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REVIEW OF EDUCATIONAL RESEARCH 2006 76: 1
DOI: 10.3102/00346543076001001

Review of Educational Research
Spring 2006, Vol. 76, No. 1, pp. 1–62

Does Homework Improve Academic Achievement? A Synthesis of Research, 1987–2003

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In this article, research conducted in the United States since 1987 on the effects of homework is summarized. Studies are grouped into four research designs. The authors found that all studies, regardless of type, had design flaws. However, both within and across design types, there was generally consistent evidence for a positive influence of homework on achievement. Studies that reported simple homework–achievement correlations revealed evidence that a stronger correlation existed (a) in Grades 7–12 than in K–6 and (b) when students rather than parents reported time on homework. No strong evidence was found for an association between the homework–achievement link and the outcome measure (grades as opposed to standardized tests) or the subject matter (reading as opposed to math). On the basis of these results and others, the authors suggest future research.

Keywords: homework, meta-analysis.

Homework can be defined as any task assigned by schoolteachers intended for students to carry out during nonschool hours (Cooper, 1989). This definition explicitly excludes (a) in-school guided study; (b) home study courses delivered through the mail, television, audio or videocassette, or the Internet; and (c) extracurricular activities such as sports and participation in clubs. The phrase "intended for students to carry out during nonschool hours" is used because students may complete homework assignments during study hall, library time, or even during subsequent classes.

Variations in homework can be classified according to its (a) amount, (b) skill area, (c) purpose, (d) degree of choice for the student, (e) completion deadline, (f) degree of individualization, and (g) social context. Variations in the *amount* of homework can appear as differences in both the frequency and length of individual assignments. Assignments can range over all the *skill areas* taught in school.

The *purposes* of homework assignments can be divided into (a) instructional and (b) noninstructional objectives (cf. Epstein, 1988, 2001; Epstein & Van Voorhis, 2001; Lee & Pruitt, 1979). The most common instructional purpose of homework is to provide the student with an opportunity to practice or review material that has already been presented in class (Becker & Epstein, 1982). Preparation assignments introduce material to help students obtain the maximum benefit when the new material is covered in class (Muhlenbruck, Cooper, Nye, & Lindsay, 1999). Extension homework involves the transfer of previously learned skills to new situations

(Lee & Pruitt, 1979). Finally, homework can require students to integrate separately learned skills and concepts (Lee & Pruitt, 1979). This might be accomplished using book reports, science projects, or creative writing.

Homework has other purposes in addition to enhancing instruction. It can be used to (a) establish communication between parent and child (Acock & Demo, 1994; Balli, Demo, & Wedman, 1998; Epstein, Simon, & Salinas, 1997; González, Andrade, Civil, & Moll, 2001; Scott-Jones, 1995; Van Voorhis, 2003); (b) fulfill directives from school administrators (Hoover-Dempsey, Bassler, & Burow, 1995); and (c) punish students (Epstein & Van Voorhis, 2001; Xu & Corno, 1998). To this list might be added the public relations objective of simply informing parents about what is going on in school (Coleman, Hoffer, & Kilgore, 1982; Corno, 1996; Rutter, Maughan, Mortimore, & Ouston, 1979).

Homework assignments rarely reflect a single purpose. Rather, most assignments serve several different purposes; some relate to instruction, whereas others may meet the purposes of the teacher, the school administration, or the school district.

The *degree of choice* afforded a student refers to whether the homework assignment is compulsory or voluntary. Related to the degree of choice, *completion deadlines* can vary from short term, meant to be completed overnight or for the next class meeting, to long term, with students given days or weeks to complete the task. The *degree of individualization* refers to whether the teacher tailors assignments to meet the needs of each student or whether a single assignment is presented to groups of students or to the class as a whole. Finally, homework assignments can vary according to the *social context* in which they are carried out. Some assignments are meant for the student to complete independent of other people. Assisted homework explicitly calls for the involvement of another person, a parent or perhaps a sibling or friend. Still other assignments involve groups of students working cooperatively to produce a single product.

Overview

The Importance of Homework and Homework Research

Homework is an important part of most school-aged children's daily routine. According to the National Assessment of Educational Progress (Campbell et al., 1996), over two-thirds of all 9-year-olds and three-quarters of all 13- and 17-year-olds reported doing some homework every day. Sixteen percent of 9-year-olds reported doing more than 1 hour of homework each day, and this figure jumped to 37% for 13-year-olds and 39% for 17-year-olds. More recent surveys support the extensive use of homework, although the amount of homework that students report varies from study to study, depending perhaps on how the question is asked. For example, Gill and Schlossman (2003) reported recent declines in time spent on homework. However, among the youngest students, age 6 to 8, homework appears to have increased between 1981 (52 minutes weekly) and 1997 (128 minutes weekly; Hofferth & Sandberg, 2000).

Homework likely has a significant impact on students' educational trajectories. Most educators believe that homework can be an important supplement to in-school academic activities (Henderson, 1996). However, it is also clear from the surveys mentioned earlier that not all teachers assign homework and/or not all students complete the homework they are assigned. This suggests that whatever impact homework might have on achievement varies from student to student, depending on how much each student is assigned or completes.

Homework is often a source of friction between home and school. Accounts of conflicts between parents and educators appear often in the popular press (e.g., Ratnesar, 1999; Coutts, 2004; Kralovec & Buell, 2000; Loveless, 2003). Parents protest that assignments are too long or too short, too hard or too easy, or too ambiguous (Baumgartner, Bryan, Donahue, & Nelson, 1993; Kralovec & Buell, 2000; Warton, 1998). Teachers complain about a lack of support from parents, a lack of training in how to construct good assignments, and a lack of time to prepare effective assignments (Farkas, Johnson, & Duffet, 1999). Students protest about the time that homework takes away from leisure activities (Coutts, 2004; Kralovec & Buell, 2000). Many students consider homework the chief source of stress in their lives (Kouzma & Kennedy, 2002).

To date, the role of research in forming homework policies and practices has been minimal. This is because the influences on homework are complex, and no simple, general finding applicable to all students is possible. In addition, research is plentiful enough that a few studies can always be found to buttress whatever position is desired, while the counter-evidence is ignored. Thus advocates for or against homework often cite isolated studies either to support or to refute its value.

It is critical that homework policies and practices have as their foundation the best evidence available. Policies and practices that are consistent with a trustworthy synthesis of research will (a) help students to obtain the optimum education benefit from homework, and (b) help parents to find ways to integrate homework into a healthy and well-rounded family life. It is our intention in this article to collect as much of the research as possible on the effects of homework, both positive and negative, conducted since 1987. We will apply narrative and quantitative techniques to integrate the results of studies (see Cooper, 1998; Cooper & Hedges, 1994). While research rarely, if ever, covers the gamut of issues and circumstances confronted by educators, we hope that the results of this research synthesis will be used both to guide future research on homework and to assist in the development of policies and practices consistent with the empirical evidence.

A Brief History of Homework in the United States

Public attitudes toward homework have been cyclical (Gill & Schlossman, 1996, 2004). Prior to the 20th century, homework was believed to be an important means for disciplining children's minds (Reese, 1995). By the 1940s, a reaction against homework had set in (Nash, 1930; Otto, 1941). Developing problem-solving abilities, as opposed to learning through drill, became a central task of education (Lindsay, 1928; Thayer, 1928). Also, the life-adjustment movement viewed home study as an intrusion on other at-home activities (Patri, 1925; San Diego City Schools Research Department, 1936).

The trend toward less homework was reversed in the late 1950s after the Russians launched the Sputnik satellite (Gill & Schlossman, 2000; Goldstein, 1960; Epps, 1966). Americans became concerned that a lack of rigor in the educational system was leaving children unprepared to face a complex technological future and to compete against our ideological adversaries. Homework was viewed as a means of accelerating the pace of knowledge acquisition. But in the mid-1960s the cycle again reversed itself (Jones & Colvin, 1964). Homework came to be seen as a symptom of excessive pressure on students. Contemporary learning theories again questioned the value of homework and raised its possible detrimental consequences for mental health.

By the mid-1980s, views of homework had again shifted toward a more positive assessment (National Commission on Excellence in Education, 1983). In the wake of declining achievement test scores and increased concern about American's ability to compete in a global marketplace, homework underwent its third renaissance in 50 years. However, as the century turned, and against the backdrop of continued parental support for homework (Public Agenda, 2000), a predicable backlash set in, led by beleaguered parents concerned about the stresses on their children (Winerip, 1999).

Past Syntheses of Homework Research

Homework has been an active area of study among American education researchers for the past 70 years. As early as 1927, a study by Hagan (1927) compared the effects of homework with the effects of in-school supervised study on the achievement of 11- and 12-year-olds. However, researchers have been far from unanimous in their assessments of the strengths and weaknesses of homework. For example, more than a dozen reviews of the homework literature were conducted between 1960 and 1987 (see Cooper, 1989, for a detailed description). The conclusions of these reviews varied greatly, partly because they covered different literature, used different criteria for inclusion of studies, and applied different methods for the synthesis of study results.

Cooper (1989) conducted a review of nearly 120 empirical studies of homework's effects and the ingredients of successful homework assignments. Quantitative synthesis techniques were used to summarize the literature. This review included three types of studies that help answer the general question of whether homework improves students' achievement. The first type of study compared achievement of students given homework assignments with students given no homework. In 20 studies conducted between 1962 and 1986, 14 produced effects favoring homework while 6 favored no homework. Most interesting was the influence of grade level on homework's relation with achievement. These studies revealed that the average high school student in a class doing homework outperformed 69% of the students in a no-homework class, as measured by standardized tests or grades. In junior high school, the average homework effect was half this magnitude. In elementary school, homework had no association with achievement gains.

The next type of evidence compared homework with in-class supervised study. Overall, the positive effect of homework was about half what it was when students doing homework were compared with those not doing homework. Most important was the emergence once again of a strong grade-level effect. When homework and in-class study were compared in elementary schools, in-class study proved superior.

Finally, Cooper found 50 studies that correlated the amount of time students spent on homework with a measure of achievement. Many of these correlations came from statewide surveys or national assessments. In all, 43 correlations indicated that students who did more homework had better achievement outcomes, while only 7 indicated negative outcomes. Again, a strong grade-level interaction appeared. For students in elementary school, the average correlation between amount of homework and achievement was nearly $r = 0$; for students in middle grades it was $r = .07$; and for high school students it was $r = .25$.

The Need for a New Synthesis of the Homework Literature

There are three reasons for conducting a new synthesis of the homework literature: (a) to update the evidence on past conclusions about the effects of homework and determine if the conclusions from research need modification; (b) to determine whether some of the questions left unanswered by the earlier syntheses can now be answered; and (c) to apply new research synthesis techniques.

In the years since the completion of Cooper's (1989) meta-analysis, a substantial new body of evidence has been added to the homework literature. For example, a search of ERIC, PsycINFO, Sociological Abstracts, and Dissertation Abstracts between January 1987 (when the search for the earlier synthesis ended) and December 2003 indicated that over 4,000 documents with homework as a keyword had been added to these reference databases. When we delimited this search to documents that the reference engine cataloged as "empirical," nearly 900 documents remained. Yet we know of no comprehensive attempt to synthesize this new literature. Therefore, a reassessment of the evidence seems timely, both to determine if the earlier conclusions need to be modified and to benefit from the added precision that the new evidence can bring to the current assessment.

Cooper's meta-analysis revealed a consistent influence of grade level on the homework–achievement relationship. However, it produced ambiguous results regarding the possible differential impact of homework on different subject matters and on different measures of achievement. Specifically, research using different comparison groups (i.e., no homework, supervised study, correlations involving different reported amounts of homework) produced different orderings or magnitudes of homework's relation to achievement for different subject matters and achievement measures. Also, Cooper (1989) found uniformly nonsignificant relationships between the sex of the student and the magnitude of the homework–achievement relationship. However, some recent theoretical perspectives (Covington, 1998; Deslandes & Cloutier, 2002; Harris, Nixon, & Rudduck, 1993; Jackson, 2003) suggest that girls generally hold more positive attitudes than boys toward homework and expend greater effort on it. Emerging evidence from some homework studies (Harris et al., 1993; Hong & Milgram, 1999; Younger & Warrington, 1996) lends empirical support to these perspectives.

While these theories and results do not directly predict a stronger *relationship* between homework and achievement for girls than for boys (that is, they predict a main effect of higher levels of achievement among girls than among boys but do not indicate why differences in homework attitude and effort within the sexes would be more closely tied to achievement for one sex than the other, an interaction effect), they do suggest that this remains an important issue. Therefore, exploring these moderating relationships will be a focus of the present synthesis.

Also, the Cooper (1989) synthesis paid only passing attention to the ability of the cumulated evidence to establish a causal relationship between homework and achievement. Clearly, the 50 studies that took naturalistic, cross-sectional measures of the amount of time students spent on homework and correlated these with measures of achievement cannot be used to establish causality. About half of the studies that introduced homework as an exogenous intervention and then compared achievement for students who did homework with that of students who did not, or who had in-school supervised study, employed random assignment of students to conditions. The other half sometimes did and sometimes did not employ a priori matching or post hoc statistical equating to enhance the similarity of homework and no-homework groups. When homework was compared with no-homework, Cooper reported that studies that used random assignment produced positive effects of homework similar to nonrandom assignment studies. However, when compared with in-school supervised study, random-assignment designs revealed no difference between the homework and in-school study students. We will test to determine whether these findings still hold for the new evidence.

Also, since the earlier synthesis appeared, numerous studies have employed structural equation modeling to test the fit of complex models of the relationship between various factors and student achievement. Homework has been used as a factor in many of these models. The earlier synthesis did not include these designs, but this synthesis will.

Methodologically, the past two decades have introduced new techniques and refinements in the practice of research synthesis. These include, among others, two important advances. First, there is now a greater understanding of meta-analytic error models involving the use of fixed and random-error assumptions that add precision to statements about the generality of findings. Second, new tests have been developed to estimate the impact of data censoring on research synthesis findings. These give us a better sense of the robustness of findings against plausible missing data assumptions. We will use these in the synthesis that follows.

Potential Measures of the Effects of Homework

As might be expected, educators have suggested a long list of both positive and negative consequences of homework (Cooper, 1989; see also Epstein, 1988; Warton, 2001). Table 1 presents a list of potential outcomes that might be the focus of homework research and the potential measures of interest for this synthesis.

