clude many sites other than craters (such as large or unspecified areas in all the regular maria, Taurus Mountains, Pico), but they are consistently within the topographical patterns described. The lack of reported events in the highlands is striking, and, although this may be due to observational selection [as we noted (*3*)], there are many other differences between the highlands and the maria; we question the suggestion that lack of attention to the highland areas by observers is entirely responsible for the lack of reported events there.

Morphologically, apart from ring plains, many sites of events are associated with craters having both central peaks and extensive systems of cracks on their floors (uncommon in the highlands), and the new series of Orbiter IV photographs has increased their number. The photographs provide much new detail, and the presence of cracks in the floors and of central peaks now appears to be extensively associated with the crater sites, with the exception noted above of the ring plains (see also *4*).

We mentioned the size of the telescope used with particular reference to the recovery rate and the numbers of events recorded on the dark side. We need not comment on Chapman's statements about the relevance of telescope size to observations and to observers' methods.

Chapman shares with us the wish to see more permanent records, particularly of spectra (*5*). At the time of writing, permanent records are relatively few, but the distributions of their sites over the moon's surface and with respect to the lunar orbit are consistent with the findings based on the larger number of visual reports. One hopes that during the next few years the efforts of many interested groups will produce more and better permanent records and that a cooperative program of systematic observation, with participation by many professional observatories and by amateurs, can be established. Most phenomena are brief and intermittent, but evidence is accumulating that events may continue sporadically for several days. We now know that the probability of observation of an event may increase near lunar perigee and, to a lesser extent, near apogee also, but this probability remains rather small; two telescopes are usually needed if auxiliary apparatus (such as a spectrograph) is to be brought into use at short notice. Given a good communications system, available facilities of amateur and professional groups might be integrated, with corresponding increase in the chance of obtaining permanent records. Although the time scale is shorter, some aspects of the observational problem are quite similar to those of comet observations, where many amateurs have played vital roles in discovery.

The question of the reliability of past observations of lunar phenomena may soon become academic as new data become available. Nevertheless they may have been of considerable value in affording evidence helping to determine the nature of lunar events. If the events are of internal origin, lunar eruptions will offer a potential hazard for any program of manned lunar landing, such as Apollo. Few scientists now question the reality of at least some events, whether or not they agree on the origin. We should make more efficient use of available equipment; observations, both visual and permanent, have so far been almost always matters of chance.

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## Solar Models

Goldreich and Schubert have recently reported (*1*) an instability in a solar model with a rapidly rotating core. While this is an important result and must be taken into account in all future work on differential rotation, I believe the authors have overstepped the bounds imposed by their own careful linearized treatment (*2*) of the stability problem when they conclude that "differentially rotating solar models are unstable unless their angular momentum is an increasing function of distance from the rotation axis. This latter condition is violated by all solar models whose interiors rotate sufficiently rapidly to account for Dicke's oblateness measurement" (*1*).

Any discussion of instability in a fluid system must be based on a set of assumptions about the fluid, its nature, and its motion. Goldreich and Schubert assume that the fluid is free of magnetic fields and that it is purely rotational about a fixed axis.

In my article (*3*) I mentioned the possibility of stabilization of motion induced by toroidal magnetic field in the zone of differential rotation. Also, for the past 2 years I have been considering a solar model with a rapidly rotating core for which there is a strong dipolar poloidal field in the core. In this model the fluid motion is not purely rotational, and the meridional component of the fluid velocity is periodic and can exceed 1 m/sec.

Goldreich and Schubert have noted (*2*) that a meridional velocity in excess of 10 cm/sec could stabilize the flow. With their mechanism, latitude circles in the form of fine grooves develop on a surface of constant angular velocity in the zone of differential rotation. The fluid must flow very nearly parallel to these grooves or they are destroyed, eliminating the instability. Any mechanism for causing a meridional flow, whether it be a simple circulation or an oscillation, can eliminate this instability.

Goldreich and Schubert noted that the instability they observed is very similar in form to the thermohaline instability discussed by Stern (*4*). However, it would be a mistake to conclude that because of this instability there can be no large bodies of water with a salt concentration increasing upward. Such cases are not only known, they are the rule in tropical seas (*5*). Ocean currents probably prevent the development of the instability (*2*). Hence, I would also conclude that the instability described by Goldreich and Schubert does not preclude a solar model with a rapidly rotating core. It does prohibit a purely rotational model free of internal magnetic fields.

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