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The causal effect of education on HIV stigma in Uganda: Evidence from a natural experiment

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A B S T R A C T

Rationale: HIV is highly stigmatized in sub-Saharan Africa. This is an important public health problem because HIV stigma has many adverse effects that threaten to undermine efforts to control the HIV epidemic.
Objective: The implementation of a universal primary education policy in Uganda in 1997 provided us with a natural experiment to test the hypothesis that education is causally related to HIV stigma.
Methods: For this analysis, we pooled publicly available, population-based data from the 2011 Uganda Demographic and Health Survey and the 2011 Uganda AIDS Indicator Survey. The primary outcomes of interest were negative attitudes toward persons with HIV, elicited using four questions about anticipated stigma and social distance.
Results: Standard least squares estimates suggested a statistically significant, negative association between years of schooling and HIV stigma (each $P < 0.001$, with t-statistics ranging from 4.9 to 14.7). We then used a natural experiment design, exploiting differences in birth cohort exposure to universal primary education as an instrumental variable. Participants who were <13 years old at the time of the policy change had 1.36 additional years of schooling compared to those who were $\geq$13 years old. Adjusting for linear age trends before and after the discontinuity, two-stage least squares estimates suggested no statistically significant causal effect of education on HIV stigma (P-values ranged from 0.21 to 0.69). Three of the four estimated regression coefficients were positive, and in all cases the lower confidence limits convincingly excluded the possibility of large negative effect sizes. These instrumental variables estimates have a causal interpretation and were not overturned by several robustness checks.
Conclusion: We conclude that, for young adults in Uganda, additional years of education in the formal schooling system driven by a universal primary school intervention have not had a causal effect on reducing negative attitudes toward persons with HIV.

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1. Introduction

While there is a general scientific consensus that socioeconomic status as measured by educational attainment correlates positively with health outcomes (Montez and Friedman, 2015), there has been considerably more debate about the link between education and HIV risk. Analyses of cross-sectional data have generated somewhat conflicting conclusions (de Walque et al., 2005; Hargreaves et al., 2008a; Hargreaves and Glynn, 2002; Hargreaves et al., 2008b). A more recent and growing body of work has demonstrated causal associations between greater educational attainment and lower HIV risk in several sub-Saharan African countries (Alsan and Cutler, 2013; Behrman, 2015; De Neve et al., 2015; Dupas, 2011). The mechanisms underlying this observed association may include increased exposure to HIV prevention messages and uptake of related HIV prevention activities, such as condom use, abstinence, and male circumcision (Agüero and Bharadwaj, 2014; Gummerson, 2013; Gummerson et al., 2013).

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Less clear is whether (lack of) educational attainment influences other important HIV-related outcomes, particularly negative attitudes toward persons with HIV. In many countries throughout sub-Saharan Africa, HIV is highly stigmatized (Genberg et al., 2009; Kalichman et al., 2005; Maman et al., 2009; Nyblade et al., 2003; Tsai, 2015). This is an important public health issue because, in general population samples, negative attitudes toward persons with HIV have been associated with reduced uptake of HIV testing and increased HIV transmission risk behaviors (Corno and de Walque, 2013; Delavande et al., 2014; Kalichman and Simbayi, 2003; Kelly et al., in press; Pitpitan et al., 2012). Among persons with HIV, internalization and anticipation of these negative attitudes are associated with isolation and depression (Fife and Wright, 2000; Link, 1987; Link et al., 1989; Simbayi et al., 2007; Takada et al., 2014; Tsai, 2014; Tsai et al., 2012) and may compromise HIV treatment adherence and engagement in care (Govindasamy et al., 2012; Katz et al., 2013; Sikkema et al., 2010; Tsai et al., 2013b, 2010).

In cross-sectional studies, education and HIV-related knowledge have been found to be inversely associated with HIV stigma (Babalola et al., 2009; Chiao et al., 2009; Girma et al., 2014; Stephenson, 2009), but the estimation strategies employed in these studies do not permit inference about the degree to which the observed association is causal. The hypothesized causal influence of education on HIV stigma is motivated by a larger body of research in political science and sociology in which formal schooling is thought to be one of the most important socializing agents in deepening one's commitment to the norm of tolerance (Prothro and Grigg, 1960). Consistent with this line of research, formal schooling has played a major role in weakening negative attitudes toward stigmatized “outgroups” in the U.S., including racial (Hyman and Sheatsley, 1964), religious (Rosenfield, 1982), and sexual minorities (Loftus, 2001), as well as increasing tolerance in general (Bobo and Licari, 1989; Knoke and Isaac, 1976). Overall, the link between formal schooling and tolerance for outgroup members has been described as “one of the most stable and consistent findings in empirical social research of contemporary American society” (Weil, 1985) (p.458). Quinley and Glock (1979), writing about anti-Semitism in the U.S., proposed a threefold mechanism of action through which education was thought to increase tolerance: providing students with greater knowledge about the historical, social, and political factors responsible for group differences; teaching students to recognize prejudice and its negative social impacts; and imparting cognitive skills, which in turn increase capacity to recognize prejudice. The dominant approach to HIV stigma reduction in the intervention literature has accordingly relied on psycho-educational strategies such as information provision, counseling, and testimonials (Brown et al., 2003; Sengupta et al., 2011; Stangl et al., 2013). However, many of these studies have employed designs that lack sufficient rigor to permit causal inference and in general have had varying degrees of success. Furthermore, some studies have shown that HIV stigma is actually increasing in countries like South Africa and Uganda (Chan et al., 2015b; Maughan-Brown, 2010) where average educational attainment has been rising, potentially calling into question the hypothesis that formal schooling reduces stigma. To date, the causal influence of education on negative attitudes toward persons with HIV still remains unclear.

