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THE HOOKE IMPRINT ON THE HUTTONIAN THEORY

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ABSTRACT. Robert Hooke's system of the Earth as presented in his Cutlerian lectures, published posthumously as *Discourse of Earthquakes* in 1705, is almost identical to the theory James Hutton announced to the Royal Society of Edinburgh in 1785. This similarity had been noted by Gordon L. Davies (1964). The similarity, however, is not by happenstance. That Hutton was thoroughly aware of Hooke's writings is shown not only by the extent to which the intelligentsia of the 18th century cited, quoted, and adopted Hooke's ideas but also by Hutton's own text in both his *Abstract* of 1785 and his *Theory of the Earth* of 1788. In the few places where Hutton disagreed with Hooke, Hutton's style became polemical. He seemed to argue against specific points originated by Hooke, which then act as a Hookian signature on the Huttonian Theory. Hooke's influence on the development of geological thought and especially in the foundation of the pre-continental-drift paradigm was a significant one.

INTRODUCTION

Most geologists and historians of science credit James Hutton (1726-1797) with the initial establishment of a working modern hypothesis in geology to which all geologists ascribed until the advent of seafloor spreading and plate tectonics. But the progress of scientific development is rarely due to the inspiration of one great personality. Actually the foundation of modern geology was firmly laid in the 17th century. While the Dane Nicolaus Stensen (1638-1686), better known as Steno, is generally recognized as a founder of geology, the geological work of Robert Hooke (1635-1703), more extensive and more profound than Steno's, has not gained the recognition it deserves.[1] My purpose is to show that Hooke's geological writings were not forgotten as some histoians claim (Lyell, 1830; Carozzi, 1970; Turner, 1974) but were read, quoted, cited, and adopted by later writers. In particular, they had a direct influence on Hutton. The similarity between Hooke's system of the Earth and the Huttonian Theory as recognized by Davies (1964) is not a coincidence.

HOOKE'S SYSTEM OF THE EARTH

Hooke's ideas on geology were mostly published posthumously as *Discourse of Earthquakes* (1705), composed largely of the Cutlerian lectures he read to the Royal Society of London over a period of more than 30 yrs. Hooke's original emphasis in his writings was on the organic origin of fossils. References to this subject appear as early as 1664 and most notably in his famous book *Micrographia* (1665). While the idea that fossils have an organic origin was not original with him (earlier scholars, for example, Leonardo da Vinci, had similar thoughts on the subject), Hooke and Steno stood out from their contemporaries in this issue. The prevailing belief was that fossils somehow materialized in situ through some "plastick vertue." Much ahead of his time, in dis-

[1] The comparison of the geological contributions of Steno and Hooke is the subject of another article (Drake, in press).

cussing the various types of fossils Hooke also speculated on the evolution of species and their generation and extinction, saying (1705, p. 291) that "there have been many other Species of Creatures in former Ages, of which we can find none at present, . . . and that there may be divers new kinds now, which have not been from the beginning."

In presenting the various ways dead organic material could be laid down in water, buried in sediment, and then turn into stone as the sediment becomes consolidated and raised as dry land, Hooke developed a clear concept of the cyclic nature of the dynamic processes that alter the surface features of the Earth. He was possibly the first actualist (rather than uniformitarian) since Classsical times. He described cycles of deposition and erosion, subsidence and uplift. He attributed mountain-building to earthquakes by which term he meant any kind of earth movement — violent at times or at others progressing slowly "by degrees." He wrote (1705, p. 313), "All things almost circulate and have their vicissitudes," and "Nor are these Changes now only, but they have in all probability been of as long standing as the World."[2]

He then placed these dynamic processes in a universal context taking into account the motion of the Earth around the sun in the plane of the ecliptic and the tilt of the axis of diurnal rotation. He described polar wandering and a possible shift in the Earth's center of gravity that could cause earth movements and changes in the "magnetical Power and Vertue of the Body of the Earth." The term isostatic compensation was unknown to Hooke, but he had a clear idea of its action.

