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ABSTRACT

Nursery school children (N=64) received seven tests of conservation of number which varied in the type and number of perceptual supports for conservation. Most of the tests with these supports facilitated performance in comparison to the standard conservation test. Conservation appeared earlier than usual. There were significant effects of supports which emphasized correspondence and deemphasized length cues, unclear effects of the number of objects, and no effect of interest value of the stimuli. There was a set effect; i.e., beginning with the easiest test produced more conservation overall than did beginning with the most difficult test. It was proposed that the development of conservation involves several levels, varying from an early rudimentary understanding of invariance to the final, mature conception of number. (Author/SET)

FACILITATION OF CONSERVATION OF NUMBER IN YOUNG CHILDREN

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Abstract

Preschool and kindergarten children received seven tests of conservation of number which varied in the type and number of perceptual supports for conservation. Most of the tests with these supports facilitated performance in comparison to the standard conservation test. Conservation appeared earlier than usual. There were significant effects of supports which emphasized correspondence and deemphasized length cues, unclear effects of the number of objects, and no effect of interest value of the stimuli. There was a set effect, i.e., beginning with the easiest test produced more conservation overall than did beginning with the most difficult test. It was proposed that the development of conservation involves several levels, varying from an early rudimentary understanding of invariance to the final, mature conception of number.

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The current controversy over whether preschool children believe in conservation of number is far from settled. Attempts to make the conservation task appropriate for young children have necessarily altered aspects of Piaget's original assessment procedure (Piaget, 1952). It is clear from such attempts (e.g., Mehler & Bever, 1967; Rothenberg & Courtney, 1968; Siegel, 1971) that the ability to "conserve" is dependent on the procedures and criteria used in its assessment. It is not clear exactly when "genuine" conservation emerges.

One way to interpret these studies is in terms not of conservation vs. nonconservation but of levels of understanding. That is, preschool children may have a rudimentary understanding of the invariance of number which they can demonstrate under facilitating conditions. This understanding would be genuine but limited, and it may be several years before the concept becomes fully generalized, easily explained verbally, and demonstrated under all conditions.

The purpose of the present study was to delineate levels of conservation by identifying conditions which facilitate or hinder conservation judgments. The approach was similar to that of Whiteman and Peisach (1970), who gradually added perceptual and sensorimotor supports to standard tests of conservation of number and mass. They found that these supports facilitated judgments by kindergarten and third-grade children on the number but not on the mass task. The present study examined the effects of several variables: degree of emphasis on one-to-one correspondence between objects in the two arrays; the presence of length cues which may distract the child's attention from number; the number of objects in each array; and the interest value of the stimuli.

Both theoretical and empirical reasons underlay the choice of these variables. In discussing conservation of number, Piaget (1952) has emphasized the child's understanding that each object in one row corresponds to an object in the other row. It seems reasonable, therefore, that any procedure which highlights this correspondence should facilitate conservation. In support of this prediction are Piaget's (1952) findings on the difference between spontaneous and provoked correspondence, as well as the report by Whiteman and Peisach(1970) that using guide lines between similarly colored objects enhances conservation. The salience of length cues for the young child is also quite clear in Piaget's reports and in numerous other studies (e.g., Wallach & Sprott, 1964). Conservation trials typically end with rows of unequal length, and nonconservers tend to believe that the longer row has more. Unexamined, however, are the possible effects of the fact that conservation trials typically <u>begin</u> with two rows of equal length. This procedure would seem to encourage children to define number in terms of length and thus to attend to length following the transformation.

The evidence with respect to the remaining two variables, number of objects and meaningfulness of stimuli, is unclear. Zimiles (1966) found some tendency for children to perform better on small numbers than on large numbers; Rothenberg and Orost (1969) and Wohlwill and Lowe (1962), however, have reported no differences. Finally, meaningfulness of materials has generally been confounded with other variables (e.g., provoked vs. spontaneous correspondence, whether the child can keep the toy); thus, no conclusions are possible.

A further issue explored in this study is the influence of set on the performance of young children. In a standard assessment for conservation of number, the salient length cues which mislead young children are operative from the start. That is, the experimenter typically begins the task by laying out two rows of equal length, then spreads one row while the child watches. Such a procedure may establish a set for the child to attend to length throughout the assessment and thereby ignore other bases for response which are potentially available to him (e.g., his knowledge that no objects have been added or subtracted). That such alternative bases may be present is suggested by Gelman (1969), who found that training apparent nonconservers to ignore length resulted in a striking increase in conservation judgments. More generally, there may be many aspects of the standard procedure which maximize the likelihood of a nonconservation judgment. Presence of these aspects on early trials may create a set to respond with nonconservation judgments, a set which persists even on later trials when misleading cues are minimized. Conversely, a procedure which begins with a simplified version of the problem may create a set to respond to invariance, a set which the child can maintain even on more difficult problems. The only direct evidence for this suggestion is from a study by Zimiles (1966), who found that children whose first trial contained a small number of objects were more likely to conserve on later trials than children whose first trial contained larger numbers. The procedure of the present study permits a more detailed analysis of set effects.

