Education and Fertility:
Evidence from a Policy Change in Kenya

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ABSTRACT

This paper investigates the relationship between a woman's education and fertility by exploiting a 1985 policy change in Kenya that lengthened primary school by one year. An instrumental variables approach exploits exogenous variation in the extent of treatment across birth cohorts, which allows for an examination of the reform's effect on outcomes such as the age at first intercourse, marriage, and birth, as well as total fertility. The reform led to an increase in education, a delay in marriage, and lower levels of fertility beginning at the age of 20. The effect becomes increasingly negative through age 25, casting doubt on whether the "incarceration" hypothesis explains the results. While other potential mechanisms can be ruled out, the findings suggest that a postponement of marriage and increased use of modern contraceptives may be contributing to the reduction in fertility. Furthermore, the evidence is consistent with women gaining bargaining power over their household fertility decision.

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I. Introduction

Over the past two decades the international community has led a concentrated effort to promote the education of women throughout the world. A report from the World Bank (Tembon and Fort, 2008) proposes that "Women's economic empowerment is essential for economic development, growth and poverty reduction – not only because of the income it generates, but also because it helps to break the viscous cycle of poverty." This belief is so strongly held that in 2000 the United Nations established the United Nations Girl's Education Initiative (UNGEI), whose sole purpose is to promote the education of young women around the world. Education can both lead to increased knowledge of available healthcare options and change social attitudes and norms, and it has been consistently found to be negatively correlated with fertility.[1] Little is known, however, about whether small increases in schooling have lasting impacts on fertility and even less is known about the channels through which such impacts may occur, but both are policy relevant issues.[2]

This paper exploits an increase in education generated by a 1985 schooling reform in Kenya to examine the effect of education on fertility and related behavior. The key aspect of the reform is that it required students to complete eight years of primary education, instead of the previous seven years, before being eligible to earn the certificate of primary education. This had the effect of adding an additional year of schooling for all students who completed primary school, but it also generated an extra year of education for students who continued on to secondary school. The scale of the reform allows for the use of a nationally representative sample. I use data from five rounds of the Demographic and Health Survey (DHS) conducted in Kenya, which generates a large sample of Kenyan women. The reform's

[1] The inverse relationship between education and fertility was largely established from conditional correlations generated by single equation models, which are susceptible to omitted variable bias (Shultz, 1994 and 1998; Ainsworth, Beegle, and Nyamete, 1996; Lam and Duryea, 1999). A discussion of the omitted variable bias concern can be found in Becker (1993).

[2] Examining the relationship between education and fertility in developing countries Osili and Long (2008) and Breierova and Duflo (2004) both used increased investment in primary education to identify an exogenous increase in education. Osili and Long (2008) found that the increased education led to reduced fertility for 25 year old women in Nigeria. Breierova and Duflo (2004) found evidence of an effect at the age of 15, but not at 25 in Indonesia. Duflo, Dupas, and Kremer (2011) used experimental evidence from Western Kenya and found a reduction in fertility for young women.

timing, along with these data, enables a thorough investigation of its effect on Kenyan women into their mid-twenties.

Due to either entering school late or the high amount of grade repetition, the effect of the reform's implementation is distributed across many birth cohorts.[3] Given the continuity in the transition from untreated birth cohorts to treated cohorts, I use an instrumental variable (IV) approach to evaluate the impact of increased schooling. Specifically, I use pre-treatment data to estimate the likelihood of being treated by the reform. This concept is broadly inspired by Angrist and Lavy (1999) and the exact application is similar to the one employed by Borkum (2010) who used the procedure to examine the impact of higher education on wages in Botswana.

I show that the reform led to an increase in schooling, providing a unique setting through which to evaluate the impacts and mechanisms of a wide-scale schooling increase on fertility. I find that the reform substantially affected the outcomes of women. The reform lowered the fraction of women who began child bearing at the age of 20 by 8.4 percentage points. More importantly, the effect on total fertility persisted and became increasingly negative at older ages, eventually lowering a woman's fertility by more than a third of a child at the age of 25. The reform's effects exhibit heterogeneity even within ethnic communities and groups with similar levels of education, highlighting the importance of this study's broad scope. Both the persistence of the reform's effect and the characteristics of the within country heterogeneity suggest that there are mechanisms at work other than merely the incarceration effect. I find that women substantially delay marriage, have an increased role in household decisions, and increase their use of modern contraceptives.

In a set of placebo regressions, I also show that none of the previous results are found for women whose education should not have been affected by the reform. For example, using data from a subset of women who did not complete five years of education and whose education level should not have been affected the lengthening of primary school, I find no evidence of a reduction in fertility. For this group of

[3] A previous study, Ferre (2009), also attempted to exploit this policy reform. However, the paper did so using a regression discontinuity (RD) design. The dispersion of the reform's impact on education across a number of cohorts makes a regression discontinuity design unsuitable.

women, along with two other placebo tests, I am able to demonstrate that the model is not capturing a general change that is occurring throughout the population, but rather an effect that is isolated to women who were treated by the reform.

The breadth of the Kenyan education reform and depth of data available through the DHS enables a uniquely detailed investigation into the potential mechanisms driving the reduction in fertility. I find that the reform led to an increase in the use of modern contraceptive methods for both married and unmarried women, and that married women became more likely to participate in final household decisions. This suggests that the reform increased the bargaining power for women in the household fertility decision. Increases in maternal education can lead to improved access to information (Thomas, Straus, and Henriques, 1991), which in turn, can lead to a better understanding and further investment in modern contraceptive methods (Rosenzweig and Shultz, 1989), and consequently greater bargaining power in the intra-household fertility decision (Rasul, 2008).

I am also able to rule out a number of potential mechanisms. The most direct link between schooling and reductions in a woman's fertility is the "incarceration effect" proposed by Black, Devereux, and Salvanes (2008). This is simply the idea that while women are in school they do not have the desire or time to have children. The persistence of the reform's effect for older ages and the lack of evidence that higher educated populations saw a larger reduction in fertility suggest that the incarceration effect is not a significant cause of the decline in fertility. An increase in a women's education could also lead to increased selectivity when finding a partner. This assortative matching (Lavy and Zablotsky, 2011) could then affect fertility outcomes through spousal characteristics. However, I find no evidence that the reform led to a change in the age or education of the husband, or the age difference between the woman and her husband. Increased employment opportunity and earnings from higher levels of schooling could increase the opportunity cost of raising children and potentially reduce fertility (Rosenzweig and Evenson, 1977; Rosenzweig, 1982). However, I find evidence of increased employment for both women treated by the reform and primary school dropouts, even though reductions in fertility are isolated to only women treated by the reform. This suggests that the increase in employment is not driving the decline in fertility.

Finally, I find no evidence that the reform impacted the likelihood that a woman has ever had a pregnancy terminated.

This paper contributes to existing literatures on the effect of education on fertility in developing and developed countries. Previous work using exogenous variation in education in developed settings has found less robust results. For example, using data from California and Texas, McCrary and Royer (2011) found no evidence that increasing education by beginning school at younger ages reduced fertility. Exploiting changes in compulsory school laws in the U.S. and Norway, Black, Devereux, and Salvanes (2008) found that increased schooling led to a reduction in fertility. However, the results from Monstad, Propper, and Salvanes (2008) suggested that the result in Norway may be evidence that women were only postponing their births from their teens and early twenties to their late thirties. Work by Lavy and Zablotsky (2011) found that increased access to school in parts of Israel due to the end of military rule led to decreased fertility for Arab women.

The literature focusing on the effect in developing countries suggests a larger and more prevalent effect of education on fertility, as may be expected. In Indonesia, Breierova and Duflo (2004) found mixed evidence of education's effect on fertility. They estimated that increased levels of primary education generated by a large-scale school construction project led to fewer births for women by the age of 15, but had no effect on the number of children born to women by the age of 25. However, they also found evidence that the increase in maternal schooling led to reduced levels of child mortality. More robust evidence was discovered in Nigeria where Osili and Long (2008) found that the expansion of universal primary education led to reductions in fertility for women at the age of 25.

Given its focus on Kenya, the paper is most closely related to the work of Duflo, Dupas, and Kremer (2011). They conducted a field experiment in Western Kenya where they provided free uniforms to girls in the sixth grade of primary school. This lowered the cost of education and increased schooling for girls treated by the intervention. They found evidence that the program led to a reduction in marriage and fertility, but the results for risky sexual behavior were not as robust. Their estimates for fertility are large for the modest increase in education provided by the reform. I find similar results, but for a nationally representative population, for a policy reform that is easy to replicate, and I am able to examine the effects over a longer period of a woman's life. More importantly, I am can also examine in detail the pathways through which increased education impacts fertility.

The paper proceeds as follows. The 1985 reform is explained in greater detail in section II. The data are described in section III. The empirical model and instrument used to estimate the effect of the reform on education and fertility is presented in section IV. The results are discussed in section V, and section VI concludes.

II. Education Reform in Kenya, 1985

a. 1985 Education Reform: Change in the Education Structure

In January 1985, Kenya restructured their education system. The new 8-4-4 system consisted of eight years of primary school, followed by four years of secondary school, and four years of tertiary education. This system replaced the 7-4-2-3 structure that had been in place since Kenya gained its independence. The previous education structure included seven years of primary education, followed by four years of "lower secondary" education, two years of "upper secondary," and finally three years of university.

The reform was implemented nationally in January 1985, the beginning of the school year. This was functionally executed by admitting no class of students to secondary school in 1985. The class of students who completed their seventh year of primary school in 1984 would have normally proceeded to secondary school in the following year. However, due to the restructuring of the education system these students remained in primary school for an eighth year. The last pre-reform class of students was the class that completed their primary school exam in 1983 and progressed into secondary school through the old system in 1984.

As shown in Figure 1, this reform lengthened the time a student needed to stay in primary school before becoming eligible to obtain a certificate of primary education from seven to eight years.[4] The length of the principal stage of secondary school remained four years; students who ended their education during or directly after the completion of secondary school also received an extra year of education due to the additional year in primary school. Students completing four years of secondary education completed 11 years of school prior to the reform, and 12 years of school after the reform. The reform had no clear effect on years of schooling for students who continued their education beyond four years of secondary school, three quarters of pre-reform students who completed "upper secondary" school continued to tertiary education, and students who earned their college degree completed 16 years of schooling under both education systems.

There are additional characteristics of the reform that may have lessened its effect on schooling and are important to consider. First, starting secondary school a year later may have simply reduced the number of students moving from primary school to secondary school due to attrition. Second, the reform put added stress on primary schools with an additional 320,000 students attending primary school in 1985 (Kenya Ministry of Education, 1988). Schools responded by hiring roughly 16,000 additional teachers, leading to lower student-teacher ratios in 1985 compared to the previous years; however, most of the new teachers were untrained (O'Eiseman, 1988; Kenya Ministry of Education, 1988; Somerset, 2009). The increase in the size of the student body and unprepared teachers likely had the effect of initially reducing the quality of the extra year of education. The increased attrition and lower quality of school possibly lessened the reform's effect on schooling.

b. Concurrent and Prior Education Reforms in Kenya

The reform also led to a redesign of the curriculum that was taught in Kenyan schools. The new curriculum was structured with the understanding that the primary level of education was likely to be the

[4] Prior to the reform, the Certificate of Primary Education (CPE) examination was given after the seventh grade of primary school. After the reform, the test was renamed the Kenya Certificate of Primary Education (KCPE) and administered after eighth grade.

terminal level of schooling for many students, and encouraged skills that could be used as soon as they left the education system (Rharade, 1997).[5] Hasty implementation of the reform meant inadequate teacher training and that syllabi were prepared in less than a month (O'Eisemon, 1988). Furthermore, in 1990 less than a quarter of primary schools had the workshops required to complete the technical training included in the updated curriculum (O'Eiseman, 1988).

It is also important to take into account that the likelihood an individual from a given cohort was affected by the 1985 reform is strongly linked to two primary school fee reductions in the 1970s. In 1974, primary school entrance fees were abolished for the first four years, and were annually extended to an additional year of primary school (Nkinyangi, 1982; Somerset, 2009; Ohba 2009). This led to a one time increase in enrollment in 1974, but the increase quickly dissipated due to overcrowding, which led to a poorer quality of education and increases in alternative fees levied by many schools (Nkinyangi, 1982; Somerset, 2009). The alternative school fees that had been implemented in response to the increased demand for education from the 1974 abolishment of fees were banned in 1979. The 1979 ban on additional school fees required community groups to raise the additional funds, and did not allow direct fees to be collected from the student's families (Somerset, 2009; Ohba, 2009). The 1979 reform again led to a large increase in primary school enrollment, but after the 1979 reform, enrollment rates remained at similar levels in the subsequent years. These reforms greatly affected the timing of when students began, or possibly continued, their education. This had a significant impact on the probability of a student advancing to secondary school before the 1985 reform was implemented. These fee reductions must be taken into account when calculating a cohort's likelihood of being affected by the 1985 reform.

III. **Data**

a. Sources

All individual level data are from the Demographic and Health Surveys (DHS) conducted in

[5] Family planning and sex education were not subjects included in the introduced curriculum (Kenya Ministry of Education, 1986 and 1988).

Kenya. I use data from five rounds of the survey: 1989, 1993, 1998, 2003, and 2008. Only women born between 1950 and 1980 are included in the sample. Data are from the individual women's and births record datasets. The individual women's dataset provides detailed personal information regarding birth date, age of marriage and first intercourse, contraceptive use, and later rounds include personal sentiment towards their ability to make decisions in their everyday life. Demographic characteristics such as ethnicity and retrospective data on the type of location lived in during their childhood (urban/town/rural) are also included. The birth data are retrospective information encompassing the respondent's complete birth history. I exploit the information gathered on the age of the mother at each birth to be able to measure the number of births a woman has by any given age.

In addition to the restrictions on birth cohorts, the analysis sample only includes respondents who report both their birth year and birth month. This is essential to correctly identifying the effect of the reform due to the fact that the likelihood of treatment is assigned based on the woman's time of birth. Individuals who report their complete date of birth information are on average a year younger, with about two-tenths fewer children by age 25, and a year more of school.[6]

Summary statistics for the sample used in this paper are shown in Table 1. The average respondent included in this paper's sample is 28 years old with seven and a half years of education. Woman in the sample, on average, began sexual activity before the age of 17, followed by marriage, before giving birth to their first child shortly after the age of 19. Large fractions of women are married and have their first child between the ages of 19 and 21, although the largest increase in the fraction of women becoming sexually active is between the ages of 16 and 18. Finally, women included in the sample have an average of about two and a half births by the age of 25.

I also use school enrollment and failure rate data from the World Bank's Education Statistics and UNESO's Institute for Statistics, information on the age of students enrolled in grade one in 1978 and

[6] The results for the full sample are similar to the results found in this paper for the sample of respondents with complete date of birth information. When date of birth data are missing, the DHS attempts to estimate the correct month or year of birth using other information collected in the survey. The inclusion of observations with imputed month or year data only introduces some inconsistency across outcome variables for younger ages. Estimates for the full sample are shown in the appendix.

1979 from Somerset (2007), and transition rates from the last year of primary school to secondary school from Ohba (2009). This information allows me to estimate the likelihood that a student progresses through primary school and into secondary school before the 1985 reform. These data are explained in more detail below.

b. Measuring schooling

Measuring an individual's level of educational attainment is critical to the project. Although the DHS has a two-question approach to measure an individual's highest completed level of schooling, its method is unable to accurately capture the exact number of years of schooling for some key cohorts used in the analysis. The DHS records the highest education level an individual attended. This can be seen in Panel A of Table 2. Then the DHS records the highest year of education completed at that level, Panel B. The years of education are then calculated combining these two variables. If an individual completes five years of primary school, the years of schooling variable will be equal to five years. The modal number of years of secondary school completed both before and after the reform is four years. This is the length of "lower secondary" in the pre-reform period and the length of secondary school in the post-reform period (e.g., see Figure 1). If an individual reports having completed four years of secondary school, their total years of schooling is equal to the four years of secondary school plus the number of years of primary school completed. This means that the number of total years of school is dependent on whether the respondent completed primary school before or after the reform, but this information is not included in the data. Therefore, an individual who reports having completed four years of secondary school could have completed either 11 years of school (pre-reform, with seven years of primary school), or 12 years of school (post-reform, with eight years of primary school).

