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ABSTRACT

In an attempt to selectively improve student performance, one-half of a set of difficult test items from a FORTRAN programming class had handouts explaining the concepts underlying the items distributed to the students. Each handout contained a written learning objective, a short prose passage explaining the objective, and one or more practice test questions. Change scores were computed by subtracting the proportion of students getting a test item correct during the experimental quarter from the same proportion in a baseline quarter. Analysis showed no significant difference in the change scores for handout-treated-difficult items and untreated-difficult items. Since objectives had been shown to improve performance on difficult items in a previous experiment, the hypothesis was advanced that the effectiveness of an instructional improvement effort may depend on the learning situation in which the effort is made. (Author)

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An Attempt to Influence Selected Portions of Student Learning

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Abstract

In an attempt to selectively improve student performance, one-half of a set of difficult test items from a FORTRAN programming class had handouts explaining the concepts underlying the items distributed to the students. Each handout contained a written learning objective, a short prose passage explaining the objective, and one or more practice test questions. Change scores were computed by subtracting the proportion of students getting a test item correct during the experimental quarter from the same proportion in a baseline quarter. Analysis showed no significant difference in the change scores for handout-treated-difficult items and untreated-difficult items. Since objectives had been shown to improve performance on difficult items in a previous experiment, the hypothesis was advanced that the effectiveness of an instructional improvement effort may depend on the learning situation in which the effort is made.

Educational Assessment Center Project: 503

An Attempt to Influence Selected Portions of Student Learning

Edwin R. Anderson[1]

In past research we found that student performance on tests given in identical courses taught by several instructors showed stable learning patterns across classes (Anderson, 1975a,b). When proportions of students getting each test question correct (item-difficulty) are matched on the basis of test questions and then correlations are computed, the average correlation from several classes is between .70 and .80. The high correlations between classes indicate that questions answered easily by members of one class will likely be answered easily by another class and that questions the first class of students found difficult will tend to prove difficult for students of a second class.

The stability of performance exhibited by different classes of students in different quarters of the same course leads naturally to the idea of selective improvement efforts. If questions answered easily by one class are strongly predictive of the ease with which a second class will answer those same questions, there seems to be no reason to modify course materials designed to teach the concepts underlying the items. Since questions easily answered are against a performance ceiling, expending effort to revise course content related to them would most likely be wasted. Conversely, questions students have trouble answering strongly encourage the modification of associated course materials. (See Anderson, 1975b, for an example of the predicted treatment

[1]The author wishes to thank Professor Walter L. Dunn of the civil engineering department. Professor Dunn wrote the test questions used as measures and allowed the research to be done in his class. The cooperation of Professor William D. Scott in providing us with a second class for piloting half of the test questions is also appreciated.

X item-difficulty interaction).

The experiment reported here was designed to test our ability to improve performance on a subset of difficult test questions. We chose to modify the course by supplementing existing course materials with handouts. Since written objectives (e.g., Anderson, 1975b; Duchastel & Brown, 1973; Duchastel & Merrill, 1974; Kaplan & Rothkopf, 1974; Rothkopf & Kaplan, 1972) and practice questions (e.g., Frase, 1968; Rothkopf & Bisbicos, 1967; Watts & Anderson, 1971) have been shown to facilitate learning (at least some of the time), handouts including them were expected to improve performance. In addition, the handouts included a short prose passage specifically explaining the learning objective provided for each treated test item.

