LUNAR STRUCTURE AS DEDUCED FROM MUONG NONG TEKTITES

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One avenue toward the exploration of the Moon's surface is undoubtedly furnished by the fragments of that surface which, on any theory, must fall on the Earth. Because it has no atmosphere, the Moon is exposed to bombardment by particles at velocities up to nearly 80 km/sec. Laboratory experiments show that at these velocities, ejecta will be produced which will travel at speeds in excess of the escape velocity from the Moon, 2.3 km/sec, and which, in the absence of an atmosphere, will leave the Moon and eventually, at least in part, find their way to the Earth.

Since the time of the Dutch mining engineer R. D. M. Verbeek (1897), it has been suspected that tektites form a portion of the debris from the Moon. The idea has been advanced in recent years by the discovery of E. C. T. Chao et al. (1962, 1964) that the nickel-iron spherules that are the hallmark of impact in some terrestrial glasses are also present in tektites. It has also been advanced by the work of D. R. Chapman and H. K. Larson (1963) and E. W. Adams and R. M. Huffaker (1962), who have deduced from the forms of the australites that the bodies arrived in the Earth's atmosphere with trajectories that are inconsistent with a terrestrial origin but not inconsistent with an origin on the Moon.

Recently, important new light has been shed on the problem by the study of the Muong Nong–type tektites. This name was given by Virgil Barnes (1961a) to a class of tektites, found chiefly in Thailand and Indo-China, that have the characteristic that the internal structure is layered. The layering is of somewhat the same nature as the contorted, fluidal structure seen in almost all tektites; but in the Muong Nong material, the structure is arranged in parallel layers and is accompanied by a remarkable appearance within each layer to which Barnes has given the name of "shimmering." The Muong Nong materials have been clearly shown by analysis by Barnes and Pitakpaivan (1962) to be chemically identical with the other tektites (to which Barnes has, for contrast, given the appropriate name "splash-form tektites") except for a minor difference in a state of oxidation of the iron. The resemblance is close enough so that we may be sure not only that the Muong Nong materials are genuinely tektites, but even that they belong to the Far Eastern strewn field. Barnes (1961a) further found that the splash-form tektites, when examined as whole bodies between crossed nicols, show a pattern of strain birefringence that indicates that each one cooled as a unit. There is no such overall strain pattern associated with the Muong Nong material. This fact suggests that Muong Nong tektites are fragments of larger bodies. The same idea is suggested by the fact that Muong Nong tektites are generally found in large associations (Barnes, 1963) weighing tens of kilograms and covering a few tens of square meters. Further support comes from the external shapes of the Muong Nong tektites, which are generally chunky fragments and contrast sharply with the splash-form tektites. The latter, when complete, normally take the forms found in liquid rotating masses: spheres, spheroids, rods, dumbbells, tears, and so forth.

There is every reason to subscribe, therefore, to Barnes's thesis that, in a fundamental way, the Muong Nong materials are more primitive than other tektites and point the way to their origin.

Recently, Barnes (1964) drew attention to the presence of angular voids in some of the Muong Nong tektites, particularly those from Kan Luang Dong. His photograph is shown in Fig. 32. The voids appear black because

