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ABSTRACT

In two experiments (N=210) conservers, transitional conservers, and nonconservers were directed to lie or pretend to other children that their judgments and explanations of a series of conservation problems were the opposite of what they really were. Nonconservers and transitional subjects in both studies made large and significant gains in conservation compared to appropriate control groups and according to pre- and post-test. Conservers did not regress. The second experiment, moreover, indicated that conservation gains were stable and that the newly acquired conservation was not extinguished by a second dissonance treatment in which subjects gave nonconservation responses. (Author/JMB)

Acquisition of Conservation Through Cognitive Dissonance

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Abstract

In two experiments (N=210) conservers, transitional conservers, and nonconservers were directed to lie or pretend to other children that their judgments and explanations to a series of conservation problems were the opposite of what they really were. Nonconservers and transitional subjects in both studies made large and significant gains in conservation compared to appropriate control groups and between pre- and posttests. Conservers did not regress. The second experiment, moreover, indicated that conservation gains were stable and that the newly acquired conservation was not extinguished by a second dissonance treatment in which subjects gave nonconservation responses.

Acquisition of Conservation Through Cognitive Dissonance

In How We Think (1910) Dewey conceptualized thinking itself as a directed problem solving activity that reduced conflict between conceptual elements and produced congruity and coherence in thought. Upon this notion was based a general class of psychological and pedagogical theories of cognitive motivation that stressed that cognitive inconsistency, or dissonance, or disequilibrium was for some reason motivating and that experimental or pedagogical procedures which induced such conflict or dissonance would induce cognitive change and growth. Roger Brown (1965, p. 604) has observed that the principle that human nature abhors incongruity, dissonance, or imbalance is one of the class of principles or explanations that seems always to have existed and consequently was never really invented. Indeed, intellectual coherence or consistency has been extended by some philosophers to be the very criterion for truth itself and a statement of the character of reality (e.g., Blanshard, 1939).

Equilibration is the mechanism Piaget has proposed for cognitive change, and while Piaget is quite clear (1961, 1964) that it is not a maturational, conditioning, imitation, or linguistic mechanism, it has never been clear just what the process is itself or how it might be linked to traditional psychological mechanisms. Bruner (1961) concluded that it was an unnecessary construct in Piaget's theory,

and Elkind (1967) claimed that it was not so much a separate mechanism as it was the balance or interaction between maturation, learning, imitation, and language. Piaget has quite generally described equilibration as a mechanism of self-regulation which maintains a balance between assimilation and accommodation, compensates for disturbances whether internally or externally initiated, and produces coherence in the system of schemes and the general accord of thought with itself (Mischel, 1971). In this general sense, Smedslund (1961) argued that equilibration may be similar to Festinger's cognitive dissonance or Heider's balance mechanisms. Mischel (1971) has viewed the "need for establishing cognitive consistency" as an interpretation of cognitive motivation that "is clearly consistent with Piaget's account of cognitive development as a process of equilibration (p. 331)." Indeed the recent Genevan operativity training procedures (Inhelder, Sinclair & Bovet, 1974) which ostensively simulate equilibration are based upon the premise that "disequilibria are expressed by a child as conflicts or contradictions" (p. 259) and that "two different answers to one problem . . . stimulates the subject [to seek] a certain coherence . . ." (p. 265). A conservation training procedure based upon a cognitive dissonance experimental paradigm (e.g., counter attitudinal role-playing, Insko, 1967) would be expected to be successful in advancing children from preoperational to concrete operational thought. Such was the rationale of the two experiments of the present study in which a classic cognitive dissonance procedure was used to train nonconservers to conserve. Dissonance in a subject has been predicted to result, for

example, from the presence of "two cognitions which psychologically do not fit together: one of these is the knowledge that he believes 'x', the other the knowledge that he has publicly stated he believes 'not x'" (Festinger and Carlsmith, 1959, p. 203).

Murray (1972), Silverman & Geiringer (1973) and Botvin & Murray (1975) have demonstrated that conservation can be acquired effectively as a result of a social conflict situation in which conservers and nonconservers confronted each other and were required to agree and give a simple group conservation judgment on a series of problems. While it was not clear at the time why the social conflict procedure was effective (Kuhn, 1974), it may have been effective because the nonconserver's acquiescence in the social confrontation induced a cognitive conflict or dissonance which motivated the subsequent cognitive growth.

