

A Behavior-Based Intervention That Prevents Sexual Assault: the Results of a Matched-Pairs, Cluster-Randomized Study in Nairobi, Kenya

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Abstract

Design The study's design was a cluster-randomized, matched-pairs, parallel trial of a behavior-based sexual assault prevention intervention in the informal settlements.

Methods The participants were primary school girls aged 10–16. Classroom-based interventions for girls and boys were delivered by instructors from the same settlements, at the same time, over six 2-h sessions. The girls' program had components of empowerment, gender relations, and self-defense. The boys' program promotes healthy gender norms. The control arm of the study received a health and hygiene curriculum. The primary outcome was the rate of sexual assault in the prior 12 months at the cluster level (school level). Secondary outcomes included the generalized self-efficacy scale, the distribution of number of times victims were sexually assaulted in the prior period, skills used, disclosure rates, and distribution of perpetrators. Difference-in-differences estimates are reported with bootstrapped confidence intervals.

Results Fourteen schools with 3147 girls from the intervention group and 14 schools with 2539 girls from the control

group were included in the analysis. We estimate a 3.7 % decrease, $p = 0.03$ and 95 % CI = (0.4, 8.0), in risk of sexual assault in the intervention group due to the intervention (initially 7.3 % at baseline). We estimate an increase in mean generalized self-efficacy score of 0.19 (baseline average 3.1, on a 1–4 scale), $p = 0.0004$ and 95 % CI = (0.08, 0.39).

Interpretation This innovative intervention that combined parallel training for young adolescent girls and boys in school settings showed significant reduction in the rate of sexual assault among girls in this population.

Keywords Adolescent · Rape/prevention and control · Gender-based violence · Rape/statistics and numerical data · Prospective study · Randomized controlled trial · School-based · Sub-Saharan Africa

Introduction

Background

The prevalence of sexual assault among adolescent girls varies depending upon a number of risk factors including age, geographic location, socioeconomic status, cultural gender norms, and women's economic dependence on men. Because of the complexity of the contributing factors, interventions to reduce both the prevalence and incidence of violence against women and girls have utilized a wide range of approaches with variable results (Ellsberg et al., 2014a, b). Most interventions have been evaluated in high-income countries with fewer studies in low- and middle-income countries (Ellsberg et al., 2014a, b), and there is a paucity of data specifically addressing sexual assault among adolescents (Lundgren & Amin, 2015). In the informal settlements of

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Nairobi, Kenya, we have previously shown that approximately one in four high school girls experienced sexual assault in the preceding year, and in the majority of cases, the perpetrator was known to the victim, most commonly a boyfriend (Sinclair et al., 2013). In two cross-sectional studies, we found that a 6-week classroom-based girls' empowerment and self-defense program successfully reduced the incidence of sexual assault by a factor of 38–63 % compared to a control group. However, these studies were limited by relatively small sample sizes, retrospective design, and lack of randomization (Samquist et al., 2016; Samquist et al., 2014; Sinclair et al., 2013).

There is increasing recognition that gender-based violence (GBV) prevention efforts need to target boys and men in addition to girls and women (Devries et al., 2013; Ellsberg et al., 2014a, b). We have previously shown that, in the same settlements in Nairobi, a parallel classroom-based educational intervention in adolescent boys successfully improved boys' attitudes toward girls regarding gender stereotypes and that these changes were sustained 1 year later (Keller et al., 2015).

Based on our previous findings discussed above, and our belief that this problem is best tackled by addressing boys' and girls' needs simultaneously in the same communities, we conducted a trial to test the hypothesis that a 6-week classroom-based girls' empowerment program in parallel with a boys' educational program would significantly reduce the incidence of sexual assault in the year after intervention compared to the group randomized to receive a standard of care (SOC) life skills class. The study design was a large-scale cluster-randomized controlled trial with primary school students clustered at the school level. This study addresses three key gaps in the current literature: (i) to address the methodological limitations of prior studies; (ii) to broaden the intervention to include both boys and girls taught in tandem; and (iii) to initiate the intervention at an earlier age, among primary school students rather than secondary school students, which may increase the effectiveness by reaching children before they enter the later teenage years, the ages of greatest risk for sexual assault.

