Investigating Adverse Effects of Adolescent Group Interventions

by

Jessie Jong-Chee Wong

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Graduate Supervisory Committee:

Nancy A. Gonzales, Chair
Stephen G. West
Laurie Chassin
Thomas Dishion

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ABSTRACT

This study examined an adverse effect of an adolescent group intervention. Group interventions represent one of the most economical, convenient, and common solution to adolescent behavior problems, although prior findings from program evaluation studies have suggested that these groups can unexpectedly increase the externalizing behaviors that they were designed to reduce or prevent. The current study used data from a longitudinal, randomized controlled trial of the *Bridges to High School / Puentes a La Secundaria Program*, a multicomponent prevention program designed to reduce risk during the middle school transition, which has demonstrated positive effects across an array of outcomes. Data were collected at the beginning of 7th grade, with follow-up data collected at the end of the 7th, 8th, 9th, and 12th grade from a sample of Mexican American adolescents and their mothers. Analyses evaluated long-term effects on externalizing outcomes, trajectories of externalizing behaviors across adolescence, and potential mediators of observed effects. Results showed that the adverse effect that was originally observed based on adolescent self-report of externalizing symptoms at 1-year posttest among youth with high pretest externalizing symptoms was not maintained over time and was not reflected in changes in adolescents' trajectories of externalizing behaviors. Moreover, neither of the peer mediators that theory suggests would explain adverse effects were found to mediate the relationship between intervention status and externalizing symptoms at 1-year posttest. Finally, only beneficial effects were found on externalizing symptoms based on mother report. Together, these findings suggest that the Bridges intervention did not adversely affect adolescent problem behaviors and that future studies should use caution when interpreting unexpected adverse effects.

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INTRODUCTION

Adolescent behavior problems present a major dilemma to American society. Immediate and long-term outcomes of adolescent behavior problems place a major burden on public health and social services with oppositional defiant disorder and conduct disorder accounting for the greatest proportion of referrals to outpatient child and adolescent mental health clinics (Loeber, Burke, Lahey, Winters, & Zera, 2000). Convenience and cost-effectiveness have made group interventions the most commonly used method for preventing and treating behavioral problems in youth (Ang & Hughes, 2002; Dishion, Dodge, & Lansford, 2006). Although group formats have also been favored in that they allow opportunities to utilize peer influence to improve social skills and reinforce positive behaviors (Caplan et al., 1992; Dishion, McCord, & Poulin, 1999; Weissberg, Barton, & Shriver, 1997), evaluation studies have shown adverse effects of group interventions on targeted behavioral outcomes including antisocial acts, tobacco use, externalizing behaviors, delinquent peer associations, and anger outcomes (Cho, Hallfors, & Sánchez, 2005; Dishion, Capaldi, Spracklen, & Li, 1995; Feldman, 1992; Leve & Chamberlain, 2005; Poulin, Dishion, & Burraston, 2001). Although some reviewers conclude that adverse effects occur in 24%-29% of intervention studies (Ang & Hughes, 2002; Lipsey, 1992), other reviewers have attributed findings of adverse effects to methodological limitations, such as sampling error (Handwerk, Field, & Friman, 2000; Weiss et al., 2005). Thus, several questions remain unanswered, including whether adverse effects are meaningful, whether they last over time, and what mediators might explain these effects.

The Bridges to High School Program Efficacy Trial

The Bridges to High School Project / Puentes a La Secundaria (Bridges) is a multicomponent, culturally competent preventive intervention for 7th graders to build youth and family competencies to prevent multiple problems following middle school transition (Gonzales et al., 2012). Bridges was evaluated through an efficacy trial that included 516 Mexican American adolescents from 4 middle schools. Recruitment and randomization were stratified on the dominant family language, based on family preference, to accommodate for variability in participants' language proficiency and acculturation levels and to ensure participants could communicate with other group members. Adolescents in the groups were taught a range of coping and self-regulation strategies to manage interpersonal and school stressors, explored possible selves to increase academic motivation and engagement, and learned strategies to balance family relationships and obligations with school, activities, and friends.

Outcome analyses revealed numerous positive program effects on several outcomes, including substance use, teacher report of internalizing, and GPA at 1-year posttest. Additionally, mediation modeling showed positive indirect effects through targeted mediators, such as coping efficacy, school attachment, and parenting techniques, on long term distal outcomes including reduced deviant peer association, substance use, internalizing symptoms, and externalizing symptoms. Many effects differed as a function of the language in which the program was provided. Analyses also showed a positive intervention main effect on mother report of adolescent externalizing in the full sample (including both English and Spanish subsamples). However, an adverse effect on

adolescent self-report of externalizing was discovered for a subgroup of adolescents at 1-year posttest. Among adolescents with higher externalizing at pretest in the Spanish subsample, those assigned to the intervention condition showed higher self-report of externalizing symptoms at 1-year posttest compared to those assigned to the control condition. This result suggests that Spanish-group adolescents with greater externalizing symptoms before the intervention groups experienced increased externalizing problems as a result of the intervention. Similar to previous findings of adverse effects, this result was unexpected and difficult to interpret. Further research that expands upon the previous literature may be necessary to understand this finding, its implications, and provide a broader perspective on adverse effects of adolescent group interventions.

Previous Research on Adverse Effects

Evaluation studies are often designed to test whether the program was successful in producing the positive effects that were expected based on in its underlying theory. In fact, no program evaluation studies to date have been designed to test the hypothesis that the group intervention will produce adverse effects among youth participants, likely due to the enormous ethical complications inherent in such a research design. Previous findings of adverse effects were found in evaluation studies that examined an array of outcomes, all of which were expected to reveal positive effects, and with no a prior hypothesis related to adverse effects. In addition, there are several methodological limitations in previous studies of adverse effects, such as less than rigorous randomization procedures, alpha inflation, and lack of follow-up data collection, such

that findings must be interpreted with caution and consideration for potential threats to internal and external validity.

Despite these methodological limitations, the literature on adverse effects has largely developed around the findings of three landmark studies. The first study to report adverse findings of adolescent group interventions was the Cambridge-Somerville Youth Study (McCord, 1978, 1992), which began in 1939. This study sample included 500 community referred boys between the ages of 5 and 13 who were identified as either "difficult" or "average." Participant boys were matched according to an array of characteristics and randomly assigned within pairs to either the treatment or control condition. The treatment condition offered 5-6 years of services, including academic tutoring, medical or psychiatric attention, summer camps, and community programs. Researchers found no differences between the treatment and control groups at either 5- or 10-year follow-up on any measured outcomes. Conversely, 30-year follow-up assessments revealed more "undesirable outcomes" within the treatment group than the control group and even worse outcomes among those who attended the summer camps.

Several aspects of the Cambridge-Somerville Youth Study should be considered in interpretation of its findings. For instance, given that significant effects only arose 30 years after the intervention and not in earlier follow-ups, the results are unlikely to reflect a reliable or long-term effect of the treatment. In addition, the measure of "undesirable outcomes" blanketed an array of constructs including criminal behavior, psychiatric disorders, and death, not all of which could have resulted from the intervention (Weiss et al., 2005). Standardized and validated measures of outcomes must be used in order to

interpret such findings as evidence that an intervention produced either clinically or statistically significant changes in participants. Next, the majority of the treatment components that were provided to the boys did not involve group settings. Finally, random condition assignment was not upheld throughout treatment components. For instance, the summer camp component, one of the few that used group formats, was assigned to participants based on camp counselors' selection (Weiss et al., 2005), which leads open the possibility that worse outcomes among summer camp attendees may have been influenced by the assignment process beyond any effects of the intervention. Research that employs rigorous randomization procedures may help to draw stronger causal conclusions about the effects of group interventions.

The second study is the St. Louis Experiment (Feldman, 1992). This study examined the effects of peer group treatment on youth development and compared effects between aggregated high-risk only and mixed groups. This sample included 263 high-risk boys between the ages of 7 and 15 who were randomly assigned to either unmixed (high-risk only) or mixed (high-risk and non-referred) semi-structured groups. Data were collected before, during, and immediately after groups which showed adverse effects in the high-risk only groups and positive effects in the mixed groups. Specifically, boys in the high-risk only groups showed a non-significant increase in observed antisocial behavior and a statistically significant increase in self-report of antisocial acts from pretest to posttest. In contrast, high-risk boys in the mixed groups showed significant improvements on both these measures.

This research design did not include a control or comparison group and tested intervention effects according to pre-post differences. Such procedures inherently capture non-intervention related changes in behavior over time as well as effects of the intervention on behavior. Without a control condition, the two sources of variance are indistinguishable. Additionally, the lack of follow-up data precludes the ability to determine whether observed behavioral shifts represent a temporary peak in externalizing behaviors that occur across all youth or an erroneous finding that only occurred at one time point. It is also important to note that the semi-structured group "treatments" were primarily comprised of free-form sessions in which adolescents were provided a variety of activity options by minimally trained group leaders. Findings from a study of a structured, manualized group intervention, such as Bridges, may be needed in order to generalize findings to the types of group interventions that are more commonly implemented today. Furthermore, later analyses showed that adverse effects did not remain significant once group leader experience was controlled for (Mager, Milich, Harris, & Howard, 2005).

The most influential findings of adverse effects came from the efficacy trial of the Adolescent Transitions Program (ATP; Dishion, Capaldi, et al., 1995; Dishion et al., 1999; Dishion & Patterson, 2006; Poulin et al., 2001). The ATP efficacy trial included 158 self-referred families with children between the ages of 10 and 14. Families were screened according to a criteria checklist and the Achenbach's Child Behavior Checklist (Achenbach, 1991). Random assignment conditions included: 1) parent groups only, 2) teen groups only, 3) dual parent and teen groups, and 4) self-directed change with video.

After the study began, researchers included a second comparison group comprised of subjects who matched the treatment participants on various characteristics (Dishion & Andrews, 1995). This group was added to safeguard against the possible threat to internal validity related to the self-direct change condition, such as an unintended positive effect within this group (Weiss et al., 2005). For instance, if any positive change actually occurred in the first comparison group, analyses that used this group as a control could potentially distort a small or null intervention effect into an adverse effect. In addition, this study design included follow-up data collection across several years following the intervention and measures were selected to capture an array of adolescent outcomes.

Results showed that participants assigned to the teen groups only reported an increase in tobacco use 3 months following the intervention. At the 1-year follow-up, researchers observed markedly higher increases in tobacco use and teacher report of externalizing behaviors in the teen focus group compared to the control groups (Dishion & Andrews, 1995). Although the teen-and-parent focus condition showed no effect on teacher report of externalizing at post-intervention, a marginally significant adverse effect was found in this group at 1-year follow-up. In contrast, the parent focus groups showed a marginally significant positive effect on teacher report of externalizing at post-intervention with no significant effects thereafter. Furthermore, the adverse effects found among the teen groups persisted through the 3-year follow-up. Specifically, conditions that included the teen focus groups (i.e. teen only and parent-and-teen groups) were associated with more tobacco use and delinquency than the control conditions (Dishion et al., 1999). Additionally, longitudinal growth modeling revealed significant "iatrogenic

growth" in both tobacco use and teacher report of externalizing symptoms over five data collection periods (Dishion, Poulin, & Burraston, 2001; Poulin et al., 2001).

Similar to the Bridges findings, in which adverse effects were found on adolescent report of externalizing symptoms only, the ATP study's adverse effect on externalizing symptoms was only found for teacher report. Such inconsistency in effects may suggest that these results were significant due to alpha inflation (increased likelihood of finding a significant effect when no effect actually exists), which occurs when a large amount of analyses are conducted. However, another possible explanation for this pattern may relate to context-consistent reporting by informants (Funder & West, 1993). Specifically, parent and adolescent reporters each contribute context-specific information, which tends to diverge in adolescence (Achenbach, McConaughy, & Howell, 1987). Discrepancies across informants (i.e. teachers, parents, and adolescents) can represent meaningful variability in how and when problem behaviors occur, such as in the classroom, at home, or elsewhere (De Los Reyes, Henry, Tolan, & Wakschlag, 2009). Adolescents' self-report may better capture externalizing behaviors exhibited during peer interactions, which often occur without direct adult supervision (Laird, Pettit, Dodge, & Bates, 1999). For these reasons, Bridges may have, in fact, shown beneficial effects on parent report of externalizing symptoms (which are likely exhibited in the home) in the full sample, while also producing an adverse effect on adolescent report of externalizing (reflecting change in externalizing behaviors in peer contexts) in a high-risk subsample in the Spanish group. Furthermore, the ATP study found an adverse intervention effect on teacher report of externalizing in the full sample (which included high-risk youth only),

with no effects on parent report and self-report was not collected (Dishion & Andrews, 1995; Dishion et al., 1999). Previous research suggests that parent reports are more focused on the daily, irritating problem behaviors, whereas teachers appear to focus on fewer and more specific behaviors, such as physical attacks on others and associations with deviant peers, which are better predictors of more severe problem behaviors later on (Bank, Duncan, Patterson, & Reid, 1993). In addition, the ATP study found that adverse effects on teacher report of externalizing symptoms persisted over follow-up periods, which would be unlikely if the initial finding was a result of alpha inflation. Whether the effect found in the Bridges study is also maintained across follow-up data collection requires further investigation.

The ATP study design includes several important features that were lacking in the two previously discussed studies, such as longitudinal data, random assignment, and a variety of outcome measures. However, further research may expand upon this area of research by investigating whether similar patterns of adverse effects are maintained over time and exhibit the same pattern of longitudinal growth when examined in the context of a study that meets the rigorous standards of randomized controlled trials (West, 2009). Furthermore, additional research is necessary in order to determine whether observed adverse effects, which have been primarily found for externalizing symptoms, also are associated with a later diagnosis of a disruptive behavior disorder.

