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Nickel for Your Thoughts: Urey and the Origin of the Moon

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Summary. The theories of Harold C. Urey (1893–1981) on the origin of the moon are discussed in relation to earlier ideas, especially George Howard Darwin's fission hypothesis. Urey's espousal of the idea that the moon had been captured by the earth and has preserved information about the earliest history of the solar system led him to advocate a manned lunar landing. Results from the Apollo missions, in particular the deficiency of siderophile elements in the lunar crust, led him to abandon the capture selenogony and tentatively adopt the fission hypothesis.

"Oh, I'd love to go to the moon," Harold Urey said in an interview in July 1969 (*1*). "I wish I could go rock-hunting with the astronauts this month. . . . I think I'd go to the moon . . . even if I knew I could never get back." The business executives who read this statement in *Forbes* magazine on the eve of the Apollo 11 lunar mission may have smiled at the dream of an eccentric scientist, but they could not ignore the biggest technological project of the decade—a project on which Harold Clayton Urey (1893–1981) had a major influence. Although the primary reason for a manned lunar landing may have been political (to demonstrate superiority over the Russians) and a landing was feasible from an engineering standpoint, why did the United States undertake such an extensive program of lunar exploration? Some scientists would have preferred that resources be shifted to planetary missions. It was Urey who provided a scientific rationale for Apollo, and persuaded the leaders of the American space program that lunar exploration would yield more valuable information than other feasible missions (*2*).

Even without the brilliant success of Apollo, Urey would still deserve much of the credit for reviving interest in solar system research during the last three decades. His 1952 book *The Planets* (*3*) persuaded many physical scientists to enter a field largely abandoned or downgraded by astronomers, who favored the more spectacular realm of stars and galaxies. Urey showed how chemistry and physics could enrich planetary science, previously dominated by astronomy and geology. Many now active in the field acknowledge the stimulus Urey gave to their careers even if they disagreed with his conclusions. As one who worked closely with Urey said, his hypotheses and methods "defined the paradigm from which we all started" (*4*).

Astronomers and geologists who studied the moon's surface were curious about the origin of the craters and other features, but they did not expect to learn much about selenogony from such study. Urey convinced himself that the moon was a primordial object, almost unchanged since its formation during the infancy of the solar system; unlike the earth, it should have preserved a record of conditions billions of years ago and provide clues to the formation of the earth and other planets. In particular, Urey concluded that the moon was never part of the earth but was formed independently and later captured.

Urey was well aware of the temptation to reinterpret new data in a way favorable to one's own hypotheses (*5*). It must have been difficult, as samples were returned from the moon, for him to abandon his hypothesis that the moon is a primordial body captured by the earth, but he did. Whether the fission hypothesis, to which he turned, will ultimately be accepted by the scientific community, or whether by an ironic twist the leading Russian hypothesis (accretion in orbit) will turn out to have been established by the U.S. effort intended to beat out the Russians, is not settled (*6*).

Three Theories

Before Apollo, theories of the origin of the moon could be classified into three groups (*7*).

1) The "sister" or binary planet theory, derived from Laplace's nebular hypothesis, proposed that the moon had been formed by condensation from a cloud of material surrounding the earth. This theory was given a quantitative base in 1873 by Edouard Roche, whose formula for the tidal-stability limit of a satellite plays a crucial role in many modern theories. A variation of the sister theory was the hypothesis proposed in the 1890's by the American geologist Grove Karl Gilbert, that the moon formed from a ring of small solid particles; the final stage of the process would produce the lunar craters.

2) The "daughter" or fission theory was based on the discovery in the mid-19th century that the apparent secular acceleration of the moon is partly due to a gradual decrease in the earth's speed of rotation. (At present the moon is observed to be speeding up in its orbital motion, but when cyclic perturbations are subtracted, the net effect is a real deceleration.) As a result of tidal dissipation, angular momentum is transferred from the earth's rotation to the orbital motion of the moon, which recedes from the earth as its angular speed, as seen from the earth, diminishes. George How-

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Fig. 1. Harold Urey and the lunar globe. [Courtesy of Von Del Chamberlain]

ard Darwin, son of Charles Darwin, traced this process back more than 50 million years and proposed that the moon was then only 6000 miles from the earth's surface. (The present earth-moon distance is nearly 240,000 miles.) The period of revolution of the moon and the earth's period of rotation would have been about 5 hours. Darwin suggested that the moon and the earth had earlier been part of the same rapidly spinning fluid body. Because a fluid body with the combined mass of the earth and moon rotating in 5 hours would not be unstable, Darwin proposed that the actual breakup had been triggered by the action of the sun. The free oscillations of the protoearth might have had a period of about 2½ hours, somewhat longer than that of the present earth, since the density would have been lower. The tides raised on the protoearth by the sun, peaking twice each "day" (that is, a 5-hour day) at a given place on the surface, might have been in resonance with the free oscillations, thus producing distortions sufficient to disrupt the body (*8*).

