Hu suggests that our data support a consolidation interpretation on the strength of two arguments: (i) on inspection of our figure 1 (*1*), that the only two groups which showed "savings" (these are, in fact, not savings scores but mean-error differences) on the first day of retention testing [groups LD(1-10) and LD(1)] received strychnine 24 hours after initial training; and (ii) that any facilitation in group LD(1) is masked by training to a criterion of learning. Let us consider these points.

Since retrograde facilitation of memory consolidation has been reported after a single injection of strychnine sulfate (*2*), it seems likely that a significant facilitation should have been detected on day 1 of retention testing if consolidation were affected. However, as Hu points out, differences among the groups on day 1 of retention testing were not significant (initial errors: $F = 2.14$, d.f. = 5,100, and $.5 < P < .10$; total errors: $F = 1.90$, d.f. = 5,100, and $P > .10$). Consequently, we are reluctant to draw conclusions on the basis of such nonsignificant effects. In a recent analysis of this phenomenon, we looked at the effects of several analeptic and stimulant drugs on the long-term memory store and used a similar paradigm (*3*). We examined retention on day 1 and on learning to a criterion in that study, and we found no consistent relation between the strength of drug effects on day 1 and criterion. For strychnine sulfate, the facilitating effect was of approximately equal magnitude in both measures. If the observed facilitation in this paradigm were due to consolidation enhancement, one would expect the strongest effect on day 1.

McGaugh and Krivanek (*4*) administered strychnine sulfate (either 0.1 mg/kg or 1.0 mg/kg) to mice at several intervals before and after daily maze training. The higher dosage of strychnine was effective at a longer pretrial interval than the lower dosage. Although they did not obtain a parallel dosage-time relationship for administration posttrial it would have been manifested between 1 and 2 hours posttrial, but they did not examine this interval. A similar dosage-time relationship has also been demonstrated for posttrial administration of *d*-amphetamine sulfate (*5*)—that is, a higher dosage produced effects at a longer interval than the lower dosage. Since 24 hours is long beyond the effective interval reported for strychnine, we would expect that the higher dosage we used (1.0 mg/kg) would have had at least equal efficacy to the lower dosage (0.2 mg/kg) at this extreme interval. However, neither groups HD(1) nor HD-(1-10) showed savings.

A consolidation interpretation is unlikely for a number of other reasons. In the only direct measures of short-term memory (STM) in mice, Alpern and Marriott have demonstrated that the gradient of STM in the C57BL/6 strain is less than 20 minutes, and no longer than 20 minutes in any of the other strains examined (*6*). Even allowing for differences in task, the discrepancy between 20 minutes and 24 hours is most impressive. More up-to-date reviews of the consolidation literature than the one cited by Hu have not reported facilitation of memory by strychnine or other neural excitants administered more than a few hours after training (*7*). Hu suggests that the consecutive injection schedule used in our study may have extended in some unknown way the STM gradient to 24 hours. In most consolidation studies, however, consecutive training-injection sessions, almost always separated by 24 hours, were used (*7*). If the supposed extension of the STM gradient is due only to the repetition of injections, then the time dependency reported for strychnine's effect on memory should never have been obtained. In other words, regardless of the interval posttrial, the simple repetition of drug administration each 24 hours should have always produced facilitation by extending the gradient of susceptibility through Hu's unknown mechanism. Examination of the investigations cited above do not support this notion.

For these reasons, we believe that our data are best explained as supporting an hypothesis of facilitation of the long-term store of memory.

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2 May 1973

Radar Mapping of the Moon: Central Peaks

Topographic mapping of the lunar surface through radar interferometry can provide critical information for interpreting lunar processes. However, a discrepancy in elevations determined by radar and photographic techniques raises questions concerning the precision of present radar-derived lunar altitudes. From radar mapping of the Alphonsus-Ptolemaeus-Arzachel region, Zisk (*1*) indicated that whereas the floor elevations of Alphonsus and Arzachel differ by 600 m the central peaks of the craters are at the same altitude. This led Zisk to suggest that the peaks are volcanic edifices contemporaneously fed from a common magma chamber. Radar measurements gave the heights of the central peaks of Alphonsus and Arzachel as 600 and 1200 m above their respective floors, with a probable error of better than 200 m. However, from measurements of the lengths of the shadows cast on Orbiter IV (*2*) and Ranger IX (*3*) photographs, Alphonsus' peak is 1100 m high and Arzachel's is 1900 m high, with a probable error of 50 to 100 m. The highly accurate topographic map of Alphonsus prepared by Wu *et al.* (*4*) from photogrammetry of Apollo 16 metric camera pictures confirms this peak height for Alphonsus, and measurements of shadows on photographs taken from the earth corroborate the Orbiter height for Arzachel's peak (*5*). Experience indicates that heights derived from shadow measurements are not affected by systematic errors large enough to explain the inconsistency with the radar results. Thus, topographic data do not support the contention that the peaks are volcanoes.

