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ABSTRACT

Four experiments focused on ways children could be trained to imitate others in imposing on themselves higher performance standards in game situations. The study also attempted to determine whether this internal achievement motivation behavior would be transferred by the children to situations such as learning in a classroom. The subjects were 122 male and 119 female elementary school children in grades two through six. The experiments involved children observing a peer model exhibiting high or low standards of self-reward in a novel game after which the subjects played the game, or subjects observing peer models choosing either difficult or easy goals in a novel athletic game after which the subjects played the same game, and, finally, subjects being given training in self-monitoring and simple goal-setting. Substantial persistence of the effects of exposure to the models and generalization of these effects to a new game was demonstrated. Subjects who had seen a model prefer more difficult goals had, themselves, chosen more difficult goals. Exposure to self-monitoring training produced an interest in achievement, although the goal-setting procedures had no effect on either study behavior or achievement. The study suggests that even relatively brief systematic attempts to affect children's goal-setting and self-monitoring behavior have significant beneficial effects, and that investigation into the adaptation of such techniques to educational contexts seems highly worthy of further pursuit. (Author/MS)

FINAL REPORT

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Generalized Effects of Modeled Self-Reinforcement Training

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ABSTRACT

This project has involved the pursuit of a series of empirical investigations of the generalized effects of training procedures designed to alter children's self-imposed standards and self-monitoring abilities. The intent, procedures, and outcomes of these four studies, summarized briefly below, are described in more detail in the remainder of this report.

Experiments 1 and 2 examined the persistence and generalization of effects of exposure to modeled self-reinforcement standards. Children observed a peer model exhibiting either a High or Low Standard for self-reward at a novel game, or saw no model. Subjects then played the game, either with or without specific instructions to follow the model's example. Two weeks later, in a new situation, subjects played either the same game or a different game. In these subsequent sessions, substantial persistence of the effects of exposure to the model and generalization of these effects to a new game were demonstrated. Initial differences between the two modeling conditions and between instructed and noninstructed subjects, however, did not generally persist in this later test.

Experiment 3 explored the generalization of changes in children's preferences for easy or difficult goals, induced by exposure to peer models, to novel subsequent situations. Elementary school subjects in two modeling conditions observed a peer model play a novel athletic game, choosing consistently either difficult or easy goals for himself, while control subjects saw no model. Immediately afterwards, subjects played this same game themselves. Three weeks later, subjects participated in a "spelling bee" in which they could select the difficulty level of the words they wished to attempt. Two months later, in their regular classrooms, subjects were offered a choice of puzzles of differing levels of difficulty by their teachers. During the first session, subjects who had seen a model who had preferred difficult goals themselves chose more difficult goals than subjects who had seen either a model who preferred easy goals or no model. Three weeks later, this same pattern of results was evident in subjects' choices of spelling words. Two months later, similar, though weaker, effects appeared in girls' (but not boys') choices of puzzle difficulty in their classrooms.

Experiment 4 investigated the effects of training in self-monitoring and goal-setting skills on classroom study behavior and academic achievement among elementary school students in an individualized mathematics program. In Self-Monitoring Conditions, students were shown a simple system for observing and maintaining daily records of their own study behavior during their math classes; orthogonally, in the Goal-Setting Conditions, students were shown a simple method of setting and recording daily performance goals during their math classes. Exposure to self-monitoring procedures produced an increase in appropriate study behavior, and for subjects with high initial motivation, a concomitant increase in achievement in the mathematics program; while exposure to goal-setting procedures had no effect on either study behavior or academic achievement. Nor, contrary to expectation, did exposure to the goal-setting instructions enhance the effectiveness of the self-monitoring system.

EXPERIMENTS 1 AND 2

Generalization and Persistence of Effects of Exposure to Self-Reinforcement Models

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Recent years have witnessed a growing recognition of the incompleteness of models of human behavior based solely on an examination of environmental response contingencies, and a concomitant expansion of interest in the dynamics of self-control and the processes by which people impose contingencies upon themselves (Bandura, 1971; Kanfer, 1971; Mischel, 1973). Increasingly, evidence from both theoretical (Bandura, 1971; Kanfer, 1971) and applied (Thoreson & Mahoney, 1973; Mahoney & Thoreson, 1974) contexts suggests the importance of the manner in which individuals set standards for their own performance. Evaluation with respect to these self-imposed demands may often be at least as significant a determinant of their behavior as the external contingencies imposed upon them in a particular situation.

One consequence of this heightened interest in self-control processes has been the development of a considerable literature concerning the effects of modeled patterns of self-reward on children's adoption of stringent or lenient patterns of self-reinforcement (Bandura, 1969, 1971; Bandura & Kupers, 1964; Bandura & Whalen, 1966; Bandura, Grusec, & Menlove, 1967; Liebert & Allen, 1967; Liebert & Ora, 1968; Liebert, Hanratty, & Hill, 1969; Mischel & Liebert, 1966; Grusec, 1971; Masters & Mokros, 1974). Typically, in these studies, children are exposed to a model who plays a novel game, adopting either a very stringent standard or a relatively lenient standard for self-reinforcement at this task. On each trial that the model's performance exceeds his standard for self-reinforcement, the model rewards himself with either candy or tokens exchangeable for attractive prizes, and may additionally verbalize positive self-evaluations or the reasons why he felt that he had earned a reward. Subsequently, the children play this same game alone, receiving a predetermined series of scores. The trials on which subjects reward themselves then provide a measure of the child's adoption of the modeled self-reinforcement standard.

These studies have repeatedly demonstrated that children can be quite easily induced to adopt surprisingly strict standards of self-reward at a novel activity by a model who has demonstrated a high performance standard on the experimental activity. In addition, the effectiveness of such modeling procedures in promoting the adoption of high performance standards appears to vary as a function of a number of variables, such as the prior nurturance and the power of the model, the structure of the rules verbalized by the model, and the incentive value of the rewards available to the subject, which have been shown to effect the amount of imitation children will show in many experimental situations (Bandura, 1973).

While these studies provide strong evidence of the importance of modeling in children's acceptance of stringent performance standards in these experimental situations, however, the fundamental question which this research leaves unanswered is the extent to which such training procedures have any relevance for children's behavior beyond the immediate situation and activity on which the child was "trained" -- in short, the question of generalization.

Several authors, for example, have suggested that the effects of such modeling procedures may be simply to teach the child that there are certain "rules" or "norms" by which a particular novel game is played (Kuhn & Langer, 1968; Kohlberg, 1969; Hoffman, 1970; Masters & Driscoll, 1971), suggesting implicity that these procedures should have little effect on behavior in different situations with different tasks. One might well argue, of course, that a primary strategy a child will employ in adjusting to any new or ambiguous situation is precisely one of trying to figure out what is expected of him in that setting, and then complying with these perceived expectations (Silberman, 1970; Parton & DeNike, 1966; Turnure & Zigler, 1964). Clearly, however, generalization of the effects of such training procedures to other situations represents an issue of both theoretical and practical significance.

The two experiments reported in this paper represent a preliminary attempt to investigate the generalization of the effects of exposure to models exhibiting either very strict or more lenient standards of self-reward, by examining children's self-reinforcement behavior both immediately following exposure to the model, and some weeks later in a different situation with either the same or a different, though similar, experimental activity. In doing so, these studies attempt to extend and to clarify the implications of previous experimentation relying on immediate subsequent measures.

EXPERIMENT 1

The design of Experiment 1 was, therefore, quite straightforward. In an initial session, children in the modeling conditions were presented with an explanation of the mechanics of a novel game and were shown a peer model who played this game, rewarding herself either for the highest possible scores only (High Standard Conditions) or for both the highest and next highest scores possible (Low Standard Conditions). In the control condition, children were exposed to the explanation of the mechanics of the game, but were not presented with a model. All children were then given an opportunity to play this same game, in the experimenter's absence, and to reward themselves with pennies when they felt their performance deserved a reward. Unobtrusive measures of the performance standards children adopted in this situation provided data comparable to previous studies in this area.

Three weeks later, the children were brought to a different setting by a different experimenter to play either the same game they had previously played or a different, though somewhat similar, game they had not seen before. Unobtrusive measures of children's self-reinforcement standards were again obtained, to assess the persistence and generalization of the effects of the modeling procedures observed in the initial session.

Method

Subjects.

The subjects for Experiment 1 were 69 second and third grade children attending the Ladera Elementary School in Menlo Park, California. The children ranged in age from 7-0 to 9-1, with a mean age of 8-0. The sample included 37 males and 34 females, from a predominantly white, middle-class population. Eight additional subjects were lost from the study, five due to equipment failure and three who were unavailable for the second session.

Apparatus and Experimental Setting.

Session I of the experiment was held in a research trailer, divided into three rooms. The center room of the trailer served as the observation room and contained two one-way mirrors which looked into the two experimental rooms Each of the experimental rooms contained a carousal slide projector and a cassette tape recorder, for presentation of the modeling treatments. In addition, each experimental room contained one of two experimental games -- a "bowling" game and a "pinball" game.

The bowling game was a modification of that used by Bandura and Whalen (1966) and consisted of a miniature bowling alley with a 3 foot runway with an upright scoreboard at the end, facing the bowler. The scoreboard contained thirteen signal lights arranged in a pyramid. Each light was labeled with a score from 1 to 4, with the bottom row of lights worth "1", and the second row of lights worth "2", and so forth. On each shot, the ball would disappear from view, one of the scoreboard lights would indicate the subject's score, and the ball would return to the subject.

The pinball game was approximately 4 feet long and 2 feet wide and consisted of a large, glass-covered-tilted playing board containing 4 pairs of different colored vertical partitions through which a ball could pass. The ball was put into play by pulling and releasing a spring lever, pushing the ball onto the playing board. At the head of the game, opposite the player, was an upright scoreboard with the score "5", "10", "15", and "20" printed across it. After each shot one of these numbers would light up to indicate the subject's score for that shot. Both games were preprogrammed to yield a single series of scores for all subjects. Universally the games seemed both appealing and credible.

Also attached to each of the games was a penny dispenser, consisting of a 7-inch square box from which pennies were ejected singly, at the press of a button, through a small coin-sized slot at the bottom of the box. Both games and penny dispensers were connected to an Esterline-Angus event recorder inside the observation room, which recorded unobtrusively the number of pennies a child took following each shot.

Session II of the experiment was held in a large workroom in the school. The experimental games were arranged so that only the game in use was visible to the subject. Subjects' self-reward behavior was again recorded unobtrusively on a hidden Esterline-Angus event recorder.

Session I

For Session I, subjects were brought individually to one of the rooms in the research van, to play one of the two games. Before the child was given a chance to play the game, however, he was shown an automated slide presentation, with a taped sound track, which showed a peer model being introduced to, and, in the experimental conditions, playing the game.

Each slide show lasted approximately ten minutes, and began with an adult explaining the mechanics of the game, the value of each of the four possible scores, and the use of the penny dispenser. In the No Model, Control condition, the presentation was terminated following this introduction. In the two experimental conditions, the adult in the slide show then left the peer model, to whom the game had been explained, alone to play the game by herself. The model was told that she could take a penny whenever she felt her performance deserved a reward.

In the High Standard condition, the model rewarded herself with a penny only when she obtained the highest possible score (i.e., either a "4" on the bowling game or a "20" on the pinball game); in the Low Standard condition, the model rewarded herself for either of the two highest scores possible (i.e., "3" and "4", or "15" and "20"). On each trial, in addition, the model verbalized a contingency between the score she obtained and her decision to reward herself or not (e.g., "That's the best score. I sure deserve a penny for that one." vs. "That's an O.K. score, but it's not good enough to deserve a penny."). The model's responses were identical in both conditions, except when she obtained the second highest score.

