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The Effect of Work-Related Programs on Dropout Rates: A Meta-Analysis

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LOYOLA UNIVERSITY CHICAGO

THE EFFECT OF WORK-RELATED PROGRAMS ON DROPOUT RATES:

A META-ANALYSIS

A THESIS SUBMITTED TO

THE FACULTY OF THE GRADUATE SCHOOL

IN CANDIDACY FOR THE DEGREE OF

MASTER OF ARTS

PROGRAM IN RESEARCH METHODOLOGY

BY

JILL Y. YOUNG

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ABSTRACT

One in four students drop out of school, which has long-lasting implications for the individual, employers, and society at large. Work-related programs, such as those that include career exploration or vocational training, are often employed by schools and communities to reduce school dropout rates. This thesis provides an update to a meta-analysis of experimental and quasi-experimental evaluations of dropout prevention programs conducted by Wilson, Tanner-Smith, Lipsey, Morrison, and Steinka-Frey in 2011, focusing on work-related dropout prevention programs. This study determined through meta-analysis of the logged odds-ratios that work-related programs have an odds-ratio of 1.66, indicating that work-related programs significantly reduce dropout rates. Two meta-regressions were conducted to determine which program or participant characteristics are associated with reduced dropout rates. There were no statistically significant differences in effect sizes for any of the variables, indicating that work-related programs are successful at reducing dropout rates across participant characteristics and program settings.

CHAPTER 1

INTRODUCTION

One in four students drop out of school, which has long-lasting negative implications for the individual, employers, and society at large (Bloom, 2009). Research has been conducted to investigate the effectiveness of programs that aim to reduce the school dropout, but until the rigorous meta-analysis conducted by Wilson, Tanner-Smith, Lipsey, Morrison, and Steinka-Fry in 2011, results had been mixed. The Wilson et al. review showed that students who participated in prevention and intervention programs were less likely to drop out of school and more likely to graduate from school. One common and successful program type was the work-related program, which focuses on giving students work experiences and skills. The research in this thesis provides an update to the Wilson et al. review (2011), which concluded in 2010, focusing on work-related programs to determine an updated effect size for these programs from 1980 to 2013. Additionally, meta-regression analyses were conducted to determine whether certain program factors - such as setting or dosage - and participant characteristics -such as gender or race - are associated with better outcomes.

Chapter 1 of this thesis discusses the background of the dropout issue, the relevance of work-related programs in addressing the issue, and current gaps in research. It offers theoretical frameworks that drove the purpose of the research, the problem statement, and the research questions. Limitations to the study are also identified.

Background

According to the National Dropout Prevention Center/Network, high school dropouts earn an average of \$9,245 a year less than those who graduate high school (n.d.). High school dropouts are also “disproportionately represented in prison populations, are more likely to become teen parents, and more frequently live in poverty” (Wilson et al., 2011, p. 11). Not only do these students suffer individual consequences; there are also larger economic and social consequences. Each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Balfanz, 2012, p. 5).

Dropout rates in the United States vary, not only by geographic location but also by demographic characteristics such as ethnicity, gender, and socioeconomic status. Boys are more likely to be dropouts than girls (8.5% vs. 6.3%), and racial/ethnic minority groups are also more likely to be dropouts (15.1% for Hispanics, 12.4% for American Indian/Alaskan Natives, and 8.0% for Blacks vs. 5.1% for Whites; Wilson et al., 2011, p. 11). Additionally, students from low-income households drop out of high school more often than those from higher income households (10% for low-income vs. 5.2% for middle income and 1.6% for high income students; National Dropout Prevention Center/Network, n.d.). The percentage of freshmen that did not graduate high school in four years ranges from 13.1% to 44.0% across all states, with an average dropout percentage of 26.8% (Cataldi, Laird, & Kewel Ramani, 2009).

According to the 2011 study by Wilson et al., there are a large number of intervention and prevention programs that address reducing dropout rates, and the

National Dropout Prevention Center/Network lists 333 model programs. However, few focus exclusively on dropout prevention but rather as one of many other program objectives, such as increasing attendance or reducing truancy. Wilson et al. (2011) conducted a large comprehensive meta-analysis to address the dearth of research on the effectiveness of dropout prevention and intervention programs, covering 187 studies from 1980 to 2010. Their research focused on intervention or prevention programs delivered in school settings or community-delivered interventions with an explicit dropout reduction focus for school-aged children and youth or recent school dropouts aged 22 and under. The primary outcomes of interest included school completion/dropout/graduation, and secondary outcomes of interest were centered on absences attendance, and academic achievement. Only randomized or quasi-experimental designs were included. The researchers coded for several different types of programs, including academic (e.g., tutoring, homework assistance), school structure (e.g., class/grade reorganization and small learning communities), family engagement (e.g., family outreach and parent or teacher consultation enhancement), college-focused (e.g., college preparatory curriculum and college campus visits), work related (e.g., internships and career exploration), linking to services (e.g., case management and health services), social relationships (e.g., mentoring and peer support, etc.), personal/affective (e.g., skills training and cognitive behavior therapy), behavioral (e.g., attendance monitoring and token economy), and other programs. While a focal program category was selected for each program in the review, most programs implemented multiple components and many included components from several categories.

Among the most common of these programs was work-related programs, which included internships, career exploration, vocational training, job placement assistance, employment, living allowance, and bonuses and sanctions applied to welfare grant. These programs seek to create more relevant educational experiences for a broad range of students. The Wilson et al. 2011 study uncovered 49 samples and 51 effect sizes for vocational programs (p. 27). The review generated a mean odds-ratio effect size for general dropout programs of 1.72 at 95% CI [1.56, 1.90]. This was a positive and statistically significant finding that indicated that youth who participated in dropout prevention programs dropped out at a lower rate and graduated at a higher rate than youth in control or comparison groups (Wilson et al., 2011). This translates to a 13% dropout rate among program participants. This rate is 8% lower than the average percentage of students who dropped out from the comparison groups (Wilson et al., 2011, p.32).

Additionally, the authors conducted three meta-regression models for general dropout programs as part of a moderator analysis. The first model focused on methodological characteristics of the study (e.g., dummy codes for random assignment and matched design), the second model included subject variables (percent male, percent white, and average age), and the third model focused on dosage, format, and implementation. Within these models, the authors investigated the effects of different program types on school dropout. All types of programs, with the exception of those classified as “other,” produced statistically significant mean odds-ratios, meaning that students in those programs had significantly lower dropout rates than students in the comparison group. The second highest mean odds-ratio was for vocational/employment programs at 2.64, which translates into a 9% dropout rate (Wilson et al., 2011, p. 37).

Vocational programs are one program type used to address high school dropout and the need for adolescents to be ready for post secondary or work-related experiences after high school. Students may become frustrated with their academic identity if they do not perform well in school, causing them to become disengaged and even drop out (Agodini and Deck, 2004, p. 3). Vocational training may reengage these youth because of the focus on workforce preparation, which may seem more interesting and practical to students who are not successful academically (Agodini and Deck, 2004, p. 3).

Rigorous evaluations of vocational programs are few in number, and research reviews of these programs provide mixed results. This updated research determines the overall effects of such programs as well as investigates possible program and participant characteristics that may be associated with better outcomes.

Theoretical Framework

There are several theoretical frameworks that have been applied by researchers in an attempt to understand dropouts and the approach of work-related programs to address the dropout issue. The focus of these frameworks varies greatly, from social backgrounds, such as social control theory; economics, such as economic strain theory; and delinquency. Because this research is an updated meta-analysis of work-related programs, only theoretical frameworks that capture how vocational programs address dropout prevention will be considered.

Rational Choice Theory

Rational choice theory argues that teens may choose to drop out of school because it seems like a reasonable alternative. One such reason is often work. Students may consider their current job opportunities and earnings and decide they are better off

without school, or they may need to help support themselves or their families sooner than high school graduation. Working becomes the more rational choice, and in this respect, students are “pulled” away from school (McNeary, 2011, p. 307). McNeary (2011) noted that while “adolescent employment may yield positive benefits for the student, it may also decrease educational attainment by ‘pulling’ students out of school” (p. 307). Two common pulling factors are the prospects of job employment and actual earnings. An adolescent who is already working may feel he or she has better job prospects if he or she is already a member of the workforce. Adolescents may find they enjoy the workforce more than school, especially if they are unsuccessful in traditional academic courses. McNeary (2011) also argues that “students are pulled out of high school when there is a greater demand for low skill, low pay labor” (p. 306). Vocational programs can address this pull by providing opportunities inside or outside of school for adolescents to explore careers, gain skills, make valuable connections to the workforce, and in some cases, earn pay for their work.

Social and Psychological Theories

The frustration-self esteem model, developed by Finn in 1989, contends that students with poor academic performance may develop low self-esteem about their place in school, causing them to become disengaged and drop out (DeLuca, Estacion, and Plank, 2008, p. 346). This theory relates closely to the participation-identification model. The participation-identification model is the inverse of the frustration-self esteem model: students with positive experiences will feel better about themselves, have a sense of belonging to their school, and will stay in school (DeLuca et al., 2008, p. 346). Work-related programs provide an opportunity for an adolescent to have an identity outside of

their academic performance, where they may have previously failed. Gaining new skills may give these youth a better sense of school belonging and a more positive sense of self.

Developmental Theory

Erikson (1968) considered adolescence to be primarily concerned with developing an identity. These adolescents are trying to determine who they are, who they want to be, and where they want to go. They also try to understand how they fit into the adult world (Hirsch, 2011, p.13). Hirsch (2011) described work-related programs as a prime operationalization of developmental theory because these programs “allow HS [high school] students the opportunity not only to explore a specific line of work, but probably even more importantly, to also acquire a taste of the culture of the world of work and gain an appreciation of core, generic features of work life” (p. 13).

Problem Statement

The Wilson et al. study (2011) was able to provide the overall effects of rigorously studied dropout prevention and prevention programs and also provided mean odds-ratios for various types of programs. One of the types of programs included was the work-related program. But these programs are broad in description, from what services they include, the settings in which they occur, and the participants for whom they are targeted, and research does not exist to clarify these issues. It is important to determine how effective these programs are, in what settings, and for whom they work best.

Additionally, there has been an increasing demand by politicians, school officials, funding agencies, employers, and youth advocates alike to prepare students to graduate high school with some work skills so that they are ready for life after high school. This research provides an update on the effectiveness of programs that prevent dropout

through work-related programs by including studies from 2010 to 2013 in addition to the original sample. It also investigates program and participant related characteristics and how they relate to dropout rates, which has not been previously studied.

Purpose

The objective of this systematic review is to summarize the available evidence on the effects of work-related prevention and intervention programs that are aimed at school-aged students for increasing school completion or reducing school dropout. The primary focus of the analysis is to update the effect size for work-related programs, and the secondary focus is to explore what program characteristics may be related to better outcomes in these types of programs. The research examines differences associated with the program structure itself, including treatment modality, implementation quality, and program location or setting. In addition, evidence of differential effects for students with various characteristics is explored, including age, gender, and race/ethnicity. Due to the large ethnic differences in graduation rates, it is particularly important to identify programs that may be more or less effective for disadvantaged students. The ultimate objective of this systematic review is to provide school administrators and policymakers with an integrative summary of research evidence that is useful for making decisions about programmatic efforts to reduce school dropout and increase school completion for all types of students through the use of work-related programs.

Research Questions

1. How effective are work-related programs at preventing school dropout?
2. Within work-related programs, what participant characteristics are associated with lower school dropout rates?

3. Within work-related programs, what program characteristics are associated with lower school dropout rates?

Limitations, Assumptions, and Design Controls

The Wilson et al. original study (2011) cited several limitations of the review. Due to concerns about publication bias, the researchers conducted a publication bias analysis. Results from the analysis indicated the possibility of small study bias in results for the general program results, indicating a small chance that the results from the original meta-analysis may have over-estimated the effects of general programs on dropout outcomes if the sample was missing studies with small sample sizes and null or negative results. Wilson et al. attempted to minimize the possibility of publication bias by conducting an extensive literature search, with particular emphasis on locating gray literature. In fact, their review includes many more studies than any previous review on the topic. However, many of the unpublished technical reports were large multi-site studies, and very few small sample size studies were included in the meta-analysis. The updated meta-analysis also places emphasis on gray literature and uses funnel plots to determine the possibility of publication bias.

It should also be acknowledged that the meta-analytic work of Wilson et al.'s (2011) synthesized effect sizes from studies used both experimental and quasi-experimental research designs, thus introducing a risk of bias associated with any lower quality quasi-experimental research studies. The researchers attempted to minimize bias by requiring non-randomized studies to include baseline pre-test or group equivalence information that could then be statistically adjusted for in the final meta-regression models used to estimate the comparative effectiveness of different program types. The

updated meta-analysis focusing on work-related programs maintains these stringent research design criteria in order to minimize methodological bias.

Chapter 1 Summary

Chapter 1 of this thesis briefly discussed the background of the dropout issue, the theoretical frameworks that will be used to approach the research, statement of the problem, purpose of the study, and limitations. Chapter 2 discusses relevant literature, including facts and figures related to dropout and graduation rates, work-related programs and how they work to prevent dropout, and previous research conducted to explain the effectiveness of these programs. Chapter 3 outlines the methodology and research design that will be used to investigate the research question, criteria for the study sample, methods of searching, and statistical techniques. Chapter 4 shares the results of the updated meta-analysis and meta-regression models. Chapter 5 serves as the final chapter, sharing findings, conclusions, implications, and recommendations for future research based on the results.

CHAPTER 2

LITERATURE REVIEW

Though the high school graduation rate has been steadily increasing over the last several years, the fact remains that one in four high school students drop out of high school, amounting to one million students a year who are out of high school without the necessary credentials, such as a diploma or GED, and the skills to enter the workforce (Bloom, 2009, p. 1). The National Dropout Prevention Center/Network reports that school dropouts in the United States earn an average of \$9,245 a year less than those who complete high school, have unemployment rates almost 13 percentage points higher than high school graduates, are disproportionately represented in prison populations, are more likely to become teen parents, and more frequently live in poverty (2009). Not only do these students suffer individual consequences, but also there are larger economic and social consequences. Each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Balfanz, 2012, p. 5).

There are prevention and intervention programs that focus either directly or indirectly on reducing dropout rate and increasing graduation rates as a way to address

the issue. Wilson et al. (2011) conducted an extensive and rigorous meta-analysis that examined the effects of dropout prevention and intervention programs on several outcomes, including dropout related outcomes such as drop rate and graduation rate, as well as absence and truancy related outcomes. Their analysis showed that these prevention programs were effective overall compared to control and comparison groups at reducing school dropout rates. The researchers also determined effects by program type; one of the most common types of programs included in the study was the work-related program. These programs may include career exploration, vocational training, internships, and the like.

This research largely builds on the Wilson et al. (2011) study, providing an update focused on work-related programs. The update includes a meta-regression of program and participant factors in work-related programs to investigate possible settings, treatment types, or population criteria that may lead to better outcomes in these programs.

Chapter 2 of this thesis discusses the impact of school dropouts, including the cost to the individual, society, and the job market. A brief discussion of relevant legislation is also included to highlight the national interest in reducing dropout rates and improving high school graduation rates. A description of work-related programs is provided, as well as the results of evaluations of these programs. Gaps in current research are discussed as well as variables of interest.

The Impact of Dropping Out

Individual and Economic Consequences

In Balfanz's *Building a Grad Nation*, he reveals that one in four youth drop out of high school, and the rate is even higher for minority teens (2012, p. 1). The overall

dropout rate amounts to more than one million students a year (Balfanz, 2012, p. 1).

Dropping out can have enormous impacts on the individual. High school dropouts are “disproportionately represented in prison populations, are more likely to become teen parents, and more frequently live in poverty” (Wilson et al., 2011, p. 11). According to the National Dropout Prevention Center/Network (n.d.), the death rate is 2.5 times higher for people with less than 12 years of education compared to the rate of those who have achieved 13 or more years of education. High school dropouts are 3.5 times more likely to be arrested in their lifetime; three-quarters of state prison inmates in the United States are high school dropouts, and 59% of inmates in federal prisons in the country are high school dropouts (National Dropout Prevention Center/Network, n.d.). National Dropout Prevention Center/Network also reports that high school dropouts earn an average of \$9,245 a year less than those who graduate high school (n.d.). Balfanz (2012) reports that over a lifetime, high school dropouts earn \$130,000 less compared to students who do not drop out (p. 5).

Not only do these students suffer individual consequences, but also there are larger economic and social consequences. Each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Balfanz, 2012, p.5). Dropouts from the Class of 2011 would have generated up to \$154 billion in additional earnings over their lives had they graduated from high school (Balfanz, 2012, p. 5). Balfanz (2012) offers additional information about the cost of a dropout:

- Moving just one student from dropout status to graduate status would yield more than \$200,000 in higher tax revenues and lower government expenditures over his or her lifetime (p.5).
- Graduating half of one class of dropouts would save the U.S. taxpayer \$45 billion in that year (p. 5).

These consequences may be exacerbated for schools or students with certain characteristics. Dropout rates in the United States vary by demographic characteristics such as ethnicity, gender, and socioeconomic status. According to Wilson et al. (2011), the percentage of first-year college students that did not graduate high school in four years ranges from 13.1% to 44% across all states, with an average dropout percentage of 26.8% (p. 11). Boys are more likely to be dropouts than girls (8.5% vs. 6.3%), and racial/ethnic minority groups are also more likely to be dropouts (15.1% for Hispanics, 12.4 for American Indian/Alaskan Natives, and 8.0% for Blacks vs. 5.1% for Whites; Wilson et al., 2011, p. 11). Additionally, students from low-income households also drop out of high school more often than those from higher income households (10% for low-income vs. 5.2% for middle income and 1.6% for high income students; National Dropout Prevention Center/Network, n.d.).

Consequences for the Workplace

There are also massive gaps in the job market. The United States will need 22 million students with a college degree in order to meet demands, but the nation is expected to fall short of this figure (Balfanz, 2012, p. 6). According to the National Dropout Center/Network, only 40% of adults who dropped out of high school are employed. This statistic improves by 20% with a high school degree and doubles among

those with a bachelor's degree (n.d.). The agency also reports that employment projections for jobs requiring a high school diploma will grow much more slowly than jobs that require a bachelor's degree, illustrating the necessity of having some post-secondary experience in today's workplace. In a survey of employers reported in "Are They Really Ready to Work" (2006), skills deemed important for employees included professionalism, work ethic, teamwork/collaboration, oral communications, ethics/social responsibility, and critical thinking/problem solving (p. 31). But the authors share that 42.3% of employers rated the overall preparation of high school graduates for their entry-level roles as deficient for such important skills (p. 31).

The U.S. Bureau of Labor Statistics projects total employment to increase by 20.5 million jobs from 2010 to 2020 (2012). Jobs that require a master's degree are expected to grow at the fastest rate during this time, while jobs requiring only a high school diploma are expected to grow the slowest (Bureau of Labor Statistics, 2012). The share of the labor force held by workers ages 16 to 24 is expected to decrease by 13.6% in this time, and jobs that require some postsecondary education are expected to grow at higher rates than those that require a high school diploma. Jobs requiring a master's degree are projected to grow by 22%, those that require a bachelor's and associate's degree are expected to grow by 17% and 18% respectively. Occupations requiring a doctoral or professional degree are expected to grow by 20%. Jobs that require a high school degree are expected to grow the slowest by 12% (Bureau of Labor Statistics, 2012).

