when rats must traverse the grid for water. The measured force of press on a lever decreases as the interval between the brain stimulus and the next lever press is increased. If thirsty animals are given a choice between brain stimulation on one side of a T-maze and water on the other side, the probability of their choosing brain stimulation declines rapidly as the interval between the brain stimulus and the next trial is increased. Not only do such results demonstrate the generality of drive decay, they also avoid most of the factors that complicate interpretation in tests involving extinction.

J. A. DEUTSCH
Department of Psychology, Stanford University, Stanford, California

References

1. S. S. Pliskoff and T. D. Hawkins, *Science* **141**, 823 (1963).
2. J. A. Deutsch, *The Structural Basis of Behavior* (Univ. of Chicago Press, Chicago, 1960).
3. ———, *J. Theoret. Biol.* **4**, 193 (1963).
4. C. I. Howarth and J. A. Deutsch, *Science* **137**, 35 (1962).
5. D. H. Bullock and W. C. Smith, *J. Exptl. Psychol.* **46**, 349 (1953).
6. L. J. Herberg, *J. Comp. Physiol. Psychol.* **56**, 686 (1963).

In a statement of the drive-decay hypothesis (*1*), Deutsch wrote: "cessation of responding in the situation where the 'reward' is an electrical stimulus should be a function solely of the time since the electrical stimulus was switched off and not of the number of unrewarded presses executed." In fact, the subsection of the paper from which that statement is taken is entitled "Extinction or drive decay." The implication is strongly made that extinction is entirely accounted for by the drive-decay hypothesis.

Our data (*2*) indicated the relevance of a time-dependent process in extinction after reward by electrical stimulation of the brain. However, we also found that the details of the acquisition procedure were quite critical. So much so, in fact, that the extinction data and the effect of free stimulation during withdrawal of the lever were very different in our second experiment from what they were in our first. It was in the second experiment that we introduced the "lever-out, lever-in" training which significantly changed performance in extinction. Deutsch's letter is an attempt to explain the extinction data resulting from the modified procedure within the context of the drive-decay hypothesis. The critical concept advanced by Deutsch in his explanation is the "learned probability of reward." The latter concept is postulated to be a function of rewarded and unrewarded lever pressing. Deutsch states: "Where the learned probability is low, as it is when an animal has repeatedly found that response no longer produces reward, the animal will stop pressing the lever as soon as the intracranial stimulation is discontinued." Hence, extinction performance after brain-stimulation reward is a function not only of drive decay but also of unrewarded responding. Another factor determining extinction performance is, according to Deutsch's letter, effortfulness of response. Of course, the latter variable can be felt in the animal's extinction performance only if the animal is responding during extinction. Thus, extinction after brain-stimulation reward emerges as a process dependent on at least several variables, including unrewarded responding.

If that is Deutsch's present position on the matter, he is quite correct in stating that there is no essential contradiction between our results and his position. The absence of a conflict is the result, we submit, of a change from his original position—the view that extinction after brain-stimulation reward was a function "solely of the time since the electrical stimulus was switched off.

At certain critical points in our exposition (*2*) we referred to a "time-dependent process" rather than drive decay. Our reason for doing so is that one can easily conceive of another interpretation of the same data for which the drive-decay hypothesis was invented. That interpretation is nonmotivational in character and involves a well-known behavioral mechanism: stimulus control. The longer the period of time between the presentation of a discrete discriminative stimulus and the occurrence of the criterion or cued behavior, the less probable that behavior is. That observation formed the essence of Hull's (*3*) stimulus trace notion, and it has been documented by Smith (*4*). In addition to being a powerful reward, brain stimulation is a powerful stimulus. It is not surprising that when such a stimulus is removed from the behavioral situation, dramatic changes in behavior take place in a manner that appears time-dependent. As a matter of fact it is entirely possible that behavior maintained by brain stimulation is unmotivated and that such behavior indicates the effects of a pure reinforcer. Note that this position is diametrically opposed to the drive-decay hypothesis but consistent with the data for which that hypothesis was invented (*5*).

S. S. PLISKOFF
T. D. HAWKINS
Department of Psychology, University of Maryland, College Park

References and Notes

1. J. A. Deutsch, *J. Theoret. Biol.* **4**, 193 (1963).
2. S. S. Pilskoff and T. D. Hawkins, *Science* **141**, 823 (1963).
3. C. L. Hull, *Principles of Behavior* (Appleton-Century-Crofts, New York, 1963).
4. M. P. Smith, *J. Comp. Physiol. Psychol.* **44**, 154 (1951).
5. The experiments reported here and in (*2*) were performed under contract DA 49-193-2288 between the Office of the Surgeon General, U.S. Army, and the University of Maryland.

Exobiology

Several sources have privately suggested a new journal on "exobiology," the study of extraterrestrial life. My profound objections are not to their optimism; but the field is too important to be sequestered. The policy issues of interplanetary quarantine and of large-scale expenditures in scientific programs deserve the widest critical attention; so do scientific questions that range from the origin of life to the extraction of interstellar signals from cosmic noise. A specialized journal would only isolate the field from the badly needed critical judgments of a scientific community which, in the main, is not primarily preoccupied with exobiology.

The merits of this proposal apart, it points up a serious problem in our system of communication. The motivation for a new journal is a variable mixture of idealistic enthusiasm, egogratification, capitalistic enterprise, and rebellion against the critical judgments of the existing establishment. Owing to the operation of the copyright laws (which here convert a common good into a private interest), the proponents of a journal have a unique advantage, whatever their motivation. At least according to present custom we are morally accountable for its content according to our profession. If our societies continue to abdicate their responsibility for scientific communications, the successors will not always be so scrupulous.

JOSHUA LEDERBERG
Stanford University School of Medicine, Palo Alto, California