Method

Subjects

The 64 children (30 boys and 34 girls) included in the study were from two predominantly white, middle to upper middle class nursery schools in Ann Arbor, Michigan. Their ages ranged from 3 years, 1 month to 5 years, 10 months, with a mean age of 4 years, 4 months. Thirteen additional children were rejected because of failure to understand the necessary verbal terms in the pretest. The children were randomly assigned to two conditions, each with a mean age of 4 years, 4 months.

Procedure

The subjects were tested individually in a small room at the school. They were first given a pretest for the verbal terms which would be used in the conservation trials. Each child was shown an array of two small decorative stickers which served as the standard and three arrays of one, two, and three stickers.

He was asked successively to indicate the array that had the same number, more, not as many, and just as many stickers as the standard. Paired comparisons of the various arrays were then presented, and the child was asked, "Do both groups have the same number of stickers, or does one have more stickers?" (on half the trials the order of the phrases was reversed). This procedure familiarized the child with the type of question that would be used in the actual testing. If at any point the child experienced difficulty he was told the correct verbal term. Any child who had more than minor difficulty with the verbal terms was rejected.

The pretesting was followed by seven conservation trials. The general procedure was the same for all trials. The child first agreed that the number of objects in the two groups was equal. The transformation was performed, and the child was asked, "Do we both have the same number of animals (things) or does one of use have more animals (things)?" The order of the two parts of the question was alternated from trial to trial. After the child answered, he was asked, "How did you figure that out?"; if the explanation was unclear, the experimenter probed further with "What do you mean by ...?" The test materials were always removed from the child's view between trials, even when consecutive trials utilized the same materials.

All children received the seven trials described in Table 1. The trials were

Insert Table 1 about here

designed to allow separate examination of each of the relevant variables. For example, trials A and B differed only in the number of objects -- four or eight pairs. Similarly, trials D and E differed only in the materials used.

The seven trials were ordered according to expected degree of difficulty. Trial A was expected to be the easiest, with trials B through G gradually increasing in difficulty. Trial A presented a small number of objects, interesting stimuli, no distracting length cues, and an emphasis on correspondence (two zebras, two turtles, etc.). When compared to trial A the other trials presented the following "obstacles": trial B, more objects; trial C, objects transformed into lines of unequal length; trial D, objects in lines both before and after the transformation; trial E, less interesting stimuli (flat bead-like objects) in lines before and after the transformation; trial F, beads in lines before and after the transformation with no emphasis on correspondence (pairs of beads not the same color); trial G, a large number of beads in lines before and after the transformation with no emphasis on correspondence. Trial G was thus equivalent to the typical test for conservation of number.

On all trials with correspondence (trials A through E), the correspondence was emphasized both verbally and perceptually. Before the transformation the experimenter placed like animals in pairs and noted that each animal had a friend who was the same type of animal. Likewise, the fact that the pairs of beads were identically colored was pointed out to the child.

The plastic toy animals (Carrousel Party Favors #T 574) were 1 to 2 inches long, approximately the same size as the wooden beads. Each animal always appeared in its own "cage" (a small paper box with low sides but no roof). Animals in one set (designated as the experimenter's zoo) occupied pink cages; animals in the other set (designated as the child's zoo) occupied yellow cages. (A similar "yours-mine" designation was used on trials with the beads.) The purpose of the colored cages was to identify clearly the two sets that were being compared.

Whenever rows of stimuli appeared they were vertical to the child's line of vision. This arrangement avoided the tendency of some young children to choose the closer of two rows as having more (Rothenberg & Courtney, 1969). The degree of transformation (how much one row was lengthened) was constant across trials, resulting always in a ratio of 4 to 7.

As an attempt to avoid a response bias of always saying "same," a display with unequal numbers was presented following the fourth trial. The child was shown two groups of four animals each; two animals were then removed from one group and the standard conservation question asked. All children responded that one group now had more.

There were two experimental conditions. In one, the order of the trials was A through G; in the other it was G through A. Children in condition A-G thus had the trial that was expected to be easiest as their first problem, whereas children in condition G-A began with the trial that was expected to be most difficult. Set effects could be assessed by comparing the two conditions.