All individuals born after 1971 are by construction required to complete eight years of primary school to be eligible for the Kenyan Certificate of Primary Education exam. Likewise, virtually all individuals born before 1964 were eligible for the equivalent exam after only seven years of education. However, for the cohorts in between, it is not possible to determine whether respondents who continued their education beyond primary school completed seven or eight years of primary school. Subsequently, the traditional "years of education" variable does not accurately measure schooling for all respondents.

Given these limitations, I construct two alternate measures of educational attainment. First, I can identify individuals whose highest level of schooling is the eighth year of primary school. I do this using the variables described in Table 2, "education level" in Panel A and "education years" in Panel B. The variable, *Education8,* can be defined in the following way:

$$Education8_i = \begin{cases} 1 & if\ education\ level = 1\ and\ education\ years = 8 \\ 0 & otherwise \end{cases}. \qquad (1)$$

Only individuals who complete primary school after the 1985 reform will have completed eight years of primary school. This variable is graphed in Figure 2 for each birth quarter from the first quarter of 1950 through the last quarter of 1980. The graph is the most direct representation of the reform's impact. The figure is separated into three sections: post-reform, transition, and pre-reform. The first post-reform class, is the cohort born in 1972. Individuals in this cohort may begin their schooling as early as the year they turn six, 1978, and if the student graduates on time would reach the seventh grade in 1984. Due to late entry into school and high repetition rates there are roughly seven transition cohorts whose likelihood of being affected by the reform increases the closer their birth year is to 1972.

It is also possible to correctly identify if a student completes at least eight years of schooling.

$$AtLeast8_i = \begin{cases} 1 & if\ education\ level = 1\ and\ education\ years = 8 \\ 1 & if\ education\ level = 2\ and\ education\ years \geq 1 \\ 1 & if\ education\ level = 3 \\ 0 & otherwise \end{cases}. \qquad (2)$$

Any student who completes at least his first year in secondary school would also have completed at least seven years of primary school. This variable, plotted in Figure 3, captures the reform's effectiveness in increasing the education of primary completers, those who would have had seven years of schooling before the reform but completed eight years of school after the reform, but does not take into account that all students who completed between one and four years of secondary school also gained an extra year of schooling. As can been seen in Figure 3, the fraction of students completing at least eight years of school increases at a much faster rate for the birth cohorts of the transition period, the slope of the transition trend line is nearly twice as steep as the trend line of pre-reform period.

IV. Methodology

a. Baseline Estimates via Ordinary Least Squares

As discussed in the introduction, the previous literature has established a negative relationship between fertility and education (Schultz, 1994 and 1998; Ainsworth, Beegle, and Nyamete, 1996; Lam and Duryea, 1999). I begin by demonstrating that this relationship exists within Kenya for individuals born between 1950 and 1980. This is done by estimating an ordinary least squares (OLS) model. In the initial specification, years of schooling, $yrschl_{icq}$, is used as an individual's measure of education, although as noted above, this variable is measured with some noise for the transition birth cohorts. The model estimated is defined by the equation

$$y_{icq} = \alpha + yrschl_{icq}\beta + \sum_{p=1}^{3} age_{icq}^{p}\pi_p + \sum_{n=1}^{2} qtrend_{cq}^{n}\varphi_n + X_{icq}\theta + \varepsilon_{icq}\ , \qquad (3)$$

for some outcome *y*, for individual *i*, from birth cohort *c*, born in quarter *q*. The variable age_{icq} is the respondent's age at the time of the survey. A quarterly trend in birth cohorts, $qtrend_{cq}$, is also included.[7] X_{icq} is a vector of individual level covariates, including fixed effects for quarter of birth, ethnicity, and childhood place of residence (city/town/rural). These covariates are either constant over an individual's life or occur prior to schooling decisions being made, which leaves few available variables. Estimates are weighted using sampling weights provided by the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Before estimating equation (3), it is useful to provide a visual display that establishes an expectation for what the results will yield. To do this, I focus on the underlying issue being examined in this paper, total fertility. In Figure 4, I plot the total fertility for women at age 25 for each birth cohort and quarter of birth combination. Only women age 26 and older are included in the figure. As with the

[7] A quarterly time trend is used in place of an annual trend, due to evidence from the 1993 DHS showing that quarter of birth greatly affects the likelihood that a child is in the highest grade possible for their birth cohort. As will be shown later, the results are robust to various specifications of the trend.

previous figures, Figure 4 is separated into three periods. Consistent with the change in schooling seen in Figure 2 and Figure 3, the slope of the transition period is steeper than the pre-treatment period, leading to accelerated reductions in fertility prior to the post-reform period. This pattern establishes an unconditional inverse relationship between the increase in education caused by the reform and levels of total fertility. The following estimations for fertility and related variables will establish that this relationship persists in Kenya even when conditioning on other covariates.

I estimate the relationship between years of schooling and four outcomes: first intercourse, marriage, and birth, as well as, a woman's total fertility. I do this for each age between 15 and 25. In Panel A of Table 3, I show the relationship between schooling and first intercourse. Each cell is a separate regression, and the outcome variable is equal to one if the respondent reported having intercourse before or at the specified age. The number of observations changes due to restricting the sample to include only observations older than the given age.[8] The results in Panel A suggest a statistically significant negative correlation between schooling and young women becoming sexually active, with the largest negative relationship occurring at age 17. The results in Panel B are evidence that there is also a negative relationship between schooling and marriage at each age. In Panel C of Table 3, I show the relationship between schooling and first birth. As with the two previous panels, the estimates are statistically significant across all ages, and a pattern emerges showing that the correlation between schooling and a mother's first birth is most negative at age 19. Finally, the estimates for total number of births at each age are shown in Panel D of Table 3. The correlation between schooling and total fertility becomes increasingly negative at older ages.

The OLS results suggest that as schooling increases women postpone their sexual activity, marriage, and childbearing. The results also suggest that the effect persists and grows even after women leave school. However, these results are likely biased if schooling is correlated with unobservable characteristics that also affect the woman's likelihood of postponing the establishment of a family. Individuals who are more likely to continue further in their schooling are likely to be the same individuals

[8] Restricting all samples to only observations above 25 years old produces similar results.

who would have fewer children and possibly postpone marriage due to both a better innate understanding of personal health and possibly career ambition. This suggests that the negative relationship between schooling and the various outcomes could be driven by these omitted characteristics. In order to address this concern, I implement an instrumental variables strategy by exploiting the exogenous change in schooling due to the 1985 education reform in Kenya.

b. Identification: The Estimated Likelihood of Being Treated By the 1985 Reform

Students who completed the seventh year of primary school and passed the primary school exam by 1983 graduated with the certificate of primary education. During this time period primary education was not compulsory in Kenya, and students could begin school at age six. However, in 1978 less than half of the students enrolled in grade one were six years old (Somerset, 2007). Students entering school at later ages and high repetition rates means that the effects of the reform were dispersed across the transition cohorts, as seen in Figure 2 and Figure 3. Following the concept from Angrist and Lavy (1999), I construct an instrument that predicts the probability that an individual from cohort *c*, born in quarter *q*, is affected by the 1985 reform. Similar to Borkum (2010), I use pre-reform data on grade-one enrollees to predict the probability that an individual born in a specific year and quarter was faced with an eighth year of primary school in order to receive a certificate.[9] In addition to the age of students enrolled in grade-one, I use data on enrollment and repetition at every grade of primary school, and transition from the last year of primary school to secondary school to predict the likelihood of treatment.

Using these pre-reform data allows me to take into account large increases in primary school enrollment in the 1970s. As documented by Somerset (2007, 2009), the removal of primary schooling fees in the pre-reform period led to large increases in the number of students that entered the first year of primary school. These fee reductions also led to increased dropout rates in the early grades of primary

[9] Information regarding the age of grade one enrollees is only available for 1978 and 1979 (Somerset, 2007). I assume that the age distribution for grade one students in 1974 is similar to 1979 due to the fact that these years saw large fee reductions and enrollment spikes, that pre-1974 had fewer late enterers due to high fees, and that the years from 1975 to 1978 were similar due to the fact there were no school fee reforms.

school, although some of the enrollment effect persisted through the later grade levels leading to an increase in primary school completion and secondary school enrollment.

The instrument is defined as one minus the probability that an individual from a given birth cohort and born in a specific quarter advances to the first year of secondary school no later than 1984. Students who entered secondary school in 1984 or before were not affected by the extension of primary school. However, students who did not matriculate to secondary school prior to 1985 faced eight years of primary education. The instrument can be represented by the following equation:

$$Inst_{cq} = \begin{cases} 1 \quad if\ c \geq 1972 \\ Pr(Treated) = 1 - Pr(Secondary\ School\ before\ 1985)\ \ if\ 1963 < c < 1972 \\ 0 \quad if\ c \leq 1963 \end{cases} \qquad (4)$$

The instrument is equal to one for individuals born in 1972 and later. I choose the lower cutoff by estimating the probability of entering secondary school before 1985. The probability of being treated is estimated to be zero if born in the first quarter of 1964. I then assign a value of zero to all previous cohorts as well.

The probability of reaching secondary school before 1985 for a woman in birth cohort c is defined by the equation

$$Pr(Secondary\ School\ before\ 1985) = \sum_{A=6}^{1977-c} \sum_{r=0}^{1971-A-c} \Pr(Start\ School = A) \Pr(Complete\ Primary | repeat = r)\ . \qquad (5)$$

The right hand side of this equation is equal to the probability of starting school at age A multiplied by the probability of completing primary school, summed over the range of ages at which one could start school and number of grades one could repeat while still graduating to secondary school before 1985. For example, if a student was born in 1970 she could start school at either the age of six or seven, repeat one grade if starting at six, and still advance to secondary school before the reform.[10] The probability of

[10] For simplicity, it is assumed that if an individual completes seven years of school that they will enter secondary school, and that the probability of repetition at any grade is independent from past history and is only defined as a

starting school at age *A* varies by quarter of birth. Children born in the first quarter of each year are more likely to start school in their age six year, and the likelihood of entering school at this age becomes progressively lower for each of the following quarters. Also note that the model does not account for dropout. For this reason the model estimates that the probability of reaching secondary school prior to the reform is equal to one for students born in the first quarter of 1964 and before.

The value of the instrument across cohorts is shown in Figure 5. The instrument ranges from 0 to 1, with the pre-reform, transition, and post-reform periods nearly matching the pattern seen in Figure 2. To see this relationship in more detail, in Figure 6 I plot both the fraction of students whose highest grade completed is the eighth grade in primary school and the estimated probability of being treated. Although the instrument increases more rapidly for the early cohorts of the transition period, it is fairly successful at predicting the likelihood that an individual, within a given birth cohort and quarter of birth, is affected by the 1985 restructuring of the education system.

c. The Reduced-Form and 2SLS Model

I verify that the instrument predicts the change in education, the endogenous variable in the OLS model. This first stage relationship can be described by the following equation:

$$Education_{icq} = \alpha_F + Inst_{cq}\beta_F + \sum_{p=1}^{3} age_{icq}^{p}\pi_{Fp} + \sum_{n=1}^{2} qtrend_{cq}^{n}\varphi_{Fn} + X_{icq}\theta_F + \varepsilon_{Ficq} . \quad (6)$$

The variable $Inst_{cq}$ is the instrument described in equations (4) and (5). The *F* subscript on the parameters denotes that the parameters are estimated in the first stage equation, all covariates are the same as described in the OLS model, equation (3). Acknowledging the drawbacks of each education measure, I estimate the first stage equation across all three measures of education, years of schooling, and the *Atleast8* and *Education8* variables, to ensure that the findings are not unique to a single measure. I also estimate the first stage for a group of observations for which I know that the years of schooling is

given cohort's probability of matriculating to the next grade. A student could fail any grade (1 to 7). If a student fails second grade, for example, it is assumed that she will then join the following cohort's second grade class and obtain their probability to matriculate to the next grade. The number of permutations of a student's path to secondary school expands rapidly as the number of possible "failures" increases.

correctly recorded in the DHS. To do this, I remove the observations from the transition period and estimate the first stage using only observations from the pre and post periods, these groups have a probability of zero and one, respectively, of being treated.

In Table 4, I present the estimates of β_F using all three measures of education, and the restricted sample. All estimates in Table 4 show that the instrument predicts statistically significant increases in education due to the reform, with large F-statistics across all measures of education. When all cohorts are included, the reform is estimated to increase schooling by 0.766 years, a ten percent increase based on the sample mean. The result in the second column, using years of schooling for only the non-transition cohorts, estimates that the reform increased schooling by 0.861 years. Although the samples used for these two estimates are not independent, the confidence intervals of the estimates overlap, somewhat lessening the concern of measurement bias in the years of schooling variable. The magnitude of these results coincide with the expectation that the one year increase in schooling for treated individuals is lessened due to attrition from requiring an extra year to complete primary school. The model also estimates a 23.3 percentage point increase in the fraction of women completing at least eight years of school, and a 26.5 percentage point increase in the fraction of women ending their education after eight years of primary school.

Due to the fact that each measure of education does not perfectly capture the full impact of the 1985 reform, I mainly focus on the estimates from the reduced-form model. In this model, the outcome of interest, such as intercourse, marriage, or fertility, is regressed on the instrument itself.

$$y_{icq} = \alpha_R + Inst_{cq}\beta_R + \sum_{p=1}^{3} age_{icq}^{p}\pi_{Rp} + \sum_{n=1}^{2} qtrend_{cq}^{n}\varphi_{Rn} + X_{icq}\theta_R + \varepsilon_{Ricq} \; . \qquad (7)$$

The main focus of the following section of the paper will be on the estimates of β_R, which represents the estimated effect of the reform on a selected outcome. Although the 1985 reform increased schooling by one year for students who are primary completers, secondary completers, or between, the extension of primary school may have also led to increased attrition, yielding an effect of the reform similar to those estimated in the first two columns of Table 4. The estimated effect of the reform is important, but the effect of a year of schooling may be of more general interest. The impact of an additional year of schooling is likely greater than the point estimate of β_R. This is because the first stage results are less than one, estimating the effect of a year of schooling using a 2SLS model, with the instrument predicting years of schooling, the coefficient of interest would be equal to the reduced-form effect of the instrument (β_R) over the first stage effect (β_F).

V. Results

a. Baseline Reduced-Form Results

In Table 5, I report the reduced-form results for each age from 15 to 25, using the same four outcomes examined in Table 3: first intercourse, marriage, and birth, along with total fertility.[11] Each point estimate is generated from a separate regression. The results in Panel A, for first intercourse, are statistically insignificant across all ages. However, there is a noticeable pattern. The point estimate dips at ages 17 and 18 before reverting back towards zero.[12] In Panel B, for marriage, the results follow a similar pattern; beginning at age 18 the effect on marriage is statistically significant at the 99 percent confidence level. The estimates indicate that the reform led to women being 6.5 percentage points less likely to be married by the age of 18, and the estimates remain similar through the age of 21. These results suggest that women are postponing marriage from their late teens to early twenties. The estimated effect on marriage remains slightly negative, although smaller in magnitude, through the age of 25. The results for age at first birth, reported in Panel C, establish a similar pattern. Trailing the dip in the marriage results, the estimated effect of the reform on the likelihood that a woman has given birth by age 19 is statistically significant at the 90 percent confidence level. The result then increases in both magnitude and significance, estimating that the reform led to an 8.4 percentage point reduction in the likelihood of a woman giving birth by age 20. The estimated effect becomes less negative as women move into their

[11] I also estimate the effects between ages 11 and 14, there are no statistically significant results for those ages. The results are robust to excluding respondents 25 years old and younger in all regressions.
[12] I do find that the reform postponed the age of first intercourse by about half a year, using a subset of women who have become sexually active.

twenties. The effect of delaying a women's first birth lags the effect on marriage by between one and two years, suggesting that the effect on first birth may be related to women delaying marriage. However, the estimated effect on first birth is larger in absolute value than the effect on marriage for the ages from 22 to 25. Although, the reduction in marriage may play a role in delaying child bearing, it cannot explain the entire result.

