This Week's Citation Classic®

Gelman R. Conservation acquisition: a problem of learning to attend to relevant attributes. *J. Exp. Child Psychol.* 7:167-87, 1969.
[Brown University, Providence, RI]

Five-year-old children who failed standard Piagetian tasks of conservation were trained, with a modified learning-set procedure, to focus their attention on relevant quantity dimensions in a three-item set. Experimental subjects quickly came to respond correctly and applied what they learned on "immediate" and "six-month delayed" post-tests of their ability to conserve. [The *Social Sciences Citation Index*® (*SSCI*®) indicates that this paper has been cited in over 150 publications.]

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This *Citation Classic* was based on my dissertation. How I came to do one of the first studies showing it is possible to train children who fail Piaget's conservation tasks is less than a noble tale. I was gearing up to do a multidimensional scaling study of how adults respond to music. It would not have been an easy study to do, and I wasn't sure I would learn anything musicians did not already know. I began to get cold feet and allowed myself to get talked into a brief skiing trip to Mammoth, California, but I took a bit of work along—a copy of Flavell's *The Developmental Psychology of Jean Piaget*.[1]

The choice of book may seem odd. I had resisted the fact that developmental topics interested me most because I did not like being told that women fared better in this area. So throughout graduate school I did two lines of work in parallel, one in what is now called cognitive psychology, one in developmental psychology. The two lines came together while I was lying in a hospital bed with a broken leg and with nothing else to read but Flavell's book. As I read and reread it, the work I was doing on the role of attention in learning kept coming to mind. Its juxtaposition alongside my thoughts of Piaget led me to the idea that I might succeed in teaching children to conserve quantity if I adopted the methods of those who treated learning as a function of the ability to attend to relevant attributes or dimensions in a display.[2]

The translation of the attention argument into a Piagetian training study involved thinking of the standard conservation stimuli as two complexes of relevant and irrelevant dimensions. The relevant dimension was the quantity, e.g., the liquid in two identical containers; the irrelevant dimensions were the height, width, size, color, etc., of the containers. Given that the irrelevant and relevant dimensions are redundant to start, probability favors attention to one or more irrelevant dimensions. Since Harlow could teach monkeys to ignore irrelevant dimensions and respond to the odd stimulus of three,[3] I thought it reasonable to try to adapt his learning-set method to teaching five-year-olds.

I could not rush out and work with children. I had a cast on, and they attended more to the fact I had no shoe on my left foot than to my questions about same or different number or length—my trinket reinforcers notwithstanding. With time to think, I realized that children needed to see how different transformations varied the relevant and irrelevant dimensions—even though Harlow did not let monkeys watch him set up the within-problem trials.

The young children in my experimental group responded quickly to the training; they also applied what they learned on follow-up tests of conservation. More importantly, they could justify their answers and, when given a post-test six months later, continued to conserve. I had at least met some of the Piagetian transfer criteria.[4]

My effort to publish the work introduced me to the world of conflicting reviews. The journal editor would accept the manuscript if I could deal with the "enclosed reviews." One reviewer as much as said I could not have done what I said I had and would know this had I cited comparable studies. Worse yet, I could not have read any Piaget, either in the original or as presented in Flavell's book on Piaget. The other encouraged publication and asked me to try to explain the robustness of the transfer effects, especially the explanation data. It was signed by John Flavell! I shipped the reviews off to Tom Trabasso and Wendell Jeffrey, the cochairmen of my dissertation, who explained that the article had not been rejected—quite the contrary.

It wasn't until 1982 that I published another conservation training study.[5] I reasoned that the children in the first one caught on too quickly if they really lacked underlying structures to interpret the environment I was presenting them; they had to have known more about quantity. I turned my efforts to studying what preschool children do know about quantity—and to other domains.[6,7]

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Conservation Acquisition: A Problem of Learning to Attend to Relevant Attributes[1]

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Five-year-old children who failed on conservation tests of length, number, mass, and liquid amount were given discrimination learning set (LS) training on length and number tasks. Posttests of conservation showed near perfect specific (length and number), and approximately 60% nonspecific (mass and liquid amount) transfer of training. This effect was durable as measured 2–3 weeks later. Analyses of LS learning results and the effects of other training conditions support the hypothesis that young children fail to conserve because of inattention to relevant quantitative relationships and attention to irrelevant features in classical conservation tests.

In general, a test for a child's ability to conserve quantity involves the following sequence of events: (1) An *S* is shown two identical objects or sets of objects; (2) he is then asked to judge whether the two objects are quantitatively equal; (3) if *S* says that they are equal, *E* alters some perceptual but no quantitative properties of one of the stimuli; (4) *S* is asked once more if the two objects (changed versus altered) are still equal with respect to amount; (5) and finally *S* is asked to explain his judgement. If *S* says the stimuli still have equal amounts and is able to explain his answer logically (Piaget, 1952), he is judged a "conserver." Alternatively, if he fails to indicate that the amounts are equal or gives a nonlogical explanation, he is judged a "nonconserver."

This general procedure has been used to test for the conservation of a variety of quantitative concepts: number, length, mass, and liquid

[1] Based on a dissertation submitted in partial fulfillment of the requirements for a Ph.D., University of California, Los Angeles. The research was supported by a University of California, Los Angeles Fellowship to the author and by grant MH-08741 to Tom Trabasso from the Institute of Mental Health, USPHS. I thank M. Friedman, W. Jeffrey, E. Keislar, T. Trabasso, and M. Wittrock for serving as members of my dissertation committee; Ludwig Mosberg for serving as a rater; and the staff at Sherman Oaks Elementary School, Sherman Oaks, California, for freely providing research facilities and making their students available for this study. Special thanks are due Tom Trabasso for his encouragement and advice during all phases of this research.

[2] Now at the University of Pennsylvania.

amount (Flavell, 1963). The findings from these tests tend to indicate that young children under seven years of age do not conserve, children approximately seven years of age conserve on some tasks, and children eleven years of age and older conserve on all tasks (Inhelder and Piaget, 1958).

