

Evaluation of The Intervention Initiative, a Bystander Intervention Program to Prevent Violence

Against Women in Universities

Rachel A. Fenton, PhD and Helen L. Mott, PhD

University of Exeter, Exeter, UK

University of the West of England, Bristol, UK

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Abstract

Violence against women students is increasingly recognized as a significant public health and human rights issue. *The Intervention Initiative* is a facilitated bystander intervention educational program commissioned by Public Health England for use by all English universities to prevent violence, abuse and coercion. The success of the program with first-year Law students at a large university in the South West of England was evaluated through course evaluation feedback and in a questionnaire study. Student experience was exceptionally good across all measures. In paired sample t-tests, prosocial bystander behavior did not increase significantly from pretest to posttest immediately after taking part in the program. Rape myth acceptance, domestic abuse myth acceptance and denial decreased significantly ($p < .001$; $d > .599$). Bystander efficacy, readiness to help and responsibility increased significantly ($p < .001$, $d = .408 - .703$) and intent to help increased significantly ($p = .007$, $d = .248$). Exposure to a concurrent social marketing campaign on campus had a significant strengthening effect on improvement of attitudes to rape myths ($p = .010$) but not any other outcome measures. No significant backlash was identified.

Violence against women is a global public health problem (García-Moreno et al., 2013). There is a growing body of international evidence documenting universities as significant sites for violence against women (DeGue, 2014; Fisher, Cullen, & Turner, 2000; Krebs, Lindquist, Warner, Fisher, & Martin, 2007), where risk factors converge (Fenton, Mott, McCartan, & Rumney, 2016; Powell, 2011; Schwartz, DeKeseredy, Tait, & Alvi, 2001; Schwartz & Pitts, 1995). Recently, an evidence base has begun to emerge from a number of surveys conducted in English universities indicating that violence against women is a serious problem with considerable impact for English university students in terms of their health and academic performance (NUS, 2011; Stenning, Mitra-Kahn, & Gunby, 2012). Further, the cultural phenomenon of ‘lad culture’, perhaps akin to the American ‘guyland’ (Berkowitz, 2011) has been reported extensively in the popular media and in an associated body of sociological research, and is recognized as a concern for universities (e.g. Phipps & Young, 2015).

Bystander intervention

A growing research base, predominantly from the US, indicates that bystander intervention shows particular aptitude for addressing prevention and response in university settings (DeGue, 2014; DeGue et al., 2014; Fenton et al., 2016). The rising importance of bystander programs is exemplified by US legislation which requires bystander programming in public and private colleges and universities participating in federal student aid programs (Campus SaVE Act, 2013). Bystander training aims to empower participants to become prosocial bystanders who recognize and intervene to prevent problematic behaviors which support violence or may become violent. By intervening, powerful messages are sent to wrongdoers, which, if constantly reinforced at community level, can shift social norms to exclude undesirable behavior.

Bystander models are complex and they address prevention at multiple levels. They aim to increase participants' likelihood of intervening and to deliver changes to attitudes, beliefs, social norms and peer group relationships with regard to gender roles and stereotyping, sexism, the acceptability of violence and rape myth acceptance that may (interrelatedly) affect conditions of perpetration and victimization. These aims of bystander programs relate to the outcome measures which have been commonly reported on in the literature. Indeed, a number of studies have reported statistically significant changes in: 1. violence perpetration and victimization (Coker et al., 2016; Foubert, Newberry, & Tatum, 2007; Gidycz, Orchowski, & Berkowitz, 2011); 2. increased interventions made, and increased responsibility, confidence and intention to make interventions (Ahrens, Rich, & Ullman, 2011; Amar, Sutherland, & Kesler, 2012; Banyard, Moynihan, & Crossman, 2009; Banyard, Moynihan, & Plante, 2007; Banyard, Plante, & Moynihan, 2005; Cares et al., 2015; Langhinrichsen-Rohling, Foubert, Brasfield, Hill, & Shelley-Tremblay, 2011; Moynihan, Banyard, Arnold, Eckstein, & Stapleton, 2011); 3. decreased rape myth acceptance and sexist attitudes, and decreased perception of peer sexist attitudes (Amar et al., 2012; Banyard et al., 2009; Banyard et al., 2007; Cares et al., 2015; Cissner, 2009; Coker et al., 2011; Foubert & Newberry, 2006; Gidycz et al., 2011; Stewart, 2014); and 4. increased knowledge about violence, empathy towards survivors and decreased denial of violence as a problem (Amar et al., 2012; Banyard et al., 2005; Cares et al., 2015; Foubert & Newberry, 2006; Gidycz et al., 2011). Some scholars have theorized and reported so-called 'backlash' effects (Flood, 2006; Hilton, 2000, Moynihan et al., 2011) from some short interventions, such as worsening attitudes (Winkel & DeKleuver, 1997) or increased likelihood to engage in rape-supportive behaviors (Berg, Lonsway, & Fitzgerald, 1999).

Recently, Coker and colleagues have reported on the distal outcome of a reduction of violence from a bystander program in a rigorous design over a 4-year period (Coker et al., 2016). A controlled trial across three comparable campuses found significant positive effects for victimization and perpetration at the community level suggesting efficacy of the bystander approach in changing culture on university campuses.