Following the slide presentation, the experimenter indicated that the subject would now have a chance to play the game by himself, since the experimenter had work to do in another room. The experimenter then rehearsed with the child the mechanics of the game and the penny dispenser, and indicated to the child that, after each shot, he could take a penny if he thought he deserved one. The child was given a small opaque envelope for the pennies he earned and was told that the experimenter would return only when the child rang a signal bell after he had finished playing. The experimenter then left the room.

Each subject was allowed 20 trials on the game, and all subjects received the same sequence of scores, including six each of the two highest scores and four each of the two lowest scores. Records of subjects' self-reinforcement behavior were obtained made on a concealed Esterline-Angus event recorder. To assess the accuracy of this procedure, subjects' self-reinforcement behavior was also recorded by an observer behind a covered one-way mirror. The comparability of these two records (r = .96) indicated the adequacy of the automated recording procedure.

Session II

About three weeks following Session I (mean interval = 23 days), these same subjects were brought by a different experimenter, "blind" to the subject's treatment condition, to a different location -- a general-purpose workroom located in the school complex itself -- to maximize the separation of the two sessions.

During this second session, half of the children within each condition were retested on the same game they had played and had observed the model play in Session I. The remaining subjects were tested on whichever of the two experimental games they had not been exposed to in Session I, to provide a test of the generalization of subjects' self-reinforcement standards. Subjects were not exposed to a model. Instead, the experimenter simply explained the mechanics of the game and the values of each of the scores at that game, and then left the subject to play the game alone. As before, subjects were told thay they could reward themselves with a penny after each shot, when they felt that their score deserved a reward, and were given an opaque envelope in which they could put the pennies they earned. The experimenter then left the room.

As in Session I, each child was allowed twenty trials at whichever game he played, and received the same number of successes and failures as before. The pennies taken on each trial were recorded by the concealed event recorder. When the subject finished and signaled the experimenter to return, the experimenter administered a brief verbal questionnaire, to measure the child's attributions of luck and skill at the game, his "strategy" for winning (if any), his memory of the model's behavior in Session I, and his liking for the games. At The end of the experiment, children were given their pennies.

Results

Since the number of pennies taken after each trial was recorded, subjects' self-reinforcement behavior following each of the four possible scores could be analyzed. Perfect adherence to the modeled High Standard would have resulted in a subject's taking pennies on six trials; perfect adherence to the modeled Low Standard would have resulted in a subject's taking pennies on twelve trials At each level of success, therefore, the data were analyzed in terms of two orthogonal contrasts: one comparing the two modeling groups to the control groups and a second contrasting the two modeling procedures. It was expected that subjects in both modeling conditions would reward themselves less following the two lowest scores, than control subjects not exposed to a model and that subjects exposed to a High Standard model would reward themselves less for the second highest score than subjects exposed to a Low Standard model.

Preliminary analyses examined the effects of sex of subjects and the particular game to which subjects were initially exposed. Since these variables showed no significant effects or interactions with treatment conditions, the data were collapsed across these dimensions for further analysis.

Session I

The mean number of pennies taken by subjects at each success level during Session I are presented in the top third of Table 1. These data, obtained imme-

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Insert Table 1 about here

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diately following subjects' exposure to the model, subtantially replicate the results of earlier research (cf. Bandura, 1971). A one-way analysis of varianc performed on the total number of pennies taken by subjects yielded a highly significant treatment effect ($F_{2,68} = 11.7$, $p < .001$). Orthogonal contrasts indicat both that subjects in the two modeling conditions combined showed significantly more stringent standards for self-reward than subjects in the control condition ($F_{1,68} = 17.1$, $p < .001$), and that subjects exposed to the Low Standard Model set more lenient standards than those exposed to the High Standard Model ($F_{1,68} = 5.$ $p < .05$).

Table 1

Mean Number of Pennies Taken for Each Score, by Condition, Experiment 1

SESSION 1

	High Standard Model	Low Standard Model	No Model - Control
Highest Score	6.40	6.13	7.00
Second Highest Score	2.72	5.87	5.30
Lowest Two Scores	.48	.43	2.96
TOTAL (All Scores)	9.60	12.43	15.26

SESSION 2

Same Game

	High Standard Model	Low Standard Model	No Model - Control
Highest Score	6.92	6.91	8.15
Second Highest Score	5.25	5.91	5.92
Lowest Two Scores	2.17	1.27	1.92
TOTAL (All Scores)	14.32	14.09	16.00

Different Game

	High Standard Model	Low Standard Model	No Model - Control
Highest Score	6.42	6.82	7.80
Second Highest Score	4.67	6.27	6.20
Lowest Two Scores	1.08	.82	5.30
TOTAL (All Scores)	12.17	13.91	19.30

Separate analyses on the number of pennies taken on trials where the highest score was obtained revealed, as expected, no differences among treatments. On trials where the second highest score was obtained, the effect of treatments was highly significant ($F_{1,68}$ = 15.67, $p < .001$), as was the contrast between the High Standard and Low Standard groups ($F_{1,68}$ = 27.42, $p < .001$). A similarly significant treatment effect was also apparent for the number of pennies taken on trials where the two lowest scores were obtained ($F_{1,68}$ = 17.6 $p < .001$). As predicted, for these trials, the contrast between the two modelin conditions was not significant ($F < 1$), but the contrast between the combined modeling groups and the control condition was highly significant ($F_{1,68}$ = 35.51 $p < .001$).

In short, during Session I, subjects' self-reinforcement standards in the two modeling conditions showed very close, though not complete, adherence to the standards displayed by the peer model immediately prior to the test situation.

Session II

Three weeks following Session I, subjects were presented with an opportuni to play either the same game they had played in the first session or a differen and novel game in Session II. The data for this second session are presented in the lower two-thirds of Table I. A 2 x 3 (Same/Different Game x Modeling Condition) analysis of variance performed on the total number of pennies taken by subjects in this second session yielded a significant main effect for Modeling Condition ($F_{2,65}$ = 4.66, $p < .05$) but no effect for Same vs. Different game ($F < 1$) and no significant interaction between the two variables (F = 1.60, n.s.). Orthogonal contrasts indicated no difference between High and Low Stand. conditions ($F < 1$) but a significant difference between the two modeling conditi versus the control condition ($F_{1,65}$ = 8.83. $p < .005$).

Although the interaction term in this two-way analysis of variance does no approach significance, it appears from Table 1 that the effects of the modeling treatments were quite different for subjects in the Same Game and the Different Game Conditions during Session II. Specifically, for subjects who played the same game in Session II, none of the comparisons among the three treatment cond: tions approach significance. By contrast, for subjects who played a different game during Session II, there was a significant overall treatment effect ($F_{2,65}$ 4.55, $p < .05$), which is almost entirely accounted for by the significant contrast between the two modeling conditions combined and the control condition ($F_{1,65}$ + 8.56, $p < .005$). Considered separately for each level of success, more· over, it is apparent that this effect arises primarily from the highly significant difference between the two modeling conditions and the control condition on trials where subjects received the two lowest scores ($F_{1,65}$ + 20.99, $p < .001$) Both modeling groups, in short, continued to display significantly more stringer standards of self-re nforcement than control subjects, with a different activity in a later situation, although the initial difference between the two modeling groups was no longer evident.

Discussion

The results of Experiment 1 are both encouraging and paradoxical. Conside alone, the data from subjects presented with a novel game in Session II provide impressive evidence of the durability and generality of the effects of the modeling treatments. Confronted, some three weeks after the initial session, with a new experimenter and a different game in a novel setting, subjects in the modeling conditions continued to impose upon themselves more stringent standards for self-reward than control subjects. In light of this impressive evidence, however, the lack of persistent effects during the second session for subjects exposed a second time to the same game seems quite puzzling. Certainly traditional accounts of the generalization process, based on the similarity of the two sessions, would predict that the effects of the modeling treatments should be stronger for subjects asked to play the same game a second time than for those asked to play a novel game.

Fortunately, some data which may help explain these unexpected findings were provided by the post-experimental interviews with subjects following Session II. In these interviews, subjects were asked how one could obtain a "good" score on the game they had just finished playing. Most subjects indicated that they believed they had discovered some "strategy" which would produce high scores; some, however, either did not have or could not state, despite considerable probing, a strategy for producing high scores. Although the number of cases in some cells is too small to permit statistical analysis, the data for these two groups of subjects appear quite different: data from subjects who professed to have a strategy seemed to show both persistence of the modeling effects in the same game conditions and generalization in the different game conditions; data from subjects who did not acknowledge having a strategy, however, seemed not show any clear efforts. Furthermore, subjects in the Same Game conditions tended to be more likely than subjects in the remaining conditions to lack a hypothesis as to how the game should be played ($\underline{X}^2 = 3.20$, $\underline{p} < .10$). Although speculative, these data suggest that repeated exposure to the same game, in actuality pre-programmed and response-independent on two separate occasions may have led a number of subjects in the Same Game Modeling Conditions to infer that success at the game was not related to their ability. For these subjects, the setting of high "performance" standards may have been largely irrelevant.

Experiment 2

In view of these findings, Experiment 2 was designed with two primary purposes. First, Experiment 2 attempted to replicate and extend the results of Experiment 1, with a procedure altered to minimize the likelihood that children exposed twice to the same game would interpret the game as either impossibly difficult or determined by chance rather than skill. The Modeling Only Conditions of this experiment serve this purpose. In addition, Experiment also sought to examine the effects of an addition to the modeling presentation of a more direct "instruction" to the child that he should adhere to the modeled standard in the immediate test situation, to address issues raised by theoretical accounts of prior research on self-control. Previous research on the effects of direct instruction and rule structure on self-reinforcement behavior (Liebert & Allen, 1967; Liebert et al., 1969), for example, suggested that the addition of such a procedure should enhance the effects of the modeling presentation; other related evidence on the effects of modeling and rehears on children's charitability (Rosenhan, 1967; White, 1972), by contrast, suggested that the immediate effects of such a procedure might dissipate or even prove deleterious in subsequent tests of generalization or persistence. The subsequent effects of this Modeling plus Instruction procedure thereby promised to be of considerable theoretical interest. Finally, in Experiment 2, data on subjects' general perceptions of locus of control and teachers' ratings of subjects' classroom behavior were also obtained, to explore the possible relationship of subjects' self-reinforcement behavior to these variables.

To these ends, subjects were exposed in Session I to either a High Standard Model, a Low Standard Model, or a No Model Control procedure. In the Modeling Only Conditions and the Control Condition, subjects then played a game alone. In the Modeling plus Instruction Conditions, subjects played the game in the presence of the experimenter, who on the first critical trial indicated explicitly that he expected the subject to follow the model's example One to two weeks later, subjects were presented by a different experimenter wit either the same game or a different game. In both sessions, subjects' self-reinforcement behavior was unobtrusively recorded, providing data on the immediate effects and subsequent persistence and generalization of treatment effect

Method

Subjects

The subjects for Experiment 2 were 75 second, third, and fourth grade children attending the St. Raymond Elementary School in Menlo Park, California. The children ranged in age from 7-2 to 10-6, with a mean age of 8-9. The sampl included 30 males and 35 females, from a predominantly white, middle-class, and Catholic population.

Apparatus and Experimental Setting

The equipment in this study was identical to that employed in Experiment 1 In the experimental sessions, subjects played either the bowling game or the pinball game described earlier, and after each trial were allowed to reward themselves by taking a penny from the penny dispensers. As before both games were pre-programmed, and the number of pennies taken after each shot was recorded automatically.