Theoretical accounts for these results may be found in several sources (Almy, Chittenden, and Miller, 1966; Bruner *et al.*, 1966; Flavell, 1963; Wallach, 1963; Wallach, Wall, and Anderson, 1967). In many of these explanations, there appears to be an emphasis upon the nature of the internal cognitive structures of the child. Thus for example, there is consideration of the extent to which a child uses one or more logical operations of multiplication, addition and subtraction, compensation, and inversion. Other explanations deal with the effects of misleading cues, set, reinforcement, and conflict. In particular, explicit reference to factors controlling or directing *S*'s *attention* to quantitative attributes are lacking. The present study examined the possibility that a young child's failure on conservation tests may be a function of inattention to the relevant quantitative attributes of the test or attention to irrelevant features such as changes in size, shape, and color. The single important implication of this hypothesis is that a young child may in some way be able to conserve, and would do so were it not for his strong tendencies to attend to stimulus changes (Bruner *et al.*, 1966). This is illustrated by considering the nature of conservation tests and how a young child's attention might operate during such tests.

To begin an assessment of the role of attention in conservation tests, one may first note Zimiles' (1966) observations that a young child variously responds to numerical quantity on the basis of cues such as length, shape, spacing, and actual number, i.e., his definition of number is multidimensional. Of interest is that Trabasso and Bower (1968) have shown that an *S* presented with a multidimensional stimulus responds to the attribute or attributes which are most salient, or attract his attention. If one assumes that, in general, young children do define quantity multidimensionally, then it is possible to show that differential attention to the various "quantity" attributes may determine whether or not a child will behave as a conserver.

To make matters specific, consider the liquid conservation test. To begin, *S* is shown two glasses of water that are the same size, shape, height, and width; and equal amounts of water are poured into each glass. The stimulus complex of each glass with water can be thought of as a multidimensional pattern with at least six attributes; these being size, shape, height, width, water level, and actual amount of water. The *S* may perceive relations and aspects other than those defined by *E*;

hence we shall use the term "cue" to refer to any stimulus attribute to which *S* may attend, code, and use as a basis for his response (Lawrence, 1963). From the *E*'s point of view only one cue is relevant, i.e., related to the solution of the conservation problem, and this is amount of water. All others are irrelevant. However, from the viewpoint of a young *S* all cues are potentially relevant to his definition of amount. When he is asked to judge amount, he may do so on the basis of any or all cues in the complex. At the start of the conservation problem *E* has no way of knowing to which cue *S* is attending. He is, however, most likely attending to one of the *n* irrelevant dimensions rather than the one quantity dimension. Furthermore, when *E* changes a stimulus array, *S*'s attention may be drawn to the irrelevant cues since these all change while quantity does not. In fact, this manipulation should serve to enhance the likelihood of *S* using irrelevant features since movement or change is a way of bringing attention to an attribute. If *S* does use an irrelevant cue at the start of the conservation task, it does not matter, for he will still be able to judge the stimuli as equal. Furthermore, he is not asked to explain his response. What does matter, is which cue *S* attends to after *E* transforms one of the stimuli. If *S* attends to an irrelevant cue, he will judge the amounts as different and therefore not conserve. Since our analysis shows that he is most likely to attend to irrelevant cues, it would seem the conservation task needs be modified to control for attention. Alternatively, *S* could be trained to attend to relevant and ignore irrelevant cues before being tested on the classical conservation problems.

Now consider a comparison between conservation and discrimination learning tests. In both, *S* is shown multidimensional stimulus patterns; presumably can respond to any of the definable aspects therein; and is to attend, and then respond to a relevant attribute (s). Unlike in the conservation test, *feedback* in discrimination training serves to inform *S* as to which attribute he should key his responses, since reward is consistently associated with the relevant, but not the irrelevant attributes. It is always possible for *S* to correct himself. In conservation tests, correction is not possible, since *S* is not told that his answers are wrong nor given chances to try alternative cues. The implication of this comparison is that *S*s might be brought to attend to quantity attributes, and thereby conserve, via discrimination training on problems related to conservation. In this regard, learning set (LS) training procedures seem most appropriate (Braine, 1962). Here *S* undergoes training with a large number of problems containing many different stimuli; although stimuli differ, across problems there is one common relationship, and attentional responses to this common cue are reinforced. The oddity task is an example. Here *S* is presented with a series of problems and in each, three stimuli are shown—two are identical, the third different. In each case, *S* is rewarded for choosing the "odd" object; concrete features are changing and irrelevant. Subjects learn these problems by ignoring irrelevant hypotheses, or eliminating "error factors" until they come to attend and respond correctly to the relevant aspects (Harlow, 1959).

The above considerations lead us to use oddity LS training to "teach" *Ss* to attend and respond to *quantitative relations*. If *Ss* learn to attend, and respond to quantity and not other relational cues, then they should conserve when transferred to standard conservations tests.

METHOD

General Design

The experiment consisted of three phases: pretesting, training, and posttesting. In pretesting *Ss* were given standard length, number, mass, and liquid amount conservation tests. If *S* failed to conserve (see criterion below), he underwent the remaining two phases, each taking 2 days. Training was initiated within 2 weeks following testing. The training and initial transfer testing were always conducted on 3 consecutive days. A second posttest followed within 2–3 weeks. Each *S* was seen individually.

There were three kinds of training; (1) modified learning set with length and number stimuli; (2) experience with the latter problems without feedback; and (3) modified learning set with "junk" stimuli. Conditions (2) and (3) served as controls for the effects of feedback on the learning of length and number concepts, and general discrimination and labelling experience, respectively. Following training, all *Ss* were retested on length, number, mass, and liquid amount conservation tasks thereby providing tests of specific and nonspecific transfer.

Subjects

The *Ss* were 110 children (57 girls and 53 boys) haphazardly selected from the kindergartens of Sherman Oaks Elementary School in Sherman Oaks, California. Their ages ranged from 4 years, 9 months to 6 years (median age, 5 years 4 months). Three *Ss* were dropped from the experiment because they were unable to count, leaving an N of 107. Of these, 60 were assigned as described below to one of the three training conditions. The median age of the LS group was 5 years 4 ½ months, and for the other two groups, 5 years 5 months.

Conservation Pretest

The tests and items were presented in a random order for each *S*. There were two items in each of the four conservation tests. Materials in the mass test were colored plasticine balls; in the liquid test, 2 identical beakers with equal amounts of water, a tall thin glass, and a short wide glass; in the number test, 2 sets of five black checkers; in the length test, 2 yellow sticks, 10 inches each in length.