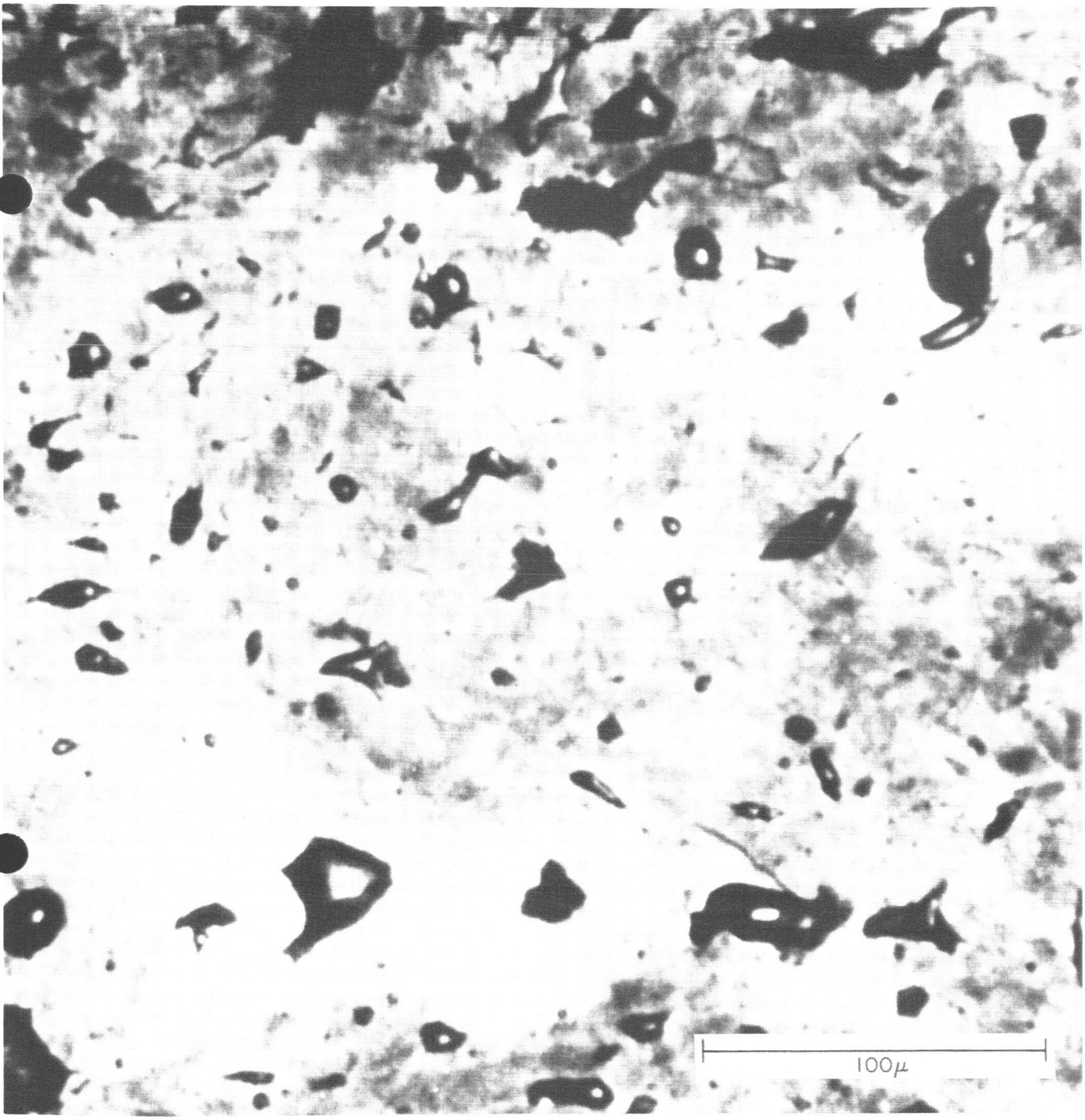

Fig. 32. Angular bubbles observed in a Muong Nong tektite from Kan Luang Dong. Courtesy of V. Barnes. The black marks are voids.

the light is deflected by the bubble surface, except for the central part of some of them, where light can enter the bubble. The significance of the angular voids was pointed out by Barnes, who remarked that they clearly indicated that these tektites had once been some kind of clastic, that is, fragmental material whose grains had been incompletely welded together. The angular voids, therefore, represent the spaces between grains. Barnes pointed out that this implied that these tektites had never been thoroughly melted, since it is well known that voids in a liquid, even a viscous liquid, are spherical or ellipsoidal.

Now this at once opened the way for a decisive test of the theories of the origin of tektites. If tektites are fused sandstone as claimed by Barnes himself (1958), by H. C. Urey (1959), and by S. R. Taylor (1965), then in the Kan Luang Dong tektites we would expect to find the minerals that normally compose a sandstone (Pettijohn, 1957, p. 318), namely, quartz, chert, feldspar, mica, clay, and other minerals existing as separate chemical entities. It is, of course, entirely possible that each of these minerals might have been converted to a glassy form; for instance, quartz to lechatelierite or feldspar to maskelynite as a result of shock. But these materials should, nevertheless, remain chemically distinct. From the appearance of Fig. 32, it is clear that the grain size of the source material was on the order of 50 μ. In Fig. 33, we see an enlarged portion of a similar Muong Nong tektite, from Phaeng Dang. It is evident that this tektite is composed of separate pieces of material that were pressed together while soft. The region chosen contains two inclusions; these permit easy orientation in the subsequent figures. Actually such inclusions cover about 15% or less of the area of this portion of this tektite.

In Fig. 34, we see the results of a microprobe scan, using the K_{α} line of potassium over the region between the two large spots. (The region was chosen so as to permit easy orientation; the inclusions actually cover only about 15% of the section.) The uniform nature of the material is obvious. Remelted bits of feldspar would have shown up as bright spots. Figure 35 is the same kind of thing for silicon; here the silica in the inclusions shows up. Figure 36 is for aluminum; it is uniform outside the black spots (in the inclusions, the voids have become filled with grinding compound, Al_2O_3, which thus

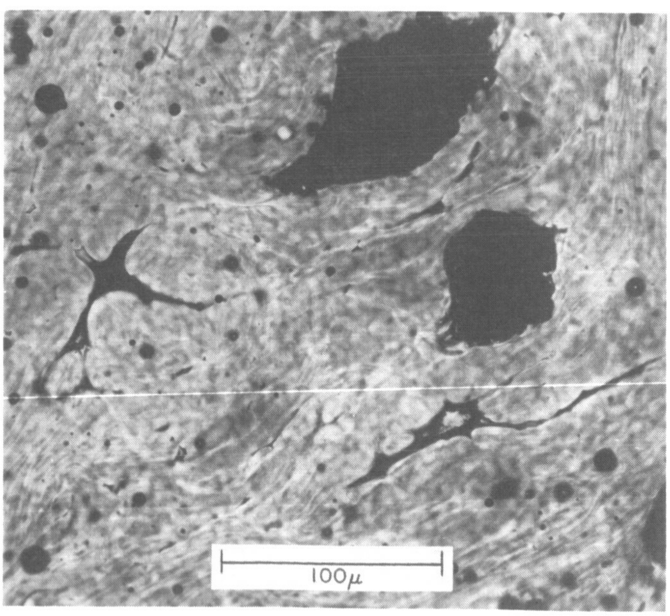

Fig. 33. Microphotograph of a portion of a Muong Nong tektite from Phaeng Dang, showing internal structure. The two large black spots are inclusions. The small, streaky black lines are voids between shard-like glass bodies. The voids do not extend through the section.

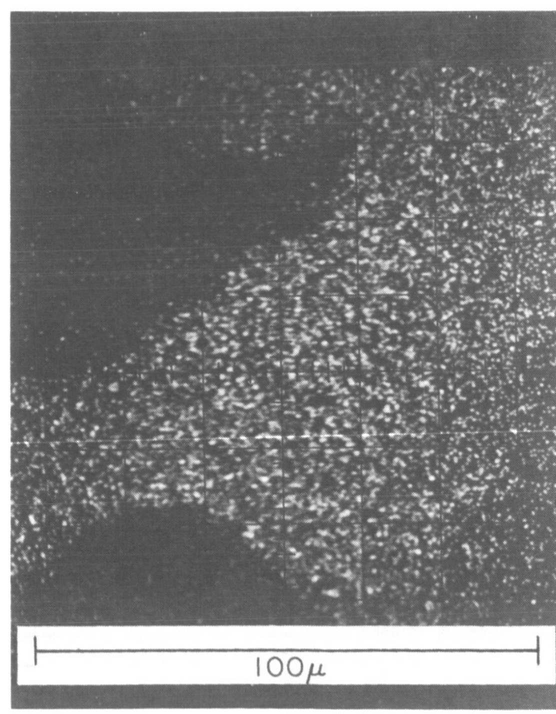

Fig. 34. A portion of the same region as Fig. 33 in the line of the potassium K_{α} line prepared by microprobe scanning using X-ray fluorescence. The image is reversed compared with Fig. 33. Note the homogeneity of the material outside the black inclusions.

