

gested that the glucose inhibition of pyruvate utilization in ascites cells was caused by a combination of substrate competition and inhibition of respiration (the Crabtree effect); they proposed that the latter effect of glucose was on the decarboxylation of pyruvate. More recent studies indicate that the mechanism of the Crabtree effect which occurs in malignant tissues as well as in some normal tissues, involves the availability of adenosine di- or triphosphate or inorganic phosphate (3), and that the effect is not primarily due to a specific inhibition by glucose.

Rakitzis supports the concept that the cause of the aerobic glycolysis of malignant tissues is at the site of glucose absorption, and he cites the low incidence of cancer in diabetics (4) as evidence. In regard to this suggestion a quotation from Bell's paper deserves attention: "It appears that the total incidence of cancer in males over 40 years of age is about twice as large in non-diabetic as in diabetic cases, and in females there is an even greater preponderance in the non-diabetic cases. This is to be expected since every disease which shortens life shows a decreased incidence of malignant disease. The total incidence of cancer is likewise greatly reduced in tuberculosis, heart disease, and cirrhosis of the liver."

GEORGE E. BOXER

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1. H. J. Hohorst, F. H. Kreutz, M. Reim, *Biochem. Biophys. Res. Commun.* **4**, 159 (1961).
2. L. Bloch-Frankenthal and D. Ram, *Cancer Res.* **19**, 835 (1959).
3. A. C. Alsenberg, *The Glycolysis and Respiration of Tumors* (Academic Press, New York, 1961).
4. E. T. Bell, *Am. J. Pathol.* **33**, 499 (1957).

Probability Learning

In a report in *Science* (1), S. H. Revusky criticizes certain procedures ("forced trials" and "correction") which have been used to control the distribution of reinforcement in experiments on probability learning. With rats trained by Revusky's own ("nonreinforced trials") procedure choosing the more frequently reinforced side of a T-maze on 67.2 percent of trials in what may seem to be a conventional 67:33 probability-learning experiment, the casual reader is apt to gain the impression (i) that probability matching has been

demonstrated in the rat, and (ii) that previous failures to demonstrate it may be attributed simply to faulty procedures. Neither of these conclusions would be justified.

Has Revusky demonstrated "probability matching" in the rat? Not in the usual sense of the term (2). Nor has he even given us an experiment on "probability learning" in the original (3) and still current (4) sense of *that* term, which implies a random or quasi-random schedule of reinforcement. The schedule used by Revusky is far from

random, and a corresponding nonrandomness appears in the behavior of his animals. Examination of the protocols (5) shows, not the gradual emergence of a stable 67-percent preference for the more frequently reinforced alternative (as Revusky's mean values suggest), but a considerable amount of perseveration in one or the other choice—long runs of the preferred response *m* separated by somewhat shorter runs of the alternative response *l*. The tendency toward perseveration may be seen in the choices of one of the animals on the