Strauss (1972) reviewed the conservation training studies which attempted to provoke disequilibrium and the subsequent cognitive growth which presumably stems from it. He viewed these in two categories--"those which attempt to induce (a) external, adaptational disequilibrium by means of prediction-outcome conflict and (b) internal, organizational disequilibrium through structural mixture conflict (p. 33). The intention of the procedure in the present studies was, in a sense, to provoke a structural mixture conflict by an external cognitive dissonance inducing procedure.

The cognitive dissonance manipulation in the present two experiments was simply to have subjects pretend in the presence of another

child to give conservation judgments and reasons that contradicted those they gave on a pretest measure of conservation. After this dissonance inducing situation, which presumably introduced a conflict between what the subject truly believed and what he had publicly said he believed, subjects were questioned about what they truly believed. The first experiment was designed to determine how such a procedure would effect conservation and the second was designed to replicate the effect, whatever it was, and to determine its durability and resistance to extinction.

Dissonance theory would predict that nonconservers after telling the conservation lie publicly would come to believe their public position, and that conservers after publicly telling a nonconservation lie would also adopt their public position and therefore would regress to nonconservation. The prediction from a structural or organismic theory, such as Piaget's, would need to be more complex and cautious since the exact conditions under which the organism assimilates rather than accommodates are not clear. However, since conservation is a symptom of an equilibrated coherent state, namely the stage of concrete operations, and since structural development is unidirectional (Reese and Overton, 1970), the expectation would be that conservers would not regress and nonconservers would progress.

Method (Experiment I)

Subjects

There were 96 kindergarten, first, second, and third grade children with a mean age of 6.96 years (SD=.95) from the New York City public schools.

Procedure

Each child was individually pretested on eight traditional conservation tasks--two on the conservation of number of blocks, two on conservation of the mass of clay balls, two on the conservation of liquid amount, and two on the conservation of the weight of clay balls. The transformations for mass and weight were that one clay ball was rolled into a sausage shape or flattened into a pancake shape; for liquid amount one of two equal glasses of juice was poured into a taller and thinner glass or a shorter and wider glass; and for number one of two rows of five blocks was spread apart or pushed together. Subjects were scored one point for a correct judgment on each of the eight tasks and one point for an appropriate supporting reason (viz. identity, reversibility, compensation). On the basis of these pretest scores, subjects were divided into four groups of 24 subjects each: Nonconservers (score of 0), Transitional Conservers (scores 5-10), Conservers (scores of 16) and a Control group of an equal number of nonconservers and transitional conservers. Subjects with scores of 1-4 and 10-15 served as the observers in the second session of the experiment.

In a second session each group received directions and training to give conservation judgments and reasons which contradicted those they gave on the pretest. The Nonconservers (N=24) were told by the experimenter to give "conservation lies" or to pretend to conserve as follows:

> Hi. When you were here before you told me that when I did this (transformation), this glass would have more juice than that one. Remember? Okay. This time we're going to play a little game. I'm going to bring another boy (girl) in to listen while we do the same thing we did before . . . only

this time I want you to pretend that after I do this (transformation) there is still the same amount of juice in both glasses . . . And when I ask you why, I want you to say that they are the same because I didn't add anything or take anything away. Do you think you can remember that? We're going to do the same thing with all these (tasks) . . . with the clay, the juice, and the blocks. Okay? Good. Let's try it. We'll go through it so you can get the hang of it.

If a child made a mistake on any task, he would be told, "No, remember I want you to pretend that they're the same" or if the mistake was made on the reason he was told, "Remember, when I ask you 'why' you're supposed to say, 'they're the same because nothing was added or taken away.'" After the child had responded correctly to all the pretest conservation problems in a row, with a conservation response and reason (the conservation lie), he was questioned in front of another child on all the problems.