Hooke was also in advance of his age in his attitude toward the age of the Earth. Although his concept of time did not extend to the millions and billions of years of the modern geological time-scale, he refused to be bounded by the Scriptural chronology and claimed a much longer time-scale than his contemporaries, including Steno, would allow. In many respects, therefore, Hooke in the 17th century delineated what came to be the pre-continental-drift geological paradigm.[3]

HUTTON'S AWARENESS OF HOOKE'S SYSTEM

Without actually ascribing any influence on Hutton from Hooke, Gordon L. Davies (1964) stated that "by 1668 Hooke had formulated the shadowy outline of a theory of the earth that is almost identical with the theory that James Hutton presented to the Royal Society of Edinburgh in 1785. Hooke deserves to be remembered as a precursor

[2] Clearly, Hooke's concept of the term *earthquake* has been misinterpreted by some historians; Richard Westfall (1972), for example, labeled Hooke "the first catastrophist."

[3] The term "pre-continental-drift paradigm" as used here refers simply to the working hypothesis of geologists before the acceptance of drift theory. It does not imply that many of the important concepts incorporated in pre-drift geology were not carried over into the plate-tectonics paradigm. Although the emphasis on vertical displacements in geology eventually shifted when large-scale horizontal movements became accepted, such important ideas as uniformity live on and are as much a part of the new paradigm as of the old. Kuhnian interpretations, therefore, are not specifically implied in the use of this term.

of Hutton." D. R. Oldroyd (1972) interprets Davies as asserting that "in a number of instances Hooke should receive the credit for ideas which are usually believed to have originated in the work of James Hutton." The suggestion that Hutton was familiar with Hooke's hypotheses is compelling.

Hutton made no reference to the writings of other workers in his 1785 *Abstract* and only a few in his *Theory of the Earth* communicated to the Royal Society of Edinburgh in the same year but published in 1788. E. B. Bailey (1967) excuses Hutton by saying, "This might give the impression that he was ungenerous; but it was probably largely due to ignorance of what others had written up to date."

It is almost certain, however, that Hutton was aware of Hooke's writings in geology. The spread of geological knowledge in the 18th century was much more extensive than some historians have suggested (Eyles, 1969; Porter, 1977). Furthermore, Playfair (1803) tells us that Hutton had "carefully perused almost every book of travels from which anything was to be learned concerning the natural history of the earth."

Nor does the argument that Hooke was a forgotten author in Hutton's days seem valid. A number of well-known naturalists wrote about Hooke's ideas in the 18th century, some in the most matter-of-fact manner, thus giving the impression that Hooke was well known to naturalists for his hypotheses and needed no further explication. John Whitehurst (1786), for example, famous for his geologic work in Derbyshire, quoted freely from "Dr. Hooke's Post." all through the first part of his book before he finally identified the citation on p. 132 as "the posthumous work of the learned Doctor Robert Hooke."

Another contemporary, Rudolf Erich Raspe, was so convinced of the correctness of Hooke's ideas that he decided to publicize them and add corroborating data and observations. In 1763 he published *Specimen Historiae Naturalis Globi Terraquei, . . .* , displaying the words *Hookiana Telluris Hypothesi* prominently in the long title. On the strength of the publication of *Specimen,* Raspe was elected a Fellow of the Royal Society of London in 1769. This recognition shows that the Royal Society accepted the publication as significant and, more important, that Hooke's posthumous works were still alive.

Raspe, a colorful character, famous as the author of the Travels of Baron Münchausen, was not unimportant. He was among the first to recognize the significance of Desmarest's discovery of the volcanic origin of basalt in the Auvergne published in 1774 in the memoirs of the Académie des Sciences in Paris, which had been communicated to the Académie in 1765. Raspe immediately saw that the basalts in the vicinity of Kassel were also of volcanic origin and communicated his idea to the Royal Society of London in a letter which was read on February 8, 1770. In 1774 he published his findings and interpreted the Habichtswald as the remains of an ancient volcano. This work was hailed by no less a personage than Goethe as epochal because it introduced the idea of the

volcanic orgin of basalt in Germany(Raspe, 1771, 1774; Iverseu and Carozzi, 1970).

