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The Effects of the Evidence-Based *Safe Dates* Dating Abuse Prevention Program on Other Youth Violence Outcomes

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Abstract

In response to recent calls for programs that can prevent multiple types of youth violence, the current study examined whether *Safe Dates*, an evidence-based dating violence prevention program, was effective in preventing other forms of youth violence. Using data from the original *Safe Dates* randomized controlled trial, this study examined (1) the effectiveness of *Safe Dates* in preventing peer violence victimization and perpetration and school weapon carrying 1 year after the intervention phase was completed and (2) moderation of program effects by the sex or race/ethnicity of the adolescent. Ninety percent ($n=1,690$) of the eighth and ninth graders who completed baseline questionnaires completed the 1-year follow-up assessment. The sample was 51% female and 26% minority (of whom 69% was black and 31% was of another minority race/

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ethnicity). There were no baseline treatment group differences in violence outcomes. Treatment condition was significantly associated with peer violence victimization and school weapon carrying at follow-up; there was 12% less victimization and 31 % less weapon carrying among those exposed to *Safe Dates* than those among controls. Treatment condition was significantly associated with perpetration among the minority but not among white adolescents; there was 23 % less violence perpetration among minority adolescents exposed to *Safe Dates* than that among controls. The observed effect sizes were comparable with those of other universal school-based youth violence prevention programs. Implementing *Safe Dates* may be an efficient way of preventing multiple types of youth violence.

Keywords

Youth violence prevention; Dating abuse; Youth violence; Safe Dates

Introduction

In recent years, there have been calls for crosscutting prevention strategies, i.e., single programs that can prevent multiple adolescent health risk behaviors (Flay et al. 2004; Centers for Disease Control [CDC] and Prevention 2009; Vivolo et al. 2010; DeGue et al. 2013). Calls for crosscutting strategies to prevent youth violence have been particularly prominent. Programs for preventing violence among youth tend to focus on preventing only one type of violence (dating violence, sexual violence, or peer violence) (Ozer et al. 2004; Swahn et al. 2008). However, with increasing recognition that various types of violence co-occur and have shared risk factors (Brendgen et al. 2001; Gorman-Smith et al. 2001; Ozer et al. 2004; O'Donnell et al. 2006), identifying interventions that can prevent multiple types of violence among youth is considered critical (Ozer et al. 2004; CDC 2009; DeGue et al. 2013). For example, the Injury Research Agenda of the Centers for Disease Control and Prevention states

“A single prevention strategy might be able to prevent more than one form of violence resulting in more efficient use of resource.... Given the limited funding available to prevent youth violence, strategies should address multiple forms of violence whenever feasible.”

(pp 88 and 110).

DeGue et al. (2013) suggest that an efficient way to identify crosscutting intervention strategies is to determine whether existing evidence-based programs are effective in preventing risk behaviors that are related but not the primary targeted behavior. They suggest that doing so can lead to quicker community implementation of evidence-based programs for preventing multiple risk behaviors. In this study, therefore, we determined whether *Safe Dates*, a school-based adolescent dating abuse prevention program that prevented multiple types of dating abuse victimization and perpetration (Foshee et al. 2005), with effects lasting up to 4 years beyond intervention exposure, also prevented other types of youth violence, including peer violence victimization and perpetration and school weapon carrying.

Safe Dates, which comprises a play performed by students, a ten-session curriculum, and a poster contest based on curriculum content, is one of the most widely used adolescent dating abuse prevention programs in the USA. Hazelden Publishing and Educational Services, the publisher of *Safe Dates*, reports that adolescents in all 50 states and some territories (Puerto Rico) have been exposed to the program; it has been used in a number of states to fulfill state mandates to teach dating abuse prevention to adolescents; and it is being used in 12 other countries: Australia, Canada, Chile, Greece, Iceland, Ireland, Japan, the Netherlands, Switzerland, Taiwan, Thailand, and the UK. Several researchers have pointed to the importance of determining whether *Safe Dates* is effective in preventing types of youth violence other than dating abuse since *Safe Dates* is designed to alter risk and protective factors that may also be relevant for other violent behaviors and experiences (Swahn et al. 2008; DeGue et al. 2013).

