Because lunar sinuous rills look "deceptively like terrestrial meanders" and run "parallel to the regional slope," Schumm and Simons have cast aside our "ingenious mechanism" and have devised the pseudo-alternative that "parts of some of the channels" are the "coalescence of chain-crater systems." However, it is our opinion that the differences between lunar sinuous rills and coalesced chain craters are fundamental. If we consider only the examples cited by these authors, Rima Prinz I and II, the sinuous channel in Schroeter's Valley, Rima Marius, and Rima Plato II, it is obvious that their basic morphological characteristics (continuous and uniform meandering channels, mature meanders, goosenecks, distributary channels, and flood plains) cannot be imitated by coalesced chain craters. As can be seen in some straight rills, such as Hyginus, coalesced chain craters do not resemble sinuous rills nor should they be confused with them. Coalescence of craters produces depressions with irregular floors and opposing walls that are mirror images of each other, that is, like ( ), rather than the observed smooth floors and matching walls, that is, like ((, of the lunar sinuous rills.

Using the lunar astronautical charts, Schumm and Simons state that sinuous rills do not follow the local gradient and that Rima Marius and the rill at the end of Schroeter's Valley both cross ridges. However, the Lunar Orbiter photographs have shown that these charts are so inaccurate that they cannot be used as a basis for the study of sinuous rills. Even such large features as the Cobra's Head of Schroeter's Valley are grossly distorted on the charts. From a survey (*1*) of Lunar Orbiter IV photographs of about 130 sinuous rills, we find that, wherever it is possible to determine a gradient, the rills meander from higher to lower elevations. Lunar Astronautical Chart 39 shows a "ridge" crossing Rima Marius, whereas Lunar Orbiter IV photograph H150 reveals that this "ridge" is in fact two ridges offset by 10 km, which do not cross the rill but terminate on either side of it. Similarly the Schroeter's Valley rill does not cross any "ridges" but meanders between isolated hills (Lunar Orbiter IV photograph H157).

Despite the erroneous examples cited by Schumm and Simons, there is no reason to doubt that a channel eroded by surface water could not be subsequently uplifted. A possible example of this might be Rima Prinz II. Since its channel is deeper on the plains to either side of the ridge, the rill must either have been uplifted subsequent to its formation, or must have passed through a gap in the ridge depressed below the level of the surrounding plain.

Schumm and Simons' contention that the course of Rima Prinz I is "unusual" fails to recognize the fact that the course of this rill and of neighboring ones is partially controlled by a rather conspicuous regional fracture pattern, as are the courses of terrestrial rivers. Their statement that there has been no major mass movement on the walls of Schroeter's Valley is contradicted by the fact that "only half of the channel is visible." The only places where Rima Plato II appears discontinuous are those where the channel has been obliterated by obvious impact craters.

The very distinctive morphology of the lunar sinuous rills, particularly the mature meanders, goosenecks, distributary channels, flood plains, and other features similar to those of terrestrial rivers, requires that they be features of surface water erosion.

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27 March 1969

Hierarchical Structures

The portion of the summary of the Conference on Hierarchical Structures describing the "cosmic diagram" (*1*) contains the same error in Fig. 1, the caption, and the text.

In Fig. 1, the limit parallel to the Schwarzschild limit marked $m = Sr$ should be marked $m/r = Sm_p/a_0$. In the caption, the limit $m/r = S = 10^{39.4}$ should read, $m/r = Sm_p/a_0 = 10^{23.8}$ g/cm. In the text (p. 1229, right-hand column, line 17), the phrase "or at $m = Sr$" should be similarly changed.

The maximum observed gravitational potential for stars, galaxies, and clusters of galaxies appear to have closely the same value in the neighborhood of $10^{23.5}$ g/cm. In dimensionless terms—expressing mass in units of baryon mass m_p, and lengths in units of the Bohr radius a_0—the observed potential limit takes the value $ma_0/m_p r = 10^{39}$ or fS where f is a number of the order of unity. From the definitions, $S = e^2/Gm_p m_e$ and $a_0 = e^2/\alpha^2 c^2 m_e$, it follows that for the observed limit $Gm/c^2 r = f\alpha^2$ compared to $GM/c^2 r = \frac{1}{2}$ for the Schwarzschild limit. The fine structure constant thus emerges from astronomical measurements, under the assumption that all dimensionless physical numbers of the order of 10^{39} are the same (*2*).

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1 April 1969

Granitic Rock: Properties in situ

Simmons and Nur (*1*) have reported that laboratory measurements of sound velocity and electrical resistivity of granitic rocks yielded results that were inconsistent with certain measurements *in situ*. One possibility they offered to explain this inconsistency is that the rock *in situ* lacks the small, open cracks evident in the laboratory specimen. They conclude that "the absence of small, open cracks that close due to lithostatic pressure with depth in the earth's crust holds serious implications for geophysics." I do not wish to treat here the important question of whether cracks are present in rock *in situ* but simply to suggest that the conclusions reached by Simmons and Nur may be based on doubtful evidence. My principal objections to their comparison of measurements *in situ* and in the laboratory are as follows:

1) The lithology of the Matoy well is extremely complex (*2*), with wide variations in composition, grain size, and texture. It seems highly questionable to compare a measurement made *in situ* over a wide suite of rocks with laboratory measurements for a single rock or rock type. Although half the cuttings examined by Ham *et al.* (*2*) were described as diorite or diabase rather than granite, the velocity of these cuttings *in situ* was compared with the velocity of granites.

