act, the being who should exercise it would be among the greatest of benefactors of the human race. But this stage of human perfectibility is yet far remote. The glory of the first attempt belongs to France. France first surveyed the subjects of weights and measures in all its extent and all its compass. France first beheld it as involving the interests, the comforts, and the morals, of all nations and of all after ages. In forming her system, she acted as the representative of the whole human race, present and to come. She has established it by law within her own territories; and she has offered it as a benefaction to the acceptance of all other nations. That it is worthy of their acceptance, is believed to be beyond a question. But *opinion* is the queen of the world; and the final prevalence of this system beyond the boundaries of France's power must await the time when the example of its benefits, long and practically enjoyed, shall acquire that ascendency over the opinions of other nations which gives motion to the springs and direction to the wheels of power." Clearly, the metric system has provided all nations the convenience and uniformity of measurement for which it was created.

Now France and all of Europe, except England, are in position to aid in the commercial and cultural development of Africa, a continent which rivals North America in potential for human welfare. That most of Africa will use the metric system is already established.

No period in history has been more critical than this one for conversion to the metric system by English-speaking countries. The active support of engineers, scientists, and educators is needed. Congress should provide the means (i) to thoroughly explore this important opportunity (*4*), (ii) to expedite increased use of the metric system, and (iii) to set deadlines for stepwise conversion. This is in the national interest. Such a government-sponsored transition, properly explained and motivated, will stimulate not only world trade but domestic business as well.

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I think working scientists will agree that it is far better for them to use one system of measurement than two, and it is easier for them to record data and compute in a decimal system than in a system having fractions like 1/12 or 1/64. The fact that the wavelength of a line in the spectrum of krypton-86 is a more precisely measured unit of length than is a metric or inch unit does not destroy the usefulness of the metric as a working system. It is the micron-to-kilometer orders of magnitude that are the concern of the majority of scientists in their daily work.

One does not expect industry and commerce to convert to the metric system unless they themselves choose to do so in their long-term interest, but since scientists depend in large part on scientific and technical discoveries for their advances, surely they are entitled to the system with which they prefer to work. What scientists prefer may be ascertained by anyone who picks up and looks into a few scientific journals in any library in any part of the world.

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Drive Decay and Differential Training

While Pliskoff and Hawkins (*1*) present some interesting evidence, there seems no essential contradiction between their results and the drive-decay hypothesis (*2*, *3*) of "extinction" in the Olds effect. They found that if rats were trained, by being repeatedly rewarded by electrical stimulation of the brain, to emit further responses in a Skinner box after the lever had been withdrawn and then reinserted, the number of responses up to extinction was greater than when no such training had been given. [In the latter condition the results of Howarth and Deutsch (*4*) were confirmed.] This is the only difference, in findings for the trained and untrained groups, for which Pliskoff and Hawkins claim statistical significance, and it is presumably mainly on the basis of this difference that they call in question the generality of the drive-decay hypothesis. However, this hypothesis does not state that drive level is the sole determiner of performance in the Olds phenomenon. It merely postulates that the differences between normal habits and habits formed through electrical stimulation can be understood if we assume that each electrical stimulus induces a drive for further stimulation (as well as a reward), and that this drive decays rapidly with time. The way this drive, in intracranial self-stimulation, enters into the determination of performance of a habit is not postulated to be different from the way drive operates in the determination of performance in normal habits. This being so, whether a given level of drive eventuates in performance depends on a multitude of factors which have already been shown to contribute to normal habits—for example, effortfulness of response (*3*). One such factor may be loosely termed the "learned probability of reward" (*5*). 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. This is the explanation of Herberg's results (*6*). On the other hand, in Pliskoff and Hawkins's experiment the learned probability of reward after the lever had been withdrawn and reinserted was much higher than the learned probability for the untrained group—the group in which the results of Howarth and Deutsch's (*3*) study were confirmed. As contrasted with the experimental conditions of Howarth and Deutsch's study and also of Pliskoff and Hawkins's first experiment, the animals in their later experiment had been highly trained and frequently rewarded for returning to the lever. It is to be expected that, as drive decays to an asymptote, lower levels of drive will continue to produce lever pressing in the group which has been trained in lever pressing.

Discussion of the other objections raised by Pliskoff and Hawkins appears unjustified, since they do not claim statistical significance for the data they present.

Since the generality of the phenomenon of drive decay has been called in question, it should be pointed out that many different experimental designs and situations have been used to verify its occurrence (*3*). For instance, it has been shown that an animal's speed of traversing an electrified grid decreases sharply as the investigator lengthens the interval between trials for brain stimulation, but that the speed increases 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.

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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*).

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References and Notes

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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, ego-gratification, 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.

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