Safe Dates content and activities focus on changing norms (acceptance of dating violence and gender role norms), improving anger management and conflict resolution skills, and increasing help seeking by strengthening the belief that dating violence victims and perpetrators need help, improving help-seeking skills, and raising awareness of sources of help. All of these risk and protective factors are also relevant to the prevention of peer violence. Numerous studies have demonstrated that normative beliefs about violence predict violence against peers (Huesmann and Guerra 1997; Dahlberg 1998; Hawkins et al. 2000; Henry et al. 2000; Jouriles et al. 2013), and several youth violence prevention programs have achieved effectiveness by changing attitudes about the acceptability of violence (Powell et al. 1995; Sheehan et al. 1999). Acceptance of traditional gender roles has been related not only to intimate partner violence (Reidy et al. 2009; Duràn et al. 2010; Vandiver and Dupalo 2013) but also to other types of youth violence. For example, higher levels of endorsement of masculinity have been associated with bullying among both boys and girls (Gini and Pozzoli 2006; Navarro et al. 2011) and with male barroom aggression (Wells et al. 2011). In addition, endorsement of femininity has been associated with girls' (Gini and Pollozi 2006) and boys' (Navarro et al. 2011) victimization by peers. Numerous studies have found that anger dysregulation and poor conflict management skills are associated with the use of violence against peers (Slaby and Guerra 1988; Dishion et al. 1996; Dodge 2001; Dodge et al. 2006) and youth violence prevention programs targeting these skills have been found to be effective (Cooper et al. 2000). Finally, a youth connected to community resources may learn ways of resolving conflict that do not escalate violence and feel assured that they are not alone in handling complex relationship issues that have the potential to lead to violence. Also, learning that community resources are available for addressing violence may convey the message that the community is paying attention and not tolerating violence (Leventhal and Brooks-Gunn 2003; Boxer et al. 2005).

Although in *Safe Dates*, the information and activities designed to change these factors focus on dating abuse prevention, what the adolescent learns could generalize to other types of violence through a process of analogical problem solving. Analogical problem solving, in which the solution to one problem can be used to solve a related problem, is ubiquitous in human learning and interaction. Extensive research in the field of education has identified the conditions under which analogical problem solving is most likely (Chen and Klahr 2008). For example, analogical problem solving is more effective when there are structural

similarities between the initial problem and the new problem, the tasks required to solve the initial problem are similar to the tasks required to solve the new problem, and the context in which the first problem occurs is similar to the context in which the new problem occurs (Glick and Holyoak 1983; Chen and Klahr 2008). The structures of dating violence and peer violence are similar in that they both involve conflict and strong emotion; the tasks employed to prevent dating abuse (e.g., anger management, effective communication skills, and the belief that the use of violence against dates is unacceptable) are similar to those employed to prevent other types of violence; and the context in which dating violence occurs, in social relationships with peers, is similar to the context in which peer violence occurs. Thus, it is possible that a dating abuse prevention program could also prevent peer violence.

