copleston], Review of Reliquiae Diluvianae, by William Buckland, Quarterly Review, XXIX (1823), 138-65.

[5]William Buckland, Letter to Rev. W. Vernon Harcourt: December 3, 1823, in Mrs. Elizabeth Oke (Buckland) Gordon, The Life and Correspondence of William Buckland, D.D., F.R.S., Sometime Dean of Westminster, Twice President of the Geological Society, and First President of the British Association (New York: D. Appleton and Company, 1894), p. 77. See also a letter of the same date to Lady Mary Cole, in North, p. 112.

[6]William Buckland, Letter to Rev. W. Vernon Harcourt, December, 1823, in Gordon, Life of Buckland, p. 77. See also a letter to Lady Mary Cole in December, 1823, in North, p. 113.

was already at work on a second volume which, however, was never published.[7]

The character of the "more philosophical" treatment that Buckland desired may be inferred from examining Copleston's review, which began by employing the common, but deceitful, device of confusing those who only denied that the Biblical deluge had left any traces with those who denied it altogether.[8] Copleston then proceeded to flog the Huttonian theory, which

> professed to explain the actual condition as well as the past history of our planet, without reference to any beginning of things, or any supernatural interposition in the changes which have taken place.[9]

Some parts of the theory were "hardly to be entitled to a serious answer":

> To assume an infinite series of centuries merely that weather may have time to remove mountains and plant them in the sea, and that water may cut through the ridge of a mountainous chain, (a thing to the performance of which in all eternity it could never tend to approximate,) is too monstrous an outrage upon common sense to be treated without ridicule. Nothing but scepticism could venture to make so large a demand upon human credulity--and all for the purpose of drawing away the mind from the contemplation of any beginning of things, and of teaching that 'there is no occasion (to use Dr. Hutton's own words)' for having recourse to any destructive accident in nature or the agency of any preternatural cause, in explaining that which actually appears.[10]

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[7]North, pp. 113, 124.

[8][Copleston], p. 140.

[9]Ibid.

[10]Ibid., p. 142. What aroused Copleston's ire was the following famous statement of Playfair in his Illustrations of the Huttonian Theory of the Earth (Edinburgh: William Creech, 1802), pp. 104-105:

It was incredible, Copleston said, that the Huttonian theory should have gained support:

> That a theory so extravagant, so gratuitous, so utterly unsupported by fact or by testimony should have been allowed even an indulgent hearing in a philosophical age, was hardly to be expected. That it should have had what is called a run, that it should have been illustrated and defended by a man of science, a professed admirer of the Baconian method of inquiry, and one of the ablest writers of his day, is to be reckoned among those anomalies of human nature which, according to the humour we are in, provoke either our regret, our indignation or our contempt.[11]

He believed that Playfair's recommendation of the theory for its "originality, grandeur and simplicity" indicated

> a remarkable confusion of thought, when applied to a system of philosophy. . . . In discussing the merits of an invention in the arts of life, or of any new method or plan, its originality, its simplicity or its grandeur may indeed be proper objects of

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"On observing the Patowmack, where it penetrates the ridge of the Allegany mountains, or the Irtish, as it issues from the defiles of Altai, there is no man, however little addicted to geological speculations, who does not immediately acknowledge, that the mountain was once continued quite across the space in which the river now flows; and, if he ventures to reason concerning the cause of so wonderful a change, he ascribes it to some great convulsion of nature, which has torn the mountain asunder, and opened a passage for the waters. It is only the philosopher, who has deeply meditated on the effects which action long continued is able to produce, and on the simplicity of the means which nature employs in all her operations, who sees in this nothing but the gradual working of a stream, that once flowed as high as the top of the ridge which it now so deeply intersects, and has cut its course through the rock, in the same way, and almost with the same instrument, by which the lapidary divides a block of marble or granite." This same passage was also quoted by Francis R. Conder in a review article, "Scepticism in Geology," Edinburgh Review, CXLVII (1878), 370. Conder called it "absurd" and added that "the philosopher, in this case, has drawn, in our opinion, far more unwarrantably upon his own imagination than the man 'little addicted to geological speculations,' whom he despises."

[11] [Copleston], p. 141.

> consideration, and a fair ground of praise. But when the inquiry is concerning a matter of fact, when we have to demonstrate, not to invent; to inquire what has been, not to speculate on what may be, the introduction of these ideas is manifestly improper, and has a tendency to mislead and confound us. It is precisely that error against which the great founder of modern philosophy cautions his reader. The true and only object of philosophy is the interpretation of nature. We must take nature as we find her, and dismiss from our thoughts the vain desire of modelling her according to any pre-conceived fancy of our own.[12]

The "miserably meagre collection of facts . . . upon which this fabric was raised" and the "sudden flight to the first principles of things, after a superficial examination of a few phenomena," were evidence, he said, of a "marvellous neglect of that code of inductive reasoning which Bacon delivered, and which is continually quoted as an oracle by that very school from which this theory sprung."[13]

Copleston argued that inability to assign a physical cause to the deluge was no reason to doubt the fact of such an event. The occurrence of the deluge had been abundantly proven, he believed; and, although he had no objection to attempts to find such a cause, "because even miraculous agency is often . . . combined with natural means," he did not believe that an adequate natural explanation for the event had yet been offered. He preferred to attribute it to "the immediate work of God."[14] It is evident that Buckland and Copleston were not at all disturbed by the lack of an adequate physical explanation for the deluge. As long as none was forthcoming, it

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[12] Ibid.

[13] Ibid.

[14] Ibid., pp. 158-59.

could be argued that the occurrence of a miracle had been proven by science.

That the position of Copleston and Buckland was a moderate one, bent on a reconciliation between science and religion, is indicated by the former's closing arguments, which were against interpreting the Bible too literally. He pointed our the dangers to organized religion of over-dependence on literal interpretations. In particular Biblical interpretation needed to be reconciled with the abundant geological evidence for a great antiquity for the earth:

> The more the strata which compose the crust of the earth are examined, the stronger evidence do they present of revolutions and catastrophes occurring at wide intervals of time, of slow progressive advancement to its present state, and of the existence of various orders of created beings which successively occupied its surface before it was finally fitted for the abode of man.[15]

This could be done in at least two ways: by considering "Genesis as setting forth the last formation only and the final adjustment of our globe to the occupation of man, (in which case the days may be regarded as portions of time equal to our natural days)," or by considering it "as declaring the whole series of changes the planet has undergone from the beginning of time," in which case the days would have to be regarded as indefinite periods of time. He pointed out that Christianity in a number of other instances, had ignored the letter of scripture without serious consequences.[16]

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[15] Ibid., p. 162.

[16] Ibid., p. 164.

It seemed as if Buckland were going to succeed in his attempt to reconcile religion and geology, and in return for his efforts the Church of England rewarded him handsomely. He was prominent enough to run, although unsuccessfully, for the presidency of Corpus Christi College in February, 1823.[17] In 1825 he was presented by his college to a living at Stoke Charity, Hampshire; and the same year he received a canonry at Christ Church, Oxford, and the degree of D.D.[18] His income from these benefices was such that he was able to marry at the end of the year.[19] He also that year played a leading part in getting a charter for the Geological Society, and he became its first president under the charter.[20] Eventually, in 1845, he was appointed by Sir Robert Peel to the office of Dean of Westminster.[21] The appointment of a man of science, even a man so identified with religion as Buckland, to so high a position in the Church was opposed by many people; yet it had been felt for some time, among scientists at least, that science was not given sufficient recognition in Great Britain.[22]

Buckland's book received some stiff criticism from the British Critic, the review journal most closely identified with the

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[17]North, pp. 107-08.

[18]Gordon, Life of Buckland, p. 87.

[19]Ibid., p. 90.

[20]Horace B. Woodward, The History of the Geological Society of London (London: Geological Society, 1907), p. 69.

[21]Gordon, Life of Buckland, p. 219.

[22]See Mrs. Buckland's complaints about lack of government support of science in ibid., p. 220.

Church of England.[23] The anonymous author of this review had no quarrel with the bulk of the book or with Buckland's conclusions respecting the caves or the occurrence of the deluge. His criticism was directed solely at the appendix, which contained some speculations upon the direction of the diluvial currents and the manner in which they excavated valleys in the Thames basin.[24] His dissatisfaction with these speculations was expressed in the following manner:

> This is the portion of the author's labours which has given us the least satisfaction. We perceive not how in the case of a universal deluge there could be those rapid currents which his theory requires; for if the whole globe was covered with water at the same period, whence would proceed the proximate cause of motion in the circumambient fluid, and of such a motion as would be necessary to excavate immense valleys. Supposing, as the author seems to think, that the great rush of water was downwards, in the direction of the present rivers, and that gravel was carried from the hilly country into the lower and more level parts by the weight of the diluvial current, what reason is there for concluding that the waters retired over the same ground, like soldiers after a charge, and formed the valleys in their retreat? Was water likely to make a retreat up an inclined plane; or where did Mr. Buckland learn that there was any such reflux of the diluvial waves? . . . The Professor is great on general principles, but he stumbles like other men when he makes haste to apply them: he shines in the collection of facts, and renders his theories at once ingenious and consistent; but in hypothetical matters he is by no means infallible. He begs or rather usurps a principle, and then uses it as tyranically and unwisely as the youngest logician or the most bigotted geologist. These remarks, we beg leave to add, apply solely to the latter portion of his volume: the former is unimpeachable in fact, reasoning, and conclusion.[25]

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[23]Anon., Review of Reliquiae Diluvianae, by William Buckland, Britich Critic, new series, XX (1823), 607-23.

[24]William Buckland, "Valleys of Denudation, and Beds of Diluvial Gravel, in Warwickshire, Oxon, and Middlesex," in Reliquiae Diluvianae (2d ed.), 249-58.

[25]Ibid., pp. 622-23. One gets the impression that the reviewer was opposed to most geological speculation.

William Fitton, who reviewed Buckland's book for the Edinburgh Review, after deploring his attempt to relate geology and scripture and paying tribute to the skill and energy of his researches, posed objections to some of Buckland's conclusions.[26] The Edinburgh Review had been friendly to the Huttonian theory, both Playfair and Leonard Horner, the brother of one of its founders, having contributed to it. Its review could therefore be expected to be critical of a work that violated so many Huttonian principles. Fitton, however, was not then a Huttonian, although on his way to becoming one. He was also a friend of Buckland's, so his criticism was not as severe as it might otherwise have been.[27]

Fitton accepted without argument Buckland's hypothesis that Kirkdale cave had been the den of hyenas and that the animals whose remains were found there had been inhabitants of the surrounding country.[28] He questioned whether the mud in the cave was necessarily due to a universal flood:

> If, as the author supposes, the Vale of Pickering was at one time a lake, the mud may have been produced by the overflow of its waters; and their rise to a sufficient height for that purpose, before the breaking down of the present gorge at New Malton, seems not at all improbable.[29]

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[26][William H. Fitton], Review of Reliquiae Diluvianae, by William Buckland, Edinburgh Review, XXXIX (1824), 196-234, 501.

[27]Apparently the editor, Francis Jeffrey, thought that Fitton's criticism might be sharpened a bit, for on page 501 of the same volume of the Edinburgh Review is a note by Fitton denying the authorship of two footnotes containing additional critical comments.

[28][Fitton], p. 210.

[29]Ibid., p. 215.

He noted also that there was some inconsistency involved in assuming, as Buckland did, a lake to have existed before the deluge in the Vale of Pickering and then ascribing to the same deluge the excavation of valleys. It may have been that, before the deluge, no valley was present there.[30]

Fitton claimed that Buckland had not demonstrated that the animals in question had "existed as inhabitants of England" in the period that "immediately preceded the formation of the diluvial gravel." In the cave itself, he said,

> there is unquestionably no evidence as to time; and the only standard by which we can be enabled to estimate the interval between the first deposition of the animal remains, and the period when they were enveloped by the mud, is derived from the perfect preservation of the bones; which Mr. Buckland ascribes to their encasement and protection by the mud having taken place at too short an interval after the death of the animals, to admit of decay. We do not absolutely object to this reasoning; but the argument, upon a point of so much importance, is far from being decisive; since the time required for the decomposition of bone, though not so protected, is in itself uncertain; while the assumed fact of long continued occupation of the caves, before the deluge, would have led us to expect some variety of condition, and some traces of decay, in the bony fragments themselves.[31]

On the question of the climate at the period in question, Fitton thought that the balance of probability was in favor of a climate more tropical in character than at present. He recognized that the evidence of the solid strata, of the crocodiles and coal vegetation, was "not strictly applicable to the case immediately before us, since the fossils of the solid strata belong to a geological era

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[30] Ibid., p. 209.

[31] Ibid., pp. 213-14.

altogether different from that of the diluvial gravel."[32] The frequent occurrence of beds or sandstone and conglomerate was accepted by Fitton as evidence for "repeated submersions of the surface, with alternations of violent action and repose." As for the diluvium, he said, "all geologists, we believe, now agree in regarding the latter gravel as the product of a revolution comparatively recent."[33] The excavation of valleys he attributed also to extreme violence: "it is now almost universally admitted, that valleys have been excavated by causes no longer in action,--contrary to the opinion of Dr. Hutton and Mr. Playfair."[34]

Fitton thus accepted much of Buckland's diluvial theory, but he objected to arbitrary assumptions made by Buckland in order to make his deluge resemble the Biblical one:

> That there has been a deluge, affecting universally all parts of the earth's surface, and producing every where the same or similar effects, no person who has duly examined the evidence can deny. . . . The only question is, whether that great event . . . is proved, by physical evidence, to have been recent, transient, and simultaneous; and upon these points . . . the facts appear to us to afford but imperfect evidence as to the date, and still less as to the duration, of the submersion. . . . For to speak of the support to the Sacred narrative afforded by extrinsic inquiry, if the narrative itself be made to form a part of the evidence, is a mode of reasoning that appears to us to be altogether inadmissible.[35]

The evidence for simultaneous submersion of all parts of the earth was very weak, he said. A more probable hypothesis was that of "the

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[32] Ibid., pp. 213-14.

[33] Ibid., p. 216.

[34] Ibid., p. 227.

[35] Ibid., pp. 229-30.

successive inundation of large portions of the earth," perhaps caused by a wave or a succession of waves produced by some great convulsion. This would obviate the difficulty of providing and removing a vast body of water.[36]

Fitton concluded that there was not "sufficient data from which to reason with safety, upon any general question touching the comparison of the antediluvian population, with the actual inhabitants of the globe." Among the "obscurities which time and observation have to remove," he noted the following:

> The partial extinction of species,--the mixture in certain diluvial accumulations, of the remains of extinct with those of existing animals,--the change of climate in high northern latitudes; or--if no such change have taken place--in the economy of the races that once inhabited those regions.

He also pointed out that there were "some circumstances indicating tranquillity of deposition, and long abode of the sea upon the surface in certain places, the combination of which, with so many proofs of violent and more transitory action, it is not easy to explain." For example "in Italy and Siberia, the bones of elephants, &c., abound along the banks of rivers, where they do not seem to have been disturbed."[37]

Fitton's attitude was thus one of caution. He was much more aware than Buckland was, judging from his public statements, of difficulties involved in arriving at an understanding of diluvial phenomena. He also was much opposed to bringing religious preconceptions

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[36] Ibid., p. 231.

[37] Ibid., p. 232.

into geology, and he was quite critical of the features of Buckland's deluge that seemed to have no basis other than a Biblical one, such as its recent date, short duration, and simultaneity. He pointed out that Buckland's deluge did not agree with the Biblical account on one important point: that of the preservation of the antediluvian animals.[38] He accepted, nevertheless, much of the evidence for diluvial action.[39]

Buckland's book received a very favorable review in the American Journal of Science, edited by Benjamin Silliman.[40] The reviewer, Edward Hitchcock, had read the reviews of Copleston and Fitton, and much of his article was a paraphrase of them.[41] He accepted Buckland's views without criticism, remarking on how well the periods inferred by him from the phenomena of the Kirkdale cave fitted the

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[38]Ibid., p. 233.

[39]Charles C. Gillispie, in his Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology, and Social Opinion in Great Britain, 1790-1850 (New York: Harper & Brothers, 1959), states that Fitton "did not oppose the Buckland-Cuvier interpretations in the twenties," (p. 111) This statement, without qualification, is misleading, for Fitton did, as has been shown, oppose certain aspects of Buckland's theory.

[40][Edward Hitchcock], "Notice and Review of the Reliquiae Diluvianae," American Journal of Science, VIII (1824), 150-68, 317-38.

[41]For evidence that Hitchcock was the author, see Benjamin Silliman's Outline of the Course of Geological Lectures, Given in Yale College (New Haven: Hezekiah Howe, 1829), p. 76. Benjamin Silliman (1779-1864), American chemist and geologist; professor of chemistry and mineralogy at Yale College, 1802-53; editor of the American Journal of Science, 1818-64. Edward Hitchcock (1793-1864), American geologist; professor of chemistry and natural history at Amherst College, 1825-45; professor of natural theology and geology, 1845-64.

Biblical history:

> Every one will see how exactly these periods correspond to the history of the world, as given in the scriptures, and handed down by tradition. The first and second period clearly point us to the antediluvian age of the world, the third, to the Noachian deluge, and the fourth, to the state of the world since that catastrophe.[42]

This statement, coupled with the fact that Hitchcock noted that the first period, during which the Kirkdale cavern existed in its present state before it was inhabited by hyenas, was "apparently of no great length," seems to indicate that he meant to apply the Biblical chronology, that is, to allow only about sixteen hundred years for the first two periods. It is surprising that any geologist at this late date could have believed that the secondary strata had been formed in so limited a period of time, yet Conybeare in 1822 proposed this as an acceptable alternative:

> If we adhere to the common interpretation of the periods of creation as having been literally days of twenty-four hours, and refuse to admit the existence of another order of things previous to that recorded by the inspired writer, we might still perhaps find a sufficient space of time for the purposes required in the interval between the creation as thus limited, and the deluge. Upon this hypothesis we must suppose the present continents (in the greater part of their extent) to have been included in the channel of the primitive ocean, and to have gradually emerged thence during this period, becoming occupied, as they appeared, by the land animals whose remains we find among the diluvial gravel; the primitive continents may upon this supposition either have been limited portions of the present (such as present no secondary rocks), for at first it seems evident that a limited space only would be requisite; or if more extensive, they may have been submerged in whole or in part, during those great convulsions which accompanied the deluge.[43]

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[42] [Hitchcock], p. 168. It is not clear whether Buckland intended this interpretation.

[43] W. D. Conybeare and William Phillips, Outlines of the

This interpretation was not the one that Conybeare favored, and one might wonder whether he was serious in proposing it; but the fact that it was proposed should make us aware that even geologists at this time had no conception of the time now believed necessary for the formation of the fossiliferous strata and that when geologists argued for more time, for "indefinite periods" instead of "days," their request was really a very modest one. This attitude of mind, which looked upon the hypothesis requiring the least amount of time as the simplest one, was a significant cause of the prevailing catastrophism of the times; it may also have been behind the opposition voiced by some geologists to any hypothesis, such as Cuvier's assumption of alternations of land and sea in the tertiary deposits, that would increase the time required.

Hitchcock agreed with Buckland that valleys were excavated by the deluge, adding that he did not believe that "one thousandth part of our present vallies were excavated by the power of existing streams." He conceded that "mountain torrents do exert, within narrow limits, a powerful agency." However, in level countries, "and where the stream has no great descent, it is found that rivers have not power to move except in a few extraordinary instances, even small pebbles."[44] It

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Geology of England and Wales, with an Introductory Compendium of the General Principles of That Science, and Comparative Views of the Structure of Foreign Countries (London: William Phillips, 1822), pp. lix-lx.

[44] [Hitchcock], p. 333.

had only been recently that the diluvial explanation of the origin of valleys had come back into favor:

> the general belief is, that existing streams, avalanches and lakes, bursting their barriers, are sufficient to account for all their phenomena, and not a few geologists, especially those of the Huttonian school, at whose head is Professor Playfair, have till recently been of this opinion.[45]

Buckland's views were also accepted by Benjamin Silliman, and they formed the basis of his geological lectures by 1829.[46] The impact that Buckland's book had on some geologists may be illustrated by its effect on Silliman's views. In a review in 1821 of a work by Horace H. Hayden on the alluvial deposits, Silliman called the idea that the Noachian deluge had deposited them a "peculiar theory which the author so zealously espouses and defends." It would have been better, he said, for the author to have "made it a deduction from the facts rather than to have prefixed it to them."[47]

Silliman thought that Hayden had underrated the efficacy of the decomposition of rocks in the formation of soils and loose earth and had not conceded enough to the ravages of time.[48] He preferred

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[45] Ibid., p. 332.

[46] Silliman, esp. pp. 68-98.

[47] [Benjamin Silliman], Review of Geological Essays, by Horace H. Hayden, American Journal of Science, III (1821), 47-57. For evidence that Silliman was the author, see American Journal of Science, VIII (1824), 331. The complete citation of Hayden's book is Geological Essays: or, an Inquiry into Some of the Geological Phenomena to be Found in Various Parts of America, and Elsewhere (Baltimore: the Author, 1820).

