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To educate a woman and to educate a man: Gender-specific sexual behaviour and HIV responses to an education reform in Botswana

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To educate a woman and to educate a man: Gender-specific sexual behaviour and HIV responses to an education reform in Botswana.

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Abstract

Education has been suggested as a 'vaccine' against HIV infection, but there is not much causal evidence behind this claim. Moreover, the few studies that exist on the impact of education on HIV infection and related outcomes have focused mostly on women, despite the fact that there are reasons to expect the responses of women and men to differ. This study analyses mechanisms that link education to HIV with a focus on gender differences, using data from four nationally representative surveys in Botswana. To estimate the casual effect, an exogenous one-year increase of junior secondary school is used, which in previous studies has been found to reduce HIV infection rates and increase incomes. The key finding is that women and men responded differently to the reform. It led to delayed sexual debut by up to a year among women and an increase in risky sex among men, measured by number of concurrent sexual partnerships and the likelihood of paying for sex. The increase in risky sex among men is likely to be due to the reform's positive impact on income. The school reform reduced the likelihood of HIV infection among women, but had no statistically significant impact on this variable among men.

Keywords: Education, HIV, Sexual behaviour, Gender

JEL codes: I12, I15, I25, I26

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1. Introduction

HIV infection rates are decreasing in most countries, but still, about 800 000 individuals in Eastern and Southern Africa became infected in 2017 (UNAIDS 2018a). Since provision of antiretroviral treatment to a rapidly growing number of HIV-positive citizens is a major challenge, the international efforts to deal with HIV instead need to focus more strongly on prevention. As a result, increased schooling has received a great deal of attention and is being described as a powerful prevention method (UNAIDS, 2015a, 2015b; United Nations, 2016; World Bank, 2016).

In this paper, we analyse the links between education and HIV infection with an emphasis on gender differences.[1] In both policy circles and the academic literature, the focus has previously been on the impact of education on women. Yet, according to theory, schooling may be linked to HIV through various channels with ambiguous aggregate effects, and these are likely to differ between women and men. Studying gender differences in sexual behaviour and HIV responses to schooling is important because the impact of education on men is of interest in itself and because they matter for HIV infection rates among both men and women, since men infect women and the other way around.

A simple model of sexual behaviour guides the empirical analysis. One channel of HIV transmission is adolescent sexual behaviour. Schooling is predicted to affect adolescent sexual behaviour mainly because of the increased cost of pregnancy for women. Pregnancy is costly because it usually results in school dropout. Another channel of HIV transmission is adult sexual behaviour. Education is predicted to increase income, which in turn reduces the value of transfers of money or gifts to or from sex partners. Since the transfers usually are from men to women, men might increase their spending on risky sex while women reduce their supply of risky sex. Increased income also makes having children more affordable. However, it increases the opportunity cost of children for women. As a result, women may increase or decrease risky sex

[1] Our analysis is limited to HIV transmission between men and women, since it accounts for the vast majority of HIV infections in Southern and Southeastern Africa. Although men who have sex with men comprise a high-risk group that contributes to the general epidemic, there is no nationally representative data on these individuals.

(that may lead to pregnancy) depending on the relative strength of the two effects. Men are expected to increase risky sex due to the income effect.

To estimate casual effects, we exploit an education reform in Botswana, a high HIV prevalence country. The education reform, implemented in 1996, shifted the 10th year of education from senior secondary to junior secondary school. Since a large share of students finish their schooling after junior secondary school (40% in our data), the reform constituted a dramatic increase in the number of students completing ten instead of nine years of education. The reform also included some curricula changes and a shift in the location at which students completed their tenth year.

Our identifying assumption is that nothing else affected children of relevant ages in 1996 in the same way as the school reform, i.e. there was nothing else that affected only those who would start secondary school after the implementation of the reform and not those who had already started or finished secondary school. Thus, as Borkum (2009) and De Neve et al. (2015), we assume that the education reform constituted an exogenous increase in the number of years of schooling.

We use the reform to estimate the impacts of secondary education on adolescent and adult sexual behaviour for women and men, measured by age at first sex, concurrency (having more than one simultaneous sexual relationship) and transactional sex (reception or payment of money or gifts in exchange for sex). Since income is likely to be an important channel for adult sexual behaviour, we also estimate the impact on occupational skill level; we do not have data on income. Lastly, we estimate the impact on the likelihood of contracting an HIV infection.

The data are from four Botswana AIDS Impact Surveys carried out in 2001, 2004, 2008 and 2013. The Botswana AIDS Impact Surveys are nationally representative cross-sectional surveys with an individual and a household component. The 2004, 2008 and 2013 surveys include HIV testing of household members over the age of 9 year.[2] One advantage of basing the study

[2] In 2008, all children more than 18 months old were tested, and in 2013, all children more than 6 weeks old were tested.

on the Botswana school reform is that previous research suggests it should offer enough statistical power to identify an impact of education on HIV prevalence among both women and men (De Neve et al., 2015).

The present paper builds on the work by De Neve et al. (2015), who estimate the impact of the Botswana education reform on the likelihood of HIV infection. However, there are some important differences between our analysis and theirs. First, we focus on gender differences and investigate a number of theoretically motivated links between education and HIV. Second, we use two different measures of exogenous exposure to the reform. Our simplest measure is similar to the one they use. It utilises information on whether an individual should have been exposed to the reform assuming that all pupils are in their perfect age-appropriate grade. However, as in other African countries, many children in Botswana are not in their age-appropriate grades. This implies that exposure to the reform increased gradually across birth cohorts, and consequently, there was no discrete jump in exposure to the reform between one birth cohort and another. Therefore, similar to Borkum (2009) and Chicoine (2012), and along the lines of Angrist and Lavy (2002), we also use the probability of exposure to the reform measured by the actual shares of girls and boys who were exposed to the reform in each birth cohort.

Third, we include respondents who were 10–20 years old in 1996, while De Neve et al. (2015) also include younger and older birth cohorts. The restriction to respondents who were about to start secondary school in a year closer to the reform year is particularly important, as we want to identify gender-specific responses. HIV infection depends not only on own sexual behaviour, but also on the sexual behaviour of partners. And sexual behaviour, in turn, depends not only on own intentions, but also on the intentions of potential sex partners. A high correlation between exposure to the reform among the respondents and their (potential) sex partners therefore implies that the responses to the educational reform among sex partners probably have a large influence on the estimated effect, making it difficult to identify gender differences. Yet, because of age differences between sex partners, exposure to the education reform was not the same for sex partners as for the respondents themselves. Women tend to have older partners who were less likely than them to have been exposed to the reform, while the opposite is true for men. The exception is young men, who tend to have partners of similar age. This implies that the reduced form effect of the reform on men's age at sexual debut will be affected by the response of young women to the reform, i.e. it becomes more challenging to find a partner if women delay their sexual debut. With a narrower sample, the response to increased education among sex partners has less of an influence on the estimated effect. However, there is a trade-off between statistical power and closeness to the reform year, not least since there is not a discontinuous jump in exposure to the reform, but rather a gradual increase.

Finally, compared with De Neve et al. (2015), we use two additional rounds of the Botswana AIDS Impact Survey. While they use only the 2004 and 2008 surveys, we also include those from 2001 and 2013. The 2001 survey does not have data on HIV infection, but it does contain information about other outcomes of interest. The 2013 survey lacks data on years of schooling, but has useful information on educational attainment.

Our key findings are as follows: The school reform delayed the sexual debut by about a year among women and possibly, depending on specification, a few months among men. It seems to have led to better jobs, i.e. an increase in occupational skill levels. Among men, there was an increase in concurrency and transactional sex, but no such changes were found among women. This impact on men's risky sexual behaviour is most likely due to increased occupational skill levels and the resulting increase in income. As for the overall impact of education on HIV incidence, education seems to reduce the likelihood of contracting HIV among women, while no statistically significant effect was found for men.

These results are supported by tests of the differences in responses between women and men, which are statistically significant for both measures of exposure for age at first sex, transactional sex and concurrency. However, there is no statistically significant gender difference in the effect on HIV infection rates. This is due to the relatively large standard errors of the estimates, particularly for men.

Our HIV results partly diverge from those presented by De Neve et al. (2015), who find a reduced HIV infection rate also among men. We show that their finding is driven by the inclusion of more age cohorts in the estimation sample.

Our study contributes to the literature on the potential links between education or schooling and sexual behaviour in developing countries. As documented by Hardee et al. (2014), a dozen of studies find a negative association between schooling and the risk of HIV infection in sub-Saharan Africa. A few of them employ strategies to identify a causal effect of education on HIV (Alsan and Cutler, 2013; Agüero and Bharadwaj, 2014; De Neve et al., 2015; Durevall et al., 2019). Among these, De Neve et al. (2015) provide the most convincing evidence of a casual effect, and it is the only study that includes both men and women. Using the Botswana school reform as an instrument, they find that one more year of secondary schooling reduced the risk of HIV infection by 8 percentage points (12 for women and 5 for men), which should be compared with a baseline prevalence rate of 25.5%. Among the other studies, Alsan and Cutler (2013) only investigate age at first sex among women in Uganda and link age at first sex to HIV infections with model simulations. Agüero and Bharadwaj (2014) analyse and find an impact on Zimbabwean women's number of sex partners and HIV knowledge, but no statistically significant effect of education on HIV infections.[3] And Durevall et al. (2019) fail to find a causal effect of current school attendance among young women in South Africa.

Our study also contributes to the literature on the impact of education on related issues. Chicoine (2012) finds that schooling delays marriage and increases early use of modern contraceptives among women in Ethiopia. Several studies find that schooling reduces or delays child bearing (Breierova and Duflo, 2004; Osili and Long, 2008; Baird et al., 2011; Chicoine, 2012; Duflo et al., 2014; Dinçer, 2016; Chicoine, 2016; Keats, 2018). Yet another finding is that increased schooling is likely to raise incomes (Borkum, 2009), which in turn has been found to reduce sexual risk-taking among women and increase such

[3] They argue that this is due to a lack of statistical power.

behaviour among men (Kohler and Thornton, 2011; Robinson and Yeh, 2011; Burke et al., 2015).

The following section outlines the theoretical framework, while Appendix I gives a detailed description of the formal model. Section 3 describes the education reform and Section 4 provides information about the data and the empirical approach. Section 5 reports the results and Section 6 concludes the paper.

2. Theoretical framework and hypotheses

We investigate two specific links between education and HIV infection by gender: adolescent sexual behaviour and adult sexual behaviour. To this aim, we have developed a simple model to guide the empirical analysis (see Appendix I). Here we describe the model intuitively, relate it to earlier economic models of education and HIV transmission, and discuss empirical support of model assumptions and predictions.

Economists typically assume that people engage in activates if the marginal benefits of doing so exceed the marginal costs. Models of risky sex and HIV infection are no exception (Philipson and Posner, 1995; Kremer, 1996; Magruder, 2011; Greenwood et al., 2013; Duflo et al., 2015; Yao, 2016).[4] Initially, the models of risky sex did not distinguish between men and women and were therefore criticised for lacking a gender perspective (Christensen, 1998). Examples of single-gender models are Philipson and Posner (1995) and Kremer (1996). Some later models bring in the gender dimension but consider only women, treating male partners' behaviour as exogenous (Duflo et al., 2015; Yao, 2016). The general equilibrium model by Greenwood et al. (2013) stands out in modelling the sexual behaviour of both men and women. In this model, transfers[5] from men to women 'clear markets' for different types of sexual relationships.