As most evidence emanates from the US, research is needed into the potential for effectiveness of bystander initiatives in other country university contexts (Fenton & Mott, 2017).

The Intervention Initiative (*III*)

III (Fenton, Mott, McCartan, & Rumney, 2014) was commissioned by Public Health England and is the first evidence-based bystander program for UK university settings. The 8-hour program is underpinned by a theory of change developed from evidence of the effectiveness of bystander programs as detailed above, integrated with theories of behavior change and of social psychological processes (Mott & Fenton, 2014) and culturally adapted for a UK audience (Fenton & Mott, 2017). The theory of change itemises the core immediate learning outcomes from the program (such as ‘learning to identify violence – including warning signs, risk factors, trajectories and continuum’) for individuals and then maps the different activities and processes that the program is designed for them to experience (such as ‘desire to change own behavior’). The (measurable) intermediate outcomes for individuals include ‘decreased rape and violence myth acceptance’ and ‘increased confidence to intervene’. The distal outcome is the ‘decrease in community incidence of violence’.

During *III*, participants are taken through the stages of bystander intervention moving from inaction to action (Latané & Darley, 1969; 1970). Sessions 1-5 are concerned with knowledge, attitudes and beliefs, and aim to enable participants to recognize sexual and domestic violence as

a problem and feel responsible for dealing with it. These sessions seek to generate an understanding of problematic behaviors along a continuum, risk factors, critical awareness of one's own gender inequitable attitudes, and empathy for victims. Sessions 5 to 8 correspond with the essential final stage of bystander intervention – that the bystander has the capacity and skills to be able to intervene safely and effectively (Banyard & Moynihan, 2011; Powell, 2014). Experiential skills training allows participants to plan for and practice intervention skills for acting as an empowered bystander in their own social settings. The full program is available online at <http://socialsciences.exeter.ac.uk/research/interventioninitiative/>, and a detailed exposition of the program is available in Fenton and Mott (2017).

The current study

Bystander prevention research in the UK is far behind that of the US with no peer-reviewed published data from the UK available (Fenton et al., 2016). The main consideration for this first evaluation of a bystander approach in a UK university setting was to evaluate *TII*'s effectiveness while allowing results to be comparable with other extant, similar or less similar prevention programs (see Schewe & O'Donohue, 1993). The aim for this first evaluation of *TII* was to evaluate its effectiveness in this setting, measure any 'backlash' effects and assess potential interaction or signal-boosting effects of a concurrent student-led awareness raising campaign on campus. Effectiveness measures were based on expected outcomes in the theory of change model corresponding to existing scales reported upon in the literature. The initial intention for this study was to run a two group pre- post- design comparing intervention against control but we found that numbers of control group participants completing pretests and posttests were insufficient to justify detailed reporting. This study therefore uses a single group repeated-measures analysis.¹

A crucial component of this research was to address the sustainability of the program in the real world by measuring its acceptability to students. We theorized that for maximal effectiveness, a community-level intervention should be scheduled formally into university education rather than be delivered to self-selecting groups. Thus an important distinctive feature of this study was that *TII* was situated within the curriculum for the year cohort of Law students.

Hypotheses

We developed 10 hypotheses for this article. Our first hypothesis (a) was that students would evaluate *TII* positively for course content, efficacy and delivery, demonstrating acceptability to students of the *TII* program. The nine statistical hypotheses relate to the theory of change model (Mott & Fenton, 2014). The scale and scope of this study defined the parameters of our statistical hypotheses, *viz.* a number of intermediate outcomes measured primarily in reported attitude change among students taking part in *TII*. Primary statistical hypotheses were that taking part in *TII* would show a positive benefit across a range of measures: (b) decreased rape myth acceptance; (c) decreased acceptance of myths concerning domestic abuse; (d) increased bystander efficacy; (e) readiness to help – reduced denial; (f) readiness to help – increased responsibility; and (g) increased intent to help. We further hypothesized (h) that positive effects of the program would be enhanced among students who had been exposed to the campus awareness-raising campaign “The Anti-Abuse Campaign”.

We had two secondary hypotheses. We hypothesized that: (i) students taking part in *TII* would report increased engagement in bystander intervention behavior. The secondary status of this hypothesis came from the knowledge that posttest timing would weaken the data. Our final hypothesis (j) was that ‘backlash’ (a significant negative change in attitude) would occur in no more than 10% of participants, or half the number of participants experiencing a significant

positive change in attitude, whichever was the greater proportion. Hypothesis testing was bidirectional given the importance of capturing any (theoretically potential) negative effects.

Method

Participants

Participants were 354 first year students studying Law at a large university in the South West of England. The pretest was completed by 274 participants. 210 students completed the posttest, of whom 131 completed both questionnaires. 213 students completed the course evaluation feedback form. See Figure 1 for a diagram describing the flow of participants. The study was conducted in compliance with the University's Faculty of Business and Law Research Ethics Committee, endorsed and supported by Public Health England. Electronic informed consent was provided by each research participant prior to beginning the online questionnaire.

