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THE DEVELOPMENT OF THE IDEA OF THE GEOSYNCLINE

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THE DEVELOPMENT OF THE IDEA OF THE GEOSYNCLINE

CHAPTER I

INTRODUCTION

The problem of providing a satisfactory explanation for the phenomena of mountains has perplexed geologists during the whole history of the science of geology. During the nineteenth century there was a marked growth of interest in all sorts of geological investigation and a proliferation of geological theory including theories of mountains. During the middle and latter decades of the century, a new and very useful concept, which plays a significant role in theories of mountain formation, came into being, a concept today called the "geosyncline." A considerable amount of literature has appeared about the concept and the place it occupies in present-day geological theory, but there has been no comprehensive examination of its historical development. The purpose of this study is to provide an analysis of the historical growth of the idea, including an investigation of its antecedents, how it developed during its earlier decades, how it complemented other contemporary concepts, and how it fitted into broader geologic theories.

Definitions of the Geosynclinal Concept

The concept of the geosyncline seems to have been clearly defined first by James Hall (1811-1898), long-time geologist and palaeontologist for the state of New York. He described the general type of geological phenomena associated with the geosyncline as follows:

> It has long since been shown that the removal of large quantities of sediment from one part of the earth's crust, and its transportation and deposition in another, may not only produce oscillations, but that chemical and dynamical action are the necessary consequences of large accumulations of sedimentary matter over certain areas. When these are spread along a belt of sea bottom, as originally in the line of the Appalachian chain, the first effect of this great augmentation of matter would be to produce a yielding of the earth's crust beneath, and a gradual subsidence will be the consequence. . . .
>
> The line of greatest depression would be along the line of greatest accumulation; and in the direction of the thinning margins of the deposit, the depression would be less. By this process of subsidence, as the lower side becomes gradually curved, there must follow, as a consequence, rents and fractures upon that side. . . .
>
> The sinking down of the mass produces a great synclinal axis; and within this axis, whether on a large or small scale, will be produced numerous smaller synclinal and anticlinal axes. . . . I hold . . . that it is impossible to have any subsidence along a certain line of the earth's crust, from the accumulation of sediments, without producing the phenomena which are observed in the Appalachian and other mountain ranges.[1]

James D. Dana (1813-1895), Yale University geologist, generally receives acknowledgment for first using the term geosyncline itself. His first reference to the concept is found in this passage:

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[1] James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I: Palaeontology of New York (Albany: State of New York, 1859), pp. 69-70.

> The making of the Allegheny range was carried forward at first through a long-continued subsidence--a geosynclinal (not a true synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments, which occupied the whole of Paleozoic time. . . .
>
> The Green Mountains are another example in which the history was of the same kind: first, a slow subsidence or geosynclinal, carried forward in this case during the Lower Silurian era or the larger part of it; and, accompanying it, the deposition of sediments to a thickness equal to the depth of the subsidence; finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, etc. between the sides of the trough.[2]

In 1923 Charles Schuchert (1858-1924) defined the term geosyncline as he thought Hall had intended and brought others to task for broadening the concept too much. "To extend the meaning of the term geosyncline," he said,

> to all subsiding areas of sedimentary accumulation, to mediterranean and even to oceanic basins, as was done, it is true, for the first time by Dana, is to befog the brilliant idea of James Hall. Our understanding of geosynclines (both monogeosynclines and polygeosynclines) is that they are variably long and variably wide, very mobile, sinking areas that always originate within a continent; they are more or less long in geologic development, and receive great quantities of sediments derived in the main from the borderlands. The more rapidly sinking side of a geosyncline is adjacent to the inner side of a borderland, while the subsidence of the trough becomes less and less toward the neutral area of the continent.[3]

Schuchert here imposed a sophistication of the concept that

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[2] James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science, series 3, V (1873), 430.

[3] Charles Schuchert, "Sites and Nature of the North American Geosynclines," Bulletin of the Geological Society of America, XXXIV (1923), 209-10. Schuchert was the last of a long line of geologists and palaeontologists who had been assistants to James Hall, spending some thirty months

cannot be found anywhere in Hall's writings.

In 1947 two German geologists, M. F. Glaessner and Curt Teichert, stated their views on the problem, asserting that the concept at that time did not differ greatly from that defined by Hall and Dana who, they believed, had defined it broadly enough to allow for later developments and interrpretations. Yet, they wrote, "there is . . . a certain vagueness in this concept which makes it impossible at present to define a geosyncline in absolute terms and quantitative relations so as to decide once and for all what is a geosyncline and what is not."[4] Another and perhaps more useful way of defining the term, they believed, was to break up the definition into several elements, each of which must be satisfied by any geological structure before it could be called a geosyncline. Among these elements are first, that geosynclines are represented by masses of sediments in elongated belts of regional extent; second, that some of these masses become folded to varying degrees and generally are formed into folded mountain regions while some remain unfolded; and third, that sites of geosynclines

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at that position from 1889-91. He thus could be expected to have familiarity with Hall's concepts and to speak favorably of them. John M. Clarke, James Hall of Albany: Geologist and Palaeontologist, 1811-1898 (Albany: n. p., 1923), pp. 526-28.

[4] M. F. Glaessner and Curt Teichert, "Geosynclines: A Fundamental Concept in Geology," American Journal of Science, CCXLV (1947), 585-86.

are not stationary but tend to move about within certain limits.[5]

In a recent publication, the American Geological Institute defined the geosyncline as follows:

> A surface of regional extent subsiding through a long time while contained sedimentary and volcanic rocks are accumulating; great thickness [es] of these rocks are almost invariably the evidence of the subsidence, but not a necessary requisite. Geosynclines are prevalently linear, but non-linear depressions can have properties that are essentially geosynclinal.[6]

Jean Aubouin, a contemporary French geologist at the Sorbonne, pointed out that the term has different meanings to different individuals and to different groups of geologists. "Indeed," he said, "it could be stated almost without exaggeration that for many years the word geosyncline has held a different significance for every geologist; thus today many authors justifiably hesitate to use the term for fear that misunderstanding may arise." He recounted that at various times, authors have attempted to create substitute terminology, each of which was ultimately abandoned after limited usage. However imprecise and vague the concept geosyncline is, to Aubouin it "undoubtedly expresses the fundamental palaeogeographical disparity between certain types of mountain chains and others." Differences between the Pyrenees and the Jura mountains on the

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[5] Ibid.

[6] A. C. Trowbridge, (ed.), Glossary of Geology and Related Sciences; A Cooperative Project of the American Geological Institute (Washington: The American Geological Institute, 1957), p. 122.

one hand and the Alps on the other are easily defined when the concept of the geosyncline is used as a reference. The very persistence of the idea can readily be explained by such usages.[7]

Development of the Term Geosyncline

Nowhere in Hall's writings did he use the term geocyncline itself, and Dana seems to have been the first to use it. In 1873 he published an article concerning the origin of mountain ranges, and therein he used the term "geosynclinal" to describe the same general type of phenomena that Hall had in earlier statements. Dana gave as the origin of the term the Greek γῆ, earth, σύν, together, and κλίνω, incline, as representing a bend in the earth's crust.[8]

Before many decades had passed, the term geosynclinal became altered to the now familiar geosyncline, and Dana, originator of the term, also appears to have been instrumental in this modification. In one paper of 1890 he used the terms synclinal and anticlinal frequently, but at one place he shifted to the now-familiar anticline.[9] A

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[7] Jean Aubouin, Developments in Geotectonics 1: Geosynclines, trans. Express Translation Service (New York: American Elsevier Publishing Co., 1965), p. 1.

[8] Dana, American Journal of Science, series 3, V, 430 and n.; Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History (4th ed.; New York: American Book Co., 1896), p. 102.

[9] James D. Dana, "Rocky Mountain Protaxis and the Post-Cretaceous Mountain-Making along Its Course," American Journal of Science, series 3, XL (1890), 181-96, passim.

year later he used the word geosyncline,[10] and by 1896, when the fourth edition of his Manual of Geology appeared, he had dropped the previous terminology entirely. Joseph LeConte (1823-1901), geologist at the University of California, used the now-accepted terms much earlier than Dana, speaking frequently of synclines and anticlines and reserving synclinal and anticlinal for the adjective forms of the terms.[11]

Contemporary Geological Philosophy

During Hall's professional career, geologists tended to favor one of two general philosophies of geology, namely, catastrophism or uniformitarianism. Hall expressed views that were oriented toward the uniformitarian view, and his subsidence concept was developed within the general guidelines of that philosophy. However, Dana, who provided one avenue through which the geosyncline gained a substantial measure of acceptance, adhered to catastrophism. Because the geosyncline became associated with both of these general philosophical orientations, the following descriptive

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[10]James D. Dana, "On Percival's Map of the Jura-Trias Trap-Belts of Central Connecticut, with Observations on the Up-Turning, or Mountain-Making Disturbance, of the Formation," Ibid., series 3, XLII (1891), 442.

[11]Joseph LeConte, "On the Structure and Origin of Mountains, with Special Reference to Recent Objections to the 'Contractional Theory,'" American Journal of Science, series 3, XVI (1878), 95-112, passim. The terms syncline and anticline are included because their evolution from the older usages seems to have been taking place at the same time.

paragraphs are included.

Catastrophism

Archibald Geikie (1835-1924), an English geologist contemporary with Hall and Dana, summarized catastrophist views in these terms:

> The Catastrophists saw in the composition of the crust of the earth distinct evidence that the forces of nature were once much more stupendous in their operation than they now are, and that they from time to time devastated the earth's surface; extirpating the races of plants and animals, and preparing the ground for new creations of organized life.[12]

Though Geikie's description was published early in the twentieth century, it agrees in its basic assumptions with the general description of the development of catastrophism which follows.

Late in the seventeenth century, Thomas Burnet (1635?-1715) and William Whiston (1667-1752) each gave his conception of the stupendous forces of nature which Geikie later referred to. Burnet postulated a world that after its creation some 5,000 years ago existed in a state which he called paradise, a world that was smooth, even, and regular. The posture and situation of the earth to the sun were regular and not inclined and oblique as they now are, and the shape of the globe was more apparently and regularly oval than now.[13] About 1,600 years after the creation,

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[12]Archibald Geikie, "Geology," Encyclopaedia Britannica, 11th edition, XI, 643.

[13]Thomas Burnet, The Theory of the Earth: Containing an Account of the Original of the Earth, and of All the

said Burnet, "it was over-flow'd, and destroy'd in a Deluge of water. Not a Deluge that was National only," but one that "overspread the face of the whole Earth, from Pole to Pole, and from East to West, and that in such excess, that the Floods over-reacht the Tops of the highest Mountains. . . ." Later those waters receded and "the Mountains and Fields began to appear, and the whole habitable Earth in that form and shape which now we see it in." According to Burnet, this event was "the greatest revolution and the greatest change in Nature" that had yet happened to the world.[14] Some time during the great flood the outer arch or shell of the earth fractured and parts of it fell into the abyss below, but as there was not room for all of the materials of the outer shell in the abyss, parts of it were left standing higher than others. Those parts which were above the level of the water quite naturally became the dry-land areas of the globe, while those that remained higher yet became our hills and mountains. This configuration of the landscape is essentially what remains today.[15]

Whiston, an English mathematician who succeeded Sir Isaac Newton (1642-1727) as Lucasian professor of mathematics

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General Changes Which It Hath Already Undergone, or Is to Undergo, till the Consummation of All Things. The Two First Books Concerning the Deluge, and Concerning Paradise (London: Walter Kettilby, 1684), pp. 193-94.

[14]Ibid., pp. 7-9. [15]Ibid., p. 147.

at Cambridge, in 1696 described how he thought the earth's surface had acquired its present configuration. He computed that the great comet that was visible in England in 1680 has a period of about 575 years, and he speculated that it had appeared in the year 2349 B. C. On that appearance, he said, it had passed within 10,000 miles of the earth, and that passage produced dramatic changes in the status of the earth. The gravitational attraction of the comet and the earth for each other altered the alignment of the earth's axis, changed its orbit from a circle to an ellipse, started the earth rotating in its diurnal movement, and lengthened its annual period by about ten days. The most dramatic events, according to Whiston, were associated with a catastrophic flood. The earth passed near enough to the comet so that it actually passed through the comet's tail, causing huge quantities of water to be condensed from the comet's atmosphere and to be deposited on the earth in the form of violent rains. The attraction of the comet for the earth caused massive tides in the fluid which Whiston believed underlay the crust of the earth. These tides fractured the crust, "breaking up the Fountains of the great Abyss, or the causing such Chaps and Fissures in the upper Earth, as might permit the Waters contain'd in the Bowels of it when violently press'd and squeez'd upwards to ascend, and so add to the quantity of those which the Rains produced." The deluge destroyed all men and land animals except those with Noah in the ark, said Whiston, as well as completely altering the structure of the surface of the earth, depositing fresh strata or layers of earth at that time.[16]

Both Burnet and Whiston included the Biblical deluge as part of their cosmological schemes, but neither provided much in the way of supporting evidence for their speculations. Several decades later, however, Johann Gottlob Lehmann (d. 1767), a German mineralogist, recommended getting out into the mountains and plains to observe the phenomena itself before undertaking a definitive work on the structure of the earth.[17] Following this dictum, he formulated a theory of the earth in which he classified mountains into three groupings. When the globe assumed a definite shape at the time of the creation, he said, the earth's great mountain ranges were formed, composed of non-fossiliferous, relatively homogeneous rocks. These

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[16]William Whiston, A New Theory of the Earth, from Its Original, to the Consummation of All Things, wherein the Creation of the World in Six Days, the Universal Deluge, and the General Conflagration, as Laid Down in the Holy Scriptures, Are Shewn to Be Perfectly Agreeable to Reason and Philosophy, with a Large Introductory Discourse Concerning the Genuine Nature, Stile, and Extent of the Mosaick History of the Creation (London: Benj. Tooke, 1696), pp. 123, 133-36, 188-90, 199-201.

[17]Johann Gottlob Lehmann, Versuch einer Geschichte der Flötz-Gebürgen, betressend deren Entstehung, Lage, darinne besindliche Metallen, Mineralien und Fossilien, grostentheils aus eigenen Wahrnehmungen, chymischen und physicalischen Versuchen, und aus denen Grundsätzen der Natur-Lehre hergeleitet, und mit nöthigen Kupfern versehen (Berlin: Klüterschen Buchhandlung, 1756), pp. [A5, recto] - [A6, verso].

mountains he called gang-gebürge or primitive mountains, and on their lower reaches there rest lesser mountains composed of materials deposited there by the Biblical deluge. The latter type is stratified and contains vast quantities of organic remains of all types. These he designated as flötz-gebürge or stratified mountains. The third class came into being through the action of catastrophic events of lesser magnitude than the deluge and were thus more localized in their placement. To this group he did not assign a name, perhaps because of their miscellaneous character.[18]

Another German mineralogist, Abraham Gottlob Werner (1750-1817), also insisted that knowledge of the earth could come only from a detailed examination of its component materials. One of Werner's students, John Murray (d. 1820), noted that Werner "has not indulged in hypothesis, but has approached as nearly to an induction of facts as the subject admits."[19] Another Werner student, Jean Francois d'Aubuisson de Voisins (1769-1847), recorded the following statement of Wernerian geological theory:

> Professor Werner concludes, that the globe of the Earth is of remote antiquity; that its surface was inhabited by animals, and covered with vast forests, when it

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[18] Ibid., p. 117

[19] [John Murray], 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), pp. 12-15.

> underwent a great revolution, perhaps the last of several which it has experienced; that this revolution occasioned the disintegration of many of the rocky masses already existing,--the total destrcution [sic] of the forests,--and was followed or accompanied by a mighty inundation, which rose to a height, equal perhaps to that of the highest mountains; that this immense and necessarily raging sea produced accumulations of gravel and sand, over which, when it had somewhat abated of its agitation, were deposited the earth, clayey, and bituminous particles with which it was charged. . . .[20]

Peter Simon Pallas (1741-1811), a contemporary of Werner, classified mountains in much the same manner as Lehmann, but called the three classes primitive, secondary, and tertiary.[21] Primitive mountains, he said, were created in the beginning as islands in the surface of the primitive ocean and were composed largely of granitic rocks. Secondary mountains were formed in a number of ways, the most significant being volcanos and the tides and currents of the sea. Recognizing that the tertiary mountains had maritime origins and not willing to admit that enough water could have been present on the earth at any time to cover the surface to a depth above the highest mountains, he postulated a different maritime catastrophe. He assumed that a

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[20] Jean Francois d'Aubuisson de Voisins, An Account of the Basalts of Saxony, with Observations on the Origin of Basalt in General, trans. P. Neill (Edinburgh: Archibald Constable & Co., 1814), p. 239.

[21] Peter Simon Pallas, Observations sur la formation des montagnes et les changemens arrivés au globe, particuliérement a l'égard de l'empire Russe; lues à l'assemblée publique de l'Académie Impériale des Sciences de Russie du 23 Juin, 1777; que Monsieur le Comte de Gothland daigna illustrer de sa présence (St. Petersbourg: L'imprimerie de Académie Imperiale des Sciences, n. d.), pp. 4-5.

cataclysmic, general uprising of the volcanic islands of the Indies from Africa to Japan, which he said are yet filled with volcanos or the vestiges of them, thrust a huge wall of water against the lands of the entire world, transporting and depositing on the slopes of the earlier mountains all the materials which now make up the tertiary mountains. This catastrophe, he said, corresponded quite well in time to the Mosaic deluge and was a more reasonable explanation for it than older accounts had been.[22]

Horace-Bénedict de Saussure (1740-1799) placed less emphasis on the Noachian deluge as a geological agent, concluding from an examination of several maritime deposits appearing at various levels above the surface of the sea, that there had been at least five such major cataclysmic events in the history of the world.[23]

Several decades later, a French geologist, Léonce Élie de Beaumont (1798-1874), further developed the idea that successive catastrophes could account for the configuration of the crust. Furthermore, such events seemed to coincide with the extinction of species of animals and plants described to him by zoologists and botanists. He theorized that the appearance of certain systems of mountains

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[22] Ibid., pp. 40-47.

[23] Horace-Bénedict de Saussure, Voyages dans les Alpes, précédés d'un essai sur l'histoire naturelle des environs de Geneve (Neuchatel: Samuel Fauche, 1796), IV, 431-32.

coincided with several successive revolutions of the surface of the globe and with the almost periodic renewal of the animal and vegetable population in various areas.[24] Sudden variations in the character of remains of animal and plant life could be observed in several series of sedimentary deposits, according to Élie de Beaumont, and these came at the end of the Jurassic deposits, the chalk deposits, the tertiary deposits, and the oldest of the alluvial deposits. He considered these variations to be in agreement with the convulsions of the surface of the earth that have given to it the principal features of its actual relief, and these he believed could be observed in four mountain systems of southern Europe. In each of those systems, the dislocation and alteration of sedimentary beds seemed limited to a certain series but had affected with equal intensity all of the preceding beds. This ostensibly showed that the phenomenon of rearrangement was not continuous and progressive but sudden and of short duration. A similar convulsion interrupted the formation of sedimentary deposits, and from this he believed he could trace each of these anomalies by the height above sea level of the various beds.[25]

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[24] Léonce Élie de Beaumont, Recherches sur quelques-unes des révolutions de la surface du globe, présentant différens exemples de coïncidence entre le redressement des couches de certains systèmes de montagnes, et les changemens soudains qui ont produit les lignes de démarcation qu'on observe entre certains étages consécutifs des terrains de sediment; (Memoire lu par extrait a l'Académie des Sciences, le 22 Juin 1829) (Paris: Crochard, 1829), pp. 298-99.

[25] Ibid., pp. 3-4.

Élie de Beaumont took special pains to point out that geological evidence not only tended to establish the truth of his hypothesis but also disproved ideas gaining momentum at that time which attributed the changes in the surface of the globe to slow and progressive modification.[26]

Leopold von Buch (1774-1853), friend and associate of Élie de Beaumont, added still another feature to catastrophist explanations of mountains. Von Buch studied volcanos and volcanic mountains extensively during his lifetime, concluding that volcanic mountains were not really volcanos at all but "Erhebungscratere" (craters of elevation). He speculated that these were not formed by successive lava flows as popularly believed, but had come into being by a sudden uplift of extensive areas of the terrain and are the remains of a giant display of internal forces. On the other hand, volcanos are merely chimneys or canals that unite the interior of the earth with the outer world.[27]

Uniformitarianism

Charles Lyell (1797-1875), the greatest exponent of

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[26] Ibid., p. 297. Although Charles Lyell's Principles of Geology, with its comprehensive development of uniformitarian geology, had not yet been published, Élie de Beaumont expressed familiarity with Lyell's ideas through the latter's numerous friendly communications to contemporaries in France. Ibid., p. 297, n. 1.

[27] Leopold von Buch, "Ueber Erhebungscratere und Vulcane," Annalen der Physik und Chemie, XXXVII (1836), 169.

uniformitarian geological doctrine in the nineteenth century, defined the philosophy of uniformitarianism succinctly in the title to a major geological publication which reads: "Principles of Geology, Being an Attempt to Explain the Former Changes of the Earth's Surface by Reference to Causes Now in Operation."[28] The key to all geologically oriented definitions of the term centers around the assumption that the geologist need not resort to any geologic process other than those which can now be observed and, furthermore, that the forces involved are acting with intensities that are unchanged from what they have been throughout geological history.

An impression sometimes received when reading of the two opposing schools of nineteenth-century geological thought, uniformitarianism and catastrophism, is that the doctrine of successive catastrophes appeared first chronologically and that uniformitarianism grew out of a reactionary movement to catastrophism.[29] Lyell concluded, however, that natural philosophers as long ago as antiquity

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[28](3 vols.; London: John Murray, 1830-33).

[29]For instance, see William Whewell, History of the Inductive Sciences from the Earliest to the Present Time (3rd ed.; London: John W. Parker and Son, 1857), III, 506-18. This chapter is entitled "Two Antagonist Doctrines of Geology," and in it the author examines briefly the two opposing systems and their development. Karl Alfred von Zittel, History of Geology and Palaeontology to the End of the Nineteenth Century, trans. Maria M. Ogilvie-Gordon (London: Walter Scott, 1901), pp. 186-97, gives essentially the same picture of the relationship of the two philosophies.

had espoused doctrines that had uniformitarian overtones, and if these authors had not given a strictly geologic orientation to their ideas, at least they were in basic agreement with his own concepts. Aristotle, he said, believed that nature followed an orderly and regular course in bringing about the great changes that have been observed in times past. From the works of Aristotle and from the system of Pythagoras, Lyell felt free to infer that those philosophers "considered the agents of change now operating in Nature, as capable of bringing about in the lapse of ages a complete revolution; and the Stagyrite even considers occasional catastrophes, happening at distant intervals of time, as part of the regular and ordinary course of Nature."[30]

In seventeenth and eighteenth-century geological theory, Lyell found an undesirable situation. The period, he said, was replete with examples of the retardation of its study. Extravagant systems had been developed by men of acknowledged talent, and there was a "constant and violent struggle between new opinions and ancient doctrines, sanctioned by the implicit faith of many generations, and supposed to rest on scriptural authority." This period he felt to be singularly devoid of advancement in geological science.[31]

The Scottish geologist, James Hutton (1726-1797), earlier expressed ideas that can be categorized as

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[30]Lyell, I, 15. [31]Ibid., I, 30.

uniformitarian in their implications. One of Hutton's premises reads;

> 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 alleged 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.[32]

Hutton willingly accepted the agents of fire and water as legitimate, but he believed they act in such a way as not to preclude the propagation of plants and animals. Chaos and confusion must not be introduced into the order of nature merely because of the appearance of disorder which one views in the landscape, nor should the geologist invent causes for events when ordinary forces and processes seem insufficient.[33] He concluded that there could be no limit on geological time, asserting it "cannot be bounded by any operation that may have an end, the progress of things upon this globe, that is, the course of nature, cannot be limited by time, which must proceed in a continual succession."[34]

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[32] James Hutton, _Theory of the Earth, with Proofs and Illustrations, in Four Parts_ (Edinburgh: W. Creech, 1795), II, 547.

[33] _Ibid_.

[34] James Hutton, "Theory of the Earth; or an Investigation of the Laws Observable in the Composition, Dissolution, and Restoration of Land upon the Globe," _Transactions of the Royal Society of Edinburgh_, I (1788), 215.

Hutton's concepts attracted a number of followers in the decades following their initial publication in 1788 including John Playfair (1748-1819), Sir James Hall (1784-1856), and Thomas Allan (1777-1833). These individuals, however, seemed not to feel any urgent need to accept in toto the theory as Hutton had published it, and as Allan stated, they did not feel bound as "the pupils of the Wernerian School have been peculiarly fettered, by an ideal necessity of supporting the principles of their master. . . ."[35] Furthermore, there seemed general agreement amongst them that Hutton's writing had lacked precision, and that "the obscurity of these has been often complained of."[36] On another occasion Allan gave further indication of his willingness to depart from a strict adherence to Hutton's views when he said:

> that theory, [of Hutton] in itself so beautiful, and in many points so perfect, I am very far from embracing entirely. I am very far, indeed, from following him through his formation and consolidation of strata, or the transportation and arrangement of the materials, of which they are composed.[37]

Sir James Hall likewise expressed reservations about

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[35]Thomas Allan, "Remarks on the Transition Rocks of Werner," Transactions of the Royal Society of Edinburgh, VII (1815), 110.

[36]Ibid., p. 111; John Playfair, Illustrations of the Huttonian Theory of the Earth (Edinburgh: William Creech, 1802), p. 111.

[37]Thomas Allan, "On the Rocks in the Vicinity of Edinburgh," Transactions of the Royal Society of Edinburgh, VI (1812), 408.

accepting Huttonian theory completely. He hesitated not at all in accepting

> 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. . . .[38]

Instead he felt more inclined to modify the general theory by addition of catastrophes of the sort postulated by Pallas and de Saussure as a result of their travels in Russia and the Alps respectively. Their evidence, he said, led him to believe that a torrent of water had at one time swept across Asia and Europe, causing some of the phenomena observed in those regions.[39]

Insofar as the elevation of mountains is concerned, Hutton's hypothesis saw mountains uplifted solely by the expansive effect of subterranean heat operating slowly over a long period of time. Playfair attributed the uplift of continental areas, including mountains, to the same cause. Furthermore, he ascribed the additional effects of displacement, fracturing, folding, and various other observable phenomena to this cause.[40]

Hutton thus seems more to have established a general

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[38] Sir James Hall, "On the Revolutions of the Earth's Surface," Transactions of the Royal Society of Edinburgh, VII (1815), 140. Sir James Hall is not to be confused with the American James Hall who was the long-time geologist for the state of New York.

[39] Ibid. [40] Playfair, pp. 53-55.

attitude and a general philosophical basis for his followers than to have built a detailed body of geological data and theory to which they would willingly adhere.[41]

Lyell's first published views on uniformitarianism appeared in 1826. He first called attention to statements of Georges Cuvier (1769-1832) and William Buckland (1784-1856) who, he said, had declared that the geologist would search in vain amongst the processes now visible for those "mighty disturbing forces" that would have produced the revolutions and catastrophes of which the traces are now exhibited. Lyell then commented 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." Furthermore, he insisted, ideas such as those of Cuvier and Buckland tend to destroy all hope of understanding the geological past through a search of the present phenomena.[42]

The following year Lyell reviewed a work on volcanos by George Poulett Scrope (1797-1876). The reviewer's uniformitarian biases were further exposed, and he said

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[41]James Hall of New York may not have been familiar with Hutton's work directly, but he did acknowledge familiarity with at least part of the work of Sir James Hall. Hall, Palaeontology . . ., III, 81.

[42][Charles Lyell], "Transactions of the Geological Society of London. Vol. i. 2d Series. London. 1824," Quarterly Review, XXXIV (1826), 517-18.

that authors such as Buckland could not conceive of the amount of time necessary for geologic processes and therefore found it necessary to resort to catastrophes.[43] Lyell also criticized strongly the expositors of Mosaic diluvial theory, pronouncing that "too much caution cannot be used against rash or premature attempts to identify questionable theories in physical science with particular interpretations of the sacred text, . . ." thus warning against the perils of interpreting the scriptures as a scientific document.[44]

Lyell published his views on uniformitarian geology in his Principles of Geology, the first volume of which appeared in 1830. In a letter to John Fleming (1785-1857) the same year, Lyell declared that "as a staunch advocate for absolute uniformity in the order of Nature, I have tried in all my travels to persuade myself that the evidence was inconclusive, but in vain."[45]

Lyell next undertook a severe criticism of Élie de Beaumont's theory on the origin of mountains which had appeared the previous year.[46] It is preposterous, he

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[43] [Charles Lyell], "Memoir on the Geology of Central France; Including the Volcanic Formations of Auvergne, the Velay, and the Vivarais, with a Volume of Maps and Plates. By G. P. Scrope, F. R. S., F. G. S. London. 1827," Ibid., XXXVI (1827), 477.

[44] Ibid., 481-83.

[45] Letter from Lyell to Fleming, Temple, February 3, 1830, quoted in [Charles Lyell], Life, Letters, and Journals of Sir Charles Lyell, Bart., Author of 'Principles of Geology' &c., ed. K. M. Lyell (London: John Murray, 1881), I, 260.

[46] See note 24, supra.

concluded, to assert that the Allegheny Mountains in North America had been upheaved at the same time as European chains simply because they are parallel. He also criticized Élie de Beaumont's assumption that the upheaval of the mountains was sudden and of short duration. Lyell asserted he would not like to be answerable for the ideas put forth by his French fellow geologist.[47] His argument with Élie de Beaumont carried on for several years, and in 1838, when Lyell was revising his <u>Principles</u> prior to the issue of a sixth edition, he set forth his views to Charles Darwin (1809-1882), saying:

> In recasting the 'Principles,' I have thrown the chapter on De Beaumont's contemporaneous elevation of parallel mountain chains into one of the Preliminary Essays, where I am arguing against the supposition that nature was formerly parsimonious of time and prodigal of violence.[48]

In this edition of the <u>Principles</u>, Lyell again discussed Élie de Beaumont's theory of the sudden rise of parallel mountain chains. He concluded that although the geological facts adduced by Élie de Beaumont might be true, still the conclusion that certain chains were simultaneously upraised was by no means a legitimate consequence. Lyell called attention to large areas of land that are slowly and

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[47] Two letters from Lyell to G. Poulett Scrope, London, June 20, 1830, and Havre, France, June 25, 1830, quoted in [Lyell], <u>Life, Letters . . .</u>, I, 272-75.

[48] Letter from Lyell to Darwin, Kinnordy, September 6, 1838, quoted in [Lyell], <u>Life, Letters . . .</u>, II, 43.

insensibly rising, while others gradually sink. Furthermore, all the existing continental areas and oceanic regions probably originated in such movements, continuing through incalculable periods of time. Nor could he believe that so many deep valleys and wide areas could have been denuded without the action of running water over long periods of time, thus tending to controvert the idea of sudden upthrow of continental masses.[49]

One of Lyell's clearest statements of uniformitarianism is found in a letter to William Whewell (1794-1866) in which he observed

> it . . . impossible, I think, for anyone to read my work, and not to perceive that my notion of uniformity in the existing causes of change always implied that they must for ever produce an endless variety of effects, both in the animate and inanimate world. I expressly contrasted my system with that of 'recurring cycles of similar events.' . . . I never drew a parallel between a geological and an astronomical series or cycle of occurrences. I did not lay it down as an axiom that there cannot have been a succession of paroxysms and crises, on which '*à priori* reasoning' I was accused of proceeding, but I argued that other geologists have usually proceeded on an arbitrary hypothesis of paroxysms and the intensity of geological forces, without feeling that by this assumption they pledged themselves to the opinion that *ordinary forces and time* could never explain geological phenomena. . . . I complained that in attempting to explain geological phenomena, the bias has always been on the wrong side; there has always been a disposition to reason *à priori* on the extraordinary violence and suddenness of changes, both in the inorganic crust of the earth, and in organic types, instead of attempting strenuously to frame

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[49] Charles Lyell, *Principles of Geology: or, the Modern Changes of the Earth and Its Inhabitants, Considered as Illustrative of Geology* (6th ed.; London: John Murray, 1840), I, 304-13.

> theories in accordance with the ordinary operations of nature.[50]

By 1850 Lyell considered that substantial progress had been made in refuting catastrophist views and that his uniformitarian doctrines had gained some measure of acceptance. He summarized the situation in that year in a statement made in the eighth edition of his Principles in these words:

> These [catastrophist] views were gradually modified, and some of them entirely abandoned in proportion as observations were multiplied, and the signs of former mutations more skilfully interpreted. Many appearances, which had for a long time been regarded as indicating mysterious and extraordinary agency, were finally recognized as the necessary result of the laws now governing the material world; and the discovery of this unlooked-for conformity has at length induced some philosophers to infer, that, during the ages contemplated in geology, there has never been any interruption to the agency of the same uniform laws of change. The same assemblage of general causes, they conceive, may have been sufficient to produce, by their various combinations, the endless diversity of effects, of which the shell of the earth has preserved the memorials; and, consistently with these principles, the recurrence of analogous changes is expected by them in time to come.[51]

Uniformitarianism and Catastrophism in the United States

The controversy between the uniformitarian and catastrophist philosophies took place largely in a European setting with most of the active participants being British,

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[50]Letter from Lyell to Whewell, Bloomsbury Square, March 7, 1837, quoted in [Lyell], Life, Letters . . ., II, 2-3.

[51]Charles Lyell, Principles of Geology: or the Modern Changes of the Earth and Its Inhabitants Considered as Illustrative of Geology (8th ed.; London: John Murray, 1850), p. 64.

French, and German geologists, but their attitudes are reflected in the writings of Americans as well. Among those who developed a uniformitarian view, Hall can be singled out for consideration. He seems to have come under the influence of Lyell early in his career and by 1843 had made statements that reflected a knowledge of Lyell's Principles. In an introduction to one of his numerous reports on the geology and palaeontology of New York, Hall declared that

> the doctrine of violent catastrophes, and of sudden changes in the inhabitants of the ocean, was based upon the examination of limited districts, where the entire series of deposits had never existed, or had been subsequently obliterated. And gradual and tranquil as the changes now seem to us, they may appear infinitely more so when a perfect sequence among the strata of the whole globe shall become known--when a complete succession shall be established from the oldest to the newest rock. From what we now know, compared with the knowledge existing a few years since, we can readily infer that some distant places, or even nearer localities, may furnish links now wanting in the chain.
>
> In learning to regard nature as always the same, and her laws unchanging, we have made a grand step towards the explication of phenomena before unexplained, except through a suspension of the natural laws, or a miraculous interposition of creative power.[52]

Catastrophism in United States geological theory assumed a less spectacular form than it had in Europe.

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[52] James Hall, Geology of New York. Part IV, Comprising the Survey of the Fourth Geological District (Albany: State of New York, 1843), p. 10. Hall and Lyell were personally acquainted and when the latter visited the United States in 1841-42 and again in 1845-46, Hall conducted his British colleague to various points of geological interest in the state of New York including Niagara Falls. Charles Lyell, Travels in North America, in the Years 1841-2; with Geological Observations on the United States, Canada, and Nova Scotia (New York: Wiley and Putnam, 1845), I, 14-47; Lyell, A Second Visit to the United States of North America (London: John Murray, 1849), II, 348, 353.

Proponents of this form assumed the basic postulate that the earth is a gradually cooling globe; that the interior of the earth continues to contract as it progressively cools while the crust itself, having long since reached an equilibrium of temperature, no longer contracts. In adjusting to the contraction of the interior, the crust forms into great, mountain-sized folds in a fashion somewhat analogous to the skin of an apple or a prune as it loses moisture.[53] Dana, the greatest American proponent of this concept, called the contractional theory, first wrote of it in 1847. His version of the theory postulated movements of the crust in paroxysmal increments.[54]

Lyell discussed the contractional hypothesis and the secular loss of heat by the earth, but he found it unnecessary to assume such a postulate. Though he agreed that the earth does indeed lose heat, he speculated that there might be a compensating process that would restore

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[53]James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History, for the Use of Colleges, Academies, and Schools of Science (Philadelphia: Theodore Bliss & Co., 1863), p. 718.

[54]James D. Dana, "On the Origin of Continents," American Journal of Science, series 2, III (1847), 94-100; Dana, "Geological Results of the Earth's Contraction in Consequence of Cooling," Ibid., 176-88; Dana, "Origin of the Grand Outline Features of the Earth," Ibid., 381-99; and Dana, "A General Review of the Geological Effects of the Earth's Cooling from a State of Igneous Fusion," Ibid., series 2, IV (1847), 88-92. Dana reiterated and elaborated these basic ideas on numerous occasions throughout the remainder of his life.

the heat and light that constantly emanate from the earth.[55] Hall, oriented to the uniformitarian view, described the argument of a cooling, contracting globe as "not always philosophical," and always unsatisfactory as a solution for the problem of explaining mountains.[56]

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[55]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 (1st American ed.; Philadelphia: James Kay, Jun. & Brother, 1837), I, 145-47.

[56]James Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 69.

CHAPTER II

THE ORIGINS OF JAMES HALL'S CONCEPT OF SUBSIDENCE

As noted before, James Hall did not use the now-familiar term geosyncline in any of his writings, and his views became known to many of his contemporaries as a concept of subsidence.[1] In this chapter a further definition of Hall's concept will be made, followed by an examination of a number of possible sources of ideas that contributed to it.

Definitions of Hall's Subsidence Concept

Though Hall was still a comparatively young man in 1856, he had already attained a reputation as an outstanding palaeontologist and geologist.[2] In 1855 he was elected

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[1]See for instance, George L. Vose, Orographic Geology; or, the Origin and Structure of Mountains. A Review (Boston: Lee and Shepard, 1866), pp. 48-49; Josiah D. Whitney, "Volcanism and Mountain-Building," North American Review, CXIII (1871), 266; and James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science, series 3, V (1873), 426.

[2]Letter from Edward Hitchcock to the governor of Iowa, Amherst College, February 13, 1855; letter from Louis Agassiz to an unknown correspondent, Cambridge, Massachusetts, February 14, 1855; and letter from Édouard de Verneuil to James Hall, Boston, May 22, 1846. All three cited letters are in the New York State Library, Albany, New York, Hall Papers. Hereinafter, the library location of the Hall Papers will be omitted.

president of the American Association for the Advancement of Science to preside over its annual convention at Albany, New York, in August, 1856.[3] The following year the same group convened at Montreal, Canada, for a similar session, and it was on this occasion that Hall, in his capacity as the retiring president of the Association, delivered an address that contained his first comprehensive statement of the subsidence concept.[4] In 1858, while acting as geologist for the state of Iowa, Hall published a brief resume of the concept in an introduction to the report of the geolgical survey of the state.[5] His most comprehensive exposition of the concept appeared as the introduction to volume three of the Palaeontology of New York, a monumental, multi-volume work to which Hall devoted nearly half a century of his career.[6]

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[3]Joseph Lovering, "Executive Proceedings of the Albany Meeting, 1856," Proceedings of the American Association for the Advancement of Science, X (1856), part II, 229; John M. Clarke, James Hall of Albany, Geologist and Palaeontologist, 1811-1898 (Albany: n. p., 1923), pp. 320-23.

[4]James Hall, MSS of the 1857 Address, "Contributions to the Geological History of the American Continent," Hall Papers, fol. 15; hereinafter cited as: MSS of the Montreal Address.

[5]James Hall and J. D. Whitney, Report on the Geological Survey of the State of Iowa: Embracing the Results of Investigations Made during Portions of the Years 1855, 56, & 57 ([Des Moines]: State of Iowa, 1858, I, part I, pp. 35-44.

[6]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I: Palaeontology of New York (Albany: State of New York, 1859), pp. 1-96. This will hereinafter be referred to as

The Montreal address appeared in print in 1883 when Hall at last consented to its publication.[7]

Hall postulated a very close relationship between significant and rapid deposition of sedimentary materials on the one hand and mountain chains on the other. Alignment of the original deposition gives direction to mountain chains, and in a general way, the amount of deposition determines their elevation. Immense deposits of sedimentary materials abraded from contiguous continental areas are transported and deposited by ocean currents over significantly large areas near the coastlines of continents, and the areas receiving these deposits tend to subside with the increased weight of the materials in direct proportion to the amount of the sedimentary deposits. As a result, mountain chains lie along the line of greatest accumulations of materials, and subsidence of the mass of accumulated sediments produces a great synclinal axis.[8]

Hall turned to the Appalachian chain as an example, and to explain its existence, he was "able to deduce some general principles" in regard to its production. First, he said, "we are to look to the original accumulation of matter

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Hall's "Introduction" of 1859. The eight volumes of this series were published between 1847 and 1894.