To address this gap in the literature, we sought to determine the extent to which education is causally related to HIV stigma. The implementation of a universal primary education (UPE) policy in Uganda in 1997 provided us with a natural experiment to test this hypothesis. Specifically, the primary aim of this study was to estimate the causal relationship between additional years of schooling in the formal education system and negative attitudes toward people living with HIV.

2. Methods

2.1. Ethics statement

The ICF International Institutional Review Board approved all data collection procedures for the 2011 Uganda Demographic and Health Survey (UDHS) and the 2011 Uganda AIDS Indicator Survey (UAIS). The UAIS was additionally reviewed and approved by the Science and Ethics Committee of the Uganda Virus Research Institute and a review committee at the U.S. Centers for Disease Control and Prevention. Consistent with national guidelines, the UAIS received clearance from the Uganda National Council of Science and Technology. The specific analysis described in this article was approved by MEASURE DHS. Because this analysis was based on deidentified, public-use data, no additional approval was sought from the Partners Human Research Committee.

2.2. Data sources

The data for this analysis were pooled from the 2011 UDHS and UAIS. The UDHS is a publicly available, population-based survey implemented by the Uganda Bureau of Statistics with technical assistance from ICF International (through the MEASURE DHS project), the Ugandan Ministry of Health, Makerere University School of Public Health, and the Biochemistry Department of Makerere University. The UAIS is a publicly available, population-based survey implemented by the Uganda Ministry of Health with technical assistance from ICF International, the Uganda Bureau of Statistics, and the Uganda Virus Research Institute. Both the UDHS and UAIS employed a multistage stratified design with probabilistic sampling, with enumeration areas selected from a list of previously sampled clusters and a fixed number of households selected from within each cluster. In the UDHS, all women of reproductive age (15–49 years) who were either permanent household residents or visitors who slept there the night before the survey were eligible for participation; and in a subsample of one-third of the households, all men aged 15–54 years were eligible for participation if they were either permanent household residents or visitors who slept there the night before the survey. In the UAIS, all women and men aged 15–59 years who were either permanent household residents or visitors who slept there the night before the survey were eligible for participation. Among the 10,086 households selected for the UDHS, the response rate was 94 percent among women and 89 percent among men. Among the 11,434 occupied households selected for the UAIS, the response rate was 98 percent among women and 96 percent among men. Additional details regarding pretesting, field training, and survey implementation can be found in the UDHS and UAIS country reports (Uganda Bureau of Statistics & ICF International Inc., 2012; Uganda Ministry of Health, 2012).

2.3. Measures

The primary outcomes of interest were negative attitudes toward persons with HIV, which were elicited in the UDHS and UAIS with four questions administered to study participants who reported that they had “ever heard of an illness called AIDS.” These questions have been proposed as core indicators for monitoring the HIV epidemic by the Joint United Nations Programme on HIV/AIDS (UNAIDS, 2000) and have been incorporated – with minor variations by country – in DHS conducted throughout sub-Saharan Africa (Mishra et al., 2009). The first question is: “If a member of your family got infected with the AIDS virus, would you want it to remain a secret or not?” An affirmative response to this question indicates subjective awareness of negative attitudes toward persons with HIV and concern about the social consequences of a hypothetical disclosure. Typically this subjective awareness is assessed among persons with HIV and is sometimes termed “felt stigma” (Scambler and Hopkins, 1986) or “anticipated stigma” (what Link (1987) described as “expectations of rejection”). However, anticipated stigma can also be assessed among persons in general population samples (irrespective of HIV serostatus) using parallel questions (Steward et al., 2008; Visser et al., 2008). The remaining three questions elicit respondents' willingness to interact with persons with HIV under hypothetical scenarios. The questions are as follows: “Would you buy fresh vegetables from a shopkeeper or vendor if you knew that this person had the AIDS virus?”; “If a member of your family became sick with AIDS, would you be willing to care for her or him in your own household?”; and “In your opinion, if a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in the school?” Negative responses to these questions indicate a desire for social distance from persons with HIV (Link et al., 1987), which is often motivated either by a distaste for the symbolic meaning of HIV in a given cultural context or by instrumental concerns about HIV acquisition through casual transmission (Pryor et al., 1989). The construct validity of these questions is supported by their consistent correlation, in diverse settings throughout sub-Saharan Africa, with outcomes and predictors conceptually thought to be associated with HIV stigma, including: HIV testing (Hutchinson and Mahlalela, 2006; Koku, 2011; Leta et al., 2012; Sambisa et al., 2010; Weiser et al., 2006), HIV transmission risk behavior (Delavande et al., 2014), utilization of skilled birth attendant services (Turan et al., 2012), and the availability of effective HIV treatment (Chan et al., 2015a; Wolfe et al., 2008).

The exposure of interest was years of completed formal schooling. UDHS and UAIS participants were asked to describe their highest level of schooling attended (primary, lower secondary [“O-level”], upper secondary [“A-level”], tertiary, or university) and the highest class they had completed at that level. The number of years of schooling is then imputed in the UDHS and UAIS by assuming 7 years for completion of primary school and 13 years for completion of both primary and secondary school (MEASURE DHS & ICF International, 2013). For example if a participant reported attending secondary school but dropping out after two years, she would have been coded in the UDHS and UAIS as having attended nine years of schooling.

