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Heterogeneous Effects of Women's Schooling on Fertility, Literacy and Work: Evidence from Burundi's Free Primary Education Policy*

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Abstract: This article investigates women's returns to schooling by exploiting Burundi's free primary education policy (FPE) of 2005 as a natural experiment. Credibly exogenous variation in education is identified through a fuzzy regression discontinuity design (RDD). Our results show that while educational attainment was positively influenced by Burundi's FPE for women situated at all wealth levels, the relevant downstream effects of schooling – measured by fertility, literacy and employment – reveal heterogeneous treatment effects by wealth. Poorer women profit in terms of higher literacy, employment as well as reduced fertility through policy induced education, while there are almost no effects of additional education for non-poor women. Our findings help in evaluating the generalisability of the nexus between women's education and fertility as well as associated factors.

JEL Classification: I25, I26, J13, O55

Keywords: Female Education, Fertility, Sub-Saharan Africa, Regression Discontinuity Design

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I. INTRODUCTION

Teenage childbearing is a prevailing problem in many developing countries. In Sub-Saharan Africa, 104 out of 1000 women between the ages 15-19 have already given birth to at least one child (United Nations 2019). The negative consequences associated with early childbirth are outlined by increased maternal mortality, sexually transmitted infections, adverse infant and child health, as well as limited human capital and labour market outcomes (Chevalier and Viitanen 2003; Ganchimeg et al. 2013; Granja et al. 2001; Hobcraft 1993). The role of education has often been put at the forefront in helping to resolve these issues (United Nations 2015; World Bank 2017). Education is said to reduce teenage fertility by empowering women through knowledge acquisition, the familiarisation of alternative family norms and by improving women's labour market opportunities (Becker 1960, Caldwell 1980, Cochrane 1979).

Early empirical literature has consistently shown a negative association between schooling and fertility (Ainsworth, Beegle, and Nyamete 1996; Cochrane 1979; Martin 1995). However, causal interpretations of these findings are challenging (McCrary and Royer 2011). To address endogeneity concerns, a growing body of research explores the effect of education on fertility in developing countries by employing quasi-experimental methods. In Sub-Saharan Africa, this literature regularly exploits the enactment of free primary education (hereafter FPE) policies (see among others Adu-Boahen and Yamauchi 2018; Behrman 2015b; Chicoine 2020; Keats 2018; Makate and Makate 2016, 2018; Masuda and Yamauchi 2020; Moussa and Omoeva 2020). While some of these articles have shown that the educational increases of FPE policies are largely felt in the poorer groups of the population (see Deininger 2003; Hoogeveen and Rossi 2013; Keats 2018; Lucas and Mbiti 2012; Makate and Makate 2016), specific heterogeneities in the outcomes affected by schooling have only been partially explored.

We contribute to this literature in two ways. First, we add on to the existing evidence by presenting a negative effect of education on women’s adolescent fertility outcomes.¹ Second, we demonstrate that the effects of additional schooling are distinctly heterogeneous, in particular, there is a differential effect governed by women’s household wealth. We thereby expand on the previous studies by providing evidence of heterogeneities materialising in the downstream factors influenced by education.

We consider the implementation of Burundi’s free primary education (FPE) policy in 2005 as a quasi-experiment, which to our knowledge, has not been used to assess potential effects before. We employ a fuzzy regression discontinuity design (RDD) to identify exogenous increases in schooling. From there, we conduct instrumental variable (IV) estimations examining the impact of education on fertility, literacy and work. The RDD takes advantage of the fact that at the time of policy implementation, only women of primary school age or younger were able to benefit from tuition-free schooling, whereas older women were not. The data we use come from Burundi’s Demographic and Health Surveys (DHS) conducted in 2010/2011 and 2016/2017. To explore the heterogeneous effects of Burundi’s FPE policy, we make use of the DHS wealth index score.²

Our first-stage results show statistically significant and economically relevant increases in education, both for poor and for wealthy women. Being young enough to benefit from Burundi’s FPE policy (i.e. being 13 years and younger at the time of implementation) increased girls’ education by 1.22 years compared to women too old to benefit from the policy (14 years and older). This corresponds to a 34% increase compared to the mean years of schooling for untreated cohorts. Given that education was significantly increased for both the poor and the

¹ As recent literature has done, we also show schooling’s impact on associated influencing factors such as marriage, reproductive behaviour, literacy, and employment.

² We later refine the classification “poor” to households with dirt flooring, as is done in Keats (2018).

wealthy subgroups, we have the opportunity to additionally test for heterogeneity in the outcomes influenced by education.

Indeed, our IV-estimates provide evidence for heterogeneous treatment effects: an additional year of schooling reduces women's fertility, measured by desired- as well as realised fertility outcomes, for poor women only.³ The same holds for the rate of literacy which was raised only for girls from poor households. On the contrary, the likelihood of teenage marriage was reduced for both the poor and wealthy women, by 17% and 11%, respectively. To the extent that education influences employment opportunities of women, we find no effect of added education on women's likelihood of having worked in the last calendar year. However, conditional on having work, one year of added education increases the likelihood that women work for a third party (contrary to working domestically or being self-employed) and to be paid in cash (contrary to in-kind remuneration or none). Again, both effects are apparent only for women from poor households. In sum, all of our results strongly suggest that the effects of additional education are driven by women situated below the median wealth level.

To further explore the reasons for such marked differential returns to schooling, we investigate the levels of schooling at which the educational gains were realised through FPE, by analysing the shift in the cumulative distribution function (of schooling), induced by our instrument. Results reveal that poor women had significantly higher increases in early school exposure via FPE, whereas for the wealthy, increases were primarily felt at later primary- and early secondary school grades. Combined with our main results, this suggests that, educational quality held equal, once women reach some sort of baseline level of schooling, further increases

³ Keats (2018) also suggests that the decreased fertility outcomes were driven by poor women using a differences-in-differences-in-differences specification. We explore the pathways through which education affects fertility of the poor and show that education works through decreased teenage marriage, sexual activity and an increased usage of condoms.

at higher levels may not influence women's literacy rates, their desired or actual fertility, nor their employment outcomes as much as earlier schooling years.

The remainder of this paper is structured as follows: Section II discusses the related literature. Our data and the identification strategy are presented in Section III. Section IV provides empirical results and robustness tests, followed by a discussion. Concluding remarks are offered in Section V.

II. RELATED LITERATURE

This paper adds to the body of research examining the effect education for women in developing countries and relates to the literature analysing the effects of free primary education policies in Sub-Saharan Africa in particular.

The suggested pathways through which education influences fertility can be broadly categorised into two mechanisms, namely income and learning (McCrary and Royer 2011). The income mechanism rests on the theoretical advances of Gary Becker's (1960) seminal work. Bearing and nurturing children is assumed a time-intensive commodity (Willis 1973). Hence, an expansion of women's human capital will increase the earning potential and thereby the opportunity cost of having children (Becker 1965).⁴ Moreover, education may influence fertility by learning, which works through the dissemination of new information as well as the overall expansion of the capacity to access and absorb information. As such, education may directly exert an effect on women's fertility behaviour through curricula (Bankole et al. 2007); both by raising the overall stock of health knowledge as well as the (more effective) usage thereof (Grossman 1972; Rosenzweig and Schultz 1989). Education may also influence fertility decisions and health knowledge by providing women with the skills necessary, such as literacy and numeracy, to attain knowledge about family planning outside the premises of the school,

⁴ The rising costs associated with bearing children results in a shift from a higher quantity of children towards fewer, more qualitative children (Becker and Lewis 1973, Lam and Duryea 1999).

for instance by the usage of mass media outlets (Glewwe 1999; Thomas, Strauss, and Henriques 1991). And while formal education advances student's factual knowledge, schools are also an important venue for the socialisation of students, especially in developing economies (Caldwell 1976; 1980). In essence, schools expose women to knowledge on family planning and implementation as well as novel information on potentially overhauled family norms and gender roles; both of which can be reinforced by the social interaction with like-minded peers (Bongaarts and Watkins 1996). Besides these two distinct workings, it has been proposed that schooling reduces fertility simply by keeping women in school and thereby reducing the available time for unsafe sexual practices, a so called "incarceration effect" (Berthelon and Kruger 2011; Black, Devereux, and Salvanes 2008).

Early empirical literature investigating the effect of education on fertility has consistently presented a negative relationship of schooling and fertility (Ainsworth, Beegle, and Nyamete 1996; Martin 1995; Cochrane 1979). However, the causal validity of these findings, especially concerning developing countries, has been questioned (Ferrè, 2009; McCrary and Royer 2011). And given that there are many unobservable factors which influence women's schooling and fertility simultaneously, endogeneity problems are likely (Angrist and Pischke 2009).⁵

A growing body of research explores the linkage between education and fertility as well as other associated factors in developing regions by employing randomised controlled trials (see Baird et al. 2010; Dupas 2011; Duflo, Dupas and Kremer 2015) or quasi-experimental methods. Our work is related to the latter, whose identifying strategies have mainly relied on instrumental variable approaches, exploiting the exogenous increases in schooling generated by specific educational policies. These studies exploit policy changes such as the vast school construction project in Indonesia (Breierova and Duflo 2004), the removal of tuition fees in

⁵ Note that the decision to continue schooling or to engage into early childbearing is often jointly determined. This is emphasised by the practices of many Sub-Saharan African countries which call for the expulsion of pregnant women (Duflo, Dupas, and Kremer 2015).

Ethiopia (Behrman 2015b; Chicoine 2016; 2020; Moussa and Omoeva 2020), Ghana (Adu-Boahen and Yamauchi 2018), Malawi (Behrman 2015a, 2015b; Behrman, Peterman, and Palermo 2017; Makate and Makate 2016), Nigeria (Osili and Long 2008) and Uganda (Behrman 2015a, 2015b; Behrman, Peterman, and Palermo 2017; Makate and Makate 2018; Masuda and Yamauchi 2020), the extension of primary school tenure from seven to eight years in Kenya (Chicoine 2012; Ferré 2009), the shortening of primary school in Egypt (Ali and Gurmu 2018), the privatisation and increased supply of higher education in Ethiopia (Tequame and Tirivayi 2015), or the performance of students in qualifying exams for secondary education in Kenya (Ozier 2018). All of these newer findings present a robust negative effect of education on fertility in developing countries.

Where tested, a significant decrease and delay in teenage marriages is present in all findings and serves as the most plausible mediating factor for the reduced occurrences of early childbearing. Many of these studies further explore the proposed mechanisms of fertility reduction, i.e. income and learning, by testing mediating factors such as literacy, the engagement with mass-media outlets, knowledge and implementation of family planning, mating (partner's characteristics) as well as employment and remuneration. The studies are mixed in finding clear support in terms of these mechanisms.

Only a limited set of studies explores differential effects of FPE policies on schooling and especially, the outcomes influenced by schooling. For instance, analysing the effects of increased education on child mortality in Malawi, Makate and Makate (2016) find a differential effect of exposure to FPE in the first-stage discontinuity of schooling. They find that only women from poor households experienced statistically significant increases in education. Similarly, Keats (2018) asserts that the educational increases of Ugandan women linked to FPE are likely driven by individuals living in poor households (measured whether main flooring of the household consists of dirt). Since both find that educational increases were apparent only for the poor, neither of these studies is able to explore the (causal) differential effect of

education on relevant outcome variables in an IV setting. In studies where a more (causally) robust estimate was possible, results are not indicative of a profound heterogeneity along women's household wealth, or along other separating indicators for that matter (Adu-Boahen and Yamauchi 2018; Makate and Makate 2018).

III. INSTITUTIONAL BACKGROUND, EMPIRICAL STRATEGY, AND DATA

To establish credibly causal effects of education, we draw upon the recent body of research and exploit Burundi's free primary education policy (FPE) of 2005 as a natural experiment. We identify exogenous (policy induced) variation in schooling by employing a fuzzy regression discontinuity design (RDD). The RDD takes advantage of the fact that the policy intervention at the national level can be treated as an event which exogenously sorts women into treatment or control (Lee and Lemieux 2010). In the context of FPE policies, the allocation to treatment is solely dependent on women's age at the time of implementation. As a result, differences in (fertility, literacy, etc.) outcomes between these two groups can be attributed to the exposure to the treatment, i.e. to an individual's ability to access free primary schooling.

Institutional Background

The World Declaration On Education For All in 1990 and the Dakar Framework For Action in 2000 manifested the promise of developing countries to achieve universal primary education (UPE) by the year 2015. Prompted by these commitments, many countries in Sub-Saharan Africa enacted policy changes towards free primary education at the turn of the century, mainly by focusing on the elimination of user fees (Kattan 2006). User fees are repeatedly stated as the largest obstacles of school attendance in developing countries, especially for the poor, who constitute the lowest educated strata of the population to begin with (Bold, Kimenyi, Sandefur 2013; Deininger 2003; Grogan 2008). Experiences of the early movers of FPE policies, such as Ethiopia, Ghana, Malawi and Uganda in the early- to mid-1990s, inspired other countries in removing direct school fees in later years (World Bank 2009). And indeed, countries in Sub-Saharan Africa have made tremendous progress in increasing educational enrolment within the

last decades, improving the net enrolment rate (NER) from 53% to 79% between 1990 and 2018 (UIS 2019). Yet, many countries in Sub-Saharan Africa are still facing several obstacles in moving towards universal primary education: large within-country differences in educational attainment prevail and are triggered by discrepancies between individuals' wealth, ethnicity, place of residence, and gender (Levine et al. 2009; United Nations 2015; World Bank 2017).

Burundi is one of the poorest countries in the world with an income per capita of \$783 and over 65% of the population living below the national poverty line (World Bank 2019).⁶ The country is densely populated and most of its 11.2 million inhabitants (87%) live in rural areas (Dunlop and King 2019). Burundi's fertility rate and population growth stand at 5.4% and 3.1%, respectively, well above the Sub-Saharan African average of 4.8% and 2.7% (United Nations 2019). Since its independence in 1962, Burundi has undergone several periods of violence usually related to the ethnic conflict between the Hutu and the Tutsi. The most noteworthy conflict was the genocidal massacre of 1993 which triggered the subsequent civil war lasting, at least formally, until the year 2000, in which the Arusha Peace and Reconciliation Agreement (APRA) was signed.⁷

Access to education was part of the struggle citizens fought for in the war and was a core element of APRA as well as the constitution adopted in 2005, when the direct fees of primary schooling were removed (Dunlop and King 2019).⁸ The waiver of school fees was

⁶ The figure represents the GDP per capita in current international dollars, adjusted for purchasing power parity.

⁷ Scattered conflicts continued to persist until at least 2006 (Bundervoet, Verwimp, and Akresh 2009; Travaglianti 2017). The massacres (1993-1994) and the subsequent civil war (1995-1998) were the most conflict intense periods (Bundervoet, Verwimp, and Akresh 2009; Verwimp and Van Bavel 2014). The years after were outlined by much less casualties from an average of 112 (by locality) between 1993-1998 down to 19 between 1999-2007 (Mercier, Ngenzebuke, and Verwimp 2020).

⁸ Travaglianti (2017) argues that free schooling was a major determining factor in Nkurunziza's re-election in 2010, evincing how important universal access to education was to citizens. Note that as was the case in many other countries implementing FPE, while the direct fees associated with schooling were removed, other costs of schooling such as uniforms, materials and contributions to the school's budget continued to

accompanied by a large surge in the construction of schools and an overall increase in budgetary spending on primary education, especially at the primary level, which rose from 37% in 2001 to 54% in 2008 of the total educational budget (Travaglianti 2017). Primary school enrolment increased from .97 million students in 2004 to 1.32 million in 2006, expanding the gross enrolment ratios from 76% to 103% in just two years (UIS 2019).⁹ School participation continuously increased thereafter, arriving at a maximum gross enrolment ratio of 141% in 2011.

Identification

In Burundi, the full primary school cycle entails six years of schooling to be attained during the official primary school ages 7 to 12 (MPBGP et al. 2017). However, as is the case in many developing countries, overage enrolment (as evinced by late entry to primary grade one) is common. The Multiple Cluster Indicator Survey (MICS) of 2005 provides us with a good source of enrolment trends at the time of the policy implementation: Girls' mean age of entry into the first grade was 8.3. Thus, women would begin their last year of primary school at the age of 13 rather than 12. We account for this de facto range of primary school years in our estimations (see also Adu-Boahen and Yamauchi 2018; Keats 2018).

Fees were removed effective from the school year starting September 2005. Hence, women who were still of primary school age during this time, i.e. born in 1992 or later, were able to benefit from free schooling. As in Behrman (2015a, 2015b, 2017), Keats (2018), Makate and Makate (2018) and Moussa and Omoeva (2020), the RDD design in this setting compares women who were “just-treated”, i.e. women who were just young enough to benefit from the removal of fees (age 13 at policy implementation) to women who were just too old to benefit

persist. The direct fees amounted to approx. 1,500 Burundian Francs out of approx. 10,000 total schooling costs (Travaglianti 2017 and sources therein). Despite being only a small fraction of the total costs, removing direct schooling fees had a “psychological effect” (Travaglianti 2017, 112) on people, making it seem more affordable than before.