National Priority

Addressing the dropout issue is a prominent concern for policymakers. President Obama has made it a national priority for students to graduate high school ready for

college or a career (Mac Iver, 2009, p. 1). A coalition of organizations met in 2010 and developed strategies to achieve the goal; these strategies are now called the Civic Marshal Plan (Balfanz, 2012, p. 19). The goals are to achieve a 90% high school graduation rate nationwide by the class of 2020 with all students ready for the college and the workplace, and to have the highest college attainment rates in world by 2020 (Balfanz, 2012, p. 20). To reach these goals, the proportion of college graduates will need to increase by 50%, meaning eight million more youth will need to earn associate's or bachelor's degrees by 2020 (The White House, 2011). Recommendations in Balfanz's report include a focus on college and career readiness as the goal of education, making a rigorous and comprehensive understanding of work-related programs of the utmost performance for school administrators and policymakers.

Work-related Programs

Description

Administrators and policymakers in education alike have tried multiple methods to address the dropout issue in high schools, "particularly those that serve students who are at risk of leaving high school without the skills needed to pursue further education and make successful transitions to the world of work" (Kemple, 2001, p. 4). According to the Office of Juvenile Justice and Delinquency Prevention, vocational or job training programs that focus on preventing high school dropout typically focus "attainment of basic skills competencies, opportunities for academic and occupational training, and exposure to the job market and employment" (Office of Juvenile Justice and Delinquency Prevention Model Programs Guide, n.d.). Vocational program activities can include tutoring or remedial education, GED preparation, vocational training, internships, job

shadowing, work experience, adult mentors, career exploration, career guidance and counseling. The intention of these programs is to prepare students for life after high school. These programs “address several risk factors, including academic failure, alienation and rebelliousness, association with delinquent and violent peers, and low commitment to school. At the same time, vocational training enhances protective factors by providing job skills, on-the-job experiences, and recognition for work performed” (Office of Juvenile Justice and Delinquency Prevention, n.d.).

Employers too are interested in preventing high school dropout because of the gap of skills in the workforce. Employers are demanding work-readiness skills from their entry-level employees, yet not all youth are positioned to be successful after they leave high school. Youth who do not have these skills are at increased risk of “unemployment, poverty, welfare dependency, substance abuse, criminal activity, and teenage childbearing. Finding effective approaches to assisting these youth in achieving economic self-sufficiency is critical to avoiding the personal losses resulting from such life events” (Schirm, 2006, p. 1). The job market is a tough place for young adults, regardless of whether they have a diploma. The unemployment rate for 16 to 24-year-olds in the summer of 2011 was double the overall employment rate at 18% and it was much higher for minority groups such as African Americans and Hispanics (Blemfield, 2012, p. 4).

Over 40% of employers reported the overall preparation of high school graduates is insufficient, citing the main areas of deficiency as written communications, professionalism, and critical thinking (Are They Really Ready to Work, 2006, p. 2). The report authors also discuss the existing in gap in literature that links social context,

adolescent employment, and educational attainment, arguing that adolescent employment and educational attainment are associated with a youth's future prospects; employment experiences vary by socioeconomic status, gender, and race; educational experiences vary by socioeconomic status, gender and race; and the linkages between educational and employment experiences vary by socioeconomic status, gender, and race (Are They Really Ready to Work, 2006, p. 306). Some studies report positive and compelling gains for students involved in work-related programs, such as better market prospects in post-secondary years (Kemple, 2004, p. 1).

Impact of Work-related Programs on Dropout

Overall, evaluations of the effectiveness of work-related programs on reducing dropout rates or increasing graduation rates have been mixed. Additionally, the number of evaluations that use rigorous designs and explore the settings or populations in which the intervention may be most appropriate are few in number, as reported by most reviews examining the effectiveness of dropout programs.

Job Corps is a frequently evaluated vocational program that attempts to impact high school completion. Job Corps is a free training program for low-risk youth over the age of 16 that focuses on teaching youth a career, helping them graduate, and keeping them employed. A 2001 of Job Corps evaluation revealed that a significantly higher number of program participants received a GED than the comparison group participants, showing positive effects. Another commonly evaluated program is the JOBSTART program, which is a vocational training program designed to improve the economic prospects of youth by increasing educational attainment and developing work skills through basic skills instruction, occupational skills training, training related support

services, and job placement assistance. The 1993 JOBSTART evaluation uncovered a significant impact of program participation. Specifically, program participants were 13% more likely than the control group to earn a GED or high school diploma, with the greatest effect on high-risk youth (Office of Juvenile Justice and Delinquency Prevention, n.d.). It has been evaluated several times since, and an evaluation as recently as 2008 found that JOBSTART had positive effects on completing school (What Works Clearinghouse, 2008, p. 1).

Career Academies are small learning communities housed in schools (school within a school) that are organized around a common theme or career. The programs generally provide information, build technical and academic skills, enhance performance and engagement in school, and enable participants to successfully transition from high school to postsecondary education or the workplace. A 2000 evaluation showed a significant difference in high school dropout rates for high-risk students (21% for the students in the program compared to 32% of non-participants), but only modest improvements for the full sample (65% of participants completed the credits to graduate compared to 59% of non-participants (Child Trends, n.d.). There were no significant differences in graduation rates for the treatment group versus the comparison group. Further evaluations have shown similar results.

Systematic reviews of programs focused on dropout or graduation rates are also mixed. For instance, the U.S. Department of Education's What Works Clearinghouse report on dropout prevention found only 15 qualifying studies that reported outcomes related to staying in school or completing school (n.d.). This report, however, focused solely on interventions in the United States and did not include a meta-analysis of

program effectiveness or an examination of the particular settings or populations for which interventions may be more successful.

In 2002, Public/Private Ventures conducted a review of outcome evaluations for vocational programs during the 1990s. The review concluded that most programs had only modest success at best, citing the need for more rigorous program evaluations. In a 2003 systematic review broadly focused on the effectiveness of intervention programs to reduce school dropout or increase school completion, Lehr et al. (2003) identified 17 experimental or quasi-experimental studies with related outcomes. While this review of current literature was more rigorous in its inclusion criteria than other reviews, the authors did not perform a meta-analysis to determine overall effectiveness.

In 2008, Child Trends reviewed 40 experimental evaluations of programs and interventions that take place in out-of-school time and that aim to produce educational outcomes. The review summarized which programs worked, which programs did not work, and which programs produced mixed results. The report noted that vocational programs did not consistently work for high school completion or reducing high school dropout, noting that there were three programs with positive effects, two with mixed reviews, and one program with no effects (What Works for Education, 2008).

Another review on best practices in dropout prevention summarized the results of 58 studies of dropout programs (ICF, International, 2008). This report provided effect sizes for individual program types but did not examine potential moderators or examine the influence of research design on effect size. The report also presented a narrative review of important variables associated with implementation quality, but did not include that as a variable in the meta-analysis.

In 2009, a review by Klima, Miller, and Nunlist identified 22 experimental or quasi-experimental studies examining outcomes related to dropout, academic achievement, and attendance. The review excluded programs for general at-risk populations, such as minority or low-income students, limiting the conclusions that could be drawn about the broader range of populations that programs could potentially impact. Another 2009 review of truancy and dropout programs conducted by the Washington State Institute for Public Policy included 22 programs. The authors noted that although career academies appeared to be an effective model, “further research on specific programs is necessary because the interventions contained within each program class are diverse, and it is possible that some programs are more effective than others” (What Works, 2009, p. 9).

Based on the design and lack of consistency in results of these reviews and meta-analyses on dropout rates, Wilson et al. (2011) decided to provide an updated meta-analysis of programs focusing wholly or in part on improving dropout or graduation rates. The researchers conducted a large comprehensive meta-analysis to address the dearth of research on the effectiveness of dropout prevention and intervention programs, covering 187 studies from 1980 to 2010. Their research focused on intervention or prevention programs delivered in school settings or community-delivered interventions with an explicit dropout reduction focus for school-aged children and youth or recent school dropouts aged 22 and under. The primary outcomes of interest included school completion/dropout/graduation, and secondary outcomes of interest were centered on absences attendance, and academic achievement. Only randomized or quasi-experimental designs were included. The researchers coded for several different types of

programs, including academic (e.g., tutoring and homework assistance), school structure (e.g., class/grade reorganization and small learning communities), family engagement (e.g., family outreach and parent or teacher consultation enhancement), college-focused (e.g., college preparatory curriculum and college campus visits), work-related (e.g., internships and career exploration), linking to services (e.g., case management and health services), social relationships (e.g., mentoring and peer support), personal/affective (e.g., skills training and cognitive behavior therapy), behavioral (e.g., attendance monitoring and token economy), and other programs. While a focal program category was selected for each program in the review, most programs implemented multiple components and many included components from several categories.

Among the most common of these programs was the work-related programs, which included internships, career exploration, vocational training, job placement assistance, employment, living allowance, and bonuses and sanctions applied to welfare grant. These programs seek to create more relevant educational experiences for a broad range of students. The Wilson et al. 2011 study uncovered 49 samples and 51 effect sizes for vocational programs (p. 27). The review generated a mean odds-ratio effect size for general dropout programs of 1.72 at 95% CI [1.56, 1.90]. This was a positive and statistically significant finding that indicated dropout prevention programs were effective at reducing school dropout rates. (Wilson et al., 2011). This translates into a dropout rate for treatment programs of 13%, which is 8% lower than the average percentage of students who dropped out from the comparison groups (Wilson et al., 2011, p.32).

Additionally, the authors conducted three meta-regression models for general dropout programs as part of a moderator analysis. The first model focused on

methodological characteristics of the study (e.g., dummy codes for random assignment and matched design), the second model included subject variables (percent male, percent white, and average age), and the third model focused on dosage, format, and implementation. Within these models, the authors investigated the effects of different program types on school dropout. All types of programs, with the exception of those classified as “other,” produced statistically significant mean odds-ratios, meaning that students in those programs had significantly lower dropout rates than students in the comparison group. The second highest mean odds-ratio was for vocational/employment programs at 2.64, which translates into a 9% dropout rate (Wilson et al., 2011, p. 37).

In summary, the results of evaluations of programs focused on reducing dropout rates are mixed. There are no current systematic reviews of how work-related programs impact dropout rates and school completion, though work-related programs are a common method for addressing the dropout issue. Though some positive effects have been documented for individual programs, these effects are usually small and for students most at-risk. Furthermore, many reviews of dropout prevention programs, both vocational and otherwise oriented, reveal that there are few rigorous evaluations of these programs available, making it difficult to report with any confidence the overall effectiveness of such programs. Finally, there is no information available in the literature to suggest what types of program settings or program participants may have better outcomes in work-related programs. This research builds on the review provided by Wilson et al. (2011), following the same rigorous criteria.

Importance of Current Research

As previously noted, this research provides the most recent review to date on the impact of work-related programs on dropout. In order for school administrators and policymakers to make informed decisions about programming to address the dropout and graduation rates and respond to the national agenda, they need to understand the overall effectiveness of work-related programs. Work-related programs are commonly used to curb the dropout rate, but results have been mixed on their effectiveness, and not much known about whom these programs impact most or the program characteristics that may lead to the best results. Building on the comprehensive study of dropout rates conducted by Wilson et al. (2011), only the most rigorously designed studies are included so that results can be reported with more confidence.

Variables of Interest

Based on the report by Wilson et al. (2011) as well as other studies and available statistics, there are many important variables of interest that may help explain work-related program effectiveness as it relates to staying in school and graduating. These characteristics are not only related to the program participants but also the settings and characteristics related to the program itself.

Participant characteristics. Research has shown discrepancies in dropout rates for students by gender, race, and socioeconomic status. Boys are more likely to be dropouts than girls (8.5% vs. 6.3%), and racial/ethnic minority groups are also more likely to be dropouts (15.1% for Hispanics, 12.4 for American Indian/Alaskan Natives, and 8.0% for Blacks vs. 5.1% for Whites; Wilson et al., 2011, p. 11). Additionally, students from low-income households also drop out of high school more often than those

from higher income households (10% for low-income vs. 5.2% for middle income and 1.6% for high income students; National Dropout Prevention Center/Network, n.d.). This updated review examines the effectiveness of work-related programs for specific program participants, including age, gender, and race.

Program characteristics. There are mixed results on whether the location of the school a youth attends may impact dropout. Moderator analysis includes an examination of program activities, such as GED preparation, internships, career exploration, vocational training, job placement assistance, and employment. It also examines effectiveness related to the treatment site, such as regular class time, special class, resource room, school facility (not during school hours), home, private office, public office, work site, park, private institution, public institution, school and home, or mixed. Since formats of programs can vary, from one on one to group to family, those formats were included. Other variables of interest include program duration, and dosage.

Chapter 2 Summary

Chapter 2 reflected on relevant literature that highlights the impact of dropping out of school, and the consequences for both the individual who drops out as well as the economic consequences for others. Dropping out has drastic consequences for the workforce as well, as employers cite lack of preparation for entry-level workers with no post-secondary experience. A description of work-related programs was offered, and a discussion of their impact confirms that there are mixed results on the impact of work-related programs on reducing dropout rates or improving graduation rates. The importance of the current research was discussed, as well as the participant and program

characteristics that may help shed light on the population and settings in which work-related programs may work best.

CHAPTER 3
RESEARCH DESIGN AND METHODOLOGY

This updated meta-analysis preserves the same methodology as the Wilson et al. study (2011).

Problem Statement

The Wilson et al. (2011) study was able to provide the overall effects of rigorously studied dropout prevention and prevention programs, even providing mean odds-ratios for various types of programs. One of the types of programs included was the work-related program. But these programs are broad in description, from what services they include, the setting in which they occur, and the participants for whom they are targeted, and research does not exist to clarify these issues. It is important to determine how effective these programs are, in what settings, and for whom they work best.

Additionally, there has been an increasing demand by politicians, school officials, funding agencies, employers, and youth advocates alike to prepare students to graduate high school with some work skills so that they are ready for life after high school. This research provides an update on the effectiveness of programs that prevent dropout through work-related programs by including all studies from 1980 to 2013. It also investigates program and participant-related characteristics and how they relate to dropout and graduation rates, which has not been previously studied.

Purpose

The objective of the proposed systematic review is to summarize the available evidence on the effects of work-related prevention and intervention programs that are aimed at school-aged students for reducing school dropout. The primary focus of the analysis is to update the effect size for work-related programs, and the secondary focus is to explore what program characteristics may be related to better outcomes in these types of programs. The research examines differences associated with the program structure itself, including treatment modality, implementation quality, and program location or setting. In addition, evidence of differential effects for students with various characteristics is explored, e.g., age, gender, and race/ethnicity. Because of large ethnic differences in graduation rates, it is particularly important to identify programs that may be more or less effective for disadvantaged students. The ultimate objective of this systematic review is to provide school administrators and policymakers with an integrative summary of research evidence that is useful for making decisions about programmatic efforts to reduce school dropout and increase school completion for all types of students through the use of work-related programs.

Research Questions

1. How effective are work-related programs at preventing school dropout?
2. Within work-related programs, what participant characteristics are associated with lower school dropout rates?
3. Within work-related programs, what program characteristics are associated with lower school dropout rates?

Population and Sample

Types of Interventions

Though the Wilson et al. research collected outcomes for several types of programs, this review focuses exclusively on work-related programs including internships, career exploration, vocational training, job placement assistance, and employment.

Only school-based or affiliated prevention or intervention programs are included. The definitions used in the Wilson et al. research were retained. School-based programs include programs that are administered under school authorities and delivered during school hours, while school affiliated programs are those that are delivered in collaboration with school authorities. School affiliated programs are typically offered by community-based organizations, and they make take place before or after school and on or away from the school campus. Community-based programs that specifically focus on dropout prevention are included, regardless of whether these programs collaborate with schools in order to deliver their intervention.

Types of Participants

Programs that serve school-aged youth are included. This includes youth expected to attend pre-kindergarten to 12th grade primary and secondary schools or the equivalent in countries with another grade structure, focusing on youth ages 4 through 18. The age of the population needed be presented in enough detail in each study to determine or infer that it meets the requirement. Youth between the ages of 18-21 who are recent dropouts are included for programs specifically aimed at high school completion.

General and at-risk population samples are included, including samples from inner city schools, students with low socioeconomic statuses, teen parents, students with poor attendance records, students who have low test scores or who are overage for their grade. Specialized populations, such as students with special needs or mental or physical disabilities, were not included. The rationale for this criterion is that the current research aims to focus on programs for mainstream students rather than a highly specialized population. However, such individuals may be part of the broader sample in which they are the minority were included.

Types of Research Designs

Studies needed to use an experimental or quasi-experimental design in order to be included. More specifically, studies needed to include the comparison of treatment and control conditions where students are either (1) randomly assigned; (2) non-randomly assigned but matched on pretests, risk factors, and/or relevant demographic characteristics; or (3) non-randomly assigned but statistical controls (e.g., covariate-adjusted means) or sufficient information to permit calculation of pre-treatment effect sizes on key risk variables or student characteristics is provided. Studies that compare two or more treatments without a control group are included if one group receives a treatment that is equivalent to a control condition, such as practice as usual. Studies that do not meet these design criteria are not included.

Types of Outcomes

To be included, a study must have assessed intervention effects on school dropout rates.

Date and Form of Publications

The original Wilson et al. (2011) study required the date of publication or reporting for eligible studies needed to be as of 1985 or later, though the research itself could have been conducted as early as 1980. Eligible studies could be published in any language and conducted in any country as long as they met all other eligibility criteria. Campbell Collaboration affiliates outside the United States assisted with the location of studies published in other countries and languages other than English. This update spans includes the original studies found in the Wilson et al. study, but is expanded from 2010 to 2013, and includes only the addition of English-language research.

Study Inclusion Decision-making

Study titles and abstracts were reviewed for the first part of the screening process. Full-text articles were then retrieved for seemingly relevant studies. The decision to include or exclude a study was made after reading full reports. Any questions or doubts about the inclusion of a study were discussed with the thesis chair.

Data Collection and Instrumentation

This section provides information on the search strategy for identification of eligible studies.

Resources Searched

Wilson et al. (2011) incorporated a very comprehensive and diverse strategy to identify the appropriate literature, including unpublished studies and international research, in order to reduce the possibility of omitting potentially relevant studies. This update of the research attempted to utilize many of those same resources. Electronic bibliographic databases searched included Dissertation Abstracts International, Education

Resources Information Center (ERIC), ISI Web of Knowledge (Social Science Citation Index, SSCI), PsycINFO, and Sociological Abstracts. Research registers to be searched include: the Cochrane Collaboration Library, the National Dropout Prevention Center/Network, and the National Research Register (NRR). Additional sources included National Research Center for Career and Technical Education, Association for Career and Technical Education, Academic Search Complete, Applied Social Science Index and Abstracts, Child and Adolescent Development Studies, CQ Researcher, Education Research Complete. Reference lists in previous meta-analyses and reviews, and citations in research reports screened for eligibility were also reviewed for potential relevance to the review.

Key Words

A comprehensive list of search terms and key words related to the population, intervention, research design, and outcomes was used to search the electronic bibliographic databases. These include the following terms, with synonyms and wildcards applied as appropriate:

School dropouts, school graduation, high school graduates, school completion, GED, high school diploma, dropout, alternative education, alternative high school, career academy, schools-within-schools, high school equivalency, school failure, high school reform, educational attainment, grade promotion, grade retention, school engagement, and graduation rate, internship, career, career exploration, vocation, vocational training, job placement, job placement assistance, employ, employment;

AND

intervention, program evaluation, random, prevent, pilot project, youth program, counseling, guidance program, summative evaluation, RCT, clinical trial, quasi-experiment, treatment outcome, program effectiveness, treatment effectiveness, evaluation, experiment, social program, effective.

The following search terms will be used to exclude irrelevant studies: higher education, post- secondary, undergraduate, doctoral, prison, and inmate.

