COMMENTS ON RINGWOOD'S PAPER

"PETROGENESIS OF APOLLO 11 BASALTS AND IMPLICATIONS FOR LUNAR ORIGIN" *

Ringwood's (1970) paper consists of two parts: The first summarizes and organizes in an exceedingly commendable fashion the geochemical and petrological data obtained from lunar exploration. The second part is a discussion of various theories of lunar origin, with particular emphasis on the so-called "precipitation mechanism" for the origin of the Moon (Ringwood 1966). In spite of my agreement with the major summaries of the experimental features and direct interpretations, I cannot agree to the statements put forward by Ringwood with regard to the origin of the Moon.

Let us discuss first of all the composition of the lunar basalts and show that many theories for the origin of the Moon can give a satisfactory explanation. Ringwood makes much of the apparent similarity between Earth basalts and mantle rocks and lunar basalts to imply a genetic relation. Two things might be said here. First of all, if we find that the Martian basalts are similar, does this imply a genetic relation in the sense that Mars should have been created from the Earth? And, secondly, is the relationship really that close? Who is to say that the similarities are stronger than the differences. It really becomes a matter of taste. The major geochemical

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features to explain are certainly the low iron content in the Moon and the depletion of volatiles. Let us see how various hypotheses of lunar origin deal with this explanation.

Ringwood himself has a rather special mechanism for achieving these geochemical separations. He assumes that in the process of accretion the Earth heats up to precisely the temperature at which the silicate materials will vaporize but the iron will not. This temperature is supposed to be maintained while a very large fraction of the mass of the Earth accretes. Without any kind of thermostat, this is very difficult to believe. In the first place, the surface temperature must surely depend on the rate of accretion as well as on the gravitational potential. (In fact, if the accretion rate is low enough, then the Earth may well accrete in a cold state as has been previously supposed by Harrison Brown, Urey, and others.) There are good reasons to believe that the accretion rate goes through a maximum when the planet is of intermediate size, and certainly diminishes after most of the accreting material has been swept up. Incidentally, it is not permissible simply to equate the average kinetic energy of the accreting matter to an averaged heating of the surface by assuming a heat loss proportional to $(\text{ave. temp.})^4$. On the contrary, since each accretion constitutes a high-speed impact (with the attendant production of hot gases and intense radiation), the effective heat loss will be much larger, and the retained heat will only be a fraction of the energy input. In addition, much of the iron will vaporize in each impact.

To return to Ringwood's discussion, he has the silicates evaporating into the atmosphere. For some reason the atmosphere is also at a high temperature whose cause is not explained. In general, atmospheres would be much cooler. If so, then the silicate material ought to precipitate out at some altitude and then simply fall out back to the Earth's surface. (There it may reevaporate so that, in essence, we have here a very effective additional cooling mechanism for the Earth's surface.)

After the accretion is completed, Ringwood brings in a special event, a T-Tauri stage of the Sun, to remove the atmosphere except for the silicate materials. How they happened to be left behind is not explained. Three other mechanisms for dissipating the primitive atmosphere are mentioned, but they are all rather vague. It is not explained how the "rapidly spinning high-molecular-weight terrestrial atmosphere" can mix with the low-molecular-weight solar nebula in view of the existence of a magnetosphere, and why this mixing, in any case, should remove the terrestrial atmosphere. If "magnetohydrodynamic coupling" transfers angular momentum from the condensed Earth to this primitive atmosphere, this would certainly not dissipate it but, on the contrary, implies that the magnetic lines of force would hold on tightly to the atmosphere. The fourth mechanism which depends on the formation of a core (why did it not form during accretion?) would not produce an instability of the atmosphere. It would simply prevent the accretion of more material which would be thrown off as soon as it hit the Earth.

Therefore, I am not convinced that an atmosphere could be so conveniently dissipated. I am further not convinced that this dissipation would carry away just the volatile materials so as to explain their absence on the Moon. If the removal mechanism is based on atomic physics or plasma physics phenomena, then there should be vast differences exhibited between lead, on the one hand, and sodium on the other. Yet no systematic variations among the volatiles have been shown except that volatility is the primary determinant.

The fission theory (O'Keefe 1969, 1970) can explain some of these features more directly. The absence of iron on the Moon is, of course, explained simply, since the iron has condensed to form the core of the Earth. The depletion of volatiles on the Moon is explained by O'Keefe on the basis of intense heating of the Moon which must have occurred during the fission process, and a resulting evaporation and dissipation of the volatiles.

The capture theory has no built-in explanation for the low abundance of iron on the Moon. If Urey's old arguments (1962, 1966) concerning the iron abundance on the Sun or concerning the origin of diamonds in meteorites are invalid, then this does not invalidate the capture theory as such. It simply removes Urey's justification for the capture theory. (It is important to keep the argument logical.) The capture theory must explain the absence of iron in other ways. It is possible, for example, that when the solar nebula condensed and planets formed, there was formed essentially one nucleus for iron at the orbit of the Earth, and that at a later time when the nebula had cooled, several nuclei formed for the silicates. In view of the fact that there are other examples in the solar system of bodies that are genetically related; e.g., meteorites, but have large differences in iron content, we don't necessarily have to burden a theory of lunar origin with explaining this fact.