[48] [Silliman], American Journal of Science, III (1821), 54.

to attribute the rounding of pebbles and boulders to the action of the universal ocean:

> The attrition of the common waters of the earth, and even that exerted during the comparatively short period of the prevalence of the deluge of Noah, would do very little towards producing so mighty a result, and we must assign this operation to the more recent periods of the prevalence of the great chaotic deluge, whose existence is distinctly recorded in the first chapter of Genesis, and equally admitted by all geologists.[49]

Silliman did not disagree with the diluvial theory; on the contrary, he rather favored it. He did not, however, believe that a work of science should indulge in speculation not firmly based on factual evidence:

> We are not averse the author's particular theory, but, still, we could wish to see the present volume grow into a regular systematic work upon alluvion, excluding extraneous matter, and including a digested arrangement of all the important facts connected with that subject, with as much theory as those facts will warrant, and the theory would then flow naturally as an induction, according to the strict Baconian mode of philosophising.[50]

Silliman's position was that Hayden's hypothesis was very interesting and even agreeable, but he had not sufficiently substantiated it and should be encouraged to do so. He acknowledged that Hayden was not "bound to prove the immediate physical cause" of the Noachian deluge.[51] In contrast to his caution with respect to Hayden, Silliman apparently

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[49] Ibid., p. 50. Silliman's view is a typically Wernerian one.

[50] Ibid., p. 56.

[51] Ibid., p. 51. Silliman's interpretation of Genesis was similar to that of Buckland, for he wrote: "We consider the accurate chronology of . . . Genesis as commencing only with the creation of man, and the first formation and chaotic state of the globe, as not included in any of the periods called days." (p. 53, footnote).

considered Buckland's presentation of the diluvial theory as sufficiently rigorous, for by 1829 he had adopted it in full.[52]

The most persistent and troublesome opponent of Buckland's theory was the zoologist John Fleming, a Wernerian and friend of Jameson.[53] As early as 1818 he had advocated "the examination of the laws which regulate the physical and geographical distribution of recent shells, as the most suitable preparation for investigating the condition of those extinct races, whose memorials are preserved in strata."[54]

In 1822 he published a work, Philosophy of Zoology, in which he again emphasized the study of existing laws of change in the animal kingdom, in particular the role of man in altering the geographical distribution of animals.[55] His interpretation of the past history of the earth rejected the idea of elevations and depressions of the land or the sea. The tendency of erosional forces to wear down mountains

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[52]Silliman, Outline of the Course of Geological Lectures . . ., pp. 68-98.

[53]The Rev. John Fleming (1785-1857), Scottish Presbyterian minister; joined the free church, 1843; professor of natural philosophy, Aberdeen, 1834; professor of natural science, Free Church College of Edinburgh, 1845.

[54]See Fleming's article, "Conchology," in Supplement to the Fourth, Fifth, and Sixth Editions of the Encyclopaedia Britannica, with Preliminary Dissertations on the History of the Sciences (6 vols.; Edinburgh: Archibald Constable and Company, 1824), III, 316. This volume appears to have been first published in 1818, judging from the date of the preface.

[55]John Fleming, The Philosophy of Zoology; or a General View of the Structure: Functions, and Classification of Animals (2 vols.; Edinburgh: Archibald Constable & Co., 1822), II, 97-98.

and fill up hollows has been exerted, he said, since the formation of the first fossiliferous rocks and has been responsible for the increase in the quantity of dry land:

> The increase of land, by this process of upfilling, and the reduction of the number of mountains supporting glaciers, must have altered greatly the temperature of the globe; and, in every region, increased the difference between the heat of summer, and the cold of winter, by promoting the intensity of each.[56]

These changes, especially the filling up of lakes and the alternations of temperature and humidity, have been primarily responsible, he thought, for changes in organic life, although he admitted that "the universal deluge of Noah, and the numerous local inundations, the traces of which may be perceived in every country, must have greatly contributed to produce changes in the animal and vegetable kingdom."[57]

After Buckland's book was published, Fleming attacked the diluvial theory in a series of articles in the Edinburgh Philosophical Journal, which was edited by Jameson. In the first article, on the influence of man upon the distribution of animals, he remarked that the "proofs" of the diluvial hypothesis as an explanation of the diluvium

> have ever appeared to me extremely faulty. The partial occurrence of these strata, their limited extent, great difference of character in neighbouring districts, the presence of the remains of terrestrial animals, and the absence of marine exuviae, demonstrate that a "universal" flood, possessing the velocity which some have assigned to it, had no share in this formation. The phenomena

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[56] Ibid., pp. 100-01.

[57] Ibid., p. 104.

> which they exhibit, indicate a cause, partial, sudden, and transient, like the bursting of a lake.[58]

He suggested that perhaps the mud in the Kirkdale cave had been deposited by a subterranean river, applying an hypothesis that Buckland himself had proposed to explain similar mud found, together with human bones, in another cave.[59] If the deluge "drowned the hippopotamus," Fleming asked, "how did the ox and the horse escape?"[60] The extinct animals found in the diluvium, he said, were representatives of species that had been destroyed, not by the deluge, but by man, helped by disease, climate, and local inundations. As evidence for this, he mentioned a number of cases in which animals of extinct species had been found in post-diluvial deposits.[61] Fleming concluded the article with a severe censure of the "rashness" of those who would mix religion and geology:

> It would be favourable to the progress of geology, were its cultivators more disposed to examine the structure of the earth, and the laws which regulate the physical distribution of its inhabitants, and less anxious to give currency to their conjectures, by endeavouring to identify them with deservedly popular truths. It would be equally favourable to the interests of Revelation, were the believer to reject such faithless auxiliaries, and, instead of exhibiting a morbid earnestness to derive support to his creed from sciences but remotely connected with his views, calmly to consider, that Geology never can, from its very nature, add the weight of a feather to the moral standard which he has embraced,

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[58]John Fleming, "Remarks Illustrative of the Influence of Society on the Distribution of British Animals," Edinburgh Philosophical Journal, XI (1824), p. 299. This letter is dated July 19, 1824.

[59]Ibid., p. 301. See also Buckland, Reliquiae Diluvianae, pp. 165-66.

[60]Ibid., p. 203.

[61]Ibid., pp. 297-98, 304.

> or the anticipations of eternity in which he indulges, even should he fancy that it has succeeded in disclosing the dens of antediluvian hyaenas, in exhibiting the skeleton of a rhinoceros drowned in the flood, or in discovering the decayed timbers of the ark. This indiscreet union of Geology and Revelation can scarcely fail to verify the censure of Bacon, by producing "Philosophia phantastica, Religio haeretica."[62]

In his next article, Fleming discussed the materials of the "modern strata," which included Buckland's diluvium and alluvium, and held that the history of the earth consisted of a number of epochs, each characterized "by the peculiarities of the strata which were then deposited, and the organised beings with which the Earth was then peopled."[63] Each epoch corresponded with a series of strata; and "it seems to be determined," he said, "that the organised species, if connected with one series, differs from the organised species of every other series, and that the inorganic materials of the series have likewise a co-existing peculiar character."[64] He appears to have recognized at least three of these epochs: the older, the newer, and the modern. If we consider a genus, he said, we shall find that there are different species from it belonging to each of these epochs.[65]

Fleming divided the diluvium into two parts: lacustrine and marine, due respectively to the bursting of lakes and to violent

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[62] Ibid., p. 305.

[63] John Fleming, "Remarks on the Modern Strata," Edinburgh Philosophical Journal, XII (1825), 116. This article is dated December 3, 1824.

[64] Ibid., pp. 116-17.

[65] Ibid., p. 117.

transient inroads of the sea, caused by storms.[66] He said that if we

> consider the causes by which the different modern strata have been produced, as analogous to those which have contributed to the formation of the strata, belonging to the more ancient epochs of the Earth's history, . . . many of the irregularities, in thickness and extent, and arrangement, which the strata exhibit, will more easily be referred to their true cause. In such circumstances, the geologist will discover the importance of attending to the geognostical relations of the modern strata, and the laws which influence the physical and geographical distribution of the present races of organised beings; in order that, by proceeding from the distinct to the obscure, he may qualify himself for illustrating, with a greater chance of success, the various changes which the crust of this globe has undergone.[67]

Buckland was provoked to reply and tried to overwhelm Fleming by citing authorities who upheld his contention that the diluvium could not have been produced by existing causes.[68] He dismissed as not well substantiated all of Fleming's examples of extinct quadrupeds found in the alluvium, except the case of the fossil elk. But, even if these animals had existed since the deluge, he argued, the evidence for his theory "would remain unaffected by this discovery, and the great and universal phenomena of diluvial deposits would still be equally inexplicable, without appealing to the agency of a transient and general inundation of the Earth."[69] He denied that there was any opening at the far end of the Kirkdale cave through which a river could have

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[66] Ibid., pp. 122-26.

[67] Ibid., pp. 126-27.

[68] William Buckland, "Reply to Some Observations in Dr. Fleming's Remarks on the Distribution of British Animals," Edinburgh Philosophical Journal, XII (1825), 304-19. This article is dated December 16, 1824.

[69] Ibid., pp. 309-12.

flowed; and he asked whether land floods could explain the diluvium at Gibraltar, high above the sea.[70] On the whole, Buckland's reply was weak; a much more satisfactory answer to Fleming was that prepared by Sedgwick.[71]

Buckland's theory was defended by Sedgwick in the Annals of Philosophy in 1825.[72] The articles were intended to answer "responsible" criticism, such as that of Fitton and Fleming, for "the greater part of the objectors" were "undeserving of any animadversion, as they appear entirely ignorant of the very elements of geology, and far too imperfectly acquainted with the facts about which they write to have it in their power to turn them to any account, or to draw a single just conclusion from them."[73]

Sedgwick's scorn was directed at the fundamentalist critics of Buckland, for the latter's book had come under heavy attack from those who were alarmed at his departures from the accepted interpretation

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[70]Ibid., pp. 315-16.

[71]Buckland and Lyell visited Scotland from August to October, 1824, and, among other things, visited Sir George Mackenzie, Sir James Hall, and Jameson. They were probably there when Fleming's first article appeared about October 1. See Lyell's letters to his father, on August 10 and September 6; to his sister (Eleanor), on September 26; and to his mother, on October 18, 1824, in Life, Letters and Journals of Sir Charles Lyell, Bart., Author of Principles of Geology &c., ed. Katherine M. Lyell (2 vols.; London: John Murray, 1881), I, 153-59. In the letter to his father on August 10 (p. 154), Lyell commented upon a geological mistake that "Dr. Fleming" had made.

[72]Adam Sedgwick, "On the Origin of Alluvial and Diluvial Formations," Annals of Philosophy, new series, IX (1825), 241-57; "On Diluvial Formations," Annals of Philosophy, new series, X (1825), 18-37. These articles are dated March 11 and May, 1825, respectively.

[73]Sedgwick, Annals of Philosophy, new series, IX (1825), 241-42.

of Genesis, particularly his assumption of an indefinite age for the earth.[74] Since the hypotheses of these "scriptural geologists" assumed the formation of all the secondary rocks in the short period of about sixteen hundred years between the creation and the deluge, their works were regarded with contempt by virtually all geologists. The most popular of these writers were Granville Penn and George Bugg.[75] The work of another of them, Andrew Ure, who had some reputation as a scientist and was even a member of the Geological Society, received severe criticism from Sedgwick in his presidential address to the Society in 1830.[76]

Sedgwick devoted most of his defense of 1825 to attempting to establish that the diluvium was distinctive in character and could not have been produced by the ordinary operations of nature. Where undisturbed, the diluvium could be clearly distinguished by him from the alluvial deposits lying above it. The diluvial deposits in England were made up of "great irregular masses of sand, loam, and coarse gravel, containing through its mass rounded blocks sometimes of

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[74]Milton Millhauser, "The Scriptural Geologists: an Episode in the History of Opinion," Osiris, XI (1954), 65-86, has an interesting discussion of this type of literature.

[75]Granville Penn, A Comparative Estimate of the Mineral and Mosaical Geologies: Revised, and Enlarged with Relation to the Latest Publications on Geology (2 vols.; 2d ed.; London: James Duncan, 1825). [George Bugg], Scriptural Geology; or, Geological Phenomena, Consistent Only with the Literal Interpretation of the Sacred Scriptures, upon the Subjects of the Creation and Deluge; in Answer to an "Essay on the Theory of the Earth," by M. Cuvier . . . and to Professor Buckland's Theory of the Caves, as Delineated in His "Reliquiae Diluvianae," . . . (2 vols.; London: Hatchard and Son; L. B. Seeley & Son, 1826-27).

[76]Adam Sedgwick, Presidential Address Delivered to the

enormous magnitude." They were found generally on the plains, but were not confined to the river banks, being "spread over all the face of the country," often appearing at elevations much higher than any river flood could have attained.[77] In contrast the alluvial deposits consisted of "nearly horizontal deposits" of fine or pulverized gravel, silt, loam, and other materials "accumulated by successive partial inundations" and found generally in the upper parts of the valleys or in the river deltas.[78] Sedgwick thus apparently confined the alluvial deposits to matter which obviously was due to river floods--layered material, with particles no larger than sand, found near the existing rivers. Anything else was diluvium, which could vary considerably in its composition.

The two types of deposits might "sometimes become mixed and confounded," but their order was never inverted and they never alternated with each other over any appreciable area. The diluvial deposits rested directly on the older strata and did not contain alternating layers, as did the alluvial deposits, indicating the "long continued and tranquil operation of the agents by which they have been produced."[79]

Geological Society of London, February 19, 1830, Philosophical Magazine, ser. 2, VII (1830), 310-13. Andrew Ure, A New System of Geology, in Which the Great Revolutions of the Earth and Animated Nature, Are Reconciled at Once to Modern Science and Sacred History (London: Longman, Rees, Orme, Brown, & Green, 1829).

[77]Ibid., pp. 243-44.

[78]Ibid., p. 243.

[79]Ibid., p. 247.

The diluvial deposits therefore originated in a system of causes which were never repeated and which preceded the alluvial deposits.[80]

Sedgwick denied, except in the case of the fossil elk, that extinct species of animals had ever been found in the alluvium. None of Fleming's examples were derived from "undisturbed" alluvium:

> A sober-minded naturalist who makes his inductions after an extended examination of facts, and who does not view all things through the distorting medium of an hypothesis, will never derive from such localities as these any argument for the true arrangement of spoils found in different parts of the superficial gravel.[81]

"No contiguous formations in the crust of the earth are separated from each other by more clear and decisive characters," than are the alluvium and diluvium, he said.[82]

Fleming's criticism of the diluvial theory because of its failure to explain why some species of animals survived and others didn't was of little effect, Sedgwick remarked, because

> the same difficulty meets us in classing many of the regular strata of the earth. The suite of fossils derived from one formation may be widely different from the suite derived from another; yet we know by experience that both suites may contain many individuals of a common species.

"Still less," he said,

> are the conclusions shaken by the hypothesis, that the weapons of the hunter completed the extinction of many species of animals. . . . From the only physical evidence which we can have on such a subject, we believe that not a single hunter had ever trodden in the woods of Europe at the time when the mammoth, the rhinoceros, and the hyaena were its inhabitants.[83]

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[80] Ibid., p. 248.

[81] Ibid., p. 250.

[82] Ibid., p. 253.

[83] Ibid., pp. 252-53.

It is difficult to see how Sedgwick's statement about the same species being common to two formations of the regular strata could have had any force unless he believed that catastrophes similar to the deluge had caused the changes observed in the mineral character and in the organic remains of the successive formations. This was the belief that the formations correspond to periods in the history of the earth, each of which was characterized by its own organic life. Each period would be ended by a catastrophe that destroyed certain species, which were replaced in the following period by newly created species. An opponent, such as Fleming, might have replied that the assumption of catastrophes to explain the appearances of the regular strata was just as invalid as it was with respect to the appearances in the diluvium.

According to Sedgwick, the hypothesis that the diluvium was the result of local floods caused by the bursting of lakes was not justifiable because it was an ad hoc hypothesis, no positive evidence being available for the former existence of such lakes. In any case it postulated a cause inadequate to explain the appearances: it could not explain why the diluvium did not alternate with the alluvium, nor did it explain the difference in their organic remains. Furthermore, the alluvium appeared to have been deposited within a very limited period, judging from its extent and the present rate of depositional processes.[84]

Sedgwick thus argued that neither long-continued erosion nor local floods produced by the bursting of lakes could have excavated

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[84] Ibid., p. 254.

valleys. Since it was generally admitted that the form of most valleys was due in large part to the action of water, that left only violent, large-scale deluges as a cause. He referred to the Weald "valley" in southeast England in support of this contention. The Weald is a denuded anticline whose streams flow outward from a low range of hills in the center through gorges in the high chalk ridges or "downs" (the upturned edges of the chalk formation) that fringe the area to the north and south. Sedgwick maintained that it was "physically impossible" that the "singular contour" of the Weald "should have been produced by the long-continued erosion of the waters." If the rivers had excavated the longitudinal valleys of the Weald there is no reason "why they should not flow down these valleys at this moment." Assuming this, "it is inconceivable how they should ever have forced their way (in no less than eight places) through the high ridges of the North and South Downs."[85] He did not, however, specify in what way a catastrophic disturbance could have produced the phenomena.

Sedgwick conceded most of the points that Fitton had raised. He admitted that the occurrence of diluvial forces had not been established for all the world, although it was probable that their effects were universal. The duration of the diluvian era was "impossible to ascertain; for as the powers of the agent are unknown, it is obviously impossible for us to form an estimate of the time which was necessary to the production of such effects as are visible." The floods, however,

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[85]Sedgwick, Annals of Philosophy, new series, X (1825), 20. He pointed out a similar case in the Isle of Wight. Sedgwick's arguments on the Weald were the same as those made by Conybeare(Conybeare and Phillips, pp. xxvii, 145).

were probably sudden and transient. There was no evidence "to prove that the highest elevations of the globe were submerged by the diluvian waters," but they had acted on some of the highest points of Europe. He agreed that the causes of these diluvial currents were completely unknown and their direction was not much better determined.[86]

Sedgwick later denied that he had ever held the diluvial theory to the same extent as Buckland, and his conclusions were, indeed, more cautious than Buckland's had been.[87] His major concession to Fitton was his admission that the effects that could have been produced by a series of sudden and transient deluges, occurring over a long period of time. The evidence, however, confirmed Buckland's "general argument" without a doubt:

> Indeed, the facts brought to light by the combined labours of the modern school of geologists, seem, as far as I comprehend them, completely to demonstrate the reality of a great diluvian catastrophe during a comparatively recent period in the natural history of the earth. In the preceding speculations, I have carefully abstained from any allusion to the sacred records of the history of mankind; and I deny that Professor Buckland, or any

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[86] Ibid., pp. 33-34.

[87] In a letter to Murchison, dated 17 November, 1831, he wrote the following: "If I have been converted in part from the diluvian theory (which by the way I never held to the same extent with Buckland, as you may see if you read the last page of the only paper I ever wrote on the subject) it was . . . by my own gradual improved experience, and by communicating with those about me." See John Willis Clark and Thomas McKenny Hughes, The Life and Letters of the Reverend Adam Sedgwick, LL.D., D.C.L., F.R.S., Fellow of Trinity College, Cambridge, Prebendary of Norwich, Woodwardian Professor of Geology, 1818-1873 (2 vols.; Cambridge: At the University Press, 1890), I, 371.

> other practical geologist of our time has rashly attempted to unite the speculations of his favourite science with the truths of revelation.[88]

Sedgwick disagreed with Fitton's contention that the scriptures should not be used to test the results of science:

> The conclusions established on the authority of the sacred records may, . . . consistently with the soundest philosophy, be compared with the conclusions established on the evidence of observation and experiment; and such conclusions, if fairly deduced, must necessarily be in accordance with each other. This principle has been acted on by Cuvier, and appears to be recognized in every part of the "Reliquiae Diluvianae." The application is obvious. The sacred records tell us--that a few thousand years ago "the fountains of the great deep were broken up"--and that the earth's surface was submerged by the waters of a general deluge; and the investigations of geology tend to prove that the accumulations of alluvial matter have not been going on many thousand years; and that they were preceded by a great catastrophe which has left traces of its operations in the diluvial detritus which is spread out over all the strata of the earth.
>
> Between these conclusions, derived from sources entirely independent of each other, there is, therefore, a general coincidence which it is impossible to overlook, and the importance of which it would be most unreasonable to deny. The coincidence has not been assumed hypothetically, but has been proved legitimately, by an immense number of direct observations conducted with indefatigable labour, and all tending to the establishment of the same general truth.[89]

It would seem that Sedgwick in 1825 was almost completely in sympathy with Buckland's views, despite his later denial, but was more cautious in expressing his opinions.