The conserver group (N=24) were told by the experimenter to give nonconservation judgments and reasons (the "nonconservation lie") as follows:

Hi. When you were here before you told me that when I did this (transformation), this glass (clay, etc.) would be the same as that one. Remember? Okay. This time we're going to play a little game. I'm going to bring another boy (girl) in to listen while we do the same thing we did before . . . only this time I want you to pretend that after I do this

(transformation) there will be different amounts of juice (clay, etc.) in the two glasses (balls, rows, etc.). Remember, even though you really don't believe it, I want you to tell the other boy (girl) that after I do this (transformation) there is more in this one than in that one. And when I ask you why, I want you to say that this one looks like it's more because it's higher (longer, bigger). Do you think you can remember that? Good. We're going to do the same thing with all these (tasks) . . . with the clay, the juice, and the blocks. Okay? Let's try it. We'll go through some so you can get the hang of it.

If a child made a mistake, the same directions were used with him as were used with the nonconservers.

One-half of the Transitional Group (N=12) received the instructions given to the Conserver Group and the other half (N=12) received those given to the Nonconserver Group which means that the responses they were taught to pretend did not always conflict with those they had given on the pretest since as transitional subjects they had conserved and nonconserved on some of the pretest problems.

In a third session approximately 15 minutes after the second session each child was individually posttested on the eight pretest problems, but prefaced with the following important instructions;

Now I want to find out what you really believe. We're not playing the game anymore. You can either tell me what you just told me or what you told me before. You can give me the same answers you gave me before the game or you can give me different ones. Okay? Just tell me whatever you really believe.

The Control Group subjects (N=24) received only the pre- and posttests.

Results (Experiment I)

The mean scores (0-16) for each group on the pre- and posttest appear in Table 1 and matched t tests between pre- and posttests indicated significant differences between these means for the Nonconserver Group (t (23) = 24.82, $p < .001$) and Transtional Group (t (23) = 2.80, $p < .02$). In the Transitional Group the pre-post test mean difference for those who told conservation lies was significant (t (11) = 14.03, $p < .001$) but it was not significant for those who told nonconservation lies (t (11) = 1.91, $p > .05$). Differences in pre-post test means were insignificant obviously for the Conserver Group, for the entire Control Group (t (23) = .80, $p > .05$), and within the Control Group for the nonconservers (t (11) = 1.47, $p > .05$) and transitional conservers (t (11) = 1.47, $p > .05$). These constitute the principal results of the experiment; however a detailed treatment of the responses of the transitional conservers follows and is important.

Insert Table 1 About Here

Within the transitional Group the difference in mean posttest performance between those who told conservation lies (C lies) and those who told nonconservation lies (NC lies) was significant (t (10) = 3.10, $p < .02$).

The presence of regression from conservation to nonconservation between pre- and posttests occurred only on problems when the transitional subject's nonconservation lie conflicted with the pretest

judgment and on the problems solved by the control transitional. The differences in proportions of improvement, regression, and no change in performance on problems between pre- and posttests were not significant between problems solved by the subjects in the control group and subjects whose nonconservation lie conflicted with the pretest (χ^2 (2) = 2.18, $p > .05$).

Across all transitional subjects who told conservation lies there were 40 problems on which there was a conflict between the conservation lie and pretest performance, and of these there was an improvement in conservation performance (0 to 1 or 1 to 2) on 38 of the problems. This was a significantly greater proportion of improvement than occurred on the problems in any other problem condition (C lie no conflict, χ^2 (1) = 31.85, $p < .001$; control, χ^2 = 29.6, $p < .001$; NC lie conflict, χ^2 (1) = 31.74, $p < .001$; NC lie no conflict, χ^2 (1) = 23.17, $p < .001$).

When simply the change in performance itself, regardless of direction, between the pre- and posttests were considered, it was found that there was a significantly greater proportion of change from pre- to posttest on problems where the lie conflicted with the pretest judgment than where it did not (χ^2 (1) = 8.36, $p < .01$) and than where no lie was told at all (control, χ^2 (1) = 5.26, $p < .05$). When the conservation lie did not conflict with the initial judgment, the proportion of problems on which there was change in conservation was not significantly greater than it was in the control condition (χ^2 (1) = .46, $p > .05$) nor greater than it was in the condition where nonconservation lie did not conflict with initial judgment (χ^2 (1) = .17, $p > .05$).

Discussion (Experiment I)

It seems clear that the effects of the dissonance procedure were large and that nonconservers and some transitional conservers made significant gains in conservation (judgments and reasons) while controls did not.

