Comment

Wagman & Allen recently reported results which they interpreted as demonstrating a "... conditioned positive reinforcer based upon the termination of shock" (Psychon. Sci., 1964, 1, 363-364). In view of the theoretical significance of such a conclusion (e.g., Mowrer, 1960), and in view of the difficulty other investigators have reported in attempting to substantiate it (e.g., Beck, 1961), it is important that any such claim be carefully documented. In my opinion, the paper in question suffers from several shortcomings.

First, the index of response strength was a "ratio score," consisting of the number of responses made in the post-test session divided by the number made in the pre-test session. Such an index makes it impossible to determine whether differences were due to pre-test or to post-test scores, or to combinations of both. Presumably the various groups were roughly equal on pre-test scores (indeed, one wonders why they were not matched on these scores, since the independent variables were not introduced until later), but if they were not, such matters as regression to the mean and ceiling effects must be considered before a conclusion concerning treatment effects is justifiable. No such considerations were evident.

Second, the results are summarized by stating "The response ratios rose to a maximum value at 12 pairings and declined rapidly at the higher numbers of shock-tone pairings," implying an orderly functional relationship. I fail to see such order in the accompanying figure... it appears that all the experimental groups performed at the same level save one. It would appear that an over-all test of the difference in means should have preceded the tests between selected pairs of means. No such test is reported.

Third, the "control" groups do not isolate the presumed independent variable: pairing or non-pairing of the tone with shock reduction. Two of the control groups received shock but no tone prior to testing. Differences between these groups and experimental groups could thus be due to the lack of prior experience with the tone, quite independent of whether it had been paired with shock reduction or not. Moreover, the largest ratio in the direction predicted by the hypothesis was not by an experimental group at all, but by a third "control" group which had experienced neither shock nor tone prior to testing. Nevertheless the authors mysteriously conclude that the hypothesis was supported.

Finally, ignoring these shortcomings, the sample size (5) does not contribute much to one's confidence in the results. This consideration is of more than the usual importance when it is recognized that just one experimental group (out of 6) furnished the entire "support" for the published conclusion.

(See page 260 for reply by W. Wagman and J. D. Allen)

Final note: Wagman and Allen's reply clears up the question of ratio scores but leaves the other three points unchanged. More specifically, (1) it was argued that an overall test of mean differences was required. The subsequent F-test with 6 & 28 df leaves me as much in the dark as ever: there were 6 experimental groups and 3 "control" groups. Why seven means evaluated? The reported df can hardly be a typographical error, since I already pointed out an error in a previous F-test by the same authors over the same point (personal communication), where only one df for the numerator was reported. The correct df is neither 6 nor 1, but more likely 5 or 8, depending upon whether experimental means or all means are compared. (2) The authors apparently missed my point concerning appropriate control groups, since their replay states, "...we feel that the reinforcing effects of a postshock stimulus can only be determined by comparing animals who have been exposed to shock." With this I heartily agree: the original criticism was that such Ss must also be exposed to the unpaired tone, a procedure which was not followed. (3) The Ns, of course, remain small.

Finally, a re-examination of the original paper suggests that data more crucial to the hypothesis were collected but apparently overlooked. Reference is made to the fact that responses in the test phase were recorded for each 10-min. period. The hypothesis clearly predicts a divergence over test trials between experimental and "control" groups, since higher rates for experimental groups at the beginning of the test phase could hardly be attributed to the subsequent action of the tone. No reference is made to this aspect of the data, suggesting these trends were not examined.

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References

BECK, R. C. On secondary reinforcement and shock termination. Psychol. Bull., 1961, 58, 28-45.

MOWRER, O. H. Learning theory and behavior. New York: Wiley, 1960.

Note

1. This comment was written while the author was a Joseph P. Kennedy Visiting Professor at Peabody College, on leave from the University of Southern California.