Methods

Trial Design

This was a parallel-group, matched-pairs, cluster-randomized study conducted in Nairobi, Kenya, from October 2013 to October 2014. Student-participants formed natural clusters at the school level. Clustering and matched pairs were done at the school level. This study used an "open-cohort" design: the participants were allowed to enter and exit between baseline and final measurement; measurements were taken on all students within the

school at time of measurement at baseline and follow-up. We follow most closely to the cluster-focused, intent-to-treat approach as described in Vuchinich et al. (2012). See the "Statistical Methods for Primary and Secondary Outcomes" section for discussion of how to interpret the results of an open-cohort, cluster-randomized trial (CRT). No important changes to trial design were made after commencement. This trial was not registered with a major registry.

Participants

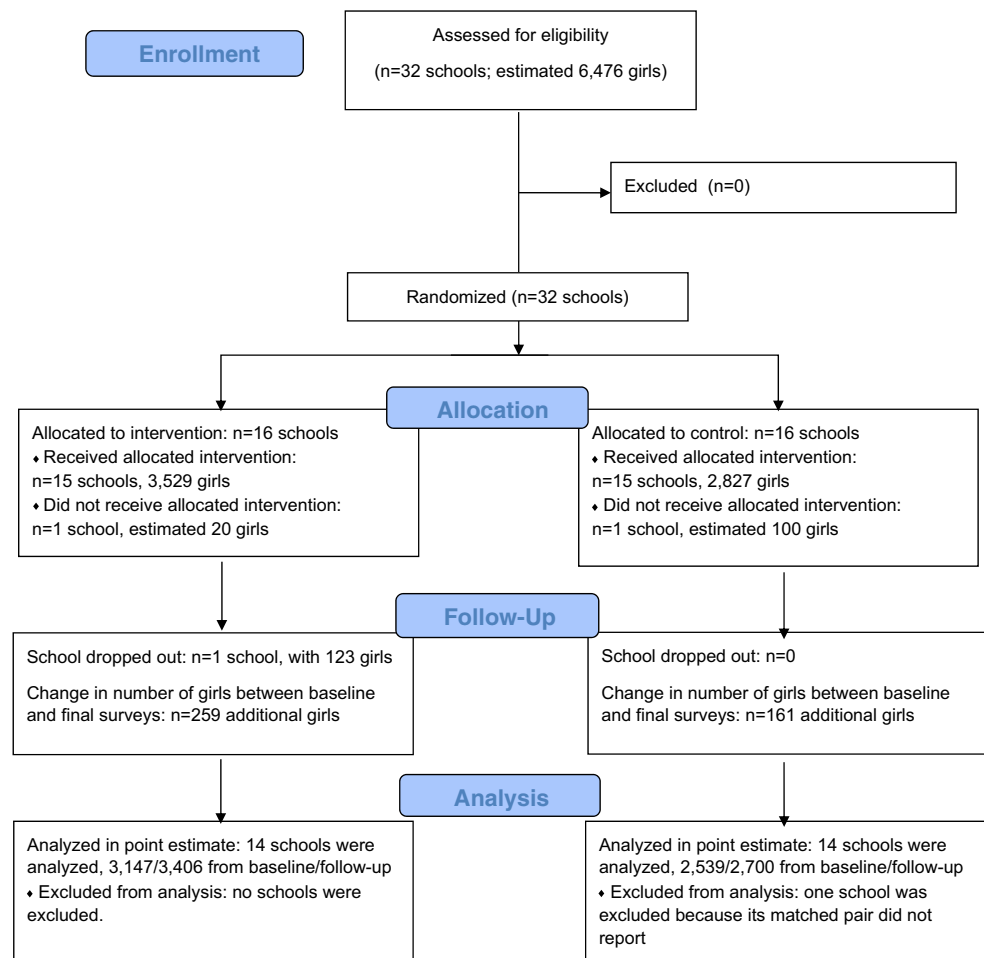
Enrolled participants were adolescent girls and boys, attending 30 primary schools in the informal settlements of Nairobi, Kenya, who agreed to undergo the trainings (Fig. 1). At baseline, the participants were in classes 5, 6, and 7 and were in 6, 7, and 8 at follow-up—thus, though an open-cohort design, many were the same participants at both times. Age of girls in this study ranged from 10 to 16 with an interquartile of 12 to 14. Boys in this study were of a similar age distribution as the girls. See Table 1 for baseline covariate information about the intervention and control groups. The schools came from the following informal settlements: Korogocho, Huruma, Dandora, Kibera, and Mukuru. The schools were selected by the implementing partner, Ujamaa-Africa, for the schools' location in the informal settlements and their school administrators' willingness to participate in a year-long CRT of the classroom-based intervention. Schools were also selected so as to be naïve to the intervention, having never received the trainings before.