2.4. Statistical analysis

The Stata statistical software package was used for statistical analysis (version 13.1, StataCorp LP, College Station, Tex.). All analyses employed robust estimates of variance corrected for clustering at the level of the primary sampling unit (Froot, 1989; Rogers, 1993; Williams, 2000) and the sampling weights provided by ICF International. First, we estimated the association between education and negative attitudes toward persons with HIV using a conventional regression-based approach. We fitted linear probability models to the data, specifying participants' affirmative or negative responses to each of the four HIV stigma questions as the dependent variables and the number of years of schooling as the primary explanatory variable of interest. Estimates were adjusted for age, sex, ethnicity (Baganda tribe vs. other), marital status, religion (Catholic, non-Catholic Christian, and other), and an index of household asset wealth (Filmer and Pritchett, 2001). We also adjusted for data source (UDHS vs. UAIS) to capture any potential differences in the two surveys. Estimation was restricted to UDHS and UAIS participants born in the 1979 through 1989 birth cohorts to be consistent with the birth cohorts used in the instrumental variables analyses described below. We examined the predicted values of the outcome variables from these regression models to assess the extent to which our estimates might be subject to bias resulting from predicted outcomes lying outside of the [0,1] interval (Horrace and Oaxaca, 2006). We observed no predicted probabilities greater than 1, and between 0 and 10.5 percent of the predicted probabilities were less than 0, suggesting the bias would be minimal. Furthermore, the linear probability model coefficient estimates were consistent with the estimated odds ratios produced by fitting comparable logistic regression models to the data (Appendix Table 1). The estimated odds ratios could not be interpreted as approximate risk ratios because the rare event rate assumption (Greenland, 1987) did not hold for three of the four outcomes. The principal reason why we employed the linear probability model is because the regression coefficients – which are of primary interest rather than the predicted values of the outcomes – can be straightforwardly interpreted as marginal effects with no additional computation (Wooldridge, 2002).

We then employed an instrumental variables design to estimate the causal effect of education on negative attitudes toward persons with HIV. In the setting of our study, the natural experiment was Uganda's UPE policy, which commenced on January 1, 1997. UPE eliminated the cost of primary schooling (i.e., P1–P7, which most children complete by the age of 13) for up to four children per household of which at least two had to be girls; the policy was subsequently expanded in 2003 to include all children. Both direct (tuition) and indirect costs (such as school uniforms and informal, obligatory parental contributions through “parent–teacher associations”) were eliminated. The policy was remarkably effective: as documented in Deininger (2003) and Grogan (2008), UPE was associated with a dramatic and immediate decrease in the average amount of school fees and an accompanying increase in primary school attendance, particularly among girls and among children in rural areas. Because only children of primary school age were affected by UPE, a person's year of birth functioned as the assignment variable.

In practice, compliance with treatment was variable. Individuals born in and after 1984 (i.e., who were younger than 13 years old at the time of the UPE policy change) had a greater probability of receiving additional education compared to people who belonged to the 1983 and earlier birth cohorts. The probability was not equal to unity, however, because there may have been primary school-age children for whom no amount of school fee reduction would be sufficient to induce school attendance (e.g., due to extreme poverty, geographic isolation and the costs of transportation, or the need for child labor to support household agricultural livelihood activities). Moreover, late initiation of schooling and/or grade repetition is not uncommon in Uganda (Alderman et al., 2012; Nishimura et al., 2008). We therefore used an estimation strategy in which the treatment was specified as number of years of education completed, and birth cohort (i.e., being 13 years old or younger at the time of the UPE policy change) served as the instrumental variable.

Formally, we fitted local two-stage least squares (2SLS) regression models (Baum et al., 2003, 2007) in which the first-stage regression (Eq. (1)) specified years of schooling as a function of birth cohort membership:

$$W_i = \pi_0 + \pi_1 Z_i + \nu_i \tag{1}$$

where W_i is a continuous variable indicating the number of years of education attained by participant i at the time of the 2011 survey, and the Z_i are a series of dichotomous variables indicating whether participant i belonged to certain age categories at the time of UPE implementation in 1997. The second-stage regression (Eq. (2)) specified the HIV stigma outcomes as a function of the predicted values of W_i from the first stage

$$Y_i = \beta_0 + \beta_1 W_i + \varepsilon_i \quad (2)$$

where Y_i are the four dichotomous variables representing participants' affirmative or negative responses to the four HIV stigma questions described above.

Because this study design would not provide useful information about the causal effect of schooling for study participants whose treatment (additional schooling) could never be manipulated by the instrumental variable – "never-takers" and "always-takers", to adopt the terminology of Angrist et al. (1996) – we restricted estimation to just those UDHS and UAIS participants born in the 1979 through 1989 birth cohorts, or a bandwidth of five years above and below the age threshold. Doing so also reduced the possibility that secular trends in schooling are driving the estimates: the narrower the bandwidth, the less likely the estimates are prone to bias (Lee and Lemieux, 2010). Despite the relatively narrow bandwidth we chose, it remains important to adjust for the general effects of age. For example, it is possible that older persons hold more negative attitudes toward persons with HIV, and that the "age effect" could cancel out the "education effect." To adjust for the general effects of age, we fitted regression models in which we adjusted for age trends before and after the discontinuity. The first-stage model (Eq. (3)), in this case, was then:

