ment of Blandau (*3*) in which the deposition of a vaginal plug was shown necessary for sperm transport. Subsequent work (*4*) has shown that a number of intromissions prior to ejaculation are also necessary for sperm transport to follow ejaculation. Implicit in these studies was the idea that sperm were transported into the uterus within a matter of seconds after ejaculation (*5*). Blandau says, for example, "It thus seems certain that at the time of ejaculation the spermatozoa of the rat are normally propelled in masses through the cervical canals into the uterine cornua . . ." (*3*, p. 263).

In our study (*1*), we found that an immediate, permanent ingress of sperm does not automatically follow ejaculation and deposition of a plug, and that copulatory behavior itself may inhibit the ingress of sperm. Current investigations indicate that sperm do not normally reach the uterus in maximum amounts immediately after ejaculation. [During the first 6 minutes after ejaculation, female rats had on the average 68×10^5 sperm ($n = 9$). Females killed from 6 to 8 minutes after ejaculation had 498×10^5 sperm in their uteri ($n = 8$).]

Finally, Dziuk faults our interpretation of our double-mating experiment, stating that pregnancy could not have been disrupted by copulatory behavior because "litters of normal size resulted from matings with the second male." This is precisely what one would expect since the second male's ejaculation was not followed by more copulatory stimulation. Dziuk cites his and other's work on cattle and rabbits (*6*) [species in which the males do not deposit solid, coagulating vaginal plugs as male rats do (*7*)]. He correctly states that several factors may influence which male's sperm ultimately fertilize a female's eggs (for example, the sperm's time in the uterus and the superiority of one type of sperm over another). In fact, in our study, the pigmented male's sperm are normally at a competitive disadvantage with respect to the albino male's sperm; the only case in which the pigmented sperm "win out" is when the pigmented male rat begins stimulating the female rat soon after the albino male has ejaculated. (Dziuk's reference to capacitation involved hours, not minutes.) We conclude that postejaculatory cervical stimulation is contraceptive in rats; it inhibits the effects of a previous ejaculation.

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Tektites from the Earth

Recently, O'Keefe (*1*) has published another report in which he tries to maintain that tektites come from the moon, and, as usual, he has attempted to answer my argument for the low probability of objects coming from the moon and arriving on the earth in a localized area (*2*). He compares an unusual rock of the moon—in fact, parts of the unusual rock of the moon—with some unusual tektites. He finds rough agreement for the more abundant elements and no evidence for agreement for the less abundant ones. He mentions Taylor's (*3*) work in a reference but does not discuss his results. Taylor found a rock in Australia, a subgraywacke (which is a muddy sandstone), which for some 45 or 50 elements agrees in composition remarkably well with the abundant class of tektites found in that area. Taylor did not maintain that this rock was the particular one that produced the tektites, nor did he maintain that the tektites came from material in Australia. Subgraywacke is rather a common form of sandstone. O'Keefe's handling of the data, as has been usual for 10 years both by him and by others, is of a very partisan character, and he has not considered the high improbability of the lunar origin. The rocks of the moon, at the present time, would seem to indicate that tektites have not come from the moon.

O'Keefe proposes that tektites are propelled from the moon by volcanoes (*1*). It would seem likely that this process, requiring a velocity of at least 2.38 km/sec for the objects expelled, would probably be produced only by rather large, vigorous volcanoes and only in a vertical direction from the lunar surface. At a velocity of 2.38 km/sec, the objects expelled would travel in orbits near that of the moon, since they would have the angular momentum of the moon.

If the object were propelled in the forward direction from the moon at a velocity greater than 2.60 km/sec, it would leave the earth-moon system. On the other hand, if it were propelled in the backward direction at a velocity of 2.60 km/sec, it would fall directly to the earth. If we assume that it were expelled by a volcano in other areas of the moon and in various directions at velocities between 2.38 and 2.60, it would surely remain in the earth-moon system with orbits quite different from that of the moon. If the objects were to hit the earth, very special directional velocity considerations are required to make a direct hit on the first pass. My remarks are, of course, based on the assumption that the moon moves in a circular orbit, which is approximately true. If the object should be propelled with high velocity, it is quite easy to calculate the probability that it would be captured by the earth, since it is only a matter of the angle subtended by the earth at the moon. In this case, with an equal probability of the objects' being propelled in all directions, it would mean that about one in ten thousand would arrive at the earth. As O'Keefe states, there is a focusing effect of the earth's gravitational field for low velocities; hence, the probability would be somewhat greater than this. What appears to be true is that one must expect that a great variety of objects of various kinds would be propelled from the moon on the basis of a reasonable probability and would move in a great variety of orbits in the earth-moon system. They would cross the orbit of the moon, of course, having originated at the moon, and pass near it; they would then be captured by the moon, thrown out of the earth-moon system by interaction with its gravitational field, or thrown into orbits such that they would hit the earth. On the basis of this probability, we would expect tektites to be found in terrestrial deposits of all ages on all parts of the earth. Also, one must expect to find many well-preserved large volcanoes on the moon, which is certainly not true.