At the start of each test, stimuli were presented so that they were perceptually and quantitatively equal. Thus the 2 sticks were aligned horizontally so that their ends matched, the checkers were placed in one-one correspondence, equal amounts of water were poured into two like beakers, and the round balls of plasticine were the same. Then *S*s were asked if the stimuli had the "same or different amount"; *S*s who judged the stimuli as "same" watched *E* rearrange or alter one of the stimuli and were asked a series of questions. An example of questions asked after each transformation is:

(1) "Do these have the same or different amounts of clay?"
(2) (If *S* answered "different.") Does this one (the altered object) have more or less clay?" and "Why do you think so?"
(3) (If *S* first answered "same.") "Why do you think so?"

Similar questions were used in all conservation tests, except the wording was changed as required by the particular test. Table 1 summarizes the items used in both pre- and posttests.

Nonconservation Classification

The *S*'s answers to the questions and his explanations were used jointly to define nonconservation. Explanations given to "same" responses were rated as either "adequate," "inadequate," or "ambiguous." A response was rated adequate if it referred to former equality, reversibility of the transformation, compensation, addition and subtraction, the irrelevancy of the transformation, and partition or matching schema. Examples of adequate responses observed are; "you just moved it (the stick), and if you moved it back they'd be the same length," "because it's water and will go down further and be wider in this jar, but stays the same amount," or "it's the same number because you didn't put another one there." A response was rated inadequate if it was "magical," provided no information, or made reference to perceptual cues or events in the experiment. Examples of these are "my mommy told me," "I don't know," "they are the same size," or "you made this into a pencil." Any response that could not be rated as adequate or inadequate was designated ambiguous. All explanations given to a "same" answer were rated independently by two judges and the percent of agreement for 149 correct answers was 95.6, indicating high inter-rater reliability.

An *S* was defined as a nonconserver if he gave (1) no correct answer or explanations or (2) one or two correct answers, but no correct explanations for any of the right items. On this basis, 70 children were defined as nonconservers. Sixty-six met the first criterion, four the second. Ten of these were lost due to illness on training days. The *S*s were tested until a squad of 12 nonconservers was observed; 4 *S*s from each squad were randomly assigned to one experimental condition. This procedure continued until 20 *S*s were assigned to each condition.

TABLE 1
DESCRIPTION OF TEST ITEMS ON PRE AND POSTTEST OF CONSERVATION

Conservation test	Item	Pretest	Item	Posttest
Length	(1)	one stick moved on the horizontal to *S*'s right	(1) + (2)	same as pretest
	(2)	one stick placed at the center and vertical to standard stick	(3)	V's placed at the end of variable stick so as to induce Muller-Lyer illusion. The V's pointed inward and made the stick look shorter
			(4)	V's pointed outward and made the stick look longer
Number	(1)	one row spread out to look longer	(1) + (2)	same as pretest
	(2)	one row made into a circle	(3)	one row moved together to look shorter
			(4)	one row divided into two groups: One of three and one of two
Liquid	(1)	contents of one beaker poured into a tall thin glass	(1) + (2)	same as pretest
	(2)	contents of one beaker poured into a short wide glass	(3)	contents of one beaker poured into two smaller glasses
			(4)	contents of one beaker poured into a triangular shaped beaker
Mass	(1)	one ball made into a long thin sausage shape	(1) + (2)	same as pretest
	(2)	one ball made into a cross shape	(3)	one ball made into two smaller balls
			(4)	one ball made into a square

Oddity Control (OC) Training

The stimulus materials for oddity problems consisted of small, three-dimensional toys glued, face up, on 2 × 2-inch blocks of wood. Thirty-two sets of three blocks (two alike and one different) were made by combining 16 separate pairs of identical stimuli, so that each stimulus was included in four different sets; twice as the member of the identical pair and twice as the odd object. An example of a stimulus set would be two toy lions and one toy cup.

These 32 stimulus sets were used in 32 training problems, with 16 being presented per day for 2 consecutive days. Each problem consisted of six trials. The position (left, middle, or right) of the odd object within a problem was randomized with the restriction that the odd object occurred twice in each position. The *S*'s task was to point to either two objects that were the "same" or two that were "different." On half the trials, *E* asked *S* to point to two toys that were the "same" and on the other half to two that were "different." The latter response had to include a choice of the "odd" stimulus in order to be judged correct. Whenever *S* made a correct choice, he was told; "Yes, that is right, and here is a 'prize'." When wrong, he was told; "No, that is not right." A noncorrection procedure was followed. The prizes were trinkets.

Learning Set Training

Problems. There were 32 six-trial problems, 16 were length and 16 were number. Each problem consisted of three stimulus objects, two that contained identical, and one that contained different quantities (e.g., two rows of five chips versus one row of three chips, or two 6-inch sticks versus one 10-inch stick).

Thirty-two six-trial problems were used to assure that *S*s received extensive training with a large number of different examples of the relevant conservation principles. The choice of number and length concepts derived from examination of the nature of the problems. It has been noted (Piaget, 1952) that children often define numerosity in terms of length cues. For example, children say that the number of chips in a row increases when the row is made longer. Alternation between number and length problems here meant that sometimes the length was relevant and sometimes irrelevant. The interchange of number and length tasks was viewed as one way of forcing the child to see that a quantity cue can be either relevant or irrelevant, and that he has to discriminate when a particular cue is, in fact, relevant. To solve all problems, the child would have to learn to separate out the different cue functions of length, as well as, ignore irrelevant cues within a problem.[3]

[3] In regard to the above it should be noted that Beilin (1965) used a feedback

(a) *Between problem variations.* The changes that occurred between, but not within problems were color (red, green, yellow, or blue), size and shape (for length, large or small square or circular sticks; for number, large or small rectangular or circular chips), starting arrangements (horizontal, vertical, horizontal and vertical, and geometrically arranged rows of chips or sticks), and quantity combinations. For length,