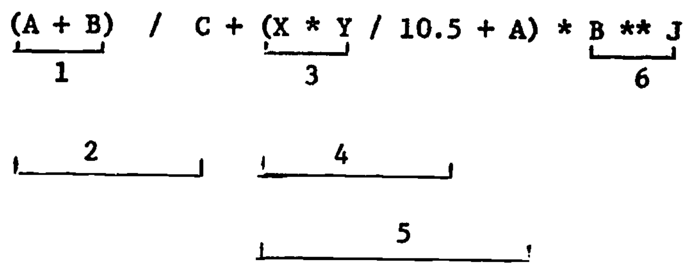
Method

Subjects. Since we had a large number of multiple-choice questions available, the questions were divided into two sets and given to two, fall quarter sections of the University of Washington introductory FORTRAN programming class. Data from these sections served as baseline data for the subsequent experimental quarter. The experiment was conducted in a single section of introductory programming; one of the instructors from the fall quarter served as the instructor of the experimental class. Students from all the classes arrived in the class through their own choice. The students were not offered any incentive for learning other than a gain in knowledge of programming. There were 34 students in the experimental class with a majority from the freshman and sophomore classes.

Course materials. Reading assignments for all three sections were taken from Nickerson, Fundamentals of Fortran Programming, 1975. The students were expected to learn most of the course content from the text; very little lecturing was done in class. Class sessions were devoted primarily to administrative detail, programming, testing (once a week), and individual help with programming problems. What lecturing there was duplicated the text.

Handouts were prepared for one-half of the difficult test items from the third through the fifth weeks of the quarter. Difficult items were those which fewer than 80% of the fall quarter pilot students answered correctly. Each handout consisted of a written objective stating what was to be learned, a short prose passage explaining the objective, and one or more practice questions with answers. Figure 1 is a sample handout written for the following difficult test question,

The order in which this expression would be evaluated by the computer is



A. 6, 3, 4, 2, 1, 5
B. 1, 2, 3, 4, 5, 6
C. 1, 3, 4, 5, 6, 2
D. 6, 3, 4, 5, 1, 2

Figure 1

<u>Written Objective</u>: Recall and recognize that parenthesis can be used to adjust the order of arithmetic operation precedence. All arithmetic operations within parenthesis are executed before arithmetic operations outside of the parenthesis are performed. If the parenthesis are nested within each other, the inside parenthesis is evaluated first.

<u>Explanation</u>: If a parenthesis is included in an arithmetic expression,

the inside of the parenthesis will be evaluated before the operations outside the parenthesis are performed. For example,

```
BAKER = ABLE / (DOG + CAT)
```

will have the values of DOG and CAT added first and will then divide ABLE by the sum. The parenthesis reverses the normal order of division and addition.

Within parenthesis the order of evaluation follows the exponent, multiply and divide, add and subtract order. For example,

```
BAKER = ABLE / (DOG + CAT * BIRD)
```

would be evaluated by first multiplying CAT and BIRD, second adding DOG to the product of CAT and BIRD, and finally dividing ABLE by the result of step 2.

When double parenthesis are present, the inside parenthesis takes precedence, e.g.

```
BAKER = ABLE / (DOG + (CAT * BIRD - RABBIT))
                      |__________|
                           1
                      |____________________|
                                 2
                 |_________________________|
                            3
          |________________________________|
                          4
```

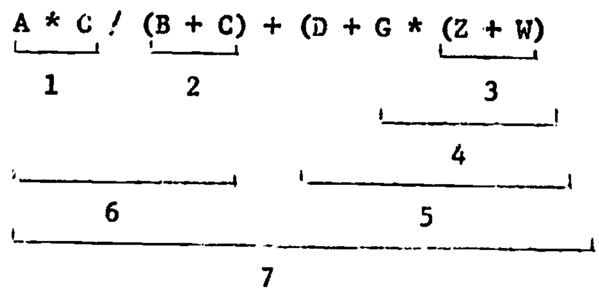
is evaluated in the 1, 2, 3, 4 order.

Finally if there is more than one set of parenthesis, they are evaluated from left to right. For example,

```
BAKER = (ABLE * FOXTROT) / (DOG + (CAT * BIRD - RABBIT))
        |_______________|         |__________|
               3                       1
                                  |____________________|
                                              2
                           |___________________________|
                                         4
        |______________________________________________|
                              5
```

gets evaluated in the 1, 2, 3, 4, 5 order.