Description of Methods Used in Primary Research

Eligible studies were required to use experimental or quasi-experimental research designs that compare a treatment and control or comparison group. The control or comparison group could include no treatment, observation only, treatment as usual, or wait-listed groups. While many of the eligible studies may include pretest measurements, the posttest measurements were used to compare the treatment and the control groups in this review. For studies that include measured outcomes at multiple follow-up points, the first follow-up point that occurred after program completion was used.

Criteria for Determination of Independent Findings

Multiple reports were available for the same study. Some reports may provide multiple codable effect sizes, and in those cases, all effect sizes for a study were included. As Wilson et al. (2011) noted in their original protocol, this circumstance creates statistical dependencies that violate the assumptions of standard meta-analysis methods. However, the studies that included multiple effect sizes did so because of multiple sites within the report. For these studies, multiple effect sizes within studies were included only if the effect sizes were derived from independent samples.

Details of Study Coding Categories

Eligible studies were coded on variables related to study methods, the nature of the intervention and its implementation, the characteristics of the subject samples, the outcome variables and statistical findings, and contextual features such as setting, year of publication, and geographic setting. A detailed coding manual is included in Appendix A. The researcher coded and directly entered all data in three different SPSS databases: an effect-size level dataset; a group dataset that describes the treatment and control groups; and a dataset with heading information, which includes study-level information. Effect sizes were calculated using Excel and SPSS.

Data Analysis

Analysis was conducted using Excel, SPSS, and the specialized meta-analysis macros available for that program (Lipsey & Wilson, 2001).

Effect Size Metrics

The author used odds-ratios as the effect size metric for dropout and other binary outcomes. All effect sizes were coded such that larger effect sizes represent positive outcomes.

All computations with odds-ratios were carried out with the natural logarithm of the odds-ratios, defined as follows:

$$\ln(OR)=\ln\left(\frac{A}{B} * \frac{D}{C}\right)$$

where A and B are the respective counts of “successes” and “failures” in the treatment group, and C and D are the corresponding counts of “successes” and “failures” in the

comparison group. The sampling variance of the logarithm of an odds-ratio can be represented as:

$$Var_{\ln(OR)} = \frac{1}{A} + \frac{1}{B} + \frac{1}{C} + \frac{1}{D}$$

Analytic results from the logged odds-ratios effect sizes were converted back to the original odds-ratio metric for interpretation.

Missing Data

The author made all reasonable attempts to collect complete data on items listed in the coding manual. Authors of the reports were contacted if key variables of interest could not be extracted from study reports.

Outliers

The author examined the effect size distributions using Tukey's (1977) inner fence to identify any outliers. Any outliers identified were recoded to the inner fence value to ensure these outliers did not disproportionately influence the results. The author also examined the distribution of sample sizes to ensure that there are no excessively large weights. For odds-ratio effect sizes, this examination of outliers was performed by reviewing the distribution of weights, rather than sample sizes.

Analytic Techniques

All analyses with effect sizes were inverse variance weighted using random effects statistical models. Specifically, the weighting function was:

$$w_i = \frac{1}{Var_i + \tau^2}$$

where w_i is the weight for effect size i , Var_i is the sampling variance for effect size i as defined above for the respective effect size metric, and τ^2 is the random effects variance

component estimated for each analysis with a method of moments or maximum likelihood estimator. The unit of assignment to treatment and comparison groups was coded for all studies, and appropriate adjustments were to effect sizes to correct for variation associated with cluster-level assignment (Hedges, 2007).

Narrative and descriptive statistics of the study-level characteristics, methodological characteristics, group and participant level characteristics, as well as outcome characteristics were used to describe the included research studies. Main effects analysis was conducted to provide an updated effect size for work-related programs. Meta-regression analyses were conducted on program level and participant level characteristics using random effects models. Random effects weighted mean effect sizes were calculated for all studies using 95% confidence intervals. Estimates of Cochrane's Q and τ^2 were used to assess variability in the effect sizes.

The main objective of the analyses was to describe the direction and magnitude of the effects of work-related programs on high school dropout. Additionally, moderator analysis using random effects meta-regression models attempted to identify the characteristics of the programs and participants that are associated with larger and smaller effects. Based on prior theory and research, the following moderators were examined for their influence on effect sizes:

- Treatment modality
- Implementation
- Treatment duration/program dosage
- Program location/setting

- Race/ethnicity of sample
- Gender of sample
- Age level of sample

Examination of funnel plots was used to assess the possibility of publication bias and its impact on the findings of the review.

Chapter 3 Summary

Chapter 3 reviewed the research questions for this thesis as well as the methodology. The study is a focus on work-related school-based or affiliated programs or community programs explicitly aimed at reducing dropout rates. School-aged youth are the population of interest, and at-risk youth are eligible, while special populations such as teen moms or special needs youth samples are not. Only studies that evaluate the programs using random or matched to control for larger effects found in less methodologically rigorous evaluations. The outcome of interest was dropout rate. This study is an update of the Wilson et al. study (2011), which included studies published between 1980 and 2010 for most databases, so this thesis's focus is on eligible studies after 2010. Data collection methods were discussed, including researches to be searched and key words. Finally, the data analysis section outlined effect size metrics, missing data, outliers, and analytic techniques used in this thesis.

CHAPTER 4

RESULTS

The objective of the proposed systematic review is to summarize the available evidence on the effects of work-related prevention and intervention programs that are aimed at school-aged students for increasing school completion or reducing school dropout. The primary focus of the analysis is to update the effect size for work-related programs, and the secondary focus is to explore what program characteristics may be related to better outcomes in these types of programs. The research examines differences associated with the program structure itself, including treatment modality, implementation quality, and program location or setting. In addition, evidence of differential effects for students with various characteristics will be explored, e.g., age, gender, and race/ethnicity. Because of large ethnic and socioeconomic differences in graduation rates, it was particularly important to identify programs that may be more or less effective for disadvantaged students. The ultimate objective of this systematic review is to provide school administrators and policymakers with an integrative summary of research evidence that is useful for making decisions about programmatic efforts to reduce school dropout and increase school completion for all types of students through the use of work-related programs.

This chapter first discusses the process used in collecting and screening additional studies included in the meta-analysis, including the search engines used and the number of studies screened. The search results section is followed by a description of the studies and samples included in the meta-analysis. The research questions are then addressed by reviewing the results of the mean effect size and three meta-regression models.

Search Process and Results

Information Sources

Wilson et al. (2011) incorporated a comprehensive and diverse strategy to identify the appropriate literature, including unpublished studies and international research, in order to reduce the possibility of omitting potentially relevant studies. This research attempted to utilize many of those same resources. Electronic bibliographic databases searched included Dissertation Abstracts International, Education Resources Information Center (ERIC), ISI Web of Knowledge (Social Science Citation Index, SSCI), PsycINFO, and Sociological Abstracts. Research registers to be searched include: the Cochrane Collaboration Library, the National Dropout Prevention Center/Network, and the National Research Register (NRR). Additional sources included National Research Center for Career and Technical Education, Association for Career and Technical Education, Academic Search Complete, Applied Social Science Index and Abstracts, Child and Adolescent Development Studies, CQ Researcher, Education Research Complete. Several databases searched by Wilson et al. were not easily accessible, and therefore were excluded from this updated study. Those databases included Education Abstracts, National Technical Information Service (NTIS), System for Information on

Grey Literature (OpenSIGLE), Australian Education Index, British Education Index, CBCA Education (Canada), and Canadian Research Index.

All searches included studies dated after 2010, and studies were accessed between March 2013 and July 2013. This time period was chosen because the meta-analysis completed by Wilson et al. (2011) searched the years 1985 through 2010.

Search Terms and Criteria

Key words for literature searches varied by database, depending on the capabilities of each database. Larger databases accommodated more complex searches.

The main search terms included:

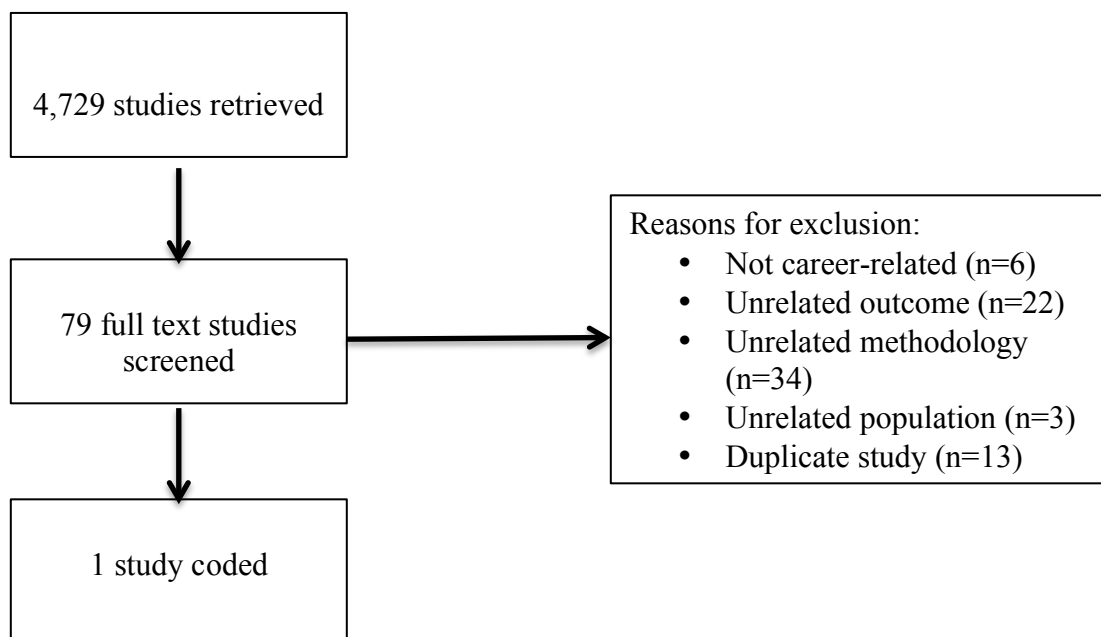
school AND (career OR vocational) AND (graduate OR dropout) AND (intervention OR program evaluation) NOT (undergraduate OR prison).

Data Collection Process

Only studies in written in English were collected. Some databases retrieved well over 10,000 results but would only provide the first 4,000 results, making it necessary to then filter the results by subcategories within the database. For example, the Dissertation Abstracts International database search yielded over 15,000 results, so after the first 4,000 results were reviewed, the results were filtered for the following subcategories: secondary education, elementary education, curriculum development, middle school education, educational evaluation, and social research. The same was true for Applied Social Science Index and Abstracts, which returned over 17,000 results. The subcategory filters included education, multicultural education, school counseling, social research, educational evaluation, educational tests and measurements, early childhood education, educational policy, social psychology, curriculum development, teacher education,

elementary education, secondary education, educational psychology, school administration, and educational leadership. This greatly reduced the results to 3,942 studies. Overall, 4,279 studies were retrieved from the various databases, and from those 79 studies were selected based on the relevance of the titles and abstracts, with most of the studies being eliminated due to not being work-related. Of those studies, 79 were deemed eligible and the full-text articles were reviewed for their eligibility. Only one study met all of the eligibility criteria in order to be included in this updated meta-analysis. The other 78 studies were excluded because they were not work-related programs, the outcomes were not related to dropout, the methodology was not appropriate, the population was ineligible, or the study was a duplicate. Many of the studies failed to meet multiple eligibility requirements for inclusion. Below represents a table that describes the data collection process.

Figure 1. Data Collection Process



Coding and Protocol

The additional study was coded directly into three separate SPSS databases that were provided by Wilson et al. Since this meta-analysis is an update of the work by Wilson et al. (2011), their coding protocol was used to code the additional study. Coded data included several levels, including study-level, group-level, and effect-size level. The first file was dedicated to study level information, including the following: year of publication, country in which the study was conducted, type of publication, unit of group assignment, method of group assignment, confidence in assignment ratings, and data related to group equivalence comparisons.

The second file captured group equivalence effect size coding, including type of effect size, wave number, variables on which groups were compared, which group was favored, and significance of group comparison. It also included pre-test and post-test effect size information, such as type of effect size, how effect size was derived, whether adjusted data were used, confidence in effect size calculation, assigned and observed N for treatment and control groups, and other effect size data fields, including means by treatment/comparison group, difference in group means, standard deviation for both groups, pooled standard deviation, N successful for both groups, Ns failed for both groups, dependent and independent t-values, Chi-squares, and effect sizes or odds-ratio reported by authors. Finally, the type of outcome variable was also coded.

The third and final file contained treatment and control group coding. Each group was coded for the following: type of treatment received (including placebo, attention, or straw man groups for comparison or control groups), program name and description, intervention type (school-based, school-affiliated, community-based), and program

components. Several program components were coded, including academic, school structure, family engagement, college focused/connecting students to attainable future, work-related/financial support, linking to services, social relationships, personal/affective, behavioral, and other. These components had several subcategories that were coded separately. Programs were coded for every subcategory component they offered, but were also assigned a focal component. Additional data coded included treatment site, role of the evaluator/researcher, role of the program developer, whether the research was a routine practice or program vs. research project, whether personnel received training, treatment format, duration of treatment, frequency of contact, mean hours of contact time (by week and over the duration of the treatment), implementation issues, and subject characteristics. Subject characteristics included gender composition, ethnicity, age, grade level, and predominant level of risk of youths in the sample.

Effect Size Indices

The original meta-analysis conducted by Wilson et al. (2011) collected several effect sizes for group equivalence testing, pre-tests, and post-tests across several subgroups, waves, and outcomes. This study focused exclusively on odds-ratios post-test effect sizes from the first wave, and did not include any subgroup effect sizes, focusing on overall group effect sizes for the treatment and comparison groups. The outcome construct included was dropout rates.

Programs were required to contain a work-related program focus, including internships, career exploration, vocational training, job placement assistance, or employment. A program containing any of these elements was included in the study, regardless of whether that element was the primary focal point for the program.

Differences Between Current Study and the Wilson et al. Study

While the current research discussed in this thesis is based on the Wilson et al. (2011) study in that it retains much of the criteria and uses data collected from the study, there are some differences between the two studies. These are outlined in Table 1.

Table 1. Differences Between Wilson et al. Study and Current Study

Difference	Wilson et al. Study	Current Study
Outcomes	Dropout, graduation, graduated/GED	Dropout only
Unique Sources	Education Abstracts, National Technical Information Service (NTIS), System for Information on Grey Literature (OpenSIGLE), Australian Education Index, British Education Index, CBCA Education (Canada), and Canadian Research Index	National Research Center for Career and Technical Education, Association for Career and Technical Education, Academic Search Complete, Applied Social Science Index and Abstracts, Child and Adolescent Development Studies, CQ Researcher, Education Research Complete
Work-related Programs	Defined by the focal point of the program; each program had one primary focus	Defined by whether the program included any work-related components, regardless of program focal point

Study and Sample Characteristics

A total of 45 studies were included for final analysis in this research. Forty-four of the studies came from the original the study by Wilson et al. (2011). This research added one new study. Descriptive statistics were analyzed to gain a better understanding of the studies and treatment groups included in this research.

General

The average year of publication was 1992, with studies from 1983 through 2010. The studies came from various types of publications. The majority of studies were technical reports (64.4%) and theses or dissertations (26.7%), followed by journal articles (6.7%), and books (2.2%). Nearly all studies took place in the United States (93.3%), while a handful was conducted in Britain. Table 2 provides more information.

Table 2. General Characteristics

	Min	Max
Publication year	1983	2010
Type of publication	# of studies	%
Book	1	2.2
Journal article	3	6.7
Thesis or dissertation	12	26.7
Technical report	29	64.4
Country	# of studies	%
USA	42	93.3
Britain	3	6.7

Methods

The unit of assignment for the studies was primarily at the individual level (91.1%), with only one study that assigned at the group level and three that assigned participants at the program level. The actual method of assignment varied across the studies. About a third (28.9%) were non-random and not matched, 20.0% were random with no matching, and 20.0% were random with matching. Other methods included wait list control (4.4%), matched on pretest and personal characteristics (13.3%), and matched

only on demographics (13.3%). Table 3 provides more information on the methodology of the studies included in this research.

Table 3. Methodological Characteristics

Unit of assignment	# of studies	%
Individual	41	91.1
Group	1	2.2
Program areas	3	6.7
Method of assignment	# of studies	%
Random with matching	9	20.0
Random, no matching	9	20.0
Wait list control	2	4.4
Matched on pretest and personal characteristics	6	13.3
Matched only on demographics	6	13.3
Non-random, not matched	13	28.9

Participants

On average, 55% of the participants are male across the studies. The average percent of youth by race was 28.9% for white youth, 35.7% for black youth, 23.2% for Hispanic youth, and 14.4% for other minorities. The average age of participants in the treatment programs was 15.3. Grades ranged from fifth grade to twelve grade, with a majority of youth in the tenth grade.

Table 4. Participant Characteristics

Gender	Mean	SD
Percent male	55.1	13.9
Race/ethnicity	Mean	SD
Percent white	28.9	27.7
Percent black	35.7	31.3
Percent Hispanic	23.2	26.3
Percent other minority	14.4	25.7
Age	Mean	SD
Average age	15.3	1.8

Program

This meta-analysis focused on dropout prevention programs that were either a) school-based or affiliated or b) community-based programs that were explicitly aimed at reducing school dropout. The majority of the programs were school-based or affiliated, with only 6.3% of programs based at a community organization. Work-related program components included internships (11.7%), vocational training (66.7%), career exploration (34.2%), job placement assistance (29.7%), and employment (34.2%). All programs but one offered multiple program components, including components that were not necessarily related to career exploration or work. Treatment sites included regular class time (12.6%), special class time (55.0%), resource room or counselor room (4.5%), school facility outside of school time (4.5%), private office (6.3%), public office (0.9%), work site (3.6%), or mixed (10.8%). Treatment groups varied in terms of how often they received treatment, with three-quarters of the programs involved daily contact with the youth (77.5%).

Table 5. Program Characteristics

Intervention type	# of effect sizes	%
School-based	99	89.2
School-affiliated	5	4.5
Community-based	7	6.3
Work-related components (may include more than one)	# of effect sizes	%
Internships	13	11.7
Career exploration	38	34.2
Vocational training	74	66.7
Job placement assistance	33	29.7
Employment	38	34.2
Treatment site	k	%
Regular Class Time	14	12.6
Special Class	61	55.0
Resource Room, School Counselor Office	5	4.5
School facility, not during school hours	5	4.5
Private office, clinic, center	7	6.3
Public office, clinic, center	1	0.9
Work site	4	3.6
Other mixed	12	10.8
Cannot tell	2	1.8
Frequency of treatment	# of effect sizes	%
Once a week	6	5.4
2 times a week	3	2.7
3-4 times a week	7	6.3
Daily contact	86	77.5
Cannot tell	9	8.1
Dosage	Min	Max
Treatment duration (days)	6	312
Treatment duration (hours)	37	6815

Research Questions

The questions that this research intended to answer included the following:

1. How effective are work-related programs at preventing school dropout?

2. Within work-related programs, what participant characteristics are associated with lower school dropout rates?
3. Within work-related programs, what program characteristics are associated with lower school dropout rates?

Research question number one was answered through a mean effect size calculation and the analysis of several related statistics. Research questions two and three are both answered by separate meta-regressions, one focusing on program characteristics and the other on participant characteristics.

Mean Effect Size

The first research question examined was: how effective are work-related programs at preventing school dropout? In order to answer this question, a mean effect size was calculated across the studies. There were 111 effect sizes included in 45 studies.

