Original Research

Journal of Interpersonal Violence
1–24

DOI: 10.1177/08862605211006354
journals.sagepub.com/home/jiv
SAGE

How Does *Green Dot* Bystander Training in High School and Beyond Impact Attitudes Toward Violence and Sexism in a Prospective Cohort?

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Abstract

Bystander interventions are recognized as "promising" programming to reduce sexual violence. Gaps in current evaluations include limited follow-up post-training (beyond 24 months) and knowledge of additional bystander training during follow-up. In this prospective cohort study, nested in a cluster randomized controlled trial (RCT), three cohorts of high school (HS) seniors were recruited (Fall 2013-2015) and followed through Spring 2018 (n = 1,831). Training was based on their school cluster RCT assignment and receipt of additional Green Dot (GD) training after HS. Training was hypothesized to be associated with lower scores indicating less acceptance of violence or sexism. Sixty percent reported GD training after HS (68.7% of 986 in intervention and 50% of 845 in control conditions). No significant differences ($p < .05$) were observed by GD training for four of the five violence acceptance or sexism attitudinal measures at recruitment or final surveys. For "ambivalent

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sexism" alone was there a significant reduction in scale scores over time in the intervention versus control condition. Additional GD training after the RCT significantly reduced neither violence acceptance nor sexism scores over time. GD training does not appear to have a consistent longer-term impact on reducing violence acceptance and sexism.

Keywords
intervention/treatment, domestic violence, adolescents, sexual harassment, intervention, sexual assault

Sexual violence (SV), sexual harassment, and intimate partner violence (IPV) continue as significant health threats for young women and men (Basile & Smith, 2011; Smith et al., 2018). Centers for Disease Control and Prevention guidance was used to operationally define SV, IPV, and teen dating violence (Basile et al., 2014; Breiding et al., 2015). Briefly, SV includes sexual coercion, in which a person is pressured verbally or intimidated to consent or acquiesce to sex; alcohol or drug-facilitated sex acts where consent cannot be given, and physically forced sex, which may or may not result in completed penetrative acts. Sexual harassment is included within SV and is described as unwanted sexual contact or non-contact unwanted sexual experiences. SV impacts the academic trajectories of victims (Huerta et al., 2006; Griffin & Read, 2012; Mengo & Black, 2015), and these forms of violence have significant economic costs (Peterson et al., 2017; Peterson, et al., 2018).

IPV was defined to include physical or SV, stalking, psychological aggression, and control of reproductive or sexual choices by a current or former partner of the opposite or same sex (Breiding et al., 2015). Teen dating violence is a type of IPV experienced by teens (age 13-19 years), and among those in a close, yet not necessarily sexual, relationship. As with IPV, teen dating violence includes physical violence (e.g., hitting, kicking, or using another type of physical force), SV (e.g., forcing or attempting to force a partner to take part in a sex act, sexual touching, or a non-physical sexual event like sexting), when the partner does not or cannot consent, psychological aggression (e.g., verbal and non-verbal communication with the intent to harm another person mentally or emotionally and/or exert control over another person), and stalking (e.g., repeated, unwanted attention, and contact by a partner that causes fear or concern for safety).

While intimate partner and SV have recently received due recognition (e.g., 2013 reauthorization of Violence Against Women Act; Not Alone 2016) given their health and economic impact, rates of SV have not significantly

declined over the past two decades among adolescents and young adult women and men (Smith et al., 2018). This is particularly true for young persons who identify as LGBTQ—lesbian, gay, bisexual, transgender, and questioning (Walters et al., 2013).

Primary Prevention Approach

Primary prevention of SV has been recognized as an essential strategy to reduce the above-highlighted life and health impact of SV and teen dating violence. Bystander approaches to SV prevention have specifically been recognized as "promising prevention strategies" (DeGue et al., 2014). Briefly, bystander training was theorized to reduce SV by changing attitudes that support or accept SV within a community and enabling motivated individuals to recognize and effectively act, as an engaged bystander, in those situations they deem as at risk for SV or other forms of violence (Darley & Latane, 1968). Several systematic reviews have summarized the efficacy of bystander programs to reduce attitudes toward violence (Bell et al., 2019; Kettrey et al., 2019; Storer et al., 2016). Taken together, findings from 12 randomized and non-randomized studies observed significant and positive reductions in the most commonly used measure of SV norms, the Rape Myth Acceptance (RMA) in the immediate post-training period (Amar et al., 2015; Baker et al., 2014; Banyard et al., 2007; Foubert et al., 2007; Moynihan et al., 2010), at 1-4 months post-training (Baker et al., 2014; Banyard et al., 2007; Gidycz et al., 2011; Moynihan et al., 2010), at 6-7 months (Foubert et al., 2007; Gidycz et al., 2011; Salazar et al., 2014), and up to 12 months post-training (Cares et al., 2015; Coker et al., 2019; McMahon et al., 2014). To summarize, bystander training appears to change SV acceptance over time and across programs for up to 12 months post-training yet little is known of bystander intervention efficacy beyond 24 months. Further, little is known of the effect of additional bystander training over time on attitudes toward violence or sexism. To realize the promise of bystander training, and specifically, the Campus SaVE requirement that institutions of higher learning receiving Title IX funding offer bystander training to reduce SV, prospective evaluations of bystander programs need to consider all bystander training received.