Meta-Analytic Studies

The interpretation of adverse effects has become quite controversial. Specifically, some researchers have argued that such findings represent harmful outcomes that may

result from adolescent group interventions (Dishion et al., 1999), whereas other reviewers attribute these findings to methodological issues, such as sampling error (Mager et al., 2005; Weiss et al., 2005). As a result, meta-analyses have been conducted to verify whether a pattern of adverse effects exists across studies and conditions under which these effects may arise.

Ang and Hughes (2002) reviewed 38 treatment studies for youth ages 6 to 18. These authors found a total of 8 adverse effects with comparable outcomes between mixed and deviant-only groups. Their results also showed attenuated effects among groups comprised of deviant-only youth compared to individual treatments or mixed group treatments. Weiss and colleagues (2005) examined treatments for externalizing problems among 115 treatments that spanned 66 studies. These authors found that the presence of a peer component in an intervention actually *reduced* the likelihood of an adverse effect. The comparison of effects was also moderated by age, such that the probability of an adverse effect peaked at age 11 in programs with a peer component; whereas this probability peaked at age 8.6 in interventions without a peer group component. Finally, Lipsey's (2006) review of interventions for adolescents between 12 and 18 years old included 371 studies of prevention programs and 197 studies of probation programs. They found a positive association between positive program effects and amount of group leader experience, which supports the St. Louis Experiment finding that showed stronger adverse effects with inexperienced group leaders (Feldman, 1992; Mager et al., 2005). Similar to Weiss and colleagues (2005), Lipsey also found group format to be unrelated to the probability of adverse treatment effects and that group

leader experience moderated effects, such that positive program effects were attenuated when experience was low. In addition, participant risk level was related to larger positive effects among prevention programs, though higher risk participants showed smaller effect sizes in the probation studies. Age also moderated prevention effects in that attenuated effects were found among youth in the lower range of 12 to 18 years old.

Overall, these studies showed no significant moderating effects of factors including participant gender, duration of group, treatment type, behavior problem subtype, and group size (Lipsey, 2006; Weiss et al., 2005). Conversely, several studies have found significant moderated effects of group structure (Dishion et al., 2006; Mager et al., 2005; Smith, Dumas, & Prinz, 2006) and group leaders' experience and training (Feldman, 1992; Lipsey, 2006; Mager et al., 2005). In addition, moderation analyses also suggest that adverse and attenuated effects of group interventions are most likely around ages 11 or 12 (Lipsey, 2006; Weiss et al., 2005). These findings suggest that age at time of participation may influence adverse effects; it is also possible that intervention effects vary across age and development following adolescent participation. Specifically, adverse effects may vary across follow-ups, which might explain previously found inconsistencies of adverse effects measured across different discrete time points (Cho et al., 2005; Dishion et al., 1999; Poulin et al., 2001).

Adolescent Development and Externalizing Behaviors

Program effects on externalizing outcomes may vary throughout adolescence due to developmental shifts during this period. Neurological research has found pubertal changes in the brain's socio-emotional system to lead to increased risk-taking behavior

during the transition from childhood to adolescence (Steinberg, 2008). Specifically, drastic remodeling of the brain's dopaminergic system increases reward-seeking among adolescents, especially in the presence of peers. This internal shift may explain sudden inflations in behavioral problems among adolescents with no prior history of antisocial behavior (Moffitt, 1990) as well as findings of heightened risk for affiliating with antisocial peers among youth between the ages of 10 and 12 years old (Arnold & Hughes, 1999). External factors, such as parenting, may protect adolescents from externalizing problems during this transition. Indeed, research supports that parental monitoring moderates genetic and environmental risk for externalizing problems (Dick et al., 2009; Formoso, Gonzales, & Aiken, 2000) and that parental behavioral control can halt upward externalizing trajectories among adolescents with deviant peers (Galambos, Barker, & Almeida, 2003). However, adolescence often produces a shift away from parent-dominated contexts toward more peer-oriented interactions (McDowell & Parke, 2009), which may limit the protective effects of parenting. As adolescents approach adulthood, however, maturation of the frontal lobe improves executive control and self-regulation processes (Steinberg, 2008), which may compensate for diminished effects of parenting and lead to an overall decline in vulnerability for externalizing behavior. As a result, research has shown diminished rates of delinquent activities during late adolescence and early adulthood (Moffitt, 1990).

In fact, a temporary increase in adolescent externalizing behaviors, which is referred to as “adolescence-limited delinquency,” may represent adolescents’ responses to the widening gap between biological and social maturity (Moffitt, 1993). Therefore,

intervention effects that demonstrate this pattern in behaviors through adolescence may actually reflect a temporary bolstering effect on normative and adaptive coping processes.

Adolescent Risk for Externalizing Behaviors

Meta-analytic findings suggest that participant risk level at baseline may influence intervention response. Specifically, adolescent risk is associated with larger beneficial effects among prevention studies, whereas higher risk levels appear to attenuate positive group program effects within probation studies (Lipsey, 2006). These contrasting moderation effects are likely explained by the level of risk represented within study samples. Risk levels vary widely across samples in studies of universal prevention programs, selected prevention programs, and juvenile probation programs. So while the Bridges' finding of an adverse effect among "higher risk" youth in the Spanish groups may initially seem to contradict this previous research, it should be taken into consideration that these "higher risk" youth likely resemble the "lower risk" youth in previous studies of selected and indicated prevention programs and treatment groups, given that risk levels are relative to the distributions of the study samples. As such, further research may be needed to examine the role of pretest risk in predicting long-term intervention effects on adolescent externalizing symptoms. In addition, longitudinal growth modeling also provides an opportunity to examine the interactions between intervention effects and participants' pretest levels of externalizing symptoms on externalizing trajectories (Khoo, 2001; Muthén & Curran, 1997). This approach has not yet been used to study adverse effects of adolescent group interventions and it may

provide critical information for understanding the interplay between adolescent risk and intervention effects and its impact on adolescents' mental health trajectories.

Group Interventions and Adolescent Peer Processes

Developmental research has shown that peer interactions provide the key context that propels developmental risk for externalizing problems during early adolescence (Dodge, 2009, 2011; Steinberg, 2008; Steinberg & Monahan, 2007). So while adolescent group interventions are designed to promote positive peer relations and competencies (Caplan et al., 1992; Dishion et al., 1999; Weissberg et al., 1997), they also present adolescents with greater opportunities to engage in problem behaviors (Laird et al., 1999). Dodge, Lansford, & Dishion (2006) identify two peer mechanisms that account for adverse effects of group interventions: (1) deviant peer contagion, described as the transmission of deviant behavior across peers, and (2) deviancy training, which occurs when adolescents receive positive reinforcement from peers for deviant behavior. Intervention and observational studies have found both of these mechanisms to be associated with growth in deviant behavior during early adolescence (Dishion, Capaldi, et al., 1995; Dishion, Eddy, Haas, Li, & Spracklen, 1997; Dishion et al., 2001; Dishion, Spracklen, Andrews, & Patterson, 1996). For instance, Dishion and colleagues found that deviancy training processes during group intervention sessions accounted for longitudinal growth in tobacco use and externalizing behaviors (Dishion et al., 2001). However, participation in group interventions, which often encourage youth to join and actively participate in peer group contexts within and outside of group sessions, may also orient adolescents more toward their peers and lead to increased interactions with and

exposure to peers even after the groups have ended. Such shifts in peer processes immediately following an intervention may explain the link between adolescents' involvement within a time-limited group intervention and changes in their externalizing symptoms years later. The current study seeks to examine whether the peer factors of association with deviant peers and peer attachment mediate the effect of group interventions on adolescents' externalizing outcomes.

The influence of association with deviant peers on externalizing behaviors is well-established, and this process is often referred to as deviant peer contagion (Deković, 1999; Gonzales & Dodge, 2009; Prelow et al., 2002). Early adolescence is related to particularly heightened risk for associating with antisocial peers (Arnold & Hughes, 1999), which predicts growth in externalizing symptoms over time (Dodge et al., 2009; Dodge, Greenberg, & Malone, 2008). In addition, discussions of deviancy are often the basis of peer interactions among high-risk youth (Dishion et al., 1996), which is reflected in high levels of behavioral concordance in youth friendships (Newcomb & Bagwell, 1995). In addition, adolescents who exhibit deviant behavior tend to coalesce into antisocial peer groups between the ages of 10 and 14 (Dishion, Patterson, Stoolmiller, & Skinner, 1991). Thus, interventions that aggregate youth may lead high-risk youth to more actively seek out delinquent peers.

Whereas peer contagion appears to explain the links between association with deviant peers and subsequent externalizing symptoms, deviancy training may explain how peer processes that include positive reinforcement and other forms of peer interactions might enhance externalizing symptoms among high risk youth. Adolescents

in group interventions may experience greater peer attachment, represented by feelings of trust, support, and acceptance among friends, following a group intervention. Increased peer attachment may present problems among high risk adolescents, as these individuals may begin to feel more comfortable and inclined to exhibit problem behaviors among their peers, which may lead to increased deviancy training. Research findings have shown that friendships provide antisocial youth with another context in which to practice deviant behaviors, such as using directives and negative reciprocity, and that antisocial youth feel relatively satisfied with their friendships despite these coercive exchanges (Dishion, Andrews, & Crosby, 1995). Furthermore, as high-risk adolescents become more attached to their peers, they may be more likely to inflate their own problem behaviors to match their overestimations of peers' problem behaviors, which is common among adolescents (Dishion et al., 1996; Prinstein & Wang, 2005). The current study will test both of these peer factors as mediators of adolescent group program effects on externalizing symptoms in order to better understand how these peer processes may play a role in producing adverse effects.

Acculturation and Adverse Effects among Latino Adolescents

Research suggests that Hispanic youth have considerable health needs but receive the least amount of mental health services compared to White and other minority groups (Rawal, Romansky, Jenuwine, & Lyons, 2004). Results from the National Longitudinal Study of Adolescent Health found higher rates of both violent and nonviolent delinquency among Hispanic youth compared to Caucasian and African American youth (Leiber, Mack, & Featherstone, 2009; McNulty & Bellair, 2003). Although Hispanic

youth are rarely included in studies of adverse effects, results from one study suggest that these youth may be especially at risk for adverse effects (Cho et al., 2005). In their effectiveness trial of the Reconnecting Youth program, Cho and colleagues discovered positive effects on delinquency with mixed effects on smoking and drug use across the two participating school districts, with greater adverse effects in the district that consisted of 87% Hispanic students at 6-month follow-up. These analyses did not account for ethnic differences, though they suggest that further research may be necessary to identify the factors that might enhance risk for adverse effects of group interventions among Hispanic youth.

Risk for externalizing problems appears to vary among Latino adolescents, which is a highly heterogeneous group. Studies of Mexican American (MA) youth have found higher rates of problem behavior and school dropout among more acculturated and U.S.-born youths than for less acculturated, immigrant youth (Gonzales, Knight, Morgan-Lopez, Saenz, & Sirolli, 2002; Portes & Macleod, 1996; Rumberger, 1995). Differential rates of behavioral problems among MA adolescents have been found based on language (Spanish vs. English), a prime indicator of acculturation level. Less acculturated (Spanish-speaking) MA youth have been shown to be at decreased risk for externalizing problems compared to their more acculturated (English-speaking) counterparts (Gonzales et al., 2008). Less acculturated adolescents experience higher levels of parental monitoring (Samaniego & Gonzales, 1999), are also more family oriented, and are less involved in peer groups and activities outside of the family (Rueschenberg & Buriel, 1989). When less acculturated youth do spend time with peers, they form tightly knit

groups with peers who are similarly adult-oriented (Matute-Bianchi, 1986). As a result, less acculturated adolescents have less exposure than their more acculturated counterparts to peer processes (Allen et al., 2008; Unger et al., 2000) and influences (Wall, Power, & Arbona, 1993) that are known to contribute toward increased externalizing problems and risk-taking behavior in mid-adolescence. Given that prior outcome analyses of the Bridges efficacy trial which have shown numerous moderation effects of language as well as these findings that suggest that acculturation predicts MA adolescent's externalizing problems and relevant peer processes, the current study examined language as a potential moderator of program effects on externalizing problems.

THE CURRENT STUDY

The current study used data from a long-term follow-up of this randomized trial, including data from adolescents and mothers across five time points extending from ages 12 to 18 to test the following:

1. Whether the adverse effects observed at 1-year posttest are lasting and harmful by examining long-term effects at 2- and 5-year posttests on externalizing symptoms and diagnosed disruptive behavior disorders. Mental health diagnoses, which were only assessed in late adolescence, will allow a test of the clinical significance of the adverse effects. Language and pretest externalizing symptoms were examined as potential moderators of intervention effects.
2. Whether the intervention altered developmental trajectories of MA adolescent externalizing behavior through longitudinal growth modeling with 5 waves of data extending from middle school (age 12) through high school (age 18).

3. Whether the adverse intervention effect on externalizing is mediated by peer processes of peer attachment and association with deviant peers. These mediators were chosen to represent aspects of peer exposure that may have changed in response to the peer-group intervention, potentially inadvertently increasing peer-related externalizing behaviors for some vulnerable youth that received the intervention.

METHODS

Participants

Participants included 516 MA adolescents and 494 of their mothers from the efficacy trial of the Bridges to High School Program in a southwestern metropolitan area. MA families were contacted through school rosters and were deemed eligible if they (1) identified themselves as MA, (2) included an adolescent in the 7th grade, and (3) at least one primary caregiver agreed to participate.

