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The Impact of Gender-equitable Interventions on Child Marriage and Early Childbearing

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The Impact of Gender-equitable Interventions on Child Marriage and Early Childbearing

A dissertation submitted in partial satisfaction

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Doctor of Philosophy in Health Policy and Management

by

Pragya Bhuwania

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ABSTRACT OF THE DISSERTATION

The Impact of Gender-equitable Interventions on

Child Marriage and Early Childbearing

by

Pragya Bhuwania

Doctor of Philosophy in Health Policy and Management

University of California, Los Angeles, 2022

Professor Jody Heymann, Chair

Child marriage and early childbearing disempower girls and severely impact maternal and child health. These practices often occur in the context of gender inequality and overlapping deprivations. Enhanced access to secondary schooling, economic support to girls and their families, and fostering an enabling legal and policy environment might help address these issues. While a few cross-sectional surveys and small-scale randomized experiments suggest these strategies hold promise, their impact on a national scale across multiple settings is largely unknown. Given their continued high incidence, analyzing the feasibility of effective large-scale interventions remains crucial. This dissertation comprises three research papers, each looking at a policy, program, or legal intervention that has the potential to advance progress on these outcomes.

The first paper examines whether making secondary school tuition-free delays marriage and childbearing. I exploit the natural variation in the timing of policy rollout in three Sub-Saharan African countries that extended tuition-free policy from the primary to the

secondary level. Using the difference-in-differences strategy and the Demographic and Health Surveys data, I observe significantly large reductions in the probability of marriage and childbearing before ages 15 and 18 associated with making secondary school tuition-free. The findings show it is important to support tuition-free secondary education as a policy instrument to delay marriage and childbearing.

The second paper builds evidence on the effectiveness of conditional cash transfer programs (CCT) by evaluating Apni Beti Apna Dhan (*ABAD*), a statewide CCT program in Haryana, India. Using National Family and Health Survey–4 data, representative at the state level, I compare girls who just missed the program to those who got the program in Haryana to girls born during the same period in Punjab. I find strong evidence in favor of the program's impact in delaying girls' marriage until 18 years, mixed evidence for delaying childbearing and the interval between marriage and childbearing, and no evidence of its impact on schooling and son preference. The findings suggest that while conditioning on marriage to receive cash incentives might delay marriage, cash transfer programs need to be more holistic for girls to realize their human rights fully.

The third paper evaluates whether political quotas in the form of reserved seats for women in national- and subnational-level governing bodies influence child marriage and early childbearing across six countries in Sub-Saharan Africa. Using a difference-in-differences strategy on data provided by the Demographic and Health Surveys, I compare trends in outcomes in each of these countries to trends in similar countries within the region that do not have gender quotas. While I find no impact of gender quota on child marriage and early childbearing in short- to medium-run, the long-run effects exploring its interaction with other policies warrant further investigation.

The dissertation of Pragya Bhuwania is approved.

Arturo Vargas Bustamante

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University of California, Los Angeles

2022

For my parents, Punam and Anil Bhuwania, who inspire me to follow the voice within.

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VITA

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Chapter 1

Introduction

1. Background

Child marriage and early childbearing continue to affect the health and wellbeing of girls across the globe. The United Nations Children's Fund (UNICEF) estimated that in 2014, approximately 720 million women globally had been married before they turned 18; and nearly a third among them had entered into unions before age 15 (UNICEF, 2014). While the practice of child marriage affects boys too, it is far less common; approximately 156 million boys had been married before they turned 18. Not only is the prevalence of child marriage lower among boys, it does not carry the same risks as it does for girls because of social and biological differences (Gastón et al., 2019). Closely associated with child marriage, early childbearing also remains globally widespread. Among women aged 20–24 years during the period 2012–2018, nearly 21% got married or entered a union before they reached 18 and 15% bore a child before they turned 18 (UNICEF, 2019).

Child marriage and early childbearing have long-term consequences for the health and wellbeing of girls. Not only is child marriage a violation of human rights, it impedes girls from realizing their full economic potential (Parsons et al., 2015; Wodon et al., 2017). Research shows that child marriage negatively affects girl's psychological and mental health and puts them at a greater risk of intimate partner violence and sexually transmitted diseases (Amin, 2014; Falb et al., 2015; John et al., 2019; Nour, 2009; Yount et al., 2016). Girls married as minors are less likely to use contraceptives before they had their first child and have multiple unwanted pregnancies, rapid repeat childbirth, and terminated pregnancies (Godha et al., 2013; Kamal, 2012; Raj et al., 2009). Child marriage significantly affects maternal and child health outcomes due to its close association with early childbearing. Early childbearing is not only associated with higher risks of maternal mortality and pregnancy complications, but also poor child health outcomes such as neonatal and infant mortality, preterm birth, low birthweight, malnutrition, and child morbidity and mortality (Conde-Agudelo et al., 2005; Efevbera, 2017; Hobcraft et al., 1985; Marphatia et al., 2017; Raj, 2010; UNICEF, 2014).

Given the severe consequences associated with these practices, women's age at marriage and childbearing have been the focus of development strategies across the world. This is reflected in the adoption of eliminating child marriage as a target to realize Sustainable Development Goal 5 to achieve gender equality and empowerment of women and girls. While laws prohibiting child marriage exist in most nations today, evidence suggests that they are inadequate to deter its practice due to inconsistent legal standards such as allowing early marriages with parental consent, under customary laws or other circumstances such as pregnancy, or on religious grounds (Raj et al., 2018; Maswikwa et al., 2015). Even in some countries with comprehensive legislations, laws are not always enforced or implemented locally resulting in child marriage being the norm (Chandra-Mouli et al., 2013). While laws prohibiting child marriage lay a necessary foundation, in order to achieve these development goals within the desired timeframe, effective approaches that build on this foundation are also needed.

Child marriage and early childbearing often occur in the context of gender inequality and overlapping disadvantages. Low- and middle-income countries (LMICs) bear a disproportionately large burden. Over 60% of the child brides reside in South Asia and Sub-Saharan Africa—regions with persistent gender inequalities. Individuals and households make marital and fertility choices based on their socio-economic circumstances such as poverty, poor education, economic opportunities, and restrictive social norms and expectations. Young girls face constraints in deciding the timing of their marriage and fertility when gender inequality intersects with each of these socio-economic deprivations.

In an analysis of over 40 LMICs, child marriage was found most common among girls from the poorest 20% of the households and among girls with no education as compared to those with primary or higher education (UNICEF, 2005). In many societies, early marriage and childbearing is deeply steeped in restrictive gender norms. Girls raised in societies where the role of wife and mother are central to women's identities and where premarital sexual activity is stigmatized due to the fear of dishonor and shame it might bring to the family are likely to marry earlier (Gage, 1998; Mathur et al., 2003). Social practices such as dowry further encourage early marriages as families have to pay less dowry for younger girls (Klugman et al., 2014).

While individuals make decisions regarding marriage and childbearing, laws and policies can influence behaviors by changing the perceived costs and opportunities that households face. Over time, the legal and policy framework also shapes cultural norms and attitudes. There is extensive evidence on how gender inequality and multiple deprivations adversely affect the health and wellbeing of young girls, yet there is limited research on effective at-scale solutions. Causal evidence on effective interventions come from a large number of small-scale randomized controlled trials conducted across diverse demographic, cultural, and economic settings within LMICs. This makes it difficult to generalize the results and take these interventions to scale. The World Health Organization (WHO) recommends establishing evidence on the feasibility and scalability of effective interventions that protect adolescent girls from early marriage and childbearing as a research priority (Chandra-Mouli et al., 2013).

In this dissertation, I use quasi-experimental research methods to evaluate the effectiveness of large-scale interventions in delaying marriage and childbearing outcomes in

LMICs. The dissertation comprises three research papers, each looking at a policy, program, or legal intervention that has the potential to advance progress on these outcomes by targeting the intersectionalities between gender and socio-economic circumstances. The first paper seeks to estimate the effect of tuition-free secondary education policy on the probability of child marriage and early childbearing in Sub-Saharan Africa. The second paper builds evidence on the effectiveness of conditional cash transfer schemes (CCT) in delaying marriage and childbirth by evaluating Apni Beti Apna Dhan (*ABAD*), a statewide CCT program in Haryana, India. The third paper studies how governance and women's political leadership can influence child marriage and early childbearing by estimating the effect of introducing reservations for women in national parliaments and subnational governing bodies across Sub-Saharan Africa.

2. Conceptual Framework

Drawing upon the WHO's social determinants of health (SDOH) model (Solar and Irwin, 2010) and UNICEF's model on understanding child marriage and adolescent pregnancy (Bajracharya et al., 2019), I build a conceptual framework as shown in Figure 1–1. The framework illustrates how social determinants of health shape decisions regarding marriage and fertility, thereby influencing the health of women and their children. Reading the figure from left to right, we see that the socioeconomic and political context defines an individual's socioeconomic position by generating or reinforcing social stratification in the society. It is this context, the structural mechanisms, and the resultant socioeconomic position that are together referred to as the social determinants of health. They influence decisions regarding the timing of marriage and childbearing through a set of more proximal household or individual determinants referred to as the intermediary determinants. Marital and fertility timing in turn influence women's and children's health. In this model, the health system itself is an intermediary determinant that mediates the relationship between these decisions and health outcomes by directly addressing the differences in exposures and vulnerabilities through more equitable access to care.

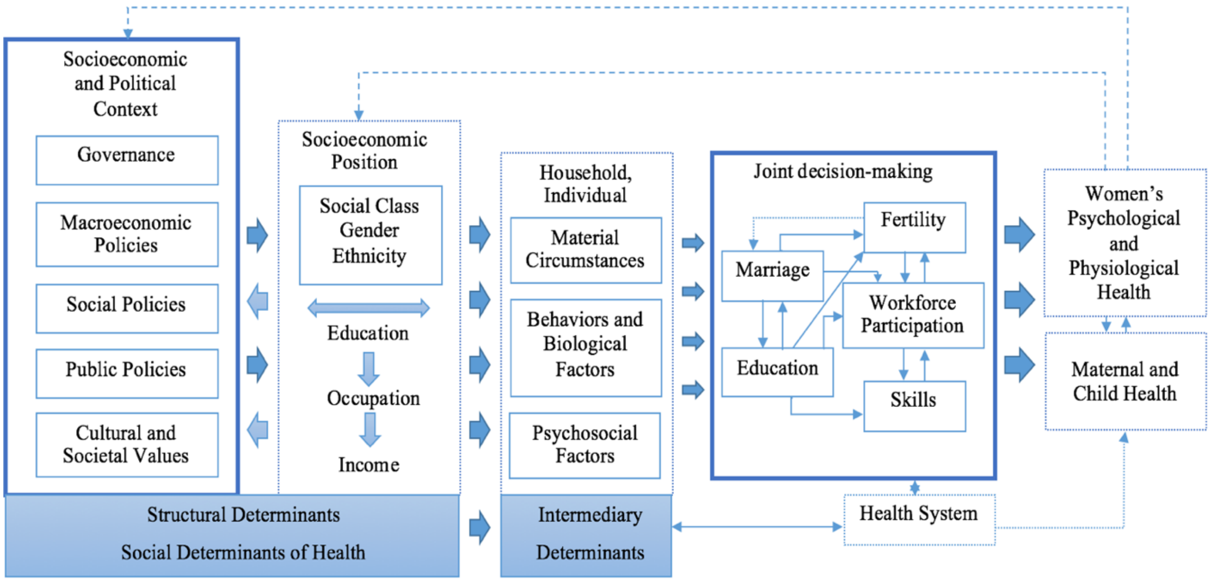


Figure 1–1: Social Determinants of Health Framework for Child Marriage and Early Childbearing.

Notes: Modified from Solar and Irwin (2010) and Bajracharya et al. (2019)

The context encompasses all social, economic, and political mechanisms that generate, maintain, or reconfigure social hierarchies, including political institutions, the labor market, social welfare schemes, education policy, and cultural norms. However, social processes shape the unequal distribution of these determinants between groups occupying unequal social positions in the society. Gender, ethnicity, education, occupation, and income are indicators of this social hierarchy. These structural determinants affect health through intermediary determinants such as housing, financial means to buy necessities, stressful living circumstances such as debt or lack of support, and biological factors such as age at menarche. In this model, I assume marriage and education as competing outcomes and therefore they are determined jointly. While fertility choices closely follow marriage, the reverse arrow indicates fertility decisions made outside of marriage. Girls and women bear the major burden of negative consequences of gender-based discrimination through unequal access to resources. The removal of gender-based inequalities is critical to achieving gender equity in health. In this dissertation, I focus on interventions that have the potential to influence the timing of marriage and childbearing by targeting the intersections between socioeconomic context and gender-based social hierarchy.

In the first paper, I study tuition-free secondary education policy, a public policy that increases equal opportunity in education. While gender equality is not its stated objective, it has the potential to benefit more girls who disproportionately bear the burden of cost-related barriers to schooling. There are several potential pathways through which schooling might affect early marriage and fertility outcomes. First, in communities where parents view education as a viable alternative to marriage but are constrained by high costs, provision of affordable primary and secondary schooling might prove effective in delaying marriage. Second, more schooling might empower girls with information, skills, and social networks that enable them to negotiate decisions regarding their marriage. Third, higher retention of girls in school might reach a critical mass that alters the marriage market and relieves the pressure of early marriage in tipping point communities (Botea et al., 2017). Fourth, better opportunities in the labor market that result from higher educational attainment might encourage individuals to choose further schooling as a better investment over marriage.

The second paper focuses on the *ABAD* conditional cash transfer program, a social policy that has gender discrimination at the heart of its conception. It provides households with cash if girls remain unmarried till they turn 18 with further economic incentives for birth registration and education of girls. CCTs targeting child marriage have the potential to delay marriage and childbearing by increasing the opportunity cost of marrying earlier. CCTs might change the behavior by providing additional income to poor households who otherwise find it difficult to invest in their daughters. Moreover, it might help offset the economic incentive poor households have in marrying their daughters early. Second, *ABAD* gave out bonus payments to girls who attended school till grade 5 and 8. The additional years of schooling might itself change the preferences of girls and their parents as education is known to be associated with delays in marriage and childbearing. Third, a statewide program investing in the future of the girl child might change the attitudes of parents to be more gender equitable and strengthen future aspirations. Fourth, in the long-run, it can change cultural norms within the community if the proportion of eligible beneficiaries is high enough to reach a critical mass.

Lastly, the third paper looks at reservations for women in national parliaments and subnational governing bodies that directly targets unequal gender representation in governance. Greater presence of women in the government might influence the spending priorities in favor of their own gender. Women lawmakers are more likely to support laws and policies that target the health and wellbeing of girls and women. Greater investments addressing the needs of women and passing of laws that increase equal opportunity across gender might delay marriage and childbearing outcomes. Furthermore, equal political representation can shape gender-related attitudes and community norms in favor of women. It might help break gender stereotypes and change the aspirations of girls and their parents.

3. Conclusion

The global rates of child marriage and early childbearing continue to remain high despite significant declines over the past decade. The WHO recommends eliminating marriage before 18 and childbearing before 20 for advancing adolescent sexual and reproductive health and rights (WHO, 2018). Currently, no region is on track to meet the SDG target of eliminating child marriages. Global progress needs to be twelve times faster than the observed declines over the past decade to meet this target (UNICEF, 2018).

Therefore, it is essential to build evidence in favor of large scale interventions that can successfully target these outcomes across LMICs. Furthermore, existing interventions need to be taken to scale. Through this dissertation, I intend to contribute to the field of Health Policy and Management by adding to the existing evidence base on effective strategies and approaches to reduce child marriage and childbearing that are generalizable, replicable, and scalable across LMICs.

I examine three interventions that work at the intersections between gender and social disadvantages, such as low education, poverty, and restrictive norms. The Lancet Series on Gender Equality, Norms, and Health identifies greater attention to the social determinants of health, including gender, as critical to achieving the Sustainable Development Goals. Furthermore, I draw upon the experiences of LMICs across regions. The first paper examines the effect of tuition-free secondary education policy in Sub-Saharan Africa. The second paper evaluates Apni Beti Apna Dhan (*ABAD*), a CCT program, in India. Lastly, the third paper determines the effect of reserved seats for women at national and sub-national levels of governance across Sub-Saharan Africa. This body of work will help determine effective ways to accelerate progress on child marriage and early childbearing, which is necessary to advance the health and well-being of women and their children.

Chapter 2

The Impact of Tuition-Free Education Policy on Child Marriage and Early Childbearing: Does Secondary Education Matter More?

1. Background

Education represents a potentially promising area in which new approaches can be pursued to delay marriage and fertility among young girls. Substantial evidence shows an inverse association between educational attainment and child marriage: girls who are married young often have less education and fewer economic opportunities (Parsons et al., 2015; Raj, 2010; UNICEF, 2005). However, it is difficult to establish causality due to the bidirectional nature of this relationship: on the one hand, being out of school puts the girl child at risk of early marriage; and on the other, girls drop out of school because of early marriage and pregnancy. Evidence shows that early marriage serves as a conduit for transmission of norms and beliefs that discourage girls' education (Asadullah & Wahhaj, 2019). Furthermore, education and marital decisions often take place in the presence of overlapping vulnerabilities such as poverty, unequal opportunities in the labor market, rural residence, and societal and cultural norms (Marphatia et al, 2017; Nour, 2006; Raj, 2010; WHO, 2003). Therefore, while many studies have been conducted on associations between education and the timing of marriage and fertility, not many have been able to control for the reverse causality or endogeneity of education.

Moreover, most studies covered in systematic reviews of the impact of educational programs on child marriage and adolescent childbearing are associational and not longitudinal (Hindin et al., 2017; Kalamar et al., 2016; Lee-Rife et al., 2012; Malhotra et al., 2011). The limited existing causal evidence in favor of investments in education to delay marital and fertility outcomes comes from rigorous program evaluations. First, the Berhane

Hewan program in rural Ethiopia reduced the prevalence of child marriage among girls aged 10–14 but, increased the likelihood of getting married among girls aged 15–19 (Erulkar & Muthengi, 2009). The program had three components, including community mentorship, economic incentives to remain in school, and a livelihood training program. However, not only did the study have a small sample size and some baseline differences between the control and intervention groups, but it is also difficult to attribute the effects to schooling alone because of its multiple program components.

Second, in a randomized evaluation in Kenya, Duflo et al. (2015) found that among girls already enrolled in grade 6 (14 years old on average) at baseline, distribution of free school uniforms led to a decline of 1.4 percentage points in the likelihood of being married and 1.5 percentage points in the likelihood of childbearing. Third, in a randomized trial in Zimbabwe, the odds of marriage were 53% lower after 5 years among girls (aged 12 years at the time of intervention) who received school fees, uniforms, supplies, and a program monitor relative to the control group (Hallfors et al., 2015). Both these studies were conducted within a few selected treatment areas and reached a limited number of beneficiaries, raising concerns over their generalizability, scalability, and sustainability beyond the short time frame of the intervention.

