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An Athletic Coach–Delivered Middle School Gender Violence Prevention Program

A Cluster Randomized Clinical Trial

Elizabeth Miller, MD, PhD,^{✉1} **Kelley A. Jones**, PhD, MPH,¹ **Lisa Ripper**, MPH,¹ **Taylor Paglisotti**, BA,¹ **Paul Mulbah**, BS,¹ and **Kaleab Z. Abebe**, PhD²

¹UPMC Children's Hospital of Pittsburgh, Division of Adolescent and Young Adult Medicine, University of Pittsburgh School of Medicine, Pittsburgh, Pennsylvania

²Division of General Internal Medicine, University of Pittsburgh, School of Medicine, Pittsburgh, Pennsylvania

[✉]Corresponding author.

Article Information

Corresponding Author: Elizabeth Miller, MD, PhD, Adolescent Medicine, University of Pittsburgh, 120 Lytton Ave, Ste 302.2, Pittsburgh, PA 15213 (elizabeth.miller@chp.edu).

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Key Points

Questions

Does an athletic coach–delivered gender violence prevention program for middle school male athletes increase positive bystander behaviors and reduce relationship abuse and sexual violence perpetration among youth athletes?

Findings

In this cluster randomized clinical trial including 973 male athletes, athletes in schools with sports teams receiving the Coaching Boys Into Men program had greater increases in positive bystander behaviors. In schools implementing the program, athletes who had ever dated were less likely to perpetrate abuse against a partner.

Meaning

This athletic coach–delivered program is one prevention strategy to consider for increasing positive bystander behaviors and reducing relationship abuse and sexual violence.

Abstract

Importance

Adolescent relationship abuse (ARA) and sexual violence (SV) reported among adolescents point to the need for prevention among middle school–age youths.

Objective

To test an athletic coach–delivered relationship abuse and sexual violence prevention program among middle school male athletes.

Design, Setting, and Participants

An unblinded cluster randomized clinical trial from spring 2015 to fall 2017 at 41 middle schools (38 clusters). The study included 973 male middle school athletes (ages 11–14 years; grades 6–8; participation rate 50%) followed up for 1 year (retention 86%).

Interventions

Coaching Boys Into Men (CBIM) is a prevention program that trains athletic coaches to talk to male athletes about (1) respectful relationship behaviors, (2) promoting more gender-equitable attitudes, and (3) positive bystander intervention when harmful behaviors among peers are witnessed.

Main Outcomes and Measures

The primary outcome was change in positive bystander behaviors (ie, intervening in peers' disrespectful or harmful behaviors); secondary outcomes were changes in recognition of what constitutes abusive behavior, intentions to intervene, gender-equitable attitudes, and reduction in recent ARA/SV perpetration (at end of sports season and 1-year follow up).

Results

Of the 973 participants, 530 were white (54.5%), 282 were black (29.0%), 14 were Hispanic (1.4%), and the remainder were multiracial, other race/ethnicity, or not reported. Positive bystander behaviors increased at end of sports season and at 1-year follow-up (relative risk, 1.51; 95% CI, 1.06–2.16 and 1.53; 95% CI, 1.10–2.12, respectively) as did recognition of abuse (mean risk difference, 0.14; 95% CI, 0.01–0.27 and 0.14; 95% CI, 0.00–0.28, respectively). At 1-year follow-up, among those who ever dated, athletes on teams receiving CBIM had lower odds of reporting recent ARA/SV perpetration (odds ratio, 0.24; 95% CI, 0.09–0.65).

Gender attitudes and intentions to intervene did not differ between study arms. In exploratory intensity-adjusted and per protocol analyses, athletes on teams receiving CBIM were more likely to report positive bystander behaviors and to endorse equitable gender attitudes and less likely to report ARA and sexual harassment perpetration 1 year later.

Conclusions and Relevance

An athletic coach–delivered program for middle school male athletes is an effective strategy for reducing relationship abuse among younger adolescents.