The story of the versatile Raspe's subsequent disgrace, arrest, and escape from jail, expulsion from the Royal Society, and ingenious restoration of reputation is a fascinating page in the history of science, and the reader is referred to other accounts (Hallo, 1934; Carswell, 1950; Iversen and Carozzi, 1970). The relevant fact for us here is that during Raspe's social re-instatment, in 1787, he was introduced to the high society of Edinburgh, which included James Hutton and his close friend Joseph Black. Raspe's publications, not to speak of his notoriety, must have been known to Hutton before his introduction to him in 1787, a year before the actual publication of his communication to the Royal Society of Edinburgh, *The Theory of the Earth.* Furthermore, while Hutton made scant mention of other workers in 1785, he did, in the later volumes of *The Theory,* cite the names of several writers including that of Raspe.

Hutton could hardly have missed Raspe's *Specimen,* even before 1785, if he was as thorough in his reading as Playfair claimed he was. Raspe, who was a first-class linguist, fluent in Latin, Italian, German, French, and English, had translated the travels and observations of some important continental naturalists, and these translations, with an abundance of notes, introductions, and prefaces by the translator, were published under the aegis of the Royal Society of London. That Raspe managed to secure the publication arrangements in spite of his expulsion from that Society is a testimony to his ingenuity as well as to the eagerness of the Society for "Englishing" important foreign publications.

My point is, if we can believe Playfair, and there is no reason to disbelieve him, that Hutton had indeed "carefully perused almost every book of travels from which anything was to be learned concerning the natural history of the earth," he could hardly have missed the very books of travels translated by Raspe and published by the prestigious Royal Society. One of the translations was of the travels through Italy by J. J. Ferber published in 1776, and another was of the travels of I. von Born through the Bannat of Temeswar, Transylvania, and Hungary published in 1777. In both books Raspe took the opportunity to review Hooke's ideas on earthquakes and volcanoes, albeit sometimes claiming them as his own, but referring to Hooke by name, nevertheless. Could a scholar like Hutton not have consulted the original publication of Hooke to verify the claims of Raspe? The publication dates of these translations were in advance of the 1785 publication date of Hutton's *Abstract* and his communication to the Royal Society of Edinburgh.

At least one other book of importance that could hardly have escaped Hutton's attention was published in London in 1757 entitled *The History and Philosophy of Earthquakes, from the Remotest to the Present Times: Collected From the best Writers on the Subject.* The authorship was given as "By a Member of the Royal Academy of Berlin," but the author was probably John Bevis, the well-known astronomer. The im-

mediate impetus for the publication of this collection was the great Lisbon Earthquake in 1755. Of 334 pages of text, this book devotes 106 to Hooke's *Discourse of Earthquakes.* Also, on the title page were two prominent quotations on the scientific method, one in Latin by Verulam from the *Novum Organum* and the other from "Dr. Hooke's *Method of improving Natural Philosophy.*" The juxtaposition of Hooke's words with Francis Bacon's again shows how highly Hooke was regarded in the 18th century.

Another line of evidence showing that Hutton could not have avoided knowing Hooke's writings through his contemporaries is that Hutton was in constant touch with members of the active Lunar Society of Birmingham, which promoted the importance of knowledge about the Earth to industrial, technological, agricultural, and transport developments. As a visiting geologist, Hutton was entertained by the Society members among whom were John Whitehurst and Rudolf Raspe. Roy Porter (1977) reports that the membership "kept up extensive correspondence with other devotees at home and abroad." Among other visiting geologists was J. J. Ferber whose very book on travels was translated by Raspe.

HUTTON'S KNOWLEDGE OF HOOKE'S IDEAS: EVIDENCE IN HIS OWN TEXT

Aside from such bibliographic evidence, Hutton's own writings show indeed not only that he was aware of Hooke's writings, but that he must have studied them very carefully. Hutton's style is a polemical one, and his arguments are directed against some of Hooke's specific lines of thinking, both in his *Abstract* of 1785 and his *Theory* of 1788. The two theories, Hooke's and Hutton's, are so similar that it is difficult to find where they differ, but where they do differ is precisely where Hutton becomes polemical.