Finally, girls who won school voucher lotteries in Colombia were less likely to get married or cohabit as teenagers (Angrist et al., 2002). The Colombian government established the PACES program on a national scale to partially cover the cost of private secondary school for low-income students. The majority of the participating private schools, however, were in large urban areas, where they are generally concentrated. While the study used the lottery system as a randomized natural experiment to provide causal evidence, it was confined to lottery applicants with access to a telephone in the two largest cities of Colombia. The generalizability of results to more resource-constrained rural areas has not been tested.

While these studies provide evidence on particular interventions, the potential of such interventions to reduce early marriage and childbearing at a national scale is unknown (Lee-Rife et al., 2012). All of these programs were implemented sub-nationally within select treatment areas. These local approaches remain important; however, at the same time, analyzing nationally implemented approaches is also crucial.

The policy of tuition-free education adopted increasingly by many countries has the potential to delay marriage and fertility at a national scale, as such policies are likely to impact a sizeable school-age population while working with national institutional actors such as national education systems. Estimating the impact of the nationwide elimination of primary school tuition fees in eight Sub-Saharan African countries, Koski et al. (2018) found reductions in child marriage in most countries despite implementation challenges. Building upon the evidence in favor of reducing the costs of primary education, I hypothesize that further gains may be realized by reducing barriers to secondary education, especially among older girls who get married because they can no longer afford to remain in school.

Girls may benefit more from free schooling at the secondary level than at the primary level alone because child marriage and early childbearing often occurs during the years they are supposed to be in secondary school. Secondary schooling is also likely to improve labor market prospects more than primary schooling, thus incentivizing school enrollment and attendance over marriage. Jain and Kurz (2007), in their analysis of 20 hotspot countries with a high prevalence of child marriage, found secondary education to be the most important factor associated with a reduction in prevalence, while primary education was most important for younger girls who marry at an early age.

Theoretically, there are several pathways through which schooling may affect early marriage and fertility outcomes. First, in communities where parents view education as a viable alternative to marriage but are constrained by high costs, the provision of affordable primary and secondary schooling might prove effective in delaying marriage. Second, more schooling might empower girls with information, skills, and social networks that enable them to negotiate decisions regarding their marriage. Third, higher retention of girls in school might reach a critical mass that alters the marriage market and relieves the pressure of early marriage in tipping point communities (Botea et al., 2017). Fourth, better opportunities in the labor market that result from higher educational attainment might encourage individuals to choose further schooling as a better investment over marriage (Jensen, 2012).

While most low- and middle- income countries abolished primary school tuition fees in the run-up to achieve universal primary education, the second of the United Nations Millennium Development Goals, there is considerable variation among countries when it comes to extending tuition-free policy to secondary education (Bose et al., 2019; Heymann, Raub, et al., 2014; Heymann, McNeill et al., 2014). In this study, I use quasi-experimental methods to estimate the effect of tuition-free secondary education policy on the likelihood of girls marrying and bearing children before 15 and 18 years of age. I estimate the effect of tuition-free education by using a difference-in-differences (DID) estimation strategy that exploits the natural variation in the timing of policy rollout in three Sub-Saharan African countries that extended tuition-free education policy from primary to lower secondary education.

2. Methods

Researchers widely use quasi-experimental research designs to estimate the causal effect of interventions in situations where randomized controlled experiments are not logistically feasible or ethical. I use one such method, the DID estimation strategy, which is designed to isolate the effects of an intervention in treatment groups by comparing them to control groups over the same time period. Because many low- and middle-income countries adopted tuition-free primary and secondary education policies at various points in time, the DID method offers us a way to compare changes in outcomes within countries that differ only in the timing of policy rollout but are otherwise similar to each other. Unless other policies exactly coincide with the timing of tuition-free education policy, the impact estimates represent the additional effect of tuition-free primary education over and above any existing relevant policies such as minimum age marriage laws. Furthermore, in countries that later extended tuition-free policy to the secondary level as well, this method allows me to estimate the additional gains of extending tuition-free policy beyond primary.

2.1. Data

Policy Data

I gathered longitudinal information on education policy in all countries from reports on education systems provided by the United Nations Educational, Scientific and Cultural Organization (UNESCO) International Bureau of Education (IBE). I then corroborated this information using UNESCO's data on the right to education and legislative documents found on each country's official website. I considered schooling to be free at a given level if no tuition fee was charged. However, it is worth noting that other miscellaneous fees—including for books, school supplies, uniforms, or lunch—may have been in effect. I determined the exact year of policy change in one of the following ways: either it was explicitly mentioned in the reports consulted or, in the absence of this information, I considered it to be the beginning of the next academic cycle immediately after the adoption of a new law or policy. I used the same national reports to identify which grades were classified under the primary and secondary education levels, as well as the expected age at which students attended each level. For this study, I determined exposure as a function of birth year and country of residence. The country of residence provides me with two relevant pieces of information: the year in which tuition-free education policy came into effect and the age at which girls typically start primary and lower secondary schooling as per their country's education system. These two variables, in turn, give me the birth year of girls who were the first to be exposed to the policy. In other words, I consider a respondent exposed if she reached the expected age of entry to school in the same year, or after, the tuition-free policy came into effect in her country of residence.

Outcomes Data

I obtained data on age at marriage and age at first childbirth from Demographic and Health Surveys (DHS) conducted in each country between 1986 and 2016. Typically conducted every five years, the DHS collects nationally representative data on health and population outcomes—including detailed information on girls' and women's health, education, and reproductive history—in over 90 low- and middle-income countries. For my analysis, the DHS provides an opportunity to study girls' and women's marital and fertility outcomes over time across many different countries. The survey asks all female respondents between ages 15 and 49 in a household to report their age at the time they were first married or began living with a partner. Similarly, it asks them to report their age at the time they had their first child.

Using this information, I created four binary variables indicating whether someone was married before 15 or before 18 years of age and whether she had her first child before 15 or before 18 years of age. I hypothesize that abolishing tuition fees at the primary and secondary levels will have differential effects on girls across the age spectrum, wherein secondary education is more likely to impact those towards the higher end. Therefore, using two age cut-offs will help me estimate differential policy effects for relevant age groups.

I pooled data from all available DHS waves conducted between 1986 and 2020 in each country to create a longitudinal dataset that included individuals born between 1970 and 2001. To estimate the effect of policy change on marriage or childbirth before the age of 15, I used information from all girls and women. As for marriage and childbirth before age 18, I included all those who were interviewed when they were between 18 and 49 years old. I excluded girls who were not at least 18 because at the time of interview they have only partial information on marital and fertility timing— those who were not yet married or had their first child have a missing information problem on this variable because I cannot determine whether they will marry or have their first child before the age of 18 until they turn 18).

Country Selection

Many low- and middle-income countries in Sub-Saharan Africa have adopted policies to eliminate tuition fees at the secondary level of schooling. However, most of these countries made both primary and secondary school free in the same year, making it difficult to parse out the effect of extending tuition-free education beyond primary schooling. Because all girls residing in such countries either have no tuition-free policy at all or have access to tuition-free schooling all the way up to secondary level, I cannot determine the unique contributions of primary and secondary tuition-free policies. Another potential issue with the simultaneous adoption is that a few birth cohorts would only be partially exposed to tuition-free policy. These girls were expected to enter secondary school when secondary was made free but were required to pay tuition at the primary level while tuition was still charged, forcing many to drop out before they even reach secondary. Consequently, it is difficult to determine exposure to tuition-free policies in this case. Therefore, to answer the question of what countries stand to gain from extending tuition-free policy beyond primary, I need the treated countries to comprise those that moved sequentially from not having a tuition-free education policy to abolishing fees at the primary level, and then later extending it to the lower secondary level.

I identified three countries in Sub-Saharan Africa—Liberia, Tanzania, and Uganda—that had a sufficient time lag between the rollout of tuition-free policy at the primary and secondary levels of schooling and for which sufficient longitudinal data on age of marriage and childbearing were available. This lag made it possible to have information on three categories of women: those who were not exposed to tuition-free education policy, those who were exposed to free-primary policy alone, and those who were exposed to free-secondary policy as well. This is true for Uganda and Liberia. For example, Uganda abolished fees for primary school in 1997 and lower secondary school in 2007. Girls in Uganda typically start primary and lower secondary school at ages 6 and 13, respectively. Therefore, girls who were born in the years 1991 to 1993 were expected to be exposed to tuition-free primary policy alone, whereas girls born in 1994 or later benefited from tuition-free secondary policy as well.

The DHS surveys in Tanzania, on the other hand, did not interview girls expected to benefit from tuition-free secondary policy; therefore, Tanzania serve as a treated group for primary-free policy alone. Finally, among all other countries with tuition-free primary and secondary policies, I identified Burkina Faso and the Democratic Republic of Congo (DRC) as countries where expected beneficiaries of any tuition-free policy were too young to be interviewed by the latest DHS surveys (the DHS interviews women aged 15–49 years only). Consequently, these countries serve as good controls, even though they adopted tuition-free primary and secondary together, because this adoption takes place subsequent to the years studied.

These countries are also similar to each other in terms of marriage customs such as bride price (Corno et al., 2020). Furthermore, in the 1980s, the gross enrollment ratio in secondary school was substantially lower than that of primary school in all the study countries and ranged from 3% to 24% (Roser & Ortiz-Ospina, 2013). Girls constituted 27–36% of all the pupils enrolled in secondary school (Roser & Ortiz-Ospina, 2013). Table 2–1 presents the years in which fees were abolished at both primary and lower secondary levels of education, the expected age at which students attend each level, and details of the birth cohorts expected to be exposed to the policy in each of the five countries in the sample.

2.2. Empirical Strategy

In this paper, I use the DID identification strategy to estimate the effect of tuition-free secondary education policy on the probability of early marriage and childbearing among girls in low- and middle-income countries. Widely used for impact evaluations (Angrist & Pischke, 2008; Lechner, 2011; Lee & Kang, 2006), the DID strategy allows me to compare the difference in outcomes before and after an intervention in a treatment group to the difference in outcomes in a control group over the same period. It exploits the natural variation in the timing of policy rollout between countries while accounting for the temporal and spatial differences between them.

There are five countries in my analytical sample with data on cohorts with different exposures. Specifically, Liberia and Uganda have data available for all three possible exposures: neither tuition-free, only primary-free, and both primary and secondary tuition-free. Tanzania has data available for girls who accessed school while neither was tuition-free and those for whom only primary school was tuition-free. Finally, Burkina Faso and DRC serve as controls because they have information available only for those girls who were not impacted by tuition-free policy over the study period. Table 2–2 presents the DHS waves used in the analysis and distribution of the sample across different levels of policy exposure. Among the total women in my sample, 5.23% were eligible for tuition-free secondary education by the time they reached the expected age of secondary school enrollment, while 5.36% were expected to be affected by tuition-free policy at the primary but not the secondary level.

Using the DID framework, I estimate the policy effect using a linear regression model as shown in equation (1):

$$Y_{ict} = \beta_0 + \beta_1 Policy_{ct} + \xi X_{ict} + \gamma_c + \delta_t + \varepsilon_{ict} \qquad (1)$$

where Y_{ict} is the outcome of interest for individual *i* in country *c* in year *t*. γ_c is a country fixed effect that controls for time-invariant differences between countries, δ_t is year fixed effect that controls for secular time trends in the outcome of interest across countries, and X_{ict} represents relevant individual controls. The variable $Policy_{ct}$ represents two dummy variables (primary and secondary, no policy being reference) indicating whether a person living in country *c* and born in year *t* was exposed to tuition-free education policy. The primary dummy equals 1 for each cohort that is exposed to free primary policy but not free secondary policy, and the secondary dummy equals 1 for each cohort that is exposed to both free primary and secondary policy. β_1 represents the vector of our coefficients of interest, measuring differential treatment effects of tuition-free policy at primary and secondary levels of education. Harper et al. (2014) used a similar strategy of multiple policy dummies within a DID framework to capture the differential effect of mandatory seat belt laws with primary or secondary level of enforcement in the United States.

I use Linear Probability Models (LPM) to analyze the impact of the policy change. The LPM coefficients are consistent direct impact estimates in large samples measured in terms of the percentage point change in the probability of an event. LPM is often recommended over Logit or Probit models in a DID setup to avoid the complications associated with the estimation and interpretation of coefficients (Ai & Norton, 2003; Buchmueller & DiNardo, 2002; Cantor et al., 2012). I also report the results from Logistic regression as a robustness check. I do not cluster standard errors by country because clustered standard errors are biased when the number of countries is fewer than 10 (Bertrand et al., 2004). Instead, I report bias-corrected bootstrap 95% confidence intervals. I also conducted all analysis with Huber-White robust standard errors as well to check the sensitivity of my estimates and found that all results remained unchanged (results available on request). I conduct all analyses in STATA 14.2.

I considered the potential use of another quasi-experimental method, the regression discontinuity design (RDD) where treatment and controls are predicted based on whether an observation lies above or below the threshold value of the forcing variable. However, I determined that birth cohorts do not perfectly predict treatment assignment due to grade repetition and early drop out issues; therefore, ruling out a sharp RDD. Furthermore, the 'fuzzy' RDD setup that uses two-staged least squares (2SLS), while instrumenting on birth cohorts, would have been helpful if I were interested in the effect of educational attainment on child marriage and early childbirth outcomes. However, because I am interested in estimating the effect of tuition-free education policy, I determined that the DID study design is more appropriate in this case. Moreover, the DID strategy also allows me to test the sensitivity of the estimates to various treatment assignments using birth cohorts.

My model controls for time-invariant differences between countries as well as secular time trends over the study period across countries. However, as far as individual-level covariates are concerned, I control only for the type of residence (urban/rural) and sex of the household head. I do not control for wealth because the DHS provides information at the time of the survey and not at the time an individual was expected to be in school (I do not include the post-treatment variables in my model because of possible endogeneity concerns). Behrman (2015a), too, raised these concerns while evaluating universal primary education policy in Malawi and Uganda using DHS data; similarly, Koski et al. (2018) did not control for any individual-level covariates in their model within a DID framework while comparing countries in Sub-Saharan Africa using DHS data. Because similar concerns could be raised for residence type, I tested my models by excluding it from the analysis and found the estimates to be stable.

The validity of the estimates is subject to the common trends assumption that treatment countries would have exhibited similar trends in outcomes as the control countries had they not adopted the policy earlier. I formally test for this assumption by regressing the outcomes of interest on treatment status interacted with the time trend during the pre-policy years between 1970 and 1985. The coefficient of the interaction term will inform us whether time trends were similar across treatment and control groups before policy rollout. Moreover, because I included only those Sub-Saharan countries in my analysis that at some point abolished tuition fees for both primary and secondary school, the common trend assumption is more plausible because of similarity among countries in terms of policy adoption. Finally, a common threat to the internal validity of DID estimates is the presence of other relevant policy shifts that might have coincided with the intervention being studied. I document and analyze the timing of minimum age marriage laws with respect to the adoption of tuition-free education policy in all the countries in my analytical sample to evaluate the possibility of confounding of the impact estimates.

2.3. Robustness Checks

A potential threat to my identification strategy is grade repetition and late entry into school because I use the expected age of entry into school to determine policy exposure. It is possible that some girls chose to start primary school later than the expected age or chose to repeat grades and remain in school in response to fee abolition at the primary level. Furthermore, some girls might have completed primary later than they were expected to and thus might have faced the decision of transitioning into secondary school after fee abolition at the secondary level. Therefore, given my current definition of policy exposure, I consider these girls to be unexposed to the policy. This might have potentially led to an underestimation of the policy effect because I assign control status to some girls who might have been treated.

I propose two sensitivity analyses to corroborate my findings. First, to define policy exposure, I use the expected age of exit (as shown in table 2–1) from primary or secondary school, rather than the age of entry. Using this definition will help me correctly assign treatment status to all those girls who could have potentially been affected by the policy. Behrman (2015a) and Tsai & Venkataramani (2015) used the age of exit to define treatment status while evaluating the effect of universal primary education policy. However, there is a possibility of incorrect treatment assignment to girls who might have been too old to be affected by the policy. Nonetheless, in the absence of complete academic histories for respondents in DHS data, this is one way of testing the sensitivity of my results to different definitions of treatment.

Second, I alter the analytical sample by dropping girls who were not in age-appropriate school grades but might still have been exposed to policy change. This approach was adopted by Osili & Long (2008) to evaluate the impact of abolishing primary school fees in Nigeria. However, my study has an additional layer of complexity because it features two groups of girls: those who were exposed to free primary school alone, and those who were exposed to free secondary as well. Consequently, there might be girls exposed to free primary policy due to over-age enrollment in primary school and, similarly, girls exposed to free secondary policy due to over-age enrollment in secondary school. A UNESCO report (2012) showed that grade repetition was declining at both levels of schooling in Sub-Saharan Africa, and highlighted grade repetition itself as an increasing function of tuition-free policy because of a considerable wave of first-time entrants. Therefore, grade repetition is more likely at the primary level than at the secondary level during the pre-policy years. As a result, I drop girls who were susceptible to tuition-free primary policy due to over-age enrollment in primary grades from my analysis (the dropped birth cohorts were 1992–1996, 1989–1994, and 1985–1990 in Liberia, Tanzania, and Uganda, respectively), thus testing the sensitivity of the results to changes in the analytical sample. Dropping these girls from the analytical sample truncates the sample by 12–13%.

3. Results

Table 2–3 reports the marginal effects (difference in predicted probabilities in percentage points) from the LPM models, as shown in equation (1). The first column shows the effect of tuition-free policy at the primary level, and the second column shows the effect of tuition-free secondary policy when compared to no tuition-free policy. Finally, the third column shows the primary estimates of interest comparing the effect of free secondary over the effect of free primary, i.e. the effect of extending the policy from primary to secondary level on marital and fertility outcomes. I find significant improvements associated with tuition-free secondary schooling for all outcomes of interest.

Impact of Tuition-free Primary Education Policy

Abolition of tuition fees at the primary level of schooling reduced the probability of marriage before age 15 by 0.8 percentage points on average, and that of marriage before age 18 by 2.5 percentage points. Although I find no improvements in the probability of childbearing before age 15, primary tuition fee removal did lead to an average decline of 3.7 percentage points in the probability of childbearing before age 18. The average probability of marriage before 15 and 18 years of age in the treated countries before the adoption of tuition-free policies was 15.0 and 50.8, respectively. Similarly, the average probability of childbearing before ages 15 and 18 was 7.1 and 39.3, respectively.

Impact of Tuition-free Secondary Education Policy

As shown in the second column, the abolition of secondary tuition fees reduced the probability of each of these outcomes by a far greater magnitude than tuition-free primary alone. When compared to no tuition-free policy, the abolition of secondary-level tuition fees reduced the probability of marriage before age 15 by 4.1 percentage points on average, and that of getting married before age 18 by 5.5 percentage points on average. Secondary tuition fee removal led to an average decline in the probability of childbearing before age 15 by 1.5 percentage points and an average decline of 8.6 percentage points in the probability of childbearing before age 18.