The positive effects of homework can be grouped into four categories: (a) immediate achievement and learning; (b) long-term academic; (c) nonacademic; and, (d) parental and family benefits. The immediate effect of homework on learning is its most frequent rationale. Proponents of homework argue that it increases the time students spend on academic tasks (Carroll, 1963; Paschal, Weinstein, & Walberg, 1984; Walberg & Paschal, 1995). Thus the benefits of increased instructional time should accrue to students engaged in home study. The long-term academic benefits of homework are not necessarily enhancements to achievement in particular academic domains, but rather the establishment of general practices that facilitate learning. Homework is expected to (a) encourage students to learn during their leisure time; (b) improve students' attitudes toward school; and (c) improve students' study habits and skills (Alleman & Brophy, 1991; Corno & Xu, 1998; Johnson & Pontius, 1989; Warton, 2001).

Also, homework has been offered as a means for developing personal attributes in children that can promote positive behaviors that, in addition to being important for academic pursuits, generalize to other life domains. Because homework generally requires students to complete tasks with less supervision and under less severe time constraints than is the case in school, home study is said to promote greater self-

TABLE 1
Potential effects of homework that might serve as outcomes for research

Potential positive effects
Immediate achievement and learning
Better retention of factual knowledge
Increased understanding
Better critical thinking, concept formation, information processing
Curriculum enrichment
Long-term academic benefits
More learning during leisure time
Improved attitude toward school
Better study habits and skills
Nonacademic benefits
Greater self-direction
Greater self-discipline
Better time organization
More inquisitiveness
More independent problem-solving
Parental and family benefits
Greater parental appreciation of and involvement in schooling
Parental demonstrations of interest in child's academic progress
Student awareness of connection between home and school
Potential negative effects
Satiation
Loss of interest in academic material
Physical and emotional fatigue
Denial of access to leisure time and community activities
Parental interference
Pressure to complete homework and perform well
Confusion of instructional techniques
Cheating
Copying from other students
Help beyond tutoring
Increased differences between high and low achievers

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direction and self-discipline (Corno, 1994; Zimmerman, Bonner, & Kovach, 1996), better time organization, more inquisitiveness, and more independent problem solving. These skills and attributes apply to the nonacademic spheres of life as well as the academic.

Finally, homework may have positive effects on parents and families (Hoover-Dempsey et al., 2001). Teachers can use homework to increase parents' appreciation of and involvement in schooling (Balli, 1998; Balli, Wedman, & Demo, 1997; Epstein & Dauber, 1991; Van Voorhis, 2003). Parents can demonstrate an interest in the academic progress of their children (Epstein & Van Voorhis, 2001; Balli, Demo,

& Wedman, 1998). Students become aware of the connection between home and school.

Some negative effects attributed to homework contradict the suggested positive effects. For instance, opponents of homework have argued that it can have a negative influence on attitudes toward school (Chen, & Stevenson, 1989), by satiating students on academic pursuits. They claim any activity remains rewarding for only so long, and children may become overexposed to academic tasks (Bryan, Nelson, & Mathru, 1995). Related to the satiation argument is the notion that homework leads to general physical and emotional fatigue. Homework can also deny children access to leisure time and community activities (Warton, 2001; Coutts, 2004). Proponents of leisure activities point out that homework is not the only circumstance under which after-school learning takes place. Many leisure activities teach important academic and life skills.

Involving parents in the schooling process can have negative consequences (Epstein, 1988; Levin, Levy-Shiff, Appelbaum-Peled, Katz, Komar, & Meiran, 1997; Cooper, Lindsay, & Nye, 2000). Parents pressure students to complete homework assignments or to do them with unrealistic rigor. Also, parents may create confusion if they are unfamiliar with the material that is sent home for study or if their approach to teaching differs from that used in school. Parental involvement—indeed the involvement of anyone else in homework—can sometimes go beyond simple tutoring or assistance. This raises the possibility that homework might promote cheating or excessive reliance on others for help with assignments.

Finally, some opponents of homework have argued that home study has increased differences between high- and low-achieving students, especially when the achievement difference is associated with economic differences (Scott-Jones, 1984; Odum, 1994; McDermott, Goldman, & Varenne, 1984). They suggest that high achievers from well-to-do homes will have greater parental support for home study, including more appropriate parental assistance. Also, these students are more likely to have access to places conducive to their learning style in which to do assignments and better resources to help them complete assignments successfully.

With few exceptions, the positive and negative consequences of homework can occur together. For instance, homework can improve study habits at the same time that it denies access to leisure-time activities. Some types of assignments can produce positive effects, whereas other assignments produce negative ones. In fact, in light of the host of ways that homework assignments can be construed and carried out, complex patterns of effects ought to be expected.

The present synthesis will search for any and all of the above possible effects of homework. However, it is unrealistic to expect that any but a few of these will actually appear in the research literature. We expected the large preponderance of measures to involve achievement test scores, school grades, and unit grades. A few measures of students' attitudes toward school and subject matters might also appear. Other measures of homework's effect were expected to be few and far between. One reason for this is because many of the other potential effects are subtle. Therefore, their impact might take a long time to accrue, and few researchers have the resources to mount and sustain long-term longitudinal research. Another reason for the lack of subtle measures of homework's effect is that the homework variable is often one of many influences on achievement being examined in a study. It is achievement as the outcome that is the primary focus of investigation with many predictors, rather than homework as the focus with many outcomes measured.

Factors That Affect the Utility of Homework Assignments

In addition to looking at homework's effectiveness on different outcomes, researchers have examined how other variations in assignments might influence their utility. Homework assignments are influenced by more factors than any other instructional strategy. Student differences may play a major role because homework allows students considerable discretion about whether, when, and how to complete assignments. Teachers may structure and monitor homework in a multitude of ways. The home environment may influence the process by creating a positive or negative atmosphere for study. And finally, the broader community provides other leisure activities that compete for the student's time.

Table 2 presents a model of the homework process presented by Cooper (1989). The model organizes into a single scheme many of the factors that educators have suggested might influence the success of a homework assignment. The model proposes that student ability, motivation, and grade level, as well as other individual differences (e.g., sex, economic background), and the subject matter of the homework assignments are exogenous factors, or moderator conditions, that might influence homework's effect. The model's endogenous factors, or mediators, divide the homework process into characteristics of the assignment and a home-community phase sandwiched by two classroom phases, each containing additional potential influences on homework's effects. Finally, Table 2 includes the potential consequences of homework as the outcomes in the process.

In this synthesis, the search for factors that might influence the impact of homework will focus only on the exogenous factors and the outcome variables, with the exception of the endogenous factor of amount of homework. Studies of the latter type are included because (a) they would include students who did no homework at all; and (b) achievement variations related to time spent on homework can reasonably be taken to bear on homework's effectiveness. Our restriction is based on the fact that most studies that look at other variations in endogenous or mediating factors rarely do so in the context of an investigation that also attempts to assess the more general effects of homework. Investigations of mediating factors typically pit one homework strategy against another and do not contain a condition in which students receive no-homework or an alternative treatment. Thus, in an effort to keep our task manageable, we focused here on studies that investigate primarily the general effects of homework, and we excluded studies that exclusively examine variations in homework assignments. (For a review of one such endogenous variable, parent involvement, see Patall, Cooper, & Robinson, 2005).

Optimum Amounts of Homework

Related to the issue of time spent on homework is the important question concerning the optimum amount of homework. Cooper (1989) found nine studies that allowed for a charting of academic performance as a function of homework time. The line-of-progress was flat in young children. For junior high school students, achievement continued to improve with more homework until assignments lasted between 1 and 2 hours a night. More homework than that was no longer associated with higher achievement. For high school students, the line-of-progress continued to

TABLE 2
A temporal model of factors influencing the effects of homework

Exogenous factors	Assignment characteristics	Initial classroom factors	Home–community factors	Classroom follow-up	Outcomes or effects
Student characteristics	Amount	Provision of materials	Competitors for student time	Feedback	Assignment completion
Ability	Purpose	Facilitators	Home environment	Written comments	Assignment performance
Motivation	Skill area used	Suggested approaches	Space	Grading	Positive effects
Study habits	Degree of individualization	Links to curriculum	Light	Incentives	Immediate academic
Subject matter	Degree of student choice	Other rationales	Quiet	Testing of related content	Long-term academic
Grade level	Completion deadlines		Materials	Use in class discussion	Nonacademic
	Social context		Others’ involvement		Parental and family
			Parents		Negative effects
			Siblings		Satiation
			Other students		Limited access to leisure and community activities
					Parental interference
					Cheating
					Increased student differences

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go up through the highest point on the measured scales, more than 2 hours. In the present synthesis, we included studies examining time on homework because of their relevance to homework's general effectiveness; therefore, we also looked for studies that might replicate or extend this finding.

Bias and Generalization in Research Synthesis

Decisions concerning how to search the literature determine the kinds of materials that will form the basis for a synthesis' conclusions. Identifying the literature is complicated by the fact that the search has two targets (Cooper, 1998). First, synthesists want to locate all previous literature on the problem. This is especially critical with regard to the retrieval of studies for inclusion in a meta-analysis. Synthesists can exert some control over whether this goal is achieved through their choice of information sources. Second, synthesists hope that the included studies will allow generalizations in the broader topic area. The generalizability of our synthesis was constrained by the students, schools, and communities represented in the literature.

We employed several strategies to ensure that our homework synthesis included the most exhaustive set of relevant documents. These strategies included (a) computerized searches of reference databases; (b) direct contact with active researchers and others who might know of unpublished or "fugitive" homework research; and (c) scrutiny of reference lists of relevant materials. In addition, analyses of the retrieved studies were undertaken to test for indications that the studies in hand might constitute a biased representation of the population of studies, and if so, to determine the nature of the bias.

Avoiding overgeneralization requires recognizing that the students, schools, and communities represented in the retrieved literature may not represent all target populations. For instance, it may be that little or no research has been conducted that examines the effects of homework on first- or second-grade students. A synthesis that qualifies conclusions with information about the kinds of people missing or overrepresented in studies runs less risk of overgeneralization. Such an examination of potential population restrictions will be included in the present work.

Methods for Research Synthesis

Literature Search Procedures

No matter how thorough the procedures may be, no search of the literature is likely to succeed in retrieving all studies relating homework to achievement. Therefore, systematic data censoring is a concern. That is, the possibility exists that more easily retrievable studies have different results from studies that could not be retrieved. To address this possibility, we collected studies from a wide variety of sources and included search strategies meant to uncover both published and unpublished research.

First, we searched the ERIC, PsycINFO, Sociological Abstracts, and Dissertation Abstracts electronic databases for documents cataloged between January 1, 1987, and December 31, 2003. The single keyword "homework" was used in these searches. Also, the Science Citation Index Expanded and the Social Sciences Citation Index databases were searched from 1987 to 2004 to identify studies or reviews that had cited Cooper (1989). These searches identified approximately 4,400 nonduplicate potentially relevant studies.

Next, we employed three direct-contact strategies to ensure that we tapped sources that might have access to homework-related research that would not be included in the reference and citation databases. First, we contacted the dean, associate dean, or chair of 77 colleges, schools, or departments of education at research-intensive institutions of higher education and requested that they ask their faculty to share with us any research they had conducted that related to the practice of assigning homework. Second, we sent similar letters to 21 researchers who, as revealed by our reference database search, had been the first author on two or more articles on homework and academic achievement between 1987 and the end of 2003. Finally, we sent similar letters to the directors of research or evaluation in more than a hundred school districts, obtained from the membership list of the National Association of Test Directors.

Two researchers in our team then examined each title, abstract, or document. If either of the two felt that the document might contain data relevant to the relationship between homework and an achievement-related outcome, we obtained the full document (in the case of judgments made on the titles or abstracts).

Finally, the reference sections of relevant documents were examined to determine if any cited works had titles that also might be relevant to the topic.

Criteria for Including Studies

For a study to be included in the research synthesis, several criteria had to be met. Most obviously, the study had to have estimated in some way the relationship between a measure of homework activity on the part of a student and a measure of achievement or an achievement-related outcome.

Two sampling restrictions were placed on included studies. Each study had to assess students in kindergarten through 12th grade. We excluded studies conducted on preschool-aged children or on postsecondary students. It was felt that the purpose and causal structure underlying the homework–achievement relationship would be very different for these populations. For similar reasons, we included only studies conducted in the United States.

Finally, the report had to contain enough information to permit the calculation of an estimate of the homework–achievement relationship.

Information Retrieved From Evaluations

Numerous characteristics of each study were included in the database. These characteristics encompassed six broad distinctions among studies: (a) the research report; (b) the research design; (c) the homework variable; (d) the sample of students; (e) the measure of achievement, and (f) the estimate of the relationship between homework and achievement.

Report Characteristics

Each database entry began with the name of the author of the study. Then the year of the study was recorded, followed by the type of research report. Each research report was categorized as a journal article, book chapter, book, dissertation, Master's thesis, private report, government report (state or federal), school or district report, or other type of report.

Research Design and Other Study Characteristics

The studies in this research synthesis were categorized into three basic design types, some with subtypes.

First, studies could employ exogenous manipulations of homework. This meant that the presence or absence of homework assignments was manipulated expressly for purposes of the study. Within the exogenous manipulation studies, the experimenters could introduce the manipulation at the student or classrooms level, either by randomly assigning students to homework and no-homework conditions or by some nonrandom process. If a nonrandom process was used, the experimenter then might or might not employ a priori matching or post hoc statistical procedures to equate the homework and no-homework groups. If procedures were used to equate groups, the variables used to enhance the equivalence of the groups could differ from study to study. Each of these variations in design was recorded for the set of studies that used exogenous homework manipulations.

In addition to these design characteristics of exogenous homework manipulation studies and their report information, we recorded (a) the number of students and classrooms included in the homework and no-homework conditions at the beginning and end of the experiment; (b) the grade level of the students; (c) the subject matter of the homework (reading, other language arts, math, science, social studies, foreign language, other, or multiple subjects); (d) the number of assignments per week and their duration; (e) the measure of achievement (standardized achievement test, teacher-developed unit test, textbook chapter unit test, class grades, overall grade point average, composite achievement score); and (f) the magnitude of the relationship between homework and achievement.

The second type of design included studies that took naturalistic, cross-sectional measures of the amount of time the students spent on homework without any intervention on the part of the researchers and related these to an achievement-related measure. This second type of design also included an attempt to statistically equate students on other variables that might be confounded with homework and therefore might account for the homework–achievement relationship. For these studies, we also coded the source of the data, that is, whether the data were collected by the researchers or by an independent third party. If data were from an independent source, we coded the source. We coded the analytic strategy used to equate students. Most frequently, this involved conducting multiple regression analysis or the application of a structural equation modeling package. Also, we coded each of the same variables coded for studies that used exogenous manipulations of homework, except for (a) the sample sizes in the homework and no-homework groups (only total sample size in the analysis was recorded); and (b) the number and duration of assignments, which was irrelevant to this design. Instead of the assignment characteristics, we coded the amount of time the student spent doing homework, as measured by student or parent report.