Synthesis Methods

All analyses were conducted on the logged odds-ratio effect sizes. All effect sizes were inverse variance weighted using random effects statistical models. Random effects models assume that “each observed effect size differs from the population mean by subject-level sampling error” as well as “a second component associated with the random effects variance” (Lipsey and Wilson, 2001, p. 119). Weighted mean effect sizes were calculated for all studies using 95% confidence intervals, and estimates of Cochrane’s Q and Tau squared were used to assess heterogeneity in the effect sizes.

Outliers

The distribution of the logged odds-ratio effect sizes was examined for outliers using interquartile ranges. Outliers were defined as values that fell below the 25th or

above the 75th percentile of the distribution (Tukey, 1977). Eight values were identified and then Winsorized to the next closest values.

Missing Data

Missing data was an issue for the second and third meta-regression models that included participant and program data. Lipsey and Wilson (2001) offer recommendations for handling variables with missing data that are to be used in a meta-regression, and suggest that any method is reasonable provided that a negligible proportion of cases are missing (p. 127). Unfortunately, that was not the case for every variable, as 133 cases are missing data for at least one variable. However, for the purpose of this research, missing data were deleted list-wise in moderator analysis. Below is a table that shows the variables with missing values.

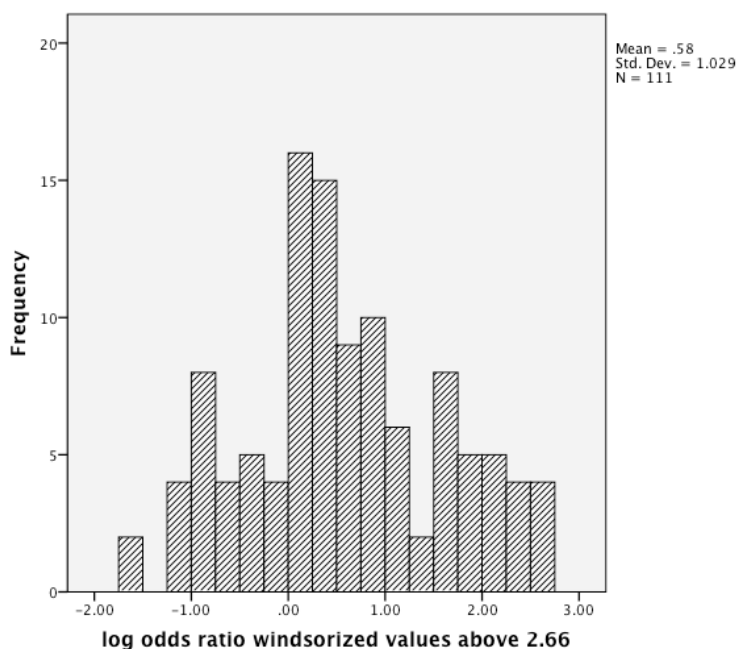
Table 6. Missing Data

Variable	N missing	N	%
Percent male	15	198	7.6%
Percent white	26	198	13.1%
Average age	104	198	52.5%
Hours per week	1	198	0.5%
Duration in days	16	198	8.1%

Results

The mean effect size was calculated twice; once with all effect sizes and once with Winsorized data, where the effect sizes deemed outliers by Tukey's test were recoded. Figure 2 provides more information.

Figure 2. Distribution of Effect Sizes



The mean odds-ratio for the work-related dropout programs was 1.66 and a 95% confidence interval of 1.49, 1.83. The odds-ratio of 1.66 is a positive and statistically significant result, indicating that participants in work-related programs exhibit lower dropout rates than those in control or comparison groups. There are “rules of thumb” used in interpreting the magnitude of effect sizes for odds-ratio (Lipsey and Wilson, 1999; Ferguson, 2009; Nandy, 2002). Though the rules of thumb differ slightly, an odds-ratio of 1.66 is considered small in social science research. The mean effect size including outliers was about the same as the mean effect size that included Windsorized data, and the interpretation of the mean effect size remained the same. Table 7 provides the relevant figures for the mean effect sizes.

Table 7. Mean Odds-Ratio Effect Size

	Mean OR	95% CI	# Effect sizes	# Studies	Q_E	T^2
All effect sizes	1.66	1.49, 1.83	111	45	482	0.34

It is also important to examine whether the distribution of the effect sizes are homogeneous. As Lipsey and Wilson (2001) note, “in a homogeneous distribution, the dispersion of the effect sizes around their mean is no greater than that expected from sampling error alone” (p. 115). The $Q_E=482$ ($df=110$, $p < 0.99$), indicating homogeneity in the distribution of the odds-ratios, meaning that the study samples did not demonstrate more variability than could be explained from sampling area alone.

The Wilson et al. (2011) study conducted moderator analysis on methodological characteristics after receiving a significant Q_E value. The researchers’ moderator analysis revealed that “random assignment and matched group designs produced smaller effect sizes than the non-random, non-matched designs, with random assignment effect sizes being statistically significantly smaller than the reference group” (Wilson et al., 2011, p. 33). Though the Q_E was not significant for the updated effect size of work-related programs, and therefore did not indicate heterogeneity in the distribution of the odds-ratios, a meta-regression was conducted to better understand true program effects, keeping in mind study method and the role of the evaluator, as was done to in the study by Wilson et al. (2011). Characteristics included random assignment (1=yes), matched group design (with the reference group being wait list control and non-random, non-matched; 1=yes), and whether the evaluator was independent from the program (1=yes).

Table 8 shows the results of the meta-regression of methodological characteristics. None of the variables included in the meta-regression were statistically significant. There were no discernible differences across studies related to study design; random assignment and matched group design did not impact effect sizes. Studies with independent evaluators who were not program providers were not associated with effect sizes either.

Table 8. Model 1: Meta-regression Results Predicting Logged Odds-Ratio Effect Sizes

Methodological characteristics	B	se	95% CI
Random assignment	-0.22	0.23	-0.67, 0.24
Matched group design	0.09	0.22	-0.33, 0.52
Independent evaluator	-0.31	0.26	-0.81, 0.20

Participant Characteristics

The second research question asks: within work-related programs, what participant characteristics are associated with lower school dropout rates? As noted in the Wilson et al. study, “participant characteristics are important from a policy perspective, inasmuch as dropout programs may be more or less effective for students with different characteristics” (2011, p. 33). Following the Wilson et al. (2011) study, the second meta-regression builds on the meta-regression used to examine study methodological impacts on effect size, but adds participant characteristics of youth who participated in work-related programs to examine whether these characteristics are associated with larger effect sizes. Participant characteristics include percent male (percent female as reference group), percent white, and average age. Table 8 below provides the results.

Table 9. Model 2: Meta-regression Results Predicting Logged Odds-Ratio Effect Sizes

Methodological characteristics	B	se	95% CI
Random assignment	0.00	0.50	-0.97, 0.98
Matched group design	-0.24	0.63	-1.47, 0.99
Independent evaluator	-0.49	0.49	-1.44, 0.47

Participant characteristics	B	se	95% CI
Percent male	-2.25	1.42	-5.03, 0.54
Percent white	1.21	0.89	-0.54, 2.96
Average age	-0.06	0.10	-0.24, 0.13

The revised model including participant characteristics did not produce any statistically significant results. Again, there were no discernable differences across studies related to study design. Studies with independent evaluators who were not program providers were not associated with effect sizes either. As in the Wilson et al. (2011) study, there were insignificant differences in gender, race, and age. Boys did not benefit more or less than girls, and white students did not benefit more or less than minority students. There was no difference in treatment effects associated with average age.

Program Characteristics

The third research question asks: within work-related programs, what program characteristics are associated with lower school dropout rates? The Wilson et al. study notes that these particular variables are important because they may be more alterable (2011, p. 33). This analysis continues to build on the previous meta-regression by including a random effects meta-regression to explore whether certain programmatic aspects of work-related programs are associated with larger effect sizes. Program characteristics included in the meta-regression are implementation quality (with a higher

value indicating no discernable implementation problems); dummy codes related to program setting, including classroom setting (1=yes), school setting but not classroom (1=yes), and mixed program sites (1=yes), with programs delivered in community settings as the reference group; program dosage, as indicated by duration in weeks, and hours of service per week; and dummy variables for program components, including internships (1=yes), career exploration (1=yes), vocational training (1=yes), job placement (1=yes), and employment (1=yes), with job assistance programs as the reference group. Table 10 below provides the results.

Table 10. Meta-regression Results Predicting Logged Odds-Ratio Effect Sizes

Methodological characteristics	B	se	95% CI
Random assignment	0.60	1.19	-1.73, 2.93
Matched group design	-0.38	1.84	-3.99, 3.23
Independent evaluator	-0.42	1.52	-3.40, 2.57
Participant characteristics	B	se	95% CI
Percent male	-1.71	2.33	-6.27, 2.86
Percent white	1.08	1.77	-2.40, 4.56
Average age	-0.23	0.17	-0.57, 0.12
Program characteristics	B	se	95% CI
Internship	-0.83	2.47	-5.67, 4.01
Career exploration	0.17	1.36	-2.49, 2.83
Vocational training	0.55	0.78	-0.98, 2.08
Job placement	0.77	0.75	-0.71, 2.25
Employment	-0.35	1.11	-2.52, 1.82
School based or affiliated	1.80	2.38	-2.87, 6.47
Classroom based	-1.82	2.09	-5.91, 2.28
Not in a classroom	-1.26	3.06	-7.27, 4.74
Mixed settings	-0.33	3.09	-6.39, 5.74
Implementation quality	0.13	0.67	-1.18, 1.44
Hours per week	0.01	0.02	-0.04, 0.05
Duration in days	-0.01	0.02	-0.04, 0.03

The results from model three did not substantially differ from models one and two in relation to the interpretation of the methodological and participant variables. In the Wilson et al. study (2011), classroom-based programs were significantly more effective than programs in the reference group, while school-based programs were neither more nor less effective than the reference group. Longer programs were not associated with better or worse outcomes, and programs with implementation issues were associated with small effects.

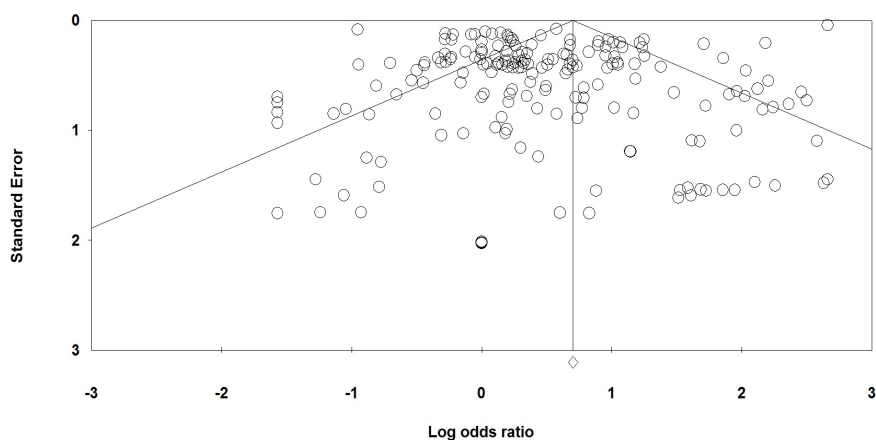
As the results from Table 10 indicate, none of the program related variables in model three of this study were statistically significant. School-based or affiliated programs produced slightly larger effect sizes than the reference group, while classroom-based programs produced slightly smaller effect sizes than the reference group, but neither association was statistically significant. Program dosage was not associated with better or worse outcomes, similar to the Wilson et al. study (2011). There were no statistical differences in treatment effects across program components. Implementation quality was not associated with smaller or larger effect sizes.

Publication and Small Study Bias

Because journal articles accounted for such a small percentage of the studies included in the meta-analysis (6.7%), there was little concern about publication bias occurring in this study. Lipsey and Wilson (2001) discuss using funnel plots to detect publication bias or potential bias due to underrepresentation of studies with small samples, as publication bias can filter out small effect sizes (p. 142). According to them, the funnel plot should follow its namesake and take the shape of a funnel (Lipsey and Wilson, 2001, p.143).

A funnel plot was created using the Comprehensive Meta-Analysis software to examine whether there were differences in the size of effect sizes based on the type of publication in which the study was shared. Figure 3 is the funnel plot generated from the logged odds ratios of the work-related studies.

Figure 3. Funnel Plot for Publication Bias



The funnel plot does not appear to be symmetric around the mean logged odds ratio, and it does not appear to be funnel shaped. This does not necessarily cause concern about publication bias; as mentioned, the percentage of published studies included was small. However, funnel plot asymmetry can be due to other bias issues, including smaller studies. Sterne and Harbord (2004) report that “smaller studies are, on average, conducted and analyzed with less methodological rigor than larger studies so asymmetry may also result from the overestimation of treatment effects in smaller studies of lower methodological quality” (p. 128). The asymmetry of the funnel plot may be explained by this.

Chapter 4 Summary

This chapter presented the results that addressed the three research questions in the study. The results of the search were discussed, including the information sources, key words used, data collection process, study selection, and coding and protocol. The study characteristics were examined, as well as the descriptive statistics for the programs and participants in the treatment groups from the included studies.

The first research question asked the overall effect of work-related programs on dropout rates. This study determined through meta-analysis of the logged odds-ratios that work-related programs have an odds-ratio of 1.66. Three meta-regressions were conducted to examine whether methodological characteristics of the studies impacted effect sizes and to primarily determine which, if any, program or participant characteristics are associated with improved dropout rates. There were no statistically significant differences in effect sizes for any of the variables, indicating that none of the methodological, participant, or program characteristic were associated with better or worse dropout rates.

CHAPTER 5

DISCUSSION

The purpose of this research was to provide the most recent review up to date on the impact of work-related programs on dropout rates. In order for school administrators and policymakers to make informed decisions about programming to address the dropout rates and respond to the national agenda, they need to understand the overall effectiveness of work-related programs. Work-related programs are commonly used to curb the dropout rate, but results have been mixed on their effectiveness, and not much known about whom these programs impact most or the program characteristics that may lead to the best results. This research built on the comprehensive study of dropout rates conducted by Wilson et al. (2011), focusing on work-related programs and only included the most rigorously designed studies so that results could be reported with more confidence.

Chapter 5 reviews the current study and discusses findings, including the results from the meta-analysis and meta-regressions. The conclusions drawn from the findings as well as the practical implications of those findings are examined. Finally, potential for future research is discussed

Summary of Research

This section provides a summary of the previous chapters in order to contextualize the findings, conclusions, implications, and future research discussed later in the chapter.

Importance of Research

Though the high school graduation rate has been steadily increasing over the last several years, the fact remains that one in four high school students drop out of high school (Bloom, 2009, p. 1). The consequences of dropping out of school will last a lifetime for an individual. The National Dropout Prevention Center/Network reports that school dropouts in the United States earn an average of \$9,245 a year less than those who complete high school, have unemployment rates almost 13 percentage points higher than high school graduates, are disproportionately represented in prison populations, are more likely to become teen parents, and more frequently live in poverty (n.d.). Not only do these students suffer individual consequences, but also there are larger economic and social consequences. Each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Balfanz, 2012).

There are consequences for the workforce as well. According to the National Dropout Center/Network, only 40% of adults who dropped out of high school are employed, while this figure is 60% for those who did complete high school and 80% for

those with a bachelor's degree (2012). In a survey of employers reported in "Are They Really Ready to Work" (2006), the authors share that 42.3% of employers rated the overall preparation of high school graduates for their entry-level roles as deficient in skills such as professionalism, work ethic, teamwork/collaboration, oral communications, ethics/social responsibility, and critical thinking/problem solving (p. 31). The supply of jobs for people who graduate high school with a diploma are bleak, and are projected to remain so, making the job force an even more terrifying place for those who do not graduate. The U.S. Bureau of Labor Statistics projects that jobs that require a master's degree are expected to grow at the fastest rate during this time, while jobs requiring only a high school diploma are expected to grow the slowest (Bureau of Labor Statistics, n.d.).

The nation has set the priority of graduating youth and enrolling them in college. The goals are to achieve a 90% high school graduation rate nationwide by the class of 2020 with all students ready for the college and the workplace, and to have the highest college attainment rates in world by 2020 (Balfanz, 2012, p. 20). To reach these goals, the proportion of college graduates will need to increase by 50%, meaning eight million more youth will need to earn associate's or bachelor's degrees by 2020 (The White House, 2011). Recommendations in Balfanz's report include a focus on college and career readiness as the goal of education, making a rigorous and comprehensive understanding of work-related programs of the utmost performance for school administrators and policymakers.

Wilson et al. (2011) completed a comprehensive research synthesis of programs targeting school-aged children that focus on reducing school dropout rates.

The research found these programs to have a positive and significant effect on dropout and graduation outcomes, including work-related programs. The intention of work-related programs is to prepare students for life after high school. These programs “address several risk factors, including academic failure, alienation and rebelliousness, association with delinquent and violent peers, and low commitment to school. At the same time, vocational training enhances protective factors by providing job skills, on-the-job experiences, and recognition for work performed” (Office of Juvenile Justice and Delinquency Prevention, n.d.).

Inclusion Criteria

Studies needed use an experimental or quasi-experimental design in order to be included. The qualifying outcome for study inclusion was school dropout. The date of publication focused on studies after 2010 in order to add on to the study by Wilson et al. (2011). Work-related programs were included, and only those that were either based or affiliated with a school, or based at a community organization that explicitly targeted dropout prevention. Only programs that targeted school-aged youth were included.

Research Questions

Results of research and evaluation studies on work-related programs and their impact on graduation and dropout rates have been mixed. This study aimed to update the meta-analysis by Wilson et al. (2011), focusing on work-related programs, and also examined program characteristics and participant characteristics for whom treatment via work-related programs has proven to be more amenable. Below are the research questions addressed by this study.

1. How effective are work-related programs at preventing school dropout?
2. Within work-related programs, what participant characteristics are associated with lower school dropout rates?
3. Within work-related programs, what program characteristics are associated with lower school dropout rates?

Findings

Description of Studies

The unit of assignment for the studies was primarily at the individual level (91.1%), with only one study that assigned at the group level and three that assigned participants at the program level. The actual method of assignment varied across the studies. About a third (28.9%) were non-random and not matched, 20.0% were random with no matching, and 20.0% were random with matching. Other methods included wait list control (4.4%), matched on pretest and personal characteristics (13.3%), and matched only on demographics (13.3%).

On average, 55% of the participants are boys. The average percent of youth by race in each sample was 28.9% for white youth, 35.7% for black youth, 23.2% for Hispanic youth, and 14.4% for other minorities. The average age of participants in the treatment programs was 15.3. Grades ranged from fifth grade to twelve grade, with a majority of youth in the tenth grade.

The majority of the programs were school-based (89.2%) or affiliated (4.5%), with 6.3% of programs based at a community organization. Work-related program components included internships (11.7%), vocational training (66.7%), career exploration

(34.2%), job placement assistance (29.7%), and employment (34.2%). All programs but one offered multiple program components, including components that were not necessarily related to career exploration or work. Treatment sites included regular class time (12.6%), special class time (55.0%), resource room or counselor room (4.5%), school facility outside of school time (4.5%), private office (0.9%), public office (6.3%), work site (3.6%), or mixed (10.8%). Treatment groups varied in terms of how often they received treatment, with three-quarters of the programs involved daily contact with the youth (77.5%).

Mean effect size. The mean odds-ratio for the work-related dropout programs was 1.66 and a 95% confidence interval of 1.49, 1.83. The odds-ratio of 1.66 is a positive and statistically significant result, indicating that participants in work-related programs exhibit lower dropout rates or graduation rates than those in control or comparison groups, though this is considered small in social science research.