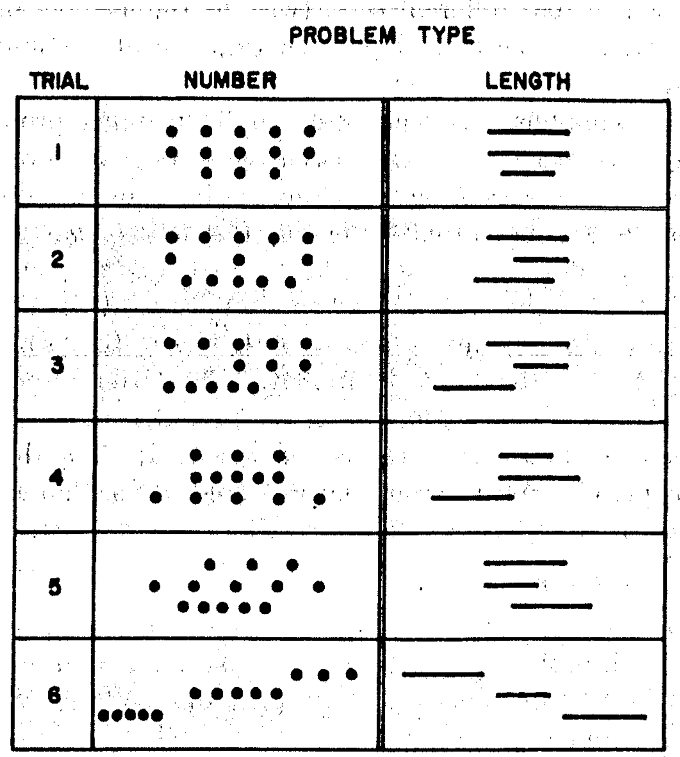

FIG. 1. Schematic representation of intraproblem variations for a length and number problem presented during SC and LS training.

the quantity combinations were: one or two of either 6- and 10- inch or 5½- and 7-inch sticks. The number quantity combinations were: one or two sets of four and six, or three and five objects. In both kinds of problems, variation in length and number values were used to assure generalized responding to length and number cues.

For each type of problem (length or number), 16 stimulus sets were constructed by combining these dimensions above. Over problems, each stimulus value occurred four times.

(b) *Within problem variations.* Examples of the stimulus variations

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training procedure with length and number problems and failed to obtain transfer on conservation tests. This may be attributed to the fact that he used significantly fewer trials and presented the length and number problems in blocks rather than randomly.

within a problem are shown in Fig. 1. The stimulus sequences within each problem were designed to approximate certain features of the conservation tests and allow for later analyses of the kinds of errors made. The six trials were arranged as follows: On Trial 1, as at the beginning of conservation tests, the stimuli consisted of patterns where all cues were relevant and redundant. For example, two sticks of equal length were placed parallel to each other and with ends aligned, but the third different lengthed stick was not aligned nor necessarily parallel with the other sticks. Thus, a choice of the aligned and parallel sticks as "same" could be on the basis of either length (which is relevant) or end matches or parallel cues (which are irrelevant). Likewise, in the number problems, on Trial 1, the number, length, and parallel cues were redundant. Trials 2–5 served as "transformation" trials, where the alignment and geometric cues varied independently of length and/or number cues. On Trial 6, the stimuli were moved so as to hold constant irrelevant cues. For example, the 3 sticks were spatially separated and nonparallel. For *S* to respond correctly, he would have to do so on the basis of the relationship between quantities per se.

Although the questions asked and the stimuli shown in LS training were different than those in OC training, other features of training (e.g., feedback presentation and randomization) were identical to those in the OC condition. After the stimuli were arranged for a particular trial, *E* said "show me two sticks that are the same (or different) length" or "show me two rows that have the same (or different) number of things in them."

Stimulus Change (SC) *Control.* The stimuli and training procedures for the SC condition were identical to those for the LS condition, except no feedback was given. At the end of each session, *S* was told that he was playing the game "very well" but nothing else.

Transfer Tests. Each *S* was tested the day after training and then 2–3 weeks later on the four conservation tasks shown in Table 1. The administration procedure was identical to that used in pretesting, except for the addition of items in each test. When the conservation tests were readministered 2–3 weeks after the first posttest, the presentation order of tasks and items within each was rerandomized, but otherwise testing was the same.

RESULTS AND DISCUSSION

Oddity Training

The *S*s in Group OC made virtually no errors in training. The mean number of errors per *S* was 1.5 indicating that understanding of "same-different" was most likely acquired before training.

SC and LS Training

Figure 2 shows the proportion of correct responses for the 32 successive problems for the LS and SC groups. The first problem is shown by halves, since learning in the LS condition began within the first 3 trials; this was not true for the SC group. Inspection of Fig. 2 reveals that both groups had the same initial probability of a correct response (approximately .60). Learning Set *Ss* began to learn *immediately* and reached an asymptotic performance level of approximately 95% correct. An *S* was considered to have learned the length and number problems, if for each he had no more than one error for at least the last two problems. Nineteen of the 20 LS *Ss* reached this criterion for both types of problems.

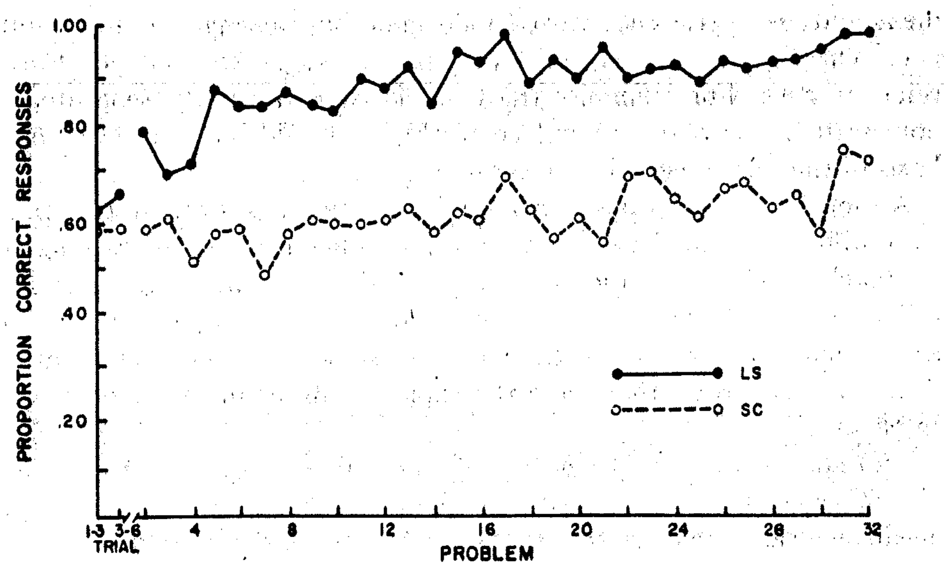


FIG. 2. Probability of a correct response per 6-trial problem for groups LS and SC.

