Education, HIV, and Early Fertility: Experimental Evidence from Kenya*

Esther Duflo Pascaline Dupas Michael Kremer

October 8, 2012

Abstract

A seven-year randomized trial among 19,000 Kenyan adolescents suggests education subsidies reduce dropout rates, teen pregnancy, and marriage, but not sexually transmitted infection (STI). Increased exposure to the government's abstinence-focused HIV curriculum does not reduce STI or pregnancy. Combining the two programs reduces STI risk, but cuts dropout and fertility rates less than subsidies alone. These results are inconsistent with a one-factor model in which teens simply choose rates of unprotected sex, but consistent with a richer model in which teens jointly choose schooling, and whether to be in committed or more casual sexual relationships.

JEL Codes: O12, I12, I38

*We are grateful to Abhijit Banerjee for his invaluable input, as well as to Frank Schilbach and seminar participants at Brown, Duke, Harvard, NBER and BREAD for helpful comments. We thank the Kenya Ministry of Education, the Kenya Institute of Education, ICS Africa, and IPA Kenya for their collaboration. We are extremely grateful to Abdulla Al-Sabah, Jeff Guo and Zhaoning Wang for outstanding research assistance; to Carolyne Nekesa, Grace Makana, and their field team for their dedication and scrupulous care collecting the data; and to Dr. Vandana Sharma for her invaluable support with the biomarkers data collection. We are also grateful to a long list of outstanding field research assistants. We list them in chronological order: Jessica Leino, Jessica Morgan, Owen Ozier, Ian Tomb, Paul Wang, Willa Friedman, Anuja Singh, Jinu Koola, Jessica Leight, Bastien Michel, Sara Hernandez, and Thomas Ginn. The funding for this study was provided by (in alphabetical order): the Hewlett Foundation, the MacArthur Foundation, the NIH, the Nike Foundation, the Partnership for Child Development, and the World Bank. We thank them, without implicating them, for making this study possible.
Duflo: MIT Economics Department and NBER: eduflo@mit.edu; Dupas: Stanford Economics Department and NBER, pdupas@stanford.edu; Kremer: Harvard Economics Department and NBER, mkremer@fas.harvard.edu.

1 Introduction

Early fertility and sexually transmitted infections (STIs), chief among them HIV, are arguably the two biggest health risks facing teenage girls in sub-Saharan Africa.[1] Since unprotected sex can lead to both early pregnancy and STIs, it is often assumed that any policy that reduces unprotected sex will be effective at fighting both. This ignores, however, that sex is not an undifferentiated commodity: one can choose how much sex to have, how much protection to use, and whether to be committed to a single partner or to have multiple partners. A young woman who chooses to be faithful to an uninfected partner but not to use condoms nor the pill will not get an STI, but is very likely to become pregnant; while another woman who often uses protection but chooses to have multiple partners whose status she does not know may not be very likely to become pregnant, but may well become infected with an STI. Taking these choices into account is essential to understanding the impacts that different interventions can have on teen pregnancy and STI.

This paper provides experimental evidence on how these two risks are affected by two leading policy instruments (and their interaction): education subsidies and HIV prevention education focused on abstinence until marriage. In conjunction with the Kenya Ministry of Education, the Kenya National AIDS Control Council, and the non-profit ICS Africa, we conducted a large randomized trial with 328 schools in Western Kenya to compare the effectiveness of two programs conducted stand-alone or jointly: 1) the *Education Subsidy* program, which subsidized the cost of education for upper primary school students by providing two free school uniforms over the last three years of primary school; and 2) the *HIV Education* program, which consisted in government-provided training of three teachers per primary school to help them deliver the national HIV/AIDS curriculum, which focuses on abstinence until marriage as the way to prevent infection. We assess the short-, medium- and long-term impacts of these two programs, implemented alone or jointly, on sexual behavior, fertility, and infection with HIV and another STI, Herpes Simplex Virus type 2 (HSV2), using a panel dataset that covers a cohort of over 19,000 youths, half of them girls, over 7 years. For both HIV and HSV2, prevalence at a point in time reflects the total number of people who were ever infected with the disease.

At the onset of our study, in 2003, youths in our sample were 13.5 years old on average and constituted the entire universe of sixth graders in the 328 schools enrolled in the study. At endline, these youths were 20.5 years old on average, and the majority were out of school.

[1] Pregnancy in adolescence is associated with greater risks for the mother as well as the child, including premature delivery (Trussell and Pebley, 1984). While part of this association reflects the fact that teenage pregnancy is more common among those socially and economically disadvantaged, there is a clear direct causal impact of biologic immaturity (Fraser et al., 2005).

Despite the gap between the onset of the study and the endline data collection, we achieved a very high follow-up rate overall. During a first wave of endline surveying, 54% of the study youths could be interviewed and almost all of them agreed to be tested for HIV and HSV2. A random subsample of 29% of the remainder was then selected for intensive tracking, and 81% of them could be found and surveyed, for an effective follow-up rate at endline of 91%.[2] The resulting data set is unique due to the combination of its size, the length of the panel, the successful tracking rate, the availability of biomarkers for HSV2 and HIV, and the randomized two-by-two design.

We find a nuanced, and at first blush puzzling, set of results:

1. When implemented alone, the education subsidy program significantly reduced pregnancy rates among girls by 17% in the first three years (from 16% to 13%). After 7 years, girls who had benefited from the subsidy were still 7% less likely to have started childbearing than their counterparts in the control group (46% with the subsidy versus 49% without). They were 18% less likely to have dropped out of primary school (just over 25% with the subsidy versus 30% without). However, their HSV2 infection rate, at 12.7%, was statistically instinguishable from that of the control group (11.8%)..

2. When implemented alone, the HIV education program did not significantly reduce either the teenage pregnancy rate or the risk of HSV2 infection. It did not affect schooling attainment. However, it reduced the rate of out-of-wedlock teenage pregnancy by 1.4 percentage points or roughly 30%.

3. When the two programs were implemented jointly, fertility fell *less* than when the education subsidy was provided alone, but HSV2 infections fell significantly. Girls who benefited from both programs were 20 percent less likely to be infected with HSV2 than the control group after 7 years.

These results are surprising for two main reasons. First, the STI and fertility results are not aligned. The only intervention that reduced STI prevalence (the joint program) is not the program that had the largest impact on pregnancy (the stand-alone education subsidy). Second, the effect of the two programs is not always additive: in particular, while neither the stand-alone education subsidy nor the stand-alone HIV education program had any impact on STI, the joint program led to a significant drop in HSV2 infections, and we can reject additivity. Clearly, these results are not compatible with a simple one-factor model in which girls only choose how much unprotected sex to have, and this in turns determines both

[2] In other words, for 91% of the youths in our initial sample, we have either personal survey information, or survey information for a representative youth.

fertility and STI jointly. These results are however consistent with a richer model of sexual behavior among teenage girls that we develop in the last section of the paper.

The model we develop has three main features. The first important feature is that we consider sex as a differentiated commodity. Namely, young women can choose between abstinence, "committed" sex (in which they are committed to a single partner who is also committed to them, and the relationship will lead to marriage in the event of a pregnancy), and casual sex, in which there is no anticipation of marriage, even if the girl becomes pregnant. The second feature of the model is that schooling and pregnancy are incompatible. This is a reasonable feature since, in many settings, including ours, pregnant girls historically have been expelled from school.[3] This makes the opportunity cost of being pregnant high for girls financially able and willing to go to school, and less so for girls who (or whose parents) have already chosen not to invest in schooling. The third feature is that sex entails a risk of STI as well as of pregnancy. For a given level of sexual activity, girls perceive the risk of STI infection to be higher when having casual sex than when in a committed relationship. What's more, the cost of pregnancy is lower (or the benefits are greater) within committed than within casual relationships. Under these three features, girls in our model choose how much education to get, whether to have a committed or casual relationship, and how much unprotected sex to have within any relationship. Since in a committed relationship the risk of STIs is lower and the cost of pregnancy is also lower, girls tend to have more unprotected sex in committed relationships than in casual relationships.

The model generates a series of comparative statics consistent with the data. First, when the cost of education decreases, as with the education subsidy program, girls invest more in education. This leads them to have less unprotected sex (to avoid pregnancy), conditional on choosing committed or casual relationships. The utility gained from a lower cost of education is higher in the context of casual relationships than in committed relationships (since the benefit of pregnancy is higher in committed relationships), therefore education subsidies shift people from committed to casual relationships. Thus, when education is subsidized, all the forces lead in the direction of a reduction in teenage pregnancy. This may not reduce STIs, however, if, given the intensity of sexual activity chosen, sex is relatively riskier in casual relationships than in committed relationships (due to different partner characteristics, for example).

Second, when the perceived STI risk from casual relationships increases, as with the abstinence-til-marriage message of Kenya's national HIV prevention curriculum, unprotected sex within casual relationships decrease. But a change in perceived STI risk from casual sex

[3]See Baird, McIntosh and Ozler (2011) for evidence that marriage/fertility and schooling are mutually exclusive in Malawi, and Ozier (2011) for additional evidence from Kenya.

also causes some girls to shift from casual to committed relationships. Because unprotected sex is more frequent within committed relationships, this second effect increases pregnancies within marriage. Overall, the total effect on teenage pregnancies, and on STIs, is ambiguous. However, the effect on unwed pregnancies is unambiguously negative.

Finally, when both programs are implemented jointly, girls want to avoid getting pregnant to take advantage of the cheap education, but they also think that casual relationships are risky. They may thus be less likely to shift from committed to casual relationships than under a pure education subsidy. Regardless, they will be having less unprotected sex than in the absence of any program. This should reduce both STIs and pregnancies relative to the control group. The effects may be larger or smaller than under the stand-alone education subsidy program, however, depending on the actual risk of STI infection in casual relationships, as well as the benefit of pregnancies within committed relationships. As we show through a numerical example, for appropriate parameter values, the joint program leads to a smaller reduction in pregnancies than the stand-alone education subsidy, but to a larger reduction in STI risk, matching the empirical results.

The model helps to interpret our results, but it also helps suggest how people might respond to alternative programs. In particular, in our model, an information campaign that increases the perceived risk of HIV associated with all types of unprotected sex (both committed and casual) would unambiguously reduce STI risk, and also increase educational attainment. Another important implication of the model is that an education message focusing on the difference between sex within and outside marriage may increase early marriages. In our data, the decline in single-parenthood pregnancies caused by the stand-alone HIV education program is matched by an increase in early pregnancy within marriage, leaving the overall teen pregnancy rate unchanged.[4]

With its wealth of data and interplay between well-identified program impacts and theory, our paper contributes to, and brings together, three distinct strands of literature.

First, we contribute to the literature on the link between education and fertility behavior. In developing countries, studies have generally found a strong causal relationship between increases in education and reduced fertility (see for example Breierova and Duflo (2002) in Indonesia, Osilii and Long (2008) in Nigeria, and Lavy and Zablotsky (2011) on Israeli-Arab women). Our finding that girls at the margin of dropping out of school in Kenya are able to delay pregnancy when the cost of education is reduced suggests that, for that age group, fertility and schooling decisions are often jointly made. This is consistent with recent evidence from India presented in Jensen (2012), who finds that an increase in the

[4] In the US, Frazer et al. (2005) argue that teen pregnancy has negative consequences for the teenage mother and her child within marriage.

perceived returns to education for young women ledto an increase in educational attainment and reduction in marriage and fertility.[5]

Second, we contribute to the literature on prevention of sexually transmitted infections among African youths. It is sometimes feared that involuntary sex is so common that education or incentive programs focusing on girls will have no impact on STIs. Yet, there are surprisingly few rigorous studies with objective biomarker outcome data on this issue (see MacCoy et al., 2010, for a review). The most promising results to date come from a study in Malawi, in which cash transfers to the families of out-of-school girls significantly reduced HIV infection rates after 18 months (Baird et al., 2012). Scaling up cash transfer programs for adolescent girls throughout Africa would be prohibitively expensive, however. At this point in time, the only types of interventions that are financially palatable to scale through a government program are information campaigns (especially school-based ones). On that front, an early community trial in rural Tanzania found that an adolescent sexual health program significantly affected HIV knowledge and attitudes, as well as some behaviors, but did not consistently reduce impact on STI outcomes in either the short- or the long-run (Ross et al., 2007; Doyle et al. 2010). With 20 communities, the study may have lacked power, however. Dupas (2011), in the same setting as ours, shows that informing girls that cross-generational relationships are particularly risky is more effective in reducing early pregnancies than Kenya's official abstinence-until-marriage message curriculum, but she does not have STI data.

Third, we contribute to the literature on the link between education and health behavior. Evidence from both developed countries (see Cutler and Lleras-Muney, 2009, for a review) and developing countries (Thomas, Strauss and Henriques, 1991) suggest that greater educational attainment reduces unprotected sexual behavior. The evidence to date on the relationship between educational attainment and HIV status in Sub-Saharan Africa is mixed, however.[6] In the Kenyan context, we find that for girls at the margin of dropping

[5]In developed countries the evidence is more mixed. Currie and Moretti (2003), Black et al. (2008), Monstad et al. (2008) all find strong impacts, while McCrary and Royer (2011), using cutoff for age at school entry, do not. But as Conti et al. (2012) show, the difference may come from the fact that different studies estimate different parameters and for different subpopulations: McCrary and Royer (2011) exploit an extra year of education that takes place in early childhood, while the other studies, like ours, estimate the effect of being in school as a teenager or young woman.

[6]Using nationally representative DHS surveys, Fortson (2008) finds evidence that education is positively correlated with HIV infection. De Walque (2007) finds that, in Uganda, the more educated were more likely to change their behavior in response to the national HIV risk information campaign than those with less education. Iorio and Santaeulalia-Llopis (2011) use DHS data from 18 countries to test whether the relationship between education and HIV status varies as the HIV epidemic progresses, and find evidence of nonstationarity, with the relationship being positive at both the early and very late stages of the epidemic, and negative at intermediate stages. Outside of the HIV literature, Jensen and Lleras-Muney (2011) finds that a randomized intervention that increased schooling among men in the Dominican Republic reduced

out of school, the increase in educational attainment brought about by the education subsidy was not sufficient, by itself, to reduce exposure to STIs.

The remainder of the paper proceeds as follows. Section 2 provides some background on the context and the study design. Section 3 describes the data. We discuss the short- and medium-run results in Section 4, and the long-run results in Section 5. Section 6 presents a model of sexual behavior and schooling decisions that can account for the findings. Section 7 concludes.

2 Background: Context and Study Design

2.1 Background

Though Kenya abolished school fees in 2003, primary education still entails other expenditures. All children in Kenyan schools wear uniforms, and at around \$6 (or 1.6% of per-capita GDP) in 2003, uniforms constitute by far the largest out-of-pocket cost of education in government primary schools. Historically, headmasters often sent children home telling them that they could only return when they had a uniform. With the introduction of free primary education, the Ministry of Education announced that this practice should not continue, but *de facto,* students face strong social pressure to wear a uniform and most do. The provision of free uniforms has been shown to reduce absenteeism in younger grades (Evans, Kremer and Ngatia, 2009).

While enrollment in the early grades of primary school is nearly universal, many students drop out before completing eighth grade, especially girls. In the comparison schools in our sample, about 30% of girls and 21% of boys who reach sixth grade drop out before completing eighth grade.

Conditional on being out of school, marriage is a potentially more attractive outcome than staying at the parental home, where teenagers bear a large share of household chores. Premarital sex is relatively common, including in rural areas, and not particularly taboo. On the other hand, Western Kenya has strong norms against sex and marriage among even distant relatives. Traditional marriage involves a brideprice, but most couples "elope" without a brideprice payment and without a traditional ceremony. Couples are considered married if they are living together, and we use that definition in this paper. Finally, students who become pregnant are expelled from school.

In 1999, AIDS was declared a "national disaster" by President Moi, and the government established a national HIV/AIDS curriculum for primary school. The curriculum was

risky behavior (such as heavy drinking and smoking), mainly by changing subjects' resources and peers.

developed with the assistance of UNICEF, based on extensive consultation within Kenyan society, including religious groups. The curriculum teaches basic facts about AIDS, HIV transmission, prevention, and care for people living with AIDS, as well as "life skills" such as saying no and resisting peer pressure. It stresses abstinence until marriage followed by faithfulness in marriage as the most effective way to prevent infection with sexually transmitted diseases. Contraceptive methods are not mentioned in the official textbook. Condoms can be discussed in class at the teacher's discretion or in response to questions, but the curriculum does not encourage teachers to discuss or demonstrate condoms, even to students in the upper grades, who are adolescents, and often sexually active.[7] This type of curriculum is far from unique to Kenya: stressing abstinence as the most appropriate behavior among teenagers is the norm in many African countries, and increasingly in the US as well.

