ORIGIN OF THE MOON AND ITS TOPOGRAPHY

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The purpose and scope of this paper are indicated in the summary at the end.

INTRODUCTION

GEOLOGY, a study of planetary evolution, is itself in evolution; the plunge of this comparatively young science into the unknown has brought many pearls of ascertained truth, but for every major principle established at least one new major problem has cropped out. Even new techniques have to be learned and applied. An outstanding illustration is found in the attempt to determine the amount and distribution of the earth's internal energy, particularly the heat. This fundamental question is forcing students of geological dynamics into the field of cosmogony. Whatever be the role of radioactivity in rocks, it is clear that the importance of primitive heat must be valued. Where there is so much darkness every ray of light is welcome, and it was with sure instinct that Eduard Suess and Thomas Chrowder Chamberlin a half-century ago sought new light from cosmogony and from comparison of earth and moon. These masters and others, later on, have been asking a multiple question: what relation have theories of the moon's origin to problems concerning: (1) terrestrial volcanism, igneous action in general; (2) the differentiation of sialic and simatic shells of the outer earth;[1] (3) the theory of isostasy in explanation of the earth's relief—a theory implying mechanical contrast between a strong crust (the lithosphere) and an immediately underlying weak layer or shell (asthenosphere); (4) theories of mountain-building; (5) the theory of the sealevel figure of the earth; and (6) the existence of continents standing shoulder-high above the floor of the deep ocean?

Fruitful comparison of planet and satellite has been greatly broadened and deepened by two recent developments: geophysics on the one hand and adequate photographic and other instrumental study of the lunar surface on the other. Some selenographers claim that the visible 59 per cent of the moon's surface is better mapped than the earth's surface as a whole. We now know of startling contrasts of topographic form in the two bodies—of remarkably few similarities among the details. Since Galileo's day many astronomers have been unduly stressing similarity of topographic forms and similarity of genesis of those forms. A large number of astronomers as well as some geologists have regarded most of the 30,000 visible pits on the lunar surface as volcanic vents whose energy resided in the heat and gas of the interior. Published in 1895, the classic paper by Grove Karl Gilbert, one of the keenest observers and most logical thinkers who have worked in earth science, contained a powerful argument for the very different impact or bombardment theory of the lunar pits, which nevertheless will probably long continue to bear the unlucky name "craters."

Thus today selenographers are divided into two groups. One emphasizes internal influences as shaping the rugged moon; the other emphasizes external influences. This difference of judgment is likely to persist until the origin of both earth and moon has been finally established. No thoroughly satisfactory theory of those beginnings and of the solar system in general is yet in sight. However, some astronomers and geophysicists have most satisfaction in assuming an initially gaseous state for the earth, and this idea seems to furnish the best basis for thought about the evolution of the earth-moon

[1] "Sial" is mnemonic for rock or rock-melt which is relatively rich in silica and alumina (plus alkalies). "Sima" is mnemonic for rock or rock-melt which is relatively rich in silica and magnesia (with little or no alkali). Sialic material, typified by granite, is less dense than simatic material, typified by basalt or peridotite.

system. The present paper is chiefly concerned with the hypothesis that some or all of the moon's material was torn out of the young earth. New variants of the fission theory as well as a capture hypothesis are to be described and then tested by the facts known about the composition, density, topography, and other characteristics of our satellite.

PUBLISHED IDEAS OF "ULTIMATE" ORIGIN

What was the earth like at its birth? No attempt will here be made to canvass all the possibilities suggested by students of this controversial subject, but a brief word about the latest ideas is not out of place.

Since the beginning of the present century cosmogonists have taken seriously the speculation that by the close approach of, or actual collision with, a visiting star, the sun, or possibly a companion of the sun, or all three of these, lost enough gaseous substance to build our planets and satellites. The speculation was at once adversely criticized on the ground that a close approach of a wandering star to the sun is extremely improbable, but this objection loses some of its force if, as reported, the stars of our galaxy were formerly much closer together than now and correspondingly more liable to mutual close approaches. What appears to be a more stubborn trouble has been indicated by Lyman Spitzer (1939). His reasoning, summarized by Whipple (1941: 237), is that gas torn out of the sun, in mass equal to our planets and satellites, would be so hot (compare Russell, 1935: 111) that it would explode and with such velocity of dispersal as to forbid condensation to planetary forms; the gas would be completely dissipated in the depths of space. However, Whipple suggests a possible way out of the difficulty: dissipating explosion would not occur if the solar disruption were relatively slow, and he believes it wise to retain the hypothesis of stellar disruption for our planetary system.

Two other modern explanations of that system may be mentioned. That of Nölke (1930, 1931) also demands a gaseous state and high temperature for each planet at its birth. The still more recent hypothesis of Weisäcker (see last footnote of the present paper) derives the planetary system from a cool nebula revolving around the sun, the nebular material being gravitatively segregated into many rotating, solid bodies. Even if Weisäcker's suggestion should be found to give the best solution to the long-standing problem, it does not preclude a gaseous state for a planet which was formed by the collision of two large masses segregated in the nebula.

A more complete listing of the many published cosmogonic schemes is not needed to enforce the conclusion that, according to the more stalwart of these hypotheses, each of the existing planets was initially hot enough to be gaseous. At any rate let us make this assumption, thus cutting the Gordian knot of uncertainties, and then with that assumption trace the evolution of the earth-moon system in a speculative way.

Because of radiative and adiabatic cooling the smaller planets condensed to liquid, rotating globes, each, through rapid molecular flight into space, having been reduced to a mass much smaller than it had when in the initial, gaseous state. In his book, *The Earth*, Jeffreys showed it to be doubtful that the moon could have come into being through condensation from a mass of stellar gas which had never been part of a larger, truly planetary mass. He supports the view of astronomers in general that the partnering of earth and moon demands a special explanation—one quite different from any that may prove to be true for the other satellites of the solar system.

The most elaborate attempt to account for our own satellite was made by George H. Darwin, using the results of his study of the moon's motions. He proved that, before it began its spiralling journey away from the earth (because of tidal friction), the centers of the two globes were no more than two or three earth-radii apart, the day then being about four hours long and a little shorter than the month. The nearness of the bodies was such that Darwin (1908: 455) thought it natural to suspect the moon to have been originally part of the earth. This suspicion of the great geophysicist is indeed one of the important reasons for the writing of the present paper.

Darwin took the angular momentum of the earth-moon system at the epoch of their separation to be the same as the total angular momentum at the present time, as it had to be if the system has kept constant mass and has not undergone dynamical interference from outside. With a four-hour period of rotation the earth was stable, even when liquid at the surface.[2]

[2] Darwin took no account of the high probability that the earth's iron core had already been separated from the silicate mantle at the epoch he was considering, but this omission is in principle not essential in itself for decision as to the possibility of fission by tidal resonance.

To eject the moon by centrifugal force alone the period of its rotation would have been no more than about three hours, but such rotational velocity could not be reconciled with the necessity for constancy of angular momentum belonging to the system.[3] Darwin thought that actual separation of the two bodies might have been brought about if the considerable rotational strain corresponding to a four-hour day were much increased by resonant upbuilding of the solar tides in the young, liquid earth. He supposed resonance to have added tidal amplitude to tidal amplitude in long succession until from the mounting bulge liquid silicate flew off and became by self-gravitation the globular moon—a separate body.

