MEDIATED STIMULUS EQUIVALENCE AS A FUNCTION OF THE NUMBER OF CONVERGING STIMULUS ITEMS

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3 experiments were performed in which the number of stimuli that converged upon a common response in a paired-associate transfer design was varied. 36 Ss served in each experiment. The number of stimuli systematically varied between the 3 studies (20, 10, and 6) while each study utilized 2 responses. Facilitation, interference, and control conditions were set up in each study by varying the stimulus relationships between the original learning and transfer lists. Results indicated that the mediated-facilitation groups generally performed better than their appropriate controls while the mediated-interference groups performed more poorly. Further, the magnitude of these effects varied directly with the number of converging stimuli.

In recent years psychologists have become aware of the close relation between the problem areas of stimulus and response equivalence, concept formation, and mediational theory, and several theoretical attempts have been made to account for a subset of equivalence phenomena within the framework of mediational theory. A number of studies demonstrating the acquisition of stimulus equivalence have been shown to fit nicely into the mediational account. The early work by Shipley (1935) and Hull (1939) as well as the recent work by Horton and Kjeldergaard (1961) are examples. However, mediational theory per se does not explain the frequent failures to find such effects (see, e.g., Jenkins, 1963). It might be argued that before we can adequately explain acquired equivalence both the theoretical formulation  

and the assessment of mediational processes must be related to data that go beyond the demonstration of the existence of the specific phenomena, important as these early studies are. Horton's (1964) study is an example of an experiment that goes beyond simple demonstration by showing the role of the meaningfulness of the presumed mediator in determining the magnitude of the mediational effect observed.

In the area of stimulus equivalence, however, there have been few studies where a parametric manipulation of a relevant variable has been attempted. The study by Richardson (1958) in which the preexperimental similarity among stimuli was varied, low or high, and the number of responses over a 16-item list was varied from 2 to 8, is probably the closest to such a study.

In the typical equivalence design, exemplified by the work of Lacey (1961), Palermo (1962), and by unpublished studies performed in the Minnesota laboratories by Gough and Odom, Greeno, and Smith, a common response is learned to two or more stimuli during original learning (OL), then on a transfer task a new response

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is learned to the same stimuli. Typically, two or three responses for six to nine stimuli have been used. (For example, VASE-1, SHEEP-1, NURSE-1, are presented in OL along with a parallel set consisting of three other stimuli and one other response; and VASE-DOB, SHEEP-DOB, NURSE-DOB, are presented on the test list along with the other three pairs with a new common response.) An interference control procedure for this design consists of randomly, rather than systematically, assigning the stimuli among the available responses on the transfer task. This is similar to the design for acquired equivalence of cues discussed by Spiker (1956, 1963), and the research reviewed by Spiker is relevant to our current concern.

The findings of the studies cited above have been equivocal. Palermo (1962) was able to demonstrate acquired stimulus equivalence with third and fourth graders but not with tenth graders or college students. Smith, in a set of carefully planned studies of the same sort, was not able to demonstrate the phenomenon in college Ss even with repeated experiences across paired-associate lists that exemplified the same systematic equivalence relations. Because of these inconsistencies the present study was planned. It appeared that Ss (at least grade-school children) could make use of mediational processes but often (at least in the case of college students) they did not do so, possibly because the mediation strategy was not utilized (because it did not occur to them or because some other strategy was easier or prepotent) or because the associational bonds were too weak.

If S mediates only when he adopts a "strategy" to do so, one way to induce him to adopt such a strategy in a learning test would be to give him a task in which any alternative to mediation would involve more difficult learning. Suppose, e.g., S is given a long list of paired associates with few different response terms, i.e., many stimuli converging upon a few responses. Such an arrangement will, in a transfer task utilizing identical stimulus groupings, give an enormous advantage to S who chooses to exploit what he has just learned relative to S who learns the list as a new one, or to another S who receives a different stimulus grouping on the transfer task and therefore cannot utilize the previous learning but must resort to learning each particular pair.

