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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References and Notes

1. S. H. Zisk, *Science* **178**, 977 (1972).
2. The data reported here are from a program of lunar crater measurements partially described by C. A. Wood [*Moon* **3**, 408 (1972)].
3. Model studies of Alphonsus by R. Turner are reported in G. P. Kuiper, R. G. Strom, R. S. LePoole, "Ranger VIII and IX, part II. Experiments' analyses and interpretation," *NASA Tech. Rep. No. 32-800* (1966), p. 188.
4. S. S. C. Wu, F. J. Schafer, R. Jordan, G. M. Nakata, in "Apollo 16 preliminary science report," *NASA Spec. Publ. SP-315* (1972), p. 30-5.
5. E. A. Whitaker and C. A. Wood, unpublished results.
6. D. J. Milton *et al.*, *Science* **175**, 1199 (1972).
7. C. A. Wood, *Commun. Lunar Planet. Lab.* **7**, 157 (1968).
8. I thank W. K. Hartmann, C. R. Chapman, R. J. Greenberg, A. B. Binder, E. A. Whitaker, and R. G. Strom for criticisms of a preliminary draft.

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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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References

1. S. H. Zisk, *Moon* **4**, 296 (1972).

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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-

scope traces of multiunit recordings be so precisely reproducible without using objective criteria other than the reproducibility of the sensory field map itself?

These queries will have to be answered before their conclusion can be accepted.

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References

1. B. R. Komisaruk, N. T. Adler, J. Hutchison, *Science* **178**, 1295 (1972).

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Konishi notes that in our report (*1*), the observed median differences in the size of the genital sensory field "are in the order of a few millimeters in width and length." Since the clitoral sheath, for example, is only 4 to 5 mm long, an increase of a few millimeters in the clitoral field (which can extend beyond the base of the clitoral sheath) represents a distinct effect of estrogen. The median field size was 4 mm in the ovariectomized, untreated control animals (C) compared to 7 mm in the ovariectomized, estrogen-treated experimental animals (E) representing a 75 percent increase in the E group ($P < .01$, Mann-Whitney U-test, one-tailed). Furthermore, the width, length, and area of the entire genital sensory field were 22.0 to 31.9 percent larger in the E group, each of these differences being significant despite overlapping ranges. At the time the measurements were made, we did not know the hormonal condition of the rats. Also, differences in field size between the E and C groups were large enough to enable us to identify correctly the C rats in 72 percent of 18 cases and the E rats in 80 percent of 15 cases ($P < .025$, Fisher test). Furthermore, responses to stimulation in the midline occurred in 50 percent of the E rats but in only 17 percent of the C rats ($P = .048$, Fisher test). We consider these effects of estrogen to be substantial.

Replies to Konishi's specific comments follow:

1) Konishi suggests that the increase in size of the genital sensory field in E rats may be due to a general effect in which estrogen would increase total body surface area. This is not likely, because the body weights of the E rats were significantly lower than those of the C rats (median weights: C, 317 g; E, 179 g; $P < .05$, Mann-Whitney U-test, one-tailed), results confirming the weight-reducing effect of estrogen (*2*). This indicates that the body surface area was, if anything, smaller than that of the controls. Even if estrogen had no effect on absolute genital field size, its effect of reducing body weight (and therefore possibly body surface area) would yield an increase in the genital sensory field relative to body surface area. The conservative nature of our method can be demonstrated by dividing each absolute field size (in square millimeters) by its corresponding body weight (in grams). By this relative measure, the median of the E group was 94.6 percent greater than that of the controls ($P < .05$, Mann-Whitney U-test, two-tailed), in contrast to our published significant value of 31.9 percent based on the absolute median field size.

There is also a functional reason for determining the absolute rather than the relative size of the genital sensory field. The perineal region is a target for the male's thrusting to elicit lordosis and achieve intromission. It is possible that the female orients to the male in response to stimulation of her perineal region. An absolutely larger sensitive area, with a gradient of sensitivity that increases toward the vaginal orifice, could facilitate her orientation to the male.

Konishi's meaning is not clear to us when he suggests that the positions of the perineal landmarks change in response to estrogen. If he means that estrogen altered the position of perineal landmarks by increasing total body area, then our remarks two paragraphs above should answer his criticism. If, on the other hand, he means that estrogen enlarged the perineal area in relation to the body surface area, then our finding of an increased sensory field under estrogen treatment remains an interesting biological phenomenon, and perineal enlargement becomes a possible mechanism for this effect. In either case, changes in the position of landmarks in relation to each other were irrelevant to the measurement of field size, for we positioned the leg in a standard orientation, and the grid was an absolute rectilinear scale centered on the tip of the clitoral sheath.

2) We were measuring total field size rather than tactile sensitivity threshold. We used intense stimulation (scratching the skin) near the less sensitive field borders. The field was defined as the boundary beyond which even intense stimulation failed to elicit any response.

3) We defined a response as any identifiable and reliable neuronal firing activity in response to the stimulus, as detected by simultaneous visual and auditory monitoring. This was the criterion we used in determining the field boundary, and it was applied to each subject by using a blind procedure and two observers. Any possible insensitivity of our observation technique to a threshold response would apply equally to the E and C groups. Our technique was validated by the occurrence of systematic changes in field size under hormonal manipulation and by the reproducibility of the field in each single animal.

4) The published dimensions of the field are based upon each first electrode placement, a procedure during which we had no knowledge of the hormonal condition of each animal, thereby eliminating systematic bias. When we repositioned the electrodes several millimeters closer to the periphery, also changing their relative placement in the cross-section of the nerve, we found empirically that the field size, although not identical, was highly correlated with that on the first determination (Spearman rho between .76 and .85; $P < .005$, one-tailed, for each of the four different measures of the field size which we used). The validity of our multiunit recording method would be questionable if the field sizes were grossly different between the first and second placements, but in fact they were significantly correlated. Thus, multiunit recording provides an efficient and reliable means of determining the size and shape of a sensory field.

For the reasons stated above, we conclude that the genital sensory field size was significantly increased by estrogen treatment.

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References

1. B. R. Komisaruk, N. T. Adler, J. Hutchison, *Science* **178**, 1295 (1972).
2. G. N. Wade and I. Zucker, *J. Comp. Physiol. Psychol.* **70**, 213 (1970).

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