The results are only partially consistent with a simple cognitive dissonance interpretation of the effect since conservers who gave nonconservation lies did not regress at all in the direction of nonconservation as they were expected to. While some regression did occur among the transitional subjects who gave nonconservation lies, it was not significantly different from the normal regression or vacillation that occurred between pre- and posttests among the control transitional subjects. The results are consistent with the notion that cognitive conflict between true belief and pretended belief can motivate cognitive change and development. The change that results appears to be, for some reason, unidirectional and thus a major Piagetian thesis of no regressive change in intellectual development is preserved.

There is the question of whether the gain in conservation was due to some feature of the conflict itself or simply to the subject's recognition that the information in the conservation lie was true, i.e., some kind of verbal rule instruction could be presumed to be operating in the procedure. The data from the transitional subjects bear on this question and indicate that the benefit from subjects' pretending to believe the conservation lie was tied to the presence of a conflict between the lie and the subject's true belief at least as measured by pretest performance. The data on the proportion of the transitional

subjects' responses which changed from pre- to posttest indicated that the proportion of changes when the conservation lie did not conflict were not different significantly from the proportions in the control group. By the same token, it is not simply the presence of a conflict or dissonance itself that produced or motivated change from pre- to posttest because the conflict produced by the nonconservation lie did not produce gain that differed significantly from the control group either.

There remain the important problems of whether the conservation gains were genuine; were they, for example, stable and were the newly trained conservers as resistant to extinction from the dissonance of a nonconservation lie as "natural" conservers seemed to be? These were the primary questions of the second experiment. As well it was thought that the natural conservers could be seduced away from conservation in the dissonance manipulation if the pretended nonconservation reason was more compelling than the one used in the first study. For this reason a "subject-matter" nonconservation justification was introduced along with the perceptual nonconservation reason (it looks heavier, etc.) used in the first study. The subject matter reason simply was that the transformation, whatever it was, really changed the property of the object in question (e.g., its weight, length, etc.).

Method (Experiment II)

Subjects

There were 114 subjects from the Newark Day Nursery in Newark, Delaware, and the kindergarten, first, and second grades of the St. John the Beloved School in Wilmington, Delaware. The mean age was 6.19 years (SD = 1.04) and there were 52 boys and 62 girls.

Procedure

There were three sessions for the subjects in this experiment. The first was a pretest on four conservation tasks--length in which one of two equal plastic rods was moved to the right of the other; mass and weight in which one of two identical clay balls was flattened; discontinuous quantity in which corn kernels from one of two identical cups was poured into a bowl that was lower and wider than the cup. Subjects were individually tested and the order of the tasks was random. Subjects were scored, as before, one point for a correct judgment and one for an adequate explanation. With four tasks a perfect score was 8. Subjects with scores of 0 were randomly assigned to the Nonconserver Group (N=30) and Control Groups (N=21); those with scores of 3, 4 and 5 were assigned to the Transitional Conserver Group (N=28) and those with scores of 7 or 8 were assigned to the Conserver Group (N=35). The remainder of the subjects, as before, were used as observers in the dissonance sessions.

In the second session all subjects except the controls were told to give responses that were generally contrary to their answers on the pretest.

Subjects in the Nonconserver Group were told

> Last time you gave me answers to the questions I asked you. Today I am going to tell you some answers to give to the questions in front of [Billy]. These answers may not be the ones you really believe, but I would like you to pretend in front of [Billy] that they are your answers, that you really believe them. Do you understand? Okay, let's try it. I am going to

change one of these clay balls.like this (transformation). Then I will ask you if it has less clay than that one (the untransformed ball), more clay or the same amount of clay. I want you to say in front of Billy that it has the same amount. Okay? Then I will ask you why and I want you to say, 'It is still has the same amount (length, weight, etc.) because you did not add any or take any away, just changing the shape does not change the amount.' Okay? What are you going to say?

This form of instruction was repeated for each of the conservation problems and when, as before, the child had mastered the "conservation lie" on each problem in a row, the observer was called into the room. After the subject had responded to the four tasks in front of the observer, the observer was dismissed and the subject was told, " Now I want you to tell me what you really believe about these objects" and the pretest was repeated. This was the first posttest.