If, on the other hand, the mediational process can be explained without the notion of strategy, i.e., if it is dependent only on the associative strength between the stimulus members, the presumed mediators, and the response members, there is still an advantage in using a large number of stimulus items converging upon the same response since it can be argued that the necessary associative chains will have more opportunities to be strengthened.

Given either line of reasoning, the first step appeared to be the investigation of the effectiveness of acquired stimulus equivalence as a function of the size of the possible stimulus groupings.

**Method**

Three procedurally identical experiments were performed in which the variable of interest was the number of stimulus items that converged upon the same response. In the first of these, 20 stimuli were paired with two responses (10 stimuli to each response) during OL. On the transfer task, the facilitation group had the same 10 stimuli that converged upon the first response in OL also converge upon the first (new) response on the transfer list; the second 10 stimuli were treated analogously. (See Table 1 for an example of the design.) For the interference group one half of the stimuli paired with the first response on OL and one half...
TABLE 1
ILLUSTRATION OF THE DESIGN USED IN THE EXPERIMENT (10:2 GROUP)

<table>
<thead>
<tr>
<th>Original Learning</th>
<th>Transfer</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Facilitation</strong></td>
<td><strong>Interference</strong></td>
</tr>
<tr>
<td><strong>S</strong></td>
<td><strong>R</strong></td>
</tr>
<tr>
<td>HAMMER</td>
<td>1</td>
</tr>
<tr>
<td>MOUNTAIN</td>
<td>1</td>
</tr>
<tr>
<td>PAPER</td>
<td>1</td>
</tr>
<tr>
<td>BIBLE</td>
<td>1</td>
</tr>
<tr>
<td>SOLDIER</td>
<td>1</td>
</tr>
<tr>
<td>NEEDLE</td>
<td>2</td>
</tr>
<tr>
<td>MUSIC</td>
<td>2</td>
</tr>
<tr>
<td>SPOON</td>
<td>2</td>
</tr>
<tr>
<td>BIRD</td>
<td>2</td>
</tr>
<tr>
<td>OCEAN</td>
<td>2</td>
</tr>
</tbody>
</table>

of the stimuli paired with the second response on OL were paired with the new first response on the transfer list. The remainder of the stimuli were paired with the new second response. A warm-up control used 20 different stimuli and two responses during OL but the same transfer list as the other two groups. The experiment involving these three groups will be referred to as the 20 (stimuli) to two (response) design (20:2). The second experiment was similar to the first except that 10 stimuli were paired with two responses, i.e., a 10:2 design. A third experiment utilized a 6:2 design, 6 stimuli and two responses being used throughout. Facilitation, interference, and warm-up control groups were perfectly analogous across the three studies.

Subjects.—Three complete experiments were performed with three groups per experiment. The Ss, 108 in all, 12 per group per experiment, were undergraduate volunteers from the introductory psychology course at the University of Minnesota. Experiments I–III were done successively, but within each experiment Ss were assigned to one of the three conditions by a random process completed before Ss appeared, subject only to the restriction that males and females were equally represented in each condition. The same E served throughout.

Material.—The verbal materials were chosen for Exp. I and a random subset of these materials was used for Exp. II. A random subset of the Exp. II materials was utilized in Exp. III.

Forty object nouns which had minimal interassociative strength as indicated on the Minnesota norms (Russell & Jenkins, 1954) and Connecticut norms (Bousfield, Cohen, Whitmarsh, & Kincaid, 1961) and by inspection were utilized as stimuli. They were high frequency nouns, most having the AA rating on the Thorndike-Lorge (1944) count. These were randomly divided into two groups, one set to serve in OL for the control group and the other for the experimental groups (facilitation and interference). The responses in OL were the digits 1 and 2; for the transfer list they were high meaningful trigrams DOB and RAL (Archer, 1960).