It is fundamental to both Hooke's and Hutton's theses that marine fossils found in high places indicate that the rocks in which the fossils are imbedded were at one time loose sediment at the bottom of the sea. Hooke states (1705, p. 298),

> Many Parts which have been Sea are now Land, and others that have been Land are now Sea; many of the Mountains have been Vales, and the Vales Mountains, etc.

Hutton similarly states (1785, p. 5),

> The solid parts of the present land appear, in general, to have been composed of the productions of the sea. . . that, while the present land was forming at the bottom of the ocean, the former land maintained plants and animals.

Hooke has four species under the category of "raising of the superficial parts of the Earth above their former level"; these are the (1) raising of a part of a country that lay level with the sea, (2) raising of the bottom of the sea above the surface of the water, (3) raising of considerable mountains out of a plain and level country, and (4) raising of parts of the Earth by the "throwing on" of a great excess of new earth covering the former surface and adding to the thickness, or height, by many fathoms. Conversely, he also cites four ways of lowering the

level of the land. These are the (1) sinking of part of the land to form a lake, (2) sinking of land below the sealevel, (3) sinking of the sea bottom to form "vast Vorages and Abysses," and (4) uncovering of land by "throwing away" material as a result of "subterraneous Motion" or by washing away (1705, p. 298-299).

Hutton seems to go along with Hooke's four species of raising of the land but has difficulty with the converse action of sinking. He seems to be arguing with Hooke when he writes (1788, p. 265),

> the sinking the body of the former land into the solid globe, so as to swallow up the greater part of the ocean after it, if not a natural impossibility, would be at least a superfluous exertion of the power of nature. Such an operation as this would discover as little wisdom in the end elected, as in the means appropriated to that end; for, if the land be not wasted and worn away in the natural operations of the globe, why make such a convulsion in the world in order to renew the land? If, again, the land naturally decays, why employ so extraordinary a power, in order to hide a former continent of land, and puzzle man?

Hutton, therefore, favors denudation and lowering of the land by natural processes rather than by sinking in the different senses that Hooke expresses. Hutton must also be aware, however, that elsewhere in his discourses, Hooke takes pains to express at length the whole concept of erosion of the land by various agents such as water, wind, and ice.

To explain how loose sediment turns into rock, Hooke again has four causes (1705, p. 290): (1) some kind of "fiery Exhalations arising from subterraneous Eruptions or Earthquakes"—that is, heat and fusion; (2) "Saline Substance, whither working by Dissolution and Congelation, or Crystallization, or else by Precipitation and Coagulation"—that is, aqueous solution; (3) some glutinous or bituminous matter, "which upon growing dry or settling grows hard, and unites sandy bodies together into hard stone"; and (4) "a very long continuation of these Bodies under a great degree of Cold and Compression."

Hutton generally agrees (1788, p. 223) that,

> Besides an operation, by which the earth at the bottom of the sea should be converted into an elevated land, or placed high above the level of the ocean, there is required, in the operations of the globe, a consolidating power, by which the loose materials that had subsided from water, should be formed into masses of the most perfect solidity.

Instead of the four species of causes of consolidation that Hooke enumerated, however, Hutton would admit to but two possibilities, heat and fusion or aqueous solution, but he attacks the latter idea. He combines Hooke's second and fourth causes in the idea of aqueous solution, thus including, in Hutton's words (1788, p. 225), "congelation from a fluid state by means of cold."

In showing that water could indeed be "petrifying" Hooke uses the example of the formation of stalactites and stalagmites in subterraneous caverns of England (1705, p. 293):

> The water itself does, by degrees, produce several conical pendulous Bodies of Stone, shap'd and hanging like Icicles from the Roof of the Vault; and

> dropping on the bottom, it raises up also conical Spires, which, by degrees, endeavour to meet the former pendulous *Stiriae*.