Since it was not possible to obtain research space within the school, both sessions in this second experiment were conducted in the mobile research van. Although the two sessions were conducted by different experimenters and took place in different rooms within the van, it seems likely that the relationship between the two sessions may have been more salient to subjects in this study.

Session I

The procedure of Experiment 2 was also patterned closely after the first study. For Session I, subjects were brought individually to a research room in the van and were shown one of the three slide presentations employed in Experiment 1. Control subjects, therefore, witnessed an adult experimenter explaining the mechanics of one of the games to a peer model; subjects in the two modeling conditions witnessed this introductory explanation followed by the peer model playing the game for 12 trials, on which she consistenly rewarded herself for only the highest scores possible (Low Standard Conditions), commenting appropriately on the standard she used.

Following the presentation of the slide show, the experimenter rehearsed briefly the mechanics of the game the subject was to play and the operation of the penny dispenser. In addition, the experimenter reiterated at several points that the game was indeed a test of skill and that the subject's task was to "figure out" how to get good scores. The experimenter suggested that the game might be difficult, but emphasized that success would be a function of the subject's ability.

In the Modeling Only Conditions, the subject was then left to play the game alone, as in the previous study. In the Modeling plus Instruction Conditions, however, the experimenter remained in the room while the child played the game. Then, on the first trial on which the child's performance fell below the standard of self-reward which the model had displayed, the experimenter stated the standard explicitly, commenting matter-of-factly to the child that the score the child had just obtained was not good enough to deserve a reward. In the High Standard Condition, this remark occurred following the subject's first score of either "15" or "3"; in the Low Standard Condition, it occurred after the subject's first score of either "10" or "2". As intended, all subjects in these conditions adhered to the modeled standard.

Session II

One to two weeks after Session I, (mean interval = 11 days), the same children were seen in a second individual experimental session, conducted by a different experimenter, "blind" to the subject's treatment condition and previous behavior, in a different part of the research trailer.

As before, in Session II, half the subjects were presented with the same game they had been exposed to in Session I, to assess persistence of the experimental effects; half were presented with a different game, to measure generalization of these effects. In all cases, the child was left to play the game by himself during this second test, and subjects were told that they could reward themselves with a penny whenever they felt that their score at the game deserved a reward.

Following the subject's completion of a preprogrammed series of twenty trials at whichever game he had been assigned, the subject signaled the experimenter to return. The second experimenter then verbally administered first the brief questionnaire employed in Experiment 1 to assess the subjects' perceptions of the game he had just played, and second a version of a scale designed to measure subjects' perceptions of Internal vs. External Locus of Control, adapted from Mischel, Zeiss, and Zeiss (1974). Approximately one month following these experimental sessions, subjects' classroom teachers were asked to rate each of the subjects in their classes on scales of conformity, self-control, obedience, and self-motivation.

Results

As in Experiment 1, it was possible to analyze subjects' self-reinforcement behavior following each of the four possible levels of success at the game during both sessions. Again, preliminary analyses indicated no significant effects or interactions with treatment conditions for sex of subject or the particular game to which subjects were first exposed. The data were therefore collapsed across these dimensions.

Modeling Only Conditions

The Modeling Only Conditions of this experiment provide a replication of

Experiment 1, under conditions which minimized the likelihood that subjects would infer that the experimental games were either impossibly difficult or more a function of luck than skill. The data from these conditions are presented in Table 2.

Insert Table 2 about here

Session I. The mean number of pennies taken by subjects in the Modeling Only Conditions at each level of success during Session I are presented in the top third of Table 2. A one-way analysis of variance performed on the total number of pennies taken by subject indicated a highly significant treatment effect ($F_{2,38} = 7.11$, $p < .005$), while orthogonal contrasts demonstrated a significant difference between the combined modeling groups and the control group ($F_{1,38} = 9.24$, $p < .005$), but no significant difference between the two modeling conditions ($F_{1,38} = 2.70$ n.s.).

When these data are analyzed separately by level of success, however, the results are informative. For trials on which subjects received the highest scores, there are no differences among conditions. For trials on which the subjects received the second highest score, the contrast between the High Standard and Low Standard Conditions, is, as expected, significant ($F_{1,38} = 6.0$[illegible] $p < .05$). Finally, on trials where subjects received the two lowest scores, the two modeling conditions combined differ significantly from the control conditio[illegible] ($F_{1,38} = 8.28$, $p < .01$). The data, in short, provide clear evidence that the modeling treatments indeed had powerful immediate effects on subjects' standard[illegible] for self-reinforcement.

Session II. One to two weeks after this initial session, subjects were presented with either the same game they had previously played or a novel game in Session II. The data on subjects' self-reward behavior at each level of success during the second session appear in the lower two-thirds of Table 2, where it is apparent that the results replicate and strengthen the findings of Experiment 1.

A 2 x 3 (Same/Different Game x Modeling Condition) analysis of variance performed on the total number of pennies taken by subjects in this second session produced a significant treatment effect for the modeling conditions ($F_{2,35} = 9.38$, $p < .001$), but no effect for Same vs. Different Game ($F < 1$) or interaction between the two variables ($F < 1$). Orthogonal contrasts within this analysis revealed no difference between the High Standard and Low Standard Conditions, ($F < 1$) but a highly significant tendency for subjects in the two modeling groups to employ higher standards for self-reward than control subject[illegible] ($F_{1,35} = 18.40$, $p < .001$).

Within both the Same and Different Game Conditions, this pattern of result[illegible] is also apparent. In the Same Game Conditions, the difference between the two modeling conditions and the control group is clear not only for the total numbe[illegible] of pennies taken ($F_{1,35} = 15.07$, $p < .001$), but is of at least marginal significance at every level of success: for the highest scores ($F_{1,35} = 13.18$, $p < .001$[illegible] the second highest socres ($F_{1,35} = 3.84$, $p < .10$), and the lowest two scores

Table 2

Mean Number of Pennies Taken for Each Score, Modeling Only Conditions, Experiment

SESSION 1

	High Standard Model	Low Standard Model	No Model – Control
Highest Score	6.38	6.36	6.86
Second Highest Score	3.08	5.29	5.79
Lowest Two Scores	.77	1.14	2.93
TOTAL (All Scores)	10.23	12.79	15.59

SESSION 2

Same Game

	High Standard Model	Low Standard Model	No Model – Control
Highest Score	6.33	6.25	8.88
Second Highest Score	5.[illegible]	6.25	8.25
Lowest Two Scores	1.50	1.00	5.63
TOTAL (All Scores)	13.00	13.50	23.00

Different Game

	High Standard Model	Low Standard Model	No Model – Control
Highest Score	6.00	6.17	7.67
Second Highest Score	4.86	6.17	7.33
Lowest Two Scores	.57	1.83	4.17
TOTAL (All Scores)	11.43	14.17	19.17

($F_{1,35}$ = 10.43, p <.005). In the Different Game Conditions, this difference between the combined modeling conditions and the control group is significant for the total number of pennies taken ($F_{1,35}$ = 5.21, p <.05), though the effects at each success level are of only marginal significance considered individually: for the highest scores ($F_{1,35}$ = 4.00, p <.10), the second highest scores (F = 1.59, n.s.), and the lowest two scores ($F_{1,35}$ = 3.89, p <.01).[2]

In comparison to the results of Experiment 1, then, the Modeling Only Conditions of Experiment 2, which attempted to minimize possible redefinition of the games as chance-determined, provide evidence of both persistence of the effects of the earlier modeling treatments when subjects are subsequently presented with the same game in a new situation, and generalization of these effects when subjects are subsequently presented with a novel game in a new situation.

Modeling Plus Instruction Conditions.

Given these strong data from the Modeling Only Conditions, it is possible to examine the effects of the Modeling plus Instruction procedures on subject's behavior in the same manner. Since this procedure produced perfect adherence to the appropriate modeled standard during Session I, no analysis need be performed on these data. The remaining data of interest, concerning subjects' behavior during the second session, are presented in Table 3, where it is apparent that the results closely parallel the comparable data from the

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Insert Table 3 about here

Modeling Only Conditions.

As above, a 2 x 3 (Same/Different Game x Modeling Condition) analysis of variance was performed. For the total number of pennies taken by subjects, this analysis yielded a significant effect of the modeling treatments ($F_{1,32}$ = 9.92, p <.001) but neither the effect of Same vs. Different Game (F = 2.38, n.s. nor the interaction of the two variables (F <1) approached significance. Orthogonal contrasts within this analysis revealed a highly significant difference between the combined modeling conditions and the control condition ($F_{1,32}$ = 15.13, p <.001) and, unlike the previous conditions, a significant difference between the High Standard and Low Standard Conditions ($F_{1,32}$ = 5.21, p <.05).

The same general pattern of data also appears within both the Same Game and Different Game Conditions. Within the Same Game Conditions, contrast analyses performed on the total number of pennies subjects received revealed significant differences between the two modeling conditions and the control condition ($F_{1,32}$ = 9.08, p <.01) as well as between the High Standard and Low Standard Conditions ($F_{1,32}$ = 5.78, p <.05). Differences within these conditions were not significant for trials on which subjects received the highest possible score, but significant contrasts were obtained, as expected, between the two modeling conditions and the control condition for trials on

Table 3

Mean Number of Pennies Taken for Each Score, Modeling plus Instruction Conditions,

Experiment 2

SESSION 2

Same Game

	High Standard Model	Low Standard Model	No Model - Control
Highest Score	6.50	8.33	8.88
Second Highest Score	1.83	6.50	8.25
Lowest Two Scores	1.00	3.67	5.63
TOTAL (All Scores)	9.33	18.50	23.00

Different Game

	High Standard Model	Low Standard Model	No Model - Control
Highest Score	6.20	6.00	7.67
Second Highest Score	2.60	5.71	7.33
Lowest Two Scores	.40	.71	4.17
TOTAL (All Scores)	9.20	12.43	19.17

which subjects received either the second highest ($F_{1,32} = 8.55$, $p < .01$) or the two lowest scores ($F_{1,32} = 4.75$, $p < .05$). In addition, there was evidence of significant difference between the High and Low Standard Conditions in the Same Game Condition for trials on which subjects received the second highest score ($F_{1,32} = 7.05$, $p < .05$).

Within the Different Game Conditions, contrasts performed on the total number of pennies obtained by subjects demonstrated a significant difference between the modeling conditions and the control condition ($F_{1,32} = 6.46$, $p < .05$) but no difference between the two modeling conditions ($F < 1$). Considered separately at each of the possible levels of success at the game; the difference between the combined modeling groups and the control group was not significant for trials on which subjects obtained the highest score possible, but was significant for trials on which subjects obtained either the second highest score ($F_{1,32} = 4.26$, $p < .05$ or the two lowest scores ($F_{1,32} = 4.82$, $p < .05$).

The Modeling plus Instruction Conditions, then, reveal almost precisely the same pattern as the Modeling Only Condition. In both cases, substantial persistence of the effects of the modeling treatments in a later test on a novel game are evident. Indeed, the only substantive difference between these two sets of data is in the significant difference within the Modeling plus Instruction Conditions between the High Standard and Low Standard Conditions, an effect which does not approach significance in any of the previous Session II data.[3]

Additional Analyses.

A number of additional correlational analyses were performed to determine whether subjects' self-reinforcement behavior would be related to subjects' general locus of control expectations or to teachers' ratings of the children's classroom behavior. None of these exploratory analyses yielded significant effects.