$$W_i = \pi_0 + \pi_1 Z_i + \pi_2 Z_i \times (Age_{1997} - 13) + \pi_3 (1 - Z_i) \times (Age_{1997} - 13) + \nu_i \quad (3)$$

where π_2 and π_3 are coefficients representing linear approximations on either side of the assignment threshold, i.e., whether a participant was 13 years old or younger at the time of UPE implementation. Eq. (3) is similar to a regression-discontinuity design (Thistlethwaite and Campbell, 1960), which has had a rich history of use in psychology, statistics, and economics but has been infrequently used in the medical and public health literature (Cook, 2008; Moscoe et al., 2015). In brief, the regression-discontinuity design can be employed whenever a known decision rule assigns a treatment based on a known threshold value of a continuously measured assignment variable. Treatment assignment may be either deterministic (e.g., the probability of a county's receipt of Head Start funding assistance increases from 0 to 1 for counties whose poverty rates fall just below a programmatic threshold (Ludwig and Miller, 2007)) or probabilistic (e.g., the probability of receiving HIV antiretroviral therapy is greater for patients with a presenting CD4+ T-lymphocyte cell count less than 200 cells/μL (Bor et al., 2014)). The intuition guiding the regression-discontinuity estimator is that, due to random noise in the assignment variable, a person near the threshold has an essentially random chance of falling either just above or just below the threshold. This assertion implies that comparisons of outcomes for persons falling just above or just below the threshold can be considered free of omitted variable bias. In the setting of our study, Uganda's UPE policy provides the basis for such a design, by comparing outcomes for persons falling just above or below the age threshold.

Appropriate use of 2SLS, and the instrumental variables strategy more generally, rests on two assumptions. The first assumption is that, conditional on other covariates, the instrumental variable should only affect the outcome through the exposure of interest (Wooldridge, 2002). In the setting of our study, this assumption implies that, within a narrow birth cohort window and/or conditional on adjusting for secular trends, assignment to UPE should not have affected negative attitudes toward persons with HIV other than through its effect on increasing schooling. Although this assumption is not directly testable, we assessed its validity using a number of checks described in more detail below. The second assumption is that the instrumental variable should actually be instrumental, i.e., the UPE should have increased schooling in Uganda. The studies by Deininger (2003) and Grogan (2008) discussed above strongly suggest this assumption is valid. In addition, we also computed the first stage *F*-statistic on the Z_i as a formal test of instrument strength. A first-stage *F*-statistic <10 is generally thought to render the 2SLS estimator prone to weak instrument bias, which typically biases the coefficient of interest towards the null (Murray, 2006; Stock et al., 2002).

2.5. Robustness checks

First, to corroborate the first-stage regression results showing that the UPE policy change was associated with increases in schooling, we fitted two additional instrumental variables regression models to estimate the causal effect of additional schooling on literacy and HIV-related knowledge. Literacy was measured by asking participants the frequency with which they read a newspaper or magazine almost every day or at least once per week. HIV-related knowledge was measured by asking participants to respond to a series of seven true-or-false questions about HIV prevention and transmission. We standardized the seven variables to a mean of zero and standard deviation of one, then defined a summary HIV knowledge index as an equally weighted average of the seven *z*-scores (Kling et al., 2007), with greater values of the index suggesting greater HIV-related knowledge. Second, we adopted two approaches to aggregating the three social distance items. First, we calculated a simple index as the sum of the three variables so that the index variable has a range of 0–3, with higher values representing greater stigma. Second, again following Kling et al. (2007), we standardized the three social distance items to a mean of zero and standard deviation of one, then defined a social distance index as an equally weighted average of the three *z*-scores. When aggregated, the three items had a relatively low internal consistency (Cronbach $\alpha = 0.53$). Therefore the estimates from these regression models are presented in the Supplementary Appendix.

Third, we compared cohorts born before and after 1984 on "pre-treatment" characteristics – sex, ethnicity, and religion – so as to rule out any systematic differences in the birth cohorts that may have been correlated with exposure to UPE. We also assessed the sensitivity of our 2SLS results to the inclusion of these covariates (Lee, 2008). Fourth, because using a narrower cohort window reduces the potential for bias, we assessed the sensitivity of our core findings to progressively narrower bandwidths of four, three, and two years above and below the age threshold. We fitted these regression models without adjusting for linear birth cohort effects, given the unreasonable demands on the data. These estimation samples corresponded to UDHS and UAIS participants in the 1980–88, 1981–87, and 1982–86 birth cohorts, respectively. Fifth, given that UPE was intended to be specific to primary schooling, we conducted the same analyses using a binary indicator of primary school completion as the instrumented variable. Sixth, because UPE may have had greater impacts on girls, particularly those living in rural areas (Deininger, 2003; Grogan, 2008), we fitted the regression models disaggregated by sex and urbanicity to assess the extent to which any potential impacts of schooling on HIV stigma may have been subgroup-specific.

3. Results

Characteristics of the UDHS and UAIS participants in the 1979 through 1989 birth cohorts, stratified by the assignment threshold, are shown in Table 1. Using a conventional observational approach, each additional year of education had a consistent and negative association with the three social distance measures (each $P < 0.001$, with *t*-statistics ranging from 4.9 to 14.7) (Table 2). Compared to the mean outcome probabilities, these estimates implied a 10 percent

Table 1
Summary characteristics of UDHS and UAIS participants in the 1979–1989 birth cohorts.