For the interference condition the stimulus grouping was rearranged so that half of the words that converged upon the digit 1 and half that converged upon the digit 2 on the original list converged upon the same CVC, e.g., non, on the transfer list. (On the 6:2 and 10:2 lists this could only be approximated, of course.)

Across all experiments four different groupings of the stimuli which converged upon the common responses in the facilitation and interference groups were utilized to obviate the possibility that specific groupings of the words had some particular preexperimental salience. The Ss in the control condition in each experiment always learned a fixed unrelated list in OL but on the transfer list were assigned to one of the four test-list groupings. Each S was, therefore, assigned in a random fashion to both a condition and a particular list structure.

The lists were presented on a Lafayette memory drum at a 2:2 rate with an 8-sec.
intertrial interval. To control for the possibility of serial learning, there were four random orders of presentation of the list for the 20:2 groups and six random orders for the 10:2 and 6:2 groups.

Procedure.—The S was seated in front of the memory drum and was read the paired-associate learning instructions. He had one familiarization trial during which he read the stimulus and response words aloud and then learned by the anticipation method to a criterion of three errorless trials. There was a brief pause while E changed memory-drum tapes and then S, after being told that the instructions were the same except that he would be seeing nonsense syllables rather than numbers and after having a familiarization trial, learned the transfer list to a criterion of two errorless trials or a total of 10 learning trials.

RESULTS

First-list learning.—The mean number of trials to criterion in OL for all groups in each experiment is given in Table 2. In general, the number of trials decreases from Exp. I to III. The magnitude of the difference between the 10:2 and 20:2 experiments is not very great, however. Within an experiment it should be remembered that list materials for the facilitation and interference groups were the same (though, of course, they were not in the same pairings) but that the stimuli were different for the control group. Although the comparability across groups within experiments does not appear very great on inspection, overall F tests failed to find significant differences between these groups: 20:2, $F(2, 33) = 2.62, p < .10$; 10:2, $F(2, 33) = .22, p > .25$; 6:2, $F(2, 33) = 1.16, p > .25$. Further, the pattern of first-list learning data is such that the facilitation group never enjoys a substantial advantage over either of the other groups. Superior test-list performance by the facilitation group in the 20:2 study would be surprising from any inference made about learning ability with the OL lists.

Test-list learning.—The transfer effects were investigated for the first 10 trials of the second list. An S who had two errorless trials in succession was considered to have learned the list. The results are graphically shown in Fig. 1. These results are plotted in terms of percentage correct since the number of items per list varied across experiments making neither number of correct responses nor errors a representative way to plot the data.

An analysis of variance was performed on the number of correct responses across the 10 trials for each experiment. The main effect for groups was significant for the 20:2 and 10:2 experiments, $F(2, 30) = 17.52, p < .01$, and $F(2, 30) = 4.18, p < .05$, respectively. This effect was not significant for the 6:2 experiment, $F(2, 30) = 1.40, p > .25$. The effect for sex of S was not significant in any of the studies, nor was the interaction of groups with sex of S. The effect for trials was significant, of course, in each of the studies: Exp. I, $F(9, 270) = 48.64, p < .01$; Exp. II, $F(9, 270) =$

$^2$ The number of Ss that learned prior to the tenth test trial varied with both condition and experiment as would be expected. For the facilitation, control, and interference conditions in the 20:2, 10:2, and 6:2 studies, respectively, these numbers are 9,4,2; 8,10,4; and 9,9,6.
Anticipation Test Trials

FIG. 1. Percentage of correct transfer list responding in (top to bottom) Exp. I, II, and III as a function of anticipation trials.

5.10, \( p < .01 \); Exp. III, \( F(9, 270) = 16.29, \ p < .01 \). The interaction of groups with trials reached significance only for the 20:2 experiment, \( F(18, 270) = 6.00, \ p < .01 \). None of the other two- or three-way interactions in any of the experiments met an acceptable level of significance.

