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Speak Up! Prosocial Intervention Verbalizations Predict Successful Bystander Intervention for a Laboratory Analogue of Sexual Aggression

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Abstract

This study evaluated a mechanism by which men's self-efficacy to intervene increases their likelihood of preventing a laboratory analogue of sexual aggression (SA) via specific verbalizations and whether alcohol inhibits this mechanism. A sample of 78 male peer dyads were randomly assigned to consume an alcoholic or nonalcoholic beverage and complete a laboratory paradigm to assess bystander intervention to prevent SA toward a female who had ostensibly consumed an alcoholic or nonalcoholic beverage. Participants' verbalizations during the task were subjected to quantitative analysis. Regardless of alcohol use, bystander self-efficacy increased the likelihood of successful bystander intervention via participants' use of more prosocial verbalizations. Findings highlight prosocial verbalizations within the male peer context that may effectively prevent SA.

Keywords

sexual violence prevention; alcohol; social influence; peer norms

Sexual aggression (SA) is a major public problem with severe mental and physical health consequences for survivors (Black et al., 2011). Unfortunately, the clear need for evidence-based interventions has not been realized. DeGue and colleagues (2014) identified only three evidence-based primary prevention programs for SA (Safe Dates: Foshee et al., 2004; the community-level portion of Shifting Boundaries: Taylor, Stein, Woods, Mumford, & Forum, 2011; and funding associated with the 1994 U.S. Violence Against Women Act: Boba & Lilley, 2009), two of which target teen dating violence and none of which were designed to target SA specifically. Because of this limited evidence base, the field has witnessed a paradigm shift that focuses on the potential impact of *bystanders* in preventing SA (e.g.,

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Authors' Note

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Banyard, Moynihan, & Plante, 2007). In essence, the bystander approach to SA prevention aims to prepare individuals to intervene when they witness SA. This approach is founded on an extensive social psychological literature (Fischer et al., 2011), and DeGue and colleagues (2014) concluded that bystander intervention programs have “substantial potential” for reducing SA (DeGue et al., 2014). Indeed, subsequent evidence indicates that web-based (Salazar, Vivolo-Kantor, Hardin, & Berkowitz, 2014) and campus-based (Coker et al., 2015) bystander intervention programs may promote pro-bystander attitudes and reduce rates of SA. A common point of intervention for bystander programs is to increase bystander self-efficacy, which represents one’s confidence that he or she can successfully intervene in a SA situation. However, the mechanism(s) by which bystander self-efficacy prevents SA has not been directly tested. To address this gap, the present study examined prosocial bystander verbalizations as a mediator of the relation between bystander self-efficacy and likelihood of preventing SA.

Who Is Likely to Intervene and What Is It That They Do?

Bystanders must make a series of decisions, with only one particular set of decisions leading to intervention (Latané & Darley, 1970). Specifically, successful intervention requires that the bystander (a) notices the event, (b) interprets it as an emergency, (c) develops a feeling of personal emergency, (d) decides how to help, and (e) chooses to act. Bystanders may be ineffective at helping due to barriers that are present at each of these steps (for reviews, see Banyard, 2015; Burn, 2009). One potential barrier at the fourth and fifth steps of intervention is bystander efficacy, or the confidence that one can perform various bystander behaviors (e.g., Banyard, 2008; Latané & Darley, 1970). In essence, a bystander who believes it is important to intervene, but does not feel they know how (i.e., low bystander efficacy), will likely not act. In support of this view, survey and laboratory-based research demonstrates that men higher in bystander efficacy are more likely to intervene in SA situations (Banyard, 2008; McMahon et al., 2015) and successfully prevent SA (Parrott et al., 2012). Consistent with these data, a critical aim of bystander intervention programs is to increase bystanders’ likelihood of intervening by strengthening bystander efficacy (Banyard, 2015).

Pertinent research on third party intervention for aggression suggests that intervention behaviors tend to be either (a) aggressive and/or escalate the high risk situation, or (b) nonaggressive/conciliatory (e.g., Levine, Taylor, & Best, 2011; Parks, Osgood, Felson, Wells, & Graham, 2013). Not surprisingly, bystander intervention programs aim to increase prosocial intervention behaviors, that is, behaviors which are nonaggressive, conciliatory, and/or function to de-escalate the high risk situation. However, in service of this aim, these programs, including those currently being developed and marketed to colleges and universities, can vary greatly in how they seek to promote self-efficacy. For instance, existing bystander intervention programs aim to promote prosocial bystander behavior that (a) interrupts a SA situation or a situation that may lead to SA, or (b) challenges social norms that support SA (e.g., Mentors in Violence Prevention: Katz, 2010; Bringing in the Bystander: Moynihan et al., 2015; Green Dot: Coker et al., 2011). In addition, existing programs draw upon a heterogeneous collection of techniques including social marketing campaigns, structured psychoeducational programs, and bystander skill building

interventions that aim to empower men and women to intervene in high risk situations to prevent SA. Finally, bystander intervention programs emphasize use of these strategies at different points on the continuum of SA.

Despite these differences in approach and focus, a key component of most bystander programs is promoting self-efficacy by offering participants potential strategies to prevent SA that meet the individual's comfort and safety considerations. Although these programs typically emphasize prosocial verbal strategies that interrupt, distract, or otherwise de-escalate the high risk situation (e.g., Banyard, 2015), the specific prosocial behavioral strategies or tactics that bystanders employ which effectively prevent SA have yet to be codified in the extant literature. And in the wake of the United States' Campus Sexual Violence Elimination Act (2013) that requires colleges to implement bystander training, myriad bystander programs have been developed and marketed to colleges and universities with minimal evidence of outcome effectiveness or empirical support for the prosocial intervention strategies that are disseminated (S. DeGue, personal communication, March, 2016). This is a significant lacuna in the evidence base that threatens the continued development, evaluation, and dissemination of bystander intervention programs. Understandably, identification and assessment of effective bystander behaviors is challenging because these strategies likely vary by situational and interpersonal context (e.g., direct interaction with a potential perpetrator vs. other bystanders).

The present study sought to address these challenges by evaluating a mechanism by which men's self-efficacy to intervene may increase their likelihood of preventing SA via observable prosocial verbalizations. A key innovation in this work was to directly assess the types of verbalizations that men naturally use within a peer context and evaluate whether these verbalizations successfully prevented a laboratory analogue of SA.