[7]James Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 26-69.

[8]Hall, MSS of the Montreal Address, pp. 66-75.

along a certain line or zone, the direction of which will be the direction of the elevation." He then postulated an agency of distribution for the materials, asserting that "the line of the existing mountain chain will be the course of the original transporting current." Furthermore, "the minor axes or foldings must be essentially parallel to the great synclinal axis and the line of accumulation." Finally, Hall described the present mountain barriers as but "the visible evidences of the deposits upon an ancient ocean bed; while the determining causes of their elevation existed long anterior to the production of the mountains themselves."[9]

Significant in Hall's statement of ideas is his denial that the large scale deposition of materials causes elevation of mountain chains in any way, and in fact he denied there could ever be any such a phenomenon as the upheaval of a mountain chain except in terms of continental elevation of large areas. Mountain chains do not occur where significant deposition has not occurred, but factors other than the deposition itself bring on elevation.[10] What Hall really did, according to T. Sterry Hunt (1826-1892), one of his most ardent advocates,

> was to show the relation between mountain-chains and great accumulations of sediments; to illustrate this by the history of the palaeozoic rocks of North America; and, moreover, to protest against the generally received theory that mountain elevations are due to local

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[9]Hall, Palaeontology . . ., III, 73.

[10]Hall, MSS of the Montreal Address, pp. 70-71.

> upthrusts; he [was], to use his own language 'going back to the theories long since entertained by geologists relative to continental elevations.'[11]

Hall's Philosophy of Geology

In 1842 Hall completed a comprehensive report of the geology of the fourth geological district of New York, a region that included the western counties of the state. This document is essentially a compilation and expansion of a series of annual reports begun in 1836 when the state geological survey began its operations.[12] Several statements in this report are of special interest, giving some good indications of Hall's general philosophy of geology and some indication that the concept of subsidence might have been in an incipient stage. He rejected any notion of catastrophism in his study of geology. Nature, he stated, is always the same and her laws do not change. Consequently, many types of geological phenomena could be explained that were previously inexplicable "except through a suspension of

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[11] T. Sterry Hunt, "Notes on Prof. James Hall's Address," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 70.

[12] James Hall, Geology of New York. Part IV. Comprising the Survey of the Fourth Geological District (Albany: State of New York, 1843). The fourth district included the western sixteen counties of the state plus a portion of Tompkins County. The geology of each of these was briefly described in this report with the exception of Wyoming County which lies in the middle of the district. Neither in his listing of the counties in the district nor in his geologic descriptions did Hall mention this county, nor did he give any reason for its omission. Ibid., pp. xvi, 414-99.

the natural laws, or a miraculous interposition of creative power." To Hall, only during a comparatively recent period of time had this interpretation become accepted and the stupendous conclusion reached that "nature has been operating through incalculable periods of time, with the same harmony and unity of design as we behold in her present creations."[13]

Hall also sharply rejected the notion that the Biblical deluge could be of any importance to the study of geology, postulating that

> the idea of a universal deluge, early inculcated, and strengthened by the arguments and facts brought forward to sustain the opinion, has led to the general belief that all superficial deposits were due to a single period, and to one agency. Geological phenomena are now studied without reference to preconceived opinions or interpretations, and by adopting more natural and rational explanations than otherwise could be done, we escape advocating numerous absurdities, without conflicting with religious opinions. No geologist, at the present time, can use the term diluvium in connection with the deluge of Scripture history, or refer the superficial detritus of a country to the same agency.[14]

In one of the earlier annual reports, Hall revealed a familiarity with the Principles of Geology by Lyell, picturing its publication as "an era" in the science of geology. This was one of his first statements of a willingness to accept the uniformitarian principles so ably expounded by Lyell.[15]

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[13]Ibid., pp. 10-11. [14]Ibid., p. 339.

[15]James Hall, "Fourth Annual Report of the Survey of the Fourth Geological District," State of New York, Communication from the Governor, Transmitting Several Reports Relative to the Geological Survey, Fourth Annual Report of

Nearly twenty years later, Hall again listed Lyell as one of the sources of his geological philosophy, saying he had "necessarily incorporated the general philosophic views so long ago clearly set forth by Babbage, Herschel, Lyell and others; since these had early been fixed in my mind as a part of the elements and principles of geological science."[16]

Statements Preliminary to the Subsidence Concept

Just when Hall first formulated the set of ideas that constitute his subsidence concept is difficult to determine. They seemed well thought out by 1857 when he presented them to the A. A. A. S. at Montreal, but he made only a few references to such ideas prior to that date. What may have been his first preliminary statements are scattered here and there in the series of reports made to the New York Assembly during the period 1837-1840 when he was employed as geologist for the fourth geological district of the state. In the report for 1837, he called attention to fossilized remains of an early trilobite known as the Lingula which he found in great quantities throughout several layers of sandstone. Many of these fossils were aligned in a single direction, generally northwest by north to southeast by south, with small ridges of stone extending

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the Geological Survey, Assembly No. 50, January 24, 1840, p. 394, hereinafter cited as "Fourth Annual Report."

[16]Hall, Palaeontology . . ., III, 81.

from the beaks of the fossils to the southeast. "It is impossible to avoid the conclusion," related Hall, "that the surface of each of these layers was once the original surface of the sandy bottom of an ocean, covered with living shells, over which a gentle current flowed." The direction of the current of this ancient sea could be determined, he believed, simply by reference to the ridges of stone built up by each of these fossils. Further evidence of an ocean current appeared in scratches on the surface of rocks, similar to those which are impressed by flowing water on soft and yielding beds of sand or clay.[17]

Two years later Hall speculated briefly about the character of deposits in the formation known as the Old Red sandstone which appears in many locations within the part of New York where he was working. "It affords us some valuable facts," he said,

> regarding the manner of deposition in many of our rock masses, being in deep basins of greater or less extent, some rising rapidly from the centre, and causing the abrupt thinning out of the deposits; others, from their more gentle ascent, admitting the gradual thinning of the strata, and their continuance over a greater strata. . . . We reasonably infer that the thicker portions of of the mass are nearer the source of the material, from whence it flowed over the bottom of the ancient ocean in the state of soft mud, its direction being determined by a current, or otherwise, until it thinned out at a distance from its origin, in proportion to the quantity

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[17] James Hall, "Second Annual Report of the Fourth Geological District of New York," State of New York, Communication from the Governor, Relative to the Geological Survey of the State, Second Annual Report of the Geological Survey, Assembly No. 200, February 20, 1838, pp. 296.

of material supplied.[18]

In this statement can be seen some of Hall's earliest suggestions of the relationship of ocean currents, sources of sediments, deposition, and areas of significant sedimentation.

In his 1843 report, Hall further amplified his postulate that ocean currents are the agency which moves masses of sediments to where they are deposited. He repeated and expanded his description of the stony formations appearing with Lingula fossils that indicated to him the existence of gentle ocean currents at the time of their deposition.[19] He also described ripple marks in Medina sandstone that were "beautifully preserved," and these, along with the diagonal lamination of this formation, indicated the existence of currents in the ancient ocean which covered the area.[20] Wave lines, ridges of sand, slight

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[18] Hall, "Fourth Annual Report," p. 408.

[19] Hall, Geology of New York . . ., pp. 52-53.

[20] Ibid., pp. 49-50. The strata which Hall studied at length in the fourth district are generally of the Palaeozoic period, and the names of the formations and groupings are those he customarily used to describe those strata. They are, proceeding from the lowest to the highest:

1. Primary
2. Potsdam sandstone
3. Calciferous sandrock
4. Black-river limestone
5. Trenton limestone
6. Utica slate
7. Hudson-river group
8. Gray sandstone and Oneida conglomerate
9. Medina sandstone
10. Clinton group
11. Niagara group
12. Onondaga-salt group
13. Helderberg series
14. Hamilton group, including Marcellus slate and Moscow shale
15. Tully simestone

(cont. on next page).

scratches, and deeper furrows in the mud of the Portage group likewise are "beautifully illustrative of the effects of oceanic currents upon the bottom."[21] In his conclusions to the report, Hall noted that in some cases oceanic currents may diffuse slowly precipitating sediments over wide areas, while other types of sediments are carried only short distances before settling to the bottom. In either case ocean currents are essential to the process.[22]

The evidence provided when he compared the Old Red sandstone with lower strata indicated to Hall that an ocean and its currents are necessary to the growth of a continent. The older groups are distributed over a wide area, oftentimes appearing consistently throughout an area that extends at least a thousand miles to the west of New York, but the Old Red sandstone is not found beyond the boundaries of the state in that direction. He attributed this change in condition to a diminution in the transporting power of the ocean currents which had in earlier times carried materials over a much wider area.[23]

Hall found it necessary to consider the source of materials that make up the formations he observed. The huge

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16. Portage group and Genesee slate
17. Chemung group
18. Old Red system
19. Conglomerate of the Carboniferous system

Ibid., p. 27.

21 Ibid., pp. 234-35. 22 Ibid., p. 521.

23 Ibid., p. 279.

area of the Appalachian chain and the immense amount of sediment of which it is constituted seemed to indicate that a source area of continental proportions was a prerequisite. ". . . We must conceive," he asserted, "of the existence of continents where no vestige of them now remains. . . ."[24] This process, whereby sediments were moved from the ancient continent to the Appalachian region, declared Hall, is the same as that of more recent geologic times and which continues in the present. Using the fourth geological district as an example, he reasoned by analogy that

> the high hills and deep valleys indicate the absence of an immense quantity of matter, which . . . was transported in the direction of the great outlets into the present ocean, there to lay the foundation of future continents in strata like those occupying our district, filled with the organic remains of successive ages, and exhibiting throughout their extent all the varying characters that we now find in the rocky strata of our continent.[25]

In the same report Hall speculated that the continental source of sediments for the Appalachians lay to the eastward of what is now the North American continent. The Chemung group is a mass of sediments which, he declared, had its origin in that direction. Fragments of land plants in that horizon may have drifted from dry land further to the east, some in a perfect state of preservation that indicated either they could not have drifted far or they were moved in a quiet sea. "The inference naturally follows," he said, "that these were derived from land on the eastern

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[24] *Ibid.*, p. 521.   [25] *Ibid.*, p. 16.

margin of the ocean; and that some fragments floated westward, and were deposited with the sand and mud."[26] He concluded that the chief source of the materials making up our present strata must have been from the east or southeast.[27]

During this early period Hall did not attack the problem of subsidence or elevation except in a very general way. He acknowledged that these processes had indeed taken place, but he did not attempt to describe how or why they had occurred. At one point he did make a statement that expressed a rudimentary idea of compensatory elevation and subsidence. In the deposition of the Onondaga-salt group, which in his district has a depth of about 1,000 feet, he concluded that some rather violent geologic event, such as the outbreak of a mud volcano, had spewed forth huge amounts of material which spread rapidly and widely over the bed of the ocean. "The elevation of one point, attendant on such an eruption," he believed, "would naturally be accompanied by a corresponding depression of another, and this portion is probably that upon which we have been making our investigation."[28] However, he did not repeat or expand this idea at that time.

From 1843 to 1857 Hall did not publish any of his ideas on elevation or subsidence, and he made only a few brief statements that can be associated with the subsidence

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[26] Ibid., pp. 254-56, 274. [27] Ibid., p. 522.

[28] Ibid., p. 133.

concept. In the first of these, which involves the deposition of the Hudson-river group, he again speculated on the direction from whence the sedimentary materials had come for the Appalachians. He thought they probably had come from the east and northeast with the coarser materials being deposited first and the finer portions being carried further into the ocean, resulting in a very wide-spread formation.[29] But he did not then seem prepared to discuss subsidence any more than he had been in 1843.

Hall's second statement appeared in a paper delivered at the 1856 meeting of the A. A. A. S. There he gave definite indication that he was nearly ready to reveal his concept of subsidence. He mentioned briefly the relationship between large deposits of sediments and mountain building, but he did no more than mention it. "The discussion of this part of the subject . . . does not properly enter into the present paper, and will be postponed to another occasion," he stated.[30] Since he was the president of the association for that meeting, Hall may have had it in mind to reveal his ideas fully at the meeting the

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[29] James Hall, "Lower Silurian System," U. S. Senate, _Report on the Geology of the Lake Superior Land District, Part II, the Iron Region, together with General Geology_, by J. W. Foster and J. D. Whitney, Executive No. 4, 32d Cong., Special Session, March, 1851, p. 150.

[30] James Hall, "On the Carboniferous Limestones of the Mississippi Valley," _Proceedings of the American Association for the Advancement of Science_, X (1856), part II, 67.

following year in his capacity as retiring president.[31] In any case that is what he did, the concept was not revealed in 1856, and it did appear in 1857. It may be reasonable to assume, however, that his concept had been comprehensively formulated by 1856. In a footnote to the published version of his 1856 paper, Hall referred the reader to volume three of the Palaeontology of New York in which the concept was given its most definitive exposition.[32] Though this volume was not published until three years later, it had been in progress for a number of years, and the printing had commenced in 1856, indicating that the textual material probably was in satisfactory form at that time.[33]

In a third brief reference to elevation and subsidence, Hall compared the thickness of the strata in the Mississippi valley and in the Rocky Mountain region with the respective elevations of the two areas. He noted that where the sediments were but a few hundred feet in thickness, the forces of elevation had virtually died out, while in the region to the west where the sediments had reached great thickness, they had been raised into high mountain chains.

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[31] Several years later, Hall asserted the address had been composed in 1856. Letter from Hall to J. Peter Lesley, Albany, December 21, 1865, American Philosophical Society Library, Philadelphia, Pennsylvania, Lesley Papers.

[32] Hall, Proceedings of the American Association for the Advancement of Science, X, part II, 67, n.

[33] Hall, Palaeontology . . ., III, xi; Letter from Hall to Lesley, Albany, December 3, 1860, American Philosophical Society Library, Philadelphia, Pennsylvania, Lesley Papers.

He indicated that in the Mississippi valley these forces would be absent for want of material to be elevated.[34] In this passage can be noted the relationship postulated by Hall between the thickness of sedimentary strata and the elevation of mountains.

Thus far in this section, Hall's own statements and ideas have been briefly reviewed to reveal what he himself had to say about the concept of subsidence in the period prior to 1857. In the paragraphs that follow, the various elements of the concept will be examined to determine which were original with Hall and which he obtained from printed or manuscript materials or by personal association with contemporary scientists.

Ocean Currents

Deposition of sediments on a scale leading to large area subsidence can take place only in ocean areas, if Hall's thesis is valid. Furthermore, the only means whereby those materials can be transported into the areas where they are finally deposited is through the agency of ocean currents. For these ideas Hall seems indebted to Lyell. Ocean currents, said Lyell, have the capability of carrying the finer particles of sediments over distances of hundreds and even thousands of miles under ideal conditions. Citing figures which indicate that minute particles of sediment

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[34]Hall, Proceedings of the American Association for the Advancement of Science, X, part II, 67.

settle very slowly in water, he demonstrated, at least to his own satisfaction, that ocean currents may distribute sediments over very large regions of the oceans.[35]

To the casual observer, said Lyell, large rivers that form significant deltas appear to be the most important of the transporting agencies, but these are far less significant when compared to the capacity of ocean currents to transport and deposit sediments. In the first American edition of his Principles, Lyell asserted that by comparison with currents, "the deltas of rivers must shrink into insignificance."[36]

As one example of this process, Lyell cited the eastern Mediterranean where, he said, the same current that rapidly erodes a part of the northern coast of Africa acts also on the delta of the Nile, carrying much of the sediment of that river eastward where it is again deposited. This, he thought, might account for the rapid build-up of

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[35]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 (1st American ed.; Philadelphia: James Kay, Jun. & Brother, 1837), I, 286. In his "Introduction" of 1859, Hall made reference to this edition of the Principles. At another point he complained about the paucity of printed materials he had available, so it may not be unreasonable to assume that he used this edition. Hall's comment about his meager library appeared in Hall, Palaeontology of New-York, Volume I, Containing Descriptions of the Organic Remains of the Lower Division of the New-York System, (Equivalent of the Lower Silurian Rocks of Europe), Vol. I, Palaeontology of New York (Albany: State of New York, 1847), p. xii.

[36]Lyell, I, 286.

land on the eastern shores of the Mediterranean where no rivers are at hand to provide sediments. The isthmus of Suez itself might have been formed by the combined action of this current and the Nile river.[37]

As another example of the interrelationship between ocean currents and the deposition of materials, Lyell described the pattern of currents between the mouth of the Amazon river and the southern coast of North America. A great current flows westward across the Atlantic from about the mid-point of the west coast of Africa, along the northern coast of Brazil, into the Caribbean sea, and finally into the Gulf of Mexico. Where it meets the current flowing outward from the Amazon, the oceanic current picks up large amounts of sediment, carrying it to the northwest as far as the mouth of the Orinoco. Along the nearby coast of Guinea, an immense tract of swamp has been formed by deposition of these materials, an area Lyell thought might eventually be converted to dry land. The sediments from the Orinoco are partly detained, settling near the mouth of that river and causing the coastline of Trinidad to be extended rapidly, and partly carried away into the Caribbean sea by the ocean current. As this current proceeds along the coast of Mexico, it prevents the growth of deltas and preserves an almost uniform curve in the shoreline of that coast. "It must, therefore," explained Lyell, "exert a great

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[37] Ibid.

transporting power, and it cannot fail to sweep away part of the matter which is discharged from the mouths of the Norte [Rio Grande] and the Mississippi."[38]

Unlike Lyell, Hall did not expressly play down the importance of the river deltas as agents of deposition, but neither did he emphasize it. He did call attention repeatedly to the transporting power of ocean currents, and he repeatedly made reference to strata which had been deposited by those currents.[39]

Insofar as the transportation of sediments over widespread areas in the ocean is concerned, the requirements for Hall's concept seem to have been satisfied by Lyell's conclusions and by his own observations. Hall had to assume first that there were currents in the ancient ocean, and this he believed had been verified by the evidence of the Lingula fossils and the wave and current markings he had gathered himself.[40] He also had to assume that the currents were an effective agency for the transport of materials over wide areas, and Lyell's conclusions tended to satisfy this requirement.[41] Hall noted that the Appalachian chain had a regular elongated shape. To provide this configuration,

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[38] Ibid., I, 287-88.

[39] Hall, MSS of the Montreal Address, pp. 39, 41, 59-61, 73, 76; Hall, Palaeontology . . ., III, 49, 52.

[40] See notes 17, 18, 19, supra.

[41] See notes 35, 36, 37, 38, supra.

the ocean currents which had transported the sediments for these mountains would have flowed for long distances parallel to a nearby coastline.[42] The examples of currents in the Gulf of Mexico and the Mediterranean sea described by Lyell seem to have satisfied this requirement.[43]

From another English geologist, Henry T. De La Beche (1796-1855), Hall also drew the idea that ocean currents play a large role in sedimentary deposition. Disregarding other factors, De La Beche believed that the transporting power of currents increased as the depth of the sea decreased, so that in relatively shallow water, more detritus is carried than in deeper areas. Thus, shallow-water areas near the coastlines of continents exhibit the greatest effects from this agency. Though not emphasizing the significance of sedimentary deposition by currents as much as Lyell and Hall, he nevertheless recognized that significant accumulations could result, especially where large rivers poured heavy loads of materials into the ocean.[44]

William W. Mather (1804-1859), one of the team of geologists who conducted the first geological survey of New York during the period 1836-1842, composed an expression of

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[42]Hall, Palaeontology . . ., III, 68, 73, 83, 96, n.

[43]See notes 37, 38, supra.

[44]Henry T. De La Beche, A Geological Manual (Philadelphia: Carey & Lea, 1832), pp. 105-06.

ideas much closer to Hall's view on ocean currents. Mather relied heavily on this agency in his explanations of the movement of sediments. The similarity of sedimentary rocks in Europe and America, he thought, might also be attributed to this factor, and

> the uniformity of composition of the particular masses, whether thick or thin, their similar mineralogical characters over vast areas, the general similarity of organic contents not only on the American continent but even in Europe, indicate that the causes of these depositions and the conditions under which they were deposited from the ocean, acted with great uniformity over extensive portions of the earth's surface. The polar and equatorial currents are believed to be adequate for the production of the effects observed.[45]

Mather's contention that deposition occurs uniformly over large areas is similar to ideas expressed by Hall in his "Introduction" of 1859. There Hall noted that strata had been deposited in one intelligible sequence from the Potsdam sandstone to the end of the coal measures over practically all of the United States from the Atlantic slopes to the base of the Rocky mountains.[46]

Elevation and Subsidence

An important part of Hall's concept involves the

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[45] William W. Mather, "On the Physical Geology of the United States East of the Rocky Mountains, and on Some of the Causes Affecting the Sedimentary Formations of the Earth," American Journal of Science, XLIX (1845), 12-13. The concepts Mather expressed in this paper were extracted from his much more comprehensive treatment in Mather, Geology of New York, Part I, Comprising the Survey of the First Geological District (Albany: State of New York, 1842), passim.

[46] Hall, Palaeontology . . ., III, 67-68.

process of subsidence itself and its complement, the elevation of the strata. Here Hall made very specific reference to the primary source of the ideas he used in his own concept. He related:

> I have already alluded to the explanation, given by Sir John Herschel, of the process by which continental areas may be elevated from the accumulation of deposits upon the ocean bed. I have seen this explanation only as published in the appendix to Babbage's Ninth Bridgewater Treatise, as an extract of a letter from this philosopher to Sir Charles Lyell. . . . This and another letter to Lyell, and one to Sir R. I. Murchison, contain many suggestions which seem to me as offering support to the views I have advanced. . . .[47]

At another place Hall gave further acknowledgment to Herschel and others, stating that "I have necessarily incorporated the general philosophic views so long ago clearly set forth by Babbage, Herschel, Lyell, and others. . . ."[48] These philosophic views of Herschel concern the effects that might be brought on by significant abrading and depositing of sediments over large areas of the earth's surface. Herschel first rejected the idea that the interior of the earth exhibited the characteristics of a fluid saying that "the ordinary repose of the surface argues a wonderful inertness in the interior, where, in fact, I conceive that every thing is motionless."[49] He then asserted

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[47]Ibid., p. 95. The individuals noted are John F. W. Herschel (1792-1871), British astronomer; Charles Babbage (1792-1871), British mathematician; and Roderick Impey Murchison (1792-1871), British geologist.

[48]Ibid., p. 81.

[49]Letter from Herschel to Lyell, Fredhausen, Cape of

that the heat in the interior of the earth tends to form in isothermal patterns, planes at which temperatures are equal. Near the center of the globe, these patterns tend to be spherical, but near the surface they tend to follow the configuration of the solid portion of the surface, meaning the bottom of the sea and the surface of the continents. If a large quantity of sediments is then deposited in any area, the equilibrium of temperature will be altered, and the isothermal surface will tend to creep upwards to its former distance from the surface.[50] The "influx of caloric from below, which must take place," speculated Herschel, "shall either heave up the whole mass, as a continent, or shall crack *it*, and escape as a submarine volcano, or shall be suppressed until the mere weight of the continually accumulating mass breaks its lateral supports at or near the coast lines, and opens there a chain of volcanos."[51]

Herschel noted Lyell's conjecture that the largest transfer of material to the bottom of the ocean is produced along a coastline by the action of ocean currents. He then speculated that variations in local pressure due to this transfer of material may bring about changes in the

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Good Hope, February 20, 1836, quoted in Charles Babbage, *The Ninth Bridgewater Treatise: A Fragment* (London: John Murray, 1837), p. 207. This letter will hereinafter be cited as: Letter from Herschel to Lyell, 1836.

[50] *Ibid*., pp. 207-13. [51] *Ibid*., pp. 212-13.

elevation of the affected areas. Where the materials are deposited, the surface will tend to subside, forcing yielding material from that location underneath the continent from which the materials had come, causing a new continental area to be elevated.[52] Herschel thus postulated a condition of equilibrium in the earth's surface, asserting that the added weight of materials in one location must cause a subsidence at that place with a compensatory elevation at another. This, he said, is produced by two different processes. First, it produces a "mechanical subversion of the equilibrium of pressure," and second, by another process, it "produces a subversion of the equilibrium of temperature." He concluded that

> thus the circuit is kept up--the primum mobile is the degrading power of the sea and rains (both originating in the sun's action) above, and the inexhaustible supply of heat from the enormous reservoirs below, always escaping at the surface, unless when repressed by an addition of fresh clothing, at any particular part. In this view of the subject, the tendency is outwards. Every continent deposited has a propensity to rise again; and the destructive principle is continually counterbalanced by a re-organizing principle from beneath.[53]

In the second of the two letters quoted by Babbage, Herschel speculated as to what a geological primum mobile might be and how it acted. "In [the] future, therefore," he said,

> instead of saying, as heretofore, 'Let heat from below invade newly-deposited strata (Heaven knows how or why), then they will melt, expand,' &c. &c., we shall

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[52] Ibid., pp. 210-12. [53] Ibid., pp. 212-13.

> commence a step higher, and say, '*Let strata be deposited*.' Then, as a necessary consequence, and according to known, regular, and calculable laws, heat *will* gradually invade them from below and around; and, according to its due degree of intensity at any assigned time, will expand, ignite, or melt them, as the case may be, &c. &c. &c.; and, I mistake greatly, if this be not a considerable reform in our geological language.[54]

In these two letters Herschel expounded a rather revolutionary geological theory. Much previous geological speculation included an assumption that changes in the earth's crust must be attributed to internal forces.[55] Herschel disagreed, arguing that the interior of the earth is essentially inert and that there are no internal forces present that will act without some sort of outside stimulus. In his view the activating principle turned out to be the ordinary process of abrading and depositing of sediments, the mass transfer of materials from one area to another. When this takes place in significant proportions, the physical processes associated with heat and pressure are activated, resulting in elevation and subsidence.[56]

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[54]Letter from Herschel to Roderick I. Murchison, Feldhausen, Cape of Good Hope, November 15, 1836, quoted in Babbage, pp. 216-17. This letter will hereinafter be cited as: Letter from Herschel to Murchison, 1836.

[55]See for instance Leopold von Buch, "Ueber Erhebungscratere und Vulcane," *Annalen der Physik und Chemie*, XXXVII (1836), 169; James Hutton, "Theory of the Earth; or an Investigation of the Laws Observable in the Composition, Dissolution, and Restoration of Lands upon the Globe," *Transaction of the Royal Society of Edinburgh*, I (1788), 263-67; John Playfair, *Illustrations of the Huttonian Theory of the Earth* (Edinburgh: William Creech, 1802), pp. 53-55.

[56]See notes 53, 54, *supra*.

The fundamental notion that crustal movements occur only when outside forces are brought into play is an essential part of Hall's subsidence concept. Herschel's part in this idea is noted above. Lyell, said Hall, also postulated that the "ordinary repose of the surface of our planet argues a wonderful inertness in the interior." From this assertion and from Herschel's, Hall argued that geologists "must look for external influences to provoke the interior manifestations," and these external forces consist in the main of abrasion, removal, and redeposition of sedimentary matter.[57]

Hall also singled out Babbage, the author of the Ninth Bridgewater Treatise,[58] as a source of his general view of elevation and subsidence.[59] Babbage had visited the Temple of Jupiter Serapis at Pozzuoli near Naples, Italy, in 1828, and he used the evidence of subsidence and elevation he noted there to explain a number of the changes that are going on in the earth's crust at the present time. His conclusions were first related to the Geological Society

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[57] Hall, Palaeontology . . ., III, 87, 96.

[58] The textual material of this treatise has little if anything to do with the study of geology, but in a series of appendices Babbage discussed a number of topics including some ideas on geological theory. Appendices F, G, H, and I are directly concerned with ideas of elevation and subsidence, and it is to these that Hall referred in his "Introduction" of 1859. Herschel's two letters are quoted in part in Appendix I of Babbage's book.

[59] Hall, Palaeontology . . ., III, 87.

of London in March, 1834, and an abstract of his paper was published in the Philosophical Magazine the same year.[60] Another shortened version of his conclusions is found in his Ninth Bridgewater Treatise,[61] but the paper was not published in full until 1847.[62]

At the time of Babbage's visit, the Temple of Jupiter Serapis stood about one hundred feet from the shoreline of the sea at an elevation very near the high tide level. He estimated the temple had been erected about the end of the second century A. D., and at some subsequent time it subsided with the surrounding land to a depth of twenty to thirty-five feet below sea level. About 1500 A. D., Babbage believed, the land around the temple rose again and the temple emerged from the sea. He contended that the land on which the temple stands noticeably subsided again between

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[60] [Charles Babbage], "Observations on the Temple of Serapis, at Pozzuoli, near Naples, with Remarks on Certain Causes Which May Produce Geological Cycles of Great Extent," Philosophical Magazine, series 3, V (1834), 213-16.

[61] Babbage, Ninth Bridgewater Treatise . . ., pp. 187-97.

[62] Charles Babbage, "Observations on the Temple of Serapis, at Pozzuoli, near Naples, with Remarks on Certain Causes Which May Produce Geological Cycles of Great Extent," The Quarterly Journal of the Geological Society of London, III (1847), 186-217. Since Hall referred specifically to the Ninth Bridgewater Treatise, one would be on safest ground by assuming that this was his source of Babbage's ideas. However, Hall elsewhere made frequent reference to issues of the Quarterly Journal, he quite possibly may have seen the full version of the paper in that journal also.

1828 and 1845.[63] As evidence that the temple had at one time been partially submerged in sea water, he noted an eight-foot long section of the temple's columns containing remains of a boring marine animal, the Modiola lithophaga of Lamarck, which still lives in the nearby Mediterranean. The lower extremity of this section is about eleven feet above the base of the column, indicating that the lower portion had probably been buried in sand or mud.[64]

In the surrounding area Babbage noted numerous indications of subterranean heat, the most striking being the volcano, Vesuvius. He supposed that in some manner the intensity of heat at lower levels might change, being alternately lower and higher. The overlying strata would thus be subjected to expansion caused by an increase in heat or contraction by its loss. Using a series of rates of expansion for various minerals compiled by Lieutenant William H. C. Bartlett (1804-1893) of the U. S. Engineers, Babbage computed that a change of temperature of only 100 degrees Fahrenheit, acting on a thickness of five miles of strata, would cause a change of level of about twenty-five feet, an elevation or subsidence nearly equivalent to that experienced by the temple.[65] This explanation he considered to

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[63] Ibid., p. 213. [64] Ibid., pp. 186-88.

[65] Ibid., pp. 203-05; William H. C. Bartlett, "Experiments on the Expansion and Contraction of Building Stones, by Variations of Temperature," American Journal of Science, XXII (1832), 136-40. Babbage also made reference to a similar series of experiments conducted by Alexander

be most tenable, because, he said,

> it is founded on facts--viz. that matter expands by heating; that great accessions of heat have at various times taken place in the neighborhood of the temple; that it is sufficient to account for the phaenomena by supposing a moderate depth of the beds below it heated to a degree which it is not unreasonable to presume must have taken place; that such changes of level would on the whole occur gradually, although they might be accompanied with earthquakes and occasionally by sudden changes of level--facts of which we have historical evidence as having happened on this spot.[66]

The example furnished by the Temple of Serapis led Babbage to consider whether or not other changes of level of the earth's surface could be explained by similar deductions. Continents and mountains might thus be accounted for by similar but much more vast cycles for which he thought geology gave incontrovertible proofs. He drew up a list of principles to which he attributed the changes which continually take place in the forms and levels of large areas of the earth's surface. The theory rests, he said, upon the following:

> 1st. That as we descend below the surface of the earth at any point, the temperature increases.
> 2nd. That solid rocks expand by being heated, but that clay and some other substances contract under the same circumstances.
> 3rd. That different rocks and strata conduct heat differently.

J. Adie who had also attempted to evaluate the expansion of different types of rocks per unit rise in temperature. Adie, "On the Expansion of Different Kinds of Stone from an Increase of Temperature, with a Description of the Pyrometer Used in Making the Experiments," Transactions of the Royal Society of Edinburgh, XIII (1836), 354-72.

[66]Babbage, The Quarterly Journal of the Geological Society of London, III, 205.

4th. That the earth radiates heat differently from different parts of its surface, according as it is covered with forests, with mountains, with deserts, or with water.

5th. That existing atmospheric agents and other causes are constantly changing the condition of the earth's surface, and that, assisted by the force of gravity, there is a continual transport of matter from a higher to a lower level.[67]

The concept of isothermal surfaces also played an important role in Babbage's thinking. In accordance with the principles listed above, the isotherms would be irregular, tending to follow the solid surface of the earth, thus descending where they pass under deep oceans. Then suppose, he explained, that "by the continual wearing down of the continents and islands adjoining the ocean, that it becomes filled up."[68] The isothermal plane in this location would tend to rise toward the surface and consequently the material through which the isotherm moved upwards would expand because of this rise in temperature, causing the surface to be uplifted. The ocean would become shallower in the operation, and this shallowness, by exposing the ocean to greater evaporation, would give increased force to atmospheric causes, tending thus to fill up the depressions even more rapidly. This whole process, contended Babbage, might become somewhat oscillatory in nature before the full effect of the of the expansion from underneath had permanently raised the entire new land above the level of the adjacent oceans.[69]

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[67] _Ibid._, pp. 205-06. [68] _Ibid._, p. 207.

[69] _Ibid._, pp. 207-11.

In the final remarks of his paper, Babbage concluded

> that from changes continually going on, by the destruction of forests, the filling up of seas, and the wearing down of elevated lands, the heat radiated from the earth's surface varies considerable at different periods. In consequence of this variation, and also in consequence of the covering up of the bottoms of seas, by the detritus of the land, the surfaces of equal temperature within the earth are continually changing their form, and exposing thick beds near the exterior to alterations of temperature. The expansion and contraction of these strata, and, in some cases, their becoming fluid, may form rents and veins, produce earthquakes, determine volcanic eruptions, elevate continents, and possibly raise mountain chains.[70]

In his memoir as it appeared in 1847, Babbage explained that the slow and irregular elevation and subsidence of various tracts of the earth's surface had been postulated through a process of "pure reason." He was surprised and obviously pleased to report that direct confirmation had become available in a work published by Charles Darwin relating to the formation of coral and lagoon islands and to the relative changes of level of land and water. These views of Darwin, he said, had resulted from "a large induction of facts, [and Darwin had] arrived at exactly the same conclusion as that which it has been the chief object of this paper to account for, from the action of known and existing causes."[71]

Both Babbage and Herschel included the concept of isothermal surfaces as important components of their views on elevation and subsidence, and from this at least one

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[70] Ibid., p. 212. [71] Ibid., pp. 212-13.

geologist, Murchison, thought their views to be identical. For this assumption, however, Herschel took him to task. Babbage's theory, he noted, ascribed the elevation of strata by pyrometric expansion of subjacent columns of rock to an invasion of subterranean heat, but Herschel denied that this was all there was to his own theory; that Murchison had rather missed his main idea.[72] And as already mentioned, Herschel's concept seems much broader in its implications than Babbage's, including a basic assumption that there is some sort of hydrostatic equilibrium in the crust of the earth. This concept is largely missing from Babbage's postulations. In any case, Hall's concept incorporated the ideas of both to a substantial degree.

As Babbage noted, Darwin's speculations about the subsidence of coral reefs and lagoon islands fits into the general scheme developed by Herschel and Babbage rather well. Drawing his conclusions from information gathered during the famed voyage of H. M. S. Beagle from 1832 to 1836, Darwin hypothesized that coral atolls and barrier reefs are evidence of submerged land or land that is subsiding gradually. This subsidence could, he believed, be attributed to the added weight furnished by the growth of the coral polypi in shallow water surrounding the land. After immense periods of time, this added material can affect an area in the same manner as the deposition of

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[72] Letter from Herschel to Murchison, 1836, p. 214.

sedimentary materials, that is, by causing it to subside gradually. In the Pacific and Indian Ocean areas he noted symmetrical areas of two kinds; "the one sinking, as deduced from the presence of encircling and barrier reefs, and lagoon islands, and the other rising, as known from uplifted shells and corals, and skirting reefs."[73] Though not stating so in specific terminology, Darwin implied some sort of an hydrostatic equilibrium in the crust of the earth in a manner reminiscent of Herschel's hypothesis. In the Pacific region, he said,

> we . . . see vast areas rising, with volcanic matter every now and then bursting forth through the vents or fissures with which they are transversed. We see other wide spaces slowly sinking without any volcanic outbursts; and we may feel sure, that this sinking must have been immense in amount as well as in area, thus to have buried over the broad face of the ocean every one of those mountains, above which atolls now stand like monuments, marking the place of their former existence. Reflecting how powerful an agent with respect to denudation, and consequently to the nature and thickness of the deposits in accumulation, the sea must ever be, when acting for prolonged periods on the land, during either its slow emergence or subsidence; reflecting, also, on the final effects of these movements in the interchange of land and ocean-water, on the climate of the earth, and on the distribution of organic beings, I may be permitted to hope, that the conclusions derived from the study of coral-formations, originally attempted merely to explain their peculiar forms may be thought worthy of the attention of geologists.[74]

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[73]Charles Darwin, "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations," Proceedings of the Geological Society of London, II (1837), 552-54.

[74]Charles Darwin, The Structure and Distribution of Coral Reefs; Being the First Part of the Geology of the Voyage of the Beagle, under the Command of Capt. Fitzroy, R. N. during the Years 1832 to 1836 (London: Smith, Elder and Co., 1842), pp. 147-48.

Hall most likely had available at least a portion of Darwin's work on corals, coral reefs, and the associated ideas of subsidence and elevation. These ideas are found in several sources, appearing first in a paper read before the Geological Society of London and published in its <u>Proceedings</u> in 1837. They were repeated in Darwin's <u>Journal of Researches in Geology and Natural History</u>, published in 1839, and in 1842 his <u>Structure and Distribution of Coral Reefs</u> appeared, giving a more comprehensive treatment of the subject. Hall made specific reference to the second of these publications, indicating he had it available.[75] Further indication that Hall had at least been made aware of Darwin's conclusions appeared in a letter from James D. Dana, who, in speaking of a forthcoming paper he was to present, said he might "also have something to say on the region of elevation & subsidence in the Pacific in which I disagree entirely from Darwin, although I adopt his general theory with regard to the formation of coral islands."[76] Though Hall at no time specifically referred to Darwin's

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[75]Darwin, <u>Proceedings of the Geological Society of London</u>, II, 552-54; Darwin, <u>Journal of Researches into the Geology and Natural History of the Various Countries Visited by H. M. S. Beagle, under the Command of Captain Fitzroy, R. N. from 1832 to 1836</u> (London: Henry Colburn, 1839), pp. 553-69; and Darwin, <u>The Structure and Distribution of Coral Reefs</u> . . . . Hall's reference to the <u>Journal of Researches</u> is in his <u>Geology of New York . . .</u>, p. 336, n.

[76]Letter from Dana to Hall, Washington City, March 28, 1843, Hall Papers.

speculations on subsidence and elevation, and though no evidence is available that they contributed to Hall's concept, still it is not entirely unreasonable to assume that Darwin may have been one of Hall's sources of ideas.

From the writings of Lyell that Hall either had or might have had, he probably obtained no specific ideas on elevation and subsidence, as Lyell did not provide any generalized theory on the subject. Instead he confined his remarks to reviewing ideas of such individuals as Leopold von Buch, Babbage, Herschel, and others who had discussed the general topic. He likewise included in his *Principles* a substantial amount of observational data on land areas that had been uplifted or had subsided and on the effects of volcanos and earthquakes.[77] In a series of lectures delivered in New York in 1842 and published there the same year, Lyell reiterated the views of Babbage on the Temple of Jupiter Serapis as a most striking example of the process of elevation and subsidence, but little material of any sort appeared in these lectures that Hall did not already have available from other sources.[78] Lyell thus does not appear to have been a major source of ideas for Hall in his generalizations on the processes of elevation and subsidence.

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[77]Lyell, *Principles . . .*, I, 348-449, *passim*.

[78]Charles Lyell, *Lyell's Lectures on Geology; Eight Lectures on Geology, Delivered at the Broadway Tabernacle in the City of New York* (New York: Greeley & McElrath, 1842), pp. 16-23.

