

# Teacher Effectiveness and Learning for Mastery

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**ABSTRACT** The effect of an excellent or master teacher's use of learning for mastery (LFM) procedures on student learning was examined. Although LFM research strongly supports a procedural effect, close scrutiny suggests a possible teacher-effect/procedural-effect confounding. The authors extended previous research reporting a main effect for mastery learning when the teacher was rated average, but no main effect for an excellent or master teacher. Performance in 2 mastery and 2 traditional classes of intermediate algebra, taught by a master teacher, was compared. Findings from a  $2 \times 2$  randomized factorial design that controlled for repeatable or mastery testing, the LFM procedure, and pretesting indicated that student achievement on a final examination did not differ across mastery and control classes; however, instructor time was more than double in the mastery classes.

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Thirty years after the publication of Bloom's (1968) landmark article, "Learning for Mastery," the basic tenets and assumptions of learning for mastery (LFM), or mastery learning have been researched, applied, and evaluated again and again. The Educational Resources Information Center data base lists nearly 2,000 articles on mastery learning, and five of the six major research syntheses substantiate the method's positive effect on student achievement (Block & Burns, 1976; Burns, 1986; Guskey & Gates, 1986; Guskey & Pigott, 1988; C. C. Kulik, Kulik, & Bangert-Drowns, 1990).

Nonetheless, questions remain not only about the method's overall effectiveness but also about the validity and interpretation of the research. Slavin (1986, 1987, 1990), whose "best-evidence synthesis" of the research has been criticized as misleading, emphasized the heavy reliance on experimenter-made tests in determining achievement and suggested research biases in favor of mastery learning. Lai and Biggs (1994) explored biases in the method's application and found that it favors surface learners and surface learning. J. G. R. Martinez and Martinez (1988, 1992) considered biases toward procedure, along with a concomitant neglect of teacher effects in research design and interpretation.

Although the questions may seem minor in the face of the strong evidence supporting mastery learning, they serve a useful function if they help researchers achieve a more bal-

anced perspective and a better understanding of the method's success. What makes mastery learning work? Is its positive effect primarily affective or cognitive or both? Does the method alone cause success, or does a caring teacher play an important role?

## *Controlling for Teacher Effects*

The tendency of researchers to overemphasize procedure and underemphasize teachers was noted as early as 1976. In a review of 96 studies, Hursh (1976) warned of a possible confounding between procedural and teacher effects. J. A. Kulik, Kulik, and Cohen (1979) found evidence of "Method  $\times$  Type of Instructor Assignment interaction" in their meta-analysis of personalized system of instruction (PSI) research (p. 316).<sup>1</sup> Although that result was not duplicated in their 1990 meta-analysis (C. C. Kulik, Kulik, & Bangert-Drowns, 1990, p. 278), the data reported show no controls for quality of teaching—a factor that has been shown to affect mastery-learning outcomes significantly (J. G. R. Martinez & Martinez, 1992). In our 1988 analysis, which focused on the teacher's role in mastery learning, we concluded that 88% of the studies reviewed lacked adequate control for teacher effects, and 56% made no attempt at control (J. G. R. Martinez & Martinez, 1992, p. 23).

The few studies that do control rigorously for teacher effects either do not support a procedural effect for LFM or support a procedural effect only with qualifications.<sup>2</sup> In an experiment with calculus students at the Air Force Academy, Thompson (1980) found no main effect for the LFM procedure; however, he reported that "instructor experience was a significant factor on the departmental examination" (p. 366). Arlin (1984), working with elementary arithmetic classes, found no support for claims "that mastery learning procedures will minimize achievement differences and time differences simultaneously" (p. 117), and his data indicate a close tie between extra student

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learning with LFM and extra teaching time. Slavin and Karweit (1984) studied ninth-grade mathematics classes with a nested analysis of covariance (ANCOVA), with pretest as the covariate and class-teacher nested within the factors Mastery and Teams. They found no effect for the mastery component of instruction and concluded that "the effects of mastery learning may depend on providing additional time for the mastery-learning classes but not for control classes" (pp. 732-733; see also Guskey & Pigott, 1988, and Guskey, 1997, for criticism of Arlin's and Slavin & Karweit's research).