Measuring Bystander Intervention for SA: Use of Laboratory Analogues

A recent meta-analysis suggests bystander intervention programs have a stronger impact on attitudes and behavioral intentions than actual bystander behavior (Katz & Moore, 2013); in fact, this review found only 25% of studies in this literature assess actual bystander behavior. Moreover, even fewer studies assess the extent to which behavior actually prevents SA or diffuses a situation perceived to be at risk for SA (e.g., Langhinrichsen-Rohling, Foubert, Brasfield, Hill, & Shelley-Tremblay, 2011). Finally, bystander behavior and its effect on SA prevention are almost exclusively assessed via retrospective self-report, which introduces well-established biases (Schwarz, 2007). For instance, it can be challenging for an individual to decide whether a situation denotes SA (e.g., attempted rape) or is at risk for escalating to SA (e.g., a man pressuring an intoxicated woman to leave a party with him). This ambiguity inhibits people from attending to and accurately labeling SA, which would prevent them from reporting it on a self-report measure.

In contrast to this literature, classic bystander studies (e.g., Latané & Darley, 1970) used observational, laboratory-based methods to provide an unequivocal assessment of intervention behavior. These studies afforded experimental control over situational factors hypothesized to predict intervention behavior. Thus, the laboratory context addresses the

aforementioned challenges in that it provides an ideal setting to observe *in vivo* whether participants identify the situation as dangerous, attempt to intervene, and successfully prevent SA. Moreover, the laboratory is perhaps the only ethical setting for direct tests of causal mechanisms without putting a victim at risk for harm. In addition, it is more feasible to carry out such work in the laboratory than in naturalistic settings, as evidenced by only one field experimental design that assessed bystander intervention in response to a simulated male-to-female stranger rape (Harari, Harari, & White, 1985). With few exceptions, such methods have not been employed to study bystander intervention for SA.

Simulation of a SA situation in the laboratory has been achieved by (a) staging an event that can presumably lead to SA (Jouriles, Kleinsasser, Rosenfield, & MacDonald, 2016), or (b) employing a laboratory analogue of SA (Leone, Parrott, & Swartout, 2017; Parrott et al., 2012). Jouriles and colleagues (2016) used virtual reality technology to assess bystander intervention in response to simulated situations that could escalate to SA. Parrott and colleagues (2012) developed and validated the integrated sexual imposition paradigm, which utilizes a validated analogue of SA (Hall, DeGarmo, Eap, Teten, & Sue, 2006; Hall & Hirschman, 1994), wherein participants must decide whether to show a sexually explicit or a nonsexually explicit video clip to a female confederate who is portrayed as strongly disliking sexual content. Because two male friends participate together, a laboratory situation is created that allows a male participant to engage naturalistically in SA (via selection of a sexually explicit video) and also allows the other male participant to intervene naturalistically to prevent it (by convincing his friend to show the nonsexual video clip). Data support both the concurrent criterion and construct validity of the paradigm. Specifically, theoretically and empirically based risk factors for SA (e.g., a history of perpetration, endorsement of misogynistic attitudes) and bystander intervention (e.g., bystander self-efficacy) predicted SA and successful bystander intervention to prevent that SA, respectively (Parrott et al., 2012).

Both laboratory-based paradigms afford researchers the ability to observe prosocial bystander behavior directly. However, the integrated sexual imposition paradigm also assesses the effective prevention of laboratory-based SA in a peer context when there exists a threat of harm to a woman believed to be another participant in the study (as opposed to a virtual character). As noted previously, a critical limitation in the extant literature is the paucity of studies, which can directly assess the link between prosocial bystander behaviors and their effects on the occurrence of SA (Langhinrichsen-Rohling et al., 2011). For this reason, the present study utilized this paradigm to test our hypotheses.

The Role of Alcohol

It is well established that alcohol use is associated with SA (Abbey, 2002; Kilpatrick, Resnick, Ruggiero, Conoscenti, & McCauley, 2007; Lorenz & Ullman, 2016). SA often occurs at or after attending bars or parties where attendees drink alcohol (Armstrong, Hamilton, & Sweeney, 2006; Flack et al., 2007; Planty, 2002), and approximately 50% to 80% of men endorse perpetrating unwanted physical contact against a woman in a bar or party setting (Thompson & Cracco, 2008). Furthermore, because men perceive intoxicated women as more sexually available and interested (Abbey, Zawacki, & McAuslan, 2000;

George et al., 1997), bystanders may be less likely to intervene because they conclude that pre-assault behaviors are consensual. Finally, within drinking contexts, bystanders themselves are likely consuming alcohol, which may influence the likelihood and type of SA intervention.

Surprisingly, few studies have examined the association between alcohol use and bystander intervention for SA. One of the first investigations in this area examined bystander intervention for SA in a drinking context and found that 79% of bystanders did not intervene when SA occurred in a bar (Graham et al., 2014). However, this study did not assess why bystanders failed to intervene, or even if they, the perpetrator, or victim had consumed alcohol. Two subsequent studies suggest that heavy-drinking men are less willing to intervene in SA than non-heavy-drinking men (Fleming & Wiersma-Mosley, 2015; Orchowski, Berkowitz, Boggis, & Oesterle, 2015), particularly when the bystander knows the perpetrator (Fleming & Wiersma-Mosley, 2015). Collectively, these data tentatively point to alcohol use as a barrier to bystander intervention for SA. This conclusion is supported by Alcohol Myopia Theory (Steele & Josephs, 1990), which posits that alcohol intoxication focuses bystanders' attention onto the most salient situational cues. For example, in SA situations, alcohol would inhibit prosocial intervention, and thus reduce the likelihood of a successful intervention attempt, in cases where attention is focused onto peers who condone forceful sexual behavior (e.g., salient and immediate cue) rather than onto the sexual disinterest or discomfort of the female (e.g., less salient and less immediate cue). However, no study to date has examined the proximal effects of alcohol on bystander intervention for SA within a social or peer context.

