evidence indicates instead that seizures can begin, proceed, and stop independently of ATP concentrations in the brain.

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1 September 1970

It should be pointed out that in our report we were dealing with the relation between energy and the onset of convulsions. We agree with Collins *et al.* that the role of cerebral metabolism in the initiation and perpetuation of seizure activity merits continued critical evaluation. There exists, in fact, a large volume of literature on this subject, most of which we believe is consonant with our results. Our report is unique insofar as it describes concentrations of adenosine triphosphate (ATP) in the brain immediately before seizures deliberately induced by methods that provide a predictable and brief preictal interval.

Collins *et al.* have accurately emphasized the importance of apnea and increased muscular activity in earlier studies of cerebral metabolism during seizures (*1*). Their experiments confirm that cerebral energy expenditure increases three- to fourfold during electrically induced seizures (*1*), under which conditions the content of ATP and creatine phosphate (CP) is maintained at normal amounts only when the animal is both paralyzed and ventilated with 100 percent O_2. We cannot, however, accept the implication that our observation of cerebral ATP depletion before induced endogenous seizures is somehow invalidated by studies of cortical ATP after administration of an electroshock to otherwise normal brain supported in this manner.

In response to the other remarks, we wish to point out the following: (i) In the reference quoted in which Metrazol was employed (*2*), depletion of either brain ATP (in animals breathing room air) or creatine phosphate (in animals breathing 100 percent O_2) was, in fact, observed in the earliest assays, obtained 15 minutes after Metrazol administration (7 to 8 minutes after disappearance of seizure activity in animals breathing room air). Furthermore, our data reveal an interictal return of cerebral ATP toward normal amounts after the administration of Metrazol (*3*, figure 1), emphasizing the necessity for measurements before the onset of seizure. (ii) Our report specifically addresses itself to the apparent contradiction provided by recent studies of cerebral energy metabolism before seizures induced by methionine sulphoximine (*4*). (iii) We have no laboratory experience with hypoglycemic (insulin-induced) seizures, largely because the onset of convulsions—2 to 4 hours—is sufficiently protracted and unpredictable to preclude immediate preictal assay. (iv) The investigators (*2*) who used secobarbitone anesthesia and electroshock actually reported "generalized clonic jerks from 3 to 6 seconds after stimulation" (*2*). By 6 seconds, a 28 percent decrease in brain ATP and a 67 percent decrease in CP was, in fact, reported. Both values represent over 80 percent of the total depletion of high-energy phosphate content measured after electroshock. When using phenobarbitone and electroshock, they observed clonic movements between 2 and 10 seconds after stimulus, which coincided with decreased brain ATP. We do not interpret these findings as a demonstration of "dissociation" between seizure activity and brain energy stores.

With respect to the observations concerning ATP assay technique, we would like to point out that our experience with brain ATP measurements in over 4000 rats and mice suggests that one-half of a rat head (approximately 15 g) freezes faster than a whole mouse (20 to 30 g) with intact circulation (*1*). We find no advantage in the use of Freon at $-150°C$ (*1*) over liquid propane at $-187°C$. Brain ATP in control animals varies from study to study depending on experimental conditions. Control values in mice have been reported to range from 1.48 (*2*) to 3.02 μmole/g (*4*). Thus, within any given experiment, we consider the variance to provide the best criterion of assay validity; in this respect our values compare quite favorably with those of Collins *et al.* (*1*), who froze the intact animal and assayed the whole brain rather than the cortex.

Finally, we believe our data strongly support but do not by any means confirm the view that deficient energy metabolism plays a significant role in seizure genesis.

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Asteroid Landing

In their article entitled "Mission to an asteroid" Alfvén and Arrhenius (*1*) have made certain gross oversimplifications and omissions. These are as follows:

1) There is some reason to believe that asteroids have high axial rotation rates. Eros, for example, has an axial rotation period of 5 hours, 16 minutes (*2*). This important characteristic of asteroids was not included in either of Alfvén and Arrhenius' tables. Without adequate preplanning to include the rotational motions and rates of the asteroid, an asteroid might be chosen upon which it would be impossible to land. Eros, being roughly brick-shaped (*3*) with a "mean diameter" of 20 km (*2*), very probably has complex precessional motions. To attempt to land on or contact a planetary body without knowing how its surface moves presents no mean task. This task is certainly not "simpler than the landing on the moon," a landing that was well-prepared for with computerized trajectories and photographic surveys.