[4] In the models developed by economists, sexual behaviour is endogenous, and this is the main factor that distinguishes them from most epidemiological models of HIV transmission, as they usually treat sexual behaviour as exogenous (Greenwood et al., 2013).
[5] Transfers can encompass everything from an explicit exchange of money or gifts for sex to economic support and transfers between spouses.

Transfers from male sex partners are also important components of models focusing on women only (Duflo et al., 2015; Yao, 2016). There is compelling empirical evidence of transfers from male to female sex partners in Southern and Eastern African countries (Tawfik and Watkins, 2007; Stoebenau et al., 2016). Moreover, Robinson and Yeh (2011) document larger transfers for riskier sex and the use of transactional sex as a coping strategy to deal with income shocks in Kenya. In our model, riskier sex implies higher transfers. A larger transfer for riskier sex implies that men have a higher preference for risky sex and/or that they respond less than women to the risks involved in such behaviour. There are a couple of explanations for why this might be the case. Men will typically bear little or none of the costs of unwanted pregnancies. In addition, norms of masculinity usually endorse risk-taking (Barker and Ricardo, 2005). Arunachalam and Shah (2013) show that there is a price premium on risky (condomless) sex in sex markets even if men are concerned about and respond to risks, as long as they respond less than the women.

Both Duflo et al. (2015) and Yao (2016) make a distinction between committed and casual sexual relationships in adolescence. Committed sexual relationships have a higher likelihood of pregnancy (and pregnancy and marriage are important aims of the relationships), while casual sexual relationships have a higher risk of HIV infection (transfers from male partners is an important motive for these relationships). Even if the probabilities of pregnancy and HIV infection differ between the types of relationships, unprotected sex can result in both outcomes in both types; in fact, Yao (2016) finds that pregnancy motives in committed sexual relationships are likely to be an important driver of the HIV epidemic.

Our model includes the most relevant aspects of the above-mentioned models, with a focus on gender-specific hypotheses. Men and women engage in risky sex if the perceived marginal benefit of doing so exceeds the perceived marginal cost. However, the costs and benefits are likely to differ between the genders because women bear a higher cost of pregnancies, because the utility of risky sex could differ between the genders and because any transfers involved are from men to women. Thus, risky sex yields direct utility but also leads to transfers from men to women and increased probabilities of HIV infection and pregnancy. One potentially important simplification is that we consider only one type of risky sex, i.e. we do not distinguish between relationships with a high risk of HIV transmission and those with a high probability of pregnancy.[6]

The model has two periods, adolescence and adulthood, with potentially important differences in the impact of education on risky sex. We describe these briefly here:

Adolescent sexual behaviour

A clear prediction is that adolescent women have an incentive to reduce unprotected sex to avoid pregnancy during the extra school year. There is ample empirical support that pregnancy is followed by school dropout (Meekers and Ahmed, 1999; Bandiera et al., 2017). Getting pregnant during the extra time spent in school is costly both because women who drop out leave without a junior certificate and because they miss any potential returns to the extra year. There are studies showing that increased education is likely to delay childbearing in African and other countries (Osili and Long, 2008; Baird et al., 2011; Chicoine, 2012; Duflo et al., 2014).

There could also be other effects of being in school. Black et al. (2008) discuss an 'incarceration effect', which encompasses the motive to avoid pregnancies while in school, but it also includes additional effects that school attendance can have on sexual behaviour. One example is the impact on social networks, which in turn could affect sexual behaviour and partner choice.

The model in the appendix points to yet some other potentially important predictions: If being in school reduces the available current income, the value of transfers increases. As mentioned earlier, Duflo et al. (2015) and Yao (2016) suggest that additional years in school can therefore be associated with a relative shift from committed to casual sexual relationships for women. Unfortunately, we are not able to study this possibility with our data. The

[6] We do not consider this distinction since we are not able to empirically distinguish between the two types of sexual relationships.

prediction for men is that reduced current income should lead to fewer sexual relationships that involve transfers to women.

Adulthood sexual behaviour

In the model, the sexual behaviour of adults is affected through improved income. Borkum (2009) has previously shown that incomes responded to the education reform in Botswana, which makes the link between education and HIV transmission that works through income relevant. The impact of higher income on adult sexual behaviour differs by gender. Increases in income make men able to afford more transactional sex, while the transfers from men to women become less valuable for women. This should increase sexual risk-taking among men and decrease it among women. Higher income also has implications for the motive to have children. With higher income, women and men can afford more children. However, it also implies an increased opportunity cost of childbearing and childrearing for women. Since the opportunity cost is arguably higher for women than for men, women should reduce their engagement in risky sex more than men in response to the change in opportunity cost. Some studies have suggested such a gender-differentiated sexual behaviour response to income shocks in the region (Bryceson and Fonseca, 2006; Kohler and Thornton, 2011; Burke et al., 2015).

While not being part of the model, an increase in expected income has been suggested to also increase the value of expected life years. This should make both women and men reduce their risky sexual behaviour in a context where the risk of HIV infection is high and contracting the virus can reduce life expectancy. Although the risk of HIV infection is high in the Botswana context, the extent to which HIV infection reduces life expectancy has decreased since the expansion of anti-retroviral treatment from 2004 and onwards. Anyhow, the life-expectancy effect should be similar for men and women.[7]

[7] Education could also affect sexual behaviour and HIV infection through improved knowledge, but again there is no reason to expect the impact to differ between women and men.

2.1 Testing of specific hypotheses

In accordance with the model in Appendix I, we investigate two specific links between education and HIV: adolescent and adult sexual behaviour. We test adolescent sexual behaviour using information on age at first sex. This information was collected retrospectively for all participants in the first three survey rounds. It should be noted that age at first sex is a limited description of adolescent sexual behaviour. For example, an individual can be committed to one partner or have several casual relationships, an aspect that we cannot investigate with the data at hand.[8]

We consider two types of adult sexual behaviours, both of which have been suggested as important drivers of the HIV epidemic: concurrent sexual relationships, here called concurrency, and reception or payment of money or gifts in exchange for sex, i.e. transactional sex. Concurrency has been suggested to be common in countries with high HIV prevalence, and to substantially increase the spread of HIV compared with serial monogamy with the same total number of sex partners (Shelton, 2009). The reason for this is that it increases the likelihood of having sex with someone else soon after being infected, when the viral load is particularly high. We measure concurrency using a binary variable that is equal to one if the respondents state that they currently have more than one sex partner.

Transactional sex has also been suggested to be common in high-HIV prevalence countries and to contribute to the spread of HIV through its connection to concurrency (Stoebenau et al., 2016). We measure transactional sex using a binary variable that is equal to one if the respondents state that they have received or given gifts or money in exchange for sex.

According to our theoretical framework, adult sexual behaviour is affected mainly through the impact of education on a person's earning potential. In our data, we do not observe income or wages, but we do have detailed information on occupations. We therefore test whether the education reform affected the

[8] There is information on protection at first sex, but it is difficult to interpret the use of protection without knowledge about who the sex partner is and the type of contraceptives used. While condoms are protective, all else equal, they are more likely to be used in casual than in committed relationships.

skill level of occupations. As mentioned, Borkum, (2009) found positive income effects of the Botswana education reform.

3. The education reform

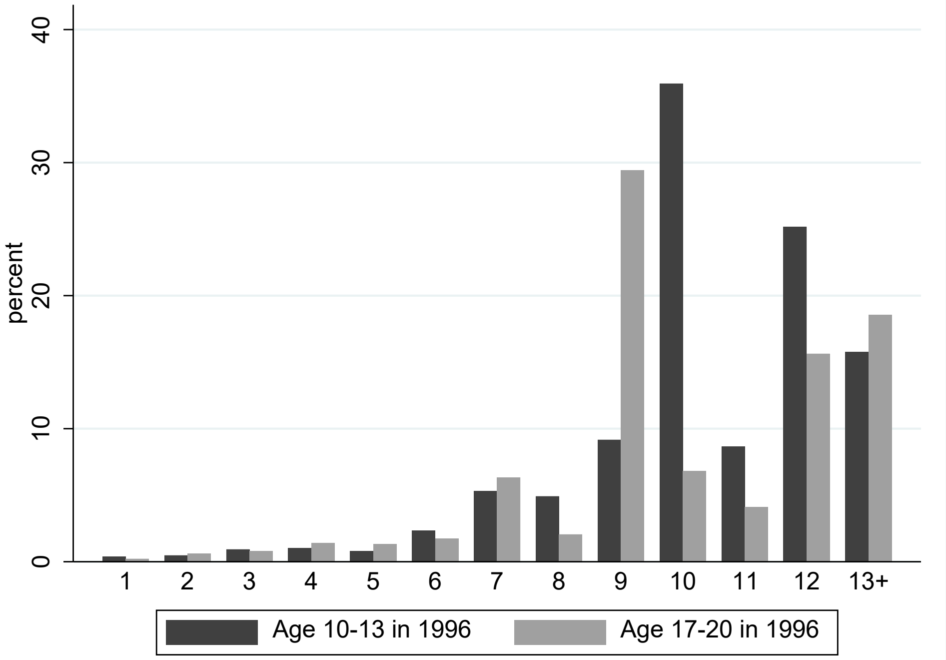
The education system in Botswana consists of seven years of primary education and five years of secondary education. The five secondary years are divided into junior and senior secondary school. Before the reform, junior secondary school lasted two years and senior secondary school three years. The political goal is that everybody should finish junior secondary school, while senior secondary school is seen as preparatory for tertiary education. Consequently, a large share of students finish their schooling after the junior secondary school level (41.12% did so in our estimation sample, 44.42% of the women and 37.13% of the men). Due to a perception that two years of secondary schooling was not enough to prepare students for work or further training, the education system was reformed from a 7+2+3 year to a 7+3+2year system, such that the 10th school year was shifted from senior to junior secondary school. The policy affected students entering secondary school in 1996 or later.

In addition to an increased number of students attending the 10th year, there was a change in the location where students attend the 10th year and of the teachers who teach it. To accommodate the increased number of students at junior secondary schools, new classrooms were built and new teachers hired at existing junior secondary schools. Access to junior secondary school seems to have been unaffected by the policy reform, and due to the existing capacity, there does not seem to have been a decline in quality (Borkum, 2009).

A shift of the 10th grade from the first year in senior secondary school to the last year in junior secondary school unavoidably involved some curricular changes, especially for the 10th year. In particular, there was an aim to make the curricula more relevant to the labour market, implying a broadening of course offerings to include technical and business subjects. However, the effect of the curricular changes are likely to be of secondary importance compared with the additional year of study (Borkum, 2009).

The school reform led to a substantial increase in the number of students completing ten years of education. Among those born 1976–1979 (who were largely unaffected by the reform), 49.31% completed 9 years of education or less; for those born 1983–1986 (who were largely exposed to the reform), the corresponding figure is 22.89 per cent. Figure 1 shows detailed information on the shares of the two age groups who finished their schooling after each grade. As can be seen, there was a clear shift from a norm of finishing school after 9th grade to one of finishing school after 10th grade.

Figure 1. Highest completed grades (at the time of the survey) by age in 1996



4. Data and empirical strategy

4.1 Data

The Botswana AIDS Impact Surveys (BAISs) were carried out in 2001, 2004, 2008 and 2013. They are nationally representative cross-sectional surveys with an individual and a household component. The focus is on HIV, and the data contain, for example, self-reported sexual behaviour and HIV status based on blood samples. There is also information on socioeconomic factors such as education, occupation and labour market participation. Each survey consists of between 4,500 (2001) and 14, 000 (2008) observations. Figure 2 shows the age distribution of the sample in 1996. As can be seen, the distribution is fairly uniform.