The Present Study

The present study sought to evaluate a mechanism by which men's self-efficacy to intervene increases their likelihood of preventing a laboratory analogue of SA via observable bystander verbalizations and examine whether alcohol intoxication inhibits this mechanism. The integrated sexual imposition paradigm (Parrott et al., 2012) was employed because it allows for the direct observation of peer-to-peer verbalizations that temporally precede a laboratory analogue of SA, which can be qualitatively coded and then scored for use in quantitative models (Morgan, 2013). Consistent with the reviewed literature (Fleming & Wiersma-Mosley, 2015; McMahon et al., 2015; Orchowski et al., 2015), a moderated mediation effect was hypothesized. Specifically, it was hypothesized that

Hypothesis 1: Bystander efficacy would predict a greater likelihood of successful bystander intervention and that this effect would be mediated by higher levels of prosocial bystander verbalizations.

Hypothesis 2: Alcohol intoxication would moderate this mediation effect, such that the proposed mediation effect was expected in sober, but not intoxicated, men.

Hypothesis 3: Perceived alcohol intoxication by the female would moderate this mediation effect, such that the proposed mediation effect was expected when the female was perceived to be sober, but not intoxicated.

Method

Participants

Participants were 156 men comprising 78 dyads of healthy, heterosexual male social drinkers ($M = 24.8$, $SD = 3.4$). This final sample was derived from an initial sample of 261 male drinkers aged 21 to 35 years. Exclusion criteria included consumption of less than three drinks on average per drinking occasion, a score of six or above on the Brief Michigan Alcoholism Screening Test (Pokorny, Miller, & Kaplan, 1972), and endorsement of past or present drug- or alcohol-related problems, serious head injuries, learning disabilities, any medical conditions in which alcohol is medically contra-indicated, or serious psychotic symptomatology as defined by self-report and a t -score of above 65 on the Global Severity Index of the Symptom Checklist-90-Revised (Derogatis, 1992). The racial composition of the sample consisted of 55% African Americans, 33% Caucasians, and 13% who identified as another race or more than one race. Participants had an average of 14.1 years of education, earned US\$22,410 per year, and were largely unmarried (84%). This study was approved by the university's Institutional Review Board.

Recruitment

Participants responded to advertisements placed on Internet classifieds and throughout the community titled "Alcohol and Views about the Media." Upon contacting the laboratory, each respondent was informed that he and a "good" male friend would be asked to complete a questionnaire battery (Session 1) and an experimental session (Session 2) on two separate days. We recruited dyads comprised of friends, as opposed to strangers, so that bystander behavior could be examined within the ecologically valid peer-based context. It was emphasized to each respondent that he must complete a telephone screening interview to confirm eligibility criteria and he must have a male friend complete the screening process and be deemed eligible for the dyad to participate. Thus, the initial respondent's eligibility was contingent upon his ability to recruit a friend who was also deemed eligible. Subsequent telephone screening evaluated eligibility criteria. For dyads in which telephone screening identified only one eligible respondent, that respondent was contacted and asked to recruit another friend. All ineligible respondents were contacted by phone, informed that they would not be eligible to participate, and thanked.

Within 1 week of completing the telephone screen, eligible participant dyads were contacted by phone and scheduled for a Session 1 appointment. Participants were told to refrain from drinking alcohol or using recreational drugs 24 hr prior to the session. Based on these procedures, 261 men presented to the laboratory for Session 1, where all eligibility criteria were reevaluated. In addition, participants' self-identified sexual orientation was also assessed. Only participants who identified as heterosexual were deemed eligible. Thus, from this initial sample of 261, 53 participants did not meet initial eligibility criteria and nine additional participants did not self-identify as heterosexual. These individuals were deemed ineligible for Session 2 and remunerated for participation. In some dyads, only one participant was deemed eligible; this individual was reminded that his participation in Session 2 was contingent upon him finding another eligible friend. Because of this requirement, 29 otherwise eligible participants did not complete Session 2 because they

were unable to find an eligible friend. An additional four participants (i.e., two eligible dyads) did not return for Session 2.

Experimental Design

A mixed experimental–correlational design was employed that included two categorical predictor variables (individual beverage condition: alcohol, no-alcohol control; perceived female beverage condition: alcohol, no alcohol), one continuous predictor variable (bystander self-efficacy, as measured by the 14-item Efficacy Scale of the Bystander Intervention Questionnaire), and one continuous criterion variable (positive partner change, which reflects a dyad wherein a participant successfully intervenes so that his friend shows the nonsexual, as opposed to the sexual, video clip). Thus, participant dyads were randomly assigned to one of four experimental groups: (a) received alcohol, told female confederate received alcohol ($n = 32$); (b) received alcohol, told female confederate did not receive alcohol ($n = 46$); (c) did not receive alcohol, told female confederate received alcohol ($n = 44$); and (d) did not receive alcohol, told female confederate did not receive alcohol ($n = 34$). Prior research evidences both pharmacological and proximal expectancy (Gross, Bennett, Sloan, Marx, & Juergens, 2001) effects of alcohol on SA, although pharmacological effects are consistently larger in size. Thus, this initial study of how actual alcohol consumption facilitates bystander intervention for SA only utilized an alcohol and no-alcohol control group, which reflects the most relevant, real-world beverage group comparison. A placebo group was not utilized. To manipulate participants' perception that the female had consumed alcohol, participants were explicitly informed of her beverage condition (alcohol vs. no alcohol) at three time points: at notification of their own beverage condition (after providing informed consent), upon receiving their own beverages, and immediately prior to the task.

Materials

Demographic form.—This form assessed basic demographic information, including age, race, self-identified sexual orientation, relationship status, years of education, and yearly income.

Bystander Intervention Questionnaire–Efficacy Scale (BIQ-ES).—The 14-item BIQ-ES (Banyard, Plante, & Moynihan, 2005) assessed participants' self-efficacy to intervene successfully in a variety of abusive or otherwise deleterious situations. Participants rated items on a 5-point Likert-type scale that ranged from 1 (*not at all likely*) to 5 (*extremely likely*), with higher scores reflecting higher levels of self-efficacy to intervene. For example, items asked participants to rate their self-efficacy to “Speak up against sexist jokes” and “Call 911 if I hear someone yelling and fighting.” The authors report adequate internal consistency ($\alpha = .87$), which is consistent with the present sample ($\alpha = .86$).