Finally, I statistically test for the difference in the magnitudes of the estimated impacts of tuition-free primary and secondary policies in the last column of table 2–3. I find that extending tuition-free policy from primary to secondary had significant gains for all outcomes of interest. Tuition-free secondary reduced the probability of marriage before 15 by 3.3 percentage points on average and that of marriage before 18 by 3.0 percentage points on average over tuition-free primary alone. Similarly, the probability of childbearing before 15 reduced by 1.8 percentage points on average following the extension of fee removal from primary to secondary. Lastly, tuition-free secondary reduced the probability of childbearing before 18 by 4.9 percentage points on average over tuition-free primary alone.

Test of Common Trends Assumption

I tested the assumption of common trends in outcomes between treatment and control groups during the pre-policy years by regressing the outcomes of interest on the treatment status and birth cohort during the pre-policy years. I include a product term between the treatment status and birth cohort to determine separate time trends for each category of treated and control groups. I distinguish between the two levels of treatment groups in the sample: the primary and secondary tuition-free group (comprising Liberia and Uganda) and the primary-only tuition-free group (Tanzania). The coefficient of the interaction term was not significant for any of the outcomes except for marriage before 15 for primary and secondary-free treatment compared to control countries, implying that time trends were similar across treatment and control groups before policy rollout for the most part. Figure 2–1 shows the plot of marginal effects stratified by treatment status for each of the four outcomes of interest. It is evident from the figure that although treatment and control groups had different baseline levels of outcomes, the change or trend in outcomes remained similar during the pre-policy period except for the differences in the probability of marriage before age 15.

Examination of Potential Change in Child Marriage Laws

I studied other possible sources of changes, such as minimum age marriage laws, in each of these countries to determine whether the marriage laws might have coincided with tuition-free policy. Figure 2–2 shows the timing of exposure of birth cohorts to minimum age marriage laws and tuition-free education policy in all five countries in the study. It is evident that these laws/policies did not coincide in any of these countries. Among the treated countries, while Uganda and Liberia mandated age 18 as the minimum age of marriage before eliminating tuition fees, Tanzania did not have any changes in the minimum age of marriage during the study period (it mandated age 15 as the minimum age of marriage for girls in 1971). As for the control countries, Burkina Faso introduced the marriage law in 1972 similarly to Uganda (1977); whereas DRC had its marriage law (1991) closer to that of Liberia (1993). It is evident from figure 2–2 that the impact estimates for tuition-free education policy are not driven by changes in minimum age marriage laws during the time period studied.

Sensitivity Analyses

I now turn to the results from sensitivity analyses, as shown in table 2–4. Panel A reports the results from models that use age of exit to define exposure status. In this way, I consider all girls with potential partial exposure who were already in school when tuition-free policy rolled out as treated instead of considering them unexposed to the policy change. I find that when compared to the base category of no tuition-free policy, the effect of both tuition-free primary and secondary polices is similar to my previous findings for all outcomes, with slightly larger effects for marriage before 18. Moreover, the additional effect of extending tuition-free policy from primary to secondary level remains robust to this alternative specification for all the outcomes, suggesting that the policies also impacted girls who were partially exposed to the policy because they were already in school at the time of policy rollout.

Panel B of table 2–4 shows the results from models using a restricted sample that drops girls who are not in age-appropriate school grades but might still be susceptible to policy change. Doing so allows us to compare the girls most likely to be impacted to girls who are the least likely to be impacted for determining the impact of free primary. However, I could not remove the girls who could have been susceptible to secondary free policy while they were in school because that would have led to dropping of girls exposed to primary. I find that doing so results in even larger point estimates for all outcomes except marriage before 18. These results show that the identification strategy is robust to changes in treatment definition as well as the analytical sample.

In Panel C of table 2–4, I test the sensitivity of the estimates to the number of pre-policy years I include in the study. Up till now all results were based on a 15-year pre-policy period from 1970 to 1984. I now reduce that to a 10-year period from 1975 onwards and find that all the estimates remain robust. Lastly, in panel D, I carry out logistic regression instead of the LPM model, and find that all estimates remain intact with slight reductions in the magnitude.

4. Discussion

In this paper, I use a quasi-experimental study design to examine the impact of tuition-free secondary education policy on the probability of girls getting married and bearing children before 15 and 18 years of age. This study is methodologically similar to other studies that use the DID strategy to assess the impact of national policies on health and education at an individual level using large survey data (Koski et al., 2018; McKinnon et al., 2015; Osili and

Long, 2008). The results suggest that the extension of tuition-free education policy from primary to secondary had a significant impact on both the age of marriage and the age of first childbearing.

I found the magnitude of impact for these national policies to be comparable to those found by Duflo et al. (2006), who evaluated similar policies on a subnational level incentivizing access to school by reducing financial barriers for girls in a trial setting. The results were robust to alternative model specifications in terms of different treatment definitions and choice of analytical sample. Moreover, the tests for similar pre-policy trends in outcomes further corroborate my findings that improvements in marital and fertility outcomes were indeed due to the extension of the policy to the secondary level of schooling. Lastly, I did not find any other laws or policies such as minimum age marriage laws or other family planning policies that might have impacted early marriage or childbearing that were adopted at the same time as tuition-free primary or secondary education policy, ruling out other competing explanations for these results.

I make an important distinction between the adoption of tuition-free education policy at primary and secondary levels in the paper. I find that while tuition-free primary policy led to significant changes in the probability of marriage before 15 or 18 and childbirth before 18, the extension of tuition-free policy to secondary level led to significantly greater reductions in all four outcomes. Across various specifications, I find that tuition-free secondary policy consistently had a significantly stronger impact on marital and fertility outcomes than tuition-free primary policy. The greatest impact occurs when public policies make both primary and secondary tuition free. This is noteworthy given the fact that up till now population-based causal evidence on the effect of national tuition-free policies on outcomes related to maternal and child health has only been at the primary level of schooling (Behrman, 2015a, 2015b; Behrman et al., 2017; Koski et al., 2018; Osili and Long, 2008). The evidence for tuition-free education at the secondary level is largely associational. While research suggests girls with secondary or higher education are less likely to enter into a union or bear children before age 18 as opposed to girls with primary education (Gupta and Mahy, 2003; UNFPA, 2012), it is not known whether marriage or childbearing force girls to drop out of secondary school or secondary school attendance protects them against these outcomes. Using longitudinal data, I establish that making secondary schools tuition free delays marriage and childbearing. This is particularly important in the context of the large number of countries that continue to charge tuition for secondary school.

This study suggests that broader gains can be realized from policies that aim to improve the affordability of education beyond primary school. The expected age of starting secondary school in Uganda, Liberia, and Tanzania is 13, 12, and 14, respectively, and it is likely that a considerable majority of girls will face marital and fertility decisions during the period they are supposed to be in secondary school as opposed to primary school. This has important policy implications for other low- and middle-income countries. Policies guaranteeing tuition free secondary school are important to fulfill all youth's right to an education as well as for its importance to work and civic opportunities and long-term income. This study highlights the potential additional importance for achieving improved population health including maternal, reproductive, and child health outcomes.

Schooling expenses represent a significant share of household income in Sub-Saharan Africa. Of the twenty-two Sub-Saharan African countries included in a 1986 study, the cost of sending two children to primary school for the poorest 40% households was above 10% of their household income in fifteen countries and above 20% in five countries (Reddy and Vandemoortele, 1996). The cost of secondary education is even higher than primary education (Bentaouet, 2006; Boyle et al., 2002). Furthermore, tuition fees are the most commonly collected at the lower secondary level compared to other types of charges such as textbooks, uniforms, community fees, and other school-based fees in Africa (Bentaouet, 2006). For the poorest 40% households across nine Sub-Saharan African countries, tuition fees constituted 36% of their total household education expenditures (UNESCO, 2011).

Consequently, many studies find that the abolition of tuition fees leads to significant increases in enrollment, attendance, and educational attainment among girls (Behrman et al., 2017; Bentaouet, 2006; Deininger, 2003; Gaddah et al., 2016; Osili and Long, 2008). Education and marriage or childbearing are usually considered time-incompatible as women tend not to marry or bear children while in school. Therefore, the additional time spent in school as a result of the removal of tuition fees has the potential to delay marriage and fertility decisions. Furthermore, greater learning and social networks resulting from additional schooling may empower girls to better negotiate these decisions within the household.

This work also highlights the need to disaggregate adolescent childbearing data by mother's age. Not only do births before the age of 15 pose significantly higher risks for the mother and the child relative to later adolescent births (Conde-Agudelo, 2005; Phipps and Sowers, 2002), I find differential policy effects on early versus later adolescent births. While tuition-free primary reduces the likelihood of childbearing before age 18, I find no effect on childbearing before age 15. However, it is encouraging to see that girls respond to tuition-free secondary for both of these age cutoffs. Yet, most studies examine adolescent childbearing and other sexual behavior outcomes before age 18 or by the end of the follow-up survey or study period in the case of control trials (Baird et al., 2010; Hindin et al., 2016; Duflo et al., 2015). Nearly 2.5 million births occur to girls under age 16 every year in low-resource countries (Neal et al., 2012). Therefore, it is crucial to examine strategies that work for these girls to make progress on reproductive health outcomes.

Research points out that the youngest among adolescent child-bearers are made the most vulnerable owing to poverty, less opportunities for educational attainment, residence in rural areas, and living within the context of differing cultural patterns and practices (Gausman et al., 2019; Neal et al., 2012, 2020). A few studies highlight the important gender norms and social barriers in determining child marriage and early childbearing outcomes (Amin et al., 2016; Heymann et al., 2019). Gausman et al. (2019) show that while low education and poverty are consistently associated with adolescent childbearing, local context matters even after controlling for the socioeconomic disadvantage and that it matters more for the poor. Amin et al. (2017) argue that school-going changes to the extent that it does not conflict with gender norms and approaches that target education alone might have limited influence on child marriage and early childbearing in such settings.

This study has several limitations worth mentioning. First, I restricted the analysis to five countries in Sub-Saharan Africa because these were the only countries that met my policy and outcomes data requirements. All other Sub-Saharan African countries with sufficient DHS data either made primary and secondary education tuition-free at the same time or extended the policy soon enough such that secondary was tuition-free by the time the first cohort of tuition-free primary beneficiaries were expected to enter secondary school. Therefore, it is not possible to parse out the effect of secondary policy from that of primary for other countries. An alternative strategy could have been to compare countries with tuition-free primary and secondary policies to countries that have only primary as tuition-free. However, unlike my adopted strategy where treatment and control countries differ only in terms of the timing of policy rollout; in this case, the treatment and control countries fundamentally differ in prioritizing tuition-free education, thus making the internal validity of the study weaker. Another difficulty with the above strategy is that there is no way to determine the exposure status of all those girls who were expected to enter secondary school after it was made free but missed out on primary school while it was still charged.

I checked extensively for other changes such as marriage laws or family planning initiatives in these countries during the study period. All five countries ratified the Convention on the Rights of the Child (CRC), which emphasizes 18 as the minimum age of marriage, between the years 1990 and 1993. All countries except Burkina Faso have sexual offense laws in place, although their implementation and effectiveness are often considered questionable. It is the minimum age marriage laws that are of greatest relevance to this study, and I showed in detail that these do not coincide with the timing of tuition-free education policy in any of the countries. Given the fact that minimum age marriage laws preceded tuition-free education policy, I am fairly certain that my estimates show the additional effect of education policy.

Second, because DHS data do not include information on the exact year in which girls begin primary and secondary school, I used the expected, rather than actual, age of enrollment to define treatment status. This might have led to misclassification of over-age girls or those who repeated grades as control observations, potentially leading to an underestimation of the policy effect. Nonetheless, the sensitivity analyses indeed show that the results are robust.

Third, my work provides evidence in favor of the potential of the removal of tuition fees at the primary and secondary levels of schooling. However, there could be other costs related to books, school supplies, uniforms, and meals that are not captured by the study. While these costs can be great obstacles to education, tuition-fee elimination is a critical first step to achieve universal education. The abolition of tuition fees has shown significant promise in leapfrogging enrolment rates in varying degrees in almost every country depending on its scope and substance (The World Bank, 2009). Therefore, even though it is important to study other costs, estimating the effects of tuition-fee elimination is important on its own. Finally, the study examines the benefit of tuition free education policy in countries that already have laws prohibiting child marriage. It cannot be generalized to countries without child marriage laws.

5. Conclusion

This work contributes to the small body of evidence on policies that may help accelerate progress on eliminating child marriage and reducing early childbearing. The study demonstrates tuition-free secondary education policy could reduce both. It adds to the actionable evidence base by examining a policy that was implemented on a national scale and is replicable at a national scale in other countries. Nevertheless, future research to better understand the potential of multipronged approaches that focus on the local context, gender norms and attitudes, and social barriers might be particularly helpful.

Tables and Figures

Table 2–1: Exposure of birth cohorts to tuition-free primary and secondary policies.

Country	Free-Primary Policy Year	Primary School Age Range	Birth Cohorts Exposed to Free Primary Policy Only	Free-Secondary Policy Year	Lower Secondary School Age Range	Birth Cohorts Exposed to Free Secondary Policy
Treated Countries						
Uganda	1997	6–12	1991–1993	2007	13–16	1994 onwards
Liberia	2003	6–11	1997–1998	2011	12–14	1999 onwards
Tanzania	2002	7–13	1995–2001	2016	14–17	2002 onwards[1]
Control Countries						
Burkina Faso	2007	6–11	2001 onwards[2]	2007	12–15	1995 onwards[1]
DRC	2015	6–11	2009 onwards[2]	2015	12–14	2003 onwards[1]

[1] These birth cohorts were expected to be the first beneficiaries of tuition-free secondary policy; however, they were not interviewed in the currently available DHS.
[2] These birth cohorts were expected to be the first beneficiaries of both tuition-free primary and secondary policies, however, they are not interviewed in the currently available DHS.

Table 2–2: DHS waves used for the analytical sample and sampling distribution of women aged 15 to 49 at the time of interview [a].

Country	DHS waves used	Sampling Distribution		
		No exposure	Primary exposure	Secondary exposure
Treated Countries				
Uganda	1989, 1995, 2001, 2006, 2011, 2016	29,152	3,576	7,589
Liberia	1986, 2007, 2013, 2020	18,307	1,199	2,059
Tanzania	1992, 1996, 1999, 2005, 2010, 2016	34,423	3,567	0
Control Countries				
Burkina Faso	1993, 1999, 2003, 2010	28,081	0	0
DRC	2007, 2014	25,134	0	0
Total (N=153,087)		135,097	8,342	9,648

[a] See Table A1 for the sampling distribution of women aged 18 to 49 at the time of interview.

Table 2–3: Policy effects of tuition-free primary and secondary policies on marital and fertility outcomes of women.

Outcomes	**Primary vs. None Free** Policy Effect (95% CI [a])	**Secondary vs. None Free** Policy Effect (95% CI [a])	**Secondary vs. Primary Free** Policy Effect (95% CI [a])
Marriage before 15	−0.75 (−1.51, −0.07)	−4.05 (−4.96, −3.20)	−3.30 (−4.06 ,−2.54)
Marriage before 18	−2.50 (−4.19, −0.85)	−5.47 (−7.81, −3.23)	−2.97 (−5.16, −0.79)
Child birth before 15	0.30 (−0.27, 0.86)	−1.54 (−2.15, −0.83)	−1.84 (−2.40, −1.28)
Child birth before 18	−3.68 (−5.40, −2.16)	−8.55 (−10.83, −6.34)	−4.87 (−6.91, −2.83)

Sample sizes for outcomes using age 15 and 18 as thresholds are 153,086 and 126,850 respectively.
[a] 95% Bias-corrected bootstrapped confidence intervals (CI).
All models control for rural/urban residence and sex of the household head.

Table 2–4: Sensitivity analyses showing policy effects of tuition-free primary and secondary policies on marital and fertility outcomes of women.

Outcomes	**Primary vs. None Free** Policy Effect (95% CI [a])	**Secondary vs. None Free** Policy Effect (95% CI [a])	**Secondary vs. Primary Free** Policy Effect (95% CI [a])
Panel A: Age of exit from primary/secondary level determines exposure			
Marriage before 15 *N=151,064*	–1.49 (–2.08, –0.83)	–4.34 (–5.23, –3.48)	–2.85 (–3.59, –2.12)
Marriage before 18 *N=126,295*	–5.31 (–6.55, –4.28)	–9.61 (–11.39, –7.67)	–4.30 (–5.92, –2.68)
Child birth before 15 *N=151,064*	–0.08 (–0.54, 0.34)	–1.32 (–1.98, –0.69)	–1.25 (–1.79, –0.71)
Child birth before 18 *N=126,295*	–3.11 (–4.25, –1.95)	–7.87 (–9.75, –6.04)	–4.76 (–6.28, –3.24)
Panel B: Excluding girls who were already in school at the time of policy rollout			
Marriage before 15 *N=131,651*	–2.76 (–3.80, –1.73)	–5.59 (–6.92, –4.53)	–2.83 (–3.79, –1.88)
Marriage before 18 *N=111,128*	–8.05 (–10.20, –5.89)	–8.10 (–10.98, –4.93)	–0.06 (–2.95, 2.84)
Child birth before 15 *N=131,651*	–0.61 (–1.36,0.23)	–2.18 (–3.00, –1.36)	–1.57 (–2.26, –0.89)
Child birth before 18 *N=111,128*	–5.87 (–8.18, –3.65)	–10.59 (–13.33, –7.59)	–4.72 (–7.69, –1.74)
Panel C: Pre-policy period starting in 1975			
Marriage before 15 *N=126,388*	–0.55 (–1.58, 0.05)	–3.67 (–4.80, –2.55)	–3.12 (–3.92, –2.33)
Marriage before 18 *N=100,542*	–2.45 (–4.25, –0.78)	–5.60 (–8.13, –3.35)	–3.15 (–5.40, –0.90)
Child birth before 15 *N=126,388*	0.34 (–0.19, 0.96)	–1.41 (–2.05, –0.75)	–1.75 (–2.31, –1.19)
Child birth before 18 *N=100,542*	–3.67 (–5.35, –2.09)	–8.84 (–10.98, –6.60)	–5.18 (–7.18, –3.17)
Panel D: Logistic Regression			
Marriage before 15 *N=153,073*	–1.38 (–2.26, –0.51)	–3.20 (–4.08, –2.31)	–1.81 (–2.88, –0.75)
Marriage before 18 *N=126,850*	–2.56 (–4.28, –0.83)	–4.96 (–7.34, –2.58)	–2.40 (–4.69, –0.11)
Child birth before 15 *N=153,073*	0.09 (–0.57,0.76)	–0.77 (–1.48, –0.04)	–0.86 (–1.68, –0.04)
Child birth before 18 *N=126,850*	–3.49 (–5.08, –1.91)	–7.53 (–9.48, –5.59)	–4.04 (–5.94, –2.14)

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).
All models control for rural/urban residence and sex of the household head.

