JOURNAL OF
**ADOLESCENT
HEALTH**

Original article

Does Keeping Adolescent Girls in School Protect Against Sexual Violence? Quasi-Experimental Evidence From East and Southern Africa

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A B S T R A C T

Purpose: We examine the relationship between educational attainment in adolescence on young women's lifetime experience of sexual violence in Malawi and Uganda.
Methods: Exposure to Universal Primary Education policies in the mid-1990s serves as a natural experiment to estimate the causal effect of schooling on women's subsequent experience of sexual violence using an instrumented regression discontinuity design and Demographic and Health Survey data.
Results: We find a one-year increase in grade attainment leads to a nine-percentage point reduction ($p < .05$) in the probability of ever experiencing sexual violence in a sample of 1,028 Ugandan women (aged 18–29 years), an estimate which is considerably larger than observational estimates. We find no effect of grade attainment on ever experiencing sexual violence among a sample of 4,413 Malawian women (aged 19–31 years). In addition, we find no relationship between grade attainment and 12-month sexual violence in either country. Analysis of pathways indicates increased grade attainment increases literacy and experience of premarital sex in Malawi and reduces the probability of ever being married in both countries.
Conclusions: Keeping girls in school results in a number of benefits for young women; however, protects against lifetime experience of sexual violence only in Uganda. It is possible that overall higher grade attainment, particularly at secondary school levels is driving this effect in Uganda. More research on this relationship is needed, as well as on effective interventions, particularly those which can be taken to scale related to enhancing the quality and quantity of education.
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IMPLICATIONS AND CONTRIBUTION

This study used quasi-experimental methods to explore the effects of increased grade attainment on young women's experience of sexual violence in Malawi and Uganda. Increasing schooling resulted in a number of benefits, although it reduced lifetime sexual violence only in Uganda.

Sexual violence against girls and women is widespread—30% of women worldwide have experienced lifetime sexual or physical violence from an intimate partner and 7.2% have experienced nonpartner-forced sex [1,2]. Sexual violence is detrimental to individuals' health and well-being [3–7]. In low- and middle-income countries (LMICs), there is little evidence of what works to prevent sexual violence [8–11], with one of the few interventions proven effective at reducing sexual violence among adolescents being a school-based intervention focusing on empowerment and self-defense in Nairobi, Kenya [12]. Structural interventions, including education, are particularly promising due to their scalability and multisectoral nature.

Education may affect risk of sexual violence, defined as been physically forced into unwanted sex or unwanted sexual acts, through various pathways. Increased education may delay entry into the labor market and improve labor market opportunities including earnings and desirability of occupation. This in turn may increase economic security and reduce financial dependence on males, reducing the need for transactional or age-disparate sex, which are often linked to forced sex [13]. Improved labor force opportunities may also reduce exposure to exploitative or dangerous environments where violence occurs. In addition, increased education may delay premarital sex or partnership formation, which are risk factors for intimate partner sexual violence [14]. Better-educated women have partners that are also better educated and closer in age [15], and partner's increased education and educational parity between partners are both associated with decreased risk of intimate partner violence [16]. Finally, education offers an important opportunity to intervene early in a girl's life to increase self-esteem, aspirations, mental health, and improve sexual education. However, increased schooling may not always be protective against sexual violence, as teachers and school peers may also perpetrate violence, and girls may be at increased risk for violence while traveling to school [17,18].

Existing empirical evidence on the relationship between education and sexual violence is largely observational [17,19]. Thus, studies have been unable to conclude whether education has a causal impact on reducing the probability of experiencing sexual violence or whether education is a proxy for omitted characteristics including parental preferences, socioeconomic status, or other factors, which are correlated with sexual violence. In this study, we examine the relationship between grade attainment and sexual violence among young women in Malawi and Uganda, as well as pathways through which these impacts may work. We address the endogeneity of grade attainment using the implementation of Universal Primary Education (UPE) policies in the mid-1990s, which removed primary school fees in government schools in Malawi in 1994 and in Uganda in 1997, allowing us to use an instrumented regression discontinuity design (RDD). Other studies have used similar strategies to examine the causal effect of education on human immunodeficiency virus/AIDS, fertility, and fertility preferences in Africa [15,20–23].