Beyond the postponement of women's initial fertility, the results in Panel D show the evolution of the reform's effect on total fertility across different ages. At age 20, the results show that the reform led to a statistically significant reduction of 0.11 births. The results in Panel D demonstrate that the effect becomes increasingly negative for women in their twenties. By age 25 the reform led to a reduction of 0.36 births, a result that is statistically significant at the 99 percent confidence level. Although a woman's fertility is not complete at this age, in results not shown, there is no evidence of a reduction in birth spacing between a woman's first few births. The results for total fertility are important for two fundamental reasons. First, as with Panel C, this shows that the effects persist beyond the years of school enrollment. Second, the magnitude of the result on total fertility is increasing for women in their twenties, providing evidence the effect is not driven by an incarceration effect. This is evidence that the human capital accrued by women with higher levels of schooling plays an important role in reducing fertility rates, even into the woman's mid-twenties.

b. Robustness: Controlling for Enrollment and an Alternative Instrument

The instrument is assigned based on an individual's quarter and year of birth, and calculated using pre-reform enrollment data. This instrument is valid if the reform only affects the outcomes of interest through its effect on schooling. The reduced-form estimates will be biased if there is a secular trend in a women's propensity to attend and advance through school. It is, however, unlikely that there is some unobserved endogenous trait that links beginning secondary school before 1985 with health outcomes.

I address this issue in two ways. First, in one set of auxiliary regressions, I include grade one enrollment for each cohort's age six year. The results with the inclusion of a control for grade one enrollment in a cohort's age six year are shown in Table 6. The results demonstrate a nearly identical pattern to the baseline results. The reform's effect on marriage is largest for the ages 18 to 21, as it was without the enrollment control, and is now also significant at ages 16 and 17. The result for first birth again lags that of marriage, and also shows increased significance at a younger age. The estimates for total fertility also remain similar to the baseline results, but increase slightly in magnitude. The results in Table 6 suggest that the large fluctuations in enrollment during the 1970's are not driving the results.

Second, endogenous changes in enrollment could also be affecting the calculation of the instrument, which is constructed using cohort-level enrollment data. The instrument used in the baseline estimates was constructed using enrollment data for the transition period cohorts to calculate a probability of treatment. However, there could be a concern that some underlying characteristics of the cohorts are different, and this may lead to members of certain cohorts to endogenously be more or less likely to advance to secondary school by 1985. This trait would be captured in the estimation of the original instrument, leading to bias in the results. To remove this potential issue I estimate an alternative instrument only using three pieces of information. I use the age of grade one school attendees from the 1989 Kenyan census. This information, which is from the post-reform period, avoids any concern of changes in children's propensity to enroll in school in the pre-reform period affecting the construction of the instrument. I use a consistent 40 percent transition rate from the last year of primary school to secondary school. This is the average of the transition period. Finally, I use a Bernoulli distribution to calculate the probability of advancing to the next grade. I choose a 90 percent success rate for all grades and years, and then scale the estimates to equal zero in the first quarter of 1964 to match the original transition period. The F-statistic for the alternative instrument in the first stage is actually larger for the years of schooling and *AtLeast8* variables, and smaller, but still greater than 100, for the *Education8* variable.

Using the alternative instrument I rerun the model; the results are presented in Table 7. The largest difference from the results seen in the previous two tables is the increased significance of the estimates in Panel A, for age at first intercourse. The result for the first intercourse variable still dips prior to the estimated effects for the other outcomes, at age 17. The estimates in the other three panels are very similar to those seen previously. The results in Table 7 are robust to the inclusion of the enrollment control.

I can also use the same two methods to construct instruments that vary only by annual birth cohorts, instead of quarterly cohorts. I then re-estimate the results using a quadratic time trend across birth year, instead of birth quarter, and cluster for each annual birth cohort. The results remain robust whether the instrument was calculated using enrollment data or the Bernoulli probabilities of advancement.

Due to the DHS limiting their data collection to women under 50, the density of observations is smaller for the older cohorts in the sample. However, the pattern seen in the results up to this point are robust to removing the early cohorts from the sample, with or without the control for grade one enrollment. When restricting the sample to include only cohorts born in and after 1960 the estimates for first birth tend to lose statistical significance, although the pattern for the point estimates is maintained. The results for marriage and total fertility largely remain consistent with previous tables. It is worth noting that only three pre-treatment cohorts remain when the sample is restricted to these cohorts.

The results are also robust to the use of alternate time trends. The baseline model uses a quadratic time trend across birth quarter; the results remain robust when using a linear trend. An alternative approach would be to assign each of the three time periods a separate time trend, pre-treatment, transition, and post-treatment. When I do this, the results are robust to this three-part trend being included as either a linear, quadratic, or cubic polynomial.[13]

[13] Equations for the three-part time trend are included in the appendix, along with results for the discussed robustness checks.

c. Placebo Tests: Results for Non-Treated Respondents

Even though the reduced-form model outlined in equation (7) includes time trends, it is possible that there remains some type of change in Kenya over this time period that led to reduced levels of fertility. Therefore, to insure there is not an unobserved fertility trend in Kenya that is driving these results, I conduct a pair of placebo tests to estimate the effect of the reform on women whose level of schooling should not have been changed by the reform.[14]

The reduced-form results outlined above indicate that cohorts impacted by the 1985 reform had lower fertility and marriage rates while the first-stage results suggest that these cohorts also had more years of schooling. The reform should have no effect on outcomes for women who left primary school well before the seventh year since their behavior is not impacted by the reform. In contrast, if there is a secular change in fertility for all women in Kenya, we would expect even women with few years of schooling to have reduced fertility over time. To test this, I re-estimate the reduced-form results for all individuals who completed less than five years of school. The results for women with less than five years of school are displayed in Table 8. Of the 44 point estimates, there is only a single statistically significant estimate, for marriage at age 17. Only the result for first birth, in Panel C, possesses a pattern similar to the one seen in the previous results, but the pattern for total fertility is no longer present. This evidence strongly suggests that the robust set of results seen up to this point is not due to some trend affecting all Kenyans over this time period.

I repeat this placebo exercise for college attendees. The reform's effect on these women is ambiguous, due to the pre-reform education structure's inclusion of "upper secondary" school. However, a college graduate under either education system is in school for the same number of years, 16.[15] Estimating the model for women who at least attended college yields results, shown in Table 9, similar to those for women with less than five years of school. The results for college attendees are even more

[14] These placebo tests are done under the assumption that changing the length of primary school did not have an effect on the demand for education.

[15] The pre-reform system consisted of 7+4+2+3 years of school for a college graduate. The post-reform system is comprised of 8+4+4 years of school.

striking. There is no persistent pattern in any of the four panels of results and there is again a solitary statistically significant variable, a positive estimate for first sex by age 22. These estimates emphasize the fact that the result is concentrated with the individuals whose schooling is increased by the reform. Individuals who would have obtained more or less schooling, regardless of the education reform, show no signs of altered behavior, suggesting that there is no secular trend in the data contaminating the basic reduced-form results.[16]

Expanding beyond Kenya, I conduct one final placebo test. Although different in many respects, such as income, ethnicity, education, and religion, Tanzania shares a common history with Kenya during this time period. Both nations were former British colonies before gaining their independence in the 1960s, and both nations originally adopted the same schooling system, a system of education that Tanzania still employs to this day. The number of births by age 25 in Tanzania, for each birth quarter, is shown in Figure 7. Unlike Figure 4 for Kenya, there is no change in slope for the transition birth cohorts, and although decreasing, the decline is generally gradual. I estimate the results for Tanzania using the same reduced-form equation, except for the ethnicity control which is not included in DHS data for Tanzania, the estimates are shown in Table 10. The results show no sign of any negative effect. There is no direct interpretation of positive results found, due to the fact that there was no education reform occurring in Tanzania. East Africa was experiencing a great deal of change over this time period, and these results show that the model is not capturing some effect from the overall transition occurring in the region.[17] This set of results highlight the fact that the instrument is capturing a change that is specific to the respondents whose level of education was enhanced by the 1985 reform.

d. Exploration of Potential Mechanisms

Investigating potential avenues through which this effect has occurred can provide important

[16] When the model is estimated for only women who completed between 7 years of primary school and 4 years of secondary school all of the results hold and the first intercourse results become significant.
[17] The statistical significance, point estimates, and pattern seen in Panel B and Panel D are not consistent across different model specifications using the alternative instrument and restricting the sample to women over 25.

information. In the first two columns of Table 11, the dependent variable is equal to one if the respondent is currently working, zero otherwise. The estimates for the currently working variable show that the reform led a 5.1 percentage point increase in employment for women. The result holds when the sample is restricted to only include women over 20, women more likely to be actively participating in the workforce. The outcome in the third column is an indicator variable equal to one if the respondent reports using a modern contraceptive method. The results also show evidence of a 10.9 percentage point increase in contraceptive use; this result is similar for both married and unmarried women. The result in Table 11's last column demonstrates that the increased education generated by Kenya's policy change has not led to more respondents reporting having ever terminated a pregnancy. Estimates not shown find that the reform did not affect the age of a woman's husband, the husband's education, or the age difference between husband and wife. These results provide evidence that the fertility decline was not driven by assortative matching in the marriage market.

When repeating the model in column (3) with the restricted education samples, less than five years and college attendance, the estimates for the use of modern contraceptive methods are positive but statistically insignificant.[18] The estimates for termination of a pregnancy remain small and statistically insignificant for all groups. For the lowest educated women, there is a statistically significant increase in the fraction of women currently working. This suggests that the reform's adjustments to the curriculum to emphasize work related skills in primary school may have been successful even for the early grades. These results also suggest that the reform's effect on intercourse, marriage, and fertility was not driven by the increase in employment.

To further investigate changes in household bargaining power, I use questions included in only the two latest rounds of the DHS that ask women if they have any part of the final say in various household decisions. Restricting the analysis to only two rounds of data greatly reduces the sample size and many of the older birth quarters are reduced to only a few observations. These results are the least

[18] The point estimate for women with less than 5 years of education is 0.05, and for women who attended college is 0.16. These estimates are sensitive to alternate control trends and the use of the alternative instrument. The full sample is robust to these adjustments, the estimates are shown in the appendix.

robust results presented in this paper, and are often statistically insignificant using the baseline specification. However, some useful, albeit suggestive, evidence can be gleaned from using the alternative cubic three-part trend discussed in the previous section.[19]

In Table 12, I present estimates using five measures of empowerment for married women: their own healthcare, large household purchases, daily purchases, visiting family and friends, and food cooked each day. The variable is equal to one if the woman has any part of the final say, whether it is her decision or joint with another individual. Across four of the five variables there is a significant increase in the woman's inclusion in the final decision. For food cooked each day, the decision is generally left to the women, there is not much room for an increase on this margin. When restricting the sample to women with less than five years of education or to those who attended college, the results in the third and fourth column for daily purchases and visiting family and friends, are statistically insignificant. This suggests that some of the increase in the empowerment of women over this time period may have been driven by increased education levels generated by the reform. The increase in the use of modern contraceptive methods, along with the results in Table 12, suggests that the higher levels education generated by the reform may have led to an increase in intra-household bargaining power for women. This growth in the bargaining power, and empowerment of women, may be crucial mechanisms through which the reduction in fertility persists even after women complete their schooling.

I also investigate a potential incarceration effect and find no consistent relationship between the conditional likelihood of attending or graduating from secondary school and the magnitude of the reform's effect on either fertility or marriage. If there was a significant incarceration effect behind the reductions in these outcomes, the reform's effect should be stronger for individuals more likely to receive their extra year in school at older ages. In Figure 8, I plot the fraction of women in each ethnicity/province group who report having entered secondary school in the 1977 World Fertility Study

[19] The results when using the quadratic trend are included in the appendix, for the full sample and a sample only including individuals with more than seven years of primary schooling and no more than four years of secondary schooling. These estimates generally yield positive, but statistically insignificant estimates. Further explanation of the three-part trend is included in the appendix, as well.

against the estimated effect of the reform for women in each corresponding group. The effect is estimated by rerunning the reduced-form model for each group. In the left panel I plot the effect on total fertility at the age of 25, and the effect on marriage in the right panel. In Figure 9, I repeat this procedure for the fraction of women who have completed four years of secondary school by type of childhood residence/ethnicity groups using data from the DHS sample.[20] The linear line represents a regression line weighted by the number of observations in each group. These figures demonstrate that the incarceration effect is not likely driving the results. Furthermore, this analysis demonstrates the large amount of heterogeneity in the reform's effect that exists within demographic groups, and even for women with similar education levels. This underscores the importance of examining the relationship between schooling and fertility as part of a large scale reform.

e. 2SLS Estimates Using Years of Schooling

As documented earlier, there is a concern with measuring years of schooling for women in transition cohorts who progress beyond primary school. Due to the measurement error in the years of schooling variable, the estimates in this section should be analyzed with caution. However, it would be useful to calculate the 2SLS estimates using years of schooling to compare the magnitude of the estimates from this paper with those found in the literature. The results in the first two columns of Table 4 showed that the first stage estimate is slightly higher for the restricted sample, where the years of schooling variable is correctly measured. However, the confidence intervals of the two estimates significantly overlap, suggesting the estimate for each sample is comparable.

Using the years of schooling variable, with the first stage described in equation (6), the second-stage equation can be described as follows:

$$y_{icq} = \alpha_F + \widehat{yrschl}_{cq}\beta_S + \sum_{p=1}^{3} age_{icq}^{p}\pi_{Sp} + \sum_{n=1}^{2} qtrend_{cq}^{n}\varphi_{Sn} + X_{icq}\theta_S + \varepsilon_{Sicq} \;. \qquad (8)$$

[20] Using entrance into secondary school for the DHS sample yields figures similar to Figure 9, the correlation between entry and completion is 0.95.

All variables are defined the same way as previous equations. I estimate the second stage equation on the same four main outcomes seen throughout this section, age at first intercourse, first marriage, first birth, and total fertility. To illustrate the estimated effect of an additional year of education, I estimate the model for each age, as was done with the baseline results, and plot the point estimates of β_S in the four panels of Figure 10, along with the 90 percent confidence interval.

The results for first intercourse, Figure 10A, show no positive effect, as expected, but the dip for late teens is evident. For the effect on marriage, the strong negative effect for the ages 18 to 21 seen in the reduced-form is now visible in Figure 10B, as is the persistence of the reduction in marriage. The estimated ten percentage point reduction for these ages is large. Only 40 percent of 18-year-old women in the sample are married; the fraction increases to 67 percent for 21-year-old women. The effect on marriage shows a sharp reversion towards zero at age 22, but the point estimates remain consistently below zero. The effect for first birth, Figure 10C, finds its lowest point at age 20 and remains negative through the age of 25. Most prominently, the effect of an extra year of schooling on total fertility continues to become increasingly negative for older women, Figure 10D. The one year increase in schooling is estimated to decrease fertility for women by nearly one-half of a birth, at the age of 25.[21]

To lend some qualitative validity to the 2SLS estimates, I also estimate the 2SLS model after removing the transition cohorts. This subsample only includes pre and post reform cohorts, for whom the years of schooling variable is correctly defined. The pattern across all four outcomes is similar to that seen in Figure 10. In fact, when using the restricted subsample, the model estimates slightly larger reductions in sexual activity for women in their late teens, child bearing across all ages, and total fertility. However, these estimates were often less precise.[22]

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[21] Using the Durbin-Wu-Hausman test of the null hypothesis that schooling is exogenous, I am able to reject exogeneity for 11 of the model specifications shown in Tables 4 and 6.