Figure 2: Distribution of the estimation sample across age in 1996

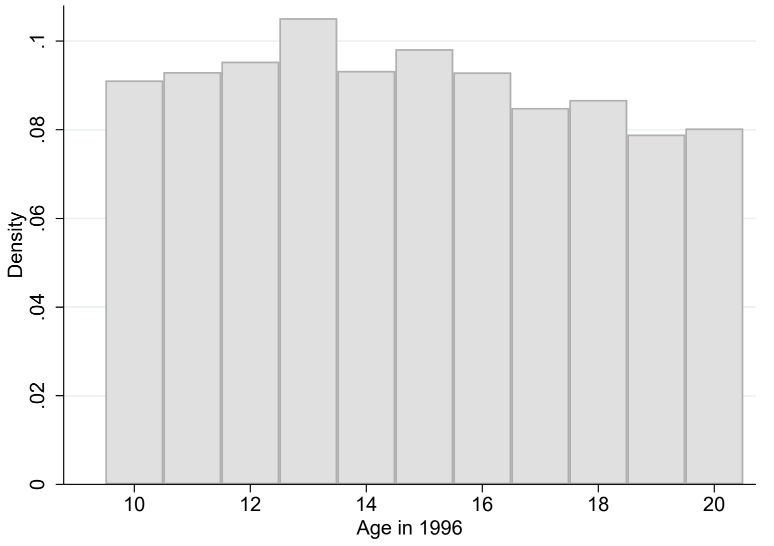


Table 1 shows summary statistics by gender for the dependent variables and the highest educational level attended by respondents. It also specifies which surveys contain information about each dependent variable, and gives a description of the sample in terms of age at survey collection and distribution of observations across the surveys.

Age at first sex is about 18 years for both women and men. Men report more concurrency and transactional sex than women. There is a large difference in HIV prevalence between the genders: 33.2% for women and 16.7% for men. A large majority of the individuals in the sample have some secondary education, while 14% have only primary education or less and 17% have at least some tertiary education.

Since there is no information on income in the surveys, we use ILO's International Standard Classification of Occupations (ISCO) to classify the occupations into skill levels, and test for the impact of the reform. A dummy variable that takes the value of one if a person belongs to one of the three highest skill groups in the labour market (Professionals; Technicians and Associate Professionals; Legislators, Administrators and Managers), and zero otherwise, was constructed. We also used the ISCO ranking for the nine main occupational categories as an alternative measure, and arrived at essentially the same results.[9]

Table 1: Descriptive statistics (10–20 year olds in 1996)

	Women Mean	Women Std. Dev.	Women N	Men Mean	Men Std. Dev.	Men N	Surveys containing the dependent variable
Dependent variables							
Age at first sex	18.297	2.357	4,363	18.266	2.979	3,152	2001, 2004, 2008
Concurrency	0.080	0.272	4,042	0.189	0.392	3,214	2004, 2008
Transactional sex	0.016	0.127	4,932	0.022	0.147	3,796	2001, 2004, 2008, 2013
HIV status	0.332	0.471	4,616	0.167	0.373	3,609	2004, 2008, 2013
Skilled occupation	0.705	0.456	2,799	0.765	0.424	3,282	2001, 2004, 2008, 2013
Further description of the sample							
Age in years	26.602	4.848	6,274	26.752	4.933	5,112	
Never attended formal education	0.036	0.186	6,003	0.067	0.250	4,759	
At least some primary education	0.102	0.302	6,003	0.135	0.342	4,759	
At least some secondary education	0.686	0.464	6,003	0.581	0.493	4,759	
At least some tertiary education	0.176	0.381	6,003	0.217	0.412	4,759	
2001 survey dummy	0.086	0.280	6,274	0.086	0.281	5,112	
2004 survey dummy	0.308	0.462	6,274	0.292	0.455	5,112	
2008 survey dummy	0.383	0.486	6,274	0.376	0.484	5,112	
2013 survey dummy	0.223	0.416	6,274	0.245	0.430	5,112	

4.2 Exposure probabilities

The school year in Botswana starts in January, and the calendar-year system is used to determine when a child should start first grade. An individual was exposed to the reform if he or she was eligible to enter secondary school in 1996 or later. We do not know with certainty whether each respondent was exposed to the reform or not for two reasons. Most importantly, as is typical in sub-Saharan African countries, many children are not in their age-appropriate grade in Botswana. Both delayed school entry and repetitions are common, and there are children who start school early. Hence, the relationship between age in 1996 (or birth year) and grade in 1996 is far from perfect. However, the individual variation in grade in 1996 is likely to be strongly correlated with ability and other unobserved factors that matter for schooling, and potentially also for sexual behaviour. This makes individual grade in 1996 endogenous

[9] These results are available on request.

and therefore unsuitable for identification anyway. Instead, we use only the variation in exposure to the reform that depends on the arguably external factors, gender and age in 1996.

The other reason we do not know which grades the respondents were eligible to enter in 1996 is that the BAIS data only contains information on age at the time of survey collection, not birth date. The BAIS surveys were collected at different times from 22 January to 31 July.[10] Hence, some respondents had already had their birthday in that year while others had not. This implies that each individual has two possible ages in the beginning of 1996. We use the mid-day of the survey collection to compute the probability that respondents already have had their birthday. More specifically, to calculate the exposure probability, let pr_{a96} be the probability that the respondent's birthday was after the date of the survey, and let pr_{a96-1} be the probability that the respondent's birthday was before the date of the survey. Then with probability pr_{a96}, the age at 1 January 1996 was the same as the age in 1996 at the date of the survey, and with probability pr_{a96-1} it was one year less than at the time of the survey.

We use two measures of exposure probability that contain different degrees of exogenous variation in the probability that someone was exposed to the reform. The simplest measure is based on the age-appropriate grade of the respondent. This measure is similar to the one used by De Neve et al. (2015).[11] The respondent should have been exposed to the reform if born in 1981 or later (if turning 14 or less in 1996). Let θ_j be the probability that the respondent (*j*) has been exposed to the reform depending on birth year/age in 1996, where $\theta_j = 0$ for a respondent who turned 15 or more in 1996 and $\theta_j = 1$ for the younger ones. The exposure probability is then:

$$Exposure1 = \mathrm{pr}_{a96-1} * \theta_{j-1} + \mathrm{pr}_{a96} * \theta_j.$$

[10] The first and second surveys were collected 12 February–31 July, the third 19 May–30 May and the fourth 22 January –22 April.

[11] Our measure differs slightly from theirs in how we deal with the fact that we only know age in years, but not the birthdays of respondents. De Neve et al. (2015) assume that current age is reported age plus 0.5, and use this to compute birth year.

Most respondents have an exposure probability of either zero or one. However, since each respondent has two possible birth years, a small number of them have a probability between zero and one.

This exposure probability can be criticised for not reflecting the fact that many children are not in their age-appropriate grade, implying that there was no sharp cut-off between age cohorts that were or were not exposed to the reform. Instead, exposure to the reform increased gradually across birth cohorts. This makes the measure of exposure an imprecise proxy of true exposure to the reform. One solution, which for example Agüero and Bharadwaj (2014) use, is to keep only birth cohorts whose exposure probability was close to either zero or one in the sample, removing the in-between cohorts. However, this implies that we disregard useful information in the data and that the birth cohorts that are compared with each other are more dissimilar than necessary, as they are far apart in time. A more appropriate approach would be to make use of the continuous variation in exposure probability for the birth cohorts, which allows us to both keep useful information and compare cohorts of similar age.

Our second measure does this. It is therefore similar to the measure used by Borkum (2009), who studies the impact of grade structure reforms in Botswana on wages,[12] and Chicoine (2012), who studies the impact of a Kenyan grade structure reform on fertility.[13] Conceptually, it is akin to Angrist and Lavy (2002), who use estimated probabilities of treatment in their analysis of the impact of class size on student performance in Israel. We use the actual shares of children of different ages and gender who attended certain grades as reported in the 1995 Education Statistics, $\gamma_{j,k}$, where k indicates gender. To be more precise, it is the share of each age group enrolled in any of the primary school

[12] Borkum (2009) uses information on school entry age from Education Statistics reports published by Botswana Statistical Office. Since he uses entry age, he has to make assumptions about grade progress. He probably uses school entry age instead of the age distribution in different grades because he studies two reforms and there is less data available from the year preceding the earlier reform.

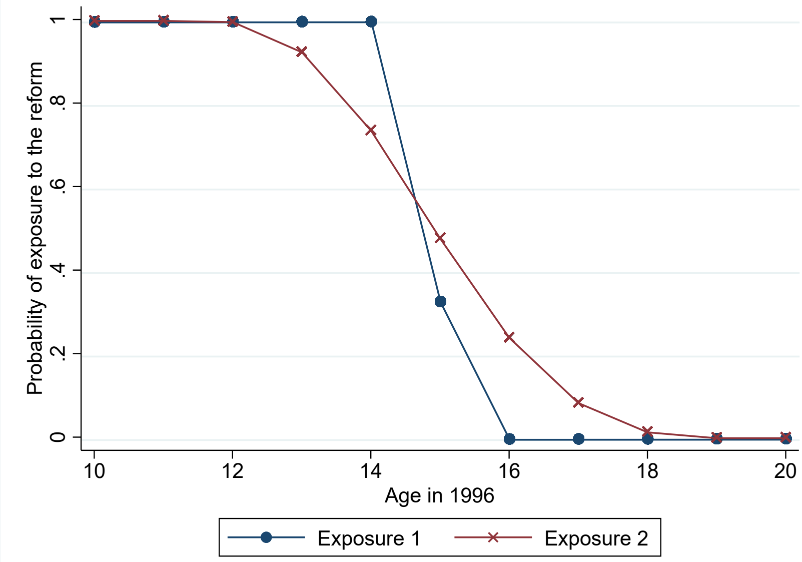
[13] Chicoine (2012) uses information on school entry ages and repetition rates. The age distribution at different grades is of course a more direct measure, when available.

grades in 1995.[14] These probabilities will be between 0 and 1. To connect them to respondents in BAIS, we again have to take into account that each respondent had two possible ages on 1 January 1996. The second exposure probability is then:

$$Exposure2 = \mathrm{pr}_{a96-1} * \gamma_{j-1,k} + \mathrm{pr}_{a96} * \gamma_{j,k}.$$

Figures 3 shows the two measures of exposure probabilities as a function of women's age in 1996.[15] It is apparent that the window around 15 years cannot be too narrow for it to be possible to identify an effect that is different from a linear trend. However, the more age groups we include, the less similar they are likely to be.

Figure 3. Treatment probabilities for women measured by Exposure 1 and Exposure 2



4.3 Identification strategy

The probabilities of exposure described above are our treatment variables. We thus estimate the impact of the education reform (the intent to treat, ITT) rather

[14] To compute the share of a certain age and gender group enrolled in primary school, the information in the Education Statistics reports was combined with population projections by age and gender. The population projection is available for 1996, so we reduced the age in the age groups by one year. In two age-gender groups, the number of enrolled students in primary and secondary schools were larger than the corresponding population cohorts. In these cases, we divided the students in the relevant age cohorts enrolled in primary school by the sum of students enrolled in primary and secondary school.

