

Safe Sisters: A Sorority-Based Bystander Intervention Program to Prevent Sexual Assault

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This study examined the effectiveness of Safe Sisters, a sexual assault bystander intervention program that targets members of college sororities. Analysis of covariance results show significant differences between treatment and waitlist control groups on posttest scores for action, bystander efficacy, intent to help friends, and intent to help strangers. Significant differences are not shown between treatment and waitlist control groups on posttest scores for precontemplation, contemplation, and rape myth acceptance. Limitations and implications are discussed.

Keywords: bystander intervention, sexual assault, sororities, Safe Sisters, bystander education

Evidence-based practices are needed to help college counselors alongside other university personnel to address campus-based sexual assault. One in five women will be sexually assaulted during her college years (Black et al., 2011; Kilpatrick et al., 2007; White House Council on Women and Girls, 2014). College men may also be survivors of sexual assault, but at a lower rate of approximately one in 16 men (National Sexual Violence Resource Center, 2015). Each year, over 300,000 college women experience rape, which equates to about 5% of the female college population in the United States (Kilpatrick et al., 2007). The prevalence of sexual assault on college campuses is a critical issue that must be addressed.

Although the legal definition may vary from state to state, *sexual assault* is generally defined as sexual behavior or contact that occurs without an individual's explicit consent (Rape, Abuse & Incest National Network, n.d.). The statistics regarding campus sexual assault may be lower than the actual number of assaults because college students do not always report incidents of sexual assault (Alderden & Ullman, 2012; Kilpatrick et al., 2007). Approximately 11% of college women who are survivors of sexual assault report sexual assaults to the police, whereas 15.8% of sexual assault survivors in the general population file police reports following a sexual assault (Kilpatrick et al., 2007). Over 81% of female sexual assault survivors experienced significant effects on their mental health and well-being (Black et al., 2011; National Sexual Violence Resource Center, 2015), including depression (Au et al., 2013) and posttraumatic stress disorder (Sharkansky, 2015). Unfortunately, many survivors of sexual assault do not receive the mental health care they need (Campbell, 2001). College counselors could help to address the per-

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sistent problem of campus-based sexual assault by providing evidence-based preventive outreach programs.

Sorority Women, Fraternity Men, and Sexual Assault

It may be advantageous for college counselors and other university personnel to begin by identifying at-risk groups prior to selecting and implementing programs to prevent sexual assault on their campuses. Female sorority members face a substantially greater risk of sexual assault during their college years (Minow & Einolf, 2009). In a recent study, 33% of sorority women in the sample were survivors of rape, and an additional 14% reported attempted rape (Minow & Einolf, 2009). Several other researchers have concluded that women who are in sororities are more likely to be sexually assaulted than college women who do not have affiliations with sororities (Brener et al., 1999; Mellins et al., 2017; Mohler-Kuo et al., 2004). Researchers have suggested that sorority women are at greater risk of sexual assault due to their increased participation in binge drinking, their greater likelihood of having experienced traditional gender role socialization, their tendency to pair with men in fraternities, and their decreased likelihood of noticing warning signs of sexual assault when interacting with fraternity men (Minow & Einolf, 2009).

In addition, several studies have concluded that men in fraternities are more likely to be perpetrators of sexual assaults against women when compared with other groups of men (Foubert et al., 2007), and fraternity houses are the location of approximately 10% of the rapes that occur on or around college campuses (Sampson, 2003). College men who consume alcohol at least twice per week and have friends who endorse mistreatment of women (e.g., emotional or physical abuse) are 9 times more likely to commit sexual assault than are men who do not have these characteristics (DeKeseredy & Schwartz, 2014). Jozkowski and Wiersma-Mosley (2017) described the phenomenon of a “male-dominated party culture” (p. 94) that promotes and perpetuates sexual assault within Greek fraternal organizations. Thus, the social norms of fraternal organizations may be the mediating variable between sorority women, fraternity men, and sexual assault (Minow & Einolf, 2009). College counselors can use sexual assault prevention programs that target the social norms of sorority and fraternity culture when attempting to reduce the prevalence of sexual assault among these groups.

The Bystander Intervention Approach and Sorority Women

Although several different approaches to campus-based sexual assault prevention have been implemented during the past few decades (Anderson & Whiston, 2005), the bystander intervention approach is a method that has received recent attention (Banyard et al., 2014; Bennett et al., 2013; Brown et al., 2014; McMahan et al., 2013). A bystander approach to sexual assault prevention targets individuals who are neither the victim nor the perpetrator, focusing on those who are present as witnesses at the time of a potential assault (Burn, 2009).