Interventions

The intervention arm and SOC arm were both classroom-based curricula taught by instructors to students. Curricula for both interventions, IMPower for girls and 50:50 for boys, were developed specifically by No Means No Worldwide (NMNW)—a US-based NGO—and focused on the unique needs of younger adolescents in Nairobi. The intervention development process involved an extensive literature review, focus groups, and piloting dozens of classes among the target population.

Both arms were taught from January–March 2014. The intervention included six 2-h sessions, followed up with booster training sessions within 3 months. The SOC did not have a refresher course as part of its curriculum design. All sessions had ratios of approximately 1 instructor to 15 students. Both male and female instructors were chosen through an intensive process that ensured that they were respected members of their respective communities and had a background in, and passion for, preventing sexual violence. All trainers received extensive instruction by

Fig. 1 Participant flow diagram for this study. See the “Losses and Exclusions” section for more discussion



expert facilitators and participated in mock interviews and field-training exercises conducted outside of the study area. Trainers were required to pass a rigorous examination consisting of a written test, oral examination, and physical skills demonstration before becoming paid employees teaching the curriculum at intervention sites. New trainers were supervised by a more experienced trainer for their first year of teaching.

Girls' Intervention

The goal of the IMPower intervention for girls was to empower the girls to avoid risky situations, advocate for themselves, and, if needed, defend themselves against an attack. Learning methods included role-plays, facilitated discussions, and verbal and physical skills practice. In Session I, rapport, definitions, and objectives were established. Session II focused on personal awareness, self-efficacy, boundaries, and assertive communication skills. Session III was an introduction to physical defense. Session IV reviewed verbal and physical skills and focused on specific strikes using bags and mitts. Session

V focused on de-escalation and negotiation to avoid fighting and covered more advanced defense techniques, such as multiple or armed attackers. Session VI reviewed all previous sessions, and facilitators also encouraged women to share assault experiences. Survivors were linking to the Sexual Assault Survivors Anonymous program, which holds free weekly meetings in all the informal settlements where Ujamaa operates. This intervention is not radically different from the intervention examined in Senn et al. (2015).

Boys' Intervention

The boys' intervention, “50:50”, was specifically designed for 10- to 13-year-old boys and focused on promoting gender equality and developing positive masculinity. As with the girls, sessions included role-plays, facilitated discussions, and verbal and physical skills practice. Session topics included developing awareness about gender interactions and negative gender roles, identifying emotions, and skill building around courage and the use of verbal interventions in harassment or assault situations.

Table 1 Self-reported covariates of girls at baseline

Covariate	Intervention	SOC
Area		
Dandora	31 %	31 %
Huruma	7 %	6 %
Kibera	33 %	24 %
Korogocho	17 %	24 %
Mukuru	12 %	15 %
Class		
5	28 %	31 %
6	35 %	33 %
7	33 %	34 %
8	3 %	1 %
Missing	1 %	1 %
Age		
Mean (years—approximate)	12.3	12.4
Skipped meals		
Never	65 %	65 %
Rarely (1–2x in 4 weeks)	20 %	18 %
Sometimes (3–10x in 4 weeks)	11 %	13 %
Often (>10x in 4 weeks)	3 %	3 %
Missing	0 %	0 %
Alcohol use		
Never	91 %	93 %
Occasionally	8 %	6 %
Weekly	1 %	1 %
Missing	0 %	0 %
Ever raped		
No	90.2 %	90.6 %
Yes	9.8 %	8.9 %
Missing	0.0 %	0.5 %
Raped in previous year		
No	92.7 %	93.3 %
Yes	7.3 %	6.4 %
Missing	0.0 %	0.3 %
GSES^a		
Mean (scale 1–4)	3.1	3.1

Denominator is 2700 girls at 14 schools in control and 3406 girls at 14 schools in intervention

^a Among non-missing responses

Standard of Care

The standard of care group received a one-time 1.5- to 2-h life skills class, taught by NMNW trainers, covering a wide range of topics such as hygiene, food safety, and personal rights. All school-aged children who attend school typically receive this curriculum. Thus, participants in the intervention arm likely will receive the SOC at some point in their educational careers.