Methods

Data

Data come from the 2004 and 2010 Demographic Health Surveys (DHS) in Malawi and the 2006 and 2011 DHS in Uganda [24–27]. DHS are cross-sectional nationally representative household-based surveys collected by ICF Macro (Fairfax, VA) in collaboration with host country governments. DHS implement standard violence modules, including information on experiences of sexual violence from partners and nonpartners, following strict ethical guidelines. The module is randomly asked to one woman per household aged 15 to 49 years in a subset of DHS households. The two countries were chosen based on availability of the violence module, timing of surveys, and expansion of UPE.

Outcomes

The main outcome in this study was sexual violence, created using three questions: (1) have you been physically forced into unwanted sex or unwanted sexual acts by your current husband/partner in the last 12 months and/or in your lifetime?; (2) have you been physically forced into unwanted sex or unwanted sexual acts by anyone other than your current husband/partner in the last 12 months and/or in your lifetime?; (3) was your first sexual intercourse forced? If a woman responded yes to any of these three questions, she was coded as having experienced sexual violence in her lifetime.

There were minor differences in how sexual violence questions were asked across countries and survey rounds. In the Malawi 2010 DHS, women were asked all three questions; however, question (2) was only asked about the last 12 months (not lifetime). In the Uganda 2011 DHS, women were asked questions (1) and (2) but not question (3). In the Malawi 2004 DHS and the Uganda 2006 DHS, women were asked all three questions.

We also explore pathways through which grade attainment could have affected lifetime sexual violence. First, we examined experience of any sexual violence in the last 12 months to see if attainment affects current experience of violence. To further elucidate pathways based on marriage dynamics, we explored the effect of grade attainment on reporting of premarital sex (constructed based on reported age at first sex and age at marriage) and probability of ever being married. Finally, to understand if grade attainment may have had long-term benefits on human capital accumulation—and thus potential pathways leading to skill accumulation and labor force arrangements—we explored effects on engaging in cash employment in the last 12 months and literacy.

Analytical strategy

In the case of endogeneity bias, where unobserved confounders may simultaneously affect educational attainment and the probability of experiencing sexual violence, ordinary least squares (OLS) estimates of the relationship between schooling and sexual violence are biased. To account for this endogeneity, we used the introduction of UPE policies, as natural experiments (i.e., exogenous shocks to grade attainment).

Historically, primary school fees were obstacles to school access for poor populations in sub-Saharan Africa [28]. Fee removal led to large increases in enrollment, particularly for girls [29,30]. We adopted an instrumented RDD, taking advantage of the fact that UPE implementation could be treated as a random event that allowed girls just below primary school exit age at policy implementation to extend their schooling with zero fees, while having little or no effect on girls just above primary school exit age. On average, girls exposed to UPE in Malawi attained 5.6 grades compared with 5.2 grades for girls not exposed, and girls exposed to UPE in Uganda attained 6.4 grades compared with 5.6 grades for girls not exposed (Table 1). Primary school completion significantly increased for girls exposed to UPE in both countries; however, secondary school attendance significantly increased for girls exposed to UPE in Uganda only (Table 1).

We assume respondents just above and below primary school exit age are similar on observed and unobserved characteristics and differed only in exposure to UPE. A plot of average grade attainment by birth cohort shows that while there are upward trends in education, there is a jump (i.e., a discontinuity) in average years of schooling induced by UPE in both countries (Figure 1). Figure 1 also shows country-level variation in the extent of fuzziness in the regression discontinuity, with Malawi

Table 1
Frequency of the sample exposed to UPE while aged 12/13 years (panel A) and descriptive statistics of key indicators of the analysis including bivariate analysis (panel B)

