for the case of the moon because of the absence of horizontal deformations.

5) The moon loses very little gas at the present time. Many terrestrial volcanoes send out bursts of millions of tons of gases and smoke, and yet 1/100,000 of these amounts would be in the range of giving the moon a temporary atmosphere that can be discovered by optical or radio means. The presence of liquid rocks at a shallow depth at the present time seems to be excluded by the paucity of any gas emission.

6) The mean density of the moon is lower than that which it would have if it were initially composed of the same material as the earth. One has therefore to suppose that some type of differentiation had taken place before the formation of these two bodies, and it thus cannot be regarded as certain that any differentiated material was differentiated on the body where it is now found.

7) The value of C/ma^2 (where C is the largest moment of inertia; m, the mass; and a, the radius) is now known to be close to 0.4, indicating the absence of any substantial central condensation. Although this is perfectly compatible with the formation of a differentiated crust, it does indicate that any melting was limited and did not lead, as in the case of the earth, to the formation of a dense core.

It may be possible to interpret recent findings in several ways that are less in discord with all these points. The meteorites demonstrate that differentiation to various degrees has taken place in the solar system in bodies other than those that now exist. Since it is generally assumed that these bodies were shattered by collisions, one may ask whether the present-day meteorites represent a selection of material left over from these earlier phases, and which type of such material was responsible for building the moon or for adding the outermost layer to it. If the basaltic achondrites represent this material, the composition would fit, and one may then even wonder whether the basaltic layer that covers most of the deep oceans on the earth has a similar origin. Of course it is generally thought that this layer in the oceans is comparatively young, but the determinations of age are based on samples that have been raised and heated in a recent epoch. There may be a new interest now, both in age determinations of the seismically observed deep ocean layer and in detailed observations of the chemical composition, in order to compare this with lunar samples when they become available.

Other possible ways of better reconciling the known evidence must be sought so that one will ask the appropriate questions of the lunar exploration program. A disservice would be done to the lunar exploration program if one of the major questions came to be regarded as settled by an answer that still leaves a mass of conflicting evidence, such as the answer given in the official Surveyor V report published in *Science* (*1*).

Note added in proof. The results of the chemical analysis of Surveyors VI and VII show a strikingly similar composition in widely different regions. The Surveyor VII sample probably represents deep subsurface material excavated by the Tycho explosion, suggesting that the same composition extends to a considerable depth. Such a similarity would be quite unexpected on the earth. The slightly higher iron content of the two lowland samples may merely represent an enrichment of the longer-exposed surface material with iron-rich present-day meteorites.

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4 March 1968

Deformation Lamellae in Quartz

In a recent contribution Greenwood (*1*) reports the occurrence of planar deformation features in quartz which he believes are identical to those considered by other investigators (*2*, *3*) as indicative of shock-wave action and meteorite impact in ancient "cryptoexplosion" structures. Greenwood's specimens occur in rocks for which a meteorite impact origin is implausible, and his conclusions, if taken at face value, appear to undermine the use by other workers (*2–5*) of unusual planar features in quartz as evidence of ancient meteorite impact.

In view of Greenwood's (*1*) conclusions, it is unfortunate that his report does not include (i) some discussion of his measurement techniques, in order to justify the assignment of exact crystallographic indices to his planar features, and (ii) a summary of all his measurements in tabular, stereographic, or histogram form, in order to allow comparison with data that has been obtained from other studies of quartz from both metamorphic and shock-metamorphosed rocks.

It has long been recognized that planar deformation lamellae develop in quartz deformed at low strain rates during normal tectonic metamorphism (*6–8*). No extraterrestrial origin has ever been proposed for such "deformation lamellae" in the narrow sense.

The "shock lamellae" or "planar features" observed in quartz from shocked rocks are quite distinct from normal metamorphic deformation lamellae, particularly in such characteristics as the number of sets per grain and the orientation of the planes within the quartz crystal (*2–5*, *9*). These unique planar features are believed characteristic of shock-wave action. They have been observed in rocks from accepted meteorite craters and have also been formed under controlled shock conditions in experiments involving nuclear and chemical explosions (*10*).

Some justification is needed for Greenwood's statement that his planar features are in fact parallel to $\{10\bar{1}3\}$. Such exact identification of the orientation is not possible unless the absolute orientation of the host quartz crystal can be established, and it is not possible to tell, from Greenwood's (*1*) description, whether such absolute orientations were actually obtained.

Measurements performed by several workers on shocked rocks (*2*, *3*, *9*) do indicate that certain of the shock lamellae are in fact parallel to $\{10\bar{1}3\}$. However, in many natural samples, particularly metamorphic rocks, such exact measurements are difficult or impossible. An alternate procedure, therefore, has been to measure the angle between the poles to the planar features and the c-axis of the quartz grain and to plot the distribution of these angles as a histogram (*3*, *4*, *9*). Such histogram plots facilitate the qualitative comparison of the distribution of shock lamellae with that of normal metamorphic deformation lamellae (*2*), but it must be remembered that it is not possible to establish exact crystallographic indices for a planar feature from such a histogram alone.