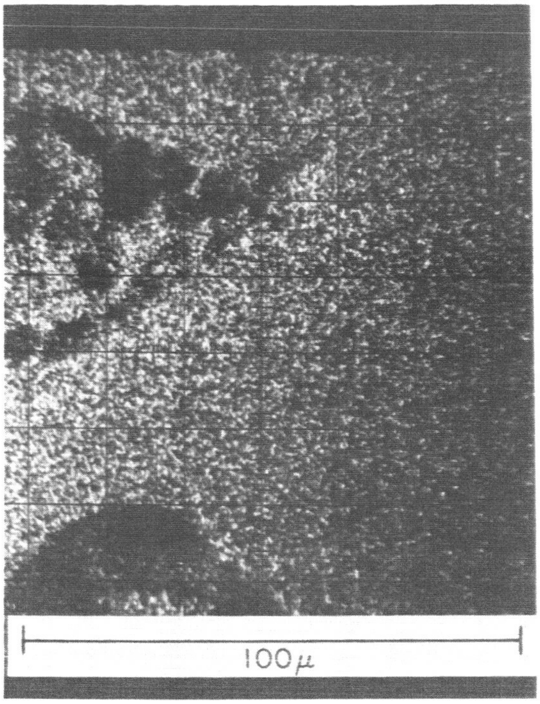

Fig. 35. The same as Fig. 34, for silicon. A network appears in the spots because these are largely composed of vesicular SiO_2 (frothy lechatelierite).

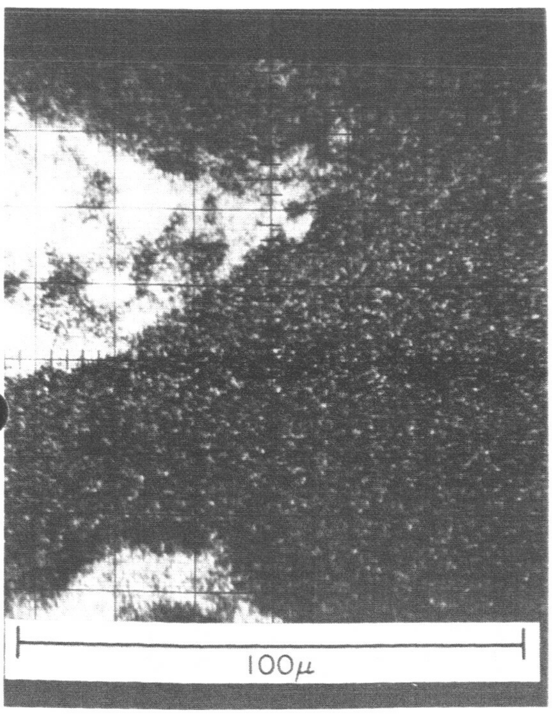

Fig. 36. The same as Fig. 34, for aluminum. Note that the voids in the silica have become blocked up with alumina from the grinding powder.

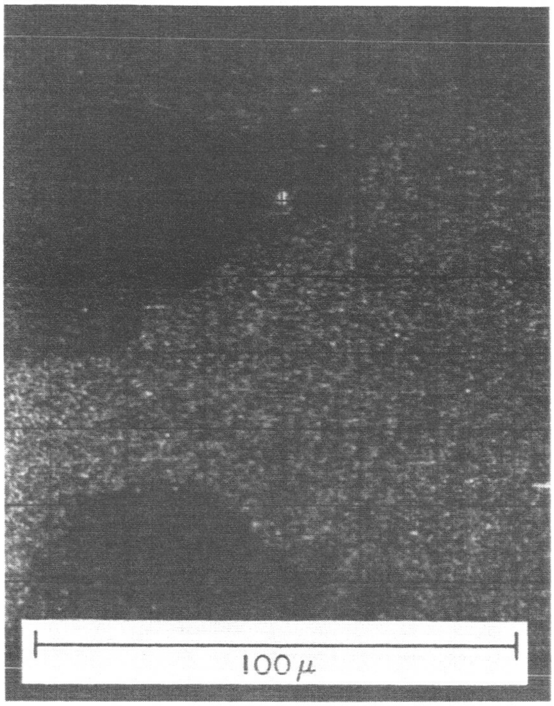

Fig. 37. The same as Fig. 34, for calcium

forms a pattern complementary to the silicon). Figure 37 shows a similar pattern for calcium; a similar pattern for iron is too faint to reproduce.

For comparison, the same techniques were employed on a chemically similar terrestrial specimen kindly supplied by E. A. King, Jr. It is a portion of a tuffaceous sandstone or siltstone, baked by natural fires in an underlying lignite bed. The chemical composition is given by King (1962), who pointed out that it resembles the composition of a tektite (in particular, a bediasite).

In Fig. 38 are similar cathode ray displays for K, Fe, Si, and Al on the Texas siltstone. Each figure is 90 μ across. It is evident that the microprobe has more than enough resolution to show the heterogeneous character of this material, if the grain size is of the order of 50 μ.

We see that the Kan Luang Dong material is chemically homogeneous and unlike fused soil.