[22] Figures outlining these results can be found in the appendix. Again using the Durbin-Wu-Hausman test, I am able to reject that schooling is exogenous in 19 of the model specifications when the transition cohorts are not included.

VI. Conclusion

The results from this paper show that education structure could be a significant policy lever for governments to employ in their effort to increase education. The increase in education, driven by the 1985 reform, is also shown to greatly impact a woman's fertility decisions. The increased schooling led to postponed marriage and reduced levels of fertility. These results demonstrate the importance of education and the positive effect that increased schooling can have for young woman.

The results for fertility demonstrate that the effect is not confined to the time period that women attend school, and is actually strengthening for women in their early twenties. This suggests that mechanisms other than the incarceration effect are at work. I am able to rule out a number of other mechanisms, as well. The results are likely not driven by employment, there was no effect on the likelihood that a woman has ever had a pregnancy terminated, and there is no evidence of assortative matching in the marriage market. Evidence of increased contraceptive use and empowerment in the household decision making process for married women suggests that the human capital accrued during a woman's extra time in school may be leading to greater bargaining power in the household fertility decision.

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Figure 1 – 1985 School Reform

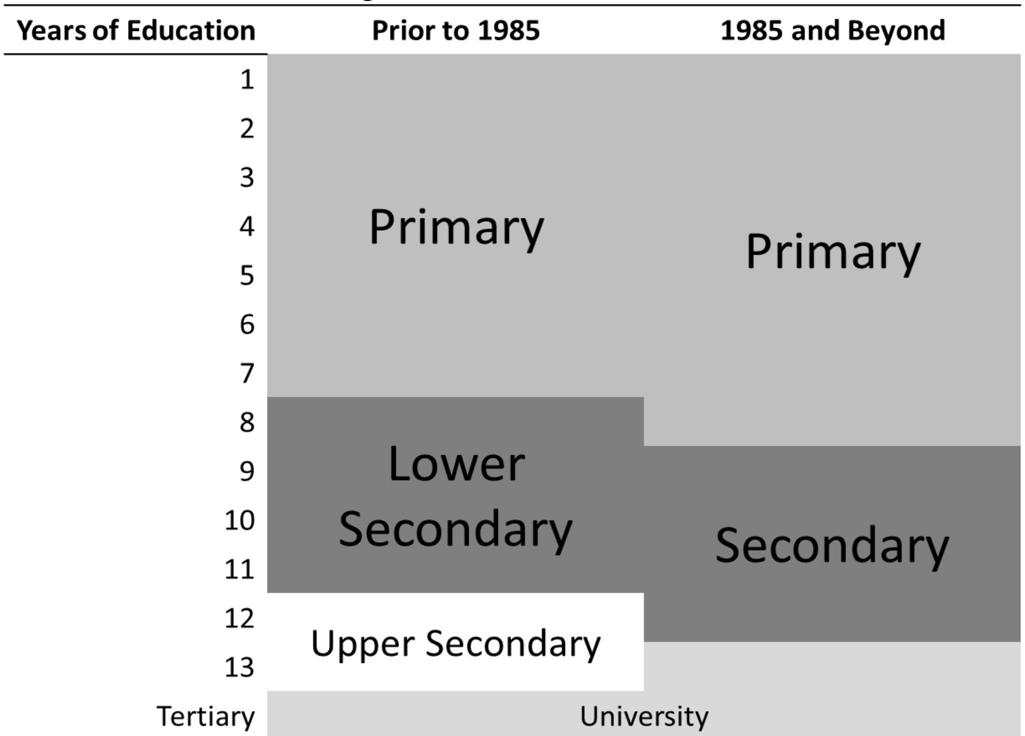


Figure 2 – Fraction of Women Whose Highest Level of Education is the Eighth Year of Primary School

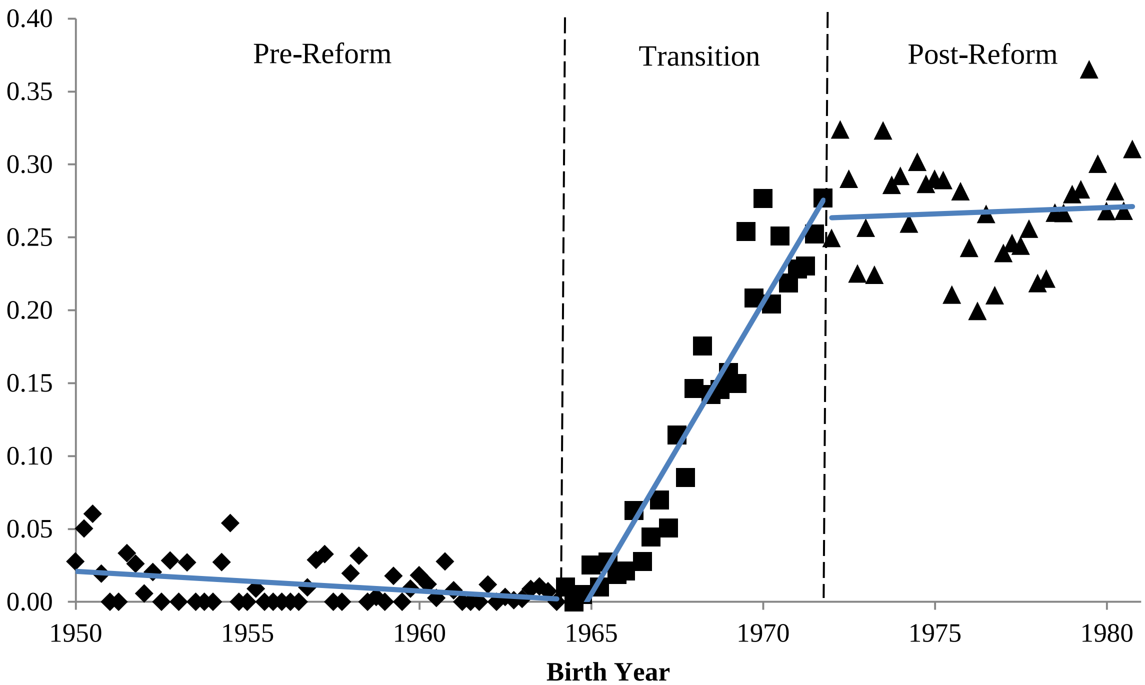


Figure 3 – Fraction of Women Who Completed At Least Eight Years of School

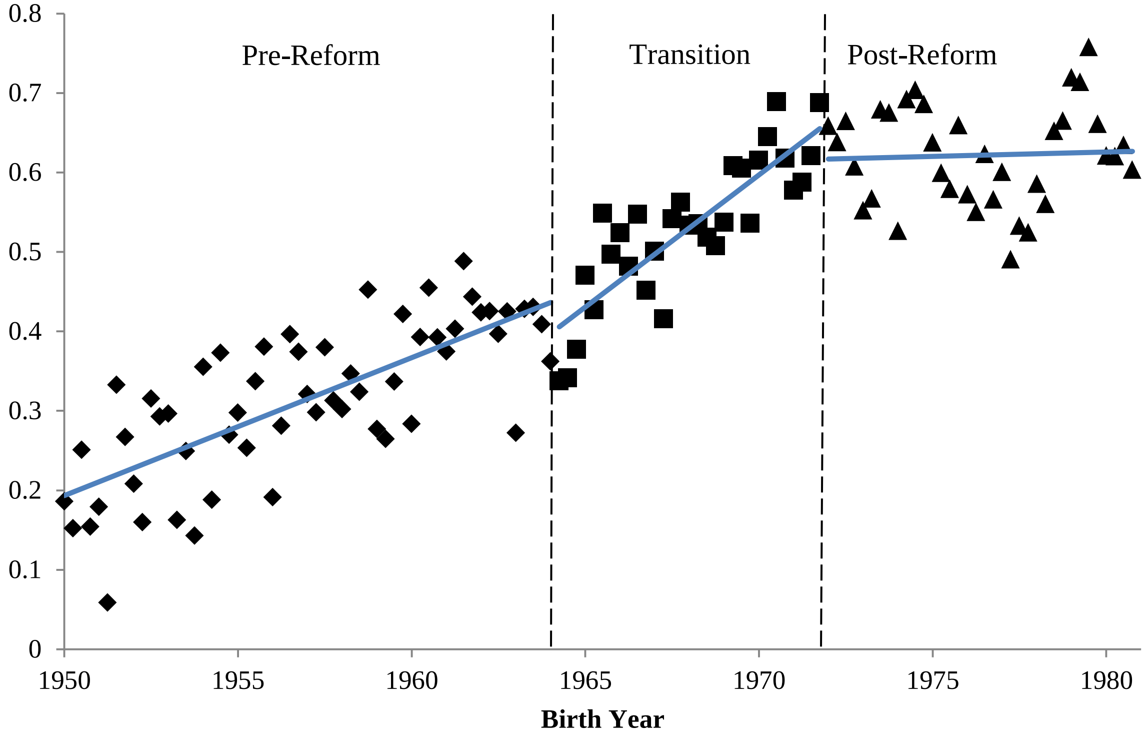


Figure 4 – Total Fertility at Age 25

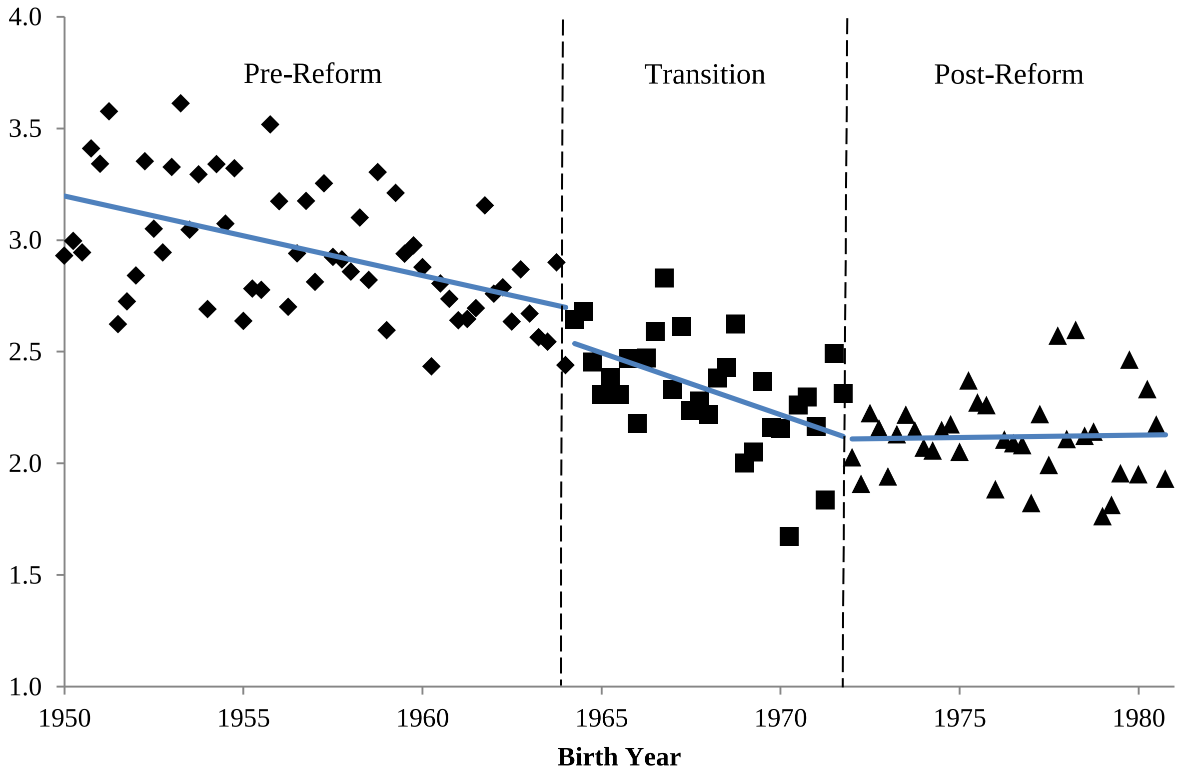


Figure 5 - Instrument: Estimated Probability of Being Treated by the 1985 Reform

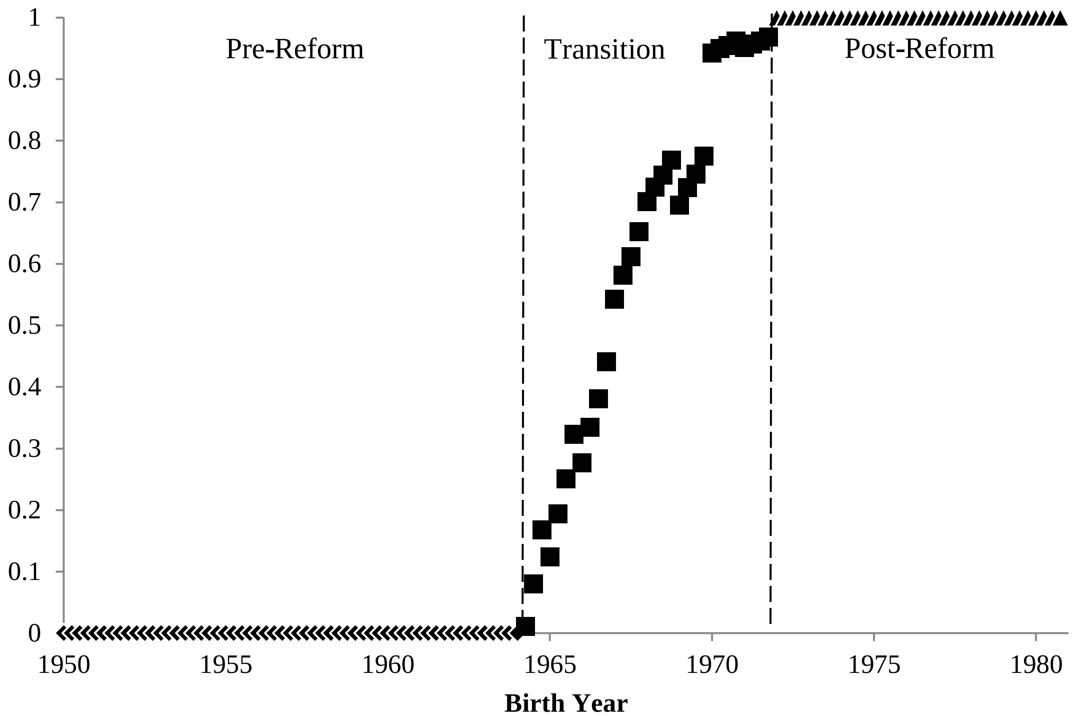


Figure 6 – Comparison of Highest Grade Completed is Eighth Grade (Xs) and the Instrument (solid shapes)