Trial Registration

ClinicalTrials.gov Identifier: [NCT02331238](#)

This randomized clinical trial tests an athletic coach–delivered relationship abuse and sexual violence prevention program among middle school male athletes.

Introduction

Adolescent relationship abuse (ARA; physical and sexual violence and psychological aggression in adolescent dating relationships) and sexual violence (SV; sexual harassment, sexual assault, and rape) are common in adolescence and increase in prevalence into young adulthood, with serious health and social consequences.^{1,2,3} Sexual harassment occurs in middle school and high school at high rates.^{4,5,6} Adolescent relationship abuse, SV, and bullying behaviors, including sexual harassment, have significant overlap.^{7,8,9,10} Homophobic teasing among adolescents is prevalent (among boys in particular) and is a form of gender-based and bias-based harassment that is a well-documented precursor to SV perpetration.^{7,8,11,12} Prevention programs working with early adolescents should address homophobic teasing and sexual harassment with the goal of preventing ARA/SV perpetration.^{13,14,15}

Despite how common ARA and SV are among younger adolescents, only 4 school-based prevention programs are considered effective in preventing ARA/SV, all requiring classroom instruction.^{5,16,17,18} Only 2 programs target middle school,^{5,18} none use adults outside the classroom, such as coaches, and none challenge gender norms combined with building positive bystander behavior skills.

Coaching Boys Into Men (CBIM), developed by a national nonprofit violence prevention organization (Futures Without Violence), is an ARA/SV prevention program that trains athletic coaches to talk to their male athletes about respect, nonviolence, consent, and responsibility to speak out against violence toward women. A randomized clinical trial with high school male athletes in California found that athletes in schools that offered CBIM had increased intentions to intervene when witnessing peers' harmful behaviors, more positive bystander behaviors, less negative bystander behaviors, and less ARA perpetration.^{19,20} This study aims to evaluate CBIM with middle school male athletes.

Methods

This trial examined the effectiveness of CBIM on male athletes in middle school–based athletic programs (male-only and coeducational sports). The experimental design involved a parallel arm, unblinded, cluster randomized clinical trial conducted with coaches and student athletes recruited from 43 middle schools in western Pennsylvania based on their size and availability of an athletics program. A cluster was defined as 1 or more middle schools sharing common sports teams. Clusters were randomly assigned to either CBIM intervention or control (standard coaching) condition by the study's biostatistician in a 1-to-1 allocation ratio and stratified by school size (<200 students per middle school vs ≥200 students). Owing to the nature of the intervention, the investigators, research staff, school administration, coaches, and athletes could not be blinded to intervention assignment. However, to prevent influencing school districts' participation, schools were assigned to a study arm only after confirming school approval to participate ([Figure](#)). The study principal investigator and study coordinator were blinded to the randomization scheme until all schools were randomized, which ensured allocation concealment. Study procedures, outcome measures, and analysis plan are presented elsewhere.²¹ The University of Pittsburgh institutional review board approved the study. Written informed consent was obtained from the parents of students, except in schools that had waivers of parents permission. The CONSORT guidelines were followed ([Figure](#)). The formal trial protocols have been published elsewhere²¹ and can be found in [Supplement 1](#).

Recruitment, Randomization, and Data Collection

Intervention and control sites included public and private schools in western Pennsylvania. Eligible schools had formal athletics for sixth through eighth grade occurring after school hours. Students enrolled in male or coeducational athletics programs at participating middle schools received information about the study, parent information letters, and consent forms. Students who

returned signed parent consent forms (or, in schools with waivers of parent permission, students whose parents did not opt out of participation) provided assent and were enrolled by research assistants. Participants completed online surveys containing primary and secondary outcomes at baseline, end of the sports season (approximately 4-8 weeks after baseline depending on the sport), and approximately 1 year after baseline. Surveys were mostly administered on electronic tablets via a web-based secure survey system, with paper surveys as backup. Participants self-identified race and ethnicity from a list of options as well as other demographic characteristics, so that baseline differences in intervention/control allocation could be assessed ([Table 1](#)). To ensure confidentiality of student responses and promote honest reporting, surveys were collected and linked anonymously, as described previously.²²