Beverage Administration

Participants who received alcohol were administered a dose of 0.99 g/kg of 95% alcohol United States Pharmacopeia mixed at a 1:5 ratio with Tropicana orange juice. This dose, which ranges from four to seven standard drinks for a 130 to 220 lb male, reliably produces breath alcohol levels between 0.08% and 0.12%. The dosing procedure was also calculated for participants in the no-alcohol control group; however, they received an isovolemic

beverage consisting of only orange juice. The beverage was poured into two glasses in equal quantities. Participants were given their two glasses at equally spaced time intervals (i.e., 10 min) during the 20-min interval to control for rate of drinking. Participants consumed alcohol while seated in the same room with their friend.

Integrated Laboratory Analogue of SA and Bystander Intervention

A modified version of the sexual imposition paradigm (Hall et al., 2006; Hall & Hirschman, 1994) was used to assess sequentially (a) men's decision to subject a female to an unwanted sexual experience (i.e., choosing a sexually explicit video for a female confederate to watch), and (b) bystander intervention to stop the unwanted experience. This paradigm is presented as a media rating task in which each participant within the dyad can subject a female to an unwanted sexual experience by making her watch a sexually explicit, as opposed to a nonsexually explicit, video. Subsequently, the members of each dyad are asked to collectively agree on which video to make the female watch. Participants are explicitly informed that they can speak to each other during the dyadic decision-making process and that the task will not proceed until both participants agree about which video to show the female. This dyadic decision-making process provides the opportunity for men to discuss their decisions and to assess men's individual likelihood of preventing their friend's decision to subject the female to an unwanted sexual experience. Thus, this paradigm operationalizes (a) SA as the selection of the sexually explicit video, and (b) successful bystander intervention (i.e., positive partner change) as the event of one participant individually selecting the sexually explicit video but then selecting the nonsexually explicit video for the dyadic choice. Evidence supports the validity of this paradigm as a measure of SA and prosocial bystander intervention (Davis et al., 2014; Parrott et al., 2012).

The entire task is administered on a computer and takes approximately 25 min to complete. Each participant within the dyad sits at a separate computer monitor and keyboard in the same room. The same stimuli (e.g., instructions, video clips, etc.) appear simultaneously on each participant's monitor, and each participant makes video choices by pressing one of two keys on his respective keyboard. The computer software that controls the task was developed by Vibranz Creative Group (Lexington, KY).

Deception Manipulation

Participants were given a fictitious cover story. The participant dyad was informed that the purpose of the study was to examine how alcohol is related to media preferences. As such, they were informed that they would consume an alcoholic or nonalcoholic beverage prior to engaging in a "media rating task" with another participant. In that task, they would rate their media preferences and then subsequently be asked to rate a series of clips from Hollywood movies. To convince participants that they were engaged in a task with another person, participants completed a videotaped demographic interview in which they answered several basic questions about themselves (e.g., first name, relationship status). The experimenter ostensibly videotaped this interview for the purpose of informing the female confederate (matched in race to the participants) about the other participants in the study. Likewise, participants were told that they would also be able to view her demographic interview. In actuality, immediately prior to the media rating task, participants viewed a prerecorded 20-s

video of a female confederate who indicated that she was “single.” As part of the study’s ostensible goal of evaluating media preferences, participants were also informed that they could watch the female confederate via the laboratory’s closed circuit video system while she viewed the film clip they jointly selected. In actuality, participants viewed a prerecorded video of the female confederate watching the selected film clip. She did not display any overt reaction to the film clip that she watched (e.g., facial expressions, verbal responses). Presentation of the prerecorded videos that depicted the female confederate served to maximize the likelihood that participants believed they were engaged in a task with a female.

Procedure

Participants came to the laboratory on two separate days. During Session 1, participants were escorted to separate rooms and informed consent was obtained. Next, participants completed a written version of the telephone screening measures to confirm eligibility and a questionnaire battery that included the BIQ-ES on a computer using MediaLab 2000 software (Jarvis, 2006). In preparation for Session 2, participants were reminded to refrain from drinking alcohol or using recreational drugs 24 hr prior to Session 2 and also to refrain from eating 4 hr prior to alcohol administration.

For Session 2, which occurred an average of 1 week later, participants were again escorted to separate rooms to provide informed consent. The experimenter then assessed their breath alcohol concentration (BrAC) using the Alco-Sensor IV breath analyzer (Intoximeters Inc., St. Louis, MO) to ensure sobriety. Participants were then randomly assigned to one of the four experimental conditions. Next, participants were escorted to the testing room, at which time the experimenter identified a room down the hall where the female confederate was ostensibly seated. Participants were seated at adjacent desks equipped with a computer monitor and keyboard, completed the bogus demographic interview, received instructions about the media rating task, and consumed their beverages. Following beverage consumption, BrACs were assessed every five min until both participants reached 0.08%, on the ascending limb of the BrAC curve, where the stimulating effects of alcohol are most likely to be produced (Giancola & Zeichner, 1997). At that time, the media rating task began.

The media rating task consisted of 15 questions that ostensibly assessed media preferences (e.g., “I like movies and/or TV shows with lots of action”) on a 9-point Likert-type scale from “1” (*strongly disagree*) to “9” (*strongly agree*). After completing these items, participants were told that their answers (along with the female confederate’s answers) would be summarized to form a “media profile.” At that time, the participant dyad viewed the female confederate’s media profile while she ostensibly viewed their respective media profiles. Her media profile was presented on each participant’s respective computer monitor and it was explicitly stated that she did not like to watch sexual content or nudity in the media. To maximize the likelihood that participants would read the female’s media profile, there was a 20-s delay before participants were able to press the spacebar to advance to the next screen. The experimenter verified that all participants were looking at the computer monitor during this time period. Next, the participant dyad and female confederate viewed

two 120-s film clips that depicted sexually explicit and nonsexually explicit content. The sexually explicit film clips were taken from Jason's Lyric (which depicts African American actors) and 9½ weeks (which depicts Caucasian actors). These clips included significant nudity and featured a male and female engaged in consensual sexual intercourse involving kissing, foreplay, and implied intercourse in numerous sexual positions, although did not explicitly depict graphic sex acts (e.g., penetration). The nonsexually explicit film clips were taken from Bad Boys II (which depicts African American actors) and Quantum of Solace (which depicts Caucasian actors), both of which feature high speed car chases and car crashes but no sexual content. Each film clip was approximately 2 min long and was matched to participants' race. In dyads with at least one participant who was not Caucasian, it was randomly determined whether the dyad was shown film clips with African American or Caucasian actors. The order of film clip administration was counterbalanced.