Research Aims

Our goal for this cohort study ("*Life's Snapshot*") was to investigate the longer-term impact of bystander training as seniors move from high school (HS) into their adult lives. Here we focused on the noted gaps of needing longer follow-up post-training and consideration of additional training reported

during follow-up. We sought to measure violence acceptance and sexist attitudes when HS seniors were recruited and at final follow-up, 2-4 years after recruitment. As noted above in this brief review, bystander interventions to reduce sexual or dating violence have been evaluated and found to reduce violence acceptance among college, HS, and middle school populations. In the cluster randomized controlled trial (RCT) on which *Life's Snapshot* was based on the bystander intervention, *Green Dot* (*GD*), was effective in reducing violence acceptance in the intervention versus control schools over time (Coker et al., 2019), and most importantly in reducing SV perpetration and victimization particularly during those years *GD* was fully implemented (Coker et al., 2017). Importantly, relative school-level changes to bystander behaviors, violence acceptance appears to explain how this (*GD*) bystander intervention appears to reduce SV perpetration and victimization over time in HSs (Bush et al., 2019).

The objectives for this cohort analysis were to examine how prior intervention training affected participating seniors' attitudes toward violence acceptance and sexism at recruitment, final survey, and the change over time; and with additional bystander training after HS. *GD* bystander training was defined based on (1) school-level randomization of participating seniors (as randomized), and (2) self-reported receipt of additional *GD* training after HS by randomized conditions (as reported).

The following two hypotheses were tested

H_1: Seniors attending HSs randomized to the bystander intervention versus control conditions would have lower scores indicating less violence acceptance (three measures) and sexist attitudes (two measures). This pattern would hold at recruitment and final time points. For analyses over time, reductions in scores would be greater among those randomized to the intervention versus control condition.

H_2: Seniors attending HSs randomized to the bystander intervention who received additional bystander training (Group 1), would have the lowest attitude scores at recruitment and final surveys. Those randomized to the intervention but did not receive additional training (Group 2); and those randomized to the control who did receive *GD* training after HS (Group 3) were hypothesized to have intermediate attitudinal scores. The reference group for all three training groups was those attending control schools who did not report additional *GD* training (Group 4). For analyses over time, reductions in scores would be greatest among those randomized to the intervention with additional *GD* training (Group 1), intermediate for Groups 2 and 3, and least for those with no training (Group 4).

Methods

Participants

This prospective analysis was based on three cohorts of HS seniors recruited into *Life's Snapshot*. The purpose of *Life's Snapshot* was to evaluate the longer-term efficacy of a bystander-based intervention program designed to reduce interpersonal violence. Recruitment into this cohort was based on the prior cluster-based RCT (U01CE001675), designed to evaluate the effectiveness of *GD* in HSs across Kentucky where 13 schools were randomized to receive the intervention (*GD*) and 13 received usual care (delayed intervention) (Coker et al., 2019).

GD Intervention (Cluster RCT). As described in detail elsewhere (Coker et al., 2017; Cook-Craig et al., 2014), *GD* was developed for college settings by Dr. Dorothy Edwards. This program was adapted for HS settings such that trained Rape Crisis Center staff (hereafter educators) delivered the adapted curriculum to students in two time-framed phases. One time 50-minute introductory persuasive speech was provided to the majority of students (>50%) in intervention schools (Phase 1) beginning in the fall of 2010 (Year 1). Phase 2 was implemented beginning spring 2011 (Year 2) using the popular opinion leader (POL) strategy in which identified student leaders were invited to participate in intensive (5-hour) bystander training. Both training phases focused on violence victimization, perpetration, and prosocial engaged bystander behaviors to recognize potentially risky situations and take direct action, distract or delegate actions to others to reduce the likelihood of violence (three Ds). Surveying to evaluate this intervention began in the spring of 2010 (Year 0 before implementation), and continued annually through spring 2014 (Year 4).