Subjects in the Conservation Group were told

Last time you gave me answers to the questions I asked about these objects. Today I am going to tell you some different answers to give in front of [Billy]. These may not be the answers you believe, but I want you to pretend in front of [Billy] that the answers you give are the ones you believe. Okay? Let's try it. Now I'm going to do this to the ball (transformation). Then I will ask you if it has more, less, or the same amount of clay as the other ball. I would like you to say that it has more clay and give this reason.

Seventeen subjects were asked to give a perceptual reason which was, "This one (E pointed to transformed object) was heavier (longer, etc.) because it looked heavier (longer, etc.)", and 18 subjects were asked to give a subject matter reason which was that the transformation made the object different with respect to the property in question (viz., "flattening makes it have more clay" or "flattening makes it weigh more," or "pouring makes more corn," or "moving it makes it longer"). After this the same procedure used for the first posttest with the Nonconserver Group was followed.

As before some of the Transitional Group, namely 12 children, followed the instructions given to the Nonconserver Group and 16 children followed the instructions (with subject-matter reasons) given to the Conserver Group.

The Control Group was divided into two parts; one part (N=11) followed the pattern of the first experiment and received only the pretest and posttests as control for any growth in conservation that could be attributed to retesting. The other part (N=10) was used as a control for the effect of simply an observer's presence might have on conservation performance. These subjects were told, "Last time you gave me some answers to questions I asked you about these objects. Today I would like you to say your answers again in front of [Billy]." After this the observer was dismissed and the posttest tasks were repeated as they were with the experimental groups.

Seven or eight days after the second session, the third session took place in which all subjects were posttested (Posttest 2) on the four conservation tasks used previously. In addition, conservation of number (one of two rows of six poker chips was spread out) and conservation of area (one of two large squares composed of nine smaller cardboard squares was rearranged) was assessed. The order of four posttest tasks and the

area (one of two large squares composed of nine smaller cardboard squares was rearranged) was assessed. The order of four posttest tasks and the additional tasks was randomly different for each child.

In the third session, all experimental subjects were then given the instructions which had been given earlier to Conserver Group in the second session; that is they were directed to give nonconservation lies. Those in the Conserver Group were instructed to give the same kind of reason, perceptual or subject matter, that they had given previously. All the Transitional Conserver Group were instructed to give subject matter reasons. as were one helf of the Nonconserver Group (the rest gave perceptual nonconservation reasons). Otherwise the same instruction and posttest procedure used in second session for the experimental and observer control subjects was followed in this part of the third session.

In sum, experimental subjects were pretested, subjected to a dissonance procedure, and posttested (posttest 1), posttested after a week's delay (posttest 2) and subjected to another dissonance procedure in which only the nonconservation lie was told, and finally posttested (posttest 3). Control subjects received all the posttests and some in addition answered the problems before an observer in the second and third session.

Results (Experiment II)

The mean scores for each group on the pretest and each of the three posttests are given in Table 1. A one-way analysis of variance on repeated measures indicated that there were no significant differences among the mean scores for the Control Group (F (2,40) = 2.12, $p > .05$, omega w^2 = .06) and for the Transitional Group who gave the nonconservation lie (F (3,33) = .78, $p > .05$, omega w^2 = .03).

Differences between the means in Table 1 were significant in the Nonconserver Group (F (3,87) = 58.74, $p < .001$; ω^2 = .46) and Transitional Group who gave the conservation lie (F (3,45) = 183.25, $p < .001$; ω^2 = .89). In each group, Dunn's planned multiple comparison test (Marascuillo, 1971) indicated that only the differences between the posttest means were insignificant in each group.

Due primarily to restricted variance of near ceiling performance in the Conserver Group, ANOVA indicated a significant difference between the means for the Conserver Group (F (3,102) = 5.34, $p < .01$; ω^2 = .04). The magnitude of the ω^2 measure of the strength of the association (Marascuillo, 1971) indicated however, that very little of the variance was explained by the treatment. The Dunn's test indicated, moreover, that the only significant difference between the means was between the pretest and the first posttest ($p < .05$) and not between any others.

All treatment means in Table 1 were significantly different by t test from the pooled Control Group mean on each test, but the comparisons of appropriate interest were between the Control Group and Nonconserver Group (post 1 t (49) = 9.07, $p < .001$; post 2, t (49) = 9.83, $p < .001$; post 3, t (49) = 9.16, $p < .001$; and transfer test, t (49) = 6.24, $p < .001$).