2.2 Study Design

We study two programs implemented through a partnership between the NGO ICS Africa, the Kenyan Ministry of Education, the Kenya Institute of Education, and the Kenya National Aids Control Council. The first program reduced the cost of education by providing free school uniforms. The second program trained teachers on how to deliver the national HIV/AIDS prevention curriculum to upper primary school students.

Sample and Randomization The study took place in all 328 public primary schools in seven divisions of two districts of Western Kenya: Butere-Mumias and Bungoma.[8] None of these schools had participated in any prior randomized experiment. All schools agreed to participate. Schools were stratified and assigned to one of four arms using a random number generator: (1) Control (82 schools); (2) Stand-Alone Education Subsidy program (83 schools); (3) Stand-Alone HIV Education program (80 schools); (4) Joint Program (83 schools).[9]

Table 1 presents school-level summary statistics by treatment group. Only four of 65 p-values testing for differences across treatment groups are smaller than 0.10, suggesting

[7]Since grade repetition and late school entry are common, many students are 15, 16, or even older, by the time they reach the end of primary school in eighth grade.

[8]These districts have recently been split into multiple districts. We use the 2003 names.

[9]Block randomization (stratification) was used. The following variables were used to create the strata: administrative zone, the quartile in which the school performance fell in the 2002 national examination, and whether the school's gender ratio among upper primary pupils was above or below the median in 2002. 76 of the 82 strata had exactly 4 schools in them, and in those strata schools were randomly assigned to experimental arms using a random number generator. In the three strata with three schools, the experimental arm that was dropped was randomized. Likewise, in the three strata with five schools, the experimental arm that was included twice was randomized.

that the randomization was effective in creating balance between the groups.

Education Subsidy Between January and July 2003, ICS distributed free school uniforms to boys and girls enrolled in grade 6 at the onset of the school year (January).[10] In fall 2004, ICS distributed a second uniform to the same students, if they were still enrolled in the same school (regardless of their grade). It was announced at the onset of the program that students still enrolled in the same school would be eligible for a second uniform after 18 months.[11]

HIV education In 2002 the Kenya government started a large-scale effort to train teachers, based in part on data suggesting that in the absence of training, many teachers were uncomfortable teaching the official HIV/AIDS curriculum. In 2003, ICS Africa helped implement the national training program for 184 primary schools by providing logistical and financial support. These 184 chools selected for the HIV Education program were asked to send three upper primary teachers to participate in a five-day training program.[12]

The training sessions were conducted jointly by one facilitator from the AIDS Control Unit of the Ministry of Education (MoE), two facilitators from the Kenya Institute of Education (KIE), and one trained staff member from ICS. Teacher training included basic facts on HIV/AIDS, a condom demonstration, information on voluntary counseling and testing, and AIDS education methodology. Because training was primarily done by MoE and KIE teams and was based on the officially approved curriculum, it should be similar to what teachers in other parts of Kenya received. In addition to delivering HIV information in the classroom, teachers were advised to set up health clubs to deliver HIV information outside of the classroom. A year after the training, 86% of the schools in the program had established health clubs.[13]

[10] Baseline enrollment was collected from all schools before announcing the education subsidy program, and only those on the original baseline enrollment list were eligible for free uniforms. We did this to avoid creating incentives for transfers across schools, and it worked – we observed negligible levels of transfers across treatment groups.

[11] It was not logistically feasible to find students who transferred schools and provide them with a uniform.

[12] There are 14 teachers per school on average, so the training program covered around 21% of teachers in program schools. Schools were encouraged to send at least one female teacher to the training; headmasters were encouraged to attend themselves or to send their deputy. Compliance with the training was high, with 93% of invitees attending.

[13] As far as we know, the remainder of the schools in the study area have not yet received the government training, as government efforts to train teachers stalled in the mid 2000s.

3 Data and Estimation Strategy

3.1 The Data

The sample consists of 19,289 students (9,487 girls and 9,802 boys) enrolled in grade 6 at the onset of the study. Two types of data were collected: short- and medium-run data on school enrollment and attendance, marital status and childbearing status through school visits between 2003 and 2007; and a rich array of longer-run data collected through a follow-up survey with study participants in 2009-2010.

3.1.1 Short- and Medium-Run Outcomes: "Roll Call" Data

Seven unannounced school visits were conducted over five years. At each visit, the list of all those in our baseline sample was read aloud to upper-grade students present at the time of the visit, and for each name on the list, data on attendance were collected, and for absent students, the following questions were asked: Is X still in school? If yes, in what grade? If no, does she still live in the area? Is she married? Does she have any children? If so, how many? How old is her first born? Is she currently pregnant? We use this roll call data to create dummy variables for "dropout", "ever married", "ever pregnant", etc.

To check whether this roll call method generates accurate data on childbearing and marital outcomes, a random subsample of 1,420 girls were visited at home in 2006 for a quality control exercise. We oversampled girls reported as having started childbearing. Information was collected from the subject in 44% of cases and in other cases it was collected from a relative, typically the mother. Appendix Table A1 presents the rates of consistency between the roll call data and the data collected through the quality control exercise, as well as how these rates vary across treatment groups. Since there was up to a four month gap between the roll call and the home visit data, the home visit data is itself not 100% accurate, and therefore the consistency between the two would not be 100% even if the roll call data was perfect. Given this, the level of consistency appears high. 83% of those who were reported as not having started childbearing had indeed not started, and 79% of those who were reported as having started childbearing by their former schoolmates had indeed started childbearing. The longer the time between the roll call and the home visit, the lower the consistency rate, unsurprisingly. The consistency level is greater when we look at the "ever had a child" outcome (rather than ever started childbearing, which includes current pregnancies). Overall, the roll call method appears to provide remarkably accurate information (if we take the information obtained through home visits as "true"). Importantly, the level of consistency between the two sources does not appear to vary across groups.

3.1.2 Long-Run Outcomes: The Long-run Biomarkers Follow-Up Survey

In 2009-2010, more than six years after the two programs had taken place, a long-run follow-up was conducted, including measurement of two biomarkers: HIV and Herpes Simplex Virus Type 2 (HSV2). Herpes is almost exclusively sexually transmitted and is a serious disease in its own right. Herpes can create lesions which are believed to facilitate HIV transmission (Grosskurth et al., 1995; Corey et al., 2004). It leads to the lifelong presence of antibodies against HSV2 in the blood, thereby providing a permanent marker of having ever been infected with HSV2, and thus an objective proxy for relatively risky sexual behavior in the past (Obasi, 1999). HSV2 was selected as the primary biomarker because a pilot study in a similarly aged cohort found greater than 10% HSV2 prevalence, but much lower prevalence of other sexually transmitted infections (Chlamydia, Gonorrhea, Trichomonas Vaginalis and HIV were all less than 5%.)

The long-run follow-up survey was administered either at the respondent's home, or at a local meeting location to which the respondent had been invited. The survey included questions on sexual behavior, past and current sexual partners, marriage, and fertility, as well as educational attainment. At the end of the follow-up survey, respondents were given a voucher for a free HSV2 test to be performed at a mobile clinic located at the local meeting location. To collect data on HIV serostatus, all respondents who attended the mobile clinic were also asked to participate in anonymous linked HIV testing. In addition, half of the study participants had been pre-selected randomly to be offered voluntary counseling and HIV testing (VCT). VCT was done at the end of the survey by the surveyors, who were all government-certified VCT counselors.

Conditional on being successfully tracked for the follow-up survey, compliance with biomarker testing (HSV2 and anonymous linked HIV testing) was remarkably high, at 97% on average, and comparable across groups. Compliance with VCT was also very high: 87% of girls and 88% of boys surveyed agreed to receive VCT, and this was not differential across groups.

The long-run follow-up survey started in March 2009. By August 2010, 10,651 youths (55% of the study cohort) had been successfully tracked. This is a relatively high tracking rate given the challenges in locating members of a mobile population of school-leavers, many of whom had married outside their initial villages. Of those tracked, 97.5% had been interviewed, 2% had been identified as dead, and less than 1% had refused to be interviewed or were deemed mentally unfit for the interview. The tracking rate was higher among boys (59%) than girls (51%), due to a combination of factors. First, because the society is patrilocal, boys were more likely to still live at home with their parents, and thus easier to find. Second, conditional on having moved to another location within the study area, boys were easier to track than girls.[14]

In August 2010, 29% of the 8,657 untracked respondents were randomly sampled for "intensive tracking."[15] Between September 2010 and March 2011, teams of field officers and lab technicians traveled to various locations in Kenya and Uganda to interview selected respondents at their current homes. 77.5% of girls and 84% of boys sampled for intensive tracking were successfully surveyed. This brings the effective tracking rate (in the terminology of Orr et al., 2003 and Baird et al., 2011) to $0.51 + 0.49 \times 0.775 = 89\%$ for girls, and $0.59 + (0.41) \times (0.84) = 93\%$ for boys.[16]

3.2 Attrition

Appendix Table A2 shows attrition in the roll-call data. There is no evidence of differential attrition for any outcome, except for dropout information after five years.

Appendix Table A3 shows attrition in the long-term follow-up data. Survey rates during the first phase of tracking (the regular tracking, or RT, phase) were significantly higher in the treatment groups than in the control group, especially among girls (column 2). Column 3 presents estimates of the survey rates, showing that intensive tracking (IT) rates were insignificantly greater among those exposed to either program than among the control group, and significantly greater under the joint program than in the control group. All in all, the sample that could be followed up after 7 years over-represents those that received the education subsidy (column 4). Incorporating sampling weights (column 5) does not solve this problem fully for girls in the joint program arm, since the intensive tracking success rate was higher in that arm.

To test whether attrition in the long-run follow-up survey was differential in terms of underlying, unobserved characteristics, we check whether the treatment effects observed in the roll call data are changed when we estimate them on the follow-up subsample rather than the full sample. The idea is that, if attrition in the long-run follow-up biased the comparability of our groups in any way, then short-run treatment effects estimated using that subsample would differ from the "true" short-run treatment effects estimated on the

[14]This is because girls are less likely to stay in touch with their parents or guardians once they have moved, especially if they have moved to marry.

[15]We randomly sampled 1/3 of those still in their district of origin and 1/5 of those outside their home district.

[16]While not as succesful as the 95% *household* re-contact rate achieved in Indonesia by IFLS3 (Thomas et al., 2012), our effective individual tracking rate of 91% after 7 years compares favorably with already highly successful panel data collection efforts conducted in the same area of Western Kenya with youths. Following up on 7,500 children sampled in 1998, Baird et al. (2011) achieved an effective tracking rate of 85% after 6 years, and 83% after 10 years. Following up on around 3,000 adolescent girls sampled in 2001, Friedman et al. (2011) achieved an effective tracking rate of 80% after 4 years.

full sample. We perform this analysis in Table A4 for one outcome (having ever started childbearing). We find that the estimates of the short- and medium-run treatment effects on childbearing measured through the roll call method are very much comparable when estimated on the full sample for which roll call data is available (columns 1 and 4 – those are the "true" effects), or on the subsample for which long-run data could be collected after 7 years (columns 2 and 6 – again, these would be biased estimates of the true effects if attrition were differential). Not surprisingly since the estimates are virtually identical in the full sample and the sample with attrition, the sampling weights correction, while decreasing precision, does not affect the estimates much (columns 3 and 7). All in all, the estimated short-run treatment effects using only the long-run follow-up sample are virtually identical to those using the full sample, even without using the sampling weights. This means that our long-run follow-up sample is representative of our baseline sample and therefore we can confidently attribute differences in long-run outcomes to treatment effects.

3.3 Estimation Strategy

The impact of the two stand-alone programs and the joint program can be evaluated by comparing outcomes across groups in a simple regression framework. For each individual-level outcome, the estimation equation is:

$$Y_{is} = \alpha + \beta U_s + \gamma H_s + \delta UH_s + X_s^{'}\mu + \eta Age_i + \varepsilon_{is} \tag{1}$$

where Y_{is} is the outcome for student i enrolled in school s at baseline. U_s is a dummy variable equal to 1 for schools in the stand-alone education subsidy arm; H_s is a dummy variable equal to 1 for schools in the stand-alone HIV education arm; and UH_s is a dummy variable equal to 1 for schools in the joint program arm. X_s is a vector of school level controls (timing of the data collection, school size, and randomization stratum). Age_i is student i's age. Error terms are assumed to be independent across schools, but are allowed to be correlated across observations in the same school (i.e. the standards errors are clustered at the school level). We present results with sampling weights, which ensure that our final follow-up database is representative of (almost) the entire initial study population.[17]

In equation 1, β measures the effect of the stand-alone education subsidy; γ measures the effect of the stand-alone HIV education program; and δ measures the effect of the joint program.

In all tables that follow, we present estimates of equation 1 for a series of outcomes. In

[17]Not surprisingly given the attrition results discussed above, the results are very similar (and more precise) when the estimation is done without sampling weights.

each table, Panel A presents the estimates for girls and Panel B presents the estimates for boys. At the bottom of each panel, we show the mean of the dependent variable for the control group, and we present the p-values for tests of the hypotheses that the effect of the joint program is equal to that of either of the two stand-alone programs, or equal to the sum of the two stand-alone programs (i.e., we test for $\beta = \delta$, $\gamma = \delta$ and $\gamma = \beta + \delta$). We report OLS linear probability model regressions without school-level controls besides strata dummies and school size, but results are essentially identical with additional school-level controls (such as teacher-pupil ratio, school performance on national exams, whether the school is rural or semi-urban, etc.). The results are also qualitatively unchanged with probit or logit specifications (results available upon request).

4 Short- and Medium Run Results: Roll Call Data

Table 2 presents estimated effects within 3 to 5 academic years after the onset of the study, obtained from the roll call data. Because the outcomes for girls are much better measured than for boys (in particular, paternity status is definitely not as observable nor verifiable as maternity status), we focus much of our attention on girls, though we report the results for boys for completeness. In what follows we discuss, in turn, the impacts of each program on girls, before briefly turning to the results for boys.

4.1 Impacts of the stand-alone education subsidy program

The stand-alone education subsidy program helped marginal girls remain in school (Table 2, column 1). While 18.8% of girls in control schools had dropped out after three years, those in the education subsidy program were 3.1 percentage points less likely to do so, a 16.5% decrease significant at the 5 percent level. Interestingly, the program did not affect attendance conditional on enrollment (column 2, Table 2), suggesting that students who avoided dropping out thanks to the education subsidy were able to attend school as regularly as other students.

The stand-alone education subsidy also substantially reduced teenage pregnancy. After three years, 16 percent of girls in the control group had ever been pregnant. This share was 2.7 percentage points (17%) lower in schools with the stand-alone subsidy (Table 2, column 4). After five years, there was still a gap of 4.4 percentage points in the likelihood of having ever been pregnant between the stand-alone subsidy group and the control group (Table 2, column 9). This gap is larger in absolute terms than after three years, but corresponds to a smaller treatment effect in percentage terms, since the childbearing rate in the control group rose from 16% in year 3 to 33% in year 5 for the control group . Nonetheless, it suggests that fertility among girls in the subsidy group did not "catch up" as soon as the subsidy ended. Black, Devereux and Salvanes (2004) refer to an "incarceration effect" of schooling on teen fertility. We can reject the hypothesis that demand for fertility was simply bottled up during the program years when girls were in school and released afterwards, and we can reject the hypothesis that the hazard rate of starting childbearing was the same in the stand-alone education subsidy group and the comparison group after year 3, when girls who did not repeat grades would have left school.[18]

Columns 5 and 6 of Table 2 show that the stand-alone education subsidy exclusively reduced within-marriage pregnancy, not unwed (i.e., single-parenthood, given our definition of marriage) pregnancy. This suggests that the pregnancies averted were primarily among girls who, had it not been for the education subsidy, would have dropped out of school and settled in a committed relationship. Indeed, girls in the subsidy program were 2.6 percentage points (20%) less likely to be married (column 3).