Discussion

Together these studies provide strong evidence of both persistence and generalization of the effects of exposure to models displaying stringent self-reinforcement standards on children's self-imposed standards. In both experiments, children exposed to a model during the first session displayed significantly higher standards for self-reward two to three weeks later when engaged in a novel activity, in a different situation, than children in an appropriate control condition. Although in the first study significant persistence of these effects at the same activity was not demonstrated, in the second experiment, where the task was presented to emphasize the dependence of the child's outcomes on his ability, highly significant persistence of the modeling effects was obtained. Although other procedural differences between the two studies may also account for these last results, the role of subjects' perceptions of experimental activities would seem an important topic for further research.

Interestingly, for five of the six comparisons during the second session, initial differences between subjects exposed to the high and low standard models did not persist, although subjects in both modeling groups did continue to reward themselves according to a consistent, self-imposed standard.[4] It is noteworthy that the modal subject in all of the modeling conditions both reported himself to be using, and tended to adhere behaviorally, to a clear standard of rewarding himself for either of the two highest scores. Although exposed to precisely the same information as control subjects concerning the new game, these subjects continued to deny themselves rewards for scores falling below their self-imposed performance standard. Similarly, as in White's (1972) previous work on altruism, the use of direct instructions or "guided rehearsal" to induce complete initial adherence to the modeled standards, in the Modeling plus Instruction conditions, did not prove an effective means of enhancing the basic effect of the modeling treatments outside of the immediate test situation.

These data have several important implications. First, they provide needed theoretical leverage for clarifying the appropriate interpretation of previous research. The present results, for example, suggest the inadequacy of theoretical accounts which suggest that exposure to models displaying a high standard of self-reinforcement teaches children nothing more than a set of rules or norms for playing a particular novel game (Kuhn & Langer, 1968; Kohlberg, 1969). At the very least, the data indicate that children were able to abstract an appropriate general rule from the model's performance, which affected their behavior on a different activity, with a new experimenter, several weeks later. As in previous research (Bandura & Kupers, 1964), the data do not imply a passive mimicry process, but rather an active cognition process for which exposure to a model is one important input. Exposure to the model seems to have indicated to the children the appropriateness of imposing performance standards on themselves in a new situation, but did not demand exact adherence to the particular standard initially exhibited by the model.

At the same time, the diminished effectiveness of the high standard model and the explicit instruction procedures in the second session suggest some caution may be necessary in generalizing from results obtained in an immediate test situation, even in the absence of the experimenter and the model, to results which will obtain in subsequent situations. Given these data, for example, it seems inappropriate to consider the results of the first session as evidence that children had "internalized" a particular standard of self-reinforcement (Hoffman, 1970). Indeed, even the present data should probably not be construed as definitive evidence of a process of internalization, though they suggest the importance for understanding this process of the collection of dependent measures in a variety of situations.

Certainly the present data suggest the utility of attempts to examine the persistence and generalization of the effects of techniques designed to alter children's self-reinforcement standards. Indeed, one might well argue that the issue of generalization should be of paramount concern in the study of self-reinforcement behavior, since it is precisely the possibility that alterations in a person's self-imposed standards, unlike alterations in the standards imposed upon him by particular others, will have important effects on the person's behavior across a wide variety of situations which makes the study of self-reinforcement processes of central practical and theoretical significance. Though often unstated, the common assumption underlying a great deal of current applied research on self-reinforcement and self-regulatory processes (Bolstad & Johnson, 1972; Glynn, Thomas, & Shee, 1973; Drabman, Spitalnik, & O'Leary, 1973; Mahonev & Thoreson, 1974), is that treatment programs which successfully alter a person's self-imposed contingencies will be more likely than programs which alter the person's immediate environment to produce generalization of treatment effects beyond the particular situation in which the program was initially instituted. It is hoped that future research in this area will investigate further the implications of this assumption through the inclusion of measures of the persistence and generalization, as well as the immediate effectiveness, of procedures designed to affect self-reinforcement patterns.

FOOTNOTES

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1. Although a few subjects were still unwilling or unable to furnish a hypothesis about how one obtained good scores at the games, these instructions were effective in significantly reducing the number of subjects who failed to report a strategy. ($X^2 = 6.45$, $p < .025$).

2. In comparing these results for the Different Game Conditons with those of Experiment 1, it should be noted that the sample size within the Modeling Only Conditions in Experiment 2 is approximately half that of the first study.

3. Comparisons between the Modeling Only and the Modeling Plus Instruction Conditions revealed no significant differences either overall or within the two modeling conditions separately.

4. This dissipation of the initial differences between the two modeling conditions does not appear to have been the result of subjects' inability to recall the model's behavior. In both studies, approximately three quarters of the subjects correctly recalled the model's behavior, and those who failed to recall the model's behavior were distributed randomly across conditions. Elimination of the data from those subjects who failed to recall the model's actions, moreover, does not alter the pattern of results obtained.

EXPERIMENT 3

Generalization of Changes in Children's Preferences for Easy or Difficult Goals Induced by Observational Learning

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The recognition that the standards individuals impose upon their own behavior and the manner in which they evaluate their behavior with respect to these self-imposed standards may play an important role in the maintenance of behavior is not a new insight in social psychology. Concern with self-imposed performance standards and self-evaluative processes has long been central, for example, to discussions of the dynamics of level of aspiration (Lewin, Dembo, Festinger, & Sears, 1944), achievement motives (Atkinson, 1957, 1964), and effectance motivation (White, 1959). Only in recent years, however, has the study of self-reinforcement and self-imposed performance standards been placed systematically within the broader theoretical context of social learning theory (Bandura, 1969, 1971; Kanfer, 1971; Mischel, 1973). Similarly, only in the past few years have investigators begun to explore the potential clinical applications of self-reinforcement principles to the modification of complex behavior patterns (Mahoney & Thoreson, 1974; Thoreson & Mahoney, 1974).

One important, though typically implicit, element in this resurgence of interest in self-imposed standards is the potential for changes in an individual's self-imposed standards to produce changes in behavior likely to transfer across different situations. Though often unstated, a common assumption underlying much current applied research on self-reinforcement and self-regulatory processes (Bolstad & Johnson, 1972; Glynn, Thomas, & Shee, 1973; Drabman, Spitalnik, & O'Leary, 1973; Jeffrey, 1974; Mahoney & Thoreson, 1974) is that treatment programs which are successful in altering a person's self-imposed contingencies will be more likely than programs which alter the person's immediate environment to produce generalization of treatment effects beyond the particular situation in which the program was initially instituted.

A concern with the generalization of effects of techniques designed to modify a person's self-imposed standards, however, is equally important on a theoretical level. Consider, for example, the extensive experimental literature on the effects of modeling procedures in altering children's standards for self-reinforcement (Bandura, 1969, 1971; Bandura & Kupers, 1964; Bandura & Whalen, 1966; Bandura, Grusec, & Menlove, 1967; Liebert & Allen, 1967; Liebert & Ora, 1968; Liebert, Hanratty, & Hill, 1969; Mischel & Liebert, 1966; Grusec, 1971; Masters & Mokros, 1974). In these studies, typically, children in the experimental conditions are first exposed to a model playing a novel game. While playing the game, this model sets for himself a very stringent or a relatively more lenient standard for self-reinforcement, rewarding himself consistently with tangible rewards (money, tokens, candy) and/or displays of positive self-evaluations only on trials when his performance at the game exceeds his self-imposed standard. Following exposure to the model, the children are allowed to play whis same game themselves, and the standards for self-reinforcement they impose upon themselves are recorded.

In these studies, across a wide variety of procedural variations, it has been repeatedly demonstrated that exposure to a model who displays a consistently strict standard for self-reinforcement in playing a novel experimental game will lead children to adopt remarkably strict standards for self-reward in playing this game subsequently. Since these studies have been conducted primarily in experimental laboratory settings, however, it has been suggested (Kohlberg, 1969; Kuhn, 1973) that children in these studies may simply be complying with what they perceive as the "correct" way to respond to a novel situation. In this view, exposure to the model serves only to teach the child the rules of a particular game, with no implications beyond the immediate training situation. It appears crucial, therefore, to determine whether such effects would persist over time and would generalize to other tasks presented in different situations.

Some evidence relevant to this issue has recently been reported by Lepper, Sagotsky, and Mailer (1975) in two experiments which investigated the persistence and generalization of the effects of exposure to modeled self-reinforcement standards. In these studies, children observed a peer model exhibiting either a very strict or more lenient standard for self-reward while playing a novel game, or saw no model. Subjects then played this same game immediately following exposure to the model, as in previous studies. Two weeks later, in a new situation, subjects were given an opportunity to play either the same game or a different game they had not seen before. In these subsequent sessions, substantial persistence of the effects of exposure to the model in a new setting and generalization of these effects to a different, novel game in a session were evident.

The present study pursues a related line of investigation, examining the generalization of the effects of modeling techniques on children's self-imposed standards, as evidenced in children's choice to engage in either easy or difficult tasks. In doing so, the study has three primary aims. First, the study attempts to investigate potential parallels between the study of "prospective" and "retrospective" self-imposed standards. Rather than examining the standards children apply in giving themselves rewards after receiving a particular score, as in the previous work in this area, the present study examines the standards children apply in advance in choosing the level of difficulty at which they approach a task, and it attempts to make explicit the parallels between previous literature on self-reinforcement and level of aspiration. Second, the study involves an investigation of performance standards involved in activities genuinely dependent upon the child's level of ability and effort, rather than activities in which apparent performance is experimentally controlled and preprogrammed. Similarly, symbolic rewards are employed, rather than rewards of inherent extrinsic value, to produce a procedure which seems more closely analogous to the covert self-congratulation processes that we believe represent the most common form of self-reinforcement in many everyday situations. Finally, the present study attempted, in addition, to provide a more stringent test of the generalization of effects produced by exposure to the modeling treatments. To this end, following exposure to the modeling treatments, children's performances for easy versus difficult tasks were assessed in three different situations, including a final measure obtained unobtrusively two months after exposure to the model in children's regular classrooms.

The design of the present experiment was, therefore, quite straightforward. Third-grade children in two modeling conditions observed a peer model play a novel athletic game, choosing consistently either difficult or easy goals for himself, while control subjects saw no model. Immediately afterwards, subjects played this same game themselves. Three weeks later, subjects participated in a "spelling bee," conducted in a very different setting, and were asked to select the difficulty level of the words they wished to attempt to spell. Finally two months later, in their regular classrooms, subjects were offered a choice of puzzles of differing levels of difficulty by their teachers, in the absence of research personnel. On each of these occasions, the dependent measure of interest was the level of difficulty at which subjects chose to approach a new activity, and the primary concern of the study was the extent to which the immediate effect of the modeling treatments of children's preferences for difficult versus easy tasks would persist and transfer to new, quite different tasks in subsequent situations.

Method

Subjects

Subjects were 38 third-grade children attending the St. Simon's School in Los Altos, California. The children ranged in age from 9-0 to 10-5 with a mean age of 9.58. The sample included 18 males and 20 females, from a predominantly white, middle-class, and Catholic population. Two subjects were absent from class during the third session of the study.

Apparatus and Experimental Setting

Session 1 of the experiment was held in a room of a mobile research trailer. The room contained a "beanbag toss" game, a motion picture projector to allow the presentation of the modeling variations, and an award certificate and a box of paste-on stars the child could use to record his performance at the game. Four differently colored buckets were spaced 1 1/2 feet, 3 feet, 4 1/2 feet, and 6 feet respectively from a starting line taped on the floor. The starting line was composed of four colored segments that corresponded to the colors of the target buckets, delineating four distinct places for the child to stand depending on his choice of target. The buckets were numbered with large, black numerals from "1" through "4," with the closest being "1" and the furthest "4." An additional bucket, containing the twenty beanbags to be thrown, was placed behind the starting line in a central location.

