dictions of its central tenet. We are not suggesting that the membranes of these cells lack the intrinsic capability of bursting; it is well known that they can burst, even in the absence of synaptic input. The intrinsic, voltage-dependent, slow, inward current described by Johnston *et al.* (*10*) may add to the depolarization produced by the postulated summated EPSP, a possibility which can now be explored further by voltage-clamp experiments.

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14. We use the term apparent reversal potential to indicate that the reversal potential measured in the soma may be more positive than that at the subsynpatic membrane. The attenuation of voltage from the soma to the synapse, however, is probably small (*13*).
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16. We thank G. F. Ayala, J. Hablitz, and R. K. S. Wong for reading the manuscript and for useful discussion. This research was supported by NIH grants NS15772 and NS11535 and by biomedical research support grant RR-05471-17.

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Minor Planet Satellites

Reitsema (*1*) challenged the reliability of the observational data from which Binzel and I (*2*) concluded that satellites of minor planets are numerous and commonplace. I offer the following replies to the questions he raised.

The confirmed satellite of 532 Herculina was detected by an experienced occultation observer, J. McMahon, along with the occultation by Herculina itself and five other occultation events. Reitsema refers to the five unconfirmed visual observations as "apparently spurious." Yet McMahon reported all six secondary disappearances and reappearances as having the same sharp, distinctively occultation appearance as the Herculina event itself. With nearly four magnitudes of light loss involved, events caused by atmospheric turbulence were out of the question. His timings of the Herculina event, the ninth and tenth timings in his series of 14, agreed well with those of other observers. And the only secondary event long enough to have had a counterpart in the Lowell lightcurve did appear there at exactly the location McMahon's timings indicated. I do not know of any valid basis for sugesting that McMahon's other events are spurious.

Reitsema gives low credence to visual occultation observations, but in my experience such observations are highly reliable. It is one of my major duties at the Naval Observatory to reduce and analyze both visual and photoelectric lunar occultation timings, which are currently made at the rate of 10,000 per year. I have analyzed 140,000 such timings. Less than 10 percent of these observations need to be rejected, and most of those because of positional errors in the star catalog used for reductions, errors in the station coordinates, or problems beyond the control of the observer. Grazing occultations are particularly relevant to the asteroid situation, since they are observed mainly by amateurs, at remote field locations, with portable telescopes, under adverse climatic and environmental conditions, and involve the possibility of a miss or an unknown number of disappearances and reappearances over about a 5-minute interval. The important difference from asteroids is that each observer's report can be checked for consistency with those of the observers on either side of him, typically 500 to 1000 feet (0.15 to 0.3 km) away, and in this way again it has been established that typical observations, including a sample of inexperienced observers and averaged over all manner of telescopes and field conditions, are nearly 90 percent reliable. Secondary events, which seem to be the rule with asteroids, simply do not occur for lunar occultations, except in poor seeing with a star close to the threshold of visibility. In the latter situation the observer is aware that the flickering is probably atmospheric from its extremely rapid and random nature. There is no reason I know of why the 90 percent reliability of visual lunar occultation timings should not also be valid for minor planet occultations. However, the minor planet occultations have been more carefully investigated than most, and I expect the reliability of the reported secondary events to be higher than average. The observations listed as "probable" in (*2*) would be particularly difficult to explain in any other way.

Photoelectric recordings of occultations can be spurious for different reasons, but there are not many ways to get spurious photoelectric events without realizing it at the time. The photoelectric method provides numerous checks, the strongest of which is on the occulted light level, which in general is neither the background level nor the zero level. For 18 Melpomene it was easy for photoelectric observers to distinguish the star's light, the light from Melpomene, their combined light, the empty field light, and the zero light levels. Clouds have never in my experience produced an intensity change so rapid as to be confused with a true occultation, and in any event the light level is not usually the same for clouds and occultations.

Further, there is no real possibility of confusing drifts out of aperture with occultations. Even without sidereal drive, the seeing disks of the stars would produce slower light changes than a true occultation (assuming one has enough time resolution to see the difference). Under ordinary circumstances, it takes 10 seconds or more for seeing disks of stars to enter or leave a diaphragm. The two spurious events mentioned by Reitsema for the 13 Egeria occultation would have to be shown in a figure that displays some of the lightcurve before their misinterpretation as occultations could be judged as näive or realistic.