⁹ Gross enrolment ratios of over 100% signal the occurrence of under- and overage enrolment, which is not adjusted in this measure. Over-age enrolment is prevalent in developing countries.

from the policy change (age 14).¹⁰ As addressed above, late entry to school, long absenteeism as well as grade repetition are a common occurrence in Sub-Saharan Africa, especially in Burundi. Plausibly, women even older than the de facto primary school age were exposed to the policy change, by enrolling to lower primary school grades at later ages or by continuing with their remaining primary school years (Behrman 2015b). On the other hand, some girls younger than the de facto primary school ages have already left schooling permanently, be it for marriage, for work or for similar duties and will thereby not benefit from the removal of tuition fees. Given such noncompliance, estimation will be made through a fuzzy regression discontinuity design.¹¹

The effect estimated in a fuzzy RDD resembles an instrumental variable (IV-) estimate: The effect is defined by the ratio of the reduced form estimate of the discontinuity in the outcome variable Y_i to the reduced form estimate of the discontinuity in the treatment D_i (Schooling) both determined by the assignment variable B_i (year of birth) and evaluated at the cut-off c (Hahn, Todd, and Klaauw 2001; Imbens and Lemieux 2008; Lee and Lemieux 2010). The cut-off value in our setting is 1992, i.e. women born in 1992 were just-treated by the policy, and women born thereafter could benefit from free primary schooling progressively longer.

Estimation therefore consists of two steps: a first-stage (1), regressing the treatment D_i on the identifying instrument Z_i , whereby D_i is a continuous measure for years of schooling and Z_i is the binary indicator $Z_i = 1[\textit{Year of Birth} \geq c(1992)]$. A second-stage (2), regressing the outcome variable Y_i on the instrumented endogenous variable, $\widehat{\textit{Schooling}}_i$, estimated in the first stage.

Recent literature in regression discontinuity favours a non-parametric, or local, estimation with lower order polynomials over a global specification with higher-order

¹⁰ Variations of the fuzzy RDD employed in the context of FPE are used by Adu-Boahen and Yamauchi (2018), Ali and Gurmu (2018), Ferrè (2009), Makate and Makate (2016) or Masuda and Yamauchi (2020).

¹¹ In 2006, there were approximately 24% over-aged children enrolled in primary school grades (UIS 2019).

polynomials (Gelman and Imbens 2019).¹² Thus, an appropriate window of data (hereafter bandwidth) has to be selected for both sides of the cut-off. Following the literature, we choose the bandwidth which minimises the modified “leave-one-out” cross-validation function (CV) introduced by Ludwig and Miller (2005).¹³ The modified CV function compiles mean squared prediction errors (MSE) of estimations at the boundary. Hence, we regress women’s schooling on their year of birth separately for bandwidth sizes ranging from 2 to 20 birth year cohorts on each side of the cut-off. The value of the CV function (MSE) stabilises at a bandwidth size of five birth year cohorts on either side of the cut-off.¹⁴ Thus, all our estimates are based on a bandwidth size of five, comparing women born in 1987-1991 to women born in 1992-1996 (aged 9-13 and 14-18 at the time of the policy). Importantly, both our first- and second-stage results are robust to varying the bandwidth size in either direction.

To fit an appropriate functional form to the bandwidth selected, we test six different specifications of the regression equation. As such, we estimate linear, quadratic and cubic specifications, each with and without an interaction term between the treatment indicator Z_i and the assignment variable B_i (allowing the slope to vary before and after the cut-off). We use the specification with the lowest Akaike Information Criterion (AIC), which turns out to be the linear specification with a varying slope before and after the cut-off. Thus, all of the results presented in this paper are estimated through the following two-stage regression specification:

$$Schooling_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 (B_i - 1992) + \alpha_3 Z_i * (B_i - 1992) + \alpha_k X_k + e_i \quad (1)$$

$$Y_i = \beta_0 + \beta_{1RD} \widehat{Schooling}_i + \beta_2 (B_i - 1992) + \beta_3 Z_i * (B_i - 1992) + \beta_k X_k + u_i \quad (2)$$

¹² Contrary to a global RDD, a local regression discontinuity design chooses an optimal data range to which a regression specification is fit, rather than choosing a (higher-order) polynomial regression specification to fit the entire dataset (Jacob and Zhu 2012). As Lee and Lemieux (2010) note, in practice, there is no need for a sharp distinction between local and global RDD; in practice, one can equate a global RDD to a local RDD with a large bandwidth.

¹³ To countercheck, we also test the Imbens and Kalyanaraman (2012) optimal bandwidth choice. We average all IK bandwidths chosen over all of our independent variables. This too, gives us an optimal bandwidth of five years before and after the cut-off.

¹⁴ The CV also confirms the bandwidth size of five within the subsamples of poor and wealthy women.

All variables are defined as above, X_k is an additional vector of control variables and e_i, u_i are the idiosyncratic error terms. β_{1RD} is the causal effect of schooling and represents the local average treatment effect (LATE) on compliers (Angrist, Imbens, and Rubin 1996; Lee and Lemieux 2010). As is standard in the RDD literature, the assignment variable B_i is centred at the cut-off point $c = 1992$, so that the constituent terms of the interaction, namely α_1 and β_{1RD} , can be directly interpreted as the effect at the discontinuity.¹⁵ Our first- and second-stage results are robust to varying the functional form to higher order degrees of the polynomial.

Data and Descriptive Statistics

The data used for this research come from the Demographic and Health Surveys (DHS). DHS surveys are nationally representative household-based surveys that collect information on demographics, educational achievement, employment and occupation, as well as knowledge on healthcare and family planning (Croft, Marshall, and Allen 2018). The main respondents are women of reproductive age (15-49).

Our sample consists of the 2010/2011 and the 2016/2017 Burundi DHS Female Surveys. As a result of regression specification (bandwidth), the sample is restricted to women born five years before and after the cut-off year 1992. Therefore, the birth year cohorts of the women in the main sample range from 1987 to 1996. This results in the overall sample size of 7,714 comprised of women aged 20-30.¹⁶ Following the recommendation by the DHS, we weigh all analysis with the included survey weights, making the outputs representative at the national level, and account for the pooling of datasets for by re-weighting the survey rounds.

¹⁵ Binary outcome variables are estimated through a linear-probability model (see also Adu-Boahen and Yamauchi 2018; Behrman, Peterman, and Palermo 2017; Dupas 2011; Keats 2018; Ozier 2018).

¹⁶ We exclude women younger than 20 in order to assess women's full teenage fertility and to analyse outcomes for women who have mostly completed schooling (following Ferrè 2009; Keats 2018). The household members survey set of the 2010/2011 and 2016/2017 Burundi DHS show that only 24% of 20-year old women are still attending school. Of them, 88% are enrolled in secondary school.

To explore heterogeneities in the effect of education, we employ the DHS wealth index score which is constructed by assessing households' living arrangements, such as the construction materials and sanitation facilities, as well as household assets such as radio and TV, bicycles, cars etc. and by placing them on a relative scale of wealth within the sample (Rutstein 2008). We consider respondents from households scoring on and below the median wealth score are considered "poor", and respondents with a denoted household wealth level above the median as "wealthy".¹⁷ Table A1 in the Appendix reports summary statistics for the main sample as well as for the poor and wealthy subgroups.

a) Dependent Variables

To assess adolescent fertility outcomes, we employ women's reported age at first birth as well as a dichotomous indicator of having had a first birth before the age of 20. Subsequently, we investigate the incidence of teenage marriage by estimating the probability of having been married before the age of 20.

To explore the pathways through which education is suggested to influence fertility, we investigate respondents' levels of literacy, their desired number of children, as well as their knowledge on family planning and their engagement with mass-media outlets as dependent variables. These serve as proxies for the pathway "learning". Literacy is assessed by the respondent's capability of reading out a written sentence displayed on a card shown to the interviewee. This measure also serves as an exploration into the effectiveness of FPE policies at large, as an additional quantity of education does not necessarily lead to improved learning outcomes.¹⁸ We also assess women's labour market outcomes, and thereby the pathway "income", by using information on whether the respondent has worked in the last calendar year,

¹⁷ We provide a robustness checks by recategorizing women as "poor" if the main flooring of their dwelling is sand, dung or dirt, as in Keats (2018).

¹⁸ It is reported that abolishing school fees led to overcrowding of schools and bad learning environments for children (Bold, Kimenyi, Sandefur 2013; Deininger 2003; Lucas and Mbiti 2012).

and, conditional on working, if the woman is engaged in employed work (contrary to working domestically/in the household or being self-employed) and what kind of remuneration she receives for her work (i.e. receiving cash for her employment rather than in kind compensation or none at all).

We test further factors associated with the proposed mechanisms. This includes women's reproductive behaviour, measured by respondents' age at first sexual intercourse, as well as one's knowledge and usage of condoms as a contraceptive method.

b) Main Independent Variables

Our main explanatory variable of interest is women's years of schooling (education). DHS surveys report the number of completed years of schooling, which is a continuous measurement of educational attainment. The variable is constructed by asking respondents: "what is the highest level of school you attended?" and subsequently, "what is the highest (standard/form/year) you completed at that level?" (The DHS Program 2015, 2).¹⁹ To investigate credibly causal effects of education, years of schooling serves as the dependent variable from which the exogenous part of education is constructed in the first-stage (1) of our IV regressions.

We also include a set of control variables which were plausibly fixed before the FPE policy change. We add dummy variables capturing women's religious affiliation, as it might influence fertility behaviour through doctrines and values stipulating certain roles of women within the family, and especially fertility behaviour (Makate and Makate 2018). We also add a control variable indicating the respondent's number of siblings. A larger number of siblings, and as such, a larger family size, could negatively influence girls in their ability to receive schooling (Ewemooje, Biney, and Amoateng 2020). It could also have a non-negligible impact

¹⁹ For example, if the respondent's highest level of schooling is secondary education and she completed 2 years at that level, then the variable is constructed by adding all primary school grades (6 in Burundi) + 2 years of secondary school = 8 years of total schooling.

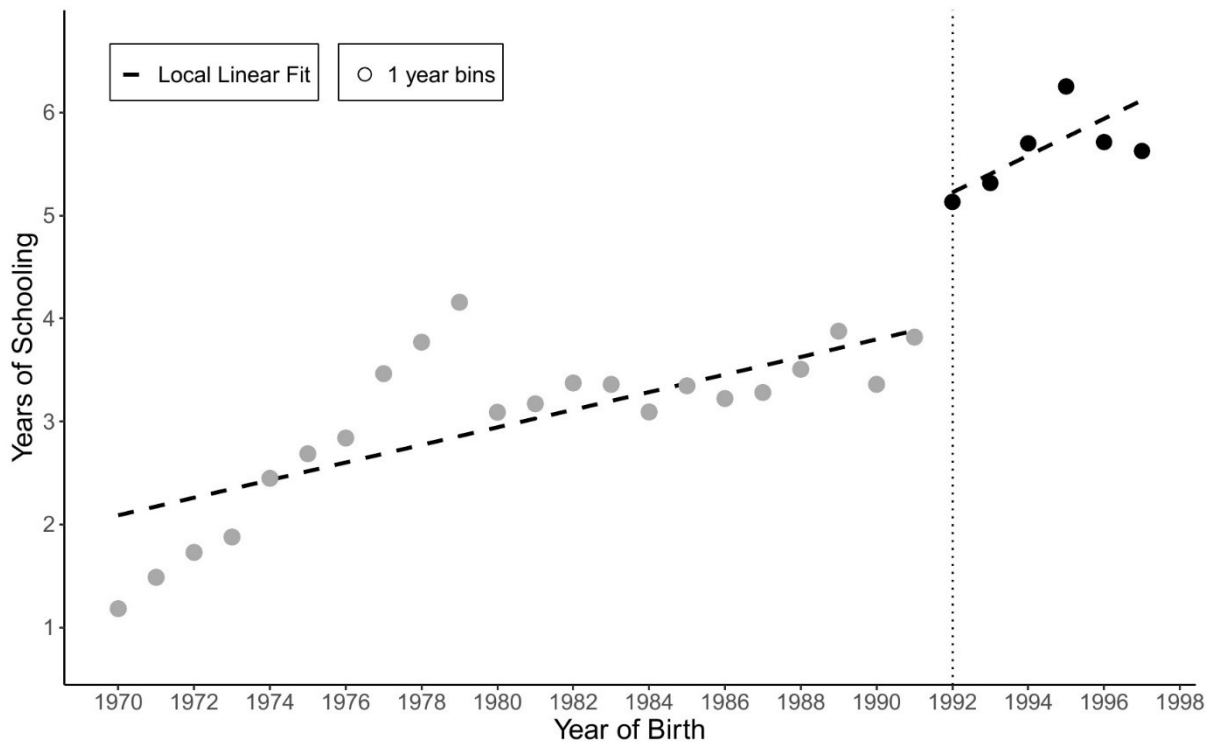
on girls' own desired family size. To account for regional discrepancies in schooling, we include province fixed effects for Burundi's eighteen provinces.²⁰ We also add a dummy variable indicating whether the respondent resides in rural or urban dwellings. Lastly, we add survey fixed effects, to account for differences in women's educational attainment or in their fertility outcomes which might vary systematically between the two periods of the survey rounds.

IV. RESULTS

First-stage Discontinuity in Schooling

Figures 1 and 2 motivate the assumption that Burundi's FPE substantially affected women's educational attainment. They depict the long-term trends in education for women by birth year using the pooled sample (Figure 1) and the two sub-samples, split by household wealth (Figure 2).

Figure 1: Women's Schooling by Birth Year Cohort



Notes: Data from the 2010/2011 and the 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights.

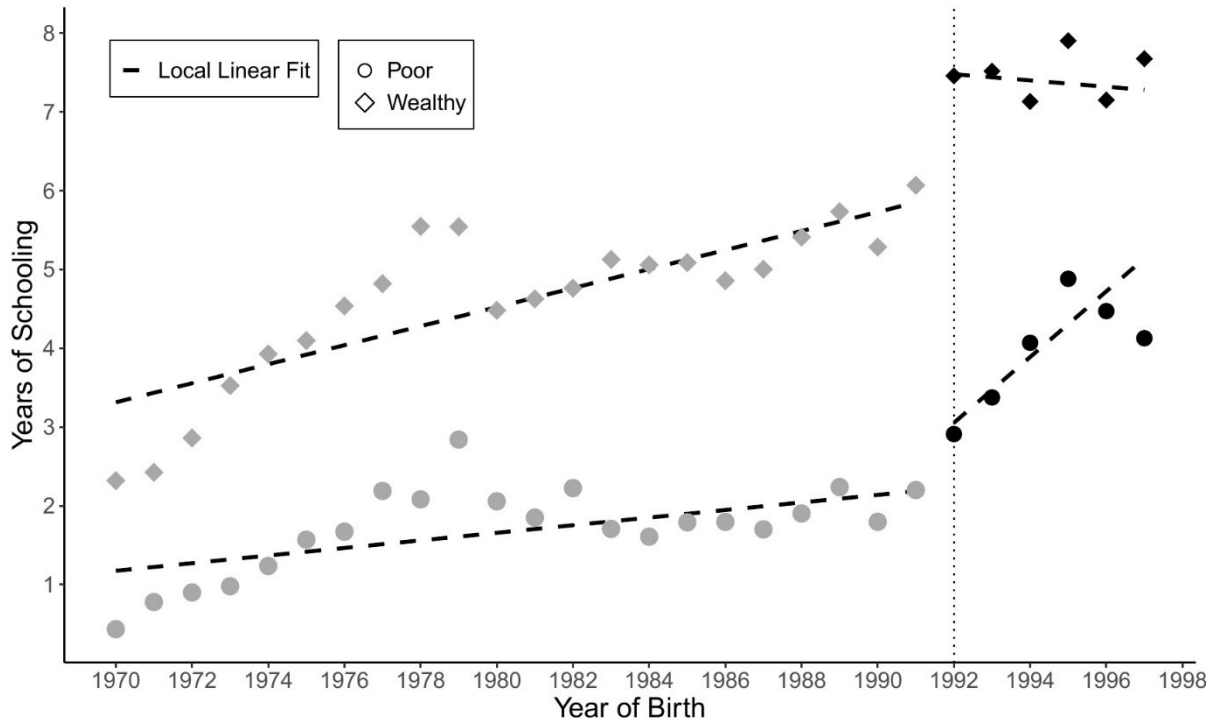
²⁰ We use the geo-coded sample clusters of the 2010/2011 survey round to allocate households across Burundi's 18 provinces, so as to "match" them with province information extant in the 2016/2017 survey round.

