common laboratory animal may have a very similar though less pronounced stimulus intensity control system to that previously observed in humans and that relation of the control system to behavior may also parallel that found in people. The ability of these neurophysiological data to predict individual differences in animal "personality" was rather striking. This opens the possibility that common laboratory animals may be used for extensive experimental study of these phenomena.

R. A. HALL
Santa Clara County Mental Health Services and *Institute for Medical Research, San Jose, California 95128*
M. RAPPAPORT, H. K. HOPKINS
R. GRIFFIN, J. SILVERMAN
Agnews State Hospital, San Jose, California 95114

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Criteria of Brain Death

I suggest that it is important, in regard to the experiments of Hossmann and Sato (*1*), to elucidate the reasons for the recovery of brain function after presumably prolonged anoxia under their experimental conditions, since this is contrary to the experience of other investigations on the effects of cerebral anoxia, beginning with the classic experiments of Sugar and Gerard (*2*). Furthermore, the conclusion that this "raises serious questions about the reliability of criteria currently used for the determination of brain death" is unwarranted. Experimental anoxia must approximate the clinical situation in man to have relevance to the problem of criteria for brain death; even at that, species differences must always be considered, as well as the fact that the human condition defies precise measurement of the degree and length of anoxic insult.

The ultimate answers to the criteria for brain death must come from the human experience. The clinical criteria evolved thus far (*3*) are eminently conservative: totally unresponsive coma, loss of all motor function (including respiration), loss of reflexes and electrocerebral silence (defined as no electrical activity over 2 μv when recording from scalp electrode pairs ten or more centimeters apart with interelectrode resistances under 10,000 ohms but over 100 ohms) existing for a 24-hour period, except in the instances of initial overdose of a central nervous system depressant drug, or hibernation, when the observation period must be extended. Evidence is accumulating that spinal reflexes may be preserved despite brain death. Whether shorter observation periods in specific clinical situations, as has been suggested by some, are appropriate has yet to be determined by systematic research in man.

DANIEL SILVERMAN
Graduate Hospital of the University of Pennsylvania, Philadelphia 19146

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We agree that an important aspect is to elucidate why, in our experiments, neuronal recovery occurred after more than 1 hour, in contrast to earlier investigations. In most of the experiments to which Silverman refers (*1*) the pneumatic cuff method was used, which produces an interruption of both the arterial blood supply to the brain and the venous outflow from the brain. Ames *et al.* (*2*) showed that this may cause an impairment of the blood recirculation (no reflow phenomenon) after a few minutes of ischemia. In our experiments, in which the venous outflow was not blocked, a "no reflow phenomenon" did not occur, and this was possibly one of the reasons for the improved recovery.

We have discussed the reliability of the criteria on brain death mainly because in our experiments the electroencephalogram (EEG) was still isoelectric when membrane excitability and synaptic transmission had already recovered for a long time. Furthermore, the reappearance of the EEG seemed to depend on a relatively high blood pressure and could be delayed at normotensive levels. We have noticed the sudden recovery of EEG activity after many hours of electrocerebral silence when the blood pressure was increased. This suggests that even prolonged electrocerebral silence does not prove the irreversible loss of neuronal function. We do not deny that the human brain is irreversibly damaged when the criteria elaborated by Silverman *et al.* (*3*) are fulfilled, but we feel that this is due to the current limitations of therapeutic measures rather than to the reliability of these criteria themselves.

K.-A. HOSSMANN, K. SATO
Department of General Neurology, Max Planck Institute for Brain Research, Cologne-Merheim, Germany

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Amino Acid Synthesis in Simulated Primitive Environments

In reference (*10*) of their report Bar-Nun *et al.* (*1*) stated: "H. R. Hulett (*2*), by confusing meteorites with meteoroid and micrometeorite fluxes, reached an erroneous value of 4×10^{-5} cal cm^{-2} yr^{-1}" for the energy flux of meteoric material on the earth. I should have used some term other than meteorite to refer to the incoming material. However, in one of the references I cited there appears the statement: "The total mass of meteoric material that enters our atmosphere per day may be something of the order of 100 tons" (*3*). This refers to all incoming solid material, not just material that

reaches the surface without disintegration. If this material is considered to have the average kinetic energy of about 2×10^4 joule/g assigned by Wood (*4*) (and also referenced in my paper), the energy flux is indeed of the order of 4×10^{-5} cal cm^{-2} yr^{-1}.

Both the figures used as the basis for the calculation of energy flux may be erroneous. However, a total influx of the order of 100 tons per day is corroborated by Barker and Anders' work (*5*) on the chemical analysis of sediments for elements (Ir and Os) depleted on the earth with respect to meteorites. They found a most probable value of $6 (\pm 3) \times 10^4$ tons per year and an upper limit of about 1.5×10^5 tons per year, much less than the value of about 3×10^6 tons per year given by Bar-Nun *et al.* As I have indicated in my paper, the flux of meteoric material might have been much greater on the primitive earth.

The kinetic energy may well be more than the value assigned by Wood. However, even if the mean relative velocity is 35 km/sec, the energy available at present is far below that from most of the sources listed in table 1 of my article (or that quoted by Bar-Nun *et al.*).