Panel A	Malawi				Uganda		
	Full sample	2010 sample	2004 sample		Full sample	2011 sample	2006 sample
No exposure to UPE at age 12/13	1,984	1,555	1,373		535	230	260
Exposure to UPE at age 12/13	2,429	874	611		493	263	275
N	4,413	2,429	1,984		1,028	493	535

Panel B	Malawi Full sample Mean (SD)	Malawi Treatment Mean (SD)	Malawi Control Mean (SD)	Malawi T versus C Pr(|T| > |t|)	Uganda Full sample Mean (SD)	Uganda Treatment Mean (SD)	Uganda Control Mean (SD)	Uganda T versus C Pr(|T| > |t|)
Background and education indicators								
Age at survey	24.25 (3.09)	23.06 (2.88)	25.72 (2.67)	***	23.97 (3.03)	22.34 (2.61)	25.35 (2.66)	***
Grade attained	5.43 (3.72)	5.61 (3.62)	5.21 (3.83)	***	5.98 (3.9)	6.4 (3.86)	5.62 (3.89)	**
Completed primary	.30 (.46)	.31 (.46)	.29 (.46)	*	.41 (.49)	.47 (.50)	.36 (.48)	***
Attended secondary	.21 (.40)	.20 (.40)	.21 (.40)		.30 (.46)	.33 (.47)	.27 (.44)	*
Sexual violence indicators								
Sexual violence (lifetime)	.34 (.47)	.35 (.48)	.34 (.47)		.40 (.49)	.35 (.48)	.44 (.50)	*
Sexual violence (12 months)	.14 (.35)	.14 (.35)	.14 (.35)		.23 (.42)	.20 (.40)	.26 (.44)	
Pathways indicators								
Premarital sex[a]	.14 (.35)	.16 (.37)	.12 (.33)	**	.11 (.31)	.14 (.34)	.09 (.28)	
Ever married	.95 (.23)	.92 (.27)	.97 (.16)	***	.90 (.30)	.85 (.36)	.94 (.24)	***
Cash employment (12 months)	.21 (.41)	.21 (.40)	.22 (.41)		.28 (.45)	.25 (.43)	.30 (.46)	
Literacy[b]	.60 (.49)	.62 (.49)	.58 (.49)	*	.52 (.50)	.54 (.50)	.50 (.50)	
N	4,413	2,429	1,984		1,028	493	535	

Mean values are weighted according to weights provided in the DHS.
[*] $p < .05$, [**] $p < .01$, [***] $p < .001$.
SD = standard deviation; UPE = Universal Primary Education.
[a] Missing values on 296 observations in Malawi and 126 observations in Uganda.
[b] Missing values on five observations in Malawi and 25 observations in Uganda.

having a sharper discontinuity than Uganda. Despite the less pronounced discontinuity in Uganda, we conducted a number of statistical tests to ensure that UPE is a viable instrument and found the RDD approach was appropriate (see First-stage RDD results).

In our RDD model, treatment (exposure to UPE) was assigned to respondents based on values of one measured variable, Z (birth year), using a sample of women born in a 3-year age band relative to UPE introduction [31]. In Malawi, respondents who were aged 13 years and younger at policy implementation in 1994 (born between 1982 and 1984) were assigned treatment status, and respondents who were 14 years and older at policy implementation (born between 1979 and 1981) were assigned control status. In Uganda, respondents who were aged 12 years and younger at policy implementation in 1997 (born between 1985 and 1987) were assigned treatment status, and respondents who were 13 years and older at policy implementation (born between 1982 and 1984) were assigned control status. Ages correspond to the official primary school ages in each country.