On the other hand, the depletion of volatiles on the Moon can be explained by the capture theory in a very natural way. If the low abundance of volatiles is due to the fact that they were accreted at the very end of the accretion process from a cooling solar nebula and added as a veneer (Ganapathy et al 1970; Anders 1970), then the observed depletion ratio of 10 to 100 between Moon and Earth can be accounted for very satisfactorily if the Moon is formed independently, but not if the Moon is formed in orbit around the Earth (Singer and Bandermann 1970).

Let us now look at the dynamical considerations. It should be stipulated right away that capture of the Moon by the Earth is an event of low intrinsic probability. If one wishes to introduce atmospheres or bodies around the Earth that can absorb some of the kinetic energy of an incoming Moon, then the process becomes easier but more *ad hoc*. The important thing to note is that the capture process does not do violence to any of the laws of physics, that angular momentum is strictly conserved, that the energies involved can be accounted for, and that the energy dissipation may in fact be responsible for initiating melting and core formation of the Earth (Singer 1968).

Ringwood misinterprets the basis for the capture hypothesis. The earlier calculations by Gerstenkorn (1955) and MacDonald (1964) which showed that the Moon was captured from a retrograde orbit can actually be used as an argument against capture since they would produce excessive dissipation in the Earth's interior, as well as other physical difficulties.

I also should point out that these tidal calculations do not define a time scale of capture. They simply give the orbit at sequential time intervals. In fact, I rejected the 2 billion year time scale as far back as 1966 simply because

I did not believe in the constancy of the tidal dissipation parameters of the Earth, and I wanted to capture the Moon immediately after its formation so as to produce a physically more reasonable capture theory (Singer 1966, 1968). Subsequent data from the Moon have borne out the fact that capture, if it occurred, must have happened about 4.5 billion years ago. This is gratifying and shows that the capture theory was not contrived to fit the experimental data.

There is another aspect of the tidal calculations that is misunderstood by Ringwood; namely, the fact that the Moon comes as close as 2.6 earth radii. (This is the semi-major axis; the perigee distance is much closer.) This distance has nothing to do with the Roche limit and depends entirely on the total angular momentum of the Earth-Moon system. This is well established in a series of computational runs in which I have chosen different initial angular momenta.

Let me say, however, that the tidal calculations cannot be used to prove that capture has occurred. They merely give an evolutionary path for the orbit, but do not say at which point the Moon has been inserted. It rather works the other way around; namely, tidal calculations can be used to eliminate certain possibilities for lunar origin, those that give an initial orbit which would not have evolved into the present orbit. Specifically, as was earlier pointed out by

MacDonald (1964) and proven more definitively by Goldreich (1966), any hypothesis of lunar origin which starts the Moon in the equatorial plane of the Earth will not fit the dynamical picture and give the presently-observed orbit. This is a firm statement and covers all theories of fission, accumulation, precipitation, etc., and should be taken very seriously. If one wants to overcome this very fundamental and basic objection by means of <u>ad hoc</u> arguments, these <u>ad hoc</u> arguments have to be well-founded and well-proven (see below).

The principal problems with the precipitation hypothesis are dynamical. How, for example, do the silicates precipitate in the primitive terrestrial atmosphere and how do these precipitating particles end up in Earth orbit? If the atmosphere is corotating with the Earth, the particles which condense below the synchronous altitude will simply fall down on the Earth, and particles which condense above the synchronous altitude will be ejected. (Can we really suppose that a thick atmosphere extended well beyond the synchronous altitude?)

But let us assume that all of these rather crucial steps can be explained and handled quantitatively, and that we end up with large chunks of silicates in Earth orbit which slowly accrete to form a Moon orbiting well within 5 or 6 earth radii, perhaps just beyond the Roche limit. This Moon would be in an equatorial orbit. We now have the difficulty of explaining how this Moon evolved into its present orbit. Ringwood suggests a way out that had been originally proposed by Wise (1969); namely, that the Earth's rotational axis was tilted by about 10° to the plane of the ecliptic before the Moon was formed (italics supplied by Ringwood). But this won't solve the problem. The Moon will still assemble in the equatorial orbit, although at a different inclination to the ecliptic; but the total angular momentum will not have the correct value.

A more sophisticated "out" has been proposed by Cameron (1970). But it won't work either. In fact, Cameron gives two possibilities: 1) that the Earth accreted further material afterthe Moon was formed, but that this material was accreted asymmetrically so that the Earth's axis was tilted by 10°. But as can be shown (Goldreich 1965), such an adiabatic change in the Earth's obliquity will maintain the Moon in the equatorial orbit. 2) The other possibility suggested by Cameron is that the Earth was suddenly hit by a large body which tilted its axis by 10°. The Moon could certainly not follow this kind of a non-adiabatic change in obliquity. However, I have calculated what it would take to produce this change and it turns out that, even under favorable conditions, the impacting body would have to have a mass about twice that of our Moon. In other words, we would have to assume the existence of moon-like objects in the vicinity of the Earth's orbit in the solar system. But then why not assume that such an object was captured in the first place?

It seems to me that Ringwood has to contend with two further points. Why is the Earth-Moon system unique in the solar system? Why hasn't the precipitation process operated for Venus? And, finally, why does Venus have no appreciable angular momentum? Was it removed by capture of a moon from a retrograde orbit (Singer 1970)? A prevalence of lunar-sized objects in the early solar system, coupled with a low but finite probability of capture can account in a very satisfactory way for these features.

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