Main-effect analyses do not exhaust the information available from these studies, however. Inspection of the graphs in Fig. 1 indicates that the differences between the facilitation and warm-up control groups is greatest on Trial 1 for each of the experiments. Most mediational analyses, and the results of previous studies, e.g., Jenkins, Foss, and Odom (1965), lead one to expect the greatest facilitation effect on the first test trial. Therefore, \( t \) tests were performed on the first-trial data from each of the studies. The facilitation groups performed significantly better than the warm-up control in the 20:2 and 10:2 experiments, \( t(22) = 3.42, \ p < .01 \); and \( t(22) = 1.79, \ p < .05 \), respectively; but did not differ from the warm-up control in the 6:2 study, \( t(22) = 1.09, \ p < .20 \). The interference and facilitation groups differed significantly in all three of the studies, 20:2, \( t(22) = 4.50, \ p < .01 \); 10:2, \( t(22) = 3.00, \ p < .01 \); 6:2, \( t(22) = 1.94, \ p < .05 \), while the interference and control groups did not differ significantly in any of them, 20:2, \( t(22) = .75, \ p > .20 \); 10:2, \( t(22) = 1.05, \ p > .10 \); 6:2, \( t(22) = .33, \ p > .30 \). Further inspection of Fig. 1 reveals that the decrement in the interference groups relative to their warmup controls is not greatest on Trial 1. The interference group does poorest relative to the warm-up control on Trial 4 in the 20:2 experiment, on Trial 5 in the 10:2 experiment, and on Trial 3 in the 6:2 study.

A breakdown of the Groups \( \times \) Trials effect into facilitation vs. control and interference vs. control, over trials, was performed for each experiment. The facilitation vs. control contrast was significant in the 20:2 and 6:2 experiments, \( F(9, 270) = 61.48, \ p < .01 \), and \( F(9, 270) = 17.74, \ p < .01 \), respectively, but was not significant in the 10:2 study, \( F(9, 270) = .51, \ p > .25 \). This interaction pattern apparently reflects the advantage that the facilitation group enjoys at the onset of test-list learning. The interference vs. control contrast was significant in the 20:2 and 6:2 experiments also, \( F(9, 270) = 30.30, \ p < .01 \); and \( F(9, 270) = 9.59, \ p < .01 \), respectively, and just failed to reach significance in the 10:2 experiment, \( F(9, 270) = 1.86, \ p > .10 \). These interactions reflect the fact that the decrement suffered by the interference groups was greatest at about Trial 4.

The performance on the first anticipation test trial varied across the experiments. For example, the facilita-
tion group was 76.3% correct in the 6:2 experiment, 75.8% correct in the 10:2 experiment, and 87.9% correct on Trial 1 performance in the 20:2 experiment.

**DISCUSSION**

The results indicate that there is a consistent relation between transfer-list performance and the number of converging stimulus items and, further, that this relation depends upon the pairing relations between the stimulus and response items across the two lists. As hypothesized it is possible to obtain impressive results with the experimental paradigm employed by varying the stimulus-response ratio. The Ss who receive the same 10 stimuli converging upon a new response on a transfer task are almost 90% correct on their first anticipation trial in a 20-pair list. The generality of these results is limited in the present design because of the obvious confounding between the number of converging stimulus items and the overall length of the lists to be learned, and interpretations must consider this limitation.