[15] The graph for men looks very similar and is available from the authors on request.

than the impact of an extra school year. We prefer reduced-form impacts of the reform instead of IV estimates of years of schooling for two reasons. First, although the most visible and important effect of the reform was that it substantially increased the number of students who attended a 10^{th} year of education, this is not the only effect. The reform came with some unavoidable changes e.g. in curricula, in the location at which some students attended their 10^{th} year (at a nearby junior secondary school instead of a more distant senior secondary school) and in the teachers who taught the 10^{th} year. There was also an aim to make the curricula more relevant to and useful in the labour market. By estimating the reduced form effect, we remain open to the possibility that part of the impact of the reform may be due to other factors than an increase in the number of years of education. Second, estimating the intent to treat has the additional advantage that we can use the fourth survey, collected in 2013, which does not contain information on years of education (it does include more detailed information on completed levels of education but we do not know if someone who completed junior secondary school did it in nine or ten years).

We report all of our results for both exposure probabilities. The second measure, which utilises information on actual age-grade attendance patterns, should be more precise than the measure that assumes a sharp cut-off. Since we have several surveys, we can control flexibly for current age of the respondent with a full set of age dummies. This is important since all the outcomes are age dependent. We also control for a continuous effect of age in 1996, which captures trends in the outcome variables over time. We control for these trends using a linear term only, a linear and a quadratic term, or survey dummies. In addition, we include district fixed effects. We estimate separate regressions for women and men. In the main analysis, we use linear regression. Since four of the outcome variables are binary, we estimate logit regressions for these as robustness checks. Our main specification is:

$$y_{i,j} = \alpha + \beta Exposure_j + \gamma age_i + \delta X_j + \varphi_d + \varepsilon_i, \quad (1)$$

where y is the outcome for individual i in age cohort j and district d, $Exposure_j$ is the probability that an individual is exposed to the reform, age_i is a vector of age dummies, X_j are birth year controls and φ_d are district dummies.

As mentioned, the more age cohorts born long before or after 1981 we include, the more dissimilar the people we compare become. On the other hand, including more age cohorts increases the statistical power and our ability to identify an impact of the reform when controlling for a continuous cohort trend (see Figure 3). In our main estimations, we include three cohorts with a zero or close to zero probability of exposure, three cohorts with a close to one probability of exposure, and five cohorts with a gradual increase in exposure probability. In our robustness analysis, we increase and decrease the window by one year at each end.[16]

Given the continuous cohort controls and the age controls, our identifying assumption is that nothing else happened that affected sexual behaviour and HIV infection rates across age-cohorts in the same way as the school reform. The main threat would be other changes in secondary school. As most high-HIV-prevalence countries, Botswana has introduced so-called life skills education, which includes HIV/AIDS knowledge, sex education and discussions about relationships. However, the policy was developed in 1998 and implemented after that, so it did not coincide with the school reform. Initially, the teaching consisted of one-off lessons about biomedical facts, and, as in many other countries, teachers lacked teaching methods and were uncomfortable discussing the topic, making the initiative a failure (Gachuhi, 1999). A teacher-capacity training programme was launched first in 2004. In fact, a general conclusion drawn by UNAIDS is that adolescents and young adults have been 'largely neglected and left behind by the national HIV response' (UNAIDS, 2018b). It also seems unlikely that other parts of the national HIV response affected age cohorts in the same way as the school reform. The initial national response to the HIV epidemic in the late 1980s was prompt, but by the mid-1990s, the efforts had lost steam, and it was not until early 2000, with the provision of free anti-retroviral drugs and the introduction

[16] If we were to increase the window much more, we would include people who went to school when junior high school lasted three years and senior high school lasted two years, which is the same as for those exposed to the 1996 education reform. Moreover, if we were to decrease the window much more, it would be almost impossible to identify an effect of the reform while also controlling for a continuous cohort trend, as evident from Figure 3.

of routine HIV testing, that the government made HIV/AIDS a priority for the country (Allen and Heald, 2004).

4.4 Exposure to the reform among potential sex partners

People's sexual behaviour does not depend on own intentions alone but also on potential sex partners. The same is true for HIV infection. We estimate reduced form effects of the education reform, which include possible effects among potential sex partners. If potential sex partners are affected by the reform in a similar way as 'treated' individuals, the impact of the education reform will be strengthened.

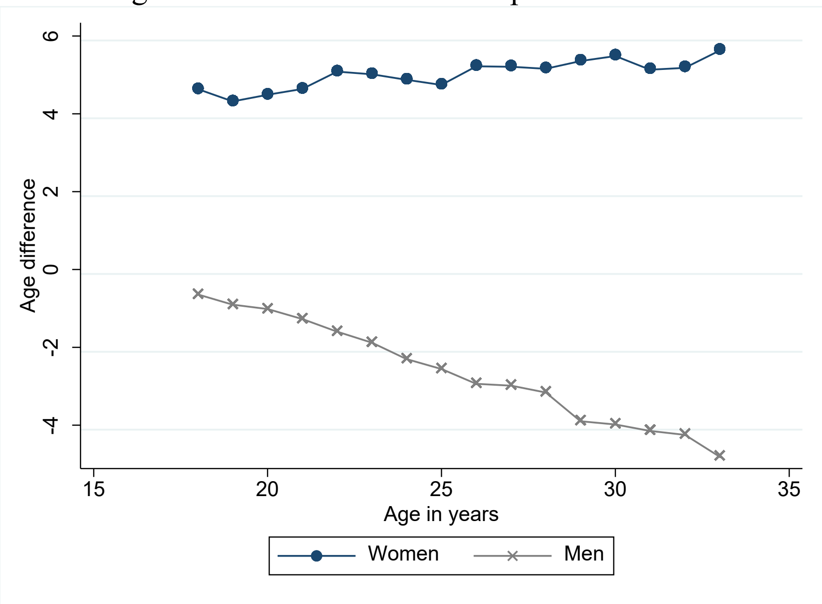
If we instead believe that women and men respond differently, there should be weaker effects. It could even be a challenge to identify gender differences. Put differently, our estimated gender differences are likely to be less than what they would have been in the thought experiment where only women or only men, but not their potential sex partners, were exposed to the reform. For example, if the reform increases men's sexual risk-taking, the impact might be muted if the reform has the opposite effect on their potential female sex partners. Similarly, if women are less interested in transactional sex for given levels of transfers, they may still not decrease transactional sex if men offer them larger transfers. While we have limited information on sex partners, and even less on the full set of potential partners, we do know the age of the most recent sex partner. This can be used to get an idea about the influence of the potential partners of the opposite sex on our estimated overall effects.[17]

Figure 4 below shows the average age difference between women and men of different ages and their most recent sex partner. A women's average sex partner is approximately five years older than her, and this age difference is fairly stable across age. The men tend to have younger sex partners. However, in contrast to women, the age difference increases gradually with own age: for 18-year-old men the age difference is around -0.5 years, while it is almost -5 years for 33-year-old men. For the women, this implies there is little variation

[17] Given the paucity of information, it is a challenge to estimate the influence of (potential) sex partners at the individual level.

in exposure status of potential partners, since most of their partners are not exposed to the school reform. As a result, the estimated effect of increased schooling for the women is unlikely to be greatly influenced by a change in the behaviour of their male sex partners. Among the men, there is a difference between young and old men. The older ones in our sample should have partners who were exposed to the reform, but the young men should have a mixture of partners where some were exposed to the reform and others were not. This implies that the estimated effect of increased schooling for men is likely to be somewhat influenced by changed behaviour of their partners, in particular when we estimate the impact on age at first sex, since the sexual debut is likely to have occurred at a relatively young age with a woman of similar age.

Figure 4: Average age difference between women and men of different ages and their most recent sex partner



5. Results

5.1 Main results

This section first analyses the impact of increased schooling on adolescent and adult sexual behaviour separately for women and for men. Then it analyses how schooling affects HIV prevalence, and finally it tests whether differences between men and women are statistically significant. We report the estimated impact of being exposed to the reform (i.e. the intent to treat) from regressions using the two measures of exposure: Exposure 1, a sharp cut-off in 1996, and Exposure 2, a measure of the probability of being exposed to the reform based on the gender and age distribution across school years in 1996. All regressions have a full set of age dummies, district fixed effects and cohort trends using a linear term, a quadratic term or survey dummies.[18]

Table 2 reports the impact on the age at first sex. There is strong evidence that the school reform delayed women's first sex: all the estimates are significant at the 1% level. The delay is about 13 months when our preferred measure, Exposure 2, is used and 7 months when Exposure 1 is used. The results for men are weaker – the estimates vary from 0.017 to 0.351 – and they are not statistically significant.

Table 2: The impact of the school reform on age at first sex

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	0.626***	0.612***	0.627***	0.017	0.029	0.038
	(0.112)	(0.091)	(0.121)	(0.189)	(0.194)	(0.184)
N	4,009	4,009	4,009	2,889	2,889	2,889
Exposure 2	1.117***	1.114***	1.137***	0.333	0.347	0.351
	(0.139)	(0.160)	(0.119)	(0.453)	(0.458)	(0.363)
N	4,009	4,009	4,009	2,889	2,889	2,889
Control for age in 1996	Linear trend	Quadrat. trend	Survey dummies	Linear trend	Quadrat. trend	Survey dummies

Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Tables 3 and 4 report the results for concurrency and transactional sex, respectively. There is no indication that schooling affects these factors among women; all coefficients are small, negative and statistically insignificant. Men, on the other hand, seem to have more concurrent partners if exposed to the school reform, and they clearly have more transactional sex. Exposure to the reform increases the probability of having more than one partner among men by roughly 14 percentage points when we use Exposure 2 and 7 percentage points when we use Exposure 1 (Table 3). The probability of transactional sex increases by 2 to 5 percentage points (Table 4).

[18] The complete results are available from the authors on request.

Table 3: The impact of the school reform on concurrency

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.025	-0.027	-0.025	0.074*	0.068**	0.074*
	(0.020)	(0.017)	(0.020)	(0.039)	(0.027)	(0.039)
N	3,738	3,738	3,738	2,973	2,973	2,973
Exposure 2	-0.027	-0.038	-0.027	0.143*	0.139**	0.143*
	(0.031)	(0.034)	(0.031)	(0.066)	(0.044)	(0.066)
N	3,738	3,738	3,738	2,973	2,973	2,973
Control for age in 1996	Linear trend	Quadrat. trend	Survey dummies	Linear trend	Quadrat. trend	Survey dummies

Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Table 4: The impact of the school reform on transactional sex

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	0.002	0.001	0.003	0.027***	0.029***	0.027***
	(0.007)	(0.006)	(0.006)	(0.005)	(0.004)	(0.005)
N	4,534	4,534	4,534	3,485	3,485	3,485
Exposure 2	0.013	0.009	0.011	0.052***	0.053***	0.053***
	(0.011)	(0.009)	(0.010)	(0.009)	(0.008)	(0.010)
N	4,534	4,534	4,534	3,485	3,485	3,485
Control for age in 1996	Linear trend	Quadrat. trend	Survey dummies	Linear trend	Quadrat. trend	Survey dummies

Notes: Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996.Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Table 5 shows the impact of the school reform on having a skilled occupation. For women, all estimates are positive but only two are significant (at the 10% level). For men, all but one of the estimates are statistically significant. Thus, the results for men are in line with an impact on adult sexual behaviour through income.