Ethical Considerations

This intervention was a behavior modification program with a low risk of an increase in harm due to the intervention. Surveys were anonymous, so incidences of sexual assault were only identified if the participants decided to disclose to the trainers or other research staff. Ujamaa-Africa instructors and researchers are trained to link students who disclose sexual assault to organizations such as Médecins Sans Frontières and to programs and services provided by Ujamaa-Africa.

Approval for the study in Kenya was provided by the Kenyan National Commission for Science, Technology, and Innovation (NACOSTI). The analysis provided by Stanford researchers received a non-human subject determination from the Stanford internal review board (IRB). Ujamaa-Africa obtained assent from all study participants.

Primary and Secondary Outcomes

Students in all intervention and SOC schools completed written baseline surveys during class in the first quarter of 2014, before beginning the training programs. The instructors read the questions to the students, and then the students were asked to mark their responses. No unique identifying information was requested on the surveys in order to provide anonymity around these very sensitive questions. Once the surveys were completed, the individual student placed her survey into a locked ballot box (Gregson et al., 2002). This level of anonymity meant that there was no way to link baseline surveys to outcome surveys for any particular adolescent. All schools, except one, completed outcome surveys in October 2014.

The primary outcome was change in self-reported incidence of sexual assault at the school level on an annualized basis. Though both boys and girls received training, outcomes were only measured on the girls in the study. At baseline, the question was stated as follows: “In the last 1 year, has anyone forced you against your will to have sex (penetration of your vagina, anus or mouth with a penis or another object)?” On the final survey the question was phrased: “Since you took the Self-Defense Classes, has anyone forced you against your will to have sex (penetration of your vagina, anus or mouth with a penis or another object)?”

There were some inconsistencies on some surveys in reporting on sexual assault. For example, a follow-up question regarding a perpetrator may indicate there was an assault even though the girl reported no sexual assault in the prior period. We considered six possible ways of adjudicating inconsistencies: five of the definitions produced qualitatively the same conclusions; only one of the definitions differed slightly by producing a non-significant result. See the *ESM 1* for detailed discussion of how these inconsistencies were resolved and the accompanying sensitivity analyses.

Secondary outcomes included the Generalized Self-Efficacy Scale (GSES), reported perpetrator, whether or not a victim disclosed the assault to anyone, and whom the victim disclosed to. The GSES was created to assess a general sense of perceived self-efficacy with the aim of predicting ability to cope with everyday hassles and ability to rebound from stressful life events. Positive correlations have been found with favorable emotions, dispositional optimism, and work satisfaction. Negative correlations have been found with depression, anxiety, stress, burnout, and health complaints (Gregson et al., 2002). The GSES has ten questions, each answered on an ordinal scale of 1 to 4. We analyzed the mean score across the ten questions. If fewer than ten questions were answered, we used the mean of the answered questions, provided at least seven were answered, as recommended by the GSES documentation (Schwarzer, 2014).

Exploratory questions regarding rates of disclosure of sexual assault and to whom the girls disclosed were asked on the survey to assess changes in patterns of disclosure (Table 3). Girls were asked to report the number of times they had been assaulted in the prior period (Fig. 2) and who assaulted them (see Table 4 in the ESM 1). We also asked if the girls used the skills learned in the intervention to prevent a sexual assault and which type of skills they used in order to stop the assault.

Type of Randomization

Prior to assignment to intervention or control, matched pairs were created using information on number of girls in the school, number of boys in the school, academic performance, public versus private school, location, materials used to construct the school, and materials used for the floor. The characteristics and assignments of the matched-pairs design are summarized in Table 2. Each school had an equal probability of being assigned to the intervention. The algorithm is described in detail in ESM 1.

Statistical Methods for Primary and Secondary Outcomes

As in all cluster-randomized trials (CRTs), statistical inference must take into account the impact of individual-level participants being more similar within a cluster than across clusters—that is, the intracluster correlation (ICC). The bootstrap method used for inference accounts for this correlation; see ESM 1 for a detailed discussion. The clustering also impacts how one should think about the meaning of the targeted estimand of the statistical inference. In this study, there is the additional complication in the study design that we used an open cohort—allowing study participants to exit and enter the study between the baseline and final survey periods.