A supplemental hypothesis, often included in later expositions of Darwin's theory, was proposed by Osmond Fisher in 1882. He suggested that the scar left by the moon's separation did not completely heal and that the ocean basins are holes left in the earth's crust after some flow of the remaining solid toward the original cavity. In this way the birth of the moon would have resulted in both the Pacific Ocean basin and the separation of the American continent from Europe and Africa (*9*).

3) The "wife" or capture theory had occasionally been suggested in the 19th century, but the first major astronomer to advocate it strongly was Thomas Jefferson Jackson See (*10*). According to See, the moon was formed in the outer part of the solar system, near the present orbit of Neptune. It lost energy as it moved through the interplanetary medium, and the orbit gradually shrank until the moon approached the earth. Because the existence of retrograde satellites of Saturn and Jupiter seemed impossible to explain by the binary-planet or fission theory, it appeared that at least some satellites must have been captured. See argued in 1909 that we must follow Newton's Second Rule of Reasoning and assign the same causes to the same effects whenever possible; that is, we must assume that all satellites were captured (*10*).

In 1930 Jeffreys (*11*), earlier a defender of the fission theory, published a criticism that persuaded most astronomers to abandon it. His objection was that viscosity in the earth's mantle would damp the motions required to build up a resonant vibration and would thereby prevent fission.

For the next 25 years there was neither any major progress in developing theories of lunar origin nor any clear agreement on the existing theories. The most important writings on the subject were those of the German astronomer Nölke (*12*), who advocated a modified Laplacian hypothesis in which the moon condensed from the outer parts of the earth's atmosphere.

In 1955 Gerstenkorn (*13*) published a calculation of the history of the lunar orbit, tracing it back through the period of closest approach (when it would have an inclination of about 90°) to an earlier epoch when its motion was retrograde. Gerstenkorn's work, at first ignored by everyone except Öpik (*14*), became the basis for the revival of the capture theory in the 1960's.

From Nuclear Chemistry to Planetology

Harold Urey, born in Indiana, earned his Ph.D. in physical chemistry under G. N. Lewis at Berkeley. One of his earliest contributions to science was a practical method for estimating the distribution of electrons among excited states of atoms in ionized gases. He received the 1934 Nobel Prize in Chemistry for his discovery of deuterium and directed an isotope-separation program for the atomic bomb project. After World War II he moved from Columbia University to the Institute for Nuclear Studies at the University of Chicago. He resigned from the Atomic Energy Commission in 1950 and was a severe critic of the nuclear arms race. The *New York Times* reported that he "would rather collect sea shells than work on atomic research"; in fact, he was measuring the oxygen isotope abundances of the shells in order to estimate the past temperatures of the oceans (*15*).

Urey recalled on several occasions that he had become interested in planetary science because of a research project started at the University of Chicago by Harrison Brown; when Brown left, Urey kept the project going, at first out of a sense of obligation and later because he became fascinated by it. His interest in the moon was piqued by reading Ralph Baldwin's book *The Face of the Moon* (*16*) on a train trip to Canada; when he returned he obtained photographs of the moon, which he posted on his office wall.

In preparing a course on "chemistry in nature," Urey read Slichter's 1941 article (*17*) on the cooling of the earth and was surprised to learn that the temperature might actually be rising rather than falling. He wrote in 1952 (*3*, p. ix):

> This led on to consideration of the curious fractionation of elements which must have occurred during the formation of the earth. One fascinating subject after another came to my attention, and for two years I have thought about questions relating to the origin of the earth for an appreciable portion of my waking hours, and have found the subject one of the most interesting that has ever occupied me.

Urey presented his first ideas on the formation of the earth at a meeting of the National Academy of Sciences on 26 October 1949 (*18*) and published a detailed account of his theory in 1951 (*19*). His basic assumption, relying on work of C. F. von Weizsäcker and others, was that the planets had condensed at low temperatures from a dust cloud. (Because chemical factors would be of primary importance, Urey could speak authoritatively.) The earth initially had a core of moon-like material surrounded by a layer of silicates and iron; later, as the earth warmed up through radioactive heating, the iron flowed to the center. He emphasized the significance of the moon's present surface features as a record of conditions in an earlier stage of the history of the solar system (*20*): "Markings on the moon's surface indicate that iron-nickel alloy objects a few kilometers in radius fell on the moon. . . . The inference is that such objects also fell on the earth at this stage." On the earth, traces of the objects were obliterated by geologic processes. The bombardment suddenly stopped, and no major changes occurred since the moon became rigid, about 4 billion years ago. Urey suggested that the moon represents more nearly the composition of the original dust cloud relative to the nonvolatile elements than does the earth. The fact that the moon did not have a shape corresponding to isostatic equilibrium also indicated that it was frozen a long time ago. Thus Urey prepared the way for the argument that exploration of the moon could provide information about the early history of the earth.