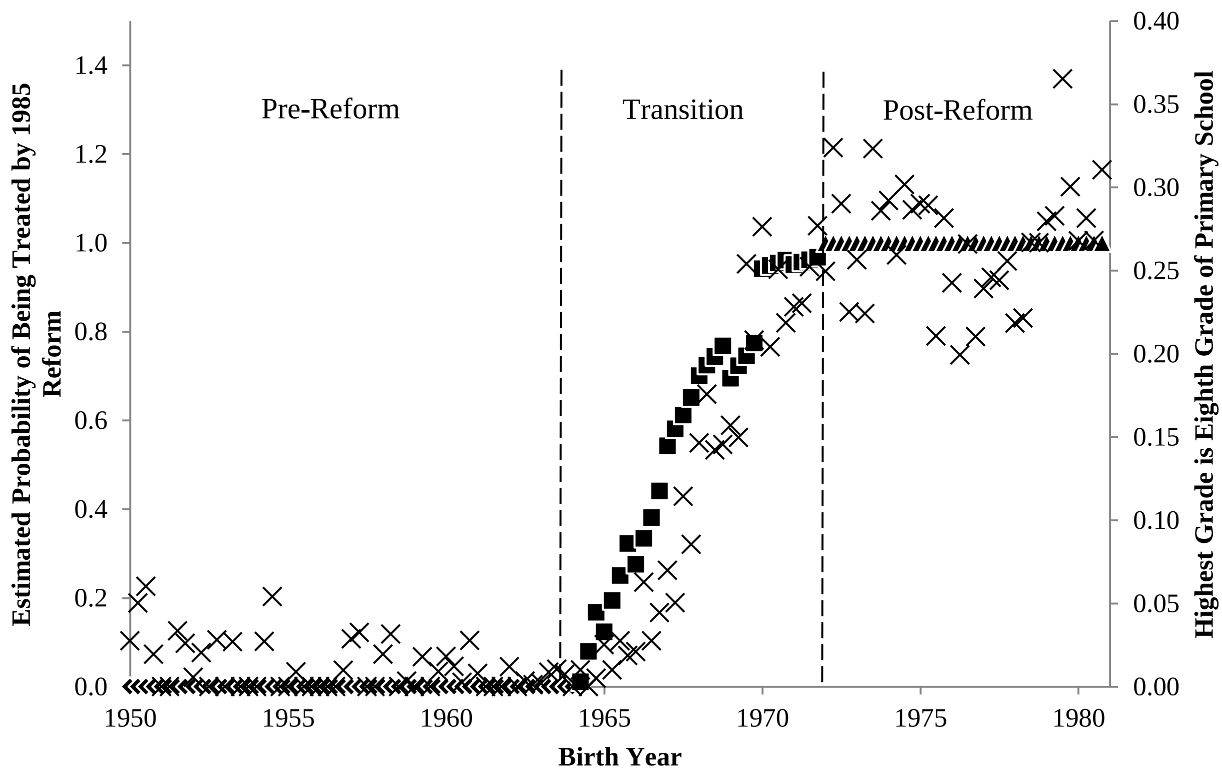


Figure 7 – Tanzania: Total Fertility at Age 25

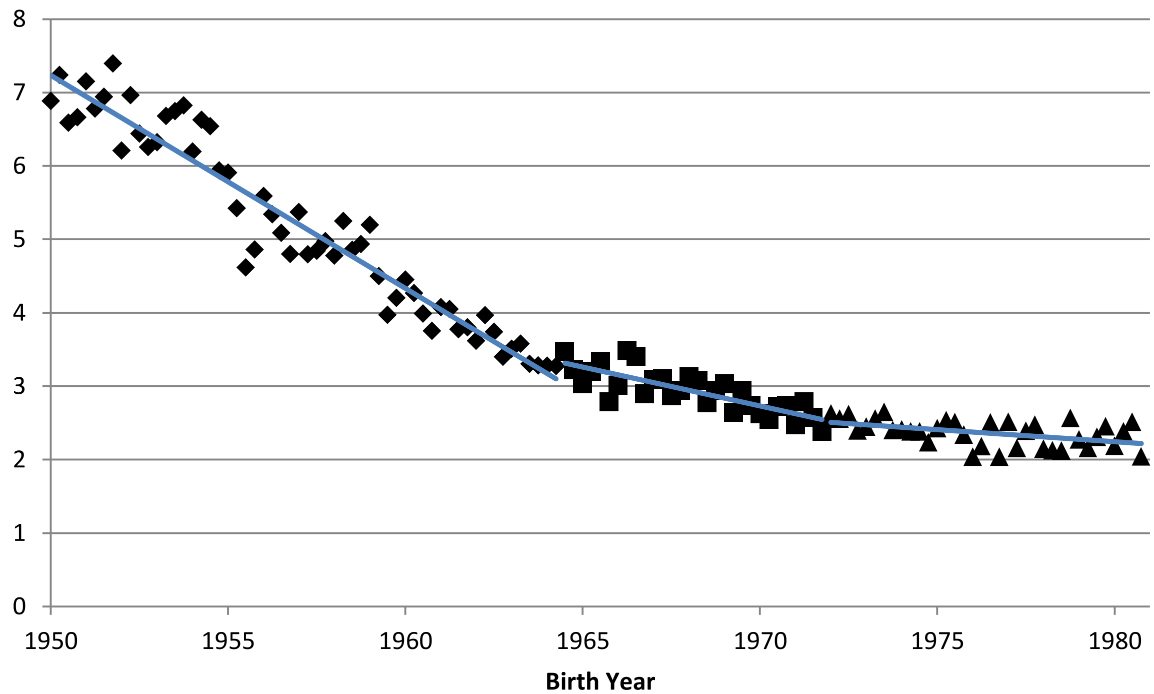


Figure 8 – Effect of Reform on Fertility and Marriage at 25 by Likelihood of Attending Secondary School (Ethnicity and Province groups using pre-reform WFS data)

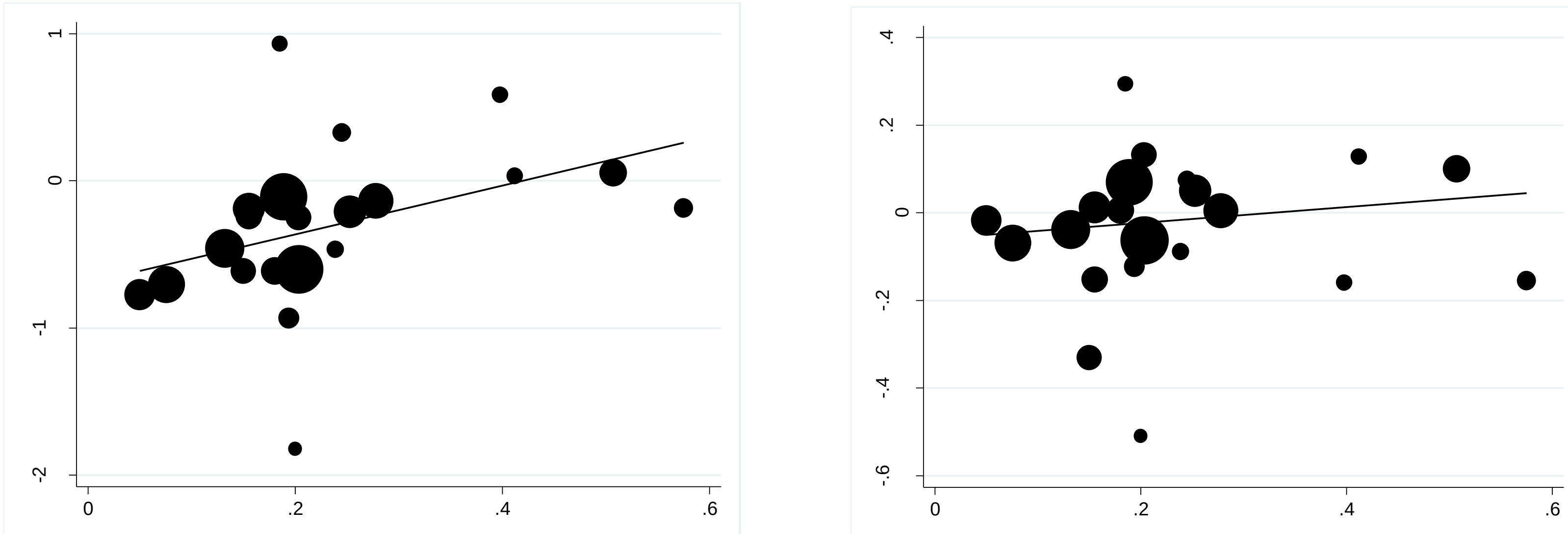


Figure 9 - Effect of Reform on Fertility and Marriage at 25 by Fraction Completing Secondary School (Ethnicity and Childhood Type of Residence groups using DHS)

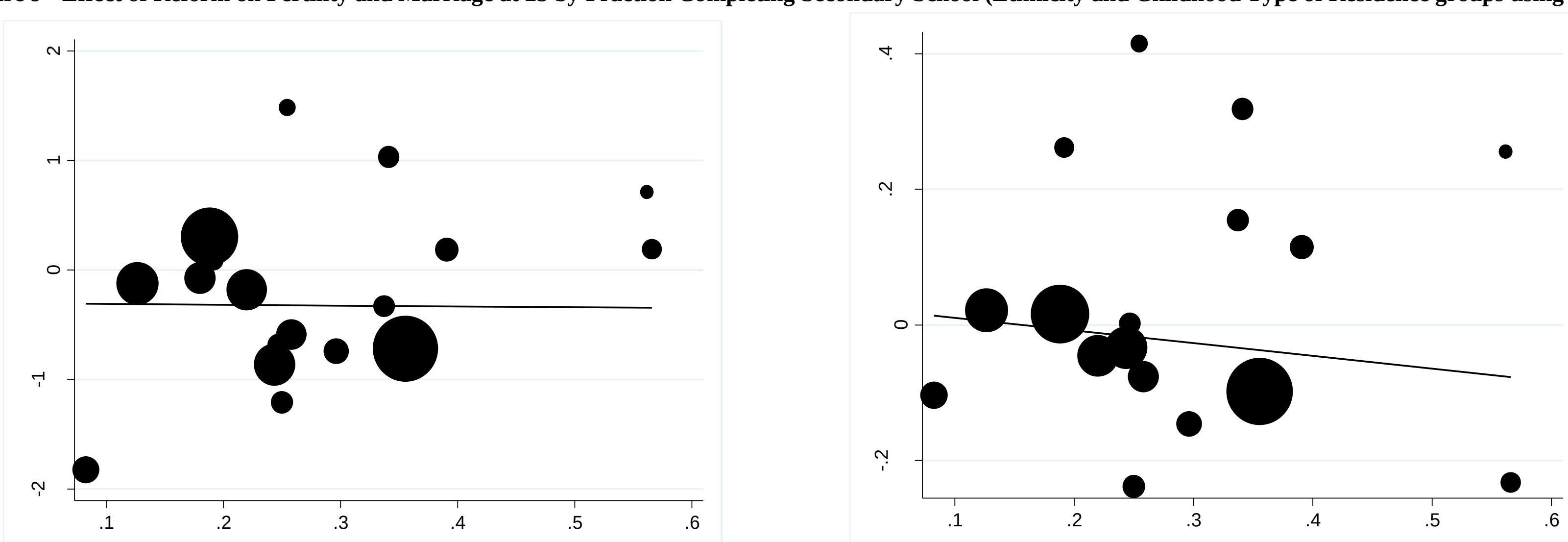
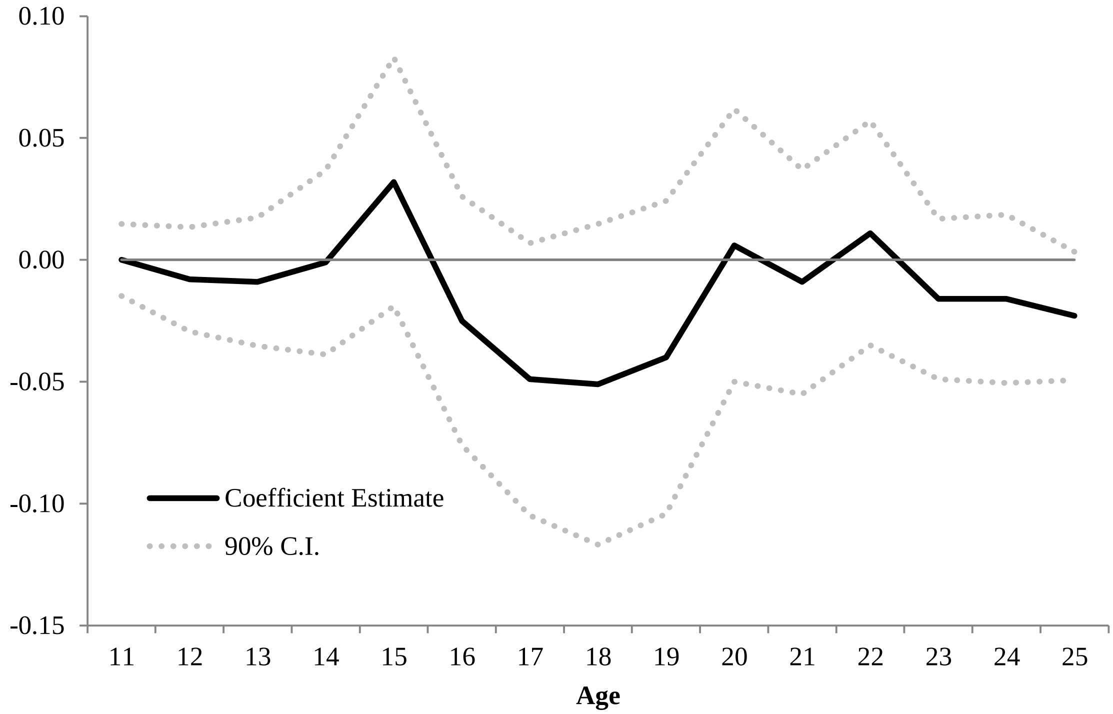
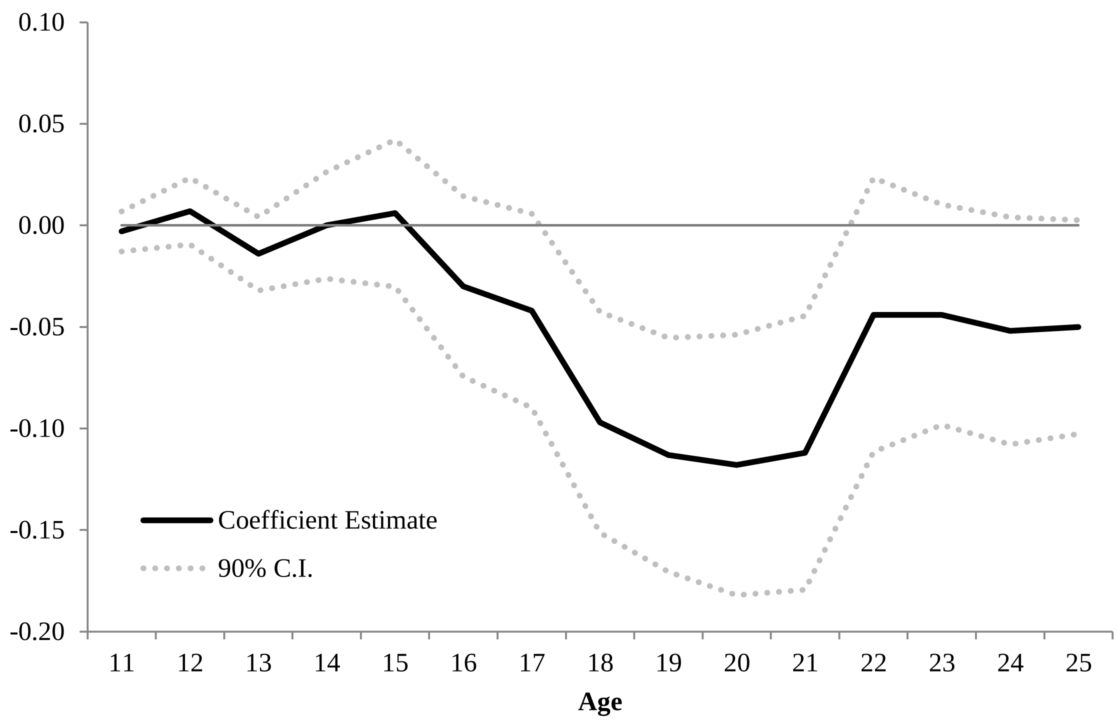


Figure 10 – 2SLS: Coefficient Estimates for the Effect of an Additional Year of Schooling