The $Q_E=482(df=610, p < 0.99)$, indicating homogeneity in the distribution of the odds-ratios, meaning that the study samples did not demonstrate more variability than could be explained from sampling area alone. Though the Q_E was not significant for the updated effect size of work-related programs, and therefore did not indicate heterogeneity in the distribution of the odds-ratios, a meta-regression was conducted to better understand true program effects, keeping in mind study method, type of outcome, type of publication, and the role of the evaluator, as was done to in the study by Wilson et al. (2011). Characteristics included random assignment, matched group design, and whether the evaluator was independent from the program. None of the variables included in the

meta-regression were statistically significant, though random assignment did produce slightly smaller effect sizes in the reference group.

Participant characteristics. A second meta-regression, which built off the meta-regression examining methodological characteristics, examined whether certain participant characteristics of youth who participated in work-related programs were associated with larger effect sizes. Participant characteristics included percent male, percent white, and average age. None of the participant characteristics were associated with better or worse outcomes in dropout rates.

Program characteristics. A third meta-regression was run to explore whether certain programmatic aspects of work-related programs were associated with larger effect sizes. Program characteristics included in the meta-regression include program components such as internships, job placement, employment, career exploration, and vocational training; implementation quality; program setting such as classroom setting, school setting but not classroom, and mixed program sites; and dosage, including program duration in weeks and hours of service per week. There were no significantly different treatment effects that were observed for any of the variables.

Conclusions

This section discusses the conclusions that can be drawn based on the findings from the study.

Effect of Work-related Programs on Dropout and Graduation Rates

The effect size for work-related programs was 1.66, which is greater than 1 and indicates that the treatment groups performed more favorably, graduating at higher rates

and dropping out at lower rates. This is a statistically positive and significant finding, which indicates that work-related programs can and do improve school dropout and graduation rates.

Vocational programs are one program type used to address high school dropout and the need for adolescents to be ready for post secondary or work-related experiences after high school. Students may become frustrated with their academic identity if they do not perform well in school, causing them to become disengaged and even drop out (Finn 1989). Vocational training may reengage these youth because of the focus on workforce preparation, which may seem more interesting and practical to students who are not successful academically (Agodini and Deck, 2004, p. 3). The results of previous evaluations of programs focused on reducing dropout rates are mixed, and there are no current systematic reviews of how work-related programs impact dropout rates and school completion, though work-related programs are a common method for addressing the dropout issue. Though some positive effects have been documented for individual programs, these effects are usually small and for students most at-risk. Furthermore, many reviews of dropout prevention programs, both vocational and otherwise oriented, reveal that there are few rigorous evaluations of these programs available, making it difficult to report with any confidence the overall effectiveness of such programs (Kemple, 2004; OJJDP Program Models; Child Trends; Lehr et al., 2003; What Works for Education; Klima et al., 2009). This finding strengthens the support for work-related programs as a method to reduce school dropout rates or increase graduation rates.

While the effect size found in this study, 1.66, favors the treatment group and is

statistically significant, it is still considered a small effect by many rules of thumb. That being said, not all outcomes are created equally, and some effect sizes are considered substantively important even if the effect size is considered small. The practical importance of an effect depends on its relative costs and benefits:

There is no wisdom whatsoever in attempting to associate regions of the effect size metric with descriptive adjectives such as "small," "moderate," "large," and the like. Dissociated from a context of decision and comparative value, there is little inherent value to an effect size of 3.5 or .2. Depending on what benefits can be achieved at what cost, an effect size of 2.0 might be "poor" and one of .1 might be "good." (Glass, McGraw, and Smith, 1981, p.104)

An effect size of 1.66 means that youth in work-related programs graduated school 1.66 times more often than those who were in the control or comparison groups.

Recall Balfanz's (2012) insight on the cost of a dropout:

- Moving just one student from dropout status to graduate status would yield more than \$200,000 in higher tax revenues and lower government expenditures over his or her lifetime.
- Graduating half of one class of dropouts would save the U.S. taxpayer \$45 billion in that year (p. 5).

Also recall that each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Wilson et al., 2011, p. 11). Therefore, even if an effect size is small in magnitude, its implications are still substantively important for an individual and the economy as whole.

Program and Participant Characteristics

The Wilson et al. (2011) study was able to provide the overall effects of rigorously studied dropout prevention and prevention programs, even providing mean odds-ratios for various types of programs. One of the types of programs included was the work-related program. But these programs are broad in description, from what services they include, the setting in which they occur, and the participants for whom they are targeted, and research does not exist to clarify these issues. There is no conclusive information available in the literature to suggest what types of program settings or program participants may have better outcomes in work-related programs. It is important to determine how effective these programs are, in what settings, and for whom they work best. This study was not able to discern any program or participant characteristics that were associated with better outcomes, indicating that work-related programs may be effective at reducing dropout rates across various program and participant related characteristics.

As noted in the Wilson et al. (2011) study, “participant characteristics are important from a policy perspective, inasmuch as dropout programs may be more or less effective for students with different characteristics” (2011, p. 33). Research has shown discrepancies in dropout rates for students by gender, race, and socioeconomic status. Boys are more likely to be dropouts than girls (8.5% vs. 6.3%), and racial/ethnic minority groups are also more likely to be dropouts (15.1% for Hispanics, 12.4 for American Indian/Alaskan Natives, and 8.0% for Blacks vs. 5.1% for Whites; National Center for Education Statistics, n.d.). Additionally, students from low-income households also drop

out of high school more often than those from higher income households (10% for low-income vs. 5.2% for middle income and 1.6% for high income students; National Dropout Prevention Center/Network, n.d.).

The percentage of boys, percentage of white students, and average age were included in a meta-regression to determine whether there were particular participant demographics associated with larger effect sizes. None of the variables resulted in statistically smaller or higher effect sizes. White students did not perform better or worse on the outcomes than minority students; boys did not perform better or worse than girls, and age was not associated with better or worse outcomes. These findings are mostly consistent with those in the Wilson et al. study (2011), where the authors concluded that:

While practitioners may still wish to tailor programs to particular racial or ethnic groups to encourage student engagement, or try different strategies with different age groups who may have different academic needs, the findings here suggest that such tailoring isn't necessary for programs to be effective at reducing dropout. (Wilson et al., 2011, p. 50)

There is not much discussion in the literature whether the location of the school a youth attends may impact dropout, or what dosage or program components are necessary in order for a student to be successful. An additional meta-analysis that built on the previous meta-analyses included an examination of program characteristics, including program components such as internships, job placement, employment, career exploration, and vocational training; implementation quality; program setting such as classroom setting, school setting but not classroom, and mixed program sites; and dosage, including program duration in weeks and hours of service per week.

There were no significantly different treatment effects that were observed for any

of the variables. None of the program components – internship, career exploration, job placement, job assistance, vocational training, and employment – were associated with smaller or larger effect sizes. The presence of implementation problems was not associated with better or worse graduation or dropout outcomes, which differed from the Wilson et al. study (2011), which found that programs exhibiting implementation issues produced significantly smaller effect sizes (p. 50). Contrary to the study by Wilson et al. (2011), program setting was not associated with dropout outcomes. In that study, “classroom-based programs and the mixed-setting programs produced significantly larger reductions in dropout than the community-based programs” (p. 50), whereas these results indicate that work-related dropout programs are effective across program sites, with no site type producing particularly smaller or larger effects than the others. The Wilson et al. study also indicated that programs that met more hours per week exhibited smaller effects due to large variability (p. 50). There were no differences in effect sizes associated with program duration or hours per week in this study, indicating that there is no “right” number of touch points as of yet.

Comparing the Original and Current Studies

There are differences between this research and the meta-analysis conducted by Wilson et al. (2011) that must be addressed. The primary purpose of this study was to update the meta-analysis conducted by Wilson et al., focusing on work-related programs. Forty-four studies were used from the Wilson et al. meta-analysis. Over 4,000 new studies were reviewed by title and abstract, focusing on studies after 2009, and only 71 were identified. Of those 71 studies reviewed in full, only one managed to meet all of the

eligibility criteria. Many studies were simply not appropriate because they did not meet methodological criteria or they were not actually related to work or vocational programs. The fact that only one study was added to this updated meta-analysis means that this research does not provide as much of an update as the researcher hoped. More importantly, it indicates that there continues to be a lack of rigorous study on the impact of work-related programs aimed at reducing dropout rates or improving graduation rates.

The odds-ratio effect size for work-related programs in the Wilson et al. (2011) study was 2.64, but in this study was 1.66, even though one study was added. The Wilson et al. study included 49 studies and 51 effect sizes, while this study included 45 studies and 111 effect sizes. The reason for these discrepancies is due to the difference in categorization of work-related programs in the two studies. The original meta-analysis categorized programs by their focal program component, meaning that a program had to be primarily considered a work-related program that offered internships, career exploration, job placement assistance, vocational training, employment, living allowance, and bonuses and sanctions applied to welfare grant. Yet many of the programs in the original study incorporated several different types of program components, not all of which were work-related. In fact, all but one program included in this updated research comprised multiple types of program offerings within the same program. This research was aimed at understanding any program that used work-related program offerings to reduce dropout rates, regardless of whether that was the primary focal point of the program. In doing so, the true impact of programs that are exclusively dedicated to offering work-related experiences and skills may be more difficult to decipher from this

research.

Wilson et al. (2011) incorporated a comprehensive and diverse strategy to identify the appropriate literature, including unpublished studies and international research, in order to reduce the possibility of omitting potentially relevant studies. This research attempted to utilize many of those same resources. This updated meta-analysis attempted to utilize all of the same electronic databases, but several databases searched by Wilson et al. were not easily accessible, and therefore were excluded from this updated study. Those databases included Education Abstracts, National Technical Information Service (NTIS), System for Information on Grey Literature (OpenSIGLE), Australian Education Index, British Education Index, CBCA Education (Canada), and Canadian Research Index.

A limitation Wilson et al. (2011) noted was the exclusion of geographic setting in the coding scheme. There may be differences in the types of program strategies used and the participants in those programs. A 2012 study examined differences in rural and urban high school dropout rates, and found that there were none (Jordan and Kostandini, 2012). Researchers have advocated for more attention to the fact that high school dropout is not only an urban issue. Over half (51%) of the lowest-performing high schools are in urban areas, 21% are in rural areas, 19% are in suburban areas, and 9% are in towns (Alliance for Excellent Education, 2010). This updated review originally intended to include geographic location as a moderator in meta-regression analyses. However, it was difficult to code studies for geographic location because either the studies did not provide the information, or, more often, the studies included multi-site level data.

For the Wilson et al. original research, eligible studies could be published in any language and conducted in any country as long as they met all other eligibility criteria. Campbell Collaboration affiliates outside the United States assisted with the location of studies published in other countries and languages other than English, but only English-language research was included in the update.

Implications

The ultimate objective of this systematic review is to provide school administrators and policymakers with an integrative summary of research evidence that is useful for making decisions about programmatic efforts to reduce school dropout and increase school completion for all types of students through the use of work-related programs.

Work-related Programs as an Intervention to School Dropout

The findings and conclusions of this research study imply that work-related programs are overall effective for addressing the issue of school dropout or high school graduation. This research addresses the mixed results on the effectiveness of work-related programs in previous evaluations, and it addresses the issue using very rigorous methodological designs in determining the effectiveness, making it the most comprehensive and rigorous study to date on the impact of work-related programs on school dropout.

As mentioned in the conclusions section, the effect size may be considered small by standard rules of thumb on odds-ratio effect sizes, but even a small effect can have a very substantial practical impact. Again, moving just one student from dropout status to

graduate status would yield more than \$200,000 in higher tax revenues and lower government expenditures over his or her lifetime (Balfanz, 2012, p. 9). This analysis indicates that work-related programs are effective in curbing school dropout, and therefore should be seriously considered as a means to decrease school dropout rates.

Effectively Organizing Work-related Programs in the Right Settings and for the Right Youth

The moderator analyses of program-related characteristics and participant demographics, though they did not produce statistically significant results, provided implications on the most effective settings for work-related interventions as well as the youth who may benefit most from the interventions.

Keeping with the Wilson et al. (2011) study, participant characteristics were not associated with dropout outcomes, implying that work-related programming may be beneficial to students regardless of gender, race, or age. This implication is interesting in that it indicates that tailoring programs to a specific gender, race, or age is not necessary in order for work-related programs to reduce dropout rates.

While the Wilson et al. (2011) study indicated that classroom-based programs and mixed-setting programs produced significantly larger reductions in dropout rates, this study did not. This study also defined work-related programs more broadly, rather than focusing on a primary component. The difference in conclusions between the studies implies that work-related programs, when defined based on whether they incorporate work-related components rather than their focal point, are effective regardless of the setting in which they take place. The program components included in the model were

internships, career exploration, vocational training, job placement, and employment were also effective regardless of which components were included in the program.

Future Research

Overall, evaluations of the effectiveness of work-related programs on reducing dropout rates or increasing graduation rates have been mixed in previous research. Additionally, the number of evaluations that use rigorous designs and explore the settings or populations in which the intervention may be most appropriate are few in number, as reported by most reviews examining the effectiveness of dropout programs. For example, U.S. Department of Education's What Works Clearinghouse report on dropout prevention found only 15 qualifying studies that reported outcomes related to staying in school or completing school. This report focused solely on interventions in the United States and did not include a meta-analysis of program effectiveness or an examination of the particular settings or populations for which interventions may be more successful.

While this current study intended to rectify these gaps in research by building on the work of Wilson et al. (2011), the fact is that only one additional study was eligible for inclusion four years after the original study. This fact demonstrates that more rigorous evaluation methods are needed to investigate the impacts of work-related programs. For example, MRDC, a social policy and research agency, estimates that career academies were first introduced 35 years ago, and over 8,000 high schools across the U.S. utilize them (MDRC, n.d.). This is a major investment with little rigorous research to show the impact on high school completion in recent years. As career academies and career and technical education continue to grow, these programs need to be rigorously evaluated to

determine overall effectiveness, as well as the most appropriate settings and program participants who will most likely benefit from the program.

The recommendation here for future research is more rigorous evaluation of work-related programs, focusing on random or matched groups. These studies should clearly indicate demographics of program participants, including the average age of the youth in the program, as well as various program attributes, including program setting, dosage, and work-related elements utilized in the program. When possible, studies should focus on examining differences within the program of study to better understand whether program or participant aspects are associated with better or worse outcomes.

Chapter 5 Summary

The objective of the proposed systematic review is to summarize the available evidence on the effects of work-related prevention and intervention programs that are aimed at school-aged students for increasing school completion or reducing school dropout. The primary focus of the analysis is to update the effect size for work-related programs, and the secondary focus is to explore what program characteristics may be related to better outcomes in these types of programs. The research examines differences associated with the program structure itself, including treatment modality, implementation quality, and program location or setting. In addition, evidence of differential effects for students with various characteristics will be explored, including age, gender, and race/ethnicity. Because of large ethnic and socioeconomic differences in graduation rates, it is particularly important to identify programs that may be more or less effective for disadvantaged students. The ultimate objective of this systematic

review is to provide school administrators and policymakers with an integrative summary of research evidence that is useful for making decisions about programmatic efforts to reduce school dropout and increase school completion for all types of students through the use of work-related programs.

There are prevention and intervention programs that focus either directly or indirectly on reducing dropout rate and increasing graduation rates as a way to address the issue. Wilson et al. (2011) conducted an extensive and rigorous meta-analysis that examined the effects of dropout prevention and intervention programs on several outcomes, including dropout related outcomes such as drop rate and graduation rate, as well as absence and truancy related outcomes. Their analysis showed that these prevention programs were effective overall compared to control and comparison groups. The researchers also determined effects by program type; one of the most common types of programs included in the study was the work-related program. These programs may include career exploration, vocational training, internships, and the like.

This research was largely built on the Wilson et al. (2011) study, providing an update focused on work-related programs. The mean odds-ratio for the work-related dropout programs was 1.66 and a 95% confidence interval of 1.48, 1.83. The odds-ratio of 1.66 is a positive and statistically significant result, indicating that participants in work-related programs exhibit lower dropout rates or graduation rates than those in control or comparison groups, though this is considered small in social science research.

While the effect size of 1.66 favors the treatment group and is statistically significant, it is still considered a small effect by many rules of thumb; however, there is

practical importance relative to costs and benefits. An effect size of 1.66 means that youth in work-related programs graduated school 1.66 times more often than those who were in the control or comparison groups. According to Balfanz (2012), moving just one student from dropout status to graduate status would yield more than \$200,000 in higher tax revenues and lower government expenditures over his or her lifetime, and graduating half of one class of dropouts would save the U.S. taxpayer \$45 billion in that year (p. 5). Each annual cohort of dropouts costs the United States over \$200 billion during their lifetime due to lost earnings and unrealized tax revenue; even a 1% increase in high school graduation rates could save \$1 billion in incarceration costs (Wilson et al., 2011, p. 11). Therefore, even if an effect size is small in magnitude, its implications are still substantively important for an individual and the economy as whole.

The moderator analyses examined participant demographic attributes and found that work-related programs were effective regardless of age, gender and race; that is, there were no associations between the demographic variables and smaller or larger effects. The finding indicates that work-related programs are suited for all youth, and no particular tailoring to demographics is necessary to make the program more successful.

Program aspects were also examined as part of moderator analysis, including program setting, dosage, implementation quality, and work-related components. Again, there were no significant associations between any of the variables and smaller or larger effect sizes. This indicates that work-related programs can be effective across program settings, and no particular approach (e.g. internships, career exploration, vocational training, etc.) works better than the others; they are equally effective.

APPENDIX A
DROPOUT PROJECT CODING MANUAL

ELIGIBILITY CRITERIA

This meta-analysis deals with the effects of prevention and intervention programs on school completion/dropout. To be eligible for coding, a study must use an eligible intervention directed toward an eligible participant sample, report data that permit calculation of a numeric effect size for at least one eligible outcome variable, and employ an eligible research design.

1. INTERVENTIONS

- a. There must be a school-based or affiliated psychological, educational, or behavioral prevention/intervention program, broadly defined, that involves actions performed with the expectation that they will have beneficial effects on student recipients. School-based programs are those that are administered under the auspices of school authorities and delivered during school hours. School affiliated programs are those that are delivered with the collaboration of school authorities, possibly by other agents, e.g., community service providers, and which may take place before or after school hours and/or off the school grounds. Community-based programs that are explicitly presented as dropout prevention/intervention programs are eligible whether or not a school affiliation is evident. Other community-based programs that may include dropout among their goals or intended outcomes, but for which it or related variables are not the main focus, and which have no evident school affiliation are not eligible. Programs that are solely medical or pharmacological in nature are not eligible. Broad programs and policies that are at the district level where no intervention can be identified as occurring at the school level, such as district line restructuring, are not eligible.

2. SUBJECTS

- a. The research must investigate outcomes for an intervention directed toward school-aged youth, defined as those expected to attend pre-k to 12th grade primary and secondary schools, or the equivalent in countries with a different grade structure, corresponding to approximately ages 4-18. The age or school participation of the sample must be presented in sufficient detail to allow reasonable inference that it meets this requirement.
 - Recent dropouts who are between the ages of 18-22 are also eligible if the program under study is explicitly oriented toward secondary school completion or the equivalent.
- b. General population samples of school-age children are eligible. Samples from populations broadly at risk because of economic disadvantage, individual risk variables, and closely related factors are also eligible (e.g., inner city schools, students from low SES families, teen parents, students with poor attendance records, students who have low test scores or who are over-age for their grade). Samples consisting exclusively of specialized populations, such as students with

mental disabilities or other special needs, are not eligible. However, inclusion of some such individuals in a broader sample in which they are a minority proportion does not make that broader sample ineligible.

