Reply to Sutherland and Andelman

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Sutherland & Andelman (1967) summarized a portion of a study reported by Babb (1957) and criticized it on the bases that the numerical differences between groups were small and the results could be explained by stimulus generalization decrement as well as by their own particular theoretical construction. Lovejoy (1968, p. 43), to whom Sutherland and Andelman gave credit for valuable suggestions (1967, p. 108), indicated that Babb's experiment (which he incorrectly referenced as having been published in 1956) had been repeated by Sutherland and Andelman with better controls. It is the present contention that the criticisms posed by Sutherland and Andelman were incorrect, that their own experiments did not constitute repetitions of Babb's experiment, and that they failed entirely in their efforts to demonstrate that stimulus generalization decrement is an inadequate explanation of Babb's results.

In the experiment that Sutherland and Andelman criticized, one group of rats was run on a compound of two relevant cues and two other groups were run on one or the other cue presented singly. Each of the three groups were later split into two subgroups, one run to criterion on one cue and the other run to criterion on the other cue. One cue was defined as more discriminable than the other on the basis that it was learned faster. It was assumed that the more discriminable cue constituted a greater proportion of the original stimulus compound than did the less discriminable cue. Accordingly, transfer to the separate components would involve more stimulus generalization decrement for the less discriminable cue. Results confirmed the predictions. The presence of the less discriminable cue had no significant effect on learning to the more discriminable cue, but the presence of the more discriminable cue reduced the amount of learning accruing to the less discriminable cue.

Sutherland and Andelman ignored the discriminability variable in Babb's experiment, the theoretical rationale for predictions, and that portion of the results which could not be readily handled within their own theoretical framework. They criticized the experiment on the basis that results could be explained by stimulus generalization decrement—even though that was the variable used to predict the experimental results in the first place. The fact that the same results might also be explained by the action of Sutherland and Andelman's "hierarchical tree" of "analyzers" cannot effectively constitute a criticism of the design of the experiment since the "analyzers" are not defined and their responsiveness, if any, to experimental variables is still relatively unknown. Moreover, since there is agreement that Babb's results can be explained on a basis of stimulus generalization decrement, an explanation based on a hypothetical internal hierarchical tree of analyzers would seem to be substantially in conflict with the Law of Parsimony. Finally, Babb's theoretical rationale adequately handles all of his experimental results, not just one portion of them.

The claim that two of Babb's groups differed in mean "errors to criterion" by scores of 1.2 vs 2.6, and that the difference "was in any case very slight," is simply inaccurate. The actual scores were 1.54 and 4.46, a mean difference between groups of 2.92 errors. That value should be compared to the mean difference of 3.15 errors between the groups that were run to each cue separately, since that difference constituted the basis for the predictor variable. The fact that both differences were as small as they were, significant at $<.01$, attests to the power of the predictor variable and the efficacy of the cues and apparatus that were employed. For example, in contrast to Babb's 30 training trials, Sutherland and Andelman report having to use 70 and 110 training trials in their two efforts to "control" for the stimulus generalization variable.

In regard to Sutherland and Andelman's "repetitions" of Babb's experiment, there was a substantial number of highly important differences between them. Babb ran his experiment with a Grice-type apparatus and a noncorrection method, while Sutherland and Andelman ran their experiments with a Lashley jumping stand and a correction method. In addition, Sutherland and Andelman ran their experimental group to two relevant cues with the relative discriminability of the component cues unstated, despite Lovejoy's stress (ibid., pp. 38-46) on the importance of the relative "distinctiveness" of cues in a two-cue compound. Controls and tests were run on only one of the relevant cues in the compound, with no logic given for the selection of the particular cue that was used. In addition, control groups not only encountered a relevant cue, they also encountered the second cue, which was presented as simultaneously or successively irrelevant. This manipulation was supposed to control for stimulus generalization decrement, but the operation of that variable is dependent on reinforcement contingencies under both acquisition and test conditions, and that particular fact was not taken into account. As a consequence, neither of the experiments effectively controlled for stimulus generalization decrement.

In both of their experiments, Sutherland and Andelman expected control groups to learn more about the relevant cue than the experimental group, despite the prevailing notion (Lovejoy, 1968, pp. 44 and 69) that adding an irrelevant cue reduces the amount of learning that occurs to the relevant cue. At any rate, their predictions were not confirmed. In their second experiment, a major change in test procedures was introduced that produced a lack of control which they admitted could not be avoided. Since they could not obtain an intermediate value for the cue which had been presented as irrelevant for the control groups, and as the second relevant cue for the experimental group, that cue was allowed to remain on test trials as a successively irrelevant cue. Consequently, the experimental group, on every test trial, encountered an opposition of previously relevant cues for one of the two choices available. For both control groups, neither choice, separately, involved any such new confrontation. Obviously, then, since changes between acquisition and test trials were greatest for the experimental group, its poorer performance is susceptible to an interpretation by response conflict and stimulus generalization decrement. The latter variable, of course, was the one which both of Sutherland and Andelman's experiments were purportedly designed to control against.

In summary, then, Sutherland and Andelman's criticisms of Babb's experiment were incorrect, and their own experiments did not constitute repetitions of his experiment. By their own admission, their first experiment did not confirm their predictions and their second experiment was confounded. Finally, the actual design of their second experiment led to results that are susceptible to interpretation by stimulus generalization decrement.

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