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AUTHOR	Zimmerman, Barry J.; Rosenthal, Ted L.
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ABSTRACT

The effects of live and symbolic modeling on the conservation of equalities and inequalities were studied with items spanning three stimulus dimensions (length, number, and two dimensional space). A total of 48 kindergarten children, who had failed to conserve on any equality items in baseline measures, were randomly assigned, in equal groups, to one of four conditions: modeling only, verbal correction, modeling plus correction, or control. Children were tested for generalization immediately after training, and for retention seven to ten days later. Brief observation of a model, briefer correction training (joining positive feedback with verbal rule provision), and the combination of observation and correction were all successful in producing learning and, without further training, transfer and retention of conservation. Unlike the controls (who also never correctly answered any quality items), the trained, experimental groups gave evidence of spontaneously generalizing their new learning to a task that required nonverbal behavior to manifest conservation. (Author/SB)

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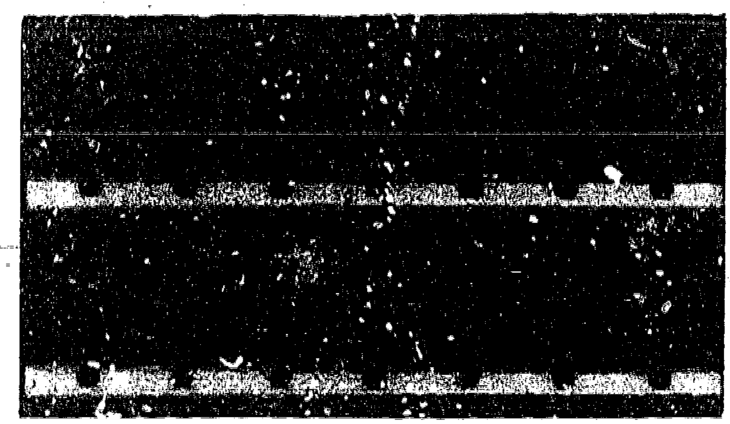

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RESEARCH AND DEVELOPMENT
College of Education
1515 East First Street
Tucson, Arizona 85719

CONSERVING AND RETAINING EQUALITIES AND INEQUALITIES THROUGH OBSERVATION AND CORRECTION

by

Barry J. Zimmerman

and

Ted L. Rosenthal

Abstract

The effects of live and symbolic modeling on the conservation of equalities and inequalities was studied with items spanning three stimulus dimensions (length, number, and two dimensional space). Brief observation of a model, briefer correction training (joining positive feedback with verbal rule provision), and the combination of observation and correction were all successful in producing learning and, without further training, transfer and retention of conservation. Unlike the controls (who also never correctly answered any equality items), the trained, experimental groups gave evidence of spontaneously generalizing their new learning to a task that required nonverbal behavior to manifest conservation.

CONSERVING AND RETAINING EQUALITIES AND INEQUALITIES THROUGH OBSERVATION AND CORRECTION[1]

Barry J. Zimmerman and Ted L. Rosenthal

University of Arizona

Early attempts to create "precocious" conservation by training young children were usually failures. Reviewing that literature, Flavell (1963) concluded that most of the methods reported appeared sound and reasonable but "most of them have had remarkably little success in producing cognitive change." A recent attempt using several "discovery" methods, and large samples, was also unsuccessful (Mermelstein & Meyer, 1969). Several behaviorally oriented efforts have brought better results. For example, Kingsley and Hall (1967) were able to increase weight conservation which generalized to conservation of substance. Rothenberg and Orost (1969) produced number conservation in kindergarten children who then generalized conservation to discontinuous quantity. In a very careful experiment, Gelman (1969) was able to train conservation of length and number by emphasizing discrimination learning rather than skill practice. In all these successful studies, extensive task or discrimination training has been necessary to modify rather delimited classes of conservation, as illustrated by the dimension italicized above.