Overall, the reduction in the number of girls who started childbearing was almost as large as the reduction in the number who dropped out of school. It would seem tempting to use columns 1 and 4 of Table 2 (or columns 7 and 9) as, respectively, the first stage and the reduced form of an instrumental variable strategy of the effect of education on early fertility. However, this would not be legitimate: this instrumental strategy would be valid only under the assumption that the education subsidy had no direct effect on the pregnancy status of girls. Under the model we develop in section 6, this identification restriction will not hold because providing free uniform reduces education costs even for girls who would not have dropped out. The model implies that that will induce some girls who would not have dropped out to delay sexual activity (or use contraception) to avoid becoming pregnant, suggesting that an instrumental variable estimate of the impact of education on early fertility using our experiment would be biased upwards. Nevertheless, the fact that the reduced form estimates of the effect of reduced education costs on averted dropout and averted fertility are approximately the same size suggests school costs have large impacts on adolescent behavior, especially the onset of fertility, in Kenya.

4.2 Impacts of the stand-alone HIV education program

Despite the emphasis on abstinence in the HIV education curriculum, the teenage pregnancy rate did not fall in response to the stand-alone HIV education program. The point estimate

[18]Among girls who had not yet started childbearing by year 3, 15.7% got pregnant between years 3 and 5 in the comparison group. This proportion was 2.5 percentage points lower (the difference being significant at the 5 percent level) in the stand-alone education subsidy group.

of program impact on pregnancy is close to zero after 3 years, and almost exactly zero after 5 years (Table 2, row 2, column 4). Coefficient estimates of the impact of the stand-alone HIV education program on schooling attainment are also very small in magnitudes (column 1). We can reject equality with the effects of the stand-alone education subsidy program at a level below 1% in all cases.

Note that this is not because teachers did not implement the curriculum – we have both qualitative and quantitative evidence that they did. The qualitative evidence comes from focus group discussions (not shown).[19] The quantitative evidence comes from a self-administered survey, distributed among students enrolled in grades 7 and 8 in 2005. We show this data in Appendix Table A5. The HIV education program increased the likelihood that students report that teachers mentioned HIV in class and moderately increased students' knowledge about HIV. It also increased the likelihood that students mention faithfulness as a means to prevent HIV, confirming that the HIV education program increased the perceived gap in HIV risk between casual and commited relationships.

One possible clue for an explanation of the lack of effect of the HIV education program on teen pregnancy is its effect on marriage. While insignificant, the point estimate for the effect on marriage is positive, at +1 percentage point, or 8% after three years, and +2.2 percentage points or 8% after five years. In contrast, unwed pregnancy is 1.4 percentage points lower among teenage girls exposed to the HIV Education curriculum (columns 6 and 11). This corresponds to a drop of 30% over the first three years and 18% over the first five years.[20]

Taken together, these results suggest that the stand-alone HIV Education program led girls to switch from casual to "committed" relationships, and to reduce unprotected sex in casual relationships. The model wer propose in section 6 predicts exactly this.

[19]Focus group discussions were held with students enrolled in grades 6 and 7 in early 2004, 12 to 18 months after teacher training. The focus group discussions were conducted separately with 5 boys and 5 girls randomly chosen among students present in class during an unannounced visit. Overall, these discussions confirmed that the teacher training increased the likelihood that teachers talked about HIV in the classroom. In particular, students in schools where teachers had been trained were 50% more likely to report that at least one teacher had mentioned HIV/AIDS in the previous week.

[20]It is unclear what the consequences will be, either for mothers or for their children, of the decline in unwed / single-parenthood teen births in the stand-alone HIV education group. There is a strong negative association between being raised by a single mother and outcomes in the U.S. (Harper and McLanahan, 2004; Ellis et al., 2009), but it is unclear how much of this effect is causal. In any case the impact could potentially be different in a rural African setting, in which care within the extended family setting in particular, by grandparents, is common, especially if the mother is a teenager. Case and Ardington (2006) and Evans and Miguel (2007) find negative effects of paternal orphanhood on children in a similar setting, but being born to a single-mom may be different from becoming a paternal orphan after birth.

4.3 Impacts of the joint program

Arguably the most surprising result in the roll call data is that the HIV education program seemed to reduce the impact of the education subsidy, both on schooling and on fertility. Indeed, the effect of the joint program (Table 2, row 3) on dropout is only half that of the stand-alone subsidy, and not significant at conventional levels (the t-statistic for the effect on education is 1.33 after 3 years (column 1) and 1.5 after 5 years (column 7)). After 3 years, we cannot reject the hypothesis that the effect of the joint program is the sum of the effects of the two stand-alone programs, but after 5 years, we can reject additivity (the effect of the joint program is lower than the sum of the two stand-alone programs).

When it comes to pregnancy risk, the estimated effect of the joint program is less than half that of the stand-alone education subsidy. Again, that effect is not significant after three years (column 4). Two years later, the effect of the joint program on pregnancy had diminished even further compared to that of the stand-alone education subsidy (column 9).

Finally, the point estimate suggests that girls who received the joint program were exactly as likely to be married as girls who received neither program (column 3). For pregnancy and marriage, we can reject the null hypothesis that the effects are the same for the joint program and for the stand-alone education subsidy. We cannot reject the null hypothesis that the joint program has the same effect as the stand-alone HIV education program, however. For most outcomes, we also cannot reject the hypothesis that the effect of the joint program equals the sum of the effects of the two stand-alone programs.

4.4 Impacts on boys

While the HIV education program did not affect either marriage or fertility for boys, both the stand-alone education subsidy and the joint program affected boys: they led to small but significant reductions in early marriage and fertility. This is despite very low marriage and reported paternity rates among boys over the sample period (Panel B of Table 2).

These effects could be direct program effects or the equilibrium consequences of changed incentives for their female classmates. We favor the first explanation. First, because we see an impact of the education subsidy on boys' dropout rate. Dropout among boys fell 2.5 percentage points in the stand-alone education subsidy group, corresponding to a 19% reduction compared to the control group. The effect of the joint program on dropout was smaller and not significant at conventional levels, but still negative and relatiely large, at 12%.

Second, the effect on marriage and fertility for boys appear too big to be an equilibrium consequence of the changed incentives for the girls in their class. Teenage girls typically marry men who are about six years older on average, not their classmates. Our follow-up data suggests that in the comparison group, only 4% of the 12.8% of girls who had married by year 3 (so overall 0.5% of girls) had married a boy their age or up to one year older. Even under the extremely conservative assumptions that (1) these are all within-school marriages (which is itself unlikely since marriage among even distant relatives is not culturally allowed and students in a given school are often related to each other), and (2) this proportion falls to zero in the stand-alone education subsidy group, the GE effect would explain only about half of the fall in marriage we observe among boys.

5 Long-Run Effects: The Biomarkers and Survey Data

5.1 Education, Marriage, and Fertility

The estimations of equation (1) on the long-run outcomes measured through the follow-up survey are presented in Tables 3, 4 and 5. In all these tables, we control for year of birth and randomization strata, as in Table 2, but also for the timing of the follow-up survey (since the follow-up survey spanned a 24-month window).

Reassuringly, the results on the schooling, marriage and fertility outcomes presented in Table 3 are consistent with the roll call data results observed in the shorter run. Namely, the stand-alone education subsidy increased educational attainment and decreased the risk of teenage pregnancy and marriage. The probability of being pregnant by age 16 falls by 2.2 percentage points in the stand-alone education subsidy group, a magnitude similar to the 2.7 percent reduction in teen pregnancy found in the roll call data after three years. The fertility effect, which persisted from the three-year to the five-year followup, continues through the seven-year followup, although estimates are noisy. The point estimate of the probability of having ever started childbearing in the long-run follow-up is 3.3 percentage points lower under the stand-alone education subsidy, but it is significant at the 12% level only. As in the roll call data, the stand-alone HIV education program had no significant impact on schooling and overall fertility. Finally, the joint program had the previously discussed muting effect on the ability of the education subsidy to improve schooling and decrease pregnancy for girls.

5.2 Long-Run Impacts on Sexually Transmitted Infections

Besides confirming the roll call results, the key new piece of evidence provided by the long-run follow-up data concerns the results for sexual health. These results are presented in Table 4. Column 1 shows the estimated program effects on infection with HIV, and column

2 show those for infection with HSV2.[21] The first interesting result is that HIV infection in our cohort is remarkably low – at less than 1% among both boys and girls in the control group. While it implies that this particular study, despite its very large sample size, is not powered to estimate the impact of the programs on HIV transmission, this low infection rate is extremely good news, and an important result in its own right. The overall STI risk level is not negligible, however, as evidenced by the relatively high rate of infection with HSV2, at 11.8% among girls and 7.4% among boys in the control group (column 2).

Despite reducing teen pregnancy, the stand-alone education subsidy program did not decrease HSV2 infection. In fact, the estimated coefficient for girls is positive, though not statistically significant. This is not a statistical power issue: for girls, we can reject a reduction of 1.8 percentage points in the risk of being infected with HSV2 at the 5% level, a 15% reduction. At best, any reduction in STI is therefore very modest. Likewise, the stand-alone HIV education program also did not have any significant impact on HSV2 infection for either gender, and once again the point estimate is positive. The joint program, in contrast, reduced HSV2 prevalence by 2.3 percentage points (with 10% significance), corresponding to a 19% decrease compared to the control group. We can reject the equality of the effect of the joint program and either stand-alone programs, as well as the equality of the joint program and the sum of the two stand-alone programs. For boys, however, the joint program had no significant effect on HSV2 infection rates.

Note that we cannot know the effect of the various programs on lifetime STI risk. We cannot, for example, rule out the possibility that girls who marry later will be exposed to the same risk of infection within marriage but have a greater chance of infection prior to marriage. Alternatively, girls who marry later may have more power within the relationship and therefore a lower risk of infection within marriage.

5.3 Summary of Results

Overall, the observed effects of the stand-alone education subsidy program are all consistent with each other. Girls were more likely to stay in school, and less likely to become pregnant. They were less likely to get married early. There is however no reduction in their HSV2 infection rate.

Likewise, the observed effects of the stand-alone HIV education program are consistent

[21]We present the results of the rapid HIV tests administered by our own surveyors (all certified VCT counselors) to the random subset of respondents sampled for VCT. That is because the Elisa HIV tests done on the blood samples collected from all respondents were performed by our partnering lab with a considerable delay and the results have not yet been released to us. We are hoping to receive them within the next three months, but we do not expect the results to be any different on the full sample compared to the random subsample.

with each other: teacher training on the HIV curriculum had no effect on HSV2 infection or pregnancies, but reduced the number of unwanted pregnancies.

What poses a puzzle is the effect of the joint program. Why did the joint program have a smaller effect on girls' teenage pregnancies than the stand-alone education subsidy, but a larger effect on HSV2 prevalence? In the next section, we propose a model which, under reasonable assumptions, predicts the pattern of results we observe in our data.

6 Model

The results suggest that a simple one-factor model in which teens simply choose how much unprotected sex to have, and this determines both pregnancy and STI, may be misleading. Instead, we propose a model in which girls decide how much to invest in education, whether to have a committed relationship or a casual relationship, and how often to have unprotected sex. When choosing how much unprotected sex to have, girls take into account the risks of sexually transmitted infection and pregnancy. Discussions with adolescent girls suggest a key factor influencing choice in this context is that girls who become pregnant are often expelled from school, and our model focuses on this.

6.1 Setup

Relationships can be either "committed" (that is, with a fixed partner, with the view of future marriage, particularly in case of pregnancy), indexed by m, or casual, indexed by c. Assume that these two categories are mutually exclusive: girls cannot be in a committed and a casual relationship at the same time.

Let κ_i denote the preference for a relationship of type i in the absence of pregnancy or HIV risk. It is useful to separately analyze the benefits and costs of unprotected sex by relationship type. Denote the benefit of a relationship of type i with a level of unprotected sex s in the absence of pregnancy or STI risk as $u_i(s) + \kappa_i$. Note that $u_i(s)$ includes any equilibrium inducements from the partner. We assume that in the absence of STI and pregnancy risks, there is some optimal level of unprotected sex $\overline{s}$, so $u_i'(\cdot) > 0$ for all s less than $\overline{s}$, and that above $\overline{s}$ $u_i'(\cdot) < 0$. We also assume $u_i''(\cdot) < 0$. We do not rule out the possibility that $u_i'(0) < 0$, so some girls prefer abstinence.

Unprotected sex carries risks of both STI and pregnancy. The cumulative perceived probability of getting an STI is $\pi(s, a_i)$, where $\pi \in [0, 1]$ is increasing in the level of unprotected sex, s, and a_i is the perceived chance of infection per unprotected sex act. This perceived chance of infection per unprotected sex act is indexed by i as it depends on the nature of the relationship, committed or casual. We assume that the risk of infection per unprotected act is lower in committed than casual relationships: $a_m < a_c$. Note that a_m and a_c should be thought of as different here because of differences in the riskiness of partners and because of potentially having multiple partners in a casual relationship. Also note that a_i, the *perceived* chance of infection per sex act, which is what enters the maximization problem, may be different from the actual probabilities of infection per unprotected sex act, which we denote as a_i^*.

For concreteness, we impose a specific functional form for the probability of STI as follows: $\pi(s, a_i) = 1 - (1 - a_i)^s$. Note that it is theoretically possible, given high a, that a further increase in a makes people fatalistic and thus they choose to have more unprotected sex (see for example Kremer, 1996). The prevalence of HIV in our study population is low enough that abstracting from fatalism in our context seems reasonable. To rule this out, we assume that the satiation point $\bar{s}$ is low enough so that the cross derivative with respect to s and a is positive, that is, $\frac{\partial^2 \pi(s,a)}{\partial s \partial a} > 0$. Given our functional form for $\pi(s, a)$, this condition holds if and only if $(1 - a)^{s-1} > 0$ and $s \ln(1 - a) + 1 > 0$, which implies that $s < \frac{-1}{\ln(1-a)}$ so that $\bar{s} \leq \frac{-1}{\ln(1-a)}$. This condition rules out the situation in which,

The cumulative perceived probability of pregnancy is $v(s, b)$, where $v \in [0, 1]$ is increasing in the level of unprotected sex, s, and the perceived chance of pregnancy per unprotected sex act, b, which is assumed independent of the relationship type. We impose the same specific functional form for the probability of pregnancy as for the probability of STI, that is, $v(s, b) = 1 - (1 - b)^s$.

Now that we have specified the probabilities for STI and pregnancy, we turn to the utility impacts of each of these two risks. Contracting an STI generates a utility cost D. Pregnancy, however, has both benefits and costs. The utility increment from having a child (including the possibility of marriage with the father of the child) is denoted B_i and depends on the type of relationship. Specifically, we assume that the utility of pregnancy is greater in committed than casual relationships: $B_m > B_c$. But pregnancy has an opportunity cost. That cost varies in the population and depends on the loss of future earnings. Potential earnings themselves depend on education investment e. We assume earnings are $y_0 + \theta_j y(e)$, with $y(0) = 0$, $y'(e) > 0$, $y''(e) < 0$, $\lim_{e \to 0} y'(e) = +\infty$, and $\lim_{e \to +\infty} y'(e) = 0$. Once a girl gets pregnant she cannot invest in education: she then earns y_0. Moreover, . We assume the return to education varies among girls due to differences in θ. We assume that the utility of having a child as a teenager is never, in itself, worth the opportunity cost of pregnancy: for all j, $B_i - \theta_j y(e)$ is negative for $i = m, c$. Finally let the cost of education be given by $C(e) = e\gamma$.[22]

[22]We assume that the cost of education is sunk at the beginning of the year but all our results go through

Girls may thus differ in θ (returns to education), B_m and B_c (the benefits of getting pregnant in committed and casual relationships), and γ (the cost of education). The utility for girl j under relationship type i and level of unprotected sex s is:

$$\begin{aligned} U_{i,j}(s,e) &= u_i(s) + \kappa_i - \pi(s,a_i)D + v(s,b)(B_{i,j} + y_0) + (1 - v(s,b))[y_0 + \theta_j y(e)] - e\gamma_j \\ &= u_i(s) + \kappa_i - \pi(s,a_i)D + v(s,b)B_{i,j} + (1 - v(s,b))\theta_j y(e) - e\gamma_j. \end{aligned}$$

We model the education subsidy as lowering the cost of education γ, and the HIV education program as increasing the perceived STI risk associated with unprotected sex in casual relationships, a_c. The next subsections provide comparative statics with respect to these two parameters.