	13 years or older at UPE		Younger than 13 years at UPE	
	N	Pct.	N	Pct.
Participant interviewed in UAIS	3042	63.6	4058	63
Negative attitudes toward persons with HIV				
Unwilling to care for HIV+ relative	279	5.8	413	6.4
HIV+ teacher should not continue to work	919	19.2	1172	18.2
Unwilling to purchase from HIV+ vendor	1031	21.6	1401	21.8
HIV in family should remain secret	2590	54.2	3691	57.3
Educational attainment				
No education	650	13.6	458	7.1
Incomplete primary	2139	44.7	2436	37.8
Complete primary	562	11.8	893	13.9
Incomplete secondary	928	19.4	1721	26.7
Complete secondary or higher	501	10.5	933	14.5
Reads at least weekly	815	17.1	1362	21.1
Female	3170	66.3	4380	68
Baganda tribe	757	15.8	1216	18.9
Religion				
Catholic	2011	42.1	2727	42.3
Non-Catholic Christian	2283	47.8	3101	48.1
Muslim or other	486	10.2	613	9.5
Age				
20–24 years	0	0	3625	56.3
25–29 years	2392	50	2816	43.7
30–34 years	2388	50	0	0
Married or cohabiting	3903	81.7	4349	67.5
Lives in rural area	3576	74.8	4521	70.2
Household asset wealth				
Poorest	1006	21	1145	17.8
Poorer	860	18	1172	18.2
Middle	787	16.5	1002	15.6
Richer	819	17.1	1079	16.8
Richest	1308	27.4	2043	31.7

UAIS, Uganda AIDS Indicator Survey; UDHS, Uganda Demographic and Health Survey; UPE, Universal Primary Education.

relative reduction in the three social distance measures for each additional year of education. The association between education and anticipated stigma was statistically significant but negligible in magnitude compared to the mean outcome probability (b = −0.007; 95% confidence interval [CI], −0.01 to −0.004).

Across all HIV stigma outcomes, older participants were less likely to have negative attitudes toward persons with HIV. The associations between other covariates and the stigma outcomes were less consistent. For example, women were less likely to agree that HIV-positive teachers should not continue to work but less willing to purchase from an HIV+ vendor and more likely to agree that HIV infection in the family should remain a secret. Similarly, study participants who had greater household asset wealth were less likely to agree with the three social distance measures but also more likely to agree that HIV infection in the family should remain a secret.

Comparing study participants across the assignment threshold on the pre-treatment covariates sex, ethnicity, and religion, we observed differences in gender (66% women younger than 13 years of age at UPE vs. 68% women 13 years or older at UPE; $\chi 2 = 3.5$, $P = 0.06$) and ethnicity (16% vs. 19% Baganda; $\chi 2 = 17.5$, $P < 0.001$) that were statistically significant but negligible in magnitude. There were no statistically significant differences in religion across the assignment threshold ($P = 0.52$). A kernel density plot did not reveal evidence of bunching at the threshold that would have suggested manipulation of age or birth cohort (Appendix Fig. 1).

Consistent with the findings of Deininger (2003) and Grogan (2008), birth cohort was associated with additional schooling: in the first-stage regression of years of education on birth cohort dummy variables and covariates, a comparison of the regression coefficients among participants who were <13 years old at the time of UPE implementation vs. those who were ≥13 years old suggests that the policy change was associated with a slightly greater than 1-year difference in total years of schooling (Fig. 1). The observed discontinuity in the graph is consistent with the first stage regression estimates, in which participants who were <13 years old at the time of UPE implementation had 1.36 additional years of schooling (95% CI, 1.20–1.53), corresponding to a first-stage *F*-statistic of 260 ($P < 0.001$). After adjusting for linear age trends before and after the discontinuity, the estimated first-stage regression

Table 2
Estimated associations between years of schooling and negative Attitudes toward persons with HIV, using least squares regression, among persons in the 1979–89 birth cohorts.

	Unwilling to care for HIV+ relative			HIV+ teacher should not continue to work			Unwilling to purchase from HIV+ vendor			HIV in family should remain secret		
	Coef.	(95% CI)		Coef.	(95% CI)		Coef.	(95% CI)		Coef.	(95% CI)	
Years of schooling	−0.006	−0.008	−0.004	−0.017	−0.019	−0.015	−0.019	−0.021	−0.016	−0.007	−0.010	−0.004
Participant interviewed in UAIS	−0.039	−0.053	−0.024	−0.069	−0.092	−0.045	−0.032	−0.056	−0.008	0.061	0.033	0.089
Female	0.001	−0.009	0.011	−0.028	−0.047	−0.009	0.058	0.038	0.077	0.125	0.097	0.152
Baganda tribe	−0.015	−0.024	−0.006	−0.003	−0.024	0.018	−0.005	−0.028	0.019	0.084	0.052	0.116
Religion												
Muslim or other	Ref			Ref			Ref			Ref		
Catholic	0.011	−0.002	0.024	−0.002	−0.029	0.026	−0.011	−0.045	0.022	−0.091	−0.132	−0.051
Non-Catholic Christian	0.008	−0.004	0.021	0.009	−0.018	0.036	0.000	−0.031	0.031	−0.072	−0.113	−0.032
Married or cohabiting	−0.002	−0.012	0.007	−0.003	−0.021	0.016	0.013	−0.007	0.034	0.005	−0.023	0.033
Age (per 5 years)	−0.010	−0.017	−0.003	−0.021	−0.035	−0.008	−0.029	−0.042	−0.016	−0.025	−0.043	−0.008
Lives in rural area	0.005	−0.008	0.018	0.020	−0.010	0.049	0.010	−0.020	0.040	0.047	0.005	0.088
Household asset wealth												
Poorest	Ref			Ref			Ref			Ref		
Poorer	−0.038	−0.062	−0.015	−0.016	−0.046	0.015	−0.013	−0.044	0.019	0.076	0.037	0.114
Middle	−0.038	−0.061	−0.016	−0.035	−0.067	−0.003	−0.046	−0.083	−0.009	0.094	0.053	0.135
Richer	−0.038	−0.062	−0.014	−0.036	−0.067	−0.004	−0.046	−0.082	−0.011	0.108	0.068	0.149
Richest	−0.045	−0.069	−0.020	−0.054	−0.091	−0.016	−0.062	−0.104	−0.020	0.127	0.080	0.173