In recent research with social learning methods, Rosenthal and Zimmerman (1972) were able to demonstrate learning and generalization of multidimensional conservation tasks, using much briefer observational techniques, confined to a single training session. In their research, a number of features, imposed by design constraints, require mention: (a) Although six diverse conservation dimensions were included in their training and transfer items, correct response always required a judgment (or judgment plus explanation) of stimulus equality, because only equal stimulus members were presented and transformed. (b) The children's answers were always given verbally; generalization was never tested with a task reflecting nonverbal evidence of conservation. (c) Retention after delay was not studied.

The present experiment was addressed to these issues. Each set of conservation items sampled the categories of length, number, and two dimensional space but half of all items required the maintenance, after transformation, of the initial stimulus inequality. In addition to the main data based on verbal judgments (and explanations) of stimulus equality and inequality, a task was given after training to determine if the children could spontaneously display manual evidence of understanding, by returning the transformed stimuli to their initial status. A retention phase, after a week's interval or longer, was included.

There is evidence that corrective feedback has been effective in training children to conserve. Beilin (1965) compared nonverbal reinforcement, verbal orientation-reinforcement, verbal rule provision (or corrective feedback), and "equalibration" methods for training kindergarten children to conserve length and number. Only the corrective feedback procedure proved effective in training children to conserve. This procedure involved: (a) presentation of the conservation problem which led to either success or failure, and (b) with failure, the provision of a verbal statement of the correct conserving rule. This rule statement was accompanied by the experimenter's repositioning of the transformed stimuli in their initial untransformed state. Thus verbal rule provision was conbimed with the experimenter's demonstration of a nonverbal reversibility response. Since demonstration both with and without corrective feedback has been effective in teaching children to conserve, it is of interest to compare modeling with a purely verbal corrective feedback procedure to determine the relative importance of each variable. Such a comparison follows Bandura's (e.g., 1969) distinction between live versus symbolic modeling.

METHOD

Subjects and Experimenters

Sixty-five kindergarten children were randomly drawn from two Tucson elementary schools serving Anglo-American, lower middle class populations. From this initial set, 24 boys and 24 girls who failed to conserve on any equality items in baseline were retained for study. Six boys and six girls were randomly assigned to each factorial combination of treatments. The children ranged in age from 5.1 to 6.4 years, with a mean age of 5.7 years. One female graduate student served as experimenter and another as the model. Both adults were Anglo-Americans in their twenties, with no striking departures from average characteristics.

Task Materials

Using items selected and modified from the Goldschmid and Bentler

(1968) Concept Assessment Kit, three sets of stimuli were prepared. Each stimulus set comprised 12 items, four of which pertained, respectively, to conservation of <u>length</u>, of <u>number</u>, and of <u>two dimensional space</u>. For the several types of conservation, every item quartet contained two equality and two inequality tasks. On conventional equality items, the child was first presented with a pair of identical stimulus members, one of which was subsequently transformed to appear perceptually discrepant; all equality stimulus pairs were counterbalanced for color, position, and which member was moved during transformation procedures. Inequality items contained pairs of unequal stimulus members but corresponded to equality items in color, position, and member transformed. Inequality pairs were further counterbalanced for correct response since one member remained larger after transformation. Thus, the same general pattern was followed on all inequality items: Two unequal stimulus members were first presented and the larger was designated for the child. Next, the experimenter transformed the spatial format of the larger or smaller member and the child was asked if the resulting arrays were equal or unequal, and then asked to explain his judgment. Inequality items tested whether the child could maintain the initial stimulus differences when one member had undergone transformation. During all procedures, when the experimenter returned arrays to their original formats, she screened the stimuli from the child so that reversibility cues were eliminated.