Bystander education, or *bystander intervention training*, is a method that interventionists such as college counselors have used in efforts to prevent sexual assault on college campuses (Banyard et al., 2014; Bennett et al., 2013; Brown et al., 2014; McMahon et al., 2013). Effective bystander intervention trainings target both psychological processes and social norms, increasing the likelihood that bystanders will feel compelled to take action to prevent sexual assault (Burn, 2009). Moynihan et al. (2011) described the typical components of bystander intervention trainings on college campuses that focus on sexual assault prevention. During these trainings, individuals receive education about the prevalence of sexual assault and are asked to help prevent sexual assault on campus. Participants then receive training, which may include scenarios and role plays, that helps them to develop the skills needed to intervene. Moynihan et al. (2011) stressed the importance of discussing ways to intervene safely so that the benefits outweigh the risks. Bystander intervention trainings may also include information about how to help a person who has already been a victim of sexual assault, including ways to provide emotional support and resources (Lonsway et al., 2009; Moynihan et al., 2011).

Bystander intervention training programs aimed at preventing sexual assault have grown on college campuses during the past 2 decades (Katz & Moore, 2013). By 2012, there were 12 outcome research studies investigating the impact of bystander intervention programs. Katz and Moore (2013) conducted an initial meta-analysis of bystander education training for the prevention of sexual assault and found moderate effect sizes for intent to help others ($d = 0.58$) and bystander efficacy ($d = 0.49$) and smaller effect sizes for bystander behavior ($d = 0.23$). In contrast, an earlier meta-analysis by Anderson and Whiston (2005) found that general interventions to prevent sexual assault on college campuses appeared to have a minimal effect on behavioral intentions ($d = 0.14$), suggesting that bystander interventions may be more likely to promote behavior change and intention to help than other forms of sexual assault prevention. More research is needed to help college counselors and other university personnel to make informed decisions regarding bystander intervention programs to prevent sexual assault.

At the time of this research, only one published study had focused specifically on sorority women's responses to a bystander intervention training program. Moynihan et al. (2011) evaluated the 90-minute version of the Bringing in the Bystander program with members of sororities. The Bringing in the Bystander program provides education regarding sexual assault and training in bystander intervention methods. Moynihan et al. (2011) found significant differences between the experimental and control groups, with the experimental group showing significantly higher scores on posttests measuring bystander efficacy, intention to help, and responsibility. Moynihan et al. (2011) concluded that the bystander training approach to sexual assault prevention is effective in changing attitudes of women in sororities and may increase the likelihood that they will intervene to prevent sexual assault.

Moynihan et al. (2011) contributed to the literature by investigating the impact of a bystander intervention program to prevent sexual assault with

college women who were sorority members. However, the study had several limitations, including the relatively small sample size and use of some measures that had not been psychometrically evaluated. In addition, Moynihan et al. (2011) used a relatively brief treatment model, and longer treatments have been found to be more effective (Anderson & Whiston, 2005). Furthermore, results may have been more significant if the intervention had been created specifically for sorority women and had targeted the sociocultural factors that put them at risk. The current study attempts to address these limitations and to present a sexual assault prevention program that was intentionally created for use by college counselors to train sorority women in bystander intervention.

Safe Sisters: A Bystander Intervention Program to Prevent Sexual Assault

Safe Sisters is a sexual assault prevention program that provides bystander intervention training specifically for sorority women. The program is a 4-hour training session that leads to membership in a group called Safe Sisters. Sorority members who participate in Safe Sisters are trained to recognize the warning signs that a sexual assault may occur and learn ways to intervene safely. The Safe Sisters program was created by the Sexual Assault Crisis Service counselors and an intern, and the training was provided by Sexual Assault Crisis Service counselors with assistance from graduate-level trainees. The current study evaluated the outcomes of the 4-hour training component of the Safe Sisters program using a randomized controlled trial design.