Figure 1 shows that women who were able to benefit from free primary schooling, i.e. women born in or after 1992, show for an immediate and substantial increase in educational outcomes compared to the cohorts born prior. And given that girls born in later years had increasingly more (primary) schooling years left to attend without having to pay fees, educational attainment rises with increasing birth year cohorts after the cut-off.

Figure 2 strongly suggests that both poor and wealthy women were able to benefit from the introduction of FPE. However, the effect of increasing exposure to schooling after the cut-off is dissimilar and can only be observed for poor women. Taking account of the very low levels of schooling of poor women born before the cut-off, it seems likely that mainly girls from poorer households were constrained by the direct cost of schooling prior to FPE. A reason for the shifted, but non-increasing trend line for wealthy women after the introduction of the FPE policy might be that the policy induced precisely the women from wealthy households who had achieved some primary education, but were constrained to complete the full six-year cycle to now finish primary schooling.²¹

²¹ We explore this explanation more thoroughly in the discussion of section IV.

Figure 2: Women’s Schooling by Birth Year Cohort separated at the median Household Wealth Level



Notes: Data from the 2010/2011 and the 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. Figures for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Table 1 presents first-stage discontinuity as specified in regression equation (1). The results are presented for the full sample (columns 1 and 2), as well as the poor (columns 3 and 4) and wealthy subsamples (columns 5 and 6), separately.²² For the discussion of results, we refer to the more stringent specification including all controls and fixed effects.²³

²² The regressions can be visually inspected in Figure A1 a) through c) (without controls and fixed effects).

²³ It is noteworthy that the coefficient estimates remain highly similar when including controls.

Table 1: Regression Discontinuity Estimate, Years of Schooling

	Years of Schooling					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
1[Year of Birth \geq 1992]	1.344*** (0.255)	1.223*** (0.223)	0.849*** (0.228)	0.908*** (0.220)	1.356*** (0.397)	1.280*** (0.367)
Mean Ctrl. Group	[3.55]		[1.99]		[5.56]	
Interaction: 1*Year of Birth	0.135 (0.090)	0.164** (0.077)	0.378*** (0.081)	0.339*** (0.078)	-0.179 (0.141)	-0.099 (0.130)
Year of Birth	0.052 (0.054)	0.038 (0.047)	0.059 (0.046)	0.064 (0.044)	0.116 (0.090)	0.070 (0.085)
Intercept	3.532*** (0.230)	6.288*** (0.442)	2.110*** (0.194)	2.804*** (0.779)	5.609*** (0.352)	7.258*** (0.622)
Effect on the second cohort treated by FPE						
1[YOB \geq 1992] + 1*YOB + YOB		1.426*** (0.197)		1.312*** (0.187)		1.250*** (0.338)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	0.054	0.263	0.099	0.183	0.041	0.187

Notes: The sample consists of the 2010/2011 and 2016/2017 Burundi DHS Female Surveys, using a bandwidth size of five birth year cohorts on either side of the cut-off. Respondents are restricted to age 20 and older at the time of the survey. The variable 'Year of Birth' has been centred at the cut-off. 'Mean Ctrl. Group' indicates the mean of the dependent variable for the women in untreated birth year cohorts (the control group). Regressions are weighted using the sample weights provided by the DHS and account for the pooling of survey rounds. The results in each column are produced by a separate regression. Columns 3 and 4 are estimated using the subsample of poor women (below and including the median wealth score), columns 5 and 6 are estimated using the subsample of wealthy women (above the median wealth score). The odd columns include survey fixed effects. The even columns include region (province) fixed effects and controls on religious affiliation, number of siblings, as well as urban/rural status. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively.

The discontinuous increase in schooling generated by Burundi's FPE policy for all women is presented in row one. The coefficient estimate represents the increase in the years of education for women who were 13 years old (born in 1992) at the time of the policy, compared to women who were 14 years old (born in 1991). The estimate is economically large and statistically significant at the 1% level throughout all columns. Being just young enough to

benefit from free schooling increased women's educational attainment between 0.908 (specification 4) and 1.280 years (specification 6).²⁴

Interestingly, Burundian women from the wealthy subgroup had a larger increase in schooling at the cut-off than the poor. However, as can be depicted in Figure 2, poor women born after 1992 had continuously increasing exposure to (free) schooling which is captured by the large positive effect of the interaction term “1[Year of Birth \geq 1992] * Year of Birth”. Hence, when comparing the effect of the second treated cohort after the cut-off (born in 1993), the poor already show for larger increases in education compared to the wealthy, $(0.908+0.339+0.064=)$ 1.312 years to $(1.280-0.099+0.070=)$ 1.250 years, respectively. Note that the mean schooling years of the control cohorts differ largely between the wealthy and the poor. Poor women not exposed to the policy had an average of 1.99 years of schooling, compared to 5.56 years for the wealthy. As such, while FPE added about the same number of schooling years for poor and rich women, it more than doubled the amount of schooling for the poor in relative terms.

Second-stage Estimates

The first-stage estimates provide evidence for substantial policy induced increases in schooling such that they present strong instruments in estimating the isolated effect of additional education on our dependent variables of interest in an IV setting. And given that both poor and wealthy women exhibit large discontinuities in schooling, we have the opportunity to additionally explore differential treatment effects of added education. We mention the use of controls, region and survey fixed effects in the first three rows of all second-stage results tables.

²⁴ In comparison, girls “just-treated” by tuition free primary schooling gained about .61 years of schooling in Ethiopia (Behrman 2015b), between .31 to .46 years in Malawi (Behrman 2015a, 2015b) and between .64 to 1.24 years in Uganda (Keats 2018; Makate and Makate 2018). Our somewhat larger estimate may be explained by the very low levels of education prior to the FPE.

Table 2: Second-stage Results, Literacy & Desired Fertility

	Sample					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Literacy (0-1)						
Schooling	0.054***	0.050***	0.067**	0.067**	0.033	0.027
Instr. by 1[YOB ≥ 1992]	(0.015)	(0.017)	(0.033)	(0.030)	(0.022)	(0.024)
Mean Ctrl. Group	[0.56]		[0.43]		[0.73]	
F-Statistic	27.7	29.9	13.8	17.1	11.4	11.9
Observations	7,708	7,704	3,856	3,855	3,852	3,849
Adj. R ²	0.424	0.430	0.418	0.442	0.336	0.310
Panel B: Desired Number of Children						
Schooling	-0.144**	-0.151**	-0.307**	-0.261**	-0.093	-0.088
Instr. by 1[YOB ≥ 1992]	(0.060)	(0.063)	(0.136)	(0.118)	(0.086)	(0.086)
Mean Ctrl. Group	[4.01]		[4.05]		[3.96]	
F-Statistic	25.9	28.3	12.5	15.8	11.4	11.9
Observations	7,520	7,516	3,764	3,763	3,756	3,753
Adj. R ²	(0.075)	(0.036)	(0.275)	(0.089)	(0.009)	0.054

Notes: The results in each panel and column are produced by a separate regression. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of the excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

a) Literacy and Desired Fertility

Table 2, panel A investigates the effect of increased schooling due to the FPE policy on women's literacy. The first-stage discontinuity delivers a strong instrument as evinced by the F-Statistic (29.9). Results show that one additional year of schooling increased women's literacy by 5.0 percentage points, which marks an increase of 9% compared to the mean of the control group. This is in line with findings from comparable studies in other developing countries, e.g. Keats (2018) or Behrman (2015a).

There is relevant heterogeneity of the effect of schooling for the poor and the wealthy as shown in columns (3) through (6). Poor women increased their rate of literacy through schooling by 16% compared to (a statistically insignificant) increase of 4% by wealthy women. Note that wealthy women have higher levels of literacy overall, 73% compared to 43% for the

poor. This suggests that for the poor, free access to schooling was successful in providing them with basic skills, presumably acquired at lower levels of schooling.

Burundi has exceptionally high rates of fertility, with an average of 5.5 births per women (World Bank 2019). Panel B of Table 2 shows that a large portion of these high rates of fertility is reflected in the number of children a woman desires to bear throughout her lifetime, with an average of 4 children per woman in our full sample. Second-stage IV-estimates suggest that education negatively influences women's desired lifetime fertility as shown in panel B of Table 2. A plausible explanation for this is that education exerts its negative impact on women's desired fertility through increased levels of literacy. This finding is supportive evidence of the pathway learning, since literacy acts as a plausible proxy for girls' learning (capacity). Our average results are comparable to recent studies: Behrman (2015b), Keats (2018) and Masuda and Yamauchi (2020) report decreases in the number of desired children between -0.11 and -0.34, well in line with our estimated coefficient of -0.151.

Different to the extant literature, we are able to show that the effect is driven mainly by women from the lower socio-economic strata of the population (columns 3 to 6). Compared to lesser educated women, treated women decreased their fertility preference by 4%, the poorer subgroup by 6% and the wealthy by (a statistically insignificant) 2%. These results suggest a convergence of fertility preference between the poor and the wealthy.

b) Reproductive Behaviour

Panel A and B of Table 3 investigate the effect of education on women's realised fertility outcomes.

Table 3: Second-stage Results, Childbearing and Marriage

	Sample					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Age at first Birth						
Schooling	0.440**	0.468**	0.435	0.449*	0.640*	0.757
Instr. by 1[YOB ≥ 1992]	(0.179)	(0.204)	(0.274)	(0.263)	(0.381)	(0.564)
Mean Ctrl. Group	[19.67]		[19.59]		[19.80]	
F-Statistic	14.9	11.9	9.1	10.3	3.9	2.1
Observations	4,918	4,917	2,830	2,830	2,088	2,087
Adj. R ²	(0.026)	0.020	0.021	0.046	(0.324)	(0.466)
Panel B: First Birth before Age 20 (0-1)						
Schooling	-0.037**	-0.034*	-0.056	-0.069*	-0.026	-0.012
Instr. by 1[YOB ≥ 1992]	(0.018)	(0.019)	(0.041)	(0.039)	(0.024)	(0.025)
Mean Ctrl. Group	[0.34]		[0.37]		[0.31]	
F-Statistic	27.9	30.0	13.8	17.0	11.7	12.2
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	0.083	0.138	0.046	0.056	0.106	0.136
Panel C: Married before Age 20 (0-1)						
Schooling	-0.063***	-0.061***	-0.078**	-0.090**	-0.059**	-0.045*
Instr. by 1[YOB ≥ 1992]	(0.018)	(0.018)	(0.039)	(0.037)	(0.025)	(0.025)
Mean Ctrl. Group	[0.48]		[0.54]		[0.41]	
F-Statistic	27.9	30.0	13.8	17.0	11.7	12.2
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	0.142	0.187	0.061	0.056	0.188	0.257
Panel D: Sex before Age 20 (0-1)						
Schooling	-0.046**	-0.044**	-0.053	-0.070*	-0.046*	-0.031
Instr. by 1[YOB ≥ 1992]	(0.018)	(0.019)	(0.038)	(0.036)	(0.027)	(0.028)
Mean Ctrl. Group	[0.54]		[0.58]		[0.49]	
F-Statistic	27.9	30.0	13.8	17.0	11.7	12.2
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	0.098	0.158	0.068	0.088	0.111	0.170

Notes: The results in each panel and column are produced by a separate regression. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of the excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

A one-year increase in schooling postpones women's age at first birth by almost half a year (panel A, column 2) and reduces the likelihood teenage childbearing by 3.4 percentage

points (panel B, column 2). We further test the probability of having had a first birth at additional age increments (from 16 to 23) in Table A4 in the Appendix and show that the effect of decreased teenage childbirth was apparent at the ages 19 and 20 which might be interpreted as evidence against an incarceration effect, given that women were most likely out of school at these age ranges.

Once more, the effect of added education seems to be driven by women from the poorer half of the population, which is shown when comparing estimates of column (4) with column (6) in panel A and B, exploring women's age at first birth and teenage childbearing.²⁵

Marriage and cohabitation are potential channels through which teenage childbearing is promoted (Presler-Marshall and Jones 2012). One year of added schooling decreases the likelihood of being married before the age of 20 by 6.1 which corresponds to a 13% decrease in comparison to the control group (panel C, column 2). As such, our results are no exception from other studies assessing the link of education and fertility in developing regions, which consistently report significant reductions in the likelihood of teenage marriage alongside decreased teenage childbearing (Keats 2018; Masuda and Yamauchi 2020). Again, we explore the effects at further age increments in Table A5. The negative effect of schooling on marriage is only significant at one age increment.²⁶

Importantly, the occurrence of teenage marriage was decreased for both the poor (panel C, column 4) and for the wealthy women (panel C, column 6), by 17% and 11%, respectively. Given that wealthy women delayed their first marriage, but not their first birth, our results suggest that a reduction in teenage marriage does not automatically lead to lower levels of (desired) fertility.

²⁵ Note that the IV-estimate for wealthy women in panel A of Table 3 suffers from weak instrument bias, with an F-Statistic of 2.1 (Staiger and Stock 1997). This is due to a sample reduction given that not all women, especially the wealthy, have born children at the time of the survey. Estimates from panel B are more reliable indicator of altered fertility behaviour via increased schooling.

²⁶ Note that we also find a significant negative effect for the wealthy subgroup before age 21. However, given the rather low F-Statistic (5.9), this observation has to be treated with caution.

To further explore the effect of education on reproductive behaviour, we investigate women's exposure to sexual intercourse. Panel D shows that an additional year of education makes women 8% less likely to report to have had their first sexual intercourse before the age of 20. Exploring estimates for all age increments shows that sexual exposure was reduced also before the age of 18 (Table A6). Given that all of our fertility indicating variables (including sexual activity, birth and marriage) were significantly reduced in later teenage years, in which women were most likely already out of school, reduced fertility through "incarceration effect" seems unlikely. Once more, the effects of increased education are heterogenous and only statistically significant for poorer women. Unaltered sexual exposure for the wealthy could explain the gap between delayed marriage but unreduced fertility.²⁷

Mechanisms

a) Learning

The reported increase in women's literacy and the accompanied decrease in their desired number of children, emphasise the potential significance of women's learning on subsequent fertility outcomes. This link is especially relevant for poorer women.

To further explore the workings of this channel, we test for women's knowledge and usage of family planning in Table A2, panel A and panel B in the Appendix. While we do not find an effect of increased awareness of modern contraceptives (panel A)²⁸, education increase the likelihood of having used a condom with the last sexual partner (panel B). Again, this effect is driven by poor women.

To assess the potential channels through which knowledge on modern family norms and contraceptive methods is dispersed, we test for women's engagement with mass media outlets

²⁷ Inasmuch as the husband-wife relationship mediates fertility outcomes, we test for the effect of education on husband's age, their educational attainment as well as the difference in husbands' and wives' desired fertility. We find no significant effects in either subgroup (results not reported but available on request).

²⁸ General knowledge on condoms as a contraceptive method is already high, 94% for untreated cohorts, such as any potential effects must be small.

such as newspapers, radio and television in Table A3. We do not find any robust indication of that more education also increased exposure to information via these sources. Overall, these findings bolster the view of (primary) schools themselves as venues of learning, socialisation and familiarisation regarding alternative conceptions of the family as well as the family planning itself (Caldwell 1976; 1980).

b) Income

We investigate the effect of additional education on indicators of work and income.

Table 4: Second-stage Results, Income

	Sample					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Worked last Year (0-1)						
Schooling	0.008	0.009	0.005	0.004	0.013	0.017
Instr. by 1[YOB ≥ 1992]	(0.010)	(0.010)	(0.020)	(0.018)	(0.016)	(0.016)
Mean Ctrl. Group	[0.071]		[0.070]		[0.073]	
F-Statistic	27.9	30.0	13.8	17.0	11.7	12.2
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	(0.012)	0.017	(0.003)	0.038	(0.033)	(0.030)
Panel B: Works for Third Party (0-1)						
Schooling	0.051***	0.054***	0.099**	0.097**	0.027	0.029
Instr. by 1[YOB ≥ 1992]	(0.018)	(0.019)	(0.044)	(0.040)	(0.019)	(0.020)
Mean Ctrl. Group	[0.134]		[0.158]		[0.098]	
F-Statistic	33.2	33.2	15.7	17.6	14.2	13.4
Observations	6,408	6,404	3,527	3,526	2,881	2,878
Adj. R ²	(0.339)	(0.282)	(0.752)	(0.563)	(0.074)	(0.006)
Panel C: Is Paid in Cash (0-1)						
Schooling	0.025	0.026*	0.055*	0.056*	-0.002	0.003
Instr. by 1[YOB ≥ 1992]	(0.015)	(0.015)	(0.032)	(0.030)	(0.023)	(0.020)
Mean Ctrl. Group	[0.143]		[0.120]		[0.178]	
F-Statistic	33.2	33.2	15.7	17.6	14.2	13.4
Observations	6,408	6,404	3,527	3,526	2,881	2,878
Adj. R ²	(0.026)	0.065	(0.272)	(0.198)	0.010	0.239

Notes: The results in each panel and column are produced by a separate regression. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of this excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Recent research reports inconclusive evidence concerning the support for the income mechanism (see Makate and Makate 2018; Masuda and Yamauchi 2020; Moussa and Omoeva 2020). Only Chicoine (2016, 2020) and Keats (2018) find an increase in (employed) work opportunities, a shift to higher skilled employment (rather than agricultural work or the like) and better remuneration.