Buckland was also defended by his friend Henry Thomas De La Beche, who reported that the geological phenomena in Jamaica upheld the diluvial theory.[90] Like Sedgwick he thought that most of the objectors

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[88] Ibid., p. 34. This was in reply to Fleming's statement, Edinburgh Philosophical Journal, XI (1825), 304.

[89] Ibid., pp. 34-35.

[90] Henry Thomas De la Beche, "Notice on the Diluvium of

to the theory were so ignorant that they required no answer. Referring to Granville Penn's book, he remarked: "In this class may not unfairly be placed the work which a writer in the Quarterly Journal of Science very gravely informs us is masterly!!"[91] The writer of the article referred to was William Brande, editor of the Quarterly Journal, who appears to have remained neutral in the controversy between the fundamentalists and Buckland, generously praising the works of both.[92]

Jamaica, according to De la Beche, contained a plain covered by diluvial gravel, which the present streams tend to destroy rather than to form:

> It is almost impossible to stand upon the gravel plain of Liguanea without feeling convinced that it could not have been formed by any causes now in action, but that the porphyry, greenstone, and other pebbles, which constitute, with a few clay and sand beds, the mass of the plain, were derived from the Jamaica mountains in the same manner, and at the same period, as the numerous European tracts of gravel, which have resulted from the destruction of European rocks, and which contain the remains of elephants, &c.[93]

He found in the white limestone hills valleys with no streams in them similar to those in the chalk downs of England:

> they could not, therefore, be formed by the waters which now traverse them, since there are none which do so: these valleys, then,

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Jamaica," Annals of Philosophy, new series, X (1825), 54-58. Sir Henry Thomas De la Beche (1796-1855), English geologist; director of the Geological Survey, 1832-55.

[91] Ibid., p. 54.

[92] William Thomas Brande, "Outlines of Geology, Being the Substance of a Course of Lectures on That Subject, Delivered in the Amphitheatre of the Royal Institution of Great Britain," Quarterly Journal of Science, Literature, and the Arts, XIX (1825), 64.

[93] De la Beche, pp. 55-56.

> are completely opposed to the theory that valleys owe their origin to the streams or rivers which now run through them.[94]

De la Beche admitted that no bones had yet been discovered in the diluvium of Jamaica, nor in any of the caves. He described the most famous of these, which had been visited by hundreds who had written their names all over it, leading him to conclude that the floor was not in the condition in which it was first discovered. It had been so hot that he was prevented from making a proper search for bones in the clay under the stalagmite on the floor. The climate in Jamaica, in fact, was really too hot to do much geological research.[95]

The replies of Buckland and Sedgwick were answered by Fleming in a carefully prepared, full-scale attack on the diluvial theory.[96] His historical introduction to this paper pointed out the futility of the various attempts in the past to produce scientific evidence for the deluge.[97] Fleming first argued that the character of the geological deluge, as supposedly indicated by the phenomena of nature, did not agree with that of the deluge of Noah as given by Moses. His arguments were similar to those of Farey and the other critics of Buckland but they were more effectively presented, for they proceeded from a clergyman of unquestioned piety. "I have formed my notions of

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[94] Ibid., p. 57.

[95] Ibid., pp. 56, 58.

[96] John Fleming, "The Geological Deluge, as Interpreted by Baron Cuvier and Professor Buckland, Inconsistent with the Testimony of Moses and the Phenomena of Nature," Edinburgh Philosophical Journal, XIV (1826), 205-39. This article is dated December 24, 1825.

[97] Ibid., pp. 205-08.

the Noachian deluge, not from Ovid, but from the Bible," he said:

> There the simple narrative of Moses permits me to believe, that the waters rose upon the earth by degrees, and returned by degrees; that means were employed by the Author of the calamity to preserve pairs of the land animals; that the flood exhibited no violent impetuosity, neither displacing the soil, nor the vegetable tribes which it supported, nor rendering the ground unfit for the cultivation of the vine. With this conviction in my mind, I am not prepared to witness in nature any remaining marks of the catastrophe, and I feel my respect for the authority of revelation heightened, when I see on the present surface no memorials of the event. . . . In other words, if the geological creeds of Baron Cuvier and Professor Buckland be established, as true in science, then must the Book of Genesis be blotted out of the records of inspiration.[98]

After disposing of the alleged scriptural authority for the deluge of Cuvier and Buckland, Fleming examined it as a scientific hypothesis, in which respect he also found it wanting. The shape of the valley-systems, formed by major rivers and their tributaries, the variable and local nature of most of the diluvium, and the absence of marine remains in it, as well as the inability of the hypothesis to explain why some species had become extinct and others had not, were all against the hypothesis. He also criticized the vague and confused notions of the deluge held by the diluvialists.[99] With respect to the Kirkdale cave, he suggested that the bones had been carried down "from caverns at a high level, by the agency of water, which deposited at the same time the mud in which they are imbedded." This would explain the broken and mixed character of the bones.[100]

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[98] Ibid., pp. 214-15.

[99] Ibid., pp. 216-35.

[100] Ibid., pp. 230-32.

Fleming's criticism was devastating, and Buckland did not reply to it. Three years later Fleming spoke of his article as

> a reply which my friends assure me gave the death-blow to the diluvian hypothesis. Certain at least it is, that, since that time, with the exception of a very few individuals who may still be found on stilts, amidst the "retiring waters," the opponents of the hypothesis have become as numerous as were formerly its supporters, and the period is probably not far distant, when the "Reliquiae diluvianae" of the Oxonian geologist will be quoted as an example of the idola specus.[101]

Whether the judgment of Fleming's friends was correct or not is debatable. However, Fleming certainly undercut the Biblical base of the diluvian hypothesis and drove its supporters onto the defensive.

Fleming's uniformitarian views were not the product of Huttonian influence, but rather derived in great part from the Wernerian school of Jameson. The change in the attitude of Jameson toward the diluvian hypothesis was a clear, if quiet, one. In his preface to Cuvier's Essay he emphasized its religious implications in the first two editions, in 1813 and 1815. His preface to the third edition, in 1817, was new and omitted all references to religion. His attitude in this and in the fourth edition of 1822 appears to have been a neutral one: a tacit acceptance of it as a geological hypothesis, but with no reference to its religious connection. In the fifth edition, in 1827, he included a section, "On the Universal Deluge," in which geological phenomena were explained in terms of the action of ordinary causes and of a slow rising and falling of the ocean level

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[101]John Fleming, "Additional Remarks on the Climate of the Arctic Regions, in Answer to Mr. Conybeare," Edinburgh New Philosophical Journal, VIII (1830), 68.

operating over a long period of time.[102] At the end of this section Jameson remarked that this anti-catastrophic view of the deluge was presented in response to frequent requests.[103]

During the controversy between Fleming and Buckland, Jameson appears to have agreed with the former; and, in a footnote to one of Fleming's articles, he remarked that Werner had not advocated "the geological diluvian hypothesis. On the contrary, his opinion was nearly the same as that stated in the text by Dr Fleming."[104]

Fleming's attitude toward Hutton is revealed in a letter that he wrote to Charles Lyell in 1826. Commenting on the latter's recent review of the Geological Society Transactions in the Quarterly Review, he remarked:

> I have read those papers in the Q. R. about which we conversed on the banks of the Tay; and with very great pleasure. In the geological one however there was one part which by no means pleased. In speaking of geology descending from the Grampians Playfair and Hutton are alone quoted as its supporters. Now whoever is acquainted with the progress of British or Scottish geology for the last twenty years must know the vast benefit the science has derived from the labours of Jameson; yet his name is left out--a striking display of the influence of English prejudices. These in geology are neither few nor much concealed. Believe me that Williams was a better geologist than either Hutton or Playfair and that but for Jameson neither Greenough Fitton, Buckland nor Daubeny would have occupied their present rank.[105]

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[102]Georges Cuvier, Essay on the Theory of the Earth, with Geological Illustrations, by Professor Jameson (5th ed.; Edinburgh: William Blackwood, 1827), pp. 417-37.

[103]Ibid., pp. 436-37.

[104]Fleming, Edinburgh Philosophical Journal, XI (1824), 299.

[105]John Fleming, MS letter to Charles Lyell: Manse of Flisk, November 11, 1826, Darwin-Lyell Correspondence, American Philosophical

Fleming reopened his controversy with the diluvialists in 1829, when he questioned the evidence for a warmer climate in the past, evidence that he said was based solely on the invalid analogy between the habits of existing and fossil animals of different species but of the same genus.[106] He was answered by Conybeare, somewhat sarcastically.[107] Fleming's reply silenced Conybeare and delivered another blow to Buckland's reputation.[108] Lyell remarked in a letter to Fleming:

> The answer to Conybeare is severe enough, and both instructive and amusing; and to those who, like true Englishmen, love to see a good fight, it has afforded more sport than any round fought for many a year. . . . The 'idola specus' is allowed to be as clever a hit as ever was given.

Lyell, however, was resolved that, when his book was attacked, he would not waste his time in replying; and he expressed some alarm at the number of personal quarrels in which Fleming was engaged.[109]

Society, Philadelphia, Pa. Fleming referred to John Williams, who had criticized Hutton so severely.

[106]John Fleming, "On the Value of the Evidence from the Animal Kingdom, Tending to Prove That the Arctic Regions Formerly Enjoyed a Milder Climate Than at Present," Edinburgh New Philosophical Journal, VI (1829), 277-86.

[107]W. D. Conybeare, "Answer to Dr. Fleming's View of the Evidence from the Animal Kingdom, as to the Former Temperature of the Northern Regions," Edinburgh New Philosophical Journal, VII (1829), 142-52.

[108]Fleming, Edinburgh New Philosophical Journal, VIII (1830), 65-74.

[109]Charles Lyell, Letter to John Fleming: Temple, February 3, 1830, Life, Letters and Journals, I, 259-61. At the same time as his quarrel with Conybeare, Fleming was involved in an altercation with William S. MacLeay, a zoologist. Fleming had written an article

One of the defenders of the diluvial hypothesis was Charles Daubeny, professor of chemistry at Oxford, who in 1826 published a volume on volcanoes.[110] In 1819 he visited the volcanic Auvergne region of Central France for the purpose of examining the basalt rocks there. He wanted to compare them with the trap rocks of Scotland and Northern Ireland, the aqueous origin of which had been advocated by Jameson. Daubeny, like so many others who had visited the Auvergne, concluded that the basalts there were certainly volcanic rocks, although he refrained from claiming that all basalts were of the same origin.[111]

Because, as he said many years later, "the position that the valleys had been excavated by the action of the Noachian deluge reigned undisputed," he classified the volcanic rocks in the Auvergne as ancient

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for the Quarterly Review ("Systems and Methods in Natural History," Quarterly Review, XLI (1829), 302-27), in which he had criticized MacLeay. The latter replied with a series of extremely vituperative articles in the Philosophical Magazine: "On the Dying Struggle of the Dichotomous System," Philosophical Magazine, VII (1830), 431-45; VIII (1830), 53-57, 134-40, 200-07; with a short reply by Fleming, VIII (1830), 52-53.

[110]Charles Daubeny, A Description of Active and Extinct Volcanos; with Remarks on Their Origin, Their Chemical Phaenomena, and the Character of Their Products, as Determined by the Condition of the Earth During the Period of Their Formation. Being the Substance of Some Lectures Delivered before the University of Oxford, with Much Additional Matter (London: W. Phillips, 1826). Charles Giles Bridle Daubeny (1795-1867), chemist, botanist, and geologist; M.D., Oxford; professor chemistry, Oxford, 1822; professor of botany, Oxford, 1834. He was a student in geology of both Buckland (1815-16) and Jameson (1816-17).

[111]Charles Daubeny, "On the Volcanoes of Auvergne," Edinburgh Philosophical Journal, III (1820), 359-67; IV (1821), 89-97; "On the Ancient Volcanoes of Auvergne," Edinburgh Philosophical Journal, IV (1821), 300-15. These are dated July 20, 1820 and January 16, 1821, respectively.

and modern, or ante-diluvian and post-diluvian.[112] The incorrectness of this statement and the fact that the position mentioned was disputed at that time may be readily inferred from his statements in 1820. In making this division into ancient and modern, he recognized that it could be objected to as being founded upon theoretical considerations; and he remarked that those who so objected could rest assured that there was a marked difference in the character of the rocks, whether they agreed or not with his interpretation of the cause of that difference. He assumed without argument that the valleys had been excavated by a deluge, but he was by no means so confident about that deluge being the Biblical one. This is indicated by the following statement:

> yet if my friend Professor Buckland be correct, in attributing the excavation of our valleys to the Mosaic deluge, the modern volcanoes of Auvergne must all have been posterior to the latter event.[113]

Daubeny said that the ancient volcanic rocks,

> like rocks of Neptunian origin in general, are cut through by the valleys which now exist; whilst the modern follow exactly the inequalities of the ground.

The modern, or post-diluvian lavas also "are more cellular, have a harsher feel, and more of a semi-vitreous aspect."[114] He considered the modern flows to be older than the earliest historical records of the region, which date from Roman times but posterior to the Mosaic

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[112]Charles Daubeny, "On the Antiquity of the Volcanoes of Auvergne," Quarterly Journal of Science, III (1866), 204.

[113]Daubeny, Edinburgh Philosophical Journal, III (1820), 360-62.

[114]Ibid., pp. 360-61.

deluge.[115] He retained this classification in his work on volcanoes; but, probably because of Fleming's articles, he no longer asserted that the deluge that had excavated the valleys was necessarily that of Moses.[116] He stated his position with respect to this question in an article in 1825 on the geology of Sicily:

> In adopting this term, I mean to express no opinion with respect to the much-agitated question, as to the identity of the particular deluge recorded in the Mosaic History, with the cause to which the excavation of the valleys and the formation of beds of gravel are to be referred.
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> That no cause, or combination of causes, now in operation, could be adequate to produce these effects, and that the best mode of accounting for them is to suppose the eruption and subsequent retreat of a vast body of water acting simultaneously over the whole surface of the globe, I am myself fully of opinion; but that this event was the same with that deluge which we see alluded to in Holy Writ, is obviously a distinct question, and one which I forbear entering upon, as it belongs rather to the province of Theological than of Scientific discussion. I make these remarks, lest I should be accused of adopting a classification founded on hypothetical principles, whereas the expression of antediluvian and postdiluvian, here used, is merely meant to imply, that the rocks so named were formed before or after the period at which the valleys were excavated, and may, therefore, be received by every one who agrees with Professor Buckland so far as to admit, that the latter effects were brought about by the simultaneous operation of one general cause, and not by a succession of partial ones.[117]

In his work on volcanoes, Daubeny asserted that antediluvian earthquakes and volcanoes were analogous in kind to, if exceeding in magnitude, those presently observed. He did not, however, extend this

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[115] Ibid., pp. 361-62. See also Daubeny, Edinburgh Philosophical Journal, IV (1821), 97, where he indicates more strongly his adherence to Buckland's diluvial hypothesis.

[116] Daubeny, A Description of Active and Extinct Volcanoes . . ., p. 9.

[117] Charles Daubeny, "Sketch of the Geology of Sicily," Edinburgh Philosophical Journal, XIII (1825), 264.

conclusion to the action of water:

> It has often been a subject of dispute amongst Geologists, whether the processes, to which the earth is supposed to owe its actual condition, were the same with any that are taking place at present, differing only in magnitude, extent, and duration; or whether they must be explained by assuming a totally distinct system of causes, which, since the commencement of the present order of things, have ceased to exist.
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> The latter is the opinion expressed by Dr. Kidd in the close of his Geological Essay; and it is favoured more particularly by the appearances presented by the rolled masses met with every where at the bottom of vallies, which are now attributed by almost universal consent, to a body of water differing both in its cause and mode of action from our present rivers.
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> But this remark, however applicable it may be to the other forces that are now in operation, does not seem to extend either to earthquakes or volcanos, from both which agents effects have resulted even within the narrow limits of our own observation, which, although inferior in point of magnitude to some of those produced at former periods, seem nevertheless analogous in kind.[118]

Although Daubeny mentioned in passing several instances where the present rivers had cut extensive valleys into lava flows he paid no particular attention to them. Many of his descriptions were based on the work of French writers, such as the Comte de Montlosier; but he did not inform his readers that the latter had attributed the formation of valleys in large part to ordinary erosion.[119] He did, however, object to an attempt by Bertrand-Roux to determine the relative antiquity of the lava flows by the amount of erosion they had undergone:

> From his statement it would appear, that the basaltic rocks of this neighbourhood are of very different ages, though I cannot admit that we are justified in estimating their relative antiquity

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[118] Daubeny, A Description of Active and Extinct Volcanos . . ., p. 2. This book was dedicated to Kidd and Buckland.

[119] Ibid., pp. 5, 11, 41. [François Dominique Reynaud, Comte de Montlosier], Essai sur la théories des volcans d'Auvergne (Riom et à Clermont: Landriot et Rousset, an X--1802).

> by comparing together the depth to which the several parts of this formation have been worn away. M. Bertrand Roux himself furnishes us in my opinion with a convincing proof that the effect has not been dependant on the longer or shorter continuance of causes now in action, when he mentions that the rock on either side of the old Roman roads, none of which can be less than 1300 years old, has undergone since that period scarcely any sensible decay. Instead therefore of considering with M. Roux the amount of the destruction that has taken place in different parts of the formation, a sort of chronometer to assist us in determining their relative age, I should rather adopt the converse of the proposition, and argue that the time required would, according to his own shewing, have been so immense, that we are in a manner driven to suppose the effect to have been brought about by causes differing in their mode of action from those at present in operation.[120]

Daubeny's caution is an indication of the effectiveness of Fleming's attacks on the diluvial theory in seriously weakening its Biblical support. Fleming had also argued that the evidence of the diluvium and its organic remains was not as conclusive as the supporters of the theory maintained. However, the theory needed to be attacked on other grounds before geologists generally became convinced that it was completely invalid. Here the most effective critics were George Poulett Scrope, whose criticism centered on the basis of the deluge in physical law and on its universality and its adequacy as an explanation of the excavation of valleys, and Charles Lyell, who eventually criticized the whole philosophic validity of catastrophes as a means of explanation in geology.

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[120] Ibid., pp. 37-38. The work referred to is J. Mathieu Bertrand-Roux, Description géognostique des environs du Puy en Velay, et particulièrement du bassin au milieu duquel cette ville est située (Paris & au Puy, 1823). Bertrand-Roux later adopted the name of Bertrand de Doue and is better known by that name.

CHAPTER IV

THE DILUVIAL THEORY MODIFIED

The diluvial theory was to be modified primarily because of the work of two men, Scrope and Lyell. George Julius Duncombe Poulett Thomson Scrope was born March 10, 1797, at Waverly Abbey, Surrey, the second son of John Poulett Thomson, the wealthy head of a trading firm. He was educated at Harrow and entered Pembroke College, Oxford, but changed in 1816 to St. John's College, Cambridge, from which he graduated in 1821. In the spring of that year he married Emma Phipps Scrope and assumed her name, her father having no other heirs. While at Cambridge Scrope was influenced towards the study of geology by Adam Sedgwick and Edward Daniel Clarke.[1] His first geological investigations were conducted in Italy in 1819-20, where he studied the volcanoes around Naples and in Sicily. In 1821 he investigated the Auvergne region in central France and went from there to Italy, where he observed Mt. Vesuvius in eruption and visited the Ponza Islands. Scrope returned to England in the fall of 1823, becoming a member of the Geological Society in 1824 and one of its secretaries in 1825.

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[1]Edward Daniel Clarke (1769-1822), traveller, antiquary; professor of mineralogy, Cambridge; university librarian.

Charles Lyell was also a secretary, and they both were elected to the Royal Society in 1826. After 1830 Scrope's time was largely taken up by politics, and he was a member of Parliament from 1833 to 1868. Scrope wrote a large number of pamphlets supporting various economic and social reforms and earned the nickname of "Pamphlet Scrope." He died in 1876,[2]

Scrope's first book, published in 1825, was on volcanoes.[3] He assumed that geological appearances were the result of the same or similar causes operating throughout the earth's history, and his definition of geology emphasized the study of the processes of change as well as the knowledge of their effects:

> Geology has for its business a knowledge of the processes which are in continual or occasional operation within the limits of our planet, and the application of these laws to explain the appearances discovered by our Geognostical researches, so as from these materials to deduce conclusions as to the past history of the globe.[4]

Nature's operations in the past can only be understood by examining its operations at present, he said:

> Geologists have usually had recourse for the explanation of these changes to the supposition of sundry violent and extraordinary catastrophes, cataclysms, or general revolutions having

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[2]There is no full-length biography of Scrope.

[3]George Poulett Scrope, Considerations on Volcanos, the Probable Causes of Their Phenomena, the Laws Which Determine Their March, the Disposition of Their Products, and Their Connexion with the Present State and Past History of the Globe: Leading to the Establishment of a New Theory of the Earth (London: W. Phillips, 1825).

[4]Ibid., p. iv.

occurred in the physical state of the earth's surface.