Session 2 of the experiment took place three weeks later in the guidance office within the school building. This room contained a large table on which was an award certificate and a box of stars as in the first session, and four stacks of index cards containing words for a "spelling bee," which were labeled easy, medium, hard, and very hard.

The third "session" of the study took place two months after the initial session in the children's natural classrooms, and was conducted by the children's regular teachers. In this session, children were asked to indicate the level of difficulty of puzzles the school had received that they would prefer to have to take home.

Session 1

For the first session subjects were brought individually to the research trailer to play the beanbag toss game. The child was first seated next to the experimenter, and shown a film which portrayed a peer model being introduced to, and, in the experimental conditions, playing the experimental game.

The films began with an adult explaining the mechanics of the game, the degree of difficulty of each of the four buckets (easy, medium, hard, and very hard), and the reward system. The model was presented with a 5 x 8 Certificate of Merit which had the words "easy," "medium," "hard," and "very hard" printed on the left-hand side. In parentheses underneath each descriptive adjective was a number corresponding to the large number painted on each bucket. The model was instructed that he would receive the award just for participating in the study, but that he would also be able to paste a star on the certificate in the appropriate space after each successful throw of a beanbag into a bucket. In the no-model, control condition, the film ended at this point. In the two modeling conditions, however, the peer model in the film proceeded to play the game, exhibiting a consistent choice of either the two easiest or the two most difficult targets. In the high standard condition, the model refused to shoot at the two easiest targets, explaining that they were too easy to be worth attempting, and that the two more difficult targets would be more fun because they were more challenging. The model alternated for ten shots between the two hardest targets, and was successful on three shots at each target. In the low standard condition, the model refused to attempt the two most difficult targets, explaining that he did not wish to risk missing, as he wanted to win a lot of stars. The model alternated for ten trials between the two easiest targets and was successful on all shots.

Following the presentation of the film, the experimenter indicated to the subject that he would now have a chance to play the game and rehearsed with the child the mechanics of the game, degree of difficulty of each bucket, and the reward system, as had been done in the film. He also indicated that the child should paste a star on his award certificate immediately after he successfully tossed a beanbag in a bucket. Because extensive pretesting had revealed strong tendencies for subjects to shoot at each target until successful and then proceed to the next one, or to simply shoot at all the targets in order, the instructions were designed to make very clear that the child was to decide on each shot which target he would like to attempt and should not feel constrained to follow any particular pattern. Each subject was allowed 20 trials on the game, while the experimenter remained in the room and kept a covert record of the child's performance.

Session 2

Approximately three weeks following Session 1 (mean interval = 19.41 days), these same subjects were seen in a second experimental session. For this second session, subjects were brought individually, by a different experimenter, "blind" to the subject's treatment condition and previous behavior. To maximize further the separation of the two sessions, Session 2 also took place in a different location, the guidance counselor's office inside the school building.

In this session, children were asked to play a "spelling bee" game. At the outset, subjects were shown a "Spelling Award," similar in design to the "Certificate of Merit" they had received in the first session. Similarly, as before, children were told they would receive the award just for participating, but could also paste a star next to the words "Easy," "Medium," "Hard," and "Very Hard," to denote success each time they correctly spelled a word at a specified difficulty level. The children were then asked to choose the level of difficulty of the ten words they wished to attempt. Four decks of cards, ten of each category, were set in front of the subject and he was asked to select ten of these cards. Again, children were explicitly informed that the decision of the levels of difficulty to be attempted was completely up to the child. Subjects were required to commit themselves to a choice of words before hearing any of the actual words, so that feedback from success and failure at the game would not influence the choice of difficulty. After each word successfully spelled, the child was allowed to paste a star in the appropriate place on his award.

Session 3

The third "session" of this study took place approximately two months (Mean Interval = 73.35 days) after Session 1 in the children's regular classrooms. For the final measure, children were informed by their teachers that the school would be able to obtain puzzles for the children in the class. Since the puzzles differed in their level of difficulty, however, from easy to medium, hard, and very hard, however, each child was asked to indicate which sort of puzzle he would most prefer to take home, by checking a box on a sheet handed out by the teacher. Apart from the words used to describe the level of difficulty of the puzzles, there was no indication that this activity was connected in any way with the individual experimental sessions subjects had experienced one to two months earlier.

Results

The experiment, therefore, provided three tests of increasing stringency of the effects of the modeling treatments on subjects' subsequent preferences for easy versus difficult tasks. In Session 1, immediately following exposure to the modeling treatments, subjects played the same beanbag toss game they had seen the model play; In Session 2, three weeks later, subjects engaged in a spelling bee; and in Session 3, two months later, subjects were given an option of the difficulty level of a puzzle they were to receive in class. Because the modeling films had differentiated between choice of the two most difficult versus the two easiest choices in the first session, data from each of these sessions are presented in terms of this distinction. In addition, since preliminary analyses suggested that sex of subject was a potentially relevant factor in determining the results of the modeling treatments, data were analyzed in terms of this dimension as well.

Session 1

In Session 1, subjects were given the opportunity to take a total of twenty shots in the beanbag toss game. The mean number of these attempts made at the two most difficult targets, by sex and condition, are presented in the top third of Figure 1.

Insert Figure 1 about here

A 3 x 2 (Treatment x Sex) unweighted-means analysis of variance performed on these data yielded a highly significant effect of modeling conditions, F (2,32) = 7.20, p <.005, as well as a significant sex effect, F (1,32) = 6.41, p <.025 reflecting a general tendency for males to choose more difficult targets than females. Individual comparisons illuminating the nature of the obtained treatment effect indicated that the modeling treatments had a decidedly asymmetrical effect on subsequent choices of difficulty level. Subjects in the High Standard Condition indeed chose more difficult targets than subjects in either the Control, F (1,32) = 11.10, p <.005 or the Low Standard, F (1,32) = 10.47, p <.005, Conditions; but there was no evidence of a difference between the Low Standard and Control Conditions, F <1, and for males the means were in fact reversed.

Because these results in Session 1 form the appropriate baseline against which subsequent generalization of the modeling effects should be assessed, it is important to note two further features of these data. First, to put the above comparisons slightly differently, it should be noted that the contrast between the High Standard Condition versus the Low standard and Control Conditions accounts for virtually all of the systematic treatment variance in these data, F (1,32) = 14.46, p <.001. Second, although the relevant interaction between the treatment conditions and sex of subject was not statistically significant, F (2,32) - 1.86, p <.25, it also appears that the effect of the modeling treatments was greater for females than for males. For males only, the contrast between the High Standard Condition and the other two conditions barely achieved a conventional level of statistical significance, F (1,32) = 4.15, p = .05; for females, this same contrast was highly significant, F (1,32) - 11.25, p <.005. Hence, exposure to the High Standard Model appears to have increased the likelihood that subjects would set high standards for themselves while the exposure to the Low Standard model seems to have had little effect on subjects' subsequent behavior.

Session 2

With this pattern of results forming the standard for comparison, it is possible to assess the extent to which these initial effects of the modeling

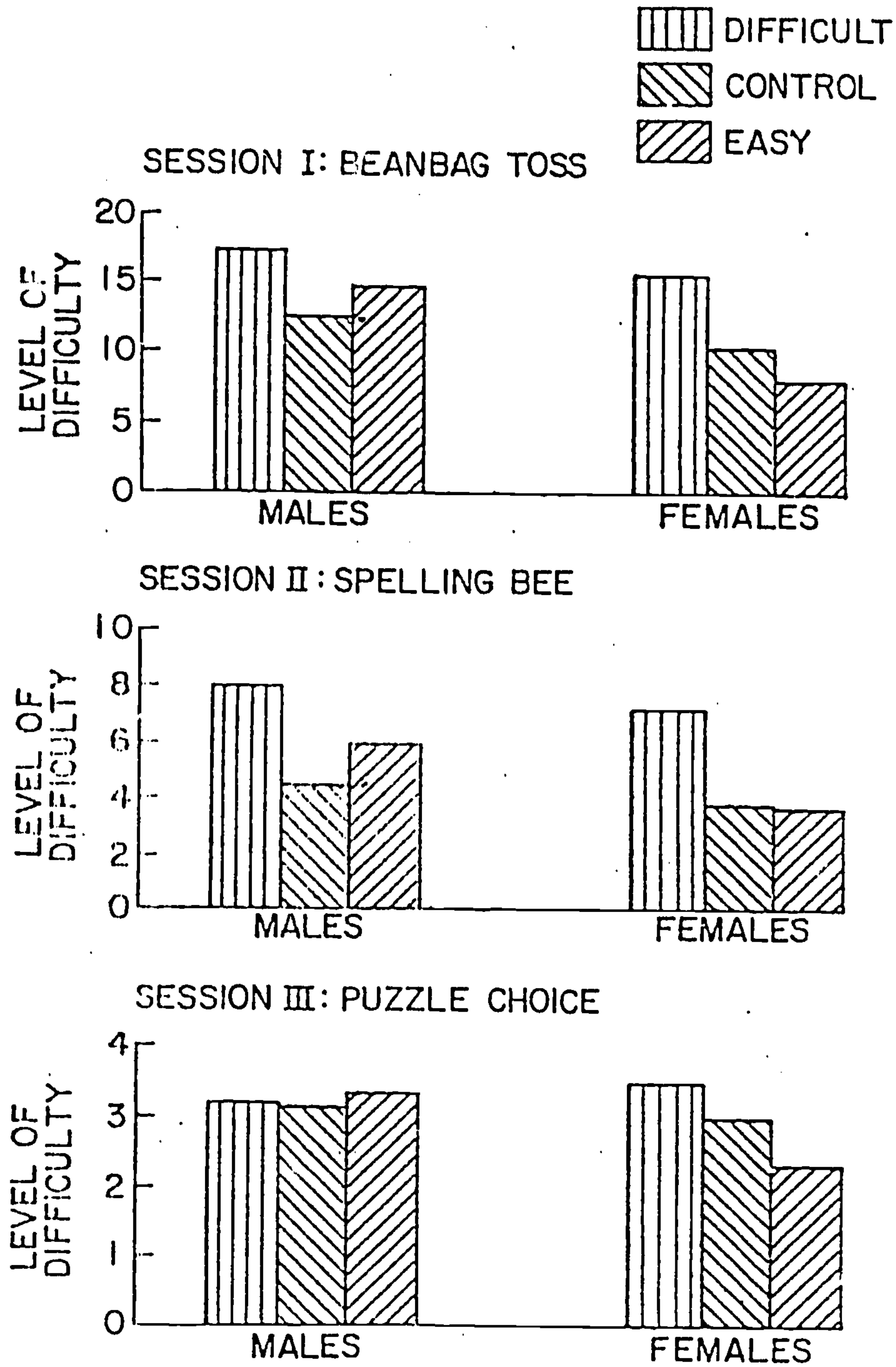

Figure 1. Mean level of difficulty chosen, by condition and sex, for each of the three experimental sessions.

treatments generalized to a new situation and a very different task several weeks later in Session 2, when subjects were allowed to choose the level of difficulty of the ten spelling words they were to attempt in the spelling bee. The appropriate data, the mean number of difficult and very difficult words subjects chose to attempt in this second session, by sex and treatment condition, are presented in the middle third of Figure 1, where it is apparent that the pattern of results obtained in this session closely parallels that obtained in Session 1.