There is an additional source of uncertainty regarding the radar topographic map [figure 2C in (*1*)]; the central peak of Alphonsus does not appear in its true position relative to the crater rim, nor to the longitude-latitude grid, which is offset ½ degree (15 km) from its correct position. Are the altitude errors related to these positional errors?

Recent studies have demonstrated that central peaks in terrestrial impact craters are rebound phenomena (*6*). Ongoing work (*2*), which shows a correlation between peak height and crater diameter, and hence impact energy, suggests that the same mechanism produced central peaks in lunar craters. Correlation of the percentage of craters with central peaks and crater rim sharpness further implies that peak and crater formation are at least approximately contemporaneous (*7*), and thus reinforces the rebound hypothesis. However, evidence for a rebound origin of central peaks generally does not preclude the possibility that the Alphonsus peak may be volcanic, as its morphology and structural setting suggest. In any case, morphological differences of the rims and peaks of Alphonsus and Arzachel, as well as the topographic data, argue against Zisk's suggestion that the two peaks had a common contemporaneous origin.

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Wood has brought up several points which were not treated in my report because of the need for brevity, but have been discussed elsewhere (*1*). The basic difference between an optical shadow measurement of mountain peaks and a radar measurement is that the optical method gives the height of each maximum on the profile of a mountain, whereas the radar method gives the weighted height of the whole surface within the 2-km resolution of the radar. Since the actual area of a peak is relatively small, and the weighting factor (the strength of the radar echo) favors slopes facing the earth, the radar method tends to ignore narrow mountain peaks and favor the elevation of the steepest slopes in hilly terrain.

Wood has provided me with several unpublished profiles of the Alphonsus peak region, made by R. Turner of the Lunar and Planetary Laboratory, University of Arizona. These profiles show that if the peak were centered on the 2-km radar resolution element (which is highly improbable) the measurement would yield a weighted elevation about 300 m lower than the optical peak, whereas if the peak were located at one edge of the element (which is equally improbable) it would yield a weighted elevation about 700 m low. The difference of about 500 m between the optical and radar peak elevations seems to be well within the probable range of the systematic discrepancy, especially since the difference is approximately the same for both the craters Alphonsus and Arzachel. The peak of the elevation measured by radar is not necessarily even coincident with the visible central peak.

The effect of these discrepancies on the argument for recent volcanism is, I believe, small. It is not so much the elevations of the mountain peaks as the elevations and alignments of the elongated ridges, where the radar data appear to be unambiguous, that give the strongest support for the hypothesized volcanic origin for these features. I see no disagreement about the prior existence of the central crater peaks, created possibly by rebound flow at the time the crater was originally formed. If we are willing to carry the discussion into the realm of conjecture, the peaks might, by blocking the flow through the presently visible fault, have been the reason for the termination of the central ridge structures only halfway across the crater floors.

The offset of ½ degree in the selenographic grids is a result of the projection of the spherical lunar surface onto the two-dimensional delay-Doppler grid. The proper coordinates were used to process the observations into maps, of course, but the computer drafting of the coordinate lines was erroneously based on the center of the surface area rather than the delay-Doppler projected area.

Although there may yet be room for a difference in interpretation of the measurements, I believe that there is no unresolved discrepancy between the two different sets of data.

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Genital Sensory Field

Komisaruk, Adler, and Hutchison (*1*) report that the size of the genital sensory field of the rat pudendal nerve is larger in ovariectomized estrogen-treated animals than in ovariectomized controls. The observed median differences are in the order of a few millimeters in width and length or 67 mm^2 in area. Also, the control and experimental samples have overlapping ranges in those measurements.

I have a number of questions regarding the methods in this study:

1) The ratio between the total body surface and the sensory area should have been used instead of the absolute field size. If the total body surface area of the estrogen-treated rat increased, the observed increase in the sensory field might not be due to specific but to general effects. Komisaruk *et al.* also used certain landmarks on the skin to detect the enlargement of the sensory field. If the positions of these landmarks are subject to changes in the total body surface area, they are not reliable points of reference.

2) The authors note that the deflection of a single hair elicited a response. If the skin was so sensitive, how could they control their manual stimulation so precisely as to be able to detect a few millimeters of differences? A slight difference in the applied pressure might result in a difference in the size of the sensory field.

3) Komisaruk *et al.* do not give any objective definition of a response other than visual inspection of the oscilloscope screen. How can this be reliable in multiunit recordings?

4) Although the authors emphasize that the placement of the electrodes did not bias the results, how can the oscillo-

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