As the idea imparted by the term Cataclysm, Catastrophe, or Revolution, is extremely vague, and may comprehend any thing you choose to imagine, it answers for the time very well as an explanation; that is, it stops further inquiry. But it has also the disadvantage of effectually stopping the advance of the science, by involving it in obscurity and confusion.

If however, in lieu of forming guesses as to what may have been the possible causes and nature of these changes, we pursue that which I conceive the only legitimate path of geological inquiry, and begin by examining the laws of nature which are actually in force, we cannot but perceive that numerous physical phenomena are going on at this moment on the surface of the globe, by which various changes are produced in its constitution and external characters; changes extremely analogous to those of earlier date, whose nature is the main object of geological inquiry.

. . . . . .

Changes, which in their general characters bear so strong an analogy to those which are suspected to have occurred in the earlier ages of the world's history, that, until the processes which give rise to them have been maturely studied under every shape, and then applied with strict impartiality to explain the appearances in question; and until, after a long investigation, and with the most liberal allowances for all possible variations, and an unlimited series of ages, they have been found wholly inadequate to the purpose, it would be the height of absurdity to have recourse to any gratuitous and unexampled hypothesis for the solution of these analogous facts.[5]

Scrope, like Sir James Hall, accepted the evidence for diluvial catastrophes in the past and assumed that they had been caused by large-scale earth movements:

Indeed, with regard to the formation of the successive conglomerate or arenaceous strata, as well as of the traces of excavation and denudation visible on the dry surfaces of the earth, . . . it certainly appears to me at once the most reasonable supposition a priori, and the best warranted conclusion from the facts, . . . that the superficial destruction of . . . the earth's crust, by the erosive force of water in motion, has gone hand in hand with the accumulation of their fragments in alluvial strata; . . . that it has proceeded generally by a lent and uniform process, gradually diminishing in energy from the

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[5] Ibid., pp. iv-vi.

> beginning to the present day; but occasionally presenting partial crises of excessive turbulence, resulting from . . . particularly the sudden elevation of continental masses.[6]

With regard to the elevation of the continents, he stated:

> we must conclude from the analogy of the volcanic phenomena . . . that they were raised by expansive shocks succeeding one another at greater or less intervals.
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> Of these the greater number were probably of minor violence, similar to the earthquakes. . . . But it is also probable that a concurrence of local circumstances favourable to a long predominance of the repressive force, will have occasionally brought on a crisis of intense subterranean dilatation, a paroxysmal expansion, the effect of which on the solid crust of the earth will have been proportionately violent and extensive.[7]

Scrope accepted the vulcanist hypothesis that the earth had originally been in a molten condition and had gradually cooled down, forming a solid crust.[8] The supporters of this hypothesis believed in a hotter climate in the past and attributed it to greater radiation of heat from a hotter earth through a thinner crust.[9] This hypothesis was also consistent with the idea of a gradual decline in earth activity as time went on. Scrope held that there had been a decline not only in the earth's internal activity but also in the rate of circulation of water on the surface, which meant a decline in the rate of surface erosion.[10] He believed that volcanoes and earthquakes were closely connected. The volcanoes acted as safety-valves, releasing the internal energy of the earth relatively harmlessly and thereby

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[6] Ibid., p. 240.

[7] Ibid., pp. 215-16.

[8] Ibid., p. 228.

[9] Ibid., p. 238.

[10] Ibid., pp. 214-15, 218.

preventing a buildup of pressure which could cause extremely violent earthquakes or convulsions. These paroxysmal convulsions should thus be preceded by a long period of relatively little volcanic or earthquake activity.[11]

Such a hypothesis would be opposed to a diluvial theory like that of Buckland, which assumed a recent deluge without postulating any physical cause. A sufficient cause for such a catastrophe, under the vulcanist hypothesis, would be large-scale earth movements, such as those that resulted in the raising of the Alps and perhaps other mountains. To this cause Scrope attributed the excavation of the larger valleys and the production of the diluvian detritus:

> it would seem that the elevation of this colossal European chain, (and perhaps therefore of the whole of Europe) from below the level of the sea, took place by some sudden and tremendous catastrophe of this nature, at a comparatively recent geological epoch.
>
> The traces of (so called) Diluvian action will, in this case, be the result of the denuding force of the waters retreating from this elevated surface, and accompanying their retreat with frequent successive oscillatory movements.
>
> If so stupendous a chain was raised in reality at once to its present height, the commotion necessarily produced in the ocean by such a change, will be fully sufficient to account for all the appearances of an extraordinarily violent action of water subsequent to the deposition of the plastic clay, which are visible over the whole continent of Europe.
>
> The boulders of Jura and the southern slope of the Alps, the filling up of the valley of the Po, and the great alluvial flats of Russia, Poland, Prussia, Denmark, North of Germany, and Holland, will date from this catastrophe; while the creation and earliest outbreakings of the volcanic fissures of France, Germany, Hungary, and Italy, may be supposed to have accompanied the same event.[12]

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[11] Ibid., p. 216.

[12] Ibid., pp. 216-17.

According to Scrope, the formation of some vallies had been caused by the retiring waters of the ocean scooping out rock which had been fissured by earth movements.[13] Fissures and fracturing were associated with only a relatively few valleys, and:

> Many other transverse vallies. . . . were no doubt originally scooped out by these retiring waters alone, without the previous existence of any directing fissure.

Furthermore, all valleys have been more less modified by the action of ordinary rain-wash and stream erosion:

> The vallies of either kind have been subsequently enlarged and otherwise modified; and many others, perhaps indeed a far greater number were wholly and entirely excavated by the slow but constant and powerful action of the same causes which are still continually in force; amongst which the fall of water from the sky, and its abrasive power as it flows over the surface of the land from a higher to a lower level, is the principal.[14]

Scrope's diluvial theory differed from Buckland's in that it connected the deluge with a definite causal event occurring long before the Biblical flood and did not deprecate the power of ordinary erosion.

Sir Charles Lyell, Bart., was born November 14, 1797, at Kinnordy, in Forfarshire, Scotland. He was the oldest of ten children of a wealthy landowner, Charles Lyell (1767-1849), who had both

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[13] Ibid., pp. 213-14. Such a case was the valley of the Weald. The Weald anticline was upheaved, Scrope said, by an upward force generated by the earth's internal heat. As the strata were elevated, fissures occurred along the ridge of the anticline, parallel to the axis of elevation. At the same time much narrower fractures developed perpendicular to this axis. These became the channels or gorges through which now run the rivers that drain the present valley, which was formed by the action of the retiring waters scooping out the broken material in the longitudinal or main fissures.

[14] Ibid., p. 214.

scientific and literary interests. His parents soon moved to the vicinity of Southampton, England, where he spent his childhood.[15] His geological interests were first aroused by reading Bakewell's Introduction to Geology; and later at Oxford, where he enrolled at Exeter College in 1815, he attended the popular geological lectures of Buckland.[16] However, he had been sent there by his father to prepare himself for the study of law, which he entered after receiving his B. A. in 1819.[17] In March, 1819, Lyell became a member of the Geological Society; and he made a number of geological field trips during the early 1820's, some of them with Buckland.[18] His letters during this time reveal him to have been critical of the latter's hasty field work, and he appears to have formed a sceptical attitude toward Buckland's merits as a geologist in general.[19] He did not at this time, however, openly question Buckland's diluvial theory.[20]

A trip to Scotland with Buckland in the late summer of 1824 led to Lyell's first important paper, in which he compared recent fresh-water formations with their ancient counterparts and found them

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[15]Charles Lyell, Life, Letters and Journals of Sir Charles Lyell, Bart., Author of "Principles of Geology" &c., ed. Katherine M. Lyell (2 vols.; London: John Murray, 1881), I, 1-2.

[16]Ibid., p. 32. He apparently attended Buckland's lectures early in 1817, at least his letters before that time do not mention geology.

[17]Ibid., p. 112.

[18]Ibid., pp. 112, 114-20.

[19]Ibid., pp. 121, 161, 164-65.

[20]See his letter of August 10, 1823, to his father, where he

remarkably similar.[21] This paper was to be the first of a long series of geological writings in support of Hutton's idea that to understand the past we must study the present. Through these writings, Lyell was to become, before his death in 1875, one of the most prominent geologists in Great Britain.[22]

In 1826 Lyell wrote a geological article for the Quarterly Review, which, although it was primarily devoted to paleontological discoveries, contained an interesting discussion of the adequacy of present causes to explain geological appearances.[23] He argued that past and present-day processes are analogous and referred to Scrope's book for evidence.[24] However, he questioned Scrope's conclusion that the power displayed by nature had continually decreased. The greater derangement of the older strata was rather the result, Lyell thought,

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states that he "fought hard" with Baron de Ferussac "for Buckland's notions of the Diluvian formation." (ibid., p. 139).

[21]Charles Lyell, "On a Recent Formation of Freshwater Limestone in Forfarshire, and on Some Recent Deposits of Freshwater Marl; with a Comparison of Recent with Ancient Freshwater Formations; and an Appendix on the Gyrogonite or Seed-Vessel of the Chara," Transactions of the Geological Society of London, second series, II, (1829), 73-96. (in Part I, published in 1826).

[22]Besides the Life, Letters, and Journals, which is little more than letters and journals, there are two biographies of Lyell, both of them brief: Thomas G. Bonney, Charles Lyell and Modern Geology (London: Cassell and Company, Limited, 1895) and Sir Edward Bailey, Charles Lyell (Garden City, New York: Doubleday & Company, Inc., 1963).

[23][Charles Lyell], Review of Transactions of the Geological Society of London, series 2, vol. I (1824), Quarterly Review, XXXIV (1826), 507-40. This review is in the number for September, 1826.

[24]Ibid., p. 519.

of the cumulative effect "of the uniform action of the same cause throughout a long succession of ages." The convulsion that produced the Alps was not inferior in violence, he said, to those of the earlier periods. The earth during the period of deposition of the secondary strata was not in a state of chaotic confusion: "There are proofs of occasional convulsions, but there are also proofs of intervening periods of order and tranquillity."[25]

Lyell thought that the strongest evidence for the operation of other than existing causes in the past were the extensive beds of conglomerate:

> There are still . . . some conglomerate rocks in Europe and in America, such, for instance, as are remarkably exhibited both in the old and new red sandstone formations, that evince a continued and destructive action over a great extent of the globe, unparalleled by existing causes. That the sudden elevation of subsidence of land might be attended with such catastrophes will, however, hardly be denied.[26]

He argued strongly for the hypothesis that the "continents have alternately ascended, and descended," which he attributed to Playfair, saying that the discovery of the alternation of marine and freshwater formations, unknown to the latter, had rendered the hypothesis very probable. Deluges and earthquakes were part of the order of nature, he said, and they "have in fact conspired in former periods and at different intervals of time to destroy the productions of nature."[27] He cautiously agreed with Cuvier that a deluge of the sea had caused

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[25] Ibid., p. 518.

[26] Ibid., p. 520.

[27] Ibid., pp. 513-14.

"with great probability the annihilation of the quadrupeds then inhabiting the ancient continents," referring to Cuvier's discoveries in the strata of the Paris basin rather than to the animals of the diluvium. Of this last deluge he said very little, remarking only that the Irish elk had been found buried in "peat and marl, evidently of origin posterior to the last extensive revolution which modified the surface of the land." He did not use Buckland's terms "diluvial" or "diluvium," preferring the older word "alluvial."[28]

Lyell's review is thus a cautious, restrained advocacy of the sufficiency of present causes to produce geological appearances. To Cuvier's opinion that they were insufficient he paid the proper deference, saying that it was

> entitled without doubt to the more respect, as it seems to have been adopted by many in these later times, when additional facts have been so industriously accumulated. The total amount of change that has fallen under the observation of mankind in the course of 3,000 years is, however, so small, that the final decision of this question may certainly be regarded as incalculably remote.[29]

In words reminiscent of Scrope's, he concluded that

> in the present state of our knowledge, it appears premature to assume that existing agents could not, in the lapse of ages, produce such effects as fall principally under the examination of the geologist. It is an assumption, moreover, directly calculated to repress the ardour of inquiry, by destroying all hope of interpreting what is obscure in the past by an accurate investigation of the present phenomena of nature.[30]

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[28] Ibid., pp. 510-11.

[29] Ibid., p. 517.

[30] Ibid., p. 518. Compare Scrope, p. iv.

Lyell thus at this time favored the catastrophic views of Hall and Scrope. Unlike the latter, however, he refused to combine this view with the "cooling-earth" hypothesis.[31]

In 1827 Scrope published his next geological work, based on his investigations in the Auvergne.[32] Most of the book was written in 1822, but it remained unpublished for five years because of the "natural unwillingness of publishers to undertake scientific works with expensive plates."[33] Much of the evidence that Scrope presented in this book against the theory of a universal deluge had long been known to French geologists, but it had not been presented before in so effective a manner or been accompanied by such a convincing chain of arguments.[34]

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[31] Ibid., p. 528-29.

[32] George Poulett Scrope, Memoir on the Geology of Central France: Including the Volcanic Formations of Auvergne, the Velay, and the Vivarais (London: Longman, Rees, Orme, Brown, and Green, 1827).

[33] Ibid., p. ix. The amount of revision between 1822 and 1827 is uncertain. Scrope stated in his preface that the memoir was written in 1822 (p. ix); but Lyell, in his review of the book, commented: "we are much mistaken if we do not recognize in the style, as well as in the arrangement of the Memoir, the revising hand of one who had acquired in the meantime both more extended information and maturer judgment." (Quarterly Review, XXXVI (1827), 439). In any case the footnotes and some of the concluding remarks are clearly later than 1822.

[34] A number of French geologists published works on the Auvergne. According to Scrope, the geologist who provided the key to the interpretation of the phenomena of the region was François Dominique Reynaud, Comte de Montlosier, in his Essai sur la théorie des volcans d'Auvergne (Riom et à Clermont: Landriot et Rousset, an X-1802). An earlier edition was published in 1789.

Scrope pointed out that near Clermont there were three long flows of basalt, two of which could be traced back to existing volcanic cones. All had flowed down into the same valley, where each must have occupied at the time of its flow the lowest level of that basin to which it had access. What appeared to be the oldest basalt flow, the one for which no cone existed, capped a hill some two hundred to four hundred feet higher than the next oldest flow, which capped another hill only a short distance away. Scrope reasoned that the hollow into which the second lava stream originally flowed must have been excavated after the date of the first flow, by which it otherwise would have been occupied. These hills, and many others like them in the same valley, had obviously been preserved by

> the capping of basalt which all alike possess, and which by reason of its superior hardness would naturally protect the underlying strata from the rains, frosts, and other meteoric agents, to which the uncovered intervals of the marly plain left by the emptying of the lake were permanently exposed. Such a capping, on the other hand, would afford a very inefficient protection against the denuding force of any violent deluge or general current of waters, to which some writers have attributed the excavation of the valleys intervening between these high basaltic platforms.[35]

He contended that the direction of any diluvial current must have been the same as that of the valley, or from South to North, whereas the basaltic platforms invariably ran East and West.

In a depression between the two hills on which the basalt flows rested lay a third flow, some five hundred feet lower than the second, appearing as fresh as some in Italy known to have occurred in

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[35] Ibid., p. 160.

historical times. A fourth step in the process of excavation was furnished by a stream which had worn a new channel into and some twenty to fifty feet below the third basalt flow. There was no place in this inferred chain of events for a deluge:

> had the whole excavation effected in the freshwater formation of the Limagne been produced at once, . . . by any diluvial or other violent catastrophe, it is clear that the remnants of the lava-currents which had flowed into the freshwater basin before this epoch, would be necessarily all found at one level, or nearly so, corresponding to the average level of the bottom of the lake-basin at that time; while on the other hand, all the lava-streams which have flowed since the . . . supposed deluge, would be found at another nearly uniform, but much lower level, viz. that of the lowest places of the excavated valley. But, as we have seen, no marked distinction of this sort exists; no line can be drawn to separate the basaltic beds met with at high or low levels. They are found at all heights from 1500 to 15 feet above the water channels of the proximate valleys; and some even of the most distant in point of level are situated geographically close to one another.[36]

The "immense abstraction of matter" that had occurred here could only have taken place "gradually and progressively," Scrope said, the only conceivable agents being the ordinary forces of nature: rain, rivers, frosts, floods, and atmospheric decomposition.

The lavas of the Bas Vivarais, to the Southeast of the Auvergne, offered to Scrope "equally incontestible proofs" of the efficacy of existing causes:

> We see there a number of deep and narrow valleys worn in the flanks of a steep range of granite, which have at a certain epoch been occupied, through a length of several miles, by lava poured in a liquid state from neighbouring volcanic vents, which has evidently filled them up to a high level, exactly as melted metal fills a mould into which it is poured. Since that epoch the valleys have been re-excavated in many parts to more

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[36]Scrope, Memoir . . ., p. 161.

> than their former depth and width, the new channel being cut in some cases through the basaltic lava, in others through the granitic sides of the original valley. Now if the first excavation of these valleys is to be accounted for by the hypothesis of a deluge,--to what are we to attribute the second process? Not, most certainly, to a second deluge; for the undisturbed condition of the volcanic cones, consisting of loose scoriae and ashes, which actually let the foot sink ankle-deep in them, forbids the possibility of supposing any great wave or debacle to have swept over the country since the production of these cones. The amount of excavation which has taken place subsequently to the epoch of these eruptions can then have been only effected by the streams which still flow there; and as this quantity bears a very considerable proportion to the extent of the original excavation, there can be no reasonable grounds for hesitating to attribute the latter to the same agency which effected the former; it being only necessary to assign a longer duration to the process to account for the difference in magnitude of the result.[37]

Scrope argued that the channels of the Loire River and its tributary streams in the basin of Le Puy must have been carved out since the flowing of the lava currents, which cover the plains intervening between the streams, for their corresponding sections are found on the opposite banks of these streams. Yet the lava currents were contemporary with the cones of loose scoriae rising from their surface, cones that would have been destroyed by a violent deluge. The erosive force of the streams which still flow in these channels, together with other existing agents, must have "hollowed out this extensive system of deep, and in some instances (as that of the Loire itself) wide valleys."[38]

The time required for the accomplishment of such great changes by such slow causes was "indeed immense," Scrope admitted. He argued

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[37] Ibid., p. 163.

[38] Ibid., pp. 163-64.

however, that it would be absurd to use this as an argument against "an explanation so unavoidably forced upon us":

> The periods which to our narrow apprehension, and compared with our ephemeral existence, appear of incalculable duration, are in all probability but trifles in the calendar of Nature. It is Geology that, above all other sciences, makes us acquainted with this important, though humiliating fact. Every step we take in its pursuit forces us to make almost unlimited drafts upon antiquity. The leading idea which is present in all our researches, and which accompanies every fresh observation, the sound which to the ear of the student of Nature seems continually echoed from every part of her works, is--
>
> Time!--Time!--Time!
>
> At least, since by a fortunate concurrence of phaenomena we are enabled to prove the valleys which intersect the mountainous district of Central France to have been for the most part gradually excavated by the action of such natural causes as are still at work, surely it is incumbent on us to pause before we attribute similar excavations in other lofty tracts of country, in which, from the absence of recent volcanos, evidence of this nature is wanting, to the occurrence of unexampled and unattested catastrophes, of a purely hypothetical nature! More it is unnecessary to say at present on this subject.[39]

Scrope, in this work, was more cautious than he had been in his earlier book about assuming "paroxysmal expansions" or earth movements resulting in large-scale deluges. He supposed that the initial or primary uplift of the mountains of Central France above the ocean was caused by "a series of successive convulsions with intervals of quiet between them," rather than by "a single and tremendous effort," although he admitted that the latter was possible. This uplift had been followed by the deposition of calcareous strata in a series of freshwater lakes, accompanied by numerous volcanic eruptions. Succeeding this was a second paroxysmal elevation," which burst the

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[39] Ibid., p. 165.

barriers of the lake-basins, causing "one or more sudden debacles, which produced extensive denudations." He felt obliged to suppose this in order to explain "vast accumulations of diluvial matter" in the valleys to the North. This event had been followed by a period, continuing to the present, of occasional volcanic eruptions accompanied by the deepening and widening of valleys by ordinary excavating forces. He speculated that the earth movements in Central France might have been related to the alternations of freshwater and marine sediments found in the Paris basin and elsewhere, these regions being uplifted at the same time as the mountains and then undergoing subsidence as the result of the extensive outpourings of lava that occurred afterwards.[40]

Scrope made no attempt in this work to relate events in Central France to the catastrophic uplift of the Alps that, in his earlier book, he had assumed was the probable cause of the diluvial matter of Europe. With respect to such large-scale convulsions he commented:

> Convulsive oscillations of the ocean, or other aqueous reservoirs, occasioned by the sudden heaving up of large masses of the earth's crust, may have . . . sent repeated waves over parts of our continents, the effect of which would be to open communications between distant basins, to create new and extensive denudations, and accumulate vast beds of transported fragments along the course of these mighty currents. But the proofs of the passage of such destructive deluges over any country are still to seek. Those which have been adduced as yet have this inherent defect, that it is impossible, in the present state of our knowledge, to say that they cannot have been occasioned by the bursting of lake-basins, or other natural agents still in operation, acting during an unlimited period. Before any just estimate can be formed of what share must be attributed to extraordinary catastrophes, and what to these minor but constant excavating forces,

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[40] Ibid., pp. 165-67.