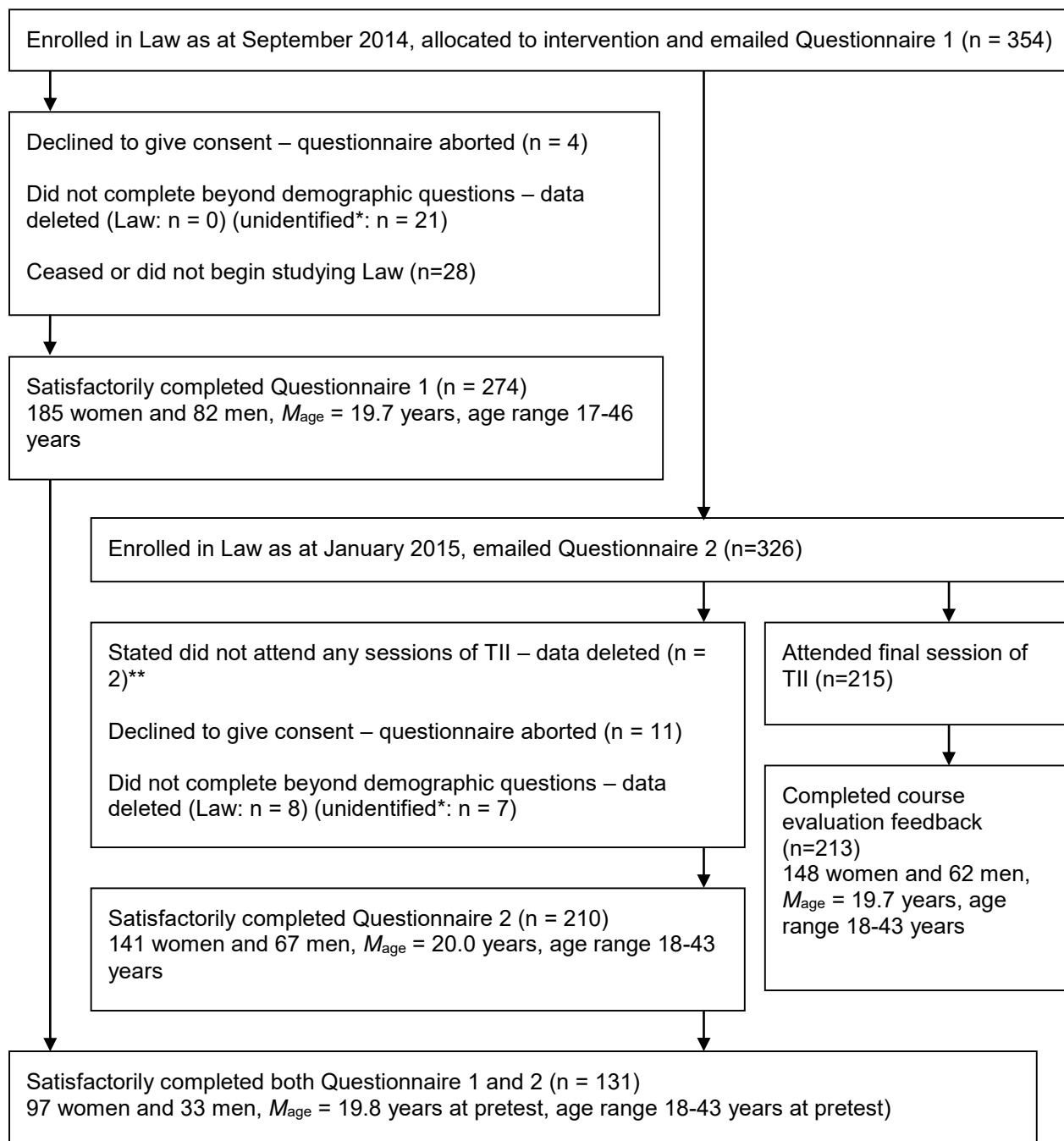
Procedure

Delivery of *THI*.

THI was timetabled within a core level one module for all Law students with academic credit available for reflective logs on each *THI* session as part of summative assessment. No other incentives were given. Students received a short motivational address encouraging them to attend and to participate in the evaluation. Students were advised about wellbeing issues and alternative assessments were in place.

THI was delivered in four 2-hour sessions to 14 groups of between 15 and 25 students between October 2014 and January 2015. A social marketing "Anti-Abuse" Campaign (<https://www.thestudentsunion.co.uk/representation/campaigns/enditnow/>) led by the Student Union took place on campus between November 2014 and January 2015.

Figure 1. Flow of participants in the 2014-15 pilot evaluation of The Intervention Initiative



Note. Numbers of students self-defining as “trans”, “other” or “prefer not to say” have not been reported to preserve confidentiality

* students in another School were also emailed the questionnaire as part of a larger study. It was not possible to identify whether some of those who failed to complete the questionnaire were Law students

**as the questionnaire was anonymous there was no way of ascertaining whether these students deliberately missed the program or were late arrivals so their data were discarded

Student course evaluation.

At the final session students were given a paper copy of the course evaluation feedback form. Anonymity was stressed. Completed forms were sealed in an envelope and given to the program administrator who collated the data.

Questionnaire administration.

