Comments on pseudomediation¹

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Abstract

Mandler and Earhard argue that supposed mediational effects may be attributed to differential interference in experimental and control paradigms employed. They present an experiment to support their contention. However, a crucial assumption is unsupported; the experiment is weakened by failure to include a mediational group; their data are subject to other interpretations; and their argument cannot be extended in any simple way to include other successful mediation paradigms.

Pseudomediation Argument

A recent paper by Mandler & Earhard (1964) argued that the presumed mediational effects shown in studies of verbal learning may not be due to mediation but rather may be attributed to a lessening of interference in the usual mediational paradigm (A-B, B-C, A-C) as opposed to the usual control paradigm (A-B, D-C, A-C). As evidence the writers presented a study in which the third-stage learning was found to be superior in a pseudomediation paradigm (A-B, B-C, A-E) when compared with a control paradigm (A-B, D-C, A-E).

Their attack on mediation and their explanation of the experimental effect stressed the role of bidirectional associations, unlearning and interference. The writers asserted, further, "The general reasoning applies similarly to the other mediation paradigms discussed by Horton & Kjeldergaard (1961) and Jenkins (1963)." However, the writers do not claim that the analysis applies to studies apparently involving mediational processes inferred from language habits, holding that these represent "conceptual rules" and are responsive to different analyses.

Their argument rests on the following analysis of the chaining paradigm.

- (1) When A-B is learned, B-A also acquires some strength. (The literature gives overwhelming support to this premise.)
- (2) When B is used as a stimulus in a subsequent stage, there is an interference effect as a consequence of A-B learning. (This is also readily documented.)
- (3) When B-C is learned, B-A is unlearned to some degree. (By analogy the well-known work of Barnes & Underwood (1959) supports this reasonable contention.)
- (4) When B-A is unlearned in this fashion, A-B is also unlearned to some degree. (This premise is by analogy with acquisition of backward associations. No direct evidence exists.)
- (5) Therefore, when A is presented as a stimulus in the final stage, there will be less interference if B has been used as a second-stage stimulus than if B has not been used in that fashion.

Comment

The results of the writers' experiment were as predicted. The third stage was learned in fewer trials in the A-B, B-C, A-E paradigm than in the A-B, D-C, A-E paradigm. The outcome, however, is short of a convincing demonstration of their assumptions and, even if it were, this single finding is not sufficient to account for even the limited set of mediational data against which the argument was addressed.

Two omissions weaken the writers' experimental attack. First, the argument that lessening of interference could account for the mediation observed in an actual mediation experiment suffers from the lack of any mediational experimental group with which the results could be compared. This is regrettable. Second, it is unfortunate that the writers did not provide a test for the fourth assumption which they could have obtained easily by inserting a free recall test, presenting A and asking for B, immediately following the second stage of learning. Such evidence would have provided further confirmation of the supposed mechanism believed to be generating the observed difference. In the absence of such evidence the assumption of unlearning acting back on the initial habit via backward unlearning appears particularly tenuous.

The data presented for third-stage learning are somewhat unusual. A significant difference is found between the paradigms with respect to trials to criterion (ordinarily thought to be an insensitive measure) but is not found for number of correct responses in the first six trials (ordinarily thought to be a sensitive measure). Not only is this a somewhat anomalous situation in itself, but also it suggests that the reliable differences observed are largely found in the later phases of list learning. This is quite unlike the mediational effects for which the investigators are trying to account. Mediational experiments typically find differences early in learning in the number correct and less often find differences in trials to criterion. Thus while Mandler and Earhard appear to be correct in predicting more interference in the control group, the finding may be irrelevant to their argument in that the interference manifests itself in a different locus from that observed for mediation effects.

Professor Mandler has kindly furnished learning curves for the third stage. A difference between pseudomediation and control groups of about 0.50 items appears on the third trial and slowly increases to about 0.65 items on the eleventh trial. The very low point of origin of the curves suggests that the emergence of differences is delayed by the Ss' difficulty in acquiring

the set of relevant responses, a problem not usually arising in mediation experiments where such responses have occurred in a previous stage. While this may account for the shift in locus of the experimental effect, it also suggests that the interference effect may reside in different rates of response acquisition, given the greater number of terms which must be learned by the control group (30 terms in all) as compared to the pseudomediation group (24 terms).

The data are susceptible to further alternative interpretations. It is tempting to suggest that the Ss in the pseudomediation condition have learned something about overcoming interference during the second-stage learning (where they had to cope with it) and have transferred this general learning to the third stage. This is surely at least as plausible as the writers' chain of assumptions. Other differences between the paradigms may also have some effect on the third-stage learning without confirming the presumed mechanism, e. g., the pseudomediation group takes longer in the second stage and is, as a consequence, further removed both in time and trials from the original learning which is supposed to be interfering in the third stage.

It should be noted in passing that neither of these alternative interpretations can be directed at the Horton and Kjeldergaard experiments because they used mixed paradigms with the S serving as his own control. Thus, both the general transfer effects resulting from experience with interference and the time between the first and third stages of learning were equated.

Even if all other arguments could be successfully answered by specific data, it is not clear that the writers can extend their argument in any simple fashion to include successful demonstrations of mediation in other paradigms as they claim. Consider, for example, the other chaining paradigm employed by Horton and Kjeldergaard in which the order of the first two stages is reversed (B-C, A-B, A-C). Here Mandler and Earhard cannot appeal to a diminution of the A-B strength due to any subsequent learning. Presumably they will have to appeal to a more complex mechanism involving several new assumptions concerning the lessening of the role of backward interference from a response term as it affects the formation of new habits in which the response is employed. Even more difficult may be the equally successful cases of stimulus equivalence and response equivalence, both of which show mediation with both orders of the first two stages. In short, the extensions of their argument must be at least as complex as the original argument and involve untested assumptions in greater numbers.

It must also be noted that if the writers permit themselves all these extensions they run the risk that they

will not be able to explain why the reverse chaining paradigm (B-A, C-B, A-C) does not show mediational effects, that is, they will have "explained away" mediation (or will have predicted interference) even where it does not occur.

It is further discouraging to note that the argument of Mandler and Earhard must be the same for the paradigms of mediated facilitation and mediated interference, i. e., if the terms in the final learning stage of a mediation paradigm are randomly paired instead of appropriately paired, they must predict facilitation over a control group for both cases. This is contrary to the results obtained by Jeffrey & Kaplan (1957) with nonsense syllable materials and to the experimental findings of many investigators employing meaningful materials.

Finally, it is difficult to understand why for these purposes Mandler and Earhard wish to create a discontinuity between experiments using infrequent (and perhaps half unknown) words and experiments using more frequent words. They have, of course, limited the domain of discourse in which their assumptive processes are to apply but it is not clear that the dichotomization is either justified or useful.

A final word of general comment seems appropriate. If we are to admit at least chaining processes in lower animals, as we must, and, if we admit that humans can monitor, edit and respond to their own implicit and explicit behavior, as it appears we must, it seems uneconomical to spend much of our resources questioning whether mediation processes exist. Of more importance are questions concerning variables affecting mediation and questions concerning what other processes we must postulate in order to provide an account of human behavior. Our problem is not that of pruning back an over-elaborate system, but rather that of developing a system which is sufficiently rich in fruitful constructs to attempt the task before us.

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Note

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