While these studies were going on, Dr. L. Walter (Walter, 1965) at the Goddard Spaceflight Center made what is perhaps the most fundamental discovery in the tektite problem. He examined some small inclusions to which Barnes has given the name "frothy lechatelierite," which are found in the Muong Nong material and which, in the specimens he examined, formed perhaps 1% or less

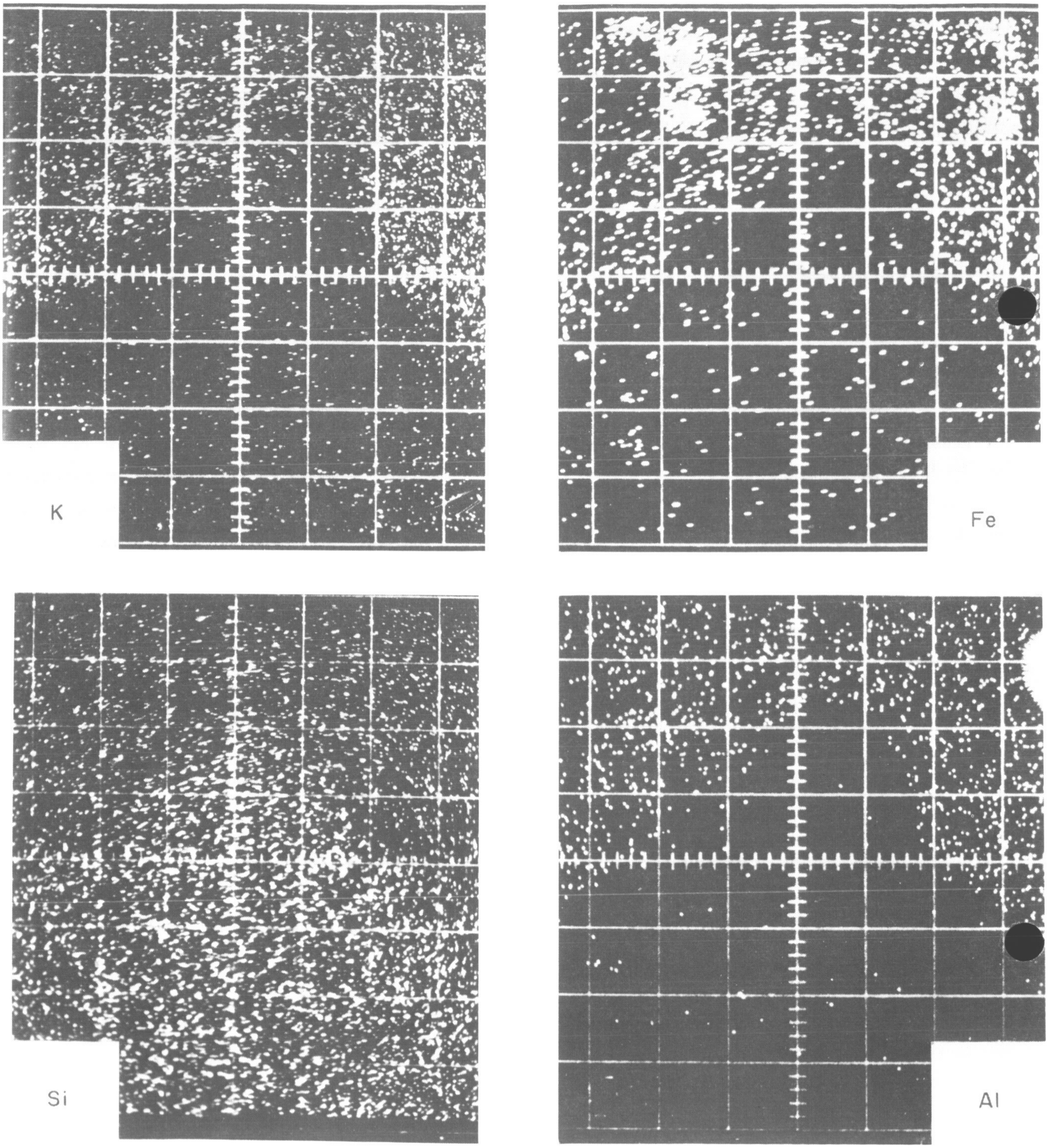

Fig. 38. Microprobe scans for the elements potassium, iron, silicon and aluminum on a sample of Texas siltstone. Note the inhomogeneity as compared with the tektite. Each figure is 90 μ across.

of the total volume. Walter discovered that the brown central portions of these inclusions contain coesite, which is a high-pressure form of silica, unstable at ordinary temperatures. Since the pioneer work of Chao, Madsden, and Shoemaker (1960), it has been believed that the presence of coesite in a natural rock is an indicator of impact by a meteorite. This interpretation is a very natural one in the present case because of the already-mentioned existence of the nickel-iron spherules. It constitutes a welcome confirmation and eliminates for good the arguments of some doubters who thought that perhaps the nickel-iron spherules were the result of some process of chemical reduction in the glass.

[illegible] the coesite has a more fundamental significance because of its instability and especially its tendency to turn into cristobalite. The presence of coesite and the absence of cristobalite constitute, in Walter's phase, a "seal" that guarantees that the material has not been substantially altered since the impact. The kind of heating that would be required to convert a sandstone, for example, into the kind of glass that we observe in the Muong Nong material would convert some of the coesite to cristobalite. In Fig. 39, we show a theoretical calculation of the rate at which a grain of tektite glass would diffuse into a glass matrix compared with some measures by Dachille et al. (1963), of the rate at which coesite disappears. A single observation by Barnes that lechatelierite particles in bediasites are half gone in ½ hour at 1600°C is plotted as a cross. It is clear that we are not likely to get rid of the quartz grains without producing cristobalite.

This theoretical conclusion is fully substantiated by experiment. The problem of dissolving quartz grains in a glassy melt is one of the fundamental problems of glass making. It is known to be a difficult problem, especially in a viscous melt, and one that requires hours, if not days. Recently, Corning Glass Works prepared an artificial tektite glass; it is my understanding that they employed a temperature of 1900°C for 16 hours. By contrast, the maximum quench time permitted by the studies of Muong Nong coesite is about 10 sec at 1700°C. This conclusion is certainly what we would expect. The destruction of the coesite demands no more than the disordering of a crystal lattice, while the dissolving of the quartz grains demands migration of the silica molecules through distances of tens of microns.

