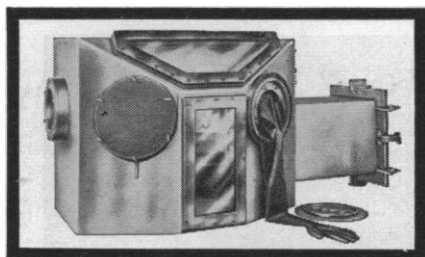


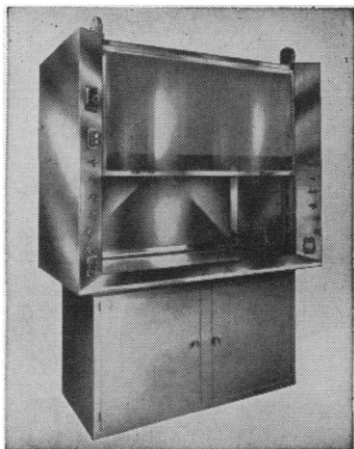
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Letters

General Semantics

There is unquestionably a need for such a book as Martin Gardner's *Fads and Fallacies in the Name of Science* [reviewed by John Pfeiffer in *Science* (126, 1296 [1957]). But there is always a danger that an overzealous debunker will throw out the baby with the bath water. And while certain "fads and fallacies" are obviously so, our debunker, who, after all, cannot be an informed specialist in every field of science, might easily be guilty of some "fallacies" himself. Such seems to be the case with Gardner, whose lumping of Alfred Korzybski and *General Semantics* [Nonaristotelian Systems and General Semantics] with the inanities of Velikovsky, Hubbard, and Reich is comparable to lumping the latter three with B. F. Skinner or Carl Rogers or Julian Huxley.

The scientific "respectability" of Korzybski's *General Semantics* is no longer seriously questioned (except by Gardner), for as a school of thought in the behavioral sciences it has actually gained at least as much "respectability" as Freud's psychoanalysis, if not more, and has blossomed into a significant International Society for General Semantics. Courses in general semantics are currently being offered by the University of Chicago and other universities of considerable "respectability," and the literature of general semantics features such prominent names as those of S. I. Hayakawa, Wendell Johnson, and Anatol Rapaport. Further, the large number of psychiatrists, psychologists, and other scientists who would disagree with Gardner's misclassification of general semantics as a "fad" or "fallacy" would seem to indicate that Gardner's ambitious attempt to draw a precise line between science and pseudoscience is neither 100 percent trustworthy nor 100 percent successful. Actually, any attempt to set up an "infallible authority" to distinguish between science and nonsense is bound to fail. Both Rome and Moscow have tried this and made themselves look rather silly.

EDD DOERR

Bogotá, Colombia

I agree with most of Edd Doerr's statements, having made the same points in my book. In both the "Introduction" and the final chapter, I discuss at some length the spectrum of science and pseudoscience and the extreme difficulty of drawing a precise line between them. I approached Count Korzybski's work by specifically pointing out that he was not to be classed with most of the men mentioned in the book (I made a similar distinction in respect to J. B. Rhine), and I emphasized the fact that I was attack-

ing only the doctrines which were original with the Count.

Many people do not realize that "semantics" is an old and highly respected branch of meaning analysis and that the Count simply drew heavily from this tradition and popularized it. College courses in semantics, even those taught by the Count's reputed followers, are little more today than elementary excursions into this field. Where Korzybski simply passed on a semantic commonplace, naturally no one disagrees, but in respect to those doctrines which are peculiar to the Count, one would be hard put to find a single philosopher of science today who did not agree with the acid opinion of Ernest Nagel, quoted in my book.

MARTIN GARDNER

Dobbs Ferry, New York

"Paramecium Controversy"

A recent addition to what is apparently developing into "the *Paramecium* controversy" was contributed by Kellogg (1), who presents a view of the main reason for disagreement between Gelber (2) and Jensen (3) that is rather surprising. According to Kellogg, Gelber hypothesizes that learning is characteristic of all living tissue, while Jensen restricts learning to the higher levels on the phyletic scale. These extreme views are not clearly expressed by either Gelber or Jensen in the articles cited by Kellogg.