All Law students were emailed a link to the questionnaire via the Qualtrics online survey platform. Data collection was closed after 17 days and before *TII* began. The posttest questionnaire included five additional questions and was administered in the same way one week after *TII* had been completed. Where possible, the order of the following scales, and items within scales, presented to participants was randomized to spread effects of survey fatigue.

Measures

Course feedback evaluation form.

Students used a five-point rating scale across all items, where 1 = “definite no or not good” and 5 = “definite yes or excellent” to rate the extent to which the program met its learning objectives (improved knowledge, attitudes, likelihood to intervene and leadership skills) and aspects of the program design and facilitation. Space was provided for comments.

Main Questionnaire.

Questions from the scales detailed below were modified where necessary for UK English language and cultural relevance. Full details are available from the first author.

Rape and domestic abuse myth acceptance.

We used McMahon’s (2011) modified short form of the Illinois Rape Myth Acceptance (IRMA) scale to measure attitudes and beliefs about sexual violence. We also developed a five-item question set (unvalidated) to assess attitudes and knowledge about domestic abuse, using

one relevant item from the Acceptance of General Dating Violence Scale (cited in Coker et al., 2011). Four additional items (such as “Domestic abuse doesn’t really happen in young people’s relationships”) were developed by the authors based on the content of *THI*. On a scale of 1-5, higher scores represent higher myth acceptance. The mean IRMA score and mean Domestic Abuse Myth Acceptance score were calculated for each participant at pretest and posttest.

Readiness to help.

Following Moynihan et al. (2011) we adapted items forming two subscales – Denial and Responsibility – calculated originally from a factor analysis of the Readiness to Change Scale II. Items relating to domestic abuse were added to the items relating to sexual violence for both subscales (it should be noted that Banyard et al. (2014) have refined the Readiness to Help measures and their new scale relates to sexual violence/abuse, relationship abuse and stalking). Responses on the Denial subscale were coded for lower scores to indicate greater denial or lack of awareness. Responses on the Responsibility subscale were coded for higher scores to indicate a greater taking of responsibility. Mean scores for all items in the range 1-5 produced a Readiness to Help score.

Bystander efficacy.

Banyard, Moynihan and Plante’s (2007) Bystander Efficacy Scale, with some modifications, was used to measure participants’ self-reported confidence to perform bystander actions. One extra item was added, “Help a friend by providing information about specialist support services for domestic abuse” to correspond with program content. A mean score was calculated from each of the scores indicating confidence in the range 1-100.

Intent to help.

We used the Intent to Help Friends Scale and Intent to Help Strangers Scale (brief versions) (Banyard et al., 2014, with minor modifications) to calculate an overall Intent to Help score from the mean of each of the items in the range 1-5, with higher scores indicating greater intent to help.

Bystander behavior.

We used Banyard's Bystander Behavior Scale – Revised (BBS-R) (as modified by McMahon, Postmus, & Koenick, 2011, with further minor modifications) to measure participants' engagement in a range of bystander behaviors in the previous two months. Five additional items borrowed from the work of Coker et al. (2011) were introduced and we followed the scoring protocol of McMahon et al. (2011) so that engaging in a behavior was scored +1, not being in the relevant situation was scored 0 and being in the situation but not engaging in the bystander behavior was scored -1. A summative bystander behavior score was calculated for each participant at pretest and posttest.

Additional posttest questions including Anti-Abuse Campaign.

Five questions were added to the questionnaire for the posttest administration in relation to attendance of *TII*. Zero attendance was an exclusion criterion. Students were asked whether they had been aware of / participated in the simultaneous awareness-raising program ("Anti-Abuse Campaign"). Responses were coded 1 for no awareness of the campaign and 2 for awareness, signing the campaign pledge or otherwise being involved in the campaign.

Backlash.

The method suggested by Moynihan et al. (2011) was used to measure potential attitude backlash. A categorical variable for each participant was computed for each of the four attitudinal variables: IRMA, domestic abuse myth acceptance, denial and responsibility. This

variable represented the difference between pretest and posttest (pretest minus posttest score for IRMA and domestic abuse myth acceptance; posttest minus pretest for denial and responsibility). Any attitude change in a negative direction equal to or larger than one standard deviation from the study population was taken to indicate a substantial negative shift that could be attributable to backlash.

Methods of Analysis

Data validity checks, a missing values analysis, and descriptive summaries were performed prior to inferential analysis. Data screening did not show any unusual or unduly influential observations. The primary analysis for this pre-test post-test design used the paired samples *t*-test. The paired samples *t*-test is known to be robust to departures from normality in large sample sizes and retains good power advantages with large sample sizes displaying a positive correlation (e.g. Rasch & Guiard, 2004). Effect size for the pre - post analysis was quantified using Cohen's *d* (e.g. Rosenthal & Rosnow, 1991); by way of general guidance, $d < 0.1$ indicates a trivial effect, $0.1 < d < 0.3$ a small effect, $0.3 < d < 0.5$ a moderate effect, $0.5 < d < 0.8$ a medium effect, $0.8 < d < 1.3$ a large effect, and $d > 1.3$ a very large effect and in practice $d > 0.5$ is often looked upon as being the minimum threshold for a meaningful change.