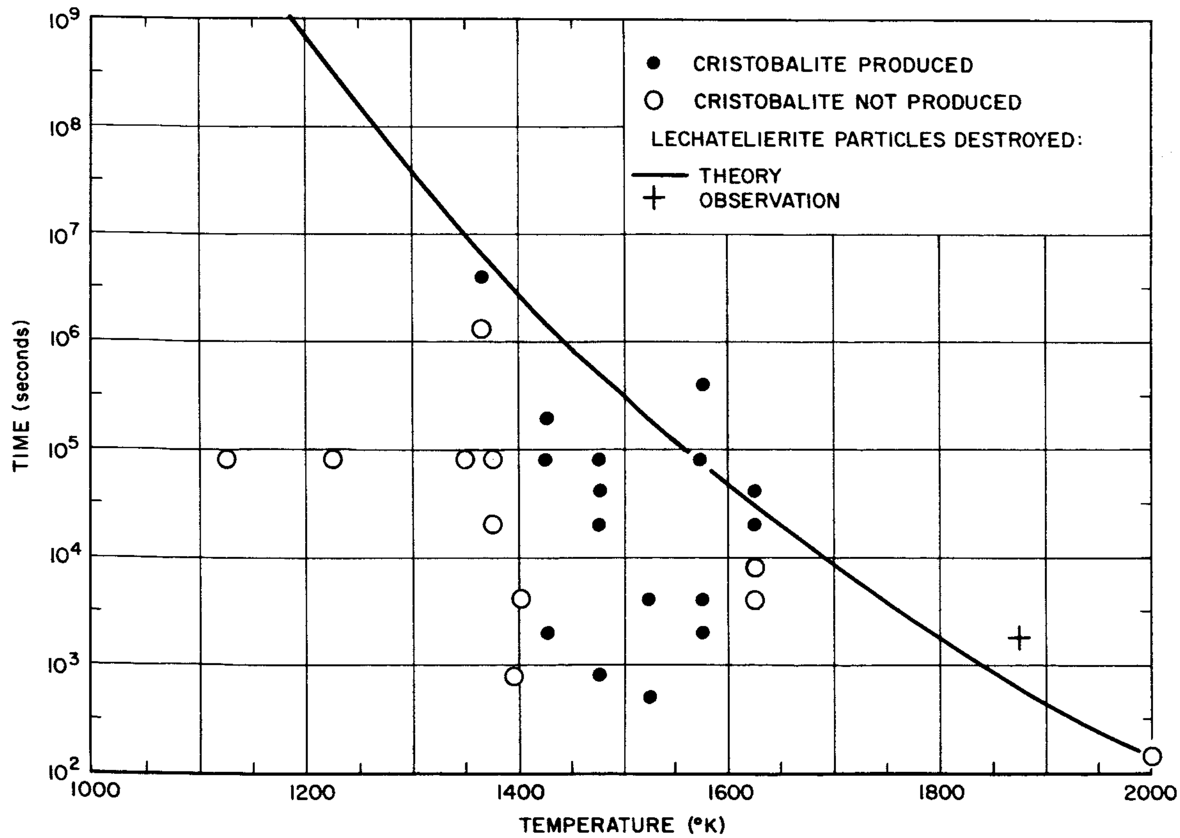

Fig. 39. Production of cristobalite vs destruction of lechatelierite particles

In fact, this migration has not even begun, as can be seen by examination of the borders of these inclusions, (see Walter, 1965, *Science*, p. 1029); the transition from tektite glass to silica glass takes place in a very short distance, much less than the diameter of the particle.

From these data, Walter (1965) drew the vitally important conclusion that the Muong Nong material was glass or finely crystalline before the impact took place. This conclusion is, as Walter has pointed out, difficult to reconcile with the origin of tektites from terrestrial material. Glasses of tektite chemistry are not found at the surface of the Earth. The kind of major changes that would be required to convert a terrestrial rhyolite into a tektite, are, if anything, more difficult to reconcile with the presence of coesite than the melting process that we have already discussed. It is highly probable that, as Taylor (1965) has pointed out, an impact by itself has little effect on the chemistry of the struck material and, in particular, is not competent to convert a rhyolite into a tektite glass. We are thus led to conclude that the impact took place on a glass whose composition was not that found on the Earth.

There is a chain of argument that says that if the tektites are not from the Earth, they must be from the Moon. The argument is based first, on the lack of Al 26 (Viste and Anders, 1962), which should have been produced by primary cosmic ray bombardment if the tektites had been in space for a long time. It is also based on the distribution of tektites, which is best understood as the result of fallout from an orbiting satellite. It is extremely difficult to see how a body could get into a satellite orbit around the Earth unless it were an impact fragment from the Moon. In what remains, we shall therefore discuss the Muong Nong tektites as samples of the lunar surface.

What was the nature of the glass that formed the Muong Nong tektites? From the remarks of Barnes about the angular voids, we are led, in a logical way, to suppose that the glass consisted of some kind of fragmental or clastic material. Clastic glass is called by geologists a tuff. Thus the Muong Nong material is a tuff.

In most specimens of Muong Nong material there are few voids. This could logically be explained if we imagined that the fragments have been welded together as sometimes happens with terrestrial tuff. Then the shimmery structure, to which Barnes has drawn attention, would represent the former boundaries of the bits of glass that formed the ash. If the material is tuffaceous and welded, then it is what we call a welded tuff.

It is possible to make direct comparisons between Muong Nong material and terrestrial welded tuff. In the Muong Nong material, the glass particles are rounded, and the voids are spiny, while in terrestrial welded tuff, it is the glass particles which are spiny.