Intervention and Control Conditions

In intervention schools, prevention advocates from local rape crisis centers and domestic violence agencies introduced the program to athletic coaches via 60-minute trainings. Advocates assisted coaches with implementation-related challenges (eg, lack of time) via biweekly check-ins recommended by program developers (<http://www.coachescorner.org>). Coaching Boys Into Men consisted of a 15-minute coach-led discussion using program training cards per week over 12 weeks. Coaches talked to their athletes about (1) disrespectful and harmful vs respectful behaviors among peers (including homophobic teasing) and in relationships, (2) myths glorifying male sexual aggression and promoting more gender-equitable attitudes, and (3) positive bystander intervention when athletes witness peers' aggressive male behaviors toward girls. Coaches completed surveys and interviews at the end of the season to assess degree of program completion. Fidelity to intervention was assessed by research staff's intermittent observations of program implementation. Control schools received no training or programming and conducted coaching as usual.

Outcomes

The primary outcome (positive bystander behavior) was measured by asking participants how they responded to witnessing 9 disrespectful/harmful behaviors among male peers in the past 3 months. Endorsements of at least 1 positive response per item were summed across items (range, 0-9; eTable in [Supplement 2](#)).^{23,24,25,26,27 Secondary outcomes included recognition of abuse, gender-equitable attitudes, intentions to intervene with peers, and dating abuse in past 3 months. Exploratory (not prespecified) outcomes included homophobic teasing, sexual harassment, and cyber sexual abuse and sexting. Violence perpetration was measured by self-report, consistent with recommendations in violence prevention research (eTable in [Supplement 2](#)), and relevant descriptive statistics were calculated for all outcomes ([Table 2](#)).}

Additionally, an exploratory outcome of experience of any ARA or SV perpetration was coded as both a summary score and a dichotomized variable. Summary scores were calculated as a count of 3 different types of perpetration (any ARA, any cyber sexual abuse, and any sexual harassment perpetration; range, 0-3). The dichotomized variable, any perpetration of ARA, cyber sexual abuse, or sexual harassment, was coded as "1."

Sample Size and Statistical Analyses

Power and sample size considerations are discussed in detail elsewhere.²¹ The planned sample size for the primary outcome assumed a within-school intraclass correlation (ICC) of 0.02 and 13 clusters per arm, which yielded a necessary sample size of 988 students per arm ($n = 1976$) to detect a standardized effect size of 0.20 with 80% power. Owing to lower-than-anticipated recruitment, 12 additional clusters were enrolled; the estimated ICC at baseline was used to update our ICC to 0.004. With these assumptions as well as a 10% attrition rate through 1-year follow up, 850 total participants were needed to maintain power.

Generalized linear mixed (GzLM) models with subject-level and school-level random intercepts accounted for clustering of observations within students and students within schools. For the primary outcome (positive bystander behavior), GzLM models with a negative binomial distribution family included variables for baseline outcome value, time, and treatment group to assess whether there was a significant and positive treatment effect. Secondary outcome GzLM models included variables for baseline outcome value, time, and treatment group. Count outcomes (ie, negative bystander behavior) used a negative binomial family. Binary and continuous outcomes used binomial and gaussian families, respectively. Primary assessment of intervention effects was based on intent-to-treat estimates of positive bystander behavior at the end of the sports season. As-treated effect parameters were estimated in 2 exploratory analyses (described elsewhere).²¹ An intervention intensity variable estimated the number of CBIM cards completed by each coach (based on research staff observations and coach self-reports) divided by 12 (full program completion); this variable substituted for the treatment group variable. For per protocol analysis, only intervention athletes whose coaches delivered 8 or more cards (intervention delivered as intended; 33 of 66 of intervention teams [50%]) was counted as receiving the intervention; others were excluded from analyses.