The third type of design involved the calculation of a simple bivariate correlation between the time the student spent on homework and the measure of achievement. In these studies, no attempt was made to equate students on other variables that might be confounded with time on homework. For these studies, we also recorded the same variables coded for studies using statistical controls of other variables except, of course, the number and nature of controlled variables. We also coded several additional variables related to the sample of students. These included the students' (a) sex; (b) socioeconomic status (low, low-middle, middle, middle-upper, upper, "mixed," no SES [socioeconomic status] information given); and (c) whether any of the following labels were applied to the sample of students (gifted, average, "at risk," underachieving/below grade level, possessing a learning disability, overachieving/above grade level).

Effect Size Estimation

For studies with exogenous manipulations of homework, we used the standardized mean difference to estimate the effect of homework on measures of student achievement. The *d*-index (Cohen, 1988) is a scale-free measure of the separation between two group means. Calculating the *d*-index for any comparison involves dividing the difference between the two group means by either their average standard deviation or by the standard deviation of the control group. This calculation results in a measure of the difference between the two group means expressed in terms of their common standard deviation or that of the untreated population. Thus a *d*-index of .25 indicates that one-quarter standard deviation separates the two means. In the synthesis, we subtracted the no-homework condition mean from the homework condition mean and divided the difference by their average standard deviation. Thus positive effect sizes indicate that the students doing homework had better achievement outcomes.

We calculated effect sizes based on the means and standard deviations of students' achievement indicators, if available. If means and standard deviations were not available, we retrieved the information needed from inferential statistics to calculate *d*-indexes (see Rosenthal, 1994).

For studies that involved naturalistic, cross-sectional measures of the amount of time spent on homework and related these to achievement but also included an attempt to statistically equate students on other characteristics, our preferred measure of relationship strength was the standardized beta-weight, β. These were derived either from the output of multiple regressions or as path coefficients in structural equation models. The standardized beta-weights indicate what change in the achievement measure expressed as a portion of a standard deviation was associated with a one-standard-deviation change in the homework variable. For example, if the standard deviation of the time-spent-on-homework variable equaled 1 hour and the standard deviation of the achievement measure equaled 50 points, then a beta-weight of .50 would mean that, on average, students in the sample who were separated by 1 hour of time-spent-on-homework also showed a 25-point separation on the achievement measure. In a few instances, beta-weights could not be obtained from study reports, so the most similar measures of effect (e.g., unstandardized regression weights, *b*) were retrieved. There were no instances in which we calculated beta-weights from other statistics.

For studies that involved naturalistic, cross-sectional measures but included no attempt to statistically equate students on third variables, we used simple bivariate correlations as measures of relationship. In some instances these were calculated from other inferential statistics (see Rosenthal, 1994).

Using three different measures of association implies that the relationship of homework to achievement cannot be compared across the three different types of design. This is not strictly true. Standardized mean differences and correlation coefficients can be transformed one to the other (see Cohen, 1988). A beta-weight equals a correlation coefficient when no other variables are controlled. However, we chose to present the results using each design's most natural metric so that the important distinction in their interpretation would not be lost.

Coder Reliability

Two coders extracted information from all reports selected for inclusion. Discrepancies were first noted and discussed by the coders, and if agreement was not reached the first author was consulted. Because all studies were independently coded twice and all disagreements resolved by a third independent coder, we did not calculate a reliability for this process (which would have entailed training three more coders and having them code at least a subset of studies).

Methods of Data Integration

Before conducting any statistical integration of the effect sizes, we first counted the number of positive and negative effects. For studies with effect size information, we calculated the median and range of estimated relationships. Also, we examined the distribution of sample sizes and effect sizes to determine if any studies contained statistical outliers. Grubbs's (1950) test, also called "the maximum normed residual test," was applied (see also Barnett & Lewis, 1994). This test identifies outliers in univariate distributions and does so one observation at a time. If outliers were identified, (using $p < .05$, two-tailed, as the significance level) these values would be set at the value of their next nearest neighbor.

Both published and unpublished studies were included in the synthesis. However, there is still the possibility that we did not obtain all studies that have investigated the relationship between homework and achievement. Therefore, we used Duval and Tweedie's (2000a, 2000b) trim-and-fill procedure to test whether the distribution of effect sizes used in the analyses were consistent with variation in effect sizes that would be predicted if the estimates were normally distributed. If the distribution of observed effect sizes was skewed, indicating a possible bias created either by the study retrieval procedures or by data censoring on the part of authors, the trim-and-fill method provides a way to estimate the values from missing studies that need to be present to approximate a normal distribution. Then, it imputes these missing values, permitting an examination of an estimate of the impact of data censoring on the observed distribution of effect sizes.

Calculating Average Effect Sizes

We used both weighted and unweighted procedures to calculate average effect sizes across all comparisons. In the unweighted procedure, each effect size was given equal weight in calculating the average value. In the weighted procedure, each independent effect size was first multiplied by the inverse of its variance. The sum of these products was then divided by the sum of the inverses. Generally speaking, weighted effect sizes are preferred because they give the most precise estimates of the underlying population values (see Shadish & Haddock, 1994). The unweighted effect sizes are also reported because in instances in which these are very different from the weighted estimates, this can give an indication that the magnitude of the effect size and sample size are correlated, sometimes suggesting that publication bias might be a concern. Also, 95% confidence intervals were calculated for weighted average effects. If the confidence interval did not contain zero, then the null hypothesis of no homework effect can be rejected.

Identifying Independent Hypothesis Tests

One problem that arises in calculating effect sizes involves deciding what constitutes an independent estimate of effect. Here, we used a shifting unit of analysis approach (Cooper, 1998). In this procedure, each effect size associated with a study is first coded as if it were an independent estimate of the relationship. For example, if a single sample of students permitted comparisons of homework's effect on both math and reading scores, two separate effect sizes were calculated. However, for estimating the overall effect of homework, these two effect sizes were averaged prior to entry into the analysis, so that the sample only contributed one effect size. To calculate the overall weighted mean and confidence interval, this one effect size would be weighted by the inverse of its variance (based primarily on sample size, which should be about equal for the two component effect sizes). However, in an analysis that examined the effect of homework on math and reading scores separately, this sample would contribute one effect size to each estimate of a category mean effect size.

The shifting unit of analysis approach retains as many data as possible from each study while holding to a minimum any violations of the assumption that data points are independent. Also, because effect sizes are weighted by sample size in the calculation of averages, a study with many independent samples containing just a few students will not have a larger impact on average effect size values than a study with only a single, or only a few, large independent samples.

Tests for Moderators of Effects

Possible moderators of homework–achievement relationships were tested by using homogeneity analyses (Cooper & Hedges, 1994; Hedges & Olkin, 1985). Homogeneity analyses compare the amount of variance in an observed set of effect sizes with the amount of variance that would be expected by sampling error alone. The analyses can be carried out to determine whether (a) the variance in a group of individual effect sizes varies more than predicted by sampling error, or (b) a group of average effect sizes varies more than predicted by sampling error. In the latter case, the strategy is analogous to testing for group mean differences in an analysis of variance or linear effects in a multiple regression.

Fixed and Random Error

When an effect size is said to be "fixed," the assumption is that sampling error is due solely to differences among participants in the study. However, it is also possible to view studies as containing other random influences, including differences in teachers, facilities, community economics, and so on. This view assumes that homework data from classrooms, schools, or even school districts in our meta-analysis also constitute a random sample drawn from a (vaguely defined) population of homework conditions. If it is believed that random variation in interventions is a significant component of error, a random-error model should be used that takes into account this study-level variance in effect sizes (see Hedges & Vevea, 1998, for a discussion of fixed and random effects).

Rather than opt for a single model of error, we chose to apply both models to our data. We conducted all our analyses twice, employing fixed-error assumptions once and random-error assumptions once. By employing this sensitivity analysis (Greenhouse & Iyengar, 1994), we could examine the effects of different assumptions on the outcomes of the synthesis. Differences in results based on which set of assumptions was used could then be part of our interpretation of results. For example, if an analysis reveals that a moderator variable is significant under fixed-error assumptions but not under random-error assumptions, this result suggests a limit on the generalizability of inferences about the moderator variable.

All statistical analyses were conducted using the Comprehensive Meta-Analysis statistical software package (Borenstein, Hedges, Higgins, & Rothstein, 2005).

Results

Studies With Exogenous Introductions of Homework

The literature search located six studies that employed a procedure in which the homework and no-homework conditions were imposed on students explicitly for the purpose of studying homework's effects. None of these studies was published. Some of the important characteristics and outcomes of each study are presented in Table 3.

Apparently, only one study used random assignment of students to conditions. McGrath (1992) looked at the effect of homework on the achievement of 94 high school seniors in three English classes studying the play Macbeth. At one point in the research report, the author states that half of the students "elected to receive no homework" and half "elected to receive homework" (p. 27). However, at another point, the report states that each student was assigned to a condition "by the alphabetic listing of his/her last name" (p. 29). Thus it might be (optimistically) assumed that the students in each of the three classes were haphazardly assigned to homework and no-homework conditions. In the analyses, the student was used as the unit. The experiment lasted 3 weeks and involved 12 homework assignments. Students doing homework did significantly better on a posttest achievement measure, $d = .39$.

A study by Foyle (1990) assigned four whole 5th-grade classrooms (not individual students) to conditions at random, one to a practice homework condition, one to a preparation homework condition, and two to a no-homework control condition. Clearly, assigning only one classroom to each condition, even when done at random, cannot remove confounded classroom differences from the effect of homework. For example, all four classrooms used a cooperative learning approach to teaching social studies, but one classroom (assigned to the practice homework condition) used a different cooperative learning approach from the other three classes. Also, the student, rather than the classroom, was used as the unit for statistical analysis, creating the concern that within-class dependencies among students were ignored. Analysis revealed that students differed significantly on a social studies pretest and on a standard measure of intelligence, but it was not reported whether there were preexisting classroom differences on these measures. Students doing homework outperformed no-homework students on unadjusted posttest scores, $d = .90$, and on posttest scores adjusted for pretest and intelligence differences, $d = .99$.

Foyle (1984) conducted a similar study on six high school classes in American history. Here, the experimenter reported that "the assignment of treatment and control groups was under the experimenter's control" (p. 90) and two intact classrooms were each assigned randomly to practice homework, preparation homework, and no-homework conditions. However, the student was again used as the unit of analysis. Analyses of covariance that controlled for pretest scores, aptitude differences, and the students' sex revealed that students doing homework had higher posttest achievement scores than students who did not. The covariance analysis and post hoc

TABLE 3

Studies that manipulated homework and no-homework conditions

Author and year	Research design	Number of classes and students, ESS	Grade level	Subject matter	Type of achievement measure	Effect size
Finstad, 1987	Nonequivalent control with no pretest differences	2 classes 39 students ESS = 5.2	2	Number place values to 100	Unit test developed by Harcourt Brace Jovanovich	+.97
Foyle, 1984	Randomized by class, analyzed by student	6 classes 131 students ESS = 15.8	9–12	American history	Unit test developed by the teacher	+.46
Foyle, 1990	Randomized by class, analyzed by student	4 classes 64 students ESS = 10.2	5	Social studies	Unit test developed by the teacher	+.90
McGrath, 1992	Randomized within class, analyzed by student	3 classes 94 students ESS = 8.0	12	Shakespeare	Unit test developed by Harcourt Brace Jovanovich	+.39
Meloy, 1987	Unlucky random assignment followed by nonequivalent control with pretest	5 classes 70 students ESS = 12.6	3	English skills	Unit test developed by McDugal, Little	+
					Researcher-shortened version of the Iowa Test of Basic Skills Language subtest	–
		3 classes 36 students ESS = 7.4	4		Unit test developed by McDugal, Little	+
					Researcher-shortened version of the Iowa Test of Basic Skills Language subtest	–
Townsend, 1995	Nonequivalent control without equating	2 classes 40 students ESS = 5.2	3	Vocabulary	Unit test developed by the teacher	+.71

Note. ESS = effective sample size based on an assumed intraclass correlation of .35.

tests revealed a significant positive effect of homework, but an effect size could not be calculated from the adjusted data (because the reported *F*-test contained two degrees of freedom in the numerator and means and standard deviations were not provided). The approximate, unadjusted homework effect was $d = .46$.

Finstad (1987) studied the effect of homework on mathematics achievement for 39 second-grade students in two intact classrooms. One unit, on place values to 100, was used, but neither the frequency nor the duration of assignments was reported. One classroom was assigned to do homework and the other not. It was not reported how the classroom assignments were carried out, but it was reported that there were no pretest differences between the classes. Data were analyzed on the student level without adjustment. The students in the classroom doing homework performed significantly better on a posttest measure, $d = .97$.

Meloy (1987) studied the effects of homework on the English skills (sentence components, writing) of third and fourth graders. Eight intact classrooms took part in the study and classes were matched on a shortened version of the Iowa Test of Basic Skills (ITBS) language subtest before entire classes were randomly assigned to homework and no-homework conditions. However, examination of pretest differences on the ITBS language subscale revealed that the students assigned to do homework scored significantly higher than students in no-homework classes. Thus a pretest-posttest design was used to control for the initial group differences, but pretests were used as a within-students factor rather than as a covariate (meaning a significant homework effect would appear as an interaction with time of testing). Also, students who scored above a threshold score on the pretest were excluded from the posttest analysis. Thus only 106 of an original sample consisting of 186 students were used in the analyses, and excluded students were not distributed equally across homework and no-homework conditions. Grade levels were analyzed separately, and classrooms were a factor in the analyses. The class-within-condition effect was not significant, so, again, the student was used as the unit of analysis. Homework was assigned daily for 40 instructional days. This study also monitored the homework completion rates in classrooms and set up reinforcement plans, different for each class, to improve completion rates. The effects of homework were gauged by using a researcher-modified version of the ITBS language subtest and a unit mastery test from the textbook. The complex reporting of statistical analyses made it impossible to retrieve simple effect estimates from the data. However, the author reported that the condition-by-time interactions indicated that homework had a significant negative effect on ITBS scores for third graders and a significant positive effect on fourth graders' unit test scores.

Finally, Townsend (1995) examined the effects of homework on the acquisition of vocabulary knowledge and understanding among 40 third-grade students in two classes, both taught by the experimenter. Treatment was given to classes as a whole and it was not stated how each class was assigned to the homework or no-homework condition. The student was used as the unit of analysis. A teacher-prepared posttest measure of vocabulary knowledge suggested that the homework group performed better, $d = .71$.

In sum, the six studies that employed exogenous manipulations all revealed a positive effect of homework on unit tests. One study (Meloy, 1987) revealed a negative effect on a standardized test modified by the experimenter. Four of the six studies employed random assignment, but in three cases assignment to conditions was carried out at the classroom level, using a small number of classrooms, and analyses were conducted using the student as the unit of analysis. In the only instance in which random assignment appears to have occurred within classes (McGrath, 1992), students also were used as the unit of analysis. Also, random assignment appears to have failed to produce equivalent groups in one study (Meloy, 1987).