Kellogg cites other studies of apparent learning in paramecia and, in referring to one report, states that French (4) "gives seemingly unequivocal evidence of trial-and-error learning in paramecia." "Unequivocal evidence" is a slippery quantity and, as Kellogg points out, there have been reports of negative findings also. Gelber, in an early publication (5), referred to many of the articles that Kellogg accuses both her and Jensen of neglecting to use as evidence.

In a very recent article Katz and Deterline (6) report the results of a replication of Gelber's basic study (5). The experimental design included several control groups set up for the purpose of testing the conflicting views of Gelber and Jensen. Jensen attributes the observed change in the behavior of paramecia in the Gelber studies to the presence of food, apart from any inference of learning. Katz and I came to the conclusion that Jensen's more conservative explanation is more credible than Gelber's. This does not mean that we deny that paramecia have the ability to learn. That phenomenon remains a fascinating possibility. We do insist that Gelber's technique does not isolate the phenomenon in an unequivocal fashion.

Apparently perception is an even bigger problem in psychology than is usually realized. Katz and I observed the be-

havior of our paramecia in replications of studies reported by Day and Bentley (7) and Smith (8) as well as of the study by Gelber. The paramecia behaved in some cases exactly as described by those authors, but our perceptual interpretations of the behavior we observed did not permit us to state that we saw paramecia behaving in a manner that indicated that a learned modification of behavior had occurred. Gelber perceives the same changes as evidence of learning. Now we find that we do not even view the Gelber-Jensen controversy in the same way that Kellogg does. Until a more satisfactory method is devised, the question of whether or not paramecia can learn will remain a perceptual one and, as such, will not receive a scientific answer.

WILLIAM A. DETERLINE
Department of Psychology,
Alma College, Alma, Michigan

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8. S. Smith, *J. Comp. Neurol.* 18, 499 (1908).

There is little I can say (1) in reply to William A. Deterline except that I find no cause for basic disagreement either with the tone of his letter or with the research report of Katz and Deterline (2).

My only regret is that the meaning of my original comments appears not to have been quite clear. I tried to point out that centuries before there was any research whatever on the learning or nonlearning of paramecia, philosophers had debated the question of whether low organisms can learn and "have minds."

Today, when we do have research data to support our speculations, there still seems to exist the same sort of controversy. For now the experimental results are open to two interpretations: (i) that they demonstrate learning in paramecia and (ii) that they do not demonstrate learning. If expressed in terms of hypotheses, the position taken by Gelber (3) is the positive one—namely, that paramecia and probably other low organisms can learn. The hypothesis which appears to describe Jensen's position (4) is that lower organisms move mechanically and are incapable of learning. These two positions are not unrelated to the vitalistic-mechanistic controversy, with roots far in the past. Thus Descartes held that all animals—high as well as low—behave like machines and that man alone possesses higher "mental" abilities.

I seem to have detected in some recent reports that have been written on the

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subject a kind of emotional or anthropocentric intolerance which looks with scorn upon the possibility of learning in lower organisms—as if the very thought of it were beneath the level of scientific thinking. Happily, there is no evidence of such a bias in the article by Katz and Deterline (2).

Those who maintain that learning in paramecia or in other Protozoa has *not* been demonstrated should acquaint themselves with the literature on the subject and should not close the door upon such a possibility. Some of this literature was cited in my original note (1). Those who adopt the more liberal attitude and hold that the available data *do* demonstrate learning should still realize that there is room for a negative position. Let not emotional bias for either view disturb a fair and intelligent appraisal of the evidence.

We need much more evidence before either position can be swept aside.

W. N. KELLOGG

Department of Psychology,
Florida State University, Tallahassee

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Equipment

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Science does not assume responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 166.

■ **MOISTURE METER** is a portable, transistorized instrument powered by mercury batteries. The instrument measures d-c resistance of woods to determine moisture content from 6 to 70 percent in three ranges. Normal battery life in the instrument is 2 yr; approximately 100,000 tests may be made before replacement becomes necessary. (Moisture Register Co., Dept. 151)

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