In his book *The Planets* Urey mentioned both the binary planet and capture theories for the origin of the moon (*3*, p. 97):

> The qualitative difference in density of the moon and earth can be explained by the assumption of two stages of growth, the first during a period in which low-density silicates collected into a primordial moon and earth, and the second subsequent to this in which metallic iron-nickel phase was an important ingredient in the material collected, together with the assumption that the rate of growth of the earth in this second phase was much more rapid than that of the moon. . . .
>
> We should explore the possibility that the moon was formed from its own protoplanet and not from a secondary nucleus within the earth's protoplanet, and that the moon or its protoplanet was captured by the earth. So far as any evidence presented here is concerned this may well have been the case. In this event there is no difficulty in accounting for the rate of growth of the earth relative to the moon. . . . The comparatively large angular momentum of the system arose then from the details of the capture collision. Such a capture would be aided by the presence of gas. . . .

In spite of this early remark Urey did not propose a definite capture hypothesis until 1959, and he seems to have been led to it indirectly through his studies of meteorites rather than directly by consideration of the moon. In 1957 he abandoned Kuiper's hypothesis that protoplanets (large masses of gas and dust of solar composition) had been involved in the formation of the terrestrial planets. Instead he postulated that two sets of objects of asteroidal and lunar size, which he called primary and secondary objects, were accumulated and destroyed during the history of the solar system (*21, 22*). The primary objects were suddenly heated to the melting point of silicates and iron, cooled for a few million years, and then broken into fragments less than a centimeter in diameter. "The secondary objects accumulated from these about 4.3×10^9 years ago, and they were at least of asteroidal size. These objects were broken up . . . and the fragments are the meteorites" (*22*). This scheme could explain the presence in meteorites of diamonds, which Urey thought required an earlier high-pressure environment such as could have been provided inside larger bodies. Later it was found that the diamonds could have been formed by impact, but the idea of lunar-sized bodies kept its hold on Urey.

At a symposium on the exploration of space in April 1959, Urey suggested that "the moon may be one of these primary objects, as I realized after devising what seemed to me a reasonable model for the *grandparents* of meteorites" (*23*). He could therefore prescribe a set of chemical and physical observations to be made from the moon's surface to give information not only about meteorites but also about the formation of the planets. He concluded his paper with the remark: "It is hoped that such observations will be forthcoming during the immediate years ahead" (*24*).

Urey discussed the nature of the moon's capture in more detail in 1960, attributing the necessary energy dissipation either to tidal effects or to collisions with small bodies that remained in orbit around the earth. "The very short period of time for the formation of the maria indicated by the surface features of the moon is quite in accord with the hypothesis that the moon was captured by the earth late in the process of the formation of the earth by the capture of smaller objects" (*25*). He stated that this "obvious explanation" of the remarkably short duration of bombardment of the lunar surface had occurred to him during the past year.

At the 1960 International Astronomical Union Symposium at Pulkovo Observatory, Urey admitted that capture of an object like the moon from a circumsolar orbit by the earth was quite improbable; thus the justification of his theory had to depend on the more general argument that the early solar system was populated by a large number of such objects, one of which happened to have been captured and survived. He surmised that the iron content of the moon was only about half that of the earth or Mars, but was comparable to that of solar material with the gaseous elements removed. From this viewpoint, the problem was not to explain why the moon has so little iron but why the earth has so much. Later, upward revisions of the solar iron abundance were to undermine this aspect of Urey's theory (*26*).

Revival of the Fission Theory

According to the Russian astronomer B. J. Levin, at the Pulkovo symposium "all participants unanimously agreed that revival of the hypothesis of the separation of the moon from the earth is impossible." Levin seemed to consider it almost a personal insult that certain scientists, such as A. E. Ringwood in Australia and A. G. W. Cameron and D. Wise in the United States, had nevertheless dared to revive the idea. According to Levin, even Urey, who "decisively repudiated" this theory in favor of capture a few years ago, had recently shown sympathy for it (*27*). But here Levin was a little premature; Urey was not yet ready to abandon the capture theory.

Ringwood's hypothesis (*28*), which he originally described as a return to the "ancient fission hypothesis," eventually developed into a sophisticated version of the Laplace-Nölke theory that the moon has precipitated from the outer parts of the protoearth's atmosphere. But Wise's theory (*29*)—and similar hypotheses proposed by Cameron (*30*) and Simpson (*31*)—was indeed an attempt to return to Darwin. The significant new feature was the postulate (alluded to by Ringwood) that formation of the earth's core would increase the rotational speed beyond the critical value and trigger fission (*32*). Wise admitted that his mechanism was still quantitatively insufficient to account for the necessary angular momentum, but he thought that modern ideas like magnetic braking or a decreasing gravitational constant might resolve this difficulty. O'Keefe, at Goddard Space Flight Center, suggested at about the same time that the fission theory could be revived with the help of the magnetic braking mechanism; he was led to the idea that the moon came from the earth by the theory that tektites come from the moon (*33*).