To assess differences at baseline between students in the intervention and control groups, demographics such as grade level, race/ethnicity, nativity, and parental education were compared while accounting for within-school clustering.

The mechanism for missing data was assessed by comparing baseline characteristics between participants who did and did not have the primary outcome measurement at the 1-year follow-up. No evidence suggested the data were missing owing to anything other than missing at random; likelihood-based analytic models were sufficient to address this study's primary question without concern for bias.^{28,29} Cluster-level ICCs are presented for the primary outcome. The primary hypothesis of positive bystander behavior at end of sports season used a 2-sided test, with a significance level of .05. Cronbach α was calculated as a measure of internal consistency for continuous measures. SAS software, version 9.4, was used for all statistical analyses (SAS Institute Inc).

Results

Fifty-two schools (48 clusters) were interested in participating and were randomized to the study arms. Eight clusters (9 schools; 5 intervention and 4 control) withdrew, and 2 clusters (2 schools, 1 intervention and 1 control) were removed owing to lack of parental consent, resulting in 38 clusters (41 schools; 20 intervention and 21 control). Of 157 eligible male or coeducational teams, 76% (119 teams) participated in the study. At the student level, 1046 of 1970 eligible male athletes (53%) returned signed parental consent forms; of those, 1046 (93%) provided youth assent and nearly 100% took baseline surveys (final n = 973).

School districts included urban, suburban, and rural schools with a range of socioeconomic status, from 24.4% to 92.3% of students receiving free and reduced lunch (median = 71.7%). Three schools were private (2 intervention and 1 control). The races/ethnicities of participating schools, reported as the median across schools, were 53.5% white, 33.5% black, 1.0% Hispanic, 0.6% Asian, and 6.5% multiracial.²¹

Study participants' self-reported ethnicities were 54.5% white (n = 530), 29.0% black (n = 282), 1.4% Hispanic (n = 14), 2.1% Asian (n = 20), 5.8% multiracial (n = 56), and 4.1% other racial group (n = 40) (3.2% [n = 31] did not report race/ethnicity; [Table 1](#)).

Compared with participating schools' overall demographics, a smaller proportion of participants were black, and more self-identified as Asian or another racial group. Most (n = 879; 90.3%) were born in the United States. Half (52.1%) of the participants reported having a parent/caregiver with a college or graduate degree; however, 20.7% (n = 201) did not know either parent's education level (a proxy for socioeconomic status). The most common participating sports teams were basketball (28.8%; n = 280) and soccer (21.5%; n = 209). Grade level, race/ethnicity, being born in the United States, parental education, and sports team did not vary by treatment arm.

All athletes on teams implementing CBIM were exposed to the program, regardless of their research participation. Participation rates in the research varied across teams from 10% to 100% of eligible athletes per team enrolling in the study, ie, returning parent consent form and completing youth assent (overall participation rate = 50%). Retention was similar by treatment for end of season (95% intervention [n = 493 of 518] and 98% control [n = 444 of 455]) and 1 year (84% intervention [n = 433 of 518] and 89% control [n = 403 of 455]). Comparing those who completed 1-year follow-up surveys and those who did not, noncompleters were more likely to be eighth graders when compared with seventh graders, more likely to identify as black or another racial/ethnic group not listed in comparison with white participants, and more likely be born outside the United States. No differences were found by study arm, parental education, or sports team.

At the end of the sports season, athletes in schools with CBIM reported more positive bystander behaviors than athletes in control schools (relative risk [RR], 1.51, 95% CI, 1.06-2.16) as well as greater recognition of abuse (mean risk difference [MRD], 0.14; 95% CI, 0.01-0.27). No other intervention effects emerged at end of the sports season in the intent-to-treat analyses ([Table 3](#)). No changes were noted in the exploratory intervention intensity adjusted or per protocol analyses.