Given the need for crosscutting intervention strategies, the wide-scale distribution of *Safe Dates* and the fact that the program is designed to alter risk factors for dating abuse that are also risk factors for other types of youth violence, it is important to determine whether *Safe Dates* prevents other violence in addition to dating abuse. Therefore, the current study used data from the original *Safe Dates* randomized controlled trial (RCT) to examine the effectiveness of the program in decreasing peer violence victimization and perpetration and in preventing weapon carrying to school. Also, since *Safe Dates* is a school-based program, it is important to know whether program effects hold for various demographic subgroups that comprise a school environment. This is particularly important given the continuing concerns about violence among minority adolescents and the growing concerns about violence among girls. Sex and race/ethnicity differences have been noted in base rates of violence (Flay et al. 2004). In addition, sex differences have been noted in the circumstances and reasons for the use of violence (Flay et al. 2004), moral reasoning (Meyer and Farrell 1998), the reactivity to varying modes of program delivery (Meyer and Farrell 1998), and the etiology of violence (Ellickson and McGuigan 2000), and race/ethnicity differences have been noted in exposure to key risk factors for violence (McNulty and Bellair 2003). Each of these differences could potentially enhance or buffer program effectiveness. Although the effects of some youth violence prevention programs have been found to vary by sex (DuRant et al. 1996; Farrell and Meyer 1997; Flay et al. 2004; Farrell et al. 2001) and race/ethnicity (Hawkins et al. 1991), sex and race/ethnicity differences in the effects of universal school-based violence prevention programs have not often been examined. Therefore, we looked at whether program effects were moderated by sex and race/ethnicity of the adolescent.

Methods

Design

The *Safe Dates* RCT was conducted in a primarily rural county in eastern NC. Adolescents were eligible for the study if they were enrolled in the eighth or ninth grade in the fall semester in one of the 14 public schools in the county, which were matched on school size. One school from each matched pair was randomly assigned to treatment and the other to the control condition. Parental consent for adolescent participation was obtained from 84 % of the parents of eligible adolescents. Baseline data (wave 1) were collected in schools in October by trained research staff from 96 % ($n=1,886$) of the adolescents who had

parental consent; 4 % of the adolescents who had parental consent chose not to participate. The adolescents in the seven treatment schools were then exposed to *Safe Dates* program activities from November to March. Program fidelity was high in the RCT: 97 % of the students enrolled in the treatment schools were present for the play, the teachers covered 90.7 % of the curriculum activities, and classroom attendance in the sessions ranged from 95.0 to 97.0 %. All students who were in school on the day of their school's poster contest (school attendance averaged 96 %) were exposed to the messages in the posters because the students were required to vote for the three best posters in the school.

Follow-up data were collected from treatment and control adolescents at 1 month, and 1, 2, 3, and 4 years after the intervention was completed, using the same procedures as for baseline data collection except that at the follow-ups, students who were absent during the data collection, including those who had dropped out of school, were mailed a questionnaire. In the current study, we used 1-year follow-up data because 1 month is too short to see behavioral effects, there was significant attrition between the 1- and 2-year follow-ups as a result of the necessity to resolicit active parental consent for adolescents to continue in the study past 1 year, the violence outcomes were not measured at the 3-year follow-up, and the ninth grade cohort had graduated by the 4-year follow-up. The adolescents were in grades 9 and 10 at the 1-year follow-up. Of the 1,886 adolescents who completed the baseline questionnaires, 90% ($n=1,690$) completed the 1-year follow-up, including 90 adolescents who completed the questionnaire by mail. The sample was 51 % female and 26 % minority. Of the minority adolescents, 69 % were African-American and 31 % were Hispanic, Asian, American-Indian, or mixed race, with the majority being mixed race. Approximately 22 % of the parents of these adolescents had a high school education or less.

Measures

Three behavioral outcomes were assessed, *peer violence perpetration*, *peer violence victimization*, and *weapon carrying to school*. At baseline and 1-year follow-up, the adolescents were asked, "How many times have you ever done the following things to someone of the same sex and the same age as you: 'beat them up,' 'hit them with my fist,' 'threatened them with a weapon,' and 'used a weapon on them.'" Response options ranged from never (0) to 10 or more times (3). Responses were summed to create a *peer violence perpetration* measure at the baseline ($\alpha=.80$) and at the follow-up ($\alpha=.86$). Parallel items were used to assess peer violence victimization. The adolescents were asked, "How many times has anyone of the same sex and about the same age as you done the following things to you: 'beat me up,' 'hit me with a fist,' 'threatened me with a weapon,' and 'used a weapon on me.'" Response options ranged from never (0) to 10 or more times (3). Responses were summed to create a *peer violence victimization* score at the baseline ($\alpha=.75$) and at the follow-up ($\alpha=.81$). The adolescents were also asked at the baseline and follow-up if they had ever brought a gun to school and if they had ever brought any other kind of weapon to school. The adolescents were coded 0 on the *weapon-carrying* outcome if they answered no to both of these questions and 1 if they responded with yes to either item.