Table 4 suggests that an added year of education does not increase the likelihood of having worked (during the last calendar year) for Burundian women. However, conditional on having work, increased education improves the likelihood that women are employed by a third party by 5.4 percentage points, rather than working in the household or being self-employed. Further, more education leads to an increased likelihood to be paid in cash, compared to in-kind compensation or receiving no remuneration at all. The now common heterogeneity regarding poor and rich women emerges again: Both statistically significant effects via increased education stem from the poor. Hence, together with our earlier estimates of schooling on women's fertility outcomes, these findings also lend support to the income pathway: more educated (poor) women show for increased opportunity costs of childbearing, as evinced by their increased ability in finding paid, employed work compared to their less educated counterparts. These higher opportunity costs are potentially reflected in (poor) women's updated fertility preferences which are adjusted accordingly. Note, however, that our IV-estimates cannot entangle whether better labour market opportunities, due to more education, lead to lower fertility (preferences) or vice versa (as also remarked by Keats 2018).

Discussion and Caveats

Our estimates confirm the negative causal effect of schooling on teenage fertility, and reproductive behaviour, found in other developing regions. In addition, increased schooling positively influenced associated factors such as women's literacy, usage of contraception as well as employment opportunities. While we observe that both poor and wealthy women gain additional years of schooling due Burundi's FPE policy, the subsequent outcomes affected via

education seem to be mainly driven by behavioural changes of the poor. This important heterogeneity is comparatively novel to the literature.

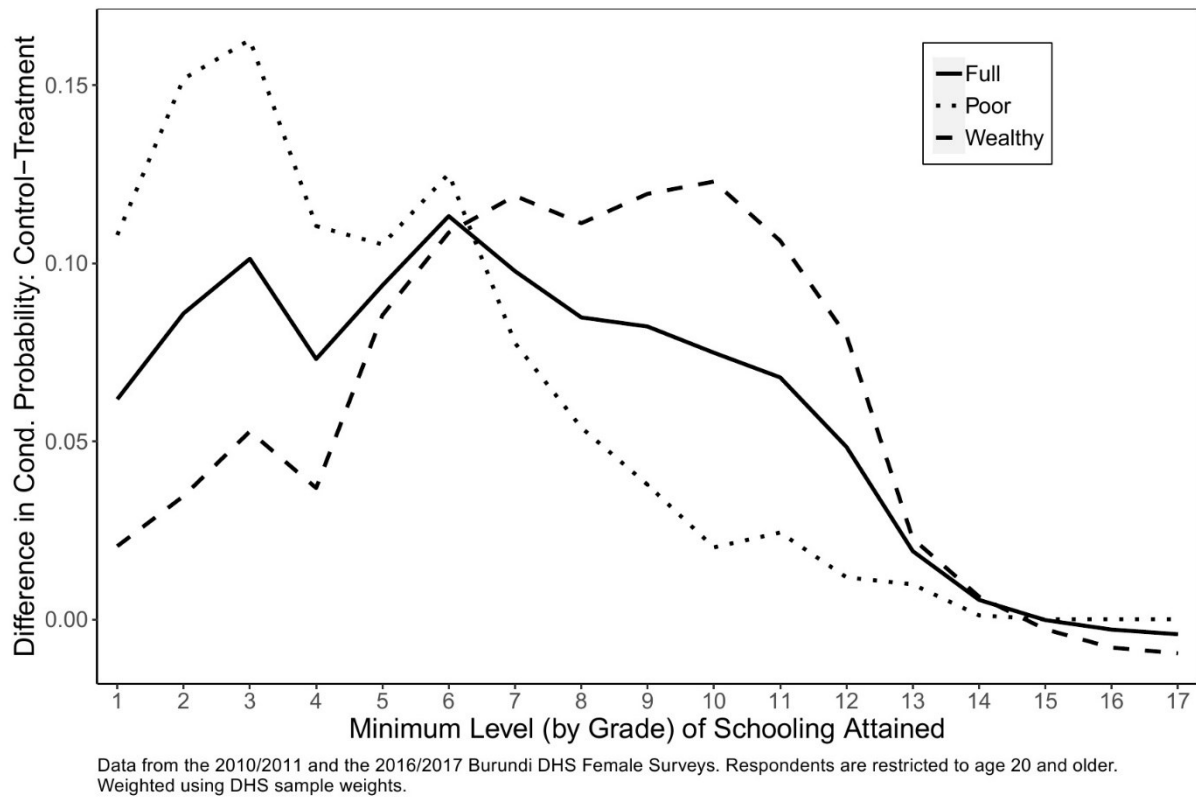
Grouping individuals by wealth is not without caveats which we want to discuss: There is the possibility of women increasing their (household) wealth via added schooling, through improved employment opportunities or the like. However, 90% of women in our sample are still dependents (not the household head of their family) and only 51% of women in the sample are the wife to the household head. Hence, even if women did increase their earning potential, we can plausibly assume that this is not reflected in their current household wealth status, at least not yet (see also Keats 2018). More importantly, if women's additional schooling did allow them to move into the wealthier subgroup of the population, the coefficients we report would be biased downwards, and as such, would represent a conservative estimate of the true effect of schooling on the poor, given that the effects of the wealthy would be inflated by these movers.

To test for the robustness of our heterogeneous effects and in particular, their sensitivity to differing wealth groupings, we follow Keats (2018) and re-categorise individuals in our sample as "poor" when living in houses where the main flooring consists of sand, dung or dirt. Results are given in Table A7. These estimates corroborate the heterogeneous nature of our results.

Another salient discrepancy between the poor and rich is their educational exposure altogether. To investigate this difference more profoundly, we explore the levels at which the gains of education were realised through FPE for our full sample, and for the poor and wealthy subgroups, separately. Given that our IV-estimates resemble local average treatment effects (LATE), this exercise informs us about the sub-population of "compliers" driving our results (see Angrist and Pischke 2009). Figure 3 depicts these shifts visually, plotting the difference in the conditional probability (on the y-axis) of having completed at least a given school grade (on the x-axis) for women that were just able to benefit from the policy change (13 years and

younger), compared to women who too old to benefit from the removal of fees (14 years and older).²⁹

Figure 3: Shifts in Grade Level Attained



The graphs show that for the poor, schooling increases induced by the instrument were highest at the (early) primary school level, and increases for the wealthy were highest at later primary- to secondary school levels: being born in or after 1992 increased poor women’s probability of having attained at least some primary school years (between 1-3 years) between 10.8 and 16.3 percentage points, contrary to increases between 2 and 5.3 percentage points for the wealthy. Increases for wealthier women catch up to the level of poor women’s increases while moving rightwards along the x-axis and are higher at secondary school grades. This

²⁹ Note that these changes are conditional CDF changes, including the usual covariates and weighing with the sample weights. Further, the changes of the three different samples are normalised by their respective first-stages as outlined in Angrist and Pischke (2009), giving us the contribution (weight) of the respective schooling level change towards the average causal response over all educational levels.

indicates that removing primary school fees induced wealthier women to also transition into secondary school.

Overall, the exercise shows that treatment groups across the poor and wealthy subsamples differ systematically in their levels of added schooling induced by their exposure to FPE. Burundian women from the wealthier strata would have attained lower grade levels independent of the policy. For poor women, however, FPE increased the probability to obtain some lower grade level schooling, which proves sufficient to induce behavioural changes.³⁰ Reconciling these insights with our main findings suggests that, next to women's household wealth, schooling attained at lower grade levels seems to matter more in influencing women's outcomes regarding literacy, fertility and work than later schooling years.

Robustness and Validity

The assumptions behind our IV-estimates require that women born (just) before, and women born (just) after the cut-off do not systematically vary in characteristics influencing their outcomes concerning fertility, literacy and work other than by their differential exposure to schooling. We included control variables as well as fixed effects to address this issue in first instance. However, given that we do not have information on women at the time of the policy implementation, there is the possibility of other unobserved differences affecting our results. To test for the influence of potentially confounding factors, we assimilate women's circumstances at the time of the intervention by making use of the Burundi MICS survey of 2005, with which we investigate the continuous nature of several influencing characteristics of treatment and control cohorts. To further validate our results, we look at the impact of

³⁰ This goes against findings prior to the year 2000 in which a handful of developing countries (including Burundi) evinced a curvilinear relationship between schooling and fertility, meaning that the early years of education actually increased women's levels of childbearing (Ainsworth, Beegle, and Nyamete 1996, Cochrane 1979; Martin 1995). Note, however, that these findings were attributed to a country's very (early) status along the fertility transition for one (Martin 1995), and secondly, to education's differential impact on mediating factors such as contraceptive usage and breastfeeding (Jain 1981).

Burundi's Civil War as a potential source of bias, and lastly, analyse the robustness of our estimates to a varying bandwidth and a varying functional form.

a) Unconfoundedness

Local randomisation is a crucial feature of the regression discontinuity design. As such, outcome determinants other than the treatment (education) should evolve in continuous fashion at the cut-off, i.e. for women born before, during and after the cut-off years. DHS surveys do not include retrospective information of respondents. To nonetheless explore the potentially discontinuous nature of important covariates at the time of the policy implementation, we utilise the Burundi Multiple Indicator Cluster Survey (MICS) conducted in 2005 and identify household members born in our treatment and control cohorts 1992-1996 and 1987-1991, respectively. We test for the assumption of unconfoundedness by estimating equation (1), replacing women's schooling with various (household) characteristics of these earlier sampled individuals as dependent variables. Although not identical to the respondents in the main sample, individuals born within these years and surveyed in 2005 represent plausible proxy-respondents corresponding to our treatment and control cohorts situated at the time of policy implementation. To further test the influence of parent's socio-economic status and mother's child investments, we also test for discontinuities in characteristics of "potential mothers" (see Keats 2018) by identifying women in the 2005 MICS who have had births between the years 1987-1996.³¹ The results are presented in Table A8 in the Appendix.

The estimates presented show statistically insignificant differences and point estimates which are close to zero, i.e. there are no apparent discontinuities between treatment and control cohorts at the time of policy implementation. It therefore seems unlikely that there were systematic differences influencing eventual outcomes of sample respondents today which are

³¹ We are restricted to analyse women who have had their first birth during this time, as the MICS only contains the year of birth of their first-born.

conflated with their differential access to schooling. Note that these results hold when dropping observations so as to include only female household members born between 1987-1996, and also, when testing for a differential discontinuous effect of poor and wealthy subgroups.

b) Burundian Civil War

The massacres of 1993 and the subsequent civil war was the most violent period in Burundi's history, with an estimated total of 300,000 deaths (UNFPA 2002). Conflicts were most intense from 1993 to 1996, the years thereafter outlined by significantly lower conflict intensities (Mercier, Ngenzebuke, and Verwimp 2020). The massacres and the civil war in Burundi have been linked to negative effects on households' wealth, children's health and children's likelihood of completing primary school (Bundervoet, Verwimp, and Akresh 2009; Verwimp and Van Bavel 2014; Mercier, Ngenzebuke, and Verwimp 2020).³² Depending on the investigated outcome, these studies define individuals affected by the war born between 1981-1998, which includes both our treatment and our control cohorts born between 1987 and 1996, i.e. there is no differential effect to be expected of the war itself between treatment and control. And although conflict intensities may have varied between years, given the timeline and the spatial dispersal of conflict, it is unlikely to systematically change for women born just after our chosen cut-off year in 1992.

If there were conflict-induced changes affecting women's eventual fertility, literacy or work outcomes differently across treatment and control, it is sensible to think that the discontinuous nature of these outcomes should be apparent at "cut-off" years other than 1992, too. To investigate this possibility, we re-estimate equation (1), but instead of women's schooling, we test for discontinuities in our main outcome variables and move our specified

³² Note that the negative effects on schooling were significantly smaller for women (Verwimp and Van Bavel 2014). Research on the impacts of similar civil wars, for example, in neighbouring Rwanda has confirmed the negative impact on schooling (and the lesser negative impact on women) at least in the short- to medium term (Akresh and de Walque 2008; Bundervoet and Fransen 2018; La Mattina 2018).

window size of five years around the corresponding placebo cut-off years starting from 1981 up until 1996. The results are plotted in Figures A12 through A20. Across all outcome variables tested, there are no discontinuities apparent in other years but at our specified cut-off date, as we would expect if all effects were induced by FPE.

Importantly, as shown in Table A8, there are no significant discontinuities in children's circumstances at the time of the policy implementation for our defined treatment cohort. Specifically, treated cohorts do not show a higher likelihood of having a living mother or a living father, which acts as a proxy for the war's intensity (Bundervoet 2009), lower household sizes or having better maternal care and health inputs as a child.³³

Note also, that our findings are highly robust to a varying bandwidth (see below), specifically to a smaller one. If conflict affected our treatment and control cohorts differently, estimates calculated from women born only a couple of birth years apart would likely cause biased effects to vanish. However, all of our effects remain significant at low bandwidth sizes, especially in the full sample. Hence, for our analysis of the impact of schooling induced by FPE, it is plausible to assume that treatment and control group were similarly affected by the civil war such that we can consistently identify the isolated effect of education with our estimation strategy.

To nonetheless test for the potential influence of differing war exposure, we remove provinces with above median conflict intensities from our sample and re-estimate our main outcome variables (Table A9). We use data from Bundervoet (2009) who calculates conflict intensity as the estimated percentage of a parent (mother or father) being killed in the massacres of 1993 using historical records from Chrétien and Mukuri (2002).³⁴ We subsequently drop

³³ These maternal health variables relate to mothers' last-born child, born within the last two years preceding the survey. We use this as a proxy for mother's maternal care in general.

³⁴ This may not fully account for provincial intensities of subsequent civil war years. To this, we test the results by leaving out the most affected provinces of the first years of the civil war as defined by Bundervoet, Verwimp, and Akresh (2009). The results hold even when excluding these regions.

provinces from our sample with above median conflict intensities, i.e. where above 6.6% of mothers or fathers are estimated to have been killed in the massacres.³⁵ All of our interpretations remain unchanged and robust when removing of conflict-intense provinces.

c) Varying Bandwidth and Functional Form

To investigate the sensibility of the estimated first-stage discontinuity as well as our second-stage results to a varying bandwidth, we re-estimate our main results using window sizes ranging from 2 to 10 birth year cohorts. Figures A2 a) and b) plot the estimated first-stage discontinuities for each bandwidth size together with the 95% confidence interval, each for the full sample, and separated along household wealth. We find significant increases in schooling from a bandwidth size of as little as two (three for the respective subsamples) birth year cohorts on each side of the cut-off. The estimates stabilise from a bandwidth size of 5 onwards.

Concerning the second-stage estimates, Figures A3 through A11 provide evidence for a robust effect of education on fertility and associated factors in regard to bandwidth size, and especially, concerning the heterogeneous nature of our main results.³⁶ Estimates for the poor subgroup are larger and more statistically significant at all data window sizes. This is especially true at smaller bandwidth sizes, which presumably, present less potential bias in our estimates given that larger, secular time trends are not as influential when comparing women born only a couple of years apart.

Next to the bandwidth choice, we test for sensitivity due to changing functional form as a further investigation into the robustness of our results. We adjust the functional form of equations (1) and (2) by adding higher order polynomials of the running variable (year of birth), alternating the interaction of them with the treatment indicator ($1[YOB \geq 1992]$). We stick to

³⁵ To compare, the mean conflict intensity across provinces is 9%, the upper quartile of provinces had over 16% of parental losses, and individuals in the highest affected province had an occurrence of over 22% parental deaths.

³⁶ We restrict the analysis to the poor and wealthy subgroups only.

our selected bandwidth size of five birth year cohorts on each side of the cut-off. The results are presented in Tables A10 and A11 of the Appendix for first- and second-stage estimates, respectively. The estimate of the discontinuity remains significant throughout all common RDD specifications, apart from specification 6. The size of the significant, discontinuous effect varies from 1.170 to 1.518 years of education. These first-stage estimates are carried over to the second-stage in Table A11, providing an interesting insight into the heterogeneous nature of our results. Throughout the specifications, the main estimates remain relatively robust in size and in significance. With increasing terms added to the equation, the IV-estimate tends to drop in precision (note also the dwindling F-Statistic as can be inferred from the estimates in Table A10), losing statistical significance for most estimates from specification 4 and onwards. It is noteworthy, though, that estimates generated from the sample of poor women are more robust than the ones estimated from wealthier half of the sample. Up to the quadratic specification, (3), all eight coefficients remain significant (four of them at the 5% level) for the poor subgroup of women, whereas none of the estimates of the wealthy subgroup are significant.