In East Africa in the mid-1990s, grade repetition, long-term absenteeism, and late entry into school were common, and so some respondents who were beyond primary school age were still in primary school at UPE introduction. Thus, we implement a fuzzy RDD and adopt an instrumented specification where the birth year becomes an instrumental variable (IV) for treatment status. The resulting model is a two-stage least squares regression where, in the first stage, grade attainment is predicted using a dichotomous indicator of exposure to UPE while aged 12/13 years or younger (the age at which girls would have completed primary school if they were on track). In this first-stage equation [1], we regressed G_i, grades attained for individual i, on Z_i, the randomly assigned instrument. In equation [2], the second stage,
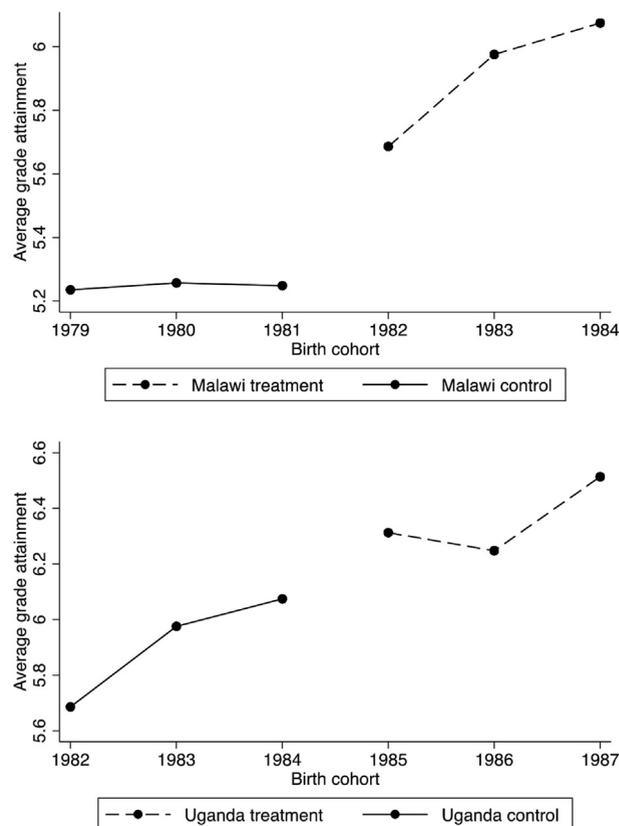
Figure 1. Grade attainment by birth cohort and exposure to Universal Primary Education in Malawi and Uganda. 2004 and 2010 Malawi Demographic Health Surveys; 2006 and 2011 Uganda Demographic Health Surveys.

we regressed Y_i, the sexual violence outcome, on the $\widehat{G}_i$, or the predicted value of G_i from the first stage.

$$G_i = \alpha_0 + \alpha_1 Z_i + \ldots \alpha_k X_k + v_i \tag{1}$$

$$Y_i = \beta_0 + \beta_1 \widehat{G}_i + \ldots \beta_k X_k + \varepsilon_i \tag{2}$$

We use linear probability models rather than probit because the lack of collapsibility of odds ratios with probit (or logistic) regression is problematic for the estimation of the average treatment effect [32]. Nonetheless, results are robust to the use of IV probit models (available upon request). In both equations (1) and (2), we control for survey wave, religion, ethnic group, and region of residence. Standard errors from regression models were clustered at the primary sampling unit level and descriptive statistics used sample weights.

The plausibility of the RDD approach depended on a number of assumptions. The first assumption is ignorability of the instrument (i.e., exposure to UPE was random). Plausibility of this assumption depends on the interval of birth cohorts considered (e.g., 10–12 and 13–15 years); correctly specifying an adequately narrow interval of ages is important to ensure treatment and control groups are comparable and differ only in their exposure to UPE. Second, there must be a nonzero correlation, or strong predictive power, of the instrument (UPE) on the treatment (grade attainment). Third, the exclusion restriction assumes no alternative pathways through which exposure to UPE could affect sexual violence. Based on the national roll out and the specificity of the policy, we argue that it is unlikely there exist alternative pathways through which UPE could affect sexual violence.

Ethics

Data used in this analysis are publicly available deidentified data collected by ICF Macro, who undertook all appropriate ethical review. Secondary analysis did not require ethics approval.