The purpose of the current study was to ascertain the effectiveness of Safe Sisters. Researchers selected indicators of effectiveness from the literature regarding bystander intervention programs (Banyard et al., 2014; Cares et al., 2015). We hypothesized that experimental group members would have lower scores for precontemplation, higher scores for contemplation, higher scores for action, higher scores for bystander efficacy, less endorsement of rape myths, and higher scores for intent to help friends and intent to help strangers compared with the waitlist control group. The following questions were addressed in this study:

1. Do individuals who participate in Safe Sisters report statistically significantly lower scores for precontemplation compared with individuals who have not participated in Safe Sisters?
2. Do individuals who participate in Safe Sisters report statistically significantly higher scores for contemplation compared with individuals who have not participated in Safe Sisters?
3. Do individuals who participate in Safe Sisters report statistically significantly higher scores for action compared with individuals who have not participated in Safe Sisters?
4. Do individuals who participate in Safe Sisters report statistically significantly higher scores in their bystander efficacy to prevent sexual assault compared with individuals who have not participated in Safe Sisters?

5. Do individuals who participate in Safe Sisters report statistically significantly less endorsement of rape myths compared with individuals who have not participated in Safe Sisters?
6. Do individuals who participate in Safe Sisters report statistically significantly higher scores in their intent to help friends in situations related to sexual assault compared with individuals who have not participated in Safe Sisters?
7. Do individuals who participate in Safe Sisters report statistically significantly higher scores in their intent to help strangers in situations related to sexual assault compared with individuals who have not participated in Safe Sisters?

Method

Participants

Participants were individuals who identified as female members of Greek PanHellenic sororities at a large midwestern university in the United States. A total of 164 cisgender women attended the program, and among those who attended, 139 women chose to participate in the study. Three participants (2.2%) identified as Asian, Asian American, or Pacific Islander; three (2.2%) identified as Hispanic or Latina; one (0.7%) identified as Native American or American Indian; six (4.3%) identified as multiracial; 125 (89.9%) identified as White, Caucasian, or European; and one (0.7%) identified as other. For sexual orientation, one participant (0.7%) identified as bisexual, and 138 participants (99.3%) identified as heterosexual. Four participants (2.9%) identified as differently abled, 134 participants (96.4%) identified as able-bodied, and one participant (0.7%) did not specify. Participants' ages ranged from 18 to 22 years, and the mean age was 19.50 ($SD = 0.90$). Twenty-seven women were 1st-year students, 74 women were 2nd-year students, 32 women were 3rd-year students, and five women were 4th-year students. One participant did not identify her age or year in school. All participants identified as PanHellenic/Greek sorority members, with 103 women identifying as active members and 36 women identifying as new members or pledges.

Measures

First, we used a demographic questionnaire to gather the sample's descriptive data. Participants indicated their current age and major(s) in response to open-ended questions. Ethnicity, sexual orientation, disability status, year in school, and sorority membership status were assessed using multiple-choice questions.

Second, we used a measure called Readiness to Help (Banyard et al., 2014) to assess participants' attitudes toward engagement in behaviors that may prevent sexual assault and intimate partner violence. Banyard et al. (2010) suggested applying Prochaska and DiClemente's (1994) transtheoretical model as a way to assess program participants' readiness to change and, ultimately, take action to prevent sexual assault. Banyard et al. (2014) developed and subsequently refined

the Readiness to Help subscales using factor analysis. The current study used the three subscales of Readiness to Help: Precontemplation (12 items), Contemplation (nine items), and Action (12 items). The Precontemplation subscale was created to align with Prochaska and DiClemente's (1994) first stage of change and was intended to capture attitudes that minimize or deny problematic behaviors associated with sexual assault (Banyard et al., 2014). The Contemplation subscale was aligned with the second stage of change and was intended to capture a sense of responsibility associated with the problem of sexual assault. The Action subscale was aligned with the third stage of change and was intended to capture participants' propensities toward taking action to prevent sexual assault. Participants rated items on a scale from 1 (*strongly disagree/not at all true*) to 5 (*strongly agree/very much true*). For example, participants were asked to rate on the 5-point scale their level of agreement with the Precontemplation item, "I don't think sexual abuse is a problem on this campus." Subscales were scored separately because each measured different stages of change. Potential scores for Precontemplation ranged from 12 to 60, scores for Contemplation ranged from nine to 45, and scores for Action ranged from 12 to 60. Whereas higher scores for Contemplation and Action at time of posttest indicated propensity toward change, lower scores for Precontemplation at posttest indicated increased propensity toward change. In the current study, the Cronbach's alpha for Precontemplation scores was .78, for Contemplation scores was .87, and for Action scores was .90.