Sex and *minority status* were conceptualized as moderator variables. *Sex* was coded as 0 = female and 1 = male. To assess race/ethnicity, the participants were asked, “What is your race?” with response options being black, white, Hispanic, Asian, American-Indian, and other. The adolescents could mark more than one category and those who did so were categorized as mixed race. Because of the small proportion of race/ethnicities other than white, a *minority status* variable was created such that 0 = nonminority, which included those who marked only “white,” and 1 = minority, which included all other adolescents including those of mixed race. *Parent education*, a proxy for socioeconomic status (Goodman 1999), was controlled in all models and was coded such that 0 = less than high school, 1 = high school graduate only, and 2 = more than high school.

Although the composite violence variables were used in the analyses, for descriptive purposes, Table 1 presents the baseline prevalence of each peer violence perpetration and victimization indicator and weapon carrying in the total sample by sex and minority status of the adolescent. Male subjects were significantly more likely to endorse each indicator than female subjects, and the minority adolescents were significantly more likely to endorse each indicator except being hit with a fist than nonminority adolescents, which did not differ by minority status.

Analysis Strategy

The analytical samples used to assess program effects included adolescents who completed the 1-year follow-up and were not missing data on any baseline control variables or on outcome measures at either assessment. Our measure of weapon carrying was a dichotomous lifetime assessment (ever had carried a weapon, yes/no). Therefore, for the weapon-carrying analyses, we eliminated the adolescents who reported at the baseline that they had ever carried a weapon because these adolescents could not change on that outcome at follow-up. Thus, the analytic sample was 1,620 for peer victimization and peer perpetration and 1,397 for weapon carrying.

The analyses proceeded in several phases. We first examined the equivalence of treatment and control groups on baseline measures of outcomes. We found no baseline differences between the treatment and control groups on peer violence perpetration ($t=0.54$, $p=.59$), peer violence victimization ($t=0.50$, $p=.62$), or weapon carrying (before eliminating baseline weapon carriers) ($t=0.89$, $p=.37$).

Attrition analyses were then conducted to identify variables associated with dropout, which could influence the external validity or generalizability of study findings, and to examine the potential for differential attrition, which could threaten the study’s internal validity. Study dropout by 1-year follow-up was negatively associated with parent education ($p=.003$) and positively associated with baseline peer violence victimization ($p=.03$), and these associations should be considered in determining the external validity of the study. The potential for differential attrition was examined by testing the interactive effects of treatment condition and the baseline value of each of the outcomes on dropout. There were no significant interactions, making it unlikely that differential attrition could explain program effects.

Multivariate logistic regression models (for weapon-carrying onset) and negative binomial regression models (for peer violence victimization and perpetration) were used to examine the effects of exposure to *Safe Dates* on the behavioral outcomes. Generalized estimating equations (GEE) were used to adjust for nesting within schools given the cluster randomized design. In addition, as recommended by Murray et al. (2004), we corrected for downward bias in the standard error of the parameter estimates due to the small number of clusters in the study, using the approach described by Kauermann and Carroll (2001).

All models were estimated using the SAS 9.3 PROC GLIMMIX procedure and the EMPIRICAL = ROOT option to produce bias-corrected standard errors. For each of the outcomes, we first estimated a model that included treatment condition, sex, minority status, parent education, and interactions between treatment condition and sex and between treatment condition and minority status. The peer violence perpetration and peer violence victimization models also adjusted for baseline levels of the outcome measure. Adjusting for baseline weapon carrying in the weapon-carrying models was not appropriate since the analyses examined the program effects on weapon-carrying onset. Nonsignificant interactions were dropped to produce a final reduced model for each outcome. Post hoc analyses were conducted on significant interactions to determine the nature of the interaction. In all models, parameter estimates were exponentiated to produce rate ratios (for the negative binomial models) and odds ratios (for the logistic regression models) as measures of effect size.