Here, Hutton argues against Hooke, stating that water could only consolidate substances that are soluble in water, and (1785, p. 11) "having found strata consolidated with every species of substance, it is concluded, that strata in general have not been consolidated by means of aqueous solution." Then in his *Theory* (p. 229), he states,

> We have strata consolidated by calcareous spar, a thing perfectly distinguishable from the stalactical concretion of calcareous earth, in consequence of aqueous solution.

He is, therefore, referring specifically to Hooke's example of stalactite formation. If Hutton were simply presenting a new and original theory, he would not be compelled to argue against straw men of his own making. He could just state, "the loose sediment is consolidated by heat and fusion." It has been generally supposed that Hutton's attack against aqueous solution as a process of consolidation is an attack against neptunism. But at the time of the publication of his *Abstract* in 1785, neptunism versus plutonism had hardly become an issue. Werner was only 35 yrs old, although he had been lecturing on "Geognosy" for a few years. He had just finished writing, but not published, his "Kurze Klassifikation." His paper on the aqueous origin of basalt was not written until 1788.

R. H. Dott, Jr. (1969) reminds us that the issue neptunism versus plutonism predated both Hutton and Werner and therefore "the neptunism attacked by Hutton himself was largely pre-Wernerian." These arguments against aqueous solution are more likely simply directed against Hooke. Hutton is compacting Hooke's theory into a Huttonian theory and must throw out those parts of Hooke that to him are untenable.

Another specific point of contention between the two, which Hutton superfluously brings out, is Hooke's idea that there might have been shifts in the Earth's axis of rotation. The question is, and it is asked by Hutton (1788, p. 222): "How such continents, as we actually have upon the globe, could be erected above the level of the sea?" To Hooke, of course, earthquakes, with his broad definition of the word, including any movements of the terrestrial crust whether violently by faulting and volcanic eruptions or slowly and imperceptibly by degrees, are the general answers. These could have been caused, he conjectures, by major changes in the integrity of the earth movement itself in the planetary system. It is not impossible, Hooke asserts, since the position of the North Star is known not to have been constant through the ages, that there might have been shifts in the center of gravity of the Earth and the position of the axis of rotation. If there is a change in the axis of rotation, there would be a change in the distribution of land and sea, for there is, as Hooke says (1705, p. 347),

> a more than ordinary swelling or raising of the Sea in those Parts which are near the Aequinoctial, and a sinking and receeding of the Sea from those

> which are near the Poles; so that as any Parts do increase in their Latitudes, so will the Sea grow shallower, and as their Latitudes decrease, so must the Sea swell and grow high; by which means many submarine Regions must become dry Land, and many other Lands will be overflown by the Sea, . . .

Hutton's answer to Hooke across a century is that (1788, p. 222),

> no motion of the sea, caused by this earth revolving in the solar system, could bring about that end; for let us suppose the axis of the earth to be changed from the present poles, and placed in the equinoctial line, the consequence of this might, indeed, be the formation of a continent of land about each new pole, from whence the sea would run towards the new equator; but all the rest of the globe would remain an ocean. Some new points might be discovered, and others, which before appeared above the surface of the sea, would be sunk by the rising of the water; but, on the whole, land could only be gained substantially at the poles. Such a supposition as this, if applied to the present state of things, would be destitute of every support, as being incapable of explaining what appears.

Having demolished this thought with such finality, Hutton seems at a loss as to what to do with it since he must realize that no one except Hooke himself would debate this issue, so he weakly continues (1788, p. 223),

> But even allowing that, by the changed axis of the earth, or any other operation of the globe, as a planetary body revolving in the solar system, great continents of land could have been erected from the place of their formation, the bottom of the sea, and placed in a higher elevation, compared with the surface of that water, yet such a continent as this could not have continued stationary for many thousand years. . .