Procedures

Recruitment and randomization. Three cohorts of families were recruited and randomized by the research team, not the schools (see Figure 1). In the first semester of each school year, Hispanic 7th graders were randomly selected from school rosters with data indicating 'primary language spoken in the home' used to select English and Spanish recruitment samples. A phone call described the intervention and determined eligibility according to the following criteria: the adolescent was of Mexican descent, at least one caregiver of Mexican descent was interested in participating, and the family was willing to be randomly assigned to the 9-week intervention or a brief workshop (control group).

Families that agreed to participate designated the predominant language used in their family and this determined their placement in either the English or Spanish subsample. When available, both caregivers were invited to participate in the intervention; only those that agreed were interviewed at pretest and subsequent follow-ups. At the beginning of the second semester (immediate posttest), the study methodologist used a random number generator programmed with the appropriate probabilities to randomize all families who completed pretest data collection and were still eligible. A greater proportion of families were randomized to the intervention than control to ensure adequate intervention group size at each school. A greater proportion of English-speaking families were randomized to the intervention than control (70/30) compared to Spanish-speaking families (60/40) because pilot testing showed higher retention for Spanish-speaking families (Gonzales, Dumka, Deardorff, Carter, & McCray, 2004). School personnel were blind to condition assignment.

The sample included 254 adolescent males (49.2%) and 262 females (50.8%) with an average age of 12.3 years (SD = .54). The majority were in two-parent families (83.5%, n =431). English (47%, n =241) and Spanish (53%, n =275) subsamples differed significantly on several demographic variables. For instance, the Spanish subsample had lower incomes, were substantially more likely to be immigrants, and were less acculturated than the English sample. A comparison of baseline measures between condition assignment groups are presented in Table 1.

Data collection. Data collection occurred prior to the intervention (pretest), immediately after the intervention (immediate posttest), one year after the intervention

(1-year posttest), two years after the intervention (2-year posttest), and 5 years after the intervention (5-year posttest). Parent and adolescent data were collected through in-home, computer-assisted interviews. Each family member received $30 for each assessment.

Intervention condition. Bridges employed three primary components: (a) a parenting intervention; (b) an adolescent coping intervention; and (c) a family strengthening intervention. A school liaison also was available to help families apply program skills to address school related problems. Components were delivered in 9 weekly 2-hour evening group sessions at the adolescents' schools and 2 home visits (pre-intervention and mid-program). The 9-session adolescent groups aimed to increase adolescents' (a) coping efficacy; (b) academic engagement; and 3) family cohesion. Active learning methods were used to teach a range of coping strategies to manage interpersonal and school stressors, and group processes were structured to enhance coping efficacy. Adolescents explored possible selves to increase academic motivation and engagement. Adolescents also learned strategies to balance family relationships and obligations with school, activities, and friends. All components were designed to optimize cultural competence.

Of families randomized to Bridges, 64% attended at least 5 and 33% attended all 9 sessions. Spanish families attended significantly more sessions (Carpentier et al., 2007); mean number of sessions was 6.15 for adolescents in the Spanish sample, and 4.91 in the English sample.

Control condition. Parents and adolescents jointly attended a single 1.5 hour evening workshop that was conducted at the school on a different night and by different group leaders than the intervention. Participants received handouts on school resources, discussed barriers to school success, and developed their own family plan to support middle school success. In contrast to the intervention, this workshop did not teach specific skills to promote school success.

Measures

All measures used in the present study were obtained from the larger parent interview battery. Published translated versions of the measures were used when available. Measures that were not available in Spanish were translated and back translated by fluent Spanish and English speakers (Foster & Martinez, 1995). All scales were investigated and refined to meet the requirements for strong language (English-Spanish) invariance (Millsap, 2011). Preliminary data cleaning analyses were done to identify and correct any errors in data collection or recording. Descriptive statistics, estimates of skewness and kurtosis, and alpha coefficients for study measures are presented in Table 2. All measures have very good internal conistency. For intercorrelations of study variables, see Table 3. Items lists for all scales are provided in the Appendix.

Externalizing symptoms. Adolescent externalizing symptoms were measured through adolescent report on the Youth Self Report (YSR; Achenbach, 1991) at pretest through 2-year posttest and the Adult Self-Report (ASR; Achenbach & Rescorla, 2001) at 5-year posttest. Mothers reported on the Child Behavior Checklist – Parent Form

(CBCL-PF) at pretest through 2-year posttest and the Adult Behavior Checklist (ABCL) at 5-year posttest. Sample items from the YSR and CBCL include "I break rules at home, school, or elsewhere" / "My child breaks rules at home, school, or elsewhere" and "I tease others a lot" / "My child teases a lot." The response options include 0 "not true," 1 "somewhat or sometimes true," and 2 "very true or often true." Item scores were summed into a total score for each reporter with higher scores indicating higher levels of externalizing symptoms. Previous analyses were conducted using the raw sum scores of all externalizing items. However, in order to produce a common scale across the child and young adult age group measure (i.e. from YSR to ASR and from CBCL to ABCL) factor scores were created for self-report and mother report of externalizing symptoms (see Wong, Toomey, & Millsap, in preparation). Factor scores were computed using item response theory (IRT) to link measures through overlapping item content across scales (Kolen & Brennan, 1995). This produced factor scores within each report that were scaled such that corresponding measures could be included in a longitudinal growth model with interpretable results. These factor scores were also used in other analyses given that they produced a more consistent measurement of externalizing symptoms across time.

Externalizing disorder diagnosis. Diagnosis of an externalizing disorder was based on adolescent and mother report on the Diagnostic Interview Schedule for Children (CDISC), a highly structured diagnostic interview of mental health disorders in youth based on the DSM diagnostic system (American Psychiatric Association, 1994). Through this measure, adolescents and parents are presented with statements that

correspond to diagnostic criteria and asked whether each statement was true of the adolescents' behavior within the past year. For instance, items within the Conduct Disorder subscale include "initiates physical fights" and "stays out late." A binary variable was created to represent diagnosis of an externalizing disorder, with 1 indicating that the criteria were met for either Conduct Disorder, Oppositional Defiant Disorder, or Attention-Deficit/Hyperactivity Disorder based on either mother or adolescent report. This variable was coded 0 for participants whose self- and mother report did not meet the criteria for any of these three externalizing disorders. These data are only available at 2- and 5-year posttests, with observed rates of 7% (n = 29) and 6% (n = 26) of adolescents who met diagnostic criteria for an externalizing disorder at 2- and 5-year posttests, respectively.

Peer factors.

Peer attachment. Peer attachment was assessed with a 9-item scale drawn from the Inventory of Parent and Peer Attachment (Armsden & Greenberg, 1987) that addresses peer support, communication, and trust. This scale has shown strong internal consistency with alpha coefficients ranging from .87 to .91 across four ethnic groups: African American, Anglo, (English- and Spanish-speaking) Mexican American middle school students (Gonzales & Jackson, 1996). Support for external validity has been established (Laible, Carlo, & Raffaelli, 2000). Adolescents were presented items, including "My friends respected my feelings" and "I could count on my friends when I needed to talk," and asked how much each item was true in the past month. Response options range from 1 "almost never" to 5 "almost always or always."

Deviant peer association. Association with deviant peers was measured with a 15-item self-report scale using items adapted from the Denver Youth Survey (Mason, Cauce, Gonzales, & Hiraga, 1994), Dishion, Patterson, Stoolmiller, and Skinner's (1991) measure of delinquent peers, Huizinga, Esbensen, and Weiher's (1991) measure, and new items based on previous literature. This measure presents items to adolescents and asks how many of their friends did the stated behaviors in the past month. Sample items include "Been in gang fights" and "Started rumors or told lies" The response scale ranges from 1 "none" to 5 "almost all." This measure has strong internal consistency ($\alpha = .90$) and demonstrated external validity (Germán, Gonzales, & Dumka, 2009) in the current sample.

Intervention status. A binary variable represented random assignment to either the intervention or control condition (0 = control, 1 = intervention). Intent-to-treat analyses were employed using this variable such that participants' data were analyzed based on their random assignment, regardless of intervention compliance or attendance.

Language. A binary variable was used to denote predominant family language as selected by family preference (0 = Spanish, 1 = English).

Gender. A binary variable (0 = male, 1 = female) represented adolescent gender. This variable was included as a covariate in all study analyses, though it has not been found to significantly moderate Bridges intervention effects (see Gonzales et al., 2011).

ANALYSES

Preliminary Analyses

Preliminary analyses have been conducted to rule out the possibility that the observed adverse program effects at 1-year posttest resulted from limitations in randomization, heterogeneity of group composition within the intervention condition, or extreme cases.

Randomization. Randomization was successful in achieving balance between the two conditions. T-tests of pretest measures comparing intervention and control conditions showed no significant differences on any study variable with low standardized effect sizes (see Table 3).

Heterogeneity of Intervention Groups. Randomization was successful in producing homogeneity of mean externalizing scores across intervention groups. One-way random effects ANOVA with intervention groups as the factor did not show significant differences on any measure of externalizing (all intraclass correlation coefficients < 0.03, all $p > .10$).

Johnson-Neyman. The Johnson and Neyman (1936) technique showed that adverse intervention effect at 1-year posttest occurred in Spanish group adolescents who reported a pretest score of 12 or higher, which constitutes 24% of the Spanish subsample ($n = 66$). These analyses were also conducted using the factor scores that were created for the externalizing scale, showing an adverse effect among high externalizers (at the 72nd percentile and above on the pretest factor score distribution), which constitutes 28% of the full sample ($n = 144$).

Analytic Plan

All analyses were conducted in Mplus version 6.1 (Muthén & Muthén, 2010), employed full information maximum likelihood (FIML) estimation to handle missing data. FIML estimation is a less biased procedure for handling data that are missing at random (MAR) than listwise or pairwise deletion strategies (Enders & Bandalos, 2001; Enders, 2010) that also retains the highest number of participants for the analyses.

Follow-up Outcome Analyses. The first Aim seeks to evaluate intervention effects on externalizing symptoms and disorder diagnosis at the 2-year and 5-year posttests. Analyses of covariance estimated effects on adolescent and mother reports of symptom levels separately at each time point. Potential moderation of intervention effects by language and pretest externalizing symptoms were tested through the inclusion of covariate by intervention interaction terms in the model. Specifically, the intervention status (binary), pretest externalizing score, adolescent gender, and language variables (binary) were included as predictors. Three two-way interactions (intervention by pretest externalizing, intervention by language, and pretest externalizing by language) and a single three-way interaction (pretest externalizing by intervention by language) were included in each model. The language group and pretest externalizing scores were centered to have a mean of zero prior to creating interaction terms (Aiken & West, 1991). If the two-way interaction between intervention status and pretest externalizing was significant (either within the full sample or in a language subsample), then the interaction was probed using "pick-a-point" procedures (Rogosa, 1980) to examine the intervention effect at two extreme levels of pretest externalizing (15^{th} and 85^{th} percentiles) within the

relevant language group. If interventions status was a significant predictor of the outcome at either extreme of the pretest externalizing distribution, then a simple intervention effect size was computed by converting the t-statistic at the specific point into a Cohen's *d* (1988) statistic.

Parallel procedures were used to examine intervention effects on externalizing disorder diagnosis at the 2-year posttest and 5-year posttest using logistic regression. Estimates were kept in the logit metric in order to maintain a linear relationship between predictors and the outcome variable.

Longitudinal Growth Analyses. The second Aim seeks to examine intervention effects on adolescent externalizing trajectories. A longitudinal growth model (see Figure 2) tested intervention effects on immediate posttest condition group differences, linear slope at immediate posttest, and quadratic change (deceleration of growth) of externalizing trajectories over time. Externalizing was centered (time = 0) at the immediate posttest to study change relative to the initial intervention effect (see Biesanz, Deeb-Sossa, Papadakis, Bollen, & Curran, 2004). Potential interactions between intervention and participants' pretest levels of externalizing symptoms on growth curve parameters were also tested to detect potential differences in intervention effects on growth based on individuals' pretest externalizing levels (Khoo, 2001; B. O. Muthén & Curran, 1997). Language group and gender were included as covariates. Analyses were run separately using self- and mother reports of externalizing.

Interactions were evaluated before main effects and, if not significant, were dropped from the models. Significant intervention by language interactions led to

separate evaluations of lower order effects by language. Any significant intervention by pretest externalizing effect was probed (in the full sample or language subsamples, as needed) by evaluating the intervention effects separately at the 15^{th} and 85^{th} percentiles on the pretest externalizing distribution.

Peer Mediator Analyses. To test whether intervening peer processes mediate intervention effect(s) on externalizing symptoms three mediation models tested the variables of peer attachment and deviant peer association. Possible effects of pretest externalizing in moderating the mediated effects were examined (see Figure 3; MacKinnon, 2008) by including the interaction between intervention status and pretest externalizing as a predictor in the model. The pretest externalizing score were centered prior to creating interaction terms (Aiken & West, 1991). Pretest scores of deviant peer association were included as a covariate in the deviant peer association models (pretest peer attachment data were not available). Language group and gender were controlled for. Adolescents with relatively higher pretest levels of externalizing symptoms were expected to report greater program effects on each peer factor at immediate posttest, leading to a significant increase in externalizing at 1-year posttest.