The last point concerns our conclusion in (*2*) that minor satellites are numerous and commonplace. After the 6 Hebe event, I argued that the secondary observation could not have been a satellite because the odds were great against one observer seeing Hebe's only satellite by chance, and even greater against his having looked at the only minor planet with a satellite. I did not accept the con-

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clusion that satellites were numerous and commonplace until Herculina's confirmed satellite observation. The fact that the 23 secondary events reported for eight minor planets (*2*) were observed by very sparse observer networks only serves to strengthen the conclusion.

In summary, I still consider that the existing observational data are reliable and support the conclusions drawn in (*2*). Nevertheless, I endorse Reitsema's call for additional observations by observers placed to provide redundancy. This idea has apparently already borne fruit during the occultation by 216 Kleopatra on 10 November 1980. Two visual observers spaced 2000 feet (0.6 km) apart parallel to the asteroid's ground track, but several hundred kilometers outside it, each observed essentially simultaneous total occultations of 1/2-second duration, which would correspond to a 5-km satellite.

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Geothermal System at 21°N

In his report on the geothermal system at 21°N, Bischoff (*1*) argued convincingly that the maximum subsurface temperature of the geothermal fluid should be higher than 350°C. He further demonstrated that the maximum temperature could be estimated from the pressure-volume-temperature (*PVT*) properties of seawater and suggested that it should be less than 420°C. However, he assumed that the *PVT* relations for seawater are the same as those for pure water and used pure water data for his arguments. This assumption is indeed valid in most cases, but should be used with caution.

For instance, it is well known that the existence of sea salts depresses the temperature of maximum density of water in the low-temperature range (*2*). On the other hand, sea salts raise the critical temperature of water in the high-temperature range (*3*), although little is known of the salinity dependence of the critical temperature of seawater. As a first approximation, I assume that the critical temperature of the deep Pacific seawater (34.7 per mil in salinity) is the same as that of a 0.615 molal NaCl solution. The resulting critical temperature is approximately 40°C higher than that for pure water (*3*).

Bischoff argued that the pressure limits for the circulating hydrothermal water are confined between 250 and 500 bars. He further argued that buoyancy is the driving force and thus the limiting factor for the maximum temperature the hydrothermal fluid acquires (*1*, *4*). Next he showed that a plot of the specific volume of pure water against temperature (his figure 7, redrawn as dashed lines in Fig. 1) indicates that water at 250 bars expands rapidly at temperatures above 380°C and water at 450 bars expands markedly above 420°C. Judging from the rapidity of water expansion above 420°C at 450 bars, Bischoff concluded that the maximum possible temperature cannot be much above 420°C.

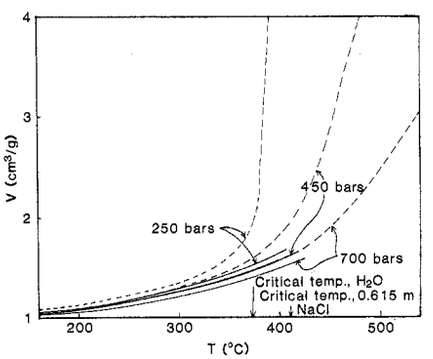

Fig. 1. Specific volumes of pure water and 0.615 molal NaCl solution versus temperature at 250, 450, and 700 bars.

The arguments of Bischoff are basically right, but he probably underestimated the maximum temperature. I have plotted in Fig. 1 the specific volumes of a 0.615 molal NaCl aqueous solution (solid lines) at various pressures (*5*). The curves are similar to those for pure water below 320°C, but are distinctively different once the critical temperature for pure water is approached.

At 250 bars, for instance, the specific volume of pure water increases drastically above 370°C, whereas that of the NaCl solution increases steadily to about 400°C. Presumably the NaCl specific volumes would start a rapid increase above the NaCl critical temperature, but no data are available. At 450 bars, the specific volume increase for the NaCl solution also lags behind that for pure water by about 40°C. In other words, it may be possible to heat the circulating hydrothermal seawater (NaCl) to near 460°C instead of only 420°C as suggested by Bischoff. This seemingly small difference in temperature could mean a significant difference in mineral solubility, and metal transport and deposition mechanisms (*1*) thus should not be neglected.

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