Procedures

Recruitment for this prospective cohort (*Life's Snapshot)* began in the fall of 2013, 2014, and 2015 with three cohorts of rising seniors included. Administrators of the same 26 HSs who participated in the cluster RCT were asked to continue participation in this cohort and sign Memoranda of Understanding with investigators agreeing to allow participant recruitment and surveying in the schools. The majority (24 of 26) of the original HSs agreed: 12 intervention and 12 control schools. With assistance from school administrators, introductory letters were sent to parents of all seniors in the

recruited HSs describing the study and providing information for parents to opt their students out of participation. Research staff read elements of consent to students at survey administration. A monetary incentive of a mailed $10 check was offered for survey completion. Students were asked to check a box indicating their decision to participate or opt out of the survey. Personal identifying data were not included in analytic files. The Institutional Review Board at the University of Kentucky (13-0012-F6A) approved this protocol. Researchers received a Certificate of Confidentiality from the National Institutes of Health, National Institute of Child Health, and Human Development for this research project.

After the initial study recruitment in the fall of the students' senior year, those who consented and completed the recruitment survey were invited to subsequent surveys each spring (2014-2018). These annual follow-up surveys were provided electronically via REDCap using personal email provided by participants. Only the recruitment and final survey, conducted in the spring of 2018, included violence acceptance and sexism measures. Each annual survey included measures of additional bystander training received.

Measures

Green dot bystander training.

Exposure to the intervention training was determined based on two indicators: (1) as randomized (H_1), and (2) as additional *GD* training was reported over follow-up by condition (Groups 1-4; H_2). All analyses were conducted among seniors, by sex (male and female), each of the three senior cohorts, and sexual minority or majority status. Analyses to address H_1 were based solely on the original RCT school-level randomization as intervention or control condition. Analyses to address H_2 used the self-reported *GD* training received in HS, and after HS, as reported in follow-up surveys. *GD* training after HS included training participants received in college or in community settings. Additional *GD* bystander training was not provided as part of the original RCT, to seniors after graduation. However, participants in this cohort may have received *GD* training after HS graduation because several colleges were offering versions of *GD* training. Furthermore, versions of *GD* training were offered in some community settings by trained volunteers. We have no data to characterize how similar this self-reported *GD* training was to that delivered by trained Rape Crisis Center Educators in the original RCT. Because bystander programming was becoming more widely available at this time, we focused exclusively on *GD* training to remain specific in this evaluation.

Demographic attributes.
The following were used to describe the participants including sex, race, sexual attraction, current employment, relationship status, receipt of free, or reduced schools meals (as a proxy for family income), and enrollment in advanced placement (AP) courses (as a proxy for plans to attend college). Sexual attraction rather than sexual orientation or gender identity was used because this item was used in the original cluster RCT.

Attitudinal outcomes.
Violence acceptance and sexism were the two constructs investigated as outcomes that may change over time and be associated with training received. Five measures were used within the two constructs: (1) violence acceptance (three subscales), and (2) sexism (two subscales). Details of the psychometric properties were provided elsewhere (Coker et al., 2021). Briefly, the language of the 20-item Illinois RMA Scale (Payne et al., 1999) was modified for HS students and used as a nine-item scale with two subscales which were identified in factor analyses: a six-item measure of SV acceptance (Cronbach's α = .87) and a three-item measure of indicating "women's dress or behavior suggesting sexual consent" (Cronbach's α = .68). A brief measure of physical teen violence acceptance (Cronbach's α = .73) was based on a reduced version of the five-item Acceptance of Couple Violence subscale developed by Foshee et al. (1998). All items measuring violence acceptance were coded such that higher scores indicated greater violence acceptance. Two measures of "sexism" included items from the "Ambivalent Sexism" Inventory (four-items Cronbach's α = .72) developed by Glick and Fiske (1996) and "Negative Attitudes Toward Women" (eight items, Cronbach's α = .80; Range 8-32) informed by the work Benson and Vincent (1980) and Spence et al. (1973).

Statistical Analysis

Our primary outcomes were attitudinal measures of violence acceptance and sexism measured in the senior's fall term between 2013 and 2015, for Cohorts 1-3, respectively, and at the final survey for all participants (spring 2018). The primary exposure, *GD* training, was measured in two ways: as randomized (two groups: Intervention: n = 1,025; Control: n = 806), and updated to include additional *GD* training reported during follow-up (four groups: GD training in and after HS (Group 1; n = 678); GD training in HS only (Group 2; n = 308); GD training only after HS (Group 3, n = 426); and no GD training in, or after HS (Group 4, n = 419). Because these attitudinal measures

were included in the recruitment and final surveys, we measured pre-post changes for participants recruited into Cohorts 1-3 with final surveys completed between 4 and 2 years after recruitment.