RESULTS

Follow-Up Outcome Analyses

Results from the ANCOVAs were conducted for self-reported externalizing symptoms at 1-year posttest, 3-year posttest, and 5-year posttest based on factor scores (see Table 4). These factor scores produced results that were similar to those found with the raw sum scores used in original outcome analyses; however, the raw sum scores

showed a significant adverse effect in the Spanish subsample among youth with high pretest externalizing symptoms (Gonzales et al., 2012), whereas the analyses with the factor scores showed a significant effect in the full sample moderated by pretest externalizing. Specifically, there was a significant two-way interaction between intervention status and self-reported pretest externalizing on externalizing at 1-year posttest (b = 0.27, SE = 0.09, p < .01). Significantly lower (15th percentile) externalizing symptoms were found in the intervention group compared to the control group (d = -.18) among youth with low pretest externalizing scores; significantly higher externalizing symptoms in the intervention group than the control group (d = .20) among those with high (85th percentile) pretest externalizing scores (see Figure 4). These factor scores were created in order to have a consistent measure of externalizing across time, which was necessary for latent growth analyses, but also provide a more psychometrically sound measurement of externalizing symptoms for single time-point analyses, such as these ANCOVAs. Indeed, using these scores showed that the adverse intervention effect that was found at 1-year posttest among high pretest externalizers was not specific to the Spanish group, as was observed using the raw externalizing scores in the original outcomes analyses. Graphs of the distribution of these factor scores and how they relate to the clinical cut-offs, according to Achenbach, are presented in Figures 5 and 6. A significant two-way interaction between intervention status and pretest externalizing was also found in the model predicting externalizing symptoms at 2-year posttest (b = 0.22, SE = 0.10, p = .03). Although a significant intervention effect was found among youth with low (15th percentile) pretest externalizing scores, with lower externalizing symptoms

in the intervention condition compared to the control condition (d = -.19), no significant intervention effect was found among high pretest externalizing youth (85th percentile; d = .01; see Figure 7). No significant moderated or main intervention effects were found on self-report of externalizing symptoms at 5-year posttest.

Follow-up analyses were also conducted with mother report of adolescent externalizing symptoms. These results revealed no significant interactions at 1-year, 2-year, or 5-year posttests. However, there was a significant main effect of the intervention on mother report of externalizing at 1-year posttest (b = -0.12, SE = 0.06, p = .04) and 2-year posttest (b = -0.19, SE = 0.07, p < .01), with significantly lower externalizing symptoms reported by mothers in the intervention condition than the control condition at both time points. The sizes of these effects fell within the range of what is considered small (d = -0.19, d = -0.27, respectively).

Results from logistic regressions predicting diagnosis of an externalizing disorder showed no moderated or main interventions effects for diagnosis at 2-year posttest, but a significant moderated intervention effect on diagnosis at 5-year posttest. Specifically, there was a significant two-way interaction effect between intervention status and pretest externalizing symptoms (b = 1.532, SE = 0.61, OR = 4.63, p = .01). Probing of this effect showed a significant intervention effect (b = -1.75, SE = 0.69, OR = 0.17, p = .01) at the 15th percentile of pretest externalizing only, with a lower rate of a diagnosis in the intervention condition compared to the control condition (d = -.24), and a non-significant intervention effect (b = 1.67, SE = 0.60, OR = 1.18, p = .78) at the 85th percentile of the pretest externalizing distribution (d = .01).

Longitudinal Growth Models

To construct a longitudinal growth model of intervention effects on trajectories of externalizing symptoms over time, first a growth only model was created to determine the type of growth model that would best fit the data. Given the a priori hypothesis that a quadratic model would best fit the data, a quadratic model was first tested. This model estimated growth in externalizing symptoms across immediate, 1-year, 2-year, and 5-year posttests including estimation of quadratic growth, without random effects. The model was time centered at 1-year posttest, since this was the time point at which the adverse effect was found. Thus, linear growth at 1-year posttest and the intercept (externalizing levels) at 1-year posttest were also estimated. According to this model, self-report of externalizing symptoms was shown to vary in the rate of change over time with a significant quadratic term (b = -0.02, SE = 0.01, $p < .01$), which suggests that the rate of change in externalizing symptoms significantly changes over time (see Figure 8). The results also show significant variance in level (b = 0.50, SE = 0.03, $p < .01$) and slope (b = 0.02, $SE < 0.01$, $p < .01$) of externalizing symptoms at 1-year posttest. As predicted, however, the slope at 1-year posttest was not significant (b = -0.15, SE = 0.02, $p = .41$). This model was found to adequately fit the data[1], CFI = 0.94, SRMR = 0.05.

This model continued to show adequate fit to the data once the predictors (intervention status, pretest externalizing, intervention status x pretest externalizing, gender, and language group) were added to the model, CFI = 0.95, SRMR = 0.02. The

[1] This quadratic model fit the data better than a piecewise model ($AIC_{quadratic}$ = 3631.16, $BIC_{quadratic}$ = 3687.50 vs $AIC_{piecewise}$ = 3691.62, $BIC_{piecewise}$ = 3710.39) which was also run. The piecewise model included piece 1 with slope from immediate posttest to 1-year posttest, and a second piece from 1-year posttest to 5-year posttest.

results of the predictor effects on the intercept and linear growth of this model are presented in Table 5. A significant intervention status by pretest externalizing effect was found on the intercept (representing externalizing symptoms levels at 1-year posttest; b = 0.20, SE = 0.08, p = .01), but this interaction term did not show significant effects on the linear growth at this time point (b = -0.01, SE = 0.03, p = .78). Probing this interaction effect at the 15th and 85th percentiles of the self-report pretest externalizing symptom distribution showed a significant intervention effect on self-report externalizing symptoms at 1-year posttest among adolescents at the 15th percentile (b = -0.17, SE = 0.08, p < .05), and a marginally significant intervention effect at the 85th percentile (b = 0.18, SE = 0.10, p = .05). Specifically, the intervention condition had fewer externalizing symptoms at 1-year posttest than the control condition among adolescents with low pretest externalizing, whereas adolescents in the intervention condition had marginally significantly higher externalizing symptoms at 1-year posttest than the control condition among youth with high pretest externalizing symptoms. This effect is parallel to that found in the follow-up ANCOVA analyses (see above), although the adverse effect found among high pretest externalizers here is only marginally significant. Among the other predictors, language group showed a marginally significant effect on the intercept and a statistically significant effect on linear growth of self-report externalizing symptoms. The linear rate of growth of symptoms was higher among adolescents in the English group as compared to the Spanish group. Gender showed no significant effects on the intercept or linear growth for self-report of externalizing. Pretest externalizing symptoms significantly predicted of the intercept (b = 0.52, SE = 0.07, p < .01) and linear growth (b

= -0.05, *SE* = 0.02, *p* = .03) at 1-year posttest. Participants with higher pretest externalizing had higher symptom levels and a greater decline in externalizing symptom growth at 1-year posttest.

A parallel quadratic model was also used to estimate longitudinal growth in mother report of externalizing, first without predictors to determine whether this type of growth depicted the mother report data adequately. This quadratic model centered at 1-year posttest without random effect for quadratic growth appeared to fit the mother report data well, CFI = 0.99, SRMR = 0.03. The results from this model showed that mother report of externalizing symptoms also had a quadratic trend, but in the opposite direction from self-report. Specifically, the quadratic term for mother report was significantly positive (*b* = 0.18, *SE* < 0.01, *p* < .01), whereas it was negative for self-report. Figure 9 demonstrates this trend with a plot of the observed and estimated means for mother report of externalizing symptoms across immediate posttest to 5-year posttest. Similar to the self-report model, this model was centered at 1-year posttest, and showed a significantly negative slope at this time point (*b* = -0.09, *SE* = 0.02, *p* < .01).

Next, the predictors were added to this quadratic mother report model (see Table 5), which also showed adequate fit to the data, CFI = 0.99, SRMR = 0.02. The predictors showed a significant interaction effect between intervention status and pretest externalizing on the linear slope at 1-year posttest (*b* = -0.03, *SE* = 0.02, *p* < .05). Probing of this interaction at the 15^{th} and 85^{th} percentiles of pretest externalizing showed that there was a marginally significant effect of the intervention on the slope of mother report of externalizing symptoms at 1-year posttest among mothers who reported externalizing

symptoms at the 15th percentile of the pretest distribution (b = 0.04, SE = 0.02, p = .09), with a non-significant intervention effect among mothers who reported high pretest externalizing symptoms (b = -0.02, SE = 0.02, p = .29). Specifically, this analysis showed that mothers in the intervention condition reported fewer externalizing symptoms than mothers in the control condition at 1-year posttest for adolescents with lower levels of reported pretest externalizing symptom. There were no intervention group differences among adolescents whose mothers reported high pretest externalizing symptoms. Although the intervention status by pretest externalizing effect was not significant on the intercept of growth in mother report of externalizing symptoms (i.e. externalizing symptoms at 1-year posttest), there was a significant main effect of the intervention on externalizing at this time point (b = -0.11, SE = 0.05, p = .02). Mothers reported lower levels of externalizing symptoms at 1-year posttest in the intervention condition compared to the control condition. This effect parallels the significant main effect on mother report of externalizing at 1-year posttest that was found in the follow-up ANCOVA analyses (see above). Furthermore, there were significant effects of pretest externalizing symptoms, language group, and gender on the intercept. Mothers reported higher externalizing symptoms at 1-year posttest for high pretest externalizers compared to low, English compared to Spanish, and females compared to males. However, none of these predictors showed significant effects on the linear growth at 1-year posttest.

Peer Mediation Analyses

Neither deviant peer association nor peer attachment were found to significantly mediate intervention effects on self-report of externalizing symptoms at 1-year posttest

(see Figure 3 and Table 6). The level of deviant peer association at immediate posttest significantly predicted externalizing at 1-year posttest (b = 0.45, SE = 0.06, p < .01), with greater associations with deviant peers predicting greater externalizing symptoms. However, no moderated (b = 0.05, SE = 0.06, p = .45) or main (b = -0.02, SE = 0.05, p = .74) effects of the intervention were found on deviant peer association, resulting in a non-significant mediation effect of the intervention on externalizing symptoms using PRODCLIN (MacKinnon, 2008; ab = -0.01, CI[-0.05, 0.03]). In contrast, there was a significant two-way interaction effect between intervention status and pretest externalizing on peer attachment at immediate posttest (b = -0.21, SE = 0.10, p = .04). Probing of this effect showed a marginally significant intervention effect on peer attachment at immediate posttest among adolescents (b = -0.21, SE = 0.12, p = .07) at the 85th percentile of the pretest externalizing distribution, with less peer attachment in the intervention condition than the control condition. There was no intervention effect (b = 0.16, SE = 0.12, p = .16) at the 15th percentile of pretest externalizing. Peer attachment at immediate posttest did not significantly predict externalizing symptoms at 1-year posttest (b = -0.05, SE = 0.05, p = .28), leading to a non-significant mediation effect at either the 15th (ab = -0.01, CI[-0.03, 0.01]) or 85th (ab = 0.01, CI[-0.01, 0.04]) percentile of the pretest externalizing symptom distribution.

DISCUSSION

The current study explored an unexpected adverse effect of the Bridges to High School adolescent group intervention. Given prior research findings that have shown adverse effects among other adolescent group interventions, several sets of analyses were

conducted to determine whether the observed effect on externalizing behaviors was lasting and meaningful. Follow-up outcome analyses using both mother and adolescent self-report data showed no indication of an adverse effect beyond that which was originally observed with adolescent self-report data collected one year following the intervention. Furthermore, a beneficial intervention effect was found on externalizing disorder diagnoses at 2-year posttest, with no signs of adverse effects on this outcome. Longitudinal growth analyses showed no signs that trajectories of adolescent self-report of externalizing symptoms were significantly impacted by the intervention, although the intervention showed a consistently positive influence on mother report of adolescent externalizing symptoms. Finally, findings from mediation analyses suggested that neither deviant peer association nor peer attachment accounted for the observed adverse effect on externalizing symptoms. Overall, these study results show no signs that the observed adverse effect was maintained over time or appeared in clinical measures or across reporters of externalizing symptoms.

Prior reviews of adolescent group interventions have reported that adverse effects on adolescent behavior occur in 24%-29% of evaluation studies of group based interventions targeting adolescent behavior (Ang & Hughes, 2002; Lipsey, 1992). Thus, even the largest estimates of the frequency of this phenomenon are still well within the minority of all studies, supporting that adverse effects are more likely the exception than the rule. The results of the current study, which included data from a randomized controlled trial of a universal prevention program, further suggest that it is unlikely that group formats inherently lead to adverse effects for this type of intervention as the

Bridges prevention program did not appear to produce an actual lasting and meaningful adverse effect. Furthermore, previous research has found adverse effects to be more likely in groups that aggregate high-risk youth (Dodge et al., 2006), are unstructured (Dishion et al., 2006; Mager et al., 2005; Smith et al., 2006), and led by individuals with lower levels of training and experience in the field (Feldman, 1992; Lipsey, 2006; Mager et al., 2005). These conditions were not met in the Bridges program, which was provided to youth across a range of risk levels (since it was a universal prevention program), using a structured, manualized curriculum, and implemented by group leaders with prior clinical experience who were provided with pre-service and ongoing training and extensive supervision. Therefore, any prior adverse effect that arose from the Bridges program would theoretically be unlikely to have the same level of severity or nature as the adverse effects that have been found in previous studies that have met these conditions.

