was no response available to the animals to turn off the stimulation, whereas in my experiment there was. The decrease in activity produced by introducing food under Hu's conditions by no means proves that activity would decrease in a similar fashion when food is made available to animals given an opportunity to perform a well-practiced response to terminate the stimulation. Lapping up the food could slow down the animals' locomotions toward the OFF side, but reference to table 1 of my report indicates that they could walk over to the OFF side within 12 to 22 seconds while feeding; why then, did two animals spend averages of over 100 seconds on the ON side when food was available? The magnitude of these effects is much too great to be accounted for solely in terms of the slowdown in locomotion produced by feeding.

Third, Hu failed to first measure his rats' preferred durations of stimulation and then use these durations in the main part of the experiment. Rather, he arbitrarily selected a stimulus duration of 47 seconds which corresponds neither to the median duration of 14 seconds selected by my rats while the shuttle box was empty, nor to the median of 31.5 seconds selected when the floor was covered with appropriate goal objects. Similarly he arbitrarily selected an interstimulus interval (OFF duration) of 47 seconds which is much greater than the OFF durations selected by my animals (3 to 5 seconds in each condition).

In Hu's experiment the rats received the same long duration of stimulation both in the presence and absence of food, while in my experiment the durations received were more than twice as long with food as without. In my experiment the animals themselves controlled the durations of stimulation, so that aversive stimulation was minimized. In Hu's experiment it may have been that the aversive quality of long durations of stimulation was responsible for the increase in activity which occurred in the empty-box condition. In fact, it is possible that Hu's rats' activity did not increase during the stimulation received in the empty box until after the intracranial stimulation (ICS) had been on for a period of time that exceeded the durations which the rats would have selected, that is, until after it had become aversive. When Hu produced an increase in activity by removing the food from his box, the increased activity could have been due either to the absence of food or to the extremely long (and presumably aversive) durations of ICS imposed on the animals. Hu confounds these two variables. In my experiment the rats rarely received aversive stimulation under either condition since they themselves controlled the ICS durations. Hu should have determined preferred durations under the food and empty conditions and then programed these durations into the animals, matching each predetermined preferred duration to the appropriate condition.

But even had Hu done this, his "control" experiment would still be unsatisfactory, since programing preferred durations of brain stimulation into rats does not guarantee that they will be rewarding. Indeed, Steiner *et al.* (*3*) have shown that rats will tend to escape from such stimulation. This suggests that to insure that ICS is rewarding we must let the rat itself choose how much stimulation it will receive and when it will receive it. The activity generated by self-selected (rewarding) durations of brain stimulation may be quite different from that generated by (aversive) stimulation which is imposed on the rat by the experimenter.

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References

1. J. W. Hu, *Science*, this issue.
2. J. Mendelson, *ibid.* **166**, 1431 (1969).
3. S. S. Steiner, B. Beer, M. M. Shaffer, *ibid.* **163**, 90 (1969).

4 February 1971

Saharan Ordovician Ice Age

It is not correct, as I stated (*1*), that visitors to the Algiers meeting of the sedimentology group were invited to "confirm or deny" the existence of a Saharan Ordovician ice age. Under the auspices of the Institut Français du Pétrole of Paris, field workers had laid out the geology of this region and had established the existence of such an ice age. I tried to bring out that there was a convergence of evidence from paleomagnetists, paleontologists, and field workers. To the field geologists S. Beuf, B. Biju Duval, O. de Charpal, and P. Rognon goes the credit for the demonstration under rugged conditions of what previously had only been hinted at. I apologize to these pioneer workers for an unintended slight to their work.

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Reference

1. R. W. Fairbridge, *Science* **168**, 878 (1970).

11 January 1971

Catalyst for Auto Exhaust

Libby's note (*1*) on the potential use of $LaCoO_3$ as a catalyst for automobile exhaust prompts me to report the similar application of Pr_6O_{11} and Tb_4O_7. In 1958, at Stanford Research Institute, we used both of these on alumina carriers in the exhaust system of a small engine and monitored the emission by infrared spectrophotometry. Virtually complete breakdown of extraneous gasoline was achieved, and the catalysts were not poisoned by the lead component of the exhaust gases; indeed, we even loaded the system with lead bromide without affecting its function.

Use of rare-earth catalysts in the petroleum industry has been known for many years, but our data on their catalytic function in auto exhaust were never published. Even in the concentrations we used—about 5 to 10 percent on the carrier—the costs of praseodymium and terbium oxides cannot bear comparison with the estimated cost for $LaCoO_3$. But it would seem from our old experiments and Libby's data that the rare earths and particularly those (or their compounds) exhibiting paramagnetism do possess a high potential in this area.

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Reference

1. W. F. Libby, *Science* **171**, 499 (1971).

10 February 1971