Multiple imputations using chained equations [MICE] was used in a sensitivity analysis to determine the effect of non-response bias and these additional analyses did not alter substantive conclusions. On this basis the results provided are based on the participating sample without imputation. All analyses were performed in SPSS version 22.

Results

Course feedback evaluation form

There was a high response rate of 99% of those present at the final session, representing 74% of all students who were still enrolled in Law. There were no striking differences in the ratings of learning outcomes or program design and facilitation, by gender or by program facilitator. Results are reported in Fenton & Mott (2015).

All learning outcomes received a mean score of more than four. All program design and facilitation items were rated more than four except “The length of the program and of each session was about right” which received a mean rating of 3.98. Written commentary indicates some students thought the sessions too short or too long. The average rating for recommending the program to others was 4.38.

Questionnaire and quantitative analysis

Outcome measures for the entire dataset.

The missing value analysis conducted on all data did not show any significant differences between those who stayed in the study and those who did not complete all measures, and the data that were missing were found to be consistent with being missing at random. The data reported are from those participants who completed the study as described in Figure 1. The maximum amount of data available was used for each analysis.

Table 1 shows descriptive statistics for the outcome measures at pretest and posttest, for the students who completed both questionnaires. Given the range of individual responses, data for this group were subjected to further analysis using the paired-sample method in order to test the hypotheses. Missing data for paired-sample students on the variables analyzed for this article constituted 0.9%. For comparison, Table 1 also shows the outcome measures at pretest and posttest for all students who satisfactorily completed the questionnaires. Missing data for all students on these variables constituted 4.5%. Cronbach’s alphas for all measures were very good

(.700 - .928) with the exception of two datasets at pretest (Domestic Abuse Myth Acceptance: $\alpha = .621$ among all students and Bystander Behavior: $\alpha = .605$ among paired samples) and one dataset at post-test (Denial: $\alpha = .693$ among paired samples).

Table 1

Means (Standard Deviations) and Cronbach's alpha for students at pretest and posttest: all participants and only those who completed both pretest and posttest

	<u>Pretest</u>				<u>Posttest</u>			
	<u>Paired samples</u>		<u>All</u>		<u>Paired samples</u>		<u>All</u>	
N	131		274		131		210	
Measure		α		α		α		α
Myth Acceptance								
Rape Myth Acceptance	2.19 (0.65)	.928	2.18 (0.66)	.923	1.88 (0.59)	.908	1.90 (0.60)	.908
Domestic Abuse MA	2.35 (0.59)	.725	2.36 (0.60)	.621	1.98 (0.63)	.700	2.03 (0.67)	.724
Bystander Efficacy	74.68 (15.00)	.903	75.11 (14.97)	.905	79.95 (13.89)	.919	79.82 (13.73)	.923
Readiness to Help	2.94 (0.59)	.830	2.93 (0.59)	.824	3.33 (0.61)	.825	3.29 (0.60)	.821
Denial	3.18 (0.79)	.744	3.11 (0.78)	.721	3.68 (0.73)	.693	3.59 (0.73)	.702
Responsibility	2.82 (0.60)	.770	2.84 (0.62)	.778	3.16 (0.67)	.788	3.14 (0.68)	.790
Intent to Help	63.74 (11.22)	.898	63.87 (11.27)	.895	66.65 (11.58)	.934	66.64 (11.53)	.920
Bystander Behavior	2.44 (3.62)	.605	3.14 (4.29)	.744	2.83 (3.76)	.706	2.79 (4.06)	.740

Paired sample tests for significant difference in outcomes.

Table 2 shows descriptive statistics and statistical test results for significant difference in the outcome measures at pretest and posttest. Students who participated in the program showed significant improvement in the desired direction ($p < .001$) in the following outcome measures: rape myth acceptance; domestic abuse myth acceptance; bystander efficacy; readiness to help (both denial and responsibility). They showed significant improvement in the desired direction ($p = .007$) in the measure for intent to help. The effect size measured by Cohen's d was also above .5 for the measures of myth acceptance and readiness to help, while the effect size for efficacy was above .4, and above .2 for intent to help. There was no significant effect of the program on the students' reported engagement in bystander behavior ($p = .226$).

Effect of concurrent awareness raising campaign.

Table 3 shows descriptive statistics and statistical test results for significant difference in pretest to posttest scores for students who were, or were not, exposed to the Anti-Abuse Campaign running on campus at the same time as the intervention. There was a significant result for the positive change in scores on rape myth acceptance ($p = .01$), suggesting that exposure to the Anti-Abuse Campaign had a strengthening effect on the improvement of attitudes to rape myths resulting from participation in *III*. Difference in the change on domestic abuse myth acceptance scores also approached significance. However, there were no significant effects found for the remaining outcome measures.

