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TESTING MEDIATED EFFECTS OF A SEX EDUCATION PROGRAM ON YOUTH SEXUAL ACTIVITY

by

Paul James Birch

A dissertation paper submitted in partial fulfillment
of the requirements for the degree

of

DOCTOR OF PHILOSOPHY

in

Psychology

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UTAH STATE UNIVERSITY
Logan, Utah

2011

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ABSTRACT

Testing Mediated Effects of a Sex Education Program

on Youth Sexual Activity

by

Paul James Birch, Doctor of Philosophy

Utah State University, 2011

Major Professor: Dr. Scott C. Bates
Department: Psychology

Empirical investigations have identified hundreds of factors that predict whether youth engage in sexual activity (YSA). To promote optimal health and the avoidance of unhealthy or problematic outcomes that can result from YSA, sex education programs have been extensively developed and evaluated. Many evaluations have identified the effect of the program on immediate outcomes such as attitudes and intentions, others have examined subsequent behavioral and health outcomes, and some have done both. The purpose of this study was to extend the evaluation literature by testing a mediated effects model. A sex education program was found to have significant immediate effects on several attitudinal factors that have been shown to predict YSA, and was shown to significantly reduce the incidence of sexual activity approximately one year after the program ($OR = 0.534$, $p = .004$). A mediating effects test showed that youth's stated intentions to engage in sexual activity was a significant mediated effect ($B = -0.182$,

Lower *CI* = -0.291, Upper *CI* = -0.073), suggesting that the program effects on sexual activity occurred through the immediate effect on intentions, which in turn was likely affected by program content, which changed other attitudinal factors such as values, efficacy, and knowledge. Using immediate changes on these mediating factors to predict the likelihood of YSA showed that accurate prediction was possible, with an overall prediction accuracy rate of 74%. It was easier to predict who was not going to engage in YSA (94% accuracy) than who would (35% accuracy). Further predictive analyses showed that a score of 4.12 (on a scale of 1 to 5) on agreement with the items comprising the mediating factors' scales was a threshold point, with the likelihood of engaging in YSA rising sharply as a function of this score until that point, and score increases above that point resulting in minimal changes in the probability of YSA. The results of this study demonstrate that it is possible to reduce YSA, that intent to engage in YSA was a primary mediator, and that accurate prediction of eventual behavioral results is possible, based on analysis of immediate results.

(102 pages)

DEDICATION

This work is dedicated to public interest in doing all we can to improve the lives of adolescents as they prepare for adulthood.

ACKNOWLEDGMENTS

There are many who deserve special acknowledgment. First, I acknowledge the careful, supportive, and consistent help of my major professor, Dr. Scott C. Bates. Vice President for Research Brent Miller said, “Scott Bates [stands] out in his outstanding dedication to his students. He is a natural advocate for undergraduate research” (http://www.usu.edu/ust/index.cfm?article=48761). I could not agree more.

Second, I acknowledge Dr. Karl White. I named him to my committee because of his reputation as an uncompromising scientist who would hold me to a quality project. While a bit nervous about that choice, he has delivered, holding me accountable for high standards and yet compassionately helping me to fulfill the requirements of this degree.

My three other committee members were gracious, responsive, and helpful. They made the product better but more importantly, made the process enjoyable.

To Stan Weed. Thanks for your untiring dedication to quality work, to the vision and priorities you instilled in me, and for providing the support to complete this project.

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Paul James Birch

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CHAPTER I

STATEMENT OF THE PROBLEM

Eight percent of U.S. teenage girls become pregnant each year (Guttmacher Institute, 2006). Adverse consequences often result, such as lowered education (Hofferth & Reid, 2002), substantial economic disparities, tax costs (Maynard, 1997), and increased risk of serious problems for the child such as drug abuse, gangs, and crime (Jaffee, 2002). Sexually transmitted diseases (STDs) are also a substantial problem for youth, with one in four U.S. teens having an STD and rates rising rapidly in recent years (Centers for Disease Control [CDC], 2001, 2008a, 2008b). STDs result in minor and serious negative health outcomes including chronic pelvic pain, infertility, cancer, and in some cases death (American Social Health Association, 1998; CDC, 2001; National Institute of Allergy and Infectious Diseases [NIAID], 2001; Sulack, 2003). The cost of STDs in the U.S. is estimated at $6.5 billion annually (Chesson, Blandford, Gift, Tao, & Irwin, 2004).

About 65% of youth have had vaginal sexual intercourse (VSI) by the end of high school (CDC, 2008a, 2008b; Eaton et al., 2006). Yet youth are more likely than adults to experience negative consequences of sexual activity such as STDs and pregnancy (CDC, 2001, 2008b). Public health efforts have thus emphasized the importance of delaying sexual debut among youth, with those efforts being generally classified as either "comprehensive sexuality education" (CSED) or "abstinence-centered education" (ABED).

Both approaches raise awareness of negative outcomes and educate youth to eliminate or reduce their risk of experiencing those outcomes. The main difference seems

to be the relative emphasis on abstinence and condom use. CSED stresses abstinence by endorsing it as an important and viable option, but emphasizes the importance of correct and consistent condom use, including efforts to increase access to and awareness of how to use them. ABED primarily emphasizes the importance of abstinence until marriage as the best way to reduce negative outcomes. There is a long history of discussion about which of these approaches ought to be used (Bogle, 2008; Cook, 2003; Grossman, 2006; Grossman, 2009; Luker, 2006; McDowell, 2002; McIlhaney & Bush, 2008; Santelli & Kantor, 2008; Santelli, Ott, Lyon, Rogers, & Summers, 2006; Smith, 2007; Stepp, 2008; Weed & Olsen, 1998; Wilcox, 2008). The current status of those debates is best reflected in recent changes where federal funding for ABED programs was redirected to new and substantial outlets largely supporting CSED programs with research evidence of success (U.S. Department of Health and Human Services, 2010).

However, though many rigorous studies have shown statistically significant effects, few programs have demonstrated very large or lasting results (Institute for Research and Evaluation, 2009). Little is known about why these programs did work, making it hard to improve on those small effects that have been found (Kirby, 2007; MacKinnon, 2008). Given the strong calls from some to abandon ABED (Kirby, 2006, 2007; Santelli & Kantor, 2008; Santelli et al., 2006; Smith, 2007), largely based on critiques of ineffectiveness, there is a particular need to test ABED programs. And given the high rate of sexual activity among U.S. youth, it may also be valuable to test whether such programs reduce sexual activity of those who have already begun having sex.

Needed Research

We need to identify better programs, understand their effects on both sexually inexperienced and sexually active youth, and we must understand what factors mediate those effects so that those factors can be addressed by increasing numbers of programs. With knowledge of such mediating factors, programs will be able to predict long term effects based on immediate measures of program impact, allowing them to make changes and achieve peak performance. Once such peaks are attained, longer-term experimental evaluations can be employed to show the hopefully larger effects attainable by programs that develop in this way.

Purpose

The purpose of this paper was to examine the effects of an ABED program on YSA, both among sexually inexperienced and experienced youth, and in so doing, improve on past evaluation methods in this area by testing a mediated effects model. The predictive value of the identified mediating factors is then demonstrated.

CHAPTER II

LITERATURE REVIEW

To inform the development of the current study, literature on what is known about effective programs is reviewed, with emphasis on identifying possible methodological improvements. Second, literature about factors related to YSA is reviewed to identify potential mediators of program effects.

Program Effectiveness Research

The literature is replete with research on YSA and on programs designed to affect it (Kirby, 2007). For purposes of this study, research efforts in this area are reviewed to provide the context for the evaluation conducted in this study. First, a general review of research on sex education effectiveness is provided. Second, meta-analytic evidence is reviewed. Finally, evidence relating to mediated effects tests is reviewed.

General Review

A review of early sex education efforts, both CSED and ABED (DiCenso, Guyatt, Willan, & Griffith, 2002) concluded that there was no evidence from randomized trials that primary prevention programs affect sexual initiation, use of birth control, or pregnancies.

Since then, a large body of studies of CSED programs have shown significant impacts on factors such as frequency of condom use, using a condom at first intercourse, intention to use condoms, whether a condom is carried on person or not (Kirby, 2006).

There are a few CSED programs that have shown significant effects on delaying initiation of or reducing frequency of YSA (Kirby, 2006). This represents a promising step in that it appears possible to affect condom use and to a lesser extent, sexual activity with a CSED program. While there are many findings with statistically significant effects from well-designed studies, when the size, duration, and breadth of those effects are examined, there appears to be room for improvement. Recent reviews (Ericksen, Weed, & Osorio, 2010; Weed, Ericksen, Birch, White, & Evans, 2007) report that only one CSED program studied has produced a significant difference in the rate of consistent condom use by youth for up to a year, with none showing such an effect for more than one year, and thirteen controlled trials showing no increase in youth condom use.

There are studies showing that ABED programs can reduce YSA (Borawski, Trapl, Lovegreen, Colabianchi, & Block, 2005; Denny & Young, 2006; Jemmott, Jemmott, & Fong, 1998, 2010; Weed, Anderson, & Ericksen, 2005; Weed, Ericksen, & Birch, 2004; Weed, Ericksen, Lewis, Grant, & Wibberly, 2008). However, there are also studies of programs that showed no effect on YSA (Kirby, 2006; Trenholm et al., 2007). Only one study measured whether an abstinence program affected the sexual behavior of those who are already sexually active (Borawski et al., 2005), where sexually active program youth had fewer instances of self-reported sexual intercourse and fewer partners than comparison youth during the evaluation period. Few ABED evaluations have measured condom use outcomes, with those showing no difference between program and control participants (Jemmott et al., 1998; Trenholm et al., 2007).

Meta-Analytic Evidence

Two attempts to conduct meta-analysis of the effectiveness of ABED programs failed to identify sufficient numbers of studies for strong meta-analytic techniques (Silva, 2002; Underhill, Montgomery, & Operario, 2007), reporting no overall effect for the programs.

A recent strong meta-analysis (CDC, 2010) reviewed evidence for CSED and ABED programs separately. Specifically, CSED programs were found to have significant effects on sexual activity (*OR* = 0.84, *95% CI* = 0.75 – 0.95), frequency of sex (*OR* = 0.81, *95% CI* = 0.72 – 0.90), number of partners (*OR* = 0.83, *95% CI* = 0.74 – 0.93), and unprotected sex (*OR* = 0.70, *95% CI* = 0.60 – 0.82). Abstinence programs showed a significant effect on sexual activity (*OR* = 0.81, *95% CI* = 0.70 – 0.94) and a favorable, but insignificant effect on frequency (*OR* = 0.77, *95% CI* = 0.57 – 1.04).

The researchers discounted this evidence on the basis of significantly larger effects found for quasi-experimental designs than for randomized trials, and because multiple studies were conducted by the same authors. The authors concluded that there was sufficient evidence to recommend CSED interventions, but insufficient to recommend ABED (CDC, 2010).

Two external consultants to the CDC project issued a minority report taking exception to the conclusions (Ericksen & Ruedt, 2009). The authors point out that the significant results found in the CSED programs included both community-based interventions delivered to high risk youth (e.g., youth at an STI testing clinic) and those in general risk populations receiving classroom-based curricula in a school setting. When

school-based CSED programs were evaluated on their own, no significant effect was found for sexual activity or condom use. Alternately, the ABED programs tested were mostly comprised of classroom programs provided to general populations. When ABED and CSED programs were statistically tested against one another, no significant effects were found between the two.

The value of the meta-analysis (CDC, 2010) is that it shows that sex education programs in general had significant effects on sexual activity levels, which is the focus of this study. It should further be noted that when the CSED programs that had significant effects on sexual activity were examined, they were found to be very similar in content to the ABED programs in terms of heavy emphasis on abstinence from sexual activity as the best way to mitigate risks of potential consequences of YSA. Thus, in an indirect way, the findings of this meta-analysis lend support to the idea that programs that encourage delayed YSA may be valuable. Yet, the overall modest size in reduction of sexual activity also supports a continued emphasis to learn more about why programs work and how to strengthen them.

Mediated Effects Tests

Among sex education program evaluations reviewed, many short-term outcomes that are related to sexual behavior were investigated by examining program effects on those outcomes from pre to post. Many program evaluations that reported behavioral effects also reported effects on those short-term outcomes. MacKinnon (2008) outlined a clear method for taking such information and conducting a test of the mediated effect, or the effect of the program on the outcome that can be explained via its effect on the

mediating factors. The effect of the hypothesized mediators on the outcome is tested along with the program effect on both those mediators and the outcome. The mediated effect is then tested by examining the amount of the difference between groups in behavioral outcomes that can be explained by the differences between the groups in the effects on the mediating outcomes. No study was found that empirically tested the mediated effect on those short-term outcomes.

There were four studies found that closely tested the role of mediators (Trenholm et al., 2007; Weed et al., 2004, 2005, 2008). These studies tested the relationship between the mediators and the outcome, tested effects on both the mediators and the outcome, but did not empirically test the mediated effect (MacKinnon, 2008). A key point to be taken from the review of these four studies is that they all measured nearly the same set of mediators (intention to engage in sex, values about sex, understanding possible consequences of sex, and efficacy to refuse sex) and that the studies that showed significant and sizeable effects on these mediators (the three Weed studies) all found a significant behavioral effect with the one study that showed no behavioral effect (Trenholm et al., 2007) also showed small and most insignificant effects on mediators. This suggests that these mediators may be important, yet without an empirical test of that relationship, it cannot be evaluated.

Summary of Program Effectiveness Research

In summary, while some progress has been made, sex education as an enterprise has not shown a consistently strong or durable effect on the ultimate outcomes they target. More importantly, none has produced large enough effects to leave few or no

youth at risk. While such a goal is lofty and perhaps unattainable, further research that can improve and build on past successes is warranted. Little is known about mediated effects that could be targeted for future development because studies have largely not tested for such effects.

Limitations of Current Program Effectiveness Research and Proposed Improvements

The purpose of the review of sex education effectiveness was to provide a context for the current evaluation by identifying ways to improve the evaluation methods. Three main limitations were identified.

Magnitude, internal validity, and breadth. While this body of literature has identified a *statistically significant* connection between programs and reduced risk of sexual behavior, it is unclear whether the *magnitude* or *durability* of those effects justifies the cost. Future research should continue to identify effective programs, replicate success of already proven programs, and use ongoing formative evaluation to maximize the size of the effect produced by the program. And most studies have focused on delaying initiation of YSA, with little information about how to affect sexual behavior of those who are already sexually active.