CI, confidence interval; UAIS, Uganda AIDS Indicator Survey.

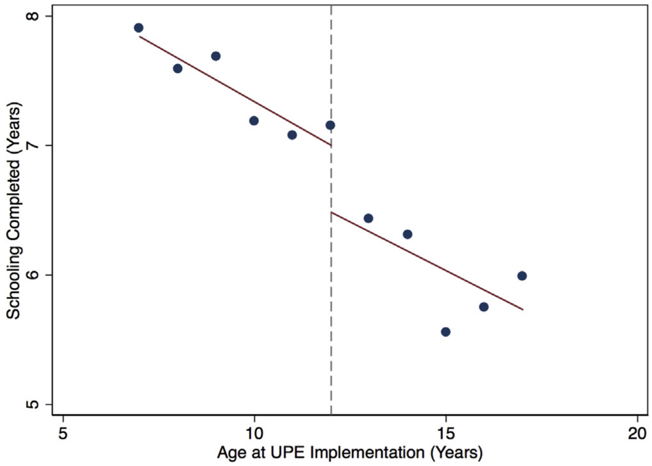

Fig. 1. Binned scatterplot showing years of schooling completed, According to age at implementation of universal primary education in 1997, among persons in the 1979–89 birth cohorts.

coefficient was reduced in magnitude but remained statistically significant (b = 0.42; 95% CI, 0.06–0.78), with a first-stage F-statistic of 5.35 ($P = 0.02$). When disaggregated by sex and urbanicity, the effect of UPE appeared to be greater for women compared to men, and no clear rural–urban difference was observed (Appendix Fig. 2). In the first stage regression estimates, women who were <13 years old at the time of UPE implementation had 1.52 additional years of schooling (95% CI, 1.33–1.72), while men had 0.88 additional years (95% CI, 0.57–1.19; $P < 0.001$ for the interaction term). Consistent with the figure, the rural–urban interaction was not statistically significant ($P = 0.90$).

Foreshadowing the instrumental variable estimates, a similar discontinuity was not observed for any of the HIV stigma outcomes (Fig. 2). Our instrumental variables (second stage) estimates are shown in Table 3. There was no statistically significant causal effect of education on any of the social distance measures, either in the simple models (P-values ranged from 0.33 to 0.50) or in the models adjusting for linear age trends before and after the discontinuity (P-values ranged from 0.21 to 0.69). Given the linear probability model specification, the estimated coefficients can be interpreted as marginal effects, e.g. each additional year of schooling was associated with a (non-statistically significant) 4.9 percentage point reduction in the probability of reporting that an HIV-positive teacher should not continue to work. We estimated a statistically significant causal effect of education on anticipated stigma, but the sign of the coefficient was positive (b = 0.024; 95% CI, 0.009–0.04; $P = 0.002$) and was not statistically significant after adjusting for linear age trends ($P = 0.29$). Six of the eight estimated regression point coefficients were positive (although small in magnitude), and the lower confidence limits of the estimated confidence intervals excluded large negative effect sizes. Regarding the only outcome for which the estimated regression coefficient was negative ("HIV-positive teachers should not continue to teach"), the lower confidence limit suggested that, at most, each additional year of schooling reduced the probability of the outcome by 1.7–4.9 percentage points. As discussed above, after adjusting for linear age trends, the first-stage F-statistic was <10, the threshold below which weak instrument bias may be an important source of bias towards the null. However, the fact that the magnitude of the estimates actually increased slightly when adjusting for secular cohort-related trends around the assignment threshold suggests that this bias is likely not material and certainly not likely to lead us to falsely exclude large negative effects.

These core findings were not overturned by several robustness checks. First, statistically significant causal effects of additional schooling were observed for both literacy and HIV-related knowledge (Appendix Table 2). Thus, even though we obtained null estimates of the causal effect of additional schooling on negative attitudes toward persons with HIV, these ancillary findings demonstrated that UPE did result in other important downstream impacts in the expected direction (specifically by its effects on increasing schooling). Second, we aggregated the three social