The first four items of stimulus set I assessed conservation of length. For equality items, pairs of unlike-colored (red and blue) rods, 1 x 1 cm. in girth and 25 cm. long, were used. For inequality items, a

20 cm. rod, otherwise identical, was presented with a longer 23 cm. rod of the other color. The transformation involved placing one rod (to the right or the left) so that it was no longer flush to the subject's left with the second rod. The four items (generated by the left and right displacements in equal and unequal rod lengths) were presented in a fixed, randomized sequence. The next four items of set I assessed conservation of number. On equality items, six red and six white 3.5 cm. poker chips were presented in two parallel arrays. On inequality items, the red or the white array contained only five chips. The transformation reduced the spatial range of one array by placing its members closer together. Because of counterbalancing constraints, the numerically larger array was reduced in range on half of the inequality items, and the numerically smaller array was reduced on the remaining half of these items. As described above, all items were appropriately counterbalanced and were presented in a random sequence. The last four items of set I assessed conservation of two dimensional space. For equality items, an array of 16 red and another of 16 green tiles (2.8 $cm.^2$ in area) were first presented in 4 x 4 tile square formats. For inequality items, two tiles were removed from one array before giving the initial display; in all other aspects, the inequality items were treated like the equality stimuli. The transformation changed the shape of one array to a triangular format. As before, all item properties were counterbalanced, and items were presented in a fixed random sequence. The set I stimuli were used for measuring conservation in baseline, and also in the retention phase some 10 days after training.

Stimulus set II was constructed analogous to set I, with similar randomization of presentation sequence, counterbalancing, and distinctions between equality and inequality items. In set II, the number conservation quartet came first, using the 2.8 cm. red and green tiles instead of the larger red and white poker chips For conservation of two dimensional space, the same stimulus tiles of set I were used, but they were transformed into a linear array instead of a triangular format. The final set II items assessed conservation of length using stimulus rods of the same size as before. The new rods were red and green (instead of red and blue), and were now transformed into a perpendicular array that joined the separate colors to form a reversed "L". Set II was used in the training phase.

Set III was constructed like the others in terms of random presentation sequences, counterbalancing of item attributes, and types and numbers of items. The first stimuli measured conservation of two dimensional space with the same 2.8 cm. tiles as prior, but the transformed stimulus members now became a large, "hollow", square array. Next, conservation of length was assessed with stimulus rods of the same size as before, but green and orange in color. The transformed stimulus member produced an inverted "T", instead of the prior formats. The final quartet of items assessed number conservation with white styrofoam eggs and pink egg-cups. First, parallel arrays of eggs and cups were presented, and then one array was transformed by reducing separation among objects to diminish its spatial extent. The set III stimuli were used in the immediate transfer phase.

Procedures and Training Variations

All children were taken individually from class to a testing room for baseline. The verbal instructions for equality items were very similar to those of Goldschmid and Bentler (1968) which produced highly reliable estimates of conservation response according to several stringent psychometric criteria. For example: "Here are a red and a blue stick. They are both the same length. The red stick is just as long as the blue stick. Now watch. (The experimenter performed the transformation.) Now are both sticks the same or is one longer?" The inequality items required minor alterations. For example: "Here are a red and a blue stick. The red stick is longer than the blue stick. Now watch what I do. (The experimenter transformed the stimuli.) Now are both sticks the same length or is one longer?" On all items, after the child had made his judgment he was asked "Why?", and his explanations were recorded. Both the experimenter and the model (who took down all responses) were present throughout all procedures. If, in baseline, the child failed to give any correct judgments (ignoring explanations) on equality items, he was brought back for training on the next schoolday.

In the modeling only condition, children were told to watch while the model played the game. After the model gave a correct judgment and a correct explanation to an item of set II, that same item was presented to the child. On equality items, the model explained her judgment as follows: "because they were both the same length (had the same amount) in the first place." On inequality items, the model's explanations were as follows: "because it was longer (had more) in the first place."

No additional verbal guidance or directions were given to the children.

Verbal correction training combined positive feedback for acceptable answers with verbal rule-provision. In this training condition, children were treated in a manner similar to baseline, but when they judged correctly, they were told "that's right". When they judged an equality item wrongly, they were told "They may look different but they were both the same length (had the same amount) in the first place and they still are the same length (have the same amount)." When an inequality item was misjudged, the experimenter explained, "Whatever they look like, they don't have the same amount because (pointing) that one had more in the first place."

In the modeling plus correction condition, children first observed the model perform (as above). When the child attempted each modeled item, he was given positive feedback when right, and when wrong was instructed as in the verbal correction treatment. Control condition subjects simply received the set II items without observing the model or obtaining any verbal information about their responses.