At 1-year follow-up, athletes in schools receiving CBIM reported more positive bystander behaviors (RR, 1.53; 95% CI, 1.10-2.12) and greater recognition of abusive behaviors (MRD, 0.14; 95% CI, 0.00-0.28) than control schools. For the overall sample, there was no change in ARA, cyber sexual abuse, or sexual harassment perpetration (combined variable). Among those who ever dated, athletes on teams receiving CBIM had lower odds of reporting ARA perpetration (OR, 0.24; 95% CI, 0.09-0.65). Gender attitudes, intentions to intervene, cyber abuse perpetration, sexual harassment, and homophobic teasing perpetration were not significantly different from control participants.

In intensity-adjusted and per protocol post hoc analyses at 1-year follow-up, positive bystander behaviors were more likely to be reported among athletes in CBIM schools compared with control schools (RR, 2.48; 95% CI, 1.62-3.78 and 2.16; 95% CI, 1.47-3.17, respectively). More equitable gender attitudes (higher scores) were noted among athletes in CBIM schools in intensity-adjusted analyses only (MRD, 0.09; 95% CI, 0.01-0.16), as were greater reductions in any ARA, cyber sexual abuse, or sexual harassment perpetration (combined variable) compared with control schools. Reductions were present when this outcome was modeled as a dichotomous variable (OR, 0.47; 95% CI, 0.22-0.99) and as a summary score (RR, 0.69; 95% CI, 0.48-1.00). Among those who ever dated, both intensity-adjusted and per protocol analyses revealed greater reduction in odds of relationship abuse perpetration (0.10; 95% CI, 0.03-0.35 and 0.18; 95% CI, 0.06-0.58, respectively) compared with control participants.

Discussion

The CBIM violence prevention program has already been shown to be effective in reducing relationship abuse perpetration among high school-aged male athletes.¹⁹ This study found that this program appears to be effective in increasing positive bystander behaviors and recognition of abusive behaviors and reducing relationship abuse perpetration (among those who dated) with younger male athletes attending private or public middle schools in urban, suburban, and rural districts. The program did not result in changes in intentions to intervene nor in exploratory outcomes, ie, negative bystander behaviors, sexual harassment, cyber sexual abuse perpetration, or homophobic teasing. When intervention intensity was considered, increases in equitable gender attitudes emerged.

High school coaches report that the program is acceptable and feasible to implement.³⁰ We found middle school athletic programs to be less structured (eg, shorter practice times) with much shorter seasons (generally 4-8 weeks) compared with high school sports. The short sports season resulted in less time for curriculum delivery, for practicing bystander behavior skills discussed in the program, and for peer-to-peer reinforcement of program content. This may explain the lack of changes in most outcomes of interest at the end of the sports season. Coaches struggled with completing the program as intended; only 50% completed 8 or more cards ($n = 33$ of 66). Coaches of middle school teams may not be as connected to their athletes compared with high school athletic programs; the coaches for middle school sports may not be closely affiliated with their schools and have infrequent contact with students, which may affect the coach-athlete relationship, a core component of this prevention program. Qualitative analyses of implementation facilitators and barriers at the middle school level are planned. Strategies to improve implementation are needed, especially because the intervention-adjusted analyses revealed greater odds of using positive bystander behaviors and shifting gender attitudes as well as reduced relationship abuse and SV. Despite these implementation challenges, the program still revealed significant changes in the primary outcome at both end of season and 1 year later and could be an important adjunct to comprehensive dating and sexual violence prevention efforts in schools.

As with the high school CBIM program, there was a relative reduction in relationship abuse perpetration among daters (ie, the frequency increases in the control arm over time while slightly decreasing in the intervention arm). This pattern was not observed with cyber abuse, sexual harassment, or homophobic teasing perpetration, suggesting that these behaviors (which are frequent and likely normative in early adolescence) may require even more intensive prevention programs. Coaches may be less comfortable addressing homophobic teasing and may have less familiarity with social media to address cyber abuse. Greater focus on these card topics during the CBIM training may be needed.