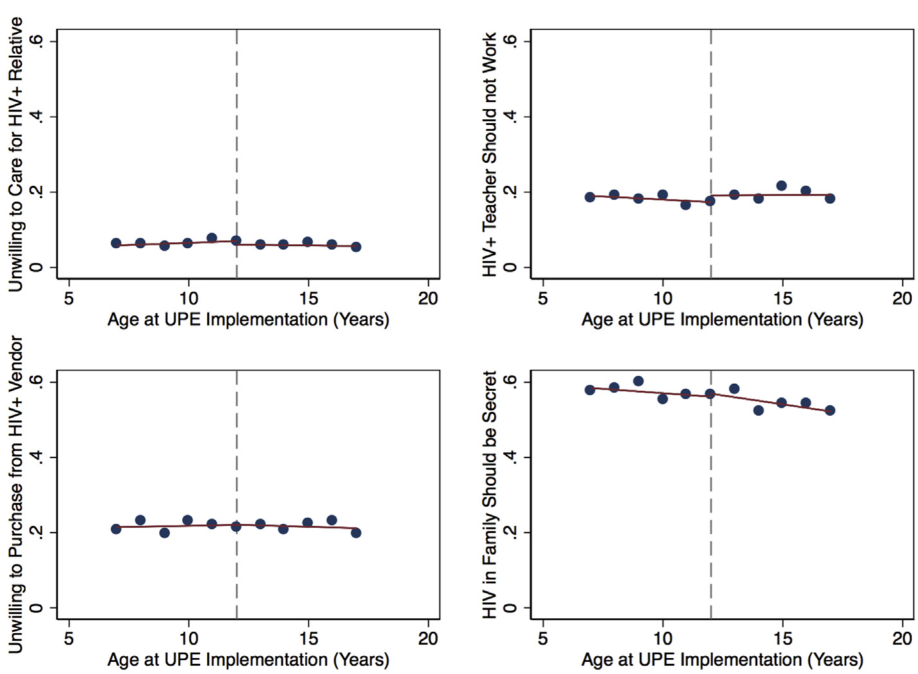

Fig. 2. Binned scatterplots showing negative Attitudes toward persons with HIV, According to age at implementation of universal primary education in 1997, among persons in the 1979–89 birth cohorts.

Table 3
Two-stage least squares estimates of the causal effect of years of schooling on negative Attitudes toward persons with HIV, among persons in the 1979–89 birth cohorts.

	Simple IV			Simple IV adjusting for linear age trends		
	Coef.	(95% CI)		Coef.	(95% CI)	
Unwilling to care for HIV+ relative	0.003	−0.003	0.010	0.018	−0.030	0.067
HIV+ teacher should not continue to work	−0.004	−0.017	0.008	−0.049	−0.127	0.028
Unwilling to purchase from HIV+ vendor	0.005	−0.008	0.018	0.017	−0.066	0.101
HIV in family should remain secret	0.024	0.009	0.040	0.051	−0.044	0.1462

SLS, two-stage least squares; CI, confidence interval; UAIS, Uganda AIDS Indicator Survey; UDHS, Uganda Demographic and Health Survey.
This table represents the abbreviated output from a total of eight regression models. Each row in column 1 represents the output from a 2SLS regression model fitted to estimate the causal effect of additional years of schooling (using exposure to Universal Primary Education as an instrumental variable) on the outcome specified in the row header, with an additional dummy variable to adjust for data source (UDHS vs. UAIS). In these models, the first-stage F-statistic was 260 ($P < 0.001$). Each row in column 2 represents the output from a similar 2SLS regression model that also adjusts for linear age trends before and after the discontinuity. In the 2SLS models represented in column 2, the first-stage F-statistic was 5.35 ($P = 0.02$).

distance items into a single scale. Findings from these regressions were similar to the analyses in which the social distance items were specified as separate outcome variables (Appendix Table 3). Third, we observed no substantive changes in the estimates when the pre-treatment covariates sex, ethnicity, and religion were included in the regression models (Appendix Table 4). The insensitivity of the estimates to inclusion of pre-treatment covariates strongly suggests the validity of the assumption of local random treatment assignment. Fourth, when estimation was restricted to progressively smaller cohort windows around the assignment threshold, the estimates remained largely unchanged (Appendix Table 5). The insensitivity of the estimates to differing cohort windows suggests that our preferred specification (using a bandwidth of five years above and below the assignment threshold) is unlikely to be subject to bias.

Fifth, instead of years of schooling, we used primary school completion as the instrumented variable. As with years of schooling, a similar discontinuity was observed in the probability of primary school completion (Appendix Fig. 2). For example, the predicted probability of primary school completion was 0.52 among participants who were 10 years old at the time of UPE implementation compared to 0.39 among participants who were 15 years old. The effect of UPE on primary school completion appeared to be greater for women: women who were <13 years old at the time of UPE implementation had a 15.6 percentage point increase in the probability of primary school completion (95% CI, 13.2–18.0), while men had a corresponding 7.4 percentage point increase (95% CI, 4.0–10.8; $P < 0.001$ for the interaction term). Compared to years of schooling, the first-stage F-statistic for primary school completion was slightly lower: $F = 170$ ($P < 0.001$), or $F = 3.94$ ($P = 0.05$) after adjusting for linear age trends before and after the discontinuity. The pattern of instrumental variables estimates revealed a similarly null effect on HIV stigma (Appendix Table 6). Finally, we fitted instrumental variables regression models disaggregated by sex and urbanicity (Appendix Table 7). Although UPE had a greater effect on girls' schooling, we did not observe a comparably larger effect of education on stigma among the women in the sample. If anything the pattern of estimated regression coefficients suggested a larger effect of education on HIV stigma for men, but the wide confidence intervals precluded definitive conclusions. Thus, even in the subgroup that benefited the most from UPE, no statistically significant impact on HIV stigma was observed.