V. CONCLUSION

This article investigates the causal link between education and fertility by exploiting Burundi's free primary education policy (FPE) of 2005 as a natural experiment. We identify exogenous increases in schooling through a fuzzy regression discontinuity design (RDD). Subsequently, we conduct instrumental variable estimations examining the causal effect of additional schooling on adolescent fertility and associated factors.

Our results show that Burundi's FPE policy increased educational outcomes for women regardless of their socio-economic standing. Being young enough to benefit from free primary schooling (age 13 or younger) increased women's educational attainment by a substantial 1.22 years. Employing policy induced change in individual education as an instrument, we find that one year of additional education delayed women's first birth by half a year and overall,

decreased their probability of having their first childbirth before the age of 20 by 10%. In accordance with similar studies on the topic, we also find an accompanied decrease in teenage marriage. Investigating the mechanisms through which education is proposed to reduce women's fertility, we find supportive evidence for both the learning- and the income mechanism. More schooling leads to higher literacy levels, decrease desired family sizes, and improves labour market opportunities. Numerous robustness and sensitivity tests support our baseline findings and interpretations.

However, almost all the effects of additional education seem to be driven by women from the poorer half of the population, providing ample evidence for substantial treatment heterogeneity governed by wealth. Additionally, our results suggest that women from poor households increased educational exposure via FPE primarily at (early) primary school grades, whereas wealthy women did so in late primary to secondary school. Combined with our main results, this suggests that achieving some fundamental years of schooling might matter more in altering women's fertility behaviour and associated factors, than schooling attained at later stages along the educational track. While relatively richer women may always achieve some level of primary education, poorer women in developing countries may not achieve even low levels of schooling absent policy intervention. This suggests that, apart from fostering more schooling, FPE can contribute to induce changes in fertility behaviour, at least for the poor.

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ONLINE APPENDIX

Paper: “*Heterogeneous Effects of Women’s Schooling on Fertility, Literacy and Work:
Evidence from Burundi’s Free Primary Education Policy*”

Table A1: Summary Statistics

	Sample					
	Full Sample		Poor		Wealthy	
	Control	Treatment	Control	Treatment	Control	Treatment
Number of Observations	4476	3238	2302	1556	2174	1682
Age at Survey	24.19 [3.05]	21.96 [1.46]	24.16 [3.03]	21.93 [1.47]	24.23 [3.08]	21.99 [1.45]
Education (compl. schooling years)	3.55 [4.18]	5.60 [4.30]	1.99 [2.79]	3.98 [3.61]	5.56 [4.77]	7.45 [4.29]
Never received schooling (0-1)	0.44	0.23	0.57	0.32	0.26	0.13
Residence: Rural (0-1)	0.87	0.85	0.99	0.99	0.73	0.69
Wealth Quintile	3.01	3.09	1.94	1.97	4.38	4.38
Number of Siblings	6.20 [2.47]	5.91 [2.42]	6.18 [2.45]	5.95 [2.33]	6.22 [2.51]	5.86 [2.53]
<i>Religion</i>						
Catholic	0.59	0.55	0.62	0.61	0.54	0.49
Protestant	0.34	0.36	0.31	0.31	0.38	0.42
Muslim	0.03	0.04	0.01	0.02	0.05	0.05
<i>Fertility</i>						
Age at first Birth	19.67 [2.55]	18.98 [2.06]	19.59 [2.43]	18.99 [1.99]	19.80 [2.72]	18.97 [2.20]
Has given Birth before Age 20 (0-1)	0.34	0.32	0.37	0.39	0.31	0.25
<i>Reproductive Behaviour</i>						
Age at first Sex	15.81 [12.86]	12.11 [11.34]	16.34 [12.20]	13.69 [11.86]	15.12 [13.64]	10.31 [10.42]
Has had Sex before Age 20 (0-1)	0.54	0.50	0.58	0.58	0.49	0.42
Used Condom with last Sex. Partner (0-1)	0.02	0.06	0.01	0.03	0.04	0.10
Knows Condom as contr. Method (0-1)	0.94	0.95	0.92	0.94	0.96	0.96
<i>Mating</i>						
Age first Marriage	18.78 [2.72]	18.17 [2.19]	18.60 [2.52]	18.03 [2.09]	19.07 [2.98]	18.42 [2.34]
Married before Age 20 (0-1)	0.48	0.39	0.54	0.49	0.41	0.28
<i>Pathway: Learning</i>						
Literacy: Able to read sentence (0-1)	0.56	0.71	0.43	0.60	0.73	0.84
Desired Number of Children	4.01 [1.42]	3.67 [1.33]	4.05 [1.43]	3.63 [1.30]	3.96 [1.41]	3.72 [1.37]
<i>Pathway: Income</i>						
Worked in last 12 Months (0-1)	0.07	0.09	0.07	0.08	0.07	0.09
Works for Third Party (0-1)	0.13	0.16	0.16	0.18	0.10	0.14
Paid in Cash (0-1)	0.14	0.18	0.12	0.16	0.18	0.20

Notes: The sample consists of the 2010/2011 and 2016/2017 Burundi DHS Female Surveys using a bandwidth size of 5 birth year cohorts on either side of the cut-off. Respondents are restricted to age 20 and older at the time of the survey. The 'poor' and 'wealthy' subsamples include women who are on and below and above the median wealth level, respectively. The 'Control' group is comprised of individuals born from 1987-1991, the 'Treatment' group is comprised of individuals born in 1992-1996. The statistics are weighted using the DHS sample weights and account for pooling between survey rounds.

Table A2: Second-stage Estimate, Knowledge and Use of Condoms

	Sample					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Knows Condom as contr. Method (0-1)						
Schooling	0.010	0.010	0.011	0.012	0.010	0.011
Instr. by 1[YOB ≥ 1992]	(0.009)	(0.009)	(0.020)	(0.019)	(0.011)	(0.011)
Mean Ctrl. Group	[0.94]		[0.92]		[0.96]	
F-Statistic	27.9	30.0	13.8	17.0	11.7	12.2
Observations	7,713	7,709	3,857	3,856	3,856	3,853
Adj. R ²	0.015	0.024	0.015	0.029	(0.006)	(0.007)
Panel B: Used Condom with last sex. Partner (0-1)						
Schooling	0.040**	0.042*	0.033*	0.031*	0.070	0.090
Instr. by 1[YOB ≥ 1992]	(0.020)	(0.025)	(0.019)	(0.018)	(0.071)	(0.126)
Mean Ctrl. Group	[0.023]		[0.012]		[0.038]	
F-Statistic	10.7	8.5	8.4	9.8	1.6	0.7
Observations	4,801	4,799	2,703	2,702	2,098	2,097
Adj. R ²	(0.300)	(0.302)	(0.304)	(0.240)	(0.997)	(1.650)

Notes: The results in each panel and column are produced by a separate regression. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of this excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A3: Second-stage Estimate, Mechanisms: Learning

	Sample					
	Full		Poor		Wealthy	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		Yes		Yes		Yes
Region Fixed Effects		Yes		Yes		Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Reads Newspaper at least once per Week (0-1)						
Schooling	-0.011	-0.013	-0.003	-0.003	-0.025	-0.026
Instr. by 1[YOB ≥ 1992]	(0.008)	(0.008)	(0.011)	(0.011)	(0.015)	(0.016)
Mean Ctrl. Group	[0.06]		[0.03]		[0.10]	
F-Statistic	27.9	30.0	13.9	17.1	11.7	12.2
Observations	7,712	7,708	3,856	3,855	3,856	3,853
Adj. R ²	(0.101)	(0.082)	(0.003)	0.019	(0.290)	(0.260)
Panel B: Listens to Radio at least once per Week (0-1)						
Schooling	0.029*	0.025	0.015	0.014	0.023	0.019
Instr. by 1[YOB ≥ 1992]	(0.017)	(0.019)	(0.034)	(0.032)	(0.026)	(0.027)
Mean Ctrl. Group	[0.42]		[0.30]		[0.58]	
F-Statistic	27.8	29.8	13.8	17.0	11.6	12.1
Observations	7,712	7,708	3,857	3,856	3,855	3,852
Adj. R ²	0.123	0.136	0.087	0.100	0.101	0.136
Panel C: Watches TV at least once per Week (0-1)						
Schooling	0.014	0.010	0.007*	0.007*	0.013	0.012
Instr. by 1[YOB ≥ 1992]	(0.009)	(0.008)	(0.004)	(0.004)	(0.019)	(0.017)
Mean Ctrl. Group	[0.061]		[0.003]		[0.137]	
F-Statistic	28.0	30.2	13.9	17.1	11.7	12.2
Observations	6,408	6,404	3,527	3,526	2,881	2,878
Adj. R ²	(0.026)	0.065	(0.272)	(0.198)	0.010	0.239

Notes: The results in each panel and column are produced by a separate regression. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of this excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A4: Second-stage Estimate, Birth before Age (0-1)

	Age 16	Age 17	Age 18	Age 19	Age 20	Age 21	Age 22	Age 23
Panel A: Full								
Mean Ctrl. Group	[0.03]	[0.07]	[0.12]	[0.22]	[0.34]	[0.48]	[0.60]	[0.68]
Schooling	0.002	0.012	0.009	-0.031*	-0.034*	-0.033	-0.033	-0.018
Instr. by 1[YOB ≥ Cut-off]	(0.007)	(0.010)	(0.014)	(0.017)	(0.019)	(0.029)	(0.027)	(0.029)
F-Statistic	30.0	30.0	30.0	30.0	30.0	12.6	13.8	11.3
Observations	7,709	7,709	7,709	7,709	7,709	6,412	5,553	4,576
Adj. R ²	(0.002)	(0.052)	(0.001)	0.095	0.138	0.173	0.205	0.195
Panel B: Poor								
Mean Ctrl. Group	[0.03]	[0.07]	[0.13]	[0.23]	[0.37]	[0.52]	[0.65]	[0.74]
Schooling	-0.003	0.019	0.016	-0.054	-0.069*	0.001	-0.041	-0.025
Instr. by 1[YOB ≥ Cut-off]	(0.013)	(0.021)	(0.029)	(0.034)	(0.039)	(0.066)	(0.045)	(0.042)
F-Statistic	17.0	17.0	17.0	17.0	17.0	4.9	9.8	8.7
Observations	3,856	3,856	3,856	3,856	3,856	3,217	2,792	2,325
Adj. R ²	0.020	(0.069)	(0.010)	0.052	0.056	0.082	0.144	0.135
Panel C: Wealthy								
Mean Ctrl. Group	[0.03]	[0.07]	[0.11]	[0.19]	[0.31]	[0.44]	[0.52]	[0.60]
Schooling	0.008	0.010	0.006	-0.021	-0.012	-0.062	-0.037	-0.008
Instr. by 1[YOB ≥ Cut-off]	(0.009)	(0.014)	(0.018)	(0.023)	(0.025)	(0.039)	(0.043)	(0.058)
F-Statistic	12.2	12.2	12.2	12.2	12.2	5.9	4.5	3.1
Observations	3,853	3,853	3,853	3,853	3,853	3,195	2,761	2,251
Adj. R ²	(0.078)	(0.048)	(0.003)	0.105	0.136	0.186	0.242	0.171
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results in each column and for each panel generated by separate regressions. The estimates at each age increment test the outcome that women has been married before the indicated age while restricting the sample to respondents aged at least that of the investigated age. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1. See full notes below regression Table 1.

Table A5: Second-stage Estimate, Married before Age (0-1)

	Age 16	Age 17	Age 18	Age 19	Age 20	Age 21	Age 22	Age 23
Panel A: Full								
Mean Ctrl. Group	[0.07]	[0.13]	[0.22]	[0.34]	[0.48]	[0.58]	[0.66]	[0.71]
Schooling	-0.012	-0.007	-0.007	-0.032	-0.061***	-0.044	-0.032	-0.017
Instr. by 1[YOB ≥ Cut-off]	(0.011)	(0.013)	(0.017)	(0.019)	(0.018)	(0.027)	(0.025)	(0.028)
F-Statistic	30.0	30.0	30.0	30.0	30.0	12.6	13.8	11.3
Observations	7,709	7,709	7,709	7,709	7,709	6,412	5,553	4,576
Adj. R ²	0.046	0.070	0.102	0.178	0.187	0.251	0.262	0.240
Panel B: Poor								
Mean Ctrl. Group	[0.08]	[0.14]	[0.24]	[0.38]	[0.54]	[0.65]	[0.74]	[0.79]
Schooling	-0.021	0.009	-0.008	-0.059	-0.090**	-0.020	-0.024	-0.009
Instr. by 1[YOB ≥ Cut-off]	(0.022)	(0.028)	(0.033)	(0.037)	(0.037)	(0.064)	(0.039)	(0.041)
F-Statistic	17.0	17.0	17.0	17.0	17.0	4.9	9.8	8.7
Observations	3,856	3,856	3,856	3,856	3,856	3,217	2,792	2,325
Adj. R ²	0.033	0.026	0.095	0.127	0.056	0.149	0.150	0.118
Panel C: Wealthy								
Mean Ctrl. Group	[0.07]	[0.11]	[0.19]	[0.30]	[0.41]	[0.49]	[0.55]	[0.61]
Schooling	-0.006	-0.023	-0.004	-0.011	-0.045*	-0.066*	-0.042	-0.020
Instr. by 1[YOB ≥ Cut-off]	(0.013)	(0.017)	(0.022)	(0.027)	(0.025)	(0.038)	(0.043)	(0.050)
F-Statistic	12.2	12.2	12.2	12.2	12.2	5.9	4.5	3.1
Observations	3,853	3,853	3,853	3,853	3,853	3,195	2,761	2,251
Adj. R ²	0.057	0.064	0.094	0.166	0.257	0.278	0.318	0.289
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results in each column and for each panel generated by separate regressions. The estimates at each age increment test the outcome that women has been married before the indicated age while restricting the sample to respondents aged at least that of the investigated age. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1. See full notes below regression Table 1.

Table A6: Second-stage Estimate, Sex before Age (0-1)

	Age 16	Age 17	Age 18	Age 19	Age 20	Age 21	Age 22	Age 23
Panel A: Full								
Mean Ctrl. Group	[0.08]	[0.15]	[0.26]	[0.41]	[0.54]	[0.65]	[0.72]	[0.77]
Schooling	-0.007	-0.003	-0.039**	-0.002	-0.044**	-0.034	-0.017	-0.023
Instr. by 1[YOB ≥ Cut-off]	(0.011)	(0.015)	(0.018)	(0.020)	(0.019)	(0.027)	(0.024)	(0.026)
F-Statistic	30.0	30.0	30.0	30.0	30.0	12.6	13.8	11.3
Observations	7,709	7,709	7,709	7,709	7,709	6,412	5,553	4,576
Adj. R ²	0.042	0.054	0.080	0.106	0.158	0.182	0.155	0.185
Panel B: Poor								
Mean Ctrl. Group	[0.09]	[0.16]	[0.27]	[0.43]	[0.58]	[0.70]	[0.77]	[0.82]
Schooling	-0.018	0.001	-0.080**	-0.023	-0.070*	-0.020	-0.003	-0.013
Instr. by 1[YOB ≥ Cut-off]	(0.022)	(0.029)	(0.036)	(0.036)	(0.036)	(0.057)	(0.038)	(0.035)
F-Statistic	17.0	17.0	17.0	17.0	17.0	4.9	9.8	8.7
Observations	3,856	3,856	3,856	3,856	3,856	3,217	2,792	2,325
Adj. R ²	0.024	0.053	0.036	0.149	0.088	0.136	0.094	0.104
Panel C: Wealthy								
Mean Ctrl. Group	[0.08]	[0.14]	[0.24]	[0.39]	[0.49]	[0.58]	[0.65]	[0.70]
Schooling	0.002	-0.006	-0.014	0.017	-0.031	-0.050	-0.035	-0.037
Instr. by 1[YOB ≥ Cut-off]	(0.015)	(0.021)	(0.025)	(0.031)	(0.028)	(0.038)	(0.041)	(0.048)
F-Statistic	12.2	12.2	12.2	12.2	12.2	5.9	4.5	3.1
Observations	3,853	3,853	3,853	3,853	3,853	3,195	2,761	2,251
Adj. R ²	0.025	0.063	0.099	0.021	0.170	0.194	0.201	0.235
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results in each column and for each panel generated by separate regressions. The estimates at each age increment test the outcome that women has been married before the indicated age while restricting the sample to respondents aged at least that of the investigated age. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1. See full notes below regression Table 1.