Failure to empirically test implied mediators. None of the sex education programs reviewed in this paper made any attempt to empirically determine whether these short-term outcomes were in fact acting as mediators or not. The ability to identify effective programs, and the mediating factors through which they produce their results would be strengthened if this previously logical argument was extended by providing an

empirical test of it, based on the methods reviewed in MacKinnon (2008).

Lack of targeted information. Without proven mediators at hand, program developers have difficulty telling if programs are really working unless behavioral outcomes—more expensive to obtain and requiring years to provide feedback—are measured. With more proven mediators available and knowledge of how well they predict behavior outcomes, developers could test different approaches and rely on pre-post effects on mediators to guide them in tuning programs to achieve peak performance.

Conclusion

Testing more programs for evidence of practically significant effects, empirically testing mediators, and providing information about the predictive ability of mediators would strengthen the evaluation literature.

Deriving Possible Mediators from the Literature

There is a large body of research that identifies correlates and predictors of youth sexual activity. Reviewing this research will show which factors may be candidates for being mediators of program effects on YSA. First, findings of an exhaustive review by Kirby, Lepore, and Ryan (2007) is reviewed. Second, related research is reviewed including a review of mediators targeted by effective programs, a body of research on mediated program effects, and a discussion of theoretical models that relate to the prediction of YSA.

Factors Affecting Likelihood of Youth Sexual Intercourse

Kirby and colleagues (2007) conducted an exhaustive review of correlates and predictors of YSA. Their review identified all studies done to date with sufficient methodological rigor and organized them into conceptual categories: (a) individual biological factors such as age of pubertal onset, (b) disadvantage, disorganization and dysfunction in multiple domains such as growing up in poverty; (c) sexual values, attitudes, and modeled behavior in multiple domains; and (d) connection to adults and organizations that discourage sex, unprotected sex or early childbearing (p. 15). Once categorized, they identified factors with the strongest evidence of being important predictors by evaluating several factors such as how many studies had shown the factor to be a significant predictor and whether it was identified with multivariate tests. Thus, a key contribution of this review is the categorical identification of factors with the strongest and most consistent evidence of predicting sexual behavior. The factors identified by their review as the best predictors within each category are now summarized.

Biological factors. Certain individual biological factors have been identified as strongly related to teen sexual behavior (Kirby et al., 2007). Specific factors that predict sex that have been identified in that review include being male (Benson & Torpy, 1995; Bishai, Mercer, & Tapales, 2005; Blum, Beuhring, Shew, Sieving, & Resnick, 2000; Dittus & Jaccard, 2000; Guttmacher Institute, 1994; Kinsman, Romer, Furstenberg, & Schwarz, 1998), older age (Abma & Sonenstein, 2001; Bearman & Bruckner, 1999, 2001; Bersamin, Walker, Fisher, & Grube, 2006; Guttmacher Institute, 1994), and having

younger pubertal onset (Berenson, Wiemann, & McCombs, 2001; Browning, Leventhal, & Brooks-Gunn, 2004; Capaldi, MCrosby, & Stoolmiller, 1996; Cheng & Udry, 2002; Dittus & Jaccard, 2000; Flannery, Rowe, & Gulley, 1993).

Disadvantage and disorganization. Several studies have pointed to disadvantage and disorganization factors that contribute to or reduce teen sexual behaviors. Key findings include living in communities with higher rates of hunger, violence, and substance abuse (Lackey & Moberg, 1998; Lanctot & Smith 2001; Upchurch, Aneshensel, Mudgal, & McNeely, 2001), and substance abuse of family member (Champion et al., 2004; Hillis, Anda, Felitti, & Marchbanks, 2001), which all predicted greater likelihood of sex, and living with both parents (Afxentiou & Hawley, 1997; Bearman & Bruckner, 1999, 2001; Blum et al., 2000; Brewster, 1994), parental education levels (Abma & Sonenstein, 2001; Baumer & South, 2001; Billy, Brewster, & Grady,1994; Blum, 2002; Carvajal et al., 1999), which all predicted lower likelihood of sex.

Sexual attitudes, values, etc. Certain sexual values and attitudes are significantly related to YSA, especially when important individuals model them. Key findings in this area include frequency of parental conversations about sex predicting lower sexual activity (East, 1996; Whitaker & Miller, 2000) and peers and best friends who are sexually active predicting higher sexual activity (Bersamin et al., 2006; Black, Ricardo, & Stanton, 1997; East, Felice, & Morgan, 1993; Little & Rankin, 2001; Lock & Vincent, 1995; Loewenstein & Furstenberg, 1991). Key personal attitudes that have been shown to be related to low sexual activity include values about abstinence (Blinn-Pike, Berger,

Hewett, & Oleson, 2004; Jimenez, Potts, & Jimenez, 2002), intentions to abstain (Kinsman et al., 1998), lack of rationalizations about sex (Dittus, Jaccard, & Gordon, 1999; Sieving, Eisenberg, Pettingell, & Skay, 2006), perception of risks of sex (Cuffee, Hallfors, & Waller, 2007; DiIorio et al., 2001; Miller, 2003; Robinson, Telljohann, & Price, 1999; Santelli et al., 2004), and self-efficacy to refrain from sexual activity (Chewning et al., 2001; Kasen, Vaughan, & Walter, 1992; Robinson, Price, Thompson, & Schmalzried, 1998, Robinson et al., 1999). Permissive attitudes towards premarital sex also predict early initiation (Blinn-Pike et al., 2004; Carvajal et al., 1999; Forste & Haas, 2002; Lackey & Moberg, 1998; Lock & Vincent, 1995; Loewestein & Furstenberg, 1991; Miller, Christensen, & Olson, 1987; Teitler & Weiss, 2000; Whitbeck, Simons, & Kao, 1994), frequency of sex (Benda & DiBlasio, 1994; Jemmott & Jemmott, 1990; Ku et al., 1998; Loewenstein & Furstenberg, 1991), number of sexual partners (Jemmott & Jemmott, 1990; Milhausen et al., 2003), use of condoms (Milhausen et al., 2003), and pregnancy (Adolph, Ramos, Linton, & Grimes, 1995).

Adult connections. Finally, connection to adults or organizations that discourage risk behaviors predict lower sexual activity, including factors such as feeling connected to school (Baumer & South, 2001; Bearman & Brueckner, 2001; Bersamin et al., 2006; Hellerstedt, Peterson-Hickery, Rhodes, & Garwick, 2006; McBride et al., 1995), involvement in school clubs (Halpern, Joyner, Udry, & Suchindran, 2000; Miller, Sabo Farrell, Barnes, & Melnick, 1998), and strong religious affiliations and attendance (Baumer & South, 2001; Bearman & Brueckner, 2001; Billy et al., 1994; Day, 1992; Halpern et al., 2000; Hardy & Raffaelli, 2003).

Factors predicting frequency of sex. Relatively less research exists in the domain of factors that influence whether sexually experienced youth have sex subsequent to their initiation of sex. Kirby and colleagues (2007) reviewed these studies as well. Key factors predicting recent sex included whether substance abuse was occurring in the home (Malo & Tremblay, 1997; Newcomb, Locke, & Goodyear, 2003), parental disapproval of premarital sex (Benda & DiBlasio, 1994; Dittus & Jaccard, 2000; Dittus et al., 1999; Jaccard & Dittus, 2000; Jaccard, Dittus, & Gordon, 1996; Miller, Forehand, & Kotchick, 1999), higher-quality family interactions (Anda et al., 2001; DiBlasio & Benda, 1990; Jaccard et al., 1996; Lauritsen, 1994; Miller et al., 1998; Sabo, Miller, Farrell, Melnick, & Barnes, 1999), permissive attitudes towards premarital sex (Benda & DiBlasio, 1994; Jemmott & Jemmott, 1990; Cooper, Shaver, & Collins, 1998; Ku et al., 1998; Loewenstein & Furstenberg, 1991), belief that sex is okay if there is a plan to marry (Ku et al., 1998), and perceived benefits of having sex (Benda & DiBlasio, 1994).

Summary. There are numerous factors, in different domains, that predict sexual activity among youth. The work of Kirby and colleagues (2007) advanced our understanding of which factors are most important by identifying those with strongest evidence of being significant predictors. In particular, those factors identified as sexual attitudes and values may be the best target for sex education programs because they are the most easily targeted by such programs.

Research Pointing to Potential Mediating Factors

Kirby and colleagues' (2007) framework. In addition to identifying factors with

the best evidence being related to YSA, Kirby and colleagues (2007) further proposed a framework for organizing individually proven factors which moves towards identifying potential program effect mediating factors. This framework consisted of two criteria for evaluating the salience to a program designer of any given predictive factor. First, the factor should have strong evidence of being related to sexual risk behavior. Second, the factors should be easy to target—a characteristic that is accessible to the program such as individual attitudes, and be possible to change—such as individual attitudes. Kirby and colleagues reviewed each factor and rated them accordingly, identifying those with strong evidence of predictive value and then rating the accessibility and amenability to change to sift through the factors. The factors they identified included: *Knowledge* of sexual issues and possible consequences, *values* about sex such as whether premarital sex is justified, *perceived norms* about peer sexual activity, *efficacy* to stick to decisions about sexual behavior, and *intentions* to engage in sexual activity.

Program effects on mediators. Having identified factors that are strong predictors and amenable to change, the next step in identifying potential mediators of program effects would be to identify studies that involved quasi or experimental designs, measured pre-post change on hypothesized mediators, measured long-term behavior outcomes, and tied the outcomes to the change, thus demonstrating the value of the factors as mediators (MacKinnon, 2008). In my review of sex education programs, I found no study which conducted such tests. Instead, I found studies measuring change on mediators and measuring program/control differences on behavior, but none that empirically tied the two together.

Therefore, for purpose of this review, I identified a set of programs with strong evidence of effectiveness and identified what factors they measured and which ones they affected from pre to post to identify potential mediators. I began with a review by Kirby (2006) that identified several criteria for designating a program as "effective," exhaustively reviewed every study of sex education effectiveness meeting minimum standards of rigor, and presented the list of 11 programs that were deemed effective. An additional three programs were identified that met the same criteria (Weed et al., 2004, 2005, 2008). Using these 14 studies as a guide, I reviewed each study for clues to point to possible mediating factors. The first step was to examine the dosage, content, and delivery of each program to search for similarities and differences. Second, immediate outcomes changed by the programs were reviewed to identify potential mediators of behavioral outcomes. Finally, the studies were analyzed for ways to improve and extend the research. This review of effective programs will provide evidence for what factors should be targeted for more rigorous testing as mediators in a mediated effects model test.

The successful programs reviewed are remarkably similar in basic content. The concepts common to nearly all of the programs reviewed include increasing *knowledge* of the potential risks of sexual activity, the likelihood and consequences of STDs, HIV, or pregnancy, the importance of making a *personal decision* to protect ones future by abstaining from sexual activity, and the *benefits* that can occur with a choice to abstain. A few of the programs also emphasize the benefits of abstaining from sexual activity until marriage and dimensions of healthy long-term relationship formation (Weed et al., 2004,

2005, 2008). Others emphasized the importance of contraception and/or condom use and instructed how to obtain and use them. Finally, one program emphasized the individual worth of each youth to the community in which they live and stimulated students to make contributions to their community through service activities (Philliber, Kaye, Herrling, & West, 2002).

Several outcomes, measured at pre and post, were successfully changed by the programs in this review. The studies by Weed and colleagues focused on the same set of mediators which included efficacy, values, intentions, understanding future effects of sex. The other studies focused on these and few others. Consistent with the literature review above, the mediators changed included self-efficacy relating to various sexual negotiation behaviors (e.g. to refuse sex, to discuss sexual decisions with boy/girlfriend, discussing use of contraceptives), intentions to engage in sex, perceived peer norms about sex, sexual limits, commitment to avoid situations that might lead to sex, attitudes and values towards abstinence, justifications to engage in sex, and understanding potential future effects of sex. Most of the studies reviewed demonstrated significant effects on most or all of the mediating variables and behavior.

Particular tests of a set of mediators. One particular set of tests of this type have furthered understanding about mediators by virtue of studying the same set of mediators in multiple studies. The research of Weed and colleagues (Weed et al., 2004, 2005, 2008; Weed & Olsen, 1988; Weed, Olsen, DeGaston, & Prigmore, 1992) has used multivariate models to see which factors might be mediating factors by virtue of strong relationships to whether youth have ever had sexual intercourse (i.e., cross-sectional data) or not and to

predict whether youth will initiate sexual intercourse for the first time or discontinue if already started (i.e., longitudinal). Numerous factors in multiple and diverse datasets over 22 years have been entered into equations and tested. Factors that were significant predictors were retained in subsequent studies and analyses, assuming that they may reflect mediating effects. Over time, the same factors have repeatedly appeared as statistically significant and often sizeable predictors in the face of other new variables. The factors include: *intentions* to abstain, *values and attitudes* about sex, *knowledge* about sex, including perception of risk and consequences, and *efficacy*.

In one of the studies (Weed et al., 2008), they found that the mediators were more strongly related to sexual experience status than demographic factors. Additionally, three studies have shown examples of programs that affected most or all of these mediators with average pre-post (2 week) effect sizes of about 0.30 and which went on to have 50% reductions in initiation of sex after one year (Weed et al., 2004, 2005, 2008), suggesting a possible mediating relationship between these variables and sexual behavior.

Research by Birch and Weed (2007) has investigated whether these same factors also predict likelihood of sexually active youth discontinuing sex. Specifically, scores on the mediators were used to predict recent sexual activity (compared to sexually experienced youth who had not engaged in recent sexual activity) and found to be statistically significant. However, these analyses were not sufficiently rigorous to permit strong inferences because the sample size was small and it utilized only cross-sectional data.

In summary, this body of research is important because first, these researchers

have independently replicated the same set of predictors that emerge from the Kirby and colleagues (2007) review. Second, though yet to test these factors as mediated program effects on YSA, these factors were all tested in different program evaluations that showed significant pre-post effects on them and showed subsequent behavioral effects.