This brilliantly expounded hypothesis accounts well for the extraordinarily high mass ratio of moon to earth when compared with all other ratios of satellite to primary in the solar system; and also accounts well for the proved close approach of the infant moon's orbit to perfect circularity. Moreover, Jeffreys has shown how Darwin's essential idea might be a basis for explaining the mean density of the moon and the apparent fact that, unlike earth, Venus, and Mars, the moon has no iron core.

But there is an obvious and formidable difficulty: could the terrestrial liquid have mobility enough to give the required elongation at the equator? To produce the fission the tidal bulge, with height of more than four hundred miles, had to be built up and annulled every two hours. Recent experiments on organic liquids and on one hot glass (borate) show that with increase of all-sided pressure the friction opposing internal shear rises according to logarithmic laws, so that one must doubt sufficient mobility for the shells of the young earth. This doubt was strengthened when Jeffreys (1930: 169) made a quantitative study of the subject.

Darwin's hypothesis faces a second difficulty if his mathematical analysis be taken to prove an initial, center-to-center distance of only 9,000 miles between earth and separated lunar material (see Russell, 1935: 129). Darwin (1908: 359) pointed out that "if the moon were to revolve at a distance of less than 2.86 [earth-] radii, or 11,000 miles [Roche's limit], she would be torn to pieces by the earth's tidal force." He did not clearly indicate how the pieces could ever have been agglomerated to form the existing lunar globe.

Nölke (1931: 56) has offered a third objection: even if the crown of the exaggerated tide were torn off, this mass would have to fall back into the earth.

Although it is probable that the tide-resonance explanation of the moon must be discarded, Darwin's investigation is of permanent value, giving results as important as they are secure in principle. He found for us the amount and distribution of angular momentum in the earth-moon system.[4] As already observed, he discovered the clear fact that the two bodies were once close together, and he showed why they separated so far—at first rapidly and then with increasing slowness. He showed why we can see only 59 per cent of the moon's surface, and he firmly founded the premise on which Jeffreys (1929: 229) has so well explained the moon's elongation toward our planet. All these determinations are full of meaning for the moon problem.

What condition other than tidal resonance could have brought about the separation of much or all of the moon's substance from the young earth? Was the primary condition internal or external? To neither query can a definite answer now be given, but the writer, feeling keenly the importance of both for his own science of geology, offers herewith some speculations which may possibly engage the attention of better judges of their merits.

Manifestly such guesses must be founded on a general conception of the earth's beginning. Together with a stellar origin, initial gaseous state, and corresponding high temperature, it is assumed that the terrestrial gas-ball was chemically a mixture of stony-meteorite and iron-meteorite material with much hydrogen and helium; and that the initial mass was several times that of the existing earth-moon system, the excess having been lost by molecular flight into space and perhaps also by catastrophic explosion.

Jeffreys (1929: 35) has shown reason to think that the liquefaction of such a massive gas-ball would be guaranteed more by surface radiation than by adiabatic expansion and cooling. He supposes liquefaction to have been a straight-

[3] Ross Gunn (*Jour. Franklin Institute* **213**: 655, 1932) has recomputed the length of the day just before fission and found it to be 4.1 hours; he also concluded that the liquid earth would have been unstable if its day were 3.3 hours long.

[4] The revolving moon now carries 4.82 times the angular momentum of the rotating earth.

forward process, liquid droplets formed in the chilled gas falling to the center of the ball, there to accumulate until the whole became liquid except for an atmosphere of trivial mass. The droplets of iron, olivine, and other substances with high boiling-points were the first to appear and sink toward the center. Droplets of compounds with lower "points" for boiling were formed later and in general were endowed with densities smaller than those of the first crop of droplets. For both reasons it would seem inevitable that the planet, when wholly liquid, was stratified by intrinsic (chemically determined) density. This type of layering could hardly favor deep convection; yet Jeffreys assumes cooling by thermal convection and consequent pressure-crystallization of the whole silicate mantle, the solidification beginning at the bottom and rapidly extending to the surface. His theoretical scheme implies that, just before the moon became a revolving partner with the earth, the planet had at most only a slight excess of mass over its present mass; that the day was about four hours long; and that therefore the rotation of the earth was too slow for ejection of material ultimately constituting the moon as a coherent, independent body.

If that evolutionary scheme be adopted, the partnering of the moon must be ascribed to an agency external to the condensing but already liquefied earth. Such an agency will be suggested later, but beforehand it may not be amiss to speculate as to whether, because of an internal condition of the earth, the moon was torn out when the planet was still largely gaseous—that is, at a stage earlier than the one chosen by Darwin.

Whether the agency was external or internal, it will be supposed to have operated when the earth was surfaced with a thick layer of ultrabasic liquid (with composition much like that of the average achondritic meteorite), and thus at a time before the granitic and basaltic earth-shells were differentiated.

INTERNAL INCENTIVE TO FISSION

According to our basal assumptions the terrestrial gas-ball had at the beginning of its history the following characteristics: (1) a high proportion of hydrogen and helium, in which the materials of the silicate mantle and iron core of the existing earth (these having the composition of meteorites) were "diluted"; (2) a corresponding excess of mass; (3) internal temperature much higher than that due merely to the self-compression of the ball;[5] (4) rotation about an axis highly inclined to the plane of revolution around the sun. (Compare Daly, 1943.)

Without delay three major changes would be expected: dissipation by molecular flight of most of the hydrogen and helium into space; condensation of the residual gas; and formation of liquid droplets of metallic iron and silicate having high boiling-points, to be followed by droplets of silicates with lower boiling-points. To picture correctly the details of general condensation is beyond the writer's power, but it seems reasonable to suppose that there was a stage when a surface layer of liquid was in floating equilibrium on iron-rich and therefore denser material in the critical state, and this to the center of the ball.[6] Let us assume that the liquid layer was a few hundreds of kilometers in thickness, the mean radius of the inner spheroid of "supercritical gas" being about 8,000 kilometers and thus 1,600 kilometers greater than the mean radius of the earth at present. Let us suppose that, as a result of the condensation, the period of rotation had by now been shortened to about three hours. Darwin's mathematical analysis shows that the ball would be unstable, a large mass of the liquid being thrown off at some point in the equatorial zone. The sudden release of pressure on the supercritical gas in the same earth-sector would cause this material to flash into highly explosive gas. Once started, the explosion would lead to the evisceration of the whole equatorial zone.[7] The explosion would be likely to eject both liquid and gaseous substances at speeds above the escape velocity corresponding to the attraction of the original spheroid, and exceeding in still greater measure the escape velocity corresponding to the attraction of the residual planetary mass.

After the explosion the residual mass would assume the form of a stable spheroid because of equatorward flow of liquid from the polar sectors,

[5] Nölke (1931: 65) estimates the initial temperature at the center of the earth-ball at something like 50,000°C.

[6] At this stage we have an analogy with the pictures of the existing earth as drawn by Günther (1897: 358) and Arrhenius (1900).

[7] Nölke (1931: 65, 67) has suggested that under certain circumstances the cooling and condensation of a planetary gas-ball would not be a straightforward process dominated by radiation from a spheroidal surface but would be interrupted by catastrophic explosion of the body, with of course great, though temporary, acceleration of the cooling by both adiabatic and radiative means.

where the liquid had had low rotational velocity. Let it be assumed that, when equilibrium was established, the mass of the diminished earth had become about equal to the existing mass, that the day had been lengthened to four hours, and that the spheroid was still liquid at the surface. These conditions are those accepted by Darwin for the epoch when the moon was close to the earth.