Given the issues outlined above, one way to understand the estimates are as longitudinal changes on the cluster level—e.g., estimating the change in probability of sexual assault for

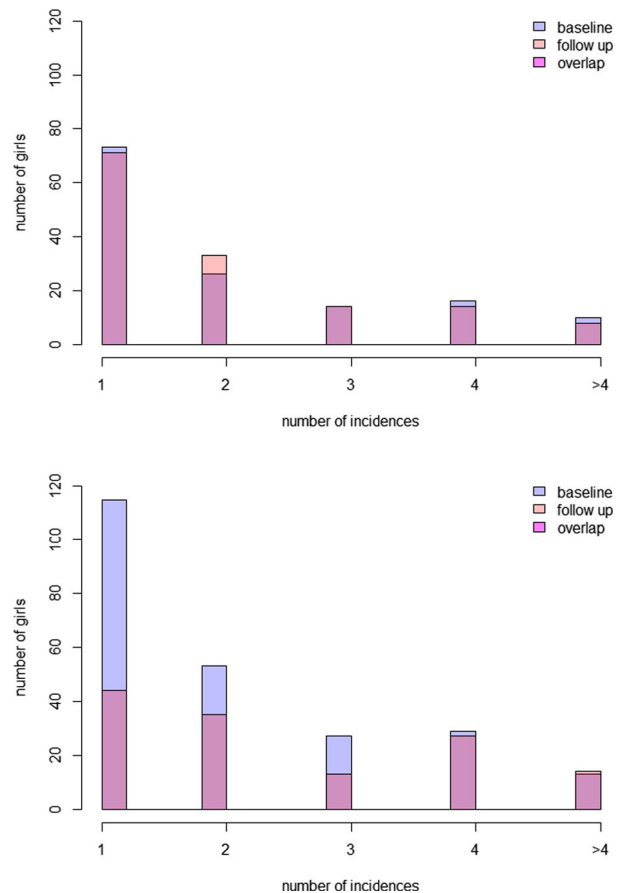


Fig. 2 This figure shows the changes in distributions of number of incidences between baseline and follow-up surveys and how these distributions either did not change much (SOC group) or reduced quite a bit in the lower counts (intervention group). As measured by the Pearson’s chi-squared statistic, the change in the intervention group is significant (*p* value approximately 0.016)

a school a year after receiving the intervention. Note that open-cohort estimands are useful for answering the question,

Table 2 Summary of school-level covariates at baseline from the 28 schools that reported at both baseline and follow-up

Covariate	Intervention schools	SOC schools
Area (count)		
Dandora	2	2
Huruma	3	2
Kibera	5	3
Korogocho	2	4
Mukuru	3	4
Number of girls in school (mean)	243	193
Ever raped (%) ^a	11.9 %	8.1 %
Raped in previous year (%) ^a	8.3 %	6.3 %
GSES (mean) ^a	3.1	3.1

GSES is the mean of ten questions, each on an ordinal scale from 1–4

^a Among non-missing responses

“What will happen to the rate of rape within a school a year after introducing the intervention, assuming natural turnover in the enrollment of the school?” Assuming that “community immunity” and other interference effects are minimal, a closed-cohort design would be better at answering the question, “What will happen to the probability of rape for a particular girl who received the intervention?”

Our study design and estimation procedure make use of a difference-in-differences estimator detailed in ESM 1.

The preintervention questionnaires asked about rape in the previous year, but the postintervention questionnaires asked about the period after the end of the trainings, a period of less than 1 year. This means that the girls were likely reporting on different exposure times (i.e., 12 months in the baseline, 9 months for the follow-up period). In order to account for this, we adopted a bootstrap resampling testing methodology (5000 resamples with resampling of girls done within school and time period) with an adjustment of the observed proportions in follow-up using a Poisson process approximation for the primary outcome. See the ESM 1 for more details on the estimation procedure. All analyses were conducted using R version 3.1.1.

Methods for Additional Analyses

To assess changes in disclosure patterns, we compared rates of disclosure and to whom the girls disclosed. We also assessed the distribution of the number of times victims reported being assaulted in the prior period, as well as who forced them. The study was not powered to assess these questions, so these analyses should be considered exploratory analyses. We report these rates using observed rates and compare using Pearson’s chi-squared.

Several sensitivity analyses were performed, assessing different assumptions and approximations used in this analysis. We include a permutation-based analysis of the primary outcome which only makes use of the researcher-controlled randomization. See ESM 1 for more detail.