For this prospective cohort analyses across three cohorts of *Life's Snapshot*, all analyses were adjusted for cohort (1-3), sex (male or female), sexual minority status (exclusively attracted to the opposite sex [majority] or other [minority]), and whether the participant had taken an AP class in the past 12 months as a senior in HS (yes or no). Sub analyses were conducted by sex, cohort, and sexual minority status. Linear regression (ANCOVA) was used to estimate adjusted mean attitudinal scores among seniors by two sets of GD exposure measures: "as randomized" and "as reported." We used PROC GLM (SAS version 9.4 TS Level 1M3 X64_8PRO platform; Kauermann & Carroll, 2001). Because some attitudinal outcomes were correlated, analyses were repeated using MANCOVA; all findings were essentially identical to those reported here. All analyses were conducted between 2018 and 2020.

Results

Response Rates

Excluding parental refusals (n = 76; .6%), 13,434 seniors attending HSs on scheduled survey dates were eligible to participate (4,580 in Cohort 1, 4,603 in Cohort 2, and 4,251 in Cohort 3; refer to Coker, et al., 2021). Seniors were given the option to participate in this research and were instructed to come to identified locations and times within each school to complete surveys. Across all 24 schools and all three cohorts, 11,965 interested seniors were present and 11,285 completed the survey; 680 opted out of the survey. The response rate was calculated based on the more conservative estimate recommended by the American Association for Public Opinion Research (2015) where the denominator was the number of seniors in attendance at school on the survey date. This overall survey rate was 84.0% (RR = 11,285/13,434) and by cohort the response rate was 77.0% in Cohort 1 (24 school range: 47.5-100%), 85.7% in Cohort 2 (24 school range: 66.5-96.5%), and 89.7% in Cohort 3 (24 school range: 67.0-99.4%). Because the recruitment survey was conducted in school, during school hours, we anticipated and received a high response rate. All follow-up surveys were launched via REDCap using the email provided by participants while they were in HS. We followed back to consenting participants who did not complete REDCap surveys to confirm that they had received these online surveys. Given the differing nature of the surveys, recruitment as an in-school, paper survey, versus electronically delivered follow-up surveys completed on participant's own time, we anticipated and

received a much lower response rate for the final electronic survey (16.8%: 1,897 of 11,285; 11.7-21.3%, respectively, for Cohorts 1-3). The incentive for the final survey, delivered in two parts, totaled $50 or $25 for each part. Survey responses from 66 participants were excluded due to missing data; for the recruitment survey 24 were missing demographics and 31 were missing attitudinal items, and for the final survey, three were missing demographic and eight attitudinal items. A total of 1,831 participants completed both recruitment and final surveys, and have no missing exposure, outcome, or demographic data. Given the analytic sample size (n = 1,831), a p value of ≤.05 was considered statistically significant.

Demographic attributes of participants completing the cohort versus non-completers.

Because many seniors completing the recruitment survey did not go on to complete follow-up surveys, we investigated demographics differences in "completers" (n = 1,831) and those completing only the recruitment survey (n = 6,991). Briefly, "non-completers" were significantly more likely than completers to have been recruited from cohort 1 (refer to Table 1; 32.3% versus 21.6; $p < .0001$), to be male (56.5% versus 30.0%; $p < .0001$), non-white (18.7% versus 16.2%; $p = .01$), sexual majority (86.5% versus 84.6%; $p = .04$), not to have taken AP classes (51.5% versus 27.4%; $p < .0001$) nor attended college (51.5% versus 24.7%; $p < .0001$), not to have received GD training after HS (37.5% versus 29.7%; $p < .0001$), and to live outside an urban area (48.5% versus 41.5%; $p < .0001$; not reported in Table 1). To address potential confounding bias, analyses were adjusted for cohort, sex, sexual minority status, and have taken AP classes. We did not additionally include college attendance and race as confounders, because these demographic attributes were highly correlated with having taken AP classes. Kentucky region was not included as a confounder because the region was factored into the original cluster RCT design. As part of the primary exposure, GD training in HS was not included as a confounder. We noted that "completers" were more likely to come from HSs who received the *GD* intervention thru the cluster RCT (70.3%) than from schools randomized to the control condition (62.5%). This difference was a function of the somewhat larger student body size of intervention versus control schools. Importantly, as a for potential selection bias, "non-completers" had significantly higher violence acceptance and sexism scores for all five measures relative to those "completers" included in this analysis ($p < .0001$).