Urey rejected the fission theory at this time because of the criticisms of Jeffreys and others. Later he could cite Goldreich's proof that the moon's orbit could never have been in the same plane as the earth's equator, as it would presumably have been had the moon spun off from the earth (*34, 35*).

Urey Defends the Capture Theory

During the 1960's the dynamics of lunar capture was discussed by many scientists. Three conclusions emerged from most studies. (i) Capture was possible if some special initial conditions are satisfied; thus one is dealing with an event of very low probability. (ii) The lunar orbit would have had to come inside the Roche limit before expanding to its present size; thus it seems likely that the moon, if captured as a single body, would have been fragmented at close approach, and this makes extrapolation to earlier epochs uncertain. (iii) If the rate of tidal dissipation in the past were the same as it is now (as inferred from secular acceleration), closest approach would have had to occur only about 1 billion years ago; there is little evidence for such a recent catastrophic event, and no suitable place to "store" the moon for 3 billion years before its capture (*34, 36*).

As already mentioned, Urey's response to the first point was to admit that although capture was improbable there may have been numerous lunar-sized bodies, making it more reasonable that the earth might capture one of them. He met the second objection by following G. J. F. MacDonald's ideas and conceding that there may have been several smaller protomoons which eventually coalesced. He never had to worry about the third objection since he had started from the assumption that the moon was at least as old as the earth and was captured while the earth was still accreting; thus he could simply reject the assumption that the rate of tidal dissipation was as great as that implied by the present secular acceleration (*37*).

Urey argued that the moon has been cold ever since it was captured, and this was an essential part of his case for giving high priority to lunar exploration. If the moon has been melted or undergone the same kind of processing as the surface of the earth, it would reveal little or nothing of interest about conditions in the early solar system.

One consequence of this line of reasoning was Urey's suggestion that the lunar maria were not formed by lava flows (a sign of heating) but by water. The water might have come from the primordial stuff of which the moon was formed (for example, carbonaceous chondrites) or even have been splashed from the earth during the capture process. "If indeed the surface of the moon carries a residue of the ancient oceans of the earth at about the time that life was evolving, the Apollo program should bring back fascinating samples which will teach us much in regard to the early history of the solar system, and in particular with regard to the origin of life" (*38*). That was indeed a tempting prospect to dangle in front of Congress as well as the scientific community.

Another argument used by some leaders at the National Aeronautics and Space Administration to gain public support for Apollo, though apparently not one made by Urey himself, was an extrapolation from his cold-moon theory. If the solar system was formed by an encounter of two stars, as suggested earlier in the 20th century by T. C. Chamberlin, F. R. Moulton, J. H. Jeans, and H. Jeffreys (*39*), then the moon and planets must have been hot, and their iron and other heavy elements would have been separated into central cores. But if the moon and the planets had condensed from cold gas and dust, the iron might not flow to the center except in bodies as large (or with as much radioactive minerals to provide heating) as the earth. Thus the moon might be found to have bits of iron scattered throughout its interior. In that case we could accept the nebular hypothesis, which implies that planets are normally formed by the same process that builds stars. Hence there is probably other intelligent life in the galaxy, whereas the encounter theory implies that planetary formation is an improbable process and life rare (*40*).

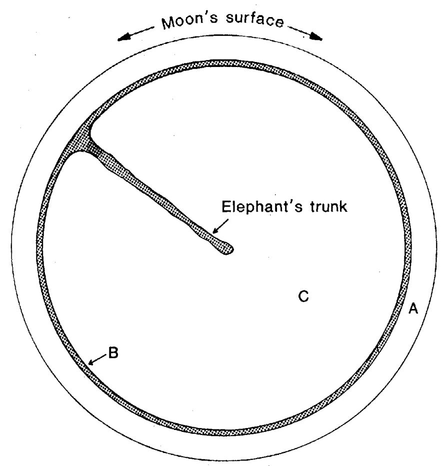

Fig. 2. Flow of iron-nickel liquid toward the moon's center, as shown in original version of the O'Keefe-Urey paper "The deficiency of siderophile elements in the moon."

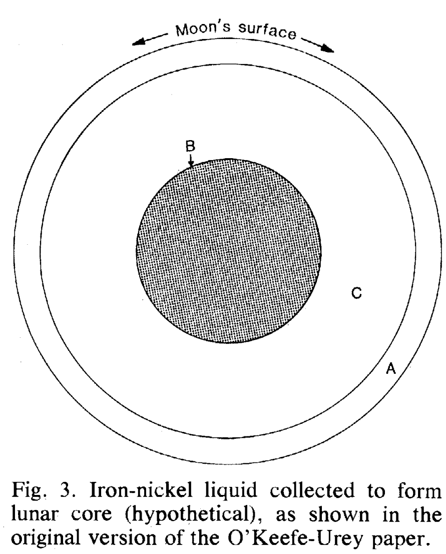

Fig. 3. Iron-nickel liquid collected to form lunar core (hypothetical), as shown in the original version of the O'Keefe-Urey paper.