The third measure was the Bystander Efficacy Scale (Banyard, 2008), which was developed to measure individuals' perceptions of their ability to help others in situations related to sexual assault and/or interpersonal violence. Respondents were provided with 18 statements related to bystander intervention and were asked to indicate their degree of confidence in their ability to perform each behavior on a scale from 0% (*can't do*) to 100% (*very certain*). For example, Item 6 asked participants to assign a percentage to "Able to ask a stranger who looks very upset at a party if they are OK or need help." The total score for each respondent was the mean of all 18 items, and mean scores could potentially range from 0% to 100%. Scores were converted from percentages to decimals for the purpose of statistical analysis. In previous studies, Bystander Efficacy Scale scores correlated significantly with scores on other measures of bystander efficacy, including the efficacy scale used for the Mentors in Violence Prevention program (Banyard, 2008; Cissner, 2009). In the current study, the Cronbach's alpha for Bystander Efficacy Scale scores was .90.

Fourth, the Illinois Rape Myth Acceptance Scale (IRMA; McMahon & Farmer, 2011) was used to measure participants' endorsement of sexual assault-related myths. Participants rated 22 items on a scale ranging from 1 (*strongly agree*) to 5 (*strongly disagree*). For example, participants rated their level of agreement with the statement "If both people are drunk, it can't be rape" (Item 12) using the 5-point scale. Potential scores for the IRMA ranged from 22 to 110. A low score indicated high endorsement of rape myth, and a high score indicated low endorsement of rape myth. A multivariate analysis of covariance provided support for the criterion validity of the IRMA (McMahon & Farmer, 2011). In the current study, the Cronbach's alpha for IRMA scores was .85.

Fifth, we used the brief versions of the Intent to Help Scales (Banyard et al., 2014), which assess the self-reported likelihood that an individual will participate in specific behaviors to prevent sexual assault and intimate partner violence. This measure had 18 questions, and respondents were asked to rate the likelihood that they would engage in each behavior on a scale of 1 (*not at all likely*) to 5 (*extremely likely*). The measure included both Intent to Help Friends (consisting of the first 10 items) and Intent to Help Strangers (consisting of the last eight items) subscales, because past research has indicated that the relationship between a bystander and a potential victim influences intention to help (Banyard et al., 2014; Bennett et al., 2013). For example, Item 1 on the Intent to Help Friends subscale is “I approach someone I know if I thought they were in an abusive relationship and let them know I’m here to help.” The corresponding question on the Intent to Help Strangers subscale states, “I approach someone I don’t know if I thought they were in an abusive relationship and let them know that I’m here to help.” Potential scores for Intent to Help Friends range from 10 to 50, and potential scores for Intent to Help Strangers range from 8 to 40. Banyard et al. (2014) created the brief subscales on the basis of a factor analysis of the original 79-item Intent to Help Scale (Banyard, 2008). The new subscales correlated significantly with other measures of bystander attitudes and behaviors (e.g., Bystander Behaviors Directed at Friends and Strangers). In the current study, the Cronbach’s alpha for Intent to Help Friends scores was .90 and for Intent to Help Strangers scores was .93.

Procedure

Sorority members were notified of the Safe Sisters training through the Pan-Hellenic representatives in their chapters via email and during closed chapter meetings. Representatives made announcements directly to their membership, emphasizing that attending the program was completely voluntary. Individuals who arrived to attend the Safe Sisters training were asked if they would like to voluntarily participate in the research study. The request for participation was delivered by the principal investigator, who read a script describing the research study and then distributed the Study Information Sheet. After the informed consent process, we randomly assigned participants to Group A (the first group to be trained, $n = 77$) or Group B (the second group to be trained, $n = 62$) using note cards. Measures were distributed in paper format, and participants were asked to write their group assignment on the packet of measures. Individuals who chose not to participate in the study were allowed to participate in the Safe Sisters training but did not complete the measures.

Each participant created a distinct participant code to deidentify their data. Participants in the treatment group (Group A) and in the waitlist control group (Group B) were administered a demographic form, the Readiness to Help Scale, the Bystander Efficacy Scale, the IRMA, and the brief versions of the Intent to Help Scales. After completion of the pretest measures, Group B was asked to leave and to return in 4 hours after Group A participated in the 4-hour Safe Sisters training. Following the training, both Group A and Group B were administered the Readiness to Help Scale, the Bystander Ef-

ficacy Scale, the IRMA, and the brief versions of the Intent to Help Scales. Following completion of posttest materials, the study ended, Group A left the training site, and Group B participated in the Safe Sisters training. The sample size for Group A ($n = 77$) was larger than the sample size for Group B ($n = 62$) because 15 women who were assigned to Group B subsequently opted not to participate in the study or dropped out of the study following completion of the first set of measures. An a priori power analysis determined that the initial sample size ($N = 139$) would result in sufficient power (.997) to conduct an analysis of covariance (ANCOVA).