For some years, collisions of objects with the moon have been proposed as a method of getting tektites from the moon. We see from the ray craters on the moon that materials thrown out in these great collisions travel with rather low velocities. It may be that there is evidence for circumlunar objects propelled from Tycho as a means of explaining displaced rays relative to the center of the crater. These must have moved with velocities between 1.68 and 2.38 km/sec. However, the rays are mostly rather short, and, hence, velocities were mostly less than 1.68 km/sec. Thus, collisions have produced some rather slow-moving objects from the moon. It is, of course, probable that some of them escaped, and, if they did, my remarks in regard to the directional effects for the proposed volcanic origin apply here. They could escape from other parts of the moon and, if they had the correct direction and correct velocity, could collide with the earth. Again, this possibility is exceedingly improbable, and one would surely expect to find many Tychos or many recent large craters on the moon of this type if the four sets of tektites that are generally recognized on the earth's surface originated in this way, and many collisions of similar magnitude should have occurred on the earth. As stated above, it seems most improbable that tektites are coming from the moon. This is true on the basis of the mechanics but, also, on the basis of chemical composition as we know from the Apollo 11 data.

I should like to mention again briefly the possibility that cometary collisions with the earth are responsible for tektites (*2*). They must occur occasionally. One small comet apparently collided with the earth in 1908 in Siberia. Comets would produce an enormous blast of gases and, if they were large enough to penetrate the atmosphere of the earth to its surface, would produce substantial collisional effects on the earth; a blast of gases that should carry small objects to great distances would subject them to high temperatures, probably producing melting, reduction of iron (even perhaps to metallic iron), degassing of water, and so forth. Tilton (*4*) showed that the isotopic composition of the lead of tektites approximates the composition of modern terrestrial lead and thus is consistent with a terrestrial origin of tektites, as he concluded. Recent work on lunar samples shows that tektite lead is not similar to lunar lead (*5*). Barnes (*6*) has discussed the mineralogy and general geology of tektites in great detail, and he concluded that they are most probably of terrestrial origin. Moreover, Taylor's studies show that the chemical composition of materials found on the earth will agree approximately with those of tektites. The more glassy or acid ones with high silica content and high aluminum content would be very viscous liquids and would, in the melted state, hold together under the blast of gases. Also, the more basaltic type of rocks would probably be melted and disrupted into exceedingly fine particles, just as are found in the microtektites that have recently been discussed. It seems there is no reasonable objection to the cometary origin of the larger tektites from terrestrial materials of the graywacke type and related types of rocks, which occur in abundance on the surface of the earth, or of the microtektites from terrestrial basaltic rocks. The mechanics of this process were discussed some years ago by Lin (*7*), who is an exceedingly capable person in this particular field of high velocity gaseous physical phenomena. The failure to find a crater somewhere in or near Vietnam, where massive tektites have been found, is an unsolved puzzle. Faul (*8*) reviewed much evidence in regard to this problem and showed that the dates of the moldavites and the Ries and Steinheim craters and the dates of the Ivory Coast tektites and Lake Bosumtwi were closely the same. Zähringer (*9*) shows that the gases in tektites have terrestrial compositions and not those characteristic of lunar samples.

To me, it has been most depressing that so much effort has been put into the very improbable "tektites from the moon," and that this has discouraged efforts in regard to the undoubtedly correct origin from the earth and the possible mechanisms of distribution. However, the evidence at last shows that the correct origin was outlined in the 1950's and possibly now the outstanding work of Barnes, Tilton, Faul, Lin, and others, as well as the suggestions of the writer, will be recognized.

It seems to me that the problem of the lunar origin of tektites might be closed out, since we now have definite information in regard to the composition of the moon, due to the work of Turkevich *et al.* (*10*) and to the many people who have worked on the lunar Apollo 11 and 12 samples (*11*). It has seemed unnecessary to me to discuss this subject, and I am surprised that it is again being brought up by anyone.

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The formation of tektite glass by meteorite impact from a subgraywacke is impossible. The proof follows.

The word subgraywacke is due to Pettijohn (*1*), who defines it in part as having a sand framework, plus no more than 15 percent of fine-grained matrix. He further defines sand as particles in the range of 1/16 to 2 mm. From Pettijohn's charts, it is clear that, in most sandstones, the mean diameter of the particles exceeds 125 μm; he further states that the void space between the grains is 30 to 35 percent of the total volume. In this size range, he finds that over 90 percent of the grains are composed of a single mineral.

Varshneya (*2*) has studied the diffusion of silicon and iron in tektite glass; he finds that the conversion of the Henbury subgraywacke to tektite glass would take at least a 4-hour soak at 1800°C. Let us make a calculation from his data for the problem of meteorite impact. His graph for the diffusion coefficient D of silicon can be expressed in units of square centimeters per second as

$$\log_{10} D = -1.0 - 13{,}500\ (1/T)$$

where T is the absolute temperature. Adams and Spreuer (*3*) have calculated cooling curves for tektites radiating into space. The measurements of strain in tektites by Hammond (*4*) indicate rough agreement with these curves and suggest that they had the right mechanism. By combining the two curves we calculate $\int D\,dt$ from the boiling point of tektite glass at 2600°C (*4*) to a temperature of 1400°C, below which diffusion is negligible; the result, for a sphere with a diameter of 1 cm, is 7×10^{-7} cm^2. The usual diffusion equation is

$$Dt/a^2 = Ne$$

where t is the time, a is a characteristic length, and Ne is the Newton number. Carslaw and Jaeger (5) show that for a sphere the Newton number should be taken as 0.2 for the case when the initial irregularity has been reduced to about half. Substituting our integral for Dt, we find that the characteristic length for diffusion in our problem is around 25 μm.