Athletes in schools that received CBIM noted greater recognition of abusive behaviors and reported engaging in more positive bystander behaviors 1 year after the program. In contrast to findings from the CBIM high school study, there did not appear to be intervention effects on intentions to intervene. Continued attention to helping athletes develop skills to intervene positively and safely is needed while simultaneously discouraging negative bystander behaviors (ie, laughing, going along with such behaviors, or staying silent). In contrast to the high school study that found no changes in gender attitudes, middle school athletes reported greater shifts toward more equitable gender attitudes when adjusting for receiving greater intensity of the program. This finding suggests that the gender-transformative elements embedded in the program may be particularly relevant for this developmental stage when early adolescents are beginning to explore romantic relationships.

Limitations

The rigorous approach using a cluster-randomized trial design combined with partnerships with school districts, athletic directors, coaches, and violence prevention advocates were significant strengths. The study also had limitations. While the schools included a mix of urban and suburban schools, more than half of the sample was white, and findings may not be generalizable. Almost all the school districts required signed parental consent forms for participation in the study, which limited students' participation (ie, students who forgot to bring back consent forms and those without a parent readily available were not eligible to participate). Thus, the study sample is biased toward students and schools with greater parent engagement, but this was balanced by the treatment arm. Several schools withdrew from the study and coaches declined participation for their teams following randomization but prior to participant enrollment. We did not systematically collect reasons for school and coach declinations, but it is possible that administrators or coaches felt uncomfortable with the material. However, rates of withdrawal were similar among those assigned to control, suggesting other unmeasured factors may have driven their decisions. Middle school athletic programs tend to be small and informal, which necessitated recruiting many more middle schools than originally anticipated (41 compared with 26) to achieve a sufficient sample size, which may also have affected the quality of implementation at each site because we had a limited number of advocates owing to finite resources.

Conclusions

In summary, this study evaluated the effectiveness of an athletic coach-delivered SV and relationship abuse prevention program for male athletes in middle school and found increases in positive bystander behaviors and recognition of abusive behavior as well as reductions in relationship abuse perpetration. Findings add to the growing evidence base regarding developmentally appropriate and age-appropriate violence prevention programs as well as the role of coaches and other supportive adults in adolescent health promotion.

Notes

Supplement 1.

Trial protocol.

Supplement 2.

eTable. Individual Youth-Level Outcome Measures

Supplement 3.

Data Sharing Statement

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Figures and Tables

Figure.

CONSORT Diagram

LTF indicates lost to follow-up; T2, end-of-season surveys; T3, 1-year follow up.

^aA waiver of written parental consent was obtained in 1 intervention and 1 control school for the final season of enrollment (fall 2016).

Table 1.

Sample Characteristics

Abbreviation: GED, general education development.

^aPercentages may not sum to 100 owing to small amounts of missing data.

Table 2.

Outcome Values at Each Time

Abbreviation: ARA, adolescent relationship abuse.

^aStandard errors account for school-level clustering.

^bRange of 0-9.

^cRange of 0-3.

^dMean scores were on a 1-5 scale, with a higher mean score indicating greater ARA recognition, more equitable attitudes, and greater intentions to intervene.

Table 3.

Intervention Effects at End of Sport Season and 1-Year Follow-up^a

Abbreviations: ARA, adolescent relationship abuse; ICC, intraclass correlation; MRD, mean risk difference; NA, not applicable; OR, odds ratio; RR, relative risk.

^aAll models account for within-participant clustering. Within-school ICCs are presented for primary outcome models; the school-level random effect for the end-of-season per protocol analysis was 0, and thus an ICC for this model could not be calculated. No demographic differences were found by treatment arm and thus are not included as covariates.

^bAdjusted for having ever dated at baseline.