Identical trainings were offered approximately 4 months apart during the fall and spring semesters. The Safe Sisters facilitators, training materials, and training methods were consistently used during fall and spring. The only difference in methods was that the fall training group was not administered the brief versions of the Intent to Help Scales, which were added during the spring training. Consequently, analyses for the brief versions of the Intent to Help Scales had smaller sample sizes ($N = 52$).

Results

An ANCOVA is used to compare two or more groups when there are one or more covariates, a dependent variable, and an independent variable (Tabachnick & Fidell, 2013). Seven ANCOVA tests were run separately to test the seven hypotheses in this study. The covariates were pretest scores for each measure; this model allowed for assessment of differences in the posttest means between treatment and control groups after accounting for pretest scores. In other words, the ANCOVA helped to control for variation in posttest means that resulted from initial differences between groups. Random assignment is designed to reduce error variance between the two groups, but the conservative approach of controlling for the pretest as a covariate helped to reduce error variance and the potential for systemic bias. Following the ANCOVA analyses, a Bonferroni correction was calculated to control for Type I error (Tabachnick & Fidell, 2013). The adjusted alpha was found to be .007. Means and standard deviations are reported in Table 1.

Three analyses were conducted using scores on subscales of the Readiness to Help measure. The first ANCOVA analysis explored the difference between treatment and control group scores for Precontemplation after controlling for pretest scores. When tests of normality were conducted, both treatment and control groups were right skewed. The results met assumptions for homogeneity of variance (i.e., nonsignificant Levene test) and homogeneity of regression slopes, $F(1, 131) = 0.23, p = .631$. For Precontemplation, there was a nonsignificant difference at the posttest, $F(1, 131) = 5.75, p = .018$. The effect size was $d = 0.50$. A second ANCOVA analysis was conducted to explore the difference between the treatment and control group scores for Contemplation after controlling for pretest scores. Concerning assumptions, both treatment and control groups were right skewed when tests of normality were conducted. The results met assumptions for homogeneity of variance and homogeneity of

TABLE 1
Pretest and Posttest Means for Treatment and Control Groups

Measure	Pretest				Posttest			
	Treatment		Control		Treatment		Control	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Precontemplation	18.35	4.17	19.36	6.05	15.41	5.53	18.19	5.56
Contemplation	40.07	4.94	40.10	5.09	41.26	4.21	40.13	4.16
Action	28.19	11.89	28.14	12.34	52.04	8.97	29.98	13.45
Bystander Efficacy	0.84	0.12	0.85	0.12	0.93	0.11	0.87	0.10
IRMA	77.62	7.09	75.83	8.32	93.96	9.09	91.81	10.25
Intent to Help Friends	44.33	5.14	46.55	4.18	47.70	3.39	46.09	3.96
Intent to Help Strangers	28.90	7.35	32.58	5.95	35.53	4.71	33.45	6.17

Note. IRMA = Illinois Rape Myth Acceptance Scale.

regression slopes, $F(1, 131) = 2.39, p = .125$. For Contemplation, there was a nonsignificant difference at posttest, $F(1, 131) = 2.88, p = .092$. The effect size was $d = 0.27$. A third ANCOVA analysis was conducted to explore the difference between treatment and control group scores for Action after controlling for pretest scores. The control group was right skewed, and the treatment group was flat when tests of normality were conducted. The assumption of homogeneity of regression slopes was not met, $F(1, 131) = 36.86, p < .001$. On the Action subscale, there was a statistically significant ANCOVA, $F(1, 131) = 193.123, p < .001$. The effect size was $d = 1.98$.

A fourth ANCOVA analysis was conducted to explore the difference between treatment and control group scores on the Bystander Efficacy Scale after controlling for pretest scores. In terms of assumptions, the treatment group was slightly skewed, and the control group was similarly skewed when tests of normality were conducted. The results met assumptions for homogeneity of variance and homogeneity of regression slopes, $F(1, 131) = 0.02, p = .889$. On the Bystander Efficacy Scale, there was a statistically significant ANCOVA, $F(1, 131) = 22.41, p < .001$. The effect size was $d = 0.51$.