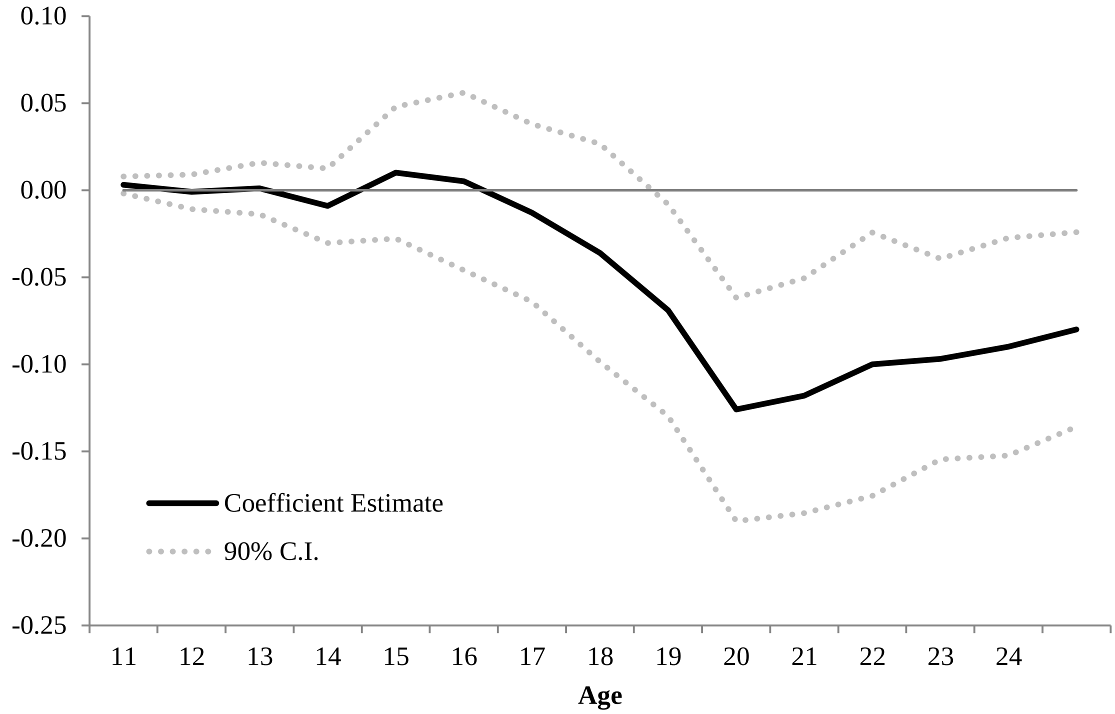
A. Effect on First Intercourse, by Age



B. Effect on First Marriage, by Age



C. Effect on First Birth, by Age



D. Effect on Total Fertility, by Age

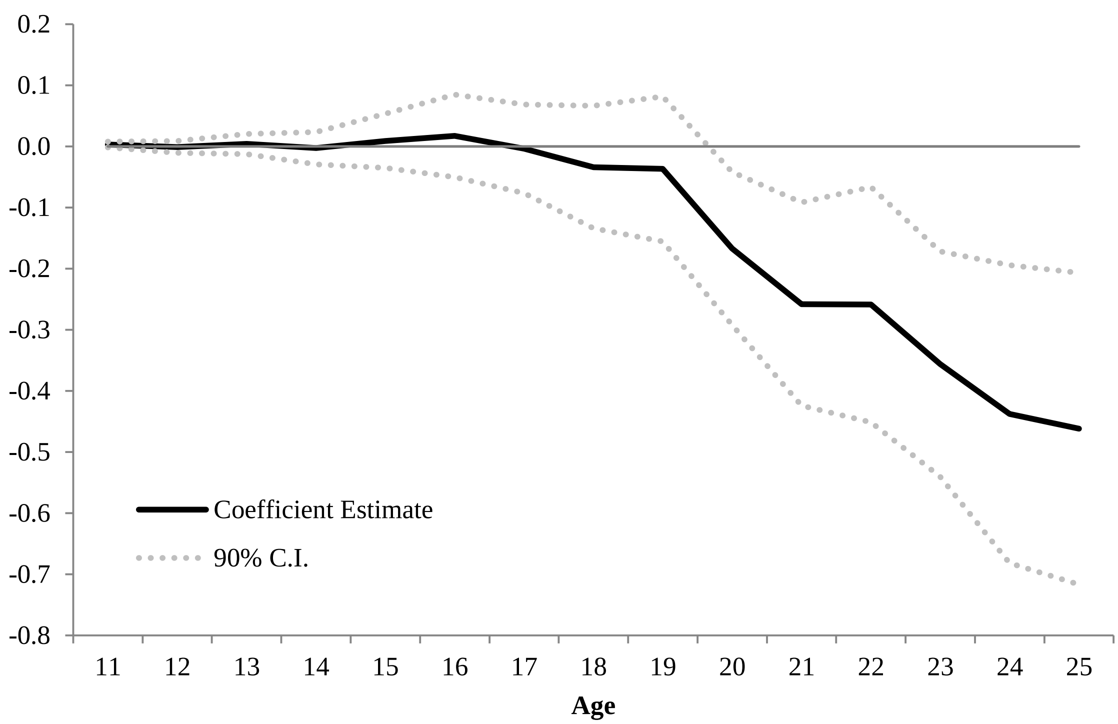


Table 1 – Summary Statistics

	Mean	Std. Dev.
Age	28.07	(8.19)
Years of School	7.416	(3.717)
Fraction with at Least 8 Years of School	0.485	
Age at First Intercourse	16.87	(3.01)
Age at First Marriage	18.91	(3.77)
Age at First Birth	19.10	(3.34)
Fraction with First Intercourse by age:		
15	0.307	
18	0.698	
21	0.880	
25	0.952	
Fraction Married by age:		
15	0.120	
18	0.397	
21	0.672	
25	0.854	
Fraction with First Birth by age:		
15	0.086	
18	0.398	
21	0.715	
25	0.894	
Total Fertility by age:		
15	0.101	(0.351)
18	0.551	(0.793)
21	1.364	(1.185)
25	2.535	(1.578)
Fraction Working	0.507	
Fraction Using Modern Contraceptive	0.419	

Source: Demographic and Health Survey: 1989, 1993, 1998, 2003, and 2008. Sample includes women born between 1950 and 1980.

Table 2 – DHS Education Variables

A. Highest Education Level Attended	B. Highest Year of Education
	0
	1
	2
0 – No Education	3
1 – Primary	4
2 – Secondary	5
3 – Tertiary	6
	7
	8
	9
	10

Table 3 – OLS: Schooling and First Birth and Total Fertility, by Age

A. First Intercourse by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Yrsschl	-0.036***	-0.044***	-0.046***	-0.043***	-0.037***	-0.026***	-0.021***	-0.015***	-0.012***	-0.008***	-0.004***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
B. First Marriage by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Yrsschl	-0.024***	-0.035***	-0.045***	-0.049***	-0.050***	-0.045***	-0.040***	-0.034***	-0.029***	-0.022***	-0.017***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
C. First Birth by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Yrsschl	-0.017***	-0.028***	-0.038***	-0.045***	-0.049***	-0.048***	-0.043***	-0.036***	-0.030***	-0.023***	-0.018***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
D. Total Fertility by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Yrsschl	-0.021***	-0.037***	-0.055***	-0.076***	-0.096***	-0.113***	-0.126***	-0.135***	-0.142***	-0.147***	-0.147***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. The sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 4 – First Stage

	Years of Schooling		AtLeast8	Education 8
	All Cohorts	Excluding Transition		
	(1)	(2)	(3)	(4)
$Inst_{cq}$	0.766***	0.861***	0.233***	0.265***
	(0.163)	(0.195)	(0.025)	(0.017)
F-Statistic	21.97	19.55	89.35	252.92
N	19,693	13,469	19,693	19,693

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. The dependent variable is a measure of education. The years of schooling variable is taken from the DHS, atleast8 and education8 are calculated as explained in the text. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 5 – Reduced From: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.022	-0.017	-0.033	-0.034	-0.026	0.004	-0.006	0.006	-0.011	-0.011	-0.018
	(0.020)	(0.022)	(0.024)	(0.029)	(0.027)	(0.023)	(0.019)	(0.016)	(0.014)	(0.014)	(0.012)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.004	-0.021	-0.029	-0.065***	-0.074***	-0.079***	-0.072***	-0.026	-0.031	-0.036*	-0.039*
	(0.015)	(0.020)	(0.021)	(0.024)	(0.023)	(0.024)	(0.022)	(0.024)	(0.023)	(0.022)	(0.020)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.007	0.003	-0.009	-0.024	-0.045*	-0.084***	-0.076***	-0.059**	-0.067***	-0.063**	-0.062***
	(0.015)	(0.021)	(0.022)	(0.026)	(0.027)	(0.025)	(0.027)	(0.027)	(0.024)	(0.026)	(0.021)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.007	0.012	-0.003	-0.023	-0.024	-0.112**	-0.166**	-0.153**	-0.246***	-0.307***	-0.361***
	(0.018)	(0.027)	(0.030)	(0.042)	(0.049)	(0.055)	(0.069)	(0.072)	(0.082)	(0.094)	(0.097)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 6 – Reduced From with Control for Grade One Enrollment: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.009	-0.037	-0.044	-0.037	-0.028	-0.007	-0.015	-0.011	-0.020	-0.015	-0.028**
	(0.020)	(0.026)	(0.031)	(0.038)	(0.034)	(0.028)	(0.024)	(0.021)	(0.017)	(0.016)	(0.013)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
B. First Marriage by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.006	-0.040*	-0.046**	-0.092***	-0.096***	-0.093***	-0.082***	-0.008	-0.034	-0.032	-0.040
	(0.017)	(0.021)	(0.022)	(0.027)	(0.026)	(0.028)	(0.027)	(0.030)	(0.028)	(0.026)	(0.026)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
C. First Birth by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.014	-0.003	-0.023	-0.058*	-0.062*	-0.089***	-0.092**	-0.066	-0.071**	-0.065*	-0.055*
	(0.016)	(0.022)	(0.024)	(0.033)	(0.033)	(0.031)	(0.037)	(0.041)	(0.036)	(0.039)	(0.028)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
D. Total Fertility by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.014	0.004	-0.023	-0.083*	-0.069	-0.155**	-0.239***	-0.218**	-0.320***	-0.390***	-0.435***
	(0.019)	(0.028)	(0.031)	(0.047)	(0.059)	(0.065)	(0.086)	(0.096)	(0.106)	(0.115)	(0.119)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, the number of students enrolled in grade one during the respondent's age 6 year, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 7 – Reduced From using an Alternative Instrument: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	-0.011	-0.053	-0.070**	-0.057*	-0.052*	-0.025	-0.045**	-0.027	-0.048***	-0.032*	-0.035**
Instrument_{cq}	(0.024)	(0.032)	(0.034)	(0.035)	(0.030)	(0.026)	(0.022)	(0.020)	(0.018)	(0.019)	(0.017)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	-0.004	-0.031	-0.040	-0.082***	-0.083***	-0.089***	-0.082***	-0.008	-0.022	-0.031	-0.045
Instrument_{cq}	(0.021)	(0.027)	(0.028)	(0.029)	(0.029)	(0.030)	(0.028)	(0.031)	(0.033)	(0.033)	(0.036)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	-0.010	-0.032	-0.042	-0.067	-0.085**	-0.105***	-0.094**	-0.075**	-0.070**	-0.068**	-0.077**
Instrument_{cq}	(0.017)	(0.027)	(0.028)	(0.041)	(0.039)	(0.034)	(0.042)	(0.037)	(0.032)	(0.032)	(0.033)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	-0.010	-0.022	-0.055	-0.089	-0.101	-0.204***	-0.270***	-0.255***	-0.322***	-0.374***	-0.414***
Instrument_{cq}	(0.019)	(0.031)	(0.035)	(0.055)	(0.070)	(0.064)	(0.086)	(0.086)	(0.096)	(0.107)	(0.120)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. Alternative Instrument_{cq} is calculated without using enrollment data, as outlined in the text. It is the estimated probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 8 – Reduced-form, Only Women with Less Than Five Years of School: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.028	-0.026	-0.003	0.033	0.010	0.023	0.041	0.045	0.035	0.028	-0.001
	(0.062)	(0.066)	(0.065)	(0.052)	(0.051)	(0.041)	(0.040)	(0.040)	(0.040)	(0.046)	(0.046)
N	2,980	2,939	2,888	2,818	2,755	2,662	2,603	2,528	2,447	2,349	2,257
B. First Marriage by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.005	-0.017	-0.069*	-0.012	-0.015	-0.062	-0.037	-0.067	-0.043	0.003	0.008
	(0.028)	(0.037)	(0.039)	(0.059)	(0.077)	(0.085)	(0.074)	(0.070)	(0.070)	(0.078)	(0.071)
N	2,980	2,939	2,888	2,818	2,755	2,662	2,603	2,528	2,447	2,349	2,257
C. First Birth by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.087	0.056	-0.021	-0.027	0.001	-0.051	-0.061	-0.058	-0.035	-0.055	-0.089
	(0.072)	(0.072)	(0.069)	(0.087)	(0.065)	(0.072)	(0.071)	(0.080)	(0.071)	(0.077)	(0.085)
N	2,980	2,939	2,888	2,818	2,755	2,662	2,603	2,528	2,447	2,349	2,257
D. Total Fertility by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.087	0.105	0.015	-0.018	0.107	-0.039	-0.076	-0.028	-0.026	0.005	-0.190
	(0.104)	(0.117)	(0.132)	(0.174)	(0.203)	(0.247)	(0.285)	(0.304)	(0.339)	(0.381)	(0.449)
N	2,980	2,939	2,888	2,818	2,755	2,662	2,603	2,528	2,447	2,349	2,257

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column, and those who completed less than five years of education. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 9 – Reduced-form, Only Women who Attended College: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.104	0.084	0.073	0.034	0.179	0.258*	0.093	-0.020	-0.025	0.067	0.119
	(0.065)	(0.075)	(0.103)	(0.133)	(0.129)	(0.150)	(0.145)	(0.135)	(0.122)	(0.098)	(0.087)
N	989	989	988	986	983	981	974	949	904	864	829
B. First Marriage by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.028	-0.010	0.012	-0.021	0.010	0.018	0.099	0.079	0.172	0.175	0.149
	(0.022)	(0.030)	(0.040)	(0.043)	(0.075)	(0.084)	(0.107)	(0.120)	(0.121)	(0.124)	(0.113)
N	989	989	988	986	983	981	974	949	904	864	829
C. First Birth by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.000	0.007	0.041	0.016	-0.007	0.003	0.055	0.271*	0.145	0.170	0.166
	(0.031)	(0.038)	(0.042)	(0.057)	(0.080)	(0.098)	(0.116)	(0.141)	(0.126)	(0.138)	(0.120)
N	989	989	988	986	983	981	974	949	904	864	829
D. Total Fertility by Age:											
	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.000	0.007	0.034	0.002	-0.022	-0.013	0.040	0.230	0.022	0.010	0.104
	(0.031)	(0.038)	(0.056)	(0.071)	(0.104)	(0.147)	(0.182)	(0.213)	(0.245)	(0.295)	(0.305)
N	989	989	988	986	983	981	974	949	904	864	829

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column, and those who attended some level of tertiary education. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 10 – Reduced-form, Tanzania: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.025	-0.009	0.002	0.022	0.024	0.009	0.013	-0.000	0.005	0.013	0.009
	(0.031)	(0.035)	(0.037)	(0.032)	(0.029)	(0.020)	(0.019)	(0.016)	(0.015)	(0.014)	(0.011)
N	13,713	13,169	12,683	12,183	11,574	10,973	10,435	9,898	9,420	8,716	7,967
B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.010	0.006	-0.003	0.047	0.055*	0.046	0.055*	0.053*	0.053**	0.055**	0.047*
	(0.022)	(0.028)	(0.030)	(0.032)	(0.029)	(0.028)	(0.028)	(0.027)	(0.025)	(0.024)	(0.024)
N	13,713	13,169	12,683	12,183	11,574	10,973	10,435	9,898	9,420	8,716	7,967
C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.027	-0.031	-0.032	-0.018	0.033	0.034	0.057**	0.037	0.021	0.018	0.023
	(0.017)	(0.024)	(0.030)	(0.032)	(0.029)	(0.028)	(0.026)	(0.027)	(0.025)	(0.023)	(0.024)
N	13,713	13,169	12,683	12,183	11,574	10,973	10,435	9,898	9,420	8,716	7,967
D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.118	0.148	0.168	0.203	0.279**	0.339***	0.398***	0.378***	0.368***	0.377***	0.359**
	(0.132)	(0.129)	(0.126)	(0.124)	(0.119)	(0.117)	(0.114)	(0.114)	(0.118)	(0.132)	(0.145)
N	13,713	13,169	12,683	12,183	11,574	10,973	10,435	9,898	9,420	8,716	7,967