Other research and theoretical basis. The core mediators suggested by the Kirby and colleagues (2007) review, those targeted by a set of programs shown to be effective (Kirby, 2006), and those found by Weed at al. (Weed et al., 1992, 2004, 2005, 2008; Weed & Olsen, 1988) converge on what appear to be fairly common constructs: *knowledge, values, efficacy, and intent.*

These four factors are also supported as important mediators for health behaviors in general (Armitage & Conner, 2000; Kirby, 2002; Miller & Moore, 1990; Plotnik, 1992; Resnick et al., 1997). They are also strongly related to the Theory of Planned Behavior (Jaccard, 2009), which is based on seminal research (Ajzen, 1985, 1991; Ajzen & Fishbein, 2005; Armitage & Conner, 2000). The essence of the theory is to explore why some people intend to do a behavior and some people do not, and further, why some people who intend to do a behavior do so while others do not. The theory posits that the most obvious predictor of behavior is *intent*. For example, if someone says they are going to brush their teeth right now, they are more likely to do so than an individual who when asks if he intends to brush his teeth right now says no. Further, some people who intend to brush their teeth do so and some do not. In sexual behavior, the general intention to engage in sexual activity is important because unlike whether someone intends to brush their teeth right now, sexual activity involves many other factors which operate over time

and in a particular situation to translate into both the formation of an intention to have or not to have sex, and affect whether whatever intention is had is translated into behavior or not. A further implication of the *intention* construct is that given its relatively straightforward nature as a predictor of a behavior, it is a good outcome variable for programs. If a program is based on the assumption that highlighting risks of a behavior will reduce that behavior, then it should follow that an immediate change in intent should be found, else why would we expect the behavior to change.

Therefore, in terms of studying YSA, the theory would suggest that *intentions* to have sex are the primary mediator through which other constructs such as values or knowledge exert their influence on subsequent behavior. For example, programs which increase efficacy to avoid unwanted sexual advances may assist those who intend to avoid having sex to actually do so. Buhi and Goodson (2007) conducted a systematic review of those studies that tested constructs derived from the theory of planned behavior, including whether the construct of *intention* would be consistently related to sexual behavior. Eight out of eight studies that tested *intentions* as a predictor of sexual activity found a significant relationship, with none finding it to be unrelated. Other factors such as knowledge, efficacy, and values were not as strongly related, but gained support as predictors of sex. Taken together, this review further substantiates these constructs as possible candidates for a test of mediated effects.

Summary

The research reviewed suggested that there are many factors that are related to YSA and points to a framework for determining which of those factors are most

promising to test as mediated program effects. Several factors emerged as having a strong relationship with YSA. These factors are consistent with major constructs identified by theoretical and empirical work in the health literature as underlying healthy behavior. Finally, they are among the category of variables that seem most targetable by this type of program; personal, proximal attitudes.

Specifically, the common thread throughout all of the research reviewed is that *intentions* to engage in sex is a good predictor of whether YSA will occur. *Intent* is thus a good candidate for a mediated effect test both because it is a good predictor and because it is a good ultimate indicator of intent; no matter what else changed due to the program, it may not matter if it did not change the intent to engage in the targeted behavior. In addition to *Intent*, the constructs of *knowledge, values,* and *efficacy* also stand out as predictors of sexual activity, as individual, personal, proximal variables that can targeted by a program, and thus as possible mediators of program effects on YSA. These same factors that predict initiation of sex also seem related to the likelihood of sexually active youth continuing to engage in sex.

Conclusion

While there may be other candidates for mediated effects and indeed others should be sought, the convergence between theoretical predictors, predictors shown to relate to health behavior in general, to sexual behavior in particular, empirical work by Weed and colleagues, and the findings of an exhaustive review (Kirby et al., 2007) suggest these factors are a good place to begin with a test of mediated effects.

Summary of Literature Review

Current program approaches addressing the risks of YSA from either CSED or ABED programs have demonstrated that while not all programs succeeded, it is possible to affect YSA, but it is apparent that the size and durability of those effects could likely be improved. Little is known about affecting sexual behavior of those already sexually active. There is a lack of empirical evidence to support the value of any given factor as a mediator of program effects and a lack of information about how well any given mediator predicts sex when affected by a program. Yet research has identified several factors that predict initiation and reduced frequency of sexual behavior, suggesting initial factors to test as mediators (Kirby et al., 2007). When potential mediators are sifted through a theoretical and practical lens, combined with an analysis of common mediators that have been tested for pre-post effects in studies that went on to show behavioral effects, an initial set of mediators with good rationale for testing emerges. These include *intentions*, *efficacy, values,* and *knowledge.*

Purpose

The purpose of this paper is to address the limitations of the current research by testing an ABED program to provide evidence that YSA, both initiation of sex and the sexual behavior of those already sexually active, can be programmatically influenced. Second, potential program effect mediators will be tested empirically and their value as predictors analyzed.

Research Questions

Based on the review above, and consistent with the purposes of this study, the following questions were answered regarding the effects of a well-developed ABED program.

1. What effect, if any, will an abstinence-centered program have on possible mediators of YSA and on the incidence of YSA itself, both among sexually inexperienced youth and those already sexually active when the program begins?

2. To what extent can program effects on YSA, if found, be explained by observed changes on potential mediating factors?

3. How well can YSA program outcomes be predicted based on change scores on the mediating factors?

CHAPTER III

METHODS

Data on the effects of ABED programs in two sites were used for this study. Each site had a program and a comparison group. Each site collected data from participants to assess changes in attitudes, intentions and behaviors related to YSA. Participants were given a pencil and paper survey prior to beginning the program, a posttest upon completion of the program, and a follow-up measurement approximately one year later. Details on the sites, selection methods, measurement, sample composition, attrition, research design, and analysis are now provided.

Sites, Selection Methods, and Research Design

Georgia

The Choosing the Best abstinence education program is an 8-hour, abstinence-centered curriculum provided in school settings to youth in health classes. Previous results have shown strong results of the program at changing pre-post scores on key predictors of YSA (Birch & Weed, 2007; Weed et al., 2005). The program was provided in a suburban junior high school in the fall of 2004 through spring of 2006. The school randomly assigned ninth-grade students to receive one of two health teachers, one of which taught the program and the other provided a standard health class curriculum. The seventh- and eighth-grade students were assigned to the program or comparison based on whether they had signed up for a standard health class or a standard health class with a physical fitness emphasis. School administrators indicate that in practice, the choice of

students to select one or the other of these is largely driven by scheduling, not preference. Thus, though no clear indication to the contrary was found, it can't be certain that the assignment to the program group for seventh and eighth grade was completely random, thus resulting in what can best be labeled a services-as-usual comparison group.

Two cohorts of data were combined to create this data set, one from each of two school years, with a third year in which only follow-up data were collected. Pretests from the latest occasion each youth appeared in the first or second year were linked to the first time they appeared in year 2 or 3.

Virginia

The Reasons of the Heart curriculum is a 20 session abstinence-centered curriculum provided in school settings to youth in health classes. A previous study showed that the program produced a 50% reduction in the rate of initiation of YSA (Weed et al., 2008). The study did not measure the effects on sexually active youth and did not correct for baseline differences rigorously; the inclusion of this site allows for a replication of the YSA effect with methodological improvements and a test of effects among sexually experienced youth. All seventh graders from three middle schools received the ROH program, and seventh graders from two similar middle schools from the same geographic region who could not accept the program at the time formed a services-as-usual comparison group.

Measurement Development

The Institute for Research and Evaluation has developed measures of sexual

activity and of constructs that predict sexual activity which have been used in previous studies (Birch & Weed, 2007; Weed et al., 2004, 2005, 2008). Over the last 22 years, the Institute has tested over 120 different implementations of abstinence education. In the early years, literature searches identified sets of potential predictors of sexual activity and these were used as short-term outcome measures to see if programs produced pre-post change on them. Additionally, Institute staff analyzed curriculum content to identify constructs that were being targeted on the assumption that the program developers' program theory was made explicit in what they chose to target. Thus, in discussion with the curriculum designers, staff would develop and pilot additional measures as potential predictors of sexual activity. The initially chosen measures would be included with a set of new potential measures each year and at the end of each year, multivariate analysis would examine how well the measures were related to current sexual experience status. Those with the strongest relationship tended to be retained from year to year while those with weaker relationships would be dropped.

This process occurred iteratively for about 15 years before long-term data became routinely available. At that point, it became possible to see how well the measures predicted sexual behavior 6 to 12 months after the program. The availability of long-term data also began to yield publishable information, causing the need for deeper literature reviews.

At the point when these papers were being prepared for publication, these literature reviews revealed a convergence between the items and constructs the Institute had developed and those in the health behavior literature as well as those identified in the

field of youth sexual activity studies (Weed et al., 2004, 2005, 2008). Following this somewhat informal process, carried out in a context that for most of the Institute's history did not support the costs of publication, the Institute arrived at some core constructs that merit further scrutiny and testing.

The method for measuring these constructs was through paper and pencil surveys containing self-report items related to sexual behavior, attitudes, values, and basic demographics. These survey items are contained, in differing combinations, in virtually every survey sent out by the Institute. A brief description of these core measures follows, including sexual behavior measures and measures of sexual attitudes, values, and intentions.

Sexual Behavior

Sexual behavior items on Institute surveys are assessed through self-report questions that ask students if they had ever had sexual intercourse, which is defined to the students as "by sexual intercourse, we mean vaginal sex, or 'going all the way'; the sex that makes babies." Additional questions clarify the nature of their sexual behavior including a question that asks how many times they had ever had sexual intercourse, when the most recent time was, how many people they have had sexual intercourse with, and theirs and their first partner's age the first time they had sexual intercourse.

Sexual Attitudes and Values

Institute surveys contained several 5-point agreement Likert scales that measured students' attitudes, values, and beliefs about sex. These items formed scales

corresponding to the five core mediating variables identified in the literature review section of the paper. These scales assess such constructs as self-efficacy to maintain sexual abstinence, beliefs about the impact sex could have on their future, intentions regarding whether or not they planned to engage in sex, and the value they placed on abstaining from sex until marriage. The core mediator of *justifications* was not available at the time the survey in one of the sites was conducted; therefore, only four of the five core mediators were tested in this study.

Reliability of Measures

Reliability of the measures has been demonstrated in three previous studies (Weed et al., 2004, 2005, 2008). Table 1 lists the primary measures used in this study, their definitions, a sample item, and the typical Cronbach alpha reliabilities that have been calculated on other similar samples in the literature. For this study, the survey used in the two sites (Georgia and Virginia) contained slightly different items measuring the same constructs. Thus, only the scale scores were used in the analyses. Table 2 lists the exact wording of all items, categorized by which construct they addressed. To assess the possibility of collinearity between these measures, a correlation matrix was computed. The average interscale correlation was substantial ($r = 0.63$, with all being statistically significant) with the highest being between values and intentions ($r = 0.80$, $p < .001$). All analyses proposed in this study in which the four mediators are simultaneously entered into regression equations as independent variables would be affected by collinearity. To check for whether these intercorrelations result in collinearity, VIF and Tolerance statistics were computed when these independent variables are regressed on the sexual

Table 1

Names, Definitions, Sample Item, and Reliabilities of Proposed Measures

Name	Definition	Sample item (# items)	Reliability
Abstinence efficacy	Self-efficacy to engage in behaviors instrumental to sexual abstinence.	How sure are you that you could stay away from situations that might lead to sex? (4)	.88
Abstinence intentions	Intentions to be sexually abstinent	If someone wanted you to have sex with them during the next year, what would you do? (2)	.76
Abstinence values	Value abstinence as a lifestyle choice	It is against my values for me to have sex while I am unmarried. (4)	.87
Future impact of sex	Awareness of possible impact of having sex.	Having sex as a teenager could make it harder for me to get a good education in the future. (3)	.76

Note. All scales are 1-5 point scales with higher scores indicating lower probability of engaging in YSA.

activity variable. In all cases, the tolerances were well above standard limits of 0.10 and VIF well below 5, suggesting that while intercorrelated, the factors may not be linear combinations of one another.

Validity of Measures

Validity of these constructs has been demonstrated (Armitage & Conner, 2000; Kirby, 2002; Miller & Moore, 1990; Plotnik, 1992; Resnick et al., 1997; Weed et al., 2004, 2005, 2008). These studies tested the basic questions of whether the constructs were related to current sexual experience status (concurrent validity) and whether they predict future sexual behavior (predictive validity). The studies found that each construct was significantly related to sexual experience status, with inexperienced youth showing higher scores than experienced youth. Further, groups with higher proclivity to have engaged in YSA (e.g., males, older youth) tended to score lower, even when controlling

Table 2

Self-Reported Survey Items Used to Measure Constructs by Site

Site	Construct	Items	
Georgia	Efficacy	q20a	How sure are you that you could Stay Away from situations that might lead to sex?
		q20b	How sure are you that you could Talk to your girl/boyfriend about your decision not to have sex?
		q20c	How sure are you that you could Explain your reasons if your girl/ boyfriend pushes you to have sex?
		q20d	How sure are you that you could Firmly say "no" to having sex?
		q20e	How sure are you that you could Stick with your decision not to have sex?
		q20f	How sure are you that you could Stop seeing your girl/boyfriend if he/ she continues to pressure you to have sex?
	Intentions	q38	If someone did want you to have sexual intercourse with them during the next year, what would you do?
		q39	How likely do you think it is that you will have sexual intercourse at any time before you get married?
	Values	q12	It is important to me to wait until marriage before having sex.
		q13	It is against my values for me to have sexual intercourse while I am unmarried.
		q14	Having sex before marriage is against my own personal standards of what is right and wrong
		q28	I have a strong commitment to wait until marriage before having sex.
		q40	It is against my parents values for me to have sexual intercourse while I as an unmarried teenager.
	Future	q9	Do you think that having sex as a teenager would make it harder for you to get a good education in the future?
		q10	Do you think that having sex as a teenager would make it harder for you to have a good marriage in the future?
		q11	Do you think that having sex as a teenager would make it harder for you to get a good job or have a successful career in the future?
Virginia	Efficacy	q45a:	How sure are you that you could avoid getting into a situation that might lead to sex (like going to a bedroom, drinking alcohol, doing drugs)?
		q45b:	How sure are you that you could firmly say "no" to having sex?