While the introduction of homework as an exogenous intervention is a positive feature of these studies, other methodological considerations make it difficult to draw strong causal inferences from their results. Still the results are encouraging because of the consistency of findings. The measurable effects of homework on unit tests varied between $d = .39$ and $d = .97$. Also, the three studies that successfully used random assignment, fixed weighted $d = .53$ (95% CI = .29/.79), random weighted $d = .54$ (95% CI = .26/.82), produced effect sizes that were smaller than those of two studies that used other techniques to produce equivalent groups and for which effect sizes could be calculated, fixed weighted $d = .83$ (95% CI = .37/1.30), random weighted $d = .83$ (95% CI = .37/1.30); but the difference in mean d-indexes between these two sets of studies was not significant, fixed $Q(1) = 1.26$, *ns,* random $Q(1) = 1.12$, *ns.* Collapsing across the two study designs and using fixed-error assumptions, the weighted mean d-index across the five studies from which effect sizes could be obtained was $d = .60$ and was significantly different from zero (95% CI = .38/.82). Using a random-error model, the weighted average d-index was also .60 (95% CI = .38/.82).

To take into account the within-class dependencies that were not addressed in the reported data analyses, we recalculated the mean effect sizes and confidence intervals by using an assumed intraclass correlation of .35 to estimate effective sample sizes. In this analysis, the weighted mean d-index was .63, using both fixed and random-error assumptions, and both were statistically different from zero (95% CI = .03/1.23, for both). The mean d-index would not have been significant if an intraclass correlation of .4 was assumed. Additionally, the tests of the distribution of d-indexes revealed that we could not reject the hypothesis that the effects were estimating the same underlying population value when students were used as the unit of analysis, $Q_{fixed}(5) = 4.09$, *ns,* $Q_{random}(5) = 4.00$, *ns,* or when effective sample sizes were used as the unit, $Q_{fixed}(5) = .54$, *ns,* $Q_{random}(5) = .54$, *ns.*

And finally, the trim-and-fill analyses were conducted looking for asymmetry using both fixed and random-error models to impute the mean d-index (see Borenstein et al., 2005). Neither of the analyses produced results different from those described above. There was evidence that two effect sizes might have been missing. Imputing them would lower the mean d-index to $d = .48$ (95% CI = .22/.74) using both fixed and random-error assumptions.

The small number of studies and their variety of methods and contexts preclude their use in any formal analyses investigating possible influences on the magnitude of the homework effect, beyond comparing studies that used random assignment versus other means to create equivalent groups. The studies varied not only in research design but also in subject matter, grade level, duration, amount of homework, and the degree of alignment of the outcome measure with the content of assignments. Replications of any important feature that might influence the homework effect are generally confounded with other important features, and no visible pattern connecting effect sizes to study features is evident.

Studies Using Cross-Sectional Data and Control of Third Variables

Studies Using the National Education Longitudinal Study (1988, 1990, or 1992)

The literature search located nine reports that contained multivariate analyses of data collected as part of the National Education Longitudinal Study of 1988 (NELS) or in one of the NELS follow-ups on the same students in 1990, 1992, 1994, or 2000. These studies are described in Table 4. The NELS was conducted by the National Center for Educational Statistics and involved a nationally representative two-stage stratified probability sample. The final student sample in the first wave included 24,599 eighth-grade students. Each student completed achievement tests in mathematics, reading, science, and social studies in 1988, 1990, and 1992, as well as a 45-minute questionnaire that included questions about school, school grades, personal background, and school context. Various waves of the NELS also included surveys of teachers, school administrators, and parents. Student transcripts were collected at the end of their high school careers. Questions on homework were completed by both students and teachers, and they were asked about the total minutes of homework completed or assigned in different subject areas.

Several of the studies using the NELS data sampled students from the NELS itself for the purpose of examining questions regarding restricted populations. For example, Peng and Wright (1994) were interested in studying differences in relationships between predictors of achievement across ethnic groups, with a focus on Asian Americans. Davis and Jordan (1996) focused on African American males, while Roberts (2000) restricted the subsample to students attending urban schools only.

Examined as a group, the studies using NELS data use a wide variety of outcome measure configurations and different sets of predictor variables, in addition to homework. Still, every regression coefficient associated with homework was positive, and all but one were statistically different from zero. The exception occurred in the study of African American males on a composite measure of class grades (Davis & Jordan, 1996).

The study revealing the smallest beta-weight was a dissertation by Hill (2003). This report presents an unclear description of how the subsample drawn from the NELS was defined. The text reports that students were omitted from the sample if they "attended public schools, live in suburban areas, are neither Black nor Hispanic; and whose teachers are male, not certified in [the subject of the outcome variable], have neither an undergraduate degree in education or in [the subject of the outcome variable], and have neither a graduate degree in education or [the subject of the outcome variable]" (pp. 45, 86, 120). However, the tables in the report suggest that White students were included in the samples. The regression models suggest that students with teachers who had degrees in subjects other than the outcome variable also were included. Thus it is difficult to determine whether sampling restrictions might be the cause of the small regression coefficients associated with homework.

The dissertation by Lam (1996) deserves separate mention. In this study using data from 12th graders, the amount of homework students reported doing was entered into the regression equation as four dummy variables. This permitted an examination of possible curvilinear effects of homework. As Table 4 reveals, students who reported doing homework always had higher achievement scores than students who did not do homework (coded as the dummy variable). However, the strongest relationship between homework and achievement was found among students who reported doing

TABLE 4

Characteristics of studies using data from the National Education Longitudinal Study (1988, 1990, or 1992) and performing multivariate analyses

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Davis & Jordan, 1994 Journal article	1,236 NELS: 88 Grade 8 African American males[a]	Multiple regression	Achievement test composite (math, science, and history achievement test scores)	Academic workload, urbanicity, attendance rate, school SES, number of Black teachers, discipline stressed, class size, college preparatory classes, dropout rate, teacher expectancies, motivation, teacher characteristics, homework, attendance, math coursework, science coursework, remediation, suspensions, retention, prior reading learning, SES	Achievement test composite: $\beta = .05$	$t = 2.21$, $p < .01$
			Class grades composite (self-reported grades in math, science, history, and English)		Class grades composite: $\beta = .03$	$t = 1.05$, $p > .05$
Hill, 2003 Analysis 1, dissertation	9,329 Math 1,104 Reading 12,302 Science NELS: 88, 90, 92 Grades 8, 10, 12[b]	Multiple regression (panel estimation)	Math Item Response Theory score	Homework (student report), school setting, SES, work time, TV time, teacher academic degree, class size, teaching style	Math: $\beta = .01$	$t = 28.85$, $p < .05$
			Reading Item Response Theory score		Reading: $\beta = .01$	$t = 13.96$, $p < .05$

	Multiple sample restrictions		Science Item Response Theory score		Science: β = .01	$t = 26.56$, $p < .05$
Hill, 2003 Analysis 2, dissertation	9,329 Math 1,104 Reading 12,302 Science NELS: 88, 90, 92 Grades 8, 10, 12[b] Multiple sample restrictions	Multiple regression (panel estimation)	Math Item Response Theory score	Homework (student report), school setting, SES, teacher ethnicity, teacher academic degree, class size, teaching style	Math: β = .02	$t = 31.31$, $p < .05$
			Reading Item Response Theory score		Reading: β = .01	$t = 16.00$, $p < .05$
			Science Item Response Theory score		Science: β = .01	$t = 29.42$, $p < .05$
Hill, 2003 Analysis 3, dissertation	9,329 Math 1,104 Reading 12,302 Science NELS: 88, 90, 92 Grades 8, 10, 12[b] Multiple restrictions	Multiple regression (panel estimation)	Math Item Response Theory score	Homework (teacher report), school setting, SES, TV time, work time, teacher ethnicity, teacher academic degree, class size, teaching style	Math: β = .01	$t = 11.62$, $p < .05$
			Reading Item Response Theory score		Reading: β = .01	$t = 8.73$, $p < .05$
			Science Item Response Theory score		Science: β = .01	$t = 12.59$, $p < .05$
Hill, 2003 Analysis 4, dissertation	9,329 Math 1,104 Reading 12,302 Science NELS: 88, 90, 92 Grades 8, 10, 12[b] Multiple restrictions	Multiple regression (panel estimation)	Math Item Response Theory score	Homework (teacher report), school setting, SES, teacher ethnicity, teacher academic degree, class size, teaching style	Math: β = .01	$t = 13.01$, $p < .05$
			Reading Item Response Theory score		Reading: β = .01	$t = 9.97$, $p < .05$
			Science Item Response Theory score		Science: β = .01	$t = 14.17$, $p < .05$

(continued)

TABLE 4 (*Continued*)

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Lam, 1996 Dissertation	8,362 NELS: 92 Grade 12 175 Chinese Americans 8,187 Caucasian Americans	Multiple regression	Reading achievement test score	Ethnicity, sex, SES, TV time, absenteeism, hours of work per week, plus five dummy codes comparing students who do less than 1 hour of homework with those who do 1–6 hours of homework, 7–12 hours of homework, 13–20 hours of homework, more than 20 hours of homework	Reading: 1–6 hrs, $\beta = .13$ 7–12 hrs, $\beta = .21$ 13–20 hrs, $\beta = .16$ Over 20 hrs, $\beta = .12$	All *ps* < .005
			Math achievement test score		Math:	
					1–6 hrs, $\beta = .09$	$p < .005$
					7–12 hrs, $\beta = .19$	$p < .05$
					13–20 hrs, $\beta = .16$	$p < .005$
					Over 20 hrs, $\beta = .10$	$p < .005$
			Science achievement test score		Science: 1–6 hrs, $\beta = .09$ 7–12 hrs, $\beta = .17$ 13–20 hrs, $\beta = .12$ Over 20 hrs, $\beta = .09$	All *ps* < .005
			Social studies achievement test score		Social studies: 1–6 hrs, $\beta = .11$ 7–12 hrs, $\beta = .18$ 13–20 hrs, $\beta = .13$ Over 20 hrs, $\beta = .11$	All *ps* < .005
			Composite achievement test score		Composite: 1–6 hrs, $\beta = .12$ 7–12 hrs, $\beta = .21$ 13–20 hrs, $\beta = .16$ Over 20 hrs, $\beta = .12$	All *ps* < .005

Modi, Konstantopoulos, & Hedges, 1998 Conference paper	12,856 NELS: 92 Grade 12	Multiple regression	Achievement test composite (reading, math, science, and social studies)	Ethnicity, sex, educational aspirations, self-confidence, attendance, homework (more than 6 hours a week), reading time, TV time, religiosity, extracurricular activities, parent reliance, family background, location, school background	$\beta = .11$	$p < .0001$
Peng & Wright, 1994 Analysis 1, journal article	9,685[c] NELS: 88 Grade 8	Multiple regression	Achievement test composite (reading and math)	Family background, homework, TV time, extracurricular activities, educational aspirations, parental assistance with homework	$\beta = .10$	$p < .01$
Peng & Wright, 1994 Analysis 2, journal article	9,685[c] NELS: 88 Grade 8	Multiple regression	Achievement test composite (reading and math)	Ethnicity, family background, homework, TV time, extracurricular activities, educational aspirations, parental assistance with homework	$\beta = .08$	$p < .01$

(continued)

TABLE 4 (*Continued*)

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Roberts, 2000 Dissertation	7,178 NELS: 88 Grade 8 Urban schools	Multilevel random coefficients modeling	Science achievement test score	Parent involvement, fear of asking questions in class, homework, non-English classroom	$b = 1.48$ Unweighted sample; multilevel sample	$p < .01$
		Multiple regression			$b = 1.10$ Weighted sample; multiple regression	$p < .01$
Schewior, 2001 Dissertation	7,138 NELS: 92 Grade 12	Multiple regression	Math achievement test score	Sex, ethnicity, location, school, family income, parent education level, teacher experience, class size, homework (two dummy codes comparing completion of homework "all of the time" with "most or all of the time")	"Most or all of the time," $b = 1.28$ "All of the time," $b = 2.59$	$t = 4.62$, $p < .0001$ $t = 8.09$, $p < .0001$
Thomas, 2001 Analysis 1, research report	450 NELS: 88 Grade 8 Picked at random	Logistic regression	Reading proficiency dichotomous score reflecting whether the	Sex, advanced math, grades, SES, homework, reading proficiency	$\beta = .28$	$p < .01$

			student exhibited proficiency at the highest level			
Thomas, 2001 Analysis 2, research report	450 NELS: 88 Grade 8 Picked at random	Logistic regression	Reading proficiency score recategorized to provide four exhaustive and nonoverlapping categories with higher scores indicating greater proficiency	School type, religiosity, sex, advanced math, ethnicity, grades, postsecondary plans, locus of control, self-concept, SES, parent education, homework, reading proficiency	$\beta = .17$	$p < .05$
Thomas, 2002 Research report	450 NELS: 88 Grade 8 Pick at random	Discriminant analysis[d]	Math proficiency score, reading proficiency score, science proficiency score (all 3-level scales)	Grades, motivation, advanced coursework, family background, homework, ethnicity, sex	Math: $\beta = .17$ Reading: $\beta = .13$ Science: $\beta = .15$	NR $F = 5.07$, $p = .02$ $F = 7.27$, $p = .02$

Note. NELS = National Education Longitudinal Study, NR = not reported.

[a]These students were required to have participated in NELS:90 as well.

[b]Students were included in the sample when data were available from NELS:88, NELS:90, and NELS:92; or from NELS:88 and NELS:90; or from NELS:88 and NELS:92.

[c]The authors write that this number represents the "effective" sample size, which was the actual sample size adjusted by the design effect, citing Ingels, Abraham, Karr, Spencer, & Frankel (1990).

[d]This study also reports results from canonical correlation and MANOVA analyses that are largely consistent with the findings of the discriminant analysis. The discriminant analysis is used here because it is most similar to analyses reported in other studies.

7 to 12 hours of homework per week, followed by students who reported doing 13–20 hours per week. Students who reported doing more than 20 hours of homework per week revealed a relationship with achievement test scores nearly equal to those reporting between 1–6 hours of homework per week. While this result is suggestive of a curvilinear relationship between homework and achievement, we must bear in mind that Lam restricted the sample of students to Asian Americans and Caucasian Americans.