Results

Descriptive statistics

In Malawi, approximately 34% of women in the sample (n = 4,413) report lifetime sexual violence (35% of the treatment [n = 2,429] and 34% of control women [n = 1,984]; Table 1). In Uganda, approximately 40% of women in the sample (n = 1,028) report lifetime sexual violence (35% of treatment [n = 493] and 44% of control [n = 535] women; $p < .05$). Therefore, not only is the overall prevalence of lifetime sexual violence higher in Uganda, but the difference between treatment and control groups is significant. Contemporaneous measures (last 12 months) of these figures are also high (ranging from 14% to 23% in Malawi and Uganda, respectively).

To further contextualize these relationships, we created lowess plots, representing distributions between sexual violence and grade attainment computed through locally weighted regressions among our analysis sample (Supplementary Figure 1, top panel). In both countries, rates of sexual violence remain constant or slightly increasing through the first five to six grades and start to gradually fall after the end of primary school. In

Table 2
OLS association between grades of schooling attained and lifetime experience of sexual violence in Malawi and Uganda

Variables	(1) Malawi, lifetime sexual violence	(2) Uganda, lifetime sexual violence
Grades attained	.00 (.00)	−.02*** (.00)
Observations	4,413	1,028
R-squared	.02	.04

All models include controls for survey round, religion, ethnolinguistic background, and region. Robust standard errors are clustered at the primary sampling unit level.
*$p < .05$ (not indicated), **$p < .01$ (not indicated), ***$p < .001$.
OLS = ordinary least squares.

Uganda, the slope of sexual violence rates is steeper, dropping between grades 5 and 13 (end of primary and secondary school) and continues to fall for grades 14 and above (tertiary education). Among women with no education, rates of sexual violence are considerably higher in Uganda than those in Malawi, however by university level, rates converge. Graphically, rates of sexual violence remain constant throughout early adulthood in both countries; however, in Malawi, rates show gradual declines throughout our sample (Supplementary Figure 1, bottom panel).

Naïve OLS regression results

In observational analyses conducted with OLS (endogeneity of grade attainment not addressed), in Malawi, there is no significant association between grades attained and lifetime sexual violence (Table 2). In Uganda, a one-year increase in attainment is associated with a two-percentage point decrease in lifetime experience of sexual violence ($p < .001$).

First-stage RDD results

In Malawi, girls exposed to UPE at age 13 have an average of .4 more years of schooling attainment than girls not exposed to UPE at age 13 ($p < .001$) (panel A, Table 3). In Uganda, girls exposed to UPE at age 12 have an average of .79 more years of grade

Table 3
First-stage results for the association between exposure to UPE at age 12/13 and grades attained in Malawi and Uganda (panel A) and second stage results for the effect of predicted grades attained on lifetime experience of sexual violence in Malawi and Uganda (panel B)

Panel A, variables	(1) Malawi, grade attained	(2) Uganda, grade attained
Exposure to UPE at age 12/13	.40***	.79***
	(.11)	(.23)
Observations	4,413	1,028
F-statistic	12.73	12.1
R-squared	.09	.25

Panel B, variables	(1) Malawi, sexual violence lifetime	(2) Uganda, sexual violence lifetime
Grades attained (predicted)	.03	−.09*
	(.04)	(.04)
Observations	4,413	1,028

All models include controls for survey wave, religion, ethnolinguistic background, and region. Robust standard errors are clustered at the primary sampling unit level.
*$p < .05$, **$p < .01$ (not indicated), ***$p < .001$.
UPE = Universal Primary Education.

Table 4
Second-stage results for the effect of predicted grades attained on recent sexual violence (panel A) and pathway indictors (panel B) in Malawi and Uganda

Panel A, variables	(1) Malawi, sexual violence last 12 months						(2) Uganda, sexual violence last 12 months	
Grades attained (predicted)	.01						−.04	
	(.03)						(.03)	
Observations	4,413						1,028	