Slavin (1987), in his much-maligned but thought-provoking review of LFM research, used controlling for teacher or class effects as a key selective criterion for his "best-evidence synthesis" (pp. 204-205). He excluded single-teacher and multisubject studies because they "confounded teacher and class effects with treatment effects" (pp. 183-184). Unlike previous and subsequent reviewers, Slavin (1990) concluded that "group-based mastery learning has modest to nonexistent effects on student achievement," and he speculated that quality of instruction, feedback, and teaching materials may have affected outcomes significantly (pp. 300-301). (For criticism of Slavin's approach and conclusions, see C. C. Kulik, Kulik, & Bangert-Drowns, 1990, pp. 287-292; J. A. Kulik, Kulik, & Bangert-Drowns, 1990; Guskey, 1997, pp. 179-180.)

### *Identifying Teacher Effects*

In response to evidence of a possible confounding, a series of studies was conducted to explore the role of the teacher in a group-based mastery-learning system. Participants were mathematics students at a medium-sized, private college. In the first study (J. G. R. Martinez & Martinez, 1988), a curriculum development project to determine whether the college should implement mastery methods in its remedial mathematics program, three volunteer instructors taught Introductory Algebra with mastery learning. A regression analysis employing dummy coding on the dichotomous variable (mastery method vs. traditional method) showed significantly higher achievement for the mastery students on a summative measure, the department final examination,  $F(1, 190) = 12.5, p < .001$ . However, the study also reported (p. 27) that student achievement and supervisor ratings of instructors used as a measure of teacher quality were significantly related,  $F(1, 190) = 25.14, p < .0001$ . The "teacher within treatment" variable qualified the initial finding, making a direct interpretation of main effect for treatment unwarranted, and also raised questions about masking effects.

In a second study, J. G. R. Martinez and Martinez (1992) studied teacher quality, measured by student and supervisor ratings and teaching experience, as an important variable. An average teacher and an excellent teacher each taught a mastery and a control section of Introductory Algebra using common texts, examinations, and methods. An analysis of

variance (ANOVA) showed a main effect for the mastery component and repeated testing,  $F(1, 84) = 14.25, p < .01$ , but also an interaction between the teacher and procedural factors,  $F(1, 84) = 4.01, p < .05$ . Probing the interaction with Scheffé tests involving orthogonal coefficients revealed that the main effect was a function of the performance of the average teacher's control group. There was no significant difference between performance by the excellent teacher's control and mastery groups, but there was a statistically significant difference between performance by both the average teacher's and the excellent teacher's control groups (pp. 359-360).

The findings emphasized questions explored by the current study: What happens when a master or excellent teacher uses mastery learning? Does the procedure have an effect on student achievement and, if so, is that effect statistically significant? Is the extra teaching time required by the mastery procedure justified by extra student learning? Of particular interest was whether the findings reported for the excellent teacher in the previous study would prove stable.

### *Confirming Teacher Effects*

As in our previous study (J. G. R. Martinez & Martinez, 1992), repeatable mastery testing was identified as a critical component of the experimental procedure (Abbott & Falstrom, 1977; Barkmeier, Duncan, & Johnston, 1978; Catanzano & Wilson, 1977; Chang, 1985; Deboer, 1980; Fuchs, Fuchs, & Tindal, 1986; Gaynor & Millham, 1976; Glucksman, 1973; C. C. Kulik, Kulik, & Bangert-Drowns, 1990, pp. 268-269; Livingston & Gentile, 1996; Martin & Srikameswaram, 1974; Rohm, Sparzo, & Bennett, 1986). In addition, Campbell and Stanley (1963) and Solomon and Lessac (1968) found that pretesting affected posttest performance; therefore, we included it as an independent variable. A final summative test, which was not repeatable, assessed achievement in both the experimental and control classes.