The participant dyad was then ostensibly selected at random to choose one of the two clips to make the female confederate view a second time. At the same time, participants were offered a US\$5 incentive if they did not speak to one another prior to or while making their individual film clip choices. The purpose of this incentive was to prevent collusion between dyad members prior to the subsequent dyadic choice phase of the procedure. Participants were also told that they would be able to view the female confederate via closed circuit television as she watched the film clip they selected. Each participant then recorded separately his individual video choice on his computer keyboard with the knowledge that his or his friend's selection would be randomly selected for the female confederate to view. Immediately thereafter, the participant dyad was told that they had received the financial incentive (all participants complied); however, they would need to make a dyadic choice about which video to make the female confederate view a second time. The participant dyad was informed that they could take as much time as they needed to decide, but that to advance the screen they would need to agree upon the film clip to show the female confederate. Dyads spent an average of 42 s discussing this decision. Again, participants recorded their dyadic choice separately on their respective computer keyboards. During the individual and dyadic decision, participants were video recorded. The participant dyad then viewed the prerecorded video that depicted the female confederate watching the film clip jointly selected by the dyad. Following the testing procedure, participants were separated, asked questions to indirectly assess the credibility of the experimental manipulation (see below), debriefed, and compensated at a rate of US\$10 per hour. All individuals who received alcohol were required to remain in the laboratory until their BrAC fell to 0.03%, at which point they were escorted to pre-arranged transportation.

Qualitative Analytic Strategy

Although each participant's verbalizations were in response to their partner, the two sides of the conversation were separately analyzed to allow linkages with each participant's quantitative survey data. We conducted a three-stage qualitative coding strategy. In the first stage, a researcher who had knowledge of the data collection procedures selected transcripts from participants who expressed prosocial intervention verbalizations during the discussion ($n = 81$). In the second stage, four researchers independently sorted the verbalizations selected in Stage 1 into discrete categories using Q methodology (Brown, 1986). Stage 2

researchers were intentionally given little information prior to and during the categorization task; they were not briefed on the purpose of the study, study hypotheses, potential categories, or expected categories. There was strong overlap between categories the researchers independently developed; category definitions (codes) were honed during a research meeting attended by the four Stage 2 researchers, the Stage 1 researcher, and the first and second author.

In the third stage, two additional researchers not involved with Stages 1 or 2 applied the codes generated in Stage 2 to the entire data set. To reduce potential Stage 1 selection bias and account for definitional changes that occurred during Stage 2, codes were applied to all conversations, including those that were initially judged to lack intervening verbalizations. Stage 3 researchers were also blind to the study goals and hypotheses. They each independently coded the entire data set and met with one another, as well as the second author, after coding 25%, 50%, 75%, and 100% of the data to establish consensus both in regard to the general coding strategy and specific code applications (Saldana, 2012). Coding was conducted within MAXQDA v.10 qualitative data analysis software, which allowed the researchers to apply weights to each coded segment of text, ranging from 0 (a participant's verbalizations do not convey a given theme at all) to 100 (a participant's verbalizations strongly and emphatically convey a given theme). This strategy is consistent with Morgan's (2013) description of scoring in mixed methods research. The entire 0 to 100 weight range was utilized for each code, because noncoded segments were weighted at 0 for quantitative analysis.

Because the exchanges were often relatively brief, each participant's entire side of the conversation was treated as the unit of analysis throughout the three analytic stages. Therefore, qualitative themes were generated from, applied to, and scored based upon the entirety of a participant's comments during the video selection task. Intercoder reliability remained acceptable throughout Stage 6 (Cohen's $\kappa > .60$; $r = .70$); therefore, categorizations and weights were combined after all data were coded. Coding weights were exported from MAXQDA, then imported and integrated with participants' survey data and video choice outcome data managed in SPSS.

Quantitative Analytic Strategy

Participants' initial video choice was coded as sexually explicit = 1 or nonsexually explicit = 0. Positive partner change was coded 1 for partners who initially chose the sexually explicit video and groups that ultimately chose the nonsexually explicit video; others were coded 0. Objective consideration and moral justification scores from the qualitative analysis were averaged to create a composite prosocial intervention verbalization variable, ranging from 0 to 100. To directly test our proposed research questions, we restricted our sample to only include participants whose partners initially chose the sexually explicit video ($n = 74$), and thus had the opportunity to intervene with their partner. All models were fit via Mplus v.8.1 within a multilevel modeling framework to account for the nested data and using weighted least squares estimation, which estimates a normally distributed latent response variable indicating the probability of positive partner change (Finney & DiStefano, 2013). The indirect effect was assessed using Delta parameterization.

Results

Manipulation Check

Deception.—Prior to debriefing, participants were interviewed to confirm their belief that they were engaged with a single female on a media rating task and that the procedures were not a measure of SA or bystander intervention. Six participants (4%) from three distinct dyads reported that they did not believe they were engaged in a media rating task with another person and were removed from analyses. In addition, one dyad was removed because a participant did not reach a target BrAC of 0.08 and another dyad was removed due to a technical error. This left a final sample of 156 men, all of whom correctly indicated that the female confederate was not currently involved in an intimate relationship.

Experimental manipulations.—All participants had BrACs of 0% upon entering the laboratory. A repeated-measures ANOVA indicated that participants' BrACs in the alcohol group were significantly higher at post-task ($M = 0.11$, $SD = 0.02$) than at pre-task ($M = 0.10$, $SD = 0.02$), $F(1, 77) = 45.43$, $p < .001$. Means, as well as inspection of these data at the individual level, confirmed that all intoxicated participants were on the ascending limb of the BrAC curve during the experimental procedures. Participants in the no-alcohol control condition had a mean BrAC of 0% before and after the experimental procedures.