Table A7: Redefined Poor and Wealthy Subgroups

	Dependent Variable							
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Literacy (0-1)		Ideal Number of Children		Age at first Birth		First Birth before Age 20 (0-1)	
	Poor	Wealthy	Poor	Wealthy	Poor	Wealthy	Poor	Wealthy
Schooling	0.055**	0.047**	-0.215**	-0.045	0.604**	0.082	-0.051*	0.004
Instr. by 1[YOB ≥ 1992]	(0.024)	(0.020)	(0.091)	(0.089)	(0.276)	(0.346)	(0.028)	(0.028)
Mean Ctrl. Group	[0.50]	[0.83]	[4.07]	[3.72]	[19.56]	[20.40]	[0.37]	[0.23]
F Statistic	20.91	8.31	20.64	7.61	8.63	2.34	21.03	8.23
Observations	5,801	1,903	5,651	1,865	4,040	877	5,804	1,905
Adj. R	0.42	0.38	(0.05)	0.03	(0.19)	0.24	0.09	0.02
	Married before Age 20 (0-1)		Worked last Year (0-1)		Works for Third Party (0-1)		Paid in Cash (0-1)	
	Poor	Wealthy	Poor	Wealthy	Poor	Wealthy	Poor	Wealthy
Schooling	-0.088***	-0.008	0.008	0.010	0.083***	-0.001	0.041*	-0.004
Instr. by 1[YOB ≥ 1992]	(0.028)	(0.026)	(0.014)	(0.016)	(0.029)	(0.017)	(0.022)	(0.021)
Mean Ctrl. Group	[0.52]	#N/A	[0.07]	[0.08]	[0.13]	[0.19]	[0.11]	[0.33]
F Statistic	21.03	8.23	21.03	8.23	21.56	14.87	21.56	14.87
Observations	5,804	1,905	5,804	1,905	5,167	1,237	5,167	1,237
Adj. R	0.06	0.16	0.03	0.00	(0.63)	0.13	(0.10)	0.25

Notes: The results in each column and within each panel are produced by a separate regression. The samples for the poor and wealthy subgroups is redefined. Poor women live in households with sand, dung or dirt flooring. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year '1[Year of Birth (YOB) ≥ 1992]'. The strength of this excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A8: Smoothness Test of Covariates

	Dependent Variable						
	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel A: Household Characteristics							
	Wealth (1-5)	Urban (0-1)	Region of Residence	Household Size	Mother Alive (0-1)	Father Alive (0-1)	Religion (Household Head) (1-6)
I[Year of Birth \geq 1992]	-0.047 (0.043)	-0.001 (0.004)	0.035 (0.057)	0.154 (0.145)	0.007 (0.013)	0.023 (0.018)	-0.079 (0.241)
Mean Ctrl. Group	[3.07]	[0.05]	[2.85]	[8.05]	[0.85]	[0.70]	[1.40]
Observations	11,598	11,598	11,598	11,598	10,531	10,531	2,266
Adj. R2	0.103	0.087	0.018	0.000	0.012	0.016	0.034
Panel B: Mothers' Characteristics							
	Education	Ever attended School (0-1)	Literacy (0-1)	Ever Married (0-1)	Height at Birth (Last born Child)	Has been Breastfeeding (Last born Child) (0-1)	Received Vitamin A (Last born Child) (0-1)
I[Children's Year of Birth \geq 1992]	-0.010 (0.407)	0.003 (0.048)	-0.001 (0.053)	-0.035 (0.119)	-0.034 (0.153)	0.016 (0.024)	0.028 (0.071)
Mean Ctrl. Group	[4.65]	[0.61]	[0.30]	[0.58]	[2.75]	[0.96]	[0.39]
Observations	694	2,247	2,108	412	1,026	1,057	1,057
Adj. R2	0.099	0.038	0.039	0.043	0.023	0.003	0.017

Notes: Unlike results presented in all other Tables, these estimates were constructed using Burundi's Multiple Cluster Indicator Survey (MICS) of 2005. See section on robustness and validity, part a). The results in each column within each panel are produced by a separate regression. The discontinuities estimated in panel A reflect changes in household characteristics of members born in or after 1992. The discontinuities estimated in panel B reflect changes in characteristics of "potential mothers", i.e. of women who have had their first birth born in or after 1992. All estimations include controls for household size, as well as residency (urban-rural indicator and region fixed effects). The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A9: Subsample Estimation: Provinces with below-median Conflict Intensities

	Dependent Variable							
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Literacy (0-1)	Ideal Number of Children	Age at first Birth	First Birth before Age 20 (0-1)	Married before Age 20 (0-1)	Worked last Year (0- 1)	Works for Third Party (0-1)	Paid in Cash (0-1)
Schooling Instr. by I[YOB \geq 1992]	0.078*** (0.017)	-0.166** (0.076)	0.448** (0.206)	-0.043** (0.020)	-0.062*** (0.022)	0.012 (0.011)	0.028 (0.019)	0.032* (0.018)
Mean Ctrl. Group	[0.54]	[4.27]	[19.46]	[0.37]	[0.51]	[0.07]	[0.12]	[0.14]
F-Statistic	22.59	20.82	9.85	22.60	22.60	22.60	21.74	21.74
Observations	3,823	3,692	2,471	3,824	3,824	3,824	3,197	3,197
Adj. R	0.49	(0.00)	(0.01)	0.11	0.16	0.03	(0.11)	(0.03)

Notes: The results in each column are produced by a separate regression. The sample is comprised of regions with below median conflict intensities. The IV-estimate and key explanatory variable 'Schooling' is instrumented by the binary indicator of being born in or after the cut-off year 'I[Year of Birth (YOB) \geq 1992]'. The strength of this excluded instrument is given by the reported F-Statistic. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A10: First-stage Discontinuity Estimate by Functional Form

	(1) Linear			(2) Linear Interaction			(3) Quadratic			(4) Quadratic Interaction			(5) Cubic			(6) Cubic Interaction		
	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy
Years of Schooling																		
1[Year of Birth ≥ 1992]	1.234*** (0.223)	0.956*** (0.220)	1.284*** (0.366)	1.223*** (0.223)	0.908*** (0.220)	1.280*** (0.367)	1.170*** (0.228)	0.772*** (0.230)	1.348*** (0.365)	1.518*** (0.364)	0.856** (0.353)	1.796*** (0.618)	1.228*** (0.296)	0.683** (0.296)	1.526*** (0.477)	0.413 (0.731)	-0.510 (0.717)	0.569 (1.283)
Interaction: 1*Year of Birth				0.164** (0.077)	0.339*** (0.078)	-0.099 (0.130)				0.989*** (0.304)	0.966*** (0.329)	0.706 (0.484)				-1.385 (0.959)	-1.706* (1.000)	-1.698 (1.596)
Interaction: 1*(Year of Birth)^2										-0.028 (0.059)	-0.084 (0.063)	0.023 (0.090)				-0.116 (0.408)	-0.294 (0.449)	-0.182 (0.696)
Interaction: 1*(Year of Birth)^3																##### (0.055)	-0.170*** (0.063)	-0.147* (0.085)
Year of Birth	0.105 (0.223)	0.200 (0.220)	0.028 (0.058)	0.038 (0.047)	0.064 (0.044)	0.070 (0.085)	0.127*** (0.040)	0.257*** (0.043)	0.004 (0.060)	-0.385 (0.241)	-0.099 (0.236)	-0.470 (0.414)	0.103 (0.095)	0.295*** (0.097)	-0.069 (0.148)	1.252 (0.874)	1.919** (0.881)	1.301 (1.523)
Year of Birth^2							0.011 (0.007)	0.028*** (0.007)	-0.013 (0.013)	-0.068* (0.039)	-0.026 (0.038)	-0.087 (0.066)	0.013 (0.009)	0.026*** (0.010)	-0.008 (0.015)	0.535* (0.315)	0.718** (0.322)	0.568 (0.549)
Year of Birth^3													0.001 (0.004)	-0.002 (0.004)	0.003 (0.006)	0.066* (0.034)	0.081** (0.036)	0.072 (0.060)
Intercept	6.497*** (0.788)	3.238*** (0.788)	7.135*** (0.596)	6.288*** (0.442)	2.804*** (0.779)	7.258*** (0.622)	6.440*** (0.436)	3.099*** (0.780)	7.197*** (0.601)	5.653*** (0.518)	2.483*** (0.855)	6.521*** (0.809)	6.393*** (0.461)	3.172*** (0.811)	7.051*** (0.659)	7.023*** (0.830)	4.172*** (0.986)	7.933*** (1.403)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7709	3856	3853	7709	3856	3853	7709	3856	3853	7709	3856	3853	7709	3856	3853	7709	3856	3853
Adj. R^2	0.262	0.179	0.187	0.263	0.183	0.187	0.263	0.182	0.187	0.262	0.180	0.187	0.263	0.182	0.187	0.264	0.185	0.187

Notes: The results in each column are produced by a separate regression. The estimate in row one '1[Year of Birth ≥ 1992]' represents the discontinuous increase in schooling by women just-treated by the policy (13 years old), compared to women just too old to benefit from free schooling (14 years old). The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Table A11: Second-stage Estimates by Functional Form

	(1)			(2)			(3)			(4)			(5)			(6)		
	Linear			Linear Interaction			Quadratic			Quadratic Interaction			Cubic			Cubic Interaction		
	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy	Full	Poor	Wealthy
Literacy (0-1)																		
<i>Schooling</i>	0.051*** (0.017)	0.070** (0.028)	0.027 (0.024)	0.050*** (0.017)	0.067** (0.030)	0.027 (0.024)	0.043** (0.018)	0.060* (0.033)	0.025 (0.022)	0.069*** (0.025)	0.081 (0.055)	0.041 (0.030)	0.052** (0.023)	0.052 (0.058)	0.039 (0.027)	0.043 (0.203)	0.143 (0.204)	0.043 (0.216)
F-Statistic	[30.4]	[19.0]	[12.0]	[29.9]	[17.1]	[11.9]	[26.2]	[11.3]	[13.4]	[16.7]	[5.6]	[8.0]	[17.0]	[5.3]	[10.0]	[0.3]	[0.5]	[0.1]
Ideal Number of Children																		
<i>Schooling</i>	-0.150** (0.063)	-0.248** (0.112)	-0.088 (0.086)	-0.151** (0.063)	-0.261** (0.118)	-0.088 (0.086)	-0.153** (0.066)	-0.306** (0.148)	-0.076 (0.079)	0.026 (0.085)	-0.091 (0.205)	-0.027 (0.093)	-0.063 (0.075)	-0.177 (0.207)	-0.025 (0.084)	-1.233 (2.850)	0.759 (1.049)	-0.657 (1.416)
F-Statistic	[28.9]	[17.5]	[11.9]	[28.3]	[15.8]	[11.9]	[24.4]	[10.5]	[12.8]	[17.4]	[5.7]	[9.1]	[17.8]	[5.2]	[11.2]	[0.2]	[0.8]	[0.2]
Age at first Birth																		
<i>Schooling</i>	0.401** (0.191)	0.367 (0.245)	0.710 (0.554)	0.468** (0.204)	0.449* (0.263)	0.757 (0.564)	0.632** (0.256)	0.676** (0.342)	0.837 (0.607)	0.225 (0.215)	0.433 (0.421)	0.115 (0.319)	0.324 (0.205)	0.566 (0.415)	0.238 (0.273)	1.942 (3.569)	8.224 (139.187)	11.278 (160.028)
F-Statistic	[12.9]	[11.6]	[2.1]	[11.9]	[10.3]	[2.1]	[9.5]	[7.6]	[2.0]	[10.3]	[5.0]	[3.0]	[11.0]	[5.2]	[4.0]	[0.3]	[0.0]	[0.0]
First Birth before Age 20 (0-1)																		
<i>Schooling</i>	-0.033* (0.019)	-0.066* (0.037)	-0.013 (0.025)	-0.034* (0.019)	-0.069* (0.039)	-0.012 (0.025)	-0.038** (0.020)	-0.083* (0.047)	-0.016 (0.023)	-0.047 (0.031)	-0.122 (0.079)	0.003 (0.035)	-0.037 (0.027)	-0.125 (0.080)	0.003 (0.032)	-0.251 (0.454)	0.167 (0.370)	-0.176 (0.419)
F-Statistic	[30.5]	[19.0]	[12.3]	[30.0]	[17.0]	[12.2]	[26.3]	[11.3]	[13.6]	[16.9]	[5.6]	[8.3]	[17.2]	[5.3]	[10.2]	[0.3]	[0.5]	[0.2]
Married before Age 20 (0-1)																		
<i>Schooling</i>	-0.062*** (0.018)	-0.088** (0.035)	-0.044* (0.025)	-0.061*** (0.018)	-0.090** (0.037)	-0.045* (0.025)	-0.059*** (0.019)	-0.099** (0.044)	-0.037 (0.023)	-0.088*** (0.033)	-0.180** (0.090)	-0.039 (0.034)	-0.074*** (0.027)	-0.179** (0.087)	-0.027 (0.031)	-0.226 (0.399)	-0.030 (0.239)	-0.364 (0.763)
F-Statistic	[30.5]	[19.0]	[12.3]	[30.0]	[17.0]	[12.2]	[26.3]	[11.3]	[13.6]	[16.9]	[5.6]	[8.3]	[17.2]	[5.3]	[10.2]	[0.3]	[0.5]	[0.2]
Sex before Age 20 (0-1)																		
<i>Schooling</i>	-0.044** (0.018)	-0.069* (0.035)	-0.031 (0.025)	-0.044** (0.018)	-0.070* (0.037)	-0.031 (0.025)	-0.040** (0.019)	-0.074* (0.044)	-0.025 (0.023)	-0.047 (0.033)	-0.116 (0.080)	0.002 (0.038)	-0.040 (0.028)	-0.118 (0.080)	-0.003 (0.035)	-0.096 (0.253)	0.074 (0.274)	-0.017 (0.256)
F-Statistic	[30.5]	[19.0]	[12.3]	[30.0]	[17.0]	[12.2]	[26.3]	[11.3]	[13.6]	[16.9]	[5.6]	[8.3]	[17.2]	[5.3]	[10.2]	[0.3]	[0.5]	[0.2]
Worked Last Year (0-1)																		
<i>Schooling</i>	0.009 (0.010)	0.005 (0.018)	0.017 (0.016)	0.009 (0.010)	0.004 (0.018)	0.017 (0.016)	0.009 (0.011)	0.002 (0.022)	0.017 (0.015)	0.016 (0.017)	-0.005 (0.035)	0.027 (0.022)	0.014 (0.015)	-0.008 (0.035)	0.032 (0.020)	0.048 (0.149)	-0.037 (0.145)	0.035 (0.151)
F-Statistic	[19.0]	[19.0]	[12.3]	[30.0]	[17.0]	[12.2]	[26.3]	[11.3]	[13.6]	[16.9]	[5.6]	[8.3]	[17.2]	[5.3]	[10.2]	[0.3]	[0.5]	[0.2]
Works for Third Party (0-1)																		
<i>Schooling</i>	0.053*** (0.018)	0.092** (0.038)	0.029 (0.020)	0.054*** (0.019)	0.097** (0.040)	0.029 (0.020)	0.058*** (0.021)	0.111** (0.050)	0.030 (0.021)	0.084** (0.035)	0.155* (0.088)	0.055* (0.029)	0.090*** (0.032)	0.176* (0.090)	0.056* (0.029)	-3.373 (132.681)	-0.130 (0.322)	-0.602 (3.889)
F-Statistic	[19.0]	[19.0]	[12.3]	[33.2]	[17.6]	[13.4]	[28.0]	[12.5]	[13.3]	[17.4]	[6.5]	[9.1]	[18.4]	[6.7]	[10.8]	[0.0]	[0.3]	[0.0]
Paid in Cash (0-1)																		
<i>Schooling</i>	0.026* (0.015)	0.054* (0.029)	0.003 (0.020)	0.026* (0.015)	0.056* (0.030)	0.003 (0.020)	0.025 (0.016)	0.061* (0.036)	0.001 (0.021)	0.057* (0.030)	0.147 (0.104)	0.031 (0.026)	0.064** (0.025)	0.130* (0.071)	0.031 (0.026)	-1.040 (41.339)	0.043 (0.244)	-0.322 (2.179)
F-Statistic	[34.4]	[19.4]	[13.4]	[33.2]	[17.6]	[13.4]	[28.0]	[12.5]	[13.3]	[14.1]	[3.4]	[9.2]	[18.4]	[6.7]	[10.8]	[0.0]	[0.3]	[0.0]
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The results in each panel and in each column are produced by a separate regression. The estimate 'Schooling' represents the causal effect of education, the IV-estimate, instrumented by the dichotomous indicator '1[Year of Birth ≥ 1992]'. The standard errors reported are clustered at the survey-cluster level. ***, **, * represents significance at the 1, 5 and 10 percent level, respectively. See full notes below Table 1.