## **Method**

### *Participants*

Participants in this study were enrolled in a basic skills mathematics course titled Math 132, Intermediate Algebra. The setting was the same as in the previous studies—a medium-sized, private college in the Southwest. The students were typically in their 1st year of college, with a mean age of 29; 56% were women and 44% were men; 51% were minorities.

Eighty students were enrolled in four sections of Intermediate Algebra, with 20 students in each section. They were randomly assigned to treatment conditions—two experimental groups and two control groups. All participants had taken and received a grade of "C" or better in Math 120, Introductory Algebra, which was a prerequisite.

*Teacher Assignment*

The excellent or master teacher from the previous experiment (J.G. R. Martinez & Martinez, 1992) was selected for further study. Criteria for excellence included quantitative and qualitative measures. The teacher's student evaluations were consistently excellent, with a mean rating of 9.6 on a Likert-type scale with ratings ranging from *poor* (1) to *excellent* (10) for all courses taught. Evaluations by department heads and deans described the teacher as "outstanding," "an inspiration to students," and "a role model for aspiring teachers." Student and supervisor ratings at the three other colleges where the instructor had taught had been similarly outstanding, and the teacher had extensive experience—12 years in the college classroom and 5 years in basic skills courses.

As in the previous study, the teacher used a group-based rather than an individualized approach to instruction in both the experimental and the control sections of the course. The class format was interactive and included problem solving and board work designed to involve the entire class. Classroom dynamics were active rather than static; most students participated in most discussions. Unlike in the previous study, we kept a careful record of teaching time per class and per student, including time spent teaching class, preparing and grading tests, meeting with students during office hours, and providing corrective feedback.

*Experimental Design*

We used a 2 × 2 randomized factorial design that was an example of a Solomon and Lessac (1968, p. 147), pretest/posttest four-group design (see Table 1).

There were two independent variables: testing frequency (repeatable mastery testing—up to three attempts on a chapter test vs. only one attempt) and pretesting. The two classes that permitted only one attempt were control groups for mastery testing; the two classes that did not pretest were control groups for pretesting.

The two independent variables were varied across the following four treatments:

1. Pretesting and repeatable mastery testing
2. Repeatable mastery testing, but not pretesting
3. Pretesting, but not repeatable mastery testing
4. Neither pretesting nor repeatable mastery testing

The dependent variable was final examination performance. The following null hypotheses were tested at the .05 level:

1. There will be no main effect for repeatable mastery testing.
2. There will be no main effect for pretesting.
3. There will be no repeatable mastery testing–pretesting interaction.

*Procedure*

During the semester-long experiment, the classes shared common course objectives, syllabi, and textbooks. All students were given the same final summative examination and seven formative chapter tests (Form A); in addition, however, students in the repeatable mastery-testing experimental groups were allowed two repetitions for each chapter test using alternate test Forms B and C. The alternate forms covered the same objectives and content as Form A, but to avoid a practice effect (Catanzano & Wilson, 1977; Deboer, 1980; Glucksman, 1973), the difficulty of the problems on each form was increased. Both the experimental and control groups were allowed only one attempt on the final examination.

Students in the pretesting experimental groups were pretested with a departmental examination during the first week of class. The pretest included the following three types of items designed to evaluate previous learning as well as to assess readiness for the current course: (a) items covering the objectives of the previous course, Introductory Algebra; (b) logic items posed in mathematical language; and (c) items covering the objectives of the current course, Intermediate Algebra.

The pretest and all Form A chapter tests were administered in class by the teacher. Forms B and C were given to individual students in the testing center. All chapter tests were graded by the teacher; corrective feedback on Forms B and C for the repeatable mastery testing experimental groups were provided in the teacher's office. Final examinations were graded blind by a single grader. A passing score on both the chapter tests and on the final examination was 70%.