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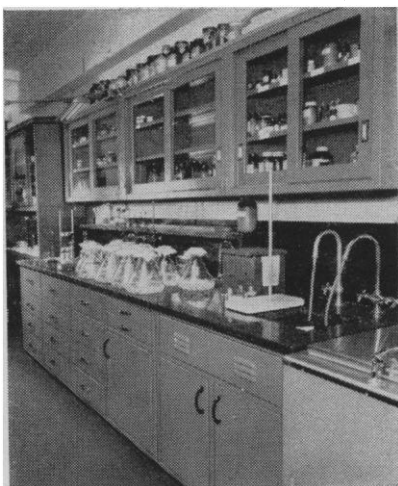
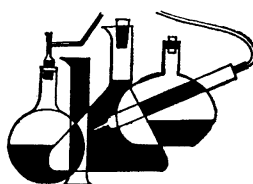
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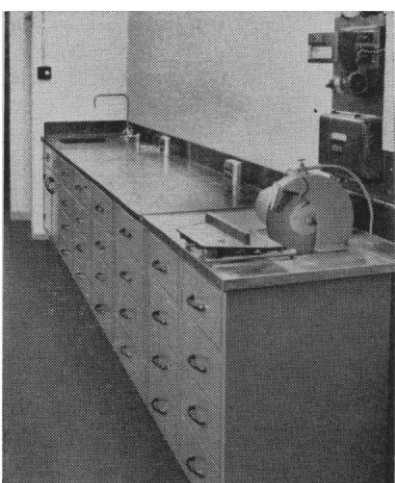
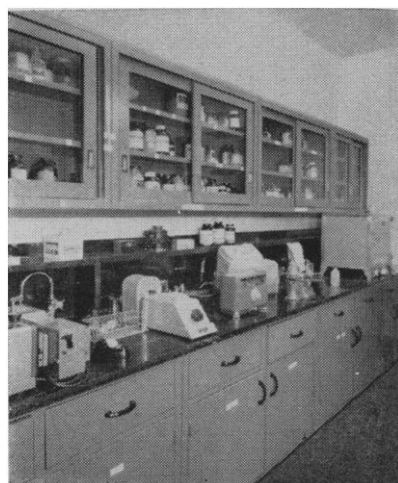
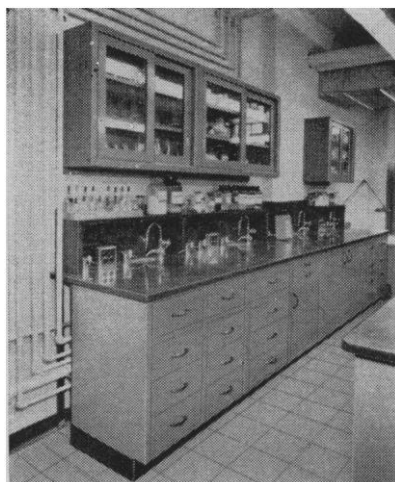
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last 50 trials (certainly not the most extreme instance): mmmmmmlmmmm-
mlllllmmmmmmmmmmmlmmmm-
mmllmmmmmmllm. For the group as a whole, the one-sample runs test (see 6) makes it possible to reject the hypothesis of randomness at the 0.001 level.

We do not wish to quarrel about definitions, but if control of the relative frequency of reinforcement in a choice situation is all that is necessary to constitute an experiment on "probability learning," and if a choice ratio approximating the reinforcement ratio is all that we are to mean by "probability matching," then matching in the rat was demonstrated long ago. For example, the single-alternation experiment (in which one of two alternative responses is reinforced on odd trials and the other on even trials) may be treated as a 50:50 problem, and the adaptive alternation of choices which the rat displays under such conditions may be taken as evidence of matching.

Are procedures which involve forcing or correction inadequate to produce matching? Not at all. Two experiments reported in 1958 (7) yielded some quite close approximations to matching in the fish *Tilapia macrocephala* (the African mouthbreeder), despite the use of forced correction ("guidance") in 70:30 problems. Without guidance, the animals "maximized"—that is to say, they tended to choose the higher-probability alternative on about 100 percent of the trials. These findings have since been confirmed in some further experiments with mouthbreeders in 80:20, 60:40, and 50:50 problems as well as in 70:30 problems (8), and like results have been obtained with pigeons (9). Trained under conditions analogous to those which have yielded matching in mouthbreeders and pigeons—that is, with guidance—rats (7) and monkeys (10) "maximize." We seem to be dealing here with a phylogenetic difference. The difference may lie in the way in which the various species are affected by guidance, or other factors may be responsible; but it would be unwise, in the light of the results in submammals, to discount previous failures to demonstrate matching in rats and monkeys on the basis of a priori criticisms of procedures which involve forcing or correction.