But before we move on, it is worth mentioning a few limitations of the model. First, the model does not include a risk of rape. While this is a limitation, the comparative statics in the model would be qualitatively similar if all girls were subject to some constant risk of pregnancy or STI infection through rape, unless this risk were so large as to induce fatalism, which our data does not suggest. Second, all benefits of various types of relationships and of unprotected sex are taking the market equilibrium prices as given, which we assume do not change substantially when our parameters of interest, γ and a_c, change. This assumption is reasonable for the purpose of understanding the treatment effects in our field experiment, since only one cohort of students (those in grade 6 in 2003) was affected by the programs, representing only a minority of adolescents in a given village. Moreover, as discussed earlier, teenage girls rarely have sex with boys their age, and therefore the fact that boys were directly impacted by the programs is unlikely to have indirecly affected the market equilibrium for girls. Finally, strong norms against sex with even distant relatives mean most marriages are outside of the immediate neighborhood, and while the program covered many schools in the area, the treatment arm assignment of neigboring schools would not be correlated with the treatment arm of a student's own school.

6.2 The optimal choice of relationship type, unprotected sex, and education

To solve the utility maximization problem, one can first solve for the levels of s and e that maximize U_m conditional on having a committed relationship, then solve for the levels of s and e that maximize U_c conditional on having a casual relationship, and finally compare the utility levels achieved in each.

if we remove this assumption.

To solve for the optimal level of s, e and relationship type, it is necessary to consider the Kuhn-Tucker conditions since the optimum could potentially be at a corner where $e = 0$ or $s = 0$. The appendix works through the full solution, while we focus here on comparative statics for the interior optima. As discussed in the appendix, comparative statics at the corners are either similar to those at the interior solution or uninteresting because there is no change in behavior in response to reductions in the cost of education or increases in the perceived chance of infection per unprotected sex act.

Interior optima must satisfy the following first-order conditions:

$$\begin{aligned}\frac{\partial U}{\partial s} &= u'(s) - \frac{\partial \pi(s,a)}{\partial s}D + \frac{\partial v(s,b)}{\partial s}[B - \theta y(e)] = 0 \\ \frac{\partial U}{\partial e} &= (1 - v(s,b))\theta y'(e) - \gamma = 0\end{aligned}$$

and the following second-order conditions:

$$\begin{aligned}\frac{\partial^2 U}{\partial s^2} &= u''(s) - \frac{\partial^2 \pi(s,a)}{\partial s^2}D + \frac{\partial^2 v(s,b)}{\partial s^2}[B - \theta y(e)] < 0 \\ \frac{\partial^2 U}{\partial e^2} &= (1 - v(s,b))\theta y''(e) < 0 \\ \frac{\partial^2 U}{\partial s \partial e} &= -\theta \frac{\partial v(s,b)}{\partial s} y'(e).\end{aligned}$$

Finally, the following condition implies concavity of the utility function:

$$[-\theta \frac{\partial v(s,b)}{\partial s} y'(e)]^2 < \left[u''(s) - \frac{\partial^2 \pi(s,a)}{\partial s^2}D + \frac{\partial^2 v(s,b)}{\partial s^2}[B - \theta y(e)]\right](1 - v(s,b))\theta y''(e).$$

6.3 Comparative Statics: Intensive Margins

We start with comparative statics conditional on relationship type, committed or casual, and then examine (in the next subsection) how changes in parameters affect switching of relationship type. We prove only the first lemma in the text: the other proofs are in the appendix.

Lemma 1 *When γ (the cost of education) increases, conditional on relationship type, the optimal e decreases and s increases.*

Proof. Taking the total derivative of the first-order conditions with respect to γ yields:

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e} de = 0$$
$$\frac{\partial^2 U}{\partial s \partial e} ds + \frac{\partial^2 U}{\partial e^2} de - d\gamma = 0.$$

Solving this expressions for $\frac{de}{d\gamma}$ and $\frac{ds}{d\gamma}$, we obtain:

$$\frac{-\left(\frac{\partial^2 U}{\partial s \partial e}\right)^2 + \frac{\partial^2 U}{\partial e^2}\frac{\partial^2 U}{\partial s^2}}{\frac{\partial^2 U}{\partial s^2}} = \frac{d\gamma}{de}$$
$$\frac{\left(\frac{\partial^2 U}{\partial s \partial e}\right)^2 - \frac{\partial^2 U}{\partial e^2}\frac{\partial^2 U}{\partial s^2}}{\frac{\partial^2 U}{\partial s \partial e}} = \frac{d\gamma}{ds}.$$

The second-order conditions allow us to sign these expressions: $\frac{de}{d\gamma} < 0$ and $\frac{ds}{d\gamma} > 0$. ■

Lemma 2 *When a (the perceived risk of STI) increases, conditional on relationship type, s decreases and e increases.*

Lemma 3 *When B (the benefit of pregnancy) increases, conditional on relationship type, s increases and e decreases.*

Lemma 4 *When θ (the return to education) increases, conditional on relationship type, s decreases and e increases.*

Since in committed relationships B is higher and a is lower, Lemmas 2 and 3 immediately imply that $s_m > s_c$ and $e_m < e_c$. That is, unprotected sex will be higher and education effort will be lower for girls who choose committed relationships than for girls who choose casual relationships.

6.4 Comparative Statics: Relationship Type

The next step is to assess how changes in the parameters a and γ affect which relationship type is chosen. First, note that lower returns to education make committed relationships more attractive and higher returns make casual relationships relatively more attractive.

Lemma 5 *Given a_c, a_m, B_m, B_c, and γ, then either:*
(1) all girls choose casual relationships or
(2) all girls choose committed relationships or
(3) there exists a threshold θ_t such that girls with $\theta < \theta_t$ choose committed relationships and girls with $\theta > \theta_t$ choose casual relationships.

Proof. Both U_c and U_m increase in θ, but at different rates: $\frac{dU_i}{d\theta} = (1 - v(s_i, b_i))y(e_i)$. Given the results in the previous section,$s_m > s_c$ and $e_m < e_c$. Therefore, $\frac{dU_m}{d\theta} < \frac{dU_c}{d\theta}$. This implies that, as long both types are chosen, there is a threshold level θ_t such that girl j chooses to engage in a committed relationship if and only if $\theta_j < \theta_t$. ■

The next step is thus simply to determine how the programs affect the threshold θ_t. The following two lemmas are proven in the appendix:

Lemma 6 *When γ (the cost of education) increases, θ_t (the threshold return to education above which girls choose casual relationships) increases.*

Lemma 7 *When a_c (the perceived chance of infection from an unprotected sex act in a casual relationship) increases and a_m does not change, θ_t increases.*

These results are intuitive. Reductions in the cost of education, such as under the education subsidy program, increase utility in both committed and casual relationships, but increase utility more in casual relationships (because e is larger in casual relationships). This leads more girls to choose casual relationships, and thus lowers the threshold return to education beyond which casual relationships are chosen. Meanwhile, increasing the perceived risks associated with casual sex, as the official HIV prevention curriculum does, decreases the relative value of casual relationships and thus increases the threshold below which girls choose committed relationships.

6.5 Predictions for the impacts of the stand-alone programs

We are now ready to consider these two margins together and discuss the overall impact of the programs on educational attainment, pregnancies, and STIs.

Proposition 1 *The stand-alone education subsidy program unambiguously reduces pregnancy and increases educational attainment. The effect on STIs depends on the (real) relative riskiness of casual and committed relationships.*

Since the education subsidy reduces the cost of education, it reduces s and increases e for girls in both casual and committed relationships. Moreover, it increases the number of girls who choose casual relationships, in which e is higher, over committed relationships. The education subsidy thus unambiguously increases education and reduces teenage pregnancy. The effect on STIs will depend on the real chance of infection per unprotected sex act in casual and committed relationships, a_c^* and a_m^*. If a_c^* is greater than a_m^*, education subsidies could potentially increase STI risk by inducing a shift to casual partners.

In section 7 of the appendix, we introduce a numerical example which shows the ambiguous effect of the education subsidy on STIs. In our example the education subsidy reduces STIs for girls who choose a committed relationship, while increasing STIs for those who it induces to choose casual relationships. In general, the population effect is ambiguous since it depends on the relative size of each population type.

Proposition 2 *The stand-alone HIV Education program increases early marriages but the impact on pregnancy and education is ambiguous. The effect on STI depends on the actual relative riskiness of casual and committed relationships.*

An increase in the perceived risk of STI per casual sex act, a_c, due to the HIV education program reduces unprotected sex and increases educational effort for those who choose casual relationships. Committed relationships become relatively more attractive, and so some switch to those relationships. Since the level of unprotected sex is higher in committed than in casual relationships, the effect on teenage pregnancy is ambiguous. However, there could be a decline in the number of unwed teen pregnancies, as long as breakups in committed relationships are rare (i.e, as long as partners considered as committed are, indeed, committed). The effect on STIs is ambiguous. It depends on the actual relative riskiness of committed and casual relationships and the extent to which unprotected sex is more frequent in committed relationships: if a_m^* is in fact not much lower than a_c^*, the effect of a reduction in casual relationships on STI rates may be outweighed by the effect of an increase in sexual activity within a committed relationship, even though each sexual interaction is safer in a committed relationship.

The model's predictions in propositions 1 and 2 fit the pattern for the main outcomes of interest in the data quite well: the education subsidy reduced dropout and teenage pregnancy rates, but not STI rates as much, perhaps because casual relationships are indeed somewhat more likely to lead to STIs than committed relationships. Training teachers on the official HIV curriculum did not reduce teenage pregnancies but reduced single-motherhood (at least in the short-run). It also did not reduce the STI risk, presumably because the increase in unprotected sex associated with moving to committed relationships counteracts the lower risk of transmission in committed relationships, holding the level of unprotected sex constant. Committed relationships are generally associated with older partners. Older men are more likely to be infected with HIV but girls are not always aware of it (Dupas, 2011).

6.6 Predictions for the joint program, and a numerical example

The most surprising and interesting set of results is the impact of the joint program. Recall that the joint program had a weaker effect on both dropout and teen pregnancy rates than the stand-alone education subsidy, but it reduced STI risk more than the stand-alone education subsidy. While the model's prediction on the joint program depends on the specific parameters and the utility function, it can indeed account for this set of results. To see this, observe that the joint program may either increase or decrease θ_t, because it pushes individuals in opposite directions: the education subsidy reduces the relative appeal of committed relationships, but the emphasis on abstinence in the official HIV prevention curriculum pushes people towards committed relationships. Because switching from committed to casual relationships reduces sexual activity and increases educational effort, the joint program may increase education and reduce pregnancy by less than the stand-alone education subsidy. However, the joint program will lead to a larger decrease in STIs than under education subsidies alone, because the decrease in sexual activity will be less likely to be counterbalanced by a switch to casual relationships. Thus, the model predicts that the effect of the joint program need not be equal to the sum of the two programs implemented in isolation.

The impact of the joint program on STIs depends on both perceived (through behavior) and actual relative riskiness of casual and committed relationships. In the model appendix, we propose a simple (and plausible) numerical example where the pattern of results replicates our current set of empirical results. We consider an example with four levels of θ such that in the control group, the two types with lower θ choose committed relationships, and the two types with higher θ choose casual relationships. The stand-alone education subsidy leads the second to lowest type to switch to casual relationships, and the stand-alone HIV education program leads the second to highest type to switch to committed relationships. However, under the joint program, it is optimal for each type to remain in their original type of relationship instead of switching. When the model's parameters are set to match pregnancy and STI rates in the control group, this model delivers pregnancy and STI rates for each treatment group similar to those observed in the data. Moreover, the direction and relative magnitudes of the effects of each treatment in the numerical example match what we observe in the experiment. In particular, with our parameterization, the joint program produces the biggest drop in STI rates, but it does not reduce pregnancy or dropout rates by as much as the stand-alone education subsidy program.

6.7 Additional evidence

The long-run follow-up survey includes self-reported sexual behavior data that can help check whether we observe some of the pathways for the effects predicted under the model. This data, shown in Table 5, needs to be treated with considerable caution, as self-reports are easily tainted by social desirability bias.[23] Nevertheless, there are some suggestive patterns that are consistent with the model. First, girls in the stand-alone education subsidy schools are significantly more likely to report that they have never had sex, and more likely (though not significantly) to report abstinence as one of the methods they use to protect themselves from HIV (columns 1 and 2). On the other hand, they are significantly less likely to report faithfulness as a way they use to protect themselves (column 3). They also have younger partners (consistent with the shift to casual sex and away from sex with "committed" older men, i.e., those able to marry them if a baby is conceived).

Second, girls in the stand-alone HIV education schools report having had their first sexual experience at a younger age than the control group and are significantly more likely to report faithfulness as a way they protect themselves from HIV. They are less likely (though not significantly) to report condom use and have slightly older partners. These results are broadly consistent with the model.

For the joint program, the results on self reported behavior are less clear-cut: we do not find indication of more abstinence or more condom use in that group than in the other two groups, which is what we would expect under the model.

Table 6 provides further suggestive evidence, by looking at HSV2 prevalence within marital status and education categories. Of course such categories are endogenous, and therefore comparing HSV2 rates across categories (i.e., across rows of Table 6) is not particularly informative. For example, married women are much more likely to be infected with HSV2 than unmarried women, but this likely reflects the fact that married women have more unprotected sex (now and probably in the past as well) – it does not necessarily mean that, conditional on the level of sexual activity, committed sex is riskier. What is potentially more informative, however, is to compare categories across experimental arms (i.e., make comparisons across columns of Table 6). For example, compared to the control group, under the stand-alone education subsidy young women are slightly more likely to be HSV2 positive within each category, which is consistent with them moving towards more risky, casual relationships. The reason why, despite this, the overall level of STI does not increase in the education subsidy group relative to the control group is that, as we saw earlier in Tables 3 and 4, more girls stay in school (and hence unmarried), the safest group. In contrast, prevalence is lower for girls in the joint program within each category, consistent with the model's prediction that the reduction in STIs does not come from a composition change, but from a decline in unprotected sexual activity within each category.

[23] For example, 4.6% of girls and 4.8% of boys who report that they never had sex test positive for HSV2 (this is still below the rate among those who report that they ever had sex, which is 14.6% on average, but this indicates considerable underreporting of sexual activity). These figures are shown in the bottom row of Table 6.

7 Conclusion

It is widely believed that improving the education of women is a critical step in reducing fertility and improving maternal and child health in developing countries. This paper sheds light on this important question. Using experimental data, we show that reducing the cost of education by providing free uniforms reduces school dropout, teen childbearing and early marriage. This suggests that girls have some agency regarding sexual activity. Whether girls defer childbearing, however, depends on their beliefs about the value of marriage. We find that HIV prevention curricula that focus on an abstinence-until-marriage message increase early marriage and counteract the effects of increased access to schooling on fertility.

More generally, our findings show that the interplay between perceived HIV risk, schooling opportunities and early fertility is complex, and policies that focus on only one of these issues at a time may have unanticipated effects. We find that combining an education subsidy with Kenya's abstinence-until-marriage curriculum can reduce the rate of STIs for young women, but it does not reduce their risk of early fertility and it does not increase their schooling attainment as much as the education subsidy implemented alone.

Of course, despite the fact that we followed up with our study participants for an unusually long period of time, our results are relatively "short-run." The youths in our sample are only at the onset of their adult life and we cannot speak to what their lifetime STI risk will be. More than 50% of the girls in our study cohort were not yet married as of our last survey. These girls will eventually marry and, depending on who they marry, might get infected through their spouse.

Nevertheless, our findings imply a particularly important role of the ability to stay in school: the reduction in teenage pregnancies obtained through the education subsidy alone is almost as large as the reduction it caused in school dropouts. This does not imply that every girl who did not drop out because of the program would have had a child otherwise; some girls who would have stayed in school if not pregnant may also have been induced by the program to remain sexually inactive or use contraception. Instead, this suggests that giving girls additional motivation to delay their first pregnancy (the opportunity to go to school if they want to do so) is an extremely powerful (and inexpensive) way to reduce early fertility. Most government and international efforts have focused on ease of access to basic education (up to grade 6 or 9). Our results suggest that education gains in the upper end of that range, or even secondary school, especially for girls, may have a much larger impact on reducing early fertility than we would expect based on the causal effect of years of primary education on early fertility.