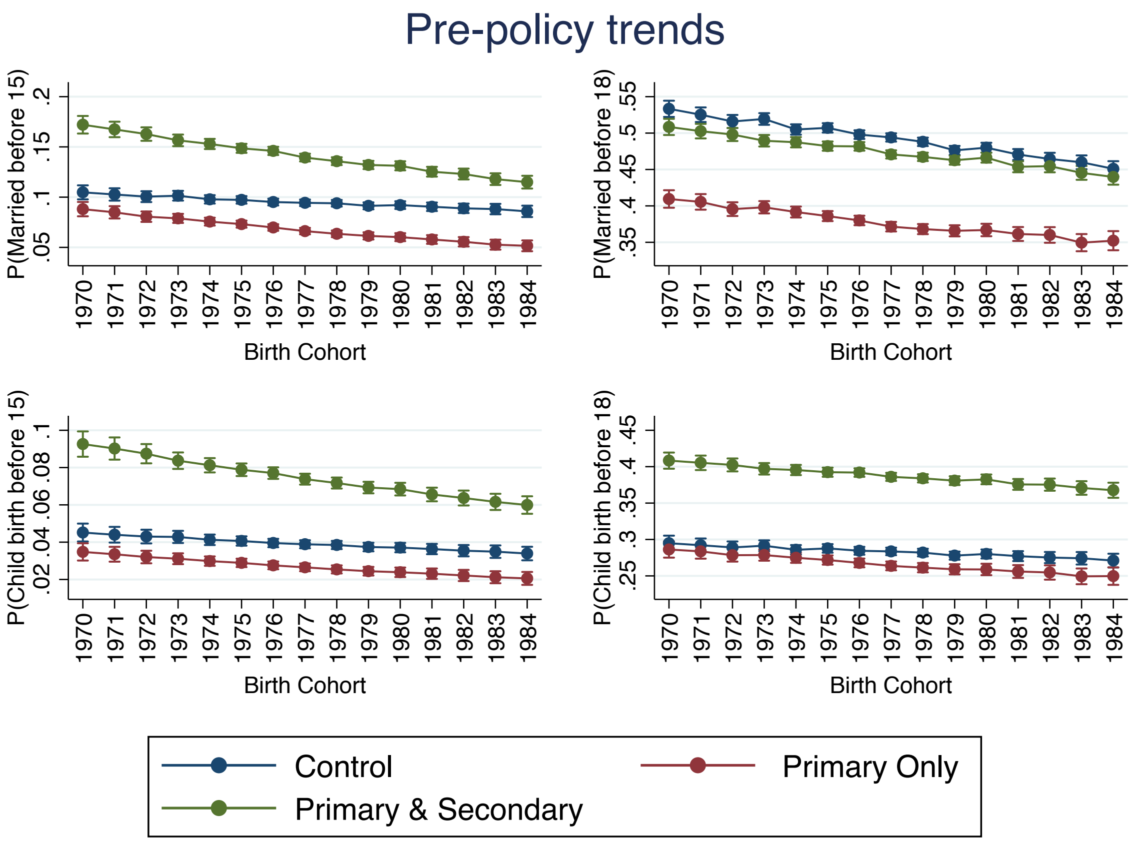


Figure 2–1: Pre-policy trends of marital and fertility outcomes by treatment status.

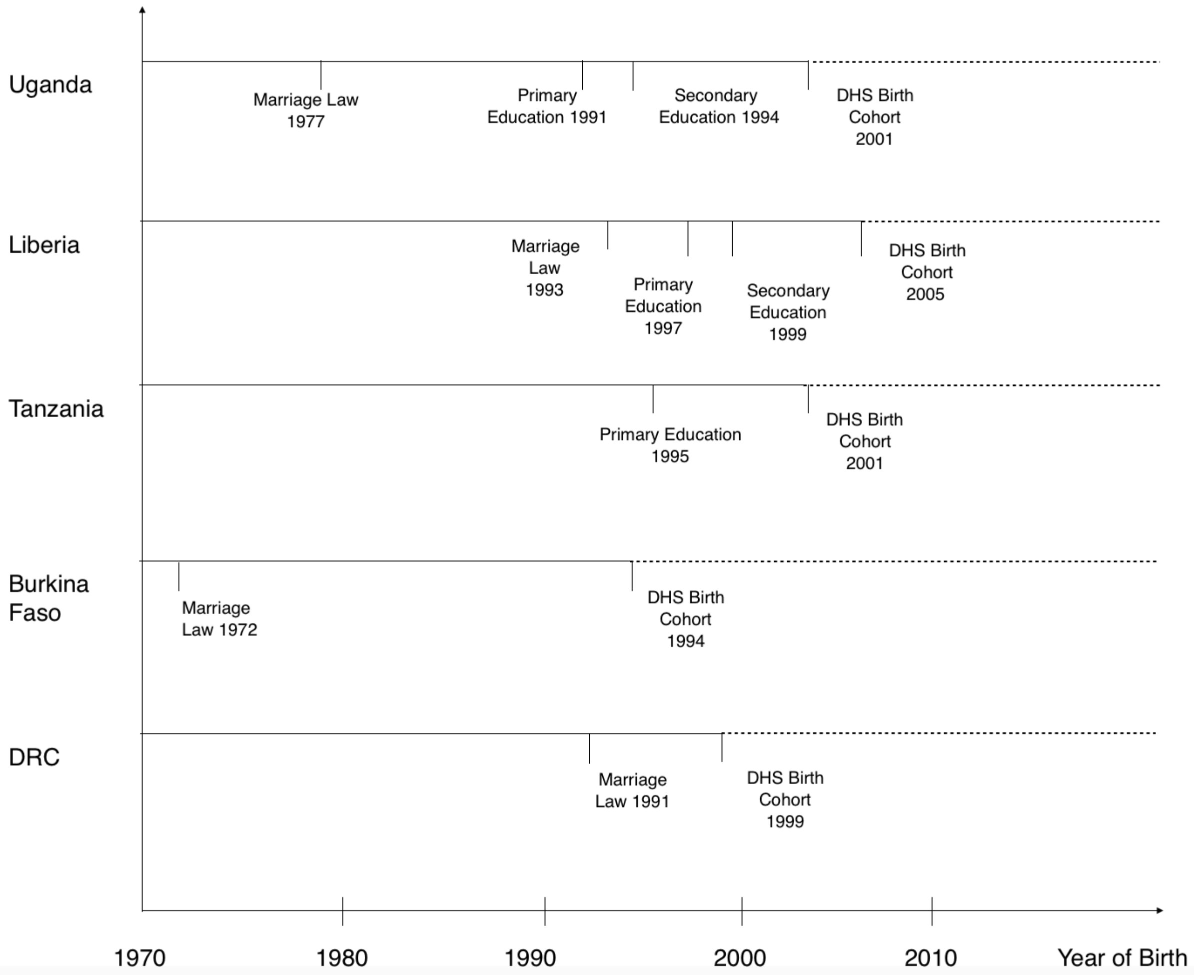


Figure 2–2: Exposure of birth cohorts to minimum age marriage laws and tuition-free education policy in each country.

Notes: The years represent birth cohorts. Each law/policy year represents the first birth cohort expected to be impacted by the relevant law/policy. The DHS birth cohort shows the last birth cohort to be surveyed by DHS in each country.

APPENDIX

Table 2–A: DHS waves used for the analytical sample and sampling distribution of women aged 18 to 49 at the time of interview.

Country	DHS waves used	Sampling Distribution		
		No exposure	Primary exposure	Secondary exposure
Treated Countries				
Uganda	1989, 1995, 2001, 2006, 2011, 2016	26,294	3,181	3,772
Liberia	1986, 2007, 2013	16,943	571	1,018
Tanzania	1992, 1996, 1999, 2005, 2010, 2016	28,771	1,727	0
Control Countries				
Burkina Faso	1993, 1999, 2003, 2010	23,028	0	0
DRC	2007, 2014	21,546	0	0
Total (N=126,851)		116,582	5,479	4,790

Table 2–B: Descriptive statistics of key characteristics of women born during the pre-policy years (1970 to 1984).

Sample Characteristics	Marriage before 15	Marriage before 18[a]	Child-bearing before 15	Child-bearing before 18[a]	Urban Residence	Sex of the Household Head	Sample Size[a]
	% Mean (SD)	% Mean (SD)	% Mean (SD)	% Mean (SD)	% Mean (SD)	% Mean (SD)	Age 15–49 (18–49)
Treated Countries							
Uganda	14.97 (35.67)	50.75 (50.00)	7.09 (25.66)	39.34 (48.85)	19.66 (39.74)	28.54 (45.16)	21,001 (19,387)
Liberia	12.38 (32.94)	40.68 (49.13)	7.67 (26.62)	38.18 (48.59)	50.51 (50.00)	37.59 (48.44)	9,648 (9,648)
Tanzania	6.65 (24.92)	38.19 (48.59)	2.77 (16.42)	26.81 (44.30)	29.56 (45.63)	20.60 (40.45)	23,606 (20,678)
All Treated	10.84 (31.09)	43.55 (49.58)	5.28 (22.35)	33.81 (47.31)	29.31 (45.52)	26.56 (44.17)	54,255 (47,713)
Control Countries							
Burkina Faso	8.08 (27.25)	55.17 (49.73)	2.98 (17.01)	29.22 (45.48)	22.93 (42.04)	7.03 (25.56)	19,318 (17,477)
DRC	12.21 (32.74)	42.05 (49.37)	5.22 (22.24)	27.67 (44.74)	40.89 (49.17)	22.91 (42.03)	11,320 (11,320)
All Control	9.60 (29.45)	50.05 (50.00)	3.80 (19.13)	28.62 (45.20)	29.53 (45.62)	12.86 (33.48)	30,638 (28,797)

[a] Sample restricted to women aged over 18 years at the time of survey for characteristics that are affected by censoring.

Notes: Mean values are weighted according to the weights provided in the DHS.

Chapter 3

Marriage, Childbearing, and Long-term Financial Incentives: Evidence from Haryana, India

1. Introduction

Since the 1990s, many low- and middle-income countries (LMICs) have increasingly adopted conditional cash transfers (CCT) as instruments of social protection policy to reduce poverty and encourage investments in human capital. CCTs are cash payments made to eligible households, typically poor or disadvantaged, if they fulfill certain conditions that policymakers see as desirable behaviors to realize policy objectives. Currently, 64 LMICs across the globe have introduced CCTs (World Bank, 2015), not just to tackle poverty but to achieve a range of other desirable policy outcomes such as health, education, savings and investment, employment, and women's empowerment (Bastagli et al., 2016). Due to their widespread presence and favorable impact on certain outcomes, there is an ever-growing interest in determining whether CCTs can impact outcomes that influence girls' long-term prospects, such as marriage and fertility.

Many recent reviews identify cash transfers as a promising policy instrument to delay marriage and fertility among girls and women (Malhotra et al., 2011; McQuestion et al., 2012; Millan et al., 2019). There are several pathways through which cash transfers may affect marital and fertility outcomes. First and foremost, the economic incentives offered to households may increase the overall resources available to the household and help offset the cost of postponing marriages. A World Bank study (Klugman et al., 2014) shows that girls in poor households are almost twice as likely to get married before 18 than girls in higher-income households. In many countries, particularly in South Asia, dowry is rampant and usually increases with the bride's age and level of education (Huda, 2006; Srivastava et al., 2021). Dowry poses a substantial financial burden on households, and poverty and economic insecurity leave little for investments in the health and education of young girls (Schurmann, 2009). By way of income effect, cash transfers have the potential to enable households to delay girls' marriage.

Second, cash transfers can delay marriage and fertility through increased investments in girls' education. Cash transfers can influence girls' education through either the explicit conditionality of girls' enrolment and attendance in schools or intra-household resource reallocation. Most CCTs condition on the continued attendance of girls in schools, and there is robust evidence for increases in girls' education enrolment as a consequence of these programs (Bastagli et al., 2016; Fiszbein & Schady, 2009). A systematic review found that although cash transfers without the conditionality of schooling increase enrolment, the increase in enrolment is substantively smaller than that of CCTs (Baird et al., 2013). The additional years spent in school and possible human capital accumulation may push households and girls to marry and bear children later.

Third, CCTs have the potential to influence the gender norms and aspirations of young girls and their parents in the long run. Malhotra et al. (2011) report that while some studies show positive effects on attitudes related to child marriage, most studies are either inconclusive or report no change, leading them to conclude that behaviors may change more readily than attitudes contrary to the general perception. The lack of evidence in favor of this potential pathway could be due to the short time horizon of studies typically spanning five to ten years or a true null effect on such outcomes.

Notwithstanding the promising potential of CCTs in influencing long-term outcomes such as marriage and childbearing, little is known about these impacts due to two significant reasons—not many CCTs explicitly target marriage and childbearing outcomes as program objectives and the long time horizon required to study the impact of CCTs on marriage and childbearing outcomes. Over the years, many CCT programs were launched to alleviate poverty and improve human capital outcomes, but very few of these explicitly targeted ages at marriage or childbearing. Most of the current causal evidence comes from CCTs that require girls to attend school but put no condition on girls to remain unmarried. These include the Zomba Cash Transfer Program (ZCTP) in Malawi, the Punjab Female School Stipend Program (FSSP) in Pakistan, the *Oportunidades* program in Mexico, Programe de Asignacion Familiar (PRAF) in Honduras, and Red de Protección Social (RPS) in Nicaragua.

An important discussion around the effectiveness of economic incentives for delaying marriage and fertility is shaped by studies on ZCTP in Malawi (Baird et al., 2011; 2019; Baird et al., 2010). The authors compared CCTs and unconditional cash transfers (UCTs) to a control group along the margins of whether girls were out of school or in school at baseline. They found that girls in the UCT group had a lower likelihood of marriage and fertility than the control group by the end of the 2-year study. Although no significant differences were found for the CCT group among girls who were in school at baseline, they found significant declines in marriage and childbearing in the CCT group among girls who were out of school at baseline. Interestingly, they again evaluated the program at the end of 4 years and found that gains associated with UCT had been completely wiped out; the CCT group among baseline school girls did not show any effect at any point during the study. Only the CCT group among baseline dropouts had sustained reductions in the likelihood of being ever married or ever pregnant. The authors conclude that the impact of CCTs on schooling needs to be substantial to have a knock-on effect on marriage and fertility. They caution against weak effects in settings where income-generating opportunities or systems of dowries exist.

As for *Oportunidades*, many studies find no delays in marriage or fertility among women in rural areas (Arenas et al., 2015; Behrman et al., 2009; Parker & Todd, 2017; Parker & Vogl, 2018) except one that found urban women to have delayed marriage and first and second births (Gulemetova-Swan, 2009). The other two Latin American CCTs, PRAF and RPS, did not affect marriage and fertility (Stecklov et al., 2007). There was evidence of a slight delay in marriage but not childbearing due to FSSP in Pakistan; however, it was too little to translate into reductions in the probability of being ever married during the study period (Alam et al., 2011). So far, research points to a weak link between CCTs that condition on schooling alone and the timing of marriage and childbearing among women.

Only two interventions, the Female Secondary Stipend and Assistance Program (FSSAP) in Bangladesh and Berhane Hewan in Ethiopia, explicitly targeted child marriage through economic incentives. The primary mechanism through which FSSSP targeted marriage was education while requiring girls to remain unmarried over the five-year duration of the intervention. It fully paid secondary school fees for girls enrolled in grades 6–10 (typically aged between 11 and 15 years) and provided a modest monthly stipend to girls to maintain 75% attendance in school and score at least 45% on the test scores. A few studies show that the FSSAP in Bangladesh successfully delayed marriage and fertility (Arends-Kuennig & Amin, 2000; Hahn et al., 2018; Hong & Sarr, 2012); however, they lack sufficient rigor to establish causality. Starting in 1990, Bangladesh embarked on a different phase in education (Unterhalter et al., 2003) wherein it introduced free and compulsory primary education in 1990, free tuition for girls in grades 6–8 in 1990, food for education (FFE) program in 1993 and finally the FSSAP in 1994.

Furthermore, these programs also have been complemented with other supply-side interventions such as curriculum reforms, instructional materials development, teacher training, recruitment of female teachers, improvement of school infrastructure, awareness programs at the community level, and institutional capacity building (Khandker et al., 2003). The impacts of these individual interventions might be inextricable from one another. Moreover, a World Bank project assessment report (World Bank, 2003) points out that girls' transition rates to secondary school overtook boys' in 1991, and the difference remained constant later on, further undermining the evidence in favor of FSSAP.

The second intervention, 'Berhane Hewan' in Ethiopia, offered families a goat if girls remained unmarried over the two years of intervention, along with other program components such as community awareness, group formation, and formal and non-formal education for girls. Erulkar and Muthengi (2009) find that the program delayed marriage to later adolescence, i.e., fewer girls aged 10–14 got married, but a more significant proportion of girls aged 15–19 in the program district got married by the end of the study. The authors report that 65% of the former group received formal schooling support, whereas only 37% of the latter did so. It is plausible that girls out of school during later adolescence were too old to go back to school and the economic incentive was not enough to remain unmarried.

While these studies report the impact of specific cash transfer interventions on marriage and childbearing, the potential of cash transfers in delaying these outcomes on a national scale in regions with a high incidence of child marriage and adolescent childbearing remains unknown. Most of the rigorous evaluations of at-scale cash transfers come from Latin America. Nguyen and Wodon (2015) show that during the period 1985–90 before cash transfers were introduced anywhere in the world, Latin America had the lowest rate of child marriage at 24%, while South Asia and Sub-Saharan Africa were at 45% and 39% respectively. The lack of evidence in favor of CCTs on marriage and childbearing outcomes might stem from a difference in the cultural context and already low incidence levels. Other rigorous evaluations come from Sub-Saharan Africa, but both ZCTP and Berhane Hewan were randomized controlled trials implemented sub–nationally within one treatment area. The only program implemented at a national scale within a region steeped in traditional norms and high incidence levels is Bangladesh's FSSAP, but so far has not been evaluated rigorously. Therefore, it remains crucial to evaluate at-scale cash transfer programs addressing child marriage and early childbearing in regions with a high incidence of these outcomes (Jain & Kurz, 2007; Malhotra et al., 2011; McQueston et al., 2013; Millan et al., 2019).

This paper aims to plug the evidence gap on the impact of cash transfers on the timing of marriage and fertility by studying the long-term effects of Apni Beti Apna Dhan (*ABAD*), a statewide CCT program launched in Haryana, India. Based on data representative at the state level, the study estimates the effect of *ABAD* on the likelihood of girls getting married or bearing children before the age of 18. The study hypothesis is that the program should reduce child marriage and early childbearing, primarily by providing households with an economic incentive. It also tests whether the program affected other likely mechanisms such as schooling and gender norms as reflected by the practice of son preference. The paper will add to the small body of evidence on the effectiveness of CCTs that have a unique program design in terms of the long protracted payment of the incentives and ones that do not condition on schooling outcomes. Moreover, it will help understand what might work in cultures with a glaring girl disadvantage within an entrenched dowry tradition.