(*table continues*)

Site	Construct	Items	
		q45c:	How sure are you that you could stick with your decision not to have sex?
		q45d:	How sure are you that you could resist having sex with your girl/boyfriend even if your friends are telling you it is okay?
		q45e:	How sure are you that you could say "no" to having sex with your girl/boyfriend even when you are turned on?
		q45f:	How sure are you that you could stop seeing your girl/boyfriend if he/she continues to pressure you to have sex?
	Intentions	q40:	If someone you were attracted to tried to get you to have sex with them during the next year, what would you do?
		q41:	How likely do you think it is that you will remain abstinent until you are married?
	Values	q24:	It is important for ME to remain abstinent until I get married.
		q26:	Having sex before marriage is against my idea of what is right.
		q28:	I have clear and definite ideas about why I should remain abstinent until I'm married.
		q30:	I have a strong commitment to remain abstinent until I am married.
	Future	q37:	Do you think that having sex as a teenager would make it harder for you to study and stay in school in the future?
		q38:	Do you think that having sex as a teenager would make it harder for you to have a good marriage and a good family life in the future?
		q39:	Do you think that having sex as a teenager would make it harder for you to get a good job or be successful in a career in the future?

Note. All scales are 1-5 point scales with higher scores indicating lower probability of engaging in YSA.

for sexual experience rate differences between groups, suggesting that the measures were assessing a risk dimension beyond just the probability of engaging in sexual activity. The studies also showed that the measures were related to the likelihood of engaging in sex at a later time, but the evidence for their predictability was weaker. Specifically, it seems that in competition with one another, only "intentions" was a significant predictor of future sexual activity. The other measures were all significantly related to Intention, suggesting a possible Fishbein and Ajzen (1975) attitude-intention-behavior mediating

relationship. Further suggestion of the potential predictive value of these measures is found in the fact that programs that have strong effects on them have gone on to show significant reductions in initiation (Weed et al., 2004, 2005, 2008) while other studies that have measured them and found no effect on pre-post change have not (Trenholm et al., 2007). A study in process (Weed, Birch, Ericksen, & Olsen, 2010) is testing more sophisticated predictive models using structural equation modeling to clarify the predictive relationship. Finally, other studies also suggest that the measures may be related to the likelihood of sexually active youth discontinuing sexual activity one year later (Birch & Weed, 2007).

Further evidence supporting the use of these measures is found in factor analyses that have been conducted. First, a confirmatory factor analysis conducted using the same constructs, measured with almost identically worded survey items (Weed et al., 2010). Factor loadings averaged 0.725, with efficacy loadings being the lowest (range 0.582 to 0.863), while the highest scoring was intentions (0.758 to 0.807). The overall model fit was good (RMSEA = 0.031, CFI = 0.982), suggesting that the hypothesized factor structure is a reasonable one. Earlier exploratory factor analyses (Weed et al., 2008) suggested sufficiently high loadings for the items on their hypothesized scales. Together, these results lend construct validity and thus further support the use of these measures in this study.

Key Outcome

Survey responses to the two questions of whether the youth had ever had sex

(sexual experience) and when the last time they had sex (recent sex) were transformed into one dichotomous outcome variable that was called “sexual activity,” which is used as the key outcome in the study. The method for designating youth as having engaged in sexual activity depended on whether they were sexually inexperienced or experienced at the pretest. Sexually inexperienced youth were designated as having engaged in sexual activity if they reported sexual experience at the follow-up and those that continued to report inexperience were designated as not having had sexual activity. Pretest sexually experienced youth were designated as having had sexual activity at the follow-up if they report having had sex within the previous 6 months, while those reporting no sexual activity within the previous 6 months were designated as not having had sexual activity. Thus, the key outcome examined was whether or not respondents, sexually experienced at pretest or not, reported at the follow-up to have engaged in “sexual activity.”

Sample Characteristics

The initial sample size was 1,628. Of those, 1,478 had both a pretest and a posttest (87.3%). Of these, 114 were lost due to not answering all of the survey items required for the analyses, 305 were lost to attrition, and a total of 1,059 were located at the follow-up (65.0% of the total). These 1,059 were the basis of all subsequent analyses.

The program and comparison groups were compared to examine baseline similarity. Inasmuch as the assignment to groups occurred at the site level, these comparisons were done separately by site. Tables 3 and 4 show the comparisons between groups. The groups were well matched on nearly all measures, with a few significant

Table 3

Comparison of Baseline Similarity Between Program and Comparison Groups By Site: Categorical Variables

			Comparison		Program			
Variable	Site	Label	*n*	%	*n*	%	Statistic	Significance
Total sample	GA	*N*	403		558			
	VA	*N*	245		422			
Linked at follow-up	GA		219	54.3	352	63.1	$\chi^2 = 7.41$	< .01
	VA		168	68.6	320	75.8	$\chi^2 = 4.16$	< .05
Grade	GA	7th grade	84	38.4	135	38.4	$\chi^2 = 5.85$	NS
		8th grade	50	22.8	109	31.0		
		9th grade	85	38.8	108	30.7		
	VA	7th grade	168	100.0	320	100.0		
Gender	GA	Female	118	53.9	200	56.8	$\chi^2 = 0.47$	NS
		Male	101	46.1	152	43.2		
	VA	Female	103	61.3	175	54.7	$\chi^2 = 1.97$	NS
		Male	65	38.7	145	45.3		
Race	GA	Black	18	8.2	25	7.1	$\chi^2 = 3.24$	NS
		White	100	45.7	188	53.4		
		Other	101	46.1	139	39.5		
	VA	Black	37	22.0	27	8.4	$\chi^2 = 17.9$	< .001
		White	107	63.7	242	75.6		
		Other	24	14.3	51	15.9		
Sexual experience	GA	No	193	88.1	314	89.2	$\chi^2 = 0.16$	NS
		Yes	26	11.9	38	10.8		
	VA	No	144	85.7	292	91.3	$\chi^2 = 3.55$	NS
		Yes	24	14.3	28	8.8		

Note. All scales are 1-5 point scales with higher scores indicating lower probability of engaging in YSA.

Table 4

Comparison of Baseline Similarity Between Program and Comparison Groups By Site: Continuous Variables

Variable	Site	Label	Comparison	Program	Statistic	Significance
Mediators	GA	Values	3.75	3.78	$t = 0.28$	NS
		Intentions	3.72	3.68	$t = 0.35$	NS
		Efficacy	3.84	3.68	$t = 1.80$	NS
		Future	3.68	3.64	$t = 0.43$	NS
	VA	Values	3.73	3.72	$t = 0.17$	NS
		Intentions	3.85	3.88	$t = 0.27$	NS
		Efficacy	3.82	3.74	$t = 0.74$	NS
		Future	3.93	3.85	$t = 0.82$	NS
Follow-up length	GA	Months	11.6	12.1	$t = 2.08$	$< .05$
	VA	Months	16.0	16.0	$t = 0.00$	NS

Note. All scales are 1-5 point scales with higher scores indicating lower probability of engaging in YSA.

baseline differences. In the Georgia site, the program sample was slightly younger, with the χ^2 value not quite reaching significance ($\chi^2 = 5.85$, $p = 0.054$) and not significant when tested as a continuous, rather than a categorical variable (7.92 in program, 8.00 in comparison, $t = 1.11$, $p = 0.27$). There was also a small difference in the follow-up lengths (12.1 months in the program, 11.6 in the comparison, $t = 2.08$, $p < .05$), a difference likely to favor the comparison group, if at all. In the Virginia site, there was a significant difference in racial composition, with slightly more white and fewer black youth in the program than in the comparison. No differences were found between groups for any of the four primary mediating measures we examined in this study. Overall, the groups show substantial similarity, consistent with the method of assignment to groups,

which was a fairly strong quasi-experimental method, suggesting a good comparison can be made.

Attrition

There was evidence in both sites that attrition rates were significantly higher in the comparison than in the program groups. The differences between the lost cases and linked cases were tested for significant differences. On demographic variables, there was no evidence of differential attrition (i.e., the distributions before and after attrition were statistically equivalent). On the continuous variables, there were no significant difference found between the comparison linked and lost cases in either site. However, there was evidence that in the Georgia site, all four mediator scores among program linked cases were significantly higher than program unlinked cases, with an average Cohen's *d* value of 0.24. Since the mediator scores are related to sexual activity, this represents a comparability problem. However, in general, the data suggests that on average, though there were differential levels of attrition between program and comparison data, the differences between linked and unlinked cases was generally small and the resulting linked sample was well matched. The implications of these patterns are discussed in the limitations section of the paper.

Overview of Methods for Answering Each Research Question

Question 1: Program Effects on Pre-Post Change and Behavior

The effects of the program on hypothesized mediators of YSA and on recent sex

outcome was tested first. Repeated measures analysis was be used for tests of pretest to posttest effects on mediators and logistic regression equations will test for effects on recent sex, as measured at the follow-up. The basic test just described was followed by employing standard propensity score matching methods (Rosenbaum, 2010).

Question 2: Mediated Effects Analysis: Explaining Behavior Outcomes Via Change Scores

The purpose of this is to estimate the mediated effect of the program on sexual activity via its effects on the four hypothesized mediating variables. Methods outlined in MacKinnon (2008) were followed to compute these different effects, their confidence intervals, and the individual and collective mediating effect.

Question 3: Predicting Sexual Activity Outcomes Based On Mediators

Having tested the mediated effects, the third question is to determine the nature of the prediction models of YSA program outcomes based on measures of mediating factors. Second, the nature of the relationship between scores on the mediating variables and the probability of engaging in sexual activity was examined.

Specific Analysis for Each Research Question

Question 1: Program Effects on Pre-Post Change and Behavior

Pre-post results. Program and comparison group youth were compared to see whether the pre-post change on the four hypothesized mediators—efficacy, values,

intentions, and future impact of se,—was different for the two groups. To test the significance of the difference between the pre-post change scores of the program and comparison groups on the four mediators, GLM Repeated Measures analysis was performed with program as the between subjects factor and the pre and post scores on the four mediators as within-subjects dependent variables and time (pre-post) as the within subjects factor. It would have been possible to use post scores, controlling for pretest scores instead of change scores. Change scores were selected for analysis because first, the groups were well matched at pretest on these variables (see Table 4). Second, latter research questions involve analysis of the amount of behavioral effect explained by the change produced on these variables. Thus, interpretation of mediated effects of the program on sexual activity via its effects on the mediators is aided by testing those effects (i.e., the change scores), directly. Pre-post change scores were used to compute Cohen's *d* values as well to standardize the effect sizes across different measures to allow rough comparison between the effect sizes on different mediators. Cohen's *d* is also reported to provide the size of the change, which answers the central question in this study of how much sexual activity program effect can be explained by the size of the program pre-post change effect on the hypothesized mediators.

Behavioral results. A logistic regression equation predicting sexual activity (as transformed in the Key Outcome section above) at follow-up was tested. Pretest scores on the four mediating variables, length of time between pre and follow-up, race, gender, and grade of students were entered to control for differences between sites on these variables. The site (Georgia or Virginia) variable was also entered to keep comparison students

from each site compared to their site and to check for differences between sites. Program status was entered into an equation to test whether sexual activity scores, controlling for pretest differences, were significantly different for the program and comparison groups. Respondents' pretest recent sex status was entered as a factor in both analyses to see whether differential effects for these groups might be present. The effect size of interest was the odds ratio of the program's effect on sexual activity.

The above analyses controlled for differences between program and comparison statistically. To refine the comparison, propensity score matching was employed (Rosenbaum, 2010). A propensity score equation was computed by entering all covariates into an equation predicting program membership. Propensity scores were computed as a function of all the variables compared between program and comparison groups above, separately by site (all analysis was done keeping only comparisons from each site compared to their respective program group). A new categorical variable was created by grouping cases by their propensity scores, stratified by quintiles formed on those scores. The quintile variable was used as a blocking variable in a reanalysis of the standard model and results were compared. The final model tested included the program variable, the categorical quintile propensity score variable, and an interaction term between the program and the quintile variable. The final interaction term adjusts for differences between groups on the mediating variable score vector by testing whether the program/ comparison difference is consistent across levels of program similarity.

The results of this analysis are first reported by showing that using propensity scores in fact created a better comparison. This is done by comparing the proportion of

pretest covariates on which significant differences were found between the program and comparison to the proportion significant within stratified quintiles of the propensity scores (Rosenbaum, 2010).

Question 2: Mediated Effects Analysis: Explaining Behavior Outcomes Via Change Scores

The mediated effect of the program on sexual activity via its effects on the four hypothesized mediating variables was estimated. The method for explicating the mediating nature of these variables was to (a) estimate linear regression coefficients of the program's effect on each mediator, (b) estimate the effect of each mediator on sexual activity, (c) multiply the coefficients from (1) by those in (2) to arrive at an estimate of the mediated effect (i.e., the effect on sexual activity that can be taken credit for as a function of the program's effect on the mediators). A measure of the mediated effect of each individual mediator and the collective effect were calculated. Additional detail about each step is now provided, and is summarized in Figure 1, which shows the generic model being tested.

Effect of the program on each mediator. Linear regression equations were computed with the change scores on each individual mediator as dependent variables and the program variable as an independent variable. These coefficients represent the program's effects on the mediator and is symbolized by *a* in Figure 1.

Direct effect of change on each mediator on sexual activity. A logistic regression predicting sexual activity with the hypothesized mediators (efficacy, values, intentions, future impact) plus the change scores, was calculated. These coefficients

Model (from MacKinnon, 2008):

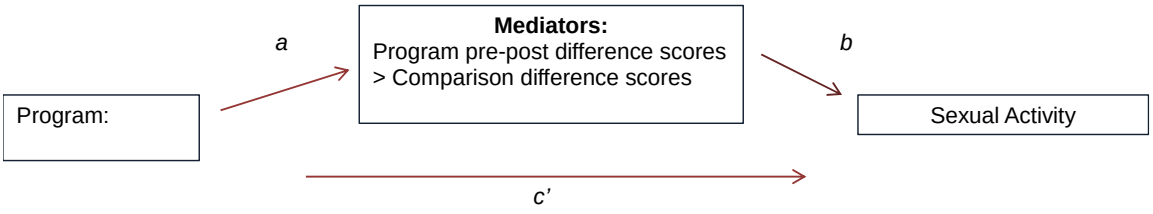

Figure 1. Generic mediated effects model being tested in this study.

represent the direct effect of change in the mediator on the likelihood of sexual activity at the follow-up. Figure 1 shows the statistic *b*, which represents this effect.