Some individual masses of the liquid matter ejected during the explosion would be expected to have travelled with less than the escape velocity corresponding to the attraction of the original ball, but would not fall back and be absorbed by the diminished earth, with much smaller power of attraction. Such fragments would therefore remain in permanent revolution. The largest of the earth fragments would attract and in the course of time absorb smaller ones until an independent, revolving spheroid with the dimensions of the moon would be developed. On account of its small size the moon would soon be crusted and its solid surface would become pock-marked with late-coming earth-fragments.

The orbits of many other earth-fragments sent flying by the explosion would undergo perturbations and ultimately fall into the planet, which, because of its great size and inherited high temperature, would still be liquid at the surface and thus would bear no permanent scars of such bombardment.

In review, the guess that the earth carried within itself the potential for the birth of the moon is seen to be no more than a hypothesis *ad hoc*. One of its chief purposes is to reconcile the idea of fission by rotational instability with the deficiency of angular momentum in the existing earth-moon system—a deficiency demonstrated by Darwin. To make the reconciliation, the chain of pure assumptions, all incapable of direct verification, is too long for ease of mind. To illustrate the truth that other guesses about the genetic relation between planet and satellite should be put into competition, the writer offers two other conceivable solutions to the problem. Each of these ascribes the separation of the lunar material from the earth because of an external influence.

EXTERNAL INCENTIVE TO FISSION

According to the first of the alternative solutions, the conditions of hypothesis no. 1 are assumed, but the eviscerating explosion of the superheated gas is now supposed to have been triggered off by local, preliminary explosion and expulsion of matter through heating by collision with a planetoid which arrived with great relative velocity. It is thought that such a preliminary explosion could relieve the deep-lying supercritical material from enough weight-pressure to cause it to flash into the gaseous state, with evisceration, loss of mass, and reduction of planetary rotation, as imagined in hypothesis no. 1. To formulate this no. 2 hypothesis we have piled speculative Pelion on speculative Ossa and in so doing have encountered new mental trouble. For example, it would be hardly possible to tell the proportions of the moon's mass assignable to fragmented planetoid and fragmented earth respectively.

Obviously either hypothesis no. 1 or hypothesis no. 2 can be formulated with a good deal of elasticity in the assignment of masses, velocities, and temperatures without destroying all worth of the speculation. The same is true of still another fundamental assumption, the terms of which have been elaborated from a directive given in a personal letter from Professor H. N. Russell. He suggested that it might be worth while to study the question whether the main part of the moon's substance represents a planetoid which, after striking the earth with a glancing, damaging blow, was captured. Acting on that hint, the writer has developed hypothesis no. 3, with the following additional postulates: that at the epoch concerned the earth had practically its present mass and was liquid at the surface; that the planetoid had direct motion and struck the earth at or near the equator; and that fragments were torn off directly by the visitor, along with others ejected because of an explosion resulting from the collision.

A preliminary word about the nature of the planetoid is in order. Jeffreys (1929: 28) found no planet with radius smaller than about 2,000 kilometers—200 kilometers greater than the lunar radius—could be formed by the condensation of gas derived from the sun or similar star. The telescope shows how few are the known bodies with radii exceeding 2,000 kilometers, and Bode's law provides no place for a planet whose orbit is now situated between the orbits of earth and Mars or between those of earth and Venus. It is therefore not easy to think that the colliding planetoid as such had independent existence from the beginning of the solar system. With more plausibility our planetoid may be supposed to have originated during Jupiter's tidal disruption of the planet whose debris is now partly represented by the asteroids (see Jeffreys, 1918: 435), or else to have originated when this parent planet blew itself to pieces (see Russell, Dugan, and Stewart, 1926: 357). The hypothesis of tidal disruption has some support from the consideration that, if the parent of the asteroids did approach Jupiter so closely, the orbit of the parent around the sun was highly elliptical and its path would have to pass within the earth's orbit. In a personal interview Professor Whipple noted a second possibility: that the odds in favor of the collision might have been increased if at that time the earth were near the aphelion of an orbit more elliptical than its present orbit around the sun. It is of course conceivable that more than one asteroid entered the gravitational fields of both earth and younger moon and were absorbed by these two bodies.[8]

The first effect of the impact of the "planetoid" on the liquid earth would be violent splashing all around the locus of contact, and each splash would have large components of its velocity in the horizontal and vertical directions. (Compare the effects of shell-fire on a military target as described by Cranz and Becker, 1921: 445.) Also violent would be similar dispersal of fragments caused by gaseous explosion if the "planetoid" had a large fraction of the moon's present mass. Supposing the infall to have been at the minimum velocity, namely, that due merely to the earth's attraction or 11 kilometers per second, the heat developed would have sufficed to raise the temperature of the planetoid by about 40,000° C. if this heat were evenly distributed through it. But in fact most of the heat would have to be concentrated at the interface between the two bodies, for along that interface would be the maximum of plastic deformation. Hence it does not seem wild to assume a temperature of at least 100,000° C. on both sides of the interface. Such heating means volatilization of much material in this region. Although the depth to which the planetoid penetrated must be an uncertain quantity, we can easily suppose that, even with a collision of the slicing kind, the locus of volatilization would be far below the planetary surface. At this depth would be located most of the explosive energy.

The explosion would drive out terrestrial material into a limited belt ringed about the planetoid. Many of the initially liquid fragments would be swept along by the visitor as it left its grazing contact with the earth, and by mutual gravitation would fall into the planetoid remnant, ultimately developing a revolving body with the mass of the existing moon. There seems to be no *a priori* objection to the idea that the new satellite was at first largely or wholly liquid, or to the supposition that after the crusting of the moon it was bombarded by latecoming earth-fragments which left permanent record in the form of the visible lunar "craters."

This third hypothesis shares difficulties connected with current theory about the development of the solar system as a whole, and has troubles more peculiar to itself. First, we have to consider how small is the chance that an asteroid or planetoid of any other origin would strike the earth near the equator. On the other hand, as Jeffreys (1929: p. 37) points out, our moon is unique among all the satellites of the solar system, and it seems allowable to postulate such a highly specialized condition for the moon's origin. Secondly, it is natural to think that a satellite which in large part represents captured material should have had an initial orbit of considerable ellipticity; if so, the problem of explaining the reduction to the approximate circularity of the lunar orbit emerges. Here too there is a theoretical solution of the difficulty: appeal to the same "resisting medium" by which cosmogonists now account for the approximate circularity of the planetary orbits.

In passing, it may be remarked that those two difficulties do not exist in the case of hypotheses nos. 1 and 2. On the credit side, however, no. 3 seems to afford a better basis for accounting for the reported (though not fully demonstrated) triaxiality of the sealevel figure of the earth. Like the other two hypotheses it avoids the difficulty emphasized by Darwin: it offers explanation of the earth-moon system in spite of the fact that the angular momentum of the system is now too small for the fission of a planet with a four-hour day and the mass of the existing system.