- Students with learning disabilities, such as dyslexia, that generally don't require them to be in specialized schools or classrooms (i.e., they attend mainstream classes and typical schools) are considered eligible. *NOTE: if studies with these types of samples are located, they should be set aside and brought to the attention of the group for discussion.*

3. RESEARCH DESIGNS

- a. An eligible study must use an experimental or quasi-experimental design; specifically, it must involve comparison of treatment and control conditions to which students are randomly assigned or nonrandomly assigned with matching, statistical controls, or evidence of initial equivalence on key risk variables or student characteristics. The following research designs are eligible:
 - a. Participants were randomly assigned to treatment and control conditions or assigned by a procedure plausibly equivalent to randomization.
 - i. Participants in the treatment and control conditions were matched and the matching variables included a pretest for at least one qualifying outcome variable. However, if the qualifying outcome variable does not lend itself to meaningful pretest or the pretest values can be assumed zero, but the groups are matched on other variables plausibly related to risk for dropout, the study is still eligible. For this purpose, use of pretest or initial risk variables as statistical controls, e.g., in an ANCOVA or multiple regression analysis, is considered the equivalent of matching.
 - b. If participants were not randomly assigned or matched, the study must have both a pretest or relevant baseline risk variables and a posttest on at least one qualifying outcome variable with sufficient statistical information to derive an effect size or to estimate group equivalence from statements of statistical significance, or provide evidence of equivalence on key risk variables and/or student characteristics.
- b. Studies that employ designs in which more than one treatment group is compared to a single control group are eligible; in these cases, effect sizes should be calculated for each treatment group compared to the control group.

Treatment-treatment studies that compare two or more treatments to each other without a control group may be eligible if one of treatment group receives a 'sham' or 'straw-man' treatment that is equivalent to a control condition, or if one of the treatments is a practice as usual condition in which that practice is not a distinctive program delivered at a relatively high level. E.g., if the school has a truancy officer engaged in routine activities for such a function, that would be acceptable as a practice as usual control condition.

NOTE: These should be set aside and brought to the attention of the group for discussion.

Excluded designs:

Posttest only non-equivalent comparisons (not randomized, matched, or demonstrating equivalence) are **not** eligible. Single-group pretest-posttest designs are **not** eligible.

Each treatment and/or control group in a study must have at least 10 subjects at the time of assignment.

4. OUTCOME VARIABLES

- a. The study must assess intervention effects on at least one outcome variable that represents school completion or dropout, or is a close proxy measure or recognized precursor for dropout. Qualifying outcome variables are those that fall in or are substantially similar to the following categories:
 - School completion/dropout;
 - GED completion/high school graduation;
 - Absences or truancy;
 - Enrollment/non-enrollment in school.

NOTE: Studies in which the majority of children are under middle school age (approximately 5th grade or age 11) must have either a school completion or dropout outcome, or have attendance measures that are assessed in middle or high school.

5. DATE OF PUBLICATION

- Eligible studies should be relatively modern, to be applicable to contemporary students. Therefore, the date of publication or reporting of the study must be 1985 or later even though the research itself may have been conducted prior to 1985. If, however, there is evidence in the report that the research was actually conducted prior to 1980 (more than five years before the 1985 cutoff date), then the study should be excluded.

6. EFFECT SIZES

- The study must report sufficient quantitative data to compute an effect size on an eligible outcome. In addition, the variables involved in the effect size must have a known direction of scoring, i.e., whether high or low values represent favorable or less favorable results. Studies that meet all eligibility criteria except this, i.e., fall short only because an effect size cannot be calculated, should be identified and held separately for further consideration. *NOTE: If a study meets all other eligibility criteria except for this one, do not exclude, but bring to the attention of the group for discussion.*

7. STUDY SITE and LANGUAGE

- The study can be published in any language and conducted in any country as long as it meets all other eligibility criteria.

FILEMAKER & GENERAL CODING INFORMATION

There are six different FileMaker files that we use to code studies for this project. All of these files are linked together so that you can navigate between them by clicking the appropriate buttons. These files are defined below. Note: you may not use all of these files in your coding.

I. Bibliographic Database: We use this database to maintain the bibliography of potentially eligible reports. This database includes bibliographic information about each report, library location information, and tracking data about how far along each report is in the retrieval and coding process. You might use this database to search for related reports or to indicate when an article has been coded.

II. Eligibility Database: This database, generally accessed from the Bibliographic Database, contains information about the eligibility of each identified study; each study may be represented by one or more reports in the Bibliographic Database.

II. Header Database: This database includes the general information pertinent to a study. A study is defined as an investigation involving one independent group of subjects. In a quasi-experiment with a treatment and control group, the “study” includes both the treatment and control subjects. There will be one record in this database for each study that is coded (there may be multiple reports per study or multiple studies per report). Variables coded at this level include information about the methodology, treatment groups, initial group equivalence, etc.

III. Groups Database: This database includes information about the various groups that comprise a study. There will be one record in this database for each treatment AND comparison group for which there is sufficient data. Aggregate treatment and comparison groups are ALL groups in a study, including all levels of treatment and any type of comparison group. Thus, in a quasi-experimental study with one treatment group and one comparison group, you will have two records in this database. Variables coded at this level include information about the treatment and subjects under study.

IV. Dependent Variables Database: Within a given study, the researcher might evaluate the effectiveness of the intervention using multiple outcomes (or dependent variables). For example, the researcher might assess dropout during an observation period and the attendance days per subject. Thus, for this study, you would have two records in the dependent variables database, one for each outcome measure that you are coding. If the same outcome measure is used multiple times in a study, as in a pretest, a posttest, and a follow-up, you will only have one record for the outcome, but you will have multiple records for the effect sizes (see VI below). This database includes a description of each

dependent variable, and some basic methodological information about the variable, such as reliability.

V. Breakouts: Breakouts are comparisons for subgroups of any treatment or control group, e.g., a treatment group compared with a comparison group using only the males in the sample. Each variable (e.g., gender, age) by which a group or groups are crossed constitutes one breakout; each value of that variable defines one subgroup; e.g., a male vs. female stratification is one breakout with two subgroups, one male and one female. If only the male subgroup is reported, there is still one breakout, but only one subgroup. Note that a simple report of the number of males and females in the treatment and control groups does not constitute a breakout (though it is relevant to group equivalence issues). To be a breakout, outcome data must be reported for the treatment-control comparison for at least one subgroup of the breakout variable. Breakouts are usually presented because the authors think that subgroups (e.g., males and females) are sufficiently different to warrant separate presentation of results (because, for example, males may be more likely to drop out than females).

VI. Effect Sizes: The effect size database tracks the actual statistical results of the study being coded. Because different researchers may present their results using different statistics (e.g., with a t-test or using means and standard deviations), we need to convert the statistics from each study into a common metric for our own analyses. The metric we use for this purpose is called an effect size. In most cases, the Effect Size database will calculate the appropriate effect size for the data that you enter. However, in some cases, you may use the Effect Size Determination Program (Excel Toolkit) or other resources to do the calculations. For each study you are coding, you will have one effect size record for EACH effect size – there can be anywhere from 1 to 50 or more effect sizes in one study and you will need to code each one separately. There will be more information below about how to do this coding.

You will also use the effect size database to record effect sizes related to the equivalence of treatment and control groups at the start of a study. As you know, it is vital that the two groups be as similar as possible at the beginning of the study on any characteristics, such as gender, age, and ethnicity, that might be related to dropout. When the two groups are similar, the differences between the two groups at the end of the study can be more easily attributed to the intervention and not these other characteristics. You will create a group equivalence record in the effect size database for each treatment-control comparison on a pretreatment variable that is relevant to group equivalence issues. That is, if the study provides you with the ages of the students in the treatment and comparison groups and the number of boys in each group, you will have two group equivalence records in the effect size database in addition to the regular effect sizes that you might code for this study.

DROPOUT PROJECT CODING MANUAL

HEADER VARIABLES CODING

Step 1. Study Identifiers, Study Context, and Group Identification & Selection

STUDY IDENTIFIERS

The “unit” you will code here consists of a study, i.e., one research investigation of a defined subject sample or subsamples compared to each other, and the treatments, measures, and statistical analyses applied to them. Sometimes there are several different reports (e.g., journal articles) about a single study. In such cases, the coding should be done from the full set of relevant reports, using whichever report is best for each item to be coded; **BE SURE YOU HAVE THE FULL SET OF RELEVANT REPORTS BEFORE BEGINNING TO CODE**. Sometimes a single report describes more than one study, e.g., one journal article could describe a series of similar studies done at different sites. In these cases, each study should be coded separately as if each had been described in a separate report.

Each study has its own study identification number, or StudyID (e.g., 619). Each report also has an identification number (e.g., 619.01), which you will find printed on the folder holding the report. The ReportID has two parts; the part before the decimal is the StudyID, and the part after the decimal is used to distinguish the reports within a study. (These two types of ID numbers, along with bibliographic information, are assigned and tracked using the bibliography.) When coding, use the study ID (e.g., 619) to refer to the study as a whole, and use the appropriate report ID (e.g., 619.01) when referring to an individual report.

While reading reports for coding, be alert to any references to other dropout studies that may be appropriate to include in this meta-analysis. If you find appropriate-looking references that are not currently entered into the bibliography, the references may need to be entered.

[StudyID] Study identification number of the study you are coding, e.g., 1923.

[Coder] Coder's initials (select from menu)

[H1] Year of publication (four digits): If more than one report, choose earliest date.

[CodeDate] Date you began coding this study (will be inserted automatically)

STUDY CONTEXT

[H2] Country in which study conducted.

1. USA
2. Great Britain

3. Canada
4. Scandinavia: Denmark, Finland, Norway, Sweden
5. Australia/New Zealand
6. Other Western European Country: _____
7. Other: _____

[H3] Type of publication. If you are using more than one type of publication to code your study, choose the publication that supplies the effect sizes (in cases where more than one report provides effect sizes, choose a “peer reviewed” choice over another option, or choose the report that provides the most effect sizes).

1. Book
2. Journal article
3. Book chapter (in an edited book)
4. Thesis or dissertation
5. Technical report
6. Conference paper/presentation
7. Other
9. Cannot tell

GROUP IDENTIFICATION AND SELECTION

At this stage, you will need to identify the aggregate treatment and/or comparison groups used in the study for which effect size statistics can be computed. To do this, you will need to distinguish aggregate groups, which you will code here, from subgroups (or breakouts), which you will code later:

(1) Aggregate treatment and/or comparison groups. Aggregate treatment or control groups are the largest participant groupings on which contrasts between experimental conditions or contrasts between time points can be made. Note that the designations “comparison group” and “control group” refer to any group with which the treatment of interest is compared that is presumed to represent conditions in the absence of that treatment, whether a true random control or not. Often there is only one aggregate treatment group and one aggregate control group, but it is possible to have a design with numerous treatment variations (e.g., different levels) and control variations (e.g., placebos) all compared (e.g., in ANOVA format) to each other.

(2) Breakouts. Sometimes researchers will present data for some subset(s) of the participants from an aggregate group; e.g., for an aggregate group composed of males and females, the researchers may present some results for the males and females separately. You will code information about breakouts later.

Identifying the Aggregate Groups

Type in the name or identifier for each aggregate treatment group and each aggregate comparison group described in the study, whether you believe the group is eligible for coding or not.

Group labels used by researchers do not necessarily conform to the definitions of group types used in this project. In some cases, for example, researchers may compare one treatment with another treatment, and may call this “other” treatment a comparison or control group. For our purposes, if this “other” treatment group can realistically be expected to be effective, list it as a treatment group below; if it is a minimal or placebo treatment, not expected to produce an effect, list it as a comparison group.

Treatment Groups [H4a-d]

- 1 _____
- 2 _____
- 3 _____
- 4 _____

Comparison Groups [H5a-d]

- 1 _____
- 2 _____
- 3 _____
- 4 _____

[H4] Total number of treatment groups: _____

[H5] Total number of control groups: _____

ASSIGNMENT OF PARTICIPANTS

[H6] Unit of group assignment. The unit on which assignment to groups was based.

- 1 individual (i.e., some children assigned to treatment group, some to comparison group)
- 2 group (i.e., whole classrooms, schools, therapy groups, sites, residential facilities assigned to treatment and comparison groups)
- 3 program area, regions, school districts, counties, etc. (i.e., region assigned as an intact unit)
- 9 cannot tell

[H7] Method of group assignment. How participants/units were assigned to groups.

This item focuses on the initial method of assignment to groups, regardless of subsequent degradations due to attrition, refusal, etc. prior to treatment onset. These latter problems are coded elsewhere.

Random or near-random:

1. randomly after matching, yoking, stratification, blocking, etc. The entire sample is matched or blocked first, then assigned to treatment and comparison groups within pairs or blocks. This does not refer to blocking after treatment for the data analysis.
2. randomly without matching, etc. This also includes cases when every other person goes to the control group.
3. regression discontinuity design: quantitative cutting point defines groups on some continuum (this is rare).
4. wait list control or other quasi-random procedure presumed to produce comparable groups (no obvious differences). This applies to groups which have individuals apparently randomly assigned by some naturally occurring process, e.g. first person to walk in the door. The key here is that the procedure used to select groups doesn't involve individual characteristics of persons so that the groups generated should be essentially equivalent.

Non-random, but matched: Matching refers to the process by which comparison groups are generated by identifying individuals or groups that are comparable to the treatment group using various characteristics of the treatment group. Matching can be done individually, e.g., by selecting a control subject for each intervention subject who is the same age, gender, and so forth, or on a group basis, e.g., by selecting comparison schools that have the same demographic makeup and academic profile of treatment schools.

5. matched ONLY on pretest measures of some or all variables used later as outcome measures.
6. matched on pretest measures AND other personal characteristics, such as demographics.
7. matched ONLY on demographics: big sociological variables like age, sex, ethnicity, SES.

Nonrandom, no matching prior to treatment but descriptive data, etc. regarding the nature of the group differences:

8. Non-random, not matched, but pretreatment equivalence information is available.

99. cannot tell

[H8] Confidence in assignment ratings. Overall confidence of judgment on how participants were assigned

1. Very Low (Little Basis)
2. Low (Best Estimate)
3. Moderate (Weak Inference)

4. High (Strong Inference)
5. Very High (Explicitly Stated)

Equivalence of the groups being compared

At this point, you should go to the Effect Size Database to code group equivalence effect sizes and descriptive information about initial group differences for the study. See the Effect Size Coding Sheet section of this manual for more information on effect size calculation.

[H9] Number of variables on which treatment and comparison group differences were statistically compared prior to the intervention. A statistical comparison is one in which a statistical test was performed by the authors, whether they provide data or not (e.g., “no statistically significant differences were found”). Include in your count any demographic or risk factor comparisons as well as any comparisons on pretest variables, that is, measures of a dependent variable taken prior to treatment, e.g., prior number of absences when subsequent number of absences is used as an outcome measure.

[H10] Results of statistical comparisons.

1. no comparisons made
2. no statistically significant differences
3. significant differences judged unimportant by coder. See note below regarding “importance” judgment.
4. significant differences, judged of uncertain importance by coder
5. significant differences, judged important by coder

[H11] Number of variables on which treatment and comparison group differences were or can be descriptively compared prior to the intervention. A descriptive comparison is any comparison across treatment and control groups that does not involve a statistical test (e.g., the actual number of males and females in each group or a statement by the author(s) about group similarity).

[H12] Results of descriptive comparisons.

1. no comparisons made or available
2. negligible differences, judged unimportant by coder. See note below regarding “importance” judgment.
3. some differences, judged of uncertain importance by coder
4. some differences, judged important by coder

Note: An “important” difference means a difference on several variables relevant to the outcome variables, or on a major variable, or large differences; major variables are those likely to be related to dropout, e.g., SES, or family circumstances.

[H13] Rating of similarity of treatment and control groups. Using all the available information, rate the overall similarity of the treatment group and the comparison group, prior to treatment, on factors likely to have to do with dropout or responsiveness to treatment (ignore differences on any irrelevant factors). Note: Greatest equivalence from “clean randomization” with prior blocking on relevant characteristics and no subsequent attrition/degradation; least equivalence with some differential selection of one “type” of individual vs. another on some variable likely to be relevant to dropout.

Guidelines: Use ratings in the 1-3 range for good randomizations and matchings, e.g., 1=clean random, 2=nice matched. Use ratings in the 5-7 range for selection with no matching or randomization or instances where it has been seriously degraded, e.g., by attrition before posttest. Within this bracket, the question is whether the selection bias is pertinent to the outcomes being examined. Were participants selected explicitly or implicitly on a variable that might make a big difference in dropout? The middle three points are for sloppy matching designs, degradations, bad wait list designs, and the like. If the data indicate equivalence but the assignment procedure was not random give it a 4 or thereabouts since not all possible variables were measured for equivalence between groups.

1. Very similar, equivalent
- 2.
- 3.
- 4.
- 5.
- 6.
7. Very different, not equivalent

[H14] Overall confidence on rating of group similarity

1. Very Low (Little Basis)
2. Low (Best Estimate)
3. Moderate (Weak Inference)
4. High (Strong Inference)
5. Very High (Explicitly Stated)

[H15] Click here to record any problems you encountered while coding this header.

GROUP EQUIVALENCE EFFECT SIZE CODING

At this point, you should go to the Effect Size Database to code group equivalence effect sizes and descriptive information about initial group differences. See the Effect Size Coding section of this manual for more information on effect size calculation.

For each measure you can identify on which the treatment and control group were compared prior to treatment (other than dependent variables) or on which you can tell equivalence (e.g. if all males then code it here), determine which group is favored and if possible, calculate an effect size (ES, standardized difference between means or odds ratio). Do not include here any comparisons on pretest variables, that is, measures of a dependent variable taken prior to treatment. In such cases the pretreatment ES is coded later as pretest information, not here as group equivalence information.

The only eligible variables for group equivalence effect sizes are: (a) gender, (b) age, (c) grade level, (d) race/ethnicity, and (e) variables relating to risk for school dropout. A pretest that is used later in the study as a posttest would not be coded here – you would code it as a pretest effect size. In matched group research designs, you will still code equivalence here for all eligible variables, even if groups were equally matched (e.g., both studies were 50% male, yielding a group equivalence effect size of .00). If the study reports group equivalence outcome data for multiple risk variables, group equivalence effect size information should be coded for up to four variables. If more than four variables are available for any of the risk factors, code the four most relevant ones. When deciding which are most relevant, use the following criteria:

1. First preference should be given to behavioral measures (e.g., prior absences, school performance).
2. Second preference should be given to measures of psychological conditions, predispositions, or attitudes (e.g., school engagement, school bonding, etc.).
3. Lowest preference should be given to broad measures of social disadvantage or family history (e.g., socioeconomic status of parents, residence in inner-city).

[StudyID] Indicate the Study ID for the study you are coding.

[ReportID] Enter the Report ID for the report in which you found the information on group equivalence. Use the complete Report ID, e.g. 1973.01.

[pagenum] Enter the page number on which you found the information on group equivalence.

[ES24] Type of effect size:

- 5 Group Equivalence (for baseline treatment-control comparisons on variables other than the dependent variables)

[ES19] Wave number. Pretests and group equivalence effect sizes always get a 1; each wave thereafter gets numbered consecutively, beginning with 1. Some studies involve more than one posttest measurement and we need to be able to distinguish one from another. Give the first posttest after treatment a 1, the second a 2, and so on.

[ES25] Variable on which comparison is made: _____ (e.g., gender, age, etc.)

[ES17] Which group is favored? Whichever group has more of the characteristic that presumably makes them better off or more amenable to treatment (e.g., less truant, higher SES, smarter, etc.) is considered favored. NOTE: You should code this item even for cases in which you are unable to calculate a numeric effect size but have information about which group is favored.