Meteoric energy is concentrated in a region where the pressure is only about 10^{-7} atm. In addition, at least in the present atmosphere, the partial pressure of H_2O is very small. Both these factors make the large-scale synthesis of complex oxygen-containing compounds by the use of meteoric energy extremely improbable. On the other hand, the temperature at such pressures is high because of heating caused by the absorption of ultraviolet radiation and the synthesized compounds themselves are labile to such radiation. Thus the approach to equilibrium would have been rapid, and the net accumulation of required compounds would have been small.

As Bar-Nun *et al.* pointed out, the shock waves associated with lightning would have been a more promising source of energy. However, the high efficiency of conversion to amino acids reported in their recent experiments is probably to some extent a function of the fact that over three-fourths of the carbon in the reaction mixture was present in the form of ethane (C_2H_6). Less extensive chemical changes are required to convert ethane to an amino acid than to convert methane, but there is no reason to believe that ethane was present at high partial pressures in the primitive atmosphere. As Bar-Nun *et al.* noted in their reference (*3*), earlier attempts at the shock synthesis of organic compounds from mixtures of methane, ammonia, and water (but no ethane) were unsuccessful.

Bar-Nun *et al.* have proposed that the products may be the result of rapid quenching of a reaction mixture close to equilibrium at about 2000°K, with glycine, for example, resulting from the combination of the components of the reaction mixture with H radicals and alanine from combination with CH_3 radicals. However, Duff and Bauer (*6*) [reference (*5*) cited in Bar-Nun *et al.*] indicated that the partial pressure of these radicals in a mixture containing about two H atoms per C atom at 2000°K and 1 atm (close to the condition of the reaction mixture at this temperature) is very low—orders of magnitude lower than that required for the reported percentages of synthesis. Thus the proposed mechanism appears of doubtful validity.

I would also like to comment on the suggestion that all the organic molecules might somehow have survived for periods of the order of 10^9 years. All synthesized compounds would have been subject to degradative processes, which would have resulted in a lifetime of a few years or less for substances like glycine and other amino acids. I computed maximum generation rates for glycine of the order of 3×10^{-8} mole cm^{-2} yr^{-1}, which would lead to concentrations in solution of about $3 \times 10^{-6}M$ for bodies of water 1 m or more in depth (*2*). If we disregard the obections I have raised and assume the synthetic rates proposed by Bar-Nun *et al.* ($\sim 10^{10}$ organic molecule cm^{-2} sec^{-1}), about 3×10^{-7} mole cm^{-2} yr^{-1} of glycine might have been synthesized, which would have led to concentrations of the order of $3 \times 10^{-5}M$—still very low to have made extensive participation in further synthetic processes possible.

H. R. HULETT
Stanford University School of Medicine, Stanford, California 94305

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We welcome Dr. Hulett's remarks for the opportunity they provide to clarify some aspects of our report (*1*). The flux of cometary meteors and micrometeorites relevant to shock production of amino acids is that characteristic of the earth within a few $\times 10^8$ years of its formation, and not the contemporary flux. Since the presently accepted e-folding time for the earth to sweep up interplanetary debris is $\sim 10^8$ years, it is clear that the flux on the primordial planet, at a time when material from the solar nebula had not yet been completely swept up, was much larger than it is today; and a value of 2×10^{-14} g cm^{-2} sec^{-1} seems conservative. But the question is not of great importance for shock synthesis, since the energy available from thunder on the early earth appears to be at least an order of magnitude larger than that from meteoroids of all descriptions (*1*).

The mechanism we propose involves several steps, of which only the first occurs under reflected shock conditions, where the system approaches a nearly equilibrium mixture of radicals and atoms. Synthesis occurs not under these conditions, but rather during the quench period when the radicals react (presumably through a chain mechanism) and eventually condense. Since the preparation of the system for reaction occurs under nearly equilibrium conditions, at high temperatures, the initial condition of the substances of potential reactivity is of no consequence and the distribution of reactions that occur during the quench period, in the operation of chain reactions and condensations, is determined by the initial distribution of radicals.

The only effect of substituting ethane for methane at constant atomic mixing ratios is a very small change in the temperature behind the shock for a given shock speed. Work on the conversion of hydrocarbons to acetylene at similar temperatures (*2*) clearly demonstrates that the original molecular composition is not critical to the results.

It is also of interest that studies of the photochemistry of methane on Jupiter (*3*) suggest that ethane is the most abundant higher hydrocarbon there, and an indication is drawn for hydrocarbon abundances monotonically increasing with time unless they are pyrolyzed at great depth. Since the surface temperatures on the primitive earth at the time under discussion were not very high, it is likely that ethane was an

important minor constituent of the early terrestrial atmosphere.

The correlation of radical ratios at equilibrium with the amino acid ratios after quenching was merely indicative of the type of processes operative in the shock tube. The recombination reactions involve several steps, presumably in a chain mechanism, and no simple bimolecular process is to be inferred from this correlation.