EARTH-FRAGMENT HYPOTHESIS

Three guesses have been made as we have ranged over a wide field of mystery. We have

[8] If the "planetoid" were one of the asteroids, its mass was additional to the total for the visible asteroids. By so much would we be better able to account for the fact that the total mass of known asteroids is far smaller than the minimum mass required on the theory of planetary derivation from the sun (see Jeffreys, 1929: 28, 62).

met uncertainties at every turn, and the most vital is the possibility that a thoroughly contrasted fourth guess might hit the truth. Perhaps, however, our speculative exploration makes more tolerable the idea of catastrophe, followed by the infall of terrestrial fragments into, and during the growth of, the moon. In any case let us accept the idea as correct, while leaving wide-open a choice among the conditions that led to the expulsion of fragments from an earth liquid at the surface—fragments later conglomerated to make the independent, revolving moon. Then let us see how this "earth-fragment hypothesis" agrees with the facts of selenography, geophysics, and geochemistry. Here the jungle of uncertainties is not quite so thick.

First, the composition of the moon demands attention. Our fundamental assumption as to the meteoritic composition of the earth makes it easy to believe that, at the moment of fission, the thick superficial, liquid shell yielding much or all of the moon's substance was "peridotitic" and held considerable volatile matter in solution. If freed of gas and crystallized at the pressure of one atmosphere the material would have a density close to 3.33, the mean density of the moon at present. If a part of the moon represents a captured asteroid, that part might well have now the density of Eros or Ceres, again not far from 3.33, a suggested value which, however, has not yet been actually proved for these two bodies. The internal pressure of the moon increases the one-atmosphere density of its material by about 0.1, but we may expect this increase to be largely or quite offset by vesicularity, due to the sudden release of dissolved gas, as bubbles, in the flying fragments from which much of the lunar globe was developed.

From measurements of the polarization angles of the moon's surface, Lyot (1929: 140) of France and Pettit and Nicholson of the Mount Wilson Observatory, as reported by Wright (1935: 113 and 1938: 67), found the surface material to react like volcanic ash or pumice. From the measured rates of cooling and heating during and after eclipses a vitreous (pumiceous) condition was suggested to Wright, whose committee is carrying on the most thorough investigation of the moon yet undertaken.

In the course of time the stony-meteorite material of the earth's mantle became differentiated into the granitic, basaltic, and peridotitic layers of the present day. Similar differentiation may have developed homologous shells just below the surface of the moon; if so, the results of the optical tests find rather direct explanation, though the uppermost, "sialic" layer may not be quite so high in silica as the granitic shell of the earth. In any case we should expect the surface material of the moon, exposed to the low gravitational pressure in our satellite and made rapidly viscid through free radiation of heat, to be vesicular to this day.

In any of its forms the catastrophe hypothesis as here presented implies the absence of a volumetrically important iron core in the moon—a fact demonstrated by the mean density of the body.

Another supporting fact is found in the shape of the moon. According to leading geodesists the sealevel figure of the earth approaches that of a triaxial ellipsoid, one with an elliptical equator. Such a figure means that the deeper and greater part of the planet is capable of resisting indefinitely considerable shearing-stresses. The required strength may well be attributed to pressure-crystallization of that part of the silicate mantle which lies between the iron core and a level somewhere between 500 and 1,000 kilometers from the surface. From this level to the base of the lithosphere at average depth of about 70 kilometers is an asthenospheric layer still too hot for complete crystallization. Now the solid figure of the moon also approaches a triaxial form. "From Hayn's discussion it appears that the equatorial diameters of the moon differ by about one third of a mile, while the polar diameter is fully a mile less than the shorter of these" (quotation from Russell, Dugan, and Stewart, 1926: 170).

Jeffreys has given the only good explanation yet offered for this departure of the moon's figure from that of an oblate spheroid in fluid equilibrium. He assumes that for a relatively short but sufficient time the infant moon was either liquid at the surface or but thinly crusted. Using the theory of tidal friction produced by the powerful tides set up by the earth's attraction, he calculated an approximate date for the stereotyping of the tidal bulge to which the moon's triaxiality is to be referred. He found that the bulge, containing the longest equatorial diameter and centering in that face of the satellite which henceforth was to point toward the earth, was fixed in position when moon and earth were about 140,000 kilometers (30 per cent of the mean radius of the lunar orbit at present) apart.

At that comparatively early date enough of the moon must have been crystallized and thus become strong, to permit permanency for the earth-directed bulge. Allowing for the speediness of pressure-freezing in depth and the rapidity with which so small a body cooled, one can well see how that condition was met. Jeffreys' success in solving his problem tends to corroborate a leading premise of the present writer's catastrophe hypothesis, namely, primitive liquidity and correlated high temperature both for the earth and for the matter which was ultimately aggregated to form the moon's body.

LUNAR TOPOGRAPHY

Finally, the suggested mode of genesis will be considered in relation to the relief of the moon. Here we have a wealth of facts, expressed in the books of Elger, Neison, Nasmyth and Carpenter, Shaler, Graff, Fisher, and others, as well as in the magnificent photographs of the moon taken at the Paris, Lick, and Mount Wilson Observatories. Of unique value is the sumptuous lunar atlas of Loewy and Puiseux, representing a dozen years of photographic study at the Paris Observatory. The older, hand-drawn maps of Baer and Maedler, Schmidt, and others are historically important and of permanent value, but for many purposes not so useful as the photographs. The following discussion will be largely confined to the so-called "craters," rayed and unrayed, and the maria.

We have seen that, according to the favored explanation of the moon's origin, its silicate material was initially hot enough to be liquid at the surface and held much gas in solution; that some free gas was present in the terrestrial fragments; and that later on additional gas was probably freed by the pressure-crystallization of silicate in the depths of the lunar globe. It is therefore natural to look for signs of moderate volcanic action in the moon's topography. In fact no other explanation seems so plausible for certain rows of small, alined pits, typified by those in the vicinity of the Bulliadus and Stadius "craters." Such comparatively minute, supposedly true craters are to be numbered in scores if not in hundreds; yet their total area is but a small fraction of the visible surface of the moon.

Many selenographers have been inclined to accept a similar origin for the thousands of much greater depressions—walled plains, mountain-rings, crater-plains, and "craters" of Neison's classification. For this conclusion there is little convincing argument. In its support emphasis has been placed on a fancied homology with the greater terrestrial depressions due to major volcanic explosions, and also with the volcanic sinks (unfortunately named "calderas") of Hawaii.

The first of these claims to true homology is at once suspect when one compares the characteristic asymmetry in the ground-plans of these terrestrial "craters" (calderas) of evisceration with the high degree of symmetry for the lunar depressions. Other contrasts are found in great disparity of size, in relative abundance, and in distribution over the respective surfaces of planet and satellite. In none of these respects is volcanism of the explosive type a good explanation for the many thousands of larger "craters."

The suggestion of J. D. Dana (1846), followed by that of W. H. Pickering (1906), that the Hawaiian sinks may more truly represent the ringed depressions of the moon is likewise unsatisfactory. The Hawaiian sinks are surface effects of slumping, caused largely by withdrawal of lava through fissures that have been opened in the mighty lava-dome of Hawaii, this withdrawal being possible because the high elevation of the visible dome above the Pacific floor affords the required condition for the draining of lava from the active pipes or conduits. In the moon there is no such difference of level to induce important withdrawal of lava in depth.