Scoring

There were two possible criteria for conservation: (a) a conservation judgment (C), and (b) a conservation judgment accompanied by an adequate explanation (EC). A conservation judgment was credited if the child stated that the groups had the same number after the transformation. Explanations counted as adequate were as follows: irrelevancy of the transformation, one-to-one correspondence, same number (e.g., "You have eight and I have eight."), previous equality, compensation, and reversibility. Two raters independently scored the responses. The percentage of agreement with respect to whether an explanation was adequate was 94%; the percentage of agreement with respect to type of adequate explanation was 95%.

Results

Since there were no sex differences on either the C or the EC measure, all analyses were done with the sexes combined. Table 2 presents data relevant to the

Insert Table 2 about here

question of whether the trials differ in difficulty. McNemar's test of change was used to analyze the differences between trials; all tests were one-tailed with one degree of freedom and with correction for continuity. When each trial is compared with trial G, the standard test of conservation, there is significant facilitation of conservation judgments (C scores) on trials A, B, and E ($p < .05$) and facilitation of borderline significance on trials C and F ($p < .10$). For EC scores, there is significant facilitation on trials A, C, D, and E ($p < .05$) and facilitation of borderline significance on trial B ($p < .10$).

The preceding analyses indicate that trials with some degree of perceptual support are generally easier than a trial with little perceptual support. A more specific question with respect to facilitation is which perceptual supports individually produce significant facilitation. Certain paired comparisons of trials are relevant to this question (see Table 1). One variable is the degree to which the dimension of length is present. There were significantly higher C and EC scores on trial A than on either trial C (for C scores, $\underline{X}^2 = 12.50$, $\underline{p} < .001$; for EC scores, $\underline{X}^2 = 4.05$, $\underline{p} < .05$) or trial D (for C scores, $\underline{X}^2 = 13.47$, $\underline{p} < .001$; for EC scores, $\underline{X}^2 = 2.78$, $\underline{p} < .05$). The latter trials differ from trial A in the presence of length cues either after the transformation (trial C) or both before and after the transformation (trial D). On the other hand, there were no significant differences between trials C and D. It can be concluded, therefore, that length cues present difficulties when they appear after the transformation. There is no support for the suggestion that the presence of length cues before the transformation might create a misleading set to respond to length.

A second variable of interest is the number of objects used. The evidence with respect to this variable is unclear. When trials A and B are compared, there is significantly more conservation with the smaller set than with the larger (for C scores, $\underline{X}^2 = 6.67$, $\underline{p} < .01$; for EC scores, $\underline{X}^2 = 4.76$, $\underline{p} < .05$). A similar comparison of trials F and G, however, reveals no significant difference. The variable of one-to-one correspondence is most directly tested by a comparison of trials E and F. Such a comparison indicates that correspondence significantly facilitated EC scores ($\underline{X}^2 = 2.77$, $\underline{p} < .05$) but not C scores. Finally, a comparison of trials D and E indicates that the interest value of the stimuli (animals vs. beads) was not a significant factor.

Data relevant to the question of set effects appear in Figure 1. It is clear

Insert Figure 1 about here

children who begin with the easiest trial give more conservation responses overall than do children who begin with the most difficult trial (t = 2.16, df = 62, $p < .05$). Thus, there was a set effect for C scores. The condition differences for EC scores, however, did not reach significance (t = 1.21, df = 62, $p < .20$). For the C scores, the difference between the two conditions was significant on trials A, B, and D (for all X^2s, df = 1, $p < .05$), of borderline significance on trial C ($p < .10$), and not significant on trials E, F, and G. That is, on trials using animals, performance was affected by whether the child had begun with the easiest or the most difficult trial. In contrast, there was no significant set effect on trials using beads.

The set effect for C scores is also evident from an analysis of individual patterns of response. Only one of the 32 children in condition A-G failed to give at least one C response; 11 children in condition G-A failed to give any C answers (X^2 = 8.31, df = 1, $p < .01$).

On the basis of many studies, it was expected that there would be an improvement in conservation with increasing age. This prediction was tested by dividing the sample at the median age (4 years, 4 months) and performing one-tailed t tests to compare the younger and older groups. The expected age differences appeared in the A-G condition (ts = 1.83 and 2.21 for C and EC scores respectively, df = 30, $p < .05$) -- not, however, in the G-A conditon (ts = .28 and 1.14 for C and EC scores, df = 30). A possible explanation for this finding lies in the greater consistency of the older subjects. Twenty of 32 older subjects gave the same answer on all seven trials, as compared to six of 32 younger subjects (X^2 = 10.95, df = 1, $p < .001$). Any such tendency to maintain the initially given answer would act against subjects in the G-A condition, since this condition began with the most difficult trial.