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The data are from the DHS for Tanzania, using the 1991, 1996, 1999, and 2004 waves. The sample is restricted to only include observations older than the age specified in each column. $Inst_{cq}$ is the probability of Kenyan students being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 11 – Reduced-form: Currently Working, Contraception, and Termination

	Currently Working		Modern Contraceptive	Ever Had a Pregnancy Terminated
	All Observations	Only Age > 20		
	(1)	(2)	(3)	(4)
$Inst_{cq}$	0.051**	0.068***	0.109***	-0.003
	(0.023)	(0.024)	(0.017)	(0.030)
N	19,631	15,288	19,250	10,888

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. The dependent variable in the first two columns is equal to one if currently working, zero otherwise. In the second column the variable is equal to one if currently using a modern contraceptive method, zero otherwise. The dependent variable in the last column is equal to one if the respondent reports ever having a pregnancy terminated, zero otherwise. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table 12 – Reduced-form: Empowerment of Married Women

Does the respondent have some part of final say regarding:					
	Own Healthcare	Large Household Purchases	Daily Purchases	Visiting Family/Friends	Food Cooked Each Day
	(1)	(2)	(3)	(4)	(5)
$Inst_{cq}$	0.261***	0.377***	0.210***	0.171***	0.030
	(0.063)	(0.060)	(0.065)	(0.057)	(0.039)
N	4,405	4,404	4,402	4,405	4,404

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. The dependent variable in each column is equal to one if the woman reports having any part of the final decision for the given category, equal to zero otherwise. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. The sample is restricted to only include married women, in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a three part cubic time trend (pre-treatment, transition, and post-treatment), and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Appendix

a. Description of Three-part Trend

When using the three part trend I assign each of the periods, pre, transition, and post, separate time trends. First, I create a count variable which identifies each individual's quarter of birth. The first quarter in the sample, 1950Q1, is equal to one, the second quarter to two, and the final quarter, 1980Q4, is equal to 124. The beginning and end period of the trends are defined by the implementation of the reform and the calculation of the instrument which identifies the first quarter of the transition period. The first quarter of the transition period is the 58th quarter of the sample, and the first quarter of the post-reform period is the 89th quarter. This variable is then used to define the beginning and end periods of the three trends. The trends are described by the following equations.

$$TrPost_{cq} = \begin{cases} bq_{cq} - 89 \; if \; bq_{cq} \geq 89 \\ 0 \; if \; bq_{cq} < 89 \end{cases} \tag{A1.1}$$

The variable defining an individual's birth quarter is bq_{cq}. In the above specification the first quarter of the post-treatment period is equal to zero. The results are not sensitive to beginning the $TrPost_{cq}$ trend with the 89th quarter being equal to one. The pre-treatment and transition trends are defined as follows.

$$TrPre_{cq} = \begin{cases} |bq_{cq} - 58| \; if \; bq_{cq} \leq 57 \\ 0 \; if \; bq_{cq} > 57 \end{cases} \tag{A1.2}$$

$$TrTran(sition)_{cq} = \begin{cases} 0 \; if \; bq_{cq} \geq 89 \\ bq_{cq} - 57 \; if \; 57 < bq_{cq} < 89 \\ 0 \; if \; bq_{cq} \leq 57 \end{cases} \tag{A1.3}$$

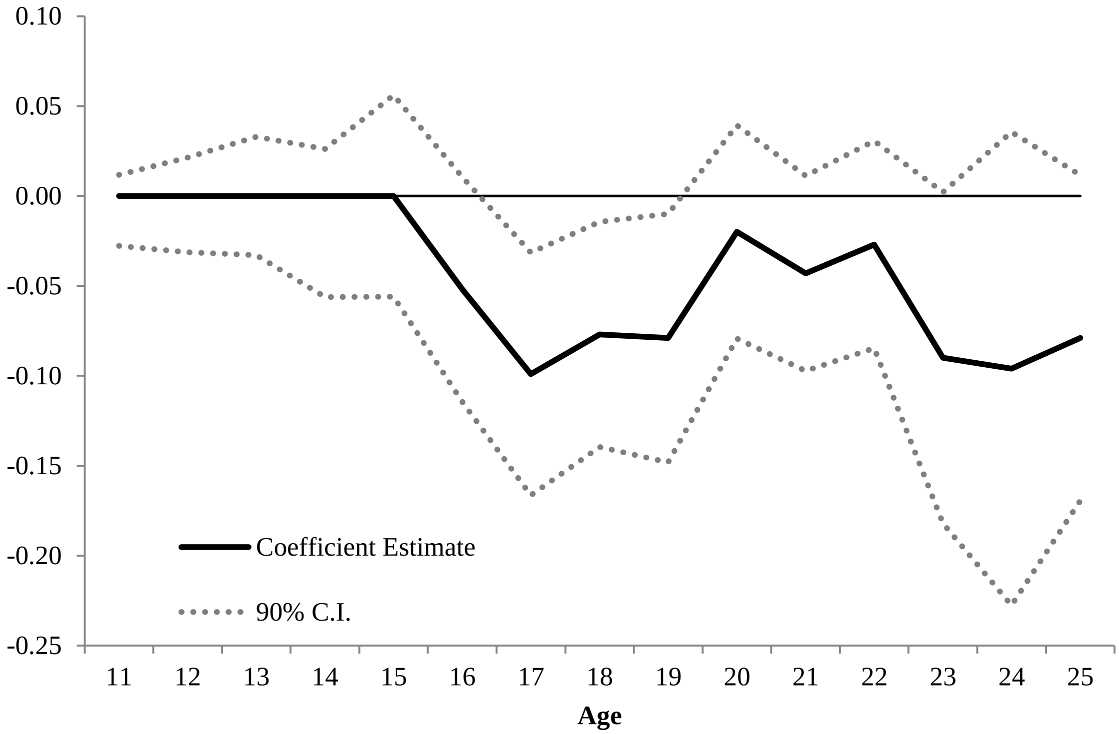
The absolute value used for the $TrPre_{cq}$ keeps the higher order values positive, but is unimportant. The $TrTransition_{cq}$ variable is currently increasing throughout the transition period, the results are insensitive to making this variable decreasing over the time period.

The equation using the three-part trend to estimate the effect of the reform uses the same set of covariates as described previously in the paper. The only difference is the $qtrend_{cq}$ variable is replaced with the three separate variables shown above. The OLS equation can then be written as follows:

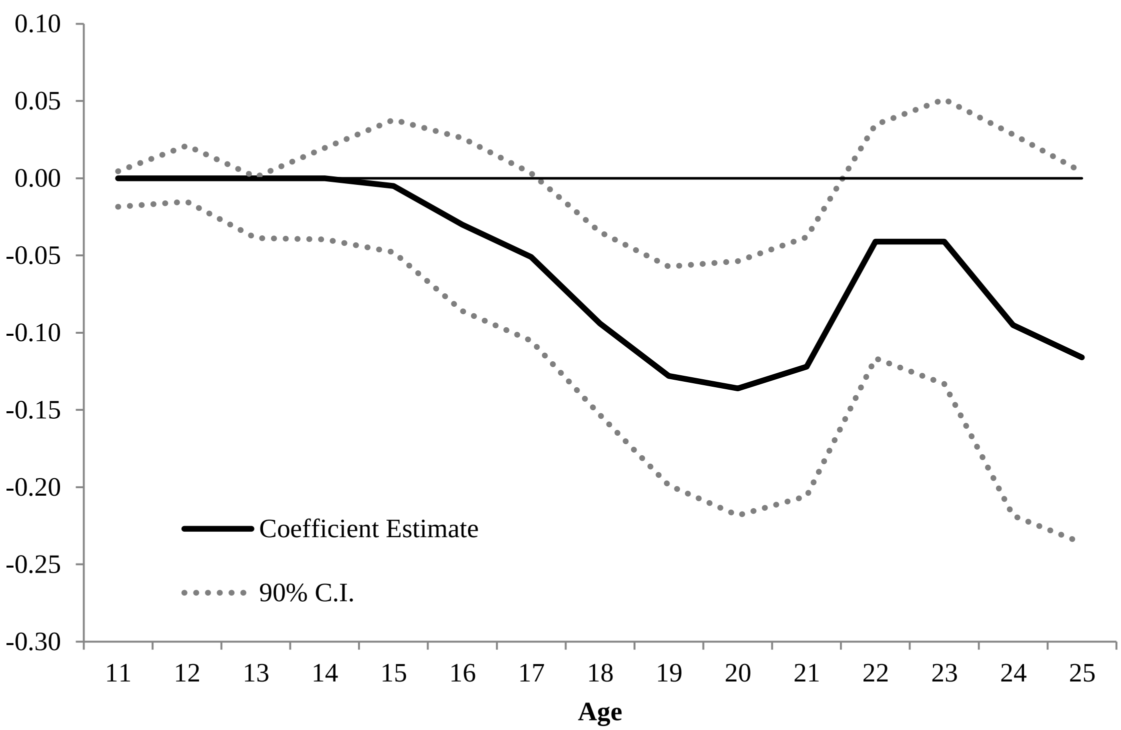
$$y_{icq} = \alpha + yrschl_{icq}\beta + \sum_{p=1}^{3} age_{icq}^{p}\pi_p + \sum_{n=1}^{N}\left[TrPre_{cq}^{n} + TrTran_{cq}^{n} + TrPost_{cq}^{n}\right] + X_{icq}\theta + \varepsilon_{icq}. \quad \text{(A2)}$$

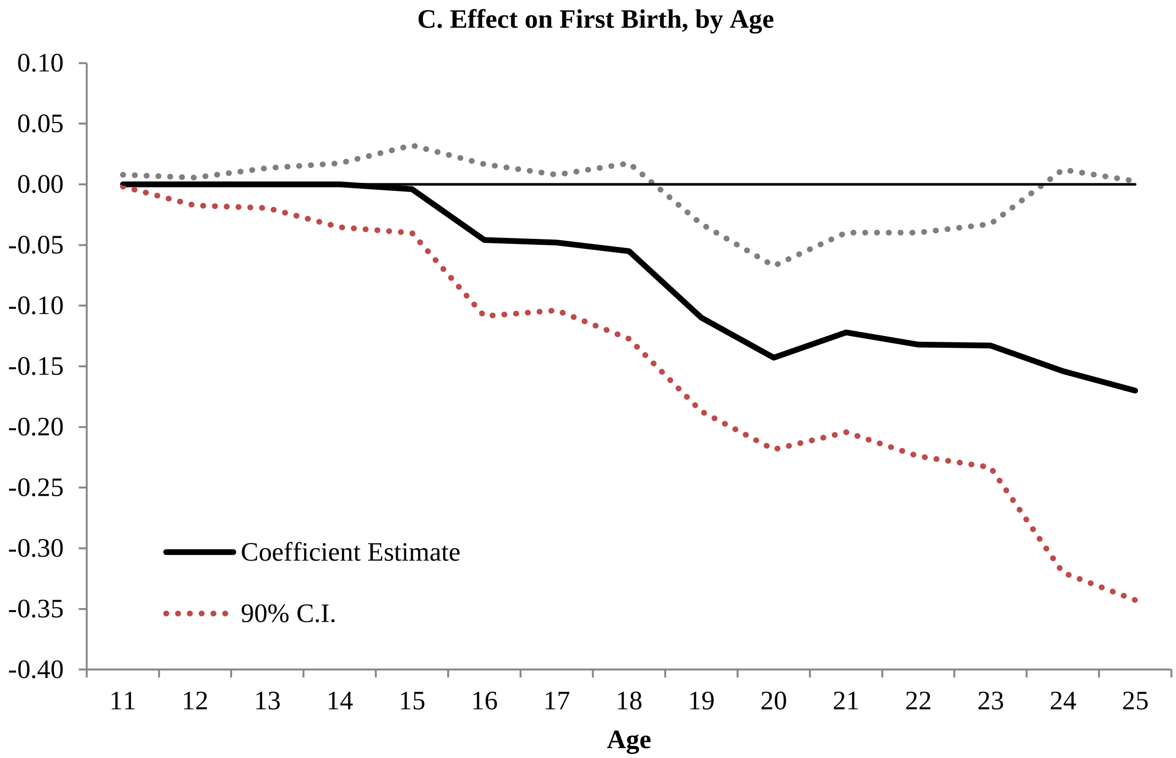
Figure A1 – 2SLS with No Transition Cohorts: Estimates for the Effect of an Additional Year of Schooling

A. Effect on First Intercourse, by Age



B. Effect on First Marriage, by Age




C. Effect on First Birth, by Age
0.10
0.05
0.00
-0.05
-0.10
-0.15
-0.20
-0.25
-0.30
-0.35
-0.40
Coefficient Estimate
90% C.I.
11 12 13 14 15 16 17 18 19 20 21 22 23 24 25
Age

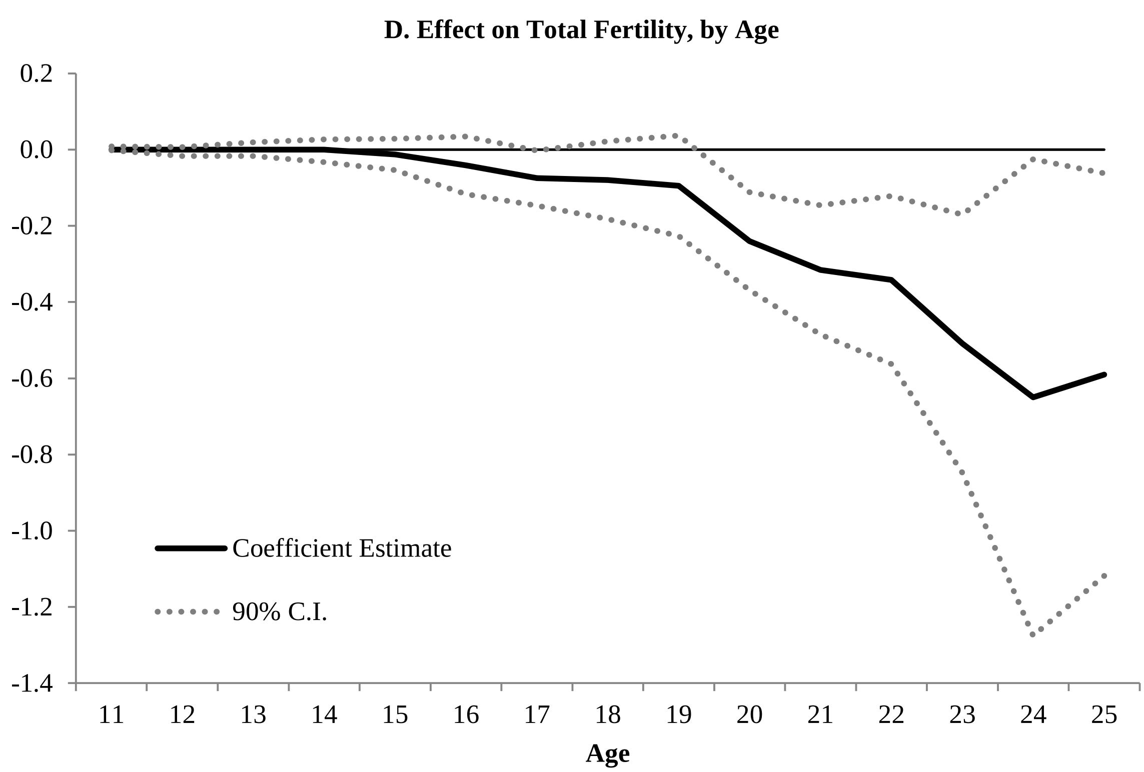

D. Effect on Total Fertility, by Age
0.2
0.0
-0.2
-0.4
-0.6
-0.8
-1.0
-1.2
-1.4
Coefficient Estimate
90% C.I.
11 12 13 14 15 16 17 18 19 20 21 22 23 24 25
Age