Table 1. Comparison of Demographic Attributes of "Non-completers" and "Completers" in Life's Snapshot.

	"Non-completers" (Recruitment Survey Only[a]; No Follow-up)	Completers (Completed Both Recruitment and Final Surveys[b])	X^2 2 Tailed *P*-value
Cohort	(*N* = 6,991)	(*N* = 1,831)	<.0001
Cohort 1 (recruited Fall 2013)	2,260 (32.3%)	396 (21.6%)	
Cohort 2 (Fall 2014)	2,514 (36.0%)	642 (35.1%)	
Cohort 3 (Fall 2015)	2,217 (31.7%)	793 (43.3%)	
Sex			<.0001
Female	3,026 (43.5%)	1,281 (70.0%)	
Male	3,927 (56.5%)	550 (30.0%)	
Missing	38	0	
Race (Abbreviated)			.01
White	5,641 (81.3%)	1,534 (83.8%)	
Non-White	1,299 (18.7%)	297 (16.2%)	
Missing	51	0	
Sexual minority status (based on sexual attraction)			.04
Majority (Exclusively attracted to opposite sex)	5,931 (86.5%)	1,549 (84.6%)	
Minority (Not exclusively attracted to opposite sex)	927 (13.5%)	282 (15.4%)	
Missing	133	0	
Taken AP classes			<.0001
No	3,568 (51.5%)	501 (27.4%)	
Yes	3,364 (48.5%)	1,330 (72.6%)	
Missing	59	0	

(*continued*)

Table 1. Continued

	"Non-completers" (Recruitment Survey Only[a]; No Follow-up)	Completers (Completed Both Recruitment and Final Surveys[b])	X^2 2 Tailed *P*-value
GD training received in HS (self-report)			<.0001
No	2,528 (37.5%)	536 (29.7%)	
Yes	4,213 (62.5%)	1,267 (70.3%)	
Attitudinal outcomes	Mean (SE)	Mean (SE)	*T*-test
SV acceptance	11.71 (.05)	10.33 (.09)	<.0001
"Womens' Dress or Behavior Suggests Sexual Consent"	6.37 (.02)	5.67 (.05)	<.0001
Physical TDV acceptance	3.81 (.02)	3.40 (.03)	<.0001
Ambivalent sexism	9.89 (.03)	9.07 (.06)	<.0001
Negative attitudes toward women	14.58 (.05)	12.50 (.09)	<.0001

Note. [a]recruitment survey was conducted in school in the fall term using a paper and pencil format.

[b]completed recruitment and final surveys with no missing data.

Outcomes

As a test of H_1, adjusted mean attitudinal outcome scores and standard errors were calculated by cluster RCT assignment (refer to Table 2). No significant differences were observed for four of the five attitudinal measures at recruitment or final surveys. Only for ambivalent sexism was a difference observed consistent with our hypothesis. While scores declined from recruitment to final surveys for seniors in intervention and control condition, the size of the decline was slightly greater in the intervention (difference of −1.07), versus the control (difference of −.78) groups. This difference was significant among females ($p = .04$) and those in Cohort 2 ($p = .02$). When testing H_2, that additional *GD* training would be associated with lower attitudinal scores over time, we did not observe significant differences in the directions hypothesized. In general, scale scores were lower at final versus recruitment surveys for four of five measures yet these changes were not significant by training

Table 2. GD Bystander Training Intervention or Control Assignment[a] and Violence Acceptance and Sexism Score Measured Over Time.

	Adjusted[b] Mean Score (Standard Error)		*T*-test *P*-value	
	Intervention*n* =1,025	Control*n* = 806	All Participants	Within Cohort or Sex
SV Acceptance (RMA)				
Recruitment	10.41 (.15)	10.33 (.16)	NS	NS
Final	8.40 (.12)	8.54 (.13)	NS	NS
Difference	−2.01 (.15)	−1.79 (.16)	NS	NS
"Women's Dress/Behavior Suggests Sexual Consent"				
Recruitment	5.64 (.08)	5.78 (.08)	NS	Cohort 1 p = .04 I<C
Final	4.40 (.07)	4.49 (.07)	NS	NS
Difference	−1.24 (.08)	−1.29 (.09)	NS	NS
Physical TDV acceptance				
Recruitment	3.48 (.06)	3.51 (.07)	NS	NS
Final	3.68 (.08)	3.64 (.09)	NS	NS
Difference	.20 (.10)	.13 (.10)	NS	NS
Ambivalent sexism				
Recruitment	9.07 (.10)	9.00 (.10)	NS	NS
Final	8.00 (.10)	8.21 (.11)	.06	NS
Difference	−1.07 (.11)	−.78 (.12)	.02	Cohort 2 p = .02 I<C Females p = .04 I<C
Negative attitudes toward women				
Recruitment	12.72 (.14)	12.75 (.16)	NS	NS
Final	11.21 (.13)	11.32 (.15)	NS	NS
Difference	−1.52 (.15)	−1.43 (.16)	NS	NS

Note. [a]Based on cluster RCT assignment at the school level: *GD* intervention versus control.