It is important to consider why an adverse effect might be observed among an array of positive program effects across adolescent outcomes. One explanation that has been proposed by Weiss and colleagues (2005) is sampling error. Specifically, that each intervention study traditionally includes analyses of many outcomes, leading to an increased possibility of finding significant effects, regardless of whether a true effect actually occurred. One way to guard against Type I error would be for future studies to examine multiple outcomes in order to explore patterns across them. It has been recommended that researchers use results from across an overall trial as a guide to the direction of effect in subgroup, rather than overemphasizing an apparent observed effect

within one subgroup (Yusuf, Wittes, Probstfield, & Tyroler, 1991). For instance, in the Bridges efficacy trial, beneficial intervention effects were found across mother, father, teacher and adolescent self-report of externalizing symptoms, as well as self-report on related outcomes, including substance use. This provides a context in which to interpret an adverse effect that occurred at one time-point and for only one reporter.

Second, it is possible that self-reports of adolescents' externalizing symptoms are less valid than mother report, which may have produced an erroneous adverse effect. However, beneficial effects were also found using adolescent self-report measures, though only among those with low pretest externalizing symptoms, at the same time points as the main effects (i.e. 1-year and 2-year posttest), showing consistency of this beneficial effect not only within but also between reporters. This pattern of findings would be highly unlikely if the adolescent self-report measure of externalizing symptoms were invalid. Furthermore, the positive main effects of the intervention that were found on mother report of adolescent externalizing behaviors might indicate that this family-focused intervention was most beneficial in improving the mother-child relationship (as was also found among improvements in mothers' parenting behaviors) and decreasing adolescent negativity and defiance, particularly within the home environment. Prior research supports that mothers are better reporters of daily, irritating behaviors of their children than clinically relevant externalizing symptoms (Bank et al., 1993). The Bridges intervention was found to decrease these types of behaviors, which is important for promoting adolescent resilience.

Another possibility is that the program did impact externalizing symptoms among high pretest externalizers at 1-year posttest that was only reflected in adolescent self-report as the behaviors that were influenced were only exhibited in unsupervised contexts that were not captured in mother report of behavior. If such an effect did occur, the current results indicate that it was neither lasting, meaningful, or had implications for overall patterns of externalizing over time, which is incongruent with the adverse effects that have been discovered in prior research (see Dishion et al., 1999 for review). For instance, Dishion and colleagues (2001; 2001) found adverse effects of ATP to influence growth in externalizing symptoms and tobacco use across 3 years following the intervention, whereas the Bridges effect was limited to 1-year posttest. Furthermore, the ATP effect was found for teacher reports, which captures adolescent behaviors in supervised contexts.

Although a lasting, meaningful adverse effect on externalizing symptoms may not have occurred, it is important to consider whether and how Bridges may have influenced externalizing symptoms more generally. Specifically, the beneficial effects of Bridges on self- and mother report of externalizing symptoms appeared to fade after 2-year posttest. However, a beneficial effect on externalizing disorder diagnosis does arise at 5-year posttest. One might argue that the inconsistency in effects within measures across time suggests that Bridges did not produce a positive and lasting influence on adolescent externalizing. Alternatively, one might argue that different measures of externalizing symptoms may vary in their accuracy across adolescence, just as the externalizing behaviors themselves vary during this developmental period. As a result, an effect that

was found in externalizing symptoms in mid-adolescence may be more predictive of subsequent externalizing disorder diagnosis in later adolescence, than a constant effect on symptoms, especially since later adolescence is a time when externalizing symptoms generally tend to decrease (Moffitt, 1993).

Since the observed adverse effect of Bridges does not appear to be lasting or clinically meaningful, this might explain why neither deviant peer association nor peer attachment appeared to explain the adverse effect that was found among high pretest externalizers at 1-year posttest. Specifically, prior research and theory suggests that deviancy training and peer contagion (Dishion et al., 2006, 2001) account for increases in externalizing resulting from adolescent group interventions. Although both of the mediators explored in the current study were merely proxies of these two processes which may account for lack of mediation effects, it is equally plausible that peer processes do not account for the observed adverse effect because no truly meaningful adverse effect occurred.

Limitations and Future Directions

The current study had several limitations. First, direct behavioral observations of adolescent externalizing behaviors were not available. As such, method variance of each reporter of externalizing behaviors could not be ruled out as a factor contributing to observed effects. In the current study, both adolescent self- and mother report were used, and these reporters were not masked to condition assignment. As such, it is possible that expectancies or other reporter biases could have influenced the current findings. Nevertheless, since there appeared to be some consistency in intervention effects found

across adolescent and mother report of externalizing behaviors at corresponding time points, as well as the finding of significant effects despite the use of intent-to-treat analyses (which provide a rigorous test), it seems unlikely that reporter bias would predominantly explain the observed effects. Furthermore, intention-to-treat analyses typically provide a conservative test and can lead to effects even in the wrong direction under special conditions of both noncompliance and attrition.

Second, given that the Bridges efficacy trial was not designed with the intention of observing an adverse effect on externalizing symptoms, it did not include the possible measures that may be more related to adverse effects of group interventions than those that were available (i.e., deviant peer association and peer attachment). Specifically, deviancy training behaviors such as modeling, reinforcing, and exposure to conduct problem behaviors have been found to adversely affect adolescent behavior within (Dishion et al., 2001) and beyond (Dishion et al., 1997, 2001) group intervention contexts. Although researchers would be advised to make efforts to prevent these behaviors from occurring and escalating as a result of an intervention, measuring these behaviors during intervention group sessions as well as through data collection following the intervention may provide critical information about the conditions in which adverse effects are produced and for whom.

Summary and Implications

Group interventions are the most economical, convenient, and common solution to adolescent behavior problems (Ang & Hughes, 2002; Dishion et al., 2006; Spoth, Redmond, & Shin, 2000), a major issue in our society. Although there is a myriad of

studies that have demonstrated the beneficial effects of such programs, several studies have found group interventions to actually increase the problem behaviors that they were designed to ameliorate (Cho et al., 2005; Dishion, Capaldi, et al., 1995; Feldman, 1992; Leve & Chamberlain, 2005; Poulin et al., 2001), which has led to the controversy surrounding the potential adverse effects of adolescent group interventions. The current study explored an unexpected adverse effect that was found in an efficacy trial of the Bridges to High School program, a universal preventive intervention for middle school students, in order to determine whether the effect, which was found at 1-year posttest among youth with high pretest externalizing symptoms, was lasting and meaningful, and whether theory-based peer mediators might explain this finding. Results showed no signs of this effect at any time-point following 1-year posttest, in the clinical measures of externalizing problems, in mother report of externalizing symptoms, or in trajectories of adolescent externalizing symptoms across adolescence. Rather, results showed a beneficial intervention effect for externalizing symptoms based on self-report among adolescents with lower levels of externalizing symptoms at pretest at 1- and 2-year posttests, for the full sample based on mother report of externalizing symptoms at 1- and 2-year posttest, as well as for combined adolescent and mother report of externalizing disorder diagnosis at 5-year posttest among adolescents with low pretest externalizing symptom levels. Furthermore, the intervention produced steeper declines in externalizing symptoms among adolescents in the intervention condition compared to the control condition, showing the positive influence of the intervention on trajectories of externalizing symptoms over time.

Although the current study found no indication that the adverse effect of the Bridges program that was observed 1-year posttest was meaningful, lasting, or explained by theory-based mechanisms that underlie adverse effect of group interventions, these findings should not be interpreted as indication that the adverse effects that have been found across other studies do not exist, as caution is still warranted in programs that aggregate high risk youth, such as those targeted in selective or indicated programs. Rather, this study represents a comprehensive analysis that may benefit other researchers by helping to determine whether an observed unexpected effect is lasting, meaningful, and aligned with prior research and theory on adverse effects. The evaluation process described in the current study would help to prevent researchers from jumping to strong conclusions on the potential detrimental outcomes associated with their program, uncover a more complete perspective on how an intervention is influencing outcomes across contexts and adolescent development, and also ensure that ethical principles of beneficence and non-maleficence (APA, 1992) are upheld. Such a process not only enhances the credibility of intervention efficacy but also provides important information to be applied to intervention design, dissemination, and public policies.

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Tables

Table 1. *Comparison of Intervention and Control Condition Means on Study Variables at Pretest*

Variable	Intervention	Control			
	Mean		t-statistic	p-value	d
Externalizing symptoms (A)	8.82	8.53	-0.457	.648	-0.05
Externalizing symptoms (M)	8.00	7.82	-0.272	.786	-0.02
Association with deviant peers	1.67	1.58	-1.633	.103	-0.14
Peer competence	3.37	3.38	0.129	.897	0.01

Note. A=adolescent report, M=mother report.

Table 2. *Descriptive Statistics, Estimates of Skewness and Kurtosis, and Alpha Coefficients for Study Variables.*

Measure	Time	N	Mean	SD	Skewness	Kurtosis	Alpha
Externalizing Symptoms Factor Scores (Self-Report)	Pretest	516	0.09	0.77	0.32	-0.29	--
	Posttest	447	0.05	0.84	0.21	-0.45	--
	1-year Posttest	439	0.11	0.85	0.27	-0.38	--
	2-year Posttest	418	0.01	0.88	0.20	-0.48	--
	5-year Posttest	420	-0.20	0.92	0.21	-0.42	--
Externalizing Symptoms Factor Scores (Mother Report)	Pretest	494	0.48	0.82	0.17	-0.58	--
	Posttest	430	0.30	0.83	0.25	-0.61	--
	1-year Posttest	417	0.24	0.81	0.33	-0.46	--
	2-year Posttest	396	0.05	0.84	0.36	-0.65	--
	5-year Posttest	388	0.13	0.87	0.29	-0.70	--
Peer Attachment	Posttest	447	3.84	0.84	-0.850	0.564	.878
Deviant Peer Association	Pretest	516	1.63	0.59	1.576	2.921	.864
	Posttest	447	1.64	0.59	1.484	2.624	.864

Table 3. *Intercorrelations of Study Variables.*

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1. Intervention status	--																	
2. Language	0.08	--																
3. Gender	-0.05	-0.02	--															
4. Ext symptoms (A, pre)	0.02	0.06	-0.10*	--														
5. Ext symptoms (A, post)	0.04	0.07	-0.09*	0.67**	--													
6. Ext symptoms (A, 1-yr)	0.04	0.11*	-0.09*	0.58**	0.74**	--												
7. Ext symptoms (A, 2-yr)	0.01	0.11*	-0.04	0.50**	0.62**	0.72**	--											
8. Ext symptoms (A, 5-yr)	<0.01	0.14**	-0.07	0.39**	0.43**	0.42**	0.60**	--										
9. Ext symptoms (M, pre)	<0.01	0.09*	-0.07	0.31**	0.31**	0.31**	0.27**	0.23**	--									
10. Ext symptoms (M, post)	-0.05	0.11*	0.03	0.31**	0.36**	0.35**	0.31**	0.27**	0.77**	--								
11. Ext symptoms (M, 1-yr)	-0.06	0.18**	0.02	0.30**	0.36**	0.35**	0.29**	0.24**	0.70**	0.80**	--							
12. Ext symptoms (M, 2-yr)	-0.09*	0.13**	-0.02	0.28**	0.35**	0.37**	0.41**	0.34**	0.68**	0.73**	0.73**	--						
13. Ext symptoms (M, 5-yr)	-0.03	0.11*	-0.01	0.25**	0.28**	0.27**	0.33**	0.40**	0.60**	0.65**	0.67**	0.69**	--					
14. Ext disorder (2-yr)	0.01	0.06	-0.03	0.16**	0.20**	0.31**	0.37**	0.25**	0.33**	0.35**	0.35**	0.42**	0.37**	--				
15. Ext disorder (5-yr)	-0.04	0.06	-0.02	0.15**	0.12**	0.14**	0.15**	0.34**	0.19**	0.22**	0.18**	0.27**	0.29**	0.22**	--			
16. Peer attachment (post)	-0.04	-0.04	0.42**	-0.25**	-0.24**	-0.21**	-0.10*	-0.12**	-0.11*	-0.10*	-0.11*	-0.08	-0.08	-0.05	-0.05	--		
17. Deviant peers (pre)	0.07	0.11*	<0.01	0.52**	0.43**	0.39**	0.32**	0.20**	0.23**	0.24**	0.22**	0.18**	0.18**	0.07	0.16**	-0.18**	--	
18. Deviant peers (post)	0.03	0.12**	-0.04	0.48**	0.56**	0.52**	0.36**	0.21**	0.20**	0.31**	0.30**	0.25**	0.25**	0.16**	0.11*	-0.26**	0.57**	--

Note. Estimates derived from Mplus, using FIML (N=516). * p <.05, ** p <.01. Ext = externalizing. A=adolescent report, M=mother report, pre=pretest, post=immediate posttest, 1-yr=1-year posttest, 2-yr=2-year posttest, 5-yr = 5-year posttest.

Table 4. *Results from Follow-Up Outcome Analyses*

Outcome Variable								
	I	L	G	P	IxP	IxL	LxP	IxPxL
Externalizing at 1-year posttest (S)	0.004	-0.174	-0.034	0.456*	0.265*	0.086	-0.252†	-0.232
Externalizing at 2-year posttest (S)	-0.058	-0.252†	0.032	0.406*	0.220*	0.163	-0.100	0.140
Externalizing at 5-year posttest (S)	0.067	0.360*	-0.077	0.217†	0.238	-0.199	0.309†	-0.174
Externalizing at 1-year posttest (M)	-0.121*	-0.138	0.112*	0.645*	0.065	-0.110	-0.033	0.012
Externalizing at 2-year posttest (M)	-0.193*	0.200†	0.040	0.673*	0.023	0.061	-0.145	0.105
Externalizing at 5-year posttest (M)	-0.046	0.150	0.061	0.700*	-0.094	-0.009	-0.217	0.162
Externalizing Disorder at 2-year posttest	-0.228	-0.143	-0.161	0.883*	-0.527	-0.089	-0.770	-0.805
Externalizing Disorder at 5-year posttest	-0.917†	-0.303	-0.145	-0.142	1.532*	-0.026	-0.877	0.561

Note. Unstandardized (raw) estimates reported. * $p < .05$, † $p < .10$. I = Intervention status, L = Language group, G = Gender, P = Pretest externalizing. S = Self-report, M = Mother report.