Results

Safe Dates Effects on Peer Violence Victimization

The interactions between treatment condition and sex of the adolescent ($p=.96$) and between treatment condition and minority status ($p=.25$) in predicting peer violence victimization were not statistically significant, and therefore, they were dropped from the final peer violence victimization model, which is presented in Table 2. The treatment condition was significantly negatively related to peer violence victimization at follow-up ($b=-0.13$, $p=.04$). The rate ratio associated with this was 0.88 (95 % confidence interval (CI) of 0.78 to 0.99); that is, peer violence victimization was 12 % lower at follow-up in the treatment group than in the control group. Also in the final model, sex of the adolescent was related to peer violence victimization ($b=1.14$, $p<.001$). The rate ratio associated with this was 3.11 (95 % CI of 2.38 to 4.07); that is, the rate of peer violence victimization was 3 times higher at follow-up for male subjects than female subjects. Minority status ($p=.69$) and parent education ($p=.37$) were not associated with peer violence victimization in the final model.

Safe Dates Effects on Peer Violence Perpetration

The interaction between treatment condition and sex of the adolescent in predicting peer violence perpetration was not significant ($p=.67$), and therefore, it was dropped from the final model. However, as shown in Table 3, the interaction between the treatment condition and minority status was significant ($b=-0.22$, $p=.004$). Post hoc analyses of the interaction indicated that *Safe Dates* was significantly negatively associated with peer violence perpetration for minority adolescents ($b=-0.27$, $p=.001$) but not for white

adolescents ($p=.48$). The rate ratio for the association between treatment condition and peer violence perpetration among minorities was 0.77 (95 % CI of 0.65 to 0.90); that is, peer violence perpetration was 23 % lower at follow-up in the treatment group than in the control group.

Although the samples for specific races/ethnicities were small, we further explored racial/ethnic differences in program effects by rerunning analyses on samples stratified by race/ethnicity. When the sample was limited to black adolescents ($n=284$), there was a significant treatment by sex interaction ($b=0.66$, $p=.03$). Post hoc probing of the interaction suggested that treatment effects on perpetration were significant for black girls ($n=164$) ($b=-0.56$, $p=.03$) but not for black boys ($n=120$) ($p=.64$). The rate ratio for the black girls was 0.57 (95 % CI of 0.35 to 0.92); that is, among black girls, peer violence perpetration was 43 % lower at follow-up in the treatment group than in the control group. When the sample was limited to white adolescents ($n=1,212$), the treatment by sex interaction was not significant ($p=.85$), and in the model without the interaction, there was no main effect of treatment condition on perpetration ($p=.57$). When the sample included only adolescents of a race/ethnicity other than white or black ($n=124$), the treatment by sex interaction was not significant ($p=.72$), but in the model without this interaction, there was a significant main effect of treatment condition on perpetration ($b=-0.41$, $p=.049$). The rate ratio associated with this was 0.66 (95 % CI of 0.44 to 0.99); that is, among adolescents in the “other” race/ethnicity category, peer violence perpetration was 44% lower at follow-up in the treatment group than in the control group. Taken together, these findings suggest that the significant interaction between treatment condition and minority status found in the full sample appears to be driven by significant treatment effects for black girls and for adolescents of a race/ethnicity other than white or black.

In the final peer perpetration model (Table 3), sex of the adolescent was related to peer violence perpetration ($b=0.92$, $p<.001$). The rate ratio associated with this was 2.52 (95% CI of 2.06 to 3.07); that is, the rate of peer violence perpetration at follow-up was 2.5 times higher for male subjects than female subjects. Parent education was not associated with peer violence perpetration ($p=.76$).