References

[1] Baird, Sarah, Richard S Garfein, Craig T McIntosh, and Berk Özler (2012). "Effect of a cash transfer programme for schooling on prevalence of HIV and herpes simplex type 2 in Malawi: a cluster randomised trial". *The Lancet*. DOI: 10.1016/S0140-6736(11)61709-1

[2] Baird, Sarah, Craig McIntosh and Berk Özler (2011). "Cash or condition? Evidence from a Randomized Cash Transfer Program," *Quarterly Journal of Economics*, 126(4): 1709-1753.

[3] Baird, Sarah, Joan Hamory Hicks, Edward Miguel and Michael Kremer (2011). "Worms at Work: Long-run Impacts of Child Health Gains". Mimeo.

[4] Black, Sandra, Paul Devereux and Kjell Silvanes. (2008). "Staying in the classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births". *Economic Journal* 118 (July) 1025-1034.

[5] Breierova, L. and E. Duflo (2004). "The Impact of Education on Fertility and Child Mortality: Do Fathers Really Matter Less Than Mothers?" NBER WP #10513.

[6] Case, Anne, and Cally Ardington (2006). "The Impact of Parental Death on School Outcomes: Longitudinal Evidence from South Africa". *Demography* 43(3): 401-20.

[7] Conti, Gabriella, James J. Heckman, Hedibert F. Lopes, and Rémi Piatek (2012). "Constructing Economically Justified Aggregates: An Application to the Early Origins of Health." Forthcoming, *Journal of Econometrics.*

[8] Corey L, Wald A, Celum CL, Quinn TC. (2004). "The effects of herpes simplex virus-2 on HIV-1 acquisition and transmission: a review of two overlapping epidemics." *J Acquir Immune Defic Syndr*. 35(5):435-45.

[9] Cutler, David, and Adriana Lleras-Muney (2009). "Understanding Differences in Health Behaviors by Education". Forthcoming, *Journal of Health Economics.*

[10] De Walque, Damien(2007). "How does the impact of an HIV/AIDS information campaign vary with educational attainment? Evidence from rural Uganda," *Journal of Development Economics*, vol. 84(2), pages 686-714.

[11] De Walque, Damien (2009). "Does Education Affect HIV Status? Evidence from five African Countries," *The World Bank Economic Review*, 23(2): 209-233.

[12] Doyle AM, Ross DA, et al. (2010). "Long-Term Biological and Behavioural Impact of an Adolescent Sexual Health Intervention in Tanzania: Follow-up Survey of the Community-Based MEMA kwa Vijana Trial." PLoS Medicine, 7(6):e1000287

[13] Dupas, Pascaline (2011). “Do Teenagers Respond to HIV Information? Experimental Evidence from Kenya”. *American Economic Journal: Applied Economics* 3 (1): 1-36.

[14] Ellis, Bruce, John E. Bates, Kenneth A. Dodge, David M. Fergusson, L. John Horwood, Gregory S. Pettit, and Lianne Woodward (2009). “Does Father Absence Place Daughters at Special Risk for Early Sexual Activity and Teenage Pregnancy?” *Child Development* 74(3): 801–821.

[15] Evans, David, Michael Kremer, Muthoni Ngatia (2009). “The Impact of Distributing School Uniforms on Children’s Education in Kenya”, mimeo, World Bank.

[16] Evans, David and Edward Miguel (2007). “Orphans and Schooling in Africa: A Longitudinal Analysis,” *Demography* 44(1), 35-57.

[17] Fraser AM, Brockert JE, Ward RH. (1995) “Association of young maternal age with adverse reproductive outcomes.” *New England Journal of Medicine* (332): 1113 –1117

[18] Friedman, Willa, Edward Miguel, Michael Kremer and Rebecca Thornton (2011). “Education as Liberation?”. NBER Working Paper #16939.

[19] Fortson, Jane (2008). “The Gradient in Sub-Saharan Africa: Socioeconomic Status and HIV/AIDS,” *Demography*, 45(2): 303-322.

[20] Grosskurth, Heiner, Frank Mosha, et al. (1995). “Impact of Improved Treatment of Sexually Transmitted Diseases of HIV Infection in Rural Tanzania: Randomized Controlled Trial,” *The Lancet*, 346: 530-536.

[21] Harper, Cynthia, and Sara McLanahan (2004). “Father Absence and Youth Incarceration”. *Journal of Research on Adolescence*, 14(3): 369-97.

[22] Iorio, Daniela, and Raul Santaeulalia-Llopis (2011). “Education, HIV Status, and Risky Sexual Behavior: How Much Does the Stage of the HIV Epidemic Matter?” Mimeo, Washington University St Louis.

[23] Jensen, Robert (2012). “Do Labor Market Opportunities Affect Young Women’s Work and Family Decisions? Experimental Evidence from India,” forthcoming, *Quarterly Journal of Economics.*

[24] Jensen, Robert, and Adriana Lleras-Muney (2011). “Does Staying in School (and not Working) prevent Teen Drinking and Smoking?” Forthcoming, *Journal of Health Economics.*

[25] Kremer, Michael (1996). “Integrating Behavioral Choice into Epidemiological Models of the AIDS Epidemic”. *Quarterly Journal of Economics* 111(2): 549-73.

[26] Lavy, Victor, and Alexander Zablotsky (2011). “Mother‘s Schooling, Fertility, and Children‘s Education: Evidence from a Natural Experiment”. Mimeo.

[27] McCoy Sandra I, Abigail Kangwende, and Nancy S. Padian (2010). “Behavior change interventions to prevent HIV infection among women living in low and middle income countries: a systematic review.” *AIDS and Behaviour*, 14(3): 469-82.

[28] McCrary, J. and H. Royer (2011). “The Effect of Female Education on Fertility and Infant Health: Evidence From School Entry Policies Using Exact Date of Birth,” *American Economics Review* 101(1): 158-195.

[29] Obasi A, Mosha F, et al. (1999) “Antibody to herpes simplex virus type 2 as a marker of sexual risk behavior in rural Tanzania.” *Journal of Infectious Diseases* 179(1):16-24.

[30] Osili, Una Okonkwo and Long, B. T. (2008) “Does Female Schooling Reduce Fertility? Evidence from Nigeria.” *Journal of Development Economics* 87(1): 57-75.

[31] Orr, Larry, Judith D. Ferris, et al. (2003). Moving to Opportunity: Interim Impacts Evaluation. Washington D.C.: U.S. Dept. of HUD.

[32] Ozier, Owen (2011). “The Impact of Secondary Schooling in Kenya: A Regression Discontinuity Analysis,” Mimeo, UC Berkeley.

[33] Ross DA, Changalucha J, et al. (2007) “Biological and behavioural impact of an adolescent sexual health intervention in Tanzania: a community-randomized trial”. *AIDS* 21:1943–1955.

[34] Thomas, Duncan, Firman Witoelar, Elizabeth Frankenberg, Bondan Sikoki, John Strauss, Cecep Sumantri and Wayan Suriastini (2012). “Cutting the Costs of Attrition: Results from the Indonesia Family Life Survey”, *Journal of Development Economics* 98(1): 108–123.

[35] Thomas, Duncan, John Strauss and Maria-Helena Henriques (1991). “How does mother’s education affect child height?”. *The Journal of Human Resources*, (26):2: 183-211.

[36] Trussell, James and Anne R. Pebley. 1984. The Potential Impact of Changes in Fertility on Infant, Child and Maternal Mortality. *Studies in Family Planning*. 15(6): 267-280.

Model Appendix

Corner Solutions and Proofs

Corner solutions to the individual's utility maximization problem

First, consider the corner where the individual chooses no education, which yields the following optimization condition:

$$\frac{\partial U(s,e=0)}{\partial s} = u'(s) - \frac{\partial \pi(s,a)}{\partial s}D + \frac{\partial v(s,b)}{\partial s}[B - \theta y(e=0)] = 0.$$

The other corner is where the individual abstains from any level of unprotected sex, which yields the following optimization problem:

$$\frac{\partial U(s=0,e)}{\partial e} = (1 - v(s=0,b))\theta y'(e) - \gamma = 0.$$

Comparative statics with respect to the intensive margin

Lemma 1: For an interior solution, an increase in γ (the cost of education) reduces e and increases s. For a corner solution at $e = 0$, an increase in γ will not affect s. For a corner solution at $s = 0$, an increase in γ will reduce e.

Proof. The case for an interior solution is proven in the main text.

Now, suppose that we have a corner solution at $e = 0$. Then the first-order condition

$$u'(s) - \frac{\partial \pi(s,a)}{\partial s}D + \frac{\partial v(s,b)}{\partial s}[B - \theta y(e=0)] = 0$$

implies that

$$\frac{ds}{d\gamma} = 0.$$

Finally, suppose that we have a corner solution at $s = 0$. Then we have

$$\frac{\partial^2 U}{\partial e^2}de - d\gamma = 0$$

so that

$$\frac{de}{d\gamma} = \frac{1}{\frac{\partial^2 U}{\partial e^2}} < 0$$

as desired. ■

• **Lemma 2:** For an interior solution, an increase in a (the perceived riskiness of the relationship) reduces s and increases e. For a corner solution at $e = 0$, an increase in a will reduce s. For a corner solution at $s = 0$, an increase in a will not affect e.

Proof. First, suppose that we have an interior solution. We take the total derivative of the first-order conditions with respect to a and solve for $\frac{de}{da}$ and $\frac{ds}{da}$.

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e} de - \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D da = 0 \tag{2}$$

$$\frac{\partial^2 U}{\partial s \partial e} ds + \frac{\partial^2 U}{\partial e^2} de = 0. \tag{3}$$

Solving the system of equations for $\frac{de}{da}$:

$$\frac{\partial^2 U}{\partial s^2} \left(-\frac{\frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial s \partial e}} de \right) + \frac{\partial^2 U}{\partial s \partial e} de - \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D da = 0 \tag{4}$$

$$-\frac{\frac{\partial^2 U}{\partial s^2} \frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial s \partial e}} + \frac{\partial^2 U}{\partial s \partial e} = \frac{da}{de} \left[\frac{\partial^2 \pi(s,a)}{\partial s \partial a} D \right] \tag{5}$$

$$\frac{-\frac{\partial^2 U}{\partial s^2} \frac{\partial^2 U}{\partial e^2} + \left(\frac{\partial^2 U}{\partial s \partial e} \right)^2}{\frac{\partial^2 U}{\partial s \partial e} \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D} = \frac{da}{de}. \tag{6}$$

In the final expression, the concavity condition ensures that the numerator is negative. The denominator is negative since $\frac{\partial^2 U}{\partial s \partial e} < 0$ (as shown earlier), $\frac{\partial^2 \pi(s,a)}{\partial s \partial a} > 0$ (by definition), and $D > 0$ (by definition). Therefore, the overall expression is positive: $\frac{de}{da} > 0$.

Solving the system of equations for $\frac{ds}{da}$ yields:

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e} \left(-\frac{\frac{\partial^2 U}{\partial s \partial e}}{\frac{\partial^2 U}{\partial e^2}} ds \right) - \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D da = 0 \tag{7}$$

$$\frac{\partial^2 U}{\partial s^2} - \frac{\left(\frac{\partial^2 U}{\partial s \partial e} \right)^2}{\frac{\partial^2 U}{\partial e^2}} = \frac{da}{ds} \left[\frac{\partial^2 \pi(s,a)}{\partial s \partial a} D \right] \tag{8}$$

$$\frac{\frac{\partial^2 U}{\partial s^2} \frac{\partial^2 U}{\partial e^2} - \left(\frac{\partial^2 U}{\partial s \partial e} \right)^2}{\frac{\partial^2 U}{\partial s \partial e} \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D} = \frac{da}{ds}. \tag{9}$$

As previously shown, this expression is negative, hence $\frac{da}{ds} < 0$.

Now, suppose that we have a corner solution at $e = 0$. Then equation (2) can be rewritten as

$$\frac{\partial^2 U}{\partial s^2} ds - \frac{\partial^2 \pi(s,a)}{\partial s \partial a} D da = 0$$

so that

$$\frac{ds}{da} = \frac{\frac{\partial^2 \pi(s,a)}{\partial s \partial a} D}{\frac{\partial^2 U}{\partial s^2}} < 0.$$

Finally, suppose that we have a corner solution at $s = 0$. Then the first order condition

$$(1 - v(s = 0, b))\theta y'(e) - \gamma = 0$$

implies that

$$\frac{de}{da} = 0$$

as desired. ■

- **Lemma 3:** For an interior solution, an increase in B (the benefit of pregnancy) increases s and reduces e. For a corner solution at $e = 0$, an increase in B will increase s. For a corner solution at $s = 0$, an increase in B will not affect e.

Proof. First, suppose that we have an interior solution. As in the proof of Lemma 2, we take the total derivative of the first-order conditions with respect to B and solve for $\frac{de}{dB}$ and $\frac{ds}{dB}$.

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e} de + \frac{\partial v(s,b)}{\partial s} dB = 0 \tag{10}$$

$$\frac{\partial^2 U}{\partial s \partial e} ds + \frac{\partial^2 U}{\partial e^2} de = 0. \tag{11}$$

Solving the system of equations for $\frac{de}{dB}$:

$$\frac{\partial^2 U}{\partial s^2}\left(-\frac{\frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial s \partial e}}de\right)+\frac{\partial^2 U}{\partial s \partial e}de+\frac{\partial v(s,b)}{\partial s}dB=0 \tag{12}$$

$$-\frac{\frac{\partial^2 U}{\partial s^2}\frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial s \partial e}}+\frac{\partial^2 U}{\partial s \partial e}=-\frac{dB}{de}\frac{\partial v(s,b)}{\partial s} \tag{13}$$

$$\frac{\frac{\partial^2 U}{\partial s^2}\frac{\partial^2 U}{\partial e^2}-\left(\frac{\partial^2 U}{\partial s \partial e}\right)^2}{\frac{\partial^2 U}{\partial s \partial e}\frac{\partial v(s,b)}{\partial s}}=\frac{dB}{de}. \tag{14}$$

The numerator is positive (concavity condition). The denominator is $[-\theta\frac{\partial v(s,b)}{\partial s}y'(e)]\frac{\partial v(s,b)}{\partial s}$, which is negative since θ, $\frac{\partial v(s,b)}{\partial s}$, and $y'(e)$ are all defined to be positive. So, the overall expression is negative: $\frac{de}{dB}<0$.

Solving the system of equations for $\frac{ds}{dB}$:

$$\frac{\partial^2 U}{\partial s^2}ds+\frac{\partial^2 U}{\partial s \partial e}\left(-\frac{\frac{\partial^2 U}{\partial s \partial e}}{\frac{\partial^2 U}{\partial e^2}}ds\right)+\frac{\partial v(s,b)}{\partial s}dB=0 \tag{15}$$

$$\frac{\partial^2 U}{\partial s^2}-\frac{\left(\frac{\partial^2 U}{\partial s \partial e}\right)^2}{\frac{\partial^2 U}{\partial e^2}}=-\frac{dB}{ds}\frac{\partial v(s,b)}{\partial s} \tag{16}$$

$$\frac{-\frac{\partial^2 U}{\partial s^2}\frac{\partial^2 U}{\partial e^2}+\left(\frac{\partial^2 U}{\partial s \partial e}\right)^2}{\frac{\partial^2 U}{\partial s \partial e}\frac{\partial v(s,b)}{\partial s}}=\frac{dB}{ds}. \tag{17}$$

The numerator is negative (concavity condition). The denominator, as previously argued, is negative. So, the overall expression is positive: $\frac{ds}{dB}>0$.

Now, suppose that we have a corner solution at $e=0$. Then equation (10) can be rewritten as

$$\frac{\partial^2 U}{\partial s^2}ds+\frac{\partial v(s,b)}{\partial s}dB=0$$

so that

$$\frac{ds}{dB}=-\frac{\frac{\partial v(s,b)}{\partial s}D}{\frac{\partial^2 U}{\partial s^2}}>0.$$

Finally, suppose that we have a corner solution at $s=0$. Then the first order condition

$$(1-v(s=0,b))\theta y'(e)-\gamma=0$$

implies that

$$\frac{de}{dB} = 0$$

as desired. ■

• **Lemma 4:** For an interior solution, an increase in θ (the index of return to education) reduces s and increases e. For a corner solution at $e = 0$, an increase in θ will reduce s. For a corner solution at $s = 0$, an increase in θ will increase e.