[b]Adjusted for cohort (1–3), sex (male, female), sexual minority status (exclusively attracted to the opposite sex [majority], or other [sexual minority]) and received AP classes in HS (yes, no).

exposure in the direction hypothesized. Additional bystander training was not associated with greater declines in violence acceptance and sexism measures (difference between recruitment and final survey measures). Specifically, relative to those receiving no *GD* training (Group 4, Table 3), those receiving training both in and after HS (Group 1, Table 3) did not have significant reductions in violence acceptance scores nor sexism measures from recruitment to final follow-up (Table 3). The exceptions were findings contrary to our hypothesis. Although "sexual violence acceptance" scores declined from baseline to final survey, the greatest decline was among those receiving *GD* only after HS relative to those not receiving any *GD* training, and this pattern held among male and sexual majority subgroups. In contrast with our hypothesis, physical TDV acceptance score *increased* for those with *GD* training in and after HS with (Group 1) and those receiving *GD* training after HS (Group 3). This *increase* in physical TDV acceptance score over time was statistically different relative to those receiving *GD* in HS only (Group 2) and those receiving no training (Group 4). Modest differences in the effect of training on changes in physical TDV acceptance over time were noted by sexual minority versus majority status. Among sexual majority students (n = 1,549), physical TDV acceptance scores increased across all four training groups with significant *increases* among those receiving *GD* in and after (Group 1) and in HS only (Group 2) relative to those with no training (Group 4). Among sexual minority students (n = 282), increases in physical TDV acceptance scores were observed only for students receiving GD after HS (Group 3) and this differed significantly from those receiving no training (Group 4) and those receiving GD in HS (Group 2).

Discussion

The majority of relevant, prior research found that bystander training resulted in reductions in SV acceptance in the short term, defined as 1-6 months post-training (Baker et al., 2014; Banyard et al., 2007; Foubert et al., 2007; Gidycz et al., 2011; Moynihan et al., 2010; Salazar et al., 2014), and up to 12 months post-training (Cares et al., 2015; Coker et al., 2019; McMahon et al., 2014). To date, this cohort study provides the longest follow-up (2-4 years post-HS) to investigate the effect of *GD* bystander training on attitudes toward violence acceptance and sexism. We found no consistent evidence that school-level RCT assignment to *GD* training was associated with consistently lower violence acceptance or sexist attitudes score over time (H_1). These findings indicate that any changes in intervention effect on violence acceptance, previously observed at the school-level (Coker et al., 2019), were not retained in the ensuing 2-4 years (Cohorts 1-3). Similarly, and in contrast with our

Table 3. *GD* Training in and/or After HS and Attitudinal Outcomes Over Time.

	Adjusted[a] Mean Attitudinal Scores (Standard Error)				*P*-value for Adjusted T test Comparisons	
Attitudinal Outcomes	Group 1GD in and After HS (*n* = 678)	Group 2GD in HS Only (*N* = 308)	Group 3GD Only After HS (*n* = 426)	Group 4No GD Training (*N* = 419)	All	Differences in Attitudes (Recruitment to Final Survey) By Sex, Cohort, and sexual minority status
SV Acceptance (RMA)						
Recruitment	10.47 (.17)	10.20 (.23)	10.68 (.20)	10.05 (.20)	$p = .04$ E	
Final	8.57 (.14)	8.32 (.19)	8.39 (.16)	8.45 (.16)	NS	
Difference	−1.91 (.17)	−1.88 (.23)	−2.30 (.20)	−1.60 (.20)	$p = .004$ E	$p = .03$ EF (Males); $p = .01$ ADE (Cohort 1); $p = .04$ E (sexual majority)
"Women's Dress or Behavior Suggests Sexual Consent"						
Recruitment	5.72 (.09)	5.66 (.12)	5.78 (.10)	5.62 (.10)	NS	
Final	4.51 (.08)	4.30 (.10)	4.55 (.09)	4.31 (.09)	$p = .03$ BCDE	
Difference	−1.21 (.09)	−1.36 (.12)	−1.23 (.11)	−1.31 (.11)	NS	NS
Physical teen dating violence acceptance						
Recruitment	3.50 (.07)	3.41 (.09)	3.52 (.08)	3.50 (.08)	NS	
Final	3.79 (.09)	3.39 (.12)	3.91 (.11)	3.40 (.11)	$p < .0001$ BCDE	
Difference	+.29 (.11)	−.02 (.15)	+.39 (.13)	−.10 (.13)	$p = .003$ BCDE	$p = .01$ BCDE (Females); $p = .01$ CE (Cohort 2);
Sexual minority	−.12	−.68	+.50	−.32		$p = .02$ DE (sexual minority);