The lava rings of Hawaii have the general shape of the larger "craters" of the moon, but this likeness can hardly be taken to mean similar origin. The lava rings, with diameters of a few scores or hundreds of feet, represent retaining dams built up by marginal chilling (solidification) as the levels of the respective lava lakes slowly rose. The more or less circular dam remained as a ring-shaped ridge after the lake level finally sank away. The lake itself was kept liquid by lively "two-phase convection" in the conduit which had a cross-section much smaller than the area of the lake. It seems out of the question that the moon could have kept issuing gas at such a rate that any of the "craters," with individual areas thousands or tens of thousands of times greater than the area of any Hawaiian lava lake, were floored with lakes made or kept liquid in the same way. Moreover, the circular ridges of the Hawaiian lava rings have relatively smooth longitudinal profiles, in contrast to the wonderfully jagged profiles of the lunar rims.

It is particularly hard to regard as volcanic the numerous small pits which are sunk in the high, massive rims of the larger "craters." From the Loewy-Puiseux atlas the writer has made a list of 52 of such damaged rims. The list is not complete and yet includes 10 per cent of all the named "craters." The high rims are among the last places on the moon where a geologist would expect to find so many gas vents.[9]

Further, the theory involving volcanic explosion is suspect because it fails to account for the relatively low albedo of the lunar regions which are crowded with "craters." At each of these the explosion of gas brought up from the interior of the moon should have led to the deposition of abundant chlorides, sulphates, and probably free sulphur. Whether salt or element, the sublimates reflect light much better than does pumice or any of the standard rocks, whether comminuted or not. The observed proportion of solar light reflected from the "crater"-rich areas is clearly not that expected from a blanket of sublimates, widely spread from each of the many thousands of "craters." Thus once again a logical consequence of the volcanic hypothesis does not agree with fact.

The advocates of that hypothesis have paid too little attention to another fact: under even the best conditions for seeing no selenographer has been able to identify unequivocally lava flows that issued from any of the "craters." The great spurs radiating from Copernicus, Aristillus, and a few others have dimensions of a quite different order and find easier explanation as gigantic splashes of impact.

Gilbert (1895: 246) was one of the first geologists to attempt quantitative comparison between terrestrial volcanic craters and lunar "craters," with respect to abundance, size, and form. Recommending caution in using the first criterion, he wrote: "Had the terrestrial craters of all periods been exempt, like those of the moon, from atmospheric and aqueous attack, it is easy to imagine that they might now be equally abundant." He found the striking contrast in diameters to be only in small part explicable by the different values of gravity in the two bodies, while "in vertical dimensions there is no important discrepancy." Still more compelling reasons for discarding the volcanic theory for the lunar "craters" were detailed when Gilbert discussed the third criterion, form. First, he considered terrestrial volcanoes of the Vesuvian type, 95 per cent of all the visible vents. Ninety-nine per cent of the Vesuvian volcanoes, more or less conical piles, have the bottoms of their craters higher than the corresponding outer plains, while an equal proportion of the lunar "craters" have their bottoms lower than the surrounding plains. Further:

> Ordinarily the inner height of the lunar crater rim is more than double its outer height; ordinarily the outer height of the Vesuvian crater rim is more than double its inner height. The lunar crater is sunk in the lunar plain; the Vesuvian crater is perched on a mountain top. . . . The smooth inner plain characteristic of so many lunar craters is either rare or unknown in craters of the Vesuvian type.

Gilbert concludes:

> Thus, through the expression of every feature the lunar crater emphatically denies kinship with the ordinary volcanoes of the earth. If it was once nourished by a vital fluid, that fluid was not the steam-gorged lava of Vesuvius and Etna.

The present writer believes that this argument of Gilbert holds good even if it were proved that in its infancy the moon ridded itself of volatile matter with the explosive power of a Vesuvian volcano.

Concerning volcanoes of the Hawaiian type, Gilbert wrote:

[9] The old "blister" theory of Hooke and Secchi can hardly apply to these pits on the "crater" rims, and in general, like the laccolithic theory originating with Tomkins (1927) and recently approved by Marshall (1943), has little to commend it.

On the Oceanus Procellarum and on some of the maria the lava surface at many points bears smooth, circular, or elliptical domes which have major diameters of one to three miles and heights of a few scores or hundreds of feet (see Elger, 1895: 6; Shaler, 1903: 37, 38; Marshall, 1943: 424). One might be tempted to think of the domes as homologues of the low domes ("tumuli") so often seen on the earth's basaltic flows of the pahoehoe type. However, these last rarely reach lengths exceeding sixty feet and their crests are seldom as much as twenty feet above the surrounding plain of lava, and the disparity of size tends to make incredible any true homology between the "tumuli" and the lunar domes, even allowing for the low value of gravity on the moon. Moreover, if there were a true homology, the domes could not be considered evidence of as many tappings of gas from the deep interior of the moon, that is, evidence of true volcanism. An apparently much better explanation is that these domes represent older "craters" or isolated reliefs which were flooded and smoothed over by lava of the maria, and then left standing up after the flooding lava sank away on all sides of each dome.

As long ago pointed out by Dana, they resemble the moon's craters much more closely than do those of ordinary volcanoes. They agree with lunar craters in the possession of inner plains, and to a certain extent in the terracing of their inner walls. They differ in the fact that they occupy the tops of mountains; in the absence of the wreath; in the absence of the central hill, and usually in the presence of level terraces due to the formation of successive crusts. In my judgment the differences far outweigh the resemblances, and I have not succeeded in imagining such peculiarities of local condition as might account for the divergence in form.

Finally, Gilbert discussed volcanoes belonging to the remaining terrestrial type of volcanoes, namely the maars, each of which is small and represents "an explosion of steam," unaccompanied by emission of lava. He thought it possible that such vents might be identified on the moon if it were possible to have the "better seeing" necessary in the case of a relatively minute feature like a maar.

Gilbert phrased his general conclusion as follows:

The volcanic theory, as a whole, is therefore rejected, but a limited use may be found for the maar phase of volcanic action in case no other theory proves broad enough for all the phenomena.

The arguments of the master must be given special weight because they are those of a geologist who had had extensive field experience in volcanic fields and in his profession was almost unrivalled with respect to rigor of thought and fairmindedness in controversial matters. Since he wrote, a good many investigators, most of whom have never made serious study of terrestrial volcanism, have discarded the volcanic theory of the lunar pits, but few of them have so explicitly based reasoning on quantitative considerations.

Suess (1895 and 1918) was so impressed by the size of the "craters" that he was led to regard them as loci of regional fusion of the crust. To supply the needed energy he assumed a colossal amount of superheated gas in the moon's body, and thought that this gas emanated at points around which the crustal fusion took place. For Suess the lunar refusions were homologues of the regional refusions of the earth's crust by rising batholithic magma, and he seems to have relied on the collection of extremely hot gas from the deep interior as a sufficient cause of the superheat of the magma itself. However, it is now clear that so great superheat cannot be postulated for terrestrial magma. According to Suess, the high rims, the characteristic boundaries of the lunar depressions, were formed in two stages: first by the peripheral chilling of the lava derived by crustal refusion and spread over the moon's surface; and by later subsidence in the broad central area of each region of refusion. Here he relied on Ebert's experiments, intended to duplicate the effects of crustal refusion. No homologues of the results of these experiments are known to be represented anywhere in terrestrial geology on the scale of the lunar maria, and one may well question the relevancy of Ebert's experiments which failed to reproduce the spectacular ruggedness, serrateness, of the rims of the lunar "craters."