4. Discussion

In this population-based, cross-sectional study, we used a natural experiment to show that additional schooling, resulting from implementation of UPE in Uganda, did not reduce negative attitudes toward persons with HIV. A conventional observational approach would have generated the opposite finding: that additional schooling has a strong, statistically significant negative association with negative attitudes toward persons with HIV. Such conclusions have been reported previously (Babalola et al., 2009; Chiao et al., 2009; Girma et al., 2014; Stephenson, 2009). In contrast, our instrumental variables estimates have a causal interpretation and were not overturned by several robustness checks. Given the lower limits of the estimated confidence intervals, we believe our study convincingly rules out the possibility that additional schooling has yielded *large* improvements in negative attitudes toward persons with HIV. Thus, our null findings were not simply driven by lack of statistical power.

4.1. Limitations

Our null findings may appear to be at odds with much of the intervention literature. In a recently published systematic review, Stangl et al. (2013) identified 48 studies of HIV stigma interventions. Of these, four-fifths employed some type of educational approach, while another two-thirds incorporated an element of skill building. Given that an information-based approach to stigma reduction is so dominant among studied interventions, how do we reconcile our null findings with the literature? We suggest there are four potential explanations. First, while HIV stigma in Uganda may be driven by ignorance, the questions included in the UDHS and UAIS may be measuring the construct of stigma with error. For example, in a field test conducted in Tanzania, Yoder and Nyblade (2004) found that the wording of these questions confused many participants. Furthermore, because these questions describe hypothetical scenarios, they are subject to social desirability bias (Nyblade et al., 2005; Yoder and Nyblade, 2004). These forms of measurement error may be correlated with education (i.e., persons with less education may be more prone to confusion or answering questions in a socially desirable manner), biasing our estimates towards the null. However, if measurement error were a plausible explanation for our findings, a similar bias should have also been observed in the estimates from the conventional regression models. Instead, the least squares estimates suggested a strong, statistically significant, negative association between schooling and negative attitudes toward persons with HIV – providing some reassurance that measurement error is not a likely explanation for the null findings.

A second possibility is that HIV stigma in Uganda may be driven by ignorance but one would not necessarily expect formal schooling to weaken it. However, not only would this stand in contrast to dominant models of education and socialization (as summarized in the introduction), corroborative evidence from other fields suggests important spillover impacts of formal schooling on health-related behaviors and disease self-management (Goldman and Smith, 2002; Phelan et al., 2010). Consistent with this line of inquiry, we demonstrated a positive causal effect of additional schooling on HIV-related knowledge. Therefore, we believe it unlikely that, if ignorance were a key driver of HIV stigma in Uganda, additional schooling would have no ameliorating impacts.

A third possibility is that additional schooling would reduce HIV stigma if ignorance were a key driver, but that ignorance is *not* – as has commonly been described in other contexts (Herek et al., 2002) – a key driver of HIV stigma in Uganda. If factors other than magical contagion (Rozin et al., 1992) and mistaken beliefs about HIV acquisition through casual contact are driving anticipated stigma and desires for social distance, then additional schooling might not be expected to reduce HIV stigma at all. These other factors might include the symbolic association between HIV and sexual immorality (Campbell et al., 2005), and the symbolic associations between HIV, disability, and death, as well as instrumental concerns about lack of reciprocation within local support networks (Tsai et al., 2013a). These drivers imply different intervention strategies. For example, in some countries, the increasing availability of HIV treatment has attenuated terminal illness fears and improved future-oriented behaviors and health investments (Baranov and Kohler, 2014; Baranov et al., in press; Chan et al., 2015a; Tsai et al., 2013a). In other countries, the availability of HIV treatment has paradoxically worsened stigma, especially in areas where sexual immorality concerns are driving negative attitudes toward persons with HIV (Roura et al., 2009). In neither of these circumstances would additional schooling be expected to attenuate HIV stigma.

A fourth possibility has to do with the interpretation of our findings. The 2SLS models specifically estimated the complier-average causal effect (Imbens and Rubin, 1997), also referred to as the local average treatment effect (Angrist et al., 1996; Imbens and Angrist, 1994), which is the average causal effect of schooling for the subgroup of study participants whose treatment (additional schooling) *can* be manipulated by the instrumental variable (birth cohort membership) ("compliers," in the terminology of Angrist et al. (1996)). The data would be uninformative about the average causal effect for persons who would never receive additional schooling because they were too old at the time of the UPE policy change ("never-takers") and would be similarly uninformative about the average causal effect for persons who would always receive additional schooling because they were very young at the time of the policy change ("always-takers"). Therefore, always-takers and never-takers were removed from the analysis. In effect, these restrictions limit generalizability of our findings to young adults and to the margin of primary schooling. This limitation is tempered by the fact that discontinuities were observed in the probability of primary school completion. It may still be that *higher education* has countervailing effects on HIV stigma, but the local average treatment effect estimated in our study cannot prove or disprove this hypothesis.

4.2. Conclusions

Notwithstanding these interpretive possibilities, we conclude that, for young adults in Uganda, additional years of education in the formal schooling system driven by a universal primary school intervention have not had a casual effect on reducing negative attitudes toward persons with HIV. It is possible that formal schooling might not, depending on its specific curricular elements, be expected to weaken HIV stigma. However, we believe it more likely that lack of education is not a key driver of HIV stigma in Uganda. Our findings do not generically undercut the use of psychoeducational-based intervention strategies, but rather argue for careful consideration of the cultural context when designing and implementing interventions to reduce the stigma of HIV.

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Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.socscimed.2015.08.009.

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