Figure A1 a): Regression Discontinuity Estimates Full Sample, Plotted

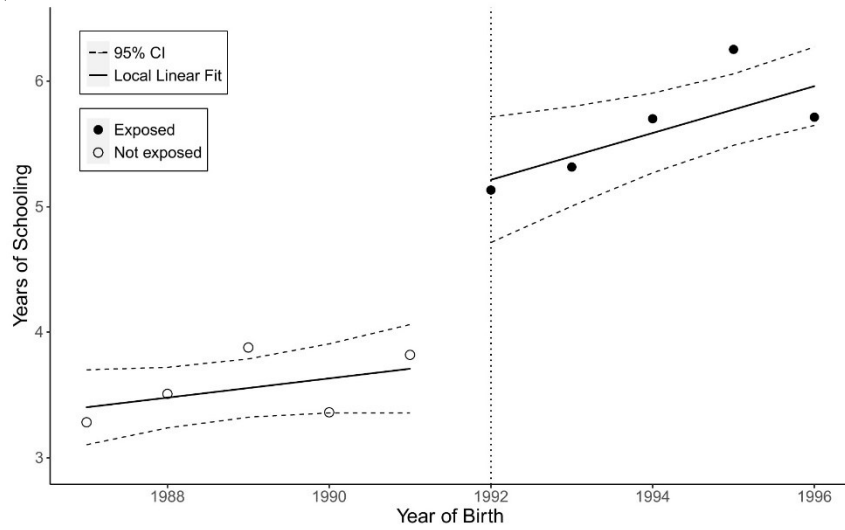


Figure A1 b): Regression Discontinuity Estimates, Plotted, Poor Subgroup

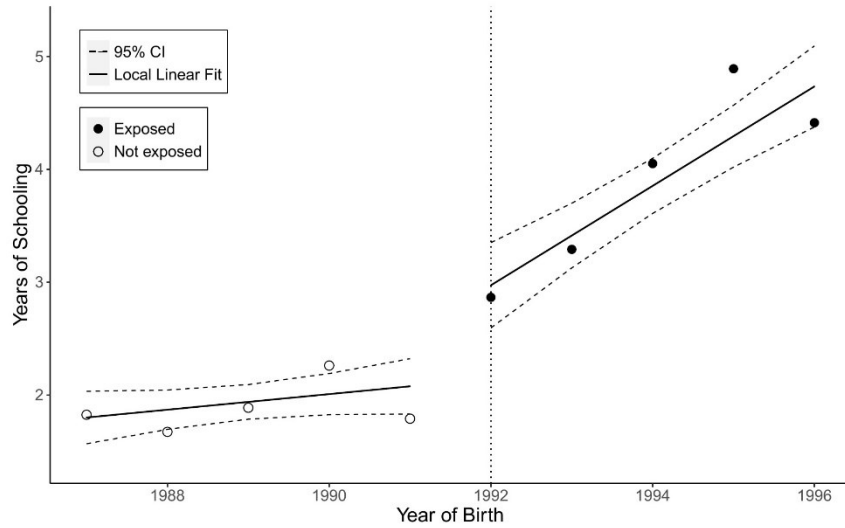


Figure A1 c): Regression Discontinuity Estimates, Plotted, Wealthy Subgroup

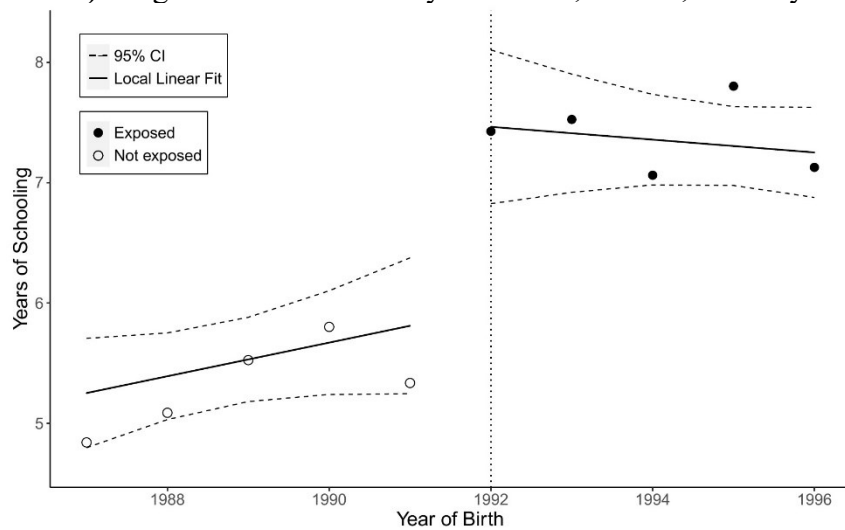
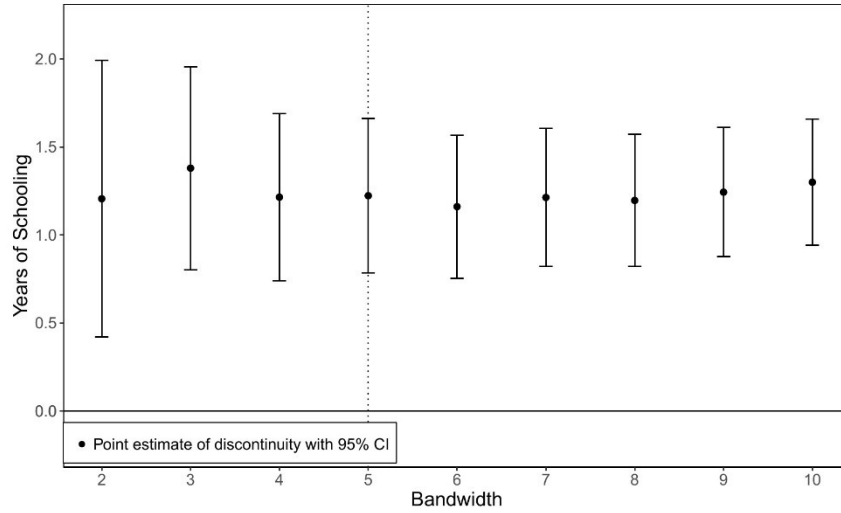
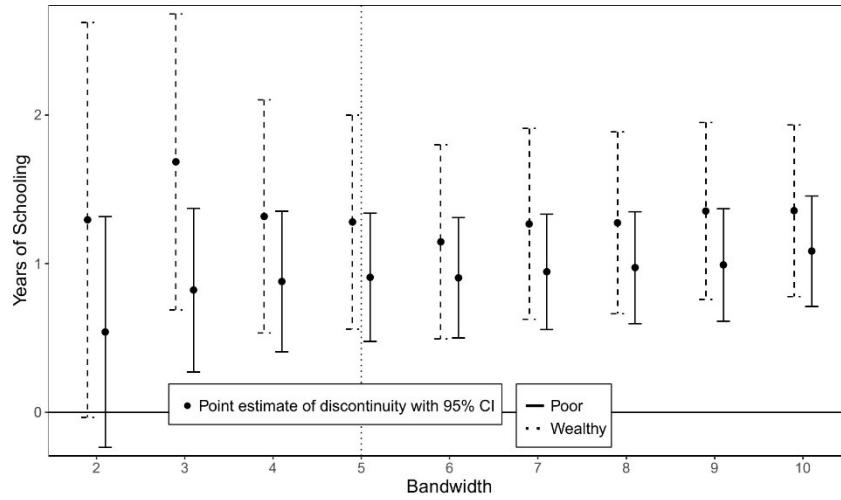


Figure A2 a): First-stage Discontinuity by Bandwidth Choice



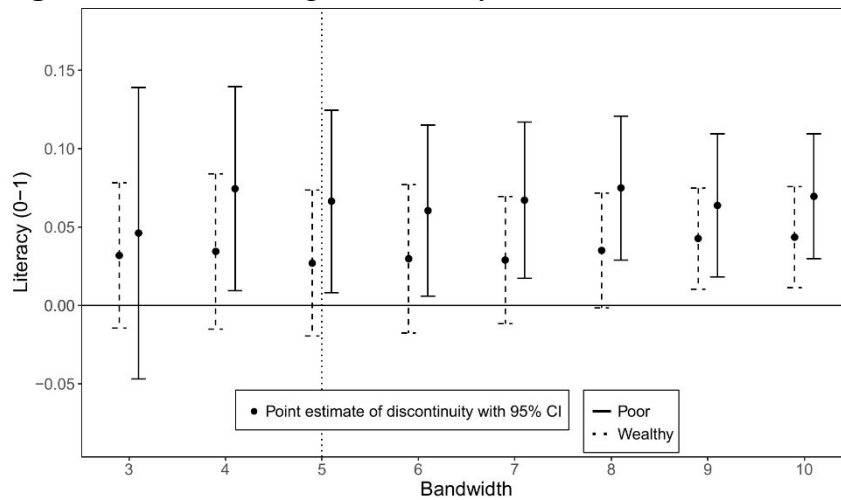
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A2 b): First-stage Discontinuity by Bandwidth Choice, Separated by Wealth



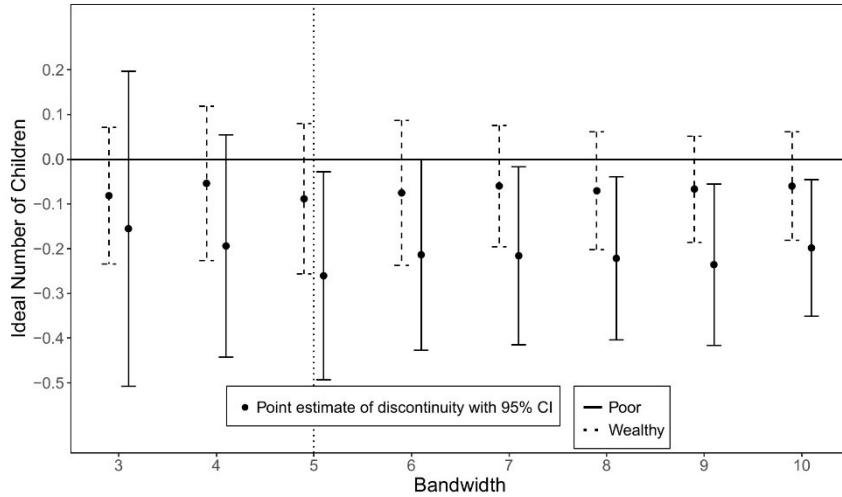
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A3: Second-Stage Estimate by Bandwidth Choice, Literacy



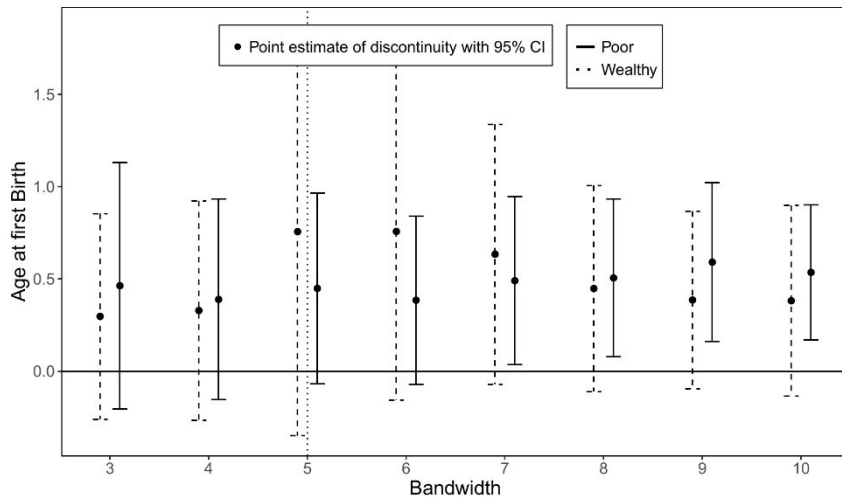
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A4: Second-Stage Estimate by Bandwidth Choice, Ideal Number of Children



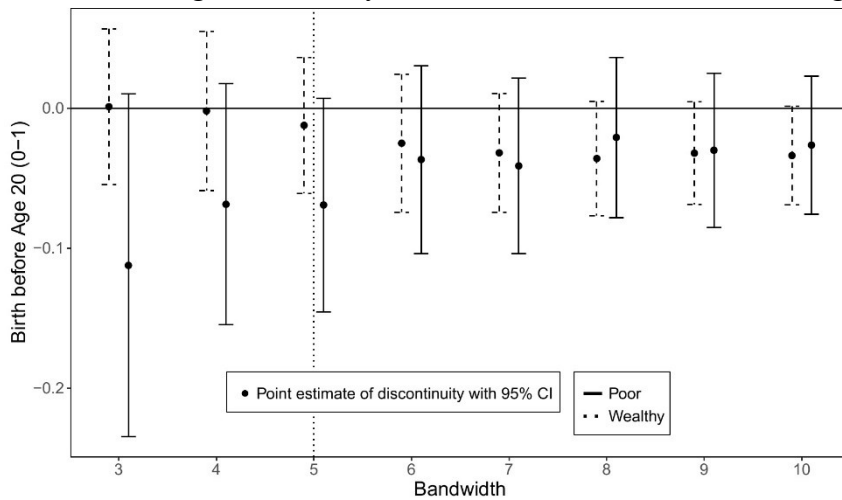
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A5: Second-Stage Estimate by Bandwidth Choice, Age at first Birth



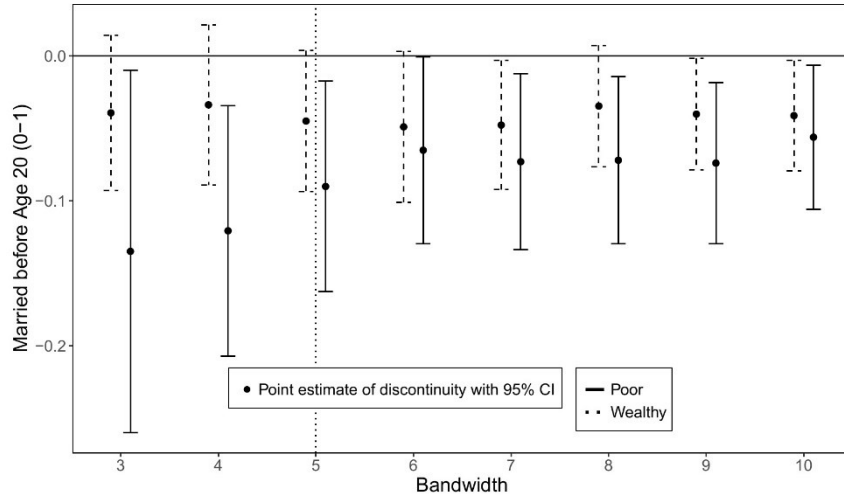
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A6: Second-Stage Estimate by Bandwidth Choice, Birth before Age 20 (0-1)



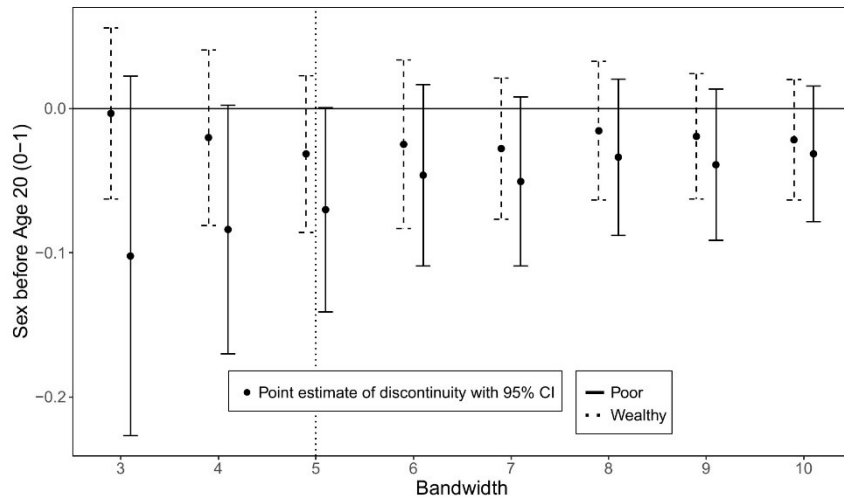
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A7: Second-Stage Estimate by Bandwidth Choice, Married before Age 20 (0-1)



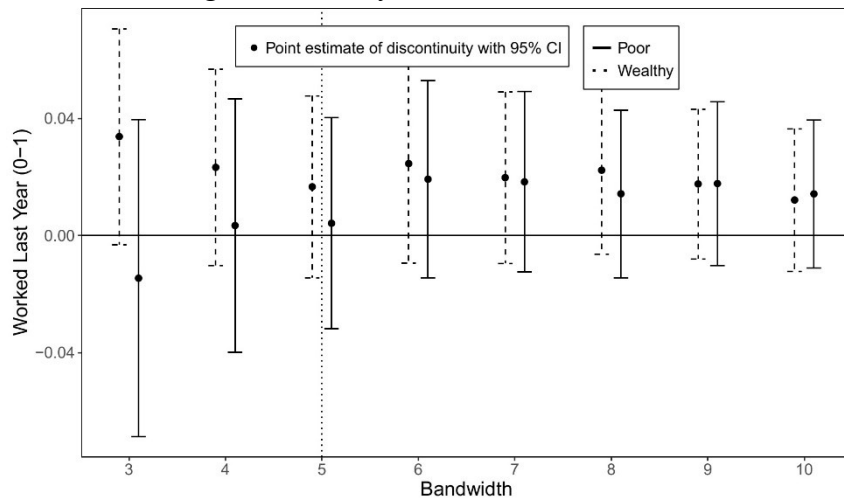
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A8: Second-Stage Estimate by Bandwidth Choice, Sex before Age 20 (0-1)