As to the accumulation of shock-produced amino acids in the oceans, a layer of water 1 m deep will become a $5 \times 10^{-6} M$ solution within 1 year without photolysis. If thicker layers of water are involved and the solution is homogeneous, the concentration will decrease linearly with depth. However, the photolysis will also decrease, exponentially with depth, because ultraviolet absorption becomes very great at some tens of meters depth in pure water, and at shallower depths when we allow for ultraviolet-absorbing solutes (*4*). Even with a residence time in the deep oceans of only a few thousand years, the expected concentration will be some orders of magnitude higher than the yearly accumulation of some 10^{-6} mole per liter. Thus a fairly high concentration of organic material will exist for further evolution in the oceans and shallow pools of the primitive earth.

A. BAR-NUN*
Department of Chemistry, Cornell University, Ithaca, New York 14850
N. BAR-NUN
Laboratory for Planetary Studies, Cornell University
S. H. BAUER
Department of Chemistry, Cornell University
CARL SAGAN
Laboratory for Planetary Studies, Cornell University

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* Present address: Department of Physical Chemistry, Hebrew University, Jerusalem, Israel.

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Tidal Anomalies and Regional Geological Structure

In a recent report, J. T. Kuo *et al.* (*1*) gave some results of tidal gravity measurements made at nine different stations across the United States. As one of their conclusions, they stated, "There is no observable correlation between tidal gravity parameters and the regional geology." This conflicts with results obtained in Belgium (*2*), Italy (*3*), Switzerland (*4*), and other countries. Where observations have been made at fairly close stations, the relationship between tidal anomalies and regional geological structure is obvious.

In the case of the U.S. stations, observed and calculated values for M_2 and O_1 constituents show a certain degree of correlation, but there are still sizable differences. Observed values can be either greater or smaller than calculated values, with the maximum difference of $\Delta\delta$ slightly exceeding 3.0 percent for M_2 and being about 4.0 percent for O_1. Observed values of $\Delta\delta$ range from -3.8 to $+4.2$ percent in the case of M_2 and from -0.5 to $+4.2$ percent in the case of O_1. Therefore, the anomaly between observed and calculated values can be rated as fairly large; it represents about 40 percent of the observed range for M_2 and about 85 percent for O_1. This anomaly has been plotted in Fig. 1. A positive anomaly characterizes the Rocky Mountains, a negative anomaly is found in the Appalachian area, and the stable interior seems to be nonanomalous. A dipping anomaly gradient is found toward the Pacific; an opposite gradient occurs near the Atlantic. It would appear that an explanation of the anomalies cannot be based solely on oceanic effects but must be correlated with geological structure.

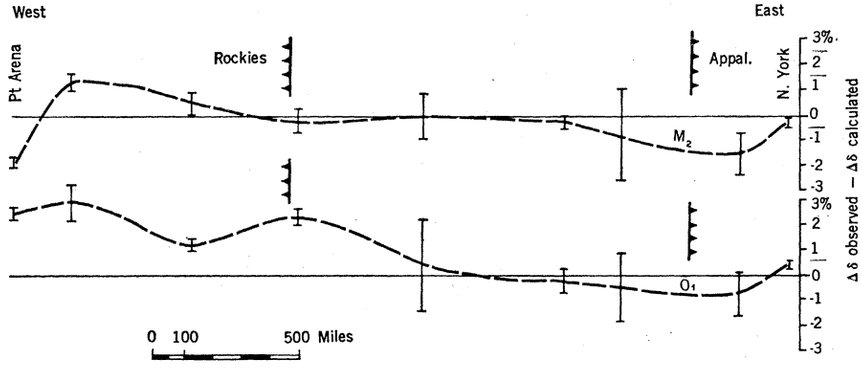

Fig. 1. Anomaly between observed and measured $\Delta\delta$ across the United States. [Data from Kuo *et al.* (*1*); 100 miles = 160.9 km]

When observed and calculated values of phase k are compared, anomalies that are very similar for M_2 and O_1 can also be detected. Slightly positive (lag) values are found over both the Rockies and the Appalachians; negative (lead) values occur over the stable interior. Anomalies range from $+0.5°$ to $-0.9°$ for M_2 and from $+0.5°$ to $-0.8°$ for O_1. Here again, it seems that oceanic effects alone cannot explain the anomalies.

It thus seems that the data obtained in the United States do show a certain correlation of earth tides with broad geological structure. It is, at present, impossible to understand fully the extent and nature of secondary geological effects. But the understanding of geological effects from static gravity measurements at nine stations across the United States would be equally difficult. With less scattered measurements, the impact of geological effects becomes more obvious. Figure 2 shows the relationship of tides measured by Melchior (*2*) and Ducarme (*5*) with geological structure.

It is therefore suggested that the extent and nature of geological effects should be investigated by observations at a number of stations which, although geographically close together, would be on definitely different structural units. In the western United States, stations equally distributed over the core of the Rockies, in their frontal thrust zone, in the foothills, and in nearby areas of the stable interior would probably prove valuable. Nearby stations on both sides of major faults should also be established (*4*).

In the past decade, an increasing number of geologists (including the late E. Marchesini) have realized that many geological lineaments could be produced mainly by tidal movements. Fracture patterns generally show one