Results

Participant Flow Diagram (Counts for Primary Outcome)

Losses and Exclusions

After randomization, two schools refused to participate in the study. One school declined to participate citing another NGO’s concurrent activity in the school. No reason was given for the other school. The first school (approximately 100 girls) was assigned to the SOC arm of the study. The second school (approximately 20 girls) was assigned to the intervention. These two schools were both from Dandora. It was

coincidental that these two schools had been matched together as part of the randomization process.

Academic administrators in a third school—an intervention school—were unable to allocate time for our trainers to administer the outcomes surveys. It appears the reason for the loss of this school’s follow-up was concern about time, rather than concern associated with the outcomes.

This was an open-cohort study, so individual student-participants were not tracked between baseline and follow-up surveys. Students could exit and enter the study between baseline and follow-up survey periods. While the number of student surveys completed in a given school tended to vary between baseline and follow-up periods, on average, schools had 91 % as many surveys at follow-up as they did at baseline. Three schools had more surveys at follow-up. Among the 28 schools providing complete paired data, four schools had 75 % or fewer at follow-up (two SOC schools and two intervention schools).

Baseline Data

See ESM 1 for details of how many observations were used in each analysis.

Estimates for Primary and Secondary Outcomes

At baseline, the girls in the intervention schools reported an annualized rate of rape of 7.3 %, while the girls in the SOC schools reported a rate of 6.4 %. Across all surveys at baseline, there was a report of 6.9 %. The point estimate for the study’s primary outcome, the risk difference in self-reported annualized rate of rape due to the intervention, was a reduction of 3.7 %, with an associated p value of 0.030. The 95 % confidence interval generated using the Poisson weighted bootstrap was a reduction of 0.4 and 8.0 %.

The point estimate of the change in mean GSES going from SOC to the intervention was an increase in GSES of 0.19 (baseline 3.1 on a 1–4 ordinal scale), with an associated p value estimated to be 0.0004. The 95 % confidence interval estimated from the bootstrap method was an increase between 0.08 and 0.39.

Ancillary Analyses

As an exploratory analysis, we investigated how the intervention was related to disclosure patterns. If a girl reported being sexually assaulted, we asked if she had disclosed it to anyone. In the baseline period, the intervention and SOC groups disclosed at nearly identical rates—63 % in the SOC reported and 62 % in the intervention group reported. At the follow-up period, the two groups differed quite a bit—52 % in the SOC and 65 % in the intervention, with a p value of 0.0510. The

SOC reported at lower rates in the follow-up period, but the intervention group remained unchanged.

To further investigate reporting patterns, we asked the girls who indicated that they had disclosed their sexual assault to whom they reported. The distribution of whom they disclosed to did not meaningfully change between SOC and intervention or between baseline and follow-up (as assessed by a Pearson's chi-squared test). Table 3 summarizes the observed distribution from the follow-up surveys.

To begin to quantify which kinds of sexual assaults are most impacted by the intervention, we investigated the number of times a girl was sexually assaulted in the prior period (Fig. 2). At baseline, the SOC and intervention groups were quite similar in their distribution. In the follow-up period, the distributions were quite dissimilar. The intervention group shifted away from many singleton reports, leaving the distribution weighted to higher number encounters. This is counter to the SOC group, which had very little change in its distribution. This observed shifting, in only the intervention group, is consistent with the intervention having a high impact on reducing "one-time" situations but perhaps having a lower impact on "high-risk" situations.

In terms of perpetrators, one-off incidences were more likely to be reported as being perpetrated by "other" or "friend/neighbor" while two or more were more likely to be "boyfriend" or "any relative." A chi-square test of a difference in these distributions produces a p value of 0.002.

In the intervention group at follow-up, 35 % of girls reported using the skills learned in the trainings to stop a sexual assault. Of these girls, they reported using only verbal skills 37 % of the time, only physical skills 23 % of the time, and both verbal and physical skills 40 % of the time.

Several sensitivity analyses were performed to assess the impact of assumptions used in this evaluation; see ESM 1 for details. The sensitivity analyses did not produce qualitatively different conclusions from those presented in this manuscript.