As before, a 3 x 2 (Treatment x Sex) unweighted-means analysis of variance was performed on these data. As in the first session, this analysis yielded a highly significant treatment effect, F (2,32) = 5.95, p <.01. Neither the main effect for sex of subject, (F (1,32) = 1.43, n.s., however, nor the interaction of treatments with sex of subject, F < 1, was significant. Again, individual comparisons illuminating the nature of the overall treatment effect indicated that subjects in the High Standard Condition chose to attempt more difficult words than the subjects in either the Control, F (1,32) = 10.74, p <.005, or the Low Standard, F (1,32) = 6.48, p <.025, Conditions, while subjects in the Low Standard and Control Conditions did not differ.

Paralleling the results from Session 1, the contrast between the High Standard group and the other two groups accounted for most of the systematic treatment variance in subjects' choices, F (1,32) = 11.34, p <.005; and this effect, as in Session 1, appeared to be weaker for males, F (1,32) = 3.16, p <.10, than for females, F (1,32) = 10.17, p <.01. Thus, the effects of the modeling treatments on subjects' choice of difficult spelling words in this second session, in a different situation some three weeks later, appear to follow closely the immediate effects of the modeling treatments during Session 1. Indeed, across conditions, the correlation between the number of difficult targets chosen in Session 1 and the number of difficult words chosen in Session 2 was a striking .67, p <.001, considering the inherent differences between the two situations.

Session 3

Given the highly consistent results obtained in the first two sessions, it becomes of particular interest to examine the results of Session 3, two months after Session 1, in which children were asked by their regular classroom teachers, in a setting divorced from the previous experimental sessions, to select the level of difficulty of the puzzle they were to receive to take home from the school. The lower third of Figure 1, therefore, presents the mean level of difficulty chosen, by sex and treatment condition, in this final test situation.

The results from this third session, as inspection of Figure 1 reveals, are clearly less striking than in Sessions 1 and 2; and a 3 x 2 analysis of variance of these data, unlike the earlier sessions, yielded no statistically significant overall treatment effect. At the same time, however, the apparent difference in the effectiveness of the modeling treatments for males and females, noted in the previous sessions, seems to have become more pronounced in this final measure; hence the data were further analyzed, as before, separately by sex. For males, who in all conditions chose relatively difficult puzzles, there are no significant effects, all F's < 1. For females, however, there is evidence of significant persistence of the experimental effects even in this dissociated posttest two months after Session 1, apparent in the continued significance of the contrast between the High Standard Condition versus the Low Standard and Control Conditions, F (1,17) = 6.67, $p < .025$. Similarly, for females, the correlation between the difficulty level chosen in Session 1 and Session 3 is a considerable .58, $p < .025$; for males this correlation, .16, does not approach significance.

Discussion

These data greatly increase our confidence in the utility of modeling procedures in producing generalized and persistent changes in children's self-imposed performance standards. Exposure to a peer model who chose to attempt only the most difficult targets in a test of physical coordination had effects not only on the level of difficulty children chose when attempting this same task immediately following exposure to the model but also produced equally significant effects on the level of difficulty at which children chose to attempt a test of academic ability, in a different situation, some weeks later. For females, similar results were apparent even in their choice of puzzles, two months after the initial session, in a regular classroom setting divorced from the earlier experimental sessions.

Considering the many apparent differences between these various tasks, these findings are impressive. Particularly since both the beanbag game and the spelling bee involved actual skill-related tasks, one might have expected, for example, that children's choice of difficulty level was determined by their expectations of success at the task, based either on their previous experience or on their success on the experimental games themselves. In fact, however, for all subjects in the first and second sessions, and for females in the third session, knowledge of which model the child had seen proved a much better predictor of the child's choices than did his actual performance at the task, his performance on the previous tasks, or other indicators, such as spelling grades, of his task ability.

Previous research on modeled standard-setting has generally involved completely novel games that were preprogrammed to eliminate variance due to children's wide range of skills. The present study demonstrated a modeling effect sufficiently robust to prove a significant influence in situations involving familiar skills such as throwing or spelling ability, for which children had previous and concurrent feedback as to their ability level. Similarly, in previous research, a reliance on preprogrammed artificial games involved an implicit equation between high self-imposed standards and overt self-denial, as those children choosing to reward themselves for the strictest standards necessarily obtained the least rewards. By using genuine tasks, the conceptualization of standard setting in the present study becomes closer to that of level of aspiration, with a child setting a high standard for himself taking a greater risk of failure but potentially receiving a greater reward for his efforts. Desire to achieve a greater reward in these situations does not necessarily conflict with setting a high standard.

Though exerting a powerful influence on subsequent behavior, however, the effectiveness of the modeling treatments was not without limits. Of greatest potential theoretical interest is the consistent asymmetry of the two modeling groups, relative to the control condition. The parallel between these data and previous data investigating the effectiveness of modeling techniques in modifying children's moral judgments (e.g., Cowan, Langer, Heavenreich, & Nathanson, 1969; LeFurgy & Woloshin, 1969) seems worth noting: in both sets of data, exposure to models behaving in a manner generally consistent with normative adult standards proved more effective in producing long-term change than exposure to models behaving in a manner more inconsistent with adult norms. Though such findings, in the area of moral judgments, have frequently been discussed as evidence favoring a sequential developmental-stage model, the present data suggest a potentially broader phenomena. That is, that the effectiveness of modeling techniques with young children may generally be importantly determined by the congruence of the model's behavior with the child's perceptions of age-appropriate or more mature behavior, leading to a rejection of response patterns which are perceived as less mature choices. Thus, in the present study, children could not be easily induced to choose an easier and less challenging plan of action than they normally would have adopted in the game situation.

Together with the previous experiments reported by Lepper et al., (1975), the present data seem sufficient to establish that "what is learned" through exposure to a model displaying stringent standards for self-reinforcement or choosing to engage in difficult tasks in some reasonably abstract principle of choice or preference sufficient to produce generalization to new settings,[3] rather than a simple re-definition of the "rules" of a particular game (Kohlberg, 1969; Kuhn, 1973). On an applied level, these findings would seem to provide further encouragement to recent investigations attempting to produce persistent and generalizable behavior change through the use of techniques designed to alter the performance standards children set for themselves (Bolstad & Johnson, 1972; Glynn et al., 1973; Drabman et al., 1973; Jeffrey, 1974; Mahoney & Thoreson, 1974; Weiner & Dubrowski, 1974). Certainly in the light of the considerable evidence relating preferences for easy versus difficult tasks to more general models of achievement motivation (cf. Weiner, 1974), the practical implications of the present findings appear to warrant further investigation.

Footnotes

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1. This same general pattern of data across the three sessions was also apparent in nonparametric comparisons. In Session 1, 100% of the children exposed to the High Standard chose a majority of difficult targets, while only 56% of the children in the other two conditions did so ($X^2 = 6.05$, $p < .025$). Similarly, in Session 2, 85% of the children in the High Standard Condition selected a majority of difficult spelling words, while only 24% of the subjects in the other two conditions did so ($X^2 = 10.37$, $p < .01$). In Session 3, the data for females appeared similar, 100% of the High Standard children asked for difficult puzzles, versus 54% of the children in the other conditions, though this difference was not significant nonparametrically ($X^2 = 2.19$, $p < .15$); for males, the percentages across conditions were virtually identical.

2. The failure to obtain any evidence of generalization for males in Session 3 is puzzling. As indicated before, the level of difficulty of puzzles chosen was quite high for males in all groups and may have produced a "ceiling" effect; in addition, across all three sessions, the modeling procedure seemed to be less effective for males than for females. Unfortunately, neither the ancillary data collected in this study nor post-experimental interviews with the subjects provide any compelling explanation of this apparent sex difference.

3. Though not tested directly in these studies, it seems worth noting that an attributional model (Bem, 1972; Kelley, 1967) may provide unexpected insight into the remarkable power of modeling treatments to produce persistence, and frequently generalization of behavior change to new situations (Bandura, Jeffery, & Wright, 1974; Cowan et al., LeFurgy & Woloshin, 1969; Lepper et al., 1974; Rosenhan, 1969; White, 1972). From such a perspective, it has been suggested (Bem, 1972; Kopel & Arkowitz, in press; Lepper, Greene, & Nisbett, 1973), for example, that techniques which induce changes in behavior will be likely to show transfer to subsequent new situations precisely to the extent that initial behavior change is accomplished in a manner which minimizes the extent to which the individual perceives his behavior as extrinsically controlled. In attributional terms, exposure to a model may be such an effective behavior change technique, in part, precisely because it induces the subject to "choose" to act similarly to the model in the fact of rather vague external contingencies, likely to promote intrinsic rather than extrinsic attributions (Kelley, 1967). Certainly future investigations might profit from a further consideration of the phenomenology of the modeling process and its relationship to the generalization of changes induced.

EXPERIMENT 4

Effects of Training in Self-Monitoring and Goal-Setting Techniques on Classroom Study Behavior and Academic Performance

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Interest in the study of self-control processes has expanded rapidly in recent years. Increasingly, in both theoretical and applied contexts, attention has shifted from a concentration on the manner in which an individual's behavior is governed by the immediate environmental contingencies to which he is exposed to a consideration of the active cognitive processes which influence and maintain an individual's behavior in the absence of immediate external reinforcement (Bandura, 1971; Lepper & Amabile, in press). Reflective of this shift has been a growing research literature concerned with the manner in which individuals impose performance standards upon themselves, monitor their own performance and evaluate and reinforce themselves with respect to their ability to meet or exceed these self-imposed performance standards (e.g., Bandura, 1971; Mischel, 1973; Mahoney & Thoreson, 1974; Thoreson & Mahoney, 1974).

One particularly interesting by-product of this increased attention to self-control phenomena has been the finding that procedures designed to induce an individual to monitor his own behavior in some systematic fashion have frequently led to appreciable changes in the incidence of the behavior being monitored. Such "therapeutic" effects of the introduction of systematic self-monitoring procedures in the absence of overt self-imposed or externally-imposed contingencies have been observed in a variety of contexts involving quite different procedures and subject populations (Broden, Hall, & Mitts, 1971; Johnson & White, 1971; Kazdin, 1974b; Gottman & McFall, 1972; Mahoney, Moore, Wade, & Moura, 1973), although the failure of other investigations of self-monitoring to demonstrate such effects indicates clearly that such changes on behavior are not a necessary or inevitable consequence of the introduction of self-monitoring techniques (e.g., Berecz, 1972; McFall, 1970; Mahoney, Moura, & Wade, 1973; Stollak, 1967).

Of those studies reporting significant behavior change as a consequence of self-monitoring procedures, one of the most dramatic and compelling instances is reported in a widely cited paper by Broden, Hall, and Mitts (1971). Working with a junior high school student who had sought help in improving her study habits from the school counselor, these authors asked the subject to record, whenever she happened to think of it, whether or not she was engaged in appropriate study behavior, by marking either a plus or minus on a slip provided each day for this purpose. With the exception of praise from the school counselor during a weekly meeting with the student, no additional contingencies were applied concerning either the accuracy or use of the self-report slips or the subject's reported or actual study behavior. Remarkably, introduction of this simple and somewhat unsystematic system for self-monitoring produced dramatic increases in observed study behavior during the class periods when self-monitoring slips were distributed. Coupled with the subsequent use of contingent teacher attention, moreover, these gains persisted over a two-month period.

While these results are striking, there are two general sorts of questions which this report raises. The first concerns the appropriate interpretation of the effects demonstrated in this case study. Because this study, like much of the literature on self-monitoring, employed an intra-subject design, it is difficult to determine whether the gains shown are best attributed to the self-monitoring procedure <u>per se</u>, to other potential motivational variables, such as attention, instructions, and praise from the subject's counselor, which were confounded with the introduction of this treatment (Kazdin, 1974b) or to the experimental demands of the situation created by the introduction and withdrawal of any novel and punitively effectively treatment. (Orne, 1970).