On the Earth, welded tuffs are normally produced by a process known as ash flow (Ross and Smith, 1961), a volcanic eruption in which the tiny ash particles, instead of floating downward through the cold air and arriving at the ground in the solid state, are immersed in the hot gas and arrive at the final point of deposition still hot. Two modes of transport are possible. In some ash flows, such as the great explosion of Mont Pelée in 1902 (Lacroix, 1904) there is a great volume of gas and very little ash. The small amount of ash, nevertheless, provides coherence for the gas by impeding the spread of the gas molecules in all directions and provides weight for the gaseous mass. The mixture of gas and dust behaves like a fluid, moves toward the lowest point of the topography, and there settles out. Deposits of this kind on the Earth are not normally welded and are, on the Earth, relatively unimportant.

A second mode of transport may be called the dense phase. In the dense phase, we have mostly solids and very little gas. The gas is just sufficient to separate the particles from one another so that the friction is greatly reduced or disappears. The mass behaves like a liquid with a more or less definite upper surface and a density of the order of 1 or 1½ g/cm³. This pseudo-liquid moves across the topography, finds a suitable bed, stops, and then collapses, first through the escape of gas from the solid particles and then through the compression of the solid particles one on the other. The solid particles when emplaced are still at a temperature of 850°C, according to the estimates of Boyd (1961). They are still plastic; welding can and does take place.

The application of these ideas to the Moon might, at first sight, seem impossible since it might seem that in a hard vacuum, the gas would immediately escape. It was first pointed out by Lyman Spitzer (1941), that the escape of gas from particles in a vacuum is a much slower process than we would have thought. It is delayed, in part, by the charging up of the solid particles at the expense of the gas. The resulting difference in charge means that very large voltages will develop if there is any significant separation of solids from gas. The internal particles, therefore, in seeking to escape from the gas, will encounter not only the solid particles but also the charged gas molecules to obstruct their way.

A physical analysis by O'Keefe and Adams (1965) shows that the behavior in the dense phase is not unlike that in the terrestrial fluidized material. The differences between the lunar and the terrestrial case are mostly in favor of the process of fluidization. The lower gravity means that the solid particles are more easily carried. It also means that as we go down through the ash flow, the pressure increases less rapidly than in a terrestrial flow. Hence the density also increases less rapidly. Now it is a paradox of the kinetic theory of gases that the viscosity of a gas is independent of its density. Since the gas viscosity is the principal agent responsible for supporting the particles, it follows that a low-density gas can support as much solid matter as one of higher density. There is, of course, a limit. When the mean free path becomes of the same length as the width of the passages between solid particles, the viscosity and thus the supporting power of the gas breaks down. But for ordinary levels of density, the lower pressures of the Moon and the resulting lower densities are pure gain. That, in conjunction with the lower weight of the particles, means that then a given amount of gas will fluidize about 30 times as much solid matter on the Moon as on the Earth.

As we approach the top of the densely fluidized layer, the diminishing pressure and diminishing density must be compensated by an increasing gas velocity in order to carry off the flux of gas that has been generated below. Hence the gas density cannot go to zero; if it did, the velocity would have to go to infinity. The limit is reached when the upward velocity of the gas exceeds the terminal velocity of the small solid particles, which are then carried upwards from the dense phase into the dilute phase we previously mentioned. Because of the absence of a lunar atmosphere, we will always have a dilute phase above a lunar ash flow; calculations seem to show that this dilute phase may carry a substantial amount of the material.

Now this gives us a fairly good explanation of the softening that has been observed in the outlines of lunar craters from the *Ranger* photographs. This softening is often attributed to erosion but there are powerful numerical reasons for thinking that erosion will not do the trick. We need at least 20 or 30 m of softening power even over craters that must be relatively recent; but from considerations of the radiation darkening of the Moon (Wehner, 1965) it appears unlikely that the Moon's surface is being eroded at a rate greater than about 1 meter per billion years. It is of no use to say that the erosion rate might have been more rapid in ancient times, because the cratering rate would also have been greater in those ancient times. What we observe on the *Ranger* photographs is that there have been 20 or 30 m of softening since the majority of craters in the range of 50 to 500 meters were formed. It is quite logical to explain this softening in terms of a layer some 20 or 30 m thick spread over the existing craters. Quite possibly this layer is not unique; no doubt there were a number of layers, as, in general, there are a number of separate flows in any large deposit of welded tuff. The important point is that the flows had a definite and limited thickness so that large craters in this region retained their original shape while the middle-sized craters were softened or even partly destroyed. Craters of less than 50 m in diameter are, it appears, completely obliterated by the flow. You will see that this implies that the flow was of the type that I have called a dilute flow, that is to say, it was a dusty gas. Had it been a dense flow of the nature of a liquid, then the hollows would have been completely filled with liquid as long as there was enough to cover the highlands. If, however, we are dealing with a pseudo-gas, then the amounts of solid material per square centimeter would be approximately the same, over the smaller terrain features. The amount of gas in a crater 2 or 3 hundred meters deep would be only a few times the amount on the level ground if the scale height in the gas were a few hundred meters as calculated by O'Keefe and Adams (1964).

After the deposit of the ash, new craters were formed that were naturally unaffected by it. We see these as the smaller craters on the lunar surface simply because there are always more little craters than big ones, but the little craters that occurred before the ash flow have been effaced. It is a striking phenomenon in the study of the *Ranger* photographs that craters in the range of a few meters look more like those in the range of a kilometer or so than like those in the range of a few hundred meters. The large craters were too big to be smoothed; the little ones, too recent.

In connection with ash flows, we should like to draw attention to the interesting possibility that the red spots, which have been observed by Kozyrev, Greenacre (1963), and others, on the Moon, may be manifestations of a sort of lunar lightning. There are two observations of the spectra of these spots, both by Kozyrev. The first of these (Kozyrev, 1958) indicated a spectrum that might be explained by C_2. The second (Kozyrev, 1963) was considerably more detailed and indicated the presence of the molecule H_2. The latter is in all ways more plausible, especially because it gives rise to a red illumination. Water is the principal gas emitted in volcanic outbursts. In his 1963 paper, Kozyrev stated that the source of the

H_2 could not be water since this would dissociate into H and OH, while he did not observe the lines of H. On the other hand, the excitation of water vapor is one of the standard methods of producing the H_2 spectrum. The OH bands are not obvious in the visible spectrum. It appears possible, in spite of the absence of atomic hydrogen lines, that the H_2 is due to water vapor, provided that we can find a suitable method of excitation.