In terms of percentage of correct responding, the comparability of the control groups for the 20:2 and 10:2 conditions is quite good. The control group for the 6:2 condition performs better, a result that is not surprising since a six-item list does not present a task of the same order of difficulty as does a 10- or 20-item list. The percentage of correct responding in the facilitation groups and their performance relative to the controls reflects the number of converging stimulus items in a fairly systematic fashion. The experimental manipulation was effective in producing apparent functional stimulus equivalence. The process involved is presumed to be a mediational one. The precise nature of the mediation cannot be exhumed from the present data; however, these data suggest a hypothesis about the nature of the process involved. When S comes to the test list and receives the same stimuli, the chances are very high that he will continue to say the old response implicitly. As the new responses appear, associations may be formed between the response on List 1 and the response on List 2. This implicit response, since it is consistently related to the new response for those in the facilitation condition, permits S to learn, in effect, a 2-item list rather than a 20-, 10-, or 6-item list and thus results in very fast learning on the test list. This is the typical associative mediational analysis. However, this may not be the only event of importance since under the hypothesis identical test-list performance should result for facilitation groups across all of the experiments and this outcome was not observed.

This consideration suggests that associative mediation may not be an automatic unitary process. As the associations between the List 1 and 2 responses build up there is a greater likelihood that they will be acquired in the longer list than in the shorter. The difference in the number of presentations of the stimuli per trial across the conditions would be enough to explain these differences. A variation of this interpretation is that S is trying to form hypotheses about the task and is busy monitoring his existing associative strengths. When he is presented with a long list, the opportunity is greater for S to note that a consistent response-response relation exists between the OL and transfer lists and he is thus more likely to adopt the strategy of responding accordingly. This is not to say that awareness is a necessary condition for the occurrence of facilitative mediation. It is, rather, to say that it is possible for S to learn that the mediating response from the first list is to be employed consistently for all pairs in the transfer list before a specific association has been acquired for each response-response pair. Corroboration of this notion about the hypothesis-formation activity of S could come from an experiment analogous in basic design to the ones presented here but consisting of, say, four responses in a 24:4 design. It would be possible to arrange the transfer list so that one of the responses did not occur until late in the list, presumably after S had produced and accepted an
adaptable hypothesis about the structure of the transfer list. (No familiarization trial would be employed.) After the occurrence of one stimulus item that is associated with the fourth response (to show S what the response is) performance on the rest of the list could be almost errorless. It would be argued that S could not have built in the relevant response-response associations over trials for the fourth response and his performance would give evidence that he had learned the system, not "merely" particular associations. The formation of a hypothesis about the structure of the system would be, however, based upon these "mere" associative strengths and would not be viewed as a capricious event.

It may be that overall frequency of the response-response relation is not the crucial event in the formation of the correct hypothesis by S, but that an event of some particular nature is necessary so that the response-response relation becomes very salient. For example, this event might consist of the same response occurring three times in succession to the varying stimuli. This would occur by chance much more often in the 20:2 experiment than in the 6:2 experiment. This notion could be tested by an experiment in which the number of successively occurring same responses was a portion of the experimental manipulation.

The arguments above do not exhaust the set of alternatives to simple associative mediation. For related work emphasizing the all-or-none character of mediation see Greeno and Scandura (1964).

Whether S is attempting to form hypotheses or is operating at a strictly associative level, the interference condition should yield a decrement in performance. Interference effects in studies of mediation have been demonstrated to be very powerful (Jenkins, Foss, & Odom, 1965; McGehee & Schulz, 1961) and this study does not contradict these previous findings. That the interference effects were greatest relative to the control-group performance on the third, fourth, or fifth trial in each of the experiments suggests an interesting extension of the above argument. The Ss in the interference conditions may discover no simple response-response relation exists between the OL and transfer lists and quickly abandon any such hypothesis. However, since there are only two responses in both lists, occasionally S will get a series of items in which these relations are consistent, thus leading to the formation of a more complex hypothesis which may be tested and then found not to hold. Under a strict associative interpretation a similar argument holds; the response-response association is occasionally consistent for a few pairs thus leading to some strengthening which in turn further interferes with second-list learning. Considering these arguments it might be predicted that a control group in which Ss learned 20 responses in OL, if learning factors could be held constant, might do better on List 2 learning than the present interference group since consistent response-response associations could not occur although both are interference conditions.

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