Panel B, variables	(1) Malawi, premarital sex	(2) Uganda, premarital sex	(3) Malawi, ever married	(4) Uganda, ever married	(5) Malawi, cash employment last 12 months	(6) Uganda, cash employment last 12 months	(7) Malawi, literacy	(8) Uganda, literacy
Grades attained (predicted)	.07[*]	.06	−.11[***]	−.13[***]	−.05	−.05	0.8[**]	.05
	(.03)	(.04)	(.03)	(.04)	(.04)	(.04)	(.03)	(.03)
Observations	4,117[a]	902[b]	4,413	1,028	4,413	1,028	4,408[c]	1,003[d]

All models run using linear probability models and include controls for survey wave, religion, ethnolinguistic background, and region. Robust standard errors are clustered at the primary sampling unit level. Values of predicted grades attained are obtained from predicted values of first stage regression using expansion of universal primary education as instruments (see Table 2).
[*] $p < .05$, [**] $p < .01$, [***] $p < .001$.
[a] Missing values on 296 observations.
[b] Missing values on 126 observations.
[c] Missing values on five observations.
[d] Missing values on 25 observations.

attainment than girls not exposed to UPE at age 12 ($p < .001$; panel A, Table 3). The F-statistics of grade attainment in the first-stage regressions are 12.7 and 12.1 in Malawi and Uganda, respectively. To use UPE exposure as an instrument for grade attainment, UPE must have actually increased grade attainment significantly in the full sample. In other words, $cov(Z_i, D_i) > 0$. For a single instrument and single endogenous regressor, the instrument is considered relevant if the F-statistic for the excluded instrument is greater than 10, which is the case for both countries [33].

Second-stage RDD results

We find no significant effect of predicted grades attained on sexual violence in Malawi in the second stage of the RDD analysis (panel B, Table 3). In contrast, in Uganda, a one-year increase in predicted grade attainment at the margin of primary and secondary school led to a nine-percentage point reduction in the probability of experience of sexual violence ($p < .05$).

We take a number of steps to control for the possibility that our results are driven by differences in respondent age and secular time trends between treatment and control groups. First, we use narrow age bands around the UPE change to ensure treatment and control samples were comparable (3-year bands on either side of the discontinuity threshold). Next, we ran a sensitivity analysis where we included age-fixed effects in the first stage; results are unchanged (available on request). However, age is highly correlated with UPE exposure and inclusion of age in the first stage caused both the IV and the age variables to be imprecise in the first stage. Finally, we explore the association between age at survey and our outcomes of interest. In Uganda, there is no significant relationship between age at survey and lifetime experience of sexual violence (Supplementary Table 1). In Malawi, there is a significant relationship between age at survey and lifetime experience of sexual violence; however, this is of less concern since there was no significant effect of school on lifetime experience of sexual violence in the RDD models in Malawi, thus suggesting that models are not conflating age and schooling effects.

A limitation is that respondents might have experienced sexual violence before exposure to UPE, thus making it difficult to rule out reverse causality. To address this, we exclude women from the treatment sample who had sexual violence before exposure to UPE based on reported age at first sexual violence (n = 37 in Malawi; n = 16 in Uganda). Results are unchanged, indicating that reverse causality is unlikely.

Second-stage results from supplementary analysis

Using the same RDD strategy, we find no significant effect of predicted grades attained on experience of sexual violence in the last 12 months (Table 4, panel A). We also explore pathways through which schooling attainment could have affected sexual violence. We find significant protective effects of predicted grade attainment on ever being married in both countries, however, also increases in premarital sex in Malawi (Table 4, panel B)—indicating that in Malawi, girls with higher attainment are more likely to delay marriage, while increasing likelihood of engaging in premarital sex. We also find significant positive effects of predicted grade attainment on literacy in Malawi; however, no effect on cash employment in the last 12 months in either country (Table 4, panel B). Note that for some of our pathway outcomes, age is significantly correlated with the outcome in bivariate regressions (Supplementary Table 1), thus, we cannot rule out the possibility that part of this effect is driven by the omitted age indicator.