In contrast, there was almost no learning about quantity in the SC condition. The average learning curve is relatively flat, rising over the last problems to .70 correct. Inspection of individual data revealed this improvement was contributed by 6/20 *Ss*. Four of these *Ss* "learned" both concepts, while the other two "learned" only the length concept. The remaining *Ss* failed to reach criterion or show any improvement over trials. Fig. 3 gives "learning" curves for both kinds of SC *Ss*, "learners" and "nonlearners." In comparing Figs. 2 and 3, note that *Ss* who came to respond to quantity in the SC condition did so much more slowly than LS *Ss*.

The LS acquisition data were analyzed separately for the length and number problems. The probability of a correct response on each problem

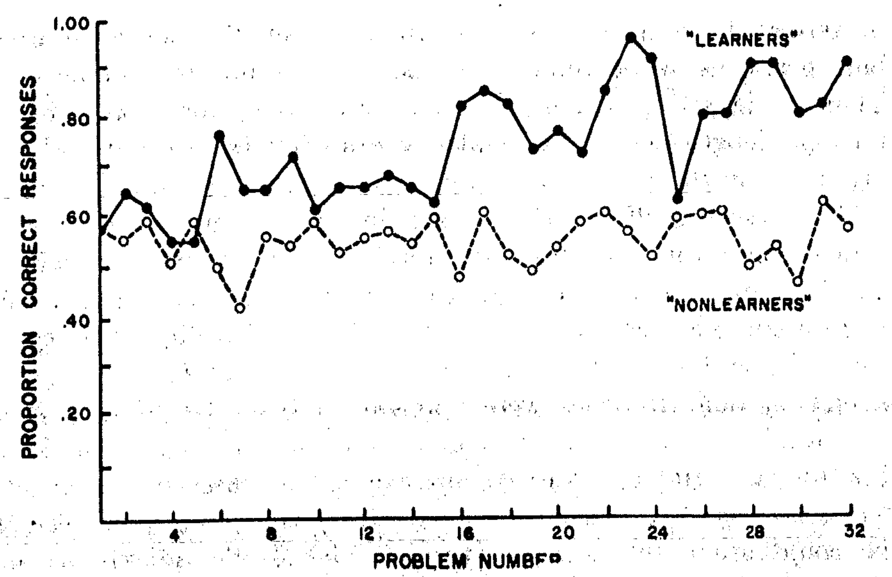


FIG. 3. Probability of a correct response per problem for "learners" and "nonlearner" in the SC group.

type during the course of training is given in Fig. 4. One can see that for problems 1–10 the learning curves do not differ in any systematic way. However, for problems 11–30, the probability of a correct response on length exceeds that on number in all but two cases. Thus, length concepts would appear to be "easier" to acquire than number ones. However, this result is understandable if one analyzes the two problems

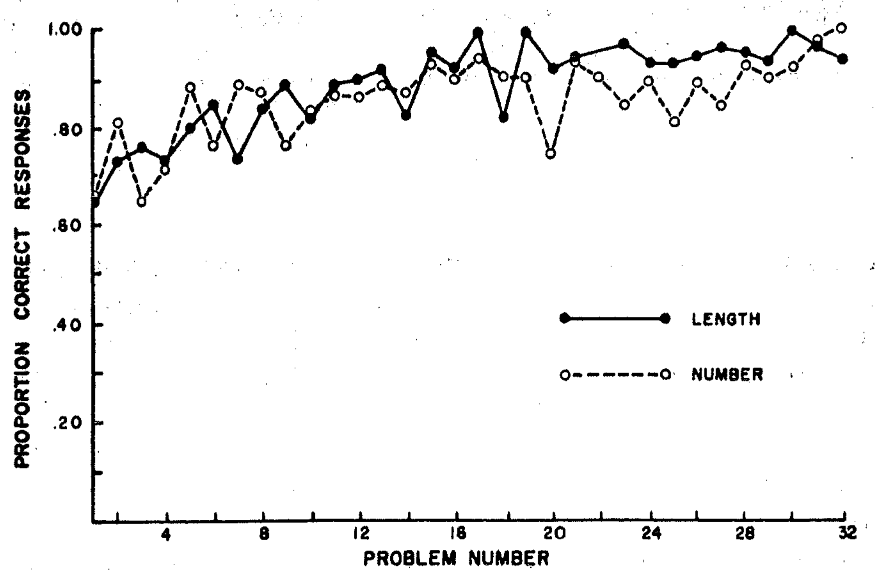


FIG. 4. Probability of a correct response on length and number problems for group LS.

in terms of the cues contained therein. Length cues were present in both problems, while number cues occurred in only the number problems. Thus in length problems, *Ss* were reinforced for responding to the relevant length cues. This tendency probably transferred and produced negative transfer to the irrelevant length cues in the number problem.

To assess the effects of feedback in LS, separate analyses were performed on error data for each kind of training trial. Recall that on Trial 1, all cues were present and redundant. Since Trial 1 offered *S* more than one "correct" alternative, the probability of a correct response should be greatest on these trials. On Trials 2–5 relevant and irrelevant quantity cues were opposed. If *S* used irrelevant cues, then this would be shown in an increased error probability on Trials 2–5. Finally, on Trial 6, all irrelevant cues were removed or held constant, which means that *S* had to respond to quantity to be correct. Thus if he could use a relevant quantity cue, he should quickly learn to respond to these. If not, the probability of a correct response should be close to chance ($p = .60$).