> of the whole amount of change which has been evidently produced by the action of water in motion on the surface of the globe, it is absolutely necessary to acquire a much more definite knowledge of the laws which regulate the circulation of water over the earth's surface, and its effect upon that surface, than we can at present be allowed to possess. It is too true that the greater number of geologists have sat down without hesitation to investigate by a sort of guesswork the origin of the changes and mode of production of the mineral masses which they observe on the surface of the globe, in complete ignorance, or at least with a total neglect, of those processes which are still daily employed by nature in the creation of fresh changes, and the production of new mineral masses on the same surface, bearing a complete analogy, to say the least of it, to the earlier phaenomena, and older formations, which it is the business of geology to account for.[41]

Scrope's book was reviewed in the Quarterly Review for October, 1827, by Lyell, who devoted most of his space to summarizing the book, saying that it was up to Scrope's opponents to refute his arguments if they could:

> It is almost superfluous to remind the reader that they who have a theory to establish, may easily overlook facts which bear against them, and, unconscious of their own partiality, dwell exclusively on what tends to support their opinions. The impression, therefore, made by Mr. Scrope's arguments and illustrative sketches, ought not to be considered as conclusive:--but we must suspend our judgment until his arguments are specifically met by some of the numerous opponents. Their authority alone might be almost conclusive, if we did not know how far the love of system may often mislead, and how prone we are to imagine strong lines of demarcation, where it would be convenient for us if nature had drawn them.[42]

Lyell considered this book to be much superior to Scrope's first work, which he regarded as having contained too many speculations made in

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[41] Ibid., p. 164 (footnote).

[42] [Charles Lyell], Review of Memoir on the Geology of Central France, by George Poulett Scrope, Quarterly Review, XXXVI (1827), 480.

ignorance of the facts. Not all of Scrope's theories in his first book had been "open to general censure," but Lyell had objected "decidedly to many of them, which unfortunately stand forth most prominent." He indicated, however, that he had found the work useful:

> We should, indeed, do great injustice to his first work if we failed to acknowledge that we derived information, as well as amusement, from its perusal; and, in declaring our dissent from many of his opinions, we must entirely disavow the influence of that fashion, now too prevalent in this country, of discountenancing almost all geological speculation.[43]

One of Lyell's purposes in writing his review of Scrope is revealed in a letter of March 2, 1827, in which he wrote concerning Lamarck:

> That the earth is quite as old as he supposes, has long been my creed, and I will try before six months are over to convert the readers of the Quarterly to that heterodox opinion.[44]

Accordingly in the review he cited Scrope on the vastness of geological time.[45] Another purpose was simply to do a favor for a friend, for on April 10, he wrote:

> Scrope has just published a volume on Auvergne. . . . As I am, with many others, indignant at an atrocious article which Macculloch wrote on his late work on volcanoes, in the Westminster, I am determined to give him a moderately long article in the 'Quarterly Review,' a sort of abstract which I conceive will take one-fourth the time of an original article, and the latter, as far as science is concerned, should not be, I am clear, given to a periodical.[46]

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[43] Ibid., pp. 440-41.

[44] Charles Lyell, Letter to Gideon Mantell: London, March 2, 1827, Life, Letters and Journals, I, 168.

[45] [Lyell], Quarterly Review, XXXVI (1827), 474.

[46] Charles Lyell, Letter to his father: April 10, 1827,

Others were also interested in converting the Quarterly Review to more liberal geological views. Lyell related a conversation with Leonard Horner, his future father-in-law, who was

> very a propos to keeping me right in my article, as he is a great education man, as well as geologist. His gratitude to me for having got into the 'Quarterly Review' an article on the liberal side of geology is very agreeable. He is eager to serve me, and wanted me to let him go carefully over the article, with his friend Brougham, which I begged him not to do, as Mr. Brougham might make a good joke out of revisals of 'Quarterly Review' articles. Horner himself is a safe man.[47]

Scrope, in later life, expressed the opinion that Lyell, during the composition of this review,

> may have imbibed that philosophical conviction as to the true method of inquiry into the past history of the globe's surface, namely through a careful study of the processes actually in

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Life, Letters and Journals, I, 170. Macculloch opened his review of Scrope's book with the following comment: "We are afraid that we can bestow no praise on the work before us. Had the author's name not bespoken his English birth, we should have decided it to be the produce of some garreted German, determined to say all that could, and much more than ought to be said, on the subject selected for his labours. It is, with much pretence to novelty, a mere compilation, tedious, endless--endless in repetitions; adding nothing to previous knowledge, and diluting through 270 weary pages, what might be contained in a dozen. Why weary our own readers with a review of it? Why tell them that where the writer fancies he has discovered a new theory of volcanoes, and a new theory of the earth, he does not perceive that he is repeating what has been a thousand times proposed, and as often disputed--hackneyed nonsense. Is it possible that he who has read all the books on volcanoes has not also read the most common geological works? When will those who set up as teachers commence by being learners?" See [John Macculloch], Review of Considerations on Volcanos, by George Poulett Scrope, Westminster Review, V (1826), 356.

[47]Lyell to his father, April 10, 1827, Life, Letters and Journals, I, 169-70. Both Horner and Brougham were frequent contributors to the Edinburgh Review, the chief competitor and political opponent of the Quarterly Review.

> operation upon it, which is the leading principle of his deservedly popular works.[48]

The influence on Lyell of both of Scrope's books was undoubtedly great. Certainly Lyell by this time had become convinced of the uniformity of past and present causes. He also was planning to write a book about which he had written to his friend Gideon Mantell:

> I am going to write in confirmation of ancient causes having been the same as modern, and to show that those plants and animals which we know are becoming preserved now, are the same as were formerly. E.g., scarcely any insects now, no lichens, no mosses, &c., ever get to places where they can become imbedded in strata. But quadrupeds do in lakes, reptiles in estuaries, corals in reefs, fish in sea, plants wherever there is water, salt or fresh, &c. &c. Now have you ever in Lewes levels found a bird's skeleton or any cetacea? if not, why in Tilgate and the Weald beds? In our Scotch marl, though water birds abound in those lakes, we meet with no birds in the marl; and they must be at least as rare as in old freshwater formations, for they are much worked and examined. You see the drift of my argument--ergo, mammalia existed when the oolite and coal, &c., were formed. . . . If I am asked why in coal there are no quadrupeds? I answer, why are there none, nor any cetecea, nor any birds, nor any reptiles in the plastic clay, or lignite formation, a very analogous deposit, and as universal in Europe.[49]

While very little of the foregoing was reflected in Lyell's review of Scrope, traces of these speculations on life in the past can be found in his 1826 review.[50]

During 1827 Lyell worked steadily on his book and by the end of the year had almost completed it. However, as he wrote later,

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[48]George Poulett Scrope, The Geology and Extinct Volcanos of Central France (2d ed.; London: John Murray, 1858), pp. vii-viii.

[49]Lyell to Mantell, March 2, 1827, Life, Letters and Journals, I, 169.

[50][Lyell], Quarterly Review, XXXIV (1826), 529-32.

"many causes concurred to delay the completion of the work, and considerably to modify the original plan."[51] One of the delays was a trip with Murchison:

> In May 1828, when the preliminary chapters on the History of Geology, and some others which follow them in the first volume, were nearly finished, I became anxious to visit several parts of the continent, in order to acquire more information concerning the tertiary formations. Accordingly, I set out in May, 1828, in company with Mr. Murchison, on a tour through France and the north of Italy.[52]

The purpose of the trip was to learn more about the secondary formations in France and in particular to relate to the geology of northern France the fresh-water formations which Scrope had described in the Auvergne region.[53] The need for this information had been pointed out by Lyell in his review.[54] Lyell and Murchison confirmed what Scrope had found with regard to the excavation of valleys, as well as adding considerably to the knowledge of the fresh-water formations in the region.

The results of this work were three joint papers, of which one, on the excavation of valleys, created a considerable stir when it was presented to the Geological Society.[55] Lyell and Murchison concluded

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[51]Charles Lyell, Principles of Geology: Being an Inquiry How Far the Former Changes of the Earth's Surface Are Referable to Causes Now in Operation (4 vols.; 3d ed; London: John Murray, 1834), I, iii.

[52]Ibid., pp. iii-iv.

[53]Charles Lyell, Letter to his father: Clermont Ferrand, Auvergne, May 16, 1828, Life, Letters and Journals, I, 184.

[54][Lyell], Quarterly Review, XXXVI (1827), 443. 446.

[55]Charles Lyell and Roderick Impey Murchison, "On the Excavation of Valleys, as Illustrated by the Volcanic Rocks of Central France," Edinburgh New Philosophical Journal, VII (1829), 15-48.

that there was no evidence in the Auvergne region of a violent universal flood: "In a word, the repeated investigations of the ablest observers have been unable to discover a single fragment of any rock inclosing marine remains, mixed up with the alluvions of the primary, tertiary, and volcanic districts of Central France."[56] Lyell, who had heard about it from Murchison, Fitton, and Scrope, described the meeting thus:

> Seventy persons present the second evening, and a warm debate, Buckland and Greenough furious, contra Scrope, Sedgwick, and Warburton, supporting us.[57]

In September Lyell went on alone to the South of Italy. He visited on the way several collectors of fossil shells and noted the large proportion of species which were still living. On the island of Ischia, he was greatly surprised to find fossil shells, of species still in existence, at an elevation of two thousand feet.[58] Sicily was also a surprise to him, for he found "full proof that half Sicily

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Read December 5 and 19, 1828. The other two papers were "On the Tertiary Fresh-water Formations of Aix, in Provence, including the Coal-field of Fuveau," Edinburgh New Philosophical Journal, VII (1829), 287-93, read June 19, 1829, and "On the Tertiary Deposits of the Cantal, and Their Relation to the Primary and Volcanic Rocks," Annales des Sciences Naturelles, XVIII (1829), 173-214 (in French), read April 3 and May 1, 1829.

[56]Lyell and Murchison, Edinburgh New Philosophical Journal, VII (1829), 46.

[57]Charles Lyell, Letter to his sister (Marianne): Rome, January 21, 1829, Life, Letters and Journals, I, 238.

[58]Charles Lyell, Principles of Geology, Being an Attempt to Explain the Former Changes of the Earth's Surface, by Reference to Causes Now in Operation (3 vols.; London: John Murray, 1830-32-33), III, ix.

was formed since the Mediterranean was inhabited by present species of testacea."[59]

On his return to Naples he wrote a letter to Murchison, in which he discussed the plan of his book:

> My work is in part written, and all planned. It will not pretend to give even an abstract of all that is known in geology, but it will endeavour to establish the principle of reasoning in the science; and all my geology will come in as illustration of my views of those principles, and as evidence strengthening the system necessarily arising out of the admission of such principles, which, as you know, are neither more nor less than that no causes whatever have from the earliest time to which we can look back, to the present, ever acted, but those now acting; and that they never acted with different degrees of energy from that which they now exert.

The whole letter radiated self-assurance and included the following confident passage:

> This year we have by our joint tour fathomed the depth and ascertained the shallowness of the geologists of France and Italy as to their original observations. We can without fear measure our strength against most of those in our own land, and the question is, whether Germany is stronger.[60]

The major result of Lyell's Italian trip was his establishment of four subdivisions of the Tertiary epoch by means of a classification system based on the proportion of fossil species still existing. Only the oldest of the four divisions (Eocene) was well represented in England and Northern France, the other three (Miocene and Older and Newer Pliocene) being established on the basis of Italian formations. He convinced himself that there had been no sharp break anywhere in

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[59]Lyell to his sister (Marianne), January 21, 1829, Life, Letters and Journals, I, 239.

[60]Charles Lyell, Letter to Roderick Murchison: Naples, January 15, 1829, Life, Letters and Journals, I, 234-35.

the life of the Tertiary epoch. On the contrary, there had been, he argued, a continuous creation of new species and the extinction of old.[61]

The paper by Lyell and Murchison on the excavation of valleys prompted a reply by Conybeare.[62] He contended that the valleys in basin of the Thames River were "exclusively the result of denudation, and therefore better suited to illustrate that operation than valleys of more complicated origin, in the formation of which the elevation and dislocation of the strata have co-operated."[63] In his introductory remarks, Conybeare referred to the opposition of the "fluvialists" and the "diluvialists":

> the former ascribing such denudations exclusively to the operation of the streams actually existing, or rather to the drainage of the atmospherical waters falling on the districts, which it is supposed have become thus deeply furrowed by the gradual erosion of these waters, continued through a long and indefinite series of ages; the latter contending that such a cause is totally inadequate to the solution of the phaenomena, and maintaining that they afford evidence of having been produced by violent diluvial currents.[64]

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[61]Lyell, Principles . . . (1st ed.), III, xiii.

[62]W. D. Conybeare, "On the Hydrographical Basin of the Thames, with a View More Especially to Investigate the Causes Which Have Operated in the Formation of the Valleys of That River, and Its Tributary Streams," Philosophical Magazine, ser. 2, VI (1829), 61-65. This is only an abstract of the paper, which was read to the Geological Society on May 15 and June 5, 1829. The paper was never published in full.

[63]Ibid., pp. 61-62.

[64]Ibid., p. 62.

Conybeare distinguished several different epochs "at which it is probable that currents must have taken place calculated to excavate and modify the existing surface." These were:

> I. In the ocean, beneath which the strata were originally deposited. II. During the retreat of that ocean. III. At the periods of more violent disturbance, which are evidenced by the occurrence of fragmentarian rocks, the result of violent agitations in the waters of the then existing ocean propagated from the shocks attendant on the elevation and dislocation of of the strata.[65]

He enumerated four periods in the third category "as having left distinct traces in the English strata," corresponding to the times of the deposition of the conglomerates associated respectively with the old red sandstone, new red sandstone, plastic clay, and diluvium. The "superficial gravel" or diluvium, he said, "may be identified as the product of one area, by the same evidence which is employed to demonstrate the unity of any other geological formation."[66]

Conybeare's arguments for the diluvial formation of the valleys of the Thames basin contained little that was new. For example, he argued that the diluvium covering the plain of Oxford could not have been deposited by the present streams, because most of it is now inaccessible to floods and, if it were produced by the present streams,

> we must suppose that they have repeatedly changed their channel so as to have flowed successively over every portion of the

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[65] Ibid., p. 62.

[66] Ibid.

> plain where these debris are now found: the oldest historical monuments attest, however, the permanence of the actual channels, and the floods at present bring down no pebbles whatsoever.[67]

His argument was continually that rivers could not have transported so much material, or could not have shifted their channels, or could not have existed where none are now found because we have no evidence that any of these things have occurred in historical times.

Lyell commented on the reading of Conybeare's paper in letters to Mantell and Fleming. To the former he wrote:

> The last discharge of Conybeare's artillery, served by the great Oxford engineer against the Fluvialists, as they are pleased to term us, drew upon them on Friday a sharp volley of musketry from all sides, and such a broadside at the finale from Sedgwick, as was enough to sink the 'Reliquiae Diluvianae' for ever, and makes the second volume shy of venturing out to sea.
> . . . . . .
> Sedgwick, who decided on four or more deluges, said the simultaneousness was disproved for ever, &c., and declared that on the nature of such floods we should at present 'doubt, and not dogmatise.'[68]

Lyell's description of the meeting in his letter to Fleming was more detailed:

> Buckland was so amazingly annoyed at my having had such an antidiluvialist paper read, that he got Conybeare to write a controversial essay on the Valley of the Thames, in which he drew a comparison between the theory of the Fluvialists, as he terms us, and the Diluvialists, as (God be praised) they call themselves.
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> Of course, in defining the Fluvialists, they (for Buckland wrote half the memoir) took care to build up their man of straw,

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[67] Ibid., p. 63.

[68] Charles Lyell, Letter to Gideon Mantell: London, June 7, 1829, Life, Letters and Journals, I, 253. Gideon Algernon Mantell (1790-1852), a surgeon of Lewes, Sussex; published many works on geology and paleontology.

> and triumphantly knocked him down again. But in the animated discussion which followed the reading of the first half of the essay, at the Geological Society, we made no small impression on them. And when, last Friday, the remainder came on, we had a hot reencounter. Buckland came up on purpose again, and made a leading speech. But after we had exposed him, and even Greenough, his only staunch supporter, had given in on many points, Sedgwick, now president, closed the debate with a terribly antidiluvialist declaration. For he has at last come round, and is as decided as you are. But you must know that Buckland now, and Conybeare, distinctly admit three universal deluges, and many catastrophes, as they call them, besides![69]

The diluvial hypothesis of the excavation of valleys was defended by De la Beche in the Philosophical Magazine for October, 1829.[70] It appeared to him that the "two rival theories may be reconciled with the facts presented by nature, and that both are, to a certain extent, correct." He readily admitted that "rivers, more particularly those discharged from the many lakes that probably once existed, have cut deeply into the land, and have formed gulleys, ravines, and gorges," but it seemed to him "utterly at variance with the relations of cause and effect, to suppose that valleys, properly so called, could have been formed either by the discharge of lacustrine waters, or by the rivers that now run, or could ever have run, in them."[71]

He referred to the Char Valley in Southwest England, drained by an insignificant river that "has not accomplished more than a cut varying from four to fifteen feet deep." He pointed out that the

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[69]Charles Lyell, Letter to John Fleming: Temple, June 10, 1829, Life, Letters and Journals, I, 254.

[70]Henry Thomas De la Beche, "Notice on the Excavation of Valleys," Philosophical Magazine, ser. 2, VI (1829), 241-48.

[71]Ibid., p. 241.

walls of the cut were composed for the most part, not of the strata out of which the valley had been excavated, but of diluvial gravel. The only adequate agent seemed to him to have been "a voluminous mass of moving waters, to the duration of which I will not venture to assign a time." He noted that this agent seemed to have operated universally, "for in all countries there are inequalities of surface, independent of stratification."[72] In general, he said, the existing streams produce "gorges, ravines and gulleys, cliffs, taluses and landslips," and may greatly modify the effects of denudation. The rounded forms of the anterior valleys, however, were produced "by a force acting generally and with enormous power; a force scarcely referable to any other cause than a voluminous mass of overwhelming waters."[73]

The general belief of most diluvialists was that all alluvial deposits had been produced after the deluge. Sedgwick, for example, asserted in 1825 that "all the alluvial detritus, of whatever kind," was "posterior" to the diluvium "because it constantly rests upon it, and never alternates with it." The diluvial deposits, he said, "rest on the ancient strata of the country without the intervention of any other deposit whatsoever."[74] He regarded the alluvial deposits, therefore, as having been "completed within a very limited period,"

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[72] Ibid., p. 243.

[73] Ibid., p. 248.

[74] Adam Sedgwick, "On the Origin of Alluvial and Diluvian Formations," Annals of Philosophy, new series, IX (1825), 247-48.

and as belonging to an epoch distinct from that previous to the deluge.[75]

Buckland had shown that there was an antediluvian epoch in England in which animals of species now extinct, but similar to existing species, had existed. There was abundant evidence, however, that at least some animals indistinguishable from members of existing species had also existed in the antediluvian epoch. Presumably these animals had lived under conditions not greatly different from existing ones. There had been rivers and lakes depositing materials not essentially different from the alluvial deposits of the present. Although the antediluvian epoch may have been so brief that no substantial deposits had accumulated, one could still conceive of antediluvian alluvium.

The contrast between the antediluvian and postdiluvian epochs was heightened by the evidence that the former epoch had a much warmer climate. As late as 1829 De la Beche supposed a general difference in the species of animal life of the two epochs. There were a few exceptions, he admitted, "but the body of evidence seems to render a new creation presumable." Man and the monkeys, he thought, were the most important of the new creations. He clearly regarded the geological deluge as much anterior to the Biblical flood and the alluvial epoch as very long, for he remarked that "geologically speaking, the epoch is recent; but, according to our general ideas of time, it

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[75] Ibid., p. 254.

appears to be one that reaches back far beyond the dates usually assigned to the present order of things."[76]

The greater the contrast that could be shown between the antediluvian and postdiluvian epochs, the more credible could be made the idea of a great and violent deluge separating them. Yet the Bible seemed to imply that conditions on the earth before the flood were not different from those after and especially that man and the present animals had existed before the flood. Many religious people were disturbed about the tendency among geologists to contrast the conditions before and after the deluge. One who did something about it was the Rev. William V. Vernon, president of the Yorkshire Philosophical Society, who investigated some fossil bones in a deposit of marl lying beneath the diluvium.[77] He carefully dug a pit in the antediluvian marl and noticed the relative positions of the various bones. The discovery of the skull of a bison, an animal of temperate climates, beneath the bones of the elephant and the rhinoceros and the finding of land and fresh-water shells identical with present ones, confirmed his opinion that there had been no change in the climate.[78] He concluded that the marl had been deposited under tranquil

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[76]Henry Thomas De la Beche, "Sketch of a Classification of the European Rocks," Philosophical Magazine, ser. 2, VI (1829), 444.