**Results**

Table 2 shows the final examination means for all four classes.

The classes finished the semester with unequal *ns*; however, the inequality was minimal, precluding the need for an adjustment. Three of the four classes finished with *ns* of 17, and the fourth group finished with an *n* of 15.

Table 3 presents the results of the ANOVA completed on the data. The analysis revealed no main effect,  $F(1, 62) = .22, p > .05$ , for repeatable mastery testing, and no main effect for pretesting,  $F(1, 62) = 1.15, p > .05$ . Also, there was no interaction between the factors,  $F(1, 62) = .01, p > .05$ .

**Table 1.—Solomon Four-Group Design**

Treatment	Experimental group		Control group	
	1	2	1	2
Pretest	X		X	
Experimental treatment (up to three attempts)	X	X		
Posttest	X	X	X	X

**Table 2.—Final Examination Means**

Factor A (test frequency)	Factor B Pretest	
	Yes	No
$f_3$	82.00	78.29
$f_1$	80.20	77.41

Note. Final-examination means for four groups—Factor A ( $f_3$ ) represents the testing frequency experimental treatment; Factor A ( $f_1$ ) represents the control treatment; Factor B shows pretesting for control or experimental treatments.

**Table 3.—Summary of Testing Frequency (A) × Pretesting (B) Analysis of Variance for Final Examination Means**

Source	SS	df	MS	F
A	33.63	1	33.63	.22
B	179.88	1	179.88	1.15
A × B	1.80	1	1.80	.01
Within cell	9,676.05	62	(156.07)	

Note. Value in parentheses represents mean square error. For all *F* tests,  $p > .05$ .

**Table 4.—Summary of Analysis of Covariance With Pretest Scores as the Covariate**

Source	SS	df	MS	F	<i>p</i>
Between groups	42.80	1	42.80	.334	> .05
Within groups	3,716.58	29	128.16		

Additional analysis examined the effect of actual pretest scores on final examination performance. While controlling for the mastery-testing factor, we designated pretest scores as the covariate to permit a direct analysis of their effect (Campbell & Stanley, 1963).

We completed a covariate adjustment (Kirk, 1968), and Table 4 presents the results of the ANCOVA.

Holding pretesting constant while varying frequency of test attempts made no significant difference on final examination performance. Therefore, the ANCOVA did not change or qualify the initial ANOVA findings, and the three null hypotheses were accepted.

Final examination means were further examined in relation to the number of teaching hours logged by the instructor.<sup>3</sup> For each point scored on the final examination by the two control classes, the instructor expended 1.4 hr and 1.45 hr, respectively. For each point scored on the final examination by the two mastery classes, the instructor expended 3.32 hr and 3.49 hr, respectively. To determine a teaching efficiency rating for each class, we divided the final examination mean score by the number of teaching hours:

$$\frac{\text{Mean score}}{\text{Teaching time}} = \text{Teaching efficiency}$$

Efficiency was highest for the control classes, .77 and .69, and lowest for the mastery classes, .30 and .29.

## Discussion

Acceptance of all three null hypotheses suggests strongly that performance of the students in the four classes was stable across experimental variations and, concomitantly, that the effect of the master teacher remained constant across variations in the mastery-learning procedure and in the pretesting condition. Of special interest, however, is the performance of the control groups. How do they compare to other traditionally taught classes not included in the experiment? Are they typical or atypical, representative or unrepresentative?

To explore those questions, we collected final examination data from four Intermediate Algebra classes not included in the experiment. The four classes were similar to the control groups in terms of class size, texts used, chapters covered, class format, number of tests given during the semester, and number of attempts allowed per test. All four classes also were given the same final examination as the classes included in the experiment, and the examinations were scored by the blind grader used in the experiment. A regression analysis was performed to determine whether the weighted mean of 78.72 for the study's control groups differed significantly from the weighted mean of 68.68 for the four Intermediate Algebra classes not included in the experiment. Dummy coding was used to differentiate between the dichotomous treatment variable of experimental control versus nonparticipating classes. Our analysis showed a statistically significant difference,  $F(1, 115) = 5.2$ ,  $p < .05$ , supporting the contentions that the control classes were not representative of traditional classes and that the teacher, rather than the procedure, affected student performance.