Table A1 – Reduced-form, Only Women with Age > 25: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

	15	16	17	18	19	20	21	22	23	24	25
A. First Intercourse by Age:											
$Inst_{cq}$	0.036	-0.020	-0.078**	-0.068**	-0.065**	-0.040	-0.050**	-0.042**	-0.040**	-0.030**	-0.018
	(0.027)	(0.030)	(0.031)	(0.034)	(0.030)	(0.025)	(0.021)	(0.019)	(0.017)	(0.014)	(0.012)
N	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105
B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.011	-0.033	-0.040	-0.071**	-0.067**	-0.067**	-0.093***	-0.057*	-0.044*	-0.051**	-0.039*
	(0.024)	(0.029)	(0.027)	(0.032)	(0.033)	(0.031)	(0.029)	(0.029)	(0.026)	(0.023)	(0.020)
N	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105
C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.010	-0.016	-0.032	-0.036	-0.075**	-0.116***	-0.106***	-0.104***	-0.084***	-0.069**	-0.062***
	(0.020)	(0.029)	(0.030)	(0.032)	(0.031)	(0.030)	(0.030)	(0.032)	(0.028)	(0.029)	(0.021)
N	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105
D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.009	-0.003	-0.038	-0.031	-0.045	-0.142*	-0.188**	-0.217**	-0.280***	-0.309***	-0.361***
	(0.025)	(0.034)	(0.042)	(0.053)	(0.058)	(0.073)	(0.084)	(0.089)	(0.096)	(0.102)	(0.097)
N	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105	11,105

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age of 25. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A2 – Reduced-form, Born After 1954 with Grade One Enrollment Control: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.034	-0.012	-0.003	-0.017	-0.000	0.012	-0.001	-0.006	-0.021	-0.022	-0.038**
	(0.022)	(0.029)	(0.033)	(0.040)	(0.036)	(0.031)	(0.026)	(0.023)	(0.019)	(0.018)	(0.015)
N	18,236	17,693	16,997	16,096	15,234	14,323	13,651	12,842	11,875	10,940	10,091

B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.013	-0.025	-0.028	-0.079***	-0.090***	-0.085***	-0.073**	-0.007	-0.026	-0.030	-0.037
	(0.019)	(0.025)	(0.025)	(0.029)	(0.026)	(0.030)	(0.029)	(0.033)	(0.030)	(0.028)	(0.026)
N	18,236	17,693	16,997	16,096	15,234	14,323	13,651	12,842	11,875	10,940	10,091

C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.004	0.016	-0.001	-0.036	-0.035	-0.070**	-0.075**	-0.052	-0.061*	-0.066*	-0.063**
	(0.021)	(0.026)	(0.027)	(0.035)	(0.033)	(0.032)	(0.037)	(0.042)	(0.036)	(0.039)	(0.029)
N	18,236	17,693	16,997	16,096	15,234	14,323	13,651	12,842	11,875	10,940	10,091

D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	-0.007	0.027	0.005	-0.045	-0.024	-0.109	-0.209**	-0.189*	-0.255**	-0.341***	-0.374***
	(0.025)	(0.035)	(0.040)	(0.056)	(0.065)	(0.074)	(0.091)	(0.102)	(0.111)	(0.122)	(0.124)
N	18,236	17,693	16,997	16,096	15,234	14,323	13,651	12,842	11,875	10,940	10,091

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1955 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, the number of students enrolled in grade one during the respondent's age 6 year, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A3 – Reduced-form, Annual Instrument Calculated Using Enrollment Data: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Annual	0.024	-0.017	-0.036	-0.038	-0.032	0.001	-0.007	0.006	-0.013	-0.014	-0.021
$Inst_c$	(0.025)	(0.025)	(0.028)	(0.028)	(0.028)	(0.022)	(0.020)	(0.017)	(0.018)	(0.017)	(0.015)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
B. First Marriage by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Annual	0.005	-0.023	-0.029	-0.066**	-0.074***	-0.086***	-0.075***	-0.028	-0.030	-0.038***	-0.042**
$Inst_c$	(0.018)	(0.023)	(0.032)	(0.025)	(0.023)	(0.022)	(0.015)	(0.019)	(0.020)	(0.012)	(0.016)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
C. First Birth by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Annual	0.010	0.006	-0.005	-0.019	-0.046	-0.086***	-0.075***	-0.058***	-0.065***	-0.061***	-0.065***
$Inst_c$	(0.020)	(0.029)	(0.025)	(0.037)	(0.032)	(0.017)	(0.021)	(0.017)	(0.019)	(0.018)	(0.018)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105
D. Total Fertility by Age:											
	15	16	17	18	19	20	21	22	23	24	25
Annual	0.008	0.014	0.002	-0.013	-0.015	-0.104	-0.162*	-0.148**	-0.242***	-0.300***	-0.357***
$Inst_c$	(0.024)	(0.038)	(0.043)	(0.062)	(0.069)	(0.067)	(0.082)	(0.069)	(0.083)	(0.092)	(0.096)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. Annual $Inst_q$ is the probability of being treated by the 1985 reform without adjusting for quarter of birth, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth cohort level.

Table A4 – Reduced-form, Using Annual Alternative Instrument: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	0.010	-0.021	-0.031	-0.037	-0.036	-0.008	-0.028	-0.014	-0.051***	-0.049***	-0.047***
Annual Inst$_c$	(0.029)	(0.028)	(0.031)	(0.029)	(0.024)	(0.018)	(0.017)	(0.018)	(0.011)	(0.013)	(0.013)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

B. First Marriage by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	0.008	-0.019	-0.021	-0.062**	-0.059**	-0.076**	-0.070***	-0.015	-0.012	-0.035**	-0.055**
Annual Inst$_c$	(0.019)	(0.023)	(0.034)	(0.023)	(0.025)	(0.032)	(0.018)	(0.021)	(0.029)	(0.014)	(0.026)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

C. First Birth by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	0.011	-0.009	-0.014	-0.026	-0.064*	-0.090***	-0.065**	-0.065***	-0.064***	-0.073***	-0.096***
Annual Inst$_c$	(0.014)	(0.030)	(0.027)	(0.039)	(0.037)	(0.018)	(0.025)	(0.015)	(0.017)	(0.015)	(0.016)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

D. Total Fertility by Age:	15	16	17	18	19	20	21	22	23	24	25
Alternative	0.010	0.004	-0.008	-0.011	-0.029	-0.127**	-0.171**	-0.177***	-0.232***	-0.264***	-0.291***
Annual Inst$_c$	(0.019)	(0.039)	(0.039)	(0.053)	(0.072)	(0.058)	(0.081)	(0.054)	(0.070)	(0.082)	(0.092)
N	19,250	18,707	18,011	17,110	16,248	15,337	14,665	13,856	12,889	11,954	11,105

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. Alternative Annual Instrument$_q$ is calculated without using enrollment data, as outlined in the text, or the quarter of birth adjustment. It is the estimated probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth cohort level.

Table A5 – Reduced-form, Including Women with Imputed Month/Year of Birth: Age at First Intercourse/Marriage/Birth and Total Fertility, by Age

A. First Intercourse by Age:

	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.017	0.000	-0.026	-0.027	-0.014	0.008	-0.001	0.014	0.003	0.007	-0.005
	(0.021)	(0.022)	(0.022)	(0.026)	(0.023)	(0.019)	(0.016)	(0.014)	(0.013)	(0.013)	(0.012)
N	28,342	27,657	26,801	25,706	24,673	23,528	22,712	21,714	20,496	19,315	18,150

B. First Marriage by Age:

	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.005	-0.018	-0.033*	-0.061***	-0.077***	-0.079***	-0.064***	-0.014	-0.010	-0.024	-0.027*
	(0.014)	(0.017)	(0.020)	(0.019)	(0.019)	(0.019)	(0.017)	(0.018)	(0.018)	(0.016)	(0.016)
N	28,342	27,657	26,801	25,706	24,673	23,528	22,712	21,714	20,496	19,315	18,150

C. First Birth by Age:

	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.019	0.016	0.005	-0.017	-0.035	-0.063***	-0.059**	-0.044*	-0.047**	-0.035	-0.036**
	(0.014)	(0.019)	(0.020)	(0.024)	(0.025)	(0.023)	(0.023)	(0.022)	(0.019)	(0.021)	(0.017)
N	28,342	27,657	26,801	25,706	24,673	23,528	22,712	21,714	20,496	19,315	18,150

D. Total Fertility by Age:

	15	16	17	18	19	20	21	22	23	24	25
$Inst_{cq}$	0.033**	0.042	0.029	0.010	0.002	-0.065	-0.115*	-0.110	-0.177**	-0.219**	-0.288***
	(0.017)	(0.026)	(0.030)	(0.039)	(0.047)	(0.057)	(0.061)	(0.069)	(0.076)	(0.085)	(0.090)
N	28,342	27,657	26,801	25,706	24,673	23,528	22,712	21,714	20,496	19,315	18,150

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. Each estimate is from a separate regression. In panels A, B, and C the dependent variable is equal to one if the event occurred by the given age, in panel D the dependent variable is total fertility by the given age. The sample is restricted to only include observations older than the age specified in each column. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. Sample includes women in birth cohorts from 1950 to 1980. The sample includes respondents who cannot specify their birth month or year, these data are imputed for them by the DHS. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A6 – Estimates Using Alternative Trends: Age at First Intercourse/Marriage and Total Fertility

A. Qtrend				**First Intercourse by Age:**			**First Marriage by Age:**			**Total Fertility by Age:**		
	YearsSchl	**AtLeast8**	**Education8**	**15**	**18**	**25**	**15**	**18**	**25**	**15**	**18**	**25**
Linear	0.774***	0.234***	0.264***	0.022	-0.036	-0.034***	0.003	-0.059**	-0.021	0.006	-0.018	-0.264***
	(0.227)	(0.026)	(0.018)	(0.020)	(0.029)	(0.013)	(0.016)	(0.024)	(0.018)	(0.018)	(0.043)	(0.091)
Quadratic	0.749***	0.233***	0.265***	0.022	-0.034	-0.018	0.004	-0.065***	-0.039*	0.007	-0.023	-0.361***
	(0.163)	(0.025)	(0.017)	(0.020)	(0.029)	(0.012)	(0.015)	(0.024)	(0.020)	(0.018)	(0.042)	(0.097)

B. Three-part Trend				**First Intercourse by Age:**			**First Marriage by Age:**			**Total Fertility by Age:**		
	YearsSchl	**AtLeast8**	**Education8**	**15**	**18**	**25**	**15**	**18**	**25**	**15**	**18**	**25**
Linear	1.507***	0.290***	0.290***	-0.039**	-0.055***	-0.002	-0.033***	-0.099***	-0.061***	-0.025**	-0.124***	-0.510***
	(0.112)	(0.018)	(0.014)	(0.016)	(0.018)	(0.010)	(0.012)	(0.016)	(0.016)	(0.012)	(0.028)	(0.051)
Quadratic	1.371***	0.318***	0.303***	-0.026	-0.031	-0.017	-0.006	-0.088***	-0.077***	-0.010	-0.051	-0.486***
	(0.165)	(0.022)	(0.018)	(0.018)	(0.026)	(0.013)	(0.015)	(0.019)	(0.022)	(0.016)	(0.035)	(0.066)
Cubic	1.688***	0.330***	0.272***	-0.076***	-0.068**	-0.031**	-0.035*	-0.127***	-0.079***	-0.043**	-0.137***	-0.452***
	(0.183)	(0.025)	(0.024)	(0.022)	(0.033)	(0.015)	(0.019)	(0.024)	(0.025)	(0.020)	(0.038)	(0.086)

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. Each estimate is from a separate regression. The variable of interest is always the instrument, the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. The first three columns represent first stage estimates, the others are reduced-form. In panel A quarterly time trend is used, in panel B the three-part time trend is used. Sample includes women in birth cohorts from 1950 to 1980. For the reduced-form equations, the sample is restricted to only include observations older than the age specified in each column. In addition to the specified time trend, all regressions include a cubic for age, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A7 – Reduced-form Estimates of the Effect of the Reform on Modern Contraceptive Use

	Full Sample	**Less Than 5 Years of Schooling**	**Attend College**
	(1)	(2)	(3)
		A. Linear Quarterly Time Trend	
$Inst_{cq}$	0.110***	-0.004	0.192*
	(0.025)	(0.047)	(0.107)
		B. Quadratic Quarterly Time Trend	
$Inst_{cq}$	0.109***	0.048	0.162
	(0.017)	(0.051)	(0.109)
	Full Sample	**Less Than 5 Years of Schooling**	**Attend College**
		C. Quadratic Quarterly Time Trend	
Alternative	0.110***	0.107	0.032
$Inst_{cq}$	(0.022)	(0.071)	(0.131)
N	19,250	2,980	989

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. The dependent variable is equal to one if the woman reports using a modern contraceptive method, zero otherwise. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1, calculated using pre-reform enrollment data, Alternative $Inst_{cq}$ is represents the same probability but is calculated without using the cohort specific pre-reform enrollment data. The first column includes all women from 1950 to 1980 birth cohorts, the second column includes only women who dropped out before completing five years of school, and the third column only includes women who attended college. All regressions include a cubic for age, specified trend control, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A8 – Empowerment of Married Women: Using Quadratic Birth Quarter Trend

Does the respondent have some part of final say regarding:	Own Healthcare	Large Household Purchases	Daily Purchases	Visiting Family/Friends	Food Cooked Each Day
	(1)	(2)	(3)	(4)	(5)
$Inst_{cq}$	0.010	-0.042	-0.072	0.088	0.001
	(0.053)	(0.056)	(0.053)	(0.054)	(0.029)
N	4,405	4,404	4,402	4,405	4,404

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. Each estimate is from a separate regression. The dependent variable in each column is equal to one if the woman reports having any part of the final decision for the given category, equal to zero otherwise. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. The sample is restricted to only include married women, in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.

Table A9 – Empowerment of Married Women: Using Quadratic Birth Quarter Trend
Sample Restricted to Those with Between Seven Years of Primary Education and Four Years of Secondary

Does the respondent have some part of final say regarding:	Own Healthcare	Large Household Purchases	Daily Purchases	Visiting Family/Friends	Food Cooked Each Day
	(1)	(2)	(3)	(4)	(5)
$Inst_{cq}$	0.080	-0.040	0.039	0.136*	0.038
	(0.074)	(0.066)	(0.066)	(0.074)	(0.041)
N	2,856	2,856	2,855	2,856	2,855

*** p<0.01, ** p<0.05, * p<0.1. Each estimate is from a separate regression. Each estimate is from a separate regression. The dependent variable in each column is equal to one if the woman reports having any part of the final decision for the given category, equal to zero otherwise. $Inst_{cq}$ is the probability of being treated by the 1985 reform, the variable ranges from 0 to 1. The sample is restricted to only include married women who completed at least seven years of school, but no more than four years of secondary school, in birth cohorts from 1950 to 1980. All regressions include a cubic for age, a quadratic quarterly time trend, and fixed effects for quarter of birth, ethnicity, and type of childhood residence. Each regression is weighted by sampling weights from the DHS, and standard errors are clustered at the birth quarter (cohort and quarter) level.