Table 2 gives the proportion of errors group LS made on Trial 1,

TABLE 2
ERROR PROBABILITIES DURING THE FIRST AND SECOND HALVES OF LS TRAINING

	Proportion of errors			
	Length		Number	
Problem type	First half training	Second half training	First half training	Second half training
Trial				
1	.07	.01	.05	.04
2–5	.19	.08	.21	.17
6	.12	.03	.10	.06

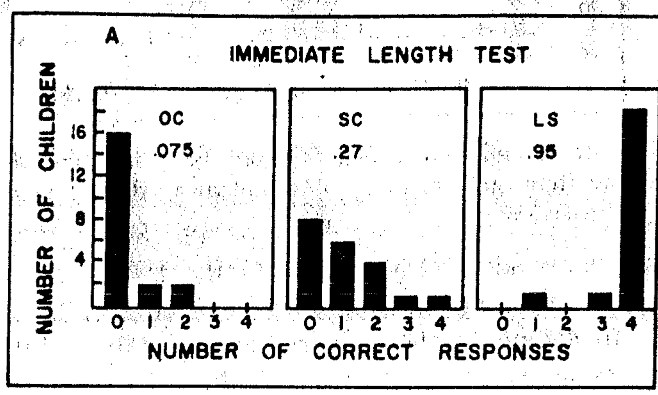
Trials 2–5, and Trial 6 on length and number problems during the first and second halves of training. As expected, the proportion of errors is very low for Trial 1. This proportion was also low for Trial 6, indicating *Ss* were able to respond to relationships of quantity per se. The larger error proportions on Trials 2–5 reflects interference from irrelevant cues. The high Trial 2–5 error probability for number problems during the second half of learning reflects the previously noted tendency for *Ss* to err on number problems 10–30.

To determine whether or not irrelevant cues dominated when introduced in LS training, a comparison was made between the number of errors due to use of such irrelevant cues versus errors due to random or position responses. In scoring the errors, it was assumed that *S*'s choice of stimuli indicated to which cue he was attending on a given trial. For example, in Fig. 1, on Trial 3, if an *S* made a "same" choice of the rows containing three and five chips, his error was judged to be one of matching ends; on Trial 5, for a similar erroneous choice, spacing cues were judged to be those controlling the choice. For a more detailed description of this scoring, see Gelman (1967).

Given that an error occured, the probability that an irrelevant cue was used is extremely high. For number problems, these were .89 and .82 for the first and second halves of training, respectively. For length problems, these same probabilities were .88 and .85.

A similar pattern of error probabilities was observed for SC *Ss*, except these were larger, reflecting the observation that most SC *Ss* did not learn.

On the basis of the LS error analysis, we conclude that with feedback, young children quickly learn to use a quantity dimension. In fact, the rapid acquisition (especially as indicated on Trial 6) strongly suggests that *Ss* had some preexisting understanding of quantitative relationships. Nevertheless, when irrelevant cues were introduced, they were frequently



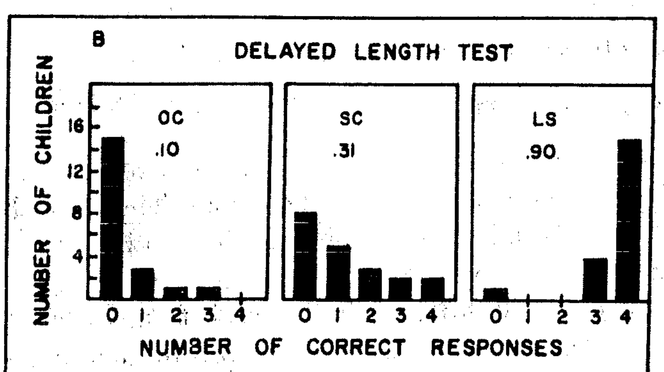


FIG. 5. Number of children in OC, SC, and LS conditions who correctly answered 0, 1, 2, 3, or 4 length items on immediate (5A) and delayed (5B) transfer tests. (Overall proportions correct indicated inside figures).

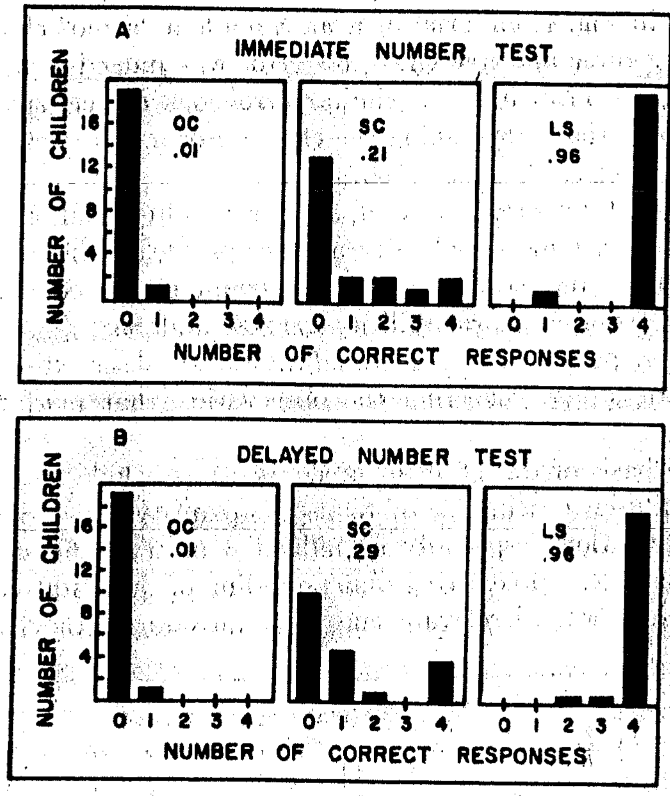


FIG. 6. Number of children in OC, SC, and LS conditions who answered 0, 1, 2, 3 or 4 number item on immediate (6A) and delayed (6B) transfer tests. (Overall proportion correct indicated inside figures).

the basis for responding. This supports the hypothesis that irrelevant nonquantitative cues are salient for the young child and that he is more likely to attend to them. Introducing feedback into the task apparently forces his to eliminate the use of irrelevant cues and to attend to and use relevant quantity cues. The question of interest is whether or not this learning transfers to conservation tests.

Specific Conservation Transfer

The specific conservation transfer data are the frequencies of correct answers given to the length and number items. These are shown in Figs. 5 and 6 for each training condition and for both immediate (one day after training) and delayed (2–3 weeks later) posttesting.

On the immediate test for length conservation, 16/20 OC *S*s failed to improve. Four *S*s answered only one or two of four items correctly. In the SC condition, 8 *S*s showed no transfer. With one exception, those *S*s who answered correctly, did so to only one or two items. In contrast, all but two LS *S*s made perfect scores on length tests. Comparing Figs.

5 and 6, one can see that the pattern of results on the immediate number tests was virtually identical to that on length tests.

The overall percentages correct for specific length and number tests were 95 and 96, 27 and 21, 7.1 and 1.3 for the LS, SC, and OC conditions, respectively.

Specific tests administered two to three weeks later yielded essentially the same results as above, indicating durability of the training effects.