In his concept Hall asserted that the line of greatest subsidence would be along the line of greatest accumulation of sediments, and toward the thinning margins of the deposit the depression would be less. As the lower side of the depression becomes gradually curved, rents and fractures occur on that side, or the diminished width of the upper surface will produce wrinklings and foldings of the strata. ". . . The folding of the strata seems," he said, "a very natural and inevitable consequence of the process of subsidence."[79]

"The sinking down of the mass produces a great synclinal axis," continued Hall, "and within this axis, whether on a large or small scale, will be produced numerour smaller synclinal and anticlinal axes." He attributed this idea to Sir William Logan (1798-1875), head of the Canadian Geological Survey.[80] It happens, said Hall, that when a crustal disturbance takes place and anticlinals are formed, the strata become weakened at the highest part of the arch and these become more susceptible at that point to denuding action. Thus the anticlinals are often worn down so much as to form valleys, while the synclinal remains to form the mountain crest. "This is very generally true in many parts of the Appalachian range," he said, and "similar features will be observed in other mountain ranges."[81]

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[79]Hall, Palaeontology . . ., III, 70.

[80]Ibid., 70, and n. [81]Ibid., pp. 71-72.

> It nowhere appears that this folding or plication has contributed to the altitude of the mountains: on the other hand, as I think can be shown, the more extreme this plication, the more it will conduce to the general degradation of the mass, whenever subjected to denuding agencies. The number and abruptness of the foldings will depend upon the width of the zone which is depressed, and the depth of the depression, which is itself dependent on the amount of accumulation.
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> . . . .
>
> It is possible that the suggestion may be made, that if the folding and plication be the result of a sinking or depression of the mass, then these wrinkles would be removed on the subsequent elevation; and the beds might assume, in a degree at least, their original position. But this is not the mode of elevation. The elevation has been one of continental, and not of local origin; and there is no more evidence of local elevation along the Appalachian chain, than there is along the plateau in the west.[82]

In the American Journal of Science, Hunt described Hall's concept in general, and specifically he credited Francois Dominique Reynaud de Montlosier (1755-1838) for the idea of broad continental uplift that Hall used.[83] In 1832 Montlosier objected to contemporary theories of mountain uplift such as von Buch's concept of craters of elevation and Elie de Beaumont's catastrophist-oriented elevation hypothesis. Continental elevation, asserted Montlosier, is the most significant process in the growth of mountains, and the great European ranges are but the remnants of much larger areas of elevation which have been cut away during past ages by ordinary denudation processes. Mountains resulting from volcanic action, foldings, and inversions

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[82] Ibid., p. 72.

[83] T. Sterry Hunt, "On Some Points in American Geology," American Journal of Science, series 2, XXXI (1861), 408.

are but local and accidental.[84]

William Hopkins (1793-1866) appeared on the list of individuals that Hall noted had either investigated or applied laws of geological structure.[85] Hopkins, an English geologist and mathematician, discussed earth movements at length during the early years of Hall's career. He represented a group of geologists and physicists who attempted during the middle years of the nineteenth century to apply physico-mathematical generalizations to the dynamic processes of geological change.[86] He postulated the existence of an unknown elevatory force that acts on the lower surface of an uplifted mass, probably through the medium of a fluid or a mass of matter in a state of fusion from heat.[87]

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[84]Francois Dominique Reynaud de Montlosier, "Sur la formation des vallées, et sur la théorie des soulèvements de montagnes," Bulletin de la Société géologique de France, III (1832-33), 215-17. Hall probably had the Bulletin available according to letters from Édouard P. de Verneuil to Hall, Paris, February 19, 1847, and June 30, 1847, Hall Papers. Furthermore, Hunt later called Hall's attention to Montlosier's article in a letter from Hunt to Hall, Montreal, October 4, 1858, Hall Papers.

[85]Hall, Palaeontology . . ., III, 81.

[86]William Hopkins, "Researches in Physical Geology," Transactions of the Cambridge Philosophical Society, VI (1838), 1-84. Also typical of this approach are Robert Mallet, "Volcanic Energy: An Attempt to Develop Its True Origin and Cosmical Relations," Philosophical Transactions of the Royal Society of London, CLXIII (1873), 147-227; William Thomson, "On the Rigidity of the Earth," Philosophical Transactions of the Royal Society of London, CLIII (1863), 573-82; and William Thomson and Peter G. Tait, Treatise on Natural Philosophy (Oxford: At the Clarendon Press, 1867).

[87]Hopkins, pp. 10-11.

One of Hopkins's speculations that may have been of interest to Hall concerned compensatory elevation and subsidence. ". . . It is immaterial," said Hopkins, "whether we suppose the mass to be bent upwards by a force underneath or downwards by its own weight, provided the regions thus subsiding simultaneously be of the same extent as those which I have spoken of as being simultaneously elevated."[88] This statement, like that of Herschel, has some implications of the principle of crustal equilibrium that appears briefly in Hall's work.[89]

Metamorphism

At the time that Hall's concept of subsidence was in the incipient stage, the question of the origin of the so-called metamorphic rocks was a controversial issue. Lyell, in a discussion of this type of rock, warned the reader that "it was once a favourite doctrine, and is still maintained by many, that these rocks owe their crystalline texture, their want of all signs of a mechanical origin, or of fossil contents, to a peculiar and nascent condition of the planet at the period of their formation." He then proceeded to point out that metamorphic rocks are now attributed to many

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[88] William Hopkins, "Reply to Dr. Boase's 'Remarks on Mr. Hopkins's 'Researches in Physical Geology,'' in the Number for July," Philosophical Magazine, series 3, IX (1836), 172.

[89] Hall, Proceedings of the American Association for the Advancement of Science, XXXI, 69.

different ages, "not only since the first introduction of organic beings into this planet, but even long after many distinct races of plants and animals had passed away in succession."[90] Lyell also concluded that metamorphism is not necessarily concerned with plutonic action, nor is it necessary that a contiguous mass of granite be the altering power;

> but merely that an action, existing in the interior of the earth at an unknown depth, whether thermal, hydrothermal, electrical, or other, analogous to that exerted near intruding masses of granite, has, in the course of vast and indefinite periods, and when rising perhaps from a large heated surface, reduced strata thousands of yards thick to a state of semifusion, so that on cooling they have become crystalline, like gneiss.[91]

In his earlier writings Hall had little to say about metamorphic rocks, merely listing them as constituting part of the strata he happened to be studying at the moment. In his address to the A. A. A. S. at Montreal in 1857, he made no significant reference to the subject, nor did he in the report of the Iowa Geological Survey in 1858. However, in his "Introduction" of 1859, he devoted several pages to the subject and its relationship to his concept of subsidence. Many of the strata in the Appalachian chain, he said, show striking evidence of metamorphic action to one degree or another. When the chain is approached from the west, this

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[90] Charles Lyell, A Manual of Elementary Geology; or, the Ancient Changes of the Earth and Its Inhabitants as Illustrated by Geological Monuments (5th ed.; London: John Murray, 1855), p. 598.

[91] Ibid., p. 603.

evidence becomes more and more noticeable, and as one nears the mountain range, the shales become more broken, are changed in color, and contain more and more particles of a talc-like substance until this becomes a predominating feature. Limestones lose their dark color, and veins of calcareous spar can be observed traversing the mass. Fossils become less distinct in their forms, often appearing distorted. Finally, the rock appears as a homogeneous, crystalline mass, but often with fossils remaining to show the original sedimentary condition of the strata.[92]

Like Lyell, Hall denied that it was necessary to show that this metamorphism had proceeded from contact with or proximity to granitic or other rocks of plutonic origin. It was clearly evident to him that the phenomenon of metamorphism appeared over wide areas where no eruptive or intrusive granitic masses were present. Even granite, he believed, might have been derived from the formation immediately below it, and it most certainly was in all cases a modification of some pre-existing sedimentary rock. But even though he denied the need for evidence to support his contentions, Hall did cite a discovery by Logan that metamorphic crystalline masses contain occasional fragments of pre-existing stratified rocks which retain some of their original characteristics. This latter fact, Hall explained,

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[92] Hall, Palaeontology . . ., III, 74-75.

furnishes proof that the metamorphic mass had never been subjected to the high degree of heat commonly believed accompanies the production of crystalline granitic rocks.[93]

Significantly, Hall observed that only when one approaches a zone of great accumulation of sediments can he find evidence of metamorphism on a grand scale. Using the Appalachians once again as an example, he asserted that

> in this mountain range, and I believe also in others, the line of metamorphic action is parallel to the mountain chain, and parallel to the minor elevations or subordinate axes of the great mass; parallel, indeed, to the great line of original accumulation of the sediments constituting the mountain mass.[94]

Here may be seen the main part of Hall's arguments on metamorphic rocks. The process of metamorphism, like that of mountain formation, simply does not occur without the large accumulations of sedimentary matter that is present in mountain regions. In areas such as the Mississippi valley, therefore, metamorphic rocks are largely absent because of the lack of significant accumulations of sediments.[95]

Furthermore, according to Hall, folding, plication, and other alterations of the strata are not a prerequisite of the metamorphic process. Mountains in which the strata are essentially horizontal contain rocks that show exactly the same metamorphic character as those in nearby formations which have been severely folded and plicated. Accumulation

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[93] *Ibid.*, pp. 75-76. [94] *Ibid.*, p. 78.

[95] James Hall, "On the Formation of Mountain Ranges," *Canadian Journal*, new series, V (1860), 543-44.

and subsequent metamorphism seems to be, speculated Hall, a more significant association than alteration of the position of the strata and metamorphism. He cited as an example some of the beds near the base of the Catskill mountains where the rocks are extremely hard and dense although in beds which are essentially horizontal. These rocks exhibit metamorphic characteristics even more than those of the severely disarranged beds on the western flanks of the Green mountains. "Whatever reason there may be," asserted Hall,

> for the hardness and density, and the incipient subcrystalline condition of these beds, placed beneath more than three thousand feet of accumulated strata, the same beds traced westerly towards the centre of the State do not exhibit the same hardness or tenacity, nor that approach to crystalline texture of some of the beds of the Catskill-mountain group near the Hudson river.[96]

Though most of Hall's statements on the place of metamorphic rocks in the subsidence concept are in his "Introduction" of 1859, a number of manuscript notes indicate that he had seriously considered the subject prior to his 1857 address at Montreal. These notes indicate he had decided, to his own satisfaction at least, that metamorphism is a factor of significant deposition. As early as 1853, he stated a need for a reorientation of thinking concerning the formation of what had theretofore been called "primary" rocks. He made reference to a number of examples in New England and New York where formations, long called primary crystalline rocks, are really altered or metamorphic rocks

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[96] Hall, Palaeontology . . ., III, 78, n.

of Silurian, Devonian, and Carboniferous ages rather than being primordial as had long been thought. He chose to look upon rocks such as granite, sienite, gneiss, mica slate, and numerous others of this class as sedimentary deposits which had been modified by heat and chemical action until they had assumed their now apparent characteristics. "No geological formations can be regarded as primary," said Hall, "until all our efforts have failed to assign them either to their original stratified conditions, or to detect the lines in bedding or stratification in the crystalline mass." This change in attitude on the part of geologists, he related, has resulted to a large extent from the application of chemical knowledge in the effort to understand the changes which have altered rocks previously deposited as simple sediments.[97]

A few years later, probably in preparation for his Montreal address, these speculations were further amplified. "Metamorphism of older rocks [is] greater on account of greater thickness," he wrote, and the "Catskill Mts. in the rocks have a metamorphic <u>aspect</u>--these are Horizontal & have suffered no rupturing or influences from the supposed <u>nucleus</u>." Regarding the relation between thickness of strata and the degree of metamorphism, he speculated that

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[97]James Hall, "Geology of New York and Its Relations with the Surrounding States, Descriptive & Illustrated," Unpublished MSS, Hall Papers, pp. 1-3 of a part added to the section entitled "Older Metamorphic Rocks." Several dates mentioned in this document indicate it was composed in 1853.

"metamorphism in mountain ranges under great thickness occurs--in older rocks more extreme because a greater thickness, greater heat & more entire chemical change has taken place."[98]

The basis for Hall's notions on metamorphism seems to have come from the ideas of two of his closest geological associates, Lyell and Hunt. To the former can be attributed a rather basic philosophy on the general role of metamorphism in geologic processes. This philosophy, discussed in preceding paragraphs, appeared in a publication that Hall had at hand and from which he quoted in the "Introduction" of 1859.[99] The metamorphic process as applied to geology had no place in catastrophist-oriented systems, and indeed, Lyell seems to have been the individual who first used the term "metamorphic" to describe a whole series of classes of rocks that had formerly been described as primary. However, the idea that "the strata called primitive were mere altered sedimentary rocks" probably originated with James Hutton late in the preceding century.[100] Hall's statement on

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[98] James Hall, MSS notes, Hall Papers, fol. 15. This folder contains a large amount of manuscript materials concerned with the 1857 address and its publication in 1883.

[99] Hall, Palaeontology . . ., III, 89-90. The work cited is Lyell's Manual of Elementary Geology. See note 90, supra, for a complete citation. Hall cited the American edition of this work, a reprint from the fifth London edition, but the pagination he gave corresponds to the fifth London edition, not the American reprint.

[100] Lyell, Principles of Geology . . ., II, 499, 506; James A. H. Murray, et al., (eds.), The Oxford English

metamorphic rocks seem to show a direct reflection of Lyell's ideas on the topic, thus once more showing that Lyell formed a significant source of geological orientation for his American associate.

For a more direct influence on Hall's orientation on metamorphic rocks, his association with Hunt must be considered. The latter, the "versatile and brilliant genius" of the Geological Survey of Canada, maintained for many years a close professional and personal relationship with Hall. Though described as not having a refined knowledge of geological science, he nevertheless performed outstanding services in the earlier phases of geochemistry,[101] and it is from him that Hall acquired some of his more specific ideas on the process of metamorphism. In the one short passage concerning metamorphic rocks that appeared in his 1857 address at Montreal, Hall gave this acknowledgment to Hunt:

> . . . Laying aside all assumption of causes not known to exist, and ignoring the supposed effects of that imaginary nucleus, he has proceeded from known and demonstrated facts derived from existing conditions, to explain most satisfactorily certain processes in metamorphism, and instead of appealing to an unknown source

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Dictionary Being a Corrected Re-Issue with an Introduction, Supplement, and Bibliography of a New English Dictionary on Historical Principles Founded Mainly on the Materials Collected by the Philological Society (Oxford: At the Clarendon Press, 1961), VI, 383; A. C. Trowbridge, (ed.), Glossary of Geology and Related Sciences: A Cooperative Project of the American Geological Institute (Washington: American Geological Institute, 1957), p. 183.

[101] Clarke, pp. 448-49.

> for the ingredients of certain metamorphic strata has demonstrated the existence of the same elements in the unaltered beds which are known to be of the same age and prolongations of the same strata.[102]

This statement accords well with Hall's rejection of the primordial or primitive nature of metamorphic strata and his assertion that metamorphic rocks are simply altered sedimentary rocks.

Hunt and Hall carried on a voluminous correspondence for many years, and in some of their letters can be detected some ideas that concern the general subject of metamorphism, volcanic action, and the accumulation of sediments. In 1858 Hunt remarked in a letter:

> I sent you the other day a note on volcanic igneous rocks, . . . and you will not fail to see how directly it is connected with your view of mountain chains. As we remarked last fall, metamorphic action coincides with mountain ranges--precisely because there has been the necessary accumulation of sediments, and from metamorphism to vulcanism is but a step. Hence they appear along mountain ranges & through recent formations, I believe in every case. It was a happy inspiration of Herschel which no one has hitherto really appreciated. Lyell says not a word of it in his books. . . .[103]

That same year Hunt published an article in the Canadian Journal in which he elaborated his reference to Herschel's "happy inspiration," referring once more to the oft-quoted letters from Herschel to Lyell and Murchison that Hall himself had admittedly used as a source.[104] As

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[102] Hall, MSS of the Montreal Address, p. 64.

[103] Letter from Hunt to Hall, Quebec, June 3, 1858, Hall Papers.

[104] Hall, Palaeontology . . ., III, 95-96.

Hunt explained the process, Herschel maintained that "with the accumulation of sediment the isothermal lines in the earth's crust must rise, so that strata buried deep enough will be crystallized and metamorphosed. . . ."[105] Herschel himself described the process in this way:

> 'Let strata be deposited.' Then, as a necessary consequence, and according to known, regular, and calculable laws, heat will gradually invade them from below and around; and, according to its due degree of intensity at any assigned time, will expand, ignite, or melt them, as the case may be. . . .
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> According to this view of the matter, there is nothing casual in the formation of Metamorphic Rocks. All strata, once buried deep enough, (and due TIME allowed!!!) must assume that state,--none can escape. All records of former worlds must ultimately perish.[106]

Herschel in turn acknowledged Lyell's contribution to the concept of metamorphic rocks in the same letter.[107]

However, at least in Hunt's mind, there seemed to be some doubt as to who actually formulated the idea of a connection between sedimentary accumulations and metamorphic action. The following year he wrote to Hall asking just who it was that established the relationship, saying that "I was not aware whether you or I had first insisted on the relation. . . ."[108] Later yet Hall queried Hunt on the authorship of the idea asking:

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[105] T. Sterry Hunt, "On the Theory of Igneous Rocks and Volcanoes," Canadian Journal, new series, III (1858), 207.

[106] Letter from Herschel to Murchison, 1836, p. 217.

[107] Ibid., p. 216.

[108] Letter from Hunt to Hall, Montreal, January 24, 1859, Hall Papers.

> Can you tell me where it was first published that the investigations of the Canadian Survey had proved that the metamorphic folded and plicated rocks of the Green Mountain Range had been traced northward into Canada where they became unaltered and nearly horizontal & proved to be of the same age as the Hudson River group--I am not at this moment . . . quite certain whether you promulgated this idea at one of our association meetings, or if it was published elsewhere first.[109]

Regardless of the authorship of this and associated ideas, their publication in Hall's "Introduction" of 1859 seems to have been the first time that the accumulation of sediments on a large scale and metamorphic processes were associated in a comprehensive geological concept. Here also, Hall appears at his best, making effective use of the ideas of other scientists in a new and effective combination.

Igneous and Volcanic Actions

When he undertook to explain large deposits of igneous and volcanic matter in certain regions of the area he had studied, Hall once again associated rapid accumulations of sediments with the phenomena he studied. In accounting for the presence of igneous matter, he speculated that whenever sediments accumulated over large areas at a slow rate, then depression of the area would be accomplished slowly. As a result, comparatively few extensive rents or fractures would have been produced. Those formed would fill with trappean matter, but rarely would any material overflow

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[109] Letter from Hall to Hunt, Albany, November 30, 1861, Hall Papers.

onto the surface. However, when very rapid accumulation occurred over relatively small areas, then "the crust below might give way, from the overload, and the whole be plunged into the semi-fluid mass beneath, causing it to overflow. Whether this reasoning be correct or otherwise," stated Hall, "I believe that the overflows of trappean matter are always coincident with the rapid accumulation of sedimentary materials."[110] He cited evidence of certain accumulations in Nova Scotia and in the Connecticut and Hudson river valleys that he believed supported his conclusion, though he did admit his evidence might be considered scanty. In any case, he stated, "I believe the law will hold true, that all great outbursts of igneous matter are accompanied by formations of rapid accumulation."[111]

Having considered the question of igneous and trappean intrusions, Hall then turned to a discussion of volcanic action itself. Volcanos proper and their products, he asserted, are invariably connected with tertiary or more modern geological formations. These phenomena are found in areas where sizeable deposits have accumulated rapidly, and furthermore, they "can never occur except as the result of such conditions. The igneous outflows," he generalized, "I regard as produced by and dependent upon other agencies, and

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[110] Hall, Palaeontology . . ., III, 79.

[111] Ibid., pp. 79-80.

are but the manifestations of rapid accumulations of sedimentary matter." Thus, the products of volcanic action are usually found only at the termination of a series, and where the observer finds an entire sequence of formations, he may also find the greatest manifestations of volcanic action for any geological period.[112] Thus Hall interposed another geological generalization into his concept of subsidence.

Lyell, in his Principles of Geology and his Manual of Elementary Geology, provided much observational information and some speculative assumptions for this part of Hall's subsidence concept, and Hunt likewise furnished some background for it. However, from the wording in the "Introduction" of 1859, it is readily apparent that Herschel provided a key idea for Hall. The latter's statement concerning the crust of the earth giving way under the immense weight of rapid accumulation of sediments, plunging the whole into a fluid mass below is worded much like that of Herschel in his 1836 letter to Lyell.[113] But Herschel was not a geologist and made little attempt to support his theses with geological evidence, while Hall's speculations, being supported by a significant amount of data collected and evaluated by a professional geologist, seem much more authoritative.

Hunt's contribution to Hall's association of igneous and volcanic processes with the subsidence concept may have

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[112] Ibid., p. 80.

[113] Letter from Herschel to Lyell, 1836, p. 209.

been considerable. As already noted, the two were closely associated during most of their professional lives. From 1847 to 1872 Hunt was employed by the Geological Survey of Canada,[114] an activity which employed Hall at one time to write one of its publications on palaeontology.[115] Hunt wrote numerous papers on the chemistry of the earth, and in them he described many experiments he had conducted in attempts to describe and verify the process of metamorphosis of rocks and the other changes that take place in the interior of the earth.[116] Hunt's influence on Hall in this connection can perhaps be expressed in Hall's own words when he related: "The investigations of Mr. Hunt, in this direction, are bringing out results of the highest interest, and as such will, I believe, when combined, achieve a complete revolution in this department of geological science."[117] From Hunt he also received encouragement to accept the views of Babbage and Herschel concerning metamorphism and

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[114] "Obituary, Thomas Sterry Hunt," American Journal of Science, series 3, XLIII (1892), 247.

[115] William E. Logan, et al., Geological Survey of Canada: Report of Progress from Its Commencement to 1863; Illustrated by 498 Wood Cuts in the Text, and Accompanied by an Atlas of Maps and Sections (Montreal: Dawson Brothers, 1863), p. ix; Clarke, pp. 303-05.

[116] See T. Sterry Hunt, Chemical and Geological Essays (Boston: James R. Osgood and Co., 1875), for a number of examples of his work. The papers contained in this book are reprints of some of his more significant papers on the subject.

[117] Hall, Palaeontology . . ., III, 77, n.

volcanism. Hunt's views are expressed in an abstract of a paper published in 1859 which reads in part:

> The author accepts the views of Babbage and Herschel as to the internal heat of the earth rising through the stratified deposits, on account of the superficial accumulation of sediments, metamorphosing the rocks submitted to its action, causing earthquakes and volcanic irruptions by the evolution of gases and vapours from chemical reactions, and giving rise to disturbances of equilibrium over wide areas of elevation and subsidence.[118]

When Hall's subsidence concept was published in 1859, Hall and Hunt seemed in rather close agreement on these particular ideas.

Hall did not formulate any strikingly new general philosophy of dynamic geology when he developed the subsidence concept. He made liberal use of the ideas of his predecessors and contemporaries, most times freely acknowledging his debt to those individuals. For his general geological orientation, Lyell's uniformitarian principles appear to have been very satisfactory. For the specifics of ocean currents, Lyell also provided the major ideas, while De La Beche and Mather contributed lesser but important ideas. On the subject of elevation and subsidence, Hall acknowledged Herschel and Babbage as his inspiration, while Darwin, Montlosier, Hopkins, and others all provided some of the lesser elements. For the relatively new concept of the metamorphosis of sedimentary strata, Lyell and Hunt were chiefly responsible, while Logan and others

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[118] T. Sterry Hunt, "On Some Points in Chemical Geology," Philosophical Magazine, series 4, XVII (1859), 149.

provided much data for its support. For ideas concerning igneous intrusions and volcanic processes, Hall likewise drew heavily from Lyell and Hunt.

CHAPTER III

THE RECEPTION OF HALL'S SUBSIDENCE CONCEPT

If measured by how well it was received by other geologists and palaeontologists, the new concept met with only limited success. A few individuals distinguished themselves by agreeing with Hall's concept almost entirely, others rejected it almost completely or paid no attention to it, while yet a third group accepted certain of Hall's assumptions and used them quite effectively in their own concepts and theories.

Initial Reaction to the New Concept

Hall's subsidence concept first became available to the public in 1857 at the annual convention of the American Association for the Advancement of Science held at Montreal, Canada. As the retiring president of the group, Hall addressed the delegates on the general subject of "Contributions to the Geological History of the American Continent," much of which concerned a delineation of his concept of subsidence. From the few scattered comments that are available, Hall was neither notably successful in presenting his ideas nor in convincing his listeners of their validity.

The Montreal meeting of the A. A. A. S. was not a smoothly conducted session. According to Josiah D. Whitney (1819-1896), one of the conferees, accomodations were extremely limited, poor, and expensive, and the geological part of the meeting "intensely Canadian; Logan, Hunt, Dawson, and Hall [putting] . . . their heads together to puff Canada, and snub everything and everybody else."[1] Another member also described a rather chaotic situation, relating that "a great entertainment given by the Nat. Hist. Society in the evening was so badly managed, that Hall's address was only half delivered and the Governor-General, Eyre, left town in a huff. . . ."[2] Whether or not this entertainment was the occasion of Hall's address on the contributions to geological history, it may give some indication of the general atmosphere of the meetings.

Hall's new concept elicited little reaction when first presented at Montreal. Few of the persons attending the Montreal meeting seem to have understood what Hall was talking about. Many years later, at the meeting of the American Association for the Year 1896, a commemorative session was held to mark the sixtieth anniversary of Hall's

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[1]Letter from J. D. Whitney to W. D. Whitney, Northampton, August 20, 1857, quoted in Edwin T. Brewster, Life and Letters of Josiah Dwight Whitney (Boston: Houghton Mifflin Co., 1909), p. 171.

[2]Letter from J. Peter Lesley to his wife, Montreal, August 14, 1857, quoted in Mary Lesley Ames (ed.), Life and Letters of Peter and Susan Lesley (New York: G. P. Putnam's Sons, 1909), I, 350.

association with the study of geology and palaeontology for the state of New York. At that session Joseph LeConte described the reception of the subsidence concept in these words:

> In 1857 the A. A. A. S. met at Montreal, and Hall as retiring President gave his memorable address on the formation of mountains by sedimentation. I can never forget the impression produced. The idea was so entirely new, so utterly opposed to prevailing views, that it was wholly incomprehensible even to the foremost geologists. There was no place in the geological mind where it could find lodgment. It was curious to observe the look of perplexity and bewilderment on the faces of the audience. Guyot was sitting immediately behind me. He leaned forward and whispered in my ear: 'Do you understand anything he is saying?' I whispered back, 'not a word.'[3]

Even when one considers that this statement was presented under circumstances that might have tended to color what LeConte had to say and that nearly four decades had passed in which his memory might have altered the original circumstances, the impression still remains that few of Hall's listeners in 1857 gave his new concept much serious consideration.

Perhaps stronger evidence that not many geologists were willing to accept the concept of subsidence may be adduced by a rather decided lack of mention of it for some time after the Montreal meeting, although part of this may be attributed to Hall's refusal for many years to permit publication of his address. It was not until after the

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[3]Joseph LeConte, et al., "Honors to James Hall at Buffalo," Science, new series, IV (1896), 699.

same ideas were presented in expanded form in 1859 in the "Introduction" to the Palaeontology of New York that much attention was given to it.[4] Hall's hesitation to publicize his concept may be attributed in part to a bit of friendly advice offered by Joseph Henry (1799-1878), secretary of the Smithsonian Institution. Henry noted that had not the remarks made in the Montreal address come from Hall, he would have supposed that there was nothing to them. ". . . They may be considered at variance with what have long been regarded as established principles," wrote Henry. "If after having brought your views to the test of the widest collection of facts you still are assured they are correct, then give them to the world, but I beg that you will be cautious and not commit yourself prematurely."[5] In his reply Hall indicated he had indeed given his views the most careful consideration, that they were "the most simple and natural conclusions from the observed facts, and so simple that I am surprised that the same idea should not have occurred to every observer." The remainder of Hall's letter gave a brief resume of the main points of the subsidence concept together with the essential items of

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[4]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859. Vol. III, Part I, Palaeontology of New York (Albany: State of New York, 1859), 1-96.

[5]Letter from Henry to Hall, quoted in John M. Clarke, James Hall of Albany, Geologist and Palaeontologist, 1811-1898 (Albany: n. p., 1923), pp. 327-28.

physical evidence that substantiated his conclusions.[6] Nevertheless, in spite of Hall's own confidence in the validity of his views, the address remained unpublished for many years.

Whether or not Hall ever convinced his cautious friend of the usefulness of his concept is questionable. In 1858 Hall visited Henry in Washington, a meeting noted briefly by Henry in his diary along with a very brief statement of what he thought were the most important elements of the concept.[7] Nowhere did he make mention of the concept in his published writings. His cautious attitude toward Hall's concept may have stemmed in part from his commitment to the theory that the earth had once been a hot, molten body that is now solidified because of a secular loss of heat. In describing this thesis, which is an integral part of the contractional theory of mountain formation, Henry postulated that "the earth was once at least in a liquid condition by heat, can scarcely be doubted, when all the cumulative evidence in favor of the hypothesis is considered." If applied to the facts of geology, asserted Henry, this hypothesis could provide a complete explanation of the whole of geological phenomena. This statement of ideas,

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[6]Letter from Hall to Henry, Albany, December 26, 1857, quoted in Clarke, pp. 328-31.

[7]Joseph Henry, Diary entry for February 25, 1858, Unpublished Diary, Smithsonian Institution, Washington, D. C., Joseph Henry Papers.

indicating a geological orientation that precluded any easy acceptance of Hall's thesis, appeared in print in 1857, the year of Hall's Montreal address.[8]

From the beginning Hall had a staunch advocate in T. Sterry Hunt who came closest of any major writer on geology to accepting the subsidence concept completely. During the decade following Hall's Montreal address, Hunt published a number of articles that clearly stated his agreement with Hall's views, and in several letters to Hall, he also expressed an extremely favorable attitude. Even before Hall's "Introduction" of 1859 appeared, Hunt asked permission to include the New Yorker's theory of mountains in a paper that he hoped to expand eventually to a full volume to be entitled "The Principia of Chemical Geology."[9] Hall apparently did not respond to the request, however, and the following year Hunt noted he had been forced to omit any reference to Hall's concept. Furthermore, he had been unable to find Hall's Montreal address published anywhere and thus did not have the necessary particulars available.[10]

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[8]Joseph Henry, Scientific Writings of Joseph Henry (Washington: Smithsonian Institution, 1886), II, 153-54. The material cited appeared originally as "Meteorology in Its Connection with Agriculture, Part III.--Terrestrial Physics and Temperature," Agricultural Report of Commissioner of Patents, (1857), 419-506; Ibid., p. 85.

[9]Letter from Hunt to Hall, Quebec, July 3, 1858, New York State Library, Albany, New York, Hall Papers. Hereinafter, the library location of the Hall Papers will be omitted.

[10]Letter from Hunt to Hall, Montreal, January 24, 1859, Hall Papers.

In reviewing a book that examined some facets of the geology of the Alps, Hunt gave further indication that he had been influenced by Hall's views. He recalled that the vast thickness of the sedimentary deposits in the area now covered by the Alps "serves to exemplify the relation which Prof. James Hall has so well pointed out, of the apparent connection of mountain elevations with original deposition." Furthermore, Hunt noted that the so-called crystalline nucleus of the Alps was not a large, extruded mass of primitive rock as had long been believed but was instead made up of altered sedimentary formations, some being more recent in origin than many of the fossiliferous deposits on the flanks of those mountains.[11] Both of these ideas indicate substantial agreement with Hall's concept. At about the same time, Hunt referred again to the general nature of Hall's conclusions on mountains in a paper read to the Geological Society of London in 1859. Again he appeared not to have had Hall's "Introduction" available, once more referring to Hall's address at Montreal and his report of the geology of Iowa as his sources of information

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[11] T. Sterry Hunt, "Review.--On Some Points in the Geology of the Alps," American Journal of Science, series 2, XXIX (1860), 118. Though this review appeared in 1860, Hunt probably wrote it before he had received a copy of Hall's "Introduction" of 1859, as he referred to Hall's report of the geology of Iowa rather than to the more comprehensive publication of 1859. The Iowa report appeared as James Hall and J. D. Whitney, Report on the Geological Survey of the State of Iowa: Embracing the Results of Investigations Made during Portions of the Years 1855, 56 & 57 (Des Moines : State of Iowa, 1858).

on Hall's concept.[12]

Sometime prior to May, 1861, Hunt gained access to Hall's more comprehensive exposition of his concept in the "Introduction" of 1859, for in that month he presented a very favorable review of the concept in the American Journal of Science. He set forth its various elements and recalled many of the individuals to whom Hall was indebted as contributors to the concept.[13] Hunt urged that "we conceive that the views which he is here urging are of the highest importance to a correct understanding of the theory of mountains."[14]

Though Hunt appeared to be a most avid proponent of Hall's subsidence concept, he attempted to apply its principles in a way that Hall likely would never have considered, making an effort to bring together the concept of subsidence and the rather contradictory contractional hypothesis.[15] Great accumulations of sediments along a particular

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[12]This paper, "Notes on Some Points in Chemical Geology," was published first in The Quarterly Journal of the Geological Society of London, XV (1859), 488-96. It was reprinted with some additional notes by the author in the Canadian Naturalist, IV (1859), 414-25. The notes along with some of the text of the paper appeared in "On Some Points in Chemical Geology; by T. Sterry Hunt," American Journal of Science, series 2, XXX (1860), 133-37.

[13]T. Sterry Hunt, "On Some Points in American Geology," American Journal of Science, series 2, XXXI (1861), 408-14.

[14]Ibid., p. 408.

[15]As noted in Chapter I, Hall described the contractional hypothesis as "not always philosophical," and as

axis, Hunt said, would tend to destroy the equilibrium of pressure in that area, causing the crust to subside. The lower strata of this zone would become altered by the heat from the nucleus of the earth causing crystallization, contraction, and plications parallel to the line of deposition. "These foldings," postulated Hunt,

> not less than that softening of the bottom strata, establish lines of weakness or of least resistance in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself in those regions and along those lines where the ocean's bed is subsiding beneath the accumulating sediments. Hence we conceive that the subsidence invoked by Mr. Hall, although not the sole nor even the principle cause of the corrugations of the strata, is the one which determines their position and direction, by making the effects produced by the contraction, not only of sediments, but of the earth's nucleus itself, to be exerted along the lines of greatest accumulation.[16]

In this application of the subsidence concept, Hunt indicated that he considered something was missing in Hall's ideas, and this deficiency concerned the elevation process itself. Though agreeing with Hall in virtually all of the individual components of the concept, he believed that Hall's vague reference to continental elevation as the basic cause of mountain formation was deficient, and thus he felt compelled to postulate contractional folding to bridge this gap. By doing so, Hunt rather contradicted some of his

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always unsatisfactory as a solution for the problem of explaining mountains. Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 69.

[16] Hunt, American Journal of Science, series 2, XXXI, 412-13.

earlier statements concerning the same general subject. He had previously indicated that to view mountains as arising from incidents of local uplift, such as contractional folding, was in error. Mountain formation should be viewed as a process of continental elevation, followed by a long process of denudation. The existing mountains are remnants of wide areas of elevated strata.[17] In expressing this thought, Hunt had repeatedly recalled ideas of Francois Dominique Reynaud de Montlosier and J. Peter Lesley, each of whom had expressed this same general view.[18]

Hunt continued to be an enthusiastic supporter of the subsidence concept. In 1867 he attended a meeting of the Société Géologique de Paris at which he read several papers, and he reported that

> in my second paper [I] gave some notions on mountain chains & rather astounded them by bringing in your views and those of de Montlosier & Constant Prevost. A return to the good old ways I told them. Your demonstration of the relations of thickening to mountain chains struck them forcibly. . . . I think the newness of the principles will show an advance & bear the mark of American Science.[19]

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[17] Ibid., p. 409.

[18] Francois Dominique Reynaud de Montlosier, "Sur la formation des vallées, et sur la théorie des soulèvements de montagnes," Bulletin de la Société géologique de France, III (1832-33), 215-17; J. Peter Lesley, Manual of Coal and Its Topography, Illustrated by Original Drawings, Chiefly of the Facts in the Geology of the Appalachian Region of the United States of North America (Philadelphia: J. B. Lippincott and Co., 1856), p. 127.

[19] Letter from Hunt to Hall, Paris, June 21, 1867, Hall Papers.

Meanwhile, the subsidence concept had become well enough known that criticism began to appear. Though Hall tended to avoid developing hypotheses that might explain the actual process of elevation of mountains, one statement of just such an idea in the "Introduction" of 1859 led to rather significant criticism. In the pertinent passage Hall asserted that

> the line of greatest depression would be along the line of greatest accumulation; and in the direction of the thinning margins of the deposit, the depression would be less. By this process of subsidence, as the lower side becomes gradually curved, there must follow, as a consequence, rents and fractures upon that side; or the diminished width of the surface above, caused by this curving below, will produce wrinkles and foldings of the strata.[20]

Elkanah Billings (1820-1876), a palaeontologist employed by the Canadian Geological Survey, undertook to refute this particular phase of Hall's concept in a paper published in 1860, not many months after the concept appeared in print for the first time. Billings concerned himself especially with the idea that the subsidence of the mass of materials would produce the wrinkling and folding described by Hall. In a footnote to the passage quoted above, Hall had used an analogy where he compared the subsiding sediments and the surface to a packet of paper which, when held tightly together at the edges and then bent in a curve, will describe a smoothly rounded arc on one surface, but on the other a series of wrinkles or folds will

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[20] Hall, <u>Palaeontology . . .</u>, III, 70.

appear.[21] Billings analyzed Hall's assertions about subsidence-caused wrinklings and foldings mathematically, computing that for a cross-sectional area the size of the Appalachian chain, the amount of lateral compression produced by a subsidence of 40,000 feet would be a mere six feet or so and that the amount of elevation from this could be no more than about three feet. These surface alterations, said Billings, can in no way be compared with the actual elevation of at least a mile and lateral compression of about four and a half miles. "Viewed in this way," he said, "the theory of plication from subsidence appears to fail altogether."[22] Furthermore, he raised another objection to Hall's concept, one that concerns the rates of deposition and subsidence. The whole amount of subsidence of some 40,000 feet must be distributed throughout the enormous period of time that elapsed during the Silurian, Devonian, and Carboniferous epochs, a time span Billings felt unable to compute. But if one uses the figure of four million years, he said, then the average subsidence rate would be approximately one foot in a hundred years. Recognizing the denuding power of running water and other erosive factors, he speculated that the ten feet of materials deposited in the first thousand years would likely disappear in the first

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[21] Ibid., p. 70, n.

[22] Elkanah Billings, "On Certain Theories of the Formation of Mountains," Canadian Naturalist, V (1860), 418-19.

ten years after it was deposited.[23]

Billings's first argument indicated that he did not fully understand the subsidence concept. In the first place, Hall denied that subsidence causes the uplifting of mountains, and he postulated no cause for their elevation other than asserting the process was a part of broad continental elevations.[24] In the second place, Hall at no time asserted that folding and plication cause the formation of mountains which are in no case the result of such local movements. Denudation of anticlinals does indeed create a system of ridges and valleys, but the actual elevation is continental in its extent.[25] Nevertheless, the wording of Hall's statement which Billings attacked left Hall open to just such criticism as Billings advanced. The latter's second criticism contained a logical inconsistency in that he postulated that abrasion of materials would proceed at the same time and place that deposition was taking place.

James D. Dana, though professing a long-time personal friendship with Hall, seldom felt compelled to agree with him when it came to geological theory. Dana was thoroughly committed to the contractional theory as a cause of major

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[23]Ibid., pp. 419-20.

[24]Hall, Palaeontology . . ., III, 72.

[25]Ibid.

alterations in the crust of the earth,[26] and he disagreed with many of the elements of Hall's concept. In the first edition of his Manual of Geology, Dana questioned the hypothesis that the weight of accumulating sediments causes the surface to subside. Noting that the great subsidences of the Appalachian region had been attributed to this cause, he argued that the surface had, during the formative period of those mountains, undergone uplift as well as subsidence and it was scarcely possible to attribute both of these effects to the same cause. One cause may ultimately produce oscillations in the surface over the entire region, but gravitation could be only one cause of subsidence.[27] Indeed, Dana's entire metaphysical orientation toward geology differed from Hall's. Uniformitarian principles played no part in Dana's scheme which displayed a rather large measure of catastrophism. The cause of mountains, he said,

> must have been one which would have produced an increasing amount of tension through the passing periods, causing oscillations of the crust and minor uplifts in the course of those long periods, and then a great catastrophe, or an epoch of plications, metamorphism, and grander uplifts, as a result of the great increase; then another slow increase and another catastrophe; then others; and a series of similar but more or less independent catastrophes in distant parts of the globe, raising, as late as the Tertiary period, many of the earth's great mountain-chains,--but one which should

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[26]See Chapter I, note 55, supra.

