Society of Systematic Zoology. (R. E. Blackwelder, Box 500, Victor, N.Y.)

Tomato Genetics Cooperative. (E. C. Stevenson, Horticulture Dept., Purdue Univ., West Lafayette, Ind.)

24–29. Atmospheric Diffusion and Air Pollution, intern. symp., Oxford, England. (F. N. Frenkiel, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.)

24–29. Mental Health, world federation, 11th annual, Vienna, Austria. (Miss E. M. Thornton, World Federation for Mental Health, 19 Manchester St., London, W.1, England.)

24–30. Prehistoric and Protohistoric Science, 5th intern. cong., Hamburg, Germany. (Büro des Internationalen Kongresses für Vor- und Frügeschichte, c/o Fremdenverkehrs- und Kongresszentrale, Hamburg 1, Bieberhaus, Hachmannplatz.)

25–28. Institute of Mathematical Statistics, annual, Cambridge, Mass. (G. E. Nicholson, Jr., Dept. of Statistics, Univ. of North Carolina, Chapel Hill.)

25–28. Mathematical Assoc. of America, 39th summer, Cambridge, Mass. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

Erratum: The correct address for the Society for the Scientific Study of Sex is 1 E. 42 St., New York 17, N.Y. The society will hold its first meeting on 8 November at the Barbizon-Plaza Hotel in New York.

Letters

Withdrawal versus Withholding of Positive Reinforcement

C. B. Ferster (*1*) in his experiments used withholding of food reinforcement (called by him withdrawal of positive reinforcement) in differentiation of conditioned motor reflexes. It seems, however, that under the circumstances of his experiments, the procedure cannot be considered as "the withdrawal of positive reinforcement" nor as "the withdrawal of the situation in which the reinforcement occurs." The situation in which the reinforcement occurred was only partially changed by switching off of the overhead lamp, or by appearance of the red light, and in spite of the fact that the animal performed the movement (pressing of the key), the reinforcement was withheld. The differentiation which was attempted in these experiments developed slowly and was only a partial one, because the reinforcement was withheld regularly only when the movement occurred during periods when the red light appeared, whereas in the absence of the red light the reinforcement was applied irregularly.

Ferster emphasizes in his report the analogy between the punishment and the withholding of positive reinforcement, both of which had as a result the elimination of the conditioned movement. But I think that there is a very important difference between the two events: the punishment, whether used in classic conditioning (type I) or in escape or avoidance conditioning (type II) (*2*), can give rise to new conditioned movements, whereas the differential inhibition can only eliminate some preexisting conditioned movements. The suggestion that punishment and differentiation have common "aversive" features is, as of the present time, based only on introspective impressions.

In my own experiments (*3*), when real withdrawal of positive reinforcement was used, a new kind of behavior was obtained. These experiments consisted in the withdrawal of food during the act of eating. The withdrawal was signaled by an acoustic stimulus applied 5 to 10 seconds beforehand. After several such trials the animal stopped eating when the stimulus sounded and turned away from the food tray. This conditioned "cessation reflex" was later differentiated: the food was withdrawn after one of the stimuli, but after another it was not. During the action of the first stimulus the dog turned away from the food, but during the action of the other (that which was not reinforced by withdrawal of food), he ate without interruptions.

Thus, both the inhibition produced by

withdrawal of food and punishment by pain reinforcement may give rise to new conditioned movements.

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References

1. C. B. Ferster, *Science* **126**, 509 (1957).
2. S. Miller and J. Konorski, *Compt. rend. soc. biol.* **99**, 1155 (1928); J. Konorski, *Conditioned Reflexes and Neuron Organization* (Cambridge Univ. Press, Cambridge, 1948).
3. A. W. Zbrozyna, *Acta Physiol. Polon.* **3**, 70 (1952); *Bull. acad. polon. Sci. Classe VI*, 5, 261 (1957); *Acta Biol. Exptl. Warszawa*, in press.

A. Zbrozyna is correct in stating that in establishing the aversive event in my experiment I did not withdraw all of the stimuli correlated with reinforcement. The stimulus I withdrew is the one differentially correlated with reinforcement. The conditioned response was *never* reinforced in the absence of the overhead light, while, alternately, it was intermittently reinforced in its presence. That the rate of pressing the key fell to zero in the absence of the overhead light is evidence of its effectiveness. The aversive nature of the absence of the overhead light is demonstrated by the suppression of key pressing during a second stimulus preceding the termination of the experiment (warning stimulus).

This experimental procedure has been studied extensively with electric shock as the aversive stimulus (see *1*). Whether the degree of aversiveness of the stimulus correlated with the nonreinforcement of the conditioned response depends on how many of the stimuli present during reinforcement are withdrawn is an experimental question worth raising. For example, would the discontinuation of the reinforcement of key pressing be more aversive if we physically removed the animal from the experiment for 60 minutes rather than "prevented" him from emitting the conditioned response by presenting a stimulus in whose presence the conditioned response has consistently gone unreinforced in the past?

The paradigm used in my experiment investigates only one aspect of an aversive stimulus: the suppression of some on-going, positively reinforced behavior by a stimulus preceding the aversive event. Other properties of aversive stimuli (such as electric shock) not studied in this experiment include (i) the property of maintaining another response which terminates, postpones, or avoids the aversive event; (ii) the property of differentially suppressing conditioned or otherwise maintained behavior by punishment—that is, by application of the aversive event to the response that is to be suppressed. Experiments demonstrating both the first (*2*, *3*) and second property (*2*) of the discontinuation of

positive reinforcement have already been carried out, although they have not yet been published.

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References

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2. C. B. Ferster, *Psychol. Monographs*, in press.
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Prepublication Problems

The editorial in *Science* [**127**, 623 (1958)] on "Pitfalls of prepublication" called attention to a new type of scientific publication problem.

Problems of printing the works of productive, perhaps overproductive, scholars are old ones. By the 17th century, for example, the practice of rushing into print was deplored by William Harvey, who wrote of "the crowd of foolish scribblers whose observations were as inconsequential as their theories were wordy" (*1*). Lilienthal, in his *De Machiavellismo Literario,* likened the offspring of such scholarly productivity to blind whelps brought forth without pain (*2*). Johann Mencken, writing in 1715 in *De Charlataneria Eruditorum* (*3*), could not overlook mentioning "those writers who consider themselves suitably blessed if no year, or better, no month passes without receiving something new from their exceedingly fruitful minds."

Until I read the *Science* editorial, however, I had been aware of only one complaint regarding prepublication productivity. That complaint concerned Paracelsus, who dictated the majority of his books. One of his students complained that they were dictated at such a speed "you'd think that the devil was speaking in him" (*4*). This prepublication complaint is interesting historically but barely applicable, because Paracelsus' books were handwritten manuscripts.

Today, however, all sorts of duplicating processes exist, making possible an extensive, but strictly informal, kind of publication—that is, prepublication. The *Science* editorial mentioned one reason for prepublication: accelerating the research process. Sending mimeographed copies of articles in press to colleagues makes them immediately cognizant of information that may not appear for months or, in the case of some journals in my own field, for years. In the field of psychology, three other reasons for duplicated copies have been advanced: (i) There is a growing tendency for convention "handouts" to take the form of full drafts of the paper to be read (*5*). (ii) Brief reports, limited to one printed page, are solicited by one journal for early publication. An author, however, is