- | | |
|---|--|
| 1 | Treatment (fewer males, younger, fewer minorities, less antisocial, less risk) |
| 2 | Control (fewer males, younger, fewer minorities, less antisocial, less risk) |
| 3 | Neither, exactly equal |
| 9 | Cannot tell, no report |

[ES15] Significance of group equivalence comparison (ONLY).

- | | |
|----|--|
| 1 | No statistically significant differences |
| 2 | Statistically significance differences |
| 3 | Negligible descriptive differences |
| 4 | Significant descriptive differences |
| 98 | N/A: No comparison made |

Data Fields: Fill in the data fields using the relevant statistical information provided in the report(s). You do not need to fill in all the fields; fill in only the information necessary to calculate an effect size. Thus, if the report provides sample sizes, means, standard deviations, and t-test scores, you need only enter the sample sizes, means, and standard deviations.

ONCE YOU HAVE FINISHED CODING THE GROUP EQUIVALENCE EFFECT SIZE INFORMATION, YOU SHOULD RETURN TO THE HEADER FILE TO COMPLETE THE CODING OF HEADER VARIABLES.

DROPOUT PROJECT CODING MANUAL
TREATMENT AND CONTROL GROUPS CODING

Create one record in this database for each of the aggregate treatment and/or control groups that you selected earlier for coding. Studies with a treatment group and a control group will have two records, etc.

Group Identification and General Nature Of Treatment

[StudyID] Type in the StudyID for the study you are coding if it does not appear automatically.

[GroupID] Number each group consecutively within a study, starting with 1.

[G1] Select the type of group you are coding.

- 1 Treatment group
- 2 Control group

[G2] What general type of “treatment” does this group receive?

Intervention Condition

- 1 Focal program or treatment. There may be several focal programs in a study, as when two different types of treatments, both of which could be expected to be effective, are compared.

Control Condition

- 2 “Straw man” alternate program or treatment, diluted version, less extensive program, etc., not expected to be effective but used as contrast for treatment group of primary interest. If the alternate treatment is not minimal and could realistically be expected to be effective, it is not a control condition and should be classified as a focal treatment instead.
- 3 Placebo (or attention) treatment. Group gets some attention or sham treatment (e.g., watching Wild Kingdom videos while treatment group gets therapy)
- 4 Treatment as usual. Group gets “usual” handling instead of some special treatment.

[G3] Program name. Write in program or treatment label for this group (e.g., Dropout Prevention Curriculum, waiting list control, etc.). REMEMBER: YOU MUST CREATE A PROGRAM LABEL FOR CONTROL GROUPS AS WELL AS TREATMENT GROUPS.

[G4] Program description. Write in a brief description of the treatment this group receives. Please try to keep the description short by focusing on the key elements of treatment, but make sure you include ALL treatment elements in your description. As much as possible, quote or give a close paraphrase of the relevant descriptive text in the study report. REMEMBER: YOU MUST CREATE A DESCRIPTION FOR CONTROL GROUPS AS WELL AS TREATMENT GROUPS.

TREATMENT CHARACTERISTICS

[G5] Intervention type:

1. School-based (administered under the auspices of school authorities and delivered during school hours)
2. School affiliated (delivered with the collaboration of school authorities, possibly by other agents, e.g., community service providers; may take place before or after school hours and/or off the school grounds)
3. Community-based (explicitly presented as dropout prevention/intervention programs; may or may not have a school affiliation)
4. Not applicable (control condition)

[G6] Program components.

For each treatment AND control condition:

First check all program types that apply to a given intervention (e.g., a program may include GED preparation, cognitive behavioral techniques, tutoring, and contingency management).

Second, choose the **one** program type that can be considered the focal program characteristic. Most programs will arguably deliver multiple service types, but do your best to narrow the focal type down to one category. It may be helpful to examine the amount of each service type delivered. For instance, if a program delivered 1 hour/week of skills training to parents and 5 hours/week of vocational training to students, you would code vocational training as the focal program component. If a program contains too many service types to distinguish a focal type, choose “multi-service” package as the focal component.

ACADEMIC:

1. Curriculum
2. ESL/ELL (English as a second language/ English language learners)
3. Remedial education (e.g., reading remediation)
4. GED preparation
5. Computer-assisted learning
6. Test-taking and study skills assistance

- 7. Tutoring
- 8. Homework assistance
- 9. Extracurricular activities (e.g., after school club). NOTE: just because a program is delivered after school does not mean it should be coded here; this program component should include academic, social, or sport activities that are separate from regular school activities.
- 10. Professional development for school staff
- 49. Individualized teaching

SCHOOL STRUCTURE

- 11. Class or grade reorganization (schools within schools, team teaching)
- 12. Small class sizes/small “learning communities”
- 13. Alternative school (e.g., small school settings comprised primarily of students with severe academic or behavioral problems that preclude them from attending regular classes; i.e., this is the ‘last chance’ for many students who may have otherwise been expelled or suspended from school)

FAMILY ENGAGEMENT:

- 14. Family outreach
- 15. Feedback to parents and students on performance
- 16. Parent or teacher consultation enhancement
- 17. Parenting skills program
- 47. Skills training for significant others

COLLEGE FOCUSED/CONNECTING STUDENTS TO ATTAINABLE FUTURE:

- 18. Academic advising
- 19. College-preparatory curriculum
- 20. Academic summer/weekend program (i.e., enrichment programs)
- 21. College campus visits
- 22. College and financial aid application assistance
- 23. College scholarships

WORK RELATED/ FINANCIAL SUPPORT:

- 24. Internships
- 25. Career exploration
- 26. Vocational training
- 27. Job placement assistance
- 28. Living allowance
- 29. Bonuses and sanctions applied to welfare grant
- 48. Employment

LINKING TO SERVICES:

30. Case management
31. Health services
32. Transportation assistance
33. Child care/day care
34. Residential living services

SOCIAL RELATIONSHIPS:

35. Mentoring
36. Peer support
37. Social events
38. Community service/volunteer service/student as tutor (“helper-therapy”)
39. Recreational, wilderness, etc. program

PERSONAL/AFFECTIVE:

40. Counseling
41. Skills training (life skills, social skills/social competence)
42. Cognitive behavioral therapy (e.g., problem solving skills)

BEHAVIORAL:

43. Attendance monitoring
44. Contingency management, financial incentives, token economy, extrinsic reward system to promote attendance/academic achievement

OTHER:

45. Multi-service package (*NOTE: Only choose this program code if the group receives an amorphous, broadly defined program with components that cannot be clearly identified otherwise. Use this program code as focal if a group has multiple “focal” treatment components and you cannot make a distinction otherwise.*)

46. OTHER (*Please, describe [prog50a]*_____)

88. control group

[G9] Treatment Site. Nature of the site in which treatment generally delivered: (select one)

School Sites

1. Regular Class Time (this includes interventions delivered during regularly scheduled classes AND in the regular classroom for youths in the group)

2. Special Class (e.g., youth in treatment are in a classroom-type setting that is different from a typical classroom, although it may be the subjects' usual classroom – includes such settings as special education classrooms, schools-within-schools, alternative schools, etc.)
3. Resource Room, School Counselor's Office, or other similar setting that is NOT the student's regular classroom; the idea here is that students are removed from class for treatment
4. Treatment delivered at school facility, but not during regular school hours (e.g., afterschool programs)

Home

5. Treatment delivered in the subject's home

Community-based, Non-residential

6. Private office, clinic, center (e.g., YMCA, university, therapist's office)
7. Public office, clinic, center (e.g., human services department, public health agency)
8. Work site (e.g., community service, trash collection on roadside, etc.)
9. Park, playground, wilderness area, etc.

Institutional, Residential

10. Private institution, residential
11. Public institution, residential (e.g., camp, reformatory)

Mixed or Multiple Sites

12. School and home
13. Other mixed, some combination of above sites

88 N/A: control group

99 Cannot tell

[G10] Role of the evaluator(s)/author(s)/research team or staff in the program. This item focuses on the role of the research team working on the evaluation, regardless of whether they are all listed as authors.

- 1 evaluator delivered therapy/treatment
- 2 evaluator involved in planning or controlling treatment or is designer of program
- 3 evaluator influential in service setting but no direct role in delivering, controlling, or supervision
- 4 evaluator independent of service setting and treatment; research role only
- 8 Not applicable, control condition
- 9 cannot tell

[G11] Role of program developer in the research project. This items focuses on the individual (or group of individuals) who created or developed the program and their role

in the delivery of the program under study. Is the program developer the researcher conducting the study, or is the program developer not participating in the research project?

- 1 Program developer is author/evaluator/delivery agent
- 2 Delivery agent/author/evaluator modified existing program, but original program developer is not involved (note: this response suggests that the author/evaluator/delivery agent takes on a sort of quasi-developer status by modifying a program)
- 3 Program developer is not affiliated with research study and program is delivered as originally intended by developer
- 8 Not applicable, control condition
- 9 cannot tell

[G12] Routine practice or program vs. research project. Indicate the appropriate level for the treatment you are coding: at one end of the continuum are research projects (option 1), in which a researcher decides to implement and evaluate a particular program for research purposes; in many cases, the program may require the cooperation of a service agency (school, clinic, etc.), but the intervention is delivered primarily so the researcher can conduct research. At the other end of the continuum are evaluations of “real-world” or routine programs (option 3): a service agency implements a program on its own, and also decides to conduct an evaluation of the program; the evaluation may or may not be conducted by outside researchers. In the middle of the continuum are demonstration projects (option 2), which are conducted primarily for research purposes, but generally have more elements of “real world” practice than typical research projects as defined under option 1. Demonstration projects generally involve a program that has been studied in prior research but is being tested for effectiveness in different settings than the original research, or on a larger scale than the original research.

If a researcher is a school principal and is conducting the evaluation as part of his/her dissertation, the decision depends on the extent of the program. If the program is small-scale and implemented in, say, a classroom or two, and supervised by the researcher/principal, code it as a research project. If the program is a broader school-wide program that the researcher/principal happens to be evaluating, code it as either a demonstration or routine program, depending on whether the program is a special program being tested (demonstration) or something that the school does on a routine basis (routine practice).

1. research project: The intervention would not have been implemented without the interest or initiative of the researcher(s). The intervention is delivered by the research staff or by service providers (regular agency personnel, teachers, etc.) trained by the researchers.
2. demonstration project: A research project that involves a new or special program being tested, rather than a routine program. Although generally implemented by researchers for research purposes, a demonstration project has more elements of

actual practice than a research project. Demonstration projects usually involve programs that have been studied previously, either in small-scale pilot projects or tightly controlled efficacy trials; demonstration projects would serve as a larger scale or quasi-real-world test of a promising program.

3. evaluation of a “real-world” or routine program: A service agency implemented the program using routine personnel and the typical clients for that program; there may be outside researchers who conduct the evaluation, but the program they are evaluating was already in place before the research began and is presumed to continue after the research has ended.

8 Not applicable, control condition

9 cannot tell

[G13] Treatment provider’s discipline. Indicate the discipline or type of treatment provider for the treatment. This item focuses on the individual(s) who have direct contact with the subjects in treatment, not necessarily the persons conducting the data analysis or evaluation. In multi-service treatment programs with multiple providers, indicate the discipline of the individual(s) who provide the focal or modal treatment modality.

1. Teacher
2. School guidance counselor
3. School psychologist
4. School personnel, other than school counselor or teacher (e.g., principal, school nurse)
5. Counselor
6. Social worker
7. Researcher or researcher’s staff, graduate students
8. Other
88. N/A: no treatment received
99. Cannot tell

[G14] Did treatment personnel receive special training in this specific program, intervention, or therapy? If the treatment is delivered by the researcher, use “yes” below, unless the report indicates otherwise.

- | | |
|---|-------------|
| 1 | yes |
| 0 | no |
| 9 | cannot tell |

[G15] If yes, write in amount of training of personnel for providing this treatment:

[G16] Treatment Format:

For each treatment AND control condition:

First check all formats that apply to a given intervention (e.g., a program may include group and individual components, or have a family component).

Second, choose the **one** format type that can be considered the focal format. This selection should match the format of the focal program type you selected above under G6. If you selected multi-service package above, select the format for the most frequent or most focal piece of the package; if this is impossible, select multiple format program.

- [1] ___ Subject alone (self-administered treatment)
- [2] ___ Subject & provider, one-on-one
- [3] ___ Subject group and provider, not classroom
- [4] ___ Subject group and provider, classroom
- [5] ___ Parents only and provider, child not present
- [6] ___ Group of parents and provider, children not present
- [7] ___ Child & parents with provider
- [8] ___ Group of families with provider
- [9] ___ Child & parents, no provider (self-administered treatment)
- [10] ___ Teachers, treatment professional, no children
- [11] ___ Parents alone (self-directed)
- [12] ___ Multiple format program; no focal format
- [88] ___ N/A: control group

Focal Treatment Implementation/Length/Integrity

[G20] Duration of treatment. Approximate (or exact) number of weeks that subjects received treatment, from first treatment event to last excluding follow-ups designated as such. Divide days by 7; multiply months by 4.3. Code 777 if a control group that receives nothing Code 999 if cannot tell. Estimate for this item if necessary, and if you can come up with a reasonable order of magnitude number. Use school year conversions listed below.

[G22] Approximate (or exact) frequency of contact between subjects and provider or treatment activity. This refers only to the element of treatment that is different from what the control group receives.

1. less than weekly
2. Once a week
3. 2 times a week
4. 3-4 times a week
5. daily contact (not 24 hours of contact per day but some treatment during each day, perhaps excluding weekends)

- 6. continuous (e.g. residential living)
- 9. cannot tell
- 88. N/A: control group

[G24] _____ Approximate (or exact) mean hours actual contact time between subject and provider or treatment activity per week if reported or calculable. Assume that high school classes, counseling, or therapy sessions are an hour unless otherwise specified. Round to one decimal place. Code 7777 for control groups that receive nothing; 8888 for institutional, residential, or around the clock program; code 9999 if not available. Use school year conversions listed below.

[G26] _____ Approximate (or exact) mean number of hours total contact between subject and provider or treatment activity over full duration of treatment per subject if reported or calculable. Round to whole number. Code 7777 for control groups that receive nothing; 8888 for institutional, residential, or around the clock program; code 9999 if not available. Use school year conversions listed below.

[G28] Were there additional untimed treatment components that were not included in the dosage estimates given above? For example, these could be wrap-around or other diffuse services like case management that aren't presented in enough detail in the study reports to estimate dosage.

- 1 Yes
- 2 No

[G51] Were the dosage estimates given above for a treatment program that was delivered to significant others of the target subjects, rather than the subjects themselves?

- 1 Yes
- 2 No

School Year Conversions

Length of School Year (i.e., duration) (approx):

1 school year = 4 quarters = 2 semesters = 9 months = 38.7 weeks = 271 days

Actual TIME IN CLASS (i.e., contact) (approx.):

1 school year = 8.4 months = 36 weeks = 180 days

1 semester = 4.2 months = 18 weeks = 90 days

1 quarter = 2 months = 9 weeks = 45 days

Misc. Conversions

Hours in a school day for STUDENTS: 6.5 - 7 hours

Hours in a school day for TEACHERS: 8 hours

Typical class period (High School): 45 min- 1 hour

Typical therapy session (counseling): 1 hour

Instructional Days per School Year

U.S. public schools average	180 days
States with less than 180	Arkansas, Colorado, Illinois, Kentucky, Louisiana, Maine, Missouri, North Dakota, Oklahoma, Vermont, Wyoming (average 174 days)
States with more than 180	Kansas (K-11, 186 days; Grade 12, 181 days) and Ohio (182 days)
U.S. private schools average	180.4 days
International average	193 days
Korean average	225 days
Japanese average	223 days
Chinese average	221 days
Australian average	196 days
Russian Federation average	195 days
Netherlands average	191 days
English average	190 days
Canadian average	188 days

Instructional Hours per School Year

(Eight U.S. states do not set a minimum number of instructional days; instead they set number of instructional hours.)

Delaware	Grades 1-11 (1060 hours); Grade 12 (1032 hours)
Idaho	Grades 1-3 (810 hours); Grades 4-8 (900 hours); Grades 9-12 (990 hours, includes 22 hours for staff development)
Michigan	1080 hours
Montana	Grades K-3 (720 hours); Grades 4-12 (1080 hours, 1050 for graduating seniors)
Nebraska	Grades 1-8 (1032 hours); Grades 9-12 (1080 hours)
Oregon	Grades 1-3 (810 hours); Grades 4-8 (900 hours); Grades 9-12 (990 hours, seniors hours may be up to 30 less)
South Dakota	Grades 4-12 (962.5 hours)
Virgin Islands	1080 hours
Average – all grades	861 hours
Average – high schools	1040 hours
Average of states with day and hour minimums	
All grades	879 hours

High schools	1099 hours
--------------	------------

- If the intervention is only one class (math, study skills, etc.) just count one hour per day for the dosage.
- If the whole school experience in the intervention (alternative school, smaller classes, ect.) use the charts above to calculate dosage.

[G29] Monitored treatment implementation. Was the implementation of the program monitored by the author/researcher or program personnel to assess whether it was delivered as intended?

- 1 Yes. Do not infer that monitoring happened. Select “yes” ONLY if the report specifically indicates that implementation was monitored.
- 0 No
- 9 Cannot Tell

[G30] Based on evidence or author acknowledgment, was there any uncontrolled variation or degradation in implementation or delivery of treatment, e.g., high dropouts, erratic attendance, treatment not delivered as intended, wide differences between settings or individual providers, etc.? Assume that there is no problem if one is not specified.

This question has to do with variation in treatment delivery, not research contact. That is, there is no “dropout” if all subjects complete treatment, even if some fail to complete the outcome measures.

- 1 yes (describe below)
- 2 possible (describe below)
- 3 no, apparently implemented as intended

[G31] Implementation Monitoring Procedures and Problems. Describe any implementation problems or issues mentioned by the authors. Also describe any procedures used to monitor implementation fidelity.

Subject Characteristics

[G40] Gender composition of group.

1. no males (<5%)
2. some males (<50%)
3. 50% to 60% male
4. mostly males (>60%)
5. all males (>95%)
9. cannot tell

[G42] Enter percent male: _____ (use decimal rather than whole number, i.e., .42 NOT 42%)

ETHNICITY CODING (Code 9999 if you cannot tell)

[G43a] Percent white.

[G43b] Percent black

[G43c] Percent Hispanic

[G43d] Percent other minority

[G43e] Percent non-white (ONLY use this category if specific minority groups are not mentioned; if you use this category, there should only be numbers in the white and non-white categories)

Rankings: 1=clear majority; 2=present but proportion unknown; 3=clear minority; 0=not present.

[G44a] White rank

[G44b] Black rank

[G44c] Hispanic rank

[G44d] Other minority rank

[G44e] Non-white rank (ONLY use this category if specific minority groups not mentioned; if you use this category, there should only be numbers in the white and non-white categories)

[G45] Describe others and/or non-whites: _____.

[G46] Enter the average age of the sample using number of years. Enter 9999 if you cannot tell.

[G46a] and **[G46b]** High and low age using years. Enter 9999 if you cannot tell.

[G47] Enter the average grade level of the sample. (dropdown menu)

[G47a] and **[G47b]** High and low grades (dropdown menu)

[G48] Predominant level of “risk” of youths in the sample:

_____. Think of the reason that the subjects in this group ended up in this group; did the researchers select potential dropouts for treatment; if yes, how were the potential dropouts identified?

[G49] Socioeconomic status: Type in a brief description of the socioeconomic composition of the sample. This might include information on the percentage of children eligible for free lunches, the income level of the children’s parents, or a description of poverty in the community. Quote or closely paraphrase the relevant descriptive information in the report.