Proof. First, suppose that we have an interior solution. To prove Lemma 4, we take the total derivative of the first-order conditions with respect to θ and solve for $\frac{de}{d\theta}$ (which will turn out to be positive) and $\frac{ds}{d\theta}$ (which will turn out to be negative).

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e} de - \frac{\partial v(s,b)}{\partial s} y(e) d\theta = 0 \tag{18}$$

$$\frac{\partial^2 U}{\partial s \partial e} ds + \frac{\partial^2 U}{\partial e^2} de + (1 - v(s,b)) y'(e) d\theta = 0. \tag{19}$$

Solving the system of equations for $\frac{de}{d\theta}$, first we solve for ds in the second equation:

$$\frac{\partial^2 U}{\partial s \partial e} ds = -\frac{\partial^2 U}{\partial e^2} de - (1 - v(s,b)) y'(e) d\theta \tag{20}$$

$$ds = \frac{-\frac{\partial^2 U}{\partial e^2} de - (1 - v(s,b)) y'(e) d\theta}{\frac{\partial^2 U}{\partial s \partial e}}. \tag{21}$$

Then we plug into the first equation:

$$\frac{\partial^2 U}{\partial s^2} \left[\frac{-\frac{\partial^2 U}{\partial e^2} de - (1 - v(s,b)) y'(e) d\theta}{\frac{\partial^2 U}{\partial s \partial e}} \right] + \frac{\partial^2 U}{\partial s \partial e} de - \frac{\partial v(s,b)}{\partial s} y(e) d\theta = 0 \tag{22}$$

$$\left[\frac{\frac{\partial^2 U}{\partial s^2} \frac{\partial^2 U}{\partial e^2} - \left(\frac{\partial^2 U}{\partial s \partial e} \right)^2}{\frac{\partial^2 U}{\partial e^2}} \right] ds + \left[\frac{-\frac{\partial^2 U}{\partial s^2} (1 - v(s,b)) y'(e) - \frac{\partial v(s,b)}{\partial s} y(e) \frac{\partial^2 U}{\partial s \partial e}}{\frac{\partial^2 U}{\partial e^2}} \right] d\theta = 0 \tag{23}$$

$$\frac{\frac{\partial^2 U}{\partial s^2} (1 - v(s,b)) y'(e) + \frac{\partial v(s,b)}{\partial s} y(e) \frac{\partial^2 U}{\partial s \partial e}}{-\frac{\partial^2 U}{\partial s^2} \frac{\partial^2 U}{\partial e^2} + \left(\frac{\partial^2 U}{\partial s \partial e} \right)^2} = \frac{de}{d\theta}. \tag{24}$$

The denominator is negative (concavity condition). The first term of the numerator is negative since $\frac{\partial^2 U}{\partial s^2} < 0$ by a second-order condition and $(1 - v(s,b)) y'(e) > 0$ by initial assumptions. The second term of the numerator is also negative since $\frac{\partial v(s,b)}{\partial s} y(e) > 0$ by initial assumptions and $\frac{\partial^2 U}{\partial s \partial e} = -\theta \frac{\partial v(s,b)}{\partial s} y'(e) < 0$ (also by initial assumptions). Since the numerator is composed of two negative terms, it too is negative. The overall expression then, with the negative numerator and denominator, is positive: $\frac{de}{d\theta} > 0$.

Solving the system of equations for $\frac{ds}{d\theta}$, first we solve for ds in the second equation:

$$\frac{\partial^2 U}{\partial e^2} de = -\frac{\partial^2 U}{\partial s \partial e} ds - (1 - v(s,b))y'(e)d\theta \tag{25}$$

$$de = \frac{-\frac{\partial^2 U}{\partial s \partial e} ds - (1 - v(s,b))y'(e)d\theta}{\frac{\partial^2 U}{\partial e^2}}. \tag{26}$$

Then we plug into the first equation:

$$\frac{\partial^2 U}{\partial s^2} ds + \frac{\partial^2 U}{\partial s \partial e}\left[\frac{-\frac{\partial^2 U}{\partial s \partial e} ds - (1 - v(s,b))y'(e)d\theta}{\frac{\partial^2 U}{\partial e^2}}\right] - \frac{\partial v(s,b)}{\partial s} y(e) d\theta = 0 \tag{27}$$

$$\left[\frac{\frac{\partial^2 U}{\partial s^2}\frac{\partial^2 U}{\partial e^2} - \left(\frac{\partial^2 U}{\partial s \partial e}\right)^2}{\frac{\partial^2 U}{\partial e^2}}\right] ds + \left[\frac{-\frac{\partial^2 U}{\partial s \partial e}(1 - v(s,b))y'(e) - \frac{\partial v(s,b)}{\partial s} y(e) \frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial e^2}}\right] d\theta = 0 \tag{28}$$

$$\frac{\frac{\partial^2 U}{\partial s \partial e}(1 - v(s,b))y'(e) + \frac{\partial v(s,b)}{\partial s} y(e) \frac{\partial^2 U}{\partial e^2}}{\frac{\partial^2 U}{\partial s^2}\frac{\partial^2 U}{\partial e^2} - \left(\frac{\partial^2 U}{\partial s \partial e}\right)^2} = \frac{ds}{d\theta}. \tag{29}$$

The concavity condition asserts that the denominator is positive. The first term of the numerator is negative since $\frac{\partial^2 U}{\partial s \partial e} = -\theta \frac{\partial v(s,b)}{\partial s} y'(e) < 0$ and $(1 - v(s,b))y'(e) > 0$ by initial conditions. The second term of the numerator is also negative since $\frac{\partial v(s,b)}{\partial s} y(e) > 0$ by initial assumptions and $\frac{\partial^2 U}{\partial s^2} < 0$ by a second-order condition. The numerator, composed of two negative terms, is negative. The overall expression, with the positive denominator, is negative: $\frac{ds}{d\theta} < 0$.

Now, suppose that we have a corner solution at $e = 0$. Then equation (18) can be rewritten as

$$\frac{\partial^2 U}{\partial s^2} ds - \frac{\partial v(s,b)}{\partial s} y(e) d\theta = 0$$

so that

$$\frac{ds}{d\theta} = \frac{\frac{\partial v(s,b)}{\partial s} y(e)}{\frac{\partial^2 U}{\partial s^2}} < 0.$$

Finally, suppose that we have a corner solution at $s = 0$. Then equation (19) can be rewritten as

$$\frac{\partial^2 U}{\partial e^2} de + (1 - v(s,b))y'(e)d\theta = 0$$

so that

$$\frac{de}{d\theta} = -\frac{(1 - v(s,b))y'(e)d\theta}{\frac{\partial^2 U}{\partial e^2}} > 0$$

as desired. ■

Comparative statics with respect to choice of relationship type

We first prove Lemma 6, which states that when γ (the cost of education) increases, θ_t (the threshold return to education above which girls choose casual relationships) increases.
Proof. θ_t is the value of θ which satisfies $U_c = U_m$. That is,

$$\begin{aligned} & u(s_c) - \pi(s_c, a_c)D + v(s_c, b_c)B_c + (1 - v(s_c, b_c))\theta_t y(e_c) - e_c\gamma \\ = \; & u(s_m) - \pi(s_m, a_m)D + v(s_m, b_m)B_m + (1 - v(s_m, b_m))\theta_t y(e_m) - e_m\gamma. \end{aligned}$$

We take the total differential with respect to $\theta_t, \gamma, s_m, e_m, s_c, e_c$:

$$\begin{aligned} & \frac{\partial U_c}{\partial s_c} ds_c + \frac{\partial U_c}{\partial e_c} de_c + (1 - v(s_c, b_c))y(e_c)d\theta_t - e_c d\gamma \\ = & \frac{\partial U_m}{\partial s_m} ds_m + \frac{\partial U_m}{\partial e_m} de_m + (1 - v(s_m, b_m))y(e_m)d\theta_t - e_m d\gamma. \end{aligned}$$

Taking into account the first-order conditions and solving for $\frac{d\theta_t}{d\gamma}$, we get:

$$\frac{e_m - e_c}{(1 - v(s_m, b_m))y(e_m) - (1 - v(s_c, b_c))y(e_c)} = \frac{d\theta_t}{d\gamma}. \tag{30}$$

The denominator is simply $\frac{dU_m}{d\theta} - \frac{dU_c}{d\theta}$, which is negative. The numerator is also negative, which confirms that $\frac{d\theta_t}{d\gamma} > 0$. ■

Finally, we prove Lemma 7, which states that when a_c (the perceived chance of infection from an unprotected sex act in a casual relationship) increases and a_m does not change, θ_t (the threshold return to education above which girls choose casual relationship) increases.

Proof. We take the total differential with respect to $\theta_t, a_c, s_m, e_m, s_c$, and e_c:

$$\frac{\partial U_c}{\partial s_c}ds_c + \frac{\partial U_c}{\partial e_c}de_c + (1 - v(s_c, b_c))y(e_c)d\theta_t - \frac{\partial \pi}{\partial a_c}Dda_c \tag{31}$$

$$= \frac{\partial U_m}{\partial s_m}ds_m + \frac{\partial U_m}{\partial e_m}de_m + (1 - v(s_m, b_m))y(e_m)d\theta_t$$

$$\frac{-\frac{\partial \pi}{\partial a_c}D}{(1 - v(s_m, b_m))y(e_m) - (1 - v(s_c, b_c))y(e_c)} = \frac{d\theta_t}{da_c}. \tag{32}$$

The numerator is negative since $\frac{\partial \pi}{\partial a_c}D > 0$. Hence, $\frac{d\theta_t}{da_c} > 0$. ■

Numerical Example

The following is a numerical example that shows that the set of results we observe in our data can be obtained under reasonable circumstances. It is neither a calibration nor a test of the model; instead, it is simply a numerical example to show that the model *can* deliver similar results to our empirical data.

Assume the following functional forms:

- $u(s) = 3.8s - 0.5s^2$
- $v(s, b) = 1 - (1 - b)^s$
- $\pi(s, a) = 1 - (1 - a)^s$
- $y(e) = 4\log(e + 1)$

We assume the following baseline parameter values: $D = 4$, $B_c = 3$, $B_m = 6.7$, $\kappa_{casual} = 1.65$, $\kappa_m = 0$, $a_c = a_c^* = 0.044$, $a_m = a_m^* = 0.03$, $b = 0.17$, and $\gamma = 0.36$. For θ, the return to education, we consider that the population is evenly distributed across four types: $\theta = 1.35, 1.4, 1.45$, and 1.5. With these baseline parameter values, we roughly match the pregnancy and STI rates observes in the control group.

We consider that the education subsidy program lowers the cost of education, γ, from 0.36 to 0.32. The HIV education program increases the perceived risk of contracting an STI when engaging in casual sex, a_c, from 0.044 to 0.051, while leaving a_m unchanged.

The chosen functional form for u: $u(s) = 3.8s - 0.5s^2$, implies that there is a satiation point for unprotected sex at $s = 3.8$, after which $u'(s) < 0$. This satiation point is sufficiently low to ensure that the cross derivative is positive at the optimum. Indeed, as shown in the main text, the cross derivative condition is $s < \frac{-1}{\ln(1-a)}$. Since the highest value of a we consider is 0.051, this condition is satisfied for any $s < 19.1$, and the satiation point $s = 3.8$ satisfies the condition.

In what follows, we first numerically solve the model for an interior solution for each θ type under each treatment (control, stand-alone education subsidy, stand-alone HIV education, or joint program), and then calculate the resulting STI rates, pregnancy rates, education attainment and utility levels for each type and for the overall population. We then solve for the corner solutions and show that the interior solution is optimal for all types.

Interior Solution

We first solve for the θ_t threshold under which committed relationships are preferred and above which casual relationships are preferred. As predicted, the education subsidy lowers the threshold while the HIV education program increases it:

	θ_t threshold
Control	1.4125
Stand-Alone Education Subsidy	1.3626
Stand-Alone HIV Education	1.4746
BothJoint Program	1.4236

Table 1: *Values of threshold θ_t under each program (interior solution)*

Thus, in the control group, half the population chooses a committed relationship (those with $\theta = 1.35$ and $\theta = 1.4$), and the other half chooses casual relationships. The education subsidy induces the type 2 girls (those with $\theta = 1.4$) to switch to casual relationships. The HIV education program induces the type 3 girls (those with $\theta = 1.45$) to switch from casual to committed relationships. The joint program induces no switching.

The following table provides STI rates that obtain for each θtype under each treatment, assuming an interior solution. For reference, bold indicates that the type of relationship chosen is casual (as per the threshold values estimated above). The last column shows the population average (which is simply the average across types since we assume the four types are equally prevalent.)

	$\theta = 1.35$	$\theta = 1.4$	$\theta = 1.45$	$\theta = 1.5$	Population
Control	0.0931	0.0908	**0.1013**	**0.0959**	0.0953
Stand-Alone Education Subsidy	0.0908	**0.1014**	**0.0959**	**0.0897**	0.0945
Stand-Alone HIV Education	0.0931	0.0908	0.0884	**0.0943**	0.0917
Joint Program	0.0908	0.0884	**0.0943**	**0.088**	0.0904

Table 2: *STI rates by θ-type under each program (interior solution)*

As in our data, the two stand-alone programs have a minimal impact on the STI rate, while the joint program reduces it substantially.

The table also illustrates how the two stand-alone programs each have an ambiguous effect on STI rates, as they affect different types in different directions. For example, the education subsidy program decreases the STI rate among girls who do not switch types of relationship. However, among those induced to switch from committed to casual relationships (the subgroup with $\theta = 1.4$), the education subsidy program increases STI rates. The population effect thus depends on the magnitude of the changes in STI rates, as well as the relative sizes of the population types. Under our parameter assumptions, the effect is slightly negative. In contrast, for the joint program, the STI rate unambiguously decreases for all types as there is no switching.

The following table provides the pregnancy rates by treatment and type. As above, bold indicates that casual relationships are chosen.

	$\theta = 1.35$	$\theta = 1.4$	$\theta = 1.45$	$\theta = 1.5$	Population
Control	0.45	0.4416	**0.3576**	**0.3414**	0.3976
Stand-Alone Education Subsidy	0.4415	**0.3577**	**0.3412**	**0.3225**	0.3657
Stand-Alone HIV Education	0.45	0.4416	0.4325	**0.3365**	0.4151
Joint Program	0.4415	0.4322	**0.3364**	**0.3171**	0.3818

Table 3: *Pregnancy rates by θ-type under each program (interior solution)*

The stand-alone education subsidy program clearly reduces the pregnancy rate, while the stand-alone HIV education program slightly increases it. The joint program decreases the pregnancy rate, but not as much as the stand-alone education subsidy.

The next table provides the education attainment by treatment and type:

	$\theta = 1.35$	$\theta = 1.4$	$\theta = 1.45$	$\theta = 1.5$	Population
Control	7.25	7.69	**9.35**	**9.98**	8.57
Stand-Alone Education Subsidy	8.43	**10.94**	**11.70**	**10.24**	10.33
Stand-Alone HIV Education	7.25	7.69	8.14	**10.06**	8.28
Joint Program	8.43	8.94	**11.03**	**11.80**	10.05

Table 4: Levels of education chosen by θ-type under each program (interior solution)

Analogously, the stand-alone education subsidy increases overall educational attainment, while the stand-alone HIV education program slightly decreases it. The joint program increases educational attainment, but not as much as the education subsidy alone.

Finally, we show the utility levels for each type under each treatment group. This is needed to rule out the corner solutions (which we solve below).