Although Urey participated actively in the discussions of the National Academy's Space Science Board, which advised NASA in the early 1960's and was probably instrumental in expanding the lunar science effort, he had little influence on how the space program actually operated. He argued successfully that the last Ranger mission (Ranger IX) should be sent to the Alphonsus area of the moon, partly because of Russian reports of gaseous eruptions there and partly because it was thought to be a very old area, but he had no influence on the choice of landing sites for later missions. He insisted on the need for manned missions to bring back carefully selected samples for detailed analysis and rejected the view that television images and unmanned sample-return missions would be a more effective use of available funds.

Siderophile Abundance: A Crucial Test

Shortly before the first moon landing O'Keefe, one of the small group of supporters of the fission selenogony proposed a modification to Darwin's theory; he suggested that both the earth and moon would have been so hot immediately after fission that they would have been largely vaporized, and thus would have suffered substantial mass loss (*41*). The moon, composed of mantle material, would have received more heat per gram and would have contained a larger proportion of volatile substances, and thus it would have lost a larger fraction of its mass. In this way the requirements of angular momentum and energy, which had plagued earlier fission theories, might be met. O'Keefe predicted that the moon would be poorer in water and other volatile substances than the earth. Moreover, a crucial test of the proposal would be the abundances of siderophile elements such as gold, platinum, and nickel, which are depleted in the earth's crust (compared to solar system abundances) presumably because they followed iron into the core. If the moon was formed by fission after core formation, its crust should also be deficient in siderophiles; since the moon probably does not have an iron core, the deficiency could not be due to a purely lunar process. If siderophiles were abundant in the moon's crust, then the capture theory would be favored.

The Mare Tranquillitatis rocks returned by the Apollo 11 mission in July 1969 provided a flood of new data. I will mention only the results most directly relevant to Urey's ideas. According to the preliminary examination team, the most striking features were a high concentration of refractory elements (titanium, zirconium, and so forth); a low concentration of volatiles (lead, bismuth, and thallium); strong depletion of siderophile elements, especially nickel and cobalt; the absence of secondary hydrated minerals, indicating that there had been no surface water at Tranquility base at any time since the rocks were exposed; and the great age of some rocks, perhaps greater than any known on the earth (*42*).

O'Keefe immediately pointed out to Urey that the depletion of siderophile elements could be considered an argument in favor of the fission theory. The relative scarcity of these elements in the earth's crust compared to solar system abundances is usually attributed to their separation into the core of the earth. We cannot assume that the moon's nickel went into its core since the moon seems to be nearly homogeneous, and hence has little or no core. Therefore, the moon must have come from the earth's mantle after the earth's core had been formed. O'Keefe reminded Urey that he had predicted the deficiency in volatiles from his fission model (*43*).

Urey did not reply to O'Keefe until 11 November; he apologized for the delay, stating that he had been completing a chapter for Kopal's book, which he was writing with Gordon MacDonald (*44*), and added (*45*):

> Neither Gordon or I are able to see any way by which the moon could have gotten off the earth and be consistent with all the evidence. As I have consistently said, I do not think the evidence is conclusive for any of the models for the origin of the Moon. . . . I find insurmountable problems for the present escape hypothesis, but also no conclusive evidence for the capture or the accumulation hypotheses.

It was becoming clear that several of Urey's theses about the moon (the presence of water and a completely cold history, for example) were contradicted by the evidence from Apollo 11 (*46*). His assumption that the moon's low concentration of iron matched the solar abundance (thus making the moon a primordial object) was refuted by redeterminations of the solar iron abundance (*47*). On the other hand, the determination that the moon's surface is at least as old as the earth's (though subject to some melting episodes) confirmed another aspect of Urey's theory.

Early in 1971 Urey suggested to O'Keefe that the depletion of siderophiles might be due to their removal by iron which had formed a layer 200 kilometers below the surface. O'Keefe argued that such a layer would be unstable because it would be denser than the material below it. Urey agreed that there were difficulties with his idea and later wished that "I had stuck to my model for the meteorites and claimed that the moon was one of these objects." The problem was, as O'Keefe said, that "it is necessary to have removed the siderophile elements from the surface of the moon by means of liquid iron" (*48*).

In November 1971, after Urey had given a speech at the University of Maryland, O'Keefe wrote to him that "we have this much at least in common: we think there must be a more volatile-rich region in the interior of the moon" (*49*). Urey replied (*50*):

> . . . I find your discussion of Darwin's paper very interesting. I am continually pointing out that unless the interior of the moon contains the volatiles, my model for the moon cannot be correct, and, therefore, I will go back to another model and it will be Darwin's model. I am very glad to hear that the difficulty which I and others have seen in this model will be reliably explained in other ways. I shall be sorry and disappointed if this is the case, for the moon then will be an incidental object and not of fundamental importance. We can decide that it escaped from the earth and then "to hell with it."