Immediately after training, the set III stimuli were presented to each child to measure concept transfer. The experimenter introduced the task as follows, giving no further assistance: "Here are some objects for you to play the game with by yourself. I can't tell you if your answers are right or wrong, but try to make all your answers correct." After completing the generalization items, the child was shown a blue and a green rod of equal lengths. Next, the experimenter transformed the rods to create a sidewise "T", and elicited a length judgment from all children. If the child judged correctly, he was asked: "How would you show a friend that the sticks were still the same length?" This procedure was included to determine if the child could spontaneously reverse logical operations. Children typically gave no verbal responses, and to receive credit for reversing, the child had to place the rods parallel to each other. Following Piaget, Goldschmid and Bentler (1968) classified the ability to so reverse stimulus members as a qualitatively different basis for conserving than did the reasons given in the modeling and correction treatments, which were classified as invariant quantity explanations. After this test for logical reversibility, the child was thanked and returned to class with no mention of any subsequent interaction.

After a delay interval of seven to ten days, the adults returned and retested each subject with the set I (baseline) items. The retention test was introduced as follows: "You probably remember that we played some games a while ago. Today we are going to play some of these games again. Try to remember how to play the game. I can't tell you if you are playing the game right or wrong, but try to play it right." The set I stimuli were then presented as in baseline, and the child's judgments and explanations were recorded.

Scoring and Design

The children's responses to each of the main stimulus sets were scored as the number of correct judgments (judgments only), and also the number of correct judgments plus appropriate explanations (judgments plus rule) that the child could state. On equality items, explanations were scored precisely according to Goldschmid and Bentler's (1968) criteria.

On inequality items, the same criteria were applied as closely as possible. Credit was given to reasons that justified why one stimulus member was larger or smaller, e.g. "because the red stick was bigger (smaller) in the first place, etc." No credit was given to explanations that failed to refer to the initial, untransformed status of stimulus members, e.g. "because it looks bigger.", "because it has more, etc.". In any phase, a child's score was the number of the 12 items correctly answered. Since no a priori reason exists for assuming that understanding stimulus equality or difference necessarily presupposes ability to verbally justify one's judgment, following prior usage (Rosenthal & Zimmerman, 1972), the data were separately analyzed for the judgments only and the judgments plus rule dependent measures.

Goldschmid and Bentler (1968) suggested that conservation of length, number, and two dimensional space subscales (among several others) functioned as "measures of a general concept of conservation--like a general factor of factor analysis," and they provided strong psychometric evidence to support their interpretation. Thus, as previously, to avoid confining results of the present study to a particular subclass of conservation, all items were combined into a single overall response measure.

The main analyses involved a 2 (sexes) x 2 (model or no model) x 2 (verbal correction or none) x 4 (repeated phases) factorial design. At each phase, it was planned to compare the modeling plus correction condition with the better of the other two experimental variations, using orthogonal comparisons. All post hoc comparisons were made with Tukey HSD tests (Kirk, 1968). The spontaneous reversibility data were scored dichotomously as right (both correct judgment plus return of stimulus members to original, parallel position) or wrong, and were analyzed by Chi-square procedures. Chi-squares were also used to explicate the relationship between type of inequality item and "correct" baseline judgments.

RESULTS[2]

Before presenting the main results, it is important to consider the comparability of the three sets of stimuli used. Since many of the test items were especially constructed for this experiment, there was substantial departure from the tasks devised and standardized by Goldschmid and Bentler (1968). Restricting the present sample to children with zero baseline scores on judged equality produced severe curtailment of baseline score ranges, and the data from this phase were therefore excluded from correlational analysis. However, the variability among the children's scores (combining all 48 subjects) in the other phases permitted computation of meaningful Pearson coefficients. On the judgments only measure, set I correlated .92 with set II, and .86 with set III; between sets II and III, the correlation equaled .84. On the judgments plus rule measure, set I correlated .83 with set II and .89 with set III; between sets II and III, the correlation was again .84. Thus, despite numerous modifications, the conservation materials displayed very substantial alternate-form reliabilities across all three sets of stimulus items.