[27]James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History, for the Use of Colleges, Academies, and Schools of Science (Philadelphia: Theodore Bliss & Co., 1863), p. 717.

> cause only minor oscillations and uplifts in more recent times, since the earth has now a degree of stability unusual in the past ages. And no cause answers to these demands, so far as known, but the one mentioned,--the contraction of a cooling globe.[28]

Dana also disputed Hall's hypothesis that metamorphism is not necessarily associated with deformations of the crust. Hall contended that rocks could be altered by the metamorphic process even though they lay in horizontal beds, and he cited the case of the Catskill mountains in New York where extensive horizontal beds near the base of the mountains exhibit the character of metamorphic rocks to a striking degree, while rocks of the same age to the west of the Green mountains show none of these characteristics even though severely folded and plicated.[29] Dana thought otherwise, saying that

> metamorphic rocks are always displaced and folded rocks, and never for any considerable distance horizontal. Where the foldings are most numerous and abrupt, reducing the strata to a system of parallel dips by the pressing of fold upon fold, there . . . the metamorphism is most complete.

He admitted there might be local cases of metamorphism without upturnings, caused by hot mineral waters and igneous dikes, but these did not suffice as examples of the "great physical conditions under which the metamorphism of the thick formations has taken place."[30]

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[28]*Ibid.*, p. 725.

[29]Hall, *Palaeontology . . .*, III, 78, n.

[30]Dana, p. 710.

On the subject of ocean currents and their effect on geologic processes, Dana's views also differed substantially from Hall's. Whereas Hall believed that ocean currents act as the primary agent of transportation for abraded materials and determine the axis of future mountain chains, Dana thought most oceanic currents too feeble to transport any great amount of material. Furthermore, they are generally too remote from the coastlines to receive any of the detritus from the continents and consequently would have little effect.[31] Thus, he concluded,

> it follows . . . that no continent can contribute to the detrital accumulations of another continent except through the aid of icebergs. Had there formerly existed a continent in the midst of the present North Atlantic, America would have received from it little or no rock-material.[32]

On the subject of volcanos and igneous intrusions, certain other basic differences can be detected between the ideas of Dana and Hall. The latter, in generalizing about volcanic and igneous processes, said that "all great outbursts of igneous matter are accompanied by formations of rapid accumulation." Furthermore, volcanos proper are always associated with later geological formations and can never occur except as a consequence of great and rapid accumulations of other deposits, the igneous materials themselves being derived from a semi-fluid mass in the interior of the earth.[33] On the other hand, Dana in his early years

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[31] Ibid., p. 655. [32] Ibid., p. 659.

[33] Hall, Palaeontology . . ., III, 79-80.

held to the hypothesis that the interior of the earth is in a completely fluid state. Volcanos, he reasoned, can readily be regarded as surface outlets for the interior fluid, especially where a pattern of volcanos appears over a wide area. A single volcano or a small cluster, such as found at Etna and in Hawaii respectively, might indicate that a comparatively small "lake of fire" is needed to provide the volcanic materials.[34] Though he recognized that volcanos do exist in patterns, in his writings there is little of the association of volcanos with other geological phenomena found in Hall's hypothesis.

Dana's part in the future of Hall's subsidence concept was significant even though he himself did not subscribe to many of its elements. As the editor of the American Journal of Science, he was for many years in a position to pass judgment on many of the scientific papers published in the United States. He wrote several books and large numbers of journal articles on the subject of geology. His Manual of Geology, designed as a textbook for colleges, academies, and scientific schools went through four editions in as many decades. Likewise, his System of Mineralogy, the Manual of Mineralogy, and the Textbook of Geology each went through at least four editions during the same period.[35]

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[34] Dana, pp. 700-01.

[35] Daniel C. Gilman, The Life of James Dwight Dana, Scientific Explorer, Mineralogist, Geologist, Zoologist, Professor in Yale University (New York: Harper & Brothers, 1899), pp. 385-94.

His total impact upon the study of geology is indicated by Henry S. Williams (1847-1918), who later filled the chair in geology at Yale University that Dana had occupied, when he wrote: ". . . we believe Dana's Manual has come nearer to the setting forth . . . an ideal system of geology than has been elsewhere attained."[36]

In 1866 George L. Vose (1831-1910), a civil engineer at the Massachusetts Institute of Technology, published his Orographic Geology, a small volume in which he discussed a number of hypotheses dealing with the origin of the earth's surface features. Among those examined appears Hall's subsidence concept which Vose thought answered the problems of dynamic geology in the best way possible at the time. He concluded that large-scale deposition and subsidence could produce the folding and compression exhibited in the Appalachian mountains. Furthermore, commented Vose, it seems more philosophical to attribute metamorphism to the same causes rather

> than to a supposed central mass of fluid, gaseous emanations, and the like, that we know nothing about, which seem opposed by important facts, and which, from all we know, should act generally and not locally; and especially not in these regions of great accumulation which from their very thickness would seem to be most removed from any source of heat beneath the earth's crust.[37]

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[36]Henry S. Williams, "James Dwight Dana and His Work as a Geologist," Journal of Geology, III (1895), 605.

[37]George L. Vose, Orographic Geology; or, the Origin and Structure of Mountains, a Review (Boston: Lee and Shepard, 1866), pp. 133-34.

At about the same time that Vose's commentary was published, Dana made his first pointed criticism of Hall's subsidence concept in a paper published in the American Journal of Science. He first criticised Hall's description of the subsidence process itself, and though not disputing that substantial subsidence had taken place, he did not agree with some of Hall's assumptions. Subsidence such as Hall had described would necessarily take place as a "foot-per-foot movement," and for each foot of accumulation there would necessarily be a foot of subsidence. This process, said Dana, would require a very thin, yielding crust with a perfectly mobile liquid underneath as there could be no impediment to this weight-induced subsidence. But, he continued, a rocky crust 800 miles thick, such as the mathematicians were then postulating, or one even ten or five miles thick would not allow for the no-resistance type of movement hypothesized by Hall and Vose. Thus, "the idea is obviously opposed to the very nature of the earth and its forces."[38]

Granted, continued Dana, that this gravitational subsidence did take place. What then might be the effect? He concluded that a lateral movement of some sort must take place in the liquid interior mass of the globe and that this would tend to produce effects where the structure was

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[38]James D. Dana, "Observations on the Origin of Some of the Earth's Features," American Journal of Science, series 2, XLII (1866), 208.

the thinnest and weakest; that is, in areas toward the edges of the subsiding mass. Very little of the action would be concentrated on the thickened portions--that area where Hall asserted the maximum foldings and plications would occur.[39]

In his evaluation of Hall's concept, another difficulty presented itself to Dana. If the crust of the earth is as sensitive to added weight as alleged and yields easily to the sedimentary deposits, would it not also subside under the weight of mountains which obviously contain an enormously greater weight of materials in them? To Dana it seemed contradictory that mountains such as the Alps and the Rockies should stand as they do while basins into which their sediments are washed should so readily subside.[40]

Dana also delivered some critical remarks about Hall's hypothesis on metamorphism and the latter's denial that heat from the so-called liquid interior nucleus is a requirement for this process. Since, in Hall's view, subsidence would not be opposed by liquid material underneath, then no heat would be generated by compression. How then, wondered Dana, could the process of metamorphosis occur at all?[41]

The most striking comment Dana had to make about Hall's entire concept is one that became quite well known and in some circles quite popular. "Mr. Hall's hypothesis

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[39] Ibid., pp. 208-09. [40] Ibid., p. 209.

[41] Ibid., p. 210.

has its cause for subsidence," wrote Dana, "but none for the lifting of the thickened sunken crust into mountains. It is a theory for the origin of mountains, with the origin of mountains left out."[42]

At about this same time, Nathaniel S. Shaler (1841-1906), long-time professor of geology at Harvard, brought forth some arguments on the formation of mountain chains that bear enough resemblance to Hall's views to warrant their discussion. In a brief summary of some ideas he had been presenting in a series of lectures at Harvard, he recalled the conclusions of Babbage and Herschel concerning the effect of movement of isogeothermal planes within the earth's crust as a result of changes in surface configurations. Shaler adopted their general views, concluding that changes in the level of the isotherms would give to overlying regions of the crust "a tendency to bend upward, or in the reverse direction," depending upon whether denudation or deposition was taking place.[43]

Later the same year, Shaler elaborated his views on the formation of mountain chains. Though some of his ideas do resemble Hall's, he, like Hunt and Dana, assumed the earth steadily contracts from a secular loss of its interior heat.

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[42] Ibid. The italics are those of the present author.

[43] Nathaniel S. Shaler, "Preliminary Notice of Some Opinions Concerning the Mode of Elevation of Continental Masses," Proceedings of the Boston Society of Natural History, X (1864-66), 238-39.

However, he denied the hypothesis then held by Dana that the interior of the earth is liquid, contending that if such is the case, the solid crust, subjected as it is to innumerable shocks and ruptures, would soon break up into pieces and sink into the fluid below. For Shaler a much more satisfactory hypothesis envisions a nucleus which, though at a high temperature, is solidified by the extreme pressure of the weight resting upon it. The surface and the crust are solid, of course, and in between the crust and the nucleus there remains a fluid or semi-fluid layer of relatively small thickness that could explain the source of materials for ordinary igneous activity.[44]

One of Shalers's assumptions that bears a strong resemblance to one of Hall's is the idea that the geologist should not look to the interior of the earth for the causes of the continuous alterations in the surface. "It is at once manifest," he said,

> that we must seek their origin in the changes going on within the crust itself, and in no way connected with the regions below. And within that crust we can find forces operating to produce contraction quite sufficient to account for all the facts.[45]

The causes and forces described by Shaler do not themselves bear much resemblance to those postulated by Hall, but the general orientation is similar.

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[44]Nathaniel S. Shaler, "On the Formation of Mountain Chains," Ibid., XI (1866), 8-10.

[45]Ibid., p. 12.

Shaler, like Hall, developed the view that all regions where deposition is taking place tend to subside, and each subsiding area is the general locale of maximum fracturing and dislocation of the crust. Furthermore, the distribution of now-active volcanic vents, along with the instructive fact that volcanic outlets of earlier periods became inactive when left inland, "indicate a peculiar liability to rupture of the superficial portions of the crust along shore lines."[46]

Though Shlaer did not associate large-scale deposition with the growth of mountain chains as clearly as Hall, he did acknowledge that this was an essential part of the phenomenon of subsidence. However, he, like so many of his contemporaries, remained committed to the contractional theory to explain actual elevation of the terrain, and here he found no common ground with Hall's concept.

During the dozen or so years following Hall's presentation of his concept to the public, it had received some favorable notices, some geologists had accepted all or at least part of his views, others made no mention of it at all, and others yet criticized all or part of the component elements of the concept. Reacting most favorably were Vose and Hunt, and Dana is perhaps the most striking example of those who initially accepted very little of the new concept. Furthermore, no one, Hunt included, seemed willing to discuss

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[46] Ibid., p. 14.

the idea without relating it to the popular contractional theory. Hall's concept did not have the breadth of structure nor the completeness of detail that would have permitted any geologist to regard it as capable of standing by itself as a complete explanation of the phenomena of mountains. Another factor which may have mitigated against popular acceptance of the concept may have been Hall's reluctance to publicize it to any substantial extent or to elaborate the theory any further than he had done in his "Introduction" of 1859. From that time forward, most of his published works dealt almost exclusively with the subject of palaeontology. The task of publicizing, elaborating, and incorporating the concept into the broader areas of geological theory remained for other geologists, and in this process it was inevitably changed and altered into what was to become the geosynclinal theory of later decades.

Later Reactions to the Subsidence Concept

In the early 1870's, Hall's subsidence concept attracted more attention and came to be considered more seriously. By the turn of the century it had become an integral part of orogenic geology, though by that time it had been altered and incorporated with other hypotheses that assured its future as a significant concept. It first became included in the contractional theory which was in turn based on the idea of a secular loss of heat by the earth. Though contraction of the earth due to a secular loss of heat now has little acceptance,[47] it seemed credible and was widely received during the latter decades of the nineteenth century and the early part of the twentieth.

Before the process of incorporation started, however, the subsidence concept became subjected to another round of criticism. Whitney, with whom Hall had been associated in various geological projects in the 1850's and 60's,[48] undertook a rather severe examination of Hall's views in 1871. In the North American Review, Whitney wrote on the general topic of volcanic processes and mountain building, examining concepts developed by a number of geologists. Hall's concept, said Whitney, had at first been applied only to the Appalachian chain but had ultimately been extended and stretched to fit all mountain ranges. He noted that Hunt had supported the theory, and he also mentioned the favorable consideration given to it in Vose's Orographic Geology, although not in overly-generous terms. "This last-named gentleman," wrote Whitney,

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[47]Charles H. Hapgood, Earth's Shifting Crust; a Key to Some Basic Problems of Earth Science (New York: Pantheon Books, Inc., 1958), pp. 10-11.

[48]These projects are described in James Hall and J. D. Whitney, Report of the Geological Survey of the State of Iowa . . .; J. W. Foster and J. D. Whitney, U. S. Senate, Report on the Geology of the Lake Superior Land District: Part II, the Iron Region, together with the General Geology, Executive No. 4, 32d Congress, Special Session, March, 1851; and James Hall and J. D. Whitney, Report on the Geological Survey of the State of Wisconsin, Volume I ([Albany]: Printed by the Authority of the Legislature of Wisconsin, 1862).

> who prints 'civil engineer' after his name on the title-page of his work, as if he feared that, by some possibility, he should be taken for a geologist, has adopted Mr. Hall's theories in toto, which he could more easily do, since he was not hampered by any of those difficulties which have their origin in a personal acquaintance with the subject.[49]

After this critical opening passage, Whitney recalled that Hall's views were similar to those of Charles Babbage and John F. W. Herschel, especially in the manner in which the heat of the interior of the earth is thought to affect regions where deposition and denudation take place. Whitney admitted that most geologists then accepted the idea that deposition of sedimentary materials could not take place over a long period of time unless subsidence did occur. He then repeated Billings's criticism of the Hall view that folding and plication would occur because of accumulation and subsidence. "But how the mountain chain is obtained from the depressed mass of strata is nowhere explained by the author of the theory in question," noted Whitney; "hence it has been aptly characterized by Professor Dana as 'a theory for the origin of mountains, with the origin of mountains left out.'"[50] In speaking of the subsidence described by Hall, Whitney advanced the same type of mathematical evaluation of the folding and plication as Billings had. Whitney concluded that this mathematical analysis

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[49] J. D. Whitney, "Volcanism and Mountain-Building," North American Review, CXIII (1871), 265.

[50] Ibid., pp. 266-67.

should prove quite conclusively that no such amount of disturbance could follow the subsidence. He summed up this general criticism with the statement that "the theory, as set forth by its author, is left in such a vague form that it seems impossible to bring it to any crucial test, and one has to be content with finding in it nothing which will bear examination."[51]

The views of Babbage and Herschel had a certain attractiveness about them, according to Whitney, but they could not be accepted without careful investigation, for after all, neither of the two men were geologists. Hall likewise had been negligent for failing to accumulate near the amount of geological evidence needed to bear out his assumptions as far as he had carried them.[52]

Whitney next referred to an idea that had been advanced some years earlier by Dana, who asked how one portion of the crust can be so sensitive that a relatively small amount of sedimentary material can cause one section of the crust to subside while another section sustains huge mountain ranges such as the Andes and the Himalayas.[53] How, repeated Whitney, can the interior of the earth be so insensitive as to support these towering ranges and yet respond readily to ordinary sedimentation?[54] He offered no

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[51]Ibid., p. 267. [52]Ibid., p. 268.

[53]Dana, American Journal of Science, series 2, XLII, 209

[54]Whitney, North American Review, CXIII, 268.

alternate solution to the questions he posed, seeming interested only in casting doubt on this particular segment of the subsidence concept.

The assumptions of Herschel, Babbage, Hall, and Hunt about the conditions necessary for metamorphism also received some attention in Whitney's analysis. He questioned the inference in their views that extensive accumulations and consequent movement of isothermal planes would cause metamorphosis of the rocks subjected to the higher temperatures. This, said Whitney, is not necessarily a valid assumption, for he recalled that examination of sections of stratified rocks several thousands of feet thick often shows no metamorphism at all. Heavy accumulations, then, are not necessary to activate the complex series of chemical changes constituting metamorphic action, but what the activating agent might be he did not say.[55]

According to Whitney, too little attention had been given to sources of sedimentary materials for such mountain ranges as the Appalachians. Without some nearby elevation, there can be no formation of sedimentary deposits, he said, and the deposits found in areas where there has been no major source of detritus are beds precipitated by either chemical or organic action. These deposits are vastly inferior in thickness to those accumulated from detrital action. In attempting to explain the original source of

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[55] Ibid.

those materials, Whitney referred to the contemporary idea that the earth had once been a hot, molten mass that had eventually cooled to its present temperature. "The fact is ignored," he related,

> that all the sedimentary formations must have been originally derived from the original crust of the earth as it existed after cooling had gone so far that water had begun to condense upon its surface; they must have had some higher region from which to be swept downwards. These higher regions were, in the first place, evidently the ridges or wrinkles of granitic and gneissoid crust raised above the general level by the first efforts of the consolidated crust to adapt itself to the interior. The detritus thus carried down the flanks of the ridges was, early in the geological history of the earth, mostly deposited in the ocean, which must originally have covered even a larger portion of our surface than it now does.[56]

In the foregoing passage, Whitney revealed a general geological orientation that had no substantial agreement with Hall's which was solidly based on the uniformitarian views of Lyell. Whitney's own attitude is strikingly revealed by his statement that uniformitarians "are trying to pull out the corner-stone from under the fabric of the science."[57] His criticism thus becomes less significant, because without some agreement with Hall's basic philosophy of geology, the detailed criticism of Hall's concept could not be done effectively. What he did, in other words, was to criticize the details of the concept against his own background of geological theory in which those details had no essential place. Whitney was not alone in this sort of

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[56] *Ibid.*, p. 269. [57] *Ibid.*, p. 249.

analysis, as many of the geologists who undertook to review Hall's concept were speaking from the same sort of a philosophical background in which Whitney was found.

The following year LeConte published his theory of how the earth's features are formed, partially rejecting Hall's concept of subsidence. LeConte devoted nearly half of his essay to theories of the condition of the earth's interior, admitting that some years before, he, like Dana, had been convinced that the interior is a hot, molten substance on which the solidified crust floats. After serious consideration, however, he had eventually concluded that such a theory was untenable; that the laws of physics simply would not permit a dense, solid material to float on a lighter, less dense liquid.[58] After rejecting that hypothesis, he discussed the postulate that whatever the interior condition of the earth may be, the globe behaves as a solid body, even under the powerful disturbing forces exerted by the sun and the moon. He recalled the reasoning of William Hopkins who had concluded from the evidence of precession and nutation of the earth's rotation that the solid shell of the earth cannot be less than a thousand miles thick.[59]

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[58]Joseph LeConte, "A Theory of the Formation of the Great Features of the Earth's Surface," American Journal of Science, series 3, IV (1872), 347-49.

[59]Ibid., p. 350. Hopkins was among the group of physicists and geologists who attempted rather unsuccessfully during the nineteenth century to apply mathematical and physical laws to geological phenomena. This particular computation is found in William Hopkins, "Researches in

William Thomson (1824-1907) and Peter G. Tait (1831-1901) provided evidence for LeConte that the earth is probably a completely solid figure. In a discussion of the rigidity of the earth, they concluded that

> the negative result of attempts to trace their influence, [i. e., the attraction of the moon and the sun], on ocean and lake tides, as hitherto observed, and on precession and nutation, suffices . . . to disprove the hypothesis, hitherto so prevalent, that we live on a mere thin shell of solid substance, enclosing a fluid mass of melted rocks or metals, and proves, on the contrary, that the Earth as a whole is much more rigid than any of the rocks that constitute its upper crust.[60]

From these and other objections to the concept of the liquid interior, LeConte became convinced that "the whole theory of igneous agencies--which is little less than the whole foundation of theoretic geology--must be reconstructed on the basis of a solid earth." In this idea he included the assumption that all igneous phenomena have their origin within the solid crust of the earth and have no connection with the remote liquid interior if indeed such a condition exists.[61]

Physical Geology.--Third Series," Philosophical Transactions of the Royal Society of London, CXXXII (1842), 50.

[60]LeConte, American Journal of Science, series 3, IV, 350-51. The quotation is from William Thomson and Peter G. Tait, Treatise on Natural Philosophy (Oxford: At the Clarendon Press, 1867), pp. 689-90, which was cited by LeConte. That this idea is most likely Thomson's rather than Tait's is supported by Thomson's earlier expression of it in Thomson, "On the Rigidity of the Earth," Philosophical Transactions of the Royal Society of London, CLIII (1863), 573-82. Thomson is perhaps better known as Lord Kelvin.

[61]LeConte, American Journal of Science, series 3, IV, 352.

LeConte then briefly examined Hall's subsidence concept, giving high praise to Hall for

> first strongly drawing attention to the fact that mountain chains consist essentially of immense masses of sediments, much thicker, indeed, than the height of the mountains themselves. His views on this subject form, I believe, an era in the history of geological science.[62]

But like Dana and Whitney, LeConte felt that the concept failed to explain effectively the uplift of mountains themselves. He criticized in particular the portion of the concept that assumed folding and plication result from subsidence alone, the same assertion that Billings and Whitney had previously denied. LeConte speculated that sedimentation and subsidence take place concurrently, and, therefore, the surface of the subsiding area is never convex at all but remains horizontal or nearly so all the while that deposition continues. Under these conditions subsidence might produce fracturing and plication of the lower strata, but the upper surface would remain relatively undisturbed. He concluded that mountain formation itself must be a process separate from and subsequent to the sedimentation and subsidence. He noted that Hall and Hunt had left the sediments "just after the whole preparation has been made, but before the actual mountain formation has taken place. . . ."[63]

In his own explanation, LeConte asserted that elevation of mountain chains and ranges is "beyond question, produced by horizontal thrust crushing together the whole

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[62] *Ibid.*, p. 461   [63] *Ibid.*, p. 462

rock mass, and swelling it up vertically; the horizontal thrust being the necessary result of secular contraction of the interior of the earth."[64] This statement reveals LeConte's general belief regarding the causes of surface alterations, and he thus can be included within the large group of geologists who then accepted the shrinking-earth hypothesis.

Even though LeConte had been somewhat critical of Hall's views, he made effective use of portions of them in his own theory. Postulating that mountain chains are composed of enormous masses of sediments, an idea first prominently developed by Hall, LeConte proposed that

> mountain chains are formed by the mashing together and the upswelling of sea bottoms where immense thickness of sediments have accumulated; and as the greatest accumulations usually take place off the shores of continents, mountains are usually formed by the up-pressing of marginal sea bottoms.[65]

This statement, when stripped of the terms "up-pressing" and "mashing together," is strikingly similar to what Hall had said about accumulations of sediments some fifteen years earlier.[66]

In his views on metamorphism, LeConte followed Babbage, Herschel, Hall, and Hunt very closely. First asking why yielding to horizontal pressure takes place along the lines of the thick, off-shore deposits, he answered by

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[64] Ibid. [65] Ibid., p. 463.

[66] Hall, Palaeontology . . ., III, 73.

reference to the "<u>aqueo-igneous fusion of deeply buried sediments</u>," an idea he noted had been first shown by Babbage and Herschel. The accumulation of sediments, the rise of the isogeotherms, and the invasion of the sediments by heat from the interior of the earth combine to soften and alter the rocks, providing an area of weakened strata which permits them to be mashed together and pushed up into mountains. Even granitic formations, previously described as primitive axes of mountain chains, might be merely the lowermost and therefore the most altered portion of the mass.[67] In this way LeConte explained subsidence, elevation, and metamorphism.

In LeConte's view volcanic action is associated with the same geographical areas in which mountains arise. Denying that a liquid interior is necessary to explain volcanos, he said that volcanic materials are contained in "<u>sub-mountain reservoirs</u>." The foci of earthquakes and volcanos, he said, "are too superficial to have any immediate connection with an interior liquid, supposing such to exist."[68] Though he did not agree with all aspects of Hall's discussion of volcanos, LeConte did accept Hall's close relationship of massive accumulation of sediments, the placement of mountain chains, and volcanic phenomena.[69]

Several principles appeared in LeConte's theory of

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[67] LeConte, <u>American Journal of Science</u>, series 3, IV, 467-68.

[68] <u>Ibid</u>., pp. 470-71. [69] <u>Ibid</u>.

mountain chains which, he asserted, account for the principal phenomena of mountains. These are:

> 1. The most usual position of mountain chains is near continental coasts.
> 2. When there are several ranges belonging to one system, the ranges have usually been formed successively coast-ward.
> 3. Mountain chains are masses of immensely thick sediments.
> 4. The strata of which mountains are composed are strongly folded, and where the materials are suitable, affected with slaty cleavage; both the folds and the cleavage planes being usually parallel to the mountain chain.
> 5. The strata of mountain chains are usually affected with metamorphism, which is great in proportion to the height of the mountains and the complexity of the foldings.
> 6. Great fissure-eruptions and volcanoes are usually associated with mountain chains.
> 7. Many other phenomena--such as fissures, slips, earthquakes, and the subsidence preceding the elevation of mountains, it equally accounts for.[70]

When he accomodated the subsidence concept to the contractional theory, LeConte fitted many elements of Hall's concept into his own. By so doing, he concurred with the work of Dana, the most able spokesman for this particular combination of ideas.

Soon after LeConte's paper appeared in the <u>American Journal</u>, Hunt reviewed LeConte's ideas in the same periodical. Many of his arguments dealt with such things as priority of publication of the concepts involved, but more important was his clarification of some of the basic ideas expressed in LeConte's paper. Hunt believed that LeConte had not really understood some of Hall's basic ideas, and

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[70] <u>Ibid</u>., pp. 471-72.

because of this misunderstanding, some of the criticism was unwarranted. Hunt called attention to LeConte's assertion that Hall's concept was "a theory of mountains with the origin of mountains left out," first used by Dana in 1866. In his rebuttal Hunt again reiterated that "neither Hall nor yet myself . . . has proposed any theory to explain this latter part of the process, that is to say, the uplifting of the deposited sediments, which LeConte calls 'the actual mountain-formation.'" Hall, he said, was merely going back to older ideas taught by Lesley and much earlier by Buffon and de Montlosier that mountain areas are but the remnants of denuded continental areas. Furthermore, Hall had attempted no explanation for the continental uplift, the real cause of mountain formation.[71] LeConte himself, said Hunt, provided no satisfactory explanation for continental uplift on the grand scale necessary for mountain formation, merely associating it loosely with the contraction of the nucleus and the conformation of the crust to the interior of the globe.[72] In his paper Hunt tried to prevent Hall's concept from being distorted and altered, giving indications that he believed LeConte and Whitney had read more into the concept than was there in the first place. Thus, he indicated that much of the criticism concerned suppositions that were just

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[71]T. Sterry Hunt, "On Some Points in Dynamical Geology," American Journal of Science, series 3, V (1873), 266.

[72]*Ibid.*, p. 268.

not there. On the other hand, Hunt did the same sort of thing himself by associating the subsidence concept with his own ideas on the contraction of the globe.

The final effort in this brief interchange between Hunt and LeConte appeared a few months later when LeConte attempted to clarify his position further. He indicated he thought Hunt's perspective was too narrow for him to understand what the important features of the mountain-building process are. Hall's views are important, LeConte implied, and the association of sedimentation and mountain ranges is valid, but he insisted that such a concept is not a theory of mountain chains and that was what he himself was trying to provide.[73]

As noted before, Dana's remark about Hall's having omitted mountains from his "mountain-building" theory drew Hunt's criticism. He contended that Hall had never intended his concept to include the actual elevation of the strata.[74] Dana composed a rejoinder for the American Journal of Science later in 1873. He noted that some writers still attached considerable importance to Hall's concept of subsidence, and he believed it worthwhile to review the essentials of the theory. He commented that it had been published only

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[73]Joseph LeConte, "On the Formation of Features of the Earth-Surface. Reply to the Criticisms of T. Sterry Hunt," American Journal of Science, series 3, V (1873), 450-51.

[74]See note 71, *supra*.

in Hall's Palaeontology of New York, perhaps indicating he thought the concept had not received the general distribution needed for its proper evaluation. He first summarized the elements of the concept and then listed his own reasons for accepting or rejecting each of them.[75] That the Paleozoic strata of the Appalachian region are of shallow-water origin, that their great thickness was attained through progressive subsidence, and that the axis of the Appalachian chain follows this zone of subsidence had, according to Dana, become generally accepted by American geologists. However, that the weight of sediments causes subsidence was again sharply rejected by Dana, and he, like Whitney, thought it unreasonable to believe a small mass of sediments could depress the surface while the huge masses of nearby mountains stand thousands of feet above the surface of the sea. Dana recognized that the combination of subsidence and sedimentation will lead to the massive accumulations like those of the Appalachians but that "a slow subsidence of a continental region has often been the occasion for thick accumulations of sediments," not the other way around.[76]

The relation between the thickness of the accumulated sediments and the height of the mountains formed from

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[75] James D. Dana, "On the Origin of Mountains," American Journal of Science, series 3, V (1873), 347-48. The elements listed by Dana in this paper are essentially the same described in Chapter II of this study.

[76] Ibid., pp. 348-49.

them was, noted Dana, "a true and important one if taken in the most general way; but the application of it as a strict ratio, or as a universal law, encounters many apparent exceptions." He noted a number of examples in the Appalachians, in the mountains of western United States, and elsewhere that do not follow the generalization. But, he admitted, "it is evidently a common fact that where mountains have been raised, there, in general, thick accumulations of sediments were previously made; and conversely."[77]

In discussing Hall's thesis that mountains are for the most part the denuded remains of areas subjected to continental elevation, Dana climaxed his arguments. Hall had made no provision for this portion of the process, he said, then repeating his earlier statement that "Professor Hall's theory is strictly a 'theory of the origin of mountains with the elevation of the mountains left out.'" Plications that are caused by simple subsidence are accounted for, but how could the continent be uplifted in the manner described by Hall without the occurrence of other plications and local uplifts? There is, said Dana, an abundance of these latter types of phenomena that Hall had simply disregarded. All in all, concluded Dana, Hall had offered nothing to explain the elevation of mountains, and his theory was thus seriously deficient and defective.[78]

Generally speaking, Dana's evaluation of Hall's

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[77] Ibid., pp. 349-50. [78] Ibid., p. 350.

hypothesis was well-founded and entirely appropriate except for the "theory of mountains without mountains" expression which Hunt had indicated went beyond Hall's intent. Some of the elements of the concept had been founded on rather skimpy evidence, and for these Hall was taken to task repeatedly. However, as will be pointed out later, Dana's critical attitude did not prevent his making effective use of certain elements of the concept in his own theory of mountains that he made public in 1873.

The observer can see a typical pattern of reaction to a new and different theory occurring during the fifteen or so years after the initial introduction of Hall's subsidence concept. Some writers such as Hunt and Vose reacted quite favorably, accepting virtually all of the elements of the concept and acting as effective proponents of Hall's views. With Hunt, however, a subtle alteration and diversion of orientation of the concept is discernible, a reaction which in itself is typical of scientific activity. Among those who either opposed Hall's views completely or chose to discuss only those elements of the concept with which they disagreed is Billings who had little good to say of the concept at any time. Representative of most of those who had anything to say about the concept are men such as Dana, Whitney, and LeConte who, although critical of certain portions of the total concept, willingly incorporated parts of the new views into their own versions of the mountain-building process. As the situation stood in 1873, Hall's concept was neither a resounding success, nor was it a total and complete failure.

CHAPTER IV

THE GEOSYNCLINE AND THE CONTRACTIONAL THEORY

The geosyncline did not emerge as a clearly defined idea in the subsidence concept of James Hall. Instead, it went through a process of modification and enlargement at the hands of a number of geologists during the last few decades of the nineteenth century. Furthermore, it became associated with at least two other major orogenic concepts, each of which took the geosynclinal concept along a different path during its course of development. The first of these, the contractional theory, most ably expounded by James D. Dana, attained a high level of acceptance among many American and foreign geologists during this period. The other major concept with which the geosyncline became associated is isostasy, the idea that the materials of the earth's crust seek some sort of hydrostatic equilibrium. Clarence E. Dutton (1841-1912) of the U. S. Geological Survey seems to have been the individual most responsible for defining this view as it emerged in the 1880's, but its beginnings can be traced back for a number of decades before Dutton's enunciation appeared. The concept of the geosyncline existed semi-independently as well, and a few writers took it under consideration without reference to either of the foregoing theoretical structures. Furthermore, the phenomena of elevation and subsidence came under considerable discussion during the later decades of the nineteenth century, and these discussions, while frequently not specifically mentioning the geosyncline, included consideration of many of its elements, both as set forth by Hall in the 1850's and by other geologists later.

In this chapter the concept of the geosyncline will be traced in its relationship with the contractional theory, while its association with isostasy and its independent existence will be examined in subsequent chapters. Each concept will be traced historically so its role in the development of geological theory may be clearly apparent.

Dana and the Contractional Theory

In his Text-Book of Geology, Sir Archibald Geikie, whose career coincided with the period during which the contractional theory was most popular, defined the theory in these terms:

> The cause to which most geologists are now disposed to refer the corrugations of the earth's surface is secular cooling and consequent contraction. If our planet has been steadily losing heat by radiation into space, it must have progessively [sic] diminished in volume. The cooling implies contraction. . . . But the contraction has not manifested itself uniformly over the whole surface of the planet. The crust varies much in structure, in thermal resistance, and in the position of its isogeothermal lines. As the hotter nucleus contracts more rapidly by cooling than the cooled and hardened crust, the latter must sink down by its own weight, and in so doing requires to

> accomodate itself to a continually diminishing diameter. The descent of the crust gives rise to enormous tangential pressures. The rocks are crushed, crumpled, and broken in many places.[1]

In virtually all of his discussions of the mountain-building process, Dana elaborated on the contractional hypothesis. In a series of papers in the American Journal of Science in the early 1840's, he described the early development of this theory, mentioning William Leibnitz, Henry T. De La Beche, Adam Sedgwick, William W. Mather, M. Constant Prevost, and a number of other writers as early proponents of the theory. In fact, he said, "there are few writers at the present day who do not admit the former igneous fluidity of our globe. In this belief they recognize the fact that the earth has undergone contraction as a consequence of cooling. . . ."[2] In these papers Dana established his basic attitudes toward dynamic geology, an orientation from which he deviated but little during the remainder of his life.

According to Dana, mountains, continents, and

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[1]Archibald Geikie, Text-Book of Geology (4th ed.; London: Macmillan and Co., Ltd., 1903), I, 394-95.

[2]James D. Dana, "Geological Results of the Earth's Contraction in Consequence of Cooling," American Journal of Science, series 2, III (1847), 176 and n., 177, n. The first of this series of articles was "On the Volcanoes of the Moon," Ibid., series 2, II (1846), 335-55; followed by "On the Origin of Continents," Ibid., series 2, III (1847), 94-100; "Geological Results of the Earth's Contraction in Consequence of Cooling," Ibid., pp. 176-88; "Origin of the Grand Outline Features of the Earth," Ibid., pp. 381-98; and "A General Review of the Geological Effects of the Earth's Cooling from a State of Igneous Fusion," Ibid., series 2, IV (1847), 88-92.

oceanic areas can be explained by reference to the following general processes. The earth, initially a hot, fluid mass, gradually cooled and rocks of a coarse, crystalline texture were produced, first on the surface of the molten mass and subsequently beneath the crust. This process extended over an extremely long period of time. For reasons not well explained by Dana, cooling did not occur at the same rate in all areas of the globe, and there were large circular or elliptical areas that remained open as centers of fluidity and volcanic activity. In such areas differences in temperature induced a "boiling movement or circulation," an action which led to variations in the distribution of mineral constituents of rocks from place to place. As cooling progressed, these centers of eruption became extinct over large regions, and volcanic activity became confined to certain areas. The contraction attendant to solidification, being unequal, caused subsidence of the surface at unequal rates also, and those portions of the crust that cooled first, being thickest, subsided the least. In areas where the crust was thinnest or was most yielding, igneous activity continued to abound, and in time such areas became more depressed than the cooler, thicker areas. In the areas of greatest subsidence, the crust sometimes became arched because of the diminishing size of the earth. This in turn caused tremendous tangential pressure which acted to the greatest degree on the edges or the weakest portions of the arched area, causing a series of immense, parallel, folded mountains. The geographical orientation of mountain ranges and coast lines and the general shape of the continents was determined by the general direction of the weakest portions of the crust and by the position of large areas of maximum contraction. Areas of least contraction, which became the continents, often present mountain ranges near their borders. Thus, the very existence of the continental areas determined the location of the mountains. These general patterns of continental and oceanic areas became fixed very early in the earth's history by the condition and nature of the earth's crust.[3]

Though Dana modified and refined this basic mountain-building theory at various times during subsequent decades, he retained the same basic pattern of ideas throughout his writings on this phase of geology. In a pair of papers published in the American Journal in 1856, he filled in his general theory of mountain building somewhat, but he made no basic changes in it, merely adding details he believed tended to support his hypothesis.[4] In the first edition of his Manual of Geology (1863), he amplified his contractional concept somewhat further, putting

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[3]Dana, American Journal of Science, series 2, IV, 88-92.

[4]James D. Dana, "On American Geological History," American Journal of Science, series 2, XXII (1856), 305-34; Dana, "On the Plan of Development in the Geological History of North American, with a Map," Ibid., pp. 335-49.

more emphasis on lateral pressure as the primary cause of folding and plication of the surface and of the birth of mountain chains. These principles, he said, could now be regarded as universal for the entire globe even though they had been deduced from the special case of the Appalachians, because in the interim it had been found that nearly all inclined strata anywhere on the globe are actually plicated strata. "There is evidence everywhere," he concluded, "that the grander uplifts have been produced by lateral movements of the crust, and generally a pushing up of the formations into folds."[5] Furthermore, he asserted, the theory he had adopted was not in the least hypothetical. His attribution of the plication and elevation of the earth's crust to lateral pressure was simply a statement of fact, but he admitted that "the conclusion that this tension is due to the contraction of a cooling globe has not yet been fully established. It is here adopted because no other that is at all adequate has been presented." Later, he concluded that "no cause answers to these demands, so far as known, but the one mentioned,--the contraction of a cooling globe."[6] Though not mentioning Hall by name in this edition, Dana did discuss the subsidence concept

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[5]James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History, for the Use of Colleges, Academies, and Schools of Science (Philadelphia: Theodore Bliss & Co., 1863), pp. 719-20. This usage of deduced is Dana's.

[6]Ibid., p. 725.

briefly, but he did not at that time include it as part of his own contractional theory.[7]

As indicated previously, Dana's geology contained a large measure of catastrophist orientation even though at least one attempt was made to make him out as a uniformitarian. In a biographical sketch of Dana, Henry S. Williams wrote that "Dana was a consistent uniformitarian, in so far as to interpret past phenomena of the earth's history by the operations of forces such as are now in action," but he admitted in the same paragraph that Dana clearly saw the necessity of special disturbances and revolutions and that such special events of recent geological periods were of much greater magnitude than those which had occurred earlier.[8] Catastrophist though Dana's version of the contractional theory was, it provided one avenue by which Hall's subsidence concept gained a new name and attained a measure of respectability that ensured its survival as a part of geological theory.

In 1873 Dana examined Hall's views in a paper published in the American Journal.[9] He indicated agreement with certain of Hall's basic assumptions including the shallow-water origin of the Palaeozoic strata of the Appalachians,

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[7] Ibid., p. 717.

[8] Henry S. Williams, "James Dwight Dana and His Work as a Geologist," Journal of Geology, III (1895), 612-13.

[9] James D. Dana, "On the Origin of Mountains," American Journal of Science, series 3, V (1873), 347-50.

the great thickness of the sediments in that region having been attained through a long period of subsidence, the axis of the range being oriented to the line of deposition of the sediments, and there being a general relationship between the thickness of the accumulated detritus and the height of the mountains that subsequently arose in any particular location. However, he questioned Hall's view on the cause of metamorphism, which he said was too vague to be understood. Furthermore, Hall's assertions that the accumulating weight of the sediments caused subsidence and that this subsidence produces folding and plication Dana called "physical impossibilities." Finally, Dana castigated Hall for failing to put forward any cause for the elevation of the mountains themselves.[10]

Soon thereafter, Dana issued a major paper in which he gave the most comprehensive development of the contractional theory he ever attempted. This article is worthy of detailed examination as it has been called one of the classic documents in the development of geological theory in the nineteenth century.[11] Even in the mid-twentieth century, it frequently appears in bibliographies dealing with orogenic geology.[12]

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[10]Ibid.