During 1972 and 1973 discussions of the moon's deficiency in nickel and other siderophile elements continued. O'Keefe was convinced that the nickel had been extracted by molten iron; since the iron was no longer present in the moon, the extraction must have occurred when the moon was part of the earth's mantle. Urey and O'Keefe agreed to collaborate on a quantitative study of the extraction process, but Urey also continued to look for places within the moon where the iron could go after it had extracted nickel from the surface. In February 1973 he wrote to O'Keefe: "Look at my model for the origin of the moon. Things are perfectly arranged to extract the nickel" (*51*). And in an article published a few months later Urey stated that the fission theory was hard to accept because of the moon's depletion in volatiles and siderophiles; he favored a modified version of his capture hypothesis (*52*). In 1974 new data on the spectral lines of iron indicated that the abundance of iron in the sun might have to be revised downward again, thus removing one objection to the idea that the moon is a primitive object (*53*).

New Evidence for Fission

The contrast between public and private views during the period 1970 to 1974 is remarkable. Those who attempted to determine the consensus of lunar scientists reported that the fission theory was being rejected and that capture, double-planet, and various modifications of these were gaining ground (*54*). Yet Urey was moving toward the fission theory and working out with O'Keefe what in their opinion would be conclusive arguments for that theory. At the same time Clayton and his group at the University of Chicago were refining their measurements of oxygen isotope abundances, which would lead them to conclude that earth and moon are related by blood, not merely by marriage (*55*).

In 1974 Wänke, in Germany, in a review of the chemistry of the moon, emphasized the difference in the FeO/MnO ratios between lunar and terrestrial rocks. He asserted that this difference "rules out the possibility that the moon was once part of the earth" (*56*). O'Keefe wrote to Wänke that the high FeO/MnO ratio of lunar rocks was due to the general depletion of volatiles, as predicted by Wise and O'Keefe before Apollo 11; they had concluded that the moon must have lost most of its initial mass immediately after fission. Wänke replied, "If you can find the physical conditions for the evaporation of about 90 percent of the original mass without disruption of the moon and with a strict fractionation of the compounds according to their boiling points a fission origin of the moon could be brought into agreement with the geochemical observations." O'Keefe responded that this "agrees well with the mass losses which I calculated from dynamical arguments" (*57*).

A few days later O'Keefe wrote to Urey: "We really seem to be making progress on the problem of the moon's origin"; Wänke's depletion factor "agrees unexpectedly well with the depletion that one finds by dynamical methods. A big loss of mass is needed because the earth breaks up via a pear-shaped configuration (piroid, as Poincaré called it) and the small end of the pear has around 1/5 of the total volume. Hence the moon probably began life with a mass 1/10 or more of the earth's mass. It now has 1/81.3. So there has been a mass change of one order of magnitude. Maybe we are on the right track after all" (*58*).

Down the Elephant Trunk

In a paper on the deficiency of siderophile elements in the moon submitted to *Geochimica et Cosmochima Acta* in 1974, O'Keefe and Urey did not try to make a strong case for the fission theory. Instead they emphasized their conclusion that the depletion was due to liquid-liquid extraction—the "blast furnace" process described in 1921 by V. M. Goldschmidt (*59*)—rather than to gaseous fractionation in the solar nebula, as suggested by E. Anders and others (*60*). Assuming that the most important reaction was the reduction of nickel oxide by free iron ($NiO + Fe \rightarrow FeO + Ni$), Urey and O'Keefe estimated that the minimum amount of metallic liquid required to lower the concentration of nickel in the lunar crystal rocks by the observed amount is about 2 percent. They conjectured that the resulting iron-nickel liquid would sink to form a layer about 200 kilometers below the surface. Since this layer would be denser than the material beneath it, a Rayleigh-Taylor instability would ensue. A column of material, called an "elephant trunk," would flow toward the center (Fig. 2). Eventually all the metallic liquid would drain to the center, forming a "dirty core" (Fig. 3).

O'Keefe and Urey could not yet refute the "dirty core" hypothesis by chemical, moment-of-inertia, or seismic tests, but they pointed out that it "does not predict the high heat flow values which are suggested by the scanty measurements available." The fission model does satisfy all the tests, if one assumes that the moon loses a large amount of mass just after fission. This mass loss would deplete the volatiles while enriching the refractories, including uranium and thorium which could account for the high heat flow at present.