Thus all published versions of the volcanic hypothesis face serious difficulties, and in the writer's opinion there is much more hope of success in accounting for the large, dominant depressions on the moon as primarily the results of as many infalls.

We now return to our main line of thought—appraisal of the earth-fragment hypothesis. We must suppose many of the fragments torn out of the earth to have been projected beyond Roche's limit. For the earth this limit is about 11,000 miles or 17,500 kilometers from the center. Inside the limit any fragments would have been comminuted by the earth's powerful attraction and the aggregation of these by their own mutual attraction would be impossible, just as in the case of the particles in Saturn's rings. It is out of the question to declare the range of sizes for the flying fragments; yet it is not unduly straining probability to assume that one of the earth-fragments or else a wrecked and captured planetoid, safely outside Roche's limit, was large enough to gather in with comparative speed the other fragments, thus growing to the mass of the existing moon. Rapid radiation might well have developed a continuous crust before many of the laggard fragments were gathered in. Would not the scars made by these late infalls account for the great majority of the lunar "craters"? Tests leading to an (affirmative) answer to this question will be the next subject for discussion.

Among the facts to be accounted for are: (1) the great sizes of the typical "craters"; (2) the close approach of their rims or "wreaths" to circularity; (3) the composition, remarkable heights, and often-extreme ruggedness of the rims; (4) the relative smoothness of floors; (5) the large number of cases where each "crater" is provided with a central peak; (6) the light-tinted rays emanating from a few of the great pits; and (7) the relation of the "craters" to the maria which occupy so much of the moon's face.

Permanent scars due to impact obviously mean true solidity for the surface material. This is thought to have been early secured by crystallization of the differentiated sialic or quasi-sialic layer. On account of the low value of lunar gravity such a crust need not have exceeded about 10 kilometers in thickness in order to support the relief of the fully scarred body. The earth presents no evidence of similar scarring (either in topography or crustal structure) by those earth-fragments that fell back after the original catastrophe. Upholders of the impact theory have satisfactorily explained this fact by the much more prolonged liquidity of the planet at surface, a condition forbidding permanent scarring. The earth-fragment hypothesis here proposed tends to support their contention.

The diameters of the walled plains like Clavius, Ptolemy, Plato, and Grimaldi range from 100 to 300 kilometers; those of the mountain-rings, from 25 to 100 kilometers. But the separation of the two classes from each other and from the still more numerous pits from 5 to 25 kilometers in diameter and curiously distinguished by selenographers under the specialized name, craters, is arbitrary. In view of their transitional relation all three classes are, in the present paper, grouped under the name "craters," the quotation marks implying that they have nothing to do with true volcanic pits. The width of the great pits means that the earth-fragments held to have been responsible for their formation must have had great individual mass, though with mean diameters considerably smaller than those of the respective "craters."

Later we shall note the possibility that a few of the "craters" were due to infall of meteorites. If a meteorite with average velocity struck the moon it would have the same scarring power as one of our earth-fragments with several times its diameter. The infall velocity of the latter would be of the same order as the maximum due to the moon's gravitational pull, namely, about 2.4 kilometers per second or about one-tenth to one-thirtieth of the speeds of "falling stars."

Most of the kinetic energy of the fallen earth-fragment would be converted into heat. If all the energy became heat and were uniformly distributed through the fragment, this would be heated about 2000° C. See discussion by R. S. Woodward, reported by Gilbert (1895: 258). However, there can be no doubt that most of the heat would be concentrated at the interface between fragment and lunar body, according to the principle already described in the case of collision between earth and planetoid. If the mass of an earth-fragment were great enough, much silicate material close to the interface would be melted and, in all probability, volatilized.[10] Thus it seems reasonable to suppose that the initial splashing and other plastic deformation of the lunar crust (including upturn of solid crust-rock around the area of impact) should in many cases have been supplemented by the displacements due to gaseous explosion. The net result would be analogous to the meteoritic deformation at Meteor Crater in Arizona. As Barringer (1924) has so well argued, this analogy eases understanding of leading features of the lunar "craters."

But the Meteor Crater analogy fails for the many lunar "craters" that have broad, flat floors—a form that almost inevitably means former liquidity for the floor material.

Theoretically, massive infalls could give such localized liquidity in more ways than one: (1) by melting at the interface between fragment and moon, with rise of this remelted rock to the surface; or (2) by rise of substratum magma through a relatively thin crust where smashed by the impact; or (3) by local but extensive down-stoping of the broken crust, accompanied by upwelling of the primary substratum melt. That the rise of substratum magma was the most important cause is suggested by the present depth of the floor below the mean level of the lunar surface. For the larger "craters" the difference of level probably averages about 1,000 meters. Explanation of this fact has to take account of the fact that each scarring earth-fragment represented material added to the moon at the "crater" made by its infall. Two different causes for the excessive deepening can be derived from the particular version of the impact theory here offered.

First, we note the vesicularity of the terrestrial fragments when these, which had been more or less saturated with gas at the depths

[10] According to C. Cranz and K. Becker (1921: 449) 95 per cent of the mass of more than one million bullets fired into a sand heap was volatilized—the velocity of each bullet being only about 1,000 meters per second.

and corresponding high pressures of the earth, were torn out and so were released from the high pressure upon them. Some of this vesicularity would persist even after the projectiles were aggregated as a globular nucleus for the moon. In view of the low value of lunar gravity and the relatively feeble power of weight to squeeze out the gas bubbles, we may suppose the surface shell of our satellite to have been, and remain, notably porous. However, the intense pressure at the place of infall, aided by any associated remelting of the crust, would tend to close the pores, with the escape of the gas enclosed in them. In this way the density of the crust at the loci of impact may well have been considerably increased—perhaps by 20 per cent or more. After isostatic adjustment and ultimate freezing of the lava pools the surface of the mended crust at these loci would stand lower than the mean surface of the lunar crust as a whole.

Secondly, there are visible details of form which strongly suggest outward splashing from the region of the "crater" floor, with the addition of much of the displaced rock to the "wreath" rimming the "crater" pit.

Much of the sialic rock forced down in the hot, liquid substratum would be expected to melt in depth, where it would diffuse outward for some distance away from the pit. If, by exception, there were little horizontal movement of that kind, the remelted sial would rise again. It is conceivable that in rare cases this relatively light melt did not rise until the simatic liquid of the respective "crater" floors was frozen. In that case the thin simatic crust might have been pushed up with a more or less complete filling of the pit. In this way can we account for Wargentin, whose flat floor is nearly on a level with the rim?[11]

About half of the larger "craters" contain one or more peaks projecting from the floors and generally not far from the centers of the wide pits. In all cases these central peaks are lower than the corresponding rims. For advocates of the volcanic hypothesis the central peaks are volcanic cones, built up by igneous action some time after much more powerful volcanism had developed rim or "wreath" and flat floor. In support of that view, appeal has been made to a fancied homology with the nested cone-crater relief seen at the tops of terrestrial volcanoes of the Vesuvian type. In the writer's opinion whatever resemblance there may be is merely formal and not genetic. In spite of prolonged search no observer has ever been able to demonstrate a craterlet at the top of any central peak, though Schmidt and a few other selenologists have claimed one in the case of Timocharis. Moreover, the shapes of many of the peaks are not those expected on the volcanic theory.