Safe Dates Effects on the Onset of Weapon Carrying

The interactions between treatment condition and sex of the adolescent ($p=.14$) and between treatment condition and minority status ($p=.23$) in predicting the onset of weapon carrying were not statistically significant, and therefore, they were dropped from the final model, which is presented in Table 4. The treatment condition was significantly related to the onset of weapon carrying (AOR=0.69, 95 % CI of 0.54 to 0.88, $p=.005$): The odds of weapon carrying were 31% lower in the treatment group than in the control group. Sex of the adolescent was also related to weapon carrying: Boys were almost six times as likely as girls to carry a weapon to school (AOR=5.84, 95 % CI of 3.99 to 8.55, $p<.001$). Minority status ($p=.88$) and parent education ($p=.35$) were not associated with carrying weapons to school.

Discussion

Our findings suggest that *Safe Dates*, which was previously found to be effective in preventing adolescent dating abuse (Foshee et al. 2005), may also be effective in preventing other types of youth violence. Adolescents in the eighth and ninth grades who were exposed to *Safe Dates* reported less victimization by peers and a lower likelihood of carrying weapons to school than the control group 1 year later in grades 9 and 10. Minority adolescents who were exposed to *Safe Dates* reported perpetrating less violence against peers than minority adolescents in the control group at the follow-up.

The magnitude of program effects was comparable with that of other youth violence prevention programs. One meta-analysis of 53 evaluations of universal school-based violence prevention programs found that the average effect size, defined by the relative change in violent outcomes in intervention compared with control groups, was 15 %; effect sizes in that analysis varied by grade of program delivery: The average effect size was 7.3 % for programs administered in middle school and 29.2 % for those administered in high school (Hahn et al. 2007). We found that effect sizes, defined in a similar way, varied by violence outcome and minority status; the effect size was 12 % for peer violence victimization, 23 % for peer violence perpetration among minority adolescents, and 31 % for weapon carrying.

Because *Safe Dates* is a school-based intervention and thus typically delivered to both boys and girls of various racial/ethnic groups, we looked at whether program effects are held for subgroups based on sex and race/ethnicity. The favorable program effects on peer violence victimization and weapon carrying did not vary by sex or race/ethnicity. However, we found stronger program effects on perpetration for minority than nonminority adolescents, and exploratory analyses suggested that program effects were strongest among adolescents of a race/ethnicity other than white or black and among black girls. The stronger effects for minority adolescents could be due to the higher base rates of violence perpetration for minority than nonminority adolescents. Flay et al. (2004) suggest that it is more difficult to reduce violence among those with lower than higher base rates of violence. However, base rates were also generally higher for minority than nonminority adolescents on the other two outcomes, but there were no race/ethnic differences in treatment effects on those outcomes, and although base rates of violence were higher for boys than girls, we found few sex differences in program effects. The sex differences in program effects for black adolescents could be due to the smaller sample size for boys ($n=120$) than for girls ($n=164$). It is difficult to compare our findings with those of others because few universal violence prevention programs have examined race/ethnicity as a moderator of program effects. For example in a systematic review of universal school-based violence prevention programs (Hahn et al. 2007), only one of the seven studies that were considered to meet the highest standards of design and execution examined race/ethnicity as a moderator of program effects (Conduct Problem Prevention Research Group 2002). In that one study, the authors did not consider the few race/ethnicity differences found in program effects to be stable findings because of the large number of interactions tested. Although our findings suggest that *Safe Dates* may not be effective in preventing peer violence perpetration among nonminority adolescents, the

favorable universal effects in preventing peer violence victimization and weapon carrying warrant its consideration by school systems.