Table 2

Paired 2-tailed t-tests before and after participating in TII

Measure	Paired Differences						<i>t</i>	<i>df</i>	<i>p</i>
	M	SD	<i>d</i>	SEM	95% CI				
					<i>LL</i>	<i>UL</i>			
Myth Acceptance									
Rape Myth Acceptance	.305	.510	.599	.046	.214	.395	6.683	124	<.001
Domestic Abuse MA	.378	.560	.675	.050	.278	.477	7.538	124	<.001
Bystander Efficacy	5.265	12.920	.408	1.133	3.024	7.507	4.647	129	<.001
Readiness to Help	.388	.552	.703	.049	.292	.485	7.985	128	<.001
Denial	.493	.779	.633	.069	.357	.629	7.183	128	<.001
Responsibility	.336	.571	.589	.050	.237	.436	6.682	128	<.001
Intent to Help	2.90	11.679	.248	1.049	.827	4.979	2.768	123	.007
Bystander Behavior	.397	3.658	.109	.326	.248	1.042	1.218	125	.226

Note. M = mean; SD = standard deviation; SEM = standard error of the mean; CI = confidence interval

Table 3

Independent t-test for difference in pretest-posttest scores between exposure to Anti-Abuse Campaign and no exposure to Anti-Abuse Campaign

Measure	Law Students (N = 123 -129)					
	M (SE)	95% CI		t	df	p
		LL	UL			
Rape Myth Acceptance	.257 (.096)	.0646	.449	2.663	73.988	.010
Domestic Abuse Myth Acceptance	.182 (.101)	-.0189	.382	1.800	87.391	.075
Bystander Efficacy	.803 (2.605)	-4.397	6.004	.308	66.862	.759
Readiness to Help	-.001 (.105)	-.210	.208	-.008	75.581	.994
Denial	-.051 (.143)	-.336	.235	-.354	81.540	.724
Responsibility	.024 (.107)	-.188	.236	.227	78.172	.821
Intent to Help	1.363 (2.065)	-2.737	5.462	.660	94.993	.511
Bystander Behavior	1.106 (.737)	-.366	2.578	1.500	65.916	.139

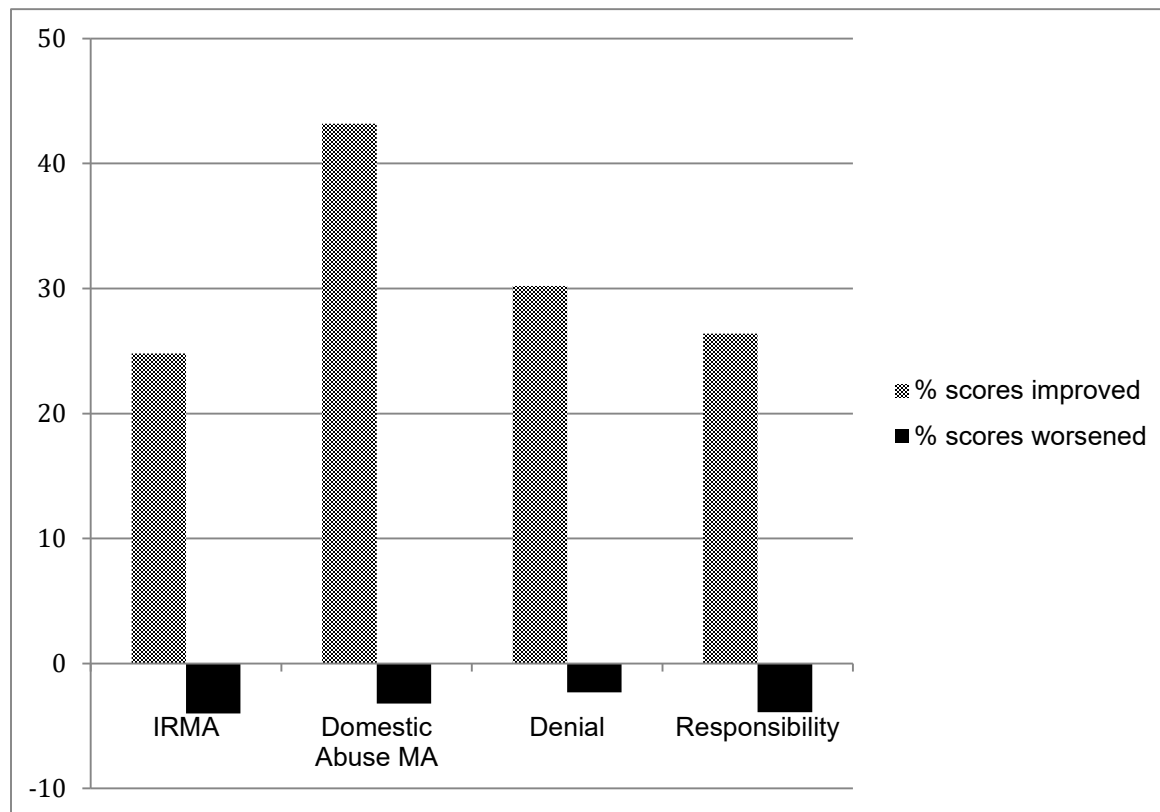
Note. Equal variances were not assumed. *M* is the mean difference in mean change scores between those who were exposed to the Anti-Abuse Campaign and those who were not. A positive value for *M* suggests a positive effect of the Anti-Abuse Campaign.

Numbers of students exposed and not exposed to the anti-abuse campaign were 84-89 and 39-40. SD = standard deviation; CI = confidence interval.

Backlash.