In sum, if we omit (a) the Hill (2003) study (which produced beta-weights of .01 and .02), as well as (b) those studies that reported unstandardized regression weights, or (c) those for which coefficients could not be determined, then the reported beta-weights for the relation between homework and standardized achievement test scores range from .05 to .28. For composite achievement scores the range is from .05 to .21; for math, it is .09 to .16; for reading, .12 to .28; for science, .09 to .23; and for social studies, .11 to .18. Thus the ranges of estimated regression coefficients appear quite similar across the subject areas. However, we would caution against drawing any conclusions regarding the mediating role of subject matter on the homework–achievement relationship from these data, because the number and type of predictors in each model are confounded with subject matter. It should also be kept in mind that these estimates refer to high school students only.

Studies Using Data Other Than the National Education Longitudinal Study and Performing Multivariate Analyses

Table 5 provides information on 12 additional studies that performed multivariate analysis on cross-sectional data in order to examine the relationship between homework and achievement, with other variables controlled. Two of the studies used the High School and Beyond database (Cool & Keith, 1991; Fehrmann, Keith, & Reimers, 1987). The High School and Beyond database drew its 1980 base-year sample of sophomores and seniors from high schools throughout the United States. Probability sampling was used with overrepresentation of special populations. Follow-up surveys were conducted in 1982 and 1984. Brookhart (1997) used the Longitudinal Study of American Youth database, containing a national probability sample of approximately 6,000 seventh and tenth graders stratified by geographic area and degree of urban development. The rest of the studies used data collected by the researchers for the specific purpose of studying variables related to achievement.

Two studies conducted by Smith (1990, 1992), using overlapping data sets of seventh, ninth, and eleventh graders, found some negative relationships between homework and achievement. One of these findings (in Smith, 1992) revealed a small but statistically significant negative relationship between the amount of time spent on homework and language achievement, $\beta = -.06$. However, this study also revealed a significant positive interaction between year in school and time spent on homework. The interaction was not interpreted. This was the only significant negative result obtained in any of the cross-sectional, multivariate studies.

The remaining studies that used secondary school students all revealed positive and generally significant relationships. The three studies that used elementary school students (Cooper et al., 1998; Olson, 1988; Wynn, 1996) all revealed positive relationships between the homework measure and achievement (in Cooper et al., $\beta = .22$ for teacher-reported overall grades; in Olsen, $\beta = .10$ for math and $\beta = .11$

for reading; and in Wynn, $\beta = .04$ for grade point average). Thus, in addition to using varying predictor variables in the regression models, these studies also included a variety of outcome measures, including not only standardized tests but also teacher-assigned grades. In one instance, (Hendrix, Sederberg, & Miller, 1990) the outcome measure was not achievement but rather an indicator of school commitment/alienation constructed by the researcher that measured the importance of successful performance on school tasks, effort, and relevance of school work for student's lives. Thus we would again caution against drawing conclusions about mediating and moderating variables from these studies. It seems safest simply to note that the positive relationship between homework and achievement across the set of studies was generally robust across sample types, models, and outcome measures.

Structural Equation Modeling Studies Using Data From the National Education Longitudinal Study (1988, 1990, or 1992)

Table 6 provides information on four studies that tested structural equation models using data from the National Education Longitudinal Study. These analyses all revealed a positive relationship between the amount of time spent on homework and achievement. Not surprisingly, they are also somewhat larger than the relationships reported in studies that used multiple regression approaches to data analysis.

Structural Equation Modeling Studies Using Data From the High School and Beyond (1980, 1982, 1984) Longitudinal Studies

Table 7 provides information on four studies that tested structural equation models using data from the High School and Beyond database. All coefficients but one are positive and statistically significant. Keith and Benson (1992) found a nonsignificant negative coefficient for a subsample of Native Americans, $\beta = -.09$. The authors caution against strong interpretation of this finding because (a) the sample size was small ($n = 147$), and (b) Native American students who attended Bureau of Indian Affairs schools were not sampled. Still, it is generally the case that coefficients for the homework–achievement relationship estimated using High School and Beyond data are smaller than those estimated using NELS data.

Structural Equation Modeling Studies Using Original Data

We could find only one study that performed a structural equation analysis on data collected by the researchers. This study was also unique in that it examined the relationship between homework and achievement for elementary school students, a total of 214 second and fourth graders, who attended three adjacent school districts, one urban, one suburban, and one rural. Cooper, Jackson, Nye, and Lindsay (2001) used the MPlus program to predict grades assigned by teachers. In addition to the amount of homework that students reported doing, the model included student ability and homework norms, parent attitude, home environment (e.g., TV and quiet time), parent facilitation, presence of alternative activities, and student attitudes. The path coefficient for the relationship between time on homework and class grade was .20, $p < .01$.

Studies Correlating Time on Homework and Academic Achievement

The literature search uncovered 32 studies that described the correlations between the time that a student spent on homework, as reported by either the student or a

TABLE 5

Characteristics of studies using data other than those from the National Education Longitudinal Study (1988, 1990, or 1992) and performing multivariate analyses

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Brookhart, 1997 Journal article	118[a] Longitudinal Study of American Youth, 1987–1991 Grade 12	Path analysis: stepwise multiple regression	Math achievement test score	Sex, SES, reading comprehension, prior Math achievement (Grades 10–11), classroom assessment environment (% class time testing; Grades 10–12), hours of homework assigned, % completing homework on time, % of homework returned	$\beta = .09$	$p < .05$
Cool & Keith, 1991 Journal article	28,051, with pairwise deletion, minimum $N = 21{,}008$ High School and Beyond Longitudinal Study, 1980 Grade 12	Path analysis	Achievement test composite (reading, math I and II scores)	Exogenous: ethnicity, sex, family background Endogenous: ability, quality of instruction, motivation, homework, quantity of coursework	$\beta = .04$	*ns*
Cooper, Lindsay, Nye, & Greathouse, 1998	285 Original data Grades 2, 4	Path analysis: multiple regression	Teacher-reported class grades	Exogenous: standardized achievement test score, parent attitudes, teacher attitudes	$\beta = .13$	$p < .02$

Analysis 1, journal article				Endogenous: amount of homework assigned, student attitudes, homework completion		
Cooper, Lindsay, Nye, & Greathouse, 1998 Analysis 2, journal article	424 Original data Grades 6–12	Path analysis: multiple regression	Teacher-reported class grades	Exogenous: standardized achievement test score, parent attitudes, teacher attitudes Endogenous: amount of homework assigned, student attitudes, homework completion	$\beta = .22$	$p < .0001$
Fehrmann, Keith, & Reimers, 1987 Journal article	28,051 High School and Beyond Longitudinal Study, 1980 Grade 12	Path analysis: multiple regression	Student-reported class grades	Exogenous: ethnicity, family background, sex Endogenous: ability, parental involvement, homework, TV time	$\beta = .19$	NR
Foyle, 1984 Dissertation	131 Original data Grade 10	ANCOVA	American history posttest scores	Covariates: pretest score, ability Main effects: sex, homework (practice, preparatory, or no homework) Interaction: Sex × homework	Not obtainable	$F = 7.81$, $p < .001$

(continued)

TABLE 5 (*Continued*)

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Hendrix, Sederberg, & Miller, 1990 Journal article	1,521 Original data Grade 12	Stepwise regression	School commitment/alienation	Nonverbal ability, verbal ability, % advanced courses, TV time, homework, job time, GPA; nonverbal ability × TV; verbal ability × GPA; advanced courses × GPA	$\beta = .21$	$t = 8.91$, $p < .001$
Olson, 1988 Dissertation	191 Original data Grades 3–6	Multiple regression	Math achievement test score (California Test of Basic Skills), reading achievement test score (California Test of Basic Skills)	Homework achievement, homework, student homework behavior, parent-involvement, % preparatory homework, % distributed homework, homework feedback, student ability	Math: $\beta = .10$ Reading: $\beta = .11$	$F = 2.71$, *ns* $F = 3.07$, *ns*
Portes & MacLeod, 1996 Journal article	5,266[b] Original data Grades 8, 9	Multiple regression	Math achievement test score (Stanford Achievement Test), reading achievement test score (Stanford Achievement Test)	Age, sex, parental SES, length of U.S. residence, homework, region, ethnicity	Math: $b = 2.50$ Reading: $b = 1.74$	$p < .01$ $p < .01$

Portes & Zady, 2001 Conference paper	5,264 Same data as used by Portes & MacLeod, 1996 Grades 8, 9	Hierarchical multiple regression	Reading achievement test score (Stanford Achievement Test)	Block 1: grade, age, English proficiency, SES, sex, school location, length of stay in U.S. Block 2: economic situation, homework, TV time, peer relations, familialism, perceived English proficiency, discrimination, motivation, ethnic pull, father presence, self-esteem	Spanish speakers: $\beta = .07$ Asian origin: $\beta = .06$	$p = .002$ $p = .03$
Smith, 1990 Journal article	1,584 Original data Grade 7, 9	Multiple regression	Overall achievement test score (California Test of Basic Skills) Reading scores Language scores Math scores	Ethnicity, sex, year in school, family background, chore time, ethnicity × chore time, homework, friends, year × friends, TV time, job × TV time, radio and recordings time, reading time	Overall: $\beta = .00$ Reading: $\beta = -.02$ Language: $\beta = .00$ Math: $\beta = .04$	*ns* *ns* *ns* *ns*

(continued)

TABLE 5 (*Continued*)

Author, year, and document type	Sample characteristics	Modeling technique	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Smith, 1992 Journal article	1,208 Original data[c] Grades 9, 11	Multiple regression	Overall achievement test scores (California Test of Basic Skills) Reading scores Language scores Math scores	Previous achievement, ethnicity, sex, year in school, family background, parent time, chore time, homework, year × homework, friends, TV time, occupation × TV time, radio and recordings time, reading time	Overall: $\beta = -.02$ Reading: $\beta = -.02$ Language: $\beta = -.06$ Math: $\beta = .01$	*ns* *ns* $p < .01$ *ns*
Wynn, 1996 Dissertation	170 Original data Grade 3	Path analysis: multiple regression	GPA (combined reading, math, spelling, language arts, social studies, and science)	Exogenous: ethnicity, SES, sex Endogenous: ability, family involvement, homework, TV time	$\beta = .04$	*ns*

Note. NR = not reported.
[a]This is a weighted sample size for 12th-grade students who participated in LSAY in all 3 years from 1987 to 1989 and who completed the mathematics achievement test.
[b]This sample consisted of Cuban, Vietnamese, Haitian, and Mexican second-generation children of immigrants from Florida and California.
[c]For this study, achievement test data from 1992 were added to the Smith (1990) data set. Thus, data are from the same students as in Smith (1990).

TABLE 6

Characteristics of structural equation modeling studies using data from the National Education Longitudinal Study (1988, 1990, or 1992)

Author, year, and document type	Sample characteristics	Program used	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Bruce & Singh, 1996 Journal article	24,599 with pairwise deletion $N = 21{,}835$, minimum $N = 18{,}029$ NELS: 88; Grade 8	LISREL	Achievement test composite (math, science, reading, and social studies)	Exogenous: ethnicity, family background, sex Endogenous: previous achievement, quality of instruction, motivation, homework, quantity of instruction	$\beta = .06$	NR
Keith, Diamond-Hallam, & Fine, 2004 Analysis 1, journal article	13,546 NELS: 88, 90, 92; Grades 8, 10, 12	Amos	1990 High school grades (English, math, science, and social studies) 1992 Achievement test scores (reading, math, science, and social studies)	Exogenous: ethnicity, family background Endogenous: Grade 8 achievement, homework in Grades 10 and 12 (in-school), homework in Grades 10 and 12 (out-of-school)	Class grades: $\beta = .28$ Achievement test scores: $\beta = .13$	NR NR
Keith, Diamond-Hallam, & Fine, 2004[a] Analysis 2, journal article	13,546 NELS: 88, 90, 92; Grades 8, 10, 12	Amos	1992 High school grades (English, math, science, and social studies)	Exogenous: ethnicity, family background Endogenous: Grade 8 achievement, homework in Grade 10 (in-school), homework in Grade 10 (out-of-school)	$\beta = .18$	NR
Keith & Keith, 1993[a] Journal article	21,814, with pairwise deletion Minimum $N = 18{,}355$ NELS: 88; Grade 8	LISREL	Achievement test scores (reading, math, science, and social studies)	Exogenous: ethnicity, family background Endogenous: Previous achievement, parental involvement, homework, TV time	$\beta = .19$	NR
Singh, Granville, & Dika, 2002 Journal article	3,227 NELS: 88; Grade 8[b]	LISREL	Math achievement (class grades in math, Grades 6 and 8, math achievement test score)	Exogenous: attendance motivation, coursework motivation Endogenous: math attitude, academic time (math homework, TV time)	$\beta = .50$[c]	$p < .05$
			Science achievement (class grades in science, Grades 6 and 8, science achievement test score)	Exogenous: attendance motivation, coursework motivation Endogenous: science attitude, academic time (science homework, TV time)	$\beta = .61$[c]	$p < .05$

Note. NELS = National Education Longitudinal Study. [a]These data also appear in a conference paper by Keith, Keith, Bickley, and Singh (1992). [b]The data were a 25% random sample from the NELS:88 data set. [c]A "clean" coefficient from homework to mathematics achievement could not be obtained for this path. The reported direct path coefficient includes parameters associated with TV time combined with time spent on mathematics homework.

TABLE 7
Characteristics of structural equation modeling studies using data from the High School and Beyond (1980, 1982, or 1984) longitudinal studies

Author, year, and document type	Sample characteristics	Program used	Outcome variable(s)	Predictor variables	Regression coefficient	Test result and significance
Camp, 1990 Journal article	7,668 Grade 10	LISREL	Student-reported class grades, high school GPA	Exogenous: sex, academic ability, family background Endogenous: TV time, homework, job time, student participation in activities	$\beta = .06$	$t = 5.46$, $p < .05$
Keith & Benson, 1992 Journal article	13,152, with pairwise deletion $N = 12{,}142$, minimum $N = 8{,}910$ 1980, 1982, 1984 Grades 10, 12	LISREL	High school grades composite	Exogenous: ethnicity, sex, family background Endogenous: ability, quality of instruction, motivation, homework, New Basics coursework	$\beta = .06$	NR
Keith & Benson, 1992 Journal article	5,658 Caucasian, 1,039 African American, 1,791 Hispanic, 248 Asian American, 147 Native American (minimum *N*s) 1980, 1982, 1984 Grades 10, 12	LISREL	High school grades composite	Ability, quality of instruction, motivation, homework, New Basics coursework	Caucasian: $\beta = .06$ African American: $\beta = .09$ Hispanic: $\beta = .05$ Asian American: $\beta = .25$ Native American: $\beta = -.09$	$p < .05$ $p < .05$ $p < .05$ $p < .05$ *ns*
Keith & Cool, 1992 Journal article	25,875, with pairwise deletion Minimum $N = 21{,}427$ 1980, 1982 Grades 10, 12	LISREL	1982 Achievement test composite (combined reading, math I and II, science, writing, civics)	Exogenous: ethnicity, sex, family background Endogenous: ability, quality of instruction, motivation, quantity of coursework, homework	$\beta = .06$	$p < .05$

parent, and a measure of academic achievement. These studies are listed in Table 8. The 32 studies reported 69 separate correlations based on 35 separate samples of students. Cooper et al. (1998) reported 8 correlations, separating out effects for elementary and secondary students (two independent samples) on both class grades and standardized tests with time on homework reported by either students or parents. Drazen (1992) reported 12 correlations, for reading, math, and multiple subjects for three national surveys (three independent samples). Bents-Hill and colleagues (1988) reported 8 correlations, for language arts, math, reading, and multiple subjects both for class grades and for a standardized test of achievement. Epstein (1988), Olson (1988), and Walker (2002) each reported 2 effect sizes, 1 for math and 1 for reading. Fehrmann et al. (1992), Wynn (1996), and Keith and Benson (1992) each reported 2 correlations, 1 involving class grades and 1 involving achievement test results. Hendrix et al. (1990) reported 3 correlations, 1 for multiple subjects, 1 for verbal ability, and 1 for nonverbal ability. Mau & Lynn (2000) reported 3 correlations, 1 for math, 1 for reading, and 1 for science. Singh et al. (2002) reported 2 correlations for math and 1 for science.