Discussion

Interpretation

This study evaluated the effect of an empowerment and self-defense training for girls, coupled with gender-equality training for boys, on reducing sexual assault among the girls participating in the intervention. We also considered the effect the training had on the perpetrator mix, self-efficacy, and skills most frequently used by the girls to prevent sexual assault. We estimate a risk difference of 3.7 % in the annualized rate of sexual assault ($p = 0.030$) for girls in the schools that participated in the intervention as compared to the SOC. In addition, there was a significant increase in self-efficacy (0.19, $p = 0.0004$). This cluster-randomized trial addresses the

Table 3 Of those girls who indicated they disclosed their sexual assault, this table reports the distribution to whom the girls reported

Follow-up period		
Who did you tell?	Intervention	SOC
Boyfriend	8 %	11 %
Any relative	17 %	17 %
Friend/neighbor	38 %	48 %
Authority figure	17 %	9 %
Other	20 %	15 %

The distributions at baseline were not substantially different

limitations of our previous quasi-experimental studies while confirming our prior findings that this intervention significantly reduced sexual assault among adolescent girls (Sarnquist et al., 2014; Sinclair et al., 2013). Importantly, the current study demonstrated the effectiveness of the interventions in a younger age group with a lower initial rate of sexual assault.

There have been calls by the international community for high-quality evidence on the effectiveness of programs to prevent sexual violence (Ellsberg et al., 2014a, b; Garcia-Moreno et al., 2005; Michau et al., 2015). This study is a unique addition to this body of literature due to its strong methodological design (a cluster-randomized trial with a large number of clusters) and focus on younger adolescents in sub-Saharan Africa. If the intervention proves to be durable, receiving the intervention at a younger age may decrease the risk of sexual assault as adolescents move into the highest-risk late teen years as well as across their lifespan. Even at this young age, however, the baseline incidence of sexual assault was nearly 10 %, suggesting that an even earlier age of intervention may be valuable.

Most of the previous studies on sexual assault (and broader GBV) prevention, as well as the current study, suggest that multipronged approaches, especially those that include modules on shifting gender norms, are necessary. For example, the IMAGE trial took a multipronged approach and showed that a structural intervention focused on gender issues and HIV prevention, combined with a microfinance program, reduced intimate partner violence in a South African cohort (Pronyk et al., 2006). In the case of IMAGE, the structural intervention ("Sisters for Life") had many elements in common with the intervention described here, including regular meetings with trainers to learn about topics such as gender roles and norms, domestic violence, and empowerment, although it was focused on a slightly older population (14–35 years of age) than our study. The 2014 SASA! study in Kampala, Uganda, showed that community mobilization with a focus on changing negative gender norms reduced both physical and sexual intimate partner violence (IPV) incidence as well as acceptance of such violence by both women and men across entire

communities (Abramsky et al., 2014). That intervention has many elements in common with our intervention, including a rigorous selection and training process where community leaders are identified and empowered to make a difference in their own communities. It is significantly different, however, in that the curriculum for our study was tightly defined and codified, whereas SASA! encourages the creation of different interventions based on local community needs. Another major violence (and HIV) prevention study in Rakai, Uganda, entitled “SHARE,” which focused on reducing IPV and HIV incidence through a combination of HIV care and community mobilization to improve IPV-related behaviors, also showed a decrease in physical (but not emotional) IPV, as well as a decrease in HIV (Wagman et al., 2015). That study was also in an older population (aged 15–59), but it further supports the need for multi-pronged approaches and curriculum specific to changing gender norms and relationships.

An important finding was the increase in reported self-efficacy among the intervention girls. We measured self-efficacy because we hypothesized that it is an essential intermediary outcome on the pathway to the longer-term impact of reducing sexual assault. This hypothesis was underscored by the two theories that drove the creation of the intervention, as both social learning theory and the health belief model include self-efficacy as a key component of behavior change (Bandura, 1977; Rosenstock et al., 1988). Our results show that our intervention was effective at improving self-efficacy, and they further support our hypothesis that increasing self-efficacy may be one effective mechanism to decrease sexual assault in these communities.

Limitations

This study made use of the planned expansion of Ujamaa-Africa’s program into new schools. The study was an add-on, meant to gain as much information as possible while minimally interfering with the natural development of the NGO and its mission. As a consequence, there are several major limitations to this study.

Surveys were designed, field tested, translated into Kiswahili, and implemented within the timescale of two months and were limited to no more than two pages and 30 min of classroom time. Thus, survey items were limited.