The fifth ANCOVA analysis explored the difference between treatment and control group scores on the IRMA after controlling for pretest scores. Both treatment and control groups were similarly slightly skewed when tests of normality were conducted. The results met assumptions for homogeneity of variance and homogeneity of regression slopes, $F(1, 131) = 0.11, p = .743$. On the IRMA, there was a nonsignificant difference at posttest, $F(1, 131) = 0.001, p = .973$. The effect size was $d = 0.22$.

The last two analyses were conducted using scores on subscales of the brief versions of the Intent to Help Scales. The sixth ANCOVA analysis was conducted to explore the difference between treatment and control group scores for Intent to Help Friends after controlling for pretest scores. When tests of normality were conducted, the treatment group was slightly right skewed, and the control

group was also right skewed. The results met assumptions for homogeneity of variance. The assumption of homogeneity of regression slopes was not met, $F(1, 50) = 38.17, p < .001$. On the Intent to Help Friends subscale, there was a statistically significant ANCOVA, $F(1, 50) = 11.79, p < .001$. The effect size was $d = 0.44$. The seventh ANCOVA analysis explored the difference between treatment and control group scores for Intent to Help Strangers after controlling for pretest scores. When tests of normality were conducted, the treatment group was right skewed, and the control group was also right skewed. The results met assumptions for homogeneity of variance. The assumption of homogeneity of regression slopes was not met, $F(1, 50) = 26.36, p < .001$. On the Intent to Help Strangers subscale, there was a statistically significant ANCOVA, $F(1, 50) = 8.89, p = .004$. The effect size was $d = 0.38$.

Discussion

We conducted this study to ascertain the impact of the Safe Sisters program on several dependent variables that are relevant to sexual assault prevention. Significant differences between treatment and waitlist control groups were shown on posttest scores for action, bystander efficacy, intent to help friends, and intent to help strangers. However, ANCOVA results for action, intent to help friends, and intent to help strangers should be interpreted with caution due to violations of assumptions. Significant differences were not shown between treatment and waitlist control groups on posttest scores for precontemplation, contemplation, and rape myth acceptance.

Implications for College Counselors

As the problem of sexual assault on college campuses persists, college counselors can play an important role in leading prevention programs by selecting and using evidence-based practices. Safe Sisters represents a unique contribution to sexual assault prevention efforts because it specifically targets college women who are in sororities and uses a bystander intervention approach. ANCOVA results suggest that the women who participated in the Safe Sisters training are more likely than nonparticipants to intervene to prevent sexual assault, presenting outcomes that are comparable with other bystander intervention programs used on college campuses (e.g., Cares et al., 2015; Moynihan et al., 2010, 2011). Safe Sisters participants endorsed higher levels of readiness to take action to prevent sexual assault, higher levels of bystander efficacy, and increased intent to help friends and strangers compared with the control group. Therefore, college counselors who want to target sorority women as an at-risk group might want to consider Safe Sisters as an evidence-based practice.

The evaluation of Safe Sisters also provides several implications regarding ways in which college counselors could improve implementation of the program. Scores for Intent to Help Friends and Intent to Help Strangers were higher for the treatment group, but the small-to-moderate effect sizes may be increased by making minor adjustments to the content of Safe Sisters. Specifically, Safe Sisters trainers could engage participants in lengthier conversations

regarding the barriers to helping others, including any reluctance they may have to stepping in when the potential victim of a sexual assault is a stranger. Reluctance to help strangers has been discussed in previous studies (Banyard et al., 2014; Bennett et al., 2013) and also was evidenced by differences in effect sizes for posttest scores for Intent to Help Strangers versus Intent to Help Friends. Currently, the program includes discussions of how helping “sisters” includes not just members of their own sororities, but also members of other sorority houses. Discussions could also emphasize how participants are equipped to intervene when the situation involves other potential victims of sexual assault, not just sorority women.