The Urey-O'Keefe paper encountered considerable criticism from the referees for *Geochimica et Cosmochimica Acta*, and while they were responding to criticisms and rewriting the paper, other scientists were publishing ideas that led Urey to reconsider his decision to support the fission theory. In the fall of 1974 he considered withdrawing the paper until questions such as the state of the earth immediately after the fission event were resolved (*61*). But O'Keefe was able to answer all the questions that Urey raised, and in October 1975, when Urey was awarded the V. M. Goldschmidt Medal of the Geochemical Society, he stated in his published acceptance speech, "I rather favor the view that the moon escaped from the earth, though I have no good idea as to how the separation of the moon from the earth occurred" (*62*).

The O'Keefe-Urey paper was rejected by *Geochimica et Cosmochimica Acta* (*63*), but when O'Keefe was invited to speak at a conference at the Royal Society of London, he took advantage of the opportunity to publish the paper in the *Philosophical Transactions* (*64*). When the paper was shortened to the required 3600 words, the elephant trunk discussion was omitted. On the other hand, the case for fission was strengthened by new seismic data indicating that the mass of metal in the lunar core was less than 0.5 percent of the moon's total mass (*65*); O'Keefe and Urey estimated that if the nickel had been extracted from the crust by liquid iron which stayed in the moon to form a core, this core would have to contain at least 1 percent of the moon's mass.

When the O'Keefe-Urey paper appeared in 1977 (*64*), it had been rewritten to stress the case for the fission hypothesis. By comparison with the cosmic composition, the earth's crust is deficient in siderophiles; this is usually explained by the migration of these elements into metals at a time when the metal and silicate portions were mixed, after which the metal sank to the core. The moon's crust is also deficient is siderophiles, but the moon now has no core; hence bodily separation of metal from silicate mass is suggested, and therefore the moon originated by fission. Fission cannot be rejected on the grounds that the moon is deficient in volatiles in comparison with the earth since dynamical calculations predict a loss of 90 percent of the moon's mass after fission.

These arguments would also support Ringwood's hypothesis that the moon precipitated from an extended terrestrial atmosphere and passed through an intermediate sediment ring, as proposed by Gilbert and Öpik. Ringwood's continued advocacy of his model during the 1970's did not harm the fission theory since many of his arguments supported the general proposition that the moon had come from the earth's mantle (*66*). Several other scientists have supported some version of the fission theory on the basis of Apollo data (*67*) although it is by no means generally accepted.

Conclusions

I have described how Harold Urey developed and defended the theory that the moon was captured by the earth and thereafter remained essentially unchanged. The fission theory, originally proposed by Darwin, was revived in the 1960's by O'Keefe and others. O'Keefe argued that the deficiency of such siderophile elements as nickel in the lunar crust implied extraction by liquid iron, which must have been left behind in the earth. After collaborating with O'Keefe in working out the details of this process, Urey abandoned his capture theory, although his support for the fission theory was tentative. Apart from theories of selenogony, this story tells us something about Urey as a scientist.

Jastrow recently recalled that, as a physicist, he was attracted by Urey's deductive approach to planetary science. From the single fact that the moon has an irregular shape, Urey concluded that it had been frozen and dead for billions of years, and it was this conviction that the moon preserved a record of the earliest days of the solar system which helped him convince NASA to give high priority to lunar exploration (*68*). The same deductive approach forced him to change his views, once he had agreed with O'Keefe that rigorous conclusions could be drawn from the deficiency of siderophiles: "Because of its similarity to iron in many respects, the marked fractionation between iron and nickel in the earth and moon is a critical phenomenon for understanding the geochemistry of the moon" (*64*, p. 572).

One must recognize that several leading scientists did not accept this reasoning and pointed to other critical phenomena that would suggest a different origin of the moon. The existence and size of the moon's core is still a subject of controversy. Some of Urey's critics may even suspect that his abandonment of his capture theory was the act of an exhausted man under pressure by a vigorous advocate of another theory. But from a glance at Urey's letters from 1972 to 1976, it is clear that he was critical of almost all of O'Keefe's ideas and rejected at least as many as he accepted.

A letter from O'Keefe to Urey, written at a time when Urey was considering withdrawing their paper in the face of adverse criticism, is relevant to this point as well as to allegations that scientists fail to adhere to certain ideal standards of behavior (*69*):

> For heavens sake don't drop out of the paper. All of the chemical thinking is yours, and is full of solutions to problems that I failed to solve. The main idea is yours. If you drop out, I'll never be able, alone, to break down those characters. I would never have undertaken this paper alone.
>
> Do you remember Ian I. Mitroff, the sociologist from the University of Pittsburgh who came around during the Apollo program with a tape recorder and asked everyone lots of questions about the origin of the moon, whether the moon was hot or cold, and so on? Well, he's coming out this fall with a book on the subject of our interactions with one another. I haven't seen the book, but I have seen four papers covering the subject; they probably give a good idea of what is in the book.

The drift of Mitroff's findings is that the lunar scientists, especially the theorists, don't give up their ideas in the face of facts and don't think logically. He says we allow personal considerations to dominate our thinking. It's a real attack on science as such.