Table 5: The impact of the school reform on having a skilled occupation

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	0.034	0.036	0.035	0.089***	0.076***	0.092***
	(0.027)	(0.034)	(0.026)	(0.025)	(0.024)	(0.025)
N	2,533	2,533	2,533	2,982	2,982	2,982
Exposure 2	0.077*	0.091	0.074*	0.108*	0.094**	0.105
	(0.039)	(0.059)	(0.037)	(0.055)	(0.042)	(0.059)
N	2,533	2,533	2,533	2,982	2,982	2,982
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01. The dependent variable is a dummy that is 1 for the three highest skill groups according to ILO's ISCO ranking.

Table 6 presents estimated effects of the school reform on HIV infection. For women there is quite a large protective effect. The size of the estimated effect varies considerably depending on exposure measure: for Exposure 2 it is about 10 percentage points and for Exposure 1 about 6 percentage points. This can be compared with an average HIV infection rate of 33% (see Table 1) in the estimation sample. The estimated effect on men's HIV infection is much smaller and not statistically significant.

Table 6: The impact of the school reform on HIV infection

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.065**	-0.062**	-0.065**	-0.021	-0.015	-0.022
	(0.026)	(0.026)	(0.028)	(0.018)	(0.011)	(0.016)
N	4,288	4,288	4,288	3,348	3,348	3,348
Exposure 2	-0.101*	-0.095*	-0.098*	-0.039	-0.036	-0.039
	(0.048)	(0.050)	(0.049)	(0.030)	(0.020)	(0.029)
N	4,288	4,288	4,288	3,348	3,348	3,348
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

A key finding this far is that the effects of the school reform appear to differ between women and men, so in the next step we test whether these differences are statistically significant. To do so we estimate fully interacted models, where a female dummy is interacted with all explanatory variables, on the pooled sample of men and women. Table 7 reports the interaction term between exposure to the reform and the female dummy for all three exposure measures. Since the results are largely stable across specifications with a linear or quadratic cohort trend or survey year controls, we only report estimates with a linear age in 1996 control to save space.

Women seem to delay first sex more than men as a result of the reform. There is also a statistically significant difference in concurrency and transactional sex between women and men. Although the probability of having a skilled occupation increases more for men than for women, this difference is not statistically significant. Finally, while the estimates of the impact on HIV infection indicate a much larger effect on women than men, the difference is not statistically significant. The reason is that the standard errors of the estimates are relatively large.

Table 7: Testing the statistical significance of gender differences

	First sex	Concurrency	Transactional sex	Skilled occupation	HIV
Exposure 1	0.609***	-0.099**	-0.025**	-0.055	-0.044
	(0.192)	(0.032)	(0.009)	(0.039)	(0.037)
N	6,898	6,711	8,019	5,515	7,636
Exposure 2	0.784*	-0.169***	-0.039**	-0.030	-0.061
	(0.391)	(0.050)	(0.016)	(0.074)	(0.067)
N	6,898	6,711	8,019	5,515	7,636
Control for age in 1996	Linear	Linear	Linear	Linear	Linear

Notes: All models are fully interacted. They include a full set of age dummies, district fixed effects, one exposure model, a female dummy and interaction terms between the female dummy and all other variables. Only the interaction terms between the exposure measure and the female dummy are reported in the table. Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. Significance levels are indicated by * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

5.2 Robustness checks

In our first set of robustness checks, we evaluate 'placebo reforms', where we shift the timing of the reform back and forth in time. For Exposure 1, we still assume perfect age-appropriate grade placement for all pupils. For Exposure 2 we assume that the gender and age distributions in different grades were the same at the end of 1991 as at the end of 1995, i.e. we still use the gender and age distributions in different grades from the 1995 Education Statistics report to compute probabilities of exposure to the reform. We first pretend that the reform occurred three years earlier than it actually did, i.e. in 1993. We then estimate the impact of fictional reforms occurring two years earlier, one year later, two years later and three years later than the actual reform. For comparison, we also report the estimated impact of the actual reform. To avoid having a very small number of exposed and unexposed individuals, we expand the window to include the 9–21 year olds in the sample. The results are presented in Figures A1–A10 in Appendix II.

The expectation is to find the strongest effect at the time of the reform, and that the effect fades away as we move away from the year of the reform. This pattern is clearly confirmed in all cases except concurrency and transactional sex among men, for which the effects peak one year later and one year earlier, respectively. Nevertheless, even in these cases the effects decline for fictional reform years further away from 1996. In general, fictional reforms are also less statistically significant as we move away from the actual reform year, but for men's concurrency there are statistically significant coefficients of opposite sign at t-3

In our next set of robustness checks, we re-estimate all regressions using a sample window that was one year wider (age 9–21 in 1996) and one year narrower (age 11–19 in 1996) than in the main analysis. The estimated effects are reported in Tables A1–A6 in Appendix II.

Let us start with the wider window. All effects that are statistically significant in the main analysis remain so. The point estimates are somewhat larger but similar in magnitude. There are three notable differences compared with the results in the main analysis: the effect on skilled occupations for women is significant, there is a significant gender difference for HIV prevalence, and the gender difference for transactional sex is not significant.

The results obtained with the narrower window differ more, primarily due to a lack of statistical significance. The only strong results are for first sex among women, which are similar to those in the main analysis. However, the effect on concurrency among women is positive and statistically significant at the 10% level, while it is statistically insignificant among men, in spite of being larger than the effect for women. Moreover, the effect on transactional sex is insignificant for both men and women. The difference compared with in the main analysis is probably due to the similarity between the exposure measures and the linear birth-year trend, as evident in Figure 3.[19] This makes it difficult to estimate the effects of the reform when the window is too narrow.

In our last robustness check, we estimate logit models for the binary outcomes. Tables A7–A11 in Appendix II report the estimated marginal effects. Although the point estimates and significance levels differ somewhat, overall, the results are similar to those in the main analysis.

5.3 Why do we get different HIV results than De Neve et al. (2015)?

De Neve et al. (2015) find that the school reform reduced HIV infection for both men and women (and their estimated reduced form effect is very similar in magnitude between the genders). De Neve et al. (2015) use a sharp cut-off similar to our Exposure 1. As opposed to them, we find no statistically significant impact on men.

Apart from the use of Exposure 2, our estimations differ from De Neve et al. (2015) in two important ways: we have an additional survey, from 2013, and we do not include people who were 6–9 or 21 years old in 1996 in our estimation sample. To investigate why our results differ, we first removed the 2013 survey from our sample and then included those of age 6–9 or 21 in 1996 in the sample.

Table 8 shows that removing the 2013 survey does not change our estimated results for men in any meaningful way, though the estimates for women become more negative. However, when we include the 6–9 and 21 year olds in the sample (Table 9), the estimates for men become significant and similar to those in De Neve et al. (2015) in the specification with a linear trend or with survey dummies.[20]

[19] While Exposure 1 differ more from a linear trend, we believe that it is a less accurate description of true exposure probabilities.

[20] Excluding the 2013 survey and keeping the 6–9 and 21 year olds give almost the same results as those reported in Table 9. See table A12 in Appendix II.

Table 8: Removing the 2013 survey.

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.075**	-0.068**	-0.075**	-0.023	-0.018*	-0.023
	(0.028)	(0.025)	(0.028)	(0.015)	(0.009)	(0.015)
N	3,405	3,405	3,405	2,600	2,600	2,600
Exposure 2	-0.149***	-0.121**	-0.149***	-0.033	-0.028	-0.033
	(0.042)	(0.047)	(0.042)	(0.029)	(0.019)	(0.029)
N	3,405	3,405	3,405	2,600	2,600	2,600
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Table 9: Including 6–9 and 21 year olds in the sample.

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.079***	-0.058**	-0.078***	-0.042**	-0.003	-0.041**
	(0.017)	(0.022)	(0.016)	(0.017)	(0.023)	(0.018)
N	6,121	6,121	6,121	4,814	4,814	4,814
Exposure 2	-0.100***	-0.079**	-0.097***	-0.066***	-0.009	-0.066***
	(0.021)	(0.032)	(0.020)	(0.019)	(0.037)	(0.020)
N	6,121	6,121	6,121	4,814	4,814	4,814
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

6. Conclusions

This paper uses an education reform in Botswana to analyse the link between education and sexual behaviour and HIV infection among women and men, focusing on gender differences. The education reform increased school attainment from nine to ten years for a large share of students and makes it possible to evaluate causal effects of increased schooling. We estimate the reduced form impact of the reform and find a difference in responses between women and men. Women delay their sexual debut by about a year, while the response among men is small and not statistically significant. Instead, men engage more in transactional sex and more often have concurrent relationships, while there are no such responses among women. The differences in sexual behaviour response to education is in line with theoretical predictions from a simple model. Finally, the increase in schooling appears to be protective against HIV infection among women, while we find no statistically significant impact on men.

Using different datasets, Borkum (2009) shows that the school reform improved wages and employment. We find that the school reform increased occupational skill levels for both men and women, supporting the findings of Borkum (2009). Our results are therefore consistent with earlier studies showing that higher income increases men's risky sex (Kohler and Thornton, 2011; Robinson and Yeh, 2011; Burke et al., 2015).

HIV infection does not only depend on own behaviour but also on the behaviour of sex partners. Women have partners who, on average, are 5 years older, implying that most of the partners to women in our sample were not exposed to the education reform. While men tend to have younger partners, the age gap is small (0.5 years) for young men but then grows with age to 5–6 years for men around age 30. Hence, exposure to the education reform among sex partners varies more among men than among women. The correlation of exposure to the reform between men and their female partners is far from perfect, but men who were exposed to the reform tend to have female sex partners who also were exposed. This could be one explanation why the impact of the reform on men's HIV infection is not positive in spite of their increased sexual risk behaviour. Another reason could be that men reduce risk-taking in other ways. For example, while better-educated men seem to engage more in transactional sex and concurrency, they could also increase condom use.

De Neve et al. (2015) find a protective effect of the education reform on HIV infections for both men and women. The main reason why our findings differ from theirs is the number of birth-year cohorts included in the estimation sample; in particular, the youngest individuals in our sample were 10 years old in 1996, while De Neve et al. (2015) include individuals who were 6 years old in 1996. There are advantages and disadvantages of including more birth cohorts in the estimation sample: using more birth cohorts increases the possibility to identify an effect of the education reform that differs from a birth-year cohort trend. However, it also implies comparing individuals who are less similar to each other. For example, many of those who were 6–9 years old in 1996 initiated their sex life after the roll-out of Botswana's mass antiretroviral therapy programme, which took off around 2004 (Center of Global Development, 2019).

We used two measures of exposure to the education reform, one that assumed perfect age-appropriate grade placement and one that used information on what grade children of different ages were in in 1995 (the year before the reform) to compute probabilities of exposure to the reform. In general, the results were similar for the two types of measures. However, point estimates are consistently larger for the second measure. This is in line with more measurement error and attenuation bias when exposure probabilities are computed assuming perfect age-appropriate grade placement.

Keeping girls in school is currently considered a powerful HIV prevention method in policy circles (UNAIDS, 2015a, 2015b; United Nations, 2016; World Bank, 2016) and academics (Remme, et al., 2012), and large investments have been devoted to this end (PEPFAR, 2016). The evidence from Botswana arguably supports this view. However, it also casts some doubt on the optimism of Remme et al. (2015), who argue that investment in the expansion of secondary education could provide both education and an HIV-free future. Men and women infect each other, so if increased schooling increases sexual risk-taking among men, the positive effect on women might be reduced or annulled in the long run, particularly if delayed sexual debut is a main reason for the decline in HIV infections. On the other hand, focusing on the education of girls only might not be a defensible strategy in a context where young women are at least as educated as young men, which is now the case in Botswana and several other southern African countries. Thus, young men's education should be encouraged, but additional policies are needed that address norms about sexual risk-taking and related issues; just attending school might not have an automatic beneficial impact on all. One potentially effective approach to alter the impact of education on sexual behaviour and HIV is to implement or improve life skills education programmes. Past experiences are disappointing, but a lot has been learned and life skills education programmes that use a genuinely educational approach to HIV and sex, while emphasising gender, power and human rights, have shown some promise (Haberland and Rogow, 2015; Aggleton et al., 2018).