Qualitative Results

This three-stage process yielded four distinct categories of intervening verbalizations: *objective consideration*, *moral justification*, *gender stereotypes*, and *clip quality*. Each type of verbalization is detailed below.

Objective consideration.—The most common form of verbal intervention involved referencing the woman's stated media preferences or the information on her media rating form. Several participants clearly used this strategy: "She put down that she doesn't like watching any nudity, you know, stuff like that. So I don't want to make her feel awkward. You know what I'm saying? I don't want her to feel like we're just messing with her" (coded 100); and "I don't want her to watch it if she don't like it" (coded 100). Other participants referenced the woman's preferences, but not as strongly: "Now, you seen on the thing she said that she doesn't like to watch the sex video" (coded 50). An average discussion containing objective consideration was weighted 84.5 ($SD = 24.3$; range = 25–100).

Moral justification.—Another common form of intervention employed moral justifications, which suggested that showing the woman the sexually explicit video was not the right thing to do. A strong example was given by a participant who exclaimed, "I want to do the right thing. Let's do [the non-sexually-explicit video]. That's the right thing" (coded 100). Less emphatic intervention attempts using moral justification included statements such as "Yeah, but I wouldn't want to force it on her, you know what I am saying" (coded 30). The average discussion containing moral justifications was weighted 53 ($SD = 29.6$; range = 15–100).

Gender stereotypes.—Some participants used stereotypical information about either men or women in their attempts to argue for the nonsexually explicit video. A strong instance was, “I wouldn’t want to necessarily make a female watch that, so I would be more interested to see how she would view something that’s more intrinsic to a male. I would rather her see something like car crashes, like car chases are something where guys are like, ‘yeah car chases!’” (coded 85); whereas, a weaker instance was “Either we want to offend her, or we don’t. We don’t know her, she seems like a nice girl, so I picked [the nonsexually-explicit video]” (coded 20). The average discussion containing gender stereotypes was weighted 65 ($SD = 36.9$; range = 80–100).

Clip quality.—The final theme referenced participants’ subjective interpretations of entertainment value of each clip, which may be viewed as a situationally specific verbalization. These arguments included statements such as “It’s better, it’s a better clip” (coded 100) and “So it comes down to, do we want something with artistic credibility?” (coded 100). References to clip quality were consistently strong, and thus weighted on average 90 ($SD = 8.6$; range = 20–100).

Although the simple frequency of a code is not sufficient to support its importance to the study, it bears noting that at least one intervention verbalization was coded in 41% of the dyadic discussions. The objective consideration theme constituted 80% (41 participants) of all coded intervention verbalizations, making it the most common verbal intervention strategy in our sample by a large margin. Ten participants referenced the moral justification theme, five referenced gender stereotypes, and seven referenced quality of the video clip. In addition to these four specific themes, the qualitative analysis uncovered a general valence among the intervention verbalizations; although they were all prosocial in the sense that they were delivered within an argument against showing the woman the sexually explicit video, those coded as containing strong objective consideration and moral justification clearly differed from those containing arguments centered on gender stereotypes and clip quality. Verbalizations that contained strong objective consideration and moral justification were the most prosocial, overall; the arguments were based upon the woman’s agency and a sense of humanistic morality that did not (directly) espouse sexism or traditional gender roles. Verbalizations that included gender stereotypes contained sexist undertones, which could support the underpinnings of sexual imposition. And clip quality was a situationally specific strategy that may have been an artifact of the design, or it may have functioned as a distractor by focusing attention on the videos rather than on the potential decision to subject the female to an unwanted sexual experience. For these reasons, coding scores assigned for the objective consideration and moral justification codes were used to create a composite prosocial intervention verbalization variable for use in the quantitative portion of the study.

Quantitative Results

Descriptive statistics.—Overall, 74 (47.4%) participants selected the sexually explicit video and 82 (52.6%) selected the nonsexually explicit video during the individual video choice task. As expected, more participants who initially chose the nonsexually explicit video made prosocial intervention verbalizations during the group video choice task (see Table 1). Eighteen participants who opted for the sexually explicit video as their initial

individual choice agreed to assign the nonsexually explicit video as their group's choice—a pattern we termed “positive partner change.” This accounted for 24.3% of participants who initially chose the sexually explicit video, and it accounted for 45% of participants who initially chose the sexually explicit video and whose partner initially chose the nonsexually explicit video. Rates of prosocial intervention verbalizations were similar regardless of actor-partner match on individual video choice. Interestingly, weights for gender stereotype and clip quality codes were significantly related ($r = .47, p < .001$), and those for moral justification and objective consideration were marginally related ($r = .15, p = .065$) suggesting that these themes respectively co-occurred within discussions. Average scores of bystander self-efficacy were 53.77 ($SD = 9.28$) and average weights for the composite prosocial intervention verbalization variable was 12.00 ($SD = 21.43$) for individuals who were unsuccessful at changing their partner's choice to the nonsexually explicit video and 32.50 ($SD = 24.27$) for those who were successful, $t(38) = 2.81, p < .01$.

Model testing.—The proposed moderated mediation model fit the data well, $\chi^2(2) = 1.33, p = .51$, root mean square error approximation (RMSEA) = .01, comparative fit index (CFI) = 1.00, standardized root mean square residual (SRMR) = .07. Although, neither individual drinking ($b = .23, SE = .47, p = .64$) nor perceived female drinking ($b = -.07, SE = .44, p = .87$) significantly predicted partner positive change, and neither the Beverage \times Bystander Self-Efficacy interaction ($b = -.22, SE = .34, p = .53$) nor the Female Beverage \times Bystander Self-Efficacy interaction ($b = .29, SE = .34, p = .40$) significantly predicted prosocial verbalizations. We therefore simplified the model by removing beverage conditions as predictors of positive partner change as well as the interaction terms as predictors of prosocial verbalizations. As depicted in Figure 1, the resulting model simply tested the extent to which prosocial verbalizations mediate the relation between bystander self-efficacy and positive partner change (Hypothesis 1). Effects of individual drinking ($b = .12, SE = .26, p = .64$), perceived female drinking ($b = -.23, SE = .27, p = .38$), and video order ($b = -.28, SE = .21, p = .17$) on prosocial verbalizations were also controlled for in the final model. This model continued to fit the data well, $\chi^2(3) = 0.23, p = .97$, RMSEA = .01, CFI = 1.00, SRMR = .01. Participants with higher levels of bystander self-efficacy tended to express stronger prosocial intervention verbalizations ($b = .29, SE = .12, 95\%$ confidence interval [CI] = [0.05, 0.52]). In turn, participants who expressed stronger prosocial intervention verbalizations were most likely to bring about positive partner change ($b = .74, SE = .16, 95\%$ CI = [0.43, 1.05]). Furthermore, prosocial intervention verbalizations mediated the relationship between bystander self-efficacy and positive partner change (indirect effect = .21, $SE = .10, 95\%$ CI = [0.02, 0.41], $p = .034$). Overall, this model accounted for 10.9% of the variance in prosocial verbalization and 41.1% of the variance in the probability of positive partner change (latent response variable).