Most writers who favor some form of the impact theory for the lunar "craters" explain each central peak as the piled-up product of centrifugal, return movements of the moon's plastic-solid material from the "crater" rims—as a phenomenon of recoil. While this explanation seems mechanically sound and supported by laboratory experiments, as well as by the existence of a low but distinct central "peak" on the original floor of the Meteor Crater in Arizona (*fide* Barringer, 1924: 5), our general earth-fragment hypothesis suggests another explanation which may well be supplementary if not competitive. Because of concentration of the heat of impact at the interface between moon and fragment, the temperature at the opposite end of the fragments might, as Gilbert (1895: 272) suggested in principle, fall short of the melting "point" or even short of the point where plasticity under lunar gravity could flatten the whole of the fragment. Retaining considerable porosity and corresponding low density, the residual, unmelted part of the terrestrial fragment would therefore float in the risen sima. That remnant would be kept not far from the center of the "crater" by the strength of the wrecked but more or less coherent crust between the locus of impact and the "crater" rim. Freezing of the risen sima would further guarantee permanent fixation of the residual of the fragment at or near the center of the pit.

Since the terrestrial fragments came from different depths in the planet, the release of pressure upon them greatly varied and the resulting porosity varied from fragment to fragment. We can find here one reason why the heights of the central peaks, regarded as in at least approximately hydrostatic equilibrium with the simatic layer, should differ so much.

On the other hand, it is not hard to conceive that fragments of low porosity and therefore of relatively high density, especially those that fell

[11] Or was Gilbert (1895: 282) nearer the truth when he suggested that Wargentin is a "mountain ring," that is, an old walled plain whose floor was raised by lava flooded in from a neighboring mare? In such case the plateau-like mass would represent an uncompensated load on the lunar crust.

into areas where because of earlier bombardments the lunar crust was specially thin or weak, were completely disintegrated or absorbed by the simatic layer, leaving no visible witness of its former existence as a separate body. In this way we might account for the "craters" that lack central peaks.

To illustrate a fact obvious to every observer with a powerful telescope, Gilbert (1895: 265) measured the "conjugate diameters and through them the ellipticities of 120 [large] craters. In three-fourths of these the ellipticity is less than .1; in eleven-twelfths it is less than .2; in twenty-nine-thirtieths it is less than .3." For some selenologists this close approach to circularity has meant a fatal objection to any form of the impact theory of the lunar "craters." During nearly a century of controversy their argument has run: since many infalls would have to be in directions at high angles to the vertical, the corresponding "craters" in large proportion should have much greater than even the maximum ellipticity determined by Gilbert. That reasoning, however, has been shown to be fallacious through variously-designed experiments, including those with rifles firing into plastic materials, and through study of the pits made by both "live" and "dud" artillery shells that entered the ground at high angles with the vertical. Barringer (1924: 14) has brought corroborative evidence from the nearly circular Meteor Crater of Arizona where he finds that the great hole was formed by a meteorite plunging at an angle of about 45° with the vertical. In view of all the data bearing on a truly complex problem in mechanics, a problem loudly crying for "second thoughts," it seems clear that the earth-fragment hypothesis gives adequate accounting for the roundness of the lunar "craters."

Ten of the great "craters," including Tycho, Copernicus, Kepler, Aristarchus, Proclus, and Anaxagoras, are centers from which "bright rays" extend to distances of hundreds of kilometers. The origin of the rays has been long sought and several different suggestions offered. Wright (1935: 108) has shown reasons for preferring that one which attributes them to violent explosion. Since similar rays were not developed around the great majority of the "craters," it is clear that the exceptional energy displayed at each center needs special explanation. Eruption of the Vesuvian type, resulting from gas which for some reason had been under exceptionally high tension, is conceivable. Another possibility is to be entertained: unusually violent explosion caused by the infall of relatively small meteorites, striking the moon at their normal velocities—generally more than ten times the velocity enforced merely by the moon's attraction. To this second explanation no *a priori* objection is manifest, but this idea is only supplementary to, and not inconsistent with, the general validity of the earth-fragment hypothesis.

One other characteristic of the "craters" needs consideration. They are numerous in all latitudes, high as well as low. This fact matches expectation if we assume that nearly all the "craters" were formed by the infall of terrestrial fragments. We have seen that when the earth itself exploded, with or without a glancing blow from a planetoid, the liquid fragments were sent out from the center of explosion in all directions. Hence many of the flying masses had trajectories highly inclined to the moon's equatorial plane. When these fragments fell they made scars at high latitudes. The scars at low latitudes were made by fragments having trajectories nearer the plane of the lunar equator. In this way we can understand the pockmarking of our satellite from pole to pole.[12]

The problem of the maria will not be discussed in detail. Gilbert, like some other selenographers, could find no evidence of a genetic distinction between "craters" and maria, and regarded these last as the loci of infall of, and melting by, specially massive bodies (the "moonlets" of his theory). He thought that the moon was solid throughout its history and never included thin crust and liquid substratum. For him this view was a logical deduction from his "moonlet" hypothesis. Shaler (1903: 8 & 15) assigned a volcanic origin to the "craters" but saw in the maria areas of melting by the infall of giant bolides, without the rise of subcrustal lava. The present writer finds more satisfaction in the belief that the flooding lava of the maria was chiefly derived from the vitreous substratum of the young moon, just as in the case of the lava-covered floors of the "craters," and relates the difference in superficial extent to the size of the

[12] Gilbert (1895: 275) sensed the difficulty of correlating his "moonlet" version of the impact theory with the abundance of "craters" at high latitudes. He met it by assuming that the moon's axis of rotation was not constant, but his suggestion as to a cause for the inconstancy seems quite inadequate.

infallen masses.[13] After the present paper was nearly completed, it was discovered that Baldwin (1942 and 1943) had come to the same conclusion. He too assumes the infallen bodies (which he calls "meteorites"), large and small, to have been fragments of the earth, but offers no theory of their disruption from the planet. His explanation of the gigantic striations and groovings of the lunar crust south and southwest of Mare Imbrium (so clearly marked in the Loewy-Puiseux atlas) is practically identical with that offered by Gilbert in 1895.

It is, of course, conceivable that the mare-making, infallen bodies were asteroids or "planetoids" rather than fragments of the earth.[14]

It is noteworthy that the observed variation of albedo over the lunar surface appears to corroborate our general hypothesis of origins. In average the surface reflects about 7 per cent of the sun's light. According to Russell, Dugan, and Stewart (1926: 174), the percentage would rise to perhaps as much as 10 if irregularities were smoothed out. This is greater than the albedo of basalt and smaller than the albedo of obsidian. We recall that, from other evidence, the Wright committee found the surface material to behave optically as if it might be volcanic ash or pumice. A pumiceous or at least highly vesicular character is expected from the terms of the favored hypothesis of the moon's genesis. Moreover, the assumption of a thin sialic layer already differentiated and solidified at the time of bombardment is fortified by the facts recorded about the moon's albedo. Selenographers have adopted a ten-step scale for estimates of light-reflecting power. From Neison's text the present writer has compiled the following table, showing the relative average brightness of wall and floor of many "craters":

Number averaged	Diameter in miles	Brightness of wall	Brightness of floor
51	10–30	6.0	3.6
17	30–50	5.0	3.6
7	50–70	5.3	2.75
3	70–100	5.0	2.3

The maria have about the same brightness as that of the broader "crater" floors.