Table 5. *Results from Longitudinal Growth Models of Externalizing Symptoms*

Self-Report	I	L	G	P	IxP
Intercept	0.015	0.089†	-0.023	0.517*	0.201*
Slope	-0.012	0.036*	-0.004	-0.049*	-0.007
Mother Report	I	L	G	P	IxP
Intercept	-0.111*	0.128*	0.104*	0.696*	0.046
Slope	0.005	0.004	-0.015	-0.006	-0.031*

Note. Unstandardized (raw) estimates reported. * $p < .05$, $^{\dagger} < .10$. I = Intervention status, L = Language group, G = Gender, P = Pretest externalizing. Both models time-centered at 1-year posttest.

Table 6. *Results from Peer Mediator Analyses*

Mediator Variable								
Outcome Variable	I	L	G	P	IxP	PM	M	MED
Deviant peer association	-0.015	0.071	-0.020	0.160*	0.049	0.443*	---	---
Externalizing at 1-year posttest	0.016	0.084	-0.033	0.317*	0.230*	---	0.445*	-0.007
Deviant peer association	-0.029	-0.024	0.652*	-0.076	-0.214*	---	---	---
Externalizing at 1-year posttest	0.011	0.116†	0.001	0.444*	0.262*	---	-0.049	0.001

Note. Unstandardized (raw) estimates reported. * $p < .05$, $^{\dagger} < .10$. I = Intervention status, L = Language group, G = Gender, P = Pretest externalizing, PM = Pretest mediator, M = Mediator at posttest, MED = Mediation effect.

Figures

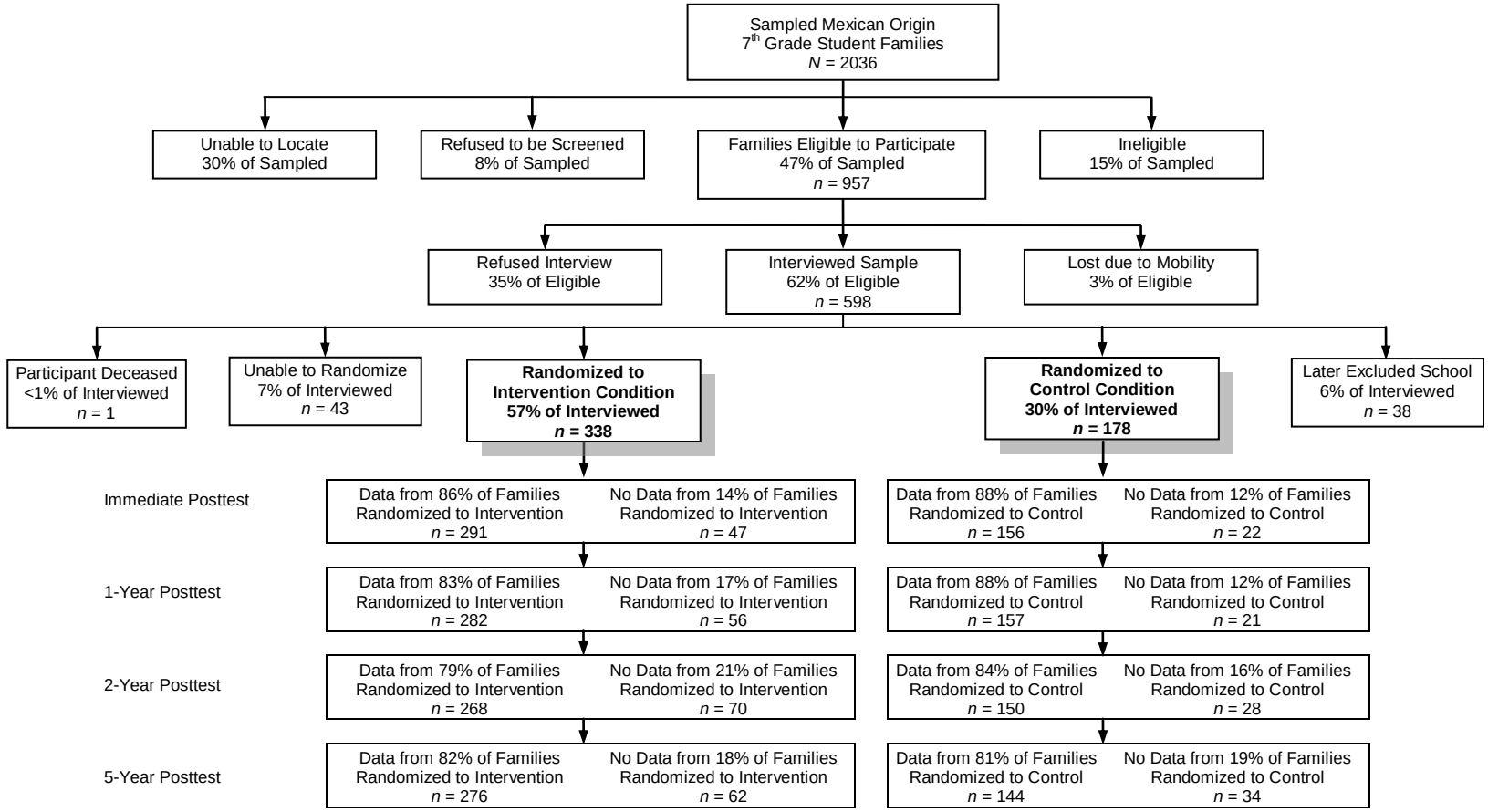

Figure 1. *Consort chart of intervention recruitment, enrollment, randomization, and retention.*

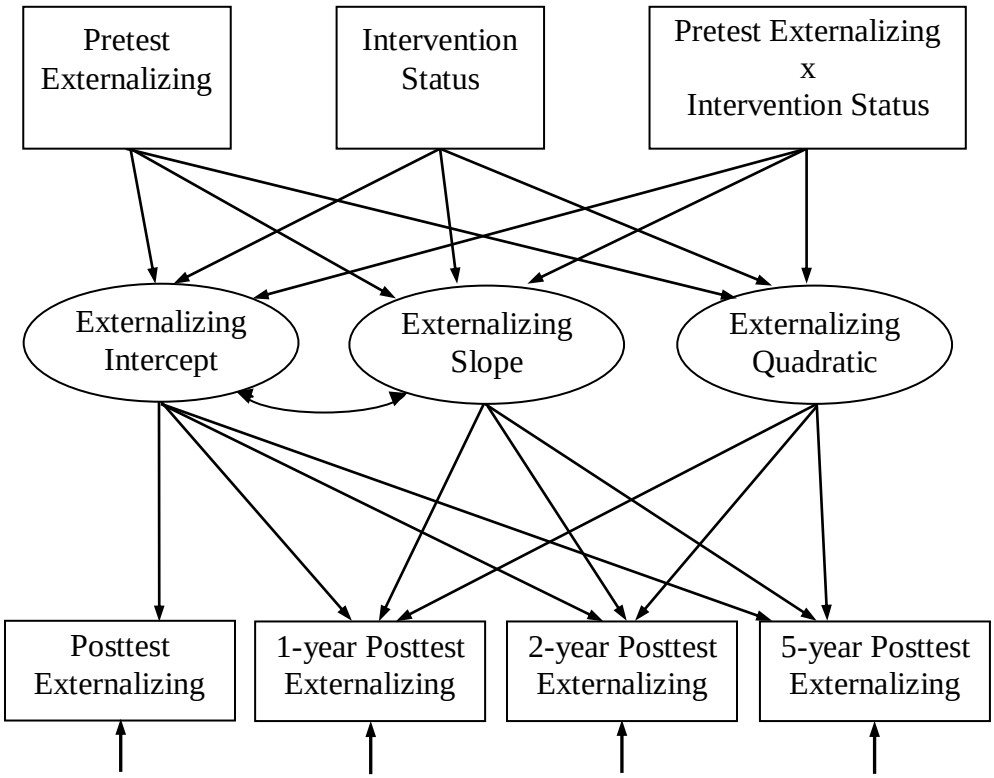

Figure 2. *Longitudinal growth model of externalizing predicted by the interaction between initial externalizing and intervention status.*

Note. Covariances between exogenous variables not depicted.

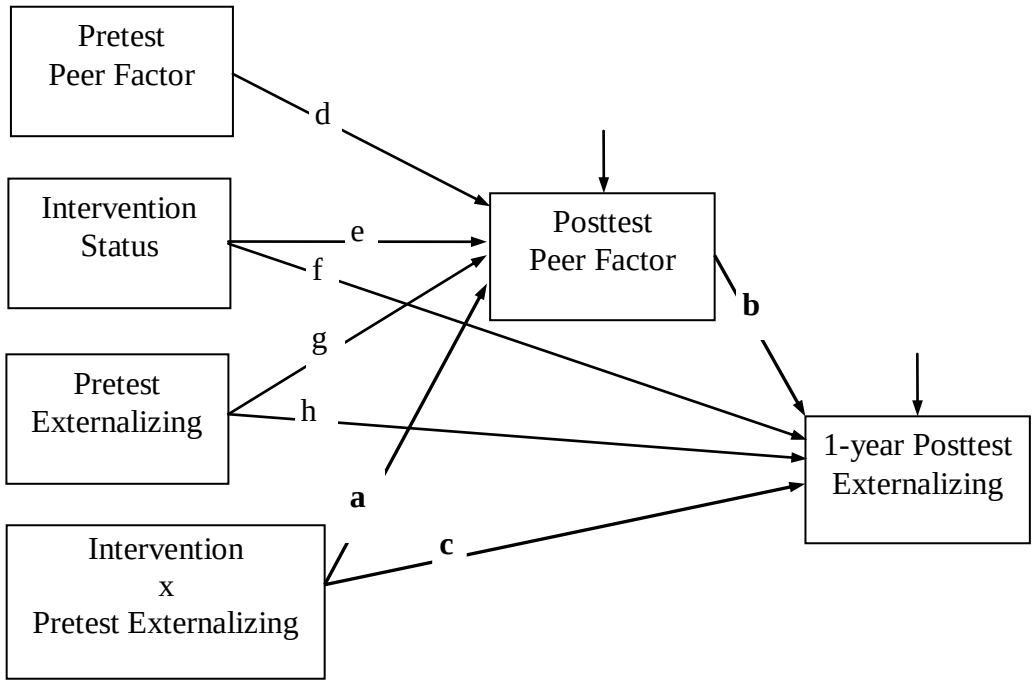

Figure 3. *Moderated mediation model of the indirect intervention effect on externalizing through each peer factor.*

Note. Covariances between exogenous variables not depicted.

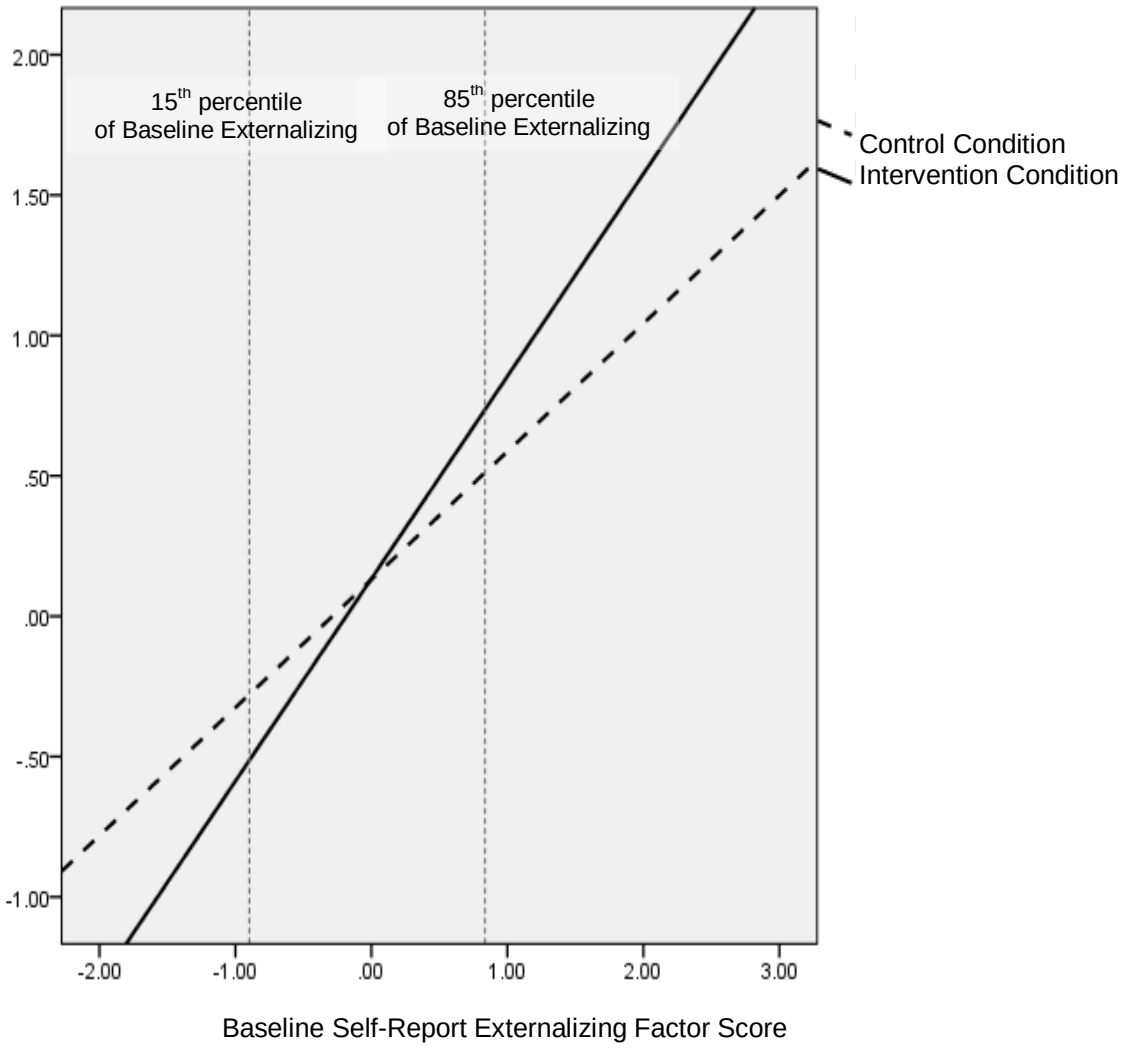

Figure 4. *Plot of two-way interaction between intervention status and pretest externalizing predicting externalizing at 1-year posttest.*