[77]William V. Vernon, "On a Discovery of Fossil Bones in a Marl-Pit near North Cliff," Philosophical Magazine, ser. 2, VI (1829), 225-30. The Rev. William Vernon Harcourt (1789-1871), Canon of York, 1821-71; F.R.S., 1824; general secretary to the first meeting of the British Association for the Advancement of Science, 1831; son of Edward Vernon Harcourt (1757-1847), archbishop of York, 1807-47; took the name Harcourt in 1831.

[78]William V. Vernon, "Further Examination of the Deposit of

conditions in the same way that river marl is deposited at present.[79] In other words, this marl was antediluvian alluvium.

Vernon objected to De la Beche's statement that a new creation had occurred after the deluge. Nor was there "any evidence at all against the creation of 'Man and the Monkeys' having preceded the geological deluge." The only inference, he said, that could be made from the absence of the bones of monkeys was "that which has been long since indicated by Cuvier, that the antediluvian animals of Europe were not the same as the animal population of the torrid zone." The only consequence that he thought could be inferred from the absence of human bones in the antediluvian deposits of Europe was that "the regions of the earth which we inhabit were not peopled before the Deluge." Vernon's conclusions from his investigation were:

> first, that there has been since the creation of the present order of animated beings a general deluge, which destroyed a great multitude of those animals and extinguished several species; and secondly, that this deluge followed the creation at no very considerable interval of time, and before mankind had overspread the earth.[81]

An anonymous correspondent pointed out that it was absurd to retain the terms alluvium and diluvium when Vernon had "clearly shown the occurrence of alluvial deposits previous to any signs of

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Fossil Bones at North Cliff in the County of York," Philosophical Magazine, ser. 2, VII (1830), 4.

[79] Ibid., p. 5.

[80] Ibid., pp. 8-9.

[81] Ibid., p. 9.

diluvial action having taken place." He suggested that these two types of deposits were not clearly distinguishable:

> the first of the alluvial deposits will manifestly be coincident with the commencement of the present state of our globe (meaning thereby, the effects of the last of the geological revolutions, the one immediately preceding the creation of the cavern hyaenas and bears), and will therefore precede, be covered, and perhaps partly destroyed by, the earliest diluvial catastrophe. Where, then, are the limits of this group to be found? Or, how are we to know the one from the other? Not surely by the presence of sand, and rolled portions of the older rocks; for these we may find among the silty deposits of any considerable river. We must in this case confine the diluvial group to the gravel and boulders on hill-tops and other situations, not likely to have ever formed part of the beds of water-courses.[82]

He asked, in the light of Vernon's discovery:

> if therefore we retain the terms, and distinguish . . . the diluvial beds by large stones being present, must we not divide the groups into three,--antediluvial alluvia, diluvia, post-diluvial alluvia? Or perhaps a fourth would be necessary, designating a silty deposit lying between two gravel beds, if we should ever meet with such a section as diluvial alluvia.[83]

Like Vernon, he criticized De la Beche for expressing the unscriptural opinion that man and the monkeys had been created after the deluge. The evidence, he said, was "purely negative, and scarcely able to bear sifting." He had no doubt that man would soon be found in a fossil state. With reference to scientific opinions contrary to the Bible, he stated:

> I am by no means an advocate for bringing geology into contact with the Bible: on the contrary, I think that for the present at least they must essentially be kept separate; and I therefore dislike the occurrence of the term "diluvial" in any system of the

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[82]Anon., "Observations on Some Parts of Mr. De la Beche's Paper on the Classification of the European Rocks," Philosophical Magazine, ser. 2, VII (1830), 192.

[83]Ibid.

> science, as continually bringing into view the Noachian Deluge, as if geologists assigned it as the cause of all the gravel and rubbish on the earth's surface. Yet, as it must be the wish of every Christian to see the two accounts of the occurrences on our globe agree (and they no doubt ultimately will agree), I think it would be better if opinions opposed to the Divine Record in parts where it is distinctly expressed, were omitted, unless supported by a very wide induction of facts collected from every quarter; and then only mentioned as true in very qualified terms. The treatises of Penn, Faber, and others, show how impossible it is at present to offer the two in connection: while the "Scriptural Geology" forms a miserable instance of what ignorance and prejudice can effect when wandering from the subjects really in debate; it stretches every expression of the inspired historian further than even common sense can warrant, and treats the Bible as a full record of science.[84]

In the spring of 1830 Lyell was hard at work correcting page proofs for his book.[85] With respect to the problem of obtaining favourable reviews he was in an advantageous position. His greatest danger, the Quarterly Review, he had obviated by his previous connection with it as a writer, by his friendship with the editor, John Lockhart, and most importantly, by his choice of a publisher. For John Murray, Lyell's publisher, owned the Quarterly. Its leading rival, the Edinburgh Review, had always been sympathetic to the Huttonian theory, so there was no danger to be expected from that quarter. It was too much, however, to expect the Edinburgh Review to notice a work published by Murray.

The choice of Scrope as the Quarterly's reviewer was apparently arranged in a conversation between Lyell and Lockhart. Lyell then wrote Scrope, urging him to accept. On May 9, Scrope replied:

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[84] Ibid., p. 193.

[85] Lyell, Life, Letters and Journals, I, 260-62.

> Your conversation with Lockhart is very agreeable flattery. He has not applied to me yet on the subject. If he does, I shall accept conditionally--demurring till I have seen your book. I misdoubt my capacity as your reviewer--more particularly as I presume there will be much discussion on the determination of age by organic remains, etc of which I am an incompetent judge. Again I fear I shall not be able to hit the exact line in discussing your ante Mosaical heresies; not being an adept at playing the hypocrite, I shall hardly write with the proper unction of a Quarterly Reviewer, concerning "that profound historical and theological cosmogonist" the worthy Patriarch. However, Sedgwick's discourse and authority will go far to help me.[86]

Scrope agreed to write the review, and after reading the advance sheets of the first part of the volume wrote Lyell to express his approval:

> What I admire in you is the assurance with which you speak of doctrines still supported by the Bucklands, Conybeares etc as exploded errors, past praying for. You stride on indeed at so rapid a pace that there is no time for them to contest the point with you. Your sketch of the History of Geology is admirable and illustrates so usefully the absurdity and mischief of the Theo-Geological systems, that to strengthen my argument against them, you must allow me to give a sort of abstract of it in my article. If between us we can succeed in freeing Geology once and forever from the clutches of Moses, we shall have deserved well of the science.[87]

Scrope also expressed some doubt as to the amount of freedom that would be allowed him by the editor with reference to religion. Lyell in reply assured him on this point:

> I am sure you may get into Q. R. what will free the science from Moses, for if treated seriously, the party are quite prepared for it. A bishop, Buckland ascertained (we suppose Sumner),

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[86]George Poulett Scrope, MS letter to Charles Lyell: Castle Combe, May 9, 1830, Darwin-Lyell Correspondence, American Philosophical Society, Philadelphia, Pa. He referred to Sedgwick's presidential address to the Geological Society in February.

[87]George Poulett Scrope, MS letter to Charles Lyell: Castle Combe, June 11, 1830, Darwin-Lyell Correspondence, American Philosophical Society, Philadelphia, Pa.

> gave Ure a dressing in the 'British Critic and Theological Review.' They see at last the mischief and scandal brought on them by Mosaic systems.

Lyell wished to convince people, not offend them, and he hoped to strike the proper balance in his book:

> I was afraid to point the moral, as much as you can do in Q. R. about Moses. Perhaps I should have been tenderer about the Koran. Don't meddle much with that, if at all.
>
> If we don't irritate, which I fear that we may (though mere history), we shall carry all with us. If you don't triumph over them, but compliment the liberality and candour of the present age, the bishops and enlightened saints will join us in despising both the ancient and modern physico-theologians. It is just the time to strike, so rejoice that, sinner as you are, the Q. R. is open to you. If I have said more than some will like, yet I give you my word that full half of my history and comments was cut out, and even many facts; because either I, or Stokes, or Broderip, felt that it was anticipating twenty of thirty years of the march of honest feeling to declare it undisguisedly. Nor did I dare come down to modern offenders. They themselves will be ashamed of seeing how they will look by-and-by in the page of history, if they ever get into it, which I doubt.
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> . . . . . .
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> I conceived the idea five or six years ago, that if ever the Mosaic geology could be set down without giving offense, it would be in an historical sketch, and you must abstract mine, in order to have as little to say as possible yourself. Let them feel it, and point the moral.[88]

In his next letter to Scrope, Lyell expressed the same feeling of cautious optimism:

> I am more anxious than I can tell you that you should hit it off well for Q. R. Of such an article as many reviews would jump at, there is no fear; but if Murray has to push my vols., and you wield the geology of the Q. R., we shall be able in a short time to work an entire change in public opinion.[89]

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[88]Charles Lyell, Letter to George Poulett Scrope: Temple, June 14, 1830, Life, Letters and Journals, I, 268-71. Charles Stokes (1783-1853), a member of the Stock Exchange and a collector of scientific and art objects. William John Broderip (1789-1859), a lawyer and naturalist.

[89]Charles Lyell, Letter to George Poulett Scrope: London, June 20, 1830, Life, Letters and Journals, I, 273.

The book appeared in early August.[90]

Scrope's review appeared in the October number of the Quarterly Review.[91] After lengthy preliminary remarks on the utility and the sublimity of geology as a pursuit and on the proofs which it furnishes of "a Designing Intelligence" and of "a First Cause, acting by uniform, invariable laws," he discussed the folly of mixing science and revelation: "To the scriptures, true knowledge has never been hostile, nor is it possible that they, when properly interpreted, should ever be enemies to it."[92] On the other hand, he argued:

> To bring forward the scriptures as the foundation of geology, or geological hypotheses as a support to the scriptural relations, is to degrade the sacred writings, as well as to impede the progress of knowledge . . . to couple the unchangeable dictates of Revelation with what has hitherto been constantly liable to change. Whenever this has been attempted, the result has been injurious to both science and religion, and the history of geology, up to the present hour, teems with instances of this truth. . . . Yet, to this hour, some are found who . . . continue to vamp up and send forth their stale and ridiculous theories as scientific commentaries on holy writ, and to write on geology as if this branch of knowledge had no other end but to afford conclusions respecting the Mosaic chronology and the phenomena of the deluge.[93]

After this blast at the scriptural geologists, Scrope introduced Lyell's book, which he hailed as marking "the beginning of a

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[90]Conybeare wrote on August 20 that he had just received it from his bookseller: see "Letter from the Rev. W. D. Conybeare, M.A. F.R.S. F.G.S. &c. on Mr. Lyell's Principles of Geology," Philosophical Magazine, Ser. 2, VIII (1830), 215.

[91][George Poulett Scrope], Review of Principles of Geology, by Charles Lyell, Quarterly Review, XLIII (1830), 411-69.

[92]Ibid., pp. 413-14.

[93]Ibid., p. 414.

new era in geology."[94] He quoted and paraphrased Lyell's historical sketch at length; and he was especially severe on Werner, whose influence illustrated "the danger of allowing authority and enthusiasm to supersede rational inquiry."[95] Even more than Lyell, Scrope stressed the immensity of geological time. Before the creation of any of the existing species:

> the earth had been inhabited by innumerable other species, and other genera, successively created and extinguished during a lapse of time wholly immeasurable, but which must have comprehended millions of ages rather than of years.[96]

Lyell was well pleased by Scrope's review and complimented him handsomely. It was, he wrote Scrope, "incomparably the best thing you ever wrote. . . . Such a broad-side will do far more than my book to sink the diluvialists, and in short all the theological sophists."[97]

Lyell's opening words in the first volume of the Principles were his definition of geology:

> Geology is the science which investigates the successive changes that have taken place in the organic and inorganic kingdoms of nature; it enquires into the causes of these changes, and the influence which they have exerted in modifying the surface and external structure of our planet.[98]

There was little difference between this definition and that of Scrope

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[94] Ibid., p. 417.

[95] Ibid., p. 422.

[96] Ibid., p. 425.

[97] Charles Lyell, Letter to George Poulett Scrope: London, November 9, 1830, Life, Letters and Journals, I, 310.

[98] Lyell, Principles (1st ed.), I, 1.

in his work on volcanoes.[99] This definition changed geology into a historical science, making it a study of processes rather than rocks. It was a reversal of the trend of the previous thirty years, which tried to make geology more like natural history--that is, to make it a science whose main object was to classify and correlate rocks and strata. This former point of view was exemplified in Conybeare's definition of geology as "the knowledge of the Earth's structure as far as it lies open to our observation."[100]

Lyell next proceeded to make explicit the analogy of geology with history:

> As the present condition of nations is the result of many antecedent changes, some extremely remote and others recent, some gradual, others sudden and violent, so the state of the natural world is the result of a long succession of events, and if we would enlarge our experience of the present economy of nature, we must investigate the effects of her operations in former epochs.[101]

As the historian uses the moral sciences to interpret the past so the geologist makes use of the physical sciences.[102]

Lyell asserted that geology is not a subordinate branch of mineralogy, as Werner apparently believed, or part of physical geography, where Desmarest put it. Geology is also not cosmogony; and

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[99]Scrope, Considerations on Volcanos . . ., p. iv.

[100]W. D. Conybeare and William Phillips, Outlines of the Geology of England and Wales, with an Introductory Compendium of the General Principles of That Science, and Comparative Views of the Structure of Foreign Countries (London: William Phillips, 1822), p. ii.

[101]Lyell, Principles . . . (1st ed.), I, 1.

[102]Ibid., p. 2.

it was Hutton, he said, who first distinguished between the two, declaring that geology was not concerned "with questions as to the origin of things." Pursuing the historical analogy, Lyell insisted that "geology differs as widely from cosmogony, as speculations concerning the creation of man differ from history."[103]

These preliminary observations were followed by a seventy-page sketch of the history of geological thought from the ancient Hindus to Lyell's own time.[104] A history of the science was not a new feature in general geological works, for most English books on the subject had some historical references, if only a mention of the Huttonian and Wernerian theories. Lyell's sketch was probably suggested by that of Conybeare's.[105] The two were so similar in their treatment of the Greek and Roman authors that Conybeare publicly asked Lyell to acknowledge the use of his work.[106]

Lyell's account of the history of geological thought was more balanced than Conybeare's, covering continental geology much more thoroughly. A major object of this history was to convince

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[103] Ibid., p. 4.

[104] Ibid., pp. 5-74.

[105] Conybeare and Phillips, pp. xxxviii-xlix.

[106] W. D. Conybeare, "An Examination of Those Phaenomena of Geology, Which Seem to Bear Most Directly on Theoretical Speculations," Philosophical Magazine, ser. 2, VIII (1830), 401. See also Charles Lyell, "Reply to a Note in the Rev. Mr. Conybeare's Paper Entitled 'An Examination of Those Phaenomena of Geology, which Seem to Bear Most Directly on Theoretical Speculations,'" Philosophical Magazine, ser. 2, IX (1831), 1-3, and W. D. Conybeare, "An Examination . . .," Philosophical Magazine, ser. 2, IX (1831), 116-17.

the reader that geology had been held back by theological prepossessions. As an illustration of this, Lyell pointed out how, in the sixteenth century,

> the clear and philosophical views of Fracastoro were disregarded, and the talent and argumentative powers of the learned were doomed for three centuries to be wasted in the discussion of these two simple and preliminary questions: first, whether fossil remains had ever belonged to living creatures; and, secondly, whether, if this be admitted, all the phaenomena could be explained by the Noachian deluge.[107]

Lyell maintained that the argument in the seventeenth century over whether fossils were the remains of living organisms had been basically a theological one. Many who held the view that the earth was created a few thousand years ago, essentially as it is now, were very reluctant to admit that the earth had been "inhabited by living beings long before many of the mountains were formed."[108] There was also an unwillingness based on theological views to believe that species could have become extinct.[109] Of the hypothesis that fossils had been buried by the flood of Noah, he remarked: "Never did a theoretical fallacy, in any branch of science, interfere more seriously with accurate observation and the systematic classification of facts."[110] As late as 1751, he pointed out, Buffon was forced by the theological faculty of the Sorbonne to renounce the principle "that the present mountains and valleys of the earth are due to secondary causes, and

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[107]Lyell, Principles . . . (1st ed.), I, 24.

[108]Ibid., p. 28.

[109]Ibid., p. 31.

[110]Ibid., pp. 29-30.

that the same causes will in time destroy all the continents, hills and valleys, and reproduce others like them."[111] Lyell commented that "it is no longer controverted that the present continents are of secondary origin," knowing well that Buckland's theory assumed that the present valleys were produced in large part by the Deluge.[112]

His criticism of Werner was severe but no more than that which had become common in British geological writings.[113] The former success of Neptunism, Lyell believed, was the result of its theological acceptability: "by a singular coincidence, Neptunianism and orthodoxy were now associated in the same creed."[114] On the other hand he gave a full and appreciative account of Hutton and his theory.[115] He cited the outcry raised against Hutton as another example of harmful religious prejudice.[116] Hutton's theory was a great step forward because it excluded "all causes not supposed to belong to the present order of nature." Its greatest defect "consisted in the undue influence attributed to subterranean heat, which was supposed necessary for the consolidation of all submarine deposits."[117] Lyell then criticized

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[111] Ibid., pp. 48-49.

[112] Ibid., p. 49.

[113] Ibid., pp. 55-58.

[114] Ibid., p. 69.

[115] Ibid., pp. 60-65.

[116] Ibid., pp. 67-70.

[117] Ibid., p. 63.

Hutton, not for going too far, as had previous critics, but for not going far enough in assuming the uniformity of nature:

> Hutton made no step beyond Hooke, Moro, and Raspe, in pointing out in what manner the laws now governing earthquakes, might bring about geological changes, if sufficient time be allowed. On the contrary, he seems to have fallen far short of some of their views. He imagined that the continents were first gradually destroyed, and when their ruins had furnished materials for new continents, they were upheaved by violent and paroxysmal convulsions. He therefore required alternate periods of disturbance and repose, and such he believed had been, and would for ever be, the course of nature.[118]

He added:

> There can be no doubt, that periods of disturbance and repose have followed each other in succession in every region of the globe, but it may be equally true, that the energy of the subterranean movements has been always uniform as regards the <u>whole earth</u>.[119]

Another defect of Hutton's thought was his inadequate appreciation of organic remains: "They merely served him as they did Werner to characterize certain strata, and to prove their marine origin.[120]

Lyell's conception of the Huttonian theory appears to have been formed largely by reading Playfair and Hall rather than Hutton. He admitted that he had "found it difficult to read and remember Hutton, and though I tried, I doubt whether I ever fairly read more than half his writings, and skimmed the rest."[121] His treatment of Hutton was to be gently criticized in 1839 by Fitton, who believed

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[118] <u>Ibid</u>., pp. 63-64. See also p. 88.

[119] <u>Ibid</u>., p. 64.

[120] <u>Ibid</u>.

[121] Lyell, <u>Life, Letters and Journals</u>, II, 47-48.

that Hutton's theory anticipated Lyell's more than he would admit.[122] In a letter to Fitton, Lyell replied that he had given Hutton adequate credit:

> Considering at how late a period . . . he came into the field, and consequently how much greater were his opportunities, I think his knowledge and his original views were confined to too small a range of the vast science of geology, to entitle him to such marked and almost exclusive pre-eminence as you contend for in his behalf. . .
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> . . . . . .
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> In my first chapter I gave Hutton credit for first separating geology from other sciences, and declaring it to have no concern with the origin of things, and after rapidly discussing a great number of celebrated writers, I pause to give, comparatively speaking, full-length portraits of Werner and Hutton, giving to the latter the decided palm of theoretical excellence, and alluding to the two grand points in which he advanced the science. First, the igneous origin of granite, secondly that the so-called primitive rocks were altered strata. . . . The mottos of my first two volumes were especially selected from Playfair's Huttonian Theory, because although I was brought round slowly, against some of my early prejudices, to adopt Playfair's doctrines to the full extent, I was desirous to acknowledge his and Hutton's priority.
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> . . .
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> It was my business . . . to estimate the importance of each writer, and adjust the quantity of space due to him, . . . not simply according to his originality and genius, but partly at least in proportion to his influence; and I still think that Werner's eloquence, popularity, enthusiasm, and position at Freyberg, placed him in this point of view as much above Hutton as I have represented him to fall below him in reference to the truth of his theories. Yet . . . all I could have wished is, that your panegryic on Hutton had appeared as aiding and seconding my efforts, since I trust that no book has made the claims of Hutton better known on the Continent of late years than mine.[123]

No mention was made in Lyell's letter about a discussion the previous year at a meeting of the Geological Society, in which Fitton

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[122][William Fitton], Review of Elements of Geology, by Charley Lyell, Edinburgh Review, LXIX (1839), 406-66.