An examination of the types of adequate explanation on each trial supports the conclusion that children used the supports for one-to-one correspondence on trials on which those supports were present. About half of the adequate explanations on trials A through E were based on correspondence. On trials F and G, in which there were no supports for correspondence, the proportions fell to 12% and 29%, respectively. It should be noted that these low proportions resulted mainly from condition G-A, in which no subjects gave correspondence explanations on their first two trials.

Discussion

The children in this study demonstrated a beginning understanding of conservation of number at an earlier age than that generally suggested by the literature. For example, 17 of the 20 three-year-olds made at least one conservation judgment, and 15 of these children could also supply at least one adequate explanation. In contrast, Piaget finds that conservation of number is usually acquired at about age 6 or 7 (Piaget, 1952). The supports provided were apparently responsible for the superior performance in this study.

The results suggest that for most children in the age range studied it is inaccurate to use the labels "conserver" or "nonconserver". The majority of the children tested (especially those in the younger age groups) were conservers under some conditions and nonconservers under other conditions. Most children had mastered certain aspects of invariance but not others. As suggested earlier, it may be fruitful to think of conservation as a multifaceted concept composed of several levels which are acquired over the course of several months or years. The most rudimentary form is delicate, easily shattered by the perceptual pull of irrelevant features. In contrast, the most mature form, the principal form examined by Piaget, is stable, generalizable, and supported by a logical explanation expressed verbally. All of the levels preceding the final form compose, in a sense, a period of transition from nonconservation to conservation. The data suggest that this long period of transition is an especially informative time for studying the development of conservation. This suggestion is not completely new, of course; Piaget's own writings often focus on the responses of transitional subjects. The present study, however, postulates a more extended transitional period than has typically been assumed, and it identifies at least some of the variables which determine the transitional child's performance.

There are two general theoretical models within which to conceptualize this transitional period. One possibility is that the levels are best considered as basic advances in the child's competence with respect to conservation. In this view, the changes from level to level would reflect alterations in the underlying system of rules with respect to conservation, with full competence dependent on mastery of all of the various sublevels. The alternative model would emphasize developmental changes in processes such as attention and memory, so-called "performance variables." In this view, the perceptual supports of the present study were facilitative to the extent that they helped the child overcome the particular performance problems (poor memory, distractibility, etc.) which were obscuring his true competence (cf. Flavell & Wohlwill, 1969, for a fuller discussion of the competence-performance distinction).

Although there may be levels of understanding in the development of conservation, it is clear that these levels do not follow an invariant order of emergence. That is, while the trials did tend to increase in difficulty from A through G, the pattern of passing or failing the seven trials varied from child to child. For example, one child might succeed on a problem with large numbers before mastering any problems with linear arrays; for another child the order might be the reverse. Such individual differences suggest that there may be several different routes to the final level of conservation. Further research is needed, of course, to verify and refine the description of these routes (e.g., by including several measures of performance at each of the postulated levels).

The approach of the present study could be very useful in attempts to design training procedures that are tailored to the cognitive level of individual children. Training research thus far has tended to define "readiness" for training solely in terms of age. Procedures such as those used here could identify which of the "nonconservers" (so labeled on a standard test of conservation) have a rudimentary understanding of conservation and consequently are most likely to benefit from training. In addition, such procedures could reveal which stimulus variables are obstacles for a particular child. The training procedure could then concentrate on experience with those obstacles, building on the rudimentary abilities that the child already has. One possibility would be to begin with tasks that the child can handle and gradually "fade in" the features that are known to distract or confuse him. Whatever the approach, the suggested diagnostic procedures could make possible a more individually oriented form of training than has customarily been the case. In turn, training research could help to clarify our conceptions of competence and performance during the long transition period to conservation.

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Numbered Footnote

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2. Requests for reprints should be sent to Patricia H. Miller, Department of Psychology, University of Michigan, Ann Arbor, Michigan 48104.