The differences in mean score on each posttest between those Conserver Group subjects who gave perceptual reasons and those who gave subject-matter differences were insignificant, although in each case the mean score for the subject-matter reason was lower than that for the perceptual reason (post 1, t (33) = .565, $p > .05$; post 2, t (33) = .942, $p > .05$; post 3, t (33) = .23, $p > .05$). This exact result held also on final posttest for the Nonconserver Group (t (28) = .493, $p > .05$) in which some subjects had given

perceptual and some had given subject matter reasons. There was considerably more regression from conservation to nonconservation on problems among the transitional conservers who gave nonconservation subject matter lies in Experiment II than there was in Experiment I among transitional conservers who told perceptual lies (χ^2 (1) = 7.31, $p < .01$). Of the 29 responses to problems on which the nonconservation subject matter lie conflicted with the pretest from the transitionals, 13 were regressive on posttest 1, 14 on posttest 2, and 18 on posttest 3 in Experiment II.

Within Transitional Group those who told the conserver lie scored higher than those who told the nonconservation lie on posttest 1 (t (26) = 3.59, $p < .01$) and posttest 2 (t (26) = 4.53, $p < .001$) and also on posttest 3 (t (26) = 5.20, $p < .001$) although of course differences in the pretest means between these two groups were insignificant (t (26) = .919, $p > .05$). On some problems the lie could conflict with the initial pretest judgment or agree with it. On problems on which there was a possibility of improvement or regression in score, the mean gain was computed for each subject on these problems when there was conflict and agreement between the lie and initial judgment. The means of these gains for each condition are presented in Table 2. A score of 1 on the pretest was taken to indicate

Insert Table 2 About Here

conservation and a score of 0 indicated nonconservation for the purposes of this analysis of gain and loss in scores as a result of conflict or no conflict. The mean gain in conservation on each posttest when the conservation lie conflicted with pretest judgment was significantly greater than

gains made under any other condition (C lie no conflict post 1, t (13) = 7.11, $p < .001$; post 2, t (13) = 6.48, $p < .001$; post 3, t (13) = 6.27, $p < .001$; NC lie no conflict, post 1, t (23) = 5.17, $p < .001$; post 2, t (23) = 5.38, $p < .001$; post 3, t (23) = 5.43, $p < .001$; and NC lie conflict, post 1, t (24) = 8.51, $p < .001$; post 2, t (24) = 8.85, $p < .001$; post 3, t (24) = 10.20, $p < .001$). There were no significant differences in mean gain between the two no conflict conditions on each posttest. However, under the subject matter nonconservation lie conflict condition there was significant loss or regression in score compared to the two nonconflict conditions (C lie, post 1, t (20) = 2.17, $p < .05$; post 2, t (18) = 3.85, $p < .01$; post 3, t (20) = 3.87, $p < .001$; NC lie post 1, t (11) = 4.37, $p < .01$,; post 2, t (11) = 4.11, $p < .01$; post 3, t (11) = 6.60, $p < .001$) and the other conflict condition has been reported.

Ninety-four percent of the reasons the Conserver Group gave on the pretest and 79%, 82%, and 79% of the reasons on the respective posttests were identity reasons (viz., that the transformation did not change the property in question). The proportion of identity reasons on the pretest was significantly greater than on any posttest (post 1, χ^2 (1) = 13.42, $p < .001$; post 2, χ^2 (1) = 7.93, $p < .01$; post 3, χ^2 (1) = 12.81, $p < .001$), but the proportion did not differ significantly among the three posttests (χ^2 (2) = 0.98, $p < .05$). Virtually all the conservation reasons the Nonconserver Group gave on the posttests were identity reasons, but these were 64%, 72%, and 65% respectively of all the reasons they gave on the posttests. These proportions of identity reasons for the Nonconserver Group did not differ significantly on the posttests (χ^2 (2) = 2.29, $p > .05$).

Subjects in the Conserver Group who told subject matter lies gave significantly more subject matter reasons and fewer perceptual reasons on th posttests than did those who told perceptual lies (χ^2 (1) = 12.93, $p < .001$). Transitional subjects who told the subject matter lie gave significantly more subject matter reasons and fewer perceptual reasons on each posttest than they did on the pretest (post 1, χ^2 (1) = 6.84, $p < .01$; post 2, χ^2 (1) = 8.35, $p < .01$; post 3, χ^2 (1) = 7.46, $p < .01$).