The SC data were analyzed in terms of those *S*s who did and did not "learn" during training. The 6 SC "learners" contributed 60% of the correct answers. This is to be compared with the 95% correct responding by LS *S*s, suggesting that the "learning" by SC *S*s was not durable.

Nonspecific Transfer

The results for immediate and delayed tests on liquid are summarized in Fig. 7; those for mass are given in Fig. 8.

As on specific tests, *S*s in group OC showed no transfer to nonspecific tests. More notably *S*s in the SC condition also failed to transfer to non-

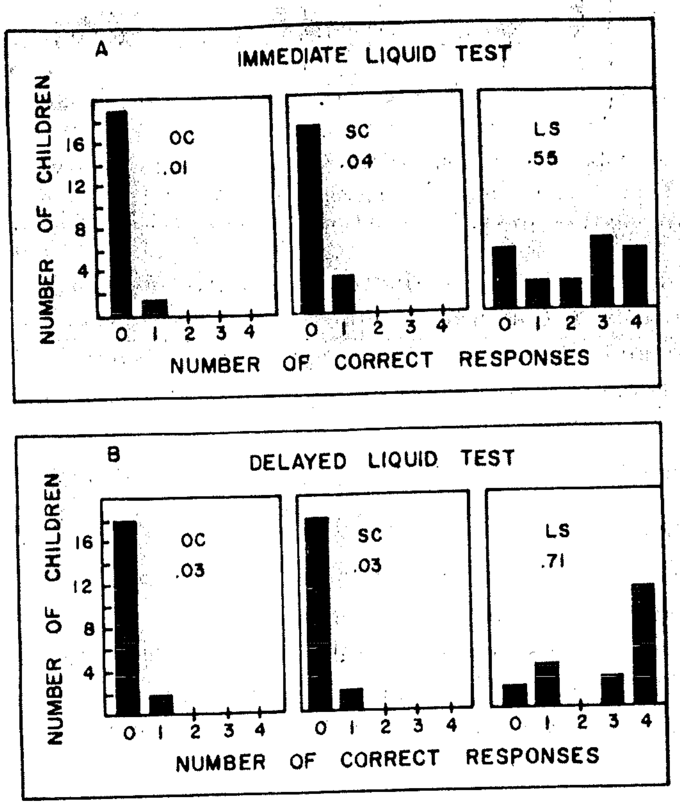


FIG. 7. Number of children in OC, SC, and LS conditions who correctly answered 0, 1, 2, 3, or 4 liquid items on immediate (7A) and delayed (7B) transfer tests. (Overall proportion correct indicated inside figures).

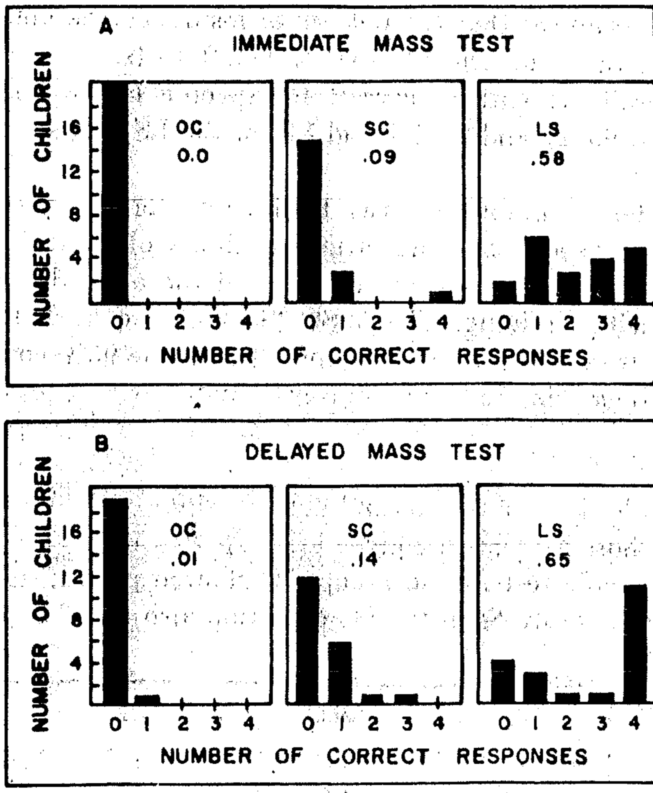


FIG. 8. Number of children in OC, SC, and LS conditions who correctly answered 0, 1, 2, 3, or 4 mass items on immediate (8A) and delayed (8B) transfer tests. (Overall proportion correct indicated inside figures).

specific tests. The LS *Ss* showed substantial generalization. On the immediate tests, the average proportion of correct test responses by LS *Ss* were .55 and .58 for liquid and mass, respectively. The proportions for the two respective delayed tests were .71 and .65, indicating retention and perhaps, some improvement from immediate to delayed tests. These strong generalization effects would seem to contradict one aspect of Piaget's theory of conservation development. In his discussion of how the concrete operational child (aged 7–11) conserves, Piaget states that the child does not have as yet the ability to use one common set of rules for all conservation problems, and learns anew to apply concrete operations to each conservation problem he confronts (cf. Flavell, 1963; p. 204). The generalization data do not bear this out; rather, they support the idea that a general rule can be applied to the conservation tasks, one which involves the ability to look for and use relevant quantity cues.

Explanations on Transfer Tests

Following the procedure used for pretest data, all explanations for correct responses were rated as adequate, inadequate, or ambiguous. The percent agreement for two independent ratings of 602 explanations was 96.2.

Regardless of test type or test interval, LS *S*s consistently gave more adequate explanations than their SC (or OC) counterparts. For LS *S*s, the conditional probability of an adequate explanation, given a correct answer on the immediate test, was .97, .78, .78, and .91 for number, length, mass, and liquid, respectively. The respective probabilities on delayed tests were .95, .86, .89, and .97. Some examples of adequate explanations which illustrate the extent to which LS *S*s conserved on posttests are; "you have to break them if you are going to change the length," "you haven't taken any away," "it's (water) lower but wider and so the same," "they were the same before and you can see if you make it (plasticine) back," or "it doesn't matter if you do that." This good correspondence between responses and explanations was not observed for the SC *S*s. Approximately 27% of all specific transfer items were answered correctly by SC *S*s, but of these, less than 50% were explained adequately. These *S*s gave 39.4% adequate explanations on immediate and 47.9% on delayed tests. Comparing kinds of specific tests, SC *S*s gave a higher percentage of adequate explanations for number (61 and 73 for immediate and delayed) than length (19 and 24).