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Appendix I. Theoretical model

In this section, we develop a simple model to fix ideas for the empirical analysis. The aim is to highlight the difference between the genders and to guide our analysis of links between education and HIV. More thorough and comprehensive models on HIV transmission can be found in for example Greenwood et al. (2013), Duflo et al. (2015) and Yao (2016).

We assume that the model has two types of individuals: women and men. We also assume two periods: adolescence and adulthood. Risky sex is the only choice variable. In particular, we do not model educational choice, since we aim to study an exogenous increase in education. Instead we use a simple approach were we reason about the impacts of an exogenous increase in education on the terms in the first-order conditions for optimal risky sex.[21] In each period, the individual engages in risky sex if the marginal benefit of doing so is perceived to be higher than the marginal cost. Risky sex provides direct utility, but it also has consequences on i) transfers of resources to or from partners, ii) the probability of pregnancy and iii) the risk of HIV infection.

The first period's utility depends on consumption, c_1, and risky sex, s_1. The benefits and costs of having children and getting infected with HIV are not experienced until adulthood.

$$(1) \qquad U_1 = u(c_1) + v^k(s_1)$$

$u()$ and $v^k()$ are both concave functions. The superscript k indicates that the utility of risky sex could differ between genders. The first period budget constraint is:

$$(2) \qquad c_1 = y_1 + \tau^k(s_1)$$

where y_1 is exogenously given income (or perhaps transfers from the family) and $\tau^k(s_1)$ are transfers to and from sex partners ($\tau^m(s_1) \leq 0$ for men).

[21] Yao (2016) develops a general equilibrium model with endogenous education.

The second period's utility depends on the presence of children and HIV infection in addition to consumption and risky sex.

$$\text{(3)}\ \ U_2 = u(c_2) + v^k(s_2) + \omega q_{ch}(s_1, s_2) - \theta_i q_{hiv}(s_1, s_2),$$

where ω is the utility of children and $q_{ch}(s_1, s_2)$ the probability of having children. This probability depends positively on unprotected sex in both adolescence and adulthood. θ_i is the disutility of being HIV infected, and $q_{hiv}(s_1, s_2)$ is the probability of getting infected with HIV. The second period budget constraint differs between the sexes. For women it is:

$$\text{(4)}\ \ c_2 = y_2 + r_i^w(1 - q_{ch}(s_1)) + \tau^w(s_2) - p_b^w q_{ch}(s_1, s_2),$$

where y_2 is exogenously given income and r_i^w is return to education. If a woman becomes pregnant in adolescence, she has to drop out of school, implying that she loses the return to education. The term $\tau^w(s_2)$ represents transfers from sex partners, and p_b^w is the cost of having children. The cost of having children is higher for women than men, since women typically have a higher opportunity cost of childbearing and childrearing. The second-period budget constraint for men is:

$$\text{(5)}\qquad c_2 = y_2 + r_i^m + \tau^m(s_2) - p_b^m q_{ch}(s_1, s_2)$$

Men receive the return to education even if unprotected sex in adolescence leads to pregnancy. $\tau^m(s_2)$ is negative, i.e. men transfer resources to sex partners. $p_b^m < p_b^w$ primarily because they do not face an opportunity cost component. The extent to which men take financial responsibility for children will vary and p_b^m could be zero if men take no such responsibility.

The individual maximises utility with respect to adolescent and adult risky sexual behaviour. In adolescence, the second period's utility is discounted with the discount factor β.

The first-order condition for women's adolescence sexual behaviour is then:

$$\text{(6)}\ \ u'(c_1)\frac{d\tau^w}{ds_1} + \frac{dv^w(\cdot)}{ds_1} + \beta\omega\frac{dq_{ch}(\cdot)}{ds_1} = \beta\left(r_i^w\frac{dq_{ch}(\cdot)}{ds_1} + \theta\frac{dq_{hiv}(\cdot)}{ds_1} + u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_1}\right)$$

The terms on the left-hand side are benefits of risky sex: utility from partners' transfers, from the risky sex itself and from having children. The terms on the right-hand side are costs of risky sex: loss of the return to education if having to drop out of school, utility loss of HIV infection and the utility loss from the cost of having children.

The first-order condition for risky sex among adolescent men is:

$$(7)\ \frac{dv^m(\cdot)}{ds_1} + \beta\omega\frac{dq_{ch}(\cdot)}{ds_1} = -u'(c_1)\frac{d\tau^m}{ds_1} + \beta\left(\theta\frac{dq_{hiv}(\cdot)}{ds_1} + u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_1}\right)$$

Note that the transfer term is a cost for men and that men do not face the cost of school dropout if risky sex leads to pregnancy.

The first-order condition for adult women's sexual behaviour is:

$$(8)\ u'(c_2)\frac{d\tau^w}{ds_2} + \frac{dv^w(\cdot)}{ds_2} + \omega\frac{dq_{ch}(\cdot)}{ds_2} = \theta\frac{dq_{hiv}(\cdot)}{ds_2} + u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_2}.$$

The first-order condition for adult men's sexual behaviour is:

$$(9)\ \frac{dv^m(\cdot)}{ds_2} + \omega\frac{dq_{ch}(\cdot)}{ds_2} = -u'(c_2)\frac{d\tau^m}{ds_2} + \theta\frac{dq_{hiv}(\cdot)}{ds_2} + u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_2}.$$

The impact of increased education

Next, we consider how increased education due to the reform affects the different terms in the first-order conditions. Let us start with adolescent women. On the cost side, the term $r_i^w\frac{dq_{ch}(\cdot)}{ds_1}$ becomes more important, since pregnancy and school dropout are more costly during the extra year. Pregnancy and the resulting school dropout is more costly both because women need to stay in school for one more year to get the junior certificate and because the extra year may raise their future earnings conditional on completing it (if r_i^w increases with the extra year). The perceived risks and consequences of HIV could also change, but it is unclear in which direction, and we have no reason to expect this term to respond differently for women and for men. The term

$u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_1}$ is not affected, since if women do get pregnant in adolescence they do not complete their education.

On the benefit side, the value of the transfer changes if school attendance affects income from other sources than sex partners. If, for example, school attendance has an opportunity cost, the value of the transfer increases. This would increase the motivation for risky sex among young women

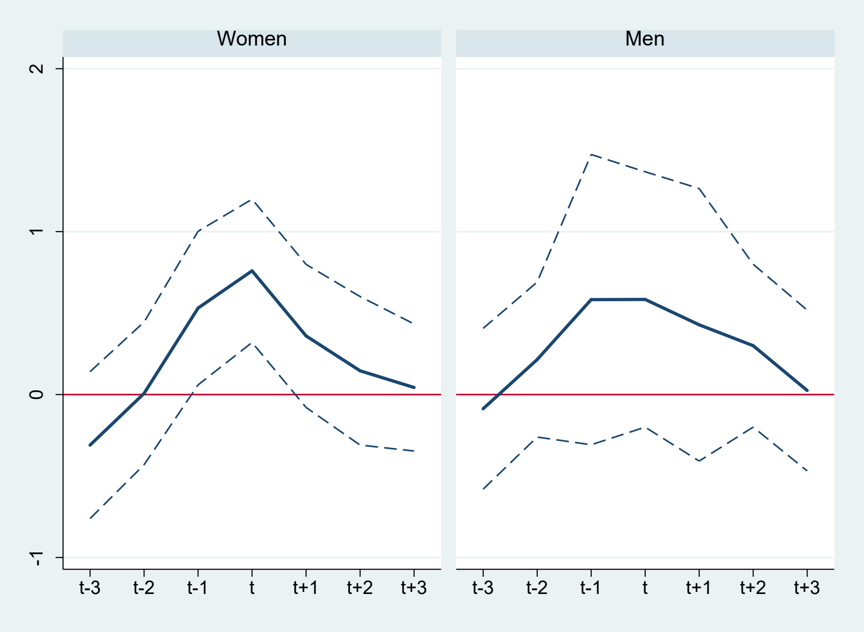
Let us turn to adolescent sexual behaviour among men. On the cost side, there is again a possible effect through changes in perceived risks and consequences of HIV, but the sign is ambiguous, and as already mentioned in the section on adolescent women, there is no reason to expect this effect to differ between women and men. If education reduces income while in school, for example since education has an opportunity cost, the value of transfers increases, which should reduce risky sex. To the extent that men take financial responsibility for children conceived in their youth ($p_b > 0$), their expected higher earnings make the cost of having children more affordable.

Turning to the first-order condition for adult sexual behaviour, for adults the additional education due to the reform is expected to increase earnings, r_i^k. This implies that the utility value of the transfer, $u'(c_2)\frac{d\tau^k}{ds_2}$, is smaller, which should decrease risky sex among women and increase it among men. The value of the cost of children, $u'(c_2)p_b\frac{dq_{ch}(\cdot)}{ds_2}$, also decreases. However, women also face a higher opportunity cost of having children. Thus, risky sex is expected to increase for men, while it is unclear whether the income or opportunity cost effect dominates for women. Either way, because of the opportunity cost, women are expected to respond in a less risky way than men.

Appendix II: Robustness checks

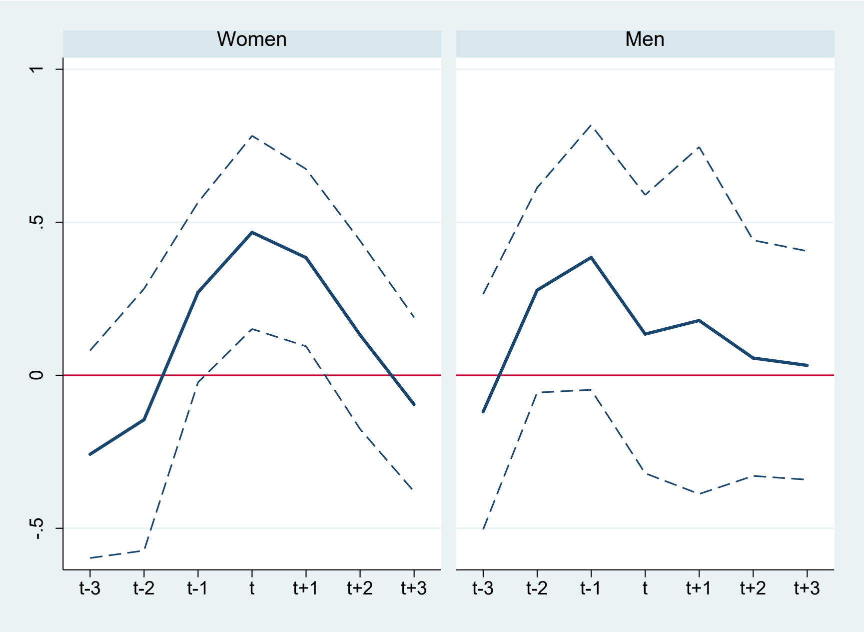
Robustness check 1: Testing placebo reforms implemented from three years before to three years after the actual reform

Figure A1: The impact of fictional reforms on age at first sex using Exposure 1 (coefficients and 95% confidence intervals)