[11]Joseph Barrell, "The Growth of Knowledge of Earth Structure," American Journal of Science, series 4, XLVI (1918), 158.

[12]Jean Aubouin, Developments in Geotectonics 1:

Dana commenced his discussion by delineating the ideas he had developed over the preceding three decades, but even though he asserted these views were not new, they showed a growth and clarity not apparent in his earlier expositions. His comparison of continental and oceanic areas is much more precise, and he more sharply distinguished between the continents, which are areas of minimum contraction and maximum crustal thickness, and the oceanic regions which continue to cool, solidify, and contract, consequently becoming depressed areas with sides that are somewhat abrupt. Unequal radial contraction, he said, caused the formation of these two diverse types of phenomena. Furthermore, Dana hypothesized a more definite relationship between lateral pressure and the formation of oceanic areas, saying that these areas had experienced the greatest subsidence and contraction and thus their edges exerted extensive lateral pressure against the contiguous land masses. Consequently, continental borders show the greatest amount of uplift, fracture, and plication, and there the highest chains of mountains arose.[13]

Geosynclines, trans. Express Translation Service (New York: American Elsevier Publishing Co., 1965), p. 293; Adolph Knopf, "The Geosynclinal Theory," Bulletin of the Geological Society of America, LIX (1948), 667; and G. Marshall Kay, "North American Geosynclines," Geological Society of America, Memoir, XLVIII (1951), 113.

[13]James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science, series 3, V (1873), 423-24.

Subsidence caused by the gravity of accumulating sediments is a basic premise of Hall's concept, but Dana sharply rejected this idea, saying that it was wholly at variance with physical law. He also rejected Joseph LeConte's assumption that compaction, lithification, and increasing density of the sediments permits subsidence. In either hypothesis, Dana asserted, subsidence would be prevented by the effect of heat from below which leads to expansion rather than contraction and subsidence. He was forced to conclude, he related, that "in the present state of science, . . . no adequate cause of subsidence has been suggested apart from the old one of lateral pressure in the contracting material of the globe."[14]

After summarily dismissing Hall's concept of continental elevation, Dana turned to LeConte's hypothesis that mountains are elevated solely by the crushing effects of lateral thrust, an idea Dana likewise described as inadequate. ". . . It must also be admitted," he wrote, "that there might have been, under tangential pressure, a bending of the strata without crushing, especially if there is beneath the earth's rind along the continental borders a region or layer of 'aqueo-igneous fusion.' . . ." Here Dana seems to have indicated that large area uplift may occur, an effect somewhat analogous to Hall's continental uplift hypothesis, but recognizing that Hall had omitted any cause for this

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[14] <u>Ibid</u>., pp. 426-27.

phenomenon, he postulated that lateral pressure must be that force.[15]

Dana thought it necessary to make a "grand distinction in orography," one that he believed to be fundamental to dynamic geology. On the one hand, he defined as monogenetic ranges those mountains which are simple or individual masses, resulting from a single process of mountain making. On the other hand, he defined as polygenetic ranges or chains those which combine two or more monogenetic ranges. The Appalachians, he said, are an excellent example of a polygenetic chain since they consist of several monogenetic ranges including the Highland, Green Mountain, and Allegheny ranges.[16]

After these preliminaries Dana described the mountain-making process itself, using the Allegheny range as an example. The first step in the process was a long-continued subsidence of a large depressed area he called a "geosynclinal." He emphasized the large extent of the geosynclinal by remarking that it might include many true or simple synclinals and anticlinals. Following the subsidence or geosynclinal, the depression filled with sediments, after which the range was finally completed by faulting, folding, plication, and other disturbances. The Green mountains, he asserted, also came into being by just such a process during the lower Silurian period. First there was a slow subsidence

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[15] _Ibid_., pp. 427-29. [16] _Ibid_., p. 429.

or geosynclinal accompanied by the deposition of sediments which equalled the geosynclinal in thickness, and "finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, etc. between the sides of the trough."[17]

In these two examples Dana introduced some significant ideas. For the first time the word geosynclinal appeared in geological literature, a term that through etymological evolution later became the more familiar "geosyncline."[18] The meaning that Dana applied to the term was quite restricted, however, indicating only an area of subsidence but not including any of the processes by which the subsidence is effected. Hall, in his description of similar phenomena, concluded that this subsidence followed the deposition of the sediments and that the added weight caused the crust to become depressed at that point.[19] Dana altered that concept radically by postulating that subsidence must either precede or coincide with deposition of sediments. In any case other causes produced the subsidence. He recognized Hall's association of accumulation and subsidence but found it necessary to reverse their order and causal relation to read: "Regions of monogenetic mountains were,

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[17]Ibid., p. 430. [18]See Chapter I, pp. 6-7, supra.

[19]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I, Palaeontology of New York (Albany: State of New York, 1859), p. 88.

previous, and preparatory, to the making of the mountains, areas each of a slowly progressing geosynclinal, and consequently, of thick accumulations of sediments."[20]

To illustrate the mountain-building processes further, Dana introduced another pair of terms to differentiate between types of mountain elevations. The first, the "synclinorium," indicates a mountain range that had grown through the geosynclinal process previously described. The other consists of large area upbendings in the earth's crust to which he applied the general term of "anticlinorium." The latter, he said, tends to be less permanent than the synclinorium, and many of them have disappeared in the course of large surface oscillations. The Cincinnati arch, extending southwestward from southern Ohio into Tennessee, he said, is a classic example of this latter form.[21]

Though he denied Hall's postulate of continental uplift, Dana described an hypothesis that seems almost analogous to it. "The geosynclinal ranges or synclinoria," he speculated, "have experienced in almost all cases since their completion, true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements. . . ." The extent of this type of movement can be described only in relation to a polygenetic mass, an area

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[20] Dana, American Journal of Science, series 3, V, 430-31.

[21] Ibid., pp. 431-32.

that by description closely approximates continental proportions. As an illustration Dana described the great uplift of the Rocky Mountain region of more than 8,000 feet which commenced after the Cretaceous period. This uplift appears as a true geanticlinal elevation of the great mass of the Rockies, being composed primarily of a combination of synclinoria.[22]

In applying the geosynclinal process to orographic operations taking place at the present time, Dana exhibited a pronounced non-uniformitarian orientation. Completion of a synclinorium involves the plication and solidification of the strata. Repeated additions of whole mountain regions to the more stable parts of the earth's crust has so stiffened the crust that "only feeble flexures of vast span are possible, even if the lateral pressure from contraction had not also declined in force." Furthermore, Dana could not conceive of any geosynclinal being in progress today that might lead to a new synclinorium.[23] This substantiated his earlier view that in the evolution of the globe, continental and oceanic areas had long since become permanent features. In 1847, for instance, he speculated that "the general forms of continents, and those of the seas, however modified afterward, were to a great extent fixed in the earliest periods by the condition and nature of the earth's crust."[24]

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[22]Ibid., pp. 432-33. [23]Ibid., p. 433.

[24]Dana, American Journal of Science, series 2, IV, 92.

Like Hall, Dana recognized that mountain chains and all of their attendant phenomena generally are found on the continental borders or in their vicinity which demonstrates that there is something peculiar about those regions.[25] Hall spoke of this relationship in much more general terms than Dana, asserting only that lines of mountain chains and the direction of the ancient ocean currents that carried the bulk of the sediments are coincident and parallel, or in other words, "the great Appalachian barrier is due to original deposition of materials, and not to any subsequent action or influence breaking up and dislocating the strata of which it is composed."[26] Thus, no mountain range of any substantial size will appear in locations other than along the borders of pre-existing continents which provide detrital materials. Unlike Dana, Hall did not attempt to lay down any strict pattern of oceanic-continental relationships, and he seemed to have left open the question of where mountains might grow as long as the necessary conditions prevail. Dana, although recognizing that mountain ranges tend to grow on continental borders, attributed this tendency to an entirely different set of causes, asserting there is another reason for the peculiarity of the border regions. Contraction of the crust has always been the greatest in the oceanic areas, and since the pressures generated by this contraction are not expended in elevating the ocean floor

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[25] *Ibid.* [26] Hall, p. 68.

it becomes applied to the border areas. Furthermore, "the lower position of the oceanic crust, and the abruptness with which the sides fall off, give it an opportunity to push beneath the sides of the continents, and this would determine the production of such mountains. . . ."[27] Dana seemed to take great pains to provide a cause and effect relationship for each minute item of the phenomena, while Hall tended to deal in generalizations that could be applied to the same phenomena without any overwhelming need to become concerned with the reasons why they turned out as they did.

In his views on the growth of mountains, Dana recognized a problem often ignored in discussions of the dynamics of crustal movements. What happens when the crust subsides 35,000 to 40,000 feet as it did in the Appalachians and this subsidence is not due to local contraction? An immense amount of material must be removed from underneath the subsiding trough to permit this movement, and by necessity this material must go somewhere. Dana and Hall each expressed conclusions on this problem, and though differing in details, their solutions are quite similar. Earlier, Dana would have simply said that the excess material would be immersed in the liquid interior of the earth, a fundamental postulate of his earlier theories.[28] By 1873,

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[27] Dana, American Journal of Science, series 3, V, 434.

[28] Dana, American Journal of Science, series 2, XXII, 340-41.

however, his views had become more precise, and he had created an hypothesis to explain such phenomena. After discussing and rejecting the thesis that subsidence could be explained by the removal of vapors or gasses that filled the spaces into which the crust subsided, he asserted that the "liquid or viscous rock" must be pushed aside. It must follow, then, that underneath a crust of undetermined thickness there must exist "a sea or lake of mobile (viscous or plastic) rock, as large as the sinking region. . . ." In the case of the Appalachians, Dana denied that much of this material could have gone north, south, or west because no evidence exists of any large scale oscillations in those areas during the Palaeozoic period. The only direction the material could have gone then was to the east, "and if driven eastward, a geanticlinal elevation of a sea-border region parallel with the area of subsidence must have been in progress from lateral pressure." Because he had no means of making an estimate, Dana did not attempt to speculate on how extensive and how high this structure might have been. It depended on how far eastward this escape was possible, but he thought it relatively small. Later, when the Alleghenies began to be pushed up in folds, the geanticlinal to the eastward would have subsided in part because of a "removal of resistance" in front of it and partially because of a tendency to "subside by gravity."[29]

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[29] James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science,

In hypothesizing lateral sub-crustal movements and consequent continental-sized elevations, Dana used an idea that had been previously expressed by both J. F. W. Herschel and Hall. In his "Introduction" of 1859, Hall recalled Herschel's association of subsidence from the accumulation of sediments and the consequent elevation of a nearby area caused by plastic or semi-liquid material being forced from the subsiding area to a position underneath the contiguous elevated area. According to Hall, "this process of depression at one point and elevation at another by the yielding mass beneath, doubtless offers an explanation of many phenomena both of recent and more ancient geological times."[30]

Dana here seemed to owe an unacknowledged debt to Hall and Herschel. Furthermore, in expressing these ideas, Dana appears to have contradicted himself several times. First, he postulated the rise of an area, which by necessity was large, if not of continental proportions, to the east of the present continent of North America. This does not follow from his proposition that the oceanic and continental areas were delineated and stabilized very early in the earth-shaping process. Second, he used a basic assumption that the oceanic areas exert pressure on the continental edges because of their more intense subsidence and

series 3, VI (1873), 7-9. The italics are those of the present author.

[30]Hall, p. 88.

contraction in the earlier stages of earth shaping, and in this case he somehow contrived to reverse this process. Finally, he asserted that the area of the geanticline eventually subsided, at least partially, from the effect of gravity, an assumption for which he had severely criticized Hall.

Dana also criticized Hall for postulating an ancient continent somewhere to the east of the present limits of North America. Hall, he said, had "designated the region, badly, as an 'eastern continent'" which he believed to have been the source of much of the sedimentary material that constitutes the Appalachian rocks. LeConte, said Dana, had also appealed to such a continent and had remarked upon its final disappearance. To Dana, neither Hall nor LeConte had regarded the continent as a part of the grand system of oscillations brought on by lateral pressure produced by the earth's contraction, instead viewing the subsidence as due to some other cause.[31]

In summarizing his mountain-making views, Dana concluded that from the example provided by the mountains in

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[31]Dana, American Journal of Science, series 3, VI, 9-10. In this section, Dana referred to ideas which he attributed to T. Sterry Hunt rather than to Hall. The ideas involved appear to be those of Hall, especially in Dana's denial that any appeal was made to the contractional hypothesis, a concept which Hunt accepted fully. For Hunt's view, see T. Sterry Hunt, "On Some Points in American Geology," American Journal of Science, series 2, XXXI (1861), 412-13. Hall, indeed, did not view subsidence as caused by contraction.

eastern North American, there was a "commencing and progressing geanticlinal on the sea-border." Further west along the border of the continent, lateral pressure activated a parallel and comcomitant geosynclinal which filled with sediments as it subsided until it was about seven miles in thickness. As a consequence the isogeotherms in the underlying crust rose an equivalent seven miles. Because of the introduction of heat from below, the crust in the area became weakened, part of it being eaten away by melting from the heat below, and the remainder becoming disarranged and elevated by a combination of lateral pressure from contraction and gravitational subsidence of the geanticlinal. This process created a "scene of catastrophe and mountain-making. . . ."[32]

Dana did not deal with the topic of metamorphism at any length. Prior to 1873 he did little more than recognize that rocks had somehow become altered by a process he did not undertake to describe. In 1866 he discussed briefly the ideas that Hall and George L. Vose had expressed on metamorphism, criticizing both because they tended to play down the effects of heat as a necessary condition of the metamorphic process. Vose, said Dana, had attempted to explain metamorphism as a consequence of pressure and whatever heat was produced incidental to compression. Likewise,

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[32]Dana, American Journal of Science, series 3, VI, 12-13.

Hall contended that rocks become altered by heat not much above the boiling point of water, and Dana believed this had been disproved by an appeal to geological evidence, several examples of which he pointed out.[33]

In his point-by-point critique of Hall's thesis in 1873, Dana again indicated he could not accept the conclusions of either Hall or Vose concerning metamorphism,[34] but neither did he elaborate in detail on the metamorphic porcess. However, he did introduce some ideas that had not theretofore appeared in his writings. He first criticized as unsatisfactory Herschel's assumption that the rise in isogeotherms introduced heat in an intensity needed to alter rocks. To Dana the evidence of unaltered rocks lying at a depth of sixteen thousand feet or more below the surface in various locations in the Appalachians indicated that mere deposition and subsidence were alone inadequate for metamorphism to occur, and, therefore, some other cause must be assigned to that process. "It seems certain," he stated, "that this method of obtaining the heat, by blanketing the surface with strata, is not sufficient."[35] He turned

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[33]James D. Dana, "Appendix to Article XXX, On the Origin of Some of the Earth's Features," American Journal of Science, series 2, XLII (1866), 252-53.

[34]Dana, American Journal of Science, series 3, V, 348.

[35]Dana, American Journal of Science, series 3, VI, 13.

instead to another source of heat that he believed could produce the intensity needed. Movements in the strata itself, such as progressive plications, shown to have been experienced by the metamorphic rocks themselves, might result in conditions favorable to metamorphis even with comparatively little help from the rise in isogeotherms. These movements are converted to heat which is produced just where it is needed to cause metamorphism. According to this idea various accumulations of sediments would be acted on according to three conditions:

> (1) The amount of motion, one principal source of heat;
> (2) The thickness of the series of beds undergoing movement, another source of heat beneath;
> (3) The amount of moisture present in the beds.

Thus, said Dana, rocks lying close to each other but appearing to be much different may have originated from the same material and may be located in the same geological horizon. Metamorphism could thus be attributed directly to the earth's contraction.[36]

The hypothesis that crustal movements can and do produce the quantity of heat needed for metamorphosis of rocks was not original with Dana, and he freely acknowledged his source of the idea. His earliest information on this view came from Henry Wurtz (1828-1910), an American chemist, who presented the idea in a paper read to the American Association for the Advancement of Science in 1866. Wurtz had

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[36] _Ibid._, pp. 13-14.

this to say about the subject:

> 'There is one related point to which I shall devote a few words; as due weight may not have been attached thereto by geologists, even supposing that it has occurred to any in precisely the same light. This is, that the tremendous dynamic agencies, whose effects of upheaval, subsidence, disruption and displacement, we find so widely manifest, while doubtless themselves engendered of the pent-up heat-energy of the interior, must have given birth to, or have been in part transmuted into, heat-motion. . . . It follows, for instance, that in our theoretical views of metamorphism, we are by no means of necessity limited, for our essential chemical excitant, merely to that portion of the hypothecated residual cosmical heat which might be supposed to have been retained by the emerging oceanic floor. Neither elevation nor subsidence (both necessarily accompanied by enormous compression) could occur without rise of temperature; though the degree of this rise would, of course, vary very much in various parts of the mass.'[37]

Though Dana suggested that Wurtz was the first to advance the association of motion and heat in its relation to geologic processes, he acknowledged that Robert Mallet (1810-1881) had also provided him with a more definitive explanation of this process. Mallet denied that simple pressure could produce heat in an intensity sufficient to cause metamorphism of rocks. Such heat, he said, has its origin "in the transformation of work into heat, the work arising from the movements (chiefly of descent) of the crust of a terraqueous cooling planet."[38]

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[37] [James D. Dana], "Prof. Henry Wurtz on Metamorphism as a Consequence of the Transformation of Motion into Heat," American Journal of Science, series 3, V (1873), 386.

[38] Robert Mallet, "Note on the History of Certain Recent Views in Dynamical Geology," American Journal of Science, series 3, V (1873), 302-03. At the time Dana published his series of articles on the results of the

By 1873, then, Dana had developed a more definite explanation for metamorphism of rocks, one which appears to have neatly complemented his views on the contraction of the globe. Nevertheless, even at that time his section on metamorphism was very brief and showed little originality.

In his discussion of igneous actions and volcanos, Dana again rejected the Hall-Hunt thesis that sedimentary accumulations can produce the heat and forces needed to activate those processes.[39] He argued that the huge masses of igneous material found in the Pacific border areas can be explained only by hypothesizing a vast, undercrust fire-sea as a source of igneous and volcanic materials. Such a fire-sea is probably a carry-over from the original liquid state of the earth, but it had quite likely been sustained, in accordance with Mallet's view of the conversion of motion into heat, by great heavings and bendings of the earth's crust that have taken place since. "Mallet's theory," he said, "presents us with a true cause" for these phenomena, but he admitted that to try and define just how effective this process might be was very difficult. Whatever the relationship between heat and movement might be, Dana seemed

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earth's contraction, he made reference to this short note by Mallet whose views were published in much more detail the following year in Robert Mallet, "Volcanic Energy: An Attempt to Develop Its True Origin and Cosmical Relations," Philosophical Transactions of the Royal Society of London, CLXIII (1873), 147-227.

[39] Dana, American Journal of Science, series 3, VI, 109.

thoroughly convinced that lateral pressure produced the conditions necessary for igneous and volcanic actions and that it produced the subsidence which forced plastic rock to the surface through volcanos or into subsurface intrusions.[40]

In reviewing Dana's long paper on the contractional theory, several significant things can be noted. First, the term geosynclinal was first introduced, and it became associated with a large area of subsidence. Second, the concept of large-area subsidence in its relationship to subsequent mountain ranges appeared again as a principle of orogenic geology. Upon superficial examination, this bears striking resemblance to Hall's concept, but Dana altered the action involved so much that it virtually became a new concept. Third, the views of Hall and Hunt regarding the formation of mountains, the metamorphic process, and volcanic and igneous actions all came under severe criticism by Dana who rejected virtually all of their conclusions. Thus Hall's subsidence concept seems to have suffered almost irrepairable damage, especially since what remained had been placed in the framework of the contractional hypothesis, a view that was in general disagreement with Hall's geological orientation.

Criticism and Modification of Dana's Views

During the quarter century following the publication of Dana's major paper on the contractional theory, a large

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[40] *Ibid.*, pp. 112-14.

amount of material appeared in geological literature that concerned this hypothesis, the subject of mountain formation, and the lesser included topics of elevation and subsidence. Though Dana gave few indications during this time that he reacted to these discussions, the fourth edition of his <u>Manual of Geology</u> (1896) reflects, to a certain degree at least, the new ideas presented and the criticism to which his theory had been subjected during the interim. What modification there was in his views brought them somewhat closer to the view expressed by Hall in 1859.

In 1873, the same year in which Dana's long paper was published, Charles Whittlesey (1808-1886), who had been associated with Hall in geological survey work on two occasions,[41] gave his general support to Dana's contractional theory. He speculated that volcanos, forces that might produce volcanos, and earthquakes were an inadequate explanation for the phenomena of mountains and their elevation, believing instead that

> the most satisfactory theory of elevation is that of lateral compression due to the contraction of the solid surface of the globe, by radiation of its heat. Such a contraction would produce wrinkles and corrugations along long lines, nearly straight, which could not be done by an explosive force.[42]

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[41] John M. Clarke, <u>James Hall of Albany, Geologist and Palaeontologist, 1811-1898</u> (Albany: n. p., 1923), pp. 221-22; Letter from Hall to Whittlesey, Albany, June 22, 1861, New York State Library, Albany, New York, Hall Papers.

[42] Charles Whittlesey, "On the Origin of Mountain Chains," <u>Proceedings of the American Association for the Advancement of Science</u>, XXII (1873), part II, 53-54.

Whittlesey's short discussion is typical of many that appeared on the subject during this period. It was short and lacked much of any detailed analysis or evidential support, but it did indicate that the contractional theory was gaining adherents.

The following year, Dutton prepared a major criticism of Dana's hypothesis. Dutton, a persistent critic of the contractional theory, discussed at some length the assumption that the secular loss of heat is and has been proceeding at the same rate since the earth was first formed. He reported trying out various sets of parameters concerning the rate of loss of heat, and from these attempts, he had concluded that the change in subsurface temperatures would not be much of a factor below a depth of about six hundred miles. By applying Fourier's theorem, he computed that below a depth of two to three hundred miles, there has been little cooling to the present time, and he had reached "the unavoidable deduction from this theorem . . . that the greatest possible contraction due to secular cooling is insufficient in amount to account for the phenomena attributed to it by the contractional hypothesis."[43]

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[43]Clarence E. Dutton, "A Criticism upon the Contractional Hypothesis," American Journal of Science, series 3, VIII (1874), 119-20. Fourier's theorem can be stated briefly as: "the rate of transmission of heat by conduction is proportional to the temperature gradient." The rate of transmission of heat may be understood as the quantity of heat transferred in unit time through unit area of cross-section of the substance, the unit area being taken perpendicular to the lines of flow. The gradient is the fall of

Two years later Dutton produced an even more devastating attack on the contractional theory. He first discussed the state of the theory of secular loss of heat as it had been advanced by two of the allegedly foremost authorities on the physics of the earth, William Thomson and Mallet, but found that they had provided no satisfactory solution to the problem.[44] "From whatever point of view the problem of the cooling globe is examined," he concluded, "we ultimately find ourselves brought back to the conclusion that it cannot be made to yield the results which are requisite to make up the contractional hypothesis." Those who had accepted the idea had grasped a factor which he described as "insignificant in itself, and enormously exaggerated" in its importance.[45]

A second major objection Dutton advanced at this time concerned the hypothesis that horizontal or tangential pressures and forces had caused the great wave-like ridges, some of which are so extremely flexed and the folds so closely pressed together that they appear to present a series of beds all dipping at an extreme angle. In spite of great displacement from their original horizontal

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temperature in degrees per unit length along the lines of flow. The definition is from Hugh L. Callendar, "Conduction of Heat," Encyclopaedia Britannica, 11th ed., VI, 891.

[44]Clarence E. Dutton, "Critical Observations on Theories of the Earth's Physical Evolution," Penn Monthly, VII (1876), 367-69.

[45]Ibid., p. 373.

position, the relative position of the strata is relatively undisturbed and they are not crushed into fragments nor disorganized. He recalled observing here and there a few acres of strata which had unmistakably been subjected to lateral pressure as a consequence of a large mass slipping down a steep incline. These examples exhibited an appearance of having completely "gone to pi," having been extensively crushed and broken up. Larger masses, then, of the extent of those in the Appalachians and other places, if subjected to such lateral forces, would certainly have become mere rubble, completely incapable of preserving their integrity.[46]

Dutton finally concluded that the objections he had raised to the contractional hypothesis were insuperable and that further discussion of it would be a waste of time. He had, however, admittedly placed himself and his fellow geologists in a rather poor position in regard to any satisfactory explanation of the dynamics of surface alterations. He had "no theory of his own to propose," he related, saying that

> the task of opposing a theory which has no competitor, is not an agreeable one, and it is especially burdensome in the present instance. For, if the opposition be well founded, it leaves geologists without any explanation of the innumerable facts which they have accumulated at the expense of so much study and labor.[47]

LeConte undertook a defense of Dana's Contractional

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[46] _Ibid._, pp. 376-77. [47] _Ibid._, p. 378.

theory against Dutton's adverse criticisms. He approached the whole problem from a slightly different viewpoint than any of his contemporaries, admitting that in the science of geology, not everything was known of the causes of contraction. But, he stated, the fact of contraction, which rests entirely upon the phenomena of geological structure, is quite a different thing from the causes of contraction. Two stages of theorizing are thus appropriate in examining the subject, and coming from these would be two separate theories. The first, which he described as the "formal" theory, includes the laws of the phenomena and the conditions under which the phenomena occur. The other stage concerns the physical causes of these laws and could thus be called a "physical" theory. Using the phenomenon of slaty cleavage as an example, he explained that the formal theory groups a multitude of facts about slaty cleavage and makes possible their explanation, thus properly constituting a theory. Unanswered, however, would be the question of how the crushing together of the strata would produce the cleavage. LeConte believed that only the formal theory was of vital importance to the geologist and that he could satisfactorily explain and support the contractional theory through such an approach, regardless of whether or not the physical theory had been satisfactorily formulated. The physical theory by itself might be of primary interest to the physicist but was not necessarily as important to the geologist.[48]

Dutton, according to LeConte, had advanced two primary objections to Dana's theory. In the first of these, Dutton contended that interior contraction could not concentrate its effects along certain lines without a slipping or shearing of the outer shell upon the interior portion, and such slippage appeared to be impossible in a solid earth. LeConte neatly parried this by asserting that this statement is hardly an objection to the contractional theory itself and would be so only if one assumed the complete solidity of the earth, an entirely unnecessary premise. By the hypothesis of a semi-liquid or plastic layer beneath the surface, this problem is satisfactorily explained.[49] In the second major objection Dutton asserted that secular loss of heat could not possibly have produced enough contraction to explain existing geological phenomena. In his reply LeConte acknowledged that Dutton might be essentially correct in his statement, but loss of heat seemed to be the most obvious cause of contraction, though not necessarily the only one. Other causes of shrinkage are certainly conceivable, some as yet undreamed of. "Other things besides the earth shrink and shrivel, and in some cases without

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[48]Joseph LeConte, "On the Structure and Origin of Mountains, with Special Reference to Recent Objections to the 'Contractional Theory,'" American Journal of Science, series 3, XVI (1878), 107-08.

[49]*Ibid.*, pp. 105-06.

loss of heat," asserted LeConte in attempting to illustrate this line of thought. "Apples shrivel by loss of moisture, and old people's faces wrinkle for the same reason," he continued. "Now is it not barely possible that there may be other causes of shrinkage of the earth and the wrinkling of its face, besides loss of heat?" LeConte thus essentially evaded this issue also.[50]

Dutton's view that lateral pressure could crush and disrupt strata instead of neatly folding them also drew a pointed rejoinder from LeConte. Singling out the examples of the Appalachian mountains and the Coast range of California, which consist almost wholly of crumpled strata with many folds pressed closely together, he said that it was "simply inconceivable that the crumpling force would have acted in any other direction than horizontally. . . ." Furthermore, there appeared to be none of the "shivering into rubble, like pack ice driven against a shore," as Dutton had implied would happen. Whether or not the strata were more plastic at the time of their crumpling than now, LeConte declined to speculate, parrying the question by asserting that it was not one that structural geologists were at the time interested in. Dutton, he said, as a physicist should have been interested in such a question, and since the present brittleness and hardness of the strata would have led to their being smashed to rubble, Dutton

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[50] Ibid., p. 106.

should have reasonably concluded that the formations were at one time less hard and brittle than now.[51]

Dutton's criticism of the contractional hypothesis in the two papers heretofore cited were but the opening round in his effort to replace that theory with another which he put forward at a later date. In the second half of his 1876 paper in the Penn Monthly, he set forth some of the essentials of the concept of isostasy, a subject that will be studied further in the succeeding chapter.

In 1875 Dana himself showed a slight change in his view of the relationship between the weight of surface materials and the stability of the crust of the earth. Earlier he had categorically denied Hall's contention that the gravity of accumulating sediments could cause subsidence, and he said that such a proposition was "wholly at variance with physical law."[52] Now, however, in discussing the effects of the weight of glacial icecaps on the stability of the surface, Dana admitted that the weight of a huge icecap might tend to cause subsidence in some small degree. Nevertheless, the preponderant cause of surface oscillations remained the lateral pressure within the crust. Furthermore, the accumulation, solidification, folding, and crystallization that had taken place during previous

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[51]Ibid., pp. 110-11.

[52]Dana, American Journal of Science, series 3, V, 426.

mountain-making processes had made the crust in mountainous areas much more rigid, and, consequently, any continental movements in those areas would be very small.[53]

The third edition of Dana's Manual of Geology appeared in 1880, and in his long chapter on dynamical geology, some further modification in the author's views on mountain making can be noted. However, he insisted that geological evidence proved that the forces which cause the uplifting of mountains in all cases act laterally. Folded mountains, such as found in the Appalachians and elsewhere, could have originated only in some general horizontal movement of the strata.[54] Likewise, Dana showed no signs of altering his opinion that the secular loss of heat and contraction of the globe create the lateral forces that uplift mountains.[55]

In this edition Dana granted a somewhat larger role to his version of the geosynclinal concept than he had earlier. He explained that a geosynclinal is a necessary prerequisite to the formation of all mountain ranges since they owe their very origins to the progress of geosynclinals.[56]

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[53] James D. Dana, "Recent Changes of Level on the Coast of Maine, with Reference to Their Origin and Relation to Other Similar Changes; by N. S. Shaler," American Journal of Science, series 3, IX (1875), 317-19.

[54] James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History (3rd ed.; New York: Ivison, Blakeman & Co., 1880), pp. 806-07.

[55] Ibid., pp. 814-15. [56] Ibid., pp. 820-21.

In the Appalachian geosynclinal, for instance, he speculated that the trough became filled with sediments as fast as the subsidence took place and that heating from below, brought on by a rise in isogeothermals, might have caused expansion of the beds, creating forces which led to flexures. He concluded, however, that the strata in the folded areas exhibited too great a conformability for such to have occurred. Indeed, he said, if there were oscillations in the level of the strata in this geosynclinal due to this cause, the subsidence overbalanced any such action that might have taken place.[57] In his description of the geological phenomena of valleys, Dana considered that most are valleys of denudation, but there are exceptions. Among the largest valleys on the earth are the depressions of the Great Lakes of North America. These may be, he said, geosynclinal in their origin.[58] Dana had also long before accepted much of Charles Darwin's thesis that coral islands and atolls in the Pacific ocean area are the only visible evidence of former islands or land masses that had subsided into the ocean.[59] By 1880, Dana had come to the conclusion

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[57]Ibid., p. 811. The idea of expansion of the strata due to an upward movement of isogeothermals is identical with that expressed by Hall in his subsidence concept. Both Hall and Dana attributed this view to Herschel and Babbage. Ibid., p. 718; Hall, Palaeontology . . ., III, 95-96.

[58]Dana, Manual of Geology . . ., pp. 824-25.

[59]Darwin published these ideas in their most comprehensive form in Darwin, The Structure and Distribution

that the entire Pacific coral-island subsidence had taken place in the form of a geosynclinal movement which had as its counterpart a geanticlinal movement in the late Tertiary and early Quaternary epochs.[60] Each of these examples represents a broadening of Dana's version of the geosynclinal hypothesis.

In the third edition of his Manual, Dana distinguished for the first time between geosynclinal depressions on the one hand and contractional depressions on the other. The former he described as downward bendings of the earth's crust, or broad basins that are not sufficiently deep to be called valleys. These, he said, have existed on the continents and along their borders, becoming filled with

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of Coral Reefs, Being the First Part of the Geology of the Voyage of the Beagle, under the Command of Capt. Fitzroy, R. N. during the Years 1832 to 1836 (London: Smith, Elder and Co., 1842). See also: James D. Dana, "On the Areas of Subsidence in the Pacific, as Indicated by the Distribution of Coral Islands," American Journal of Science, XLV (1843), 131-35; "On Coral Reefs and Islands," Ibid., series 2, XI (1851), 357-72; XII (1851), 25-51, 165-86, 329-38; XIII (1852), 34-41, 185-95, 338-50; XIV (1852), 76-84; and "Notes on the New Edition of Mr. Darwin's Work on the 'Structure and Distribution of Coral Reefs (1874),'" Ibid., series 3, VIII (1874), 312-19. Dana did not include the speculation that the weight of the coral growth led to subsidence, but Darwin did. Darwin, "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations," Proceedings of the Geological Society of London, II (1837), 552-54. Both Dana and Darwin contended that while the large area containing coral islands in the Pacific subsided, there was a compensatory elevation in other areas of the Pacific. Darwin, The Structure and Distribution of Coral Reefs . . ., pp. 142-44; Dana, Manual of Geology . . ., p. 824.

[60] Ibid.

sediments during the rock-making process. By contrast, he described contractional depressions as being made by contraction from cooling. Oceanic depressions are examples of the latter type feature.[61] By making this distinction, Dana displayed some modification of the idea expressed in his essay of 1873 where he asserted that these contractional depressions are the geosynclinals.[62]

In the following year, Osmond Fisher (1817-1914) published the first edition of his Physics of the Earth's Crust in which he undertook to examine contemporary theories of dynamic geology and to advance a theory of his own. By a mathematical examination of the contractional hypothesis, he concluded that if the globe had solidified at a temperature of 7000 degrees F., the average height of the elevations produced by loss of heat and compression from the time of solidification to the present would be a mere six and a third feet over the entire earth. He then estimated that the average height of the elevations on the earth's surface above a level of reference corresponding to the deepest part of the ocean to be at least 9,500 feet. Comparison of these two computations, he said, make it obvious that "(1) Either the inequalities of the Earth's surface are not altogether, or even chiefly, due to lateral compression, or (2) there

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[61] Ibid., p. 825.

[62] Dana, American Journal of Science, series 3, VI, 10.

has been some other cause involved in producing the needful amount of compression of the crust, besides the contraction of a solid interior through mere cooling."[63] By 1889, when the second edition of this book was published, Fisher discounted the contractional hypothesis even more strongly, saying "that the larger inequalities of the earth's surface can have little to do with elevation caused by compression of a solid globe through cooling. . . ."[64]

In spite of his lucid and thorough criticism, Fisher by no means convinced everyone familiar with the subject that the contractional hypothesis had failed. William Mackie, one of a group of rather obscure Englishmen who discussed the relationship of sedimentation, subsidence, and elevation in Nature, asserted that the connection between sedimentation and subsidence arose from the concomitant effects of lateral pressure in the earth's crust occasioned by its contraction. In the earlier stages of the solidification of the globe, certain areas had become thickened and strengthened by one or another cause, and those areas, by reason of their increased weight and thickness, offered greater resistance to the elevating force of tangential pressures. Contiguous areas became more elevated, while the thickened parts became in effect a depressed

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[63] Osmond Fisher, Physics of the Earth's Crust (London: Macmillan and Co., 1881), pp. 74-75.

[64] Osmond Fisher, Physics of the Earth's Crust (2d ed.; London: Macmillan and Co., 1889), p. 123.

synclinal area. These depressions naturally led to sedimentation which in turn led to more resistance and subsequently more subsidence of the thickened areas. At various times the tension of the crust might be relieved and the process would come to a halt for long periods.[65] Mackie's syncline and his association of subsidence, sedimentation, and lateral pressure bears a striking resemblance to Dana's.

James Durham, another of the same group, agreed with Mackie in his view of the relationships in question, asserting that elevations and depressions are caused by lateral pressure developed by the shrinking of the crust, but he denied that strengthening of certain areas by such causes was a necessary part of the process. He referred to Hall's example whereby one could generate synclinal and anticlinal curves by exerting lateral pressure on the edges of a book or magazine in a manner analogous to the way in which lateral pressure produces similar structures in the crust of the earth.[66]

In 1873 Appleton's American Encyclopaedia commissioned Hunt to write an article for its second edition on the subject of mountains and their formation.[67] Here he

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[65]William Mackie, "Elevation and Subsidence," Nature, XXVIII (1883), 488.

[66]James Durham, "Elevation and Subsidence," Nature, XXVIII (1883), 540.

[67]Letters from Hunt to Hall, Boston, January 3, 1874, April 15, 1874, and August 11, 1874, Hall Papers. The second edition of the American Encyclopaedia was published in 1883.

repeated many of the statements he had previously made about the structure of mountains and the part that Hall had had in developing what he thought were valid hypotheses. "To De Montlosier and to J. P. Lesley we owe our first conceptions of the true nature and origin of mountains and valleys," wrote Hunt, "and to James Hall its further elucidation and its illustration by the facts of North American Geology." In addition to the movements of the crust brought on by the deposition of sediments, said Hunt, "there are other movements which are conceived to be due to the contraction of the earth's nucleus, resulting also in movements of depression and elevation of the surface, and in corrugations of portions of the crust."[68] Thus Hunt once more applied Hall's subsidence concept to the more generalized contractional theory to explain mountain formation.

Hall had very little to say at any time about the contractional hypothesis, but what notice he took of it indicates a rather unfavorable reaction to it. When his 1857 Montreal address on mountain making finally was published in 1883, he appended a rejoinder to Dana's criticism that he had proposed a system of mountains with the mountains left out. He did not, he said,

> pretend to offer any new theory of elevation, nor to propound any principle as involved beyond what had been suggested by Babbage and Herschel. I did not propose

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[68] T. Sterry Hunt, "Mountain," American Encyclopaedia, 1883 edition, XII, 9.

> to discuss the theory of the contraction of the globe from cooling, or of the crumpling of the earth's crust from the gradual cooling and shrinking of the interior mass, because such arguments are not always philosophical for want of a basis in facts, and are always unsatisfactory as giving a very inadequate solution of the problem.[69]

During the second half of the nineteenth century, a heated controversy took place among geologists and physicists over what the interior condition of the earth might be. Some asserted it to be dense and solid, others described it as viscous to the extent it would act as a fluid, and some stated the interior and the crustal portions are solid with a liquid or semi-liquid layer interposed between the two. To a substantial degree many of the geological concepts advanced during the period were dependent upon or had been derived from the various views of the earth's interior condition.[70] A number of the discussions about this question included attempts to mathematize the physics of the crustal and interior portions of the earth. Included are papers by William Hopkins, Thomson, and Mallet as well as the book-length work by Fisher cited above.[71] Though these

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[69] James Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 68-69.

[70] See Chapter III, pp. 100-04, 112-13, supra.

[71] William Hopkins, "Researches in Physical Geology," Transactions of the Cambridge Philosophical Society, VI (1838), 1-84; Hopkins, "Researches in Physical Geology.--Third Series," Philosophical Transactions of the Royal Society of London, CXXXII (1842), 43-56; William Thomson, "On the Rigidity of the Earth," Philosophical Transactions

and similar compositions likely were written to assist the geologist in either formulating geological concepts or invalidating others less satisfactory, they appeared to confuse geologists rather more than anything else. Dana is a striking example of one who had become thoroughly unsure of his own theories because of the conflicting ideas about the interior condition of the earth. He wrote:

> I am led by the conflicting views of the best authorities with regard to the condition of the earth's interior to hold very loosely to any theory of mountain making. We wait for the physicists who believe in a solid earth to give us a theory which will have in view all geological facts. At present we get from them little more than an acknowledgment that no theory is yet in sight, or a disposal of the subject by passing it over to the 'geologist,' instead of recognizing that the facts, although geological, are physical and merit attention from physicists as much as the tides and more purely mathematical considerations.[72]

William B. Taylor (1821-1895), physicist at the Smithsonian Institution, displayed some little disenchantment with the idea that stratigraphic dispositions have resulted from contraction. This assumption, he said, "has of late years been so successfully assailed by critical estimates, that the hypothesis can be no longer regarded as tenable." That lateral pressure and compression have caused the varied plications and uptiltings of the crust could not

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of the Royal Society of London, CLIII (1863), 573-82; and Mallet, Philosophical Transactions of the Royal Society of London, CLXIII, 147-227.