The table gives quantitative expression to the well recognized fact that the inner slopes of the "crater" walls have a much higher albedo than the floors. The table also suggests that this contrast of wall and floor becomes still more pronounced as the diameter of the "craters" increases. The underlying cause for both rules is reasonably found if we assume, in accordance with our general hypothesis, that the original crust of the moon was largely sialic and that the bulk of the floor material is simatic because derived from a substratum which, during the period of infalls, was liquid and eruptible. Support for the second assumption has already been found; that for the first assumption is contained in the expectation that the inner slope of each "crater" should represent the edge of the crust where upturned by violent impact.

The so-called mountain ranges of the moon were explained by Gilbert as gigantic analogues of the "crater" rims, representing widespread, plastic uplift of the crust around the maria—just as the "crater" rims were for him also the effects of peripheral upturning of the crust where struck by the smaller, "crater"-making missiles. The same principle can be applied in the cases where the mountain uplifts were occasioned, not by the infall of "moonlets" of Gilbert's definition

[13] Is it possible that some "craters" in high latitudes resulted from infalls of moon-fragments sent flying in all directions when the huge masses responsible for the Imbrium and other maria struck the moon? Many of these missiles would have to fall back into the moon, permanently scarring the surface outside the liquid maria, with comparatively few falling into the mare regions after their lava was solidified.

[14] Elger (1895: 12) explains the wide "mountain rings" as walled plains which were "partially overwhelmed by the material of the 'seas.'" It does look as if the "overwhelming" took the form of flooding from the maria when the lava of these was liquid and of low viscosity. Wide-ranging spills over the old, scarred crust would be expected because of the temporary superelevation of the lava surface of a mare by (*a*) the addition of the matter of the fragment to each mare sector, and (*b*) the expanded condition of the highly heated lava of whatever source. With the progressive freezing and hence considerable contraction of the mare lava, accompanied by isostatic readjustment of the catastrophically disturbed figure of the moon, much of the still-liquid lava of the flooded region would flow back to the mare region, leaving behind a relatively smooth veneer of chilled, immobilized lava over the flooded area. The old "crater" depressions would have been greatly shallowed by the permanently-held lava of the flood. The final reliefs in the flooded area should therefore be low, the old walled plains having become mountain rings; smaller "craters" would in general have lost crater form and become comparatively smooth domes, the "laccolithic" domes of Tomkins, and originally low peaks would have lost sharpness, even approaching the shapes of elliptical domes. Many examples of such small reliefs of the last two kinds are to be found in the Carpathian Mountain region and elsewhere.

but by the infall of terrestrial fragments (or perhaps planetoids).[15]

SUMMARY

To be successful any attempt to tell how and when the moon was born must be based on a true idea as to the origin of the solar system—an unsolved problem which is not only outside the scope of laboratory experiment but also, at critical points, beyond mathematical control, the second major aid to scientific progress. Similarly, neither experiment nor mathematical reasoning can enable us to trace with absolute confidence the steps of an imagined derivation of the moon from the earth. Cosmogonists are forced to believe that, if truly a fact, such derivation included catastrophe of a kind that permits neither experimental imitation nor mathematical analysis. There is only one other method left for examining the worth of the catastrophe hypothesis: to develop all promising guesses about a possible cause, with the hope of lighting on one that best explains the facts known about the moon itself, its relation to the earth, and the relation of both to the rest of the solar system. The correlations demand the pyramiding of assumptions. In this paper the assumptions made are numerous and most of them lack strong sanction from cosmogonists.

It is suggested that the fragmentation of the young earth was the product of either internal force or external force or of their combination. According to two closely-related conjectures, the primitive earth, supposed to be liquid at the surface, lost to outer space much of its mass by explosion and that with the loss of mass went much of the original angular momentum, leaving only the respective amounts represented in the existing earth-moon system. According to a third conjecture, a "planetoid," captured because of tangential, slicing, collision with the liquid earth, brought with it so much angular momentum as to ensure its perpetuation as a separate, revolving body—the moon we know. All three guesses led to the conception that initially liquid fragments were exploded out of the planet, well beyond Roche's limit. Many of these were gravitatively aggregated by the pull of master fragment or captured "planetoid" to make the substance of our moon, and the somewhat diminished earth felt a prolonged rain of other earth-fragments, large and small.

With the earth-fragment hypothesis it appears possible to account for: (1) the measured mass and mean density of the moon and the distribution of density within it; (2) its general shape and implied degree of strength; (3) the small deviation of its orbital plane from the earth's equatorial plane at the time when the two bodies were close together; (4) the close similarity between the earth's period of rotation and the period of the moon's revolution, at the same early epoch; (5) topographic features of our satellite which many selenologists have thought to demand a volcanic origin for the larger "craters"; (6) some rare cases of hills so alined as to suggest their upbuilding by comparatively feeble volcanic action; (7) the bright "rays" stretched out from a small number of "craters," like Tycho and Copernicus; (8) both presence and absence of central peaks in the "craters"; (9) the average albedo and range of albedo over the lunar surface; and (10) the polarization-angle and specific heat of the surface material.

Other features of the lunar topography seem also to find reasonable explanation. Among them are: the terracing of some "crater" walls; the local wrinkling of, and degree of roughness of, the surfaces of the maria; the overlapping of "crater" on "crater"; the "crater"-pitting of the maria; the long-puzzling "rills" and so-called "valleys"; the rather rare cliffs typified by the Straight Wall; and the extraordinary whiteness of Aristarchus and other local areas. Discussion of these subsidiary problems would have lengthened this paper beyond intended limits.

If the moon did originate by the disruption of an earth which was liquid at the surface, the terrestrial conditions implied would be such as to aid attack on some principal, insistent, and as yet unanswered questions about the earth itself. Among the items in this formidable list are: the range and distribution of internal temperature, strength, and elasticity; the fundamental conditions for igneous action and for mountain-building; the origin of continents and ocean basins; the state of the iron core; and the supposed deviation of the sealevel figure of our

[15] If the crusting of the moon began when that body was comparatively near the earth, the tides set up in the moon should have been powerful. It seems possible that the tidal currents kept breaking up the new crust and upending and rafting-together the irregular pieces. If this were the case, the relief of the crust, when finally complete, would be extremely rough. Can we thus find partial explanation for the chaotic topography of the Apennines and other "mountain ranges" of the moon?

planet from that of a biaxial ellipsoid in fluid equilibrium.[16]

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[16] After its completion the manuscript of this paper was read by Professor H. N. Russell, who drew the writer's attention to the latest explanation of the solar system including the satellites—namely, the hypothesis of C. F. von Weisäcker as digested in the *Astrophysical Journal* **101**: 249, 1945 and published in full in the *Zeitschrift für Astrophysik* **22**: 319, 1944. In a personal letter Professor Russell wrote: "According to this the planets were formed by material which was close to the sun at the time of its origin, and circulating around it, but had never formed part of it. The opportunities for collision of the type to which you attribute the lunar 'craters' would be great." Professor Russell is of opinion that the Weisäcker hypothesis can well be consonant with the impact theory of the lunar "craters." Thus, even if the "earth-fragment hypothesis" is proved inadmissible, the present paper may still have some value in emphasizing from geological evidence the superior claim of bombardment rather than volcanism as the dominant cause for the molding of the moon's face.