2. Program Context

While child marriage and early childbearing remain pervasive worldwide, South Asia is home to an overwhelming 44% of child brides. Furthermore, India has the largest concentration of child brides, estimated at 223 million, with one in three child brides living there (UNICEF, 2019). These high rates are particularly alarming given that the minimum age for marriage has been 18 for women and 21 for men since 1978. Minimum age marriage laws alone may not be enough as these practices often occur within overlapping poverty conditions, low education levels, and traditional gender and social norms. Recognizing persistent discrimination against the girl child, many states in India came up with conditional cash transfer programs to incentivize girls to remain unmarried until they turn 18. The state of Haryana was the first to introduce such a program in 1994.

The *ABAD* program design is different from that of the most well-known CCT programs studied so far. It explicitly targeted child marriage by conditioning payments on girls remaining unmarried until 18 years of age. Other conditions to be fulfilled were birth registration and immunization. The way the conditionality works is also unusual in that the government pays Rupees (₹) 500 on the birth of the girl child and invests a sum of ₹2500 in a long-term government savings bond. This bond matured for ₹25,000 on the girl's eighteenth birthday, provided she remains unmarried. This long protracted form of payment is a distinguishing feature of *ABAD.* While schooling is not a condition for the savings bond maturation, families receive a bonus of ₹5,000 when the girl completes grade 5 and another ₹1,000 when she completes grade 8. Furthermore, the program targeted disadvantaged households. All poor households (below the poverty line) and non-poor Scheduled Caste (SC) and other backward classes (OBC) households were eligible to apply for the program. These households could enroll only up to the third girl child into the program.

The ultimate program objective of reducing child marriage has not been evaluated yet due to the long time horizon of the program. Girls born after October 1994 were eligible for the program, and the first cohort of these girls would have turned 18 in 2012, providing the first opportunity to study the program's impact. Figure 3–1 presents a conceptual framework of the relationship between ABAD rollout and the program objectives of delayed marriage and closely related childbearing outcomes. It also presents the various pathways through which the program might work and the context characteristics that modify these relationships.

Meanwhile, a few studies have looked at its impact on intermediary outcomes such as sex ratio, health, fertility preferences, and education. Using a sample of 200 households within a single block in Haryana, Krishnan et al. (2014) did not find any effect on age at marriage by comparing elder sisters of younger daughters expected to be eligible for *ABAD* with daughters-in-law from outside the state. However, the study is weak because they used elder daughters within the same household who were not eligible for *ABAD* to make this comparison. There is no economic incentive for households to delay marriage of elder daughters not eligible to receive cash payments under *ABAD*. They argue that *ABAD* should have impacted these girls due to the change in attitudes. While the change in attitudes is one of the expected mechanisms through which *ABAD* could impact the timing of marriage, the importance of the economic incentive cannot be undermined. Sinha and Yoong (2009) found positive effects on the sex ratio of living children and post-natal health investments but inconclusive or no effect on fertility preferences and school attendance. Nanda et al. (2014) conducted primary surveys in four districts to evaluate the program. While they found evidence for an increase in education, the study is still underway to evaluate the program's impact on age at marriage.

3. Methods

Data

This study uses the fourth round of the National Family and Health Survey (NFHS-4), collected from 2015 to 2016 by the International Institute of Population Sciences, Mumbai. The NFHS surveys are widely used nationally representative surveys that provide reliable information on the health and socioeconomic conditions of the Indian population. The NFHS program, first initiated in India in 1992-1993, contributes to the Demographic and Health Surveys (DHS) project. Since its inception, the NFHS has been conducting cross-sectional surveys at regular intervals. In its fourth round, the NFHS expanded considerably to provide district-level estimates of health and population outcomes using a set of household, women, and men questionnaires.

The *ABAD* program was launched in 1994, and the initial cohort of beneficiaries turned 18 in 2012. Since NFHS-4 was conducted in 2015-16, it provides an opportunity to study the long-term impact of *ABAD* on the likelihood of child marriage in Haryana. NFHS-4 interviewed all women aged 15-49 years within the surveyed households. It collected information on women's health, education, marriage, complete birth history, and background characteristics such as age, religion, caste, and economic status. It asked all women respondents to report their current marital status, the age at which they started living with a partner, and the age at which they had their first child. I use this information to create a dichotomous child marriage variable indicating whether a woman got married before she turned 18 (*ABAD's* primary objective). Marriage and childbirth are closely intertwined, especially in India, where most children are born within wedlock. Therefore, delaying the entry of adolescents into wedlock might delay childbearing. I create a dichotomous variable indicating whether a woman had her first childbirth before she turned 18 to study this outcome. I also create a linear variable capturing the interval (number of months) between marriage and first childbirth.

Each individual also reported their highest completed level of education. Because *ABAD* provided bonus payments for completing grades 5 and 8, I create two binary variables indicating whether the respondent completed these grades. Lastly, all women also report the ideal number of sons and daughters they would like to have over their life course. One of the policy objectives of *ABAD* was to reduce gender discrimination against girls, and fertility preference is indicative of women's attitudes toward the girl child. I create an indicator for son preference taking value 1 if the respondent prefers more sons than daughters.

Analytical Sample

Haryana rolled out *ABAD* in all its districts simultaneously, and therefore, I use the neighboring state of Punjab, which has similar characteristics as a control group. A key challenge in using NFHS data (as with all other publicly available secondary household surveys) is that it does not collect information on individual household participation in *ABAD*. However, NFHS-4 does provide information on the background characteristics that determine program eligibility, such as the state of residence, year of birth, caste, and whether a household is below the poverty line. I restrict the analytical sample to all respondents who meet *ABAD's* eligibility criteria residing in the states of Haryana and Punjab.

To be eligible for *ABAD*, a household needed to be below the poverty line (BPL) or belong to historically disadvantaged caste groups — Scheduled castes (SC) or Other Backward Castes (OBC). Income taxpayers and government employees were not eligible for the policy within these disadvantaged caste groups. Independent surveys are conducted in India to estimate poverty and identify households eligible for welfare schemes. NFHS-4 collects information on whether a household has a BPL card or not and their caste affiliations. However, there is no information on whether the household pays income taxes or has a member working as a gazetted government employee. The lack of this information should not impact the analysis because it is estimated that less than 3% of India's population pays income taxes (Piketty and Qian, 2009). Therefore, all respondents who have a BPL card or belong to SC/OBC caste groups comprise the analytical sample.

The policy covered up to the third girl child born within all eligible households. The study excludes girls who had not turned 18 by the time of the survey because it is not possible to determine whether they would marry or have a child before or after 18 until they reach the age of 18. As a result, data is available only for girls born up to June 1997 (i.e., up to 32 months after the policy rollout). However, there is no way to ascertain the birth order of the respondent to figure out whether she made it to the cutoff limit of up to the third girl child within the family. NFHS-1, conducted in 1992-1993, reports 3.1 and 2.7 as the average total number of children ever born and living, respectively, for currently married women in Haryana. Therefore, the lack of this data may not be a significant issue.

As mentioned earlier, Haryana introduced *ABAD* in all its districts simultaneously, eliminating the possibility of comparisons across treated and control districts. Therefore, this study borrows data from the neighboring state of Punjab, which did not have a similar policy in place. Punjab is economically, socially, and geographically similar to Haryana. Both the states were the top two wealthiest states in the country during 1990-2000, with per capita incomes of ₹25,631 and ₹23,222 in Punjab and Haryana, respectively. The annual growth rate of per capita state domestic product (SDP) was 3.3 % and 3.9% in Punjab and Haryana, respectively, from 1980-81 to 1990-91. The estimates for the period 1991-92 to 1997-98 were 2.8% and 2.7%, respectively (Ahluwalia, 2000).

Despite their rich status, both states had the worst sex ratio (females per 1,000 males) in the country as per the 1991 census. The sex ratio among the child population in the age group 0-6 years was the lowest in Punjab at 875 and the second-lowest in Haryana at 879. During 1970-75 to 1986-90, life expectancy at birth grew from 57.9 to 65.2 in Punjab and 57.5 to 62.2 in Haryana (Registrar General India, 1999). As per the 1991 census, Punjab had a literacy rate of 58.51, and Haryana had a literacy rate of 55.85. Furthermore, both states share a common administrative history as Haryana was carved out of (undivided) Punjab in 1966, and Chandigarh was made the shared capital of both states as part of the Punjab Reorganization Act, 1966. These peculiarities make Punjab a suitable control group to compare Haryana with. Other neighboring states are not suitable comparators because Rajasthan introduced Rajalakshmi in 1992, a CCT program for improving the girl child's status, Uttar Pradesh is geographically vast and culturally and politically very different, and Delhi is primarily a non-agricultural urban union territory.

Finally, the analytical sample comprises all girls who were at least 18 at the time of the interview and belonged to BPL or SC/OBC households in Punjab and Haryana. Because data is available only up to 32 months (June 1997) after the policy rollout, the study restricts the pre-policy period to 32 months (February 1992) before the policy change. This results in a total analytical sample size of 5,530. Other independent variables included in the analysis are the household's wealth status, residence type, religious affiliation, sex of the household head, and the highest education level they attained. The study drops 0.9% of cases with a missing observation on at least one of these variables to conduct a complete-case analysis on the final analytical sample of size 5,478. Table 3–A in the appendix shows differences in key sample characteristics of complete versus incomplete cases. Research shows that multiple imputation offers little advantage over complete case analysis when missing information is less than 5% (Lee et al., 2016).

Statistical Analyses

The causal estimate of the effect of the *ABAD* scheme on child marriage, early childbearing, and education is econometrically challenging because the scheme was not randomly assigned to the state of Haryana. Rampant female feticide and alarmingly high rates of child marriage prompted Haryana to launch *ABAD* in 1994. It had one of the worst sex ratios in the country when the scheme was launched. Furthermore, no household survey in India conducted after 2012 (when the initial cohort of beneficiaries turned 18) provides information on the actual participation of households in the *ABAD* scheme, resulting in intent-to-treat estimates.

Researchers widely use quasi-experimental study design methods to estimate treatment effects when a randomized experiment is not feasible. This paper uses one such method, difference-in-differences (DID), which compares treatment and control groups' outcomes over the study period. It corrects some potential biases in pooled ordinary least squares (OLS) studies by borrowing information from a control group over the same time frame. Under the identifying assumption that the treatment and control groups would have had similar trends in outcomes over the study period had it not been for the launch of *ABAD*, we can estimate the effect of *ABAD* on our outcomes of interest. The DID framework results in the estimated equation (1) below:

$$Y_{it} = \beta_0 + \beta_1 Haryana_i + \beta_2 Post_t + \beta_3 Haryana_i * Post_t + \xi X_i + \delta_t + \varepsilon_{it} \quad (1)$$

where Y_{it} are the primary outcomes of interest, $Haryana_i$ is a dummy variable indicating whether the respondent lives in Haryana versus Punjab, $Post_t$ is a dummy variable indicating whether the respondent was born after October 1, 1994, X_i is a set of individual or household characteristics, and ε_{it} is the error term. β_1 captures the average baseline differences in outcomes of respondents in Haryana and Punjab, β_2 is the average difference in the outcomes before and after October 1, 1994 in the absence of *ABAD*, ξ shows the effect of background characteristics, and δ_t represents year fixed-effects to control for secular trend in outcomes over the study period. The DID coefficient of the interaction term, β_3, is my coefficient of interest, measuring the effect of *ABAD*. A significant β_3 would imply an additional effect on the respondents in Haryana before and after the launch of ABAD as compared to the respondents in Punjab during the same period.

The analysis includes a number of individual and household-level potential confounders. While the program was targeted toward BPL households, the possession of a BPL card does not necessarily imply that the household is asset-poor (Panda et al., 2020). It therefore controls for the household's asset deprivation as captured by a composite measure of asset index indicating a household's wealth status divided into quintiles (Filmer and Pritchett, 2001). Other confounders include residence type (urban/rural), religious affiliation (Hindus, Muslims, or others), sex of the household head (male/female), and the highest education level attained (no education, primary, secondary, higher) by them.

The study uses Linear Probability Models (LPM) for all binary outcomes of interest — marriage before 18, childbearing before 18, completion of grade 5 and 8, and fertility preferences for sons over daughters. It uses linear regression to analyze the interval between marriage and childbearing measured in the number of months. For the binary outcomes, the DID coefficients estimate the change in the probability of the outcome in percentage point terms. While for the linear outcomes, the DID estimate can be interpreted as the change in the number of months. LPMs are often recommended for the evaluation of binary outcomes because of complications associated with the estimation and interpretation of multiple interaction terms in Logit and Probit models (Ai & Norton, 2003; Cantor et al., 2012). Even though post estimation command after logistic regression can help with interpretability on the probability scale, issues regarding correctly estimating the interaction term remain (Ai & Norton, 2003). LPMs provide consistent estimates of the regression coefficient in large sample, and predicted probabilities outside the range of 0 and 1 are usually not a concern when the main purpose is to not use it as a predictive model (Wooldridge, 2010).

The regression models do not cluster the standard errors because there are only two clusters, Punjab and Haryana, and doing so might bias the results when the number of clusters is fewer than 10 (Bertrand et al., 2004). All analyses were conducted using bootstrapped standard errors as well as Huber-White robust standard errors to account for heteroskedasticity. The choice of standard errors does not change any of the results, and therefore the tables report only the 95% bias-corrected bootstrapped confidence intervals along with the regression coefficients. All analyses were conducted in STATA 14.2.

Assumption Tests

The abovementioned DID analysis rests upon two causal identification assumptions. First, the models assume that the trends in outcomes before the policy-change are similar across Punjab and Haryana. Testing for parallel pre-policy trends in outcomes lends tenability to the assumption that these trends would have continued to be similar had Haryana not adopted ABAD. Second, the models assume that no other state-specific time-varying factors changed in either state during the study period that might have been jointly correlated with exposure and the outcomes of interest.

I carry out an event-study analysis (Goodman-Bacon, 2021) to test the parallel trends assumption. I replace the DID interaction term with a set of leads and lags with respect to the introduction of ABAD, ranging from 3 years before ABAD to 2 or more years after. The leads and lags were coded 0 for all respondents residing in Punjab. I estimate equation (2) below:

$$Y_{it} = \beta_0 + \beta_1 Haryana_i + \sum_{l=-2}^{-3} \beta_{lead}(ABAD_{il}) + \sum_{l=0}^{2+} \beta_{lag}(ABAD_{il}) + \xi X_i + \delta_t + \varepsilon_{it} \quad (2)$$

where the subscript l refers to the event period in one-year increments. The variable $ABAD_{il}$ denotes a series of binary indicators that take the value 1 if individual i was exposed to ABAD in period l. The first lead is the omitted base category, and this model estimates the relative difference in outcomes for leads and lags of ABAD relative to the reference year, l = –1. These relative differences are captured by β_{lead} and β_{lag}. The β_{lead} coefficients provide a means to investigate any potential violation to the parallel trends assumption. The coefficients are expected to be close to 0 and not statistically significant if the trends in the outcomes were similar across Punjab and Haryana before ABAD was implemented. The β_{lag} coefficients, on the other hand, help investigate the timing of the impact of ABAD and whether the treatment effects vary over time. The role of unmeasured confounders can be considered unlikely if the changes occur around the same time as the start of the exposure period (Venkataramani et al., 2019).

While event-study style regression is popular for testing the parallel trends assumption, it is limited in this case by data availability of only up to 32 months after ABAD's implementation. Therefore, I also test this assumption by regressing the outcomes on the state of residence interacted with the time trend over the pre-policy period from 1965 up to September 1994. The interaction term coefficient will inform us if Punjab and Haryana had differential pre-treatment trends during the period 1965–1994. Lastly, to investigate other sources of changes in the DID estimates, I document other relevant policy shifts related to the girl child launched in Punjab and Haryana during the study period. I analyze the timing and nature of these policies to determine their possible impact on the outcomes of interest.

Sensitivity Analyses

While Punjab is economically, socially, and geographically similar to Haryana, a significant difference between the two states is the religious composition of the population. Haryana is predominantly Hindu, and Punjab, on the other hand, is mainly Sikh. Religious affiliation is likely to influence marital and fertility decisions; therefore, the study tests the sensitivity of the impact estimates by analyzing only the subgroup of Hindu women in both states. Other religious groups are too few in number to carry out a similar stratified analysis.

Another concern with the current analysis is using BPL card data to determine program eligibility based on BPL status. Research shows that households' possession of a BPL card may not necessarily reflect poverty status (Panda et al., 2020). Using NFHS-3 data, Ram et al. (2009) show that non-poor households hold about two-fifth of the BPL cards in India, and similarly, a large proportion of poor households do not BPL cards. Local governance and political power asserted by local elites are known to influence the distribution of public goods such as BPL cards and public welfare programs (Besley et al., 2005; Hirway, 2003; Panda, 2015). Therefore, using possession of a BPL card in the data to determine program eligibility might lead to misclassification errors. Because households belonging to SC/OBC castes were eligible for the program regardless of their poverty status, I now rerun the analysis on the subgroup of SC/OBC girls in the sample.

Lastly, the bordering districts in Punjab and Haryana might be even more similar culturally, geographically, and socially than other parts of the states. It might be more plausible to attribute any significant differences to administrative factors rather than cultural or community norms. I restrict the analytical sample to all eligible girls residing in the bordering districts in Punjab and Haryana to test the robustness of the impact estimates to this alternative sample specification. These include Muktsar, Bathinda, Mansa, Sangrur, Patiala, Sahibzada, Ajit Singh Nagar districts in Punjab, and Panchkula, Ambala, Kurukshetra, Kaithal, Jind, Fatehabad, and Sirsa districts in Haryana.

4. Results

Descriptive statistics

Table 3–1 provides the descriptive statistics for girls who meet the program eligibility criteria and reside in the treated state of Haryana versus those residing in the neighboring control state of Punjab. The study period is restricted to girls born 32 months before and after the program rollout in October, 1994. For Haryana, I also provide the statistics separately for girls born before and after the program implementation. The analytical sample ranged from 3,302 girls in Haryana to 2,176 girls in Punjab. Over the study period, girls in Haryana were more likely to get married and bear a child before they turn 18 than girls in Punjab. The combined caste composition of SC/OBC groups are similar across both the states but while Haryana had more OBC households, Punjab had more SC households. A higher proportion of households held a BPL card in Haryana than Punjab. Furthermore, Hindus were the largest religious group in Haryana as opposed to Sikhs in Punjab. The two states were similar to each other with respect to other indicators such as wealth composition, urbanization, girls' education, and the proportion of households headed by a female.

Impact on marriage and childbearing

Table 3–2 shows *ABAD's* effect on marriage and childbearing before age 18. The difference-in-differences coefficients produced by the LPM model shown in equation 1 can be interpreted as changes in the predicted probabilities in percentage points terms. Columns (1) and (3) report the coefficients among all girls who meet the program's eligibility criteria. Columns (2) and (4) report the coefficients considering a 1-year lag in policy implementation wherein I use a restricted sample by dropping girls who might have been exposed to the program during the first year of its implementation. Comparing girls born right before the policy to those that were impacted at least after a year since its adoption takes into account possible delays in program implementation.