At the same time, if one assumes that the self-monitoring procedures were in fact primarily responsible for the improvement in the subject's classroom behavior, a second question of interest is why such self-monitoring procedures should themselves result in any behavior change in the absence of additional contingencies. One plausible hypothesis is that the efficacy of self-monitoring procedures in producing behavior change in the absence of overt contingencies will depend upon the effectiveness of these procedures in eliciting covert self-evaluative processes (Kanfer, 1970; Kazdin, 1974a). It follows that behavior change as a result of self-monitoring will be most likely to occur in situations where the individual has some active desire to modify his behavior along the dimension which is being monitored. Whether this goal of behavior change is self-generated or externally-imposed, self-monitoring under such conditions seems likely to evoke a process of self-evaluation and provide feedback allowing comparisons of one's actual behavior with one's goals.

Such a line of reasoning is, of course, consistent with a potentially related literature on the effects of procedures designed to induce individuals to set frequent and relatively specific performance goals for themselves (e.g., Locke, 1968; Latham, & Kinne, 1974; Latham & Yukl, 1975). Though conducted primarily in industrial and organizational contexts, these studies suggest that techniques which induce individuals to set specific performance goals for themselves on some regular basis can have dramatic effects of indices of performance or productivity, effects which in particular frequently appear considerably greater than comparable nonspecific motivational treatments designed to simply encourage individuals to work hard to do their best at the activity in question, although such effects are far from universal (Campbell et al., 1970). The presence of specific goals against which one's performance can be compared at frequent intervals is hypothesized to produce behavior change in these situations through the elicitation of implicit self-evaluative responses. Hamner and Harnett (1974) have documented that the amount by which people exceed their goals is positively related to their reported level of satisfaction; furthermore, Kim and Hamner (1976) have shown that it is possible for goal-setting alone to enhance performance without a formal program of feedback.

Though derived in somewhat different contexts, these literatures appear to share a common theoretical framework, in that the effectiveness of both self-monitoring and goal-setting procedures in the absence of explicit external contingencies seems best explicable in terms of the operation of covert self-evaluative processes. In this sense, the effectiveness of self-monitoring techniques alone in producing behavior change is hypothesized to depend upon the occurrence of an implicit goal-setting process; conversely, the effectiveness of goal-setting procedures per se appears to presuppose some self-monitoring processes by which performance is regularly compared to one's specific goals. Therefore, a combination of both these techniques might be expected to prove an effective means of reducing behavior change.

The present study, therefore, sought to examine the individual and joint effects of the introduction of daily self-monitoring and goal-setting procedures in affecting children's study behavior and academic achievement in an ongoing classroom situation. The study sought first to assess the relevance of these two sorts of techniques with an unselected sample of elementary school students, in an experimental design which allowed an assessment of the effects of both the self-monitoring and the goal-setting techniques per se, independent of situational demands or the effects of other nonspecific motivational variables. In addition, to clarify the potential clinical significance of such self-monitoring and goal-setting techniques, the present study also sought to examine the typically implicit assumption in studies of classroom performance (Winnet & Winkler, 1972) that increases in the amount of "appropriate" study behavior by a student can be expected to lead to corresponding increases in that student's actual academic achievement.

Following a baseline period of observation to establish initial rates of study behavior and achievement in the school mathematics program, therefore, children in three experimental conditions received training in the use of either self-monitoring or goal-setting techniques or in both of these techniques, while subjects in a control group received only nonspecific motivational instructions. The effects of these four experimental procedures, forming a 2 x 2 factorial design (self-monitoring x goal-setting), on students' study behavior during class periods devoted to the math program and actual academic performance in the school's individualized mathematics curriculum during these periods were assessed during a five-week treatment phase.

Method

Subjects and Experimental Setting

The subjects were 67 children, 37 girls and 30 boys, enrolled in the fifth and sixth grades of a suburban elementary school employing an individualized mathematics instruction program. In this program, children spent 50 minutes each day, four days a week, working through a modular mathematics curriculum consisting of a standardized series of mathematics "units." The children left their normal classrooms for the math period, during which time they were grouped according to the unit level they were currently working on. During these daily mathematics periods, children worked on each unit until they reached an acceptable level of accuracy, at which time they were allowed to progress to the next unit in the sequence. Children progressed through these units at their own speed. At the outset of the study, considerable individual variation was observed in the children's behavior, with some children initially working quite diligently and others doing virtually no work at all during the math periods. Both the principal and the teachers expressed some feeling that the program was not optimally effective and welcomed the intervention of this research project.

Behavioral Observation System

To assess the effects of experimental treatments on students' classroom study behavior, a behavioral observation system was developed for use in this study. On each day every student in the study was observed for a two-minute period, and the order that children were observed was varied from day to day in order to sample behavior from different parts of the period for each child. During this observation period, for each of eight ten-second intervals, the subject's behavior was coded into one of five mutually exclusive categories; five seconds were allowed for recording after each ten-second observation period.

The five observation categories included two on-task behaviors--at seat working)i.e.,.student in seat with pencil in hand, attending to math unit) and at teacher's desk (i.e., consulting with the teacher or waiting in line to speak with her). The remaining categories included three classes of off-task behaviors--at seat not working (i.e., any non-work behavior in seat; e.g., talking to a neighbor, laughing, playing), out of seat not working (e.g., walking around the room), and out of room. This final category was rarely seen.

Prior to the beginning of the study, each of the six observers received extensive classroom practice in employing the observational system. Observers were sent into classrooms in pairs, and each of the two observers in a classroom independently coded the behavior of the same children. Reliability in terms of percentage of agreement for each ten-second interval was quite high, ranging from 96 to 100 percent.

Procedure

Baseline Phase. At the beginning of the baseline phase, two observers were placed within each participating classroom during the daily math periods. Each observer was assigned to observe the behavior of half of the students in the class, as described above. At the end of each math period, the observers collected the children's units. Any unit that had been completed that day was corrected, and the percent of problems completed correctly was written on the front of the unit in red pencil. Completion of the unit with an accuracy of ninety percent or better was ordinarily required before the student was given a new unit, with the teachers' retaining an option to be somewhat flexible about that standard. This baseline phase continued over a four-week period.

Treatment Phase. On the first day of the treatment phase, each child was taken individually from the classroom for a brief period of instruction in the relevant self-control techniques. In these sessions, each child was seen by an experimenter who did not regularly record his classroom behavior;

thus, observers remained blind to the treatment conditions of the children they observed. Subjects in each classroom were assigned randomly to one of the four treatment conditions.

The experimenter explained to each child that he was hoping to make the math program work more effectively. Each child was told that he would be receiving a green cover sheet every day with his math unit. On this sheet, all children were instructed to write their names and the date, as well as the page and problem number on which they started and ended the math period. At this point, the instructions diverged for the different treatment conditions.

In the goal-setting condition, the subject was given a sheet which had a space for the student to write the page and problem number where he started each day, the page and problem number up to which he thought he could work that period (the goal) and the page and problem number where he actually finished at the end of the hour. The children in this condition were instructed that each day when they received their units, they should look ahead and decide how far they could get if they worked hard for the whole period. They were asked to record this goal on their cover sheets and were also asked to make a mark in their unit which they could refer to as they worked, to see how close they were coming to their goal. At the end of each period, they were to write down the page and problem number they had reached and to check to see if they had achieved their goal.

In the self-monitoring condition, the subject was given a sheet with a space to mark the page and problem where he stopped working each day. The children in this condition were told that during the math period each day, they should note from time to time whether or not they were actually working on their math units. If, when the child chose to monitor himself, he found himself working appropriately, he was asked to put a "+" in one of 12 blank boxes that were on the cover sheets for the self-monitoring conditions. If, on the other hand, he was not working at this point, he was asked to put a "-" in one of the boxes. The children were given clear examples to clarify what was meant by "working" and "not working," consistent with the observer's coding system. At the end of the math period each day, they were also instructed to mark the page and problem number where they had stopped working.

Subjects in the goal-setting and self-monitoring condition received instructions for both kinds of recording, and were given sheets which contained both spaces for recording daily goals and boxes for recording self-monitored study behavior.

Subjects in the control condition received sheets containing only a space for them to record the page and problem number where t[illegible] stopped working each day. They were asked to fill in this space at the end of each math period, but were given no instructions on either self-monitoring or goal-setting techniques.

Children in all conditions were reminded of the importance of doing well in mathematics. All subjects were urged to work hard and to complete their math units as quickly and accurately as possible, to equate the groups on nonspecific motivational factors. Children were aware that instructions varied, and were told we were asking them to try different techniques to attempt to find out which ones worked best. Beginning on the second day of the treatment phase, each child received his math unit each day with the appropriate cover sheet clipped to the top of the unit. After the units had been handed out, the teacher reminded the class that they should fill out their cover sheets before beginning to work. To avoid potential teacher biases, however, teachers remained blind throughout the study to the treatment conditions of individual children. The behavior observations were collected as in the baseline phase for the five weeks which the treatment phase lasted.

During the treatment phase, further intervention was limited to children who completely failed to fill in their forms, and who received a re-explanation of either the goal-setting or self-monitoring instructions. With this exception, children received no further feedback or interaction from the experimenter, with neither positive nor negative repercussions for the subjects' reported study behavior or goal accomplishment nor any social reinforcement contingent on an increase in actual work output.

The two primary dependent measures of interest in this study involved observations of the students' study behavior during the daily math periods and children's actual academic achievement in successfully completing their assigned mathematics units. For the behavior observations, the number of ten-second periods that each child's behavior was recorded as falling into each of the five categories was calculated. Of principal interest was a comparison across groups and phases of the amount of time spent in on-task (working and at teacher's desk) versus off-task behavior (in seat-not working, out of seat-not working, and out of room).

An index of the children's actual progress in the math program was derived by calculating the total number of problems children completed accurately and computing a problems-per-day rate. For each unit the children worked on, the number of problems completed was multiplied by the final accuracy the child received. This measure assessed both the quantity and the quality of the children's work.

Ancillary data on the children's usage of the self-monitoring and goal-setting techniques were also obtained from the sheets the children filled out. Children in the goal-setting conditions were coded as to whether or not they reached their stated goals or if they failed to even set goals. In the self-monitoring conditions, children were scored on the number of "+s" and "-s" they recorded, as well as the number of times they failed to fill out this form.

Results

The experiment, therefore, provided data on the effectiveness of the goal-setting and self-monitoring treatments in modifying both the amount of time children spent engaged in appropriate study behavior during their mathematics periods and the children's academic progress through the mathematics program during the course of the study. Preliminary analyses of the data revealed no significant differences as a function of the sex of subjects or interaction of sex of subject with treatment conditions on either measure;

the data were therefore collapsed across this dimension for further analysis. Similarly, preliminary analyses yielded no significant differences among conditions in either study behavior or task accomplishment during the baseline phase of the study and no evidence of significant trends in study behavior over time, within either the baseline or the treatment phase. As a result, the effects of the experimental treatments were analyzed in terms of difference scores assessing changes in subjects' behavior from the baseline to the treatment phases of the experiment.

Behavior Observations: Classroom Study Time

The first goal of the study was to assess the effects of the experimental treatments, as in previous studies, on the amount of time children spent engaged in appropriate study behavior during their mathematics periods. The relevant data, the changes from baseline to treatment phases in the mean percentage of time studying during the 50-minute math period for each of the four experimental conditions, are presented in Table 1.