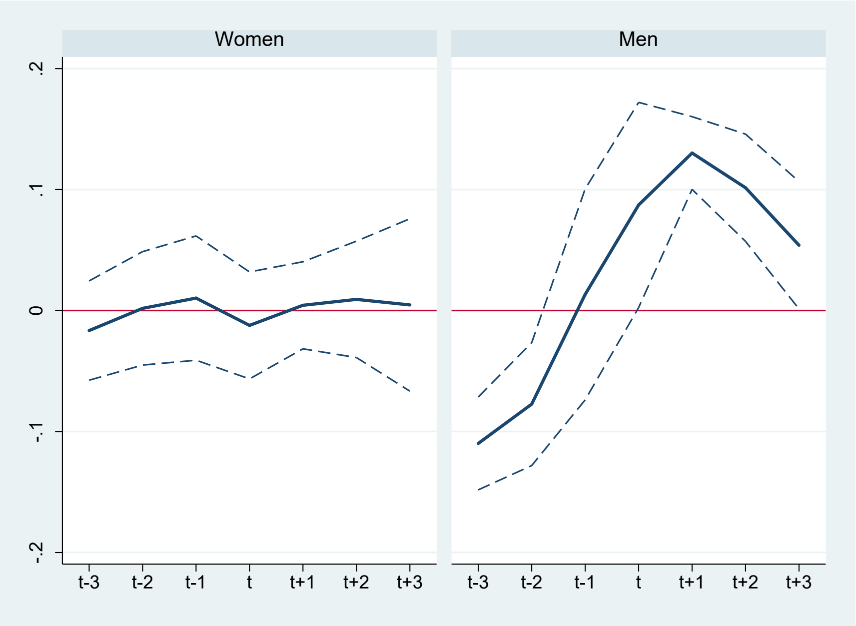
Notes: Exposure 1 assumes that everyone is in their age-appropriate grade. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A2: The impact of fictional reforms on age at first sex using Exposure 2 (coefficients and 95% confidence intervals)



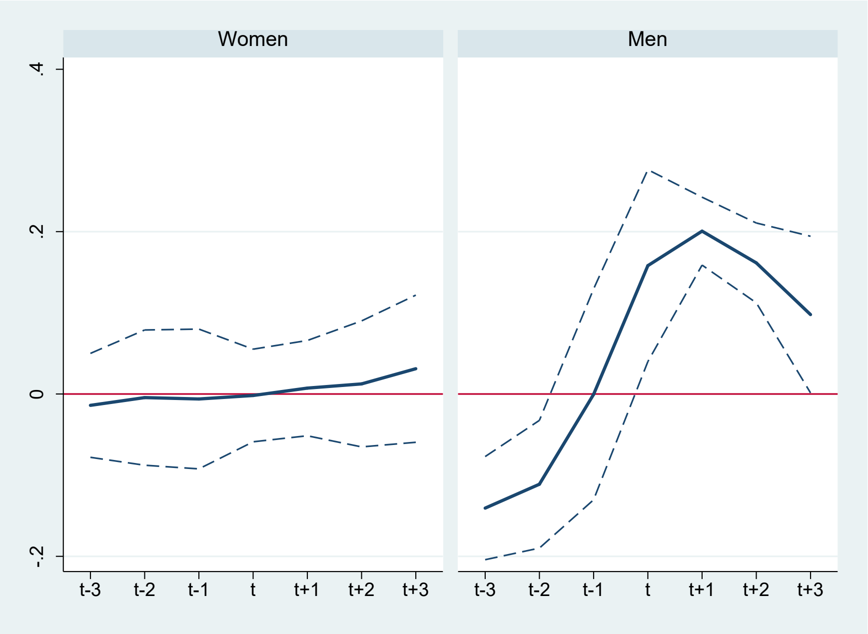
Notes: Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on gender and age in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A3: The impact of fictional reforms on concurrency using Exposure 1 (coefficients and 95% confidence intervals)



Notes: Exposure 1 assumes that everyone is in their age-appropriate grade. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A4: The impact of fictional reforms on concurrency using Exposure 2 (coefficients and 95% confidence intervals)

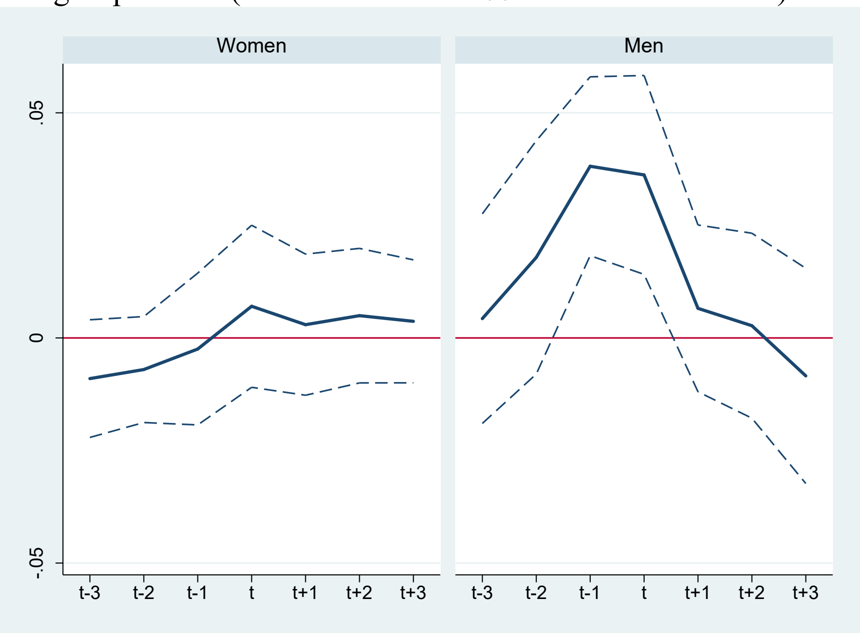


Notes: Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on gender and age in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A5: The impact of fictional reforms on transactional sex using Exposure 1 (coefficients and 95% confidence intervals)

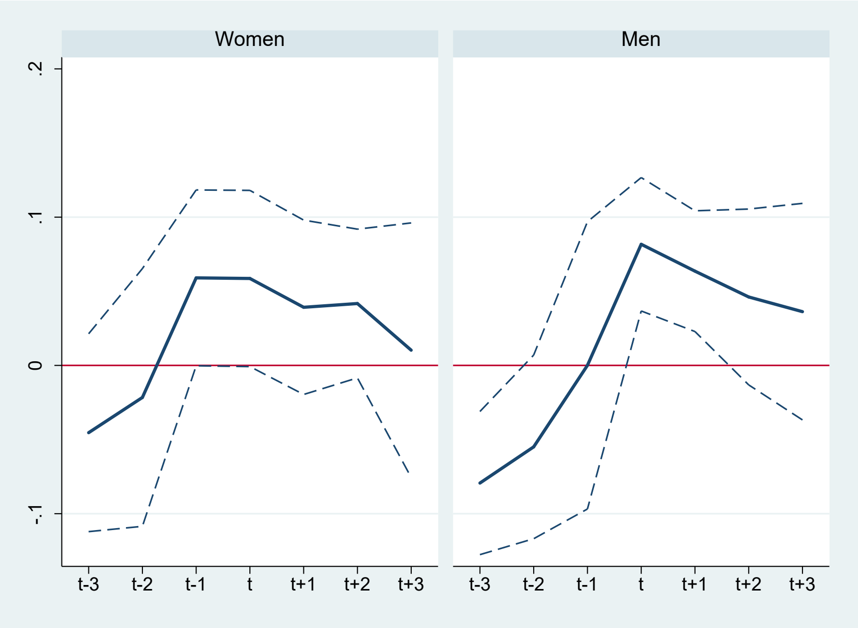
Notes: Exposure 1 that everyone is in their age-appropriate grade. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A6: The impact of fictional reforms on transactional sex using Exposure 2 (coefficients and 95% confidence intervals)



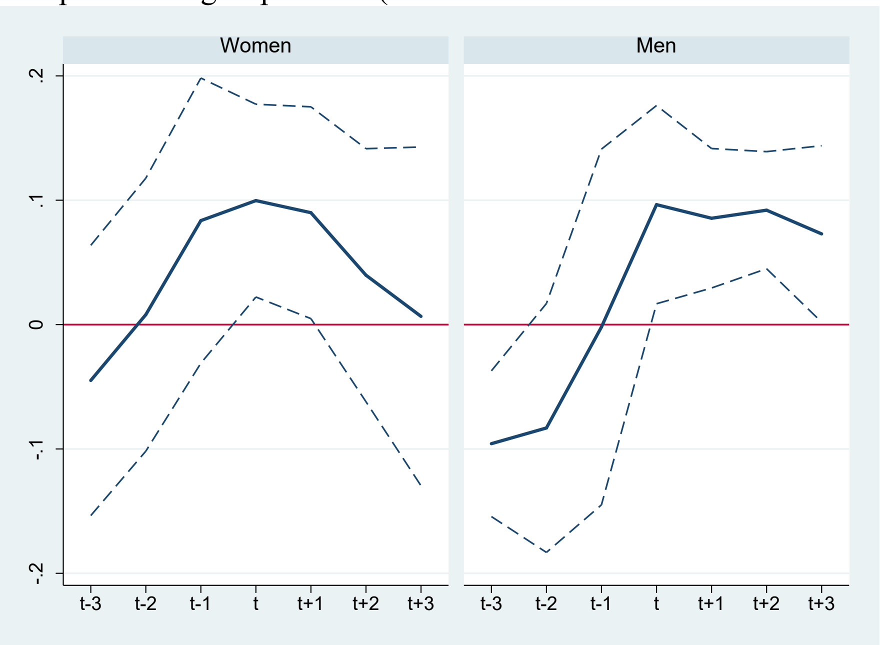
Notes: Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on gender and age in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A7: The impact of fictional reforms on having a skilled occupation using Exposure 1 (coefficients and 95% confidence intervals)



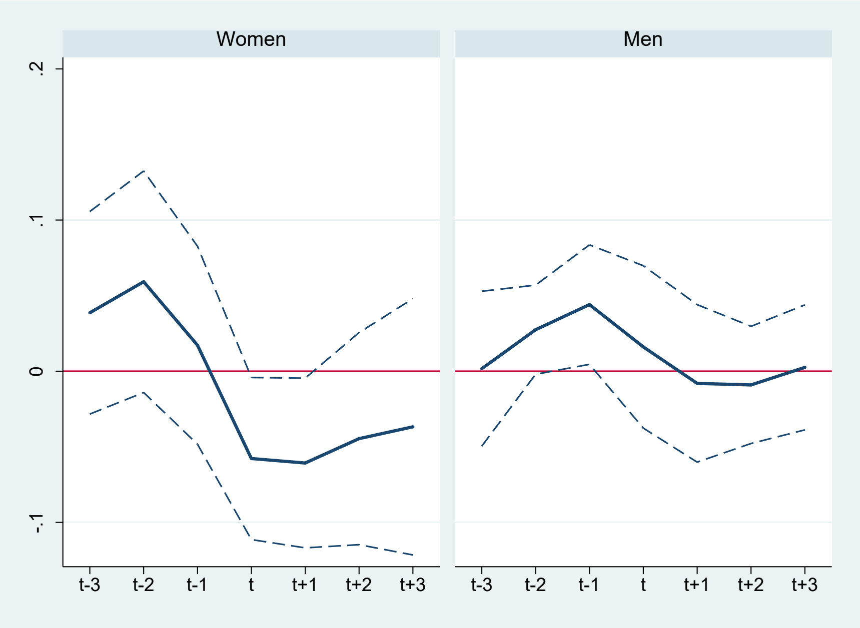
Notes: Exposure 1 assumes that everyone is in their age-appropriate grade. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A8: The impact of fictional reforms on having a skilled occupation using Exposure 2 (coefficients and 95% confidence intervals)