Individual mediated effects. The mediated effect was obtained by multiplying the coefficients from the two previous steps for each mediator. This figure represents the effect on sexual activity that the program can take credit for, given its effect on the mediator, based on that mediator's effect on sexual activity. The upper and lower confidence intervals and the significance of each effect were reported. Significance levels assumed alpha = .05; mediated effects were considered significant if the 95% CI did not include 0.00. The multiplication of *a* for each mediator by the effect *b* for each provides this statistic (*ab*). The statistic *c'* represents the effect of the program on sexual activity, aside from the effect that can be explained via the effect on the mediators. Presumably, if the mediators through which the program exerts is effect were all known, there were no differences between the program and comparison at baseline, and no error, *c'* would be 0.

Collective mediated effect. The collective effect of the program, via its combined effect on the mediators, can be expressed as the sum of all the mediated effects (MacKinnon, 2008). The total proportion of the program's overall effect that can be

explained by the mediated effect is obtained by dividing the mediated effect by the total program effect.

Question 3: Predicting Sexual Activity Outcomes Based On Mediators

Classification accuracy. The logistic regression equations tested in the previous research questions estimated the probability of sexual activity, given scores on several covariates. These equations produce an estimate of the odds of sexual activity for each individual participant in the study. The estimated odds can be examined to see how well they predict the actual outcomes. Examining the accuracy of the estimates generated by the models in turn provides a measure of the ability of the mediating variables to function as accurate predictors of sex (i.e., the practical significance of the mediating relationship). Since the purpose of this analysis is to provide models that a program administrator could use to predict behavioral outcomes from pre-post data, only program cases were used in analyses for this research question.

The accuracy of the proposed cutoff scores is assessed by examining four metrics of accuracy: (a) sensitivity score, calculated by examining how many of those who engaged in sexual activity were predicted to have done so based on pretest data, (b) specificity score, or how many of those who did not have sexual activity were predicted to not do so based on pretest data, (c) predictive value of a positive test, which is calculated by examining how many of those above the cutoff score, who are predicted to engage in sexual activity actually did so, and (d) predictive value of a negative test, calculated by examining how many of those who were below the cutoff score (i.e.,

predicted to not have sexual activity, who actually did not do so).

One way to understand the meaning of these different metrics is to phrase them in questions. Table 5 lists the questions for each, with sample numbers from a contingency table, with two additional metrics used in this study. The first is the average accuracy, calculated by averaging the sensitivity and specificity scores, and Naglekerke R^2 from the logistic regression, which give summative impressions as to the overall accuracy of the model.

The accuracy of the model in predicting sexual activity was assessed by identifying a cutoff score that maximally equalized the sensitivity and specificity scores

Table 5

Example to Illustrate the Different Cutoff Score Tests

	Predicted status					
	No sexual activity		Sexual activity			
Actual status	Type	*n*	Type	*n*	*N*	
No sexual activity	True negatives:	82	False positives:	38	120	
Sexual activity	False negatives:	4	True positives:	10	14	
Total		86		48	134	
Questions asked				Correct	Total	%
I had sexual activity...how well did this test do at finding me (Sensitivity)?				10	14	71
I had no sexual activity...how well did this test do at finding me (Specificity)?				82	120	68
I was predicted to have sexual activity...how well did this test do at accurately guessing my real status (Predictive Value of Positive Test)?				10	48	21
I was predicted to have no sexual activity...how well did this test do at accurately guessing my real status (Predictive Value of Negative Test)?				82	86	95
Average accuracy =						69
$R^2_{\text{Nagelkerke}}$ =				0.253		
Cutoff value				0.105		

(i.e., when those scores are at their joint maximum, it suggests a maximum accuracy scenario).

Threshold identification. The predicted odds of sexual activity were plotted against the posttest scores (averaged across the five mediators into one measure for heuristic purposes) and various curves were fitted to the data, including linear, logarithmic, and logistic curves.

CHAPTER IV

RESULTS

Question 1: Program Effects on Pre-Post Change and Behavior

Pre-Post Results

For efficacy, values, intentions, and future impact, I first tested the effect of the program on these hypothesized mediators. The change scores in the program were significantly larger than those in the comparison. Table 6 lists the pre and post scores for each group, the effect sizes as calculated by the standard Cohen's *d* formula (difference score divided by pooled standard error), the effect size of the program/comparison group test (program effect size—comparison effect size) and the significance tests for the program*pre-post interaction term. Effect sizes for the comparison between groups ranged from 0.22 for Efficacy to 0.60 for Future Impact. There was no strong evidence of a differential effect by categorical variables, with only one of the four mediators showing a significant three-way interaction, which was found between site, program, and pre-post

Table 6

Pre-Post Change Analysis

	Program (n = 672)			Comparison (n = 387)			Program vs. comparison		
Mediator	Pre	Post	*d*	Pre	Post	*d*	*d*	*F*	Sig.
Values	3.71	3.80	0.12	3.83	3.76	-0.10	0.22	9.8	< .01
Intentions	3.76	3.92	0.20	3.75	3.60	-0.18	0.38	42.9	< .001
Efficacy	3.68	3.78	0.12	3.82	3.74	-0.11	0.23	11.9	< .01
Future impact	3.72	4.07	0.48	3.77	3.68	-0.13	0.60	50.7	< .001

Note. All scales are 1-5 point scales with higher scores indicated lower risk of engaging in YSA.

change (intent effect was stronger in Virginia, owing to a large drop in intentions scores among the comparison group).

Behavioral Results

Standard model. Odds ratios were used in all behavioral tests. The rationale for this selection over similar methods such as the relative risk is based on three primary factors. First, when logistic regression is used, odds ratios are the most appropriate statistic for comparing the likelihood of the occurrence of a dichotomous outcome in one group to the likelihood of occurrence in another group (Deeks, 1998). Second, whenever we wish to adjust an estimate of relative likelihood of occurrence of an event by covariates, odds ratios are the more parsimonious choice because the adjustment is much simpler mathematically than the adjustment of relative risk ratios (Simon, 2001). Finally, logistic regression, which is the most appropriate analysis for the research questions at hand, are based on the logarithm of the odds and the product of such an analysis (odds) cannot easily be converted into relative risk ratios.

The odds ratios are calculated by first, dividing the number of youth engaging in sexual activity by the number not engaging, then comparing the odds of sexual activity within each quasi-experimental group, and then using logistic regression to adjust the odds for the covariates entered into the model. For example, if there were 10 of 100 program youth and 20 of 100 comparison youth engaging in sexual activity, the odds ratio would be ((10/90) / (20/80)) = 0.44, or a 56% reduction in the odds of sexual activity.

Table 7 shows the results. In this table, the results of the logistic regression

Table 7

Logistic Regression Predicting Follow-up Sexual Activity Rates by Program

Variable	*B*	*S.E.*	Sig.	Exp(B)
Program (0 = comparison, 1 = program)	-0.627	0.220	0.004	0.534
Gender (0 = male, 1 = female)	-0.304	0.198	0.125	0.738
White vs. Black/other	-0.104	0.305	0.733	0.901
Other vs. White Black	-0.050	0.336	0.881	0.951
Grade	0.316	0.146	0.030	1.372
Pretest sexual experience (0 = no, 1 = yes)	1.261	0.405	0.002	3.528
Months between pretest and follow-up	0.028	0.047	0.551	1.029
Centered pretest mediator scores				
Values	-0.044	0.152	0.769	0.957
Intentions	-0.467	0.145	0.001	0.627
Efficacy	-0.041	0.110	0.713	0.960
Future impact	-0.281	0.113	0.013	0.755
Site	-0.105	0.203	0.603	0.900
Site*program	-0.019	0.198	0.924	0.981
Sex*program	0.341	0.486	0.482	1.407
Intercept	-3.856	1.399	0.006	0.021

N = 1,059, $R^2_{\text{Nagelkerke}} = 0.341$, $X^2_{\text{Model}} = 250.3$, $p < .001$.

analysis are displayed, which estimates the odds of sexual activity as a function of several covariates and including the key factor of the program effect. The results showed that the odds of youth engaging in sexual activity as of the follow-up were significantly lower for program youth than for comparison youth (*OR* = 0.534, 95% CI, lower = 0.347, upper = 0.822, p = .004). Gender (set up as female compared to male), race (set up as two contrasts; white vs. non-white and other race vs. black or white), months between pre and follow-up, values, and efficacy were not significant predictors, while grade, intentions, future Impact, and pretest sexual experience were. Given the attention paid to the

mediating variables in the next research question and associated analysis, it is interesting to note that two of the hypothesized mediators were statistically significant (OR = 0.672 for intentions, 0.755 for future impact). This suggests that at pretest, those with higher scores are significantly less likely to end up having had sexual activity at the follow-up, which supports their plausible role as potential mediators for the program to target; if these variables can be changed by the program, it appears more likely that the likelihood of sexual activity may reduce.

No evidence for differential effects by site or pretest sexual experience were found ($OR_{site*program}$ = 0.981, p = 0.92; $OR_{sex*program}$ = 1.407, p = 0.48). These interaction terms were included in the model to test for important differences; the former between the two different implementations of a program and the latter between two different sexual subpopulations (those who were and were not sexually experienced at the time the program was provided). The lack of significance of these two interaction terms suggests that both programs worked about equally well and that the programs worked about equally well for both subpopulations.

Propensity score adjusted model. Out of 10 tests of significance of the difference between program and comparison groups, four of them were significant (0.40). Within quintiles of the propensity score, only 1 of 50 comparisons were significant, suggesting that the procedure reduced essentially all of the bias between groups, making the test of the difference between program and comparison, controlling for propensity score quintile more convincing (see Table 8). Results of this model suggested little change in the pattern of results on the standard model. There were no covariates that were

Table 8

Results of Propensity Score Adjustment Procedure

		Within quintiles	
Variable	Significance	Quintile	Significance
Gender	No	1	No
		2	No
		3	No
		4	No
		5	No
Race	Yes	1	No
		2	No
		3	No
		4	No
		5	No
Grade	Yes	1	No
		2	Yes
		3	No
		4	No
		5	No
Sex	No	1	No
		2	No
		3	No
		4	No
		5	No
Months	Yes	1	No
		2	No
		3	No
		4	No
		5	No
Values	No	1	No
		2	No
		3	No
		4	No
		5	No
Intentions	No	1	No
		2	No
		3	No
		4	No
		5	No

(*table continues*)

		Within quintiles	
Variable	Significance	Quintile	Significance
Efficacy	No	1	No
		2	No
		3	No
		4	No
		5	No
Future Impact	No	1	No
		2	No
		3	No
		4	No
		5	No
Propensity Score	Yes	1	No
		2	No
		3	No
		4	No
		5	No
Number significant	4		1
Number of tests	10		50
Proportion significant	0.40		0.02

significant in this refined model that were not in the standard model, nor vice versa, with the program effect estimate remaining significant (*OR* = 0.508, *p* = 0.002).

Question 2: Mediated Effects Analysis: Explaining Behavior Outcomes Via Change Scores

Figure 1 (displayed earlier) shows the basic model tested by this analysis. I am testing whether the hypothesized mediators change as a result of the program (difference scores, i.e., link “a” in the model depicted in the figure), whether they in fact ought to be

expected to mediate the program effect by virtue of being good predictors of the outcome (link "b"), then multiplying those effects together to obtain the mediated effects of the program, or the effect of the program on the outcome, via its effect on the mediators, and controlling for the effect of the program on the outcome (link "c'" in the model). In summary, I am testing whether the program affects sexual activity due to its effect on the mediators from pre to post. Thus, the hypothesized mediators were tested to see if the difference scores from pre to post in the program, when compared to the comparison, resulted in differences between groups on the sexual activity outcome. It should be noted that alternative models testing slightly different research questions could be tested. But for answering the question suggested by the literature and described in Chapter II, the model just described was articulated.

Effect of the Program on Each Mediator

This tests the link in Figure 1 (shown earlier) labeled "a," which is the effect of the program on each individual hypothesized mediator. The beta coefficients, *t* values, and standard errors will all be used in the calculation of the mediated effect and are listed in the first three columns of Table 9. The effect sizes (*Beta* values) ranged from 0.165 for efficacy to 0.435 for future impact and were all statistically significant. This suggests that the program had an effect on each of the mediators, with the effect sizes being small for efficacy, medium for intentions and values, and large for future impact, relative to findings from other studies (Weed et al., 2004b, 2005, 2008).

Table 9

Mediated Effects and Relevant Figures That Were Used to Calculate Them

	Program effect on mediator			Effect of mediated sexual activity			Mediated effect		Significance evaluation		
Measure	$B_{effect1}$	*t*	*SE*	$B_{effect2}$	*t*	*SE*	$B_{effect1}*B_{effect2}$	Odds	Lower CI	Upper CI	Sig.?
Values	0.352	8.092	0.043	-0.134	-0.770	0.174	-0.047	0.954	-0.168	0.074	No
Intentions	0.313	6.224	0.050	-0.581	-3.836	0.152	-0.182	0.834	-0.291	-0.073	Yes
Efficacy	0.165	3.069	0.054	0.051	0.381	0.135	0.008	1.009	-0.035	0.052	No
Future	0.435	7.082	0.061	-0.143	-1.167	0.123	-0.062	0.940	-0.168	0.044	No
Total	1.265			-0.807			-0.283	0.753			

Direct Effect of Change on Each Mediator on Sexual Activity

This tests the "*B*" link in the model from Figure 1 (shown earlier), which is to estimate the effect that change on the hypothesized mediator has on sexual activity. The beta coefficients, *t* values, and standard errors are reported in the second three columns of Table 9.

Individual Mediated Effects

The next column in Table 9 shows the mediated effect, obtained by multiplying the coefficients from the two previous steps for each mediator (or "ab" from the model shown earlier in Figure 1). The upper and lower confidence intervals and the significance of each effect are shown in the last three columns. Significance levels assumed alpha = .05; mediated effects were considered significant if the 95% CI did not include 0.00. The mediated effects ranged from -0.182 to 0.008, with only the mediated effect of Intentions being statistically significant. This effect means that the program's effect on sexual activity through its effect on Intentions is $B = -0.182$, which corresponds to an odds ratio of 0.834. Another way to interpret this is to say that if the program only had the effect on Intent that it did, it would result in a statistically significant reduction in the odds of initiation from the comparison group odds value, which in this case was 0.013, to 0.00799 ($OR_{program} * OR_{mediatedeffect} * OR_{comparison}$) or a total odds ratio of 0.626 (0.00799 / 0.013).