<u>Practice question</u>: What is the order of evaluation of the following expression?



A. 1, 2, 3, 4, 5, 6, 7
B. 3, 4, 5, 2, 1, 6, 7
C. 2, 3, 4, 5, 1, 6, 7
D. 3, 2, 4, 5, 1, 6, 7

Answer: D (Given on a different page in the actual handout)

<u>Research Design</u>. In the fall quarter, baseline data was gathered on 150 test questions, 75 from each of two programming classes. The average item-difficulty was high, perhaps due to the use of weekly quizzes. Because of this high level of performance, difficult items were defined as those on which less than 80% of the students responded correctly (approximately 30% of the items were classified as difficult under this definition). Each test in the third through the fifth weeks of the experimental quarter was composed of equal numbers of items from three groups. Two groups were formed by pooling the difficult items from the baseline sections and then randomly splitting the pool. The first group, which we shall designate "treated-difficult", had handouts associated with them while the second group, "untreated-difficult", did not. The third group was formed by randomly selecting items from an easy-item pool until the number of easy items matched the number of test items in the treated-difficult group. The mean item-difficulties from the baseline sections were 60.99 for the untreated-difficult group ($N = 21$), 64.31 for the treated-difficult group ($N = 22$), and 92.34 for the easy-item group ($N = 22$). The two difficult-item groups did

not differ in the baseline section ($\underline{t}_{41}$ = .70, p>.10). The handouts were prepared after the difficult-items were split. The students were given written instructions for using the handout in which they were told that the handouts were specifically directed at concepts prior students had trouble learning. Special pains were taken in the instructions to encourage the students to closely study the handouts. The handouts were distributed four days prior to testing.

Results

The stability of the test data from the baseline quarter to the experimental quarter is illustrated by the high correlation (r = .79) between the item-difficulties of the 43 untreated items (untreated-difficult items plus easy items).

Change scores were produced by subtracting experimental quarter item-difficulties from those collected in the baseline quarter. The change scores for the untreated-difficult and treated-difficult items did not differ significantly ($\underline{t}_{41}$ = -.51, p> .10). All three groups showed changes in the direction predicted for regression effects (See table 1).

Table 1

Mean change scores in the programming experiment

Treatment group	Mean change scores
Untreated-difficult	-4.47
Treated-difficult	-6.81
Easy-items	2.81

The failure to find a difference between the two difficult groups is quite surprising since the handouts often directly explained the test questions used to assess the objective provided to the student.

It is of interest that students in the winter class rated the handouts favorably (28 of 30, 27 of 29, and 24 of 28 students rated three questions concerning the handouts as excellent of good on a scale of excellent, gooa, fair, poor) even though the handouts did not detectably influence test performance.

Discussion

The failure to influence student learning with the handouts is puzzling. Since the handouts were specifically directed at difficult questions, the problems of producing generalization to non-similar materials were circumvented. From student reports and from data collected at the university computer center, we have concluded that students spend a large amount of time solving the assigned weekly programming problems. They may well be devoting all of the time they allot to the programming class in actual programming. Viewed from this perspective, the handouts are peripheral to the behavior they are engaged in. Because of the heavy emphasis on programming, classroom testing, which normally demands much of the students' attention, may be reduced in importance. If this explanation for the lack of results is correct, selective influence attempts might prove successful in more traditional, test-oriented classes.

Anderson (1975b) showed that supplying students with written objectives as a supplement to existing learning materials effectively increased student performance. The failure of this experiment to produce increases with more extensive supplements is suggestive. The effectiveness of pedagogical techniques may depend significantly on the context in which they are used. Variation in context may account for the mixed findings in the literature regarding the use of written objectives (see Duchastel and Merrill, 1973, for a review). This line of reasoning makes it clear that instructors can not simply assume the efficacy of various teaching techniques; each technique must be experimented with in the specific context in which it is used.

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