	$\theta = 1.35$	$\theta = 1.4$	$\theta = 1.45$	$\theta = 1.5$
Control	13.34	13.58	**13.86**	**14.17**
Stand-Alone Education Subsidy	13.66	**13.94**	**14.26**	**14.57**
Stand-Alone HIV Education	13.34	13.58	13.83	**14.11**
Joint Program	13.66	13.91	**14.21**	**14.55**

Table 5: Utility levels by θ-type under each program (interior solution)

Ruling out Corner Solutions

Corner 1: $s = 0$ **and** $e \geq 0$

The first-order condition with respect to e can be written as follows:

$$U(s = 0, e) = 4\theta \log(e + 1) - e\gamma + C$$
$$\frac{\partial U(s = 0, e)}{\partial e} = \frac{4\theta}{e + 1} - \gamma = 0$$
$$e = \frac{4\theta}{\gamma} - 1.$$

Plugging in the parameter values for θ and γ, we can easily compute the levels of education chosen by each type under each treatment and plug those back in the utility function. We obtain the following utility levels for each type under each treatment:

	$\theta = 1.35$	$\theta = 1.4$	$\theta = 1.45$	$\theta = 1.5$
Control	11.23	11.78	12.33	12.89
Stand-Alone Education Subsidy	11.83	12.40	12.97	13.56
Stand-Alone HIV Education	11.23	11.78	12.33	12.89
Joint Program	11.83	12.40	12.97	13.56

Table 6: Utility levels by θ-type, under each program (corner solution with s=0)

Comparing the utility levels in Table 6 to those in Table 5, it is clear that the corner solution with $s = 0$ is dominated for all types under all treatments.

Corner 2: $s \geq 0$ **and** $e = 0$

The first-order condition with respect to s can be written as follows:

$$U(s, e=0) = -0.5s^2 + 3.8s - \left[1 - (1-a)^s\right] D + \left[1 - (1-b)^s\right] B + C$$
$$\frac{\partial U(s, e=0)}{\partial s} = -s + 3.8 + (1-a)^s \ln(1-a)D - (1-b)^s \ln(1-b)B = 0.$$

Clearly this is independent of the type θ, and therefore all types adopt the same sexual behavior. What's more committed relationships dominate under all treatments, therefore everyone chooses committed relationships and the same level of unprotected sex. The resulting utility level is 10.30 for everyone under all treatments. Comparing this to utility levels in Table 5, it is clear that the corner solution with $e = 0$ is dominated by the interior solution for all types under all treatments.

Table 1. Baseline Characteristics, by Treatment Group

Panel A. Baseline Characteristics of Schools	Stand-Alone Education Subsidy (S)	Stand-Alone HIV Education (H)	Joint Program (SH)	Control (C)	Balance Tests p-val for test that: S = C	H = C	SH = C	SH = S	SH = H
Average Score on Primary School Graduation Exam in 2003	255.2	249.4	248.6	249.3	*0.097**	*0.66*	*0.468*	*0.146*	*0.857*
	[29.6]	[24.8]	[32.04]	[26.1]					
School Size (total number of students)	464.6	489.3	473.8	498.9	*0.292*	*0.777*	*0.587*	*0.764*	*0.611*
	[203.1]	[208.8]	[185.7]	[194.3]					
Sex Ratio (Female/Male) among Students in 2002	1.016	1.024	1.012	1.016	*0.945*	*0.455*	*0.823*	*0.857*	*0.556*
	[.124]	[.127]	[.105]	[.135]					
Number of Latrines on school compound	11.6	11.2	9.9	11.1	*0.215*	*0.635*	*0.081**	*0.068**	*0.173*
	[6.3]	[6.4]	[5.7]	[5.5]					
Number of primary schools within 2 km radius	2.01	2.16	2.06	2.06	*0.845*	*0.521*	*0.925*	*0.86*	*0.742*
	[1.95]	[1.82]	[1.8]	[1.76]					
Total Number of Teachers in 2003	14.2	14.6	13.8	14.6	*0.786*	*0.439*	*0.282*	*0.613*	*0.257*
	[4.2]	[5.3]	[4.4]	[4.7]					
Average Age of Teachers in 2003	40.0	39.6	39.6	39.6	*0.455*	*0.796*	*0.845*	*0.57*	*0.975*
	[3.1]	[3.8]	[3.8]	[3.5]					
Sex Ratio (Female/Male) among Teachers in 2003	1.22	1.18	1.30	1.15	*0.856*	*0.788*	*0.281*	*0.575*	*0.405*
	[1.]	[.848]	[.987]	[.829]					
Panel B. Baseline Characteristics of Study Cohort (Grade 6 in 2003)									
Number of Girls in Grade 6	29.3	28.8	28.0	29.4	*0.859*	*0.862*	*0.451*	*0.566*	*0.717*
	[15.4]	[15.3]	[14.4]	[14.]					
Number of Boys in Grade 6	28.2	30.7	30.3	30.4	*0.225*	*0.498*	*0.739*	*0.348*	*0.839*
	[13.3]	[14.6]	[14.7]	[14.1]					
Sex Ratio (Female/Male) among Grade 6 students	1.065	0.968	0.964	1.011	*0.127*	*0.184*	*0.156*	*0.071**	*0.939*
	[.412]	[.297]	[.356]	[.325]					
Averge Age among Girls (at baseline)	13.21	13.12	13.18	13.14	*0.464*	*0.378*	*0.856*	*0.741*	*0.519*
	[.56]	[.64]	[.59]	[.6]					
Averge Age among Boys (at baseline)	13.79	13.72	13.77	13.77	*0.878*	*0.362*	*0.965*	*0.905*	*0.601*
	[.62]	[.69]	[.62]	[.66]					
Number of Schools (Total = 328)	83	83	80	82					

Notes: School Averages. Standard deviations in brackets. p-values in italics.

Table 2. Short- and Medium Run Impacts: Roll Call Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Impacts after 3 years						Impacts after 5 years				
	Dropped Out of Primary School	Attendance rate (while enrolled)	Ever Married	Ever Pregnant[a]	Ever Pregnant and Married	Ever Pregnant and Unmarried	Dropped Out of Primary School	Ever Married	Ever Pregnant	Ever Pregnant and Married	Ever Pregnant and Unmarried
Panel A. Girls											
Stand-Alone Education Subsidy (S)	-0.031	-0.002	-0.026	-0.027	-0.023	-0.004	-0.053	-0.029	-0.044	-0.035	-0.009
	(0.012)**	(0.006)	(0.010)**	(0.011)**	(0.009)**	(0.006)	(0.017)***	(0.015)*	(0.017)***	(0.015)**	(0.007)
Stand-Alone HIV Education (H)	0.003	-0.008	0.011	-0.007	0.006	-0.014	-0.015	0.023	0.001	0.015	-0.014
	(0.011)	(0.006)	(0.009)	(0.011)	(0.009)	(0.006)**	(0.015)	(0.014)	(0.015)	(0.014)	(0.007)**
Joint Program (SH)	-0.016	0.000	0.000	-0.011	0.002	-0.013	-0.024	0.004	-0.011	-0.010	-0.002
	(0.012)	(0.006)	(0.009)	(0.010)	(0.009)	(0.006)**	(0.016)	(0.015)	(0.016)	(0.015)	(0.007)
Observations	9116	8232	9107	9072	9072	9072	8865	8391	8302	8302	8302
Mean of Dep. Var. (Control)	0.188	0.939	0.128	0.160	0.114	0.046	0.300	0.265	0.329	0.249	0.080
p-val (Test: S = SH)	*0.245*	*0.712*	*0.012***	*0.149*	*0.008****	*0.088**	*0.104*	*0.034***	*0.058**	*0.111*	*0.322*
p-val (Test: H = SH)	*0.097**	*0.201*	*0.227*	*0.728*	*0.628*	*0.872*	*0.578*	*0.243*	*0.449*	*0.117*	*0.092**
p-val (Test: S = H)	*0.005****	*0.327*	*0****	*0.083**	*0.002****	*0.092**	*0.024***	*0.001****	*0.006****	*0.002****	*0.469*
p-val (Test: SH = S + H)	*0.484*	*0.235*	*0.285*	*0.137*	*0.159*	*0.544*	*0.063**	*0.607*	*0.169*	*0.610*	*0.04***
Panel B: Boys											
Stand-Alone Education Subsidy (S)	-0.025	-0.001	-0.009	-0.002	-0.004	0.001	-0.039	-0.006	0.004	0.004	0.000
	(0.011)**	(0.008)	(0.004)*	(0.003)	(0.002)	(0.001)	(0.016)**	(0.008)	(0.005)	(0.005)	(0.002)
Stand-Alone HIV Education (H)	0.010	-0.021	0.000	-0.002	-0.002	0.000	0.010	0.006	0.004	0.004	-0.002
	(0.010)	(0.008)***	(0.005)	(0.002)	(0.002)	(0.001)	(0.014)	(0.008)	(0.005)	(0.005)	(0.002)
Joint Program (SH)	-0.015	0.000	-0.010	-0.006	-0.005	0.000	-0.011	-0.003	0.000	0.000	-0.002
	(0.010)	(0.008)	(0.004)**	(0.002)**	(0.002)**	(0.001)	(0.015)	(0.008)	(0.005)	(0.005)	(0.002)
Observations	9461	8985	9393	9433	9382	9382	9261	8577	8897	8558	8558
Mean of Dep. Var. (Control)	0.127	0.908	0.022	0.011	0.010	0.002	0.211	0.059	0.032	0.029	0.006
p-val (Test: S = SH)	*0.353*	*0.857*	*0.599*	*0.205*	*0.538*	*0.119*	*0.072**	*0.735*	*0.456*	*0.527*	*0.546*
p-val (Test: H = SH)	*0.013***	*0.005****	*0.004****	*0.091**	*0.156*	*0.419*	*0.124*	*0.242*	*0.558*	*0.509*	*0.916*
p-val (Test: S = H)	*0.001****	*0.011***	*0.039***	*0.752*	*0.488*	*0.404*	*0.001****	*0.149*	*0.880*	*0.978*	*0.460*
p-val (Test: SH = S + H)	*0.999*	*0.04***	*0.716*	*0.660*	*0.853*	*0.162*	*0.386*	*0.747*	*0.319*	*0.339*	*0.857*

*Notes: Data Source: Roll Call Data (see test section 3.1.1 for details). Estimates obtained through OLS regressions that include controls controls for year of birth, the timing of the roll call visits, school size and randomization strata dummies. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%.*

Columns 1 -6: Data collected through five school visits conducted at regular intervals over three academic years (2003, 2004, 2005). Columns 7 -11: Information updated through four additional school visits conducted in 2006 and 2007.

[a] *For boys, "ever pregnant" is equal to 1 if the respondent ever had a child or a pregnant partner.*

Table 3. Long-Run Impacts: Individual Long-term Follow-up Survey Data (after 7 years)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Girls	Reached 8th grade	Grades Completed	Ever Married	Ever pregnant[1]	Ever Pregnant, Married	Ever Pregnant, Unmarried	Had started childbearing by age 16
Stand-Alone Education Subsidy (S)	0.039	0.206	-0.039	-0.034	-0.040	0.006	-0.022
	(0.017)**	(0.091)**	(0.021)*	(0.021)	(0.020)*	-0.013	(0.012)*
Stand-Alone HIV Education (H)	-0.004	-0.021	0.020	0.018	0.023	-0.005	-0.009
	(0.017)	(0.089)	(0.020)	(0.022)	(0.020)	(0.014)	(0.014)
Joint Program (SH)	-0.008	0.008	-0.012	-0.009	-0.015	0.006	-0.005
	(0.018)	(0.097)	(0.021)	(0.022)	(0.020)	(0.013)	(0.012)
Sampling Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5685	5685	5715	5719	5715	5715	5719
Mean of Dep. Var. (Control)	0.802	9.181	0.392	0.494	0.380	0.114	0.085
p-val (Test: S = SH)	*0.009****	*0.04***	*0.223*	*0.275*	*0.233*	*0.997*	*0.133*
p-val (Test: H = SH)	*0.812*	*0.755*	*0.137*	*0.218*	*0.074**	*0.399*	*0.773*
p-val (Test: S = H)	*0.018***	*0.017***	*0.007****	*0.021***	*0.003****	*0.394*	*0.299*
p-val (Test: SH = S + H)	*0.072**	*0.159*	*0.833*	*0.836*	*0.944*	*0.784*	*0.141*
Panel B. Boys							
Stand-Alone Education Subsidy (S)	0.036	0.070	0.008	-0.004	0.006	-0.010	-0.005
	(0.014)**	(0.083)	(0.016)	(0.018)	(0.015)	(0.009)	(0.004)
Stand-Alone HIV Education (H)	0.000	-0.045	-0.003	0.003	0.002	0.002	-0.005
	(0.014)	(0.080)	(0.016)	(0.017)	(0.015)	(0.008)	(0.003)
Joint Program (SH)	0.021	-0.011	0.004	0.001	0.005	-0.005	-0.003
	(0.014)	(0.091)	(0.015)	(0.016)	(0.015)	(0.009)	(0.004)
Sampling Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6557	6557	6591	6595	6591	6591	6595
Mean of Dep. Var. (Control)	0.854	9.521	0.186	0.214	0.161	0.053	0.015
p-val (Test: S = SH)	*0.317*	*0.370*	*0.783*	*0.777*	*0.959*	*0.498*	*0.490*
p-val (Test: H = SH)	*0.121*	*0.687*	*0.644*	*0.894*	*0.848*	*0.436*	*0.411*
p-val (Test: S = H)	*0.012***	*0.152*	*0.477*	*0.687*	*0.820*	*0.107*	*0.950*
p-val (Test: SH = S + H)	*0.488*	*0.767*	*0.953*	*0.932*	*0.882*	*0.739*	*0.111*

*Notes: Source: Long-run follow-up survey (see text section 3.1.2 for details). Estimates obtained through OLS regressions that include controls for year of birth, the timing of the survey, school size and randomization strata dummies. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%.*

[1] *For boys, "ever pregnant" is equal to 1 if the respondent ever had a child or a pregnant partner.*

Table 4. Long-Run Impacts on Sexually Transmitted Infections (after 7 years)

	(1)	(2)
	Blood Test: HIV positive	Blood Test: HSV2 positive
Panel A. Girls		
Stand-Alone Education Subsidy (S)	0.004	0.009
	(0.006)	(0.014)
Stand-Alone HIV Education (H)	-0.002	0.004
	(0.006)	(0.013)
Joint Program (SH)	-0.002	-0.023
	(0.006)	(0.013)*
Sampling Weights	Yes	Yes
Observations	2382	5509
Mean of Dep. Var. (Control)	0.009	0.118
p-val (Test: S = SH)	*0.404*	*0.013***
p-val (Test: H = SH)	*0.906*	*0.025***
p-val (Test: S = H)	*0.311*	*0.701*
p-val (Test: SH = S + H)	*0.700*	*0.051**
Panel B: Boys		
Stand-Alone Education Subsidy (S)	0.001	0.005
	(0.002)	(0.009)
Stand-Alone HIV Education (H)	0.000	-0.002
	(0.002)	(0.010)
Joint Program (SH)	0.003	-0.009
	(0.003)	(0.010)
Sampling Weights	Yes	Yes
Observations	2659	6302
Mean of Dep. Var. (Control)	0.001	0.074
p-val (Test: S = SH)	*0.595*	*0.144*
p-val (Test: H = SH)	*0.421*	*0.499*
p-val (Test: S = H)	*0.483*	*0.462*
p-val (Test: SH = S + H)	*0.759*	*0.380*

Data Sources: Col 1: Rapid HIV tests administered to consenting individuals at the end of the long-run follow-up survey, among a randomly selected subsample. Col 2: Lab Tests performed on blood draws taken during after follow-up survey. See text section 3.1.2 for details.

Estimates obtained through OLS regressions that include controls for year of birth, the timing of the survey, school size and randomization strata dummies. Standard errors clustered at the school level.