Figure 2 shows that at least 20% of participants showed scores that had improved by more than one standard deviation across all the attitude measures while between 2-4% showed scores that worsened by more than one standard deviation.

Figure 2. Percentage of students whose scores worsened or improved by 1 SD from pretest to posttest



Discussion

The results of this study are promising and consistent with previous research. They indicate that *TII* has the potential for effectiveness. As the first study of bystander intervention in England, the findings represent the first empirical support from England to contribute to the published literature demonstrating the utility and potential effectiveness of bystander approaches in violence prevention in universities in local campus settings. The findings have additional importance because they demonstrate that significant results can be obtained from a cohort of

students who did not self-select into a prevention program, and that *TII* is acceptable to students as a cohort in an English university setting.

Course feedback evaluation form

The course evaluation results were very positive, suggesting a high level of engagement with the aims of the program by students and recognition of the benefits for other students. The high response rate and anonymity give reason to trust the validity of the reported ratings. This result endorses the pedagogy underpinning the structural program design (Fenton & Mott, 2017). This finding of acceptability to students is important for the sustainability of *TII* in the sector.

Questionnaire and quantitative analysis

Primary hypotheses: paired sample tests for significant difference in outcomes.

In line with our primary statistical hypotheses, the findings indicate that the program had a strongly significant effect on participants. The study findings correspond with the theoretical models upon which the program is predicated and are consistent with previous research. The program significantly decreased participants' rape and domestic abuse myth acceptance, and significantly increased their bystander efficacy and readiness and intent to help. All significant results also showed effect sizes indicating meaningful change with the highest effect sizes for myth acceptance and readiness to help, followed by efficacy and finally, intent to help.

This intervention is grounded in an understanding of a bystander program as a complex model. The first purpose of the program is to increase the ability of a bystander to identify and then intervene to prevent an act, situation or course of conduct occurring along the continuum of sexual violence. This approach appears successful as the findings demonstrate significant effects across a range of variables and factors which contribute to preparedness and increased likelihood

of intervention (bystander efficacy; denial; responsibility; intent to help). The study adds to previous research which reports positive significant changes across steps towards intervening.

The second purpose of the program is its use as a strategic vehicle for the delivery of potential changes to attitudes and beliefs which enable violence against women and correlate with the stages of bystander intervention and conditions for perpetration of problematic behavior (e.g. Banyard & Moynihan, 2011). The findings of significantly improved attitudes and beliefs as to rape and domestic abuse are consistent with previous research and support the utility of the program as based on a complex ecological model to address violence. There is little reason to support any alternative explanation for the significant changes between pretest and posttest, given that the results for the students in this study are consistent with findings from other studies. While demand characteristics are a possible explanation for some of the findings, it is reasonable to assume that demand characteristics (the desire to show researchers that the program is effective) would tend to have a similar effect upon responses to all questionnaire items across the board. However, responses were significantly different at post-test from pretest for attitudes while not significantly different for reported bystander behavior. Further research comparing responses of participants in different interventions and where there have been no interventions is needed.

Effect of concurrent awareness raising campaign.

The data suggest that the Anti-Abuse Campaign had a significant additive effect on positive change in students' attitudes towards rape myths. This effect is in line with an ecological theory of change that includes information about social norms as a factor contributing to attitudinal and or behavioral change (see e.g. Hester & Lilley, 2014; Moosa, 2012). In other words, encountering the message that the community of students stand together against abusive behavior

and reject rape myths helps to reinforce the ‘top-down’ learning occurring as a result of participation in *TII*. Other outcome measures were not significantly affected for this group although the general trend was in the expected direction.

Secondary hypotheses: bystander behavior.

The first interpretation from the null results is that *TII* was ineffective in producing behavior change. We cannot rule this out from the data. A second interpretation is that participants did not encounter opportunities to operationalize their learning at the point of posttest. In this study, posttest questionnaires were completed between one and eight weeks after the final two sessions of the program, in which intervention skills were learned (average completion of posttest = last session + two weeks). Therefore the majority of participants only had two weeks in those two months during which they could have used any newly acquired intervention skills, as newly motivated prosocial bystanders. This interpretation could be tested by further evaluation of *TII* with follow-up of this cohort of students or post-testing other cohorts at later points.

A third interpretation is that the measure for bystander behavior (amended BBS-R) was not valid or reliable for our participants. We note that most of the original questions are framed in the language of ‘acting in the moment’ to challenge unacceptable behavior in a confrontational manner (e.g. “Confront a friend who is trying to have sex with someone who is practically unconscious”). *TII* by contrast encourages the use of body language, non-confrontational challenge such as distraction, interruption and changing the focus, and strategizing to intervene productively after the event. Additionally there are issues with the utility of using a simple additive method to score bystander behaviors and missed opportunities to intervene, especially in the context where opportunities (e.g. student parties; sexual encounters; friends in relationships)

may be reasonably expected to be quite different between the 2-month period being recalled at pretest at the beginning of the first year, and at posttest in the middle of the first year.