Since there were so few explanations given to nonspecific transfer items by SC *S*s, and to all transfer items by OC *S*s, these explanations were not analyzed.

A consideration of nonspecific explanations sheds some light on why generalization occurred on the mass and volume tests following LS training. Aside from having learned to ignore specific context cues in length and number problems in LS training, *S*s may have learned to ignore some nonspecific classes of cues (cf. Harlow, 1959; Restle, 1958). Such a general class might be stimulus change, regardless of the quantitative cue. Also LS *S*s might have learned to maintain an initial judgment from Trial 1 through the stimulus changes. That this kind of learning did occur, is shown in nonspecific explanations like "you haven't done anything to change the amount" or "they used to be the same and so they have to be now." Such explanations occurred frequently; in fact, every LS *S* gave at least one such nonspecific explanation.

Immediate and Delayed Posttest Differences

Note that for all transfer conditions, save one, both the number of correct responses and quality of explanations improved from the first to the second posttest. The LS *S*s showed some forgetting on length tests. To evaluate the reliability of these changes, *t* tests for correlated observations were performed on all measures. Nonsignificant *t* scores were obtained for all but one test; the increase in number of "same" responses given by LS *S*s on liquid tests is significant, but only when a 1-tail test is used ($t = 1.857$; $p < .05$). Thus it would seem that the observed changes are not reliable.

Overview

Starting with an analysis of conservation as a problem in attention and discrimination, the present research has shown that, given appropriate training, one can elicit conservation behavior from children who initially fail to conserve on classical conservation tests. Appropriate training seems to involve two factors: (1) An opportunity to interact with many different instances of quantitative equalities and differences and (2) feedback, which presumably tells *S* what is and what is not relevant to the definition of quantity. This is supported by training and transfer results from both SC and LS *S*s. The SC *S*s received only changing stimuli, while the LS *S*s received both changing stimulus experience and feedback. Some of the SC *S*s learned to conserve in a limited way. There was a small amount (27%) of specific generalization, but almost no *nonspecific* transfer. In contrast, with LS training almost perfect *specific* and considerable *nonspecific* transfer occurred. In addition, LS *S*s were better able to explain their correct answers. Finally, it seems that LS training brought *S*s to use a general rule like "it doesn't matter what you do or pay attention to the way it is to start."

There are two alternative interpretations that might be made of these results. The first involves a statement about the nature of concept acquisition per se. We might say that the LS procedure approximated the actual conditions under which a child learns to conserve *de novo*. However, the data go against this interpretation in several ways. First, LS *S*s very quickly mastered the training task. If they were learning to define quantity and quantitative invariance *de novo,* we might expect more errors than observed and certainly would not expect learning within the first six trials. Since there was very fast acquisition, it seems more appropriate to say the five year old can work with quantity if "told" to do so. Feedback seems to be a very effective way of communicating the task requirements to a young child.

A second result of interest corroborates this interpretation. Analyses of Trial 6, which provided stimuli containing only the relevant quantity cues, showed that *S*s had little or no difficulty in responding to quantity. Even during the first half of training *S*s were able to work directly with the relevant cue. In fact, the only trials which provided a source of difficulty were those where irrelevant perceptual cues were present. These error analyses support the hypothesis that a young child can respond correctly to quantitative relationships, but has to be "instructed" on their use (cf. Braine and Shanks, 1965). The five-year-old child apparently does have to learn to respond *consistently* to quantity and not be distracted by irrelevant cues, but does not have to learn, *de novo*, to define quantity and invariance. That *Ss* adopted somewhat general rules may reflect new learning. Still, other investigators have indicated that young children know more about quantitative relationships than demonstrated in conservation tests, e.g., it has been shown that *Ss* can accurately anticipate quantitative changes (Taponier, 1962; cited by Berlyne, 1965), or predict reversible effects (Berlyne, 1965; Wallach *et al.*, 1967), and still not conserve. It could be that these responses are present in a child's repertoire, but are dominated by strategies under the control of irrelevant stimuli. If so, training which extinguishes the use of irrelevant cues should also bring out the "correct" verbal responses.

It should be noted that our basic results lend support to the positions of Wallach *et al.* (1967) and Zimiles (1963). Wallach *et al.*, have suggested that conservation depends on a child learning to ignore misleading cues; and Zimiles (1963) offers an explanation of number conservation in terms of *S* learning to ignore spatial cues, while developing a number "set." Also of interest is a recent experiment by Kingsley and Hall (1967) who have successfully trained length and weight conservation with LS procedures. Although these findings agree with those of the present study, it is not clear from an analysis of the training procedures used by Kingsley and Hall why transfer was obtained. Kingsley and Hall's use of LS training differs from that of the present study. Rather than specifically training *Ss* to ignore irrelevant and attend to relevant aspects of the conservation problem, they trained *Ss* on a graded series of subtasks related to conservation (e.g., appropriate use of scales and then the operation of addition and subtraction). It is possible that the experience on these subtasks involved ignoring the irrelevant features therein, and hence, indirectly served the same purpose as the LS experience in the present study.

A critical question often raised about the present kind of research is whether or not the LS *Ss* "really" have conserved on post-tests. Insofar as our tests measure conservation, the answer is yes. That *Ss* were able to generalize and give logical explanations on nonspecific tests seems most significant. However, Piaget (1967) might argue that such tests are not sufficient and that our LS *Ss* are "pseudoconservers" and would fail to perform correctly on other acts such as pouring an equal amount of water into large, narrow and shallow, wide jars. This can only be answered with further studies, since the present design did not include such tests.

Aside from the above, the results point to two lines of research. One calls for the development of techniques that will yield specific descriptions of what the five-year-old knows about quantitative relations, and how his behavior differs from older children. For example, the data support our assumption that young children operate on a hierarchy of quantitative response strategies and define quantity multidimensionally. What seems called for is a careful analysis of this hierarchy and how it changes. A second line of investigation points to the need to develop techniques that could be used with younger *Ss*. The present research does not tell us how the child is able to respond to relevant quantity cues or what such a statement means operationally. Put differently, if by the time the child is five he has formulated the hierarchies we postulate, we still have the tasks of tracing and explaining their development.

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