Collective Mediated Effect

The sum of all the mediated *B* values is -0.283, which corresponds to an odds of

0.753. If the combined mediator effect of -0.283 is divided by the overall program effect of -0.627 (taken from Table 7; this is the *B* value from the original equation, corresponding to the *OR* of 0.534), a proportion of 0.451 is obtained, suggesting that a substantial amount of the overall program effect on sexual activity can be explained as a function of the effects on efficacy, values, intentions, and future impact, the four hypothesized mediators. Since only the mediated effect for intentions was significant, we can compute the proportion of the program effect via that known mediator and we find a proportion mediated of -0.182/-0.627 = 0.290, suggesting that much of the program's effect is obtained via its effect on intentions.

Question 3: Predicting Sexual Activity Outcomes Based on Mediators

Classification Accuracy

For this analysis, I examined the actual status on the outcome (yes or no sexual activity), compared to the predicted status based on the covariate vector. I then calculated the sensitivity, specificity, predictive value of a positive test, and predictive value of a negative test. The results are shown in Table 10. Results were that a 74% accuracy rate was obtained, which is higher than the 50% rate one would expect by chance. Predictive value tests showed substantially better rates for identifying who did not engage in sexual activity (94% correct) than for those who did (35% correct). These classification results were obtained with a cutoff score of 0.135.

Threshold identification. Comparing the odds of sexual activity via the posttest scores and fitting different curves to the data revealed R^2 values for linear, logarithmic,

Table 10

Predicting Sexual Activity at Follow-up: Classification Accuracy for Balanced Specificity and Sensitivity

	Predicted status				
	No sexual activity		Sexual activity		
Actual status	Type	*n*	Type	*n*	*N*
No sexual activity	True negatives:	418	False positives:	147	565
Sexual activity	False negatives:	27	True positives:	80	107
Total		445		227	672

Questions asked	Correct	Total	%
I had sexual activity...how well did this test do at finding me (Sensitivity)?	80	107	75
I had no sexual activity...how well did this test do at finding me (Specificity)?	418	565	74
I was predicted to have sexual activity...how well did this test do at accurately guessing my real status (Predictive Value of Positive Test)?	80	227	35
I was predicted to have no sexual activity...how well did this test do at accurately guessing my real status (Predictive Value of Negative Test)?	418	445	94
Average accuracy =			74
$R^2_{\text{Nagelkerke}}$ =	0.334		
Cutoff value	0.135		

and logistic curves were 0.448, 0.474, and 0.557, respectively. Figure 2 shows the scatterplot with the logistic curve fitted, as it appears to best account for the pattern of data.

The curve suggests that the probability of sexual activity drop precipitously as scores increase from 1.0 to 3.0, at which point slowing begins and flattens considerably by a score of 4.0. To corroborate this data as a potentially important threshold value, a linear regression predicting probability of sexual activity via the posttest score on the

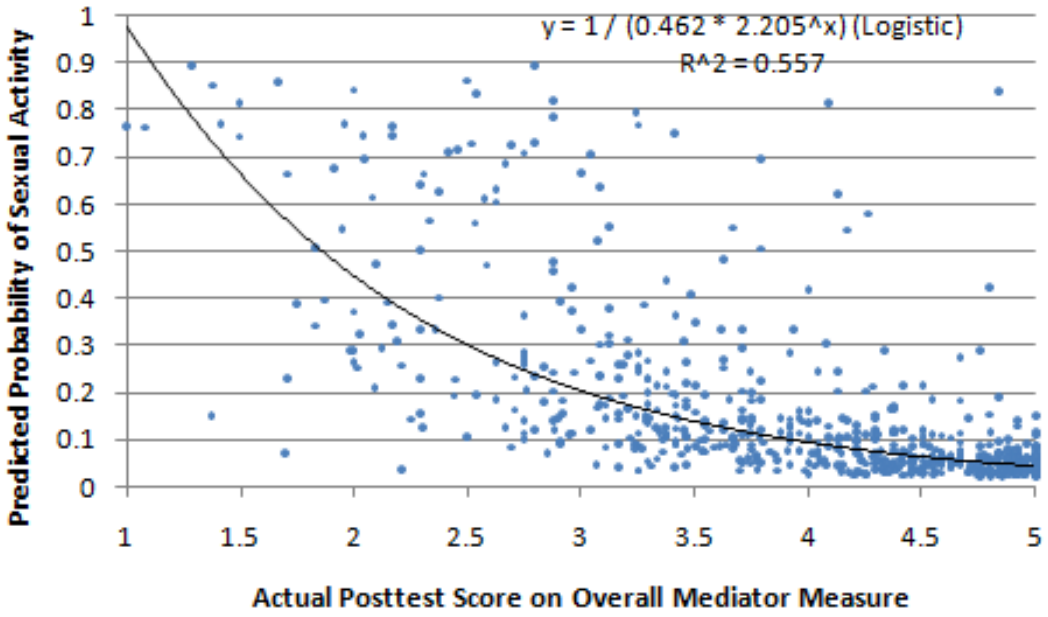

Figure 2. Scores as a function of predicted probabilities: Predicting sexual activity.

averaged mediator score was computed. The value of posttest score that corresponded to the cutoff value from Table 10 of 0.135 was algebraically obtained and stood at 4.12. To further illustrate the utility of the threshold, the rate of change before and after that point was calculated, using the fitted logistic function in Figure 2. The slope below the threshold was -0.29 and the slope after was -0.048. This illustrates how the rate of change after the threshold is much smaller than before (*OR*= 0.203).

CHAPTER V

DISCUSSION

The results of this investigation suggest that the two programs evaluated had statistically significant, modest-sized effects from pre to post on four key hypothesized mediators of youth sexual intercourse behavior. Further, significant and good-sized effects were found on reducing the sexual activity rate of youth approximately one year after the program. The hypothesized mediator intentions to engage in sex was a significant mediator of the programs effects on sexual activity. Resulting statistical models drawing on the tested mediators showed that a fairly high level of accuracy in predicting sexual activity in the context of a program designed to affect it is possible.

Each of these findings is now reviewed in more detail, conclusions drawn, implications described, and recommendations for future research provided.

Review of Findings

Research Question 1: Program Effects on Pre-Post Change and Behavior

The program had clear effects from pre to post on the mediators and appears to have reduced the incidence of sexual activity. A strong quasi-experimental approach to assigning participants to experimental groups was used, resulting in generally good comparability between program and comparison groups. Those problems were adjusted using propensity score methods and corrected most of the statistical comparability issues between groups. When these methods were used, the effects on behavior were still

significant. The program effects appeared to be consistent for both of the program sites tested and were not different for those who were sexually experienced and inexperienced at baseline. This is important because it shows that both programs appeared to have some effect on sexual activity (not just one or the other) and because it shows that an ABED program affected the behavior of both experienced and inexperienced youth, an effect which has not been widely tested in the literature on ABED.

Research Question 2: Mediating Effects
Analysis: Explaining Behavior
Outcomes via Change Scores

When tested as individual mediators of the program effect, only one of the four tested was found to be significant (intentions). If we compare the program's effect on sexual activity solely via its effect on intentions and compare this to odds of a comparison youth engaging in sexual activity, we obtain a program/comparison odds ratio of 0.626, which represents a substantial reduction in odds, and not a lot lesser of an effect than the overall odds ratio of program versus comparison odds of initiating, which were 0.534. This odds of 0.626 represents the explained odds of sexual activity, which in a sense eliminated the portion of the observed effects accounted for by validity threats and error, as well as unmeasured program effects. That this odds ratio was still a sizeable reduction in sexual activity, it strengthens the internal validity of the conclusion of a program effect and highlights the importance of Intentions as a mediating variable.

As a group, the mediators accounted for approximately 45% of the effect of the program, suggesting that other factors may be at play as well. These might be program effects on unmeasured mediators or the effects of validity threats such as selection bias.

Yet the overall effect explained by these mediators suggest that they play an important role in explaining the effectiveness of the program. The lack of significance for the mediating effect on three of the four tested mediators is disappointing. Yet other research (Birch & Weed, 2010) shows that these three mediators seem to exert an influence on sexual behavior via a mediating relationship with intent (i.e., they have an indirect relationship with sex through their relationship with intent). Thus, this study suggests further scrutiny of these factors is warranted.

Research Question 3: Predicting Sexual Activity Outcomes Based On Mediators

The mediator model seemed to be accurate at predicting sexual activity. Of those who engaged in sexual activity, 75% were correctly identified (sensitivity) and 74% of those who did not were correctly identified (specificity). As far as predicting who had sexual activity and who didn't, the model seemed to be better at identifying those who did not (94% accurate) and less so at identifying who did (35%). This suggests that low scores on the four mediators might be necessary but not sufficient to produce sexual activity; even when low scores are present, apparently other factors must be in place for sexual activity to occur. Conversely, when high scores are present, it seems necessary and sufficient to predict very low sexual activity.

A logistic relationship was found between average mediator posttest scores and the probability of sexual activity. As scores move from the lowest possible up to a score of 4.12 out of 5.0, the probability of sexual activity drops sharply for each unit increase in score. After this point, the probability decreases less so for each unit increase in score.

This suggests that when scores are high on these mediators, very little sexual activity occurs, as reflected in the 94% accuracy rate at identifying those who do not engage in sex.

Implications

Regardless of the debates about which approach should be followed, the purpose of this study was to test two ABED program's effects and more importantly, to use a method for testing those effects that addresses the limitations of previous program evaluation research in this field, regardless of which approach was used. Specifically, this evaluation (a) provided a relatively rigorous test of two abstinence-centered interventions, (b) tested them both for effects among sexually inexperienced youth (common in the literature) and among sexually active youth (uncommon), (c) empirically tested the mediating link that both provides information about mediating factors and strengthens the internal validity of the results (uncommon in the sex education literature), and (d) provided practical information about possible benchmarks useful to program administrators (absent from sex education literature).

Implications for Sex Education Program Evaluation Research

Evaluation research of sex education programs have not yet formally tested mediating models but rather have tested the pre-post effects on mediating variables (e.g., attitudes towards condom use, intention to have sex), or test the effects on behaviors at some later time, such as condom use or initiation of sex (Kirby, 2006). Occasionally, both

are tested, but when they are, they are reported as separate but conceptually related outcomes, but no attempt is made to empirically tie the two results together (Borawski et al., 2005; Weed et al., 2004b, 2005, 2008). The methodology employed in this study tested behavioral effects in a different way from previous research.

Drawing on mediating test methodology described in MacKinnon (2008), this study showed how much of the program effect on behavior could be attributable to observed effects on pre-post outcomes. This is important because first, it elucidates the mediators through which the program has its effects on behavior, providing clues to program developers of what to target.

Second, in a typical quasi-experimental design, the difference between experimental groups on the observed outcome variable is composed of differences explained by the program effect and differences due to validity threats (e.g., selection bias). The reviewer of such a study is unable to know how much of the difference is explained by each set of causes and must trust in other measures of the extent of the validity threat, such as the significance of the difference between groups on pre-test measures of outcome. By specifically estimating only the portion of the observed difference between groups that can be explained by the pre-post change on the mediators, a method for the estimate of a program effect was developed that increases confidence in the program effect and its size. This test provides additional strength towards establishing internal validity because it estimates the amount of the observed distal outcome can be taken credit for as a function of the observed change produced on the proximal (pre-post outcomes). This strengthening occurs because the pre-post change analysis, as a measure

of the program effect, is less subject to validity threats than the more distal behavior outcome. Since we can believe the pre-post change is a program effect, and since we can estimate how much of the behavioral effect is attributable to that change, we can have more confidence in the explained behavioral effect.

The use of propensity score analysis approaches also deserves some attention. Even in experimental designs, baseline noncomparability can be a problem, particularly if the sample sizes are small. In a quasi-experimental design such as this one, using propensity score methods allows for a clear, multivariate estimate of the degree of difference between groups at baseline and a method for adjusting for these differences. The implication for sex education research is that whether experimental or quasi-experimental, these methods can be used to provide an estimate of the degree of imbalance and to corroborate gross program effect estimates by seeing if they still appear significant when controlling for the imbalances.

Implications for the Field of Sex Education

This study provided a mediated effects model relying on factors identified in the general health behavior literature—and which could be used by proponents of either approach—that can be targeted by programs as tests of their effects on factors predicting abstinent behavior, an outcome both approaches report addressing.

With regards to ABED in particular, these findings address one of the main limitations of the current literature on the effectiveness of ABED by providing a strong, well-matched quasi-experimental design, adding design features which further strengthen comparability (propensity score matching), and testing a mediated effects model. These

findings suggest that it is possible to affect self-reported sexual activity with a program like this. Second, the finding of no significant differences between the effects on those who at baseline were sexually experienced versus those who were inexperienced suggests that a program such as this can reduce sexual risk behavior.

Perhaps the most significant implication of this study for the field of sex education is to examine the question of net effects on sexual risk behaviors. If it is possible to reduce net risk among sexually inexperienced and experienced youth more with an intervention that does not provide explicit condom education than with one that does, it would be cause for reanalysis of approaches. On the other hand, this study being with relatively young and proportionately inexperienced youth may suggest that at this age, such explicit condom education may not be indicated while youth at older ages may not show the same net reductions in response to a program like this. The results of this study imply that it might be possible to affect sexual risk of both experienced and inexperienced youth in a way that merits further research into this question.

Implications for Sex Education Practitioners and Administrators

The mediating variables not only provided an immediate test of program impact, they also produced models capable of fairly accurate prediction of sexual activity. An overall average mediator score of 4.12 was identified to measure program successes against. Program evaluators could use this information in formative evaluation in several ways. First, measuring program pre-post effect on factors that are known or possible mediators of program effects on sexual activity will provide a better gauge of what is

working and what is not; new program content or processes can be tested to see which have the greatest effect on these known mediators, allowing for formative changes to occur earlier in the evaluation process. This would avoid the problem that occurs when programs measure pre-post success on factors that are only weakly related to sexual activity, show large change, and proceed to spend time and money doing a longitudinal experiment only to find that little effect was had on the behavior because the mediators that were measured were not true mediators. Next, the likely behavior effects of early versions of new interventions can be estimated using the predictive models produced by this study. Finally, success of program efforts can be measured against threshold attainment to see whether sufficient practical significance is attained before widely implementing a strategy. Of course, a program who serves a population that averages very low to begin with should not be seen as unsuccessful if pre-post change does not result in scores above 4.12 (e.g., 1.00 to 4.00 pre-post change would be a phenomenally large effect, even though the 4.12 score was not reached). However, the number of youth failing to reach this threshold score have not yet attain maximal results and knowing that could spur program developers back to the formative table before moving to the more intensive summative evaluation process.