[G50] Please describe any problems you encountered while coding this record.

DROPOUT PROJECT CODING MANUAL
DEPENDENT VARIABLES CODING

Select the general construct group for the dependent variable you are coding, then select the specific construct category that best matches the dependent variable.

[DV5] Describe construct.

[DV1] Construct Group

- 100. Dropout
- 101. Attendance, truancy
- 102. Academic performance
- 103. School conduct
- 104. School engagement

[DV2] Specific Construct

Dropout

- 200. Dropout
- 201. Graduation
- 202. GED completion
- 203. Enrolled in post-secondary education
- 221. Graduated OR obtained GED

Attendance

- 204. Absences/truancy
- 205. Tardies
- 206. Attendance
- 222. Enrolled in high school; attending or not attending

Academic performance

- 207. GPA, grades
- 208. Standardized test scores
- 209. Academic track
- 210. Grade retention
- 211. Unstandardized, generic academic achievement score
- 220. Academic credits (# earned, average credits)

School conduct

- 212. Suspensions
- 213. Expulsions
- 214. Detention
- 215. Classroom behavior

School engagement

- 216. School self-concept
- 217. Academic expectations/goal setting
- 218. Attitude toward school/school bonding
- 219. Attitude toward teachers

Employment

- 223. Any employment (part-time, full-time)
- 224. Full-time employment
- 225. Hours worked per week

[DV3] Source of information. Who provided the information for this dependent variable?

- 1. Participants, self-report
- 2. Parents
- 3. Peers
- 4. Teachers
- 5. Principal
- 6. Therapist/Service Provider (treatment agent)
- 7. School Records
- 8. Researcher or interviewer
- 9. Involved other (not treatment agent, not researcher), e.g., school counselor.
- 10. Multiple sources, cannot tell which is dominant
- 99. Cannot tell

[DV4] Type of Measure.

- 1. Survey, questionnaire, or interview
- 2. Standardized test (e.g., standardized achievement test)
- 3. School records
- 4. Other: _____
- 9. Cannot Tell

[DV6] Time period covered by this dependent variable.

_____ Total number of weeks over which the information presented in this dependent variable was counted. This question applies mainly to variables like attendance that are continuously counted and thus might be presented in study reports as: number of absences in the past month (you would code 4.3 weeks for this) or weekly attendance over the past semester (you would code the number of weeks in the semester). Measures like dropout or graduation, which are measured at discrete time points and do not cover a certain time period, should be coded as 888 for not applicable.

If you have two measures of the same construct (such as attendance) that have different time period coverage, then you must create two separate dependent variables.

DROPOUT PROJECT CODING MANUAL

BREAKOUT/SUBGROUP CODING

Breakouts are comparisons involving subgroups of an aggregate treatment and/or control group. For example, the males in a treatment group might be compared with the males in a comparison group, or pretest-posttest results might be presented for males and females separately. Each variable (e.g., gender, age) by which the aggregate group(s) are subdivided constitutes one breakout, and each value of that variable defines one subgroup; i.e., a males vs. females stratification is one breakout (gender) with two subgroups, one male and one female. If only the male subgroup is reported, there is still one breakout, but only one subgroup.

Note that a simple report of the number of males and females in the treatment and control groups does not constitute a breakout (though it is relevant to group equivalence issues). To be a breakout, outcome data must be reported for the treatment-control or pretest-posttest comparison for at least one subgroup of the breakout variable. Breakouts are usually presented because the authors think that subgroups (e.g., males and females) are sufficiently different to warrant separate presentation of results (because, for example, males may exhibit more aggressive behaviors than females).

NOTE: Only certain breakout variables are eligible for coding. These include gender, age, ethnicity, and prior school completion/dropout, GED completion, or absences/truancy. If you encounter another breakout variable that may be relevant to dropout, please check with Sandra.

Create a new record for each subgroup that you will be coding for this study.

[StudyID] Study ID for the study you are coding.

[BreakID] Subgroup number. Assign a number to the subgroup such that the first subgroup you code is numbered 1, the second is numbered 2, and so on. These numbers are used within a study, so when you code subgroups from another study, you would start over with 1 again.

[Labels:B2] Write in descriptor for the subgroup you are coding, e.g., males, 8 year olds, whites, etc.

DROPOUT PROJECT CODING MANUAL

EFFECT SIZE CODING

Although this is the final section of coding, it is a good idea to identify at least one codable effect size before you start coding a study, because studies that appear eligible frequently end up presenting data that cannot be coded into an effect size.

This portion of coding requires familiarity with some basic statistics, including means, standard deviations, proportions, t-tests, chi-squares, ANOVA (or F-tests), and the like.

Step 1. General Information

[StudyID] Type in the appropriate StudyID if it does not appear automatically.

[ReportID] Report ID for this effect size. Indicate the report number (e.g., 2098.01) for the report in which you found the information for this effect size. This is important so that we can find the source information for the effect sizes later on, if necessary, and is especially important for studies with multiple reports.

[ESID] Effect size ID. FileMaker will automatically generate unique effect size ID numbers ACROSS studies.

[pagenum] Page number for this effect size. Indicate the page number of the report identified above on which you found the effect size data. If you used data from two different pages, you can type in both, but use a comma or dash between the page numbers.

There are 3 types of effect sizes that can be coded: pretest, posttest, and group equivalence (or baseline similarity) effect sizes. They are defined as follows:

- **Pretest effect size.** This effect size measures the difference between a treatment and comparison group before treatment (or at the beginning of treatment) on the same variable used as an outcome measure, e.g., school attendance measured before the treatment begins is used as a pretest for school attendance measured the same way after the treatment ends.
- **Group equivalence effect size.** Group equivalence effect sizes are used to code the equivalence of two groups prior to treatment delivery on variables that might be related to outcome. See the Group Equivalence Coding section for more information.
- **Posttest effect size.** This effect size measures the difference between two groups after treatment on some outcome variable.

This is very important!!!! These three types of effect sizes are different from the multiple breakouts and multiple dependent variables that you might have in a study. For example,

you might have a study that measures the treatment and comparison groups at pretest and posttest at 6 months after treatment on 3 different dependent variables. The results might be presented for the entire sample and broken down by gender. In this case you would have 6 group comparison effect sizes for the entire sample – three for the pretest and 3 for the 6 month posttest (the three is for your three dependent variables). In addition to these 6 aggregate effect sizes, you will have 6 more for the girls (the same as for the aggregate groups but just for the subgroup of girls) and 6 for the boys (also the same as for the aggregate groups but just for the subgroup of boys).

[ES24] Type of effect size:

- 1 Pretest (for treatment-control baseline comparison on a dependent variable)
- 2 Posttest (for the first treatment-control outcome comparison on a dependent variable)
- 5 Group Equivalence (for baseline treatment-control comparisons on variables other than the dependent variables)

[ES19] Wave number. Pretests and group equivalence effect sizes always get a 1; each wave thereafter gets numbered consecutively, beginning with 1. Some studies involve more than one posttest measurement on the same dependent variable, and we need to be able to distinguish one from another. Give the first posttest after treatment a 1, the second a 2, and so on.

[ES47] Timing of measurement. Approximate (or exact) number of weeks after treatment when measure was taken. Divide days by 7; multiply months by 4.3. Enter 999 if cannot tell, but try to make an estimate if possible. Enter 0 if pretest. If posttest measurement occurred during an ongoing treatment, use 888 here. *[es47_ck]*

Step 2. Group Selection

[GroupID1] Group 1

If you are coding a treatment-control effect size, select the appropriate treatment group here. If you are coding a treatment-treatment effect size, select the focal treatment group here or, if neither is focal, select one here and the other as Group 2 below.

[GroupID2] Group 2

If you are coding a treatment-control effect size, select the appropriate control group here. If you are coding a treatment-treatment effect size, select the second of the two treatment groups here.

[BreakID] Select Breakout group if relevant.

Step 3. Dependent Variable Selection

[VarNo] Select the dependent variable for this effect size.

Step 4. Effect Size Calculation and Data Entry

It is now time to identify the data you will use to calculate the effect size and to calculate the effect size yourself if necessary (see below). Effect sizes can be calculated ONLY from data based on the number of subjects, e.g., average number of days absent per subject and the corresponding standard deviation) or proportion of subjects who were chronic truants during a given time period. Effect sizes can NOT be calculated from data based solely on the incidence of events, e.g., total number of days absent per group. THIS IS VERY IMPORTANT—BE SURE YOU KNOW WHICH KIND OF DATA YOU HAVE.

You need to determine what effect size format you will use for each effect size calculation. There are two general formats you can use, each with its own section in FileMaker:

1. Compute ES from means, sds, variances, test statistics, etc.
2. Compute ES from frequencies, proportions, contingency tables, odds, odds ratios, etc.

Also note that within each of the above effect size formats, effect sizes can be calculated from a variety of statistical estimates; to determine which data you should use for effect size calculation, please refer to the following guidelines in order of preference:

1. Compute ES from descriptive statistics if possible (means, sds, frequencies, proportions).
2. If adequate descriptive statistics are unavailable, compute ES from significant test statistics if possible (values of t, F, Chi square, etc.).
3. If significance tests statistics are unavailable or unusable but p value and degrees of freedom (df) are available, determine the corresponding value of the test statistic (e.g., t, chi-square) and compute ES as if that value had been reported.

Note that if the authors present both covariate adjusted and unadjusted means, you should use the covariate adjusted ones. If adjusted standard deviations are presented, however, they should not be used.

[ES17] Which group is favored?

Select the group that has done “better”:

- 1 Treatment
- 2 Control
- 3 Neither, Exactly Equal
- 4 Cannot tell

For treatment-control comparisons, the treatment group is favored when it does “better” than the control group. The control group is favored when it does “better” than the treatment group.

Remember that you cannot rely on simple numerical values to determine which group is better off. For example, a researcher might assess the attendance and report this variable in terms of the average number of absences in the last semester. Fewer absences are better than more, so in this case a lower number, rather than a higher one, indicates a more favorable outcome.

Sometimes it may be difficult to tell which group is better off because a study uses multi-item measures in which it is unclear whether a high score or a low score is more favorable. In these situations, a thorough reading of the text from the results and discussion sections usually can bring to light the direction of effect – e.g., the authors will often state verbally which group did better on the measure you are coding, even when it is not clear in the data table. Note that if you cannot determine which group has done better, you will not be able to calculate a numeric effect size. (You will still be able to create an effect size record—just not a numeric effect size.)

[ES23] Effect size derived from what type of statistics?

1. Means and SDs; means and variances; means and standard errors
2. N successful/unsuccessful (frequencies)
3. Proportion successful/unsuccessful (percentage successful or not)
4. Multi-category (polychotomous) frequency or %
5. Independent t-test
6. One-way ANOVA (2 groups, 1 degree of freedom)
7. One-way ANOVA (>2 groups, >1 degree of freedom)
8. Covariance Adjusted (ANCOVA)
9. Chi-square statistic (1 degree of freedom; from 2x2 table)
10. Correlation coefficient (zero-order)
11. Hand calculated ES
17. Effect sizes as reported directly in the study
18. Other (please specify)

[ES50] For this effect size, did you use adjusted data (e.g., covariate adjusted means) or unadjusted data? If both unadjusted and adjusted data are presented, you should use the adjusted data for the group means or mean difference, but use unadjusted standard deviations or variances. Adjusted data are most frequently presented as part of an analysis of covariance (ANCOVA). The covariate is often either the pretest or some personal characteristic such as socioeconomic status. If you encounter data that is adjusted using something other than a covariate, please see Sandra or Mark.

- 1 Unadjusted data
- 2 Pretest adjusted data (or other baseline measure of an outcome variable construct)

- 3 Data adjusted on some variable other than the pretest (e.g., socioeconomic status)
- 4 Data adjusted on pretest plus some other variables

[ES22] Confidence in effect size calculation

- 1 High Estimate (e.g., have N and crude p values only, e.g., $p < .10$, and must reconstruct via rough t-test equivalence)
- 2 Moderately Estimated (e.g., have complex but relatively complete statistics, e.g., multiple regression, LISREL, multifactor ANOVA, etc. as basis for estimation)
- 3 Some Estimation (e.g., have unconventional statistics and must convert to equivalent t-values or have conventional statistics but incomplete, such as exact p values only)
- 4 Slight Estimation (e.g., must use significance testing statistics rather than descriptive statistics, but have complete statistics of the conventional sort, such as a t-value or F-value)
- 5 No Estimate (e.g., have descriptive data: means, sds, frequencies, proportions, etc.; can calculate an ES directly.)

[ES44] Significance information for this comparison.

For treatment-control and treatment-treatment comparisons: Did the authors provide any information about the statistical significance of the difference between the two groups you selected on the dependent variable you selected for the time point you have selected for this comparison? Sometimes authors will state that a particular comparison was not significant, but not provide any calculable effect size data. In these cases, you should select “5” for this item. The effect size field should remain blank. In other cases, authors will state that a particular comparison was significant, but not provide any calculable effect size data. In these cases, you should select “4” for this item. Again, the effect size field should remain blank. NOTE: the last three options (4, 5, and 6) are for cases for which you have direction (i.e., you know which group is favored) but no effect size information.

- 1 Significant result, ES data below
- 2 Non-significant result, ES data below
- 3 Significance not reported, ES data below
- 4 Significant result, no ES data
- 5 Non-significant result, no ES data
- 6 Significance not reported, no ES data

[ES55] Intent-to-treat analysis: Are results for this effect size based on an intent-to-treat analysis?

Experimental and quasi-experimental designs may employ “intent-to-treat” (ITT) or “completer” analyses. An intent-to-treat analysis is one that (attempts to) includes all randomized subjects in the groups to which they were randomly assigned, regardless of

the compliance with the entry criteria, the treatment they actually received, or any subsequent withdrawal from treatment or deviation from the protocol. A true ITT is possible only when the authors (attempt to) use outcome data for all randomized subjects; if all assigned subjects are used to present outcome results, then code as ITT, regardless of whether authors call the analysis an ITT. If the authors attempt to collect outcome data on non-completers and even if they are not 100% successful in this attempt, still code as ITT (as the missing data for non-completers is due to attrition). Sometimes researchers will use a modified ITT, in which they estimate missing data on non-completers, or include all subjects with pretests but not all who were randomized. These modified ITTs would be coded as “2” below. Completer analyses (AKA ‘per-protocol’, ‘efficacy’, or ‘exploratory’ analyses) involve only the subjects who stayed in the study, or only those who completed treatment.

- 1 Intent-to-treat analysis (all subjects who were assigned are used in posttest)
- 2 Modified intent-to-treat (not all assigned subjects are used in posttest, but authors have done some modifications to approximate a true ITT)
- 3 Completer analysis (only those subjects who completed treatment or who stayed in the study are used in posttest)

Assigned and Observed N

Assigned N, Observed N. These fields refer to the number of subjects who were originally assigned to the group(s) involved in this effect size (Assigned N) and to the number of subjects who were actually “observed” or “measured” (Observed N). If you cannot tell how many subjects were originally assigned to a group, look at the number of subjects (Observed N) at pretest; you can frequently use pretest sample sizes for assigned N. However, in cases where the authors have removed the subjects who do not have both pretest and posttest measures (such that the pretest N and the posttest N are the same), do not assume that the number of subjects at pretest is the correct number for Assigned N and, instead, leave this field blank. In cases where there is no attrition, the Assigned N is the same as the Observed N. Only use the same numbers for Assigned N and Observed N when you are SURE that there is no attrition.

[ES36] Assigned N for the treatment group (or pretest, if this is a pretest-posttest effect size).

[ES37] Assigned N for the comparison or second treatment group (or posttest, if this is a pretest-posttest effect size; if this is a pretest-posttest effect size, this value should be the same as the assigned N for the pretest).

[ES38] Total Assigned N.

[ES1] Observed N for the treatment group (or pretest, if this is a pretest-posttest effect size).

[ES2] Observed N for the comparison or second treatment group (or posttest, if this is a pretest-posttest effect size).

[ES3] Total Observed N.

Other Effect Size Data Fields

[ES9] Mean for treatment group
 [ES10] Mean for comparison group
 [ES11] Difference in group means
 [ES12] Standard deviation for treatment group
 [ES13] Standard deviation for comparison group
 [ES14] Pooled sd
 [ES31] N successful for treatment group
 [ES32] N successful for comparison group
 [ES33] N failed for treatment group
 [ES34] N failed for comparison group

[ES4] Dependent t-value
 [ES5] Independent t-value
 [ES6] χ^2 (df=1)
 [ES20] Effect size reported by authors
 [ES60] Odds ratio reported by authors

Final Effect Size Determination

[ES21] Effect size value- standardized mean difference
 [ES81] Effect size value- odds ratio

Remember that you cannot rely on simple numerical values to determine which group has done better. For treatment-control comparisons, a positive effect size should indicate that the treatment group did “better” on the outcome measure than the comparison group, while a negative effect size indicates that the comparison group did “better” than the treatment group, and a zero effect size means that the two groups are exactly equal on the measure. For single-group pretest-posttest comparisons, a positive effect size indicates that the group did better at posttest than at pretest, while a negative effect size indicates that the group did better at pretest than at posttest, and a zero effect size means that the group’s performance was exactly equal at the two time points.

You must make sure that the sign of the effect size matches the way we think about direction, such that the effect size is positive when the treatment group (or posttest) is better and negative when the comparison group (or pretest) is better.

Effect sizes can range anywhere from around -3 to $+3$. However, you will most commonly see effect sizes in the -1 to $+1$ range.

Note: If the authors report an effect size, include that in your coding and use it for the final effect size value if no other information is reported. However, if the authors also

include enough information to calculate the effect size, always calculate your own and report it in addition to that reported in the study.

[ES39] Any problems coding this effect size?

VARIABLES TO EXPORT FROM FILEMAKER

EFFECT SIZES

StudyID
ESID
ReportID
Pagenum
ES24
VarNo
GroupID1
GroupID2
BreakID
ES19
ES47
ES47_ck
ES17
ES23
ES50
ES22
ES44
ES55
ES36
ES37
ES38
ES1
ES2
ES3
ES9
ES9_ck
ES10
ES10_ck
ES11
ES11_ck
ES12
ES12_ck
ES13
ES13_ck
ES14
ES14_ck
m
ES5
ES5_ck

ES21
ES21_ck
ES31
ES31_ck
ES32
ES32_ck
ES33
ES33_ck
ES34
ES34_ck
ES82
ES83
ES84
ES85
ES86
ES71
ES71_ck
ES72
ES72_ck
ES73
ES73_ck
ES74
ES74_ck
ES75
ES75_ck
ES76
ES76_ck
ES6
ES6_ck
ES81
ES81_ck
ES25
ES15

DV
StudyID
VarNo
DV5
DV1
DV2
DV3
DV4
DV6

Breakout

StudyID
BreakID
B1
B2:Labels

Groups

StudyID
GroupID
G60
Teenage parent programs
G1
G2
G5
G3
G4
G6
G6 individual items
G6_46a
G9
G10
G11
G12
G13
G14
G15
G16
G16 individual items
G20
G20_ck
G22
G24
G24_ck
G26
G26_ck
G28
G51
G29
G30
G31
G40
G42
G42_ck
G43a
G43a_ck
G43b

G43b_ck
G43c
G43c_ck
G43d
G43d_ck
G43e
G43e_ck
G44a-e
G45
G46
G46_ck
G46a
G46a_ck
G46b
G46b_ck
G47
G47a
G47b
G48
G49

Header
StudyID
Coder
H1
CodeDate
H2
H3
H6
H7
H8
H9
H10
H11
H12
H13
H14
H16

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VITA

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