2) I have studied in detail the electrical log for the Phillips No. 1 Matoy well. It is very difficult to obtain the

true rock resistivity for resistive rocks (> 100 ohm-m) in any well, especially where a conductive (1.0 to 3.1 ohm-m) drilling mud was used, as in this case. Simmons and Nur apparently used the log of the "short normal" tool, which is known to give a poor estimate of rock resistivity under these conditions (*3*). In the complete log, both the long normal and lateral tools gave resistivities approximately an order of magnitude greater than for the short normal, resistivities which are probably closer to the actual value for the rock.

3) Resistivity of saturated rocks is determined, as the authors evidently realize, by rock porosity and pore fluid salinity, not by mineralogy. Yet they compare the resistivity of laboratory samples of three granites with the resistivity measured in the wells (*1*, Fig. 4). The comparison may as well have been with three gabbros or three shales. Such a comparison is meaningless without a knowledge of how porosity and salinity vary with depth in the wells.

4) In the upper 5 to 10 km of the crust, the principal contribution to porosity will probably be faults, joints, and other natural planes of separation rather than the intergranular cracks evident in laboratory samples. Fracture porosity under pressure does not behave like typical crack porosity in the laboratory with respect to resistivity (*4*). Geologic evidence indicates that there must be appreciable fracture porosity in the areas of the two wells, areas of major faulting and overthrusting. In fact, the presence of many faults is suggested by the wide variation with depth in the electric logs of the Matoy well.

5) Finally, a small point is that the conversion from pressure to depth may be incorrect. Effective pressure (rock weight minus pore pressure) rather than lithostatic pressure must be computed. At the bottom of the Matoy well, for example, effective pressure is about 600 bars, not 1000 bars as given in the paper. This reduces the slopes of the laboratory curves presented by about a factor of 2.

In general, studies of crustal materials in the laboratory cannot easily be extended to measurements *in situ*. That extension requires not only a basic understanding of the laboratory measurements but also an awareness of the difficulties of obtaining data *in situ* and then interpreting them.

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2 January 1969

We believe that Orange missed the significant points of our paper (*1*). The properties of the granites *in situ* do not change as much with depth (in the two wells that we examined) as one would have expected on the basis of previous laboratory data. Two explanations were offered: (i) complete saturation with water has an effect on the elastic properties of granite [demonstrated in the laboratory measurements on Troy granite (*1*)], and (ii) the microcracks that exist in the small laboratory specimens of granite either do not exist in the rocks *in situ* or do not behave as a function of depth in the way that one would predict from the laboratory observations. At present, we are unable to decide between these alternatives. Thill and Bur (*2*) recently reported the effects of saturation on the St. Cloud grey granodiorite; their results are similar to our observations on the Troy granite and show the dramatic effect of saturation on the elastic properties of another very low-porosity rock.

Our conclusions were based, in part, on data for the Phillips Petroleum Company No. 1 Matoy well, taken over several granite sections in the well. The velocity data for two such sections were shown in our original paper. The lithologic log (*3*) based on the well cuttings and a few cores indicates that rock in the interval from 387 to 523 m is granite, except for two dikes at 472 to 485 and 504 to 518 m. The other interval from 2954 to 3086 m is shown as all granite. Because we restricted our observations to granite sections, Orange's remarks that other rocks were penetrated by the drill seem irrelevant.

Because of the difficulty of obtaining the true resistivity of highly resistive rock from electrical logs, we emphasized the change of electrical properties with depth, rather than their absolute values. The presence of faults, joints, and related openings in the rocks of the Matoy well, although not numerous, has no bearing on the evidence presented since they have very little effect on the interval velocity log and can be readily recognized on the electrical logs (and therefore were omitted from consideration).

In his last point Orange suggests that the effective pressure at the bottom of the Matoy well is about 600 bars rather than 1000 bars. His suggestion contains the implicit assumption that the pore pressure is equal to the hydrostatic head, and hence that the pores, completely filled with water, are connected to the surface. Stress concentration around the borehole further complicates any analysis. Fortunately, precise knowledge of the effective pressure is not critical to our observations that the properties change much less with depth than the previous laboratory data would have led us to expect.

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22 April 1969

Carbon-14 Labeled Vasoactive Peptides Available

The following peptides are available in limited amounts (up to 20 μc of the labeled and 10 mg of the nonlabeled peptides) to qualified applicants. *Labeled peptides:* Asp^1-Ileu^5-angiotensin II (200 μc/μmole) containing uniformly labeled C^{14}-isoleucine; Lysylbradykinin (200 μc/μmole) in which the 3 and 4 positions contain uniformly labeled C^{14}-proline. *Nonlabeled peptides:* Asp^1-Ileu^5-angiotensin II, Lysylbradykinin, and Methionyllysylbradykinin.

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15 May 1969