The chief difference between histogram plots of normal metamorphic lamellae and shock lamellae is that the former distribution produces a broad, bell-shaped curve with a peak at about 15° to 25° and a skewed tail extending to higher angles (*6–8*). By contrast, the distribution of shock lamellae shows strong concentrations at certain specific angles which presumably correspond to specific planes in the quartz crystal (*2, 5, 9*). Two such concentrations are notable and appear unique to shocked rocks. One, near 0°, can be exactly specified as parallel to {0001}; another, at about 23°, has been regarded as corresponding to {10$\bar{1}$3}, by using measurements made on single quartz grains (*2, 3*). However, the occurrence of planar features whose poles make an angle of about 23° with the quartz *c*-axis is not sufficient to allow exact indexing of the plane; it is the large concentration of planes at that angle which has been regarded as indicative of shock action. The assumptions underlying these diagrams have perhaps not been emphasized sufficiently by investigators of shocked rocks, and some misunderstandings have resulted (*11*).

Greenwood admits that his samples have been "affected by relatively low-velocity shock during blasting." Even if one accepts his assumption that "the lamellae are geotectonic rather than blasting features" (*1*, p. 1180), the brief description of his results shows only that the features he has studied resemble normal metamorphic deformation lamellae (*6–8*). Estimating his uncertainty of measurement as ±4° he states that, out of 160 planes measured, five (3 percent) are "parallel to (0001)" (presumably in the interval 0° to 4°), while 48 (30 percent) are "parallel to (10$\bar{1}$3)" (presumably in the interval 19° to 27°). There is no mention of how the other 67 percent were oriented.

These characteristics are not significantly different from those of normal metamorphic deformation lamellae measured by other workers (*6–8*). Such studies (*7*, Fig. 1; *8*, Fig. 1) demonstrate that a small but definite percentage (under 3 percent) of the lamellae lie near 0°, while a larger fraction (20 to 40 percent) occurs within the interval around 23°. By contrast, plots for shocked rocks have shown maxima as high as 80 percent in the 0° to 4° interval (*2*, Fig. 1) and as high as 50 to 60 percent in the 19° to 27° interval (*2*, Figs. 1 and 2; *4*, Fig. 4). Greenwood has, therefore, not established clearly that the planar features he observed resemble the unusual shock lamellae observed in shocked rocks, and his observations apparently have no bearing on the use of such shock lamellae as evidence for meteorite impact.

Studies of the deformation features in metamorphic quartz are certainly desirable, for only through such investigations will it be possible to establish whether the unusual features found in rocks from cryptoexplosion structures are unequivocal indicators of meteorite impact. However, these investigations must be carried out carefully and their results presented in detail so that adequate comparisons can be made. Greenwood's (*1*) study fails in these respects. His work does indicate that considerable confusion exists about deformational features in "shocked" and "unshocked" quartz, but it is only through careful comparative studies, exchange of samples, and other communications that this confusion will be reduced.

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11. I am grateful to E. C. T. Chao of the U.S. Geological Survey for numerous helpful discussions and criticisms about these particular problems.

2 February 1968

I appreciate this opportunity to clarify certain points made in my earlier paper (*1*). I did not indicate, as has been claimed (*2*), that the deformation lamellae in quartz of the Star mine, Idaho, were similar to lamellae reported from shock metamorphosed rocks. I noted that orientations of lamellae parallel to (0001) and (10$\bar{1}$3) had been observed in quartz from the mine. Further, I called attention to an article by French (*3*) in which these orientations were considered to be unique indicators of shock metamorphism.

I contended that because such orientations were found in geotectonically deformed rocks, they alone were not unique and sufficient criteria for shock metamorphism induced by meteorite impact. Carter (*4*) has called my attention to his earlier paper (*5*) that reported orientations of lamellae similar to (0001) and (10$\bar{1}$3) in quartz from geotectonically deformed rocks. Carter's report supports the above contention.

In the study of the rocks of the Star mine, lamellae orientations were determined by universal stage measurements of the attitude of *c*-axes of quartz and the plane of the lamellae. Angles between the *c*-axes and the lamellae were measured on an equal-angle stereonet. The resulting angles were plotted on histograms with 4° class intervals correponding to the estimated 4° error of the measurement of planes on the universal stage. Indexing of the histogram maxima was done, following the method shown by French in his Fig. 4 (*3*). This procedure appears to correspond to the "conventional universal stage methods" (*3*) used by French in his Sudbury study. The fine grain size of the quartz in siltite and argillite of the Star mine did not make feasible the absolute grain orientation used by Carter (*5*).