Judgments Only

Table 1 presents the mean judgment responses by phase for the separate experimental groups and for subjects combined on the basis of treatment (main effect) variations. The overall analysis of variance disclosed a substantial main effect for phases ($\underline{F}$ (3/119) = 46.87, $\underline{p} < .001$).

Insert Table 1 about here

By Tukey tests, the score increases from baseline to training, to transfer, and to retention phases were separately significant (all $\underline{p}$s $< .01$), with no significant conservation differences among the three latter phases.

Training though modeling ($\underline{F}$ (1/40) = 5.79, $\underline{p} < .02$) and through correction ($\underline{F}$ (1/40) = 28.67, $\underline{p} < .001$) both produced significant main effects. Sex of child did not affect net response or interact with any other variate, and the comparability of boys' and girls' judgments can be seen in Table 1.

Modeling interacted with phases ($\underline{F}$ (3/119) = 2.33, $\underline{p} < .03$). The modeling and nonmodeling treatments each increased significantly from baseline to training, transfer, and retention phases (all $\underline{p}$s $< .01$). The modeling and nonmodeling variations did not differ significantly at baseline or transfer, but modeling children surpassed their nonmodeling counterparts both during training ($\underline{p} < .01$) and retention ($\underline{p} < .05$). Similarly, correction interacted with phases ($\underline{F}$ (3/119) = 15.93, $\underline{p} < .001$). The correction treatment significantly exceeded its baseline mean at each later phase and also outperformed the noncorrection treatment in training, transfer, and retention (all $\underline{p}$s $< .01$), although the two conditions were comparable in baseline. The noncorrection variation exceeded its own baseline during training ($\underline{p} < .05$) and transfer ($\underline{p} < .01$), but not at retention. By orthogonal comparisons, the modeling plus correction group tended to outperform the next strongest (correction only) group in training ($\underline{p} < .07$) but not in the later phases. No other significant effects were obtained.

Judgments plus Rule

Table 2 presents the mean judgment plus explanation responses by phase for the separate experimental groups and for subjects combined on the basis of treatment variations. The main analysis of variance revealed a pattern of results generally similar to that for judgments only.

Insert Table 2 about here

Thus, there was a main effect for phases ($\underline{F}$ (3/119) = 18.92, $\underline{p} < .001$) that was supported by significant Tukey tests for the gains from baseline to each later phase (all $\underline{p}$s $< .01$), with no conservation differences among the three later phases.

Modeling ($\underline{F}$ (1/40) = 5.40, $\underline{p} < .03$) and correction ($\underline{F}$ (1/40) = 13.22, $\underline{p} < .001$) training both produced significant learning, whereas sex of child did not influence the results.

There was a marginal interaction between modeling and phases ($\underline{F}$ (3/119) = 2.52, $\underline{p} = .06$). Although the variations did not differ in baseline, modeling children surpassed their own baseline scores at each later phase (all $\underline{p}$s $< .01$), and outperformed the nonmodeling treatment during training ($\underline{p} < .01$), transfer ($\underline{p} < .05$), and retention ($\underline{p} < .01$). The nonmdoeling children increased significantly from baseline to transfer and retention (both $\underline{p}$s $< .05$), but not to the training phase.

Correction interacted significantly with phases (F (3/119) = 7.67, $p < .001$). Although the treatments were comparable at baseline, the correction children surpassed their noncorrection counterparts (and their own baseline scores) in each later phase (all $ps < .01$). In sharp contrast, the noncorrection children did not significantly improve from baseline to any later phase.

Orthogonal comparisons disclosed that the modeling plus correction group significantly outperformed the next-best, correction only group in training and retention (both $ps < .05$), although not during transfer. No other significant effects were obtained.

Logical Reversibility

For each group, the frequencies of children who did, and did not, correctly return the transformed rods to initial, parallel position are presented in Table 3. A significant relationship was found between training

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Insert Table 3 about here

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condition and the number of correct reversals (χ^2 (3 df) = 11.26, $p < .02$). Examination of Table 3 reveals that this result was largely created by the failure of control children to give any correct reversals. All experimental groups displayed some reversibility and the modeling only, correction only, and modeling plus correction groups did not differ significantly from each other. These results clearly indicated that the effects of training did not signify a slavish reproduction of the model's or experimenter's words. Instead the social learning techniques produced nonverbal behavior correct by a conservation criterion qualitatively different from any directly encountered.