Although learning outcomes for the classes in the experiment did not differ significantly, there was a dramatic difference in the teaching time required by the mastery and control groups. The mastery classes required more than twice as much of the teacher's time as the control classes, without commensurate increases in student achievement, as represented by final examination performance. That this extra expenditure of time was not only inefficient but also unnecessary seems probable, because all of the master teacher's classes, experimental and control, scored well above the departmental passing level of 70.

Whether mastery learning involves inefficient use of time has been debated for years. In a tightly controlled, laboratory-style experiment with seventh graders, Arlin and Webster (1983) looked specifically at student time spent learning. They found that nonmastery students were more time efficient, "considering amount of knowledge retained on the retention test to amount of time studied" (p. 194), and that

mastery students “required almost twice as much time as nonmastery students” (p. 193). Arlin and Webster’s analysis was disputed by Guskey and Pigott (1988); C. C. Kulik, Kulik, and Bangert-Drowns (1990) found only a 4% increase for instructional time in the typical study controlling for that variable (p. 281). Nonetheless, the current study does not support the premise, commonly accepted by some LFM researchers, that additional time on task results in increased achievement, regardless of the quality of instruction. Nor does it support the idea, implicit in mastery-learning theory, that mastery methods are especially beneficial to students who rank low in the class and presumably need additional time for remediation.

A comparison of the bottom and top thirds of the pretested mastery group and pretested control group showed greater gains for the bottom thirds in both classes, but an even greater gain in the control group than in the mastery group. The bottom third of the mastery group showed twice as much gain as the top third, whereas the bottom third of the control group showed three times as much gain as that class’s top third. The mastery treatment and extra teaching time did not, therefore, disproportionately benefit the lower ranked students. In addition, standard deviations for achievement increased twice as much in the mastery group as in the control group, contesting Bloom’s (1976, 1981) claim that mastery methods decrease variability (see also Livingston & Gentile, 1996).

The finding of no main effect for mastery learning when classes are taught by a master teacher remained stable across studies using various experimental designs and students enrolled in different mathematics courses. Moreover, the finding became stronger as our study focused more closely on the master teacher. One practical implication could be that master teachers do not need the assistance of mastery methods to increase student achievement and, therefore, can eschew the time-consuming chores commonly accepted as part of LFM (Arlin, 1984; Arlin & Webster, 1983; Block, 1973; Slavin, 1987; Stockdale, 1986).

Another way to deal with the extra-time issue is to assign some of mastery learning’s time-consuming chores either to computers or to the students. Computer Assisted Instruction (CAI) can dramatically reduce extra-time tasks such as grading the different versions of formative tests, tracking student progress, and even providing corrective feedback. Involving students in learning management also reduces extra demands on teacher time. In Student Managed Mastery Learning (SMML), teachers “macromanage,” while students “micromanage.” While teachers set goals and evaluate outcomes and progress, the students select materials, diagnose problems and seek remediation, monitor progress, self-motivate, and correct homework and tests (J. G. R. Martinez & Martinez, 1996, pp. 129–132). SMML frees teachers to teach, allowing them “to take an active, creative role in the learning process” and at the same time “empowers students by placing them in charge of information” (p. 131). Teaching efficiency (student achievement/teaching time) improves,

and the affective dimension of student learning, including attitudes about learning and testing, is given a positive boost (see also Martinez, 1987, pp. 123–124; C. C. Kulik, Kulik, & Bangert-Drowns, 1990, p. 285).