Discussion

Our study is among the first to use a quasi-experimental study design to explore the effect of basic education on young women's experience of sexual violence. We use exposure to UPE policies in the mid-1990s in Malawi and Uganda as natural experiments and find that increased grade attainment is protective against sexual violence in Uganda. However, we find no significant effect of increased grade attainment on experience of sexual violence in Malawi or on recent (12-month) experience of sexual violence in either country, suggesting that schooling

impacts in Uganda might be working through protection against sexual violence at earlier stages in the life course.

This analysis demonstrates the importance of using quasi-experimental methods which address endogeneity when examining the effects of schooling. Differences in our RDD and OLS estimates indicate that OLS underestimates the magnitude of impacts in Uganda. Results suggest that policies targeted to increasing educational attainment for young women at high risk for leaving school may have broad long-term benefits. Effects may be particularly large because this study focused on young women at the margin of leaving school during the transition between primary and secondary.

Because we find reductions in sexual violence in Uganda but not Malawi, our results underscore the importance of understanding context when developing violence prevention programs. Risk factors at the individual, community, and societal levels may differ between the two countries. Reasons for the divergent findings could include differences in school quality, average educational attainment, and violence prevalence between the two countries. First, although inconclusive from our pathway analyses, there could be higher returns to schooling in Uganda that influence postschool labor and marriage market opportunities for young women. Second, it is possible that the marginal effect of UPE on schooling increases in Malawi was not large enough to induce protective effects against violence. Lowess plots demonstrated protective impacts against violence of schooling at the upper ends of the schooling distribution, whereas average educational attainment in our samples is at or near the flat or increasing point of the distribution. Therefore, it is possible that increases in schooling at the upper end of the distribution in Malawi may still be protective. Reducing barriers to secondary schooling (which still require fees) would likely have larger impacts. Indeed, our results show that more girls attended secondary school in Uganda than in Malawi, particularly in the cohorts exposed to UPE, which may help explain why we found reductions in violence in Uganda but not Malawi (Table 1). Third, differences in results could also be driven by differences in policy implementation across the two countries. For example, governments may differ in their ability to respond to rapidly increasing demand for schooling. Descriptive evidence suggests that the Uganda policy was more successful at extending average grades schooling and increasing the probability of secondary school attendance than the Malawi policy (Table 1). Finally, it is possible that impacts on sexual violence are working through pathways other than those that we were able to measure with secondary data.

There are a number of additional limitations worth mentioning. First, all our measures are self-reported, and many are of sensitive nature and subject to stigma, shame and social desirability bias, as well as measurement error due to recall bias. There has been substantial investment in survey methodologies to maximize disclosure in LMICs, including the use of audio computer-assisted interview techniques; however, it is not clear across settings and types of indicators which method is preferred [34,35]. Relatedly, research on adolescent girls and boys in Malawi has suggested that reporting bias (including inconsistent responses over time) could vary by education, particularly that reporting of premarital sexual behavior among girls may be downward biased due to low social acceptability [36]. As we use data from women who have largely completed schooling and employ an RDD approach, our analysis is likely to suffer from overall downward reporting bias, rather than differential

reporting bias by education status. A second concern is that our models could not capture nonlinearity in the relationships between educational attainment and sexual violence. Therefore, we cannot examine differences in relationship between grade attainment and sexual violence across the education distribution. A final limitation is that we cannot directly examine the role of school quality. If school quality fell simultaneously with the roll out of UPE, we may not see strong benefits of increased attainment.

Our study contributes to the small body of evidence on policies and interventions which may protect against sexual violence perpetrated against adolescent girls and young women [8,10]. Despite our mixed findings, these results contribute to a small but growing literature suggesting that schooling in particular can protect against diverse forms of violence. This link is also evidenced in rigorous evaluations of cash transfer programs, which reduce demand-side barriers to girls' access to education and have led to decreases in female age-disparate sex, transactional sex, and intimate partner violence in Kenya, Malawi, and South Africa [37–39]. Although few examples of rigorous research exist making these linkages, basic education remains a promising at scale platform to improve safe transitions for youth. More research causally linking education and violence is needed in LMIC settings.

Supplementary Data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.jadohealth.2016.09.010.

Funding Sources

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