2) The various statements concerned with the gravitational-dependent properties (for example, escape velocity and "weight") might be in serious error if they were calculated from simple con-

siderations of static, mass attractions. A sufficiently high rotational rate could offset the mass-attraction forces. With these small planetary masses, these considerations appear to me to be vital when sketching a picture of man on the asteroids. Indeed, a "landing" or any contact might be impossible.

3) The idea that a spaceship that weighs 10 tons on the earth would weigh only about 1 kg on the asteroid is, of course, quite reasonable, if we assume a static model. But to imply that the 10-ton mass could be as easily moved about as the smaller mass is extremely unreasonable. The inertial mass of a 10-ton object is about 9000 times that of the 1-kg object. The inertia is what the spaceman would encounter upon attempting to jostle his spacecraft around. I should think it would be more like trying to right an overturned *Queen Mary* while perched on a porpoise's back over the Mindanao Trench.

4) If a man were to jump "about a kilometer high and return back smoothly after some 10 minutes," wouldn't it be very probable that he would set himself spinning by such an imprudent act and return back smoothly on, say, his head?

These points I raise are the sorts of "technicalities" that must be recognized and taken into consideration only after the decision is made to pursue this type of exploration. They are cautionary in intent and are directed primarily toward those who view this journey as a simple or easy accomplishment. Would it be wise for a fly, which has just learned how to land on a slow-rising balloon, to attempt a landing on a speeding bullet?

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Our article was indeed intended as a gross oversimplification and omits a number of considerations important in the design of actual asteroid missions. A few comments on those points selected by Guth may be in order.

1) Although many asteroids are rather isometric, some display periodic changes in magnitude, which are due to either longitudinal differences in albedo or to irregular shape. If an astronaut for some reason now difficult to understand would elect to land on one of the extreme protuberances of a body with spin period and elongation similar to those of Eros, he would have to contend with a velocity due to spin of the order of only a few meters per second. If he is foresighted enough to land elsewhere, this velocity would be still less.

2) Nonetheless, it is certainly our hope that the unusual scientific interest offered by the asteroids, coupled with intriguing operational advantages, would not lead designers to abandon normal prudence in the preparation of a manned mission. Fly-by experiments, beginning with the Grand Tour in the near future, will be valuable in this respect.

3) The fact that inertia is not diminished would appear as a stabilizing advantage in moving a 10-ton mass with linear dimensions of a few meters. The spacecraft would yield easily but slowly (in a minute), whereas the time required for the same operation on the earth is of the order of seconds. The analogy with the *Queen Mary*, the porpoise, and the Mindanao Trench has poetic quality but is physically misleading.

4) The art of body-spin control by momentum distribution is already well-developed by ski jumpers, sky divers, and cats under the demanding but familiar earthly gravitation. The growing experience by astronauts, using gas jets, would give additional confidence in future mastering of this essential aspect of space activity.

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Ionic Character of Bonds in Crystals

In his article "Bonds and bands in semiconductors" (*1*) J. C. Phillips repeats some incorrect statements about my theory of covalent bonds with partial ionic character. He contrasts my 1932 definition of ionic character and what he calls my 1939 definition in such a way as to indicate that the theory had been changed. In fact, the set of points labeled "Pauling, 1932" in Phillips' figure 9 does not correspond to my theory. The theory was formulated in 1932 for single bonds, each bond involving a shared electron pair (*2*). Phillips applied the theory in an incorrect way to crystals containing fractional bonds (*3*). I had described the correct way of applying the theory to these crystals in 1939 (*4*). The unsatisfactory calculation labeled "Pauling, 1932" is unsatisfactory because of the mistake made by Phillips.

There is no justification for the publication once again of this incorrect calculation by Phillips. Over a year ago I published a paper to point out that Phillips had made this mistake (*5*). (Phillips does not refer to my paper in his *Science* article.) The mistake in applying my theory had led him to say that the theory gives discrepancies of more than 200 kcal mole^{-1} with the observed cohesive energy. I pointed out that, in fact, the theory gives good agreement with the observed cohesive energy, the apparent discrepancies having resulted from his incorrect use of the theory. Despite this clarification, Phillips has continued to publish statements about my 1932 theory such as to indicate that it is faulty, when in fact he has been applying it incorrectly.

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Apparently Pauling has chosen to overlook the postscript to the article in question. It covers virtually the same ground as his letter does.

I have also modified my earlier statements in articles not cited by Pauling, for example, the companion letter to his reference (*5*).

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