Traupmann and Wong: A reply

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question. While an analysis of variance with repeated measures over trials would seem most appropriate, *t* tests were performed. Figure 2 of McCain's paper seems to reflect a Reward by Days interaction resulting from a slower rate of extinction for the large-reward group. Such an interaction might be borne out by the appropriate statistical analysis.

McCain did recognize that "during acquisition, magnitude differences disappear under *some* conditions" (*italics* ours). He further recognized that "a number of possible confounding variables such as drive level, strain of animals, sampling, weight of Ss, particular reward magnitudes, pretraining, and performance within days are obvious. Any or all of these could explain the results." He, nevertheless, drew the sweeping conclusion that "after about 60 consistently reinforced acquisition trials in a straight alley, the acquisition effects of different reward magnitudes are either minimal or absent," and went on to challenge "most learning theories."

It seems to us that the results of both experiments could be interpreted entirely in terms of drive level, sex differences, weight of Ss, and performance within days. In this light, McCain's results contribute little to our knowledge that "under some conditions" the long-standing findings about reward magnitude during acquisition do not hold. However, if the effects of reward magnitude do, indeed, disappear with extended training while the factors we have mentioned are adequately controlled, then we certainly should be forced to revise some of our theoretical notions.

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It is always gratifying to know that there are those out there who read, even disappointingly, your written efforts.

In their first paragraph, Traupmann and Wong quote me, "... are not sufficient to convince anyone that the effects of reward magnitude are sharply decreased or disappear after extended training." So far, so good. It would have been even better if they had also quoted the first part of that sentence, "Taken alone, these two studies..." and the next three sentences, "A series of *nine other* [*italics* added] consistent reward studies has been run in our laboratory. Six of these studies have been put into one paper that is now under editorial consideration. In addition, Black (*in press*) and Bloom & Milstead (1969) have substantial evidence that, during acquisition, magnitude differences disappear under some conditions." Black presented eight studies (now in print, 1970) and Bloom and Milstead two studies. In brief, I had considered a substantial number of failures to find asymptotic differences related to magnitude before rushing into print with two short studies.

There are other quotations that might have been extended. Perhaps those interested in this discussion would be well advised to read the original article (McCain, 1970).

The present reply does not represent an attempt to answer the detailed criticisms of Traupmann and Wong. I believe these will be best answered by presentation of the entire series of studies. In the original study, I listed seven possible confounding variables, "... such as, drive level, strain of animals, sampling, weight of Ss, particular reward magnitudes, pretraining, and performance within days, are obvious." We might note that these constitute virtually an outline of Traupmann and Wong. As stated in the article, I am hopeful that presentation of all of our data will resolve some of these problems.

Perhaps I was remiss in failing to list other problems or points that need discussion in these two studies and in other magnitude studies. For example, there is an obvious need for a sequential analysis or control of the sequential probabilities in the partial-reinforcement study (Experiment 2). Again, there is no

Fig. 1.

mention of odor control, although Ludvigson and his colleagues (e.g., Morrison & Ludvigson, 1970) have clearly demonstrated its importance. The list could be even longer.

Not only do we have to worry about the "possible confounding variables" mentioned in the original study, but also we have to recognize that *all* magnitude studies to date are potentially confounded. Deese & Hulse (1967, p. 43-44) consider these problems at greater length than is possible here. One example of a confounding variable is the length of time the S is exposed to the goalbox stimuli.

There are additional studies, not cited in the original article, in which asymptotic differences related to magnitude failed to appear. Meyers (1968) concluded that "...reward magnitude is essentially a growth rate parameter but does not determine the eventual asymptote of the acquisition function." Spear and his colleagues at Rutgers (Vogel, Mikulka, & Spear, 1966; Spear & Pavlik, 1966; Spear & Spitzner, 1966, 1968, 1969a, b) have some evidence that seems worth considering. Spear (personal communication) comments, "I have enclosed some reprints including examples in which no differences in running speeds occurred in late acquisition in spite of large reinforcer differences (number of food pellets). Most of our other published papers, especially with the T-maze, have similar examples." I do not imply that Spear accepts the general statement that there are no asymptotic differences due to reinforcer magnitude. That is not the case. McHose and his colleagues at Southern Illinois (personal communication) have a series of unpublished studies in which they found no asymptotic differences related to magnitude. There are a number of other such instances in earlier literature (e.g., Gonzales, Gleitman, & Bitterman, 1962; Snyder & Hulse, 1961; Miller & Estes, 1961).

I am not attempting to make the case for the lack of asymptotic differences by citation. The point is that a substantial number of

experienced Es in a large number of studies do not get evidence for asymptotic differences. Whatever the shortcomings of the two original studies, it seems unlikely that all those involved have made the same or overlapping errors.

Traupmann and Wong do raise a specific question regarding the use of reciprocal transformations. This question is not considered directly in the monograph and might be worth some comment. I have reanalyzed Experiment 2 (partial reinforcement) using the suggested transformation. The differences are still not significant. As noted in the original article, I viewed Experiment 2 with extreme caution, since it was a single study. I place more weight on consistent reinforcement studies, since we do have much more evidence. As may be seen in Fig. 1, from the original study, the overlap between the consistent reinforcement groups seems completely in line with the analysis ($F = 1.01$). In addition, in the six studies mentioned earlier, the small-reward group sometimes ran faster than did the large-reward group. At times one group was faster on one measure and slower on another, and at times differences were within a rounding error. How these sorts of results could be converted to significant differences by transformation of the scores escapes me at the moment.

Studies in which asymptotic differences are found (e.g., Spear & Pavlik, 1966, or Wike & Chen, 1970) cannot be ignored. The fact that Es have gotten asymptotic differences in a substantial number of studies, while others have failed repeatedly, should warn us that our understanding of magnitude phenomena is, at best, very limited.

We have not resolved this issue. Many questions remain. Viewing all the evidence together with Traupmann and Wong's comment, I remain convinced that under some conditions differences due to reward magnitude disappear at asymptote.

Perhaps in desperation we should go back, or just go, to the laboratory.

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