Exposure to *ABAD* reduced the probability of marriage before age 18 by 4.2 percentage points on average among girls who met the program's eligibility criteria. Exposure to *ABAD* was also followed by a reduction in the probability of childbearing before 18 by 2.8 percentage points on average. These reductions are substantial given that the average probability of marriage and childbearing before 18 in Haryana before *ABAD* was 17.9 and 5.8, respectively. The policy-lag DID estimates are larger for both the outcomes. Considering a lag of 1-year in program implementation, *ABAD* reduced the probability of marriage before age 18 by 6.8 percentage points and that of childbearing before age 18 by 3.5 percentage points. The p-values for all the DID estimates were significant at 5% level of significance and were not sensitive to the inclusion or exclusion of any particular confounding variables considered in the regressions.

Impact on other relevant outcomes

Table 3–3 reports the difference-in-differences coefficients for other relevant outcomes such as the interval between marriage and first child (in months), completion of grade 5 and grade 8, and the practice of son preference among the respondents. The interval between marriage and childbearing is closely related to the program's primary objective of delaying marriage. Completion of grade 5 and grade 8 are conditions for bonus payments made to the program beneficiaries. Lastly, the program was launched with a view to improve the sex ratio in the state and to encourage investments in the girl child in the long run. Respondent's preference for sons is indicative of gender attitudes. As shown in Panel A, I find that exposure to *ABAD* is associated with a reduction in the interval between marriage and childbearing by 1.9

months on average, a slight increase in the probability of completion of grade 5 and grade 8, and a slight decrease in the probability of preference of sons over daughters by 1.7 percentage points. However, none of these estimates were statistically significant. The results were similar when we considered a 1-year lag in policy implementation as shown in Panel B.

Model assumptions: Event study estimates, Pre-trends, and other policies

Figure 3–2 provides a graphical representation of the event study estimates for marriage and childbearing outcomes. Each point on the graph represents the effect of introducing *ABAD* on the outcome for the specified time period relative to the program implementation. All lead coefficients before the policy implementation allay concerns regarding differential pre-policy time trends in Haryana versus Punjab. The coefficients are close to zero during the pre-policy period without any significant increase or decrease before the policy rollout. Furthermore, the lag coefficients demonstrate that while the probability of marriage and childbearing before age 18 dropped following the program adoption, it is after a year of policy implementation that we see a significant drop in both the outcomes. Figure 3–3 shows the pre-policy trends in the probability of marriage and childbearing before age 18 in Haryana and Punjab for women born between the years 1965 and 1994 (up to September). It is evident that the trends were similar in both the states and none of the DID estimates were significant during the pre-policy years.

I also studied other possible sources of changes during the study period such as other state-specific or national policies that targeted child marriage as shown in Figure 3–4. The Central Government introduced *Balika Samridhi Yojana* (BSY) nationally in 1997. Eligible girls born after August, 1997 received a post-birth grant redeemable when they turn 18 along with incentives for school completion. It is unlikely that BSY impacted my estimates because the analytical sample has girls born in both Punjab and Haryana up to June, 1997 only. The state government of Punjab introduced '*Shagun'* policy for all girls born after April, 1997 where they received a sum of ₹5,100 upon marriage. I re-ran the regression on the restricted sample of girls born up to April, 1997 and found all the coefficient estimates to remain the same.

A few other policies introduced later in the 2000s could not have impacted the beneficiary girls directly but rather indirectly through changes within the household. The Haryana government launched *Ladli* policy wherein a household with a second girl child born after August, 2005 received a sum of ₹5,000 for a period of five years if both the girls survived. The entire amount is invested in a bond that is redeemable when the younger sister turns 18. It is unlikely that *Ladli* impacted the coefficient estimates as the economic incentive can only be realized after 2023. Moreover, research shows that many of these policies implemented throughout the country were ineffective in changing gender norms and attitudes. I did not find evidence for changes in the practice of son preference either, supporting the hypothesis that *Ladli* is unlikely to confound my estimates through the mechanism of broader changes in attitudes toward the girl child.

The Punjab government launched *Balri Rakshak Yojana* in 2005 that gave monthly payouts to families with up to two girl children if the parents underwent permanent sterilization. Once again, I do not anticipate confounding because of extremely low program uptake possibly due to the strict requirement of sterilization. Only 177 households registered up to the year 2010 (Sekher, 2012). Furthermore, these programs could have impacted girls born immediately before *ABAD* as much as those born immediately after, further reducing the possibility of confounding. Lastly, *Dhanlakshmi* was a centrally sponsored policy rolled out in a few districts nationally including one in Punjab wherein it offered financial incentives that mature when the girl child turns 18 and remains unmarried. Once again, it is unlikely to impact my estimates because the first cohort that became eligible for the program was born in November, 2008 and the economic incentives cannot be realized before 2026.

Sensitivity Analyses

Table 3–4 shows the sensitivity of the impact estimates to different sample specifications. In panel A, we see that *ABAD* reduced the risk of marriage and childbearing before 18 among eligible Hindu girls by 6.3 and 4.6 percentage points on average, respectively. Similar to the earlier results, the estimates considering a 1-year lag are even larger. *ABAD* seems to have had a more substantial impact on eligible Hindu girls, as indicated by the larger effect sizes for all the outcomes. As shown in panel B, the results for the subgroup of SC/OBC girls are similar to the initial results where we also used BPL status to determine eligibility. Lastly, panel C presents the coefficients from the restricted analysis on eligible girls residing in the bordering districts of Punjab and Haryana. While we see evidence for a strong effect of *ABAD* on reducing the risk of marriage before 18, both with or without policy lag, there is not sufficient evidence for reduction in the risk of childbearing before 18.

Table 3–5 shows the sensitivity of the impact estimates for the secondary outcomes of interest. Once again, I find no evidence for *ABAD's* impact on education and son preference regardless of how the analytical sample is defined. Its effect on the time interval between marriage and childrearing too remains the same for SC/OBC girls and eligible girls residing in the bordering districts as shown in panels B and C. The only noteworthy difference is that ABAD reduced the interval between marriage and childbearing by an average of 4.6 months for eligible Hindu girls as shown in Panel A.

5. Discussion

This paper uses a quasi-experimental study design to examine the impact of Apni Beti Apna Dhan (*ABAD*), a government-run statewide CCT program, on the program objective of delaying girls' marriage until they turn 18. It also explores its impact on related fertility outcomes, such as the probability of bearing children before 18 and the interval between marriage and first child. It further looks at its impact on plausible mechanisms beyond the economic incentive that might help delay these outcomes, such as increased completion of primary and lower secondary schooling and changes in gender norms as reflected by the preference for sons over daughters. Using household data on marriage, fertility, and education that is representative at the state level from India's NFHS-4, I compare trends in the outcomes in the treated state of Haryana to that of the control state of Punjab within a difference-in-differences (DID) study setup. This study is methodologically similar to many studies that have used the DID strategy on household survey data in the absence of experimental data to conduct program evaluations (Alam et al., 2011; Lim et al., 2010; Sinha & Yoong, 2009).

Overall, this study suggests that while cash transfers conditioned on girls remaining unmarried can be effective policy instruments for reducing the incidence of child marriage, the impact of CCTs can be limited as far as other critical development outcomes are concerned. I find strong evidence in favor of the program's impact in delaying girls' marriage until 18 years, mixed evidence for delaying childbearing until 18 and on the interval between marriage and childbearing, and no evidence of its impact on schooling and son preference. I find a significant reduction in the probability of marriage before 18 across all specifications—among all eligible girls, Hindu girls who meet the eligibility criteria, SC/OBC girls, and eligible girls residing in only the districts bordering Punjab. These effects were even larger when considering girls who became eligible for the program after at least one year of the program's presence in the state. The magnitude of the reported impact is higher than most interventions introduced in the past two decades that have been evaluated for their effect on the timing of marriage (Malhotra & Elnakib, 2021). Only one randomized experiment conducted in a few villages in India that focused on increasing labor market opportunities for women found a similarly high magnitude (Jensen, 2012).

As for the probability of childbearing before 18, I find significant but smaller reductions than marriage across all specifications except one. The results indicate that the delay in marriage drives the delay in childbearing. Similarly, the program seems to have reduced the interval between marriage and childbearing, but the reduction is not significant in any of the specifications except among Hindu women. These findings are consistent with those of Dommaraju (2012), who uses NFHS–3 (2005–06) data to report that girls who marry at a younger age have a longer first birth interval on average. MacQuarrie (2016) also reports that this pattern is true for South Asia in general. The even-study estimates, examination of pre-existing trends, and analysis of policy changes during the study period further lend credibility to these results. It might be worthwhile to discuss the event-study graph for childbearing in Figure 3–2. The higher probability of childbearing among girls exposed two or more years after the program's launch could be because the target group did not have time to properly "age in" to the outcome of interest (Malhotra et al., 2011). The sample size may be too small to estimate these effects accurately.

There are several possible explanations for the null effects on the probability of completing primary and secondary schooling and women's likelihood of preferring sons over daughters. Approximately 72% of girls aged 6–10 and 66% aged 11–14 were enrolled in school in Haryana in 1993 (Kingdon, 2007). *ABAD's* bonus payments may have been insufficient to incentivize girls out of school before the program to attend school. Primary education became free in India under the Right to Education Act in 2009, and until then, families had to bear the cost of sending their children to school. Moreover, foregone wages in the labor market, informal agricultural work, domestic household responsibilities for young girls, distance to schools, and norms regarding the safety and chastity of the girl child may further offset the incentives associated with the bonus payments. It is noteworthy that these considerations are not at odds with girls remaining unmarried but kick in when households make schooling decisions. Furthermore, it is not surprising that women's preference for sons has not changed as research shows that contrary to the general perception, attitudes may be harder to change than behaviors (Malhotra et al., 2011) or may take longer to change.

This is one of the first studies to rigorously evaluate a CCT program conditioned on marriage alone within a region entrenched in a culture of gender discrimination. Furthermore, it uses large data representative at the state level to study a program implemented at scale by the state government. These findings have important policy implications. It provides evidence in favor of a scalable and replicable policy in other places, especially with a similar cultural context. Most of the current evidence on CCTs comes from relatively recent and small-scale studies, and building the evidence base for scalable programs and policies is particularly crucial (Chandra-Mouli et al., 2013). Also, South Asia is a hotspot for child marriage, where 56% of girls aged 20–24 got married before they turned 18 in 2014 (UNICEF, 2014). Out of the ten countries with the highest rates of child marriage, three are in South Asia—Bangladesh, India, and Nepal. Bastagli et al. (2016) point out that the most rigorous evidence for the long-term effects of CCTs has emerged from either Latin America or Sub-Saharan Africa and identify evaluation of programs implemented in other regions with a high population coverage as a research priority.

ABAD is a unique program that directly targets child marriage by conditioning payments on girls remaining unmarried. It is different from other CCTs that do not directly address child marriage or have other program components. The FSSAP in Bangladesh and Berhane Hewan in Ethiopia have significant complementary interventions that make it impossible to extricate the unique contribution of conditioning on marriage. Svanemyr et al. (2015) suggest it is crucial to show individual components that effectively delay marriage, and this study finds that conditioning on marriage alone can be effective in delaying marriage. It reinforces the idea that designing CCTs conditional on certain behaviors can positively affect the outcomes relating to the conditions on which the transfers are conditioned (Bastagli et al., 2016).

Yet, the program falls short in positively influencing a broader set of important outcomes such as early childbearing, schooling, and son preference. Amin et al. (2016) argue that cash transfers alone are unlikely to address deeper issues related to child marriage and suggest a shift in the focus on multi-dimensional holistic programs that work on social norms, realized rights, and access to health and education. *ABAD's* program design includes bonus payments for the completion of grades 5 and 8. However, the results suggest that this program component is not effective in encouraging girls to get more schooling. Strengthening the education component might be critical to achieving better health and overall development of young girls. Future research on building program synergies with girls' education might be worthwhile given the promise it holds for impacting human capital accumulation and long-term social norms.

Further, two features related to the program design merit attention. A few preliminary case studies report that girls delay marriage until 18 when they become eligible to receive the payments and marry immediately after. While it is essential to find ways to delay marriage meaningfully, the program effectively delays it until the UN recommended age of 18. Also, Raj et al. (2012) show that reductions in girl child marriage in South Asia are mainly attributable to delays among younger but not older adolescents aged 16–17 years, and *ABAD* successfully targets this vulnerable age group. This study's policy recommendation of meaningfully integrating education into the program design might also address the issue of girls immediately marrying once payments are received by encouraging them to get a higher education or join the labor force instead of getting married.

The second design feature relates to the targeting of the beneficiaries. *ABAD* focused only on girls from households living below the poverty line or disadvantaged castes such as

SC/OBC groups. While it is true that poverty is strongly associated with child marriage, research shows that a substantial proportion of girls from wealthy households get married before they turn 18 (Raj et al., 2009; Raj et al., 2020). It remains unclear whether CCT programs can foster behavioral change in a critical mass of individuals in a way that it spills over to other subgroups. Further research is needed to identify what might work to delay marriage for this group of girls.

The study has several limitations worth mentioning. First, it uses secondary data that does not collect information on program participation. This problem is quite common when researchers evaluate programs retrospectively using publicly available data. Nonetheless, I unpack the eligibility criteria of the *ABAD* scheme and extract relevant information from the data to narrow down the expected program beneficiaries. I also test for the sensitivity of the results to various subgroup specifications and find the results to be robust.

Second, a few other limitations arise because of the choice to conduct a quasi-experimental study over a randomized study, which is not a feasible option in this case. The fundamental identifying assumption of the DID study design is that we expect the changes in the outcomes in the control group to be similar to that of the treatment group had there been no policy change. This common trend assumption is generally untestable, but I build evidence in favor of it in many different ways. I compare the treated state to a control state that is geographically, historically, and economically similar and control for a range of individual and household characteristics that might affect program participation and the outcomes of interest. The pre-policy trends in the outcomes corroborate that the two states were evolving similarly from 1965 until the launch of *ABAD* in 1994. The event-study estimates also confirm that the two states were similar during the 32 months before *ABAD*, and changes in the outcomes coincide with the timing of *ABAD*. I also thoroughly examine the timing of other relevant policy shifts during the study period and conclude that the results were most likely not influenced by these changes.

Third, *ABAD* was designed to have protracted extended payments to beneficiaries conditioned on girls remaining unmarried until they turn 18. Except for a small payout at the time of birth registration and another relatively larger payout at the time of completion of fifth and eighth grades in school, the lump sum CCT amount was reserved until girls turned 18. While most CCT programs seek to encourage investments in children's human capital, assuming the financial vulnerability of the parents, *ABAD* paid the beneficiaries after these choices have largely been made. I do not know how beneficiary households used the cash benefits and whether it resulted in greater bargaining power among women, had any other intergenerational impacts, or was simply used to cover marriage expenses.

Last, this study uses NFHS–4 conducted in 2015–16 to evaluate the impact of *ABAD*. This is the most recent publicly available household survey data that allows analyses of the program beneficiaries. However, it restricts the scope of the evaluation on two margins. First, the data has information on girls born only up to 32 months after the program rollout. Assuming better implementation and community awareness with time, the lack of data on later program beneficiaries might underestimate the program's impact. Second, the expected program beneficiaries are too young at the time of the survey to enable the examination of a full range of marital and fertility outcomes that go beyond age 18 and other relevant long-run outcomes.

6. Conclusion

This work evaluates the effectiveness of *ABAD* in achieving its primary objective of delaying girls' marriage and other related outcomes such as fertility, schooling, and gender norms. The work contributes to the growing body of evidence on the effectiveness of cash transfer policies in eliminating child marriage and reducing early childbearing. It demonstrates that cash transfers conditioned on girls remaining unmarried can reduce child marriage but is limited in scope to achieve other critical development outcomes. Significantly, it adds to the actionable evidence base in the South–Asian region, which has the highest concentration of child brides globally, by examining a policy implemented at scale by a state government in India and replicable in other countries, especially those within the subcontinent. At the same time, the study highlights the importance of future research on strengthening the education program component that has the potential to improve not only girls' education but also fertility decisions and gender norms.

Tables and Figures

Table 3–1: Sample Characteristics of women born between 1992 and 1997.

	Panel A: Punjab	Panel B: Haryana		
Sample Characteristics	All	All	Before	After
Married before 18 (%)	8.78	15.57	17.87	12.65
Child Birth before 18 (%)	2.25	4.51	5.79	2.89
Marriage to first birth interval	17.24	19.66	19.78	19.13
(months (sd))	(8.70)	(10.66)	(10.68)	(10.56)
Caste group (%)				
General	2.76	3.39	3.57	3.16
Scheduled Caste (SC)	68.15	38.58	38.01	39.31
Scheduled Tribe (ST)	0.05	0.27	0.32	0.21
Other Backward Classes (OBC)	29.04	57.75	58.09	57.32
Religious group (%)				
Hindu	32.54	87.07	87.55	86.46
Muslim	1.98	9.63	8.72	10.79
Others*	65.49	3.30	3.74	2.75
Below Poverty Line Card (%)	21.78	32.83	30.86	35.33
Wealth Quintiles (%)				
Poorest (Wealth Quintile 1)	0.55	1.67	1.68	1.65
Poor (Wealth Quintile 2)	4.09	7.81	6.44	9.55
Middle (Wealth Quintile 3)	17.37	20.14	18.35	22.41
Rich (Wealth Quintile 4)	33.96	29.74	28.86	30.86
Richest (Wealth Quintile 5)	44.03	40.64	44.67	35.53
Urban (%)	29.46	29.29	30.43	27.84
Education (years completed (sd))	9.63	9.68	9.70	9.67
	(4.24)	(4.38)	(3.98)	(4.67)
Household Head Female	10.80	8.93	8.83	9.07
Sample Size (N=5,478)	2,176	3,302	1,847	1,455

Notes: Estimates obtained from the analytical sample using NFHS-4 2015-2016.
Analytical sample restricted to all women born 32 months before and after the policy change and are aged over 18 at the time of interview to avoid censoring (we do not know whether an individual got married or had her first child before she turned 18 at the time of the survey).
Wealth quintiles are calculated using wealth factor scores across the country.
*Other religions include Jain, Buddhist, Christian, Sikh, and Parsi.

Table 3–2: Coefficients for *ABAD's* effect on the probability of marriage before age 18 and childbearing before 18 among girls who meet the program's eligibility criteria.