(*continued*)

Table 3. Continued

	Adjusted[a] Mean Attitudinal Scores (Standard Error)				*P*-value for Adjusted T test Comparisons	
Sexual majority	+.55	+.29	+.56	+.11		p = .02 CE (sexual majority)
Ambivalent sexism						
Recruitment	9.17 (.11)	8.90 (.15)	9.15 (.13)	8.83 (.13)	p = .05 CE	
Final	8.14 (.12)	7.93 (.16)	8.19 (.14)	8.03 (.14)	NS	
Difference	−1.03 (.13)	−.97 (.17)	−.96 (.15)	−.80 (.15)	NS	NS
Negative attitudes toward women						
Recruitment	12.87 (.16)	12.42 (.22)	13.14 (.19)	12.36 (.19)	p = .002 CDE	
Final	11.37 (.15)	11.03 (.21)	11.48 (.18)	11.03 (.18)	NS	
Difference	−1.50 (.17)	−1.39 (.22)	−1.66 (.20)	−1.33 (.19)	NS	p = .02 ADE (sexual minority)

Note. [a]Adjusted for cohort (1-3), sex (male, female), sexual minority status (exclusively attracted to opposite sex versus other), and taken an AP class in HS (yes, no).
A: Difference between GD training in and after HS and GD training after HS only.
B: Difference between GD training in and after HS and GD training in HS only.
C: Difference between No GD training and GD training in and after HS.
D: Difference between GD training in HS only and after HS only.
E: Difference between GD training after HS only and no GD training.
F: Difference between GD training in HS only and no GD training.

second hypothesis, additional GD training after HS did not influence violence acceptance or sexist attitudes over followup. This pattern held by cohort, sex, and sexual minority status.

The primary contributions of these analyses to the existing literature included our extended follow-up and our ability to measure additional *GD* training after HS. We found no comparable studies to measure the effect of bystander training on attitudes toward violence acceptance or sexism over time beyond 12 months follow-up. Importantly, we found no evidence that *GD* training in HS or beyond resulted in consistently lower violence acceptance or sexism scores with extended follow-up.

These findings have important implications because bystander-based programming has been recognized as promising programming to prevent SV and TDV (DeGue et al., 2014; Niolon et al., 2017). Rigorous evaluation of several bystander-based violence prevention programs indicates their efficacy to reduce violence acceptance (Kettrey et al., 2019). Evidence now exists to support the efficacy of several bystander programs to reduce social norms supporting SV and TDV (Basile et al., 2016).

The current findings contrast with prior evaluations of *GD* conducted in HS settings. When evaluating *GD* effectiveness using a cluster RCT in HSs, this intervention was associated with declines in both violence acceptance (Coker et al., 2019) and violence rates (Coker et al., 2017) over time with intervention implementation. Results based on baseline data from the current prospective cohort found significantly lower scores indicating less violence acceptance and sexism for student attending schools randomized to the GD intervention versus control condition (Coker et al., 2020).

Our findings of modest reductions in violence acceptance and sexism associated with the *GD* training only for those in (Coker et al., 2020), versus after HS (reported here), suggests that bystander-based intervention efforts may be better placed at an earlier development stage. As highlighted in the recent systematic reviews (Kettrey et al., 2019; Mujal et al., 2019), the majority of bystander interventions have been implemented and evaluated in college settings. Because early adolescence is a key developmental window for shaping gender role attitudes and behaviors, Reyes et al. (2016), targeting middle or late elementary school-aged populations may be essential to realize the promising of bystander programming (DeGue et al., 2014). The suggestion for developmentally-appropriate bystander-based training is supported by recent findings indicating efficacy of the following bystander-based programs adapted for younger populations, and found to reduce violence acceptance or violence rates: *Coaching Boys into Men* adapted for middle school male athletes (Miller et al., 2020), *Dating Matters* adapted from *Safe Dates*

for middle school students (DeGue et al., 2021; Niolon et al., 2019), and *Bringing in the Bystander*, as modified from college to HS settings (Edwards et al., 2019).