[72]James D. Dana, "Professor W. O. Crosby on the Origin and Relations of Continents and Ocean Basins," American Journal of Science, series 3, XXIX (1885), 337.

be questioned, he continued, but the underlying cause has been proved to be entirely insufficient. Therefore, some other cause for these phenomena must be sought to supplement or supersede the secular cooling hypothesis.[73]

In England, two years later, Charles Davison (1858-1942) published a paper in which he gave qualified support to the contractional hypothesis, and though he did not mention Dana by name, the tenor of his remarks is quite similar to Dana's. Davison first attempted to mathematize the contractional forces and to estimate the extent of the contraction, but he concluded that his numerical results, being based on some rather dubious assumptions, might not stand up.[74] Furthermore, he advanced some minor objections to the theory which, he said, was not entirely free from criticism, though he did not believe any of his objections were by any means fatal to the theory. "Assuming the Earth to be practically solid, and to have been originally at a high temperature throughout," wrote Davison, "I believe it may be concluded that the peculiar distribution of strain in the Earth's crust resulting from its secular cooling has

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[73]William B. Taylor, "A Probable Cause of the Shrinkage of the Earth's Crust," Proceedings of the American Association for the Advancement of Science, XXXIV (1885), 200.

[74]Charles Davison, "On the Distribution of Strain in the Earth's Crust Resulting from Secular Cooling; with Special Reference to the Growth of Continents and the Formation of Mountain Chains," Philosophical Transactions of the Royal Society of London, CLXXVIII (1887), 231-40.

contributed to the permanence of ocean-basins, and has been the main cause of the growth of continents and the formation of mountain chains."[75]

In 1889 Dutton again had scathing criticism for the contractional hypothesis in a significant paper read to the Philosophical Society of Washington. Without resorting to violent assumptions, he asserted, "we cannot . . . find in this process a sufficient amount of either linear or volume contraction to account for the effects attributed to it." Furthermore, said Dutton, the distortions of the surface strata are not of the kind that would be produced by such a contractional process, and he thought Fisher had satisfactorily disposed of that portion of the hypothesis. The forces arising from a collapsing crust must act in every direction, and thus could not form the long parallel folds such as are exhibited in the Appalachians. Concerning the hypothesis, he continued, "I dismiss it with the remark that it is quantitatively insufficient and qualitatively inapplicable. It is an explanation which explains nothing which we want to explain."[76]

A very succinct statement of the status of the contractional hypothesis and related geological theory came from the pen of Robert S. Woodward (1849-1924) of the U. S.

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[75] Ibid., pp. 241-42.

[76] Clarence E. Dutton, "On Some of the Greater Problems of Physical Geology," Bulletin of the Philosophical Society of Washington, XI (1889), 51-52.

Geological Survey. "Closely related to the questions of secular contraction and the mechanics of crust movements," he reported,

> are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as a whole;--all questions of the greatest interest, but still lingering on the battle-fields of scientific opinion. Many of the 'thrice slain' combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or, to speak more precisely, whether the Earth may not be at once highly plastic under the action of long-continued forces and highly rigid under the action of periodic forces of short periods, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the mean time, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.[77]

In 1891 the term "geosyncline" appeared in Dana's geological vocabulary for the first time when he used the new form rather than the older "geosynclinal" of previous publications. This occurred in a paper in which he described his version of the mountain-making processes that had taken place in the area around the Connecticut River valley. The

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[77]Robert S. Woodward, "The Mathematical Theories of the Earth," Annual Report of the Board of Regents of the Smithsonian Institution Showing the Operations, Expenditures, and Condition of the Institution to July, 1890 (Washington: Government Printing Office, 1891), pp. 196-97.

geosyncline concept appeared in a close relationship with the process of sedimentation, although at that time he made no mention of which of the two necessarily came first. Perhaps because there was no necessity for making any mention of the contractional hypothesis, he did not do so, although he made liberal reference to lateral pressure as the force which produced the surface disturbances in the area.[78]

LeConte continued to be an advocate of the contractional hypothesis although his view was not identical with Dana's. When speaking of the relationship of sedimentation and subsidence, LeConte expressed a view much more like Hall's than Dana's. Mountain chains arise where great accumulations of sediments have taken place, he said, and are formed by the up-pressing of the marginal sea-bottoms where these sediments have been deposited. Whereas Dana postulated that the depressed area or geosyncline first subsided as a result of contraction and then filled with sediments, the whole history of the earth indicated to LeConte that the great areas of sedimentation have been areas of slow subsidence _pari passu_, meaning that the subsidence and deposition occurred simultaneously.[79] In discussing the

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[78]James D. Dana, "On Percival's Map of the Jura-Trias Trap-Belts of Central Connecticut, with Observations on the Up-Turning, or Mountain-Making Disturbance, of the Formation," _American Journal of Science_, series 3, XLII (1891), 442-44.

[79]Joseph LeConte, _Elements of Geology: A Text-Book for Colleges and for the General Reader_ (Rev. ed.; New York: D. Appleton and Co., 1889), pp. 265-66.

contractional hypothesis itself, LeConte acknowledged that both Fisher and Dutton had established that the rate at which the earth is now cooling is wholly inadequate to produce the observed effects, but once more he rather evaded the issue, asserting instead that other causes of contraction are certainly conceivable. What those causes might be, LeConte did not say except to make brief reference to the possibility that the loss of interior vapors and gasses might permit contraction.[80]

Like Dana and many other geologists of the period, LeConte recognized the rather poorly developed state of geological theory in the latter part of the nineteenth century. However, by his practice of seperating geology into two parts, the formal and the physical, he felt he could more effectively explain the phenomena. The formal theory, he stated, had already become well advanced toward a satisfactory condition, whereas the physical theory remained in a very chaotic state.[81] In his examination of the formal aspects of geological theory, he once more discussed the relationship between sedimentation and subsidence, displaying an even greater divergence from Dana's view.

Enormous thicknesses of sediments as often found in

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[80] Ibid., p. 271.

[81] Joseph LeConte, "Theories of the Origin of Mountain Ranges," Journal of Geology, I (1893), 549-51. LeConte had used this approach in an earlier paper on the same subject in 1878. See pp. 153-54, supra.

mountainous areas would be impossible according to LeConte, "unless the conditions of sedimentation on the same spot were continually renewed by pari passu subsidence of the sea-bottom." Abundant evidence of this sort of subsidence can be found in places where abundant sediments are now being deposited, and it can likewise be observed in the strata of all mountain ranges. LeConte then combined ideas set down earlier by Hall and Dana, speculating that

> the place of mountains while in preparation, in embryo, before birth, was gradually subsiding, as if borne down by the weight of the accumulating sediments, and continued thus to subside until the moment of birth, when of course a contrary movement commenced. The earth's crust on which the sediments accumulated was bent into a great trough, or what Dana calls a Geo-Syncline.[82]

LeConte here seems to have been the first individual to combine the idea of subsidence caused by the weight of sediments with the term geosyncline.

LeConte reflected ideas expressed earlier by Herschel, Babbage, Hall, and others when he hypothesized that accumulating sediments will cause a corresponding rise of the isogeothermal planes and an invasion of the lower regions of the subsiding sediments by the interior heat of the earth. This increase in temperature, especially when accompanied by the addition of water, he thought might cause hydrothermal softening. In turn, this leads to metamorphism in the strata and creation of a line of weakness in the geosyncline, a line along which crushing, folding, and the

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[82] Ibid., p. 552.

upheaval of mountains will occur.[83]

A postulate of lateral pressure in the crust of the earth is an essential part of LeConte's mountain-building process. All the major phenomena of mountains, he stated, can be satisfactorily explained by this postulate whereby the lateral pressure acts on lines of thick sediments deposited on marginal sea-bottoms and softened by the intrusion of heat from the interior. This view is entirely satisfactory as far as it had been carried, he believed, and it served perfectly well to bring "order out of the chaos of mountain phenomena," but it did not answer the question of what causes the lateral pressure, a problem that could be satisfactorily dealt with only by reference to physical rather than formal theory.[84]

Thus far, LeConte reported, the most obvious cause of lateral pressure is the interior contraction of the earth. This theory, which he described as a first attempt at a physical theory in this area of study, seemed to him to be the most reasonable, and he noted it had been generally accepted among geologists, at least until recently. But, he said, "objections have recently come thick and fast from many directions. Some of these I believe can be removed; but others perhaps cannot in the present condition of science, and may indeed prove fatal."[85] He then reviewed

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[83] *Ibid.*, pp. 556-58. [84] *Ibid.*, pp. 561-62.

[85] *Ibid.*, pp. 563-64. He reported that American

some of these objections, but he ultimately "found them all untenable, [and] we return again to the contractional theory, not indeed with our old confidence, but with the conviction that it is even yet the best working hypothesis we have."[86]

When compared to its third edition, the fourth and final version of Dana's Manual of Geology contained some significant alterations in its treatment of the subject of dynamic geology. The most significant change was Dana's unreserved acceptance of the concept of the geosyncline and its role in the mountain-making process. A preparatory geosyncline or trough and its load of sedimentary strata, said Dana, are necessary to the generation of any mountain range, and they occupy the area of the future mountain range.[87] The preparatory rock-making for the Appalachian mountains, for instance, took place within a gradually deepening geosyncline during all of Palaeozoic history, with the total deposition and subsidence amounting to some 30,000 to 40,000 feet of strata. That great trough had an area that corresponded in length, width, and location to the dimensions of the mountain range that ultimately occupied

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geologists had had such a prominent part in the development of the contractional theory that it had become known as the "American" theory. Ibid.

[86]Ibid., p. 573.

[87]James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History (4th ed.; New York: American Book Co., 1896), p. 380.

the area. At this point Dana finally acknowledged Hall as the originator of the idea, saying:

> The knowledge of the Appalachian facts led Professor James Hall to suggest in 1859 that a similar trough of deposition preceded the upturning in all cases of mountain-making. It was the first statement of this grand principle in orography.[88]

Subsequently, Dana briefly described the gravitation theory, as he called it, which he said had been introduced in its simplest form by Hall in his "Introduction" of 1859. "According to it," he reported,

> the making of the preparatory geosyncline, in the case of the Appalachians, was due to the gravitation of the accumulating sediments, in accordance with the principle explained by Herschel, whose views he cites; and the making of the flexures over the region was due to the same cause; that is, to the subsidence and not to heating from below. In the same paper, the general conclusion already referred to is drawn that a geosyncline of accumulation, like that of the Appalachians is a necessary preliminary in all cases of mountain-making.[89]

Though Dana later entered into some disagreement with the principles he attributed to Hall in this passage, he had for the first time established a definite and specific connection between Hall's subsidence concept and the concept of the geosyncline.

Dana pointed out a number of features of Hall's subsidence concept with which he could not agree, objections that he had stated on several previous occasions. It could not, even in its best form, he said, provide the amount of lateral pressure, contraction, or expansion that geological facts indicated were required. Assuming a maximum width of

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[88] *Ibid.*, p. 357. [89] *Ibid.*, p. 381.

250 miles and a depth of 40,000 feet for the Appalachian geosyncline, the maximum expansion due to the rise of isogeotherms in the subsiding strata, even at the bottom of the area, could be no more than about 5500 feet or about one mile. According to Dana, J. P. Lesley had computed the maximum shortening over the width of the geosyncline in Pennsylvania to have been about forty-five miles, while E. W. Claypole had estimated it to be double that amount. The discrepancy between these figures and those computed according to Hall's concept was too great for Dana to accept. Only by reference to the contractional theory, his favorite orogenic hypothesis, did he believe there would be sufficient force generated to account for the data.[90]

In this, his last exposition of the contractional theory, Dana did not give up his basic premise that mountains can be explained only by reference to contraction arising from the cooling of the globe, but he did exhibit some further modifications not present in any of his previous publications. The preparatory geosyncline had become an integral part of his view, and he now seemed to have envisioned the need for a far greater measure of geological time than he had earlier. Furthermore, he seemed willing to consider that subsidence of the geosyncline might after all proceed as a consequence of loading the area with sediments, although he had not given up the idea that lateral

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[90] *Ibid.*, pp. 381-83.

pressure is the major factor involved in the subsidence. He included, also for the first time, the Herschel-Babbage thesis that adjustment of the isogeotherms in the sinking sediments might weaken the strata in their lower regions and thus create conditions favorable to the phenomena of earthquakes, volcanic explosions, and similar events.[91]

Thus, by the end of the nineteenth century, the concept of the geosyncline had assumed a definite and imposing place within the contractional theory. Both Dana and LeConte, each of whom had had an important role in the development of this theory during the latter decades of the century, had acknowledged the concept of the geosyncline and had made it an integral part of their theoretical structures.

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[91]*Ibid.*, pp. 385-86.

CHAPTER V

THE GEOSYNCLINE AND THE RELATED CONCEPT OF ISOSTASY

The concepts of the geosyncline and of isostasy emerged as parts of geological theory more or less concurrently during the latter three or four decades of the nineteenth century. Although the authors who were instrumental in formulating the isostatic principle often did not specifically mention the concept of the geosyncline during that period, the two in fact were quite complementary and rested on some of the same basic assumptions. Furthermore, the concept of isostasy provided a means whereby the geosyncline became accepted outside the contractional theory. In this chapter the origins and development of the concept of isostasy will be examined together with its relationship to the contemporary version of the geosyncline.

One modern definition of isostasy is stated as:

> A condition of approximate equilibrium in the outer part of the earth, such that the gravitational effect of masses extending above the surface of the geoid in continental areas is approximately counterbalanced by a deficiency of density in the material beneath those masses, while the effect of deficiency of density in ocean waters is counterbalanced by an excess of density in the material under the oceans.[1]

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[1]A. C. Trowbridge, (ed.), <u>Glossary of Geology and</u>

Clarence E. Dutton, the individual usually credited with the first comprehensive statement of the principle, described it in these words:

> If the earth were composed of homogeneous matter its normal figure of equilibrium without strain would be a true spheroid of revolution; but if heterogeneous, if some parts were denser or lighter than others, its normal figure would no longer be spheroidal. Where the lighter matter was accumulated there would be a tendency to bulge, and where the denser matter existed there would be a tendency to flatten or depress the surface. For this condition of equilibrium of figure, to which gravitation tends to reduce a planetary body, irrespective of whether it be homogeneous or not, I propose the name isostasy.[2]

Though these two definitions differ in their wording, there appear to be no major differences in the basic ideas.

Although Dutton's name and isostasy are very closely associated and although Dutton himself had a major part in the development of the idea, he by no means originated the view that there are compensatory movements of elevation and subsidence in the crust of the earth. Beginning in 1836 a number of individuals expressed such ideas, and although their statements are not identical to Dutton's expression of the principle of isostasy, nevertheless they laid the foundations for it. One of the earliest descriptions of a condition in which an equilibrium of the materials is the

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Related Sciences: A Cooperative Project of the American Geological Institute (Washington: The American Geological Institute, 1957), p. 156.

[2]Clarence E. Dutton, "On Some of the Greater Problems of Physical Geology," Bulletin of the Philosophical Society of Washington, XI (1889), 53.

normal situation in the earth's crust appears in a letter from John F. W. Herschel to Charles Lyell in 1836, quoted in Charles Babbage's Ninth Bridgewater Treatise and subsequently cited innumerable times in the geological literature of the nineteenth century.[3] Herschel speculated that erosion of sedimentary materials from a continental surface and their deposition in a nearby sea causes a significant transfer of weight from the continents to the oceanic areas. The added weight on the bottom of the ocean depresses that area and by necessity the materials beneath will tend to move to one side or another. The most likely place for this material to go is underneath that area from which the sediments have been abraded, and the net result will be an upheaval of that area. Herschel cited the analogy of a body of moist clay which, when depressed at one point, will, he said, be uplifted at various points around the depressed area. The whole process involves a mechanical subversion of pressure from the norm, an effect he thought suffices to explain the upheaval of mountains.[4]

Though not mentioning any principle on which the phenomena might be based, Charles Darwin also expressed a rather rudimentary sort of a balance of forces in the crust

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[3]See Chapters III and IV, passim.

[4]Letter from Herschel to Lyell, Fredhausen, Cape of Good Hope, February 26, 1836, quoted in Charles Babbage, The Ninth Bridgewater Treatise, A Fragment (London: John Murray, 1837), pp. 210-12.

of the earth in his examination of the coral islands in the Pacific and Indian oceans. Certain aspects of coral barrier reefs, atolls, and coral islands indicated to him that the topographic features to which coral formations become attached are gradually subsiding into the crust of the earth beneath the ocean. He thought this subsidence to be part of a continental-sized down-warping taking place gradually in that area of the world. Coinciding with this subsidence, there appeared to be an area in the same general region that is being uplifted. "The Pacific and Indian seas could thus be divided into symmetrical areas of the two kinds," he reported, "the one sinking, as deduced from the presence of encircling and barrier reefs, and lagoon islands, and the other rising, as known from uplifted shells and corals, and skirting reefs."[5] Though he did not postulate any cause for this elevation-subsidence relationship, Darwin inferred that some sort of compensatory relationship did indeed exist.

In his "Introduction" of 1859, James Hall reiterated the principle of compensatory subsidence and upheaval developed earlier by Herschel. Depression of the yielding mass

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[5]Charles Darwin, "On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations" Proceedings of the Geological Society of London, II (1837), 553-54. The same principles with supporting data appeared later in Darwin, The Structure and Distribution of Coral Reefs, Being the First Part of the Geology of the Voyage of the Beagle, under the Command of Capt. Fitzroy, R. N. during the Years 1832 to 1836 (London: Smith, Elder and Co., 1842).

at one place and uplift at another, he said, could explain a considerable number of geological phenomena. He called attention to the depression of accumulated matter along the synclinal axis of the Appalachian chain which had, by displacing the yielding mass beneath, caused an uplift or bulging of the ocean bed to the west of that region. This uplift, taking place at a distance of a hundred miles or so, may have prevented the accumulation of sediments in that area. Meanwhile, the gradually sloping ocean floor allowed the formation of strata having their thickening edges toward the east, while they gradually thin out to the west such as now can be found in the area.[6] However, Hall did not develop this thesis much further than Herschel had, but he did attempt to apply it to the Appalachian mountains. However, this rudimentary expression of compensatory subsidence and elevation did provide a means of associating the concept of the geosyncline with that of isostasy, both of which grew out of these early beginnings.

Although he did not apply his speculations to any geological process, John Henry Pratt (1809-1871), an English mathematician and theologian who became archdeacon of Calcutta, did supply a part of the doctrine of isostasy. Pratt was puzzled by the amount of deflection of the plumb-line

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[6]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I, Palaeontology of New York (Albany: State of New York, 1859, p. 88.

in observations taken in the vicinity of the large mountain mass in India, a problem causing him some difficulty in the trigonometric survey of the country. After a rigorous mathematical examination of the problem, he concluded that there must be a deficiency in the mass of matter below the mountains, an idea that had been first suggested to him by Sir George B. Airy (1801-1892), the astronomer royal of Great Britain. Pratt hypothesized that when the earth had just entered a state no longer quite liquid, it was a perfect spheroid without valleys, mountains, or oceanic depressions. As the crust gradually solidified, contraction and expansion took place at various places causing corresponding depressions and elevations in the surface. If these surface movements were chiefly vertical in direction, then at any time a line is projected downward from any point on the surface to a sufficient depth, it must pass through a mass of matter which will be equal at any location. Mountains were created by expansion which forced the surface upward, and the mass thus forced up must have a corresponding attenuation below.[7] Pratt's assumption that the materials that compose mountains and their underlying structure are somewhat less dense than the materials beneath an ocean

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[7]John Henry Pratt, "On the Deflection of the Plumb-Line in India, Caused by the Attraction of the Himmalaya [*sic*] Mountains and of the Elevated Regions beyond; and Its Modification by the Compensating Effect of a Deficiency of Matter below the Mountain Mass," *Philosophical Transactions of the Royal Society of London*, CXLIX (1859), 745-47.

basin appears in both the modern definition of isostasy and that stated by Dutton in 1889.[8] Thus, before 1860 two basic assumptions of isostasy had appeared in published scientific literature; first, that there is an equilibrium of density or figure of the materials of the earth, and second, that when this equilibrium is upset for any reason, movements take place within the strata that tend to restore the equilibrium.

Elevation and subsidence go hand in hand with the principle of isostasy, and in the literature of the latter part of the nineteenth century, there appeared many discussions of these related phenomena. In 1871 Dutton expressed the tentative thought that the nature of metamorphic rocks might provide a key to the explanation of these phenomena. The action of heat, pressure, and water on sedimentary strata that brings about changes in the structure of the rocks might also cause a change in their specific gravity. Ultimately, this change might cause expansion or contraction, and since any expansion would be subject to lateral confines, the force of expansion would be directed upward and thus cause elevation. Conversely, in the case of contraction, subsidence of the surface would occur. Dutton speculated that a change in specific gravity of five per cent in 1000 feet of rock could account for a change of level of about fifty feet. When applied to rocks as thick

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[8]See pp. 177-78, <u>supra</u>.

as those found in the Appalachians, these computations could explain an alteration of level equal to the altitude of the North American continent above the ocean. Generally speaking, though, he considered that no one had yet advanced any really satisfactory views on elevation and subsidence.[9]

In 1872 Joseph LeConte restated Pratt's thesis in a major paper on the features of the earth's surface. He noted a "law of fluid equilibrium" that requires that in the early fluid state of the earth, the amount of matter along any radius of the globe was equal, a state not affected by any subsequent unequal contraction. "It seems probable, therefore," he related, "that the same equality still exists, and that, therefore, the matter along the shorter oceanic radii is denser than along the longer continental radii." Mountain masses would contain the lightest matter of all. LeConte noted a similarity between his idea and that expressed by Pratt but concluded that Pratt's hypothesis was but one possible mode of mountain formation. His own explanation was different from this entirely, he said.[10]

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[9] Clarence E. Dutton, "The Causes of Regional Elevations and Subsidences," Proceedings of the American Philosophical Society, XII (1871), 70-71.

[10] Joseph LeConte, "A Theory of the Formation of the Great Features of the Earth's Surface," American Journal of Science, series 3, IV (1872), 353. LeConte's source in this instance was John Henry Pratt, "On the Constitution of the Solid Crust of the Earth," Philosophical Magazine, series 4, XLI (1871), 307-09. Pratt also elaborated his original thesis in "The Mass of the Earth Is Arranged in Nearly Spherical Strata around Its Centre; and If the Outer Surface be a Spheroid of Equilibrium, Then All the Strata Are

Compensatory movements of elevation and subsidence involving the coral islands of the Pacific ocean area on the one hand and various continental regions on the other again became a topic of discussion in 1872, this time by Dana. Describing such a movement as "one of the great secular movements of the earth's crust," he noted that the subsidence of islands in the Pacific and the Atlantic associated with coral growth probably took place at the same time that certain other areas of the earth were being elevated, particularly the area of the North American continent. These upward movements, he speculated, may have been a balance to the downward oceanic movements that have taken place in the formation of the Pacific atolls.[11] Dana thus applied Darwin's speculations on a global basis.

In a somewhat different application of the principle of crustal equilibrium, Dana brought together elements of the concepts of isostasy and the geosyncline in his major paper on mountain making in 1873. As noted in the preceding chapter, he speculated that a geanticlinal of major proportions took shape to the east of the present continent of North America. This geanticlinal was elevated by materials forced to the eastward by the subsiding

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So Also, Whether They Acquired That Form from Once Being Fluid Or Not," Ibid., series 4, XXVI (1863), 342-46.

[11] James D. Dana, "On the Oceanic Coral Island Subsidence," American Journal of Science, series 3, IV (1872), 32-36.

Appalachian geosynclinal.[12] Since Dana believed that geosynclinals are caused by lateral pressures brought on by the earth's contraction,[13] it appears that not only did he combine portions of the concepts of isostasy and the geosyncline, but he also incorporated the contractional theory with them as well.

The part that glacial ice sheets can play in subsidence and elevation became a subsidiary factor in the subsidence concept as well as in the newly-emerging views on crustal equilibrium during the 1870's. In discussing such phenomena, Nathaniel S. Shaler noted that accumulations of ice might depress regions when the mass became a mile or more thick. "We should expect to find," he asserted, "that such depression of one part of a continent would be attended by an uplift of another region. . . ."[14] In his brief statements he brought both concepts together quite effectively. William J. McGee (1853-1912), self-educated American ethnologist and geologist, likewise wrote that these effects may be caused either by the deposition of sedimentary materials or by the formation of an extensive icecap.

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[12] See p. 140, <u>supra</u>; and James D. Dana, "On Some Results of the Earth's Contraction from Cooling," <u>American Journal of Science</u>, series 3, VI (1873), 8-9.

[13] <u>Ibid</u>., p. 10.

[14] Nathaniel S. Shaler, "Notes on Some of the Phenomena of Elevation and Subsidence of the Continents," <u>Proceedings of the Boston Society of Natural History</u>, XVII (1875), 291.

Subsidence of areas where sediments accumulate is a well-known phenomenon, he said, and it appears likely that a large mass of sediments will cause a subsidence about equal to its own thickness. On the other hand, an ice sheet has a specific gravity compared to average rock as a ratio of about one to three, and it follows that an ice sheet three miles thick should depress the surface about one mile. However, because of the relatively short span of geological time during which an ice sheet is normally present and because of the extremely slow reaction of the terrestrial crust to the added weight, the actual depression would likely be much less. After the ice disappears at the end of any glacial period, the hydrostatic principles involved demand that the crust return to its original form.[15]

In England similar discussions were going on, especially during the 1880's. J. Starkie Gardner (1844-1930), English geologist-botanist, indicated that accumulation of sediments proceeds on an almost foot-by-foot correspondence with the rate of subsidence, and he cited examples where this concurrent accumulation and subsidence is now taking place. A variation of this view is a situation where weight is transferred from a large region and then precipitated in a very circumscribed area such as in the case of subsiding coral reefs and atolls. Concerning this, Gardner cited

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[15]William J. McGee, "On Local Subsidence Produced by an Ice-Sheet," American Journal of Science, series 3, XXII (1881), 368-69.

Darwin's example of four decades earlier. After describing several more examples of concurrent elevation and subsidence, he turned to the effect produced by the weight of icecaps, which, he said, acts exactly the same as any other large accumulation except that it has a lower specific gravity and thus has proportionately less effect.[16]

Another English geologist, Charles Ricketts, likewise expressed a view in which subsidence and elevation act together to maintain a state of crustal equilibrium. After citing Hall's thesis on the general relationship, he noted that in North America, geologists generally accepted the interdependence of accumulation and subsidence. For example, Dutton considered that "'few geologists question that great masses of sedimentary matter displace the earth beneath them and subside.'"[17] Ricketts, however, disagreed sharply, noting that few geologists on his side of the Atlantic had ever taken notice of the subject.[18] Nevertheless, Ricketts

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[16] J. Starkie Gardner, "Elevation and Subsidence; or, the Permanence of Oceans and Continents," Nature, XXVIII (1883), 324-25.

[17] Charles Ricketts, "On Accumulation and Denudation, and Their Influence in Causing Oscillation of the Earth's Crust," Geological Magazine, XX (1883), 302-03. Dutton's statement appeared in Clarence E. Dutton, "The Geological History of the Colorado River and Plateaus," Nature, XIX (1879), 251.

[18] Ricketts, Geological Magazine, XX, 303. Even when only American geologists are included in Dutton's generalization, it certainly would not apply in the fashion Dutton indicated. Notable among those who disagreed with this concept were Dana and J. D. Whitney. See James D. Dana, "On Some Results of the Earth's Contraction from

reported that several others had lately taken the general subject under consideration, and that Gardner and Osmond Fisher both concurred with his conclusion that depression of the surface can be the result of accumulations, including a heavy covering of snow such as that in Greenland. Ricketts noted he had recognized these relationships several years earlier and had published similar conclusions on at least two previous occasions.[19]

Although Dutton did not apply the term isostasy to the principle of crustal equilibrium until 1889, he had enunciated many of the assumptions that are basic to the concept several years earlier. These appeared in 1876 in a paper in which he analyzed a number of the contemporary theories dealing with orogenic geology. He also indicated a general agreement with the subsidence concept as developed by Hall, although he did not at any time give specific acknowledgment to Hall as the source of his assumptions. "It has been indicated," he said,

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Cooling," American Journal of Science, series 3, V (1873), 430-31; and Josiah D. Whitney, "Volcanism and Mountain-Building," North American Review, XCIII (1871), 268.

[19]Ricketts, Geological Magazine, XX, 304. Though critical of Dutton for asserting that few geologists question the relationship between weight and subsidence, Ricketts resorted to the same type of a generalization, saying that "all writers on glacial geology recognize this progressive submersion of the land during what is called the 'Glacial Period,' when extensive districts . . . were buried under a thick covering of snow. . . . This subsidence may be chiefly ascribed to the weight of the snow heaped upon the land. . . ." Ibid., p. 305.

> that plications occur where strata have rapidly accumulated in great volume and in elongated narrow belts; that the axes of plication are parallel to the axes of maximum deposit; and that the movements immediately followed the deposition. All of these facts are covered by the cause here suggested. Wherever the load of sediments becomes heaviest, there they sink deepest, protruding the colloid magma beneath them to the adjoining areas which are less heavily weighted, forming at once both synclinals and anticlinals.[20]

In this passage Dutton not only set forth a major assumption of his own concept, but he also included several elements of Hall's subsidence concept.

At the conclusion of the paper cited above, Dutton listed a number of considerations he thought necessary to any satisfactory theory of mountain-building. They are:

> 1. The regions of great disturbances are regions of great sediments, and those of least disturbance are regions of small sediments: regard being had to the rapidity with which any stratographic series has been accumulated. This order of facts appears to be general, so far as present knowledge extends.
> 2. The epochs of disturbance have been those during and immediately following the deposition of thick strata.
> 3. The axes of displacements and vertical movement are parallel to, and probably coincident with, those of maximum and minimum deposit; where a series of the latter axes are parallel and have a definite direction, the plications and mountain forms have similar relations; and where there is no definite method in the variations in thickness, the movements have no systematic trend or parallelism.
> 4. In the process of metamorphism, it is probable that great changes occur in the specific gravity of the materials metamorphosed, an absorbtion of water rendering them lighter, and the elimination of water heavier.
> 5. All metamorphic rocks exhibit unquestionable evidence of having passed through a plastic or colloid condition; and if this condition prevails in any portion

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[20]Clarence E. Dutton, "Critical Observations on Theories of the Earth's Physical Evolution," <u>Penn Monthly</u>, VII (1876), 424.

of the crust of the earth, the equilibrium of the parts so affected must be subject to hydrostatic laws.

6. The transfer of great bodies of sediment from one portion of the earth's surface to others, is tantamount to a disturbance of the earth's equilibrium of figure, which the force of terrestrial gravitation constantly tends to restore, and which it inevitably will restore wholly or in part, if the materials of which it is composed are sufficiently plastic.[21]

In this statement Dutton indicated further unspoken concurrence with Hall's concept as well as giving a rather good statement of his own concept of isostasy. Displaying an excellent understanding of the growth of geological theory, he specifically denied that these conclusions were intended to offer a comprehensive theory for the origin of the earth's physical features. Such a theory, he said, is accomplished only by the work of a generation of great men. Instead, he offered these assumptions as indispensable factors for the final theorem, and they were advanced in an effort to break the deadlock which, he said, "has hitherto beset all inquiry into this magnificent and mysterious province of scientific research, and has apparently driven a large body of geologists into a premature acceptance of the contractional hypothesis."[22]

Recognition of the principle of hydrostatic balance in the crustal region of the earth appeared in French geological literature in 1877 in a pair of short notes by the French geologist, V. H. Hermite. He speculated that some sort of relationship existed between the weight of sediments

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[21] _Ibid._, pp. 430-31. [22] _Ibid._, p. 430.

and their subsidence. Furthermore, along with this subsidence an elevation of another portion of the surface must take place. Though not stated so specifically, some of the basic assumptions of Hall's subsidence concept also appear in this brief paper.[23] Hermite also denied that there need be any reference to the concept of a refrigerating globe for his own hypothesis to be valid.[24]

In the address that Hall delivered at Montreal in 1857 in which he first made public the concept that grew to be the geosyncline, there appeared no suggestion that there might be such a thing as isostatic equilibrium, as it was later to be called. He postulated a relationship between sedimentation and subsidence, but in the only reference he made to elevation, he indicated that continental elevations did take place but for causes then unknown.[25] During the preparation of the manuscript for publication in 1883, Hall invited comments on the essay from a few of his friends. These comments appear on the page proofs of the address and in some cases were incorporated into the printed version. One such comment by John J. Stevenson (1841-1924), a former

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[23]V. H. Hermite, "Sur l'unité des forces en géologie," Comptes Rendus, LXXXIV (1877), 459-60.

[24]V. H. Hermite, "Sur l'unité des forces en géologie," Comptes Rendus, LXXXIV (1877), 511.

[25]James Hall, MSS of the 1857 Address, "Contributions to the Geological History of the American Continent," p. 71, New York State Library, Albany, New York, Hall Papers, fol. 15.

assistant to Hall, carried a hint of orientation toward the concept of isostasy, and although it did not appear in the body of the published address, it was included in one of several notes at the end of the text. Stevenson's note reads: "Subsidence at one locality means elevation somewhere else--so while the ocean was subsiding, might not the Appalachians have risen?"[26] Hall accepted this suggestion, elaborated on it somewhat, and allowed it to be printed as follows:

> During the long palaeozoic time the area of subsidence was in the Appalachian region, though clearly enough, during some portion of that time great uplifting occurred on the northeast, to be succeeded by subsidence which may have been equal to the elevation. Why could not the area of subsidence be changed from the Appalachian region to the ocean on the east? Subsidence in one locality means a corresponding, but not necessarily equal, elevation elsewhere; so while the ocean bed was subsiding may not the Appalachians have risen?[27]

Hall earlier had acknowledged Herschel's similar hypothesis, noting it might be useful in explaining many geological phenomena.[28] In each of these statements Hall established a relationship between his own concept of subsidence and the idea of crustal equilibrium.

Hall also invited T. Sterry Hunt's comments, and these also appear on page proofs of Hall's address.[29]

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[26]John J. Stevenson, MSS note, Hall Papers, fol. 15.

[27]James Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 69.

[28]Hall, Palaeontology . . ., III, 88.

[29]T. Sterry Hunt, MSS notes, Hall Papers, fol. 15.

These notes are incorporated into a longer essay appearing immediately after Hall's published address.[30] Though he was an ardent proponent of Hall's concept, he fully accepted the contractional hypothesis and consequently he might reasonably have been expected to reject the idea of crustal equilibrium entirely. However, he did not, and in fact he made an effort to bring together all three concepts--Hall's, the contractional, and that of crustal equilibrium. That gentle and widespread movements of oscillation are, he noted,

> in some way not yet clearly explained, connected with the contracting of the nucleus, and the consequent conforming thereto of the envelope, we can scarcely doubt; or that the latter, from its nature and origin, must present great differences in constitution and in flexibility in its various parts. From this it might be expected that the movements imparted to the envelope alike by the process of secular cooling and contraction of the nucleus, and by the disturbance of the equilibrium of pressure consequent on the processes of erosion and sedimentation, would give rise to seemingly irregular oscillations, resulting in the depression or the elevation of considerable areas, constituting continental movements.[31]

Theodore Sington, another of the obscure Englishmen who published in <u>Nature</u>, pointedly criticized the idea of compensatory elevation and subsidence in a paper written in answer to Gardner's essay on the subject. Sington found himself entirely opposed to Gardner's ideas, saying he could

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[30]T. Sterry Hunt, "Notes on Prof. James Hall's Address," <u>Proceedings of the American Association for the Advancement of Science</u>, XXXI (1883), 70-71.

[31]<u>Ibid</u>.

not accept the hypothesis of pari passu subsidence and sedimentation, and that indeed, sedimentation and subsidence proceed entirely independent of each other. Accumulation of sediments continues so long as subsidence goes on, provided detrital materials are brought to the area, but if accumulation overtakes subsidence, the net result simply will be the distribution of the sediments over a wider area by action of tides and currents. If, on the other hand, deposition ceases, subsidence may very well continue, resulting in creation of a deep-water trough. Furthermore, every formation appeared to him as containing evidence that subsidence takes place independent of deposition. Likewise, Sington explained, elevation cannot be a consequence of denudation because elevation must necessarily precede denudation. Forces other than deposition and denudation cause crustal movements, but just what these are, Sington did not specify.[32]

Immediately following Sington's discussion, Gardner presented another short expose of his own thesis, although it is improbable that he knew of the former's criticism, as the two discussions appeared in the same number of Nature. After first reiterating his theory of crustal equilibrium, he concluded that

> this theory seems to me to be natural, and to accord with facts all round, but still it may be wrong. Those,

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[32]Theodore Sington, "Elevation and Subsidence," Nature, XXVIII (1883), 587.

however, who would assign all elevation and subsidence to secular cooling and tangential thrusts through shrinkage are revelling in their own imaginations, for there is no reason why the earth's nucleus should not have cooled as evenly as a cannon ball or piece of pottery, or other homogeneous body; and the records of the Palaeozoic rocks, when we may suppose shrinkage would be more active, certainly show that its surface was then relatively level, and without deep seas or great elevations on land.[33]

In an address read to the American Society of Naturalists late in 1885, G. K. Gilbert (1843-1918), geologist with the U. S. Geological Survey, presented one of the most effective arguments in favor of the hypothesis of crustal equilibrium that can be found in the geological literature of the period. As a geological example he used the extinct Lake Bonneville of Utah of which the Great Salt Lake is a remnant. He first described the general region occupied by the ancient lake, and how today this same general area appears to have been deformed so that the former lake bottom presents the appearance of a low broad dome with its crest near the center of the old lake.[34]

To account for this phenomenon, Gilbert examined three tentative hypotheses, analyzing each in terms of its total possible effect in order to determine which might be the most reasonable. The first began with the assumption

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[33]J. Starkie Gardner, "Elevation and Subsidence," Nature, XXVIII (1883), 587-88.

[34]G. K. Gilbert, "The Inculcation of Scientific Method by Example, with an Illustration Drawn from the Quaternary Geology of Utah," American Journal of Science, series 3, XXXI (1886), 290-94.