In this situation, this paper by you and me will do a lot of good because of its clear anti-Mitroffian message that scientists can get together on the meaning of the facts, no matter how different their initial ideas may have been.

Mitroff did not claim that science is "completely subjective, irrational, relativistic" (*70*) but rather that subjective factors are more important than scientists are willing to admit. Urey himself had often remarked on the tendency of scientists to interpret data as confirming their own theories (*5*), but he did abandon his theory when he decided that the facts no longer supported it. As a geologist involved in the Apollo program wrote of Urey (*71*):

In general he was surprised that his theories of the completely primitive and cold moon were wrong, and he gave them up grudgingly. But give them up he did, and it was very refreshing to observe his willingness to change his mind so drastically in later years. Other pioneers of his era did not prove so flexible.

From discussions with other lunar scientists it appears that Urey's shortcomings may have been in the opposite direction from that portrayed by Mitroff; that is, that he was too ready to change his ideas and was likely to take both sides on an issue. This tendency is apparent even in later correspondence between O'Keefe and Urey. The qualitative conclusion that the moon came from the earth was not sufficient; Urey demanded that O'Keefe produce a detailed theory for the separation process, and in May 1977 he wrote, "I'm doubtful about your origin of the moon, though there are certain features of the moon that would be fitted by your suggestion that the moon comes from the earth" (*72*). Eight months later O'Keefe was able to produce a theory which did please Urey. But shortly afterwards, when they discussed the hypothesis of Cameron and Ward (*73*) that the moon was ejected from the earth's mantle by way of a collision with a Mars-sized body, Urey saw some merits in this idea too (*68*).

I conclude that Urey was never satisfied that the problem of the moon's origin had been definitely solved. He left us with this remark (*62*):

But what can one expect? One must always leave something for the young people to solve. It would be most disappointing, I am sure, if we older people solved all the problems of science, which, of course, none of us will ever do.

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The Organization of Work in China's Communes

Nancie L. Gonzalez

There has been much speculation about what is happening in the Chinese countryside since the fall of the Gang of Four. Recent articles in the United States and in Chinese newspapers suggest there has been a trend in the agricultural sector toward what many in the United States might think of as "creeping capitalism." A new way of organizing agricultural work, referred to in China as the "job responsibility system," has been implemented widely since 1979. It is usually described in terms of individual farmers being assigned specified plots of land on which to grow negotiated quotas for sale to the state. This assignment of land is in addition to the assigned house sites and the small "private plots" on which families or households have long been permitted and sometimes encouraged to raise supplementary crops for their own use. The job responsibility system represents an apparent retreat from the collective agricultural work patterns usually associated with the units known as production teams and brigades, and is therefore of interest to social and agricultural scientists concerned with the relations between productivity and social organization.

In August and September of 1981 I was able to gather firsthand microlevel data on the job responsibility system (*1*). During this 8-week period in China I visited rural communities representing what the Chinese considered to be successful, middle-level, and lesser developed agricultural efforts and traveled by train from Liaoning in the northeast (formerly Manchuria) to Guangdong (Canton) in the south. I talked with leaders (cadres) at different organizational levels, including national, provincial, county, commune, brigade, and team locations, and visited peasants at their homes and observed them at work (*2*). Here I report what I found and describe some of the complexities and implications of the new and old organizational systems in the 17 communes that I visited.

Domestic Organization, Work Assignments, and Income

Official party policy concerning how labor should best be mobilized and organized to accomplish the goals of industrialization on the one hand and increased production of food on the other has from the beginning vacillated between the mere encouragement of cooperative work groups and the attempted enforcement of near total collectivization. The latter pattern, in which collective income and surpluses are divided evenly regardless of the amount of work contributed, was an ideal during the Cultural Revolution and is today in some areas referred to bitterly as the policy of "eating out of the same pot." Regardless of how strictly it was enforced or how the peasants felt about this policy, it is clear that in many areas agricultural production suffered greatly and that lack of motivation was only part of the problem (*2*).

The income of individual peasants is derived from their share of the collective's sale of grain crops to the state (*3*), from the sale of so-called "sidelines," such as crafts, livestock and animal products, silkworm cocoons, cultivated pearls, fish, mushrooms, and herbal medicines, to the state or to the collective which in turn markets the products, and from the sale of vegetables and animal products at local or regional markets. The markets I observed were only minimally regulated, prices being determined largely by supply and demand. Thus they are called "free markets," meaning that they operate outside the state-run procurement and sales apparatus.

Peasant family incomes are increasingly augmented by some members being given the opportunity to work in rural industrial or craft enterprises at the team, brigade, or commune levels. Some family members may work full-time in local workshops and factories or in transportation or construction, joining the agricultural effort only during the busy harvest season. Others, whose primary work assignment is in agriculture, may engage in certain industrial or

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