The current study is the first to determine whether a program designed to prevent dating violence can also prevent peer violence, but several other studies have demonstrated the ability of single interventions to affect multiple risk behaviors, including violence, substance use, and risky sexual behavior (Flay and Allred 2010; Flay et al. 2004; Hawkins et al. 1999; Kellam and Anthony 1998). To facilitate the development of new crosscutting interventions, additional research is needed on how to optimize analogical problem solving or the transfer of knowledge from one area to another and to identify the pedagogical approaches, skills taught, guiding theoretical frameworks, targeted risk and protective factors, and levels of the ecological model targeted that are most likely to produce crosscutting results. Also, for efficiency, it would be useful to determine whether other existing evidence-based dating abuse prevention and youth violence prevention programs (see <http://www.colorado.edu/cspv/blueprints/>) have crosscutting effects.

There were several limitations to the current study that should be considered when evaluating this research. One limitation is that the study was conducted in a primarily rural county, limiting the ability to generalize the findings to more urban areas. Also, adolescents who had been victimized by peers and who had parents with lower education levels were more likely to drop out between baseline and follow-up, potentially producing a lower risk sample and further limiting the generalizability of the study findings. However, the baseline response rate was high and the attrition rate was low, decreasing the likelihood that this dropout pattern influenced the study findings. Even so, whether the current findings can be replicated in other types of settings, such as inner city schools and with higher risk, youth should be examined in future studies.

We could not assess the process through which the program had favorable effects on the violence outcomes because several of the risk factors or mediators targeted for change in *Safe Dates* were measured in a way that was specific to dating violence. Therefore, they could not be tested as mediators of program effects in the current study. For example, the normative belief measure assessed the degree to which the adolescent agreed or disagreed with statements indicating that it is OK to use violence against dates in various circumstances and the help-seeking measures were specific to seeking help with a violent dating relationship.

Finally, the violence outcome measures had several limitations. For example, the time referent for reporting the violent acts was “ever,” which is less precise and could result in more measurement error than a shorter time referent. However, the potential for this measurement error would exist in both the treatment and control groups and thus would not be a threat to the internal validity of the study. Another limitation is the fact that the types of violence assessed are more typical of the types of violence used by boys than those used by girls. For example, numerous studies have found that relational aggression or aggression intended to harm relationships is commonly used by girls (Card et al. 2008), but we did not include an assessment of relational aggression. Thus, we did not capture important aspects of female peer violence. Additionally, neither the victimization nor perpetration

measure distinguished acts that were initiated from those used in self-defense. For example, the perpetration act of hitting someone with a fist could reflect initiated perpetration or, alternatively, self-defense from perpetration initiated by someone else. The victimization experience of having been beaten up could have been the result of starting a fight with a formidable opponent or the result of an unprovoked attack by a peer. Thus, program effects on victimization and perpetration may not reflect effects on “victimization” and “perpetration” per se but rather effects on producing a less violent environment in general. And lastly, the peer violence victimization and perpetration measures were anchored to violence received by or perpetrated against adolescents of the same sex and about the same age. As a result, violence between peers of different ages and violence between peers of the opposite sex who were not dating were not captured; thus, the overall prevalence of violence may be underestimated, decreasing the power to detect program effects. Also, violence between same sex dating partners, which would more accurately be defined as dating violence, was included in the peer violence measures, which could have inflated program effects on peer violence. However, the prevalence of same sex dating at the targeted ages is low. For example, pooled data from eight jurisdictions participating in the 2005 and/or 2007 Youth Risk Behavior Surveys show that just 1.2% of high school students aged 13–18 years identified as gay/lesbian and 3.4 % as bisexual (Mustanski et al. 2014).

There were many strengths of the study. The greatest strength was the use of an experimental design that controlled most threats to internal validity. Although threats imposed by differential attrition and/or differential predictors of attrition by treatment condition were not controlled by the design, we found no evidence of differential attrition; the amount of attrition was similar for the treatment and control groups, and there was no difference between treatment and control groups in the predictors of dropout. Also, participation in the program was high, and there was low attrition from baseline to follow-up. Additionally, the 1-year follow-up time period was sufficient to capture the behavioral changes.