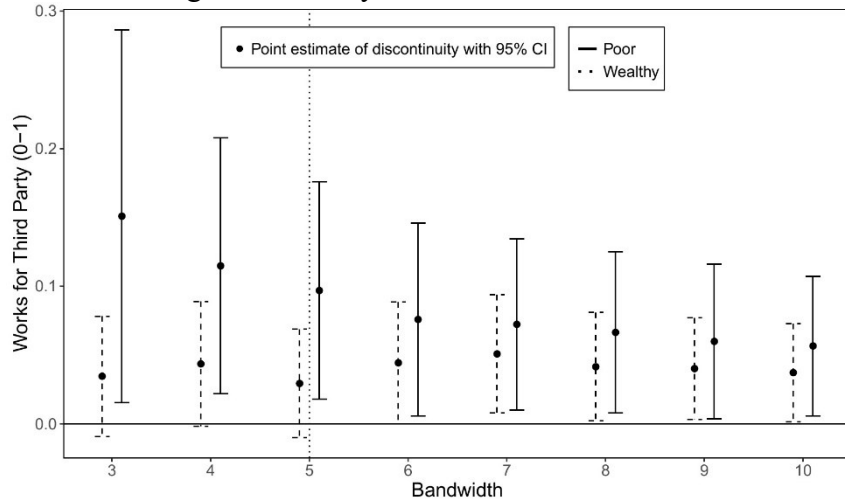
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A9: Second-Stage Estimate by Bandwidth Choice, Worked Last Year (0-1)



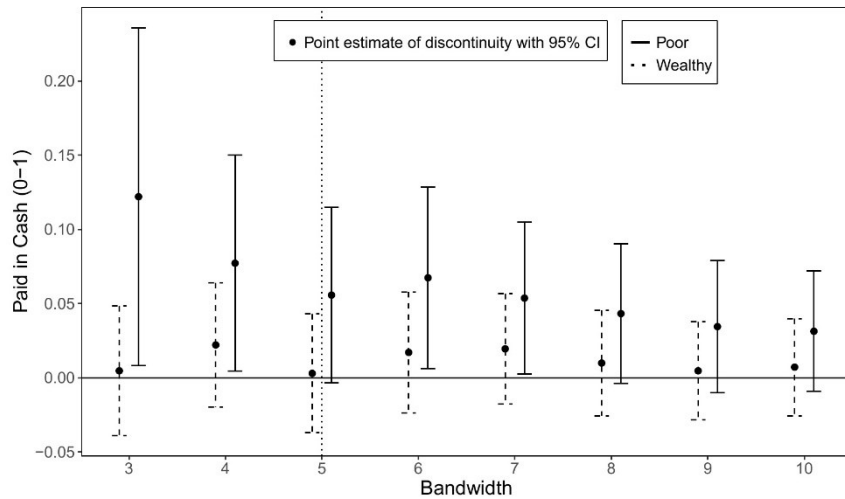
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A10: Second-Stage Estimate by Bandwidth Choice, Works for Third Party (0-1)



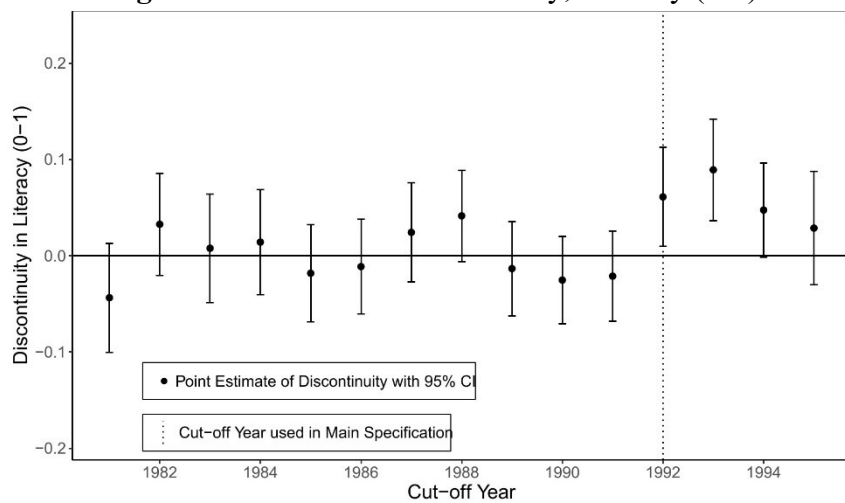
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the IV-estimate of education on the respective dependent variable (on the Y-axis), instrumented by the coefficient on the discontinuity, i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A11: Second-Stage Estimate by Bandwidth Choice, Paid in Cash (0-1)



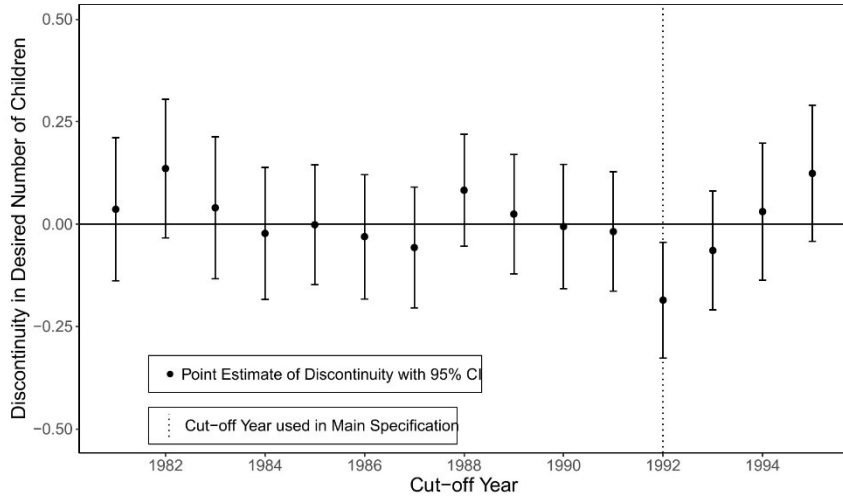
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A12: Placebo Discontinuity, Literacy (0-1)



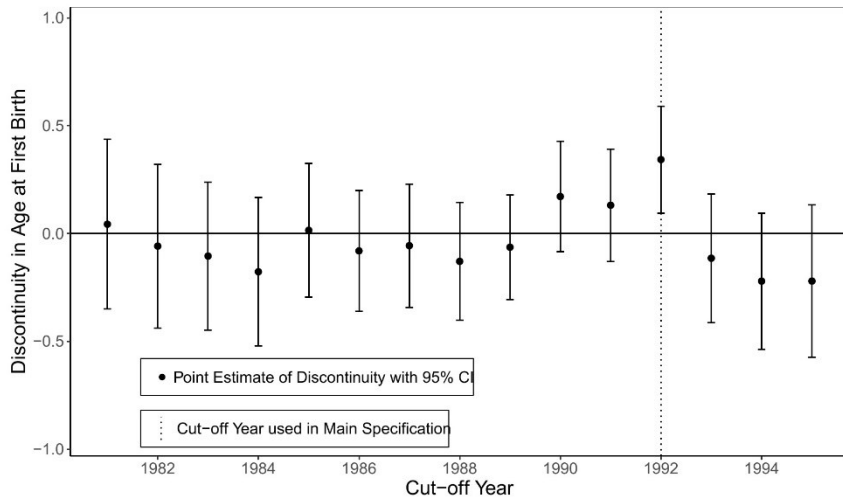
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A13: Placebo Discontinuity, Desired Number of Children



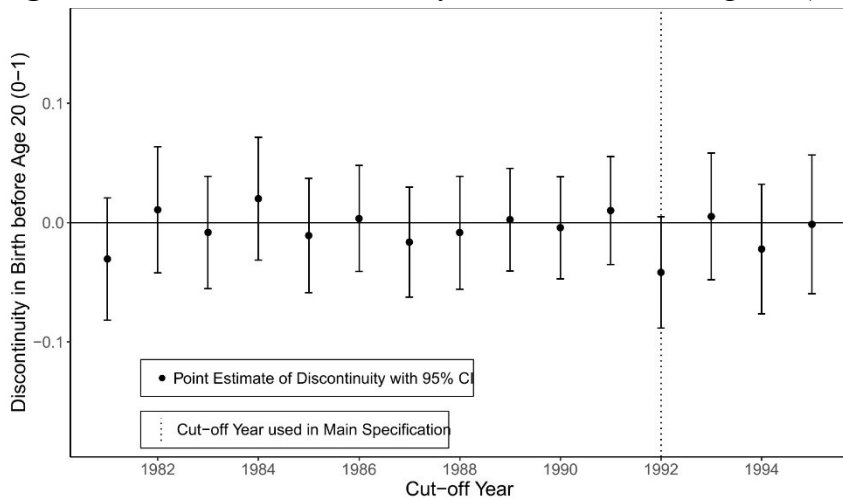
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A14: Placebo Discontinuity, Age at First Birth



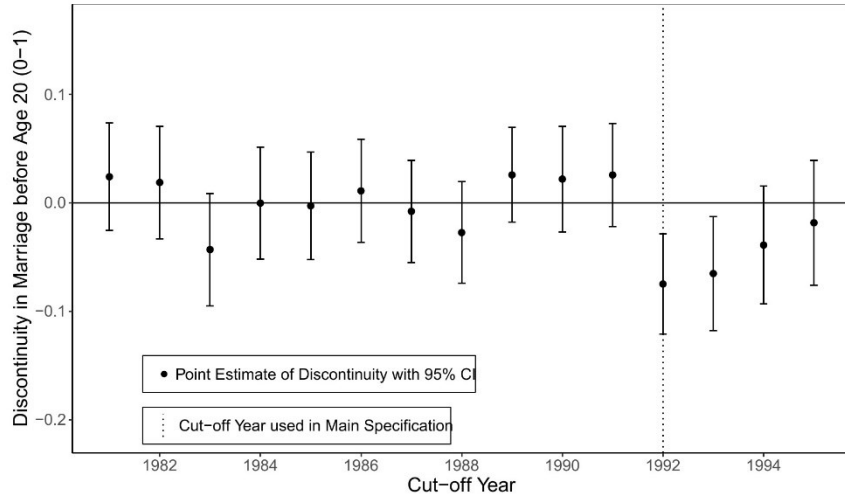
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A15: Placebo Discontinuity, First Birth before Age 20 (0-1)



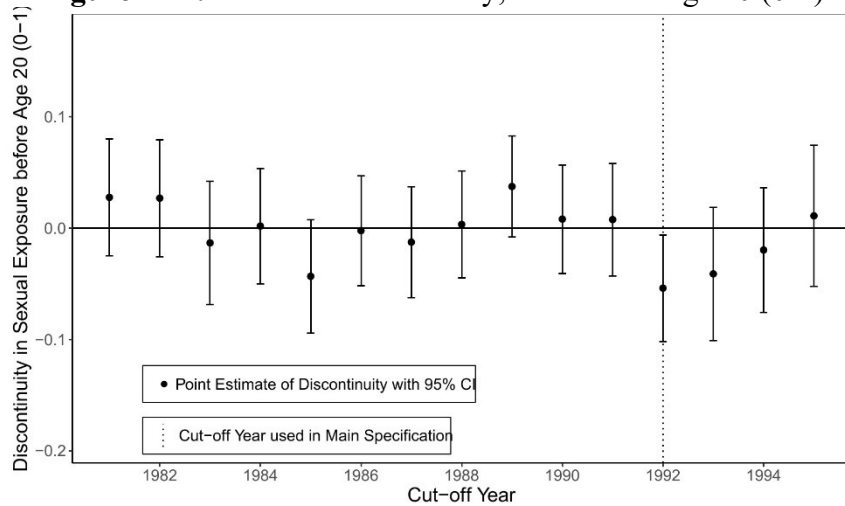
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A16: Placebo Discontinuity, Married before Age 20 (0-1)



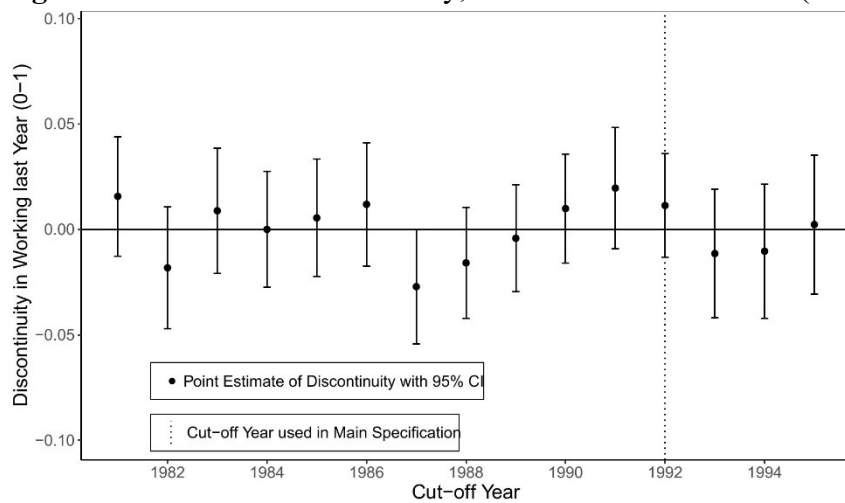
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator "1[Year of Birth = 1992]". Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A17: Placebo Discontinuity, Sex before Age 20 (0-1)



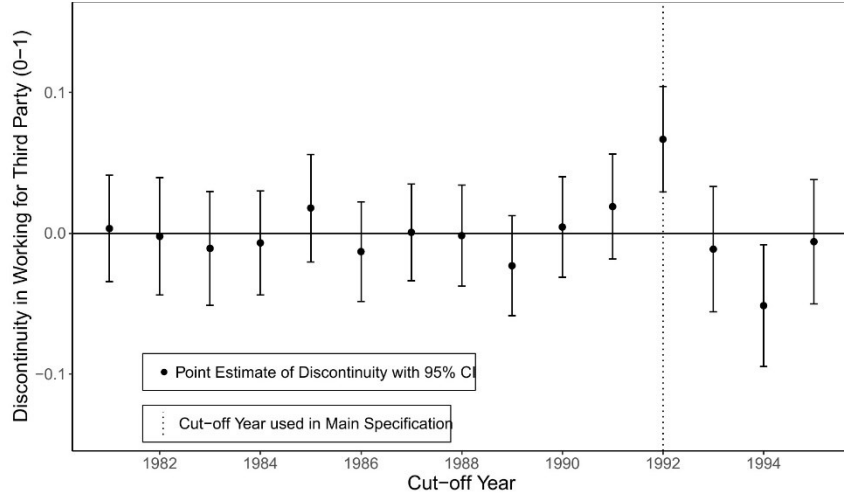
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator "1[Year of Birth = 1992]". Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A18: Placebo Discontinuity, Worked in the Last Year (0-1)



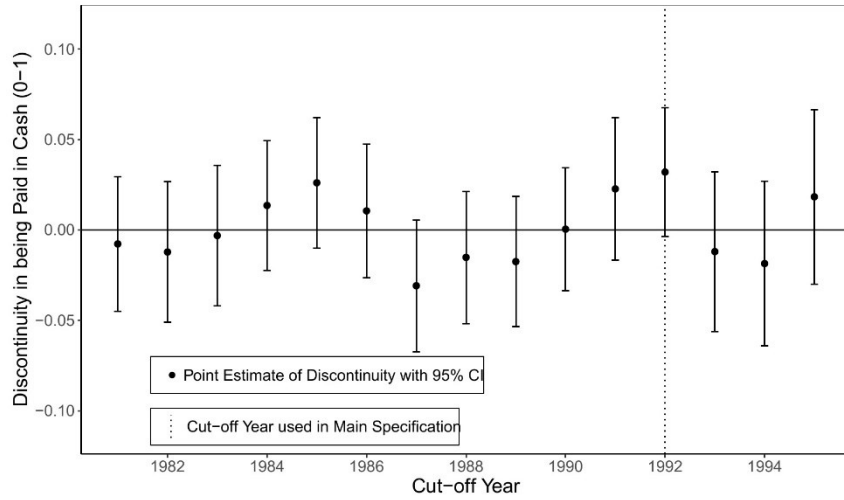
Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator "1[Year of Birth = 1992]". Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A19: Placebo Discontinuity, Works for Third Party (0-1)



Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.

Figure A20: Placebo Discontinuity, Paid in Cash (0-1)



Notes: Data from the 2010/2011 and 2016/2017 Burundi DHS Female Surveys. Respondents are restricted to age 20 and older. Weighted using DHS sample weights. The point estimate represents the coefficient on the discontinuity of the respective dependent variable (on the Y-axis), i.e. the dichotomous indicator '1[Year of Birth = 1992]'. Estimates for the 'Poor' and 'Wealthy' come from separate regressions using subsamples split at the median household wealth level.