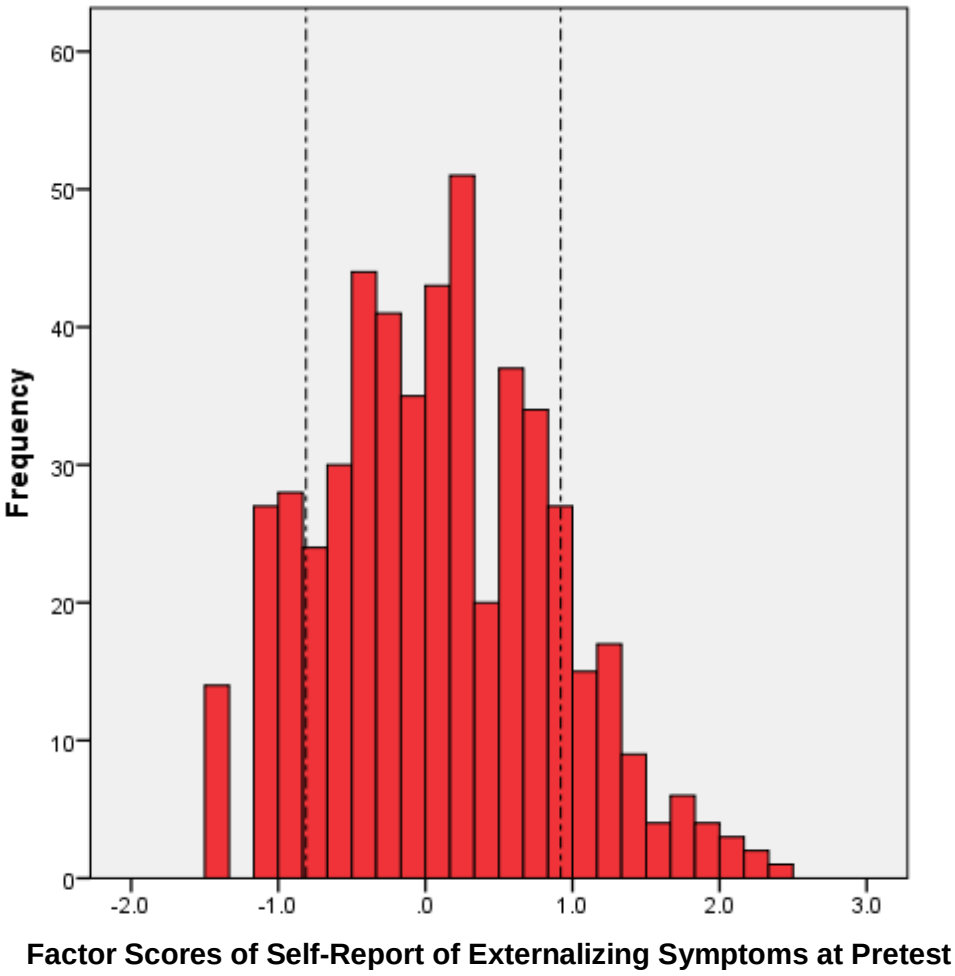

Figure 5. *Histogram of the pretest self-report externalizing factor scores.*

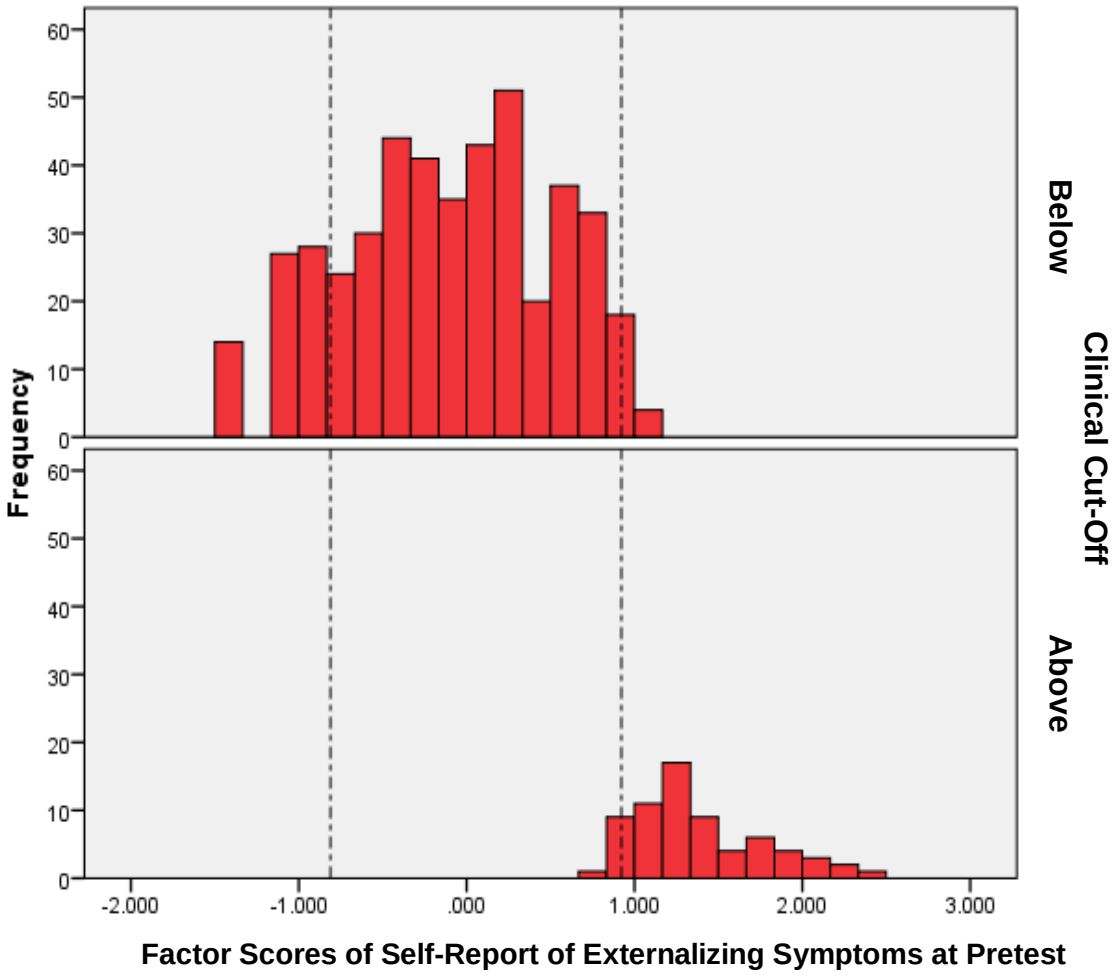

Figure 6. *Histograms of the pretest self-report externalizing factor scores divided by clinical cut-off.*

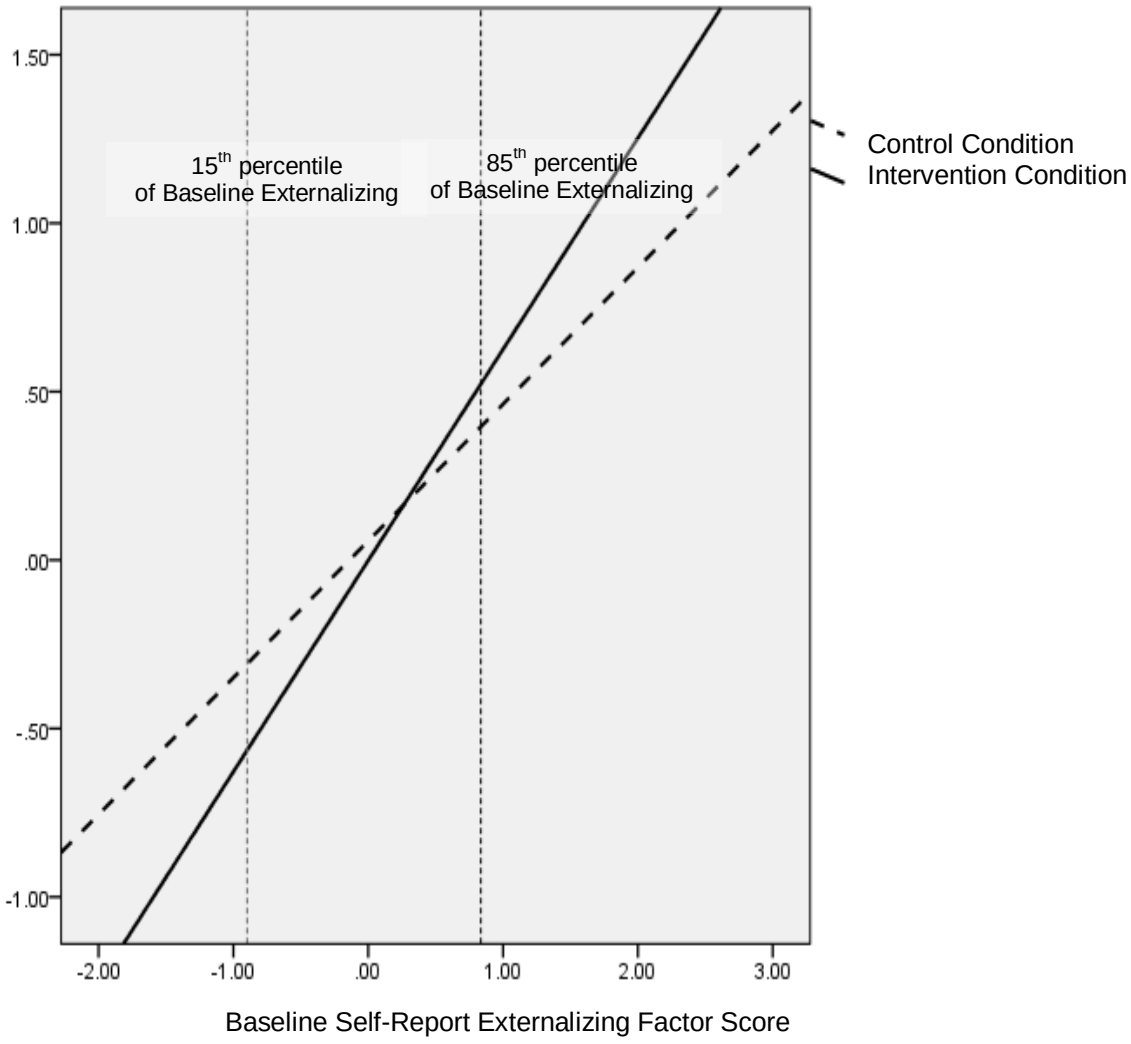

Figure 7. *Plot of two-way interaction between intervention status and pretest externalizing predicting externalizing at 2-year posttest.*