— without, he continues, also the process of consolidation of the loose sediment — a point that is not denied by Hooke. One wonders, then, why Hutton should bring up the whole idea of axis-shift, which is original with Hooke[4] and which is so superfluous to Hutton's general thesis that, having brought it up, he must retreat from it by sidestepping the issue. Furthermore, the idea of the Earth's axial displacement was without support from Hooke's contemporaries[5] and the idea was not prevalent in the 18th century. Why then, should Hutton refer to it, much less argue against it?

[4] Thomas Burnet, Hooke's contemporary, famous for his book *Sacred Theory of the Earth* (1691) also wrote of the tilt of the Earth's axis but in an entirely different context. Burnet's axis-tilt occurred as a result of God's wrath; in its paradisaical days the Earth had no tilt in its rotational axis, and therefore there were no seasons to complicate the lives of Man.

[5] According to Turner (1974), Edmond Halley, on February 15, 1687, sent an account of Hooke's theory of a non-spherical Earth and of axial displacement to John Wallis to be communicated to the Oxford Philosophical Society for discussion. Hooke had delivered his lectures on the subject to the Royal Society of London shortly before this date. Wallis wrote two letters to Halley dated the 4th of March and the 26th of April summarizing the reaction of the Oxford Society. The consensus was, "sure we are, that there is no evidence in history that ye top of ye Alps was ever sea; Except in Noah's Floud," and the notion of the Earth changing its axis "seems too extravagant for us to admit." For supportive evidence Wallis related that a "Dr. of Physick of good credit" showed the group what seemed the shell of a fish taken by the good doctor from the kidney of a woman. It is more likely, the Society concluded, that the fish-shell was formed there in the woman's kidney, "than that this kidney had once been sea."

DISCUSSION AND CONCLUSION

The evidence suggests that Hooke's hypotheses were so ingrained in Hutton's mind, because of his careful perusal of them, that he almost unconsciously brought up the Hookian points with which he disagreed — not because these were necessary to the Huttonian Theory, but because they obviously were disturbing to him and had to be disposed of. The parts of Hooke's system with which he agreed, which include practically everything else in his theory, especially the cyclic nature of sedimentation and denudation and including such concepts as the unconformity (Davies, 1964) credited to his originality by some writers (Dott, 1969), Hutton left intact or expanded with great skill, illustrating them with astute field observations in his later volumes.

It is a bit of historical irony that Hutton's debate with Hooke regarding the aqueous solution as one of Hooke's four processes of consolidation should have been inadvertently and fortuitously assumed by all who followed as an attack on neptunism. Hutton might have been more surprised than anyone that his argument with a man who lived a century before should have become the point around which the plutonists rallied their forces.

While Hutton's importance to science is readily admitted by scientists and historians alike, one wishes, nevertheless, that he could have given credit where it was due. Even if it were possible that he missed reading the publications like those of Raspe and Whitehurst, his contemporaries, in which Hooke was championed, quoted, or cited, the evidence is still clear that Hutton had read Hooke directly. The notion that axial displacement could be the cause of the exchange of land and sea areas originated with Hooke, and he apparently had no followers in it of consequence; even Raspe, Hooke's champion, had, in his *Specimen*, dismissed the idea as too philosophical and to be ignored. The idea itself, so vehemently denied by Hutton, therefore, acts like a Hooke signature stamped on the Huttonian Theory.

This article is not intended to be iconoclastic. Hutton is unquestionably an important figure in the history of science, even in the light of his contributions vis-à-vis Hooke's. Hooke was brilliant, original, and imaginative, but he threw out thousands of ideas from his fertile mind (many diasarmingly on target in the context of present knowledge) to challenge other natural philosophers to prove or disprove. Hooke served his purpose; Hutton refined Hooke's hypotheses to an elegant degree. Playfair and later Lyell eloquently publicized the theory to the world. The pre-continental-drift paradigm in modern geology certainly began its life then.

Hooke's contributions to the development of geology as a science, however, should not be forgotten, not only because his writings reveal the extent of pre-Hutton and pre-Werner geological knowledge, but also because they *were* transmitted by later writers and therefore demonstrate the continuity of the development of geological thought. The notion that there were the vague beginnings and then there was Hutton is simply not the case.

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