"well known to geologists" that large deposition of sediments will cause the sea floor to sink locally as rapidly as the sediments are added. At the same time, the adjacent continent, which had provided the sediment, rose at a rate equivalent to the amount of surface degradation. "It is a favorite theory--at least with that large division of geologists who consider the interior of the earth as mobile--," he continued, "that the sea-bottom sinks in such cases because of the load of sediment that is added and that the land is forced up hydrostatically because it is unloaded by erosion." Draining away the water of Lake Bonneville, approximately 1000 feet in depth, would give the supposed liquid interior an irresistible elevating force, which in turn created the dome-like structure.[35]

Gilbert's second hypothesis reads:

> The geoid of which the ocean's surface is a visible portion is not an ellipsoid of revolution, but differs from that symmetric surface by undulations which depend on local inequalities in the density and in the superficial configuration of the earth. . . . The surface of Lake Bonneville was part of a geoid at a higher plane than that of the ocean surface, and the removal of the water of the lake unquestionably modified the local form of the geoid.[36]

Gilbert's final hypothesis relates to the distribution of temperatures in the strata beneath the surface of the earth, temperatures which tend to take the form of isogeotherms that undulate in response to variations of conductivity and superficial temperature. At the poles of the

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[35]*Ibid.*, pp. 294-95. [36]*Ibid.*, p. 295.

earth where the surface temperature is very cold, the isogeotherms naturally lie at a greater depth than in the warmer latitudes, and if a portion of the surface undergoes a permanent change in mean temperature, the influence of this change will bring on a change in level of the isogeotherms. Where they rise nearer the surface, the crust will be locally expanded, and where depressed the surface of the crust will subside. If the surface temperature of the land at the bottom of Lake Bonneville can be shown to have been raised because of the removal of the water of the lake, then the uplift could be accounted for.[37]

The second and third hypotheses could not, said Gilbert, even by a most liberal application account for the amount of uplift that there had been in the area, and he dismissed each because it was quantitatively insufficient. However, by a mathematical analysis of his first suggestion, he found that he could retrodict an elevation of some 200 feet which agreed quite well with the approximate elevation he ascribed to the dome.[38] Nevertheless, Gilbert was not satisfied that the principle of hydrostatic equilibrium would be a completely adequate explanation of the phenomena in this case. If it is admitted that the removal of the water of the lake was the cause of the elevation of the bottom, he conceded that the "efficient *modus operandi* was an upbending of the solid crust of the earth, caused by

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[37] *Ibid.*, pp. 295-96. [38] *Ibid.*, pp. 296-98.

hydrostatic pressure communicated through a liquid substratum." However, a number of things made such a conclusion somewhat less than certain. The location of the dome-like structure and Lake Bonneville might have been only pure coincidence and have nothing in common, or admitting that the evidence upon which he had described the area as dome-shaped had been in part imaginary, the phenomenon he had been dealing with might not even exist. More observation was needed to establish a firm starting point for comparison of the hypotheses, and it seemed unlikely at that time that such investigations would be carried out. Nevertheless, he concluded that

> in the present state of observation and inference the hypothesis of the hydrostatic restoration of equilibrium by the underflow of heavy earth-matter is the only explanation which explains, and none of the observed facts antagonize it; but the alternative hypothesis is not barred out.[39]

Not long before Dutton made public his concept of isostasy, Alexander Winchell (1824-1891), professor of geology at the University of Michigan, took the same general topic under discussion. He compared the globe in some of its behavior to a hollow rubber ball filled with water. If this sphere is indented at one place, there must be a compensatory protuberance at another place, and if one section of its skin is thinner or weaker than the rest, the protuberance at that point will be greater than at any

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[39] Ibid., p. 298.

other place. Furthermore, if a small hole appears in the skin and the sphere is squeezed or depressed at any spot, some of the interior liquid will escape onto the surface. However, this analogy does not rest upon the hypothesis of a liquid interior in the earth, because whatever weight or force is adequate to cause the indentation in the surface would, by its crushing mechanical action, produce enough heat to fuse rocks and supply liquid material. "It is generally admitted," asserted Winchell, "that ocean sediments accumulated on a large scale, have in many cases, produced a subsidence of the bottom on which they rest. In some cases, we can point out the regions elevated as the counterpart to the subsidence."[40]

Escape of molten lava may also be a consequence of sedimentary pressure, Winchell continued, but geologists had failed to articulate this cause-effect relationship at all well. In the American west there are huge regions that have been at some time in the past subjected to outpourings of lava, a phenomenon he described as "an almost universal flood of molton [*sic*] material, which covered and buried the whole original face of the country--hills and dales, mountains and valleys." Furthermore, if the weight of sediments can cause depression of the surface, he asked, is it not also conceivable that the weight of the huge ice sheet

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[40]Alexander Winchell, "Some Effect of Pressure of a Continental Glacier," *American Geologist*, I (1888), 139-40.

that covered much of the northeastern part of North America during the glacial epochs could have caused this large area to subside? The net result of this subsidence may have been the production of huge amounts of molten material which escaped from volcanic vents in the areas which now make up the American west. Winchell believed that recently discovered palaeontological evidence, including human remains, indicated that these outpourings of lava coincided chronologically with the glacial period, thereby giving his hypothesis some validity it might not otherwise have.[41] His speculations provided yet another addition to the principle of crustal equilibrium.

LeConte generally accepted the principle of pari passu sedimentation and subsidence, and he stated that such a process has been very common in every period of the earth's history. Sedimentary deposition causes increased pressure on the underlying strata, and increased pressure produces an increased density in those materials. Increased density infers contraction of the materials which in turn leads to further subsidence. By a similar application of principles, he concluded that crustal erosion leads to elevation, and furthermore, this reduction of weight lessens pressure, producing expansion and further elevation. By this process he thought it possible to explain pari passu subsidence.[42]

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

[42] Joseph LeConte, "The General Interior Condition of the Earth," American Geologist, IV (1889), 43-44.

Though LeConte accepted a modified version of the principle of crustal equilibrium, he did not believe it to be the only cause of crustal movements. "There are undoubtedly other causes, far more fundamental, determining these movements," he asserted. "Subsidence is often not the result but the cause of excessive sedimentation by continual renewal of the conditions of sedimentation; and elevation is often the cause of excessive erosion by the renewal of the conditions of erosion." Great lava floods, he said, are evidence of great up-swellings caused by expansion of the sub-crust liquid which then flows out through great fissures onto the surface. Gravitational readjustments necessarily follow the outpourings of volcanic material.[43] LeConte, like Dana and Hunt, seemed unwilling to believe wholeheartedly in the principle of crustal equilibrium, finding it necessary to resort to other causes for explanations of the phenomena.

Another American geologist, Edward W. Claypole (1835-1901), entertained no doubt of a positive relationship between sedimentation and subsidence on the one hand and elevation and denudation on the other. It is a geological truth, he said, that the vast mass of Palaeozoic sediments in the Appalachian chain were removed by erosion from a contemporaneous Palaeozoic land. Likewise, there seemed no question among geologists that these sediments were

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[43] Ibid., p. 44.

deposited on the slowly subsiding bottom of the eastern part of the ocean which covered a large part of the interior of the continent.[44] A slow secular depression permitted portions of the Appalachian chain to subside at least 30,000 to 40,000 feet below their former level, and at the same time a progressively rising surface provided the sedimentary materials through erosion and denudation. To Claypole, the only factor that might stop the elevation of one area and the sinking of another would be some physical change, and indeed that did occur when the Appalachian trough became full at the end of the Palaeozoic age.[45] Claypole belonged to the large group of geologists, mostly North Americans, that recognized some sort of isostatic condition in the earth. Though he did not mention the concept of the geosyncline by name, several of its elements are present in his discussion. For instance, he described the "profound Appalachian trough in which the palaeozoic sediments were deposited" and the concurrent deposition of sediments and subsidence of the trough, all in conjunction with implications of isostasy.[46]

In the 1889 edition of his book, Physics of the Earth's Crust, Fisher added himself to the list of English geologists who accepted the yet unnamed concept of isostasy.

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[44]Edward W. Claypole, "The Materials of the Appalachians," American Naturalist, XXI (1887), 955.

[45]Ibid., pp. 960-61. [46]Ibid., p. 962.

In contrast to many others, however, he believed it necessary to hypothesize a liquid substratum, or at least one that has a plastic character. Only in this way could adequate allowance be made for local elevations, depressions, and a certain degree of lateral movement of materials toward mountain ranges. By this assumption it would be easy, he asserted, to explain how areas might sink in proportion to the overload of sediment. Thus the semi-rigid crust would assume a position of rest in a condition of hydrostatic equilibrium. Describing the Himalayan region as exhibiting this sort of equilibrium, he concluded that if much sediment were moved from the mountains and deposited on the plains below, after a time the mountains would become relatively lighter and would rise, while the plains would sink from the overload of sediments in order to reestablish the contour.[47]

After having been around for several decades in a more or less incomplete form, the concept of isostasy at long last received its present name from Dutton in 1889. After defining the concept, Dutton elaborated on his theme considerably in a paper that has been described as one of the fundamental documents in modern geological theory.[48]

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[47] Osmond Fisher, Physics of the Earth's Crust (2d ed.; London: Macmillan and Co., 1889), p. 134.

[48] Frank Dawson Adams, The Birth and Development of the Geological Sciences (Baltimore: Williams & Wilkins, 1938), p. 397; Karl Alfred von Zittel, History of Geology

Two classes of facts presenting themselves to Dutton gave added support to his contention that the surface of the earth tends to maintain a condition of isostatic balance. In the one case the evidence of massive quantities of shallow-water deposits in both the Appalachian and Rocky mountain areas indicated that the strata sank _pari passu_ with the accumulation of sediments. From this evidence he proposed the general rule that where large accumulations of sedimentary materials have been deposited over large areas, the deposition has been accompanied by a subsidence of the whole mass.[49]

In the other case Dutton noted a reciprocal action to the subsidence. Wherever broad mountainous areas have been subjected to extensive denudation, "the loss of altitude by degradation is made good by a rise of the platform." He called attention to various mountain ranges in the western United States, each containing several mountain ridges. All had been subjected to enormous erosion, and if the materials which had been removed were to be replaced, it would rebuild them to heights of eight to ten miles. To imagine them ever being so lofty would be incredible, he

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and Palaeontology to the End of the Nineteenth Century, trans. Maria M. Ogilvie-Gordon (London: Walter Scott, 1901), p. 322; Joseph Barrell, "The Growth of Knowledge of Earth Structure," _American Journal of Science_, series 4, XLVI (1918), 166.

[49]Dutton, _Bulletin of the Philosophical Society of Washington_, XI, 54-55.

remarked, and as a matter of fact they may never have been so lofty as they now are. On the flanks of these mountain platforms are upturned edges of strata resting against the platforms, indicating that the platforms themselves have slowly been pushed upwards about as fast as they have been worn away by erosions. "It seems little doubtful," concluded Dutton, "that these subsidences of accumulated deposits and these progressive upward movements of eroded mountain platforms are, in the main, results of gravitation restoring the isostasy which has been disturbed by denudation on the one hand and by sedimentation on the other."[50]

At no point did Dutton claim originality for the basic principle of isostasy, although it seems relatively certain he did originate the name for it.[51] As for the development of the principle itself, some of the highlights of its history have already been described. Dutton himself remarked that "the theory of isostasy thus briefly sketched out is essentially the theory of Babbage and Herschel, propounded nearly a century ago. It is, however, presented in a modified form, in a new dress, and in greater detail."[52]

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[50]<u>Ibid</u>., pp. 55-56.

[51]<u>Ibid</u>., p. 53. Earlier, Dutton indicated he had long believed in the doctrine of crustal equilibrium, and in an unpublished paper he had used the terms isostasy and isostatic to express this principle. Clarence E. Dutton, "<u>Physics of the Earth's Crust</u>; by the Rev. Osmond Fisher, M. A., F. G. S.," <u>American Journal of Science</u>, series 3, XXIII (1882), 289, n.

[52]Dutton, <u>Bulletin of the Philosophical Society of Washington</u>, XI, 58.

Dutton also noted that Fisher had made effective use of the concept of a plastic substratum in which various portions of the earth arrange themselves according to the density of their constituent materials. The profile of the earth in a broad sense is consequently due to the principle of flotation.[53]

Dutton read the paper containing the isostasy proposal at a meeting of the Philosophical Society of Washington, and following his presentation, a discussion took place during which Robert S. Woodward attempted to establish a relationship between isostasy and the contractional hypothesis. He contended that secular contraction does indeed play an important part in the crumpling of the earth's surface, Dutton's remarks to the contrary notwithstanding. He recognized there were severe difficulties in the contractional hypothesis, but it provided a reasonable basis from which isostasy could become a viable theory. Isostasy by itself must tend toward a condition of equilibrium at a relatively rapid rate, and consequently it would run down at an early geological age. By contrast, he said, the process of contraction goes ahead at a very slow rate, and all the while it tends to oppose the equilibrium toward which isostasy is leading. Though either process may lead to crumpling of the strata along lines of weakness, isostasy, even though the more effective of the two forces, appears to

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[53]Dutton, American Journal of Science, series 3, XXIII, 289.

have secular contraction as a prerequisite.[54]

In an examination of the forces and processes that led to the rise of the Appalachian chain, Bailey Willis (1857-1949) of the U. S. Geological Survey provided another instance in which the contractional hypothesis and isostasy complement each other. After discussing several of the conflicting hypotheses concerning that mountain range, he concluded that when Dutton asserted that secular loss of heat could never have provided the amount of contraction exhibited by the phenomena, he had used faulty assumptions, basing his conclusions on computations made by Fisher which were admittedly questionable.[55] Willis recalled Dutton's conclusion that elevation of strata on the one hand and folding and plication on the other, can be and often are entirely separate processes. Furthermore, examples can easily be located where plicated regions are little or not at all elevated or where elevated areas are very little plicated. Dutton, said Willis, "is shown by his own assumptions and by the opinions of his eminent supporters to have confused the lesser problem of zonal compression with the

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[54]Robert S. Woodward and G. K. Gilbert, "[Abstract of Remarks on 'Some of the Greater Problems of Physical Geology' by C. E. Dutton]," Bulletin of the Philosophical Society of Washington, XI (1889), 536-37.

[55]Bailey Willis, "The Mechanics of the Appalachian Structure," Thirteenth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1891-92, Part II, Geology, ed. J. W. Powell (Washington: Government Printing Office, 1893), pp. 278-79.

far greater one of deformation of the spheroid." With this confusion, then, Dutton's objection to the contractional hypothesis on quantitative grounds seemed invalid.[56]

According to Willis, Dutton had objected to the contractional hypothesis on qualitative grounds in two ways. First, he contended that any force produced by contraction would act equally in all directions, and second, there must be a zone of weakness in the strata, which, said Willis, Dutton had described as "required for the salvation of the hypothesis." Willis answered the latter objection by reference to experimental studies he himself had conducted by which he determined that conditions which ascertain places of folding and plication of the strata are inherent in the attitude of the strata, not in its thickness or the forces involved. He referred also to a statement published in 1883 by Thomas C. Chamberlin (1843-1928), founder of the <u>Journal of Geology</u>, which read:

> The portion which would yield was not necessarily that which was thinnest and inherently the weakest, but may have been that portion whose <u>attitude</u> placed it in a position unfavorable for resistance. For instance, if the strata had been previously bent downward by sedimentary accumulations upon them, or bent upward by any pre-existent circumstance, such portions would be most liable to yield and relieve the strain, though they might perhaps be even thicker than other portions which remained unflexed because more favorably situated for resistance.[57]

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[56]<u>Ibid</u>., p. 279.

[57]<u>Ibid</u>., pp. 279-80. The quotation is from Thomas C. Chamberlin, "General Geology," <u>Geology of Wisconsin. Survey of 1873-1879</u>, [ed.] Thomas C. Chamberlin (Madison: State of Wisconsin, 1883), I, 75-76.

As for Dutton's first objection, Willis noted that Dutton himself had pointed the way in his presentation of movements of isostatic adjustment. "Contraction gives to isostasy a needed force; isostasy directs contraction; the two effect a result which neither alone could bring about," a relationship which Willis attributed to Dutton.[58] Thus Willis concluded that the concepts of contraction and isostasy were not contradictory as Dutton had asserted, but rather they were complementary and each could accomodate the other. He then described how this process worked out in the case of the Appalachians. First, degradation of a pre-existing continent progressed, and the bulk of the sediments were deposited in a narrow belt along the shore of the adjacent ocean. Subsidence of this belt produced synclines of deposition, and in their depths the temperature rose as the strata sank, producing expansion and the beginnings of complex folding. Because the condition of isostasy prevails in the earth's mass, compensation must be made to the continental area for the load taken from it, and materials at a great depth flowed landward in a quantity sufficient to restore the elevation to the continent. Meanwhile, as sedimentation continued and isostatic adjustment began, the nucleus steadily contracted, resulting in a compression strain with no determinate direction or effect. Here, said Willis, are the three conditions needed for the mountain-

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[58] Willis, p. 279.

building process to take place: sedimentation, isostatic adjustment, and contraction. "There came a time when isostasy gave direction," he speculated, "and contraction gave a force to a movement of the submarine earth's crust toward the land, a movement extending seaward far beyond the zone of maximum sedimentary deposits, now folded, and including great extent of strata, now as then flat."[59] In this portion of Willis's paper can be seen elements of several contemporary concepts including Hall's of subsidence and sedimentation, Dana's of contraction, and Dutton's of isostasy.

McGee likewise concluded that isostasy alone could not account for all the phenomena of elevation and subsidence although he seemed to consider that isostasy is one of the essential principles involved. He briefly reviewed the historical development of isostasy, recalling Hall's subsidence concept, the determination of the density of various regions of the earth first by Pratt and later by Fisher and others, and finally the introduction of the concept of isostasy itself by Dutton.[60] Then by comparison of the area of degradation and sedimentation of the Mississippi river system with the known and inferred amounts of elevation and subsidence of those areas, he was able to arrive at what he believed were some significant general conclusions

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[59] Ibid., pp. 279-80.

[60] William J. McGee, "The Gulf of Mexico as a Measure of Isostasy," American Journal of Science, series 3, XLIV (1892), 177.

about the area.[61] ". . . The data relating to the condition of the earth's crust derived from the modern Gulf of Mexico indicate that throughout the vast geologic province of southeastern North America, isostasy is probably perfect," he reported, "i. e., that land and sea bottom are here in a state of hydrostatic equilibrium so delicately adjusted that any transfer of load produces a quantitatively equivalent deformation."[62]

However, in spite of how well the Gulf of Mexico illustrates Dutton's concept, McGee admitted it could not satisfactorialy explain all movements of elevation and subsidence. There are, he said, two different classes of movement involved which are called antecedent and consequent, the first including great initial movements in the crust by which continents are lifted and sometimes deformed and drowned, and the second including the isostatic movements due to loading and unloading. Recent movements of the area around the Gulf illustrate the latter type, while earlier movements of a much greater amplitude illustrate the former and must be assigned to a different but unknown cause.[63] McGee did not, however, resort to the contractional hypothesis as Willis and many other geologists had done.

Several years after Dutton's concept appeared, LeConte composed another paper on the origin of mountain

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[61] Ibid., pp. 185-88. [62] Ibid., p. 189.

[63] Ibid., pp. 191-92.

ranges, a subject to which he returned repeatedly during his career. He rejected Dana's view that a geosyncline must precede sedimentation, indicating that "the place of mountains while in preparation, in embryo, before birth, was gradually subsiding, as if borne down by the weight of accumulating sediments. . . ." Nevertheless, he contended that this was the same sort of a trough that Dana called a geosyncline.[64]

LeConte then examined the isostatic principle, noting that if the earth's crust yields under an increasing load, then it ought to rise by unloading. He recalled that in the Colorado Plateau region more than 10,000 feet have been eroded away and yet 8,000 feet remain, while in the Uinta range at least 30,000 feet of sediments have been carried away while 10,000 feet remain. Evidently, he concluded, pari passu elevation must accompany lightening by erosion. From this he agreed with Dutton that the earth in its general form as well as in its larger inequalities, is in a state of gravitational equilibrium.[65]

In LeConte's view the principle of isostasy need not succumb to the supposition of a solid earth, which he believed to follow from its cosmic behavior. Rejecting the liquid-interior hypothesis, he concluded that the earth,

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[64]Joseph LeConte, "Theories of the Origin of Mountain Ranges," Journal of Geology, I (1893), 552.

[65]Ibid., pp. 552-53.

although exceedingly rigid to a rapidly acting force, yields viscously to heavy pressures that act over a large area for extremely long periods of time. He also supposed that the earth is not necessarily or absolutely homogeneous either in density or in the conductivity of heat. In the process of secular cooling and contracting, denser and more conductive areas, because they cooled and contracted more quickly, subsided and became the ocean bottoms while the light and more slowly cooling areas remained as the prominent land surfaces. "And thus to-day the ocean basins are in gravitative equilibrium with the continental areas," he asserted, "because in proportion as oceanic radii are shorter are the materials also <u>denser</u>; and in proportion as the continental radii are <u>longer</u>, are the materials also specifically <u>lighter</u>." Thus appears the condition of gravitative equilibrium Dutton called isostasy.[66] LeConte thus effectively related the concepts of secular cooling, contraction, and isostasy, and to complete his eclectic view of geological theory, he had previously indicated acceptance of the subsidence concept.[67]

In his review of several mathematical theories of the earth, Woodward examined the relationship between the concepts of contraction and isostasy. He described isostasy as a revived version of an idea first suggested by Babbage and Herschel, an hypothesis that figures the crust to be in

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[66] *Ibid.*, pp. 553-54. [67] See p. 171, *supra*.

a state that borders on hydrostatic equilibrium but which cannot remain much out of balance without readjustment and the movement of masses of crustal materials. Transfer of any such considerable amount of material from one area to another must involve depression of the loaded area and elevation of the unloaded region. In a general way, elevation of any area tends to keep pace with erosion. The dynamics of the movements of the earth's crust are, according to the isostatic theory, thus referred to gravitation alone.[68]

Woodward noted that certain difficulties appear with the concept of isostasy, and furthermore, in a mathematical sense it is even more unsatisfactory than the theory of contraction. Moreover, isostasy tends to lead quickly to a state of equilibrium which contradicts the demands of historical geological continuity. Such a state as isostasy has not been reached, nor has there been any sign of diminished crustal movements during recent geologic time. "Hence we infer that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result," he concluded. "Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really

[68]Robert S. Woodward, "The Mathematical Theories of the Earth," Annual Report of the Board of Regents of the Smithsonian Institution Showing the Operations, Expenditures, and Condition of the Institution to July, 1890 (Washington: Government Printing Office, 1891), p. 196.

supplementary."[69] To Woodward, then, isostasy and the contractional hypothesis are not necessarily contradictory as Dutton had so strongly indicated.

A new series of gravitational measurements led Gilbert to modify somewhat his position on the concept of isostasy. Noting that a new series of data had been taken at a number of places extending from the east coast to Salt Lake City and in California, he discussed the general postulate that continents and ocean beds are in isostatic equilibrium. He noted that the great plains area between the Rockies and the Appalachians had apparently been exempt from orogenic disturbances for a number of geologic periods, a time during which there seemed to have been ample opportunity for gradual relief through the various agencies of viscous flow, degradation, sedimentation, and strains brought on by gravity in connection with descrepancies in density. Nevertheless, he found the values of gravity at all of the plains stations to be notably accordant. When compared to these computations, those made at various stations in the Rocky mountains in Colorado, Montana, Wyoming, and Utah indicated that the material in those mountains, if converted to a plateau, would have an elevation of between 2000 and 2500 feet higher than the adjacent plain. "The conclusion is thus reached," said Gilbert, "that the whole mountain mass above the level of its base is in excess of the requirement

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[69] Ibid.

for isostatic adjustment; or, in other words, is sustained by the rigidity of the earth."[70] He then generalized that

> these results tend to show that the earth is able to bear on its surface greater loads than American geologists, myself included, have been disposed to admit. They indicate that unloading and loading through degradation and deposition cannot be the cause of the continued rising of mountain ridges with reference to adjacent valleys, but that, on the contrary, the rising of mountain ridges, or orogenic corrugation, is directly opposed by gravity and is accomplished by independent forces in spite of gravitational resistance.[71]

What those independent forces were, Gilbert did not say.

In 1896 a session of the geology section of the American Association for the Advancement of Science was devoted to the celebration of Hall's sixtieth anniversary of public service. In a paper read by McGee in tribute to Hall, he brought together once more the subsidence concept of Hall, which McGee erroneously noted had first been introduced in 1856, and the complementary concept of isostasy. Though such pioneers as Babbage, Hopkins, Herschel, and other Britons had postulated such a mobility of the crust, and while J. W. Powell (1834-1902), director of the U. S. Geological Survey, and Dutton were instrumental in formulating the doctrine of isostasy, "this publication by Hall was one of the most important contributions ever made to the doctrine of isostasy."[72]

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[70]G. K. Gilbert, "New Light on Isostasy," Journal of Geology, III (1895), 331-33.

[71]Ibid., p. 333.

[72]William J. McGee, "James Hall, Founder of American

In the fourth and final edition of his Manual of Geology (1896), Dana joined the group who combined the concepts of isostasy, the geosyncline, and secular contraction. A number of his many references to the latter two concepts have already been noted, but before the issue of this book, he had not seemed willing to consider isostasy as a useful concept. He never regarded it as a total explanation for mountains in the same sense as Dutton had, but it did become an integral part of his theory of mountains.

The earth owes its shape primarily to the principle of gravitational equilibrium, said Dana, recalling that his view had first been recognized by Herschel in 1836. This idea of equilibrium, he asserted, holds whether the earth be solid, liquid, or whatever composition, though the rate of adjustment would necessarily be much slower if the earth is solid.[73] Though he did recognize the importance of the principle of gravitational equilibrium and that it was earth-shaping in its action, it nevertheless is not necessarily mountain-making. "It has been in all time conservative of existing conditions of equilibrium," he asserted. "Subsidences made by loads have caused elevations somewhere

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Stratigraphy," Science, new series, IV (1896), 702-03. Hall's address came in 1857. The publication noted by McGee is Hall's "Introduction" of 1859.

[73] James D. Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History (4th ed.; New York: American Book Co., 1896), p. 377.

around the subsided region; but the mean level, according to the principle, must have been retained."[74]

In his discussion of Hall's gravitational theory, Dana associated geosynclines of accumulation with the making of mountain ranges in a manner that recalls the subsidence aspect of gravitational equilibrium. He noted also that the principle of subsidence caused by loading had been supplemented by its apparent complement of elevation caused by denudation and unloading.[75] Nevertheless, in the final analysis, the gravitation theory, even though supplemented by the concept of isostasy, does not supply the amount of pressure, contraction, and expansion required by the facts, and Dana did not hesitate to postulate that his favorite concept, contraction of the earth due to secular loss of heat, was the only satisfactory explanation for mountain making.[76]

In a lengthy study of Pre-Cambrian geology published in the same year as Dana's last edition of the Manual of Geology, C. R. Van Hise (1857-1918), geologist at the University of Wisconsin and of the U. S. Geological Survey, indicated he thought Dana had accepted all three of the concepts. Dana, he reported, had applied the names geosyncline and geanticline to the greatest flexures of the earth, and, "generalizing from his illustrations, it appears that these may be defined as flexures which are predominantly

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[74]Ibid., p. 379. [75]Ibid., p. 381.
[76]Ibid., p. 383.

due to the force of gravity in its tendency to produce isostatic adjustment. The deforming force is mainly vertical." Nevertheless, folds and mountains result from great lateral forces acting along with the vertical forces of deformation.[77]

By 1896, isostasy, the geosyncline, aand the contractional hypothesis each had become relatively well accepted as integral parts of theoretical orogeny. Isostasy, originally conceived by Dutton as a specific replacement for the contractional theory, was frequently considered complementary to it rather than contradictory, especially among those geologists who were either thoroughly committed to the contractional view as in the case of Dana or like Woodward did not consider isostasy as a satisfactory answer to all dynamic problems of geology.

The geosyncline found a firm complement in isostasy and seems to have been ensured a good chance for survival through this association. The two concepts are not entirely dissimilar in many respects, and indeed, they appear to have common roots. Babbage, Herschel, Hall, Hunt, Dutton, and Gilbert all shared common views about the two, if not sharing terminology, at least sharing basic assumptions.

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[77] C. R. Van Hise, "Principles of North American Pre-Cambrian Geology," Sixteenth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1894-95, Part I, Director's Report and Papers of a Theoretic Nature, [ed.] Charles D. Walcott (Washington: Government Printing Office, 1896), p. 607.

CHAPTER VI

THE SEMI-INDEPENDENT EXISTENCE OF THE CONCEPT OF THE GEOSYNCLINE

During most stages of its development, the concept of the geosyncline was associated with either the contractional hypothesis or with the concept of isostasy. That it became an integral part of each of these broader theoretical structures during the latter decades of the nineteenth century has been established in the preceding two chapters. The geosyncline became associated with other theories of orogenic geology during the same period and during the early years of the twentieth century, and these associations, although not equal in significance to its association with the contractional or isostatic views, nevertheless merit some discussion.

In one sense James Hall may be considered as the first of those who discussed the basic concept that grew into the geosynclinal theory without associating it with the contractional theory or isostasy, although his view came nearer to the latter than the former. Already cited is his brief discussion in 1883 concerning the possibility of compensatory subsidence and elevation during the mountain-making period in the Appalachian chain.[1] He had expressed similar ideas some years before in his "Introduction" of 1859.[2] He never attempted to extend his concept beyond a clarification of the relationship between areas of significant sedimentation and the subsequent emergence of mountain ranges at those locations. He made this point clear in his rejoinder to James D. Dana's cutting remark that he had proposed a system of mountains with the mountains left out.[3] T. Sterry Hunt, likewise asserted that Hall's contribution "was to show the relation between mountain-chains and great accumulations of sediments, . . . and, moreover, to protest against the generally received theory that mountain elevations are due to local upthrusts, . . ." But whereas Hall had remained neutral rather than extending his concept, Hunt did not hesitate to include the subsidence concept in his own version of the contractional hypothesis, attempting to relate contraction to local subsidence and accumulation.[4]

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[1]James Hall, "Contributions to the Geological History of the American Continent," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 69.

[2]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I, Palaeontology of New York (Albany: State of New York, 1859), p. 88.

[3]Hall, Proceedings of the American Association for the Advancement of Science, XXXI, 68.

[4]T. Sterry Hunt, "Notes on Prof. James Hall's Address," Proceedings of the American Association for the Advancement of Science, XXXI (1883), 70.

In 1886 T. Mellard Reade (1832-1909), civil engineer and geologist of Liverpool, England, made a major attempt to develop a comprehensive theory of mountains, oriented neither to isostasy nor to the contractional hypothesis, yet including the basic assumptions of the geosyncline.[5] Before mountains can rise, said Reade, extensive sedimentation must take place. Large land areas must exist to supply sediments, because only continental-sized denudation can furnish the immense quantities of detrital materials involved. For the transfer of such quantities of sediments, extended periods of time must elapse, because, he said, we may "quicken the forces of nature," but in reality they operate very slowly. "The conditions of previous periods," he continued, "as made known to us by geology, do not warrant us in thinking that the forces of denudation differed much in energy in former time from those that now exist."[6]

Many geologists invoked precisely one foot of subsidence for each foot of sediment, but Reade denied this necessity. According to him, deposition occurs under a wide variety of conditions, and the results are also much varied. A "levelling-up" process may fill a depression without subsidence, or if depression occurs at a slow rate,

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[5]T. Mellard Reade, _The Origin of Mountain Ranges Considered Experimentally, Structurally, Dynamically, and in Relation to Their Geological History_ (London: Taylor and Francis, 1886).

[6]_Ibid._, pp. 84-85.

levelling-up might also occur. Depression may go on sporadically, and during quiescent periods, a combined levelling-up and extension would broaden the area of deposition. But in spite of all the variations in the process of erosion and sedimentation, it appears that

> it is in areas where the same conditions have existed for a great length of time, or successive changes have occurred altering the character and nature of the sediments, but without interference with the persistence of deposition, that mountain-ranges are built up. Part of these areas may from time to time become land, as we find happened in the history of the Andes, the Rocky Mountains, the Alps, and the Himalayas, but depression and sedimentation on the whole has held sway.[7]

Having described the conditions necessary for mountain building to commence, Reade turned to the causes which initiate earth movements and mountain making. The continued additions to already miles-deep beds of sediments has a decided effect on them, and it matters not whether the beds "have accumulated in what Dana calls 'geosynclinals,' or great bendings of the Earth," or have been built up in the ordinary form of submarine deltas such as that now being formed by the Amazon in the Atlantic. To explain the process involved, Reade turned to the hypothesis offered in 1834 by Charles Babbage to the effect that if sediments are added to any part of the earth's crust, the temperature will rise in the portion of the earth that is covered, causing

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[7] Ibid., pp. 86-88. In a footnote, Reade acknowledged that "to Prof. James Hall is, I believe, due the credit of first clearly stating the relations of sedimentation to mountain-building." Ibid., p. 88, n.

the rocks in that area to expand and the overlying sediments to become elevated.[8] Many geologists had made use of this idea, reported Reade, but almost without exception they had fixed their attention on linear vertical expansion alone, which as an explanation for mountain building is not at all satisfactory. Reade, however, did not confine his speculations to such linear expansion but widened his view to include superficial expansion or expansion in two directions, and expansion in volume, all of which follow from linear expansion. These are, he said, "far more powerful factors in mountain-building."[9]

After discussing various hypotheses on the state of the earth's interior, Reade assumed a condition of plasticity, if not in actuality at least in effect. He then noted that Dana, LeConte, and others had shown that the superposition of vast masses of rock or sediment would cause a considerable rise in the temperature of the lowest beds, and this effect is so pronounced "that it is suspected the lower bend of the geosynclinal may sometimes become actually softened." Though skeptical that this molten condition might ever actually occur, Reade believed he could consider that all rocks, though practically solid, would act as a

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[8] [Charles Babbage], "Observations on the Temple of Serapis at Pozzuoli, near Naples; with Remarks on Certain Causes Which May Produce Geological Cycles of Great Extent," Philosophical Magazine, series 3, V (1834), 215-16.

[9] Reade, pp. 89-90.

plastic material, an effect increased by heating from below.[10]

Reade postulated that the rise of the isogeothermal planes in this strata activates the "machinery of mountain-making that will cause compression, elevation, folding, and flowing and produce structural and orographic alterations on a stupendous and divers scale."[11] It follows, he continued, that the extent of mountain building is largely a factor of the thickness, area, and form of the component deposits. Furthermore, sedimentation not only leads to a rise in the temperature of the earth's crust, but it also induces volcanic activity and greater accessions of heat which further accentuate the process of mountain growth.[12]

Later in his book, Reade again discussed the larger bendings of the crust which he called regional subsidence and elevation. He repeated his disbelief in *pari passu* subsidence, asserting that such an assumption requires a fluid zone beneath the crust. Furthermore, there are numerous instances in which subsidence has taken place without loading. He cited the Mediterranean sea and the Gulf of Mexico as examples where depressions exist that cannot be the result of loading. Immense areas around continents and, indeed, the great ocean beds themselves are not free from regional fluctuations not caused by simple subsidence.

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[10]*Ibid.*, pp. 92-93. [11]*Ibid.*, p. 94.

[12]*Ibid.*, p. 95.

Secular contraction likewise would be totally unsatisfactory as an explanation, for it would tend to lead only to shrinkage, excluding the factor of expansion.[13] Actually, concluded Reade, ". . . no explanation can satisfy the conditions of the problem as we find them in nature but that of the actual mobility of the Earth."[14] And later, "I can conceive no explanation of these slow pulsations of the Earth's crust--for such they are in reality--so probable as that of a change of bulk in large masses of the Earth's heated interior."[15]

Reade thought that there must also be lateral movement of materials below the crust, most likely brought on by the weight of the accumulating sediments. Noting that he had previously shown that solid rock subjected to enormous pressure would flow in any direction to relieve strain, he thought it "therefore possible that such a pile of strata might assist to produce a geosynclinal by a lateral displacement of the underlying matter."[16]

In his final chapter Reade recapitulated all of the ideas that had appeared in a somewhat confused fashion throughout his book and came to the conclusion that

> it is thus apparent that all the phenomena of mountain-building are the result of local variations in temperature of the Earth's crust, caused by the reaction of surface influences on the heated interior. Every rise of temperature, whatever its amount, in the

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[13]*Ibid.*, pp. 266-67. [14]*Ibid.*, p. 269.

[15]*Ibid.*, p. 270. [16]*Ibid.*, p. 272.

> locus of a mountain-chain tends to elevation and permanent ridging up by a lateral displacement of materials. Every fall of temperature produces a proportionate vertical subsidence of the surface over the district affected; but as the materials laterally ridged up in mountain-ranges by expansion cannot be drawn back again during contraction, there remains a permanent total of uplift in the range with every rise of temperature, that can only be removed by atmospheric denudation. In this way mountain-ranges become permanent features on the Earth's surface, notwithstanding the vicissitudes of the larger areal subsidences and elevations that take place, and the subsidences due to faulting.[17]

Recurrent causes, such as repeated small and local changes in temperature in the strata, though relatively insignificant by themselves, collectively will hasten the process of crustal deformation and consequently the elevation is more rapid and effective. And, Reade continued,

> when we find that ordinary atmospheric changes produce such remarkable contortions in metal exposed to their influence, it would be strange indeed did not the greater, though slower, changes which take place in the Earth's crust in connection with sedimentation produce upon it an equivalent effect. It seems remarkable that this should not earlier have been perceived. The changes of temperature have been fully recognized, but their effect on the Earth's crust has been but partially apprehended.[18]

Reade's explanation of the mountain-building process seems to be partially a resurrection of the Babbage-Herschel hypothesis of a half century earlier. Reade himself denied more than a superficial resemblance between his own theory and that of Babbage and Herschel. He had made a significant addition by adding several expansive effects to the simple linear expansion described by Babbage and

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[17] *Ibid.*, pp. 328-29. [18] *Ibid.*, pp. 329-30.

Herschel, and he noted that the only similarity between the two concepts is the common postulate that isogeotherms tend to move upward with accumulation of new deposits.[19] Reade also introduced some significant ideas about the role of sedimentation and subsidence in the process, and he included the rudimentary concept of the geosyncline as it had been described by Dana a decade earlier. Furthermore, he attempted to construct a comprehensive theory of mountain building which included almost all the various geologic processes associated with mountains, something that Babbage and Herschel had not done.[20] Reade's theoretical structure, however, does not seem to have gained any great measure of acceptance among his fellow geologists,[21] and this probably reflects the disorganized state in which he found theories of dynamic geology. Reade spoke of this condition in the

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[19] T. Mellard Reade, "The Herschel-Babbage Theory of Mountain Building," Geological Magazine, XXVIII (1891), 140.

[20] Ibid.

[21] Reade found it necessary to amplify his theory further on numerous occasions after the appearance of his book in 1886. In the subsequent decade, at least twenty of his articles appeared in various journals on the subject of mountain building. Royal Society of London, [comp.], Catalogue of Scientific Papers (1800-1900) (Cambridge: At the University Press, 1923), XVIII, 80-81. For example, see T. Mellard Reade, "The Herschel-Babbage Theory of Mountain Building," Geological Magazine, XXVIII (1891), 140-41; Reade, "Physics of Mountain Building; Some Fundamental Conceptions," American Geologist, IX (1892), 238-43; and Reade, "An Outline of Mr. Mellard Reade's Theory of the Origin of Mountain-Ranges by Sedimentary Loading and Cumulative Recurrent Expansion: In Answer to Some Recent Criticisms," Philosophical Magazine, series 5, XXXI (1891), 485-96.

preface to his book, noting that many writers had undertaken a consideration of the dynamics of crustal movements, but no one had compiled a major systematic theory of mountain-building. His book, he said, was therefore an attempt to bring together "the germs of most true geological reasoning, dynamical and otherwise," that he found scattered throughout the works of many geologists in his own country and abroad. Although he had hopes that his theory might be the "true" one, he was prepared for the eventuality that ultimately it might not prove satisfactory.[22]

Several years later, Reade again indicated an awareness of the problems facing a geologist in dealing with the dynamics of mountain building. This appeared in a short criticism of an hypothesis put forward by Eduard L. Reyer (1849-1914), German geologist at the University of Wein, concerning a modification of contemporary views on the stratigraphic folding process. ". . . I welcome Mr. Reyer's fresh and vigorous treatment of the important problem of the causes of the deformation of the earth's crust," wrote Reade. "It is evidence that geologists and physicists are now allowing their minds to play freely round the subject of the orogenic changes of the earth's crust, and of the growth of philosophical conceptions on the geological evolution of our planet."[23]

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[22]Reade, The Origin of Mountain Ranges . . ., pp. iii-iv.

[23]T. Mellard Reade, "Causes of the Deformation of

In France the concept of the geosyncline appears to have become firmly established late in the nineteenth century, but it had been altered from the form it had assumed in America. At the International Geological Congress in Zurich in 1894, Marcel Bertrand (1847-1907), a mining engineer, presented a system of mountain building in which the geosyncline played a major role. A large part of the evidence he presented in support of his thesis came from the Alps of Savoy, the region with which he had become the most familiar, but he believed the results he had obtained seemed susceptible of universal application.[24]

Bertrand postulated that mountains rise in a definite sequence of geologic events, each being of the same relative importance in the development of each mountain chain. The first step is the formation of a great geosynclinal, followed by deposition of a type of material he defined as schistose flysch.[25] Following the initial stage

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the Earth's Crust," Nature, XLVI (1892), 315. Reyer's views appeared in Eduard L. Reyer, "On the Causes of the Deformation of the Earth's Crust," Nature, XLVI (1892), 224-27.

[24]Marcel Bertrand, "Structure des Alpes françaises et récurrence de certain faciès sédimentaires," Congrès Géologique Internationale, compte-rendu de la sixième session, en Suisse, Août 1894, Zurich (Lausanne: Georges Bridel & C^{ie}, 1897), p. 164.