The 32 studies appeared between the years 1987 and 2004. The sample sizes ranged from 55 to approximately 58,000 with a median size of 1,584. The mean sample size was 8,598 with a standard deviation of 12,856, suggesting a nonnormal distribution. The Grubbs test revealed a significant outlier, $p < .05$. This sample was the largest in the data set, reported by Drazen (1992) for six correlations obtained from the 1980 High School and Beyond longitudinal study. As a result, we replaced these six sample sizes with the next largest sample size in the data set, 28,051. The mean sample size for the adjusted data set was 7,742 with a standard deviation of 10,192.

Only three studies specifically mentioned that students were drawn from regular education classrooms, and one of these studies included learning-disabled students as well (Deslandes, 1999). The remaining studies did not report information on the students' achievement or ability level. Seventeen studies did not report information on the socioeconomic status of students, 11 reported that the sample's SES was "mixed," 3 described the sample as middle SES, and 1 as lower SES. Seventeen studies did not report the sex make-up of the sample, while 14 reports said the sample was comprised of both sexes. Only one study reported correlations separately for males and females. Because of a lack of reporting or variation across categories, no analyses were conducted on these variables.[1]

Of the 69 correlations, 50 were in a positive direction and 19 in a negative direction. The mean unweighted correlation across the 35 samples (averaging multiple correlations within each sample) was $r = .14$, the median was $r = .17$, and the correlations ranged from $-.25$ to .65.

The weighted average correlation was $r = .24$ using a fixed-error model with a 95% confidence interval (95% CI) from .24 to .25. The weighted average correlation was $r = .16$ using a random-error model with a 95% confidence interval from .13 to .19. Clearly, then, the hypothesis that the relationship between homework and achievement is $r = 0$ can be rejected under either error model. There were no significant outliers among the correlations, so all were retained for further analysis.

The trim-and-fill analyses were conducted in several different ways. We performed the analyses looking for asymmetry, using both fixed and random-error models to impute the mean correlation and creating graphs using both fixed and random

TABLE 8

Characteristics of studies correlating time on homework and academic achievement

Author and year	Document type	Number of students	Respondent type	Outcome measure	Grade level	Subject matter	Correlation
Antonek, 1996	Unpublished	89	Students	Other test	3–5	Foreign language	+.26
Bents-Hill, 1988	Unpublished	1,865	Parents	Class grades	3, 6	Language arts	−.01
						Math	−.04
						Reading	−.04
						Multiple subjects	−.03
						Language arts	−.06
				Standardized test		Math	−.08
						Reading	−.07
						Multiple subjects	−.09
Bowen & Bowen, 1998	Published	538	Students	Class grades and relative standing	Middle and high school	Multiple subjects	+.20
Broxie, 1987	Unpublished	55	Students	Class grades	4–6	Multiple subjects	+.65
Bruce, 1996	Published	21,835	Students	Standardized test	8	Multiple subjects	+.20
Cool & Keith, 1991	Published	28,051	Students	Standardized test	12	Multiple subjects	+.30
Cooper, Lindsay, Nye, & Greathouse, 1998	Published	~285	Students	Class grades, standardized test	2, 4	Multiple subjects	−.19
							−.04
			Parents	Class grades, standardized test			−.13
							−.06
		~424	Students	Class grades, standardized test	6–12		+.17
							.00
			Parents	Class grades, standardized test			+.24
							+.14
Deslandes, 1999	Published	637	Students	Class grades	High school	Language arts	+.18[a]

Drazen, 1992	Unpublished	~19,000 (NELS: 72)	Students	Standardized test	12	Reading	+.17
						Math	+.20
						Multiple subjects	+.20
		~58,000 (High School and Beyond: 80)[b]			10	Reading	+.25
						Math	+.29
						Multiple subjects	+.30
					12	Reading	+.23
						Math	+.28
						Multiple subjects	+.27
		~25,000 (NELS: 88)			8	Reading	+.17
						Math	+.20
						Multiple subjects	+.20
Epstein, 1988	Unpublished	1,021	Parents	NR	1, 3, 5	Math	−.05
						Reading	−.11
Fehrman, Keith, & Reimers, 1987	Published	28,051	Students	Class grades, standardized test	12	Multiple subjects	+.32
							+.25
Hendrix, Sederberg, & Miller, 1990	Published	1,521	Students	Class grades	12	Multiple subjects	+.35
						Nonverbal ability	+.16
				NR		Verbal ability	+.17
Hightower, 1991	Unpublished	9,002	Students	Standardized test	12	Multiple subjects	+.29
Keith & Benson, 1992	Published	8,910	Students	Class grades, standardized test	10, 12	Multiple subjects	+.30
							+.22
Keith & Cool, 1992	Published	21,427	Students	Standardized test	10, 12	Multiple subjects	+.30
Keith, Diamond-Hallam, & Fine, 2004	Published	6,773	Students	Standardized test	12	Multiple subjects	+.22
Lam, 1996	Unpublished	3,657	Students	Standardized test	12	Multiple subjects	+.04
Mau & Lynn, 2000	Published	20,612	Students	Standardized test	10, 12	Math	+.29
						Reading	+.24
						Science	+.23
Olson, 1988	Unpublished	191	Students	Standardized test	3–6	Math	+.11
						Reading	+.10

(continued)

TABLE 8 (*Continued*)

Author and year	Document type	Number of students	Respondent type	Outcome measure	Grade level	Subject matter	Correlation
Peng & Wright, 1994	Published	24,599	Students	Standardized test	8	Multiple subjects	+.17
Pezdek, Berry, & Renno, 2002	Published	380	Parents	Another test	4–6	Math	+.15[c]
Roberts, 2000	Unpublished	7,178	Students	Standardized test	8	Science	+.26
Rozevink, 1995	Unpublished	363	Students	Standardized test	9, 12	Multiple subjects	−.23
Schewior, 1992	Unpublished	4,930	Students	Standardized test	12	Math	+.20[d]
Singh, Granville, & Dika, 2002	Published	3,227	Students	Class grades	8	Math	+.11
						Science	+.10
				Standardized test		Math	+.30
Smit, 1990	Published	1,584	Students	Standardized test	7, 9	Multiple subjects	−.08
Thomas, 2001	Unpublished	450	Students	Standardized test	8	Math	+.22
Tonglet, 2000	Unpublished	189	Students	Class grades	5, 8	Math	+.47
Vazsonyi & Pickering, 2003	Published	764	Students	Class grades	High school	Multiple subjects	−.03
Walker, 2002	Unpublished	86	Students	Standardized test	4, 5	Math	+.17
						Reading	+.25
Wynn, 1996	Unpublished	170	Parents	Class grades, standardized test	3	Multiple subjects	+.00
							−.17
Wynstra, 1995	Unpublished	68	Parents	Standardized test	1–5	Language arts	−.25

Note. NELS = National Education Longitudinal Study, NR = no response.

[a]This effect size is collapsed across general classroom ($n = 525$, $r = .17$) and learning disabled ($n = 112$, $r = .20$) students.

[b]Effect sizes were also presented for High School and Beyond: 82: Reading, $r = .26$; Math, $r = .28$; multiple subjects, $r = .29$. These effect sizes were not used in the primary analyses because they represent the same students who were surveyed in High School and Beyond: 80.

[c]This effect size was computed from correlations between the amount of time that students spent on mathematics homework and mathematics achievement across two studies for which grade level was statistically controlled post hoc (Study 1, $n = 165$, $r = .16$; Study 2, $n = 215$, $r = .14$).

[d]This effect size is collapsed across two effect sizes representing two levels of student-reported time spent on homework. Students who completed homework all of the time ($r = +.27$) reported a larger effect for homework than did students who completed their homework most or all of the time ($r = +.13$).

models (see Borenstein et al., 2005) while searching for possible missing correlations on the left side of the distribution (those that would reduce the size of the positive correlation). None of the analyses produced results different from those described above. When we used a random-error model, there was evidence that three effect sizes might have been missing and that imputing them would lower the mean fixed-effect correlation to r = .23 (95% CI = .22/.23). The random-error results of this analysis were r = .14 (95% CI = .11/.17).[2]

Next, we carried out a moderator analysis examining the association between the magnitude of correlations and the publication status of the study report. Seventeen of the samples had been published and their results were compared with those of the 18 samples that had appeared as dissertations, ERIC documents, or unpublished research reports. Under the fixed-error model, correlations from journal articles, r = .25, were significantly higher than those from unpublished sources, r = .23, $Q(1) = 20.71$, $p < .0001$. Under the random-error model, correlations from journal articles, r = 18, were not statistically different from those from unpublished sources, r = .15, $Q(1) = 0.91$, *ns*. In both instances, the absolute size of the difference was quite small.

Moderator Analyses

Table 9 presents the results of analyses examining whether the magnitude of the correlation between time spent on homework and achievement was moderated by the type of achievement measure. Two studies using unstandardized tests scores, one using a composite of standardized tests and class grades, and one not reporting the type of achievement outcome were omitted from this analysis because there were too few studies in each of these outcome-type categories. Thus the moderator analysis compared results involving class grades with results involving standardized achievement tests.

Under fixed-error assumptions, the correlation between time spent on homework and class grades, r = .27 (95% CI = .26/.27), was significantly higher than that involving standardized achievement test scores, r = .24 (95% CI = .24/.25), $Q(1) = 26.26$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and class grades, r = .19 (95% CI = .11/.27), was not significantly different from that involving standardized achievement test scores, r = .16 (95% CI = .14/.19), $Q(1) = 0.35$, *ns*. In both instances, the absolute difference between the correlations was quite small.

Table 9 also presents the results of analyses examining whether the magnitude of the correlation between time spent on homework and achievement was moderated by the grade level of the students. Correlations were grouped into those involving elementary school students, Grades K–6, and secondary school students, Grades 7–12. One study (Tonglet, 2000) was omitted from the analysis because it included students in Grades 5 and 8 and the correlation for the two grades could not be separated. One correlation from Cooper et al. (1998) was omitted from the analysis because it included students in Grades 6–12. Tonglet (2000) and Cooper et al. (1998) reported sampling students from Grades 5 and 8, and 6–12, respectively. The correlation between time spent on homework and class grades was +.47 for Tonglet. The correlation was +.21 for Cooper et al., who also reported a correlation of +.07 between time spent on homework and standardized achievement test scores.

TABLE 9

Results of moderator analyses involving correlations between time on homework and measures of academic achievement

Grouping variable	Number of comparisons	95% Confidence interval: Low estimate		Mean		High estimate	
Overall	35	+.240	(+.134)	+.243	(+.161)	+.246	(+.188)
Outcome:							
$Q(1) = 26.26$* (.35)							
Class grades	12	+.26	(+.11)	+.27*	(+.19*)	+.27	(+.27)
Standardized test	26	+.24	(+.14)	+.24*	(+.16*)	+.25	(+.19)
Grade level:							
$Q(1) = 710.68$* (10.43*)							
K–6	10	−.06	(−.03)	−.04*	(+.05)	−.02	(+.13)
7–12	23	+.25	(+.17)	+.25*	(+.20*)	+.25	(+.22)
Subject matter:							
$Q(2) = 164.62$* (2.46)							
Reading	8	+.20	(+.07)	+.21*	(+.12*)	+.21	(+.18)
Math	13	+.24	(+.13)	+.24*	(+.18*)	+.25	(+.22)
Multiple subjects	18	+.24	(+.12)	+.25*	(+.16*)	+.25	(+.20)
Respondent:							
$Q(1) = 631.70$* (20.06*)							
Students	30	+.25	(+.16)	+.25*	(+.19*)	+.25	(+.21)
Parents	7	−.05	(−.10)	−.03*	(−.02)	−.01	(+.07)

Note. *$p < .05$. Random effects Q_b values and point estimates are presented in parentheses.

Figure 1 presents a stem-and-leaf display of the 33 correlations associated with this analysis.

Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher for secondary school students, $r = .25$ (95% CI = .25/.25), than for elementary school students, $r = -.04$ (95% CI = −.06/−.02), $Q(1) = 710.68$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and achievement was also significantly higher for secondary school students, $r = .20$ (95% CI = .17/.22), than for elementary school students, $r = .05$ (95% CI = −.03/.13), $Q(1) = 10.43$, $p < .002$. As indicated by the confidence intervals, using the random-error model, the mean correlation between time spent on homework and achievement was not significantly different from zero for elementary school students.

Table 9 also presents the results of analyses examining whether the homework–achievement correlation was moderated by the subject matter of the homework assignment. One study involving science, 1 involving foreign language, and 1 involving verbal and nonverbal ability were omitted from the analysis because there were too few studies in each of these outcome-type categories. Thus the moderator analysis compared only studies involving language arts, reading, mathematics, and achievement across multiple subject domains.

First, we compared correlations involving language arts with correlations involving reading. Using fixed-error assumptions, the three correlations involving language arts revealed a nonsignificant average weighted correlation of $r = -.01$ (CI = −.04/.02), while the eight reading outcomes produced a significant positive correlation of $r = .21$ (CI = .20/.21). These average correlations were significantly different from one another, $Q(1) = 202.94$, $p < .0001$. Using random-error assumptions, the average language arts correlation was nonsignificant, $r = .01$ (CI = −.10/.13), while reading produced a significant positive correlation, $r = .12$ (CI = .07/.18). These average

Lower Grades (1–6)	Stem	Upper Grades (7–12)
5	+.6	
	+.3	00
6	+.2	998665
1	+.2	32200000
5	+.1	877
1	+.1	
	+.0	4
689	−.0	38
1	−.1	
5	−.2	3

FIGURE 1. *Distribution of correlations between time on homework and achievement as a function of grade level.*

correlations approached being significantly different from one another, $Q(1) = 2.71$, $p < .10$. Because of these results, we chose not to combine the language arts and reading data sets but instead to use only reading correlations in the subsequent analyses examining subject matter as a moderator.