Our finding that additional *GD* bystander training did not change attitudes prospectively suggests that additional prospective research is needed to carefully evaluate the nature of bystander training in college settings. Reauthorization of the VAWA Act (2013) requires institutions of higher learning to make available bystander programming to reduce SV. This policy intervention has the potential to expand the availability of bystander programming for those attending college. However, based on key informant interviews with college administrators responsible for selecting or implementing bystander programming in response to Title IX requirements, few implemented programs include in-depth skill-building approaches to support prosocial, active bystander behaviors (Davidov et al., 2020).

A noted contribution of this prospective cohort analysis is its long-term follow-up. However, if intervention-associated changes in violence acceptance and sexism are short term only, we would miss these changes. Further, attitudes of violence acceptance and sexism may be influenced in the longer-term, by factors beyond the data collected in this evaluation. While we did have data to describe receipt of *GD* training over time, measured as in and after HS, these data can address neither the duration, source, nor fidelity of *GD* training to that provided in the original cluster RCT as directed by its developer (Coker et al., 2017). In our subgroup analyses of *GD* training received in, and after HS, we attempted to explore differential effect of training by sex, sexual minority status, and recruitment cohort as a proxy for training duration; modest differences were observed (see Coker et al., 2017 for details). Other attributes may be important to consider in future research to determine what programming works best for which students, in what settings, within peer-groups, and in settings with greater sexism or violence risk.

Both selection and information bias may have influenced the risk of null findings. Regarding the potential for selection bias, we noted that "completers" included in these analyses (n = 1,831) differed significantly from those who were recruited but did not complete follow-up surveys (n = 6,991). The latter were significantly more likely to be male, not to have taken AP classes, and had higher scores on all five attitudinal outcome measures. Thus, those included in this prospective cohort were more similar to each other than those not continuing were. This finding bodes well for the homogeneity of the cohort but those more likely to have greater violence acceptance chose not to participate in the cohort study. This self-selection resulted in limited variability violence acceptance and sexism scores. Information bias may also

explain our null results. Receipt of *GD* training was based on the school-level cluster RCT assignment. Students' receipt of training in HS was not included in this "as randomized" analysis. As a reminder, *GD* training was delivered in two phases. Phase 1, a motivational speech delivered by *GD* trained Rape Crisis Educators; and was intended for all incoming HS students within their first term (fall 2010-2012 for Cohorts 1-3, respectively). Phase 2 involved intensive engaged-bystander training to students identified as POLs; Phase 2 training began as early as fall 2011 with full implementation by fall 2012 through spring 2014. In this cohort study, seniors were recruited over 3 years, or cohorts, thus their intervention training duration and recency could differ as follows (Coker et al., 2019). Seniors from intervention schools recruited as Cohort 1 in fall 2013 could have been exposed to *GD* their entire HS career (beginning fall 2010 through spring 2014). Similarly, seniors recruited in the fall of 2014 or 2015, into Cohorts 2 and 3, respectively, could have been exposed to the intervention the first three (fall 2011 to spring 2014) or 2 years (fall 2012 to spring 2014) of HS. Given these differences in potential training exposure durations, we included cohort as a confounder and conducted parallel analyses by cohort to explore duration or recency effects (refer to Tables 2 and 3, last column). We weighed the value of exposure classification based on the more rigorous school-level cluster RCT classification against the real potential for *GD* HS training misclassification due to students' self-reported level of GD training received and opted for the former "as randomized" indicator of intervention exposure.

Strengths of this cohort study include the current prospective cohort design. Basing this cohort on a rigorous cluster RCT with significant attention to intervention fidelity across educators reduced the variability of the intervention training experienced. The additional information collected prospectively to determine additional *GD* training after HS also reduces potential exposure misclassification had this additional exposure information not been ascertained. As noted above, those completing both the recruitment and final surveys differed from those not completing follow-up surveys (Table 1), yet those included in this prospective cohort were more homogeneous may comparisons by the primary exposure less likely to be confounded by known or unknown factors.

In conclusion, while no long-term effect of bystander training on violence acceptance and sexism was observed; continued replication research is encouraged to investigate the content, dose and duration of bystander, and other training modalities, to reduce attitudes that may support violence and ultimately reduce the risk of violence. Continued violence prevention programming is important to reduce high rates of SV among college students.

However, the development, evaluation, and dissemination of evidence-based programming for middle and/or late elementary school populations can have a greater impact on reducing violence acceptance, sexism, and potential violence rates at the population-based level.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interests with respect to the authorship and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research and/or authorship of this article: This study was supported by the National Institutes of Health (R01HD075783).

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