This short diffusion distance is not enough to homogenize a sand grain; but even more is demanded. Varshneya finds that for a particular Thailand tektite, the homogeneity is of the order of 1 percent in silicon, over distances of at least 1 mm. The tektite has 72 percent SiO_2; a normative analysis shows that the assumed parent sandstone should have had about 44 percent quartz. A statistical analysis will then show that approximately 400 grains of the initial sandstone must be melted to reduce the purely statistical fluctuations to the observed level. When 30 percent is allowed for void space, these grains would fill a sphere about 500 μm in radius. There is thus a discrepancy of 20 in distance, or 400 in time, between the actual range over which homogenization must occur and the attainable range from meteorite impact.

Some benefit could be gained by stirring. In some moldavites, Barnes (*6*) has found that grains of lechatelierite have been drawn into threads that are centimeters in length. In most cases, however, the lechatelierite particles are lenticular and perhaps three times as long as they are wide. In this case, the shorter dimensions are reduced by a factor of about 0.6 to 300 μm, leaving a discrepancy of a factor of 100 in the times. For more viscous glasses (*7*) the factor would be nearer 1000.

A further problem arises from the presence of volatiles such as water. Pettijohn quotes water contents of 0.7 to 2 percent for subgraywacke; the lower figure, at 1800°C, would produce a volume of steam at atmospheric pressure which would be 50 times the volume of the melt and would turn it to foam. The problem is well known in glassmaking; it is particularly serious in the present case because tektite glass is very viscous and because, once the tektite is on a ballistic trajectory outside the atmosphere, there is no gravitational field to expel the bubbles.

In the actual case, Varshneya in making artificial tektite glass heated his batches for 60 hours at 1550°C. Although he used analytical reagents, two out of three batches were still bubbly; he therefore crushed them and soaked them for 4 hours at 1800°C; one still remained bubbly even after another heating.

These difficulties cannot be met by assuming that tektites were melted at much higher temperatures; the vapor pressure curves of Centolanzi and Chapman (*8*) show that at 1800°C, in a hard vacuum, a tektite will lose about 1 cm per 400 seconds by volatilization; and the rate of volatilization increases at about the same rate as the increase of diffusion coefficient, or the decrease of the viscosity.

It cannot be supposed that tektites were produced by volatilization followed by condensation because the schlieren, or internal layering, in tektites, is abruptly cut off by the surface. This surface is very nearly the original surface, as witness the surface manifestation called "fingers" (*9*). A body that condensed in space would be expected to show schlieren parallel to the surface, as in a hailstone or as in the Camac spherules from the Alamogordo nuclear explosion (*2*). The abrupt termination of the schlieren suggests that the splash-form tektites were detached in the plastic condition from larger bodies; the observation of long tails on some indochinites supports this idea.

Chao (*10*) and von Engelhardt and Stöffler (*11*) have shown that shock does not instantaneously homogenize glass. The instantaneous effect is to destroy the crystal structure at the level of a few angstrom units. When there is homogenization, it is the result of residual heat left behind the shock and is a problem of time and temperature, as discussed here.

In short, there is no way by which impact can produce lumps of dense, homogeneous glass and launch them into a long trajectory.

Other points must be dealt with briefly. Urey's dynamical argument means that we must think of a lower limit to the velocity of ejection; this may be reasonable for a volcano. About 1 percent of the Apollo material consists of an interstitial sialic rock, which resembles tektites in being aphanitic, anhydrous, reducing, low in Na_2O, and peraluminous. Barnes (*12*) attacks the contention of Faul that moldavites come from the Ries. Lin (*13*) estimates the size of the crater needed to form terrestrial tektites as 300 km in diameter and 40 km in depth; he suspects that it would still be traceable.

The gases found by Zähringer's colleagues, Mueller and Gentner (*14*), appear to have leaked in. The pressure is too high for gases trapped from the atmosphere at, say, 1800°C. Some resemble soil air—that is, the O_2-poor and CO_2-rich gas found in soil.

The trace element differences are instructive; they tell us that the tektites now found at the earth's surface come from some region other than the source of the Apollo basalts, perhaps the deep interior of the moon; if tektites are volcanic in origin, that source is plausible and is supported by the very magnesian bottle-green microtektites (*15*).

The study of tektites has not been futile; for instance, in 1965 Walter (*16*) predicted dry, volatile-poor, iron-rich lunar basalts of low oxygen partial pressure, containing bits of native iron, associated with small quantities of dry, highly silicic rock of low albite content.

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