The average weighted correlations between time on homework and reading, math, and multiple subjects were significantly different from one another under fixed-error assumptions, $Q(2) = 164.62$, but not under random-error assumptions, $Q(2) = 2.46$, *ns.* We then proceeded to conduct two planned comparisons, one comparing reading outcomes with math outcomes and one comparing both math and reading outcomes with outcomes involving measures of multiple subjects.

Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher for math, $r = .24$ (95% CI = .24/.25) than for reading, $r = .21$ (95% CI = .20/.21), $Q(1) = 99.92$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and achievement was not significantly different for math, $r = .18$ (95% CI = .13/.23), than for reading, $r = .12$ (95% CI = .07/.18), $Q(1) = 2.46$, *ns.* In both instances, the absolute difference between the correlations was quite small.

Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher for multiple subjects, $r = .25$ (95% CI = .25/.25) than for either reading or math alone, $r = .23$ (95% CI = .22/.23), $Q(1) = 64.70$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and achievement was not significantly different for multiple subjects, $r = .16$ (95% CI = .12/.20), in comparison with that for reading or math alone, $r = .16$ (95% CI = .12/.19), $Q(1) = 0.004$, *ns.* Again, in both instances, the absolute difference between the correlations was quite small.

Finally, Table 9 presents the results of analyses examining whether the homework and achievement correlation was moderated by who provided data on the amount of time spent on homework. All studies included information about whether it was the student or a parent who was the respondent.

Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher when students made the report, $r = .25$ (95% CI = .25/.25) than when parents reported, $r = -.03$ (95% CI = −.05/−.01), $Q(1) = 631.70$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and achievement was still significantly stronger for students, $r = .19$ (95% CI = .16/.21), than for parents, $r = -.02$ (95% CI = −.10/.07), $Q(1) = 20.06$, $p < .0001$. Using the random-error models, the correlations involving parent reports were not significantly different from zero.

Tests for Interactions Among Moderators

We next tested whether the main effects of moderator variables also held when tested within levels of other moderator variables. Specifically, we tested (a) whether the grade level of the student was associated with the magnitude of the homework–achievement correlation when the student was tested within different types of outcome measures; (b) whether the grade level of the student was associated with the magnitude of correlation when the student was tested within different types of subject matter; and (c) whether the subject matter of homework was associated with the magnitude of the homework–achievement correlation when the student was tested within different types of outcome measures.

The findings produced a pattern of results regarding the direction and significance for the moderator's effect that was consistent with the main effects in 13 of the 14 subgroup analyses. That is, both the direction of the comparison between correlations and the significance of the difference between correlations (using both fixed and random models) was the same when we compared the subgroup analyses to the main effect analyses in all instances but one. The exception was that when we used a random-error model to compare the relationship between homework and class grades for four correlations at the elementary school level, $r = .09$ (95% CI = −.10/.28), and six correlations at the secondary level, $r = .21$ (95% CI = .12/.30), the difference was not significantly different from zero, $Q(1) = 1.18$, *ns*. The direction of the difference between the mean correlations was the same as that in the main effect analyses.

Finally, we looked to see whether the respondent providing information about homework (the student or a parent) was confounded with any of the other three moderator variables. We found that 3 times parents provided information on homework in correlations involving class grades and 4 times when correlations involved achievement tests. Similarly, 3 times parents provided information when homework was associated with math, 2 times when associated with reading, and 3 times with multiple subjects.

However, all parent reports on the amount of homework were provided for students who were in Grades K–6.[3] Therefore it was possible that the significant difference suggesting that the homework–achievement relationship was smaller for elementary school than secondary school students might not hold if students were respondents. To test this hypothesis, we re-ran the grade level analyses using only students as respondents.

Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher for secondary school students, $r = .25$ (95% CI = .25/.25), than for elementary school students, $r = .06$ (95% CI = −.00/.11), $Q(1) = 47.48$, $p < .0001$. Under random-error assumptions, the correlation between time spent on homework and achievement was not significantly higher for secondary school students, $r = .19$ (95% CI = .17/.22), than for elementary school students, $r = .22$ (95% CI = .00/.42), $Q(1) = 0.57$, *ns*.

In light of these results, it is not surprising that we also found differences between student and parent reports at the elementary school level. Under fixed-error assumptions, the correlation between time spent on homework and achievement was significantly higher when elementary school students made the report, $r = .06$ (95% CI = .00/.11), than when parents of elementary school students made the report, $r = -.06$ (95% CI = −.08/−.04), $Q(1) = 14.40$, $p < .001$. Under random-error assumptions, the correlation between time spent on homework and achievement was still significantly stronger for elementary school student reports, $r = .22$ (95% CI = −.00/.42), than for parents, $r = -.05$ (95% CI = −.11/.01), $Q(1) = 5.40$, $p < .03$. It appears that, for elementary school students, parents report a small negative relationship between the amount of time their child spends on homework and their achievement, while the students themselves report a positive relationship.

Studies Correlating Time on Homework and Non-Achievement Measures

We found 5 studies that presented correlations between the amount of time students spent doing homework and student attitudes. Characteristics of these studies can be found in Table 10. Using a fixed-error model, the unweighted mean

TABLE 10

Characteristics of studies examining the correlation between time on homework and attitudes or conduct

Author and year	Document type	Respondent type	Sample size	Grade level	Subject matter	Correlation
Attitudes						
Cooper, Jackson, Nye, & Lindsay, 2001	Journal article	Students	214	2, 4	Student attitudes toward homework	+.03
Cooper, Lindsay, Nye, & Greathouse, 1998	Journal article	Students	709 total 285, Grades 2, 4 424, Grades 6–12	2–12	Student attitudes toward homework	+.06[a]
Hendrix, Sederberg, & Miller, 1990	Journal article	Students	1,521	12	Student attitudes concerning importance of school performance, relevance of schoolwork, effort, school-related self-esteem	+.37
Singh, Granville, & Dika, 2002	Journal article	Students	3,227	8	Student attitudes toward math and science	+.11[b]
Tonglet, 2000	Dissertation	Students	189	5, 8	Student attitudes concerning ability	+.03[c]
Conduct						
Epstein, 1988	Report	Parents	1,021	1, 3, 5	Conduct in school	−.01
Vazsonyi & Pickering, 2003	Journal article	Students	181	High school	Conduct as measured by the Normative Deviance Scale (Vasonyi & Pickering, 2000)	−.27[d]

Note. Effect sizes are coded so that positive correlations indicate that more homework is associated with more positive attitudes and fewer conduct problems.

[a]This effect size was computed from separate reported effect sizes of $r = .00$ for Grades 2 and 4 and $r = +.10$ for Grades 6–12.

[b]This effect size was computed from separate correlations between time spent on homework and how much students looked forward to math classes, $r = +.08$, and science classes, $r = +.10$, as well as the degree of usefulness that students attributed to classes in math, $r = +.10$, and science, $r = +.14$.

[c]This effect size was computed from the combined correlations between homework compliance and self-efficacy, $r = +.05$, and time spent on homework and self-efficacy, $r = +.01$.

[d]This effect size was computed from separate correlations between time spent on homework and Normative Deviance Scale scores for Caucasian students ($n = 627$), $r = +.28$, and African American students ($n = 182$), $r = +.24$.

correlation was $r = .12$. The weighted mean correlation was $r = .13$ (95% CI = .11/.14), which was significantly different from zero. Using a random-effect error model, the weighted mean correlation was $r = .13$ (95% CI = −.01/.26), not significantly different from zero.

Two studies looked at time on homework and student conduct problems. These studies are also presented in Table 10. Epstein (1988) found a near zero, $r = .01$, correlation between elementary-school parent reports of the time their child spent on homework and their conduct in school. However, Vazsonyi and Pickering (2003) found a significant negative relationship between how much time high school students reported spending on homework and their scores on the Normative Deviance Scale. Further, the relationship held for both Caucasian students, $r = .28$, and African American students, $r = .24$, separately.

Discussion

Summary of Studies on the Causal Relationship Between Homework and Achievement

Studies that have attempted to establish a causal link between homework and academic achievement have done so using several different research designs: (a) randomly assigning classrooms or students within classrooms to homework and no-homework conditions; (b) assigning homework to classrooms in a nonrandom manner but attempting statistical control of rival hypotheses; (c) using naturalistic measurement to assess both the amount of homework students do and their achievement, but attempting statistical control of rival hypotheses; and (d) testing structural equation models using naturalistic data.

The studies that randomly assigned classrooms or students within classrooms to homework and no-homework conditions were all flawed in some way that compromised their ability to draw strong causal inference. Thus we await studies that individually permit strong conclusions establishing the productive impact of homework on achievement. Still, the findings from the three studies that used random assignment did not differ in their mean effect size from the two studies that used other techniques to produce equivalent groups.

Further, the findings from manipulated-homework study designs were quite consistent and encouraging, if not conclusive. They revealed a positive relationship between homework and achievement that was robust against conservative re-analyses, including those using adjusted sample sizes and imputing possible missing data. The standardized mean difference on unit tests between students who did and did not do homework varied from $d = .39$ to $d = .97$. The weighted mean d-index was .60 under both fixed and random-error assumptions and was significantly different from zero when the student was used as the unit of analysis. When we substituted the effective sample size as the unit of analysis by adjusting for within-class dependency, the weighted mean d-index was .63 and was statistically significant, up to an assumed intraclass correlation of .35. Further, we could not reject the hypothesis that all the effect sizes from these studies were testing the same underlying population value. This was true whether fixed- or random-error assumptions were used.

Similarly, the range of estimated regression coefficients derived from studies using multiple regression, path analysis, or structural equation modeling were nearly all positive and significant. The regression coefficients appeared quite similar across subject areas. However, as with the studies described above, we would caution against drawing any conclusions regarding the mediating role of other variables on the homework–achievement relationship from this rather limited data set. The number and type of predictors in each model was complex, varied considerably from model to model, and potentially were confounded with one another across studies. Also, the estimates using naturalistic data and controlling for other variables were calculated primarily by using high school student samples.

While each set of studies is flawed, in general the studies tend not to share the same flaws. Across the set, a wide variety of students have provided data, and the effects of homework have been tested in multiple subject areas. The studies have controlled for or tested many plausible rival hypotheses in various combinations. Homework has been embedded within diverse structural models. With only rare exceptions, the relationship between the amount of homework students do and their achievement outcomes was found to be positive and statistically significant. Therefore, we think it would not be imprudent, based on the evidence in hand, to conclude that doing homework causes improved academic achievement. Of course, this assertion should not inhibit future efforts to establish more firmly this productive relationship.

The same diversity of research designs that permits optimism regarding a causal connection also makes the pinpointing of moderators of the homework–achievement relationship very problematic. Each study differs from other studies on multiple dimensions, and few studies are contained in each combination of multiple design features. This makes it difficult, if not impossible, to disentangle moderator effects by testing for plausible confounds when a moderating variable is found. Therefore, it seems unwise to use the limited data from these designs to draw inferences about what variables might be associated with the magnitude of the homework–achievement relationship. In order to get a first approximation of what these variables might be, we turn instead to an examination of a larger body of research that simply estimated the correlation between time spent on homework and achievement, without attempting to establish a causal direction for the relationship.

Summary of Homework–Achievement Correlations and Moderator Analyses

We found 69 correlations between homework and achievement reported in 32 documents. Fifty correlations were in a positive direction and 19 in a negative direction. The mean weighted correlation was $r = .24$ using a fixed-error model, and $r = .16$ using a random-error model, and both were significantly different from zero.

Moderator Analyses

It is important to keep in mind two cautions when interpreting the results of moderator analyses using correlation coefficients. First, synthesis-generated evidence should not be misinterpreted as supporting statements about causality (see Cooper, 1998). When groups of effect sizes are compared within a research synthesis, regardless of whether they come from simple correlational analyses or controlled experiments using random assignment, the synthesis can only establish an association between a moderator variable and the outcomes of studies, not a causal connection. For example, it might be found that a set of studies reporting a larger-than-average effect of homework was also conducted at upper-income schools. However, it might also be the case (known or unknown to the synthesist) that these studies tended to use unusually long homework assignments. The synthesist cannot discern which characteristic of the studies, if either, produced the larger effect. Thus, when different study characteristics are found to be associated with the effects of an intervention or the size of a correlation, the synthesist should recommend that future research examine these factors using a more systematically controlled design so that its causal impact can be appraised.

The second caution relates specifically to moderator analyses that use correlations. In the current synthesis, we are interested in the causal impact of homework on achievement. We are not interested in whether achievement also might effect time on homework (such that, for example, receiving higher grades causes students to work harder on assignments). However, we know that the size of the correlation between homework and achievement might reflect the size not only of (a) the homework-causes-achievement relationship but also of (b) the achievement-causes-homework relationship and (c) any spurious relationship between the two. Thus, unlike moderator analyses that use effect sizes from experiments, moderator analyses that use correlations must acknowledge the possibility that any uncovered relationships might be reflecting moderation of any of these three potential influences on the correlation (or that relationships involving moderators of interest are being suppressed by other relations captured by the correlation). Again, this suggests that moderator analyses in research syntheses should be interpreted with caution and used to guide future, more definitive, research.

Because of a lack of reporting or a lack of variation in some of the moderators we hoped to test, only four variables were used in quantitative analyses. Two of these, the type of outcome measure and the subject matter of the homework, revealed that time on homework was positively associated with both class grades and standardized test scores, and with reading-only, math-only, and multiple-subject outcomes. Under fixed-error assumptions, the association with homework was stronger for grades than for standardized tests, for math than for reading, and for multiple-subject outcomes than for reading and math combined. However, neither difference in association was significant under random-error assumptions, and in all instances the difference was quite small, never exceeding a difference between correlations of .06. Thus, beyond suggesting that the homework–achievement association was robust across these subsets of data, we would caution against drawing a conclusion that these moderators were important practical influences on the strength of the relation. This is especially true for subject areas because many subjects (e.g., language arts, writing, science, social studies) were not tested frequently enough to be included in the analysis.

The two other moderator variables, (a) the grade level of the student and (b) whether the student or parent reported about homework, present a different picture. For grade level, there was strong evidence that homework and achievement were positively related for secondary school students. A significant, though small, negative relationship was found for elementary school students, using fixed-error assumptions, but a nonsignificant positive relationship was found using random-error assumptions. Moreover, with both error models, the difference between the mean correlations involving elementary versus secondary students was significant.