Discussion

Experiment II replicated the main findings of the first experiment and showed, moreover, that the gains in conservation as a result of pretending to conserve were not temporary and were themselves quite resistant to extinction by a dissonance treatment. In this latter respect, the trained conservers and the natural conservers were the same. They were the same also in the kind of reason they gave when they conserved. There was also evidence that compared to the control group at least the gains in conservation made by the Nonconserver Group transferred to problems on which they were not trained.

Natural conservation proved quite resistant to extinction by two dissonance treatments. Although the small decline in conservation scores between the pre- and first posttest (not the other posttests) was probably artifactual, there was a significant decline in the proportion of identity judgments between the pre- and posttests, but not across the posttests themselves. Conservers and Transitional Conservers seemed to be influenced somewhat by the subject matter conservation lie in that on the few occasions when they failed to conserve they gave it as the supporting reason for their

judgment. Also there was, compared to Experiment I, significantly more regression among the transitional conservers who gave the subject matter lie than among those who gave the perceptual lie. In sum, the subject matter lie appeared to be particularly seductive among the transitional conservers.

The behavior of the transitional subjects demonstrated, as before, that the greatest gains occurred when the conservation lie conflicted with initial nonconservation. However, the interpretation of the result must be tempered by the fact that this condition allowed the greatest gain; even so the gains when the conservation lie did not conflict were no greater than those when the nonconservation lie did not conflict. Moreover, the gains when the conservation lie did not conflict did not maximize the gains available. If the subjects were simply extracting information from the conservation lie itself, then gains from the conservation lie should have been maximal both in conflict and nonconflict conditions. However, only about half the available gains were made by the transitional subjects in the nonconflict condition whereas all the available gain was made by them in the conflict situation, which would indicate that for some reason the conflict was the important factor in acquisition.

The issue of whether nonconservers were simply parroting the conservation lie as the reason on the posttests is not clear because almost the exclusive conservation reason (viz, identity) given later by the initial nonconservers was one that occurred in the dissonance treatment, but it was also the one the natural conservers gave spontaneously. Indeed, there is evidence that the identity reason is common in beginning conservers

(Hammel, 1972; Botvin & Murray, 1975; Murray, 1972). The conservation lie reason consisted of an identity component and a "nothing was added or subtracted" reason component. The later component was cited only once by a subject which would suggest that more than direct copying of the "lie" occurred as a result of the dissonance procedure.

In sum, there is little reason not to think that the nonconservers acquired genuine conservation as a result of the dissonance or equilibration procedure and that the result of the disequilibrium was a progressive change.

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Table 1

Mean Conservation Scores for Conservers, Transitional Conservers, Nonconservers and Control Subjects on Pre- and Posttests Before And After Conservation (C lie) and Nonconservation (NC lie) Lies

Experiment I	Conserver	Transitional		Non-conserver	
(N=96)	NC Lie	C Lie	NC Lie	C Lie	Control
Pretest (0-16)	16.00	7.41	6.67	0.00	3.70
Posttest (0-16)	16.00	15.50	10.16	14.50	4.79
Experiment II ((N=114)					
Pretest (0-8)	7.91	4.25	4.00	0.00	0.00
Posttest 1 (0-8)	7.11	7.56	3.83	5.70	0.33
Posttest 2 (0-8)	7.40	8.00	3.66	5.93	0.45
Posttest 3 (0-8)	7.20	7.87[a]	2.66	5.50[a]	0.00
Transfer (0-4)	3.30	3.40	1.80	2.30	0.40

[a]After NC lie

Table 2

Mean Gain or Loss in Conservation Score Between Pre- and Posttests For Transitional Subjects (Maximum Gain is 2.0 for Conflict with NC and 1 with C)

	Conservation Lie (N=16)			Nonconservation Lie (N=12)		
	Post 1	Post 2	Post 3	Post 1	Post 2	Post 3
Conflict	2.00	2.00	2.00	-.66	-.66	-.97
No Conflict	.47	.54	.54	.68	.63	.59