It could be argued that all participants who initially chose the sex video cannot be considered bystanders. We therefore fit the final model to a more restricted sample of participants who initially chose the nonsexually explicit video and whose partners chose the sexually explicit video, which also resulted in strong fit, $n = 40, \chi^2(2) = 0.21, p = .90$, RMSEA = .01, 95% CI = [0, 0.1307], CFI = 1.00. Compared with the sample of 74, this more restricted sample suggested a stronger direct effect of bystander self-efficacy on

prosocial verbalizations (.42 vs. .29) but a weaker (but still significant, $p < .01$) direct effect of prosocial verbalizations on positive partner change (.54 vs. .74). Although the sample restricted to those 40 participants demonstrated a similar general fit and pattern of findings, reducing the sample to this degree decreased power to detect the indirect effect of bystander self-efficacy on positive partner change, which was slightly weaker relative to the larger sample ($b = .15$, $SE = .15$, $p = .30$).

Discussion

The present investigation sought to determine the extent to which bystander self-efficacy facilitates successful bystander intervention via the use of prosocial bystander verbalizations. We also sought to examine the effect of acute alcohol use on the use and effectiveness of these verbalizations. A critical strength of the present study is that two male peers explicitly discussed whether or not to subject a female to an unwanted sexual experience. Thus, participants' verbalizations could be coded and subsequently linked to the likelihood of successful bystander intervention. A key result indicates that bystander self-efficacy increases the likelihood of successful bystander intervention because the bystander uses more prosocial verbalizations, which reference the woman's stated attitudes or moral justifications to counter potential SA. This finding is consistent with numerous studies, which indicate that men higher in bystander efficacy are more likely to intervene in sexually aggressive situations (e.g., Banyard, 2008). However, it is the first data to isolate the specific verbal strategies that contribute to a successful intervention to prevent SA.

Contrary to hypotheses, bystander efficacy predicted successful bystander intervention via more prosocial intervention verbalizations regardless of whether men were intoxicated or sober. At first glance, the absence of a moderation effect suggests that bystander alcohol use does not affect the likelihood or effectiveness of prosocial bystander verbalizations. However, this conclusion seems premature, as close scrutiny of relevant literature suggests a more nuanced and complex conclusion. Recent studies (Fleming & Wiersma-Mosley, 2015; Orchowksi et al., 2015) suggest that distal alcohol use is a barrier to successful bystander intervention for SA. In addition, according to alcohol myopia theory, intoxicated bystanders are more likely to focus on in-the-moment cues and salient norms, which inhibit prosocial intervention (e.g., negative social repercussions: Schwartz & DeKeseredy, 1997). This literature suggests that alcohol use should decrease the likelihood of prosocial intervention strategies; thus, bystander intervention programs can increase the likelihood that a bystander will intervene by reducing their alcohol use or focusing the drinker's attention onto intervention-promoting cues or norms. Given the lack of proximal alcohol effects in the present study, it is possible that the salience of social or peer norms or potential negative repercussions of intervening were so strong that even sober men were myopically focused on them. Similarly, individual- (e.g., shyness) and context-specific variables (e.g., nature of peer relationship) may have moderated this effect, as some men may be unlikely to speak up to a friend no matter their level of intoxication. Regardless, it is clear that prosocial verbalizations were similarly effective for sober and intoxicated male peer dyads.

We present results based on two different conceptualizations of a bystander. The first conceptualizes a bystander as a third party who is not at all engaged, even in a preliminary

way, with the perpetration of SA (i.e., our sample of 40). This more restrictive conceptualization reflects the “ideal bystander” who bystander interventionists aim to mold and promote (e.g., Banyard, 2015). The second conceptualizes a bystander as any additional person in a situation at risk for SA who themselves is not the victim (i.e., our sample of 74). This broader conceptualization is likely more reflective of many actual bystander situations among young male peers, where there is some shared misogyny and acceptance for aggression toward women (e.g., Jacques-Tiura et al., 2015), even among potential prosocial bystanders. Importantly, both of these conceptualizations yielded a similar pattern of findings. This suggests that the observed effects may be robust across bystanders and highlights that these nuanced, but important, distinctions should be considered in future research on bystander intervention.

Limitations

Because SA cannot be directly and ethically measured in the laboratory, the present study relied upon an analogue for SA. This has several implications. First, the present findings are subject to critiques regarding the artificiality of the methodology and its lack of correspondence to “real world” SA. This debate was explicitly reviewed by Davis and colleagues (2014), who concluded that there exists compelling evidence for the validity of the paradigm used in the present study and related laboratory-based measures of SA. Of particular relevance, while the superficial aspects of the laboratory task may not generalize outside the laboratory, the psychological processes evoked during the task likely do generalize to real-world situations (Anderson & Bushman, 1997; Berkowitz & Donnerstein, 1982; Mook, 1983). Second, experimental laboratory methods allow for direct observation of behavior and can control or manipulate theoretically relevant contextual factors; however, alternative approaches are needed to complement methodologies common to the bystander literature. For instance, daily diary methods allow for near real-time assessment of behavior and thus are less sensitive to self-report biases common to long-term retrospective self-report. A critical review of various methodological options is beyond the scope of this article; although, it seems reasonable to conclude that multimethod approaches are necessary to capitalize on the advantages of any given approach while compensating for the limitations of another—improving the content validity of measurement. Third, the present study included a scenario with one male bystander, one male perpetrator, and one female victim. In reality, there could be any combination of single or multiple bystanders, perpetrators, or victims, any of whom could be male or female. It will be important for future studies to consider these multiple bystander–perpetrator–victim configurations. Fourth, despite procedures designed to make salient the female confederate’s dislike of sexual content or nudity in the media, these methods could not guarantee that participants read her media profile. Finally, determination of which intervention behaviors are most effective is inherently limited to the context in which those behaviors are assessed. Indeed, effective prosocial intervention strategies likely vary by situational context and as a function of interpersonal interactions (e.g., direct interaction with a potential perpetrator vs. other bystanders). In this spirit, the present study only sheds light on effective verbalizations within a specific situation that involves intervention with a potential male perpetrator who is a friend of the bystander. Thus, results might not generalize to interactions in other

situational contexts or that involve different relationships between the bystander and potential perpetrator.