	Marriage<18		Childbearing<18	
	(1) Without lag (95% CI [a])	(2) With lag (95% CI [a])	(3) Without lag (95% CI [a])	(4) With lag (95% CI [a])
Intercept	39.11	35.35	15.29	14.81
	(24.86, 52.90)	(21.52, 50.22)	(6.27, 26.48)	(4.87, 25.50)
Post-Policy Period	1.64	–1.59	1.88	–0.82
	(–3.50, 6.94)	(–5.88, 3.05)	(–0.60, 4.81)	(–3.19, 1.91)
Haryana	6.69	6.92	1.93	2.16
(*base: Punjab*)	(3.90, 9.18)	(3.96, 9.66)	(0.17, 3.45)	(0.37, 3.94)
Haryana*Post-Policy	–4.19	–6.81	–2.78	–3.46
	(–7.63, –0.94)	(–10.66, –2.88)	(–4.63, –0.98)	(–5.57, –1.42)
Religion				
Muslim	5.12	5.07	2.75	1.62
	(0.87, 10.08)	(0.19, 10.14)	(–0.29, 6.21)	(–1.61, 5.20)
Others	–1.16	–0.97	–1.63	–1.62
(*base: Hindu*)	(–3.50, 1.29)	(–3.60, 1.53)	(–3.03, –0.42)	(–3.15, –0.29)
Wealth Quintile				
Poor (WI 2)	–19.94	–17.88	–8.54	–8.88
	(–32.49, –6.57)	(–32.65, –5.76)	(–19.31, –0.08)	(–18.60, 0.40)
Middle (WI 3)	–20.95	–16.73	–9.59	–8.92
	(–33.56, –8.55)	(–30.21, –4.30)	(–19.63, –1.61)	(–18.76, 0.21)
Rich (WI 4)	–25.13	–21.41	–10.32	–9.94
	(–37.79, –13.35)	(–34.81, –8.86)	(–20.72, –1.90)	(–19.60, –0.55)
Richest (WI 5)	–29.86	–25.68	–12.03	–11.34
(*base: Poorest, WI 1*)	(–41.65, –16.48)	(–38.63, –13.17)	(–22.25, –3.77)	(–20.95, –2.02)
Urban	1.96	1.70	0.52	0.14
	(–0.03, 3.98)	(–0.26, 4.07)	(–0.61, 1.60)	(–1.08, 1.43)
Below Poverty Line	–0.41	0.18	–0.98	–1.03
	(–2.66, 1.84)	(–2.20, 2.81)	(–2.19, 0.21)	(–2.23, 0.39)
Household Head	–2.35	–1.99	–0.60	–0.60
Female	(–5.08, 1.01)	(–2.20, 2.81)	(–2.22, 1.08)	(–2.45, 1.28)
Caste				
Scheduled Caste	–1.16	–1.25	0.34	0.68
(SC)	(–6.48, 3.98)	(–7.55, 4.68)	(–2.60, 2.45)	(–2.45, 2.97)
Scheduled	–7.06	–7.05	–3.25	–2.86
Tribe (ST)	(–21.46, 23.96)	(–21.92, 20.95)	(–6.76, –0.63)	(–6.29, –0.21)
Other Backward	–0.05	–0.24	1.07	1.13
Castes (OBC)	(–5.61, 5.39)	(–7.20, 5.49)	(–1.91, 3.18)	(–2.08, 3.52)
(*base: General Caste*)				
Household Head				
Primary	–0.39	–0.60	1.08	0.71
	(–3.45, 2.39)	(–3.78, 2.47)	(–0.55, 2.70)	(–0.97, 2.58)
Secondary	–1.61	–2.12	0.05	–0.19
	(–3.95, 0.61)	(–4.63, 0.20)	(–1.14, 1.16)	(–1.51, 1.04)
Higher	–8.72	–9.42	–1.96	–2.71
(*base: No Education)*	(–12.07, –5.09)	(–13.11, –5.70)	(–3.89, 0.22)	(–4.55, –0.43)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	5,478	4,489	5,478	4,489

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).

Table 3–3: Coefficients for *ABAD's* effect on secondary outcomes of interest.

	(1) Marriage to First Birth Interval (95% CI [a])	(2) Complete Grade 5 (95% CI [a])	(3) Complete Grade 8 (95% CI [a])	(4) Son Preference (95% CI [a])
Panel A: Without Lag				
Intercept	23.53	36.68	21.78	15.70
	(17.23, 29.09)	(25.06, 48.30)	(11.54, 32.76)	(3.01, 28.10)
Post-Policy Period	4.03	0.52	2.87	–2.83
	(–0.16, 8.14)	(–3.75, 4.82)	(–2.93, 8.73)	(–7.30, 1.85)
Haryana	1.33	0.61	4.54	1.75
	(–0.46, 2.76)	(–2.56, 3.52)	(1.01, 8.18)	(–7.30, 1.85)
Haryana*Post-Policy	–1.92	0.61	1.27	–1.69
	(–4.36, 0.68)	(–2.56, 3.52)	(–3.07, 5.36)	(–4.68, 2.36)
Observations	1,484	5,478	5,478	5,478
Panel B: With Lag				
Intercept	22.34	32.80	15.59	17.99
	(15.42, 29.26)	(21.23, 46.21)	(5.42, 26.79)	(5.19, 33.02)
Post-Policy Period	–1.81	9.31	17.96	0.30
	(–5.55, 1.93)	(5.82, 12.92)	(12.65, 23.14)	(–4.04, 4.31)
Haryana	1.43	1.34	4.80	1.92
	(–0.21, 3.08)	(–1.62, 3.74)	(1.38, 8.55)	(–1.12, 4.75)
Haryana*Post-Policy	–2.94	–0.10	–0.27	–1.62
	(–6.63, 0.76)	(–3.20, 3.36)	(–5.10, 4.20)	(–5.48, 2.08)
Observations	1,292	4,489	4,489	4,489

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).

Notes: All models control for year fixed effects, religious affiliation, caste group, wealth status, urban/rural residence, BPL status, sex of the household head, and household head's highest level of education completed.

Table 3–4: Coefficients for *ABAD's* effect on the probability of marriage and childbearing before age 18 among Hindu girls who meet the program's eligibility criteria and girls who belong to SC/OBC households only.

	Marriage<18		Childbearing<18	
	(1) Without lag (95% CI [a])	(2) With lag (95% CI [a])	(3) Without lag (95% CI [a])	(4) With lag (95% CI [a])
Panel A: Eligible Hindu Girls				
Intercept	45.57	43.84	18.57	22.11
	(26.11, 65.63)	(22.25, 66.09)	(5.20, 36.08)	(6.00, 41.86)
Post-Policy Period	2.50	–1.83	3.80	–0.42
	(–4.34, 9.16)	(–7.97, 4.62)	(–0.27, 7.93)	(–4.50, 3.66)
Haryana	8.55	8.52	2.62	2.71
	(5.32, 12.20)	(4.76, 11.52)	(0.51, 4.85)	(0.14, 4.76)
Haryana*Post-Policy	–6.29	–7.90	–4.55	–4.75
	(–10.77, –1.46)	(–13.26, –2.35)	(–7.63, –1.49)	(–8.31, –1.19)
Observations	3,583	2,945	3,583	2,945
Panel B: Only SC/OBC Girls				
Intercept	37.92	34.87	14.84	15.57
	(24.28, 50.28)	(20.72, 48.72)	(5.90, 24.45)	(5.72, 27.31)
Post-Policy Period	2.28	–1.56	1.90	–0.70
	(–3.24, 8.19)	(–5.89, 2.81)	(–0.76, 4.95)	(–3.17, 2.07)
Haryana	6.90	7.18	1.91	2.15
	(4.18, 9.88)	(4.35, 10.28)	(0.27, 3.71)	(0.25, 3.87)
Haryana*Post-Policy	–4.35	–6.95	–2.81	–3.44
	(–8.00, –0.69)	(–11.04, –3.19)	(–4.70, –1.01)	(–5.61, –1.17)
Observations	5,306	4,347	5,306	4,347
Panel C: Only Bordering Districts				
Intercept	8.94	10.05	14.36	14.62
	(–13.99, 46.95)	(–12.14, 54.80)	(–4.15, 50.13)	(–3.89, 49.59)
Post-Policy Period	–3.00	1.67	–1.30	–2.11
	(–10.95, 5.76)	(–6.67, 10.24)	(–4.11, 1.56)	(–6.53, 2.90)
Haryana	5.11	5.36	–0.99	–0.65
	(0.92, 9.77)	(1.04, 10.70)	(–3.96, 1.94)	(–3.71, 2.76)
Haryana*Post-Policy	–6.30	–7.14	–2.78	–2.40
	(–12.25, –0.22)	(–13.66, –0.68)	(–6.28, 0.47)	(–6.28, 1.81)
Observations	1,776	1,492	1,776	1,492

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).

Notes: All models control for year fixed effects, religious affiliation, caste group, wealth status, urban/rural residence, BPL status, sex of the household head, and household head's highest level of education completed.

Table 3–5: Coefficients for *ABAD's* effect on secondary outcomes of interest among Hindu girls who meet the program's eligibility criteria and girls who belong to SC/OBC households only.

	(1) Marriage to First Birth Interval (95% CI [a])	(2) Complete Grade 5 (95% CI [a])	(3) Complete Grade 8 (95% CI [a])	(4) Son Preference (95% CI [a])
Panel A: Eligible Hindu Girls				
Intercept	26.18	29.49	9.35	18.71
	(17.43, 35.83)	(12.91, 46.58)	(–5.03, 25.73)	(2.16, 36.61)
Post-Policy Period	7.06	–0.13	–0.14	–3.62
	(–0.01, 13.76)	(–5.66, 4.81)	(–7.98, 8.25)	(–9.46, 2.35)
Haryana	2.58	2.27	4.08	0.10
	(0.70, 4.27)	(–0.73, 5.68)	(0.04, 8.58)	(–3.47, 3.54)
Haryana*Post-Policy	–4.58	1.79	5.55	–0.81
	(–9.13, –0.29)	(–2.94, 6.27)	(–0.65, 11.40)	(–5.53, 4.34)
Observations	958	3,583	3,583	3,583
Panel B: Only SC/OBC Girls				
Intercept	22.88	34.17	17.43	19.29
	(17.86, 28.33)	(24.15, 44.91)	(9.53, 26.56)	(8.25, 31.28)
Post-Policy Period	4.08	0.55	2.86	–3.86
	(–0.40, 8.59)	(–4.14, 4.64)	(–3.94, 8.97)	(–8.54, 0.79)
Haryana	1.27	1.17	5.00	1.67
	(–0.43, 2.70)	(–1.24, 4.12)	(1.30, 8.30)	(–1.27, 4.32)
Haryana*Post-Policy	–2.00	0.40	0.60	–1.72
	(–4.49, 0.33)	(–2.99, 3.37)	(–3.86, 4.68)	(–5.09, 1.96)
Observations	1,438	5,306	5,306	5,306
Panel C: Only Bordering Districts				
Intercept	34.34	25.44	18.00	9.56
	(28.98, 40.38)	(–9.60, 71.58)	(–17.92, 69.28)	(–10.69, 49.16)
Post-Policy Period	0.09	3.03	10.08	–7.78
	(–5.97, 4.52)	(–3.70, 8.87)	(1.09, 20.88)	(–14.68, –1.83)
Haryana	0.26	1.07	5.31	–0.00
	(–2.48, 2.66)	(–2.94, 5.11)	(–1.06, 11.00)	(–4.92, 4.40)
Haryana*Post-Policy	–3.26	–0.43	–1.46	–2.17
	(–6.72, 1.10)	(–5.10, 5.49)	(–8.86, 5.45)	(–7.19, 3.58)
Observations	452	1,776	1,776	1,776

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).

Notes: All models control for year fixed effects, religious affiliation, caste group, wealth status, urban/rural residence, BPL status, sex of the household head, and household head's highest level of education completed.

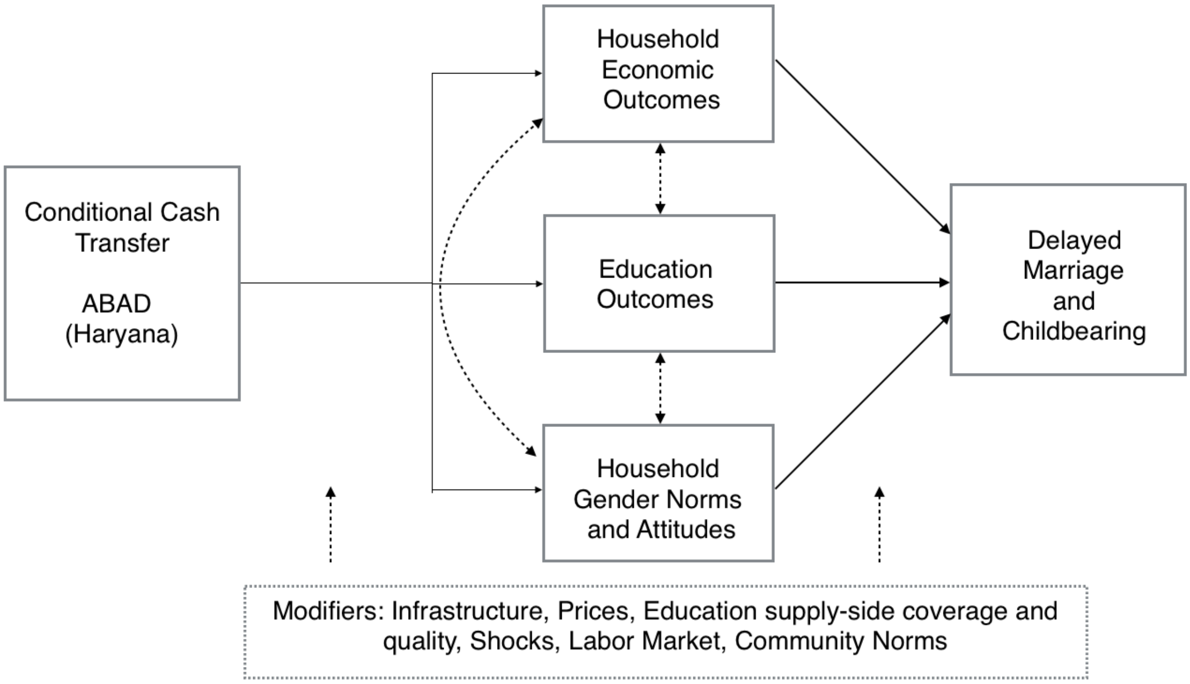


Figure 3–1: Conceptual framework for the effect of *ABAD*, a CCT program, on timing of girl's marriage and childbearing outcomes.

Notes: Modified from Handa et al. (2015) and UNICEF (2015)

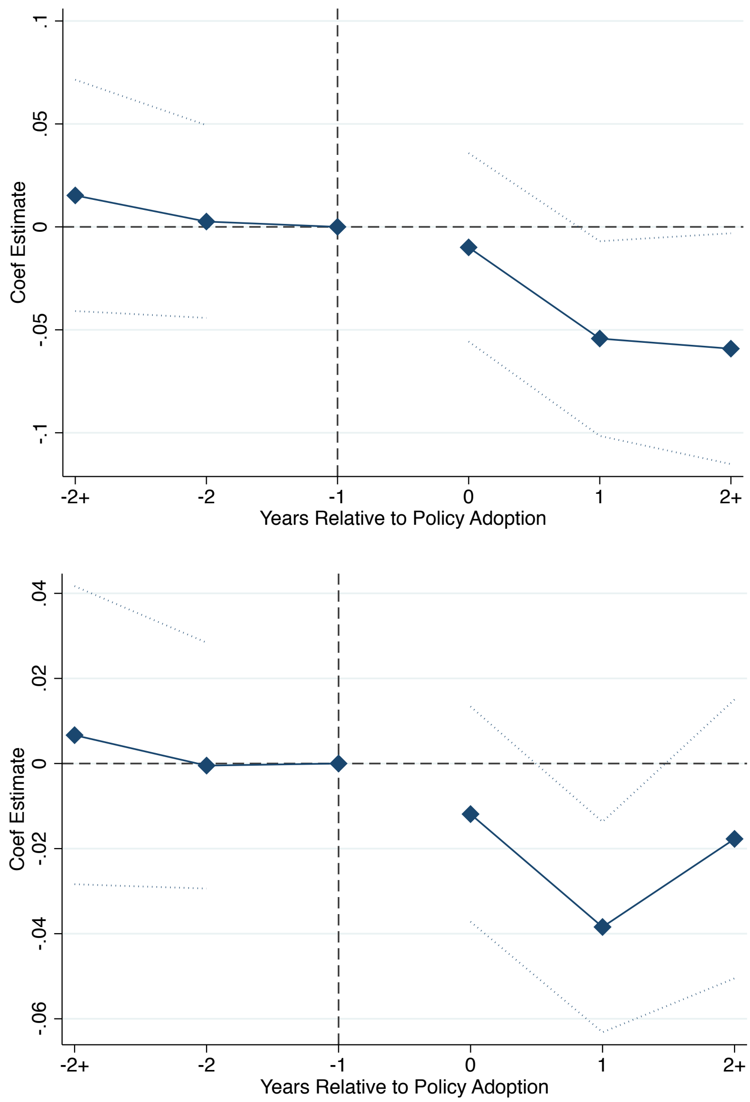


Figure 3–2: Event Study graphs for the probability of marriage before 18 (Above) and the probability of childbearing before 18 (Below).

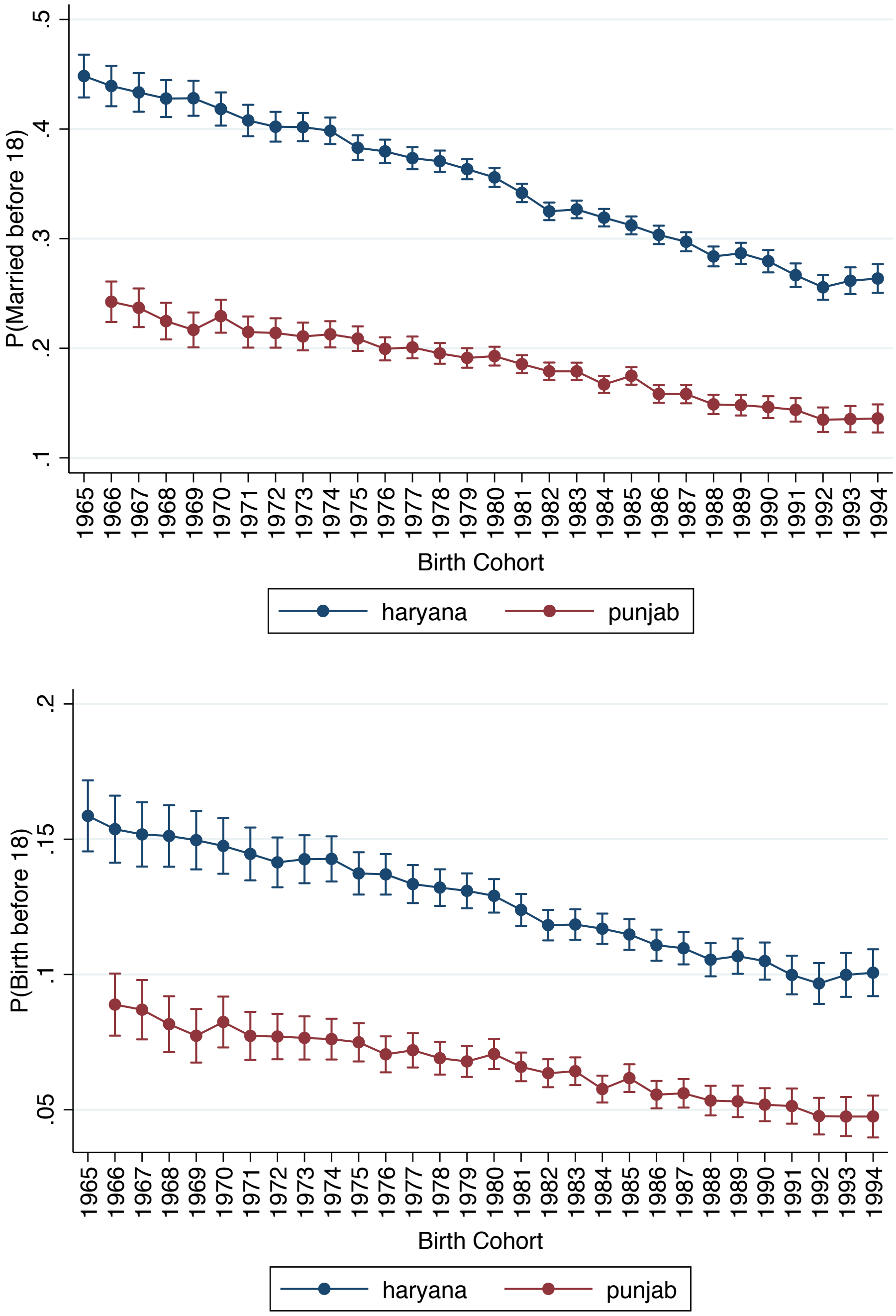


Figure 3–3: Pre-policy trends of marital and fertility outcomes by treatment status.