Another recommended area of focused intervention is dispelling rape myth. A portion of the Safe Sisters program targets rape myths in the hope of changing attitudes that blame survivors and excuse perpetrators of sexual assault. Several of the items on the IRMA (McMahon, 2010) had a floor effect because most treatment and control group members strongly disagreed with the rape myth items at the time of pretest. Few participants endorsed rape myths in the category called “it wasn’t really rape,” a set of items insinuating that lack of physical threat or failure to say “no” means that a sexual assault did not occur. However, in this study, more participants endorsed strong agreement ($n = 7$) or agreement ($n = 37$) with the statement, “If a guy is drunk, he may rape someone unintentionally,” than with any other item at the time of posttest. College counselors and other university personnel who implement Safe Sisters are encouraged to address these more subtle rape myths, including those that relate to substance use.

Limitations and Future Research

The results of the initial study of Safe Sisters were weakened by several violations of the assumptions of ANCOVA. The effect size for Action was quite robust ($d = 1.98$), but the assumption of homogeneity of regression slopes was not met; thus, this result should be interpreted with caution. Although a violation of the assumption of regression slopes can potentially increase the possibility of a Type I error, it is unlikely that the significant result was an artifact of violating this assumption. Results for the brief versions of the Intent to Help Scales should also be interpreted with caution because the assumptions of normality and homogeneity of regression slopes were not met, which may perhaps be due to the smaller number of participants who completed this measure.

Several additional limitations of the Safe Sisters study should be acknowledged, and researchers are encouraged to address these in future research. The study was conducted using a randomized controlled trial design in a real-world setting. Due to the real-world element of the research, we experienced limited control over procedures during the Safe Sisters training. For example, participants assigned to the waitlist control group were asked to return to the training site to complete the posttest and participate in the subsequent training. However, some participants did not return, which affected the size of the waitlist control group. Participants may also have been subject to the Hawthorne effect, because they were asked to participate in the study and

this knowledge may have biased their responses. In addition, the human element of real-world research means that mistakes can be made. During the fall training, an administrative assistant inadvertently left out the brief versions of the Intent to Help Scales from the packet of measures; thus, more than half of the study's participants did not complete this measure. The smaller sample size for the brief versions of the Intent to Help Scales may have influenced the results. For example, it is possible that the assumptions of normality and homogeneity of regression slopes were not met due to small sample size. In addition, the a priori power analysis was conducted before the Bonferroni correction. With an adjusted alpha of .007, an adequate sample size would have been 205 or more participants. Violations of assumptions and the small sample size weaken the results and could be addressed if the Safe Sisters evaluation is replicated in the future.

Another limitation is the lack of diversity within the sample. The majority of participants identified as heterosexual (99.3%), able-bodied (96.4%) and White (90%). Lack of diversity within the sample may affect generalizability of the results (Thomas & Hersen, 2011). The National PanHellenic Conference and North American Interfraternity Conference do not publish demographic data regarding their membership, but according to Chang (2014), these organizations do tend to be culturally homogeneous and lack diversity, primarily attracting White cisgender female college students. The lack of diversity in this study could be remedied by intentionally recruiting participants from more diverse organizations, such as members of historically Black Greek-letter organizations. Expanding the training to include marginalized groups of women certainly would fit the Safe Sisters program's mission of preventing sexual assault on college campuses and would also expand generalizability of the results.

In addition, the outcomes of this initial study of Safe Sisters need to be substantiated by future research. As the program expands to other campuses, the program evaluation of Safe Sisters could be replicated in the hope that future research will support the findings of this initial study. Future researchers could also compare Safe Sisters with other bystander intervention programs that are not specifically intended for sorority members. The inclusion of content that is specific to women in sororities may have influenced participants' intent to help, but so could a number of other factors. Furthermore, follow-up studies with the original participants could examine the longitudinal impact of the program. For example, surveys could be redistributed 3, 6, or 12 months after program completion to evaluate the long-term impact of the program. Additional research is needed both to substantiate the outcomes of the current study and to better understand the mechanisms of change.

Conclusion

The current study represents an initial effort to provide outcome data related to Safe Sisters, a bystander intervention program for sorority women to prevent sexual assault. ANCOVA results showed significantly higher treatment

group scores for bystander efficacy, readiness to take action, intent to help friends, and intent to help strangers. Statistically significant differences were not shown on two subscales for Readiness to Help (Precontemplation and Contemplation) and for rape myth acceptance. Changes to program content, including focusing more on specific subtle rape myths, may help to improve the outcomes for each of these areas. Additional research is needed to further substantiate the effectiveness of the Safe Sisters program. Nonetheless, the Safe Sisters program has shown promising results as a bystander intervention method that targets women in sororities on a college campus.

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