Table 1. Effect of magnesium pemoline on intelligence in man. There were 24 subjects in the first test, 24 in the second, and 23 in the third. Subjects were individuals showing memory defect due to various forms of aging. The individual dose of magnesium pemoline (Cylert, Abbott Laboratories) was comparable, namely, 25 to 50 mg by mouth daily. N.S., not significant.

Time of test	Mean I.Q.	Diff.	t	P
Prior to drug	73.5			
1 week later	77.4	3.9	2.010	N.S.
> 1 mo. later	82.2	8.0	4.819	0.01

ing which, he states, he carried out 3 hours after administration of the drug. But this drug does not act in man precisely as it is reported to act in the rat. In reality, the action of the drug reaches statistical significance in man only after approximately 1 month of administration, as shown in Table 1.

My second point concerns Smith's curious extrapolation from our work on RNA. Smith states that since we found that RNA is more effective in the least deteriorated patient, therefore magnesium pemoline should be more effective in normal males. One may ask, more effective than in what—normal males or brain-damaged humans?

Surely medical science has many examples which might well have corrected such a conception. Digitalis is more effective in less-damaged hearts than in extremely damaged hearts. Should, therefore, all of us who have undamaged hearts be on digitalis to benefit from this oddly hypothesized gain?

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Lunar Transient Phenomena

Recently [*Science* **155**, 449 (1967)] Middlehurst and Moore presented a preliminary analysis of a survey of lunar transient phenomena (LTP). Their critical reevaluation of historic observations is valuable in the light of recent reliable observational evidence of such phenomena. Nevertheless, one must be very cautious in analyzing past observations, even assuming that observations by famous observers are reliable.

In particular I question the conclusion that LTP occur preferentially on the edges of maria, in prominent ray craters, and along the moon's central meridian. That they have been seen mainly in those places is beyond question, but probably 95 percent of all observing time has been given to just those places. It seems to be natural for visual observers to concentrate on craters, moderate to large in size, in or on the borders of maria and to avoid the difficult lunar highlands. Since most observers like to draw pictures of "objects," little attention has been given to the flat surfaces of maria or to regions between craters. The list by Middlehurst and Moore of LTP locations is practically a roll call of the most popular craters on the moon, to which systematic observers and casual observers alike (including me) have devoted most of their attention. Most of these craters are popular because they are moderately large, relatively prominent, and easy to sketch with a pencil. A few (Aristarchus and Plato, in particular) have received special attention because of past reports of peculiar phenomena.

Moreover, Middlehurst and Moore discuss the size of the field of view as an important factor in analysis of LTP. While the size could be significant in relation to a visual patrol for LTP, it is much less so relative to past observations. Observers usually have confined their attention to single craters or other features covering only a few percent of the area of the field of view. With their eyes fixed on these features, the observers very likely missed anything short of a catastrophic phenomenon occurring elsewhere in the field of view. Most recent well-documented LTP have been rather subtle and would have been missed by an observer not looking directly at them (or for them).

This discussion leads to several conclusions. First, investigators should be thoroughly familiar with the nature of visual observation of the moon and of LTP so that they can (i) select intelligently the reliable observations from among the much greater number of incorrect or fanciful reports, and (ii) make physically meaningful interpretations of the selected reports. While Middlehurst and Moore are not unaware of some of these systematic factors and do qualify their conclusions to some extent, I believe they are overconfident. In particular, the effects of observational selection are so severe as to invalidate any conclusion at this time from the nonrandom distribution of LTP sites, except concerning the psychology of visual observers of the moon. Lastly, I should emphasize strongly the suggestion of Middlehurst and Moore that observers pick random locations on the moon (and not just craters) for future LTP patrols.

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Chapman correctly points out that many observers tend to pay greater attention to the more interesting features of the lunar surface. It is also possible that past reports of lunar phenomena may not be entirely reliable. If our conclusions had been reached from consideration of the topographical distribution alone, I would agree with Chapman's suggestion that we may have been overconfident in our deductions.

Our assumption of the reliability of the reports, however, rests also on the appearance of significant correlations of the data with quantities unrelated to observational factors. For example, a strong peak in the frequency of events reported near perigee and a smaller peak at apogee were found, although the lunar orbital period (average, 27.6 days) is incommensurate with the synodic month (29.56 days from new moon to new moon). Lighting and other observational factors could not affect this distribution materially, and the presence of these peaks at a significant level led to the conclusion that internal causes were implicated. The ratio of tidal forces on the moon to lunar gravity is about 100 times greater than the similar ratio on the earth. The strength of the lunar material at depth is unknown, but the release of internal pressures could be affected by the tidal forces to a much greater extent than on the earth (*1*).

In considering the distribution of sites of reported events on the lunar surface, we were aware of and pointed out the possibility of observational bias, but Chapman's questions about distribution may apply more to the degree of concentration than to the gross patterns. It is unlikely that 238 observers (*2*), including most of the famous lunar mappers of earlier times (Beer and Mädler, Schröter, and Elger) and men of lively intellect, such as W. Herschel, Barnard, and J. Schmidt, should have confined their attention throughout their careers to large craters only, as Chapman suggests. Our list (*3*, table 1) does include 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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