Insert Table 1 about here

A 2 x 2 (self-monitoring x goal-setting) unweighted-means analysis of variance was performed on these data. This analysis yielded a significant main effect for the self-monitoring treatment, $F\ (1,63) = 4.70$, $p < .05$; but neither the main effect for the goal-setting treatment nor the interaction between the two factors approached significance, both F's < 1. An individual planned contrast, comparing the effectiveness of the combined treatment group and the self-monitoring only condition similarly failed to approach statistical significance, $F < 1$, and the means for the two groups are, in fact, reversed.

In short, as predicted, the provision of self-monitoring techniques, even in the absence of explicit external contingencies, proved an effective means of increasing the amount of overt classroom study behavior.[2] The provision of goal-setting instructions, however, did not affect study behavior; and, contrary to prediction, did not significantly enhance the effectiveness of the self-monitoring treatment.

Task Accomplishment: Rate of Correct Problem Solutions

Given these findings on observational measures of overt study behavior, it is of considerable interest to examine the results of the experimental treatments on subjects' actual progress through the mathematics curriculum during the course of the study. The relevant data for these comparisons, the changes from baseline to treatment phases in the mean number of problems correctly completed per day for each of the four experimental conditions, are presented in Table 2.

Insert Table 2 about here

Table 1

Mean Change in Percentage of On-Task Classroom Study Behavior, from Baseline to Treatment, by Conditions

		Goal-setting Instructions	
		Present	Absent
Self-monitoring Instructions	Present	+9.14%	+10.22%
	Absent	+1.07%	+ 2.74%

Table 2

Mean Change in Number of Problems Correctly Solved per Day,

from Baseline to Treatment, by Conditions

		Goal-setting Instructions	
		Present	Absent
Self-monitoring Instructions	Present	+8.78	+3.46
	Absent	+ .26	-1.79

As before, a 2 x 2 unweighted-means analysis of variance was performed on these data. as with the measures of appropriate study behavior, this analysis revealed a significant main effect for the self-monitoring treatment, F (1,63) = 5.30, p <.025; but no significant effect for either the goal-setting treatment, F (1,63) = 1.59, n.s., or the interaction between the two factors, both F's < 1. Again, a planned contrast comparing the combined treatment group to the self-monitoring only condition also failed to yield a significant effect, F (1,63) = 1.57, n.s.

These data, then, closely parallel the findings for classroom study behavior. Providing children with self-monitoring techniques increased their rate of progress through the individualized mathematics program employed in the school. Instructions to set performance goals, however, did not affect progress through the program nor did it enhance the effectiveness of the self-monitoring treatment.

Additional Analyses

Within the conditions in which subjects received instructions in self-monitoring and/or goal setting techniques, it is possible to examine the relationship between children's usage of these techniques and observed study behavior. These internal analyses, as might be expected, suggested generally that the appropriate use of these techniques during the treatment phase did relate to children's actual study behavior.

Within the self-monitoring conditions, for example, amount of observed on-task behavior was positively correlated with the number of "+s" the subjects recorded, r = .41, p <.025, and was negatively correlated with the number of "-s" the children recorded, r = .47, p <.01. These data suggest that subjects were indeed reasonably accurate in monitoring and recording their own behavior. Likewise, within the goal-setting conditions, the proportion of days on which a child set a performance goal and successfully reached it was correlated with the amount of observed study time, r = .39, p <.05. Similarly, in the light of the differential effectiveness of the two procedures, it is interesting to note that subjects in the goal-setting conditions were much more likely to fail to use the goal-setting procedure than subjects in the self-monitoring conditions were to fail to use the self-monitoring procedure. This difference in the percentage of occasions on which subjects in the goal-setting (9.4%) and the self-monitoring conditions (2.7%) failed to complete their respective daily forms was highly significant, t = 3.38, p <.01. This differential usage of the two techniques was even evident within the combined treatment condition, where children within this condition failed to use goal-setting significantly more often than they failed to self-monitor, t = 3.71, p <.01. It was generally true, moreover, that failure to employ these forms proved to be negatively related to observed study behavior. For self-monitoring subjects, the correlation between the frequency with which subjects failed to complete the experimental forms and classroom study behavior was highly significant, r = -.52, p <.005; for goal-setting subjects, this correlation was in the same direction but was not individually significant, r = -.23, p <.20.

Finally, since the self-monitoring procedure produced significant increases in both study behavior and progress through the mathematics program, as indexed by the rate at which subjects accurately completed their units, it is somewhat surprising that these two measures were not significantly related, r = .09, n.s. Nor were there significant relationships between achievement in the math program and differences in subjects' usage of the self-monitoring technique. The effects of the self-monitoring treatment in academic achievement, therefore, appear to involve motivational effects beyond those evidenced in increased study time during the class periods.

Discussion

The results of this study provide compelling evidence that the introduction of a simple procedure for inducing children to systematically monitor their classroom behavior can produce significant increases in the incidence of appropriate study behavior displayed by an unselected sample of normal elementary school children. These gains, moreover, were maintained over the course of a five-week treatment phase, in the absence of any external feedback or rewards contingent upon actual or reported study behavior. Accompanying these increases in appropriate study behavior, in addition, were significant increases in actual academic achievement in the mathematics program presented during the periods when subjects were asked to monitor their study behavior.

That the striking success of this minimal "treatment" program seems appropriately attributable to the self-monitoring procedure itself, rather than to the general experimental demands of the situation or to other non-specific motivational factors, is clearly demonstrated by the factorial design employed. In this sense the failure of both the control group, equated in terms of demand characteristics and general encouragement, and the goal-setting treatment, which had been predicted to have a beneficial effect on study behavior, to produce gains within the same program increases our confidence in the efficacy of the self-monitoring treatment. Furthermore, since the self-monitoring program proved successful in a situation in which both the classroom teachers and observers remained blind to the treatment conditions of individual children, the present design avoids the potentially confounding effects of differential social approval and feedback evident in most case-study investigations of self-monitoring processes.

In view of the previous mixed literature on the effects of self-monitoring (Kazdin, 1974b; Mahoney & Thoresen, 1974b), it is of some interest to examine in more detail the self-monitoring technique which proved effective in this study. One interesting possibility is that the present self-monitoring procedure proved particularly successful because it contained both an implicit evaluative component, in the recording of instances of both positive and negative behavior, and a general "plan" for maintaining on-task behavior. That is, subjects in self-monitoring conditions were told that recording of off-task behavior, which carried obvious negative connotations, shoud serve as a cue for them to return to appropriate on-task behaviors. Patterson and Mischel (1975), for example, have, in a laboratory context, documented the efficacy of such plans for facilitating young children's continued on-task behavior in the face of varied distractions. Interestingly in this study, provision of a simple plan for resisting distraction did not affect the number of occasions on which children were distracted; children equipped with a plan were, however, able to return to appropriate on-task behavior with a shorter off-task interval. Hence, in the present classroom situation, where continuous task engagement is desirable but distraction is inevitable, a continuing self-monitoring procedure may provide a method by which the child becomes more conscious of his off-task behavior and is able to return to work more quickly after an interruption.

By comparison with the effects of the self-monitoring procedure, the failure of the goal-setting procedure to influence children's behavior is perhaps informative. In retrospect, relative to previous studies, the complex and heterogeneous mathematics materials involved in the study may have been particularly ill-suited for the successful application of goal-setting techniques, as the difficulty level of the problems contained in the curriculum varied widely from unit to unit and sometimes from page to page, making reasonable estimates of daily achievement a complicated process. Furthermore, completed units which failed to achieve a minimal accuracy criterion were returned to students for correction, again posing a potentially difficult estimation problem for the child. A comparison of the negative results in the present study with previous demonstrations of the effectiveness of goal-setting procedures in altering performance in other contexts suggests the hypothesis that goal-setting procedures may be most appropriate for tasks characterized by a more constant or predictable difficulty level, although perhaps even in the present situation, goal-setting might have proved effective had the children been given more extensive training on how to set challenging, yet reasonable, goals for themselves.

Finally, although overall similar findings were obtained in this study on measures of both classroom behavior and actual achievement, the lack of correlation between these two measures emphasizes the need for caution in assuming that techniques which are successful in altering classroom study behavior will necessarily result in increased academic achievement (Winnett & Winkler, 1972). Previous studies provide examples both of cases in which increases in classroom behavior produced concomitant increases in academic performance (Cobb, 1972; Cobb & Hops, 1973) and cases in which changes in classroom behaviors were not accompanied by changes in achievement (Ferritor, Buckholdt, Hamblin, & Smith, 1972; Sulzer, Hunt, Ashby, Koniarski, & Krams, 1971) but have failed to report data on the relationship between these two measures. The present data suggest that the assumption of a simple linear connection between study behavior and academic performance may frequently be unwarranted, and indicate the utility of an independent assessment of both variables in future research.

At the same time, the success of the present simple self-monitoring procedure, involving very minimal "costs" on the part of the school personnel involved in its administration (cf. Patterson, Ray, & Shaw, 1968) in eliciting substantial increases in classroom decorum and academic achievement in "normal" classrooms suggest the possibility that such techniques may have significant therapeutic value in applied behavior change programs. In many clinical applications, of course, one might wish to employ more effortful and extensive treatments, involving the use of explicit self-reinforcement or self-punishment procedures or the addition of contingent social approval from the school staff, to produce more substantial changes in behavior (Mahoney, 1970); even in such cases, however, the use of such a "reactive" self-monitoring procedure may serve as a useful component in a more complex treatment package.

Footnotes

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2. In comparison to the results of some case studies, the magnitude of the present effect may appear relatively small. It should be noted, however, that this study involved an unselected sample of virtually every student in three classrooms, many of whom were working well initially, rather than the selection of subjects because of extreme disorderly conduct or poor academic achievement. In addition, the mass administration of the treatments in the current study eliminates the increased individualized attention and social feedback which may be an additional beneficial factor in related case study investigations.

SIGNIFICANCE

A central problem in any educational program is that of maintaining children's motivation to insure that each child in such a program will achieve the maximal level of success which his abilities and aptitudes allow. Historically, when faced with children with "motivational" problems, psychologists have typically turned to manipulations of the reinforcing contingencies in the child's external environment to provide more powerful incentives for the child to engage in the activities deemed desirable. Such an approach has considerable merit.

In recent years, however, there has been a growing recognition of the incompleteness of a model of human behavior based entirely on an examination of external incentives. Among professionals concerned both with a theoretical understanding of reinforcement processes and with the application of reinforcement principles to produce behavior change in natural educational settings, there has been an increasing awareness that such a focus on externally-imposed and controlled reinforcement ignores an important facet of human behavior--namely, the predisposition of individuals to regulate their own behavior through self-imposed performance standards and self-administered reinforcement. Particularly in achievement-oriented situations, the ways in which children set standards for their own performance, and evaluate themselves with respect to these self-generated demands may be at least as important in determining their behavior as the external contingencies imposed upon them by a particular situation.

The present studies have investigated the persistence and the generality of the effects of relatively simple modeling and instructional techniques in modifying the standards children will set for themselves and the manner in which children will monitor their progress towards their self-imposed goals. Though not specifically addressed to the question of designing an optimal "intervention strategy," the general implication of these studies for educational environments seems reasonably clear: that even relatively brief systematic attempts to affect children's goal-setting and self-monitoring behavior may have significant beneficial effects. In view of the minimal "costs" involved in the implementation of such procedures relative to the potential gains accrued, investigation into the adaptation of such techniques to educational contexts seems highly worthy of further pursuit.

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