For differences among respondents, analyses using both error models suggested that student reports about homework were significantly positively related to achievement, while parent reports produced a significant, near-zero correlation using a fixed-error model. Correlations involving the two types of respondents differed significantly. Finally, because all parent reports came from parents of elementary school students, a re-analysis of the grade-level effect was conducted excluding parent reports. This analysis still showed a higher correlation for secondary than for elementary school students under fixed-error assumptions but no difference under random-error assumptions. Not surprisingly, we also found that the correlation between time spent on homework and achievement was significantly higher when elementary school students made the report than when parents of elementary school students made the report.[4]

Explaining the Grade Level Association

There are several possible explanations for why the homework–achievement relationship differs at different grade levels. First, research in cognitive psychology indicates that age differences exist in children's ability to selectively attend to stimuli (Lane & Pearson, 1982; Plude, Enns, & Broudeur, 1994). Younger children are less able than older children to ignore irrelevant information or stimulation in their environment. Therefore, we could extrapolate that the distractions present in a young student's home environment would make home study less effective for them than for older students.

Second, younger students appear to have less effective study habits. This diminishes the amount of improvement in achievement that might be expected from homework given to them. For example, Dufresne and Kobasigawa (1989) had first-, third-, fifth-, and seventh-grade students study booklets of paired word items. They found that fifth and seventh graders spent more time studying harder items and were more likely to achieve perfect recall. Older students were also more likely to use self-testing strategies to monitor how much of the material they had learned.

At least four other explanations for the weak relationship between homework and achievement in early grades are possible. These relate more directly to the amount and purposes of homework assigned by teachers, rather than to the child's ability to benefit from study at home. Muhlenbruck, Cooper, Nye, and Lindsay (1999) found no evidence to suggest that the weaker correlation in elementary school was associated with a range restriction in the amounts of homework in early grades or that teachers assigned more homework to poorly performing classes. Evidence did suggest that teachers in early grades assigned homework more often to develop young students' management of time, a skill rarely measured on standardized achievement tests. Finally, they found some evidence that young students who were struggling in school took more time to complete homework assignments.

These last two findings suggest why the grade-level effect on homework must be viewed with caution. While it seems highly plausible to suggest that the evidence on age difference in attention span and study habits can be extrapolated to the homework situation, it is also still plausible that the relationship is due, in whole or in part, to poorer achievement in young children causing them to spend more time on homework. Or it may be that in earlier grades homework is being used for purposes other than improving immediate achievement outcomes. That is, teachers may use homework for other purposes in earlier grades because they are aware of its limited potential for improving achievement. Thus, just as we would suggest that carefully controlled studies of the causal relationship between homework and achievement be undertaken, we would also recommend that these studies include students from a variety of grade levels and that grade level be used as a moderating variable.

Explaining the Respondent Association

We can turn to some early work in social psychology, related to attribution theory, to help explain why a positive relationship between time on homework and achievement is obtained when students provide homework reports and yet the relationship hovers near zero when parents provide reports (Jones & Nisbett, 1972; Green, Lightfoot, Bandy, & Buchanan, 1985). In essence, it is likely that parents view only selected segments of their child's homework behavior. Parents are unlikely to include in their estimates of time that their children spend on homework those portions completed at school and at home before parents return from work. Parents might not even be able to accurately estimate the time that students spend on homework while both parties are home if the student does the assignments behind closed doors and alternates between homework and other activities.

We are making a case here that student reports of time on homework might be more veridical than parent reports. It is important to point out, then, that this greater veracity is based on the assumption that the *distribution* of student responses aligns better with the distribution of actual student behaviors, and not necessarily that students are not inflating their estimates. It can still be the case that students exaggerate when they report time on homework, a phenomenon that would be consistent with positive impression management. However, as long as this inflation is roughly equivalent across students (that is, students don't exchange places in the distribution), then the homework–achievement correlation can still be trusted. In essence then, our argument is that the correlation between reported and actual homework behavior is higher for student reports than parent reports. And perhaps most important, the studies that manipulated homework revealed a positive effect of homework on unit test scores. This finding is more in line with naturalistically measured student responses than parent responses. Still, it is clear that an important future direction for research would be to compare both student and parent reports about homework with behavioral observations.

Outcomes Other Than Achievement

Five studies that presented correlations between the amount of time students spent doing homework and student attitudes revealed a significant positive relationship using a fixed-error model. Two studies that looked at student conduct as an outcome produced inconsistent results. Thus, while the evidence base is small and nonexperimental in nature, it appears that the dramatic case in which large amounts of homework are cited as leading to poorer attitudes toward school and subject matter may not occur frequently enough to influence broader sample statistics.

Perhaps the most important conclusion from this synthesis regarding the effects of homework on outcomes other than achievement is that most have never been put to empirical test. While a few of the outcomes listed in Table 1 were found in the studies covered herein and some others can be found in research that examines homework from different perspectives (e.g., Hoover-Dempsey et al., 2001), the majority of these outcomes remain fertile ground for future research.

A Comparison With the Results of Cooper (1989)

Cooper (1989) reported an average effect size of $d = .21$ from studies that compared students who did homework with students who did no homework. This synthesis found a mean effect size of $d = .60$. These results suggest much larger effects in more recent studies. Looking for potential sources of the difference suggests that the research designs and achievement measures across the two syntheses might not be directly comparable. For example, the 16 studies listed in Cooper's (1989) Table 5.3 (p. 66) included four studies that used nonequivalent control groups without matching. All of the studies in the current synthesis used some form of matching. This might have improved their ability to detect a homework effect. Also, four of the studies in the earlier synthesis used standardized tests as the outcome measure. In the current synthesis, all of the studies used unit tests, a measure more closely aligned with the content of the homework assignments. Regardless, it seems clear that more recent studies that introduced homework as an exogenous intervention have revealed more impressive effects of homework.

Cooper (1989) reported a mean effect size of $d = .09$ for studies that compared homework with in-school supervised study. We found no study conducted since 1987 that carried out a similar type of comparison. Conversely, we uncovered numerous naturalistic studies that controlled for other variables confounded with the homework–achievement relationship and found these to reveal near-uniform positive results. Cooper (1989) did not include this type of research design in the earlier synthesis.

Finally, the Cooper (1989) synthesis reported a mean simple correlation of $r = .19$ between homework and achievement using a fixed-error model. We found the corresponding correlation to be $r = .24$. Thus these estimates appear very consistent.

Optimum Amounts of Homework

Cooper's (1989) meta-analysis found that for high school students the positive relation between time on homework and achievement did not appear until at least 1 hour of homework per week was reported. Then the linear relation continued to climb unabated to the highest measured interval (more than 2 hours per night). For junior high students the positive relation appeared for even small amounts of time on homework (less than 1 hour per night) but disappeared entirely after students reported doing between 1 and 2 hours each night. Only one study was available for Grades 1–6 but the lack of a simple linear relationship at these grade levels suggested the line would be flat.

We found only one study that permitted interpretation regarding optimum homework amounts. Lam (1996) found that for Caucasian American and Asian American high school students the strongest relationship between homework and achievement was found among students reporting doing 7 to 12 hours of homework per week, followed by students reporting doing 13–20 hours per week. This finding extends the conclusions from the earlier synthesis because it was not able to make a distinction in time spent on homework per night beyond 2 hours for high school students. Assuming that the causal direction of these findings is predominantly one in which more homework causes better achievement, the Lam (1996) finding suggests that the optimum benefits of homework for high school students might lie between 1½ and 2½ hours. Of course, there is still much guesswork in these estimates, and optimum amounts of homework likely will be dependent on many factors, including the nature of the assignment and student individual differences. Also, the Lam (1996) study was limited to 12th-grade Chinese Americans and Caucasian Americans. Still, this new piece of evidence does suggest, as common sense would dictate, that the positive effects of homework are not linear across all amounts. Even for these oldest students, too much homework may diminish its effectiveness, or even become counterproductive.

Limitations of Generalizability

Our analyses looking for publication bias and data censoring revealed little evidence to suggest that the strategies we used to locate studies for the synthesis were in some way a biased representation of all studies that might exist. That being said, it is also the case that certain clear limitations to the generalizability of the synthesis findings need to be noted.

First, as noted above, the positive causal effect of homework on achievement has been tested and found only on measures of an immediate outcome, the unit test. Therefore, it is not possible to make claims about homework's causal effects on longer-term measures of achievement, such as class grades and standardized tests, or other achievement-related outcomes. However, the studies using naturally occurring measures of time on homework found strong evidence of a link to longer-term achievement measures. We suspect that this distinction in the types of measures used in experimental and naturalistic studies of homework will persist. This is because the large-scale manipulation of homework across multiple subject areas and long durations within the same samples of students—the type of experiment likely needed to produce homework effects on grades and standardized tests—will require considerable resources and the cooperation of educators and parents willing to participate.

With regard to subject matter, both studies that introduced homework as an exogenous intervention and studies that used statistical controls suggest that homework will have positive effects on achievement involving both quantitative and verbal material. However, our database contained too few correlations involving other subjects, such as science and social studies, to include them in the meta-analysis. Therefore, while there is evidence that the effect of subject matter on the homework–achievement relationship is small, it should be viewed as suggestive rather than conclusive.

Finally, a perusal of Tables 3 through 8 suggests that few studies exist examining the effectiveness of homework in the early elementary school grades. This may be an especially important omission because of the apparent increase in the amount of homework being assigned to students in these grades (Hofferth & Sandberg, 2000). Also, nearly all the literature that we uncovered looked at the effect of homework on students who might be labeled "average," or examined broad samples of students but did not look for moderating effects of student characteristics.

Future Research

Throughout this discussion, we have pointed to fruitful avenues for future research. As is often the case, an assessment of what we know places in bold relief what we don't. Researchers are encouraged to find in our report any of the numerous areas where research is thin or nonexistent. These areas include studies that introduce homework as an exogenous intervention, randomly assign students or classrooms to conditions, and then analyze data at the same unit of analysis as the manipulation. There are several barriers to implementing such designs. First, of course, are the barriers to random assignment in applied settings (see Shadish, Cook, & Campbell, 2002, pp. 287–288), not the least of which would be the ethics of withholding from some students an intervention (homework) with presumed benefits. Second, if treatments are implemented at the classroom level and analyzed accordingly, the statistical power to detect effects will be quite low unless large-scale studies can be mounted that involve numerous classrooms. If students within classrooms are assigned to conditions, the researcher faces issues of treatment diffusion and/or demoralization and compensation effects that can contaminate conditions, because the intervention and control groups interact and know each other's experimental assignment.

Still, given the state of evidence, it seems there is much less to be gained from carrying out "homework studies as usual" than from new attempts to pinpoint estimates of causal relationships. That being said, we would encourage, as well, the use of mixed research models that incorporate qualitative analyses—to examine the homework process, moderators, and mediators of its effects, along with its intended *and* unintended consequences—in experimental designs. Such studies provide a rich tableau and complementary sources of knowledge for guiding yet another generation of research, policy, and practice. The long-term and cumulative effects of homework remain a largely unmapped terrain. Therefore, nonexperimental, longitudinal studies that follow cohorts of students and perform fine-grained analyses of developing homework behaviors would be a new and rich source of information.

In addition, the gaps in our knowledge suggest that future studies, whether experimental, qualitative, or longitudinal, should include variations in numerous potential factors in homework effects. Most important, we think these variations should include:

1. Students in multiple grades, especially the early elementary grades;
2. Students with other varying characteristics, especially varying ability levels, SES, and sex;
3. Variations in the subject matter of homework assignments, including subjects other than reading and math;
4. Measures of the non-achievement-related effects of homework that have been proposed in the literature; and
5. Variations in the amount of homework assigned, so that optimum amounts of homework can be examined.

We might envision all of these design variations being realized within a single research project, leading to multiple replications, but it is more likely that numerous small projects will gather data on one or a few areas. Thus we more realistically call for *programs of research* that begin by establishing general principles (some of which can be gleaned from this synthesis) and then systematically vary factors 1 through 5, above, not in the same study but through a series of interrelated studies (Shadish et al., 2002). The advantages of this approach are that studies can be implemented with good control of treatment, thus enhancing their power to detect effects. And, of course, individual studies will be less expensive to conduct. A disadvantage of this approach is that it limits the ability to examine interactions between factors. For example, if the grade level of the student is examined in one small study and sex of the student in another, it is impossible to examine whether the students' grade level and sex interact in their moderation of homework's effects.

Conclusion

We hope that this report has demonstrated the value of research synthesis for testing the plausibility of causal relationships even when less-than-optimal research designs and analyses are available in the literature. Most important, we hope that the findings provide the beginnings of an empirical foundation on which educators can base homework policies and practices and researchers can build the next generation of homework research.

Notes

This research was supported by a grant from the U.S. Department of Education, Office of Educational Research and Improvement (R305T030002), to the first author. The opinions expressed herein are those of the authors and not necessarily of the Department of Education. The authors wish to thank Heather Meggers-Wright and Jeffrey C. Valentine for their assistance with conducting the research synthesis, and Angela Clinton for assistance with manuscript preparation. Correspondence regarding this article should be sent to Harris Cooper (see contact information at end of article).

[1]We could find no study that looked at the students' SES as a moderator of the homework–achievement link. Only two studies examined the sex of the student as a moderator of the homework–achievement link. Among the studies that manipulated homework, Foyle (1984) presented results of an Analysis of Covariance that included the sex of the student in interaction with homework condition. The interaction was not significant, and the cross-break of means was not reported. Among the studies reporting simple correlations, Mau and Lynn (2000) reported six comparisons of male and female correlations between homework and achievement in Grades 10 and 12 for math, reading, and science. All six comparisons revealed significantly higher correlations for females than for males.

[2]Looking for missing correlations to the right (increasing the size of the positive effect) suggested more evidence that correlations higher than those in the retrieved reports might have been missing from the data set. The fixed-error model suggested that 11 correlations might be missing and that if they were imputed, fixed graph $r = .25$ (95% CI = .25/.26). The random model imputed no additional correlations. Also, the trim-and-fill analysis was conducted separately for studies that used class grades or standardized achievement tests as outcome variables. In all cases, the analysis suggested that the findings reported in this article were robust with regard to data censoring.

[3]The Cooper et al. (1998) correlation involving parents had to be dropped from this analysis because it included both elementary and secondary students.

[4]This type of subgroup analysis is a way of disentangling the effects of confounded moderating variables. It is an example of the type of analysis that would have been beneficial to carry out as well on the studies that employed exogenous introductions of homework. However, the limited number of such studies meant that some combinations of categories within moderators would have few or no studies in them.

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