[25]Bertrand found it difficult to give a precise definition of flysch, which although vague, he said, could be applied to much of the material found on the slopes of the great ranges. He described schistose flysch as composed of fine materials deposited during the period immediately following the formation of the geosyncline. Coarse flysch,

of deposition, the original geosynclinal trough becomes wrinkled and is subdivided by a central protrusion which becomes progressively accentuated. On the borders of the two narrower troughs created by the emergence of this central protrusion, flysch of the coarser type accumulates in great quantities. Next comes a period of energetic crustal movements during which the familiar folded structure arises. This disturbance of equilibrium leads to large deposits of sandstones and conglomerates on the sides of the newly emergent chain. Associated with and following the folding stage, granitic magma is pushed up into the central part of the chain forming the familiar granitic core of the range. Volcanos, igneous intrusions, and metamorphosis of rocks also take place during this latter stage.[26] In the present-day geologic epoch, not all stages of the mountain-making process may be discernible, according to Bertrand, but this may be due to the lack of knowledge of the parts of the globe where this might be realized. The best examples of areas recently elevated by this process are the mountain

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composed of coarser materials, is deposited during the subsequent period. He emphasized that each type is associated with a definite stage of the mountain-building process. Ibid., p. 169. The term flysch is generally applied to widespread deposits of sandstones, marls, shales, and clays, but the term seems originally to have been applied only to such deposits found in the Alps. A. C. Trowbridge, (ed.), Glossary of Geology and Related Sciences; A Cooperative Project of the American Geological Institute (Washington: The American Geological Institute, 1957), p. 113.

[26]Bertrand, p. 175.

ranges in the eastern Pacific ocean, including the coastal ranges and the Sierra Nevada of North America and the Andes of South America. He noted that this whole side of the Pacific had been subject to exceptional displacements, exceptional mobility of the crust, and exceptional elevation of granite. The whole region provides the best possible example of each of the cycles and is the most likely location for emplacement of a new mountain chain in the future.[27]

Bertrand's declaration that the first event of the mountain-building sequence is the formation of a geosyncline and the second a filling-up of that trough is strikingly similar to the sequence of events postulated by Dana in 1873.[28] Bertrand made reference neither to the contractional theory nor to isostasy in connection with the geosyncline, however. He chose to deal only with the kinematic aspect of the problem, and notably missing from his system is any hypothesis to explain the causes of the crustal movements involved. His rather simplified account of mountain building, rather than being a sophistication of contemporary theories, seems very suggestive of theories of a century earlier. It is described here, not because it represents anything highly significant in the development of geological theory, but because it provided another avenue through which the concept of the geosyncline has been perpetuated.

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[27] _Ibid._, pp. 175-76. [28] See p. 134, _supra_.

Emile Haug (1861-1927), professor of geology at the University of Paris, described the concept of the geosyncline much more comprehensively than Bertrand had. In a major paper of 1900, Haug reviewed the history of the concept briefly. The notion of the geosyncline, he related, is incontestably due to James Hall who illustrated the relationship between enormous accumulations of sediments and the gradual subsidence of the bottom of the sea, establishing a proportionality at each point between the thickness of the sediments and the subsidence of the crust at that point. Furthermore, Hall established the idea that the location of the subsiding area determines the location of the subsequent synclinal axis. Haug also recalled Dana's role in providing the term, geosyncline, and in introducing the idea that lateral compression causes the geosyncline rather than the weight of the sediments.[29]

Haug noted a difference of opinion between European and American geologists as to the character of the deposits in a geosynclinal zone. In the American view, typified by Hall and Dana, the deposits are generally shallow-water in origin, or as Haug described them, neritic. The extreme opposite of this view describes deposits in the geosynclinal zone as pelagic, or of deep-water origin, a view Haug

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[29]Emile Haug, "Les géosynclinaux et les aires continentales; contribution a l'étude des transgressions et des régressions marines," Bulletin de la Société Géologique de France, series 3, XXVIII (1900), 618-19.

attributed to Eduard Suess (1831-1914), an Austrian geologist. Haug believed both sides had exaggerated the question, and that in the Appalachians, to use the favorite example of the Americans, the base of the folded series of the Cambrian and Silurian deposits is coarsely detrital and thus indicates a shallow-water origin. On the other hand, Devonian and superior deposits indicate deposition at a considerable depth. In the Alps, he said, deposits called "abyssal" by certain European geologists can be attributed to shallow-water origin. For Haug, the deposits in the geosyncline, usually called "bathyl," have been laid down in seas relatively deep but not abyssal.[30]

To support his contention that most geosynclinal deposits occur in the bathyl zones, Haug examined the similarity between successive deposits and the rate of subsidence. To explain the accumulation in shallow water of very thick sediments with the same lithological character, it is necessary to suppose an almost perfect balance between the rate of deposition and the rate of subsidence. If deposition exceeded subsidence, the geosyncline would soon fill and the character of the sediments would show a definite change. Conversely, if the rate of subsidence exceeded deposition, the sediments would soon display evidence of a

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[30] Ibid., pp. 619-21. Haug defined the neritic zone as the area of deposition to a depth of about 100 meters, the bathyl extending from 100 to 900 meter depths, and abyssal anything below the bathyl. Ibid., p. 620.

deep-water nature. However, if one assumes the bathyl zone of deposition as most common, much greater oscillations in the surface of the geosyncline may occur without disturbing the nature of the sediments.[31]

Haug also noted that American geologists assumed that mountains usually form along borders of oceans and that continents grow by addition of mountain chains in those places. By this hypothesis geosynclines originate near the limits of continents in contiguous oceans, and the sediments contained in the resulting strata are exclusively littoral in character. The zone of subsidence in such an example would be separated from the sea by a simple rim. However, asserted Haug, the American definition of the geosyncline is not necessarily valid, and it is easy to show that geosynclines are not formed under the conditions assumed by the Americans. In actuality, he said, geosynclines characteristically consist of mobile zones situated between two relatively stable continental masses.[32]

To illustrate his conception of a typical geosyncline, Haug cited the example of the Himalayan range. This vast geosyncline with immensely thick sedimentary beds presents no sediments that can be described as littoral, and

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[31]Ibid., p. 624.

[32]Ibid., p. 630. The littoral region is that area extending generally to the 100 fathom depth. Though based on different assumptions, the terms neritic and littoral, when applied to sedimentary deposits, mean about the same thing. Trowbridge, pp. 171, 197.

at no time did that region exist at the edge of a great ocean. The region has always been limited on the south by the Indian peninsula, a relatively stable fragment of a much larger continent, and on the north by the extremely old central Asian continent. Similarly, the central European chains, considered together as a geosynclinal mass, are situated between the older part of the European continent on the north and the African continent on the south. Even the Appalachian geosyncline can be interpreted in the same manner, concluded Haug, if one visualizes the ancient Algonkin continent as the stable land area on the east and the Appalachian area itself as only a part of the entire geosyncline which included the area to the west that displays sediments of a deep-water origin.[33]

Haug drew up what he called general laws for the concept of the geosyncline, using the evidence he had at hand to support them. First, geosynclines are essentially mobile regions of the crust of the earth located between two relatively stable continental masses. Second, before they become filled with sediments, geosynclines form marine depressions of a considerable depth. Third, continental areas are, by contrast, those regions that are either above water or are temporarily invaded by the sea.[34]

During the previous two decades, Haug recalled, few questions had preoccupied geologists as much as the causes

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[33]Haug, pp. 631-32. [34]*Ibid.*, p. 632.

of oscillations of land and sea. He discussed briefly the explanations then under consideration, but he rejected them all in favor of a system of alternate "transgression" and "regression" of the seas on the continental areas and the geosynclines.[35] In the final analysis, however, he admitted that positive and negative movements of the continental areas are most difficult to explain. He discussed the postulate that lateral forces are involved in the elevation of geosynclinal areas and that folding of deposits of geosynclines could be attributed to increases in tangential forces, while a cessation of pressure on the other hand would lead to a deepening of the geosyncline. He also considered the alternate hypothesis that the geosyncline itself is the source of the basic movements. A contraction in the geosyncline lessens lateral pressure on the continental areas and lessens their subsidence, while expansion of the geosyncline brings the opposite effect. Haug did not opt for either hypothesis, instead introducing the concept of isostasy as a possible method of bringing the two together. However, he did not feel competent to delve deeper into what he considered to be some of the most arduous of geological questions.[36]

With the geosyncline as defined by Bertrand and Haug, a distinct change can be noted from the views of it originally developed by Hall, Dana, and other American

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[35] *Ibid.*, pp. 681-83. [36] *Ibid.*, pp. 708-10.

geologists. Jean Aubouin asserted that in the period since Haug published his paper in 1900, there have been distinct American and European concepts of the geosyncline. For the Americans, he said, the geosyncline is essentially a basin located near a coastline in which neritic sediments accumulate to great thicknesses. The Appalachian geosyncline is ordinarily given as the classic example of an area in which the geosynclinal process has taken place, while the Gulf of Mexico is often cited as a modern geosyncline on display. For Europeans, the geosyncline is typified by the Alpine ranges, and they regard the Sunda Archipelago as a present-day example of their view. Aubouin noted that the difference in views can be traced to the classic example of the geosyncline that each group uses, as there are many differences between the characteristics of the two.[37]

There are but few examples where individuals have taken the concept of the geosyncline under discussion without relating it to either the contractional hypothesis or isostasy. Geologists either included it in a more comprehensive theory, or else found it very restricted in its usefulness. Except for Reade, the individuals noted discussed it in relative isolation and generally evaded the question of how the geosyncline fits into the larger theoretical structures. In any event, these considerations

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[37]Jean Aubouin, Developments in Geotectonics 1: Geosynclines, trans. Express Translation Service (New York: American Elsevier Publishing Co., 1965), p. 17.

indicate that the geosynclinal concept had only a "semi-independent" existence.

CHAPTER VII

THE GEOSYNCLINE IN THE TWENTIETH CENTURY

A considerable amount of literature has been produced on the concept of the geosyncline in the past two-thirds of a century. This attention may indicate that it has become a useful part of theories of dynamic geology. Much of this literature has appeared in periodicals, but as recently as 1965 an entire book-length work had the geosyncline as its main topic.[1]

During the first two decades of the twentieth century, the term geosyncline became common in geological literature, indicating that the concept had attracted enough attention to merit discussion. In a textbook of geology published early in the century, Archibald Geikie presented a brief definition of the geosynclinal concept. Geosynclines and geanticlines, he said, are "larger simple flexures of the terrestrial crust, involving a wide region in each fold where the movement has been one of subsidence or uplift without any marked deformation. . . ." The broad

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[1]Jean Aubouin, Developments in Geotectonics 1: Geosynclines, trans. Express Translation Service (New York: American Elsevier Publishing Co., 1965).

region in central Europe laid down by long-continued subsidence and deposition may be called a geosyncline, he continued, while the more complicated structures created by subsequent crustal movements may be called anticlinoria and synclinoria, to use the terminology of Dana.[2] Geikie also described an example of a geosynclinal development in the western United States where two lofty ranges--the Sierra Nevada and the Wasatch--have been pushed up from a single great geosynclinal area.[3] Aside from these two brief examples, however, Geikie had little to say about the concept. Geikie's younger brother, James (1839-1915), also a writer of geology text-books, described many of the great lake-basins of Russia and North America as geosynclinal in their configuration. These depressions, he said, are the result of local sagging or subsidence of the crust, not necessarily associated with fracture and dislocation.[4] He also described how the concept of the geosyncline had been developed by James Hall and James D. Dana.[5] He applied the

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[2]Archibald Geikie, Text-Book of Geology (4th ed.; London: Macmillan and Co., 1903), I, 678-79. Dana's terminology appeared in James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science, series 3, V (1873), 431-33.

[3]Geikie, II, 1374.

[4]James Geikie, Structural and Field Geology for Students of Pure and Applied Science (2d ed.; Edinburgh: Oliver and Boyd, 1908), p. 419.

[5]James Geikie, Mountains: Their Origin, Growth and Decay (Edinburgh: Oliver and Boyd, 1913), pp. 197-211.

concept to the Pacific Ocean area which he speculated is undergoing a general movement of depression following the major uplifts of the Cenezoic period. Many of the deep troughs which lie along the continental borders in that area may be described as true geosynclinal troughs.[6]

Reginald A. Daly (1871-1957), a Canadian-American geologist, discussed a modified version of the contractional theory in 1906 in which the geosyncline has an important role in shaping the earth's crust. Though his main topic concerned igneous injections, the accumulation of sediments in a geosynclinal trough was deeply involved in his thesis.[7]

In paper published in 1913-14, Joseph Barrell (1869-1919), professor of geology at Yale, indicated he had accepted the concept of the geosyncline as a basic assumption in his philosophy of geology. He undertook a comprehensive and detailed analysis of the upper Devonian stage of the Appalachian geosyncline, and therein he noted that geologists of his day tended to confine the outermost limits of the geosyncline to needlessly narrow limits. "It has been customary, on paleogeographic maps," he wrote, "to draw the original limits of formations at no great distance beyond

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[6]Ibid., pp. 230-31.

[7]Reginald A. Daly, "Abyssal Igneous Injection as a Causal Condition and as an Effect of Mountain Building," American Journal of Science, series 4, XXII (1906), 209-10, 216.

their present outcrops. On the other hand, areas of ancient rocks tend to become regarded, unconsciously to the thinker, as land areas through all the younger ages." This procedure works out fairly well in a general sort of a way, continued Barrell, yet when it is applied to a specific problem it may lead to large errors. Sediments may have existed in their original state hundreds of miles in distance and thousands of feet in thickness beyond their existing boundaries and yet leave no trace of their former existence there. His intent was to demonstrate that the Appalachian geosyncline of the upper Devonian period had extended northward beyond Lake Ontario and eastward to the margin of the present coastal plain, dimensions much larger than contemporary paleogeographical maps indicated.[8] Barrell's study is one of the first examples of a comprehensive geological study in which the concept of the geosyncline was used as a basic assumption and in which there seems to be no question of whether or not the concept is acceptable.

During the first quarter of the century, the subject of isostasy also received considerable attention from geologists and geophysicists. Whereas discussions about isostasy in the latter part of the preceding century had been concerned primarily with the basic assumptions of the concept and tended to be quite generalized,[9] in the early

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[8]Joseph Barrell, "The Upper Devonian Delata of the Appalachian Geosyncline," American Journal of Science, series 4, XXXVII (1913), 87-88.

[9]See for instance Clarence E. Dutton, "On Some of

years of the twentieth century a concerted attempt was made to provide evidential support for the concept. Detailed measurements of gravitational attraction were made at numerous places, especially in the United States, and comprehensive mathematical formulas were worked out in an attempt to determine what state of isostatic equilibrium various parts of the crust might be in. In the reports emanating from these projects, scant attention was devoted to geological processes and no mention is found of any comprehensive geological theory outside the confines of the concept of isostasy itself.[10] However, indication that the concept of the geosyncline had not entirely disappeared from any relationship with isostasy is found in a major paper by Barrell in which he discussed the concept of isostasy and indicated his concurrence with it. He found that geological evidence clearly indicates that subsidence and deposition are necessarily related. Geologists, he noted, have often asserted

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the Greater Problems of Physical Geology," Bulletin of the Philosophical Society of Washington, XI (1889), 51-64; and G. K. Gilbert, "The Inculcation of Scientific Method by Example, with an Illustration Drawn from the Quaternary Geology of Utah," American Journal of Science, series 3, XXXI (1886), 284-99.

[10]Two comprehensive reports that provide excellent examples of this sort of activity are: John F. Hayford, Geodesy: The Figure of the Earth and Isostasy from Measurements in the United States, Department of Commerce and Labor, Coast and Geodetic Survey (Washington: Government Printing Office, 1909); and William Bowie, Isostatic Investigations and Data for Gravity Stations in the United States Established since 1915, Department of Commerce, U. S. Coast and Geodetic Survey, Serial No. 246, Special Publication No. 99 (Washington: Government Printing Office, 1924).

that deposition causes subsidence and between the two there is a "delicate isostatic adjustment." Though the strata have recorded crustal movements in detail, Barrell noted, it is difficult to separate the cause from the effect in this relationship. However, where strata are relatively thin as throughout much of the continental interior, submergence presumably was independent of the load of sediments, but where sediments accumulated rapidly, the load imposed by the accumulations most likely was the controlling force.[11] In these statements appear clear relationships between the concepts of the geosyncline and isostasy, and between the phenomena of elevation and subsidence.

At about the same time, Eliot Blackwelder (1880-1953), geologist at the University of Illinois and Stanford University, summarized the orogenic history of the North American continent, indicating a favorable view of the geosynclinal concept. However, he had some reservations about how broadly it should be applied. He noted that contemporary geologists seemed generally familiar with the idea that the major mountain systems of the earth coincide with earlier locations of geosynclinal depressions in which enormous thicknesses of sediments had accumulated. Some individuals, he continued, thought the trough itself had been broadened by lateral pressure which led to further sedimentary

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[11]Joseph Barrell, "The Strength of the Earth's Crust," Journal of Geology, XXII (1914), 36-37.

deposits, while another group, which he described as "extreme isostasists" viewed the trough as a consequence of deposition of the beds. One should not, warned Blackwelder, "assume that there is a causal relationship between belts of thick sediments and subsequent mountain folding, merely because one preceded the other." However, sediments do accumulate rapidly along mountainous coasts, and coastlines have been subjected to repeated crumplings, and hence the two types of phenomena are generally associated.[12] Blackwelder thus described the basic distinction between the two views of geosynclines developed much earlier by Dana and Hall.

An extension of the basic concept of the geosyncline was provided in 1919 by Amadeus W. Grabau (1870-1946), American geologist and palaeontologist. He opted for the American version of the geosyncline, describing it as a belt of concurrent deposition and subsidence parallel to old land. After examining a number of geosynclines, he concluded that after the strata becomes folded in a geosyncline and has been elevated into mountains, a new geosyncline appears, parallel to the older one and within the region of the old land which furnished sediments for the previous geosyncline. Thus, the whole process might be described as a geosynclinal migration toward the old land, the source of

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[12] Eliot Blackwelder, "A Summary of the Orogenic Epochs in the Geologic History of North America," Journal of Geology, XXII (1914), 653.

the sediments.[13]

In 1922 Charles Schuchert delivered a presidential address to the Geological Society of America in which he gave a comprehensive review of the status of the geosynclinal concept. In America, he related, there are two basic kinds of geosynclines, the first having comparatively short and uncomplicated histories like the Acadian and St. Lawrence troughs, and the other being exemplified by the Appalachian and Cordilleran geosynclines which have undergone exceedingly long and complicated growth patterns. Furthermore, both types, never having been a part of the ocean, differ from the typical European trough called a "mediterranean" which lies between two continents.[14]

Contrary to Hall's assumption that subsidence of the trough results from deposition of sediments, Schuchert asserted that another cause must have been "orogenetically connected with it." In the case of the Appalachian geosyncline, it was a nearby, very mobile, progressively rising borderland known as Appalachia, which defined the borders of the geosyncline to the west, and provided sediments to it.[15]

After presenting his basic conception of the

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[13]Amadeus W. Grabau, "Migration of Geosynclines, [Abstract]," Bulletin of the Geological Society of America, XXX (1919), 87.

[14]Charles Schuchert, "Sites and Nature of the North American Geosynclines," Bulletin of the Geological Society of America, XXXIV (1923), 157-58.

[15]*Ibid.*, p. 158.

geosyncline, which combined the ideas of Hall and Dana, and specifically excluding the influence of the European view of the geosyncline described by Emile Haug,[16] Schuchert set forth in some detail the geologic history of the North American continent using the geosynclinal concept as the basis for his description. He noted that there are four primary geosynclinal areas on the continent and that all subsequent geosynclines have developed from these earlier basic developments. The best known of the four is the Appalachian, located in the eastern part of the continent, and the oldest and most active is the Cordilleran, located in the western half of the continent. The others are the Arctic Franklinian trough and the smaller, less notable Acadian geosyncline that appeared between the border land, Novascotia, and the New Brunswick anticline.[17]

Schuchert took part in further defining the concept, providing a series of definitions for different categories of geosynclines. The first of these four types he called "monogeosynclines," defined as long, comparatively narrow, deeply subsiding troughs, exhibiting at all times typical shallow-water sediments. This type, described originally by Hall and Dana, gives rise to only one synclinorium and may be typified by the Appalachian geosyncline. A more complicated structure may be called a "polygeosyncline" to designate structures of greater extent, longer endurance,

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[16] *Ibid.*, pp. 194-95. [17] *Ibid.*, pp. 205-06.

and which give rise to one or more parallel geanticlines and two or more subsequent geosynclines. The classic American example is the Cordilleran primary trough, from which have emerged the Pacific and Rocky mountain geosynclines. The "mesogeosyncline" or "mediterranean" is that type described by Haug and reflects the typical European view of the concept. The trough in this case is most often located between two continents, is generally characterized by abyssal waters and excessive mobility, and has a rather complicated history. A typical example of this cateogry is the Roman mediterranean. The fourth and final type of such structure that Schuchert included is the ocean, which, though being the largest subsiding area of all, nevertheless should not, he said, be called geosynclinal.[18]

In the conclusions to his essay, Schuchert speculated on the types and causes of diastrophism, postulating that both contractional and isostatic movements become involved in crustal deformations. "The isostatic oscillatory movements [act] in compensation for transfer of load from one place to another; areas of sedimentation tend to sink and eroding ones rise," he wrote. "Isostasy is an important cause of crustal movement, but is of secondary import to those produced by earth shrinkage." Schuchert seemed positive that the earth, regardless of its origin, is a contracting, spherical mass. This, he said, is

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[18] Ibid., pp. 195-200.

demonstrated by the folding of the surface into mountain ranges and by the subsidence of the ocean basins. Crustal shortening he attributed partially to the loss of internal heat, alteration of magma, and the expulsion of lava, water, etc., but most of all to the "molecular rearrangement of the centrosphere" caused by continuous attraction pressure. "With Kober," continued Schuchert, "we therefore say that 'shrinking of the earth is no longer hypothesis nor theory, on the contrary it is knowledge built on ascertained facts.'"[19]

The typical American description of a geosyncline appeared in a study by Robert T. Chamberlin (1881-1948) in 1924. His interpretation included a subsiding trough parallel to high mountain ranges which furnish the sediments, examples of which are the Pacific depressions in the vicinity of the Aleutian, Japanese, and Philippine Island chains. Furthermore, these downwarped or downfaulted troughs, together with bordering areas of uplift, exhibit a geanticlinal-geosynclinal pair. He also cited the Appalachian chain as the typical example of a geosynclinal area.[20] Chamberlin likewise expounded the view that the present framework of oceans and continents has existed from a very early geologic time, expressing a view much like Dana's of

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[19]*Ibid.*, p. 212.

[20]Robert T. Chamberlin, "The Significance of the Framework of the Continents," *Journal of Geology*, XXXII (1924), 568-70.

a half century earlier. This continental framework, he asserted, is an outgrowth of special conditions that existed in the geosynclinal zones that bordered the early mountain ranges. These developed into weak belts of strata which later yielded to lateral stresses and permitted extensive folding.[21]

An hypothesis that has gained many adherents in the twentieth century is the idea of continental displacement which Alfred Lothar Wegener (1880-1930), a German geologist, publicized during the early part of the century. Denying that continents are confined to specific or even general areas of the earth's surface, as Dana, Chamberlin, and others had asserted, Wegener postulated an early land mass which contained all the land areas that are now spread around the globe. The displacement theory, as Wegener called it, assumes great horizontal drifting movements of continental-sized blocks during much of geological history and which perhaps continue even today. These continental blocks "with a thickness of about 100 km. swim in a magma out [of] which they only project about 5 km." Presumably this magmatic area is uncovered on the floor of the oceans. The displacement of the continental blocks and the resistant floors of the oceans, acting in concert, have created the lateral pressure necessary to elevate the mountain ranges of the world. For instance, the mighty range of the Andes

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[21] Ibid., p. 572.

is, according to Wegener, "a result of the opposition of the ancient, well-cooled and therefore resistant floor of the Pacific."[22]

Though Wegener found the contractional theory unnecessary in his system, the concepts of the geosyncline and of isostasy seem to have been accepted and useful. He argued that isostatic adjustment in the crust lags because the magma of the underlying shell is extremely viscous and that isostasy on a small scale consequently loses some of its validity, but when dealing with large blocks such as a complete continent, "isostasy must be assumed without question." Furthermore, he noted, "this doctrine of isostasy, the flotation of the crust of the earth, has been confirmed to such an extent by experiments, especially those of gravity, that it belongs to-day to the firmest foundations of geophysical knowledge."[23] Wegener thus seemed to find a close concurrence between his concept of continental drift and that of isostasy.

The concept of the geosyncline did not seem to fit into Wegener's doctrine as well as isostasy did, although he did not eliminate it from consideration altogether. Noting the generalization that the thickness of sediments in folded mountain chains is always greater than in other

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[22]Alfred Lothar Wegener, The Origin of Continents and Oceans, trans. J. G. A. Skerl (London: Methuen & Co., 1924), pp. 1-4.

[23]Ibid., pp. 22-25.

areas had been found to be "universally true," he questioned why this condition exists. Such regions, known as geosynclinals or basins, are favored for folding for a variety of reasons, he concluded. They probably contained a higher than usual proportion of "sima," rocks such as basalt in which silicon and magnesium are the main constituents, a material tending to be more plastic than the other primary component of the crust, "sial" which is mainly silicon and aluminum. Again, the sial crust may have a lesser total thickness, and thus has less power of resistance to folding. Finally, it may be that during deposition the primitive rocks are forced down further into regions of higher temperature and therefore become more plastic.[24] Whatever the reason for this type of phenomena, the concept of the geosyncline, if not forming a perfect complement to Wegener's continental displacement theory, had attained a stature that led to its inclusion as a component of his larger theory.

Schuchert made effective use of the concept of the geosyncline in a denial of Wegener's hypothesis. Among other reasons he cited for his disbelief in continental displacement, Schuchert called attention to the Franciscan geosyncline of eastern Brazil, a trough that lies in a generally west-southwest, east-northeast orientation. If the

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[24] Ibid., pp. 36, 163-64. Wegener or the translator has indicated that Sir James Hall was the originator of the basic concept of the geosyncline, but from the description given, the man most likely being referred to was James Hall of New York.

Wegener contention that South America was at one time either attached to the African continent or lay next to it is valid, then one should be able to find a continuation of the trough in western Nigeria. But when the geology of the two areas is examined, one finds that the Franciscan geosyncline, which dates from about early Silurian times, would abut directly on a section of Africa containing strata which date from much earlier times, probably from the Proterozoic age. No continuation of this South American trough can be found in Africa where it should be according to the Wegener hypothesis.[25]

In 1933 the concept of the geosyncline was used to describe the geology of the strata underlying the northern part of the Gulf of Mexico. The formations exposed at the surface are known to dip toward the gulf, and extrapolation of information taken from drilling operations in the area indicates that the basement of the trough is at a depth of 30,000 feet or more. The trough line of the geosyncline appears to be in the vicinity of the present coast line, and this seems to be verified by gravitational calculations conducted in the area. The extent of this geosyncline and its characteristics indicate that it can be compared to the

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[25]Charles Schuchert, "The Hypothesis of Continental Displacement," Annual Report of the Board of Regents of the Smithsonian Institution Showing the Operations, Expenditures, and Condition of the Institution for the Year Ending June 30, 1928 (Washington: Government Printing Office, 1929), pp. 265-66.

Appalachian geosyncline.[26] However, the region in which this trough is located exhibits some puzzling characteristics when examined isostatically. Subsidence and sedimentation seem to have proceeded apace during most of the period of its development, and the rate of sedimentation seems to have been largely independent of the downwarping of the trough. The authors of the study concluded from their evidence that the subsidence seems to have been the result of and compensation for sedimentation. But because the unconsolidated sediments have less density than the underlying strata, the subsidence was not likely a movement toward isostatic equilibrium, and the yielding of the basement under the extra load of sediments seems to have taken place with no regard to the isostatic principle.[27] However, in spite of the rather confusing situation these authors found in that area, their presentation is further evidence of the close relationship of the concepts of the geosyncline and of isostasy in twentieth-century geological theory, just as they had during their earlier period of development.

In an essay on the progress of American geological science in the last half of the nineteenth century, Bailey Willis, after first describing how Hall's subsidence concept developed during that period, told of some of the important

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[26]Donald C. Barton, C. H. Ritz, and Maude Hickey, "Gulf Coast Geosyncline," Bulletin of the American Association of Petroleum Geologists, XVII (1933), 1446-47.

[27]*Ibid.*, p. 1458.

developments that grew around it in the years that followed. One phase of that concept that Willis said had never been settled involved an unknown factor, the strength of the earth's crust and how much of a load it will sustain. Does the loading of a subsiding trough cause the subsidence or does some other force create the depression that subsequently fills with sediments? Willis noted that Dutton had been led to postulate the idea of hydrostatic balance in the earth's crust, a concept that gained the name of isostasy. Accordingly, an unloading continent should rise at the same time that a depression being loaded with sediments should subside, and all the while there is a subterranean movement of material from the latter area to the former.[28]

Two schools of thought developed on this question of the strength of the crust. The weak-crust group, who thought the crust remains at all times in a state of almost perfect isostatic balance, was represented by J. F. Hayford, William Bowie, and R. A. Daly. The opposing school concluded that the crustal zone had a degree of strength that enabled it to sustain unequal loads such as mountain masses, and therefore, isostasy is never completely achieved. Representing the latter group were G. K. Gilbert and Joseph Barrell. Willis tended to side with the latter group, saying that research had tended to confirm the view, although many geologists still believed in a crustal condition that

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[28]Bailey Willis, "American Geology, 1850-1900," Science, new series, XCVI (1942), 169-70.

is essentially isostatic. He concluded that none of the hypotheses put forward concerning crustal movements have been very satisfactory. These include Dana's contractional theory, Hall's gravitational concept, the thermal theory of T. Mellard Reade, and an hypothesis Willis called the "subterranean drag and push" movement, a process that tends to restore isostasy. Consequently, he asserted that American geologists had become generally agnostic concerning mountain-making forces.[29]

In 1947 M. F. Glaessner and Curt Teichert collaborated on an article in which they described the main features of the historical development of the geosynclinal concept, emphasizing in particular the twentieth-century phase of its history. Firstly, they noted that the concept of the geosyncline has become an integral part of modern geological theory and terminology. Various individuals, they said, had half-heartedly attempted to eliminate the geosyncline from geological terminology or to substitute something else for it, but with little success. Secondly, the concept as established by its originators does not differ greatly from that used by modern writers. The original concept, they asserted, is broad enough in scope and founded on solid enough foundations to permit later development and interpretation. "It cannot be discarded," they said, "on the grounds of later misuses or shifts in meaning of the

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[29] *Ibid*., p. 170.

original term; we have seen, in fact, that the germs of practically all later developments are already contained in Hall's and Dana's papers on the subject."[30]

The following year Adolph Knopf (b. 1882), then president of the Geological Society of America, addressed that group using the geosynclinal theory as a topic. He concentrated on the twentieth-century phase also, describing how the theory had been significantly broadened since introduced by Hall and Dana. Generally speaking, Knopf seemed impressed by the strength, scope, and persistence of the concept, and said "it constitutes a great--probably one of the greatest--unifying principles in geological science."[31]

Knopf noted a number of additions that had been made to the concept, involving volcanism and igneous intrusions during the initial growth of the geosyncline, isostatic control during the folding process, metamorphism as a result of geosynclinal conditions, and metalliferous deposition in connection with igneous activity during orogenic revolutions.[32] In referring to these additions, Knopf rightly used the word "involving," for upon examination of the

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[30] M. F. Glaessner and Curt Teichert, "Geosynclines: A Fundamental Concept in Geology," American Journal of Science, CCXLV (1947), 585. The authors called these conclusions and impressions "facts," a rather imprecise use of the term.

[31] Adolph Knopf, "The Geosynclinal Theory," Bulletin of the Geological Society of America, LIX (1948), 651.

[32] Ibid.

concept as developed by Hall and Dana, each of these topics had been included at one time or another with the exception of that concerning metalliferous deposits. Volcanism, metamorphism, igneous intrusions, and a basic expression of isostasy had all been considered.[33]

During the past two or three decades several studies have appeared dealing specifically with the concept of the geosyncline and its elaboration. Notable among these is that by G. Marshall Kay (b. 1904), Columbia University geologist, who examined the geological history of the North American continent in terms of geosynclinal developments.[34] A major examination of the entire concept of the geosyncline and its place in geology appeared more recently in the study by Jean Aubouin.[35] In each of these, a marked proliferation of terminology concerning the geosyncline has emerged, and it is doubtful that the originators of the basic concept would recognize what their creation has turned into.

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[33]James Hall, Palaeontology: Containing Descriptions and Figures of the Organic Remains of the Lower Helderberg Group and the Oriskany Sandstone, 1855-1859, Vol. III, Part I, Palaeontology of New York (Albany: State of New York, 1859), pp. 73-80, 87-88, 95-96; James D. Dana, "On Some Results of the Earth's Contraction from Cooling," American Journal of Science, VI (1873), 13-14, 114-15; Dana, Manual of Geology: Treating of the Principles of the Science with Special Reference to American Geological History (4th ed.; New York: American Book Co., 1896), pp. 365, 379, 392.

[34]G. Marshall Kay, "North American Geosynclines," Geological Society of America, Memoir, XLVIII (1951), 1-143.

[35]Aubouin, Developments in Geotectonics, . . .

However, this phase of the evolution of the concept is beyond the scope of this study, and it has been mentioned merely to establish the persistence of the geosynclinal concept to the present.

CHAPTER VIII

CONCLUSIONS

In the middle decades of the nineteenth century, there were two basic philosophies of geological theory, the catastrophist and the uniformitarian. Although neither orientation dominated geological thought, the spectacular nature of geological phenomena led many geologists to accept with little question the general view that only through a series of catastrophic events could the crust of the earth have become shaped as it now appears. Furthermore, the forces which shaped the surface of the earth operated with a much greater intensity in the past than now. Proponents of catastrophism were numerous, including Peter Simon Pallas, Léonce Élie de Beaumont, Roderick Impey Murchison, and in America, James D. Dana.

In sharp contrast to the catastrophist view, the uniformitarians assumed that the present configuration of the earth's surface is explainable solely in terms of geologic processes now in operation. No reference need be made to any sort of catastrophic event, Biblical or otherwise, and furthermore, the forces presently at work sculpturing the surface features are of the same magnitude as they have been throughout all geological history. Uniformitarianism was most ably presented by Charles Lyell in the various editions of his Principles of Geology, and in America by James Hall.

Like so many scientific concepts, the geosyncline did not appear in a sharply defined form at its inception, nor did it originate with the work of a single individual. Instead, it experienced a gradual growth and development, and a number of individuals made contributions to it. Charles Babbage and John F. W. Herschel provided the earliest expression of ideas suggestive of the geosyncline. Each postulated a relationship between the deposition of large quantities of sediments, subsidence, and consequent uplift of mountains. Hall, although not using the term geosyncline, gave the most definitive expression of the concept during the early years of its life. His philosophy of geology reflected Lyell's uniformitarian principles, and from Lyell's writings, Hall drew many of the elemental ideas of his subsidence concept. Lyell's views on the role of ocean currents, on metamorphism, and on igneous and volcanic actions contributed much to Hall's discussions on these topics, but he also acknowledged a number of other contributors including T. Sterry Hunt, William Hopkins, and William W. Mather. As sources of his view on elevation and subsidence, Hall gave generous credit to Babbage, Herschel, Charles Darwin, and Francois Dominique Reynaud de Montlosier. Out of these elements Hall created the concept which later became known as the geosyncline.

From the beginning, Hall's new concept did not stand alone, and only as part of broader theories of mountain formation did it survive. After a reception that can be described as something less than enthusiastic, Hall's new concept gained acceptance during the 1870's, and from then on its future seemed assured. Hall's philosophy of geology was closely aligned to Lyell's uniformitarianism, but when his concept of subsidence was first included in a broader geological theory, it appeared in Dana's version of the contractional hypothesis which had strong catastrophist overtones. Dana also gave the concept a new title, first in the form of "geosynclinal" and later the now-familiar "geosyncline." With Dana's application, however, the basic concept became altered considerably from that which it had been under Hall's sponsorship. Whereas Hall had assumed that sedimentation and subsidence proceed concurrently, with the weight of the sediments causing the subsidence of the geosynclinal trough, Dana reversed this relationship. Weight could not be the motive force for the subsidence, as it was a "physical impossibility." Instead, contraction of the crust of the earth due to a secular loss of heat activates the subsidence of the geosynclinal trough. To Dana, sedimentation cannot precede but necessarily follows subsidence, nor does it cause downwarping of the crust.

Joseph LeConte agreed with Dana, postulating that contraction of the crust alone is the ultimate cause of subsidence and the formation of the geosynclinal trough. However, he did admit that subsidence can proceed pari passu with deposition of sediments. LeConte, responding to critics of the contractional hypothesis who asserted that secular loss of heat cannot possibly produce the amount of shrinkage indicated by the phenomena, parried the issue by postulating unknown forces other than secular loss of heat to produce contraction. Hunt, although the staunchest admirer of Hall's subsidence concept, likewise chose to insert the geosyncline into the framework of the contractional theory.

Isostasy, the principle that the materials of the earth seek some sort of hydrostatic balance, provided another means for the survival and growth of the idea of the geosyncline. Clarence E. Dutton revealed the concept of isostasy in 1889, having laid the groundwork for it in a series of essays between 1871 and 1889. He was dissatisfied with contemporary explanations of mountain-building processes, especially the contractional hypothesis which he criticized in the strongest possible language. Dutton's concept provided an even more accomodating base for the geosyncline than the contractional theory, and indeed the geosyncline and isostasy seem to have had common roots.

Both Hall and Dutton acknowledged the fertile speculations of Babbage and Herschel on the processes of elevation and subsidence as sources for their own concepts. Hall postulated a sort of isostatic equilibrium as a part of his subsidence hypothesis. This can be seen clearly in his assumption that the weight added by sedimentary deposits causes subsidence of the crust at one point and a compensatory elevation at another location nearby. Furthermore, Dutton's listing of the principles basic to isostasy includes several of the basic assumptions of Hall's subsidence concept. It was the concept of subsidence as enunciated by Hall that became associated with isostasy rather than the radically altered view of the geosyncline expressed by Dana and LeConte.

A third group of geologists, notably G. K. Gilbert and Robert S. Woodward, asserted that the contractional hypothesis and isostasy were not contradictory as many geologists had contended. Isostasy had become a viable theory to them, but they asserted that if left to itself, isostasy would soon lead to a state of equilibrium in the earth's crust, a condition contrary to geological data. Therefore, while isostasy tended continuously to produce a state of equilibrium, contractional forces mitigated against attainment of that state by producing tangential pressures that upset the isostatic balance. Both Gilbert and Woodward included the geosyncline as part of their hypotheses.

In Europe, near the end of the nineteenth century, the concept of the geosyncline experienced further alteration when it became included in various explanations of mountain-building processes. T. Mellard Reade revived and altered the Herschel-Babbage thesis, extended it, and included the geosyncline in a comprehensive system of mountain building based neither on the contractional hypothesis nor on isostasy, although approaching much closer to the latter. Marcel Bertrand and Emile Haug further altered Hall's hypothesis into what is frequently called the European version of the geosyncline. Whereas practically every definition of the geosyncline in America assumed shallow-water sedimentation, based on examination of the characteristic deposits of the Appalachian range, Bertrand and Haug, looking to the European Alps as their example, asserted that the geosynclinal troughs are located in relatively deep water and their deposits are bathyl rather than littoral as with the American view. This divergence of the American and European views has continued to the present.

Thus the concept of the geosyncline, after lying relatively dormant for a decade and a half after Hall first made his views public, became altered into several forms, was incorporated into several broader mountain-building theories, and has survived to the present as a major concept of orogeny. In the twentieth century the geosyncline has been further altered and enormously expanded, although in many of its presentations it retains much of the basic structure it had when first enunciated by Hall. The concept has received high praise in recent decades, being described as one of the most effective unifying concepts of contemporary geological science.

The concept of the geosyncline seems to have been largely a creation of American geologists, among whom Hall, Hunt, Dana, Dutton, and LeConte are most important. However, many of the basic assumptions used by this group originated in Europe, notably with Lyell, Babbage, and Herschel, and their ideas in turn had roots in German, French, and English scientific literature. After the geosyncline became known in Europe, French and English geologists included the new concept in their own views, but they altered the concept considerably. By and large, however, the original concept of the geosyncline was an American phenomenon.

The idea of the geosyncline provides an example of how a new concept of science comes into being, how it grows, and how it becomes interrelated with other concepts. Seldom is one individual solely responsible for the creation of a new theory, although one individual frequently has a greater role than others. In this case James Hall was the most significant of those involved in the growth of the concept, but he drew heavily on the ideas of others. Seldom does a concept emerge initially in its most useful form, and in this case it was altered and expanded, gaining credibility and utility in the process. Seldom does a concept stand alone but becomes associated with other theoretical structures, and the geosyncline, in its relationships with the contractional hypothesis and isostasy, fits this generalization nicely. Thus the historical development of the geosynclinal concept illustrates exceedingly well a typical growth pattern of a significant idea of science.

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