A second research implication evolves if one compares the current study with the previous studies (J. G. R. Martinez & Martinez, 1988, 1992). In previous research, a main effect for mastery learning was found that masks a possible teacher effect tied specifically to teacher quality—an effect that the current study substantiates further. However, if measures had not been taken to reveal the teacher effect, data from the first two studies could have been construed to support strongly, rather than question, the procedural effect. That possibility raises the question of whether a similar confounding might underlie at least a portion of the positive effect claimed by researchers for mastery learning.

Whiting and Render (1987) reported that “there are many advantages to mastery learning beyond the 80% successful learning that is not only possible but fact” (p. 278). Yet, is it not possible that at least a portion of the successful learning and other advantages must be attributed to the effect of an excellent teacher? J. A. Kulik, Jaksa, and Kulik (1978) suggested that most of the research they reviewed had been done by dedicated teachers who were willing to expend the extra time required—a suggestion supported by our experience. The volunteer teachers who participated in our first study (J. G. R. Martinez & Martinez, 1988) were those rated highest for quality of teaching by their supervisors. Mueller (1976), in “Mastery Learning: Partly Boon, Partly Boondoggle,” speculated that “teachers who enthusiastically embrace this model are more highly motivated and work harder than are teachers utilizing some alternative instructional models” (p. 45). He also cautioned that “harder working, more enthusiastic teachers using any instructional model would probably induce more positive student achievement than would uncommitted, unenthusiastic teachers using the same model” (p. 51).

The question of the master or “great” teacher was dismissed 30 years ago by Keller (1968) as a “mystique,” hampering “sober analysis of the critical contingencies” of successful learning (p. 86). Keller accused teachers of being “at best” 10% efficient in educating students (p. 88). In 1984, Bloom reiterated that negative assessment with his claim that teachers’ behaviors keep “80% of students from learning” (p. 12). That great teachers and great teaching may be critical contingencies of successful learning seems to have been overlooked by a majority of researchers in the mastery-learning paradigm. However, researchers in a different paradigm, teaching effectiveness or process-product research, have shown that positive teacher behaviors produce positive student outcomes (Evertson & Green, 1986; see also Rodriguez, Plax, & Kearney, 1996; Ryan & Harrison, 1995; Teven & McCroskey, 1996). Moreover, as research methods have become more sophisticated, findings have become increasingly stable (Brophy, 1986; Brophy & Good, 1986; Wittrock, 1986).

It is not unusual for different research paradigms to yield various and sometimes conflicting findings and interpretations (Kuhn, 1970). However, if teachers do affect student achievement in terms of measurable changes in performance (positive or negative), then whatever paradigm is used, researchers should use adequate controls for teacher effects. Studying teacher effectiveness within a mastery-learning context could even help resolve some contentious issues, such as disagreements over best-evidence research and effect sizes (Guskey, 1997; C. C. Kulik, Kulik, & Bangert-Drowns, 1990; J. A. Kulik, Kulik, & Bangert-Drowns, 1990; Slavin, 1987, 1990) and at the same time promote the development of more effective mastery-learning applications and materials.

### NOTES

1. Personalized System of Instruction (PSI) and LFM are, according to Bloom, the "two major approaches to mastery learning" (Brandt, 1979, p. 159). Often practitioners mix elements of the two procedures, and researchers including C. C. Kulik, J. A. Kulik, and a variety of co-authors usually review both types of studies.

2. Note also that these studies, as well as our research, focus on mathematics classes at a variety of levels. J. A. Kulik, Kulik, and Bangert-Drowns found that large effect sizes for mastery learning are more likely "in the social sciences rather than mathematics, the natural sciences, or humanities" (1990, p. 285).

3. Our findings here differ dramatically from those of C. C. Kulik, J. A. Kulik, and Bangert-Drowns, who found that in "the typical (or median) case, the experimental group required 4% more instructional time than did the control group" (1990, p. 281). See Guskey, 1997, pp. 166–170, and Slavin, 1987, pp. 199–202, for differing points of view.

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