Baseline Inequality Judgments

It is theoretically important to analyze the number and type of items judged correctly in baseline. To exclude children "in transition" between the "preoperational" and "concrete operations" periods, only subjects with zero baseline equality scores were retained for study. Further, no control child conserved any equality items after baseline, thus affirming the stability of the conservation items and making familiarization or maturation unlikely explanations of the results.

On the judgments only measure, all subjects combined passed an average of 3.31 inequality items in baseline, and untrained controls subsequently changed very little (see Table 1). Of special interest is the type of inequality solved in baseline. It will be recalled that six inequality items were given per phase. From counterbalancing constraints, half the inequality stimuli were so transformed that the actually greater quantity looked larger (veridical case). With no comprehension, from these visual, "gestalt" factors one wuld expect three correct inequality judgments per phase. If valid, this reasoning implies that the baseline inequality means obtained reflected virtually no pretraining conservation, an inference that can be tested by comparing baseline passes on veridical and nonveridical inequalities. Each child's correct baseline inequality judgments were separately categorized as follows: given just on veridical items; given just on nonveridical items; given to both item types; given to neither item type. For each conservation dimension, these data are presented in Table 4.

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insert Table 4 about here

The obtained frequencies were compared with the equal cell distributions that would be expected if baseline accuracy were unrelated to item type. The length conservation data revealed a systematic response pattern (χ^2 (3) = 55.32, $p < .001$); the clear majority (71%) of children only judged "correctly" when the actually larger stimulus was also perceptually larger. Similar results, and even greater Chi-square values, were found with conservation of number (χ^2 (3) = 107.49, $p < .001$) and two dimensional space (χ^2 (3) = 71.82). It thus appears that, before training, the children barely conserved unequal better than equal quantities. The superficial impression that more than three baseline items were correctly judged seems largely to have resulted from the perceptual properties of veridical inequality items, not from any really quantitative considerations. The baseline judgments plus rule data (see Table 2) also support this conclusion since all group means were below 0.5 correct responses, with no subsequent control group improvement. After training, however, the experimental groups displayed substantial conservation increases on both dependent measures.

DISCUSSION

The results revealed quite clearly that brief modeling techniques were successful in producing learning, generalization, and retention of conservation with both equal and unequal stimuli, and also when motor rather than just verbal behavior was required. The reversibility data further bear on the topic of inferential generalization. Not only did children transfer verbal judgments and reasons to new items, and to those originally failed in baseline, but they also were able to contingently perform a response (moving the reversibility materials back to original, pretransformation position) which was never directly trained. Obviously, the purely motoric accomplishment was hardly novel, but in so reversing the children spontaneously affirmed functional equivalence between their motor behavior and showing "a friend that the sticks were still the same length?." Somehow they made this coordination between the concept of equivalent lengths, and actually moving the rods, in a manner not readily explained by classical views of response generalization. In essence, they discriminated the idea of equality and the reversal actions as belonging together in a category of like events. These data provide further evidence that, in conceptual social learning, the child typically acquires information about abstract properties and relationships, rather than discrete stimulus-response connections, an issue elsewhere discussed (e.g. Rosenthal & White, 1972; Zimmerman & Rosenthal, in press).