Also as a consequence of the constraints outlined above, the surveys were read to the class (unisex) by the instructors and completed individually by the girls and boys. The instructors were trained in how to administer questions in both English and Kiswahili. This large group format is suboptimal compared to the preferred one-to-one or small group interviewing. To ensure privacy and mitigate feelings of discomfort responding to sensitive questions, this survey did not collect uniquely identifying information at baseline nor on the final survey. The ballot box method was used, and

individuals’ surveys could not be linked between the baseline and follow-up periods. On the cluster level, however, the surveys represent longitudinal measurements.

Another challenge this study faces is that the course instructors (from both the intervention and SOC arms) were also tasked with deploying the survey to the same students they instructed. We believe this could have increased the potential for demand effects such as the intervention arm students having felt compelled to report they used the skills to prevent a rape and also may asymmetrically change reporting patterns. For example, girls in the intervention arm may have felt more comfortable reporting incidences to instructors who demonstrated great care for preventing sexual assault, as compared to students who were taught the SOC by the same instructors.

Here, we identify three limitations directly related to the experimental design. First, all measures were self-reported; we plan to measure biological markers such as pregnancy and sexually transmitted infections in future studies. Second, possible cross-contamination between schools and communities is possible, although distance was used in randomization to ensure that intervention and control schools were as geographically distant as possible. Third, the follow-up period was relatively short (9 months). Nonetheless, the large effect size and rigorous design of this study, as well as the focus on younger adolescents, support the further scaling and study of this intervention.

Generalizability

These interventions, IMPower and 50:50, were developed specifically to meet the needs of young adolescents in the informal settlements in Nairobi. Outcomes were only measured on girls, though boys are known to experience sexual assault (Mulawa et al., 2016). Thus, the generalizability of these findings may be limited to girls in other low-income country settings, and especially to high-risk areas, like these settlements. The interventions, however, have solid theoretical underpinnings in social learning theory and the health belief model (Bandura, 1977; Rosenstock et al., 1988), drawing from empowerment, gender relations, and self-defense manuals and best practices from the USA, Israel, and Canada, as well as other areas of sub-Saharan Africa. The authors believe that these interventions can easily be adapted to other, somewhat similar settings, such as the informal settlements in South Africa, and likely, with more significant adaptation, would also be relevant in less impoverished settings and in other regions. The intervention curricula are cataloged in detailed, referenced manuals, which should support the process of adaptation to novel settings.

The intervention is tailored to delivery in a classroom environment. There are potential benefits to this delivery that would not be present in an individual-based training

program (e.g., larger cross-section of student-age population, “group immunity”). It is also possible that there are instructor-level effects that may vary in different settings. For example, the Ujamaa-Africa instructors are highly specialized in delivering this curriculum and are chosen for their passion for preventing GBV; one might imagine that having primary school teachers provide the same curriculum may have a different impact. Other sexual assault prevention programs can be used for both in-school and out-of-school youth (Jewkes et al., 2014). Out-of-school youth may be even more of a vulnerable population than in-school youth.

Conclusions

This study showed that this intervention can significantly reduce sexual violence in a highly susceptible population and confirms that the intervention is effective in younger adolescents in whom the prevalence of GBV is lower. While the results of the current study replicate and expand upon our earlier findings in the same region of Kenya, they need to be replicated and scaled in other settings and in other countries.

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Compliance with Ethical Standards

Conflicts of interest Members of the Stanford evaluation team did not have their time compensated for by Ujamaa-Africa and do not have ongoing financial connections with Ujamaa-Africa. Drs. Mulinge and Githua have ongoing financial connections to Ujamaa-Africa. The instructors, and thus the survey interviewers, were employees of Ujamaa-Africa. Thus, the in-country data collection was funded by Ujamaa-Africa.

Ethical approval Approval for the study in Kenya was provided by the Kenyan National Commission for Science, Technology and Innovation (NACOSTI). All procedures performed in this study were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki Declaration and its later amendments or comparable ethical standards. This study is an evaluation of a preexisting program, already being delivered by Ujamaa, in schools in these communities. The study consisted of anonymous, two-page surveys completed at baseline and follow-up. The Stanford internal review board (IRB) did a preliminary review of this project and determined that this short, anonymous survey did not raise human subject research issues and therefore did not require a full review.

Informed consent Ujamaa-Africa obtained assent from all study participants.

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