tus and before the moment of being placed on the platform, it is not possible to know what his data represent at all! The key stimulus situation is obviously the one that exists for the animal on the platform, not the complex of stimuli associated with being handled, lifted out of a cage, etc. In most laboratories this kind of stimulation has been paired with many other random events and would be difficult to establish as a CS for a response that was to come in an apparatus some time later. If this kind of handling en route to an experimental apparatus were likely to become a CS for the specific responses that are to be elicited in the apparatus, the conduct of conditioning experiments would be very difficult indeed!

His data regarding the avoidance responses are, on the contrary, pertinent to our hypothesis, dealing as they do with behavior that is under the stimulus control of features of the experimental apparatus. Here we see that the effects of ECS appear to include an absence of the tense crouching that is produced by footshock. Just as we predict, fear motivated crouching has not developed although we cannot tell whether the ECS rats are less aroused than control rats for there are no data to provide us with a base rate for stepping off or staying

on the platform by unshocked, unpunished animals.

Under the circumstances, therefore, we must point out that the hypothesis that we have advanced is not tested by this study, the results of which are hardly "crucial" in determining its fate. Coming as they do, from an ECS study that uses multiple treatments, they are, of course, irrelevant to consolidation theory also.

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Notes

1. This paper was prepared with the support of Grant No. MH 07129 of the National Institutes of Health.
2. Because of this, we cannot cite it in the references either.

Reply to Maher and Lewis by Lauren K. Gerbrandt

In publishing my paper in this journal, I intended to show that the Lewis and Maher hypothesis (1965) is based on the assumption that changes in the CER are a necessary condition for changes in the occurrence of avoidance, and that evidence to date does not bear out such an assumption. I pointed out that although some studies have shown correlations between freezing, response suppression, urination-defecation and other indicants of the CER and avoidance performance, Kamin, Brimer, & Black (1963) have clearly shown that suppression effects of the CER are routinely *lessened* while animals were becoming maximal in avoidance performance, whereas declining avoidance performance during extinction was not accompanied by decreases in suppression effects. Since changes in the CER do not seem to be a necessary condition for changes in the occurrence of avoidance, their presence or lack as affected by ECS was not deemed theoretically relevant to changes in avoidance. I simply stressed the possibility that my data were just a small part of the vast literature accruing (Thompson, Lesse, & Rich, 1963; Kamin, Brimer, & Black, 1963; Mikulus & Isaacson, 1965) which points to the same conclusion. Since Thompson, Lesse, & Rich (1963) found conditioned emotional responses such as squealing and abrupt changes in respiratory rate independent of the conventional freezing indicator used by Maher and Lewis, it was tentatively concluded that a necessary relation also does not exist between activity and the CER and the other CER effects. Since I had found a linear relationship between urination-defecation and avoidance

performance in the footshock group of a previously published study (Gerbrandt & Thomson, 1964) it was decided to replicate this portion of the study in comparison with ECS punishment. The group of animals given ECS as punishment again showed very little avoidance of ECS, although their emotionality was easily chained back by conditioning (even after a single ECS) to the cues of picking up the animal from his home cage for daily trials. Contrary to the subjective evidence of Williams (1961), while in the apparatus these subjects would often squeal and tremor. The apparatus was particularly bothersome to clean after a day's "run" with these animals. It is surprising, even though theoretically irrelevant, that Maher and Lewis question the emotionality of these animals. Because of limitations in article length in this journal, it was withheld that 75% of these rats developed severe stomach ulceration (4 or more lesions larger than 2 mm) as a result of ECS administration. Perhaps Maher & Lewis should clarify these interesting ramifications of "relaxation"! The ad hoc referral to "absence of tense crouching" in the performance of these animals might better be termed "an absence in memory of which response was punished." Deficits in recall due to ECS have been reported as well in humans (Williams, 1950). Humans have also shown evidence of *increases* in emotionality due to threat of impending ECS treatments (Kast & Zweibel, 1954). These CER effects were greatest when patients were in the place of ECS delivery.

If we are to consider viable the Lewis and Maher

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Fig. 1. Mean percentage of avoidance responses plotted in blocks of 10-trials for both the discontinuous shock group ($N = 17$) and the continuous shock group ($N = 16$).

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hypothesis of ECS effects, at least four points should be clarified. First, how do muscular or emotional tensions or their lack produce a competition with performance which simulates the existing interpretation of an inability of ECS treated patients to recall elements of word lists? The supposedly "relaxed" subjects were able to gain sufficient strength to sincerely say, "I don't remember." Second, how is it that extreme emotionality is conditioned so easily to cues outside the apparatus but not to the apparatus itself? Should we postulate yet another gradient of conditioning for these extra-apparatus events? Third, since the statement is made that an ECS study using multiple treatments is not relevant to consolidation theory, further investigation of this moot point is badly needed. Hudspeth, McGaugh, & Thompson (1964) have found results interpretable as amnesic after several ECS treatments were given to the animals. Lastly, when, if ever, are changes in the CER in any species of animal causally related to avoidance performance?

If these very fundamental questions are answered experimentally, I would acknowledge the existence of a new interpretation of ECS effects (Lewis & Maher, 1965). Until then, the reader might be interested in another hypothesis (Hudspeth & Gerbrandt, 1965) consistent with the effects observed in human patients, with phenomenal emotionality in extra-apparatus

seen. Observation of the Ss seems to indicate that the short bursts of shock produced less freezing and fewer perseverative scrambling and bar-biting responses than did the uninterrupted shock. The discontinuous shock did, however, seem to facilitate locomotor responses. Thus, discontinuous shock may facilitate avoidance learning because it moves the required locomotor response up on the Ss response hierarchy and increases the probability that the response will occur and be rewarded. If this is the case, it might be expected that discontinuous shock would be a less effective US in conditioned emotional response (CER) training and in passive avoidance learning when the measured response is one of inactivity.

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situation, with dissociations between CER effects and avoidance performance, and with neuroanatomical functions related to ECS effects.

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