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References and Notes

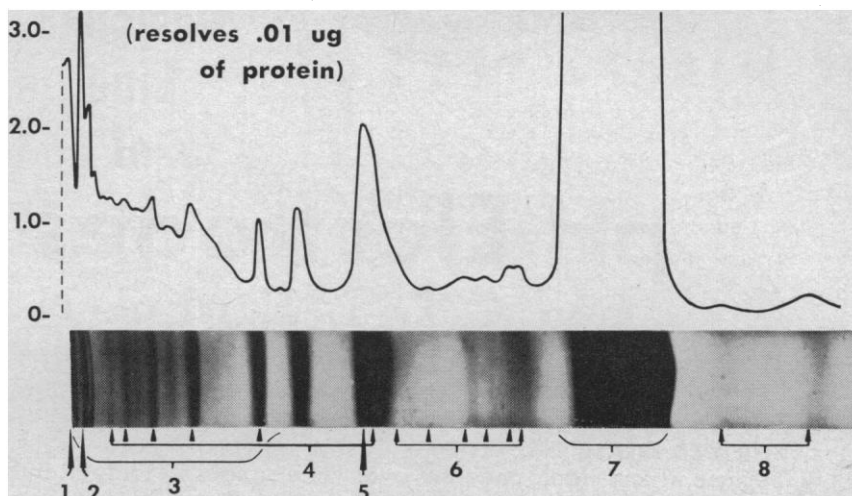
1. S. H. Revusky, *Science* 134, 328 (1961).
2. Although the term *matching* nowhere appears in Revusky's report, the implication that it has been demonstrated is clear, and he asserts in personal correspondence that "the rats were matching in the usual sense."
3. E. Brunswik, *J. Exptl. Psychol.* 25, 175 (1939).
4. W. K. Estes, in *Psychology: A Study of a Science*, S. Koch, Ed. (McGraw-Hill, New York, 1959), vol. 1, pp. 410 ff.
5. We are indebted to Revusky for lending us his records.
6. S. Siegel, *Nonparametric Statistics* (McGraw-Hill, New York, 1956), pp. 52-58.
7. M. E. Bitterman, J. Wodinsky, D. K. Candland, *Am. J. Psychol.* 71, 94 (1958).
8. E. R. Behrend and M. E. Bitterman, *ibid.* 74, 542 (1961).
9. D. H. Bullock and M. E. Bitterman, *ibid.*, in press.
10. W. A. Wilson, Jr., *J. Exptl. Psychol.* 59, 207 (1960).

From Behrend and Bitterman's criticisms of my report (1), the reader may suppose that I claimed that the forced trial and correction procedures are "inadequate to produce matching" and that the nonreinforced trial procedure does not share these inadequacies. In fact, not a word of my report was concerned with whether earlier procedures produced matching or are capable of producing it. My report stated that "much probability learning experimentation has been devoted to the development" of certain theories, and that conventional probability learning procedures "involve the introduction of factors not considered in these theories," which factors should affect the experimental results. Furthermore, the report concluded on the basis of the experimental results that "the present experiment, like previous probability learning experiments with animals, cannot decisively confirm or reject theories about the effect of reinforcement on response probability" (1). Thus, what seems to me to be the principal criticism of my report is based on a misconception of it.

Behrend and Bitterman cite two descriptions of Brunswik's procedure as evidence that probability learning implies a random schedule of reinforcement. These descriptions were not meant as definitions, and I know of no authoritative definition of probability learning. Since the nonreinforced trial procedure was labeled "a new procedure" (1), it should not have been expected to be identical with older procedures. My report describes the characteristics of the nonreinforced trial procedure which make it a probability learning procedure; the lack of random reinforcement is not of great importance because neither Estes (2, p. 612), Spence (3), nor Brunswik (4, p. 258)

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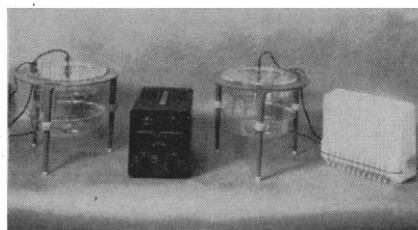


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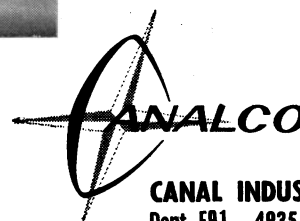
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require that reinforcement be random for their theories to hold (5). Moreover, my definition of probability learning was explicit, and a parenthetical caution was inserted in the first sentence of my report so that my definition would not be confused with other possible definitions.