Youth violence is a serious societal and public health concern. Approximately 600,000 young people are treated in emergency departments for assault-related injuries each year, and homicide is the second leading cause of death among youth aged 15–24 years (Centers for Disease Control and Prevention [CDC] 2013). Numerous studies have demonstrated the negative academic, developmental, mental health, and physical consequences of being victimized by peers (Dahlberg 1998; Ozer et al. 2004) and of using violence against peers (Lipsey and Derzon 1998); further, many of these consequences lead to problems in adulthood. Many school districts in the USA require implementation of violence prevention efforts, and many other schools implement such programs voluntarily because of the negative impact that violence has on youth development, including academics (Herrenkohl et al. 2000). With increasing school burdens and decreasing resources, implementation of *Safe Dates* may be an efficient way to prevent multiple types of youth violence. However, additional studies are needed to determine whether these program effects can be replicated.

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Table 1
Baseline prevalence of violence indicators in the total sample by sex and minority status ($n=1,690$)

	Total (%)	Male subjects (%)	Female subjects (%)	X ²	p value	Minority (%)	White (%)	X ²	p value
Perpetration									
Beat up	45	66	26	258.9	<.001	54	42	19.18	<.001
Hit with fist	57	76	40	225.2	<.001	63	55	7.69	.01
Threatened with weapon	13	20	6	66.6	<.001	22	10	42.42	<.001
Used weapon	6	10	3	27.5	<.001	22	4	27.73	<.001
Victimization									
Beat up	19	30	9	109.2	<.001	22	18	3.52	.06
Hit with fist	49	68	31	226.3	<.001	49	49	0.04	.84
Threatened with weapon	21	32	11	110.5	<.001	25	19	7.22	.01
Used weapon	7	11	2	54.2	<.001	10	6	8.32	.004
School weapon carrying	15	25	6	121.1	<.001	19	14	6.87	.01

Table 2
Effect of exposure to *Safe Dates* on peer violence victimization at 1-year follow-up ($n=1,620$)

Variable	<i>b</i>	Standard error	<i>p</i> value	EXP B	95 % Confidence interval
Treatment (vs control)	-0.13	0.05	.04	0.88	(0.78, 0.99)
Baseline victimization	0.18	0.02	<.001	1.20	(1.15, 1.25)
Male subjects (vs female subjects)	1.14	0.14	<.001	3.11	(2.38, 4.07)
Minority (vs nonminority)	0.03	0.07	.69	1.03	(0.90, 1.18)
Parent education	0.08	0.08	.37	1.08	(0.91, 1.27)
Treatment × minority	-	-	-	-	-
Treatment × sex	-	-	-	-	-

Reference group is in parentheses

Table 3
Effect of exposure to *Safe Dates* on peer violence perpetration at 1-year follow-up ($n=1,620$)

Variable	<i>b</i>	Standard error	<i>p</i> value	EXP B	95 % Confidence interval
Treatment (vs control)	-0.04	0.06	.48	0.96	(0.84, 1.09)
Baseline perpetration	0.16	0.01	<.001	1.17	(1.14, 1.19)
Male subjects (vs female subjects)	0.92	0.10	<.001	2.52	(2.06, 3.07)
Minority (vs nonminority)	0.29	0.05	<.001	1.33	(1.20, 1.48)
Parent education	0.02	0.06	.76	1.02	(0.90, 1.15)
Treatment × minority	-0.22	0.08	.004	0.80	(0.69, 0.93)
Treatment × sex	—	—	—	—	—

Reference group is in parentheses

Table 4
Effect of exposure to *Safe Dates* on weapon-carrying onset by 1-year follow-up ($n=1,397$)

Variable	Adjusted odds ratio	p value	95 % Confidence interval
Treatment (vs control)	0.69	.005	(0.54, 0.88)
Male subjects (vs female subjects)	5.84	<.001	(3.99, 8.55)
Minority (vs nonminority)	1.03	.88	(0.68, 1.56)
Parent education	1.19	.35	(0.83, 1.70)
Treatment × minority	–	–	–
Treatment × sex	–	–	–

Reference group is in parentheses