[123]Charles Lyell, Letter to William Fitton: Kinnordy, August 1, 1839, Life, Letters and Journals, II, 48-50.

had charged Lyell "with not having done justice to Hutton." Fitton had maintained that the latter had believed in "gradual elevation," to which Lyell had replied that "most of the critics had attacked me for overrating Hutton, and that Playfair understood him as I did."[124] Others, when they read Hutton, felt as Fitton did. The geologist, Andrew C. Ramsay, while preparing lectures on the history of geology, noted in 1847: "Hutton every day strikes me with astonishment. Lyell does not do him half justice."[125]

Lyell, in the first volume of the Principles, did not refer directly to the diluvial theory. His historical sketch, which emphasized the harmful effects on geology of religious preconceptions, was intended to create an attitude favorable to the discussion of the theory on its scientific merits alone. His general argument against the validity of catastrophic hypotheses in geological speculation was intended to remove what he considered to be a general bias in favor of such assumptions. Instead of confronting the diluvial theory directly Lyell preferred to rob it of its importance by attempting to provide other explanations for the phenomena that were supposed to have been caused by diluvial action. He speculated that the basin of the Thames River might have been convulsed at the time when volcanic activity in the Auvergne had been most intensive, in the same

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[124]Charles Lyell, Letter to Leonard Horner: London, March 12, 1838, ibid., 40-41.

[125]Sir Archibald Geikie, Memoir of Sir Andrew Crombie Ramsay (London: Macmillan and Co., 1895), p. 117.

way as modern volcanoes and earthquakes are sometimes related. He contended that, if the Thames valley had been thus affected,

> and the relative levels of its several parts altered (an hypothesis in perfect accordance with modern analogy), the difficulties of some theorists might, perhaps, be removed; and they might no longer feel themselves under the necessity of resorting to catastrophes out of the ordinary course of Nature, when they endeavour to explain the alluvial phenomena of that district.[126]

Lyell cited instances of streams that had been temporarily dammed by landslides created by earthquakes or heavy rains, resulting in disastrous floods and the movement of immense amounts of earth and rock. The "diluvial" matter deposited by these floods, he said, would very likely contain the remains of large quadrupeds:

> It is almost superfluous to point out to the reader that the lower alluvial plains are most exposed to such violent floods, and are at the same time best fitted for the sustenance of herbivorous animals. If, therefore, any organic remains are found amidst the superficial heaps of transported matter, resulting from those catastrophes, at whatever periods they may have happened, and whatever may have been the former configuration and relative levels of the country, we may expect the imbedded fossil relics to be principally referrible to this class of mammalia.[127]

Without entering into a discussion of the general question of the origin of valleys, he cited numerous examples of the excavation of valleys by the streams now occupying them: in Sicily, in North America (the Niagara River), and in Central France, where "there are decisive proofs that neither the sea nor any denuding wave, or extraordinary body of water, have passed over the spot," since the

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[126] Lyell, Principles . . . (1st ed.), I, 192. He was referring, of course, to Conybeare's paper on the Thames basin.

[127] Ibid., p. 194. See also pp. 433-34.

occurrence of most of the lava flows.[128] He agreed with Scrope that "the sinuosity of deep valleys is one among many proofs that they have been shaped out progressively," and he argued that river erosion, acting in concert with earthquakes over a long period of time, was capable of creating a system of deep and wide valleys:

> Provided, therefore, we suppose the elevation and subsidence of mountain-chains to be a gradual process, there is no difficulty in explaining how the rivers draining our continents have converted ravines into valleys, and enlarged and deepened valleys to an enormous extent. On the contrary, the signs of slow and gradual action so manifest in the sinuosities and other characters of valleys are admirably reconcileable with the great width and depth of the excavations, if we are content not only to suppose a great succession of ordinary earthquakes, but also the usual intervals of time between the shocks.[129]

Lyell believed that the earthquakes that uplifted "our more ancient tertiary strata" acted in conjunction with the rivers "at some former epoch" just as the earthquakes that have "upraised newer strata to the height of several thousand feet in the south of Italy" have cooperated with the streams there to produce "deep valleys and ravines." He contended that "more change is effected in two centuries" in Calabria, Italy, "than in many thousand years in a country as undisturbed by earthquakes as Great Britain." He added, alluding to Conybeare:

> He who studies the hydrographical basin of the Thames, and compares its present state with its condition when it was a Roman province, may have good reason to declare that if that river and its tributaries had since their origin been always as inactive, and as impotent as they are now, they could never, not even in millions of years, have excavated the valleys through which they

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[128] Ibid., p. 177.

[129] Ibid., pp. 432-33.

> flow; but, if he concludes from these premises, that the valleys in this basin were not formed by ordinary causes, he reasons like one, who having found a solfatara which for many centuries has thrown out nothing more than vapour and a few handfuls of sand and scoriae, infers that a lofty cone, composed of successive streams of lava and ejections, can no longer be produced by volcanic agency.[130]

In the second volume of his Principles, published in 1832, Lyell considered the problem of the extinction of species and concluded that species are destroyed in the ordinary course of nature and that the destruction of species has occurred regularly throughout the history of the earth as the result of the operation of ordinary causes. He argued that similarly new species might have continuously been created as a part of the regular order of nature. Therefore, the assumption of special acts of wholesale creation or of destructive catastrophes was unnecessary to explain the creation and destruction of species.[131]

He supported by additional evidence his argument that large land quadrupeds could be buried in great numbers by local floods; and he offered the following hypothesis as an explanation of the relatively large numbers of the bones of such quadrupeds found in the diluvium:

> Now let us suppose that in a tract of land constantly inhabited by terrestrial quadrupeds, the species are thrice changed under the gradual influence of causes before considered in this volume, and that, during the first and last of these zoological epochs, the district remains entirely free from earthquakes, but is violently convulsed by them during the intermediate era,--we should expect, for reasons above considered, that the fossil

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[130] Ibid., pp. 434-35.

[131] Lyell, Principles . . . (1st ed.), II, 168, 176, 182-83.

> remains of quadrupeds, buried in alluvium, would be confined to one period only, viz., that of the subterranean movements. If the series of shocks should happen not to have occupied the whole of the second epoch, but only a small portion of it, there might be no indication whatever, in the fossil relics, of a passage from one state of the organic world to another. The transition would appear abrupt; and they who, for the sake of economizing past time, do not hesitate to magnify the energies of natural agents in by-gone ages, might then imagine one paroxysmal earthquake to have caused all the fissures, caverns, and depressions, and one accompanying deluge to have filled the whole with alluvial matter, annihilating, at the same time, the race of quadrupeds of which the bones remain interred.[132]

Lyell discussed Buckland's cave evidence at some length and cited at least one cave, in Belgium, that did not contain the single layer of mud uninterrupted by stalagmite that Buckland had invariably observed. On the contrary, it had three layers of stalagmite with alternating beds of mud containing bones. He offered two admittedly imperfect explanations for the single layer of mud observed by Buckland. The principal cause, he suspected, was that each succeeding local flood had torn up the mud and stalagmite laid down before it. Another cause might be:

> that in a country in which torrents and rivers are gradually deepening their channels, and cutting through masses of cavernous limestone (an excavating process which is most rapid during epochs of subterranean disturbance, when the levels of a district are altered), it will only happen once that the stream will break into hollows or fissures communicating with a certain series of caverns. When the erosive action has proceeded farther, and the river has sunk to a greater depth, the drainage of the country will be effected in a valley at a level inferior to that of the caves, and consequently no transported matter will afterwards be introduced into them.[133]

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[132] Ibid., pp. 228-33.

[133] Ibid., p. 222. Curiously he did not discuss the evidence of the Kirkdale Cave perhaps because he felt that to do so would be to criticize Buckland unnecessarily.

Although he mentioned a number of cases in which human bones were found in conjunction with those of extinct quadrupeds, he hesitated to conclude that they were contemporaneous because the deposits were not clearly undisturbed and stratified:

> It is not on such evidence that we shall readily be induced to admit either the high antiquity of the human race, or the recent date of certain lost species of quadrupeds.[134]

Lyell waited until the third volume of the Principles, published in 1833, before he attacked Buckland's diluvial theory directly. The Biblical flood, he said, may have been a partial flood, affecting only that region inhabited by man, in which case it could be accounted for by the bursting of a large lake or the overflow of the sea into an extensive area that was below sea level. If the flood was universal, he agreed with Fleming that the Biblical account represents it as not violent. "For our own part," he said,

> we have always considered the flood, if we are required to admit its universality in the strictest sense of the term, as a preternatural event far beyond the reach of philosophical inquiry, whether as to the secondary causes employed to produce it, or the effects most likely to result from it. At the same time, it is evident that they who are desirous of pointing out the coincidence of geological phenomena with the occurrence of such a general catastrophe, must neglect no one of the circumstances enumerated in the Mosaic history, least of all so remarkable a fact as that the olive remained standing while the waters were abating.[135]

With regard to the diluvium, Lyell revived Hall's hypothesis that the erratic blocks found in it might have been floated away from the Alps or from other mountains on icebergs and then dumped at great

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[134] Ibid., p. 227. He here disagrees with Fleming.

[135] Lyell, Principles . . . (1st ed.), III, 273.

distances while the continents of the northern hemisphere were still submerged beneath the sea.[136] The parallel grooves, or scratches, on rock found by Hall in many places in Scotland, Lyell regarded as having been caused "by the friction of blocks rolled along the floor of the ocean before the country emerged from the deep."[137] He later modified this hypothesis so as to assume that the blocks floated along imbedded in icebergs.[138]

In this volume Lyell emphasized the activity of the ocean waves and currents in the work of erosion and in the formation of "marine alluvium": the remnants of the general alluvial covering that had been formed by the sea while the land was still submerged. Although he didn't attribute to this cause "the greater part of the alluviums," he thought that it could explain "some of those which have been justly regarded as most singular and anomalous, both in position and in the discordance of their contents with any known rocks in the adjacent countries."[139]

To the activity of the ocean waves and currents while the land was still submerged, Lyell also attributed the formation of the valleys of denudation in southeast England, in particular that of the

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[136] Ibid., pp. 148-51. He had previously proposed this hypothesis in I, 299.

[137] Ibid., p. 147.

[138] Charles Lyell, Principles of Geology: or, the Modern Changes of the Earth and Its Inhabitants, Considered as Illustrative of Geology (3 vols.; 6th ed.; London: John Murray, 1840), I, 381.

[139] Lyell, Principles . . . (1st ed.), III, 147.

Weald, which he discussed at great length.[140] The Weald valley, a denuded anticline whose axis runs east-west, was assumed by him to have been formed while the land was slowly rising above the sea. The uplift that produced the anticline so shattered the strata in the process that the center of the anticline was easily eroded away by the action of the ocean waves and currents operating over a long period of time.[141] The material thus removed formed the tertiary strata now found to the north and south of the valley. The transverse gorges, cutting through the downs that rim the valley on the north and south, he explained, like Scrope, as fractures formed at the time of uplift. He believed that they had been subsequently enlarged by the ocean waves.[142] Since the material removed from the Weald valley was supposed to have been laid down to the north and south, his hypothesis required that the bulk of it must have been carried by the ocean currents through these gorges, rather than out to the east, where the valley was presumably wide open to the sea.[143]

Why couldn't the present streams have excavated the Weald and the gorges as well? If one assumes that the center of the Weald was originally higher than the chalk downs, the present pattern of drainage can be explained by the hypothesis that the existing streams

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[140] Ibid., pp. 285-323.

[141] Ibid., pp. 294-95.

[142] Ibid., p. 302.

[143] Ibid., p. 322.

have excavated it. Lyell rejected this hypothesis, apparently because he regarded the amount of material removed from the Weald as too great to have been accomplished by streams. He was also much impressed by the resemblance of the edges of the chalk downs facing the valley to wave-cut cliffs.[144] He argued that the gorges, "so far at least as they are due to aqueous erosion, have not been produced by the rivers, many of which . . . have filled up arms of the sea, instead of deepening the hollows which they traverse."[145]

This argument was, in effect, that the rivers can not have excavated their valleys because they are not now doing so. His argument actually was only that the rivers flow "through a nearly level plain" and that they are forming deltas, believing these facts to be incompatible with the hypothesis that the rivers are excavating the gorges.[146] The whole argument was weak and remarkably similar to some that he had refuted in his book. For example, at the end of his discussion of the Weald, he criticized the following:

> that in a country free from subterranean movements, the action of running water is so trifling that it could never hollow out, in any lapse of ages, a deep system of valleys, and, <u>therefore</u>, no known combination of existing causes could ever have given rise to our present valleys![147]

Apparently Lyell was so entranced by his new agent--"the denuding

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[144] Lyell, <u>Principles . . .</u> (1st ed.), III, 289, 291.

[145] <u>Ibid</u>., p. 300.

[146] <u>Ibid</u>.

[147] <u>Ibid</u>., p. 320.

power of the ocean, during the rise of our continents from the deep," which was "more important, perhaps," than all other causes--and so anxious to demonstrate its role in the formation of the present surface that he deliberately rejected any alternative explanation of the formation of the Weald valley. He would be heavily criticized for this and other similar inconsistencies by George Greenwood in 1857.[148]

Many geologists agreed with Lyell's forthright stand against theological influence in geology but did not accept his uniformitarian bias. Scrope thought that Lyell was unjustified in ruling out catastrophes as a part of geological theory, but he was overjoyed at Lyell's attack on scriptural geology.[149] The appointment of Lyell in the spring of 1831 as professor of geology at King's College in London was a distinct victory for the liberal position in theology, as the college was under the control of the Church of England. Scrope's reaction, as quoted by Lyell, was:

> If the news be true, and your opinions are to be taken at once into the bosom of the Church, instead of contending against that party for half a century, then, indeed, shall we make a

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[148]George Greenwood, Rain and Rivers: or, Hutton and Playfair against Lyell and All Comers (London: Longman, Brown, Green, Longmans, & Roberts, 1857).

[149]George Poulett Scrope, MS letter to Charles Lyell: Castle Combe, March 20, 1832, Darwin-Lyell Correspondence, American Philosophical Society, Philadelphia, Pa., contains the following criticism: "If your antagonists deny the minor degrees of violence altogether, or as being the most frequent, they are decidedly deserting the analogy--but if you deny on y^{r} side the probability of the major and catastrophical events having sometimes taken place, you will equally sin against the same law."

step at once of fifty years in the science--in such a miracle will I believe when I see it performed.[150]

A month later Scrope wrote:

> I am delighted to hear you are secure of your appointment, not only because it may be agreeable to yourself, but as a Geologist, thinking it an immense step in the march of that Science in this country. You are the Head and Front of the offending sect who insist on separating Geology from Scripture, & pursuing the former in complete independence of any & every bearing it may be supposed to have on the latter. By espousing you, therefore, the Conclave have decidedly & irrevocably attached themselves to that liberal side, and sanctioned in the most direct & open manner the principle thus advocated. Had they on the contrary made their election of a Mosaic geologist, like Buckland or Conybeare, the orthodox would have immediately taken their cue from them, and for a quarter of a century to come, it would have been Heresy to deny the excavation of vallies by the Deluge, and Atheism to talk of anything but Chaos have lived before Adam.
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> At the same time I have a malicious satisfaction in seeing the minority of Bigwigs swallow the new doctrine 'upon compulsion,' rather than from taste, & sh[d] enjoy their wry faces as they find themselves obliged to take it, like physic, to avoid the peril of worse evils. I feel some satisfaction in the thought of having given a helping hand to the good cause at this crisis in its fate.[151]

Sir George Mackenzie, a follower of the Hall school of Huttonianism, praised Lyell's book:

> I have just concluded the perusal of your first volume, & have derived peculiar satisfaction from its containing a clear & able exposition of views which I have long entertained, & for the elucidation of which you have brought together, in an able manner, a splendid collection of facts. The system of nature is established in all its parts on perpetual destruction & renewal; and altho' I believed that some time or other this would

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[150] Charles Lyell, Letter to Gideon Mantell: March, 1831, Life, Letters and Journals, I, 317.

[151] George Poulett Scrope, MS letter to Charles Lyell: Castle Combe, April 12, 1831, Darwin-Lyell Correspondence, American Philosophical Society, Philadelphia, Pa.

> be demonstrated to extend to inanimate matter, I did not expect to see it done in my own time.

Mackenzie's approval did not extend to Lyell's anti-catastrophic stand, for later in the letter he speculated on whether diluvium in Scotland "had been caused by the elevation of land to the Westward throwing a great wave over most of Europe." He thought "that we have had successive debacles which have brought about the present order of the alluvial matter." Mackenzie did, however, agree with Lyell's attack on Moses and commented that "the Mosaic history of man will not stand examination as a divine revelation, & bears on itself marks which excite great doubt."[152]

Starting in 1828, the presidential addresses of the Geological Society were printed, and an examination of them gives some insight into the changes that geology was undergoing. Fitton, who gave the address in 1828, commented on the rapid advance that the vulcanist theory had recently made:

> It is no longer denied, that volcanic power has been active during all the revolutions which the surface of the globe has undergone, and has probably been itself the cause of many of them;--and that our continents have not merely been shaken by some mighty subterraneous force, but that strata, originally horizontal, have thus been raised, shattered, and contorted, and traversed, perhaps repeatedly, by veins of fluid matter.[153]

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[152]Sir George Steuart Mackenzie, MS letter to Charles Lyell: Cowl, November 29, 1830, Darwin-Lyell Correspondence, American Philosophical Society, Philadelphia, Pa.

[153]William H. Fitton, Presidential Address Delivered to the Geological Society of London, February 15, 1828, Philosophical Magazine, ser. 2, III (1828), 295.

These developments, he said, had vindicated some parts of the Huttonian theory:

> Whatever, therefore, be the fate of the Huttonian theory in general, it must be admitted, that many of its leading propositions have been confirmed in a manner which the inventor could not have foreseen.[154]

Fitton thought that geology had progressed because it had not degenerated into mere fact collecting, and he gave the influence of Playfair a major share of the credit for preventing this:

> his geological writings have had, indirectly, an effect in accelerating the progress of our subject, the benefit of which we experience at this moment, and probably shall long continue to feel; and which, perhaps, outweighs in value the partial success of the speculations for which he so strenuously contended. He clothed our subject with the dignity of an eloquence most happily adapted to philosophic inquiry, and redeemed the geologist from association with that class of naturalists who lose sight of general laws, and are occupied incessantly with details;--placing him, where he ought to stand, beside the mathematician, the astronomer, and the chemist; and permanently raising our science into an elevated department of inductive inquiry.[155]

Fitton, in his address in 1829, dwelled upon the confirmation by Lyell and Murchison of Scrope's findings in the Auvergne: and he forecast that

> as the doctrine of Werner, which ascribed to volcanic power an almost accidental origin, and an unimportant office, has long since expired; so the more recent views, which regard a certain class of causes as having ceased from acting, will probably give place to an opinion, that the forces from whence the present appearances have resulted, are in Geology, as in Astronomy and general Physics, permanently connected with the constitution, and structure of the Globe.[156]

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[154] Ibid., pp. 295-96.

[155] Ibid., p. 296.

[156] William H. Fitton, Presidential Address Delivered to

He spoke also of the sublimity of geology. "The geologist," he said,

> like the astronomer, is called upon to trace the operation of forces, not only vast beyond conception in themselves, but acquiring almost infinite augmentation of effect, from the numberless ages during which they have been unremittingly exerted; and the problem, to explain the condition of the earth's surface at any moment of this career, is complicated as much perhaps as any other in physics, from the nature of the agents, of which change and irregularity appear to be essential characteristics.[157]

These agents, such as "the degradation of the surface by the atmosphere, the erosion of streams and torrents, the encroachments of the sea, the growth and decay of the organized beings that successively inhabit the globe," as well as "the great phaenomena of volcanic agency," when "viewed . . . in relation to the vast periods of time, during which phaenomena of the same kind have been continually recurring, . . . acquire a sort of uniformity":

> They intimate the repetition of results in future, resembling those which seem already to have occurred repeatedly in the history of the globe; and that part of the Huttonian theory, where the course of geological revolution has been compared to the cycles, in the movements of the heavenly bodies,--in which, after a long series of periodical deviations, the same order is sure to recur,--seems to acquire new probability from every step of our progress, and to be really no less just, in a philosophic view, than it is captivating to the imagination.[158]

The growth in Fitton's estimation of the Huttonian theory is evident from these two addresses.

The presidential address in 1830 was delivered by Sedgwick, who tried to reconcile the conflict between the evidence presented

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the Geological Society of London, February 20, 1829, Philosophical Magazine, ser. 2, V (1829), 463.

[157] Ibid., pp. 463-64.