On the other hand, there is a serious problem arising from the fact that the bands of the H_2 molecule that are visible in this spectrum come from levels 15 v above the ground. The production of such a large amount of high-energy quanta is a genuine puzzle. The difficulty can perhaps best be seen by keeping in mind that the red spots have been seen on the bright portion of the Moon's surface. It follows that, over the areas on which they are seen, the amount of light coming from the red spots was a substantial fraction of the total amount of sunlight being reflected by the Moon at these points. It is known that the reflection efficiency of the Moon is on the order of 7%. The energy supplied by the Sun is on the order of 1.3 million ergs/sec cm^2. The light reflected by the Moon is therefore perhaps 100,000 ergs/sec cm^2 and the light supplied by the red spots would have to be a substantial fraction of this. The total amount of radiation supplied by the Sun in the ultraviolet region where 15-v quanta are available is of the order of 10 ergs/sec cm^2. The energy supplied by particles in the form of the solar wind is likewise of the order of 1 or 2 ergs/sec cm^2. Thus the Sun is utterly incapable of supplying the necessary energy. If, however, we suppose that the source of the energy is some kind of volcanic outburst, then it is obvious from terrestrial experience that the energy flux may greatly exceed the flux from the light of the Sun. In a terrestrial volcanic outburst, a portion of the energy goes into lightning through mechanisms which are interpreted as static electricity but whose precise nature is not known. It turns out that whenever a suspension of small solid particles is produced in a gas we are very likely to have manifestations of static electricity. Examples, in addition to thunder storms and volcanic lightning, are the lightning associated with dust storms and the manifestations of static electricity in dusty factories.

It is perhaps significant that the observations of the red spots are associated with regions that appear to exhibit contemporary volcanism. These morphological intimations are so strong that Mrs. Cameron (Cameron, 1964) predicted the appearance of red spots near the Cobra head in advance of Greenacre's report. Near Alphonsus, the *Ranger IX* photographs show plainly that recent volcanism has been at work.

We conclude therefore by suggesting that the hypothesis of ash flow processes on the Moon provides at the same time a reasonable explanation of the structure of the Muong Nong tektites, the morphology of some lunar craters, and the observations of lunar red spots.

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SUMMARY REMARKS ON THE MOON

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In spite of the beauty and remarkable clarity of the *Ranger* photographs, the attitudes of the eleven invited speakers on the Moon differed markedly, indicating the truth of Professor Gold's remark (as quoted by Professor Urey) to the effect that the *Ranger* pictures are a mirror in which every man sees himself. The participants in the discussion appeared to agree that there really is a Moon. But beyond that single statement of fact, near-unanimity of opinion was not often evident.

Concerning the great majority of lunar craters, it is now widely agreed that they were formed by meteorite impact. The curve of frequency of craters as a function of size follows, in a general way, that which one would expect on the basis of the observed sizes of terrestrial meteorite falls. It was emphasized, however, that among the smaller craters there are large numbers of secondaries presumably formed by ejected material. It was also emphasized that, in some areas, the numbers of smaller craters appear to be in a "steady state"—that is, old ones are disappearing as rapidly as new ones are being formed.

There was considerable discussion and debate concerning a variety of other lunar surface features including crevasses, dimple-craters, features thought to result from collapse, and features thought to result from erosion, as well as some that might have had their origin in tectonic activity. The existence of caves at the bottoms of some of the *Ranger* photo craters was suggested. Lava flows were discussed at some length as possibly accounting for certain apparently smooth areas, but in general it was conceded that it is difficult to prove beyond reasonable doubt the volcanic origin of any particular lunar surface feature. This listener emerges from the discussions with the distinct impression, however, that with the exception of impact craters and related features, it will be some time before the scientific community achieves something approaching a consensus concerning the origin and evolution of a substantial fraction of those surface features of the Moon that can now be identified in photographs.

The fragmentary nature and diversity of the formations on the lunar surface are emphasized by Earth-based radio and optical observations. Heat scans of the Moon during eclipses and during the lunar night show that on the whole the lunar surface is highly insulated, but that there are wide variations in conductivity. Different maria appear to have different thermal properties. Numerous "hot spots" have been found that do not necessarily correspond to the distribution of craters. Craters that appear exactly the same in photographs may have quite different thermal properties. There are also systematic differences between maria and highlands.

The thermal behavior of the Moon indicates that although the surface is very heterogeneous, about 99% of it must be covered by at least 1 mm of dust. Lunar radio emission, too, shows that there is a systematic difference in emissivity between the maria and highlands.

Measurements of radar scatter as a function of polarization indicate that there is a layer, in most areas 25 cm or more in thickness, of dielectric constant much lower than that of solid rock. It was stressed, however, that radar scattering data from the Moon can often be interpreted to support almost any model.

Recent researches reported by colleagues from the USSR suggest a porous layer on the Moon some 3–10 m in depth and an internal heat generation per unit mass some 4–5 times greater than that from the Earth.

This great heat generation, if it is real, can only be accounted for if it is assumed that the concentration of radioactive elements in the Moon is 4–5 times greater than in the Earth.

Conflicting views were presented concerning the nature of the lunar interior. Two views were expressed to the effect that a molten state of the lunar interior is incompatible with the observed deviation of the figure of the Moon from hydrostatic equilibrium. On the other hand, new thermal history calculations using revised abundances for radioactive elements in the Earth indicate that the Moon might have started melting fairly early in its life and that it might have undergone severe, radical differentiation some 3 billion years ago. An equation of state appropriate for the Earth predicts a density decrease with depth in the lunar interior.

One participant believes that the Moon and Earth have identical compositions and that Earth's core is composed of liquid silicates. Another has given evidence strongly indicating that the Earth's core is metal and that the Moon and Earth are quite different in their chemical make-up.