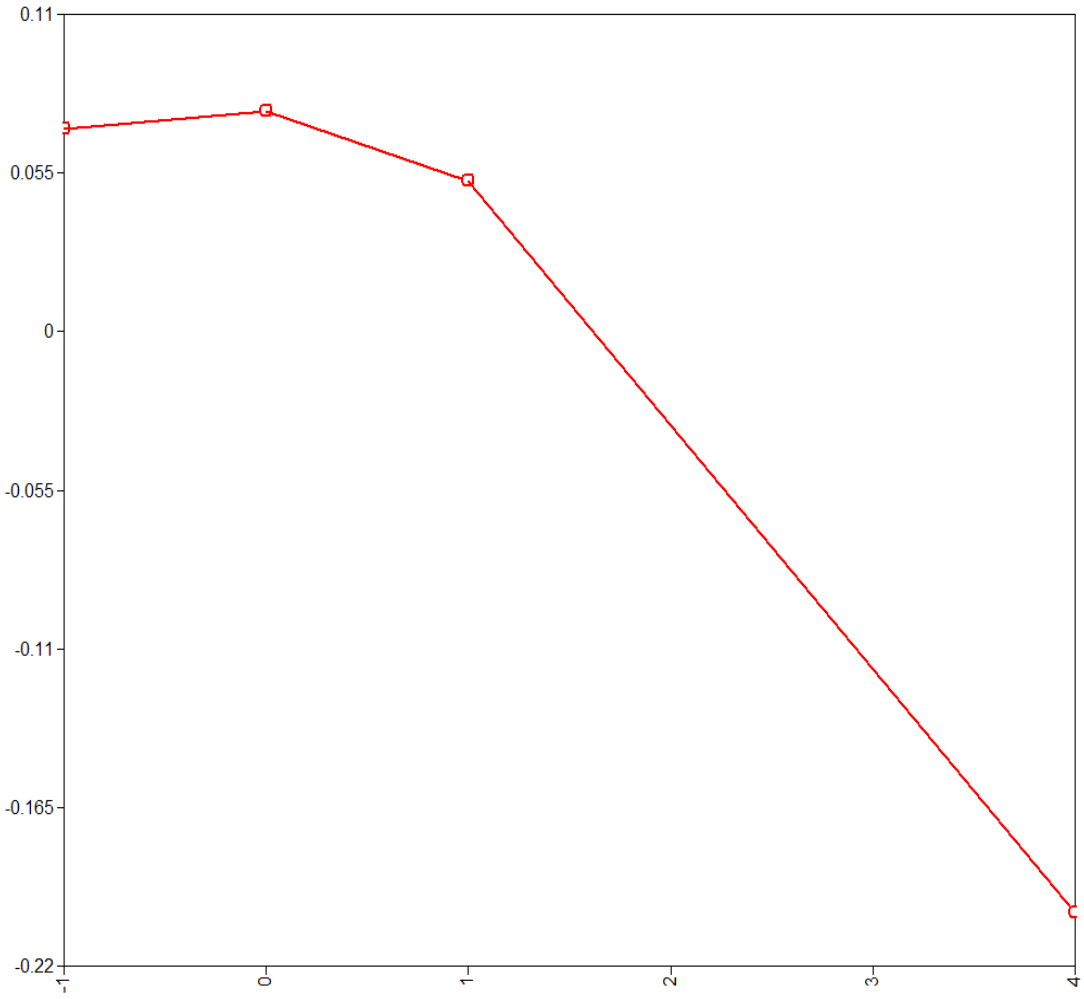

Figure 8. *Plot of estimated means of self-report of externalizing symptoms over time.*

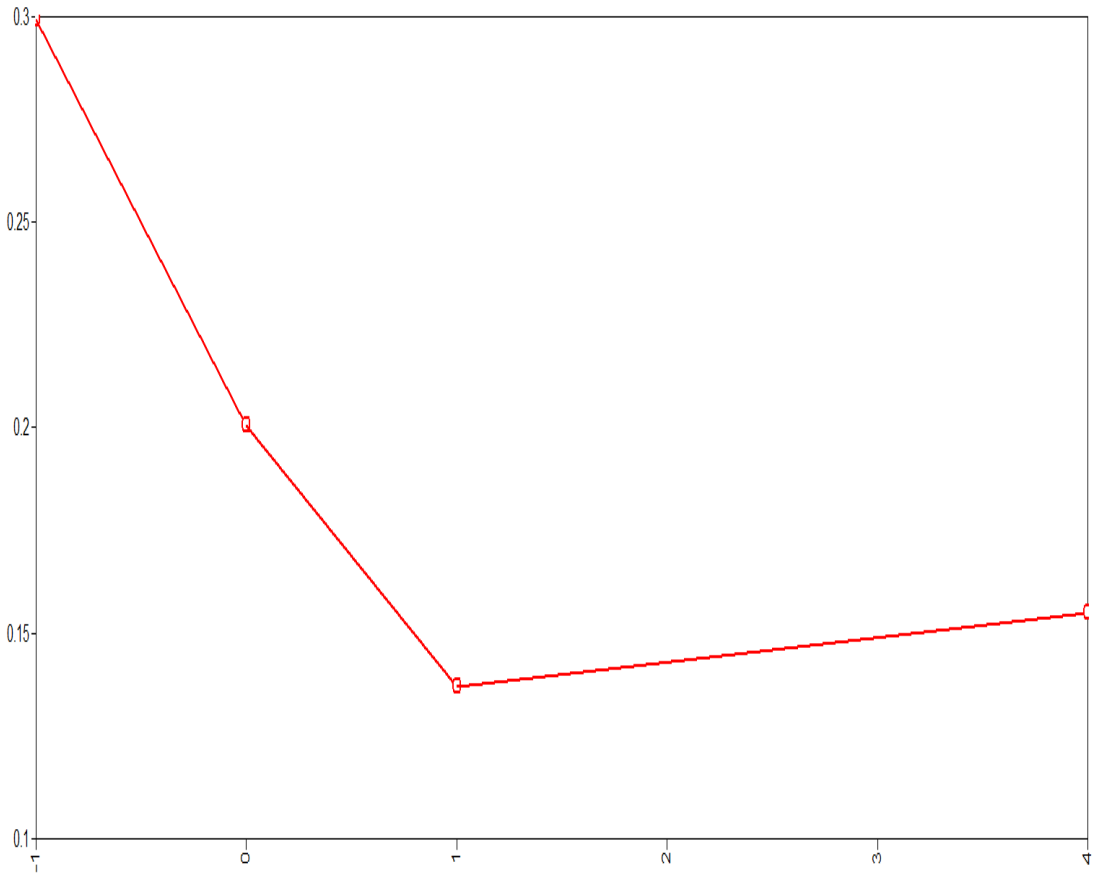

Figure 9. *Plot of estimated means of mother report of externalizing symptoms over time.*

APPENDIX A

TABLE OF EXPECTED RESULTS BASED ON PREDICTIONS

	Lasting & Meaningful Adverse Effect	**No Lasting & Meaningful Adverse Effect**
Intervention effect on self-report of externalizing at 2- and 5-year posttests among high pretest externalizers	+	-
Intervention effect on mother report of externalizing at 2- and 5-year posttests among high pretest externalizers	+	-
Intervention effect on externalizing disorder diagnosis at 2- and 5-year posttests among high pretest externalizers	+	-
Intervention effect on linear growth in self-report of externalizing symptoms	-	+
Intervention effect on linear growth in mother report of externalizing symptoms	-	+
Intervention effect on peer attachment and deviant peer association at immediate posttest among high pretest externalizers	-	+
Effects of peer attachment and deviant peer association on self-report of externalizing at 1-year posttest	+	+

APPENDIX B

EXTERNALIZING SCALES

Youth Self-Report Survey (YSR) Externalizing Scale Items

1. I drink alcohol without parents' approval.
2. I don’t feel guilty after doing something I shouldn’t.
3. I break rules at home, school, or elsewhere.
4. I hang around with kids who get in trouble.
5. I lie or cheat.
6. I would rather be being older kids than with kids my own age.
7. I run away from home.
8. I set fires.
9. I steal at home.
10. I steal from places other than home.
11. I swear or use dirty language.
12. I think about sex too much.
13. I smoke, chew, or sniff tobacco.
14. I cut classes or skip school.
15. I use drugs for nonmedical purposes, don’t include alcohol or tobacco.
16. I argue a lot.
17. I am mean to others.
18. I try to get a lot of attention.
19. I destroy my own things.
20. I destroy things belonging to others.
21. I disobey my parents.

22. I disobey at school.

23. I get in many fights.

24. I physically attack people.

25. I scream a lot.

26. I am stubborn.

27. My moods or feelings change suddenly.

28. I am suspicious.

29. I tease others a lot.

30. I have a hot temper.

31. I threaten to hurt people.

Adult Self-Report Survey (ASR) Externalizing Scale Items

1. I argue a lot.
2. I blame others for my problems.
3. I am mean to others.
4. I get along badly with my family.
5. I get in many fights.
6. My moods swing between elation and depression.
7. I physically attack other people.
8. I yell or scream a lot
9. My behavior is very changeable.
10. I am stubborn, sullen, or irritable.

11. I have a hot temper.

12. I threaten to hurt other people.

13. I get upset too easily.

14. I am too impatient.

15. I use drugs, other than alcohol and nicotine, for nonmedical purposes.

16. I damage or destroy my things.

17. I break rules at work or elsewhere.

18. I don't feel guilty after doing something I shouldn't.

19. I hang around people who get in trouble.

20. I am impulsive or act without thinking.

21. I lie or cheat.

22. My behavior is irresponsible.

23. I steal.

24. I drink too much alcohol or get drunk.

25. I do things that may cause me trouble with the law.

26. I fail to pay my debts or meet financial responsibilities.

27. I have trouble managing money or credit cards.

28. I have trouble keeping a job.

29. I brag.

30. I try to get a lot of attention.

31. I show off or clown.

32. I talk too much.

33. I tease others a lot.

34. I am louder than others.

Child Behavior Checklist (CBCL) Externalizing Scale Items

1. Drinks alcohol without parents' approval.
2. Doesn't seem to feel guilty after misbehaving.
3. Breaks rules at home, school, or elsewhere.
4. Hangs around with others who get in trouble.
5. Lying or cheating.
6. Prefers being with older kids.
7. Runs away from home.
8. Sets fires.
9. Steals at home.
10. Steals outside the home.
11. Swearing or obscene language.
12. Thinks about sex too much.
13. Smokes, chews, or sniffs tobacco.
14. Truancy, skips school.
15. Uses drugs for nonmedical purposes please do not include alcohol or tobacco.
16. Argues a lot.
17. Cruelty, bullying, or meanness to others.
18. Demands a lot of attention.

19. Destroys his/her own things.

20. Destroys things belonging to [his/her] family or others.

21. Disobedient at home.

22. Disobedient at school.

23. Gets in many fights.

24. Physically attacks people.

25. Screams a lot.

26. Stubborn, sullen, or irritable.

27. Sudden changes in mood or feelings.

28. Suspicious.

29. Teases a lot.

30. Temper tantrums or hot temper.

31. Threatens people.

Adult Behavior Checklist (ABCL) Externalizing Scale Items

1. Argues a lot.
2. Blames others for own problems.
3. Cruelty, bullying, or meanness to others.
4. Gets along badly with family.
5. Gets in many fights.
6. Moods swing between elation and depression.
7. Physically attacks people.

8. Screams or yells a lot.
9. Very changeable behavior.
10. Stubborn, sullen, or irritable.
11. Sudden changes in moods or feelings.
12. Temper tantrums or hot temper.
13. Threatens to hurt people.
14. Sulks a lot.
15. Gets upset too easily.
16. Is too impatient.
17. Uses drugs (other than alcohol or nicotine) for nonmedical purposes.
18. Breaks rules at work or elsewhere.
19. Doesn't seem to feel guilty after misbehaving.
20. Hangs around people who get into trouble.
21. Impulsive or acts without thinking.
22. Lying or cheating.
23. Irresponsible behavior.
24. Steals.
25. Drinks too much alcohol or gets drunk.
26. Does things that may cause trouble with the law.
27. Fails to pay his/her debts or meet other financial responsibilities.
28. Has trouble managing money or credit cards.
29. Has trouble keeping a job.

30. Bragging, boasting.

31. Demands a lot of attention.

32. Showing off or clowning.

33. Talks too much.

34. Teases a lot.

35. Is unusually loud.

Diagnostic Interview Schedule for Children (CDISC)

Attention-Deficit/Hyperactivity Disorder Items

1. Inattention.
2. Fails to give close attention.
3. Difficulty sustaining attention.
4. Does not listen.
5. Fails to follow through on instructions.
6. Difficulty organizing tasks.
7. Avoids, dislikes tasks that require sustained attention.
8. Loses things.
9. Easily distracted by extraneous stimuli.
10. Forgetful.
11. Fidgets.
12. Leaves seat.
13. Runs about or climbs excessively.

14. Difficulty playing quietly.

15. Often on the go/driven by a motor.

16. Talks excessively.

17. Blurts out answers.

18. Difficulty awaiting turns.

19. Interrupts or intrudes.

20. Onset before age 7.

21. Impairment in two or more settings.

Conduct Disorder Items

1. Loses temper.

2. Bullies, threatens others.

3. Initiates physical fights.

4. Used a weapon.

5. Physically cruel to people.

6. Physically cruel to animals.

7. Stole with confrontation.

8. Forced sex.

9. Fire setting.

10. Destroyed property.

11. Broken into house, building, or car.

12. Lies to obtain goods or favors, avoid obligations.

13. Stolen without confrontation.

14. Stays out late.

15. Runs away from home.

16. Truant.

17. Repetitive and persistent pattern of behavior.

Oppositional Defiant Disorder Items

1. Loses temper.
2. Argues with adults.
3. Actively defies or refuses adults.
4. Deliberately annoys people.
5. Blames others.
6. Touchy or easily annoyed.
7. Angry and resentful.
8. Spiteful or vindictive.
9. Initiates physical fights.
10. Used a weapon.

APPENDIX C

PEER FACTOR SCALES

Peer Attachment Scale Items

1. My friends respected my feelings.
2. My friends helped me understand myself better.
3. I told my friends about my worries and problems.
4. My friends helped me talk about problems and difficulties.
5. My friends showed that they understand me.
6. My friends tried to understand me when I was angry.
7. I trusted my friends.
8. I could count on my friends when I needed to talk.
9. If my friends knew something bugged me, they asked me about it.

Deviant Peer Association Scale Items

1. Used force (e.g., threats or fighting) to get things from people.
2. Been in gang fights.
3. Gotten drunk or high.
4. Lied about their age to buy or do things.
5. Started rumors or told lies.
6. Cheated on school tests.
7. Got suspended from school.
8. Missed school without an excuse.
9. Stole something worth less than $50.
10. Stole something worth $50 or more.

11. Ruined or damaged other people's things on purpose (including tagging/graffiti).

12. Started a fight with someone.

13. Used a weapon.

14. Hurt animals on purpose.

15. Sold drugs.