****, **, * indicate significance at 1, 5 and 10%.*

Table 5. Long-Run Impacts: Self-Reported Sexual Behavior of Girls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		When asked about *own behavior* to prevent HIV infection…					If ever had sex:	
	Ever had sex	Reports Abstinence	Reports Faithfulness	Reports Condom Use	First pregnancy / child was wanted	Age of oldest partner ever	Age at first sex	Used a condom last time had sex
Stand-Alone Education Subsidy (S)	-0.028	0.017	-0.030	-0.001	-0.009	-0.269	0.069	0.017
	(0.021)	(0.015)	(0.017)*	(0.010)	(0.029)	(0.225)	(0.095)	(0.021)
Stand-Alone HIV Education (H)	0.010	0.012	0.033	-0.011	0.037	0.197	-0.095	-0.010
	(0.023)	(0.016)	(0.019)*	(0.010)	(0.029)	(0.256)	(0.092)	(0.022)
Joint Program (SH)	-0.008	0.025	0.023	-0.014	-0.024	0.047	0.060	-0.021
	(0.021)	(0.016)	(0.020)	(0.010)	(0.030)	(0.248)	(0.089)	(0.023)
Observations	5717	5713	5713	5713	2457	3681	3577	3700
Mean of Dep. Var. (Control)	0.680	0.211	0.274	0.069	0.304	24.953	16.997	0.282
p-val (Test: S = SH)	*0.303*	*0.564*	*0.006****	*0.190*	*0.609*	*0.209*	*0.927*	*0.095**
p-val (Test: H = SH)	*0.398*	*0.435*	*0.603*	*0.765*	*0.037***	*0.600*	*0.110*	*0.611*
p-val (Test: S = H)	*0.068**	*0.754*	*0****	*0.286*	*0.076**	*0.088**	*0.097**	*0.222*
p-val (Test: SH = S + H)	*0.731*	*0.887*	*0.472*	*0.868*	*0.214*	*0.736*	*0.534*	*0.367*

*Notes: Data Source: Individual long-run follow-up survey conducted after 7 years. Estimates obtained through OLS regressions that include controls for year of birth, the timing of the survey, school size and randomization strata dummies. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%.*

Table 6. HSV2 Prevalence Among Girls, by Marital/Schooling status and Treatment Group

	# Obs	All	Stand-Alone Education Subsidy (S)	Stand-Alone HIV Education (H)	Joint Program (SH)	Control (C)
Ever Married	1839	18.1%	20.6%	17.9%	15.4%	18.2%
Never Married	3698	7.6%	8.8%	7.8%	6.0%	7.8%
In school	2186	5.8%	6.4%	6.1%	3.8%	6.4%
Out of school	3348	14.6%	16.8%	14.7%	12.2%	14.7%
Out of school and never married	1518	10.3%	12.4%	10.5%	8.8%	10.0%
Reports that Ever had sex	3591	14.6%	16.7%	14.7%	12.4%	14.6%
Reports that Never had sex	1948	4.6%	5.1%	4.7%	3.0%	5.3%

Notes: Data Source: Individual Follow-up Survey conducted after 7 years. We present the raw (unweighted) means.

Table A1. Accuracy of Roll Call Method

	(1)	(2)	(3)	(4)
	Dep. Var.: Dummy equal to 1 if Roll Call data is consistent with Quality Control data			
Sample:	Girls reported as having started childbearing in roll call data	Girls reported as not having started childbearing in roll call data	Girls reported as having a child in roll call data	Girls reported as not having a child in roll call data
Stand-Alone Education Subsidy (S)	0.009	0.004	0.005	0.016
	(0.033)	(0.057)	(0.040)	(0.047)
Stand-Alone HIV Education (H)	-0.040	-0.087	-0.053	0.030
	(0.040)	(0.059)	(0.044)	(0.038)
Joint Program (SH)	-0.029	-0.040	-0.059	0.023
	(0.033)	(0.063)	(0.039)	(0.041)
Observations	1144	276	931	452
Mean of Dep. Var. (Control)	0.789	0.826	0.792	0.892
p-val (Test: S = SH)	*0.303*	*0.500*	*0.162*	*0.889*
p-val (Test: H = SH)	*0.794*	*0.498*	*0.897*	*0.864*
p-val (Test: S = H)	*0.257*	*0.143*	*0.249*	*0.763*

Notes: To check the accuracy of the childbearing data obtained through the Roll Call method, a subset of girls were randomly sampled for a "Quality Control" survey administered at their home in early 2006. Girls who had been identified as having started childbearing according to the roll call were oversampled. The childbearing information collected through the home visits was obtained from the target respondent herself in 44% of the cases; from her mother in 27% of the cases; from another female relative in 10% of the cases; and from a male relative in the rest of the cases.

Table A2. Attrition in Roll Call Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Outcomes missing after 3 years				Outcomes missing after 5 years		
	Dropped Out of Primary School	Attendance rate (when enrolled) over 5 surprise visits	Ever Married	Ever Pregnant	Dropped Out of Primary School	Ever Married	Ever Pregnant
Panel A. Girls							
Stand-Alone Education Subsidy (S)	0.001	-0.002	-0.002	-0.005	-0.014	-0.001	-0.002
	(0.005)	(0.009)	(0.005)	(0.005)	(0.010)	(0.014)	(0.014)
Stand-Alone HIV Education (H)	-0.003	0.013	0.000	-0.001	-0.019	-0.021	-0.024
	(0.006)	(0.009)	(0.006)	(0.006)	(0.010)*	(0.015)	(0.015)
Joint Program (SH)	0.010	0.015	0.010	0.007	-0.005	-0.008	-0.006
	(0.006)	(0.008)*	(0.007)	(0.007)	(0.012)	(0.018)	(0.018)
Observations	9487	9487	9487	9487	9487	9487	9487
Mean Attrition (Control Group)	0.037	0.131	0.038	0.044	0.076	0.123	0.132
p-val (Test: S = SH)	*0.113*	*0.033***	*0.061**	*0.06**	*0.335*	*0.637*	*0.782*
p-val (Test: H = SH)	*0.049***	*0.777*	*0.155*	*0.263*	*0.168*	*0.418*	*0.263*
p-val (Test: S = H)	*0.490*	*0.112*	*0.777*	*0.508*	*0.482*	*0.08**	*0.051**
Panel B: Boys							
Stand-Alone Education Subsidy (S)	-0.002	0.000	0.000	0.000	0.000	-0.002	0.019
	(0.005)	(0.008)	(0.006)	(0.006)	(0.010)	(0.019)	(0.010)*
Stand-Alone HIV Education (H)	0.005	0.009	0.003	0.006	-0.002	-0.018	0.010
	(0.005)	(0.006)	(0.006)	(0.006)	(0.009)	(0.017)	(0.009)
Joint Program (SH)	0.004	0.002	0.007	0.003	-0.002	-0.008	0.007
	(0.007)	(0.006)	(0.008)	(0.008)	(0.012)	(0.021)	(0.010)
Observations	9802	9802	9802	9802	9802	9802	9802
Mean of Dep. Var. (Control)	0.030	0.077	0.038	0.037	0.059	0.133	0.085
p-val (Test: S = SH)	*0.376*	*0.803*	*0.347*	*0.654*	*0.827*	*0.707*	*0.233*
p-val (Test: H = SH)	*0.954*	*0.225*	*0.671*	*0.708*	*0.984*	*0.541*	*0.716*
p-val (Test: S = H)	*0.204*	*0.209*	*0.536*	*0.296*	*0.807*	*0.232*	*0.331*

*Notes: Dependent variables are dummies equal to 1 if the information is missing for the respondent. Estimates obtained through OLS regressions that include controls for year of birth, school size, randomization strata dummies and roll call dates. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%.*

Table A3. Survey Rates during Long-Run Follow-up (after 7 years)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Identified as Dead during Regular Tracking (RT)	If not dead: Found and Surveyed during RT	If Sampled for IT: Found and Surveyed during IT	If not Dead: Found and Surveyed (RT or IT)	Final Follow-up Sample (with sampling weights)			
					Surveyed	Non-missing Grades Completed	Non-missing fertility	Non-missing HSV2 Status
Panel A. Girls								
Stand-Alone Education Subsidy (S)	-0.001	0.087	-0.044	0.060	0.007	0.013	0.006	0.007
	(0.004)	(0.016)***	(0.029)	(0.013)***	(0.014)	(0.014)	(0.014)	(0.016)
Stand-Alone HIV Education (H)	0.001	0.044	0.008	0.022	0.015	0.021	0.015	0.015
	(0.003)	(0.017)**	(0.030)	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)
Joint Program (SH)	-0.004	0.090	0.041	0.074	0.037	0.043	0.037	0.046
	(0.004)	(0.017)***	(0.031)	(0.015)***	(0.014)***	(0.014)***	(0.014)***	(0.015)***
Observations	9487	9359	1291	9359	6021	6021	6021	6021
Mean (Control Group)	0.013	0.444	0.783	0.565	0.942	0.940	0.944	0.910
p-val (Test: S = SH)	*0.399*	*0.808*	*0.006****	*0.360*	*0.024***	*0.024***	*0.021***	*0.009****
p-val (Test: H = SH)	*0.125*	*0.006****	*0.328*	*0.001****	*0.113*	*0.119*	*0.115*	*0.05***
p-val (Test: S = H)	*0.555*	*0.01****	*0.094**	*0.007****	*0.534*	*0.541*	*0.511*	*0.630*
Panel B. Boys								
Stand-Alone Education Subsidy (S)	-0.001	0.069	0.043	0.043	0.024	0.024	0.025	0.030
	(0.004)	(0.017)***	(0.028)	(0.015)***	(0.010)**	(0.010)**	(0.010)**	(0.014)**
Stand-Alone HIV Education (H)	0.003	0.003	-0.013	-0.017	-0.005	-0.006	-0.005	-0.003
	(0.004)	(0.018)	(0.026)	(0.015)	(0.010)	(0.010)	(0.010)	(0.014)
Joint Program (SH)	0.000	0.061	0.046	0.039	0.020	0.021	0.021	0.039
	(0.004)	(0.019)***	(0.025)*	(0.016)**	(0.010)**	(0.010)**	(0.010)**	(0.014)***
Observations	9802	9643	1179	9643	6788	6788	6788	6788
Mean (Control Group)	0.016	0.554	0.845	0.670	0.969	0.969	0.969	0.918
p-val (Test: S = SH)	*0.708*	*0.697*	*0.907*	*0.813*	*0.675*	*0.704*	*0.681*	*0.521*
p-val (Test: H = SH)	*0.468*	*0.003****	*0.033***	*0.001****	*0.018***	*0.012***	*0.015***	*0.002****
p-val (Test: S = H)	*0.267*	*0****	*0.07**	*0****	*0.007****	*0.005****	*0.005****	*0.017***

Notes: RT stands for "Regualr Tracking" and IT stands for "Intensive Tracking". See Text, Section 3.1.2 for a description of the tracking procedure used.

Table A4. Checking for Differential Attrition across Treatment Arms in Long-Run Data

	(1)	(2)	(3)	(4)	(5)	(6)
	After 3 years: Ever pregnant (Roll Call Data)			After 5 years: Ever pregnant (Roll Call Data)		
Sample:	Full Sample	LR Follow-up Sample (unweighted)	LR Follow-up Sample (weighted)	Full Sample	LR Follow-up Sample (unweighted)	LR Follow-up Sample (weighted)
Panel A. Girls						
Stand-Alone Education Subsidy (S)	-0.027	-0.030	-0.022	-0.044	-0.039	-0.036
	(0.011)**	(0.013)**	(0.014)	(0.017)***	(0.018)**	(0.021)*
Stand-Alone HIV Education (H)	-0.007	-0.011	0.001	0.001	0.005	0.013
	(0.011)	(0.012)	(0.013)	(0.015)	(0.018)	(0.021)
Joint Program (SH)	-0.011	-0.017	-0.010	-0.011	-0.009	-0.009
	(0.010)	(0.011)	(0.012)	(0.016)	(0.018)	(0.020)
Sampling Weights			Yes			Yes
Observations	9072	5654	5654	8302	5341	5341
Mean of Dep. Var. (Control)	0.160	0.128	0.125	0.329	0.270	0.283
Panel B: Boys						
Stand-Alone Education Subsidy (S)	-0.002	-0.005	-0.005	0.004	0.002	0.001
	(0.003)	(0.003)*	(0.003)	(0.005)	(0.006)	(0.007)
Stand-Alone HIV Education (H)	-0.002	0.000	0.001	0.004	0.003	0.000
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.007)
Joint Program (SH)	-0.006	-0.007	-0.006	0.000	-0.002	-0.004
	(0.002)**	(0.002)***	(0.003)**	(0.005)	(0.005)	(0.006)
Sampling Weights			Yes			Yes
Observations	9433	6522	6522	8897	6317	6317
Mean of Dep. Var. (Control)	0.011	0.011	0.010	0.032	0.029	0.031

*Notes: Data Source: Roll Call Data. Estimates obtained through OLS regressions that include controls for year of birth, the timing of the roll call visits, school size and randomization strata dummies. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%. Columns 1-3: Data collected through five school visits conducted at regular intervals over three academic years (2003, 2004, 2005). Columns 4-6: Include four additional school visits conducted in 2006 and 2007.*

Table A5. HIV Education and Knowledge in Program Schools, After Two Years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	HIV was mentioned in class in the last 4 weeks	HIV was ever mentioned in class	Knows that HIV kills	Knows that healthy looking individuals can have HIV	Knows that condoms can prevent pregnancy	Knows that condoms can prevent HIV infection	Mentions abstinence when asked for ways to avoid HIV infection	Mentions condoms when asked for ways to avoid HIV infection	Mentions faithfulness when asked for ways to avoid HIV infection
Panel A. Girls									
Stand-Alone Education Subsidy (S)	0.018	-0.014	0.009	0.029	-0.003	-0.010	0.022	0.039	0.037
	(0.026)	(0.018)	(0.010)	(0.019)	(0.018)	(0.019)	(0.020)	(0.020)*	(0.013)***
Stand-Alone HIV Education (H)	0.053	0.060	-0.009	0.004	0.048	0.020	0.032	0.079	0.030
	(0.025)**	(0.016)***	(0.011)	(0.018)	(0.018)***	(0.017)	(0.021)	(0.021)***	(0.012)**
Joint Program (SH)	0.086	0.064	0.023	-0.008	0.050	0.039	0.025	0.063	0.030
	(0.029)***	(0.017)***	(0.010)**	(0.017)	(0.019)**	(0.017)**	(0.023)	(0.023)***	(0.011)***
Observations	13338	13338	13340	13281	13353	13188	13318	13318	13318
Mean of Dep. Var. (Control)	0.461	0.823	0.858	0.512	0.484	0.552	0.390	0.370	0.068
p-val (Test: S = SH)	*0.016***	*0****	*0.174*	*0.048***	*0.008****	*0.01****	*0.892*	*0.284*	*0.568*
p-val (Test: H = SH)	*0.238*	*0.797*	*0.004****	*0.525*	*0.927*	*0.268*	*0.753*	*0.497*	*0.986*
p-val (Test: S = H)	*0.170*	*0****	*0.1**	*0.201*	*0.008****	*0.124*	*0.618*	*0.057**	*0.598*
p-val (Test: SH = S + H)	*0.683*	*0.452*	*0.137*	*0.125*	*0.847*	*0.249*	*0.347*	*0.067**	*0.033***
Panel B: Boys									
Stand-Alone Education Subsidy (S)	0.006	0.000	0.009	-0.005	0.049	0.018	0.026	0.007	0.016
	(0.023)	(0.018)	(0.017)	(0.009)	(0.014)***	(0.015)	(0.020)	(0.020)	(0.010)
Stand-Alone HIV Education (H)	0.002	0.053	0.001	-0.010	0.029	0.015	0.054	0.045	0.023
	(0.024)	(0.016)***	(0.017)	(0.010)	(0.014)**	(0.016)	(0.021)***	(0.021)**	(0.010)**
Joint Program (SH)	0.059	0.056	0.016	-0.006	0.051	0.046	0.067	0.000	0.015
	(0.025)**	(0.018)***	(0.018)	(0.010)	(0.015)***	(0.017)***	(0.021)***	(0.021)	(0.010)
Observations	13693	13693	13655	13667	13682	13559	13636	13636	13636
Mean of Dep. Var. (Control)	0.479	0.794	0.567	0.862	0.648	0.655	0.393	0.520	0.079
p-val (Test: S = SH)	*0.039***	*0.001****	*0.659*	*0.934*	*0.906*	*0.064**	*0.059**	*0.746*	*0.898*
p-val (Test: H = SH)	*0.03***	*0.825*	*0.362*	*0.718*	*0.152*	*0.057**	*0.548*	*0.036***	*0.469*
p-val (Test: S = H)	*0.865*	*0.001****	*0.633*	*0.629*	*0.179*	*0.850*	*0.200*	*0.056**	*0.542*
p-val (Test: SH = S + H)	*0.135*	*0.877*	*0.784*	*0.515*	*0.184*	*0.553*	*0.678*	*0.081**	*0.103*

Notes: Data Source: Anonymous in-class survey self-administered by students in grades 7 and 8 in 2005. The overlap between those administered this survey and our study sample is only partial (and the overlap likely varies with the treatment assignment), therefore this analysis is only suggestive.
*Estimates obtained through OLS regressions that include controls for school size and randomization strata dummies. Standard errors clustered at the school level. ***, **, * indicate significance at 1, 5 and 10%.*