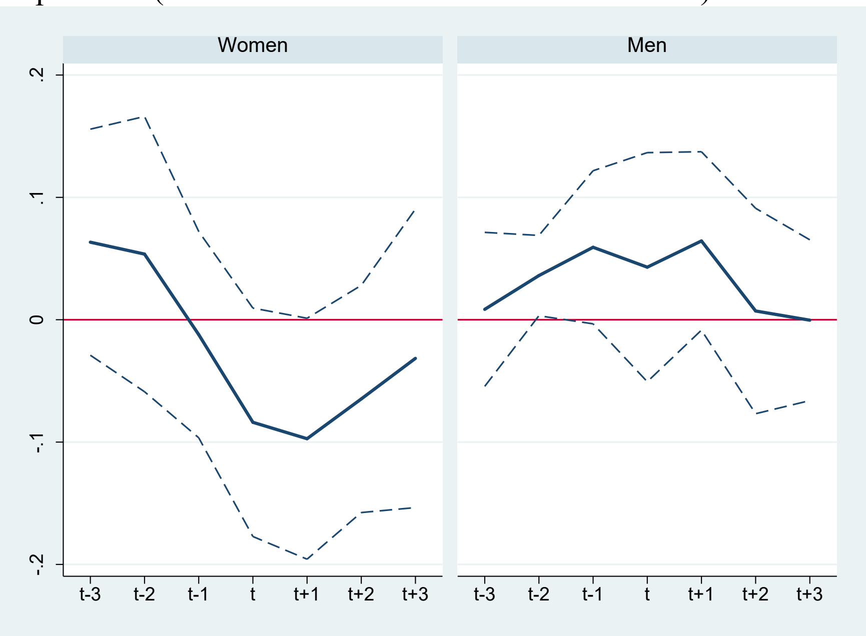
Notes: Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on gender and age in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A9: The impact of fictional reforms on HIV infection using Exposure 1 (coefficients and 95% confidence intervals)



Notes: Exposure 1 assumes that everyone is in their age-appropriate grade. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Figure A10: The impact of fictional reforms on HIV infection using Exposure 2 (coefficients and 95% confidence intervals)



Notes: Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on gender and age in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors are clustered at the birth year.

Robustness check 2a: Using a wider age in 1996 window (ages 9–21)

Table A1: The impact of the school reform on age at first sex, concurrency and transactional sex in the broader sample

	First sex		Concurrency		Transactional sex	
	Women	Men	Women	Men	Women	Men
Exposure 1	0.892***	0.631**	-0.006	0.083**	0.005	0.018***
	(0.218)	(0.266)	(0.018)	(0.035)	(0.005)	(0.004)
N	4,523	3,279	4,217	3,380	5,171	3,975
Exposure 2	0.892***	0.631**	0.008	0.135**	0.011	0.030***
	(0.218)	(0.266)	(0.020)	(0.048)	(0.008)	(0.008)
N	4,523	3,279	4,217	3,380	5,171	3,975
Control for age in 1996	Linear trend	Linear trend	Linear trend	Linear trend	Linear trend	Linear trend

Notes: Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. All regressions have a full set of age dummies and district fixed effects. Standard errors, in parentheses, are clustered at the birth year. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Table A2: The impact of the school reform on skilled occupations and HIV in the broader sample

	Skilled occupation		HIV	
	Women	Men	Women	Men
Exposure 1	0.086***	0.086***	-0.059**	0.011
	(0.025)	(0.020)	(0.022)	(0.021)
N	2,902	3,408	4,841	3,795
Exposure 2	0.139***	0.111**	-0.083**	0.022
	(0.038)	(0.037)	(0.036)	(0.037)
N	2,902	3,408	4,841	3,795
Control for age in 1996	Linear trend	Linear trend	Linear trend	Linear trend

Notes: See Table A1.

Table A3: Testing the statistical significance of gender differences in the broader sample

	First sex	Concurrency	Transactional sex	Skilled occupation	HIV
Exposure 1	0.371	-0.089**	-0.013	-0.000	-0.069**
	(0.243)	(0.032)	(0.007)	(0.031)	(0.028)
N	7,802	7,597	9,146	6,310	8,636
Exposure 2	0.260	-0.128**	-0.020	0.028	-0.105**
	(0.380)	(0.047)	(0.013)	(0.042)	(0.047)
N	7,802	7,597	9,146	6,310	8,636
Control for age in 1996	Linear	Linear	Linear	Linear	Linear

Notes: All models are fully interacted. They include a full set of age dummies, district fixed effects, one exposure model, a female dummy and interaction terms between the female dummy and all other variables. Only the interaction terms between the exposure measure and the female dummy are reported in the table. Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Robustness check 2b: Using a narrower age in 1996 window (ages 11 to 19)

Table A4: The impact of the school reform on age at first sex, concurrency and transactional sex in the narrow sample

	First sex		Concurrency		Transactional sex	
	Women	Men	Women	Men	Women	Men
Exposure 1	0.735***	-0.314	0.057*	0.063	-0.015	0.001
	(0.185)	(0.204)	(0.026)	(0.069)	(0.012)	(0.016)
N	3,388	2,419	3,093	2,426	3,818	2,905
Exposure 2	1.466***	-0.168	0.071*	0.117	-0.004	0.002
	(0.197)	(0.630)	(0.036)	(0.107)	(0.022)	(0.027)
N	3,388	2,419	2,323	1,813	3,818	2,905
Control for age in 1996	Linear trend	Linear trend	Linear trend	Linear trend	Linear trend	Linear trend

Notes: See Table A1.

Table A5: The impact of the school reform on skilled occupations and HIV in the narrow sample

	Skilled occupation		HIV	
	Women	Men	Women	Men
Exposure 1	0.038	0.156**	-0.047	0.052*
	(0.030)	(0.048)	(0.059)	(0.023)
N	2,154	2,489	3,535	2,731
Exposure 2	0.097*	0.136	-0.060	0.048
	(0.043)	(0.077)	(0.069)	(0.057)
N	2,154	2,489	3,535	2,731
Control for age in 1996	Linear trend	Linear trend	Linear trend	Linear trend

Notes: See Table A1.

Table A6: Testing the statistical significance of gender differences in narrow sample

	First sex	Concurrency	Transactional	Skilled occupation	HIV
Exposure 1	1.050***	-0.006	-0.016	-0.119	-0.099*
	(0.285)	(0.081)	(0.016)	(0.066)	(0.049)
N	5,807	5,519	6,723	4,643	6,266
Exposure 2	1.634**	-0.046	-0.006	-0.039	-0.108
	(0.560)	(0.121)	(0.033)	(0.089)	(0.101)
N	5,807	5,519	6,723	4,643	6,266
Control for age in 1996	Linear	Linear	Linear	Linear	Linear

Notes: All models are fully interacted. They include a full set of age dummies, district fixed effects, one exposure model, a female dummy and interaction terms between the female dummy and all other variables. Only the interaction terms between the exposure measure and the female dummy are reported in the table. Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996. Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.

Robustness check 3: Estimating logit models

Table A7: The impact of the school reform on concurrency (logit marginal effects)

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.035	-0.035	-0.035	0.049	0.049	0.049
	(0.027)	(0.027)	(0.027)	(0.038)	(0.038)	(0.038)
N	3,713	3,713	3,713	2,973	2,973	2,973
Exposure 2	-0.042	-0.042	-0.042	0.097	0.097	0.097
	(0.040)	(0.040)	(0.040)	(0.065)	(0.065)	(0.065)
N	3,713	3,713	3,713	2,973	2,973	2,973
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: See Table A1.

Table A8: The impact of the school reform on transactional sex (logit marginal effects)

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	0.004	0.004	0.004	0.037***	0.037***	0.042***
	(0.008)	(0.008)	(0.009)	(0.007)	(0.007)	(0.010)
N	4,100	4,100	4,100	2,732	2,732	2,732
Exposure 2	0.016	0.016	0.014	0.063***	0.063***	0.070***
	(0.013)	(0.013)	(0.016)	(0.014)	(0.014)	(0.019)
N	4,100	4,100	4,100	2,732	2,732	2,732
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: See Table A1.

Table A9: The impact of the school reform on having a skilled occupation (logit marginal effects)

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	0.034	0.034	0.035	0.088***	0.088***	0.094***
	(0.027)	(0.027)	(0.025)	(0.026)	(0.026)	(0.023)
N	2,533	2,533	2,533	2,982	2,982	2,982
Exposure 2	0.079**	0.079**	0.077**	0.108**	0.108**	0.111**
	(0.039)	(0.039)	(0.036)	(0.051)	(0.051)	(0.052)
N	2,533	2,533	2,533	2,982	2,982	2,982
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: See Table A1.

Table A10: The impact of the school reform on HIV infection (logit marginal effects)

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.058**	-0.058**	-0.059**	-0.024	-0.024	-0.027
	(0.026)	(0.026)	(0.028)	(0.018)	(0.018)	(0.017)
N	4,288	4,288	4,288	3,348	3,348	3,348
Exposure 2	-0.093**	-0.093**	-0.090*	-0.047	-0.047	-0.048
	(0.046)	(0.046)	(0.048)	(0.029)	(0.029)	(0.030)
N	4,288	4,288	4,288	3,348	3,348	3,348
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: See Table A1.

Table A11: Testing the statistical significance of gender differences

	Concurrency	Transactional sex	Skilled occupation	HIV
Exposure 1	-0.086***	-0.022**[a]	-0.069*	-0.016
	(0.033)	(0.011)	(0.041)	(0.034)
N	6,686	7,455	5,515	7,636
Exposure 2	-0.131***	-0.027	-0.051	-0.014
	(0.047)	(0.021)	(0.076)	(0.061)
N	6,686	7,455	5,515	7,636
Control for age in 1996	Linear	Linear	Linear	Linear

Notes: All models are fully interacted. They include a full set of age dummies, district fixed effects, one exposure model, a female dummy and interaction terms between the female dummy and all other variables. Only the interaction terms between the exposure measure and the female dummy are reported in the table. Exposure 1 assumes that everyone was in their age-appropriate grade at the time of the reform, and Exposure 2 uses 1995 Education Statistics to compute the probability of being exposed to the reform based on age and gender in 1996.Significance levels are indicated by * p<0.1; ** p<0.05; *** p<0.01.
a) The Davidon-Fletcher-Powell algorithm was used for maximisation instead of Stata's default, the Newton-Raphson algorithm

Table A12: With 6–9 year olds and 21 year olds in the sample and without BAIS 2013

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure 1	-0.072**	-0.075**	-0.072**	-0.036*	-0.014	-0.036*
	(0.027)	(0.029)	(0.027)	(0.020)	(0.027)	(0.020)
N	4,272	4,272	4,272	3,322	3,322	3,322
Exposure 2	-0.102***	-0.111**	-0.097***	-0.057*	-0.011	-0.057*
	(0.033)	(0.040)	(0.020)	(0.030)	(0.044)	(0.030)
N	4,272	4,272	4,272	3,322	3,322	3,322
Control for age in 1996	Linear trend	Quadratic trend	Survey dummies	Linear trend	Quadratic trend	Survey dummies

Notes: See Table A1.