Reward magnitude and instrumental responses: A comment

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McCain (1970) has recently presented two studies which, he says, "...are not sufficient to convince anyone that the effects of reward magnitude are sharply decreased or disappear after extended training [p. 140]." Yet, he concludes that "...the results ... are contrary to usual assumptions about the effects of reward magnitude... [p. 140]." We believe that these studies *do not* present evidence contrary to any assumptions regarding reward magnitude and offer the following considerations in support of our view.

In Experiment 1 McCain presents data which show that asymptotic run times are not affected by large differences in reward magnitude (45 vs 500 mg). On superficial analysis these results may be taken as contrary to a large body of evidence which shows that asymptotic performance is directly related to reward magnitude (Pobols, 1960). However, closer scrutiny reveals that a combination of several procedural factors renders McCain's results entirely compatible with previous research (e.g., Seward et al, 1958). McCain's Ss were fed a 15-g daily diet and given six trials per day, with a 6-min ITI. Furthermore, half of his Ss were females. With his deprivation schedule, it is likely that the 500-mg-reward female Ss were near satiation. It has been our experience with rats of both the Wistar and Holtzman strains that some females, especially those between 60 and 90 days of age, fail to consume 15 g of lab chow per day. We have also found that on a 12-g deprivation schedule young female rats frequently require several baited goalbox placements before they will eat in the apparatus. That they do eventually eat after pretraining provides no assurance that they will be consistent performers during training. Other investigators (e.g., Hill & Wallace, 1967) who have used relatively low deprivation regimens have reported that some females have to be excluded from the analysis because of failure to run. While attention has been focused on the female Ss of McCain's study, it should be recognized that the drive levels of males might also be reduced, given six 500-mg pellets in 36 min.

The likelihood of satiation factors suggests the possibility of a within-days interaction for run time and reward magnitude, such that initial trials would find 500-mg superiority followed by 500-mg inferiority. McCain did not present these data, though it seems that when using his procedures the trials-within-days analyses would be of utmost relevance.

Experiment 2 also presents problems. In the first place, the 45-mg group, given 42 50% rewarded training trials, appears to show no acquisition. That is, running times for the 12 trials on Days 1-2 are about the same as on Day 7, suggesting that, at best, some of the Ss never ran. Second, in considering the 138-trial groups, it was reported that terminal acquisition and

extinction response times failed to differ between the 45- and 500-mg conditions. As we see it, the problem with these data is one of measurement and scaling. In previous work of this kind, run-time data have typically been converted to speeds by reciprocal transformation. This procedure normalizes otherwise positively skewed data and tends to reduce the variance estimate. Thus, while asymptotic run times may yield small and nonreliable differences, a conversion to reciprocal time (speed) could provide highly significant results.

While we understand the statistical shortcomings of taking the reciprocal of means rather than the mean of the reciprocals, we have nevertheless replotted McCain's data, using reciprocal transformation (Fig. 1). The curves appear to indicate differences in favor of the 500-mg group, both for terminal acquisition and extinction. Furthermore, the differences are in the direction that one would expect both from theory (e.g., Amsel, 1967) and from previous investigations (e.g., Wagner, 1961).

Third, the statistical analyses performed on the extinction data from Experiment 2 are also subject to

Fig. 1. Reciprocal mean run times during acquisition and extinction, a replot of McCain's 1970, Experiment II data.

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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.