Analysis of lamellae orientations in quartz from geotectonically deformed rocks and impactites indicates that impactites appear to be distinguished, not by the presence of lamellae at or near 0° and 23° to the *c*-axes of quartz, but rather by the relative absence of lamellae at angles which deviate greatly from 0° or 23°.

In my previous report I suggested that use of negative evidence to establish unique criteria for impact was tenuous in view of the few reported samples of deformation lamellae. Carter (*6*) indicates that there are only 22 reports on lamellae, hardly a statistically valid sample of deformation lamellae in the earth. Therefore, I would again urge that additional study of deformation lamellae be made before the genetic significance of these structures is accepted.

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4 March 1968

Splashing of Drops on Shallow Liquids

I wish to offer a criticism of the explanation suggested by Hobbs and Osheroff (*1*) for the peak heights observed in Rayleigh jets issuing from fluids 7 to 8 mm deep. In addition, I offer an explanation of the seemingly paradoxical influence of surface tension on jet heights.

A vortex ring approaching a wall may be represented by the physical vortex ring and by an image ring of equal strength and opposite sense (*2*). Each ring induces outward components of velocity into the core of the other. The mutual influence will be to enlarge the diameter of each, with the velocity of approach continually diminishing. Thus, a vortex ring approaching a fixed boundary will continually increase in radius, moving outward along the boundary without ever reaching it. Presumably, the influence of viscosity would be to dissipate the energy contained in the vortex ring as heat energy. Such behavior would provide no mechanism for the "reflection" of the energy from the vortex ring at the base of the tank as suggested by Hobbs and Osheroff.

In the case of two fluids having different values of surface tension, the work done in causing a given deformation of the free surface will be less in the fluid with the lower surface tension. In either case, a substantial portion of the energy used to deform the free surface will be "stored" by the surface and recovered as the surface returns to its initial shape. If substantially the same amount of energy is added to a localized region of each fluid by an incident raindrop, the added energy will more readily cause a deformation of the free surface with the lower surface tension. There will then be a greater tendency for the crown to break up into discrete drops in the fluid of lower surface tension. These droplets are ejected from the crown at an angle to the vertical and thus carry "stored" energy away from the local area of interest, adding this energy at some other area of the surface. As the crown collapses the energy available to contribute to the formation of the Rayleigh jet will then be less in the fluid of lower surface tension. As a consequence, the jet will rise higher when the surface tension is higher.

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3. I thank J. R. Weggel for discussion.

11 March 1968

As a result of recent experiments carried out in this laboratory (*1*), I have also concluded that the tentative explanation put forward in the last paragraph of our paper (*2*) to account for the behavior of the Rayleigh jet in the splashing of drops on shallow liquids cannot be correct. By taking simultaneous high-speed photographs of the suprasurface and subsurface phenomena which follow the impact of a drop on a liquid, we have discovered that the vortex ring is not the result of the impact of the original drop on the surface. The vortex ring is formed by the Rayleigh jet and the jet drops reentering the liquid surface! It is clear, therefore, that the vortex ring and the crown cannot interact with one another.

A theoretical explanation of the complex behavior observed by Hobbs and Osheroff is, to quote these authors, "a difficult problem in fluid dynamics." The simple qualitative argument presented by Maxwell to explain one aspect of the behavior, namely, the influence of surface tension on the height to which the Rayleigh jet rises, is obviously inadequate. For example, Hobbs and Osheroff found that, when the depth of the liquid was between about 4 and 10 mm, the Rayleigh jet which was produced in dyed water (surface tension 72.75 dyne/cm) rose to a greater height than did the Rayleigh jet for milk-water (surface tension 65.07 dyne/cm). However, when the depth of the liquid fell below about 4 mm the reverse was true.

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10 April 1968

Crescentic Coastal Landforms

As Dolan and Ferm state (*1*), even the casual beach visitor is familiar with the lower-order crescentic coastal features. Their observation of hierarchical arrangement and grouping by size, with logarithmic spacing between groups, along the southeast coast of the United States is indeed interesting. It seems, however, that worldwide applicability of the hypothesis of logarithmic spacing must await further examination. Bascom (*2*) mentions measurements on the Pacific coast (admittedly there was no statistical summary) that fall directly between features of orders 1, 2, 3, and 4.

While the generating mechanism for this phenomenon is pointedly left to speculation, it may be appropriate to note that evidence of eddies, of the same scale as the Carolina capes, can be observed near land in photographs taken from space by Gemini IV and V (*3*). Specifically, the arcuate structure of the cumulus clouds off Cape Kennedy, in picture S-65-34717, may be related to an eddy of the same scale in the ocean. Picture S-65-45765 shows more definite evidence of an eddy about 100 km in diameter. Photographs not included (*3*) show other interesting examples of eddies (*4*) of this scale.

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Deformation Lamellae in Quartz

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