Taken together, what do the previous and present research suggest about the phenomenon of conservation? It appears to respond to input operations in a fashion similar to other types of conceptual material: it can be instated by live and symbolic modeling and then, without further training, transferred to new stimuli and retained after some elapse of time. It does not seem necessary to confine training to a single conservation class, since three dimensions were concurrently treated at present and six were given before. It does not seem immutably dependent on the child's attaining some maturational, age-related cognitive stage: Both now and before, learning and transfer were found with children whose baseline conservation was nil, and the present control group continued to fail *every* equality item in the later phases, a result not consistent with spontaneous intellectual growth. Further, in the prior study, modeling successfully produced conservation judgments in a preschool sample whose mean age was only 4.6 years. Even brief exposure to well-organized modeling displays seems sufficient to establish stable conservation in children whose symbolic repertoires are adequate to process the information presented: Indeed, correction, the briefest training, combining positive feedback with verbal rule provision, appeared more efficient than live modeling, although the live modeling plus correction group generally performed best. However, when a sample of Chicano *barrio* children with limited English language skills was previously studied (Rosenthal & Zimmerman, 1972, Experiment III), observation of a live model was far more effective than a verbal instruction procedure.

In light of the children's rapid response to the environmental influences studied now and before, it seems reasonable to question assumptions holding that "logical" understanding is a product of innately-programmed growth processes. Is it more "logical" to subordinate perceptual differences in spatial arrays to their numerical attributes than to follow the reverse strategy? Or is such subordination the result of enculturation in a society which exemplifies and reinforces the dominance of quantitative over perceptual characteristics?

Cross cultural research on conservation appears to support the latter interpretation. Greenfield (1966), Maccoby and Modiano (1966), Goodnow (1962), Goodnow and Bethon (1966) have found that children raised in Western societies tend to display conservation behavior at an earlier age than children from Third World nations. These researchers have attributed their findings to the cultural experiences of the child. Acknowledging cultural factors is not incompatible with a Piagetian view which does allot a role to experience in thought formation if the child (a) has appropriate (i.e., concrete operational) schemata, or (b) is at least in transition between the preoperational and concrete operations stages. What does seem needed is some clearer criteria for defining "transition" and "schemata" in terms <u>independent</u> of accomplished conservation if the Piagetian view is to be given fair experimental test that permits its confirmation or refutation on the basis of empirical data.

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Footnotes

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2. All significance levels reported are based on 2-tailed probability estimates.

Table 1

Judgments Only Mean Responses by Phase for Intact Groups

and Treatment Combinations

Group	Phase			
	Baseline	Training	Transfer	Retention
Intact Groups				
Model no Correction	3.25	6.08	5.75	5.33
Correction no model	3.17	7.17	8.50	8.17
Model plus correction	3.67	9.08	8.08	8.92
Control	3.17	3.25	4.00	3.00
Treatment Combinations				
All modeling	3.46	7.58	7.42	7.13
All nonmodeling	3.17	5.21	6.25	5.58
All correction	3.41	8.13	8.92	8.54
All noncorrection	3.21	4.67	4.88	4.17
All boys	3.38	6.50	6.79	6.29
All girls	3.25	6.29	6.88	6.42

Table 2

Judgments plus Rule Mean Responses by Phase for Intact Groups and Treatment Combinations

Group	Phase			
	Baseline	Training	Transfer	Retention
Intact Groups				
Model no correction	0.25	2.25	1.83	2.25
Correction no model	0.08	3.08	3.33	3.92
Model plus correction	0.42	5.67	5.00	6.58
Control	0.00	0.00	0.08	0.18
Treatment Conditions				
All modeling	0.33	3.96	3.42	4.42
All nonmodeling	0.04	1.54	1.71	2.04
All correction	0.25	4.38	4.17	5.25
All noncorrection	0.13	1.13	0.96	1.21
All boys	0.21	3.13	3.17	3.79
All girls	0.17	2.33	1.96	2.67

Table 3

Reversibility Task Results by Group

Number of Children	Training Group			
	Modeling Only	Correction Only	Modeling Plus Correction	Control
Reversing Correctly	3	6	7	0
Not reversing	9	6	5	12

Table 4

Baseline Frequencies of Children's Inequality Judgments

Conservation Dimension	Inequality Item Response Categories			
	Passed Just Veridical	Passed Just Nonveridical	Both Types Passed	Neither Type Passed
Length	34	5	8	1
Number	43	1	4	0
Two dimensional space	37	2	8	1

Note: Veridical items were those on which the quantitatively greater stimulus looked perceptually larger. On nonveridical items, the actually greater stimulus looked smaller.