More work is needed to adapt or develop a validated behavior measure better suited to use with *TII*. However, we also note two further difficulties with measuring bystander behavior. First, we suspect that situating one scale that purports to measure actual behavior within a long questionnaire populated largely by hypothetical attitudinal questions may confuse participants and solicit hypothetical answers. This suspicion was aroused on noting the high proportion of participants (9.2%, i.e. 32 students) who answered “Yes” when asked at pretest whether they had in the past 2 months reported a friend who committed a rape. This seems highly unlikely and it is more credible that this question was answered as though it was a hypothetical question despite its wording.

Second, the general ability of research participants to recall past behavior accurately is highly questionable (Bernard, Killworth, Kronenfeld, & Sailer, 1984). We have found only one study which reports (relatively weak) significant change in measured bystander behavior (Banyard et al., 2007). Qualitative research may be more apposite for soliciting reliable information about behavior change (e.g. Banyard, et al., 2005).

Backlash.

In line with the secondary hypothesis, attitude backlash appeared minimal among the participants in *TII* while attitude improvement appeared substantial. Unlike most reported bystander program evaluations, participation in *TII* was not by self-selection. This fact enhances confidence that the extent of backlash that may be induced by *TII* is well within the bounds of acceptability. Backlash data compares well with reported backlash in the study by Moynihan et al. (2011), whose sample was self-selecting and entirely female.

Limitations

This study was conducted as an exploratory descriptive study using the cohort of participants (Law students at one university) who were the first to undertake the newly developed bystander intervention program. This cohort was not of a size to allow large scale analysis of clinical trial proportions and data from a matched control group of sufficient size were not available. Therefore while the results are indicative of very positive effects, the possibility that there may be the chance of false significant results within the data cannot be discounted.

The statistical results from this study were consistent with results from similar studies of university bystander intervention programs, which may indicate generalizability in principle. Nonetheless, as this is the first study to be conducted upon students in an English university setting, the first to be conducted upon Law students in particular, and the first to be conducted upon students where the intervention was embedded within the curriculum, generalizability should be tested further.

Our sample size, which was further reduced by missingness between pretest and posttest data, precluded the generation of data subsets while other studies have shown that participant characteristics such as sex in particular, but also potentially ethnicity as well as year of study, can affect results (Banyard & Moynihan, 2011; Brown et al., 2014; Coker et al., 2015).

The student feedback data are original and therefore we cannot benchmark against previous research. Whilst the program was extremely well received by Law students at this university we cannot know whether these results are generalizable. The feedback data is limited to those still engaged with the course whose mean reported session attendance was 91.66% - and thus likely to be the most attentive, positive, enthusiastic students. However, as numbers in attendance at the

final session of *TII* were not any lower (and anecdotally may well have been higher) than for comparable modules, it is possible that the absent students overlap with those who are poor attenders generally, rather than constituting a significant proportion of students with particular resistance or hostility to the program whose data may have skewed the results. This assumption should be tested in further research, as should baseline characteristics of poor attenders.

We also note that as the student feedback surveys - although anonymous - were not completed online but in the classroom in the presence of peers and the facilitator, the extent of demand characteristics may have been influenced.

Methodological issues.

The method we used to enable participants to self-generate 'unique' identifying codes proved unsatisfactory, generating a number of duplicate and irreconcilable response sets. We also note that the questionnaire was too long in its entirety, needing to be shorter in order to maximize completion and accuracy. Further consideration should be given to addressing the limitations of memory/recall questions for measuring behavioral outcomes such as bystander activity.

Theoretical issues.

For further evaluation of *TII*, measures with improved construct validity of relevance to the theory of change for *TII* (Mott & Fenton, 2014) should be developed. Such measures should capture specific pretest-posttest changes, for example in victim/survivor empathy, and should be further aligned with specific course content and learning outcomes.

Conclusions and future directions

Research into bystander programs for the prevention of violence against women in universities in the UK is in its infancy. This study is an important and timely step in the

advancement of knowledge about the utility of an evidence-based bystander programming approach to prevention work in universities in England. This study provides an important model for ongoing research in localized university contexts in the UK. It is the first study to show significant improvements in an English university across a range of variables that are firmly associated in the literature with the occurrence of violence against women. Further, it shows the acceptability and feasibility of formal or curriculum based bystander programming in universities, in which the skills acquired are transferable and are skills for life. The success of *TII* provides evidence that bystander programming is an important part of a strategic response for universities (see Fenton & Mott, 2015b).

Further research is needed to test this program. Longitudinal multi-site testing using randomization, controls and incentivizing is indicated. With larger sample sizes it will be possible to shorten questionnaire length through a split-questionnaire design. When more UK-based research is conducted with *TII*, measures will require further development, using cognitive interviewing with UK students to enhance validity.

Broader issues identified with measuring outcomes in the field include the need to develop sound methodologies for capturing behavior change. Further theoretical and methodological development of the concept of backlash is required. It is accepted that some backlash is inevitable, indeed it can be a sign that culture change is occurring, but there appears to be little in the academic literature that formally theorizes or aims to measure backlash.

For bystander effectiveness to be meaningfully measured in future studies, new, rigorously tested standardized measures that capture information about all forms of violence against women need to be developed and adopted in the United Kingdom.

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ⁱ A more comprehensive report on the project and its findings including control group statistics is available from the authors: R.fenton@exeter.ac.uk and helen.mott.uk@gmail.com