Table 1

Description of Trials (Letters Refer to Type of Animal,

e.g., M = Monkey, or Color of Flat Beads)

Trial	Before Transformation	After Transformation
	Animals	
A	S–S T–T H–H C–C	S T H C T S C H
B	M–M T–T H–H D–D Z–Z C–C B–B S–S	M T H D Z C B S M S Z C H B T D
C	C–C H–H T–T S–S	T C H S T C H S
D	T–T C–C H–H S–S	T C H S T C H S
	Flat Beads	
E	R–R B–B G–G Y–Y	R B G Y R B G Y

F	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B
G	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B
	R-B	R	B

Table 2

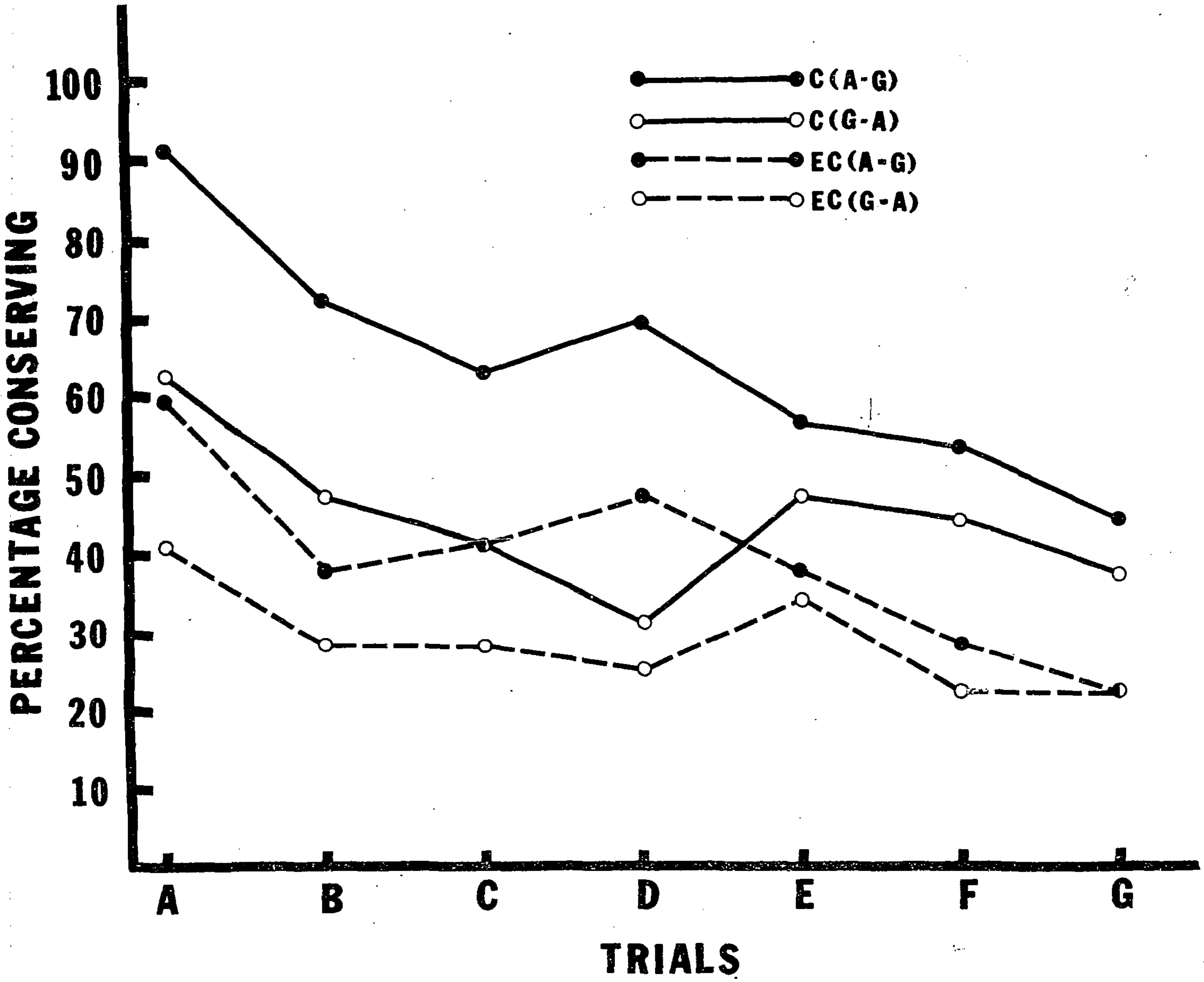
Percentage of Subjects Giving Conservation Judgments (C)

and Explanations (EC) on Each Trial

Criterion for Conservation	Trial A	B	C	D	E	F	G
C	76.6	59.4	51.6	50.0	51.6	48.4	40.6
EC	50.0	32.8	34.4	35.9	35.9	25.0	21.9

Figure Caption

Figure 1. -- Percentage of subjects in each condition giving conservation judgments (C) and explanations (EC) on each trial.


C(A-G)
C(G-A)
EC(A-G)
EC(G-A)
PERCENTAGE CONSERVING
100
90
80
70
60
50
40
30
20
10
A
B
C
D
E
F
G
TRIALS