Implications for Behavioral Health Research

The results of this study lend support to the Theory of Planned Behavior (Jaccard, 2009) and the research on which it is based (Ajzen, 1985, 1991; Ajzen & Fishbein, 2005; Armitage & Conner, 2001). Specifically, support was found for intentions to engage in sex acting as a primary mediator of sexual behavior, with values, efficacy, and

knowledge also being significantly related to sexual activity and showing amenability to change through a program intervention. This research extends the value of the theory by showing not only that the factors are related to the outcome in the way specified by the theory, but by showing that a program which changes those factors also appears to change the behavioral outcome.

Limitations

Instrumentation

First, the study relied on self-report surveys given to youth regarding sensitive, personal issues. This could affect the findings in unpredictable ways, but was likely a much bigger problem with the behavioral data than with the mediating variable measures because the behavioral measures relied on only a few survey items and were more personal and potentially threatening to admit (e.g., that they have actually had sex as opposed to their attitudes about having sex). Another related problem is that relatively few sexual behaviors were measured, preventing an analysis of effects on other important behaviors such as contraceptive use, number of partners, and self-reported pregnancy and STD rates.

Another problem was that the scale items used to construct the scale scores were not exactly the same in the two sites. However, the site variable included in the analyses ensured that the scale scores which were constructed in the same way were compared between program and comparison groups within sites, while the scale scores that were constructed slightly differently were never directly compared. This limitation affects the

degree to which generalizing these results about the relationship of these constructs can be done.

Selection Bias

As is the case in all observational studies, it is possible that the difference between program and comparison is due to preexisting, and by virtue of the quasi-experimental design uncontrolled, differences between groups. While the groups were similar at pretest and while propensity score analysis strengthened confidence, it still remains possible that estimate of the true differences between groups is biased and that the true difference may not be the same. One unique strength of this study is that the mediated effects analysis strengthens confidence that the program did in fact affect sexual behavior by estimating the mediated effect, i.e., the effect on the outcome that *can* be credited to the program, by virtue of its more clear and believable effect on the mediating factors. Thus, while we still cannot be sure of the exact size of the true effect, it seems that the program likely had a true effect on sexual activity.

Attrition-Related Selection Bias

Not all youth who took the program received a follow-up test. Attrition analysis indicates that there were not large differences between those who did and did not provide a follow-up test. However, more comparison youth were lost to follow-up than program youth, which could be a problem. Yet the facts that there were so few differences between those lost and those not lost to follow-up and that the resulting program and comparison samples were very well-matched suggests that the internal validity may not

have been compromised by attrition. In experimental designs, differential attrition may represent a larger problem because the assumption of comparability on measured and unmeasured covariates can fail to be met; we cannot be sure it was met if we lost more in one group than the other. In a quasi-experiment, comparability is only assured for measured covariates, thus the loss of more program than comparison individuals does not necessarily represent any worse of a problem than we began with in the first place. The more likely problem with this is reduced generalizeability; we cannot be sure who the population that was measured really was.

History

It is assumed that the difference between the program and comparison schools is that the program schools received the intervention while the comparison did not. However, it is possible that a selection-history interaction may have resulted in differences in what the different groups received. It is possible that additional educational services were provided in both groups' schools during the intervention period, or between the initial pre- and posttest and the follow-up test. This makes it unclear whether the effects of the program may have been augmented or diminished, depending on what was provided to whom. It also makes it unclear how much of the predictive value of the models may have either been hindered or improved based on what was provided to whom. Thus, while the data suggests that the presence of this particular program had some effect on reducing sexual behavior, the effect size estimate may change depending on what comparison schools (or program schools for that matter) did or did not also receive. For example, if the comparison schools also received education that stresses

abstinence, we would expect the actual program effect to be larger than the observed effect while if they received nothing at all, the currently observed effect size estimates are probably more accurate.

Treatment Contamination

The related threat of treatment contamination is unlikely to have significantly affected the results in the Virginia site because comparison schools and program schools were in different communities. However, in the Georgia site, the schools were in the same school and it is possible that the estimate of effect size may be biased due to unknown effects of interactions between members of each group.

Maturation

Youth in the study were more than a year older at the conclusion of the study and this maturation is going to likely affect the probability of engaging in sex. It is possible that a selection-maturation interaction could result in maturation occurring at different rates in the comparison than in the control, compromising the unbiased estimation of treatment effect sizes. Since the comparison group was slightly older than the program and since age is positively related to the probability of sexual activity, it is possible that some of the treatment effect found may be attributable to this validity threat.

Statistical Conclusion Validity

The use of change scores is not immune to criticism (e.g., Judd & Kenny, 1981; Shadish, Cook, & Campbell, 2002). These criticisms center on reliability, ceiling effects, and regression towards the mean. The first criticism applies in that the estimates of the

size of the mediated effects are affected by the reliability of the change scores. It seems most likely that the effect this might have on the results would be to diminish the probability of finding a significant mediated effect, rather than to inflate the change of finding one since the errors in reliability can be presumed to be random and there is no reason to believe they would be significantly different in the program and comparison groups. Second, ceiling effects may have affected the results because the behavioral outcomes of those students whose change scores would have been larger had there been no ceiling do not give adequate credit to the truncated change score (e.g., if a 4.8 to 5.0 effect resulted in no sexual activity, yet the true score was 4.8 to 5.5, the size of the mediated effect is being overestimated, with a 0.20 change being given credit for reducing sexual activity when in reality, a 0.70 change had been necessary). Finally, regression toward the mean is not a threat because cases were not selected based on deviance from the mean. Yet a selection-regression interaction might occur if the comparison group, whose pretest scores were lower on average, were in reality even more low scoring, but for some reason started artificially higher and then regressed to their true and lower mean, making it more likely to see a behavioral effect based on pretest differences not detected. In conclusion, since the primary question was not about effect sizes on pre-post change per se, this limitation should be noted, but does not necessarily invalidate the findings, though it does perhaps temper confidence in them.

Future Research

Future research could build on this particular study by addressing the limitations

identified in the beginning of this paper and those just described. This study raises the standard in evaluation research of sex education programs to incorporate advances in mediating analysis (MacKinnon, 2008). Specifically, it allows for any of the published literature that included pre-post and distal outcomes to be reanalyzed, and new studies to be done using this methodology. This would strengthen the literature by allowing for estimates of the explained effect size, instead of just assuming all of the observed effect on behavior is real while acknowledging some amount of uncertainty due to validity threats. It is likely that the stronger the study, the less than explained effect would differ from the observed, while weak studies with observed effects that were not really real in the first place would be shown to be less certain. Of course, the success of this approach depends on the choice of real mediators; if the mediators do not really mediate then adjusting for them may not change the overall effect size estimate, but will also not estimate sources of bias in the effect size estimate.

This leads to the next limitation that was addressed by this study; the identification of key mediators upon which to base program development efforts as well as to include in future evaluations. Increasingly elaborate models predicting sexual activity could be developed and used to help programs estimate behavior effects with greater accuracy. With the hundreds of factors that have been identified as relating to sexual activity (Kirby et al., 2007) and the dozens of programs available, each addressing their own set of hypothesized mediators (Kirby, 2006), the result is fragmentation; few studies can be compared directly.

The importance of this study for future research is that it points to the need to

identify a core set of proven mediators that can account for a large amount of program's effects on sexual activity and then to be sure that all evaluations of programs both measure those mediators and analyze them appropriately, as done in this study. Employing these and other methods (e.g., Ragin, 2008) set theoretic analysis for studying necessary and sufficient causes) will strengthen the body of information about known predictors. Doing the exact same thing for predictors of contraceptive use and other safer sex practices is also a key need for future research, as well as directly measuring the net effects of different combinations of abstinence and safer sex interventions on actual rates of pregnancy and STDs.

The implication of having such models in place is that it would allow for greater formative evaluation with an emphasis on identifying best practices more quickly, before resources for expensive longitudinal investigations are wasted on programs that could have been identified as not reaching their potential had we known what to measure them on. Then experimental designs on interventions with ample evidence of effects on these mediators could be employed, rather than using evaluation resources to only discover that the right mediators were not targeted.

Future research should focus on moving programs through stages of rigor, from initial formative efforts to gauge short-term effectiveness on key mediators, adjustments to programming until sufficiently large sexual activity effects can be predicted based on the pre-post results, then moving to strong quasi-experimental or experimental, longitudinal studies.

Finally, this study found no significant differences between the effects among

those who at baseline were sexually experienced and those who were not. However, for programs who want to test sexually active youth more closely, including contraceptive use, frequency, number of partners, and so forth, they will likely find that emphasis must be placed on obtaining large enough samples to test for significant effects among sexually experienced youth. Otherwise, they risk replicating a problem found in the current literature, which is that no school-based sex education program has ever succeeded at having strong and significant effects on both an abstinent behavior outcome and a contraceptive outcome (Ericksen et al., 2010). Part of this is because whenever studies that have measured both outcomes and found an effect on abstinent behavior, there were usually small numbers of sexually active youth in the sample, with even fewer who answer all the questions about their sexual behavior. Thus, future research should emphasize obtaining sufficient numbers to test more of the important behavioral outcomes.

Finally, future research should specifically test the question of net effects on risk behavior for programs with differing emphases among different populations. If all sex education programs were classified as either containing strong, some, minimal, or no emphasis on abstinence and the same categories of emphasis on condoms and these differing emphases were tested on younger/older and lower/higher risk youth, the question net effects on sexual risk behavior could be assessed. As is, the current literature insufficiently tests the assumptions made by either sides of the debate, which assert either that condom education inherently increases likelihood of sexual activity or that it never does.

Conclusions

The program clearly impacted the hypothesized mediating variables and seems to have influenced self-reported sexual activity rates. The study provides a new standard in sex education evaluation research by providing an analysis that empirically ties pre-post effects, mediating effects, and behavioral effects into one analysis. If future programming can take these factors into account in their evaluation approaches, greater concerted, formative evaluation can result, leading to better programs and better spent evaluation resources. Further, if program development efforts focus more on defining successful behavioral outcomes and less on which approach to claim support for, scholars and practitioners could focus more on identifying and designing programs around proven predictors of those outcomes. This would be a welcome and potentially effective change that would contribute significant positive energy in debates about what works best.

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Weed, S.E., Birch, P.J., Ericksen, I.H., & Olsen, J.A. (2010). *Testing a predictive model of youth sexual intercourse initiation.* Salt Lake City, UT: Institute for Research and Evaluation.

Weed, S.E., Ericksen, I., & Birch, P.J. (2004). An evaluation of the Heritage Keepers® abstinence education program. In A. Golden (Ed.), *Evaluating abstinence education programs: Improving implementation and assessing impact* (pp. 88-103). Washington, DC: Administration for Children and Families, U.S. Department of Health and Human Services.

Weed, S.E., Ericksen, I.H., Birch, P.J., White, J.M., & Evans, M.T. (2007). *"Abstinence" or "comprehensive" sex education?—The Mathematica Study in context*. Salt Lake City, UT: Institute for Research and Evaluation.

Weed, S.E., Ericksen, I.H., Lewis, A., Grant, G.E., & Wibberly, K.H. (2008). An abstinence program's impact on cognitive mediators and sexual initiation. *American Journal of Health Behavior, 32,* 60-73.

Weed, S.E., & Olsen, J.A. (1988). Policy and program considerations for teenage pregnancy prevention: A summary for policy makers. *Family Perspective, 22,* 235-252.

Weed, S.E., Olsen, J.A., DeGaston, J., & Prigmore, J. (1992). *Predicted and changing teen sexual activity rates: A comparison of three Title XX programs*. Salt Lake City, UT: Institute for Research and Evaluation.

Whitaker, D.J., & Miller, K.S. (2000). Parent-adolescent discussions about sex and condoms: Impact on peer influences of sexual risk behavior. *Journal of Adolescent Research, 15,* 251-273.

Whitbeck, L.B., Simons, R.L., & Kao, M.Y. (1994). The effects of divorced mothers' dating behaviors and sexual attitudes on the sexual attitudes and behaviors of their adolescent children. *Journal of Marriage and the Family*, *56,* 615-621.

Wilcox, B. (2008). *A scientific review of abstinence and abstinence programs*. Washington, DC: Pal-tech.

VITA

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EDUCATION

Ph.D. 2011	Utah State University	
	Psychology-Research and Evaluation Methodology Specialization	
M.S. 1999	Brigham Young University	
	Marriage and Family Therapy	
B.S. 1995	University of Utah	
	Psychology	

PROFESSIONAL EXPERIENCE

Teaching Positions:

Adjunct Faculty—Marriage & Family Therapy	Argosy University	2011-
Adjunct Faculty—Family Science	Brigham Young University	1998-2002

Research and Administrative Positions:

Director of Research and Development	Foundation for Family Life	2011-
Senior Research Associate	Evans Evaluation	2011-
Executive Director	Institute for Research and Evaluation	2009-2010
Project Manager, Research Associate	Institute for Research and Evaluation	2001-2009
Senior Research Assistant	Church of Jesus Christ-Latter-day Saints	1997-1999
Graduate Research Assistant	James M. Harper / Thomas Holman	1995-1997
Research Specialist	Valley Mental Health	1994-1995
Research Assistant	James F. Alexander	1993-1995

Clinical Positions:

Private Practice		2005-
Marriage and Family Therapist	LDS Family Services	2005
Director	Latter-day Families	2001-2004
Manger and Therapist	Center for Family Preservation & Progress	1998-2001
Family Preservation Therapist	Youth and Family Centered Services	1996-1998
MFT Intern	BYU Comprehensive Clinic	1995-1997
Mental Health Worker	Benchmark Regional Hospital	1993-1994

EXTERNAL FUNDING

Awarded:

State of Maryland (2011)*. Department of Health and Human Services. Baltimore, MD. $270,000

BoysTown Personal Responsibility Education Program (2010)*. BoysTown. *Personal Responsibility Education Program Innovative Strategies Grant from the U.S. Department of Health and Human Services.* BoysTown, NE. $600,000.