Clinical and Research Implications

Data from this study evidenced that prosocial verbalizations, which emphasize a potential victim's stated or implied response or the bystander's moral judgments, may effectively reduce the likelihood that one's male friend will perpetrate SA in an analogue situation. This finding indicates that the effectiveness of bystander intervention programs would be enhanced to the extent that program content facilitates bystanders' use of such statements. Importantly, evidence suggests that SA perpetrators often assume the consent of a woman (Scully & Marolla, 1984; Wegner, Abbey, Pierce, Pegram, & Woerner, 2015). Thus, for verbalizations of objective consideration to be maximally effective, bystander programs and marketing campaigns must directly target men's understanding of consent. This strategy has the potential to both promote effective bystander intervention and directly reduce SA perpetration attempts. Furthermore, it is well documented that there is wide variation in program content across different bystander programs. Although institutions collectively spend millions of dollars per year on programs specifically designed to reduce campus sexual violence, many available programs have limited effectiveness (DeGue et al., 2014). The present findings suggest that teaching bystanders how to use moral and objective consideration verbalizations might increase the effectiveness of their intervention attempt.

It is clear that continued research is needed to directly assess the link between prosocial bystander behaviors and their consequent effects on the occurrence of SA. Although 45% of the dyads that disagreed on their initial video choice ultimately selected the nonsexually explicit video clip as their group choice, 55% followed through with assigning the sexually explicit video clip. Our findings suggest the absence of prosocial intervention verbalizations during these discussions may be partly to blame, although more research is needed to better understand why these opportunities for intervention were not realized. Likewise, there is a need for more research that examines the effect of alcohol use (e.g., acute alcohol intoxication) and alcohol-related contexts (e.g., bars, house parties) on bystander intervention behavior. Data from the current project are among the first in this burgeoning area. The field requires multimethod studies that can reconcile the aforementioned limitations and further contribute to this literature. The present study provides a platform for which to base this future work.

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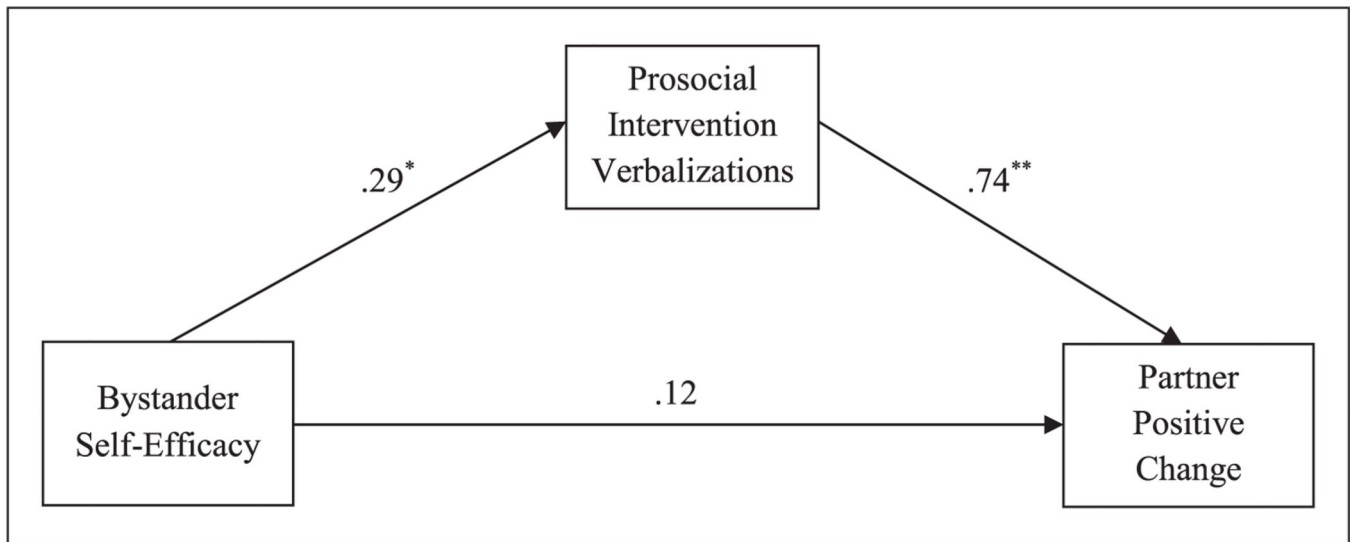


Figure 1.
Prosocial intervention mediation model.

Note. All effects standardized with regard to both predictors and outcomes.

* $p < .05$. ** $p < .001$.

Table 1.

Frequency of Qualitative Themes as a Function of Participants' Initial Video Choice and the Match Between Individual and Partner Initial Video Choice.

Individual video choice	Qualitative theme	Match with partner on individual video choice	
		Agree (%)	Disagree (%)
Sexually explicit video	Gender stereotypes	0	2.50
	Moral justification	5.90	0
	Clip quality	0	2.50
	Objective consideration	5.90	7.50
	Prosocial verbalization ^a	5.90	7.50
	<i>n</i>	34	40
Nonsexually explicit video	Gender stereotypes	2.40	7.50
	Moral justification	9.50	7.50
	Clip quality	2.40	10
	Objective consideration	45.20	40
	Prosocial verbalization ^a	52.40	42.50
	<i>n</i>	42	40

^aComposite of Moral Justification and Objective Consideration themes.