There was no consensus as to the mode of origin of the Moon. Participants agreed that it could have been formed elsewhere, then captured by the Earth; that it might have escaped from the Earth; or that it might have been formed together with the Earth in a two-body system.

In short, the participants raised questions far more than they indicated generally acceptable answers.

In the view of this participant many of the questions that were raised and that are still with us will not be answered satisfactorily until we make new kinds of observations with new lunar probes. Some of the questions that badly need answering include:

1. What do lunar landscapes look like in selected maria and in selected areas of the highlands?
2. How old are the major lunar features?
3. How thick are the layers of lunar dust and porous unconsolidated materials?
4. Is there really volcanic activity of sorts on the Moon as indicated by observations in the USSR?
5. What is the chemical composition of the lunar crust and how does it vary?
6. Do one or more groups of meteorites have their origin in the Moon, ejected by collision with asteroidal or cometary bodies?
7. Is it possible, as suggested by one participant, that tektites come from the Moon?
8. What are the causes of the color differences noted on the Moon?
9. What is the heat flow through the lunar crust?
10. Is there seismic activity on the Moon?
11. Will seismic experiments reveal that the Moon is differentiated?
12. Will we find that the density within the Moon increases or decreases with depth?

Not until we answer such questions will we really understand the Moon in relation to the Earth and the other terrestrial planets.

PART II. JUPITER

M. M. Komesaroff, Recent Radio Observations of Jupiter

G. L. Berge, Interferometry of Jupiter in the Decimeter Range

J. W. Warwick, Theory of the Jovian Structure

A. G. Smith et al., Jovian Rotation Periods and the Origin of the Decametric Burst Structure

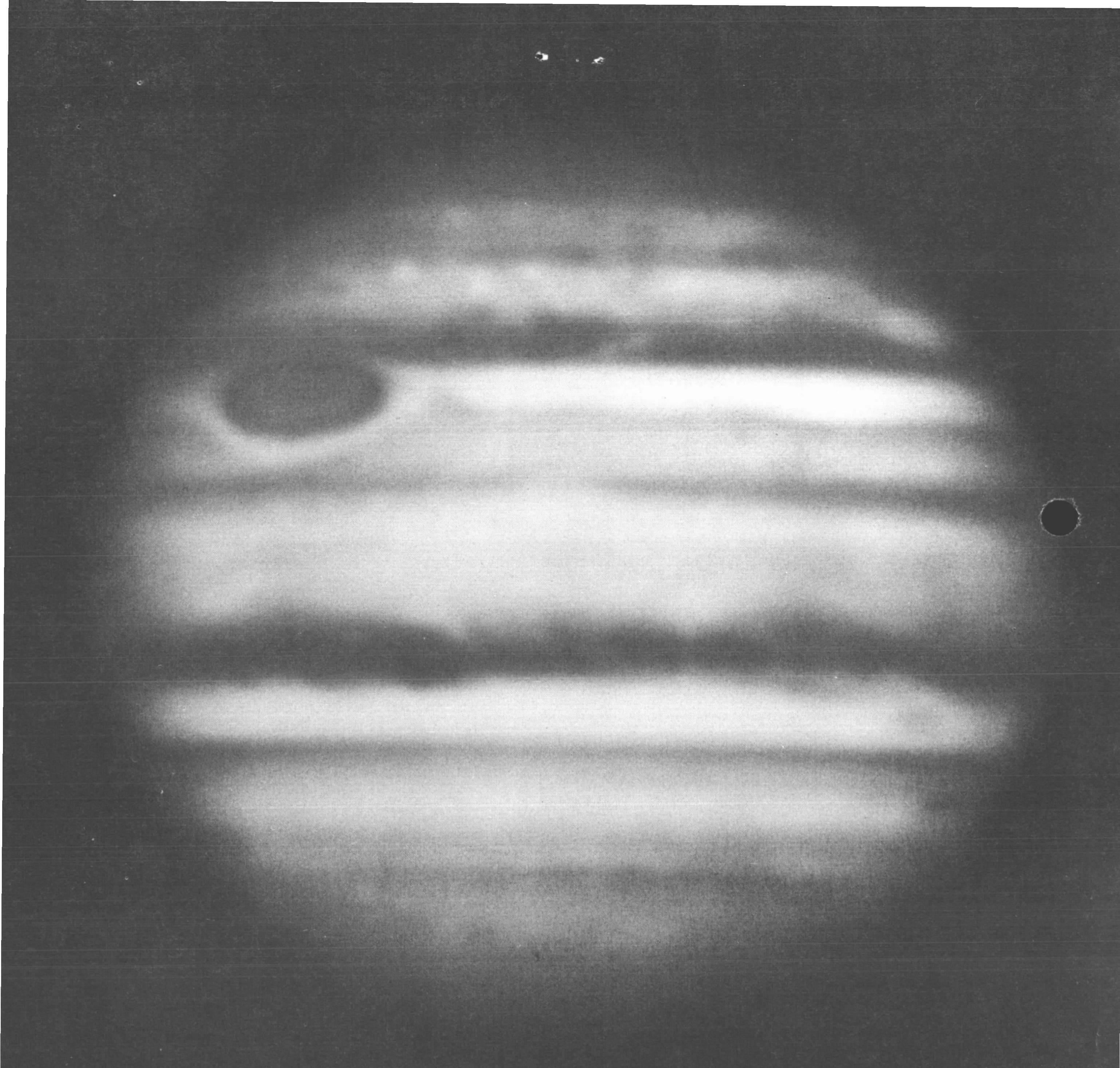
G. A. Dulk, The Effect of Io on the Radio Emission of Jupiter

L. Davis, Jr., Comments on the Discussion of Jupiter

G. B. Field, Remarks on Jupiter

G. J. Stanley, Summary Remarks on Jupiter

OVERLEAF: *The planet Jupiter, in blue light (200-inch photograph). (Courtesy of Mount Wilson and Palomar Observatories)*



II. JUPITER