Women's Care Center Teen Pregnancy Prevention Program (2010)*. Women's Care Center. *Tier 1 Teen Pregnancy Prevention Grant from U.S. Department of Health and Human Services, Office of Adolescent Health.* Kansas City, MO. $1,100,000BeTrue Social Networking Program (2008). Palmetto Family Council. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Columbia, SC. $120,000.

I Have Standards! Program (2008). ZOPS Management Firm. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Clinton, MD. $177,000

Step Up Now to Healthy Relationships Program (2008). Crisis Pregnancy Center of Fairbanks. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Fairbanks, AK. $115,000

Tribal Youth Program (2008). Mount Sanford Tribal Consortium. *Grant from U.S. Department of Education.* Chistochina, AK. $82,000

Yes You Can! Program (2008)*. St Michael's Medical Center, *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Newark, NJ. $145,000

Greater Kentucky Abstinence Education Project (2007)*. Heritage of Kentucky. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services*. Lexington, KY. $133,000

Heritage Abstinence Education Program (2007)*. Heritage Community Services. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Charleston, SC. $161,000

South Carolina Sex Education Accountability Project (2007). State of South Carolina Governor's Office. Columbia, SC. $70,000

Healthy Marriages, Healthy Kids Project (2006). Starkville School District. *Marriage Education Grants to Foster Healthy Marriage for Underserved Populations Grant from the U.S. Department of Health and Human Services*. Starkville, MS. $82,780

Heritage Keepers Healthy Marriage Initiative (2006)*. Heritage Community Services. *Marriage Education Grants to Foster Healthy Marriage for Underserved Populations Grant from the U.S. Department of Health and Human Services* Charleston, SC. $45,000

Tribal Healthy Marriage Initiative (2006). Mount Sanford Tribal Consortium. *Marriage Education Grants to Foster Healthy Marriage for Underserved Populations Grant from the U.S. Department of Health and Human Services.* Chistochina, AK. $45,000

Let's Talk Healthy Relationships Program (2005). Crisis Pregnancy Center of Anchorage. *Community-based Abstinence Education Grant from the U.S. Department of Health and Human Services.* Anchorage, AK. $117,000

Under review:

Process and Outcome of a Theoretically-based Teen Dating Violence Program (2011). Foundation for Family Life. *Research on Teen Dating Violence – R21 Grant under the National Institutes of Health*. Cincinnati, OH. $275,000

Utah MentorWorks Mentoring of Adult Offenders: Promoting Successful Reentry Through Responsible Fatherhood (2011). Foundation for Family Life. *Second Chance Act Adult Mentoring Grants to Nonprofit Organizations Grant from the U.S. Department of Justice.* Salt Lake City, UT. $300,000

* Indicates projects likely to lead to publications before 2015 (see Manuscripts in Preparation section below).

PUBLICATIONS

Peer Reviewed Publications:

Weed, S., Ericksen, I., & **Birch, P.J.** (2005). An evaluation of the Heritage Keepers® abstinence education program. In Evaluating Abstinence Education Programs: Improving Implementation and Assessing Impact, Alma Golden (Ed.). Administration for Children and Families, U.S. Department of Health and Human Services. Washington, DC.

Birch, P.J., Weed, S., & Olsen, J. (2004). Assessing the impact of Community Marriage Policies® on U.S. county divorce rates. *Family Relations 53,* 495-503.

Werner, C.M., Stoll, R.W., **Birch, P.J.**, & White, P.W. (2002). Clinical Validation and Cognitive Elaboration: Signs that encourage sustained recycling. *Basic and Applied Social Psychology, 24*(3), pp.185-204.

Larson, J.H., Peterson, D., Heath, V.A., & **Birch, P.J.** (2000). The relationship between perceived dysfunctional family-of-origin rules and intimacy in young adult dating relationships. *Journal of Sex and Marital Therapy, 26,* 161-175.

Contributor:

Weed, S.W., Ericksen, I.H., Lewis, A., Grant, G.E., & Wibberly, K.H. (2008). An abstinence program's impact on cognitive mediators and sexual initiation. *American Journal of Health Behavior*, 32, 60-73.

Books and Book Chapters:

Holman, T.B., **Birch, P.J.**, Carroll, J.S., Doxey, C., Larson, J.H., Linford, S.T., & Meredith, D.B. (2001). *Premarital Prediction of Marital Quality or Breakup: Research, Theory, and Practice.* Plenum Publishers. New York.

Holman, T.B., & **Birch, P.J.** (2001). Family-of-Origin Influences on Marital Quality. Book chapter in Holman, T.B., et al. *Premarital Prediction of Marital Quality or Breakup: Research, Theory, and Practice.* Plenum Publishers. New York.

Manuscripts in Preparation (to be submitted to peer-reviewed journals in 2011):

White, J. M., Galovan, A. M., Peeples, E., & Birch, P. E. (manuscript in preparation; submission projected Winter, 2011). Developmentally Appropriate Practice in Sex Education: A Comparative Study of Two Strategies. To be submitted to *Journal of Adolescent Health* or *Journal of Adolescent Research* or similar.

Weed, S.E., **Birch, P.J.**, Ericksen, I.H., & Olsen, J.A. (manuscript in preparation; submission projected Winter, 2011). A latent model statistical mediation analysis of a sex education program effects on youth sexual behavior outcomes. To be submitted to

Journal of Primary Prevention.

Birch, P.J. (manuscript in preparation; submission projected Winter, 2011). Causal mechanism model for explaining program effects on youth sexual intercourse behavior outcomes. To be submitted to *Journal of Adolescent Health* or *Adolescent and Family Health*.

Ericksen, I.H., **Birch, P.J.**, & Weed, S.E. (manuscript in preparation; submission projected Spring, 2011): A closer look at the evidence for the effectiveness of teen pregnancy prevention programs: A systematic review. To be submitted to *American Journal of Health Behavior.*

Birch, P.J. (manuscript in preparation; submission projected Spring, 2011). Exploratory evaluation of a clinic-based sexual risk avoidance program. To be submitted to *Prevention Science* or similar journal.

Birch, P.J. (manuscript in preparation; submission projected Summer, 2011). Descriptive evaluation of a comprehensive marriage mentoring program for low-income minority couples. To be submitted to *Family Relations* or similar journal.

Non-refereed Publications:

Weed, S.E., & **Birch, P.J.** (2009). Measuring what matters most in sex education. Opinion Editorial printed November 27, 2009 in *Washington Times.* Washington, DC.

Birch, P.J. (2004). How to improve your marital fitness. Invited article in *Cache Valley Parents Magazine.*

Birch, P.J. (2004). Keeping a pulse on your marital health. Invited article in *Cache Valley Parents* Magazine.

Birch, P.J. (2003). Book Review of Line upon Line, Precept Upon Precept. *AMCAP Journal, 28, 44-46.*

Birch, P.J. (2003). Holiday principles and practices. Invited article in *Cache Valley Parents Magazine.*

Birch, P.J. (2002). Pornography use: Consequences and cures. *Marriage and Families.* School of Family Life, Brigham Young University. Provo, Utah.

Birch, P.J. (2001). A marriage and family therapy perspective on child custody issues (working paper and abstract). *Journal of Family and Children's Law* (non-refereed). Social Science Research Network, http: //papers.ssrn. com/sol3/ papers.cfm ?abstract_ id=270031.

Unpublished Technical Reports:

30 significant evaluation reports written between 2001 and 2010.

PRESENTATIONS

Birch, P.J. (2010). Panelist. A public health approach for advancing sexual health in the United States: Rationale and options for implementation. Centers for Disease Control and Prevention. Atlanta, GA.

Birch, P.J. (2010). Prove it, improve it, repeat. Presentation to the National Abstinence Education Association conference. Washington, DC.

Birch, P.J. (2010). Reframing the sex education debate. Invited presentation to the U.S. Conference of Catholic Bishops Annual Conference of the Secretariat for pro-life activities. Chicago, IL.

White, J.M., Osorio, A., **Birch, P.J.**, & Weed, S.E. (2010). Improving Formative Evaluation with Microscopic Technology: Perception Analyzer®. Poster presented at 2010 Center for Research and Evaluation on Abstinence Education Bi-annual Conference. Washington, DC.

Institute for Research and Evaluation (2009). Sex education: Scientific evidence and publish policy. Briefing provided to the U.S. House of Representatives. Washington, DC.

McClellan, M.S., Culbreath, A., Francis, E., & **Birch, P.J.** (2009). Heritage Keepers® Healthy Marriage Initiatives. Presentation to the 2009 Administration for Children and Families Healthy Marriage Initiative Grantee Conference. Washington, DC.

Weed, S.E., (2008). Testimony before the U.S. House of Representatives Committee on Oversight and Government Reform. Washington, DC. (Multiple contributors, including Paul Birch).

Birch, P.J., Culbreath, A., & McClellan, M. (2007). Why evaluate? What's in it for me? Presentation to the 2007 Administration for Children and Families Healthy Marriage Initiative Grantee Conference. Washington, DC.

Weed, S.E., Ericksen, I., & **Birch, P.J.** (2005). *Evaluating Abstinence Programs.* Paper presented at the United States Department of Health and Human Services Welfare Conference. Washington, D.C..

Birch, P.J. (2004). *A moment-to-moment application of the gospel to marriage and parenting.* Invited presentation at Brigham Young Univerisity Family Expo. Provo, UT.

Birch, P.J. (2004). *A comprehensive self-management approach to overcoming compulsive pornography use.* Invited training workshop presented to Utah LDS Family Services therapists. Provo, UT.

Birch, P.J., Weed, S., & Olsen, J. (2004). *Effects of Community Marriage Policies® on divorce rates.* Invited poster presented at the annual conference of Smart Marriages, Happy Families. Dallas, TX.

Birch, P.J. (2004). Grant Review Panel Chairperson**.** United States Department of Health and Human Services Administration of Children and Families *Compassion Capital Fund Awards* for Marriage Education Programs. Washington DC.

Birch, P.J. (2004). *Promoting community marital well-being through comprehensive community-based systems.* Workshop. 2nd Annual LDS Family Life Education Conference. February 6, 2004.

Birch, P.J., Weed, S., & Olsen, J. (2004). *Have Community Marriage Policies® affected U.S. County Divorce Rates?* Invited presentation to the United States Department of Health and Human Services Administration of Children and Families annual Welfare and Poverty Research Conference. Washington, DC.

Birch, P.J. (2003). A call to arms: How to *really* protect our children from pornography. Presentation to LDS Marriage Network Conference. Provo, UT.

Birch, P.J. (2003). *A spiritually grounded theory of therapeutic change.* Workshop at the semi-annual convention of the Association of Mormon Psychotherapists and Counselors. Salt Lake City, Utah.

Birch, P.J. (2003). *Evaluation of the effects of community-based marriage initiative on U.S. county divorce rates.* Paper presented at the annual conference of the American Evaluation Association. Reno, NV.

Weed, S.E., & **Birch, P.J.** (2003). Overview of Marriage Savers Evaluation. Poster presented at the annual Smart Marriages Conference. Reno, NV.

Birch, P.J. (2002). *A call to arms: How to really protect our children from pornography.* Workshop at the semi-annual convention of the Association of Mormon Psychotherapists and Counselors. Salt Lake City, Utah.

Birch, P.J. (2002). *Family Life Education and Ecclesiastical Intervention: A Multi-Modal, Trans-theoretical Model.* Round table presentation. 1st Annual LDS Family Life Education Conference. March 19, 2002.

Birch, P.J. (2001). *Scientific and clinical considerations: A family therapy perspective on child custody issues.* Paper presented at the conference: The American Law Institute's Family Dissolution Principles: Blueprint to Strengthen or to Deconstruct Families? Conference co-sponsored by the J. Reuben Clark Law School, Brigham Young University and the Marriage Law Project, Columbus Law School, Catholic University. February, 2001.

Birch, P.J. (1997). *Becoming a marital therapist: A systematic unification of diverse theories of marital therapy.* Paper presented at the annual conference of the Brigham Young University Family Sciences Department, Provo, Utah.

Birch, P.J. and Harper, J.M. (1997). *Mothers and fathers in marital therapy: Empirical and clinical perspectives from across the lifespan.* Paper presented at the annual meeting of the National Council on Family Relations, Crystal City, Virginia, USA.

Birch, P.J., & Harper, J.M. (1997). *Understanding the relationship between therapeutic alliance, gender, and attrition in marital and family therapy:* Poster presented at the annual meeting of the American Association for Marriage and Family Therapy, Atlanta, Georgia, USA.

Birch, P.J., & Harper, J.M. (1996, November). *Understanding the effects of clients' expectations on important aspects of couple therapy: Implementing Pinsof & Wynne's (1995) recommendations.* Paper presented at the annual meeting of the National Council on Family Relations, Kansas City, Missouri, USA.

Birch, P.J., & Harper, J.M. (1996, October). *The development of clients' expectations for change in marital therapy.* Poster session presented at the annual meeting of the American Association for Marriage and Family Therapy, Toronto, ON, Canada.

Larson, J.H., Peterson, D. & **Birch, P.J.** (1997). *Family of origin rules and young adult intimate relationships.* Poster presented at the annual meeting of the American Association for Marriage and Family Therapy, Atlanta, Georgia, USA.

Holman, T.B., & **Birch, P.J.** (1997). *Fathers' and Mothers' Influence on Children's Marital Quality: Building A Model of Intergenerational Transmission.* Workshop presented at the annual meeting of the National Council on Family Relations, Crystal City, Virginia, USA.

Werner, C.M., Stoll, R.K., & **Birch, P.J.** (1996, June). *Validation as a method of increasing cognitive elaboration: Applications to recycling.* Poster presented at the annual international conference of the Environmental Defense Research Association, Salt Lake City, Utah.

Awards, Licenses, Certifications

2001	Licensed Marriage and Family Therapy, Utah	
2002	Utah State University Presidential Fellowship	$12,000
2003	Utah State University Presidential Fellowship	$12,000
In process	Initializing A.A.M.F.T. Approved Supervisor in Training	