Behrend and Bitterman's demonstration (6) that the sequence of responses was nonrandom is unnecessary, because my use of the statistics *Prr*, *Prrn*, and *Prrnn* (1) demonstrated the same thing; had it been random, these three statistics should not have been significantly different. The nonrandomness of the reinforcement schedule I used may well have contributed to deviations from randomness in responding (although this has not been proved), but the relevance of such deviations is not clear. Deviations from randomness do not prove that the rats were not matching in the usual sense of the term. The mathematical learning theories which supply the contexts in which that term is usually used predict matching only for *mean* response probability; they also predict that the sequence of responses at asymptote will *not* be random. Furthermore, in no published demonstration of matching that I know of has it been demonstrated that the sequence of responses is random, so that if randomness is to be considered part of its definition, matching has not yet been demonstrated (7). Finally, if any reader held the belief that matching presupposes randomness, he should not have been misled by my report, which did not use the term *matching* and did not indicate that a random sequence of responses was obtained (8).

S. H. REVUSKY

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References and Notes

1. S. H. Revusky, *Science* 134, 328 (1961).
2. W. K. Estes, *Am. Psychologist* 12, 609 (1957).
3. K. Spence, *Behavior Theory and Conditioning* (Yale Univ. Press, New Haven, 1956).
4. E. Brunswik, *Psychol. Rev.* 50, 255 (1943).
5. As for alternation experiments of the type mentioned by Behrend and Bitterman, their purpose is to show how the outcome of one trial can be a discriminative stimulus for the following trial. This has not been the usual purpose of probability learning experiments, and I eliminated such stimulation in my experiment by spacing the trials a day apart.
6. Among my 19 rats, there was one instance more extreme than that cited by Behrend and Bitterman at the end of their paragraph 2.
7. If randomness is part of the definition of matching, the binomial theorem can be used to assess whether or not matching occurred, with the outcome of each trial used as a statistically independent item. By this criterion, most of the demonstrations of matching cited in paragraph 4 of the note by Behrend and Bitterman are invalid. By con-

ventional standards, these demonstrations are acceptable (except for the second Bitterman, Wodinsky, and Candland experiment, in which conventional use of the *t*-distribution reveals a probability significantly higher than matching), but if Behrend and Bitterman wish to establish new standards, they should adhere to them.

8. I will take this opportunity to correct an error of transcription which appeared in my report. The Kendall *W* had 2 degrees of freedom, not 18.

Tobacco and Health

The report on the formation of the Tobacco Advisory Committee [*Science* 136, 972 (1962)] raises some questions concerning the role of this committee.

As stated by the Surgeon General, the mission of the committee is to "make whatever recommendations may be appropriate" regarding the tobacco-health problem. The practical effect of such vague and general instructions may be to insure a long period of delay before the committee can tackle its main job. The unofficial goal, as reported in *Science*, is "to move the government off center on the tobacco issue without delivering too severe a jolt to the tobacco industry." While this implies an interest in protecting the American public against the health hazards of tobacco, it suggests as great or even greater concern for the welfare of the tobacco industry.

What useful purpose can be served by another committee to "study" the tobacco and health issue? The subject has already been studied by at least ten official and voluntary research and health agencies. Studies have been made in the United States, Canada, Great Britain, and the Netherlands, and by the World Health Organization. In 1959 the U.S. Public Health Service reviewed the matter. All these studies came to similar conclusions: tobacco (particularly cigarettes) constitutes a serious health hazard for its users. In addition to its role in lung cancer, tobacco plays a role in cardiovascular and other diseases. It is doubtful if a Tobacco Advisory Committee review could add much to the excellent summaries already available—particularly the most recent one by the Royal College of Physicians of London [*Smoking and Health* (Pitman, New York, 1962)].

Since the evidence concerning smoking as a health hazard has been assembled, summarized, and presented so often in the past, there is little excuse for a long delay in answering the question: Is there sufficient health hazard from smoking to justify doing some-

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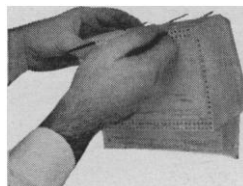
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