[158] Ibid., p. 464.

against diluvial excavation of valleys by Lyell, Murchison, and Scrope, and that presented for it by Conybeare. Sedgwick declared that in his opinion:

> the existing river drainage of our physical region, is a complex result, depending upon many conditions--the time when the region first became dry land--its external form at the time of its first elevation above the sea--and all the successive disturbing forces which have since acted upon its surface. But none of these elements are constant: no wonder, then, that results derived from distant parts of the earth should be so greatly in conflict with each other. In the formation of valleys there is therefore little wisdom in attributing every thing to the action of one modifying cause.[159]

One of the key points of the vulcanist theory is the assumption that violent large-scale earth movements have happened many times in the history of the earth. From this assumption it would logically follow that the phenomena that can be attributed to catastrophic agency are the result, not of one such event, but of several catastrophes that may have happened in the more recent geological past. The vulcanist theory would, therefore, be opposed to the assumption of a single, great, recent catastrophic deluge, but would argue that there must have been a number of such events. In this view, catastrophes are a part of nature and should have been, if any thing, more violent in the earlier history of the earth.

The strength of the vulcanist theory by 1830 is revealed by Sedgwick's comment that:

> We know by direct geological evidence, that nearly all the solid portions of the earth were once under the sea, and were lifted

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[159]Adam Sedgwick, Presidential Address Delivered to the Geological Society of London, February 19, 1830, Philosophical Magazine, ser. 2, VII (1830), 293-94.

> to their present elevation, not at one time, but during many distinct periods. We know that elevating forces have not only acted in different places at different times, but with such variations of intensity, that the same formation is in one country horizontal, in another vertical; in one country occupies the plains, in another is only found at the tops of the highest mountains. Now every great irregular elevation of the land (independently of all other results) must have produced, not merely a rush of the retiring waters of the sea, but a destruction of equilibrium among the waters of inland drainage. Effects like these must have been followed by changes in the channels of rivers, by the bursting of lakes, by great debacles, and in short by all the phaenomena of denudation. In comparison of distant parts of the earth, we may therefore affirm that the periods of denudation do not belong to one, but to many successive epochs. And by parity of reasoning we may conclude that the great masses of incoherent matter which lie scattered over so many parts of the surface of the earth, belong also to successive epochs, and partake of the same complexity of formation.[160]

He concluded that the excavation of valleys was

> a complex result, depending upon all the forces, which, acting on the surface of the earth, since it rose above the waters, have fashioned it into its present form. We have old oceanic valleys which were formed at the bottom of the sea in times anterior to the elevation of our continents. . . . We have longitudinal valleys formed along the line of junction of two contiguous formations, simply by the elevation of their beds. . . . We have other valleys of more complex origin; where the beds through which the waters now pass have been bent and fractured with an inverted dip at the period of their elevation. . . . We have valleys of disruption, marking the direction of cracks and fissures, produced by great upheaving forces. . . . Of valleys of denudation our island offers a countless number. Some are of simple origin: . . . which appear to have been swept out by one flood of retiring waters during some period of elevation. Others are of complex origin, and are referrible to many periods, and to several independent causes. Lastly, we have valleys of simple erosion: such are some of the deep gorges and rivers channels in the high regions of Auvergne, excavated solely by the long continued attrition of the rivers which still flow through them.[161]

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[160] Ibid., p. 294.

[161] Ibid.

Sedgwick and Murchison had made a trip to the Alps in the summer of 1829, and the results were described in Sedgwick's presidential address. The two geologists substantiated the theory that the Alps had recently been elevated and found large deposits of sandstone and conglomerate derived from the degradation of the Alps that could not be distinguished from the surface diluvium. These deposits, Sedgwick said, "sometimes contain bones of mammalia," but "are regularly stratified, and alternate with beds containing marine shells." Therefore, "they cannot have been caused by any transient inundation."[162] In other words, here were beds, indistinguishable from the diluvium, that could not possibly have been produced by a single, transient deluge.

This trip appears to have accelerated Sedgwick's conversion to a modified diluvial theory. In a letter to Murchison written in November, 1831, Sedgwick stated that his "change of mind (at least in part)" had begun somewhat earlier, during their journey to the Scottish Highlands in the summer of 1827, "where there are so many indications of local diluvial operations." However, he added that during their trip to the Alps, he was influenced by talking to Alexander von Humboldt, who "ridiculed" the doctrine "beyond measure when I met him in Paris," while Constant Prévost "lectured against it."[163]

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[162] Ibid., p. 297.

[163] John Willis Clark and Thomas McKenny Hughes, The Life and Letters of the Reverend Adam Sedgwick, LL.D., D.C.L., F.R.S., Fellow of Trinity College, Cambridge, Prebendary of Norwich,

Lyell received the impression of a remarkable change in Sedgwick's views, for in a letter of October, 1829, he said:

> Sedgwick and Murchison are just returned, the former full of magnificent views. Throws overboard all the diluvian hypothesis; is vexed he ever lost time about such a complete humbug; says he lost two years by having also started a Wernerian. He says primary rocks are not primary, but, as Hutton supposed, some igneous, some altered secondary.[164]

It took some time, however, for Sedgwick to announce his conversion publicly. In his presidential address in 1830 he proposed a classification, based in part on Lyell's recent findings in Italy, that assumed the diluvial gravel to have been the result of a number of successive periods of formation occurring long before Buckland's supposed deluge.[165] It was not until his presidential address to the Geological Society in 1831, after the publication of the first volume of Lyell's Principles, that Sedgwick publicly repudiated any connection between the diluvium and the Biblical flood. The work of Élie de Beaumont, he said, had shown that the diluvium in Europe was the product of several different periods of elevation.[166] For

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Woodwardian Professor of Geology, 1818-1873 (2 vols.; Cambridge: At the University Press, 1890), I, 371. Constant Prévost (1787-1856) was a French geologist of uniformitarian views. Alexander von Humboldt (1769-1859) was a celebrated German naturalist and traveller.

[164]Charles Lyell, Letter to John Fleming, 9 Crown Office Row, Temple: October 31, 1829, Life, Letters and Journals, I, 256.

[165]Sedgwick, Philosophical Magazine, ser. 2, VII (1830), 301.

[166]Jean Baptiste Armand Louis Leonce Élie de Beaumont (1798-1874), French geologist. De Beaumont's system was presented in his "Recherches sur quelques-unes des révolutions de la surface du globe, présentant différents exemples de coïncidence entre le redressement

example, in the Alps de Beaumont had distinguished two distinct deposits of diluvial gravel. Much of the Alps "were elevated after the deposit of the older diluvium," and the "newer diluvium (including all those enormous crystalline erratic blocks so admirably described by Saussure) rolled off from the regions of the higher Alps during this last period of their elevation."[167]

Since the diluvium could be shown to be of several different ages, the simple theory of its production by a single transient deluge was obviously no longer tenable:

> That these statements militate against opinions, but a few years since held almost universally among us, cannot be denied. But theories of diluvial gravel, like all other ardent generalizations of an advancing science, must ever be regarded but as shifting hypotheses to be modified by every new fact, till at length they become accordant with all the phaenomena of nature.
>
> In retreating where we have advanced too far, there is neither compromise of dignity nor loss of strength; for in doing this, we partake but of the common fortune of every one who enters on a field of investigation like our own. All the noble generalizations of Cuvier, and all the beautiful discoveries of Buckland, as far as they are the results of fair induction, will ever remain unshaken by the progress of discovery. It is only to theoretical opinions that my remarks have any application.
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> . . . . . .
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> Bearing upon this difficult question, there is, I think, one great negative conclusion now incontestably established--that the vast masses of diluvial gravel, scattered almost over the surface

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des couches de certains systèmes de montagnes, et les changements soudains qui ont produit les lignes de démarcation qu'on observe entre certains étages consécutifs des terrains de sédiment," Annales des Sciences Naturelles, XVIII (1829), 5-25, 284-416; XIX (1830), 5-99, 177-240.

[167] Adam Sedgwick, "Address to the Geological Society, Delivered on the Evening of the 18th of February, 1831," Philosophical Magazine, ser. 2, IX (1831), 281-317.

> of the earth, do not belong to one violent and transitory period. It was indeed a most unwarranted conclusion, when we assumed the contemporaneity of all the superficial gravel on the earth. We saw the clearest traces of diluvial action, and we had, in our sacred histories, the record of a general deluge. On this double testimony it was, that we gave a unity to a vast succession of phaenomena, not one of which we perfectly comprehended, and under the name diluvium, classed them all together.
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> To seek the light of physical truth by reasoning of this kind, is, in the language of Bacon, to seek the living among the dead, and will ever end in erroneous induction. Our errors were, however, natural, and of the same kind which led many excellent observers of a former century to refer all the secondary formations of geology to the Noachian deluge.[168]

The next statement was characteristic of the forthright nature of Sedgwick:

> Having been myself a believer, and, to the best of my power, a propagator of what I now regard as a philosophic heresy, and having more than once been quoted for opinions I do not now maintain, I think it quite right, as one of my last acts before I quit this Chair, thus publicly to read my recantation.
>
> We ought, indeed, to have paused before we first adopted the diluvian theory, and referred all our old superficial gravel to the action of the Mosaic flood. For of man, and the works of his hands, we have not yet found a single trace among the remnants of a former world entombed in these ancient deposits. In classing together distant unknown formations under one name; in giving him a simultaneous origin, and in determining their date, not by the organic remains we had discovered, but by those we expected hypothetically hereafter to discover, in them; we have given one more example of the passion with which the mind fastens upon general conclusions, and of the readiness with which it leaves the consideration of unconnected truths.[169]

Sedgwick demonstrated "the passion with which the mind fastens upon general conclusions" in seizing so readily upon the system of de Beaumont, which had many similarities to that proposed by Scrope in 1825. The former, he said, had "proved . . . that whole mountain chains have

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[168] Ibid., pp. 313-14.

[169] Ibid.

been elevated at one geological period" and had gathered "an immovable mass of evidence" that leads us to "conclude that there have been in the history of the earth long periods of comparative repose, during which the sedimentary deposits went on in regular continuity, and comparatively short periods of violence and revolution, during which that continuity was broken.[170] De Beaumont's system, he said, was "directly opposed" to the uniformitarian philosophy of Lyell's Principles.[171]

Sedgwick had been led to renounce Buckland's theory by his belief that science and religion are two separate and distinct roads to a unified body of truth. In his address in 1830 he had expressed this philosophy in the following manner:

> Laws for the government of intellectual beings, and laws by which material things are held together, have not one common element to connect them. And to seek for an exposition of the phaenomena of the natural world among the records of the moral destinies of mankind, would be as unwise, as to look for rules of moral government among the laws of chemical combination. From the unnatural union of things so utterly incongruous, there has from time to time sprung up in this country a deformed progeny of heretical and fantastical conclusions, by which sober philosophy has been put to open shame, and sometimes even the charities of life have been exposed to violation.
>
> No opinion can be heretical but that which is not true. Conflicting falsehoods we can comprehend; but truths can never war against each other. I affirm, therefore, that we have nothing to fear from the results of our inquiries, provided they be followed in the laborious, but secure road of honest induction. In this way we may rest assured that we shall never arrive at conclusions opposed to any truth, either physical or moral, from whatsoever source that truth may be derived: nay rather (as in

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[170] Ibid., pp. 308, 311.

[171] Ibid., pp. 303-07, 311-12. Lyell was much opposed to De Beaumont's theory: See Lyell, Principles . . . (1st ed.), III, 341-51.

> all truth there is a common essence), that new discoveries will ever lend support and illustration to things which are already known, by giving us a larger insight into the universal harmonies of nature.[172]

Sedgwick's repudiation of Buckland's evidence for the Biblical deluge did not mean that he had ceased to believe in it as a physical event or even that he had given up the hope of finding traces of it. De Beaumont's system, he felt, gave support to the physical possibility of such an event:

> Are then the facts of our science opposed to the sacred records? And do we deny the reality of a historic deluge? I utterly reject such an inference. Moral and physical truth may partake of a common essence, but as far as we are concerned, their foundations are independent, and have not one common element. And in the narrations of a great fatal catastrophe, handed down to us, not in our sacred books only, but in the traditions of all nations, there is not a word to justify us in looking to any mere physical monuments as the intelligible records of that event: such monuments, at least, have not yet been found. . . . If, however, we should hereafter discover the skeletons of ancient tribes, and the works of ancient art buried in the superficial detritus of any large region of the earth; then, and not till then, we may speculate about their stature and their manners and their numbers, as we now speculate among the disinterred ruins of an ancient city.
>
> We might, I think, rest content with such a general answer as this. But we may advance one step further--History is a continued record of passions and events unconnected with the enduring laws of mere material agents--The progress of physical induction, on the contrary, leads us on to discoveries, of which the mere light of history would not indicate a single trace. But the facts recorded in history may sometimes, without confounding the nature of moral and physical truth, be brought into a general accordance with the known phaenomena of nature: and such general accordance I affirm there is between our historical traditions and the phaenomena of geology. Both tell us in a language easily understood, though written in far different characters, that man is a recent sojourner on the surface of the earth. Again, though we have not yet found the certain traces of any great diluvian catastrophe which we can affirm to be within the human

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[172]Sedgwick, Philosophical Magazine, ser. 2, VII (1830), 310.

> period; we have, at least, shown, that paroxysms of internal energy, accompanied by the elevation of mountain chains, and followed by mighty waves desolating whole regions of the earth, were a part of the mechanism of nature. And what has happened, again and again, from the most ancient, up to the most modern periods in the natural history of the earth, may have happened once during the few thousand years that man has been living on its surface. We have, therefore, taken away all anterior incredibility from the fact of a recent deluge; and we have prepared the mind, doubting about the truth of things of which it knows not either the origin or the end, for the adoption of this fact on the weight of historic testimony.[173]

Sedgwick had simply abandoned the hypothesis that the diluvium and the excavation of valleys had been the result of a single deluge that was identical with the Biblical flood. He had not given up the idea that these appearances were the result of deluges, and most geologists adopted his position. John Phillips, for example, in a work published in 1835, explained the marl pit examined by the Rev. Vernon by assuming "the diluvial currents to have been of some duration, subject to vary in impetus and direction, and to be interrupted at intervals of at least local tranquillity." It was during one of these quiet intervals that the marl was deposited by limited floods or inundations perhaps produced by antediluvial rivers or other currents.[174]

In another work written at about the same time, Phillips acknowledged that the diluvium might be independent of the Noachian deluge and referred it instead to "a period of violent watery action":

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[173]Sedgwick, Philosophical Magazine, ser. 2, IX (1831), 314-15.

[174]John Phillips, Illustrations of the Geology of Yorkshire; or, a Description of the Strata and Organic Remains: Accompanied by a Geological Map, Sections, and Plates of the Fossil Plants and Animals. Part I: the Yorkshire Coast (2d ed.; London: John Murray,

> The present system of Nature may be considered as one of the periods of regular action. . . . But the deposits called diluvial are characteristic of a period of watery tumult and disturbance of the most extensive kind. . . . This watery tumult differs, however, from all anterior deluges, by the circumstance that we are looking upon the land and reading there the traces left by violent waves, while those of ancient times are known, to us only by the effects they produced in the sea.[175]

Charles Daubeny also did not give up his diluvial ideas and continued to dispute the significance of the evidence in the Auvergne and to argue for the usefulness of the terms antediluvian and postdiluvian.[176]

In 1836 Buckland modified his diluvial theory and moved closer to Sedgwick's view:

> The evidence which I have collected in my Reliquiae Diluvianae, 1823, shows, that one of the last great physical events that have affected the surface of our globe, was a violent inundation, which overwhelmed great part of the northern hemisphere, and that this event was followed by the sudden disappearance of a large number of the species of terrestrial quadrupeds, which had inhabited these regions in the period immediately preceding it. I also ventured to apply the name Diluvium to the superficial beds of gravel, clay, and sand, which appear to have been produced by this great irruption of water.
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> The description of the facts that form the evidence presented in this volume is kept distinct from the question of the identity

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1835), pp. 141, 143. John Phillips (1800-74), nephew of William Smith; professor of geology at Trinity College, Dublin, 1844-53; succeeded Buckland as reader in geology at Oxford, 1856-74.

[175]John Phillips and Charles Daubeny, "Geology," in Encyclopaedia Metropolitana; or, Universal Dictionary of Knowledge, on an Original Plan: Comprising the Twofold Advantage of a Philosophical and an Alphabetical Arrangement, with Appropriate Engravings, ed. the Rev. Edward Smedley, 2d Division IV (London: Baldwin and Cradock, 1836), 688-89. Daubeny apparently wrote only pp. 711-78 of the article, which includes in all pp. 529-808.

[176]Ibid., p. 713. See also Charles Daubeny, "On the Diluvial Theory, and on the Origin of the Valleys of Auvergne," Edinburgh New Philosophical Journal, X (1831), 201-29.

> of the event attested by them, with any deluge recorded in history. Discoveries which have been made, since the publication of this work, show that many of the animals therein described, existed during more than one geological period preceding the catastrophe by which they were extirpated. Hence it seems more probable, that the event in question, was the last of the many geological revolutions that have been produced by violent irruptions of water, rather than the comparatively tranquil inundation described in the Inspired Narrative.[177]

Buckland gave up almost nothing in his diluvial theory if we consider only its geological aspects. He simply acknowledged that his deluge was probably not the Biblical flood and was the last of a number of similar deluges. Buckland's disavowal of any relation between his diluvial theory and the Biblical flood meant that he had agreed with Lyell and most geologists in Great Britain that the theory should be evaluated on its geological merits alone.

The diluvial theory had originated as an attempt to explain some very puzzling phenomena. As the strata seemed clearly to have been deposited by water, the theory that their deposition was the result of a violent deluge had at one time appeared to be the simplest explanation that could be proposed, as well as that which least violated the commonly accepted religious and philosophical preconceptions. It is understandable that some geologists had gone further and identified this deluge with the Biblical flood. Although at the beginning of the nineteenth century the notion that all or most of the strata

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[177]William Buckland, Geology and Mineralogy Considered with Reference to Natural Theology (2 vols.; London: William Pickering, 1836), I, 94-95. The same statement was contained in the third edition, published in 1858, in which the geological parts had been revised by John Phillips.

was deposited by a single deluge had been rejected by most geologists, attention had then been focused on the diluvium as the most recent product of diluvial action, and some geologists, notably Cuvier and Buckland, had asserted that this deposit had been produced by the Biblical flood.

Opposition had come from those geologists who suspected that many features of the Cuvier-Buckland theory conformed more closely to theological beliefs than to the geological evidence. Fleming had argued that Buckland's theory agreed neither with the Bible nor with the evidence. Scrope had contended that no recent deluge had occurred in Central France and that the valleys there were excavated by the streams flowing in them. Even many of Buckland's defenders, such as Daubeny and De la Beche, had refused to commit themselves on the question of the relationship of the evidence to the Biblical flood. Eventually one of Buckland's strongest supporters, Sedgwick, had repudiated any such relationship.

The diluvial theory in British geology after 1830, under the impact of the new vulcanist ideas and the attacks by Lyell and others, was modified along the lines that its more moderate critics had long suggested. Those features of the theory that were primarily shaped by theological bias were eliminated. The idea that the Biblical flood was an important geological agent was given up by reputable geologists. Since only a minority of geologists had subscribed to this idea, its abandonment had less effect on geology than it had on the popular mind. Those geological appearances that were still

attributed to diluvial action, such as most of the valleys and the diluvium, were no longer referred to a single, universal deluge, but to several deluges, perhaps only partial in extent, which were ascribed to definite physical causes and were presumed to have occurred long before the advent of man. Geologists began to speak of a "diluvial period" of indefinite length, which had intervals of quiet between deluges. The term antediluvian came to mean any time prior to the last such deluge. These changes removed in general those features of the theory that had been criticized by Fitton in his review of the Reliquiae Diluvianae.

A majority of British geologists, following Sedgwick's lead, attempted to put their catastrophist views on a firmer philosophical basis by adopting the position advocated by Scrope, that the same physical causes have operated in the same manner throughout the earth's history. The interactions of these causes, they argued, could from time to time have produced catastrophes or deluges. They conceded to the ordinary operations of existing causes a much greater role than they were willing to before, but they denied that these causes were capable of producing all geological appearances.[178] A small minority of geologists adopted the modified Huttonian position of Lyell, which ascribed all geological appearances to causes identical in kind and magnitude to those within our experience.

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[178]For the catastrophist position see Sedgwick, Philosophical Magazine, ser. 2, IX, (1831), 303-07, 311-12; and Conybeare, Philosophical Magazine, ser. 2, VIII (1830), 359-62, 401-06; IX (1831), 19-23, 111-17, 188-97, 258-70.

It is difficult to distinguish the immediate impact of Lyell's Principles on the views of geologists with respect to the diluvial theory from the cumulative effect of the criticism that had gone before. Sedgwick, for example, had already privately abandoned the old theory, although it is possible that the book led him to embrace more readily the vulcanist ideas and to renounce the old theory in somewhat stronger terms than he might otherwise have. The book undoubtedly established Lyell's position as a legitimate one in geology. However, many geologists who, like Scrope and Murchison, aided Lyell in his attack on the influence of religious dogmatism in geology, would not accept his uniformitarian views. The dominant position in British geology was the vulcanist-catastrophist view.

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