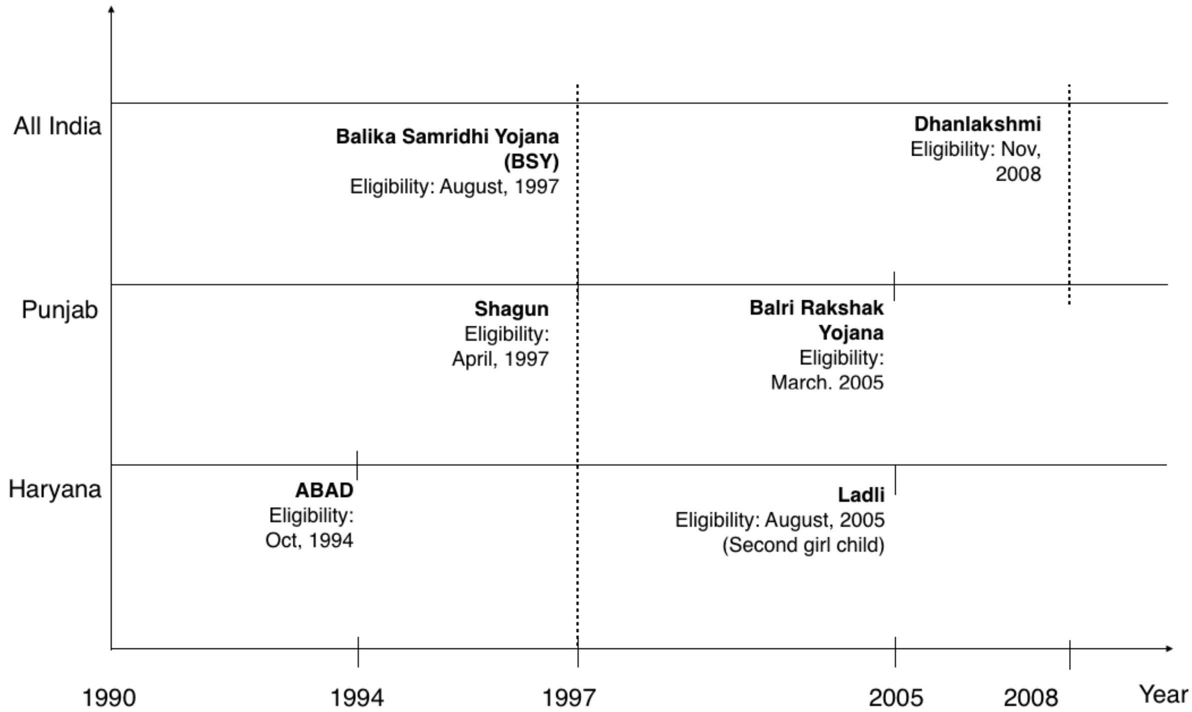


Figure 3–4: Implementation of policies addressing marriage of the girl child during the study period.

APPENDIX

Table 3–A: Sample characteristics of cases that have complete versus incomplete information.

Sample Characteristics	Complete Cases	Incomplete Cases
Caste group (%)		
General	3.1	1.9
Scheduled Caste (SC)	50.3	48.1
Scheduled Tribe (ST)	0.2	0
Other Backward Classes (OBC)	46.4	44.2
Religious group (%)		
Hindu	65.4	59.6
Muslim	6.6	11.5
Others*	28.0	28.9
Below Poverty Line Card (%)	28.4	34.6
Wealth Quintiles (%)		
Poorest (Wealth Quintile 1)	1.2	1.9
Poor (Wealth Quintile 2)	6.3	11.5
Middle (Wealth Quintile 3)	19.0	13.5
Rich (Wealth Quintile 4)	31.4	40.4
Richest (Wealth Quintile 5)	42.0	32.7
Urban (%)	29.4	34.6
Education (years completed (sd))	9.7 (4.3)	8.1 (4.4)
Household Head Female	9.7	15.4
Sample Size	5,478	52

Chapter 4

Child Marriage, Early Childbearing, and Political Quotas for Women: Evidence from Sub-Saharan Africa

1. Introduction

Women's exclusion from politics is a worldwide phenomenon. Despite attempts to make political positions gender-equitable, women continue to remain underrepresented relative to their share of the population. In 1997, women merely held 11.7% of parliamentary seats globally, which increased to 24.9% by January 2020 (World Bank, 2022). While much remains to be done, political quotas for women are often seen as a solution to improve women's share in political positions. During the fourth world conference on women in 1995, the Beijing Platform for Action set a target of 30% women in decision-making positions in the government. By 2018, globally, 15 countries adopted reserved seats (RS) quotas for women in their national parliaments (Clayton and Zetterberg, 2018).

Leadership and governance structures play a crucial role in shaping policy outcomes. Research shows that women and men policymakers have different policy priorities, and political quotas for women can increase women's ability to participate in the policymaking process and influence outcomes (Andersen et al., 2008; Chattopadhyay and Duflo, 2004; Edlund and Pande, 2002). Previous studies have shown positive effects of women's political representation on a wide range of outcomes such as maternal and child mortality, children's health outcomes, girls' educational attainment, and women's empowerment (Bhalotra and Clots-Figueras, 2014; Bhalotra et al., 2021; Macmillan et al., 2018; Quamruzzaman and Lange, 2016; Swiss et al., 2012). However, very little is known about the effect of reserved seats for women on child marriage and early childbearing. Given that these events fundamentally concern women and their children, women policymakers might help foster an enabling legal and policy environment to reduce their occurrence. Many systematic reviews and reports on programs and policies aimed at preventing child marriage indicate that a favorable policy environment can help increase investments in favor of the girl child and adopt laws that protect their human rights (Jain and Kurz, 2007; Lee-Rife et al., 2012; Malhotra et al., 2011).

Political quotas for women have the potential to influence child marriage and early childbearing through several possible pathways. First and foremost, political quotas for women in parliaments might affect child marriage and early childbearing through a shift in gender norms and attitudes toward women. A few associational studies point toward this hypothesis. Beaman et al. (2012) found that the gender gap in career aspirations was reduced in both parents and adolescents in villages with a female leader for two election cycles. They also report less time spent by girls on household chores. Because they found no evidence for changes in the labor market, they attribute the change in aspirations to the greater presence of women leaders who serve as role models. Furthermore, Priebe (2017) found greater political participation and increased empowerment levels among socially disadvantaged women in villages with a female leader. These positive deviants' leadership characteristics and role modeling could reduce child marriage (Greene, 2014). While these relaxations in traditional gender norms are encouraging, cultural norms might take some time to adjust (Beaman et al., 2009). Nevertheless, long-term changes in social norms are critical to bringing lasting solutions to the problems of child marriage and early childbearing. The resulting voice and agency might prove transformative in how households make marital and fertility decisions in the long run.

Successful passage and implementation of laws and policies depend on the political will, commitment, and governance mechanisms. Greater women's political representation might influence the participation and accountability of policy actors. Women policymakers and leaders are more likely to pass and implement interventions that advance the concerns of their gender (Jayal, 2006; Taylor-Robinson and Heath, 2003). Chattopadhyay and Duflo (2004) provide robust evidence that the reservation of village council seats for women affects the type of public goods provided depending on the needs voiced by their gender. While one causal study finds that women representatives tend to invest more in healthcare (Clayton and Zetterberg, 2018), the evidence is mixed for investments in education (Clots-Figueras, I., 2012; Halim et al., 2016). Greater women's political participation might reduce child marriage and early childbearing if it leads to the passage of gender-equitable laws and increased expenditures in policy areas known to reduce these outcomes.

Furthermore, active legislative leadership and engagement can help better implement existing laws and policies. Beyond drafting interventions, women political leaders might set up institutional oversight mechanisms, shape the political discourse, and engage civil society, community leaders, and the media. Women political leaders might not only shape how much and where resources need to be allocated but also influence the effectiveness of the provision of these resources to the public. Over the years, many child marriage prevention programs have adopted multiple strategies such as community mobilization, efforts to increase awareness among the community members and government officials about the negative consequences of early marriage and childbearing, and advocating for better implementation of existing laws. A higher presence of women political leaders might play a powerful modifying role in helping these programs reach their objectives.

While a greater number of women representatives can influence policy outcomes through direct negotiation and voting rights, the adoption of political quotas might itself affect outcomes as a potential signaling mechanism (Clayton and Zetterberg, 2018). The process of quota adoption is typically accompanied by legislative quota debates and engagement with a diverse group of stakeholders, including women's rights organizations, civil rights activists, and policy advocates. This process may bring gender equality to the center stage and lead to a re-prioritization of policies among existing legislative members, both men and women.

While many studies have shown the link between political quotas and the change in spending priorities, provision of public goods, women and children's health, and other development outcomes such as education and empowerment, only one study has examined its impact on the timing of marriage. Castilla (2018) studied India's landmark Constitutional Amendment Acts, 1993, which reserved a third of all village council head positions (Pradhan) for women. Exploiting the rotation system for the random selection of one-third of districts that elect a woman head of the village council in each election cycle, the author finds that districts with a woman leader witnessed a decrease in the likelihood of child marriage and delayed age at first marriage.

A large majority of the current evidence in this area comes from studying the increase in women representatives and leaders in local self-governance bodies within a decentralized setup in India. Building upon this evidence, it is essential to determine how political reservations for women in national parliaments affect the likelihood of child marriage and early childbearing across settings. The impact of women leaders at the national level rather than village councils who are acting members of legislatures rather than heads of local self-governance bodies could be different and needs further investigation (Bhalotra and Clots-Figueras, 2014).

Political quotas in the form of reserved seats for women in legislatures are an effective tool for increasing the presence of women representatives in legislative bodies. Tripp and Kang (2008) find that globally the introduction of quotas, mainly reserved seats for women, offers the most explanatory power for the increase in women's presence in national legislatures. Nevertheless, progressive reforms following more women representatives in decision-making bodies are not always guaranteed universally (Goetz, 2003). Women elected to quota seats often face barriers within male-dominated governance structures, especially when they attempt to reform policies that challenge traditional male authority (Clayton, 2021). Therefore, while women legislative members might advocate for reforms, it might not lead to tangible changes in the policy output (Devlin and Elgie, 2008; Longman, 2006; Yoon, 2011).

The effectiveness of women representatives might also be determined by whether quotas were adopted due to bottom-up grassroots women's movements or top-down political pressure. Clayton (2012) argues that reserved seats for women might undermine effectiveness by removing women from the sphere of contestations as men and making them either run on redundant seats or be appointed by existing male-dominated parties. The quota system is also marred by tokenism, wherein many women candidates are representatives of their husbands or those who are the least likely to challenge existing structures rather than politically empowered women connected to women's organizations (Hillman, 2018). Besides, gender quotas can also lead to a backlash. Two causal studies provide compelling evidence in this regard. Brulé (2020) reports male resistance to better enforcement of gender-equalizing inheritance rights by local women leaders. Beaman et al. (2009) find that gender quotas increase men's explicit bias against women leaders.

Therefore, the question of how gender quotas might affect child marriage and early childbearing is essentially an empirical one. This paper studies the policy consequences of reserved seats for women in national and sub-national governance on the likelihood of girls' marriage and childbearing before turning 18. An enabling legal and policy environment is often considered a potential solution to these problems, but there is very limited understanding of its specific workings. This study will add to the small body of evidence on the effectiveness of encouraging greater women's political participation in tackling these issues. As of March 2022, women held 26.1% of seats in national legislatures worldwide, with considerable variation across regions and countries. With most countries yet to achieve the target of 30% women in positions at decision-making levels in the government, it is further necessary to research whether political gender quotas have other downstream effects on development outcomes beyond increasing women's political representation.

While the representation of women in national legislatures increased steadily from 7.8% in 1990 to 25% in 2020 in the Sub-Saharan African region (Tripp and Kang 2008; the World Bank, 2022), I isolate the effect of the general trend by using the abrupt adoption of gender quotas by different countries across the region since the 1990s. I take advantage of the variation in the timing of the adoption of reserved seats for women at the national and sub-national levels of governance to estimate its effect on child marriage and early childbearing. I build cross-national evidence by comparing these outcomes across countries over time using quasi-experimental methods.

2. Methods

2.1. Data

I gathered longitudinal information on political quotas for women from the International Institute for Democracy and Electoral Assistance (International IDEA) database. It provides details on whether a country has gender-based political quotas in place, the type of quota introduced, and the level of governance structure (national or sub-national) it applies to. Using the relevant constitutional and legislative sources listed on their website, I established the year of reform and the threshold level of quota mandated for women in all countries in Sub-Saharan Africa. I further corroborated this information using the Inter-Parliamentary Union’s database on political quotas and the Quota Adoption and Reform Over Time (QAROT) database (Hughes et al., 2017).

I use the Demographic and Health Surveys (DHS) conducted between 1986 and 2019 for information on the timing of marriage and childbearing and other individual characteristics of women and their households. The DHS is typically collected every five years in over 90 developing countries and provides a reliable source for estimating nationally representative health and population trends. Using various survey instruments, it collects detailed information on women's marriage, fertility, reproductive history, education, socio-economic status, and type of residence, among many other women and household characteristics. For this analysis, the DHS provides an opportunity to consistently study the trends in marriage and childbearing across Sub-Saharan Africa over the study period, during which many countries introduced reservations for women in the national parliament.

The survey asks all female respondents aged between 15 and 49 years within the selected households to report the age at which they first got married or started living with a partner. Similarly, it asks them to report their age at the time of their first childbirth. I used this information to create two binary variables indicating whether a woman got married before she turned 18 and whether she had her first childbirth before turning 18. The use of age 18 as the cutoff was informed by the Sustainable Development Goals (SDG) target 5.3 to eliminate child marriage.

I combine the political quota database and the pooled DHS database for countries in Sub-Saharan Africa with available data. I first identify countries that introduced reserved seats for women in the national parliament. The data on the year of introduction, DHS survey years, and women's age at the time of the interview in each country help identify the treated countries with sufficiently available outcomes data. For this study, I consider all individuals under the age of 18 years at the time of the policy rollout to have been impacted by it. Girls over this age cutoff would have already made their marriage and childbearing decisions before reservations for women kicked in and thus considered not exposed to the policy change. To estimate the probability of these outcomes before age 18, I only include those girls and women who were at least 18 at the time of the interview in my analytical sample. Girls younger than this have only partial information up to their age at the time of the survey, and there is no way to determine their outcomes for when they would turn 18. Restricting the sample to those over 18 helps avoid biases due to this censoring of the outcomes data.

Table 4–1 presents all the countries with reserved seats for women at national and sub-national levels of governance and relevant information on the outcomes of interest for girls expected to be exposed to the policy change —these are the treated countries (N=6). Table 4–1 shows for each country the year and level of seats reserved at the national and subnational levels of governance, and the earliest birth cohort expected to be impacted by the policy change at each level. All other countries (N=13) that have relevant DHS data for girls and women born during the same period but did not mandate any kind of quotas for women, reserved seats, or legislated candidate quotas, comprise the pool of potential control countries. The study period spans women born during the period 1962 to 2000. The starting cutoff was determined by at least ten years of pre-policy data for the earliest birth cohort exposed in the study sample. Table 4–2 presents the DHS waves used in each treated country, the youngest birth cohort surveyed, and the number of birth cohorts surveyed after the policy reform.

2.2. Empirical Strategy

I use difference-in-differences (DID), a popular quasi-experimental research design, to study the effect of introducing political reservations for women on the timing of women's marriage and childbearing. As shown in Table 4–1, many countries introduced reservations for women at various times that were not randomly assigned. Although it is not a perfect alternative, DID offers a way to study the causal relationship in a setting where a randomized controlled trial (RCT) is not feasible. Similar to an RCT, the DID study design compares the changes in outcomes before and after the policy reform between treatment and control groups. In an RCT, it is easier to establish causality because it is likely that the treatment is independent of any other factors that are likely to affect the outcomes due to the treatment's random assignment. However, in a DID study, such confounding cannot be ruled out, and therefore a few assumptions are imposed to reduce the possibility of confounding. Gender quotas are likely to have direct impacts over and above women's numerical representation in governing bodies either through changes in the nature of political competition or through other mechanisms such as signaling. Therefore, I dropped the possibility of using other quasi-experimental methods such as the instrumental variables (IV) approach because it measures the impact of women's representation instead of quota adoption.

The DID design combines pooled OLS comparisons across countries over time and potentially corrects for their respective biases. By exploiting the natural variation in the timing of the reform across countries, it compares the within-country trends in outcomes before and after the reform between treated and control countries over the study period. Under the identifying assumption that the treated and control countries would have exhibited similar trends in marriage and childbearing in the absence of any reform, this design can be used to estimate the effect of political reservations for women. I estimate a two-way fixed effects linear regression model, as shown in equation (1):

$$Y_{ict} = \beta_0 + \beta_1 Policy_{ct} + \xi X_{ict} + \gamma_c + \delta_t + \varepsilon_{ict} \qquad (1)$$

where, Y_{ict} is the outcome of interest for individual *i* in country *c* in year *t*. γ_c is country fixed effect, δ_t is year fixed effect, X_{ict} represents relevant individual controls, and ε_{ict} is the error term. The variable $policy_{ct}$ represents a dummy variable that switches to value 1 when an individual living in country *c* and born in year *t* was exposed to the women's reservations policy reform.

The DID design with two-way effects for country and year accounts for potential confounders that are either time-invariant or country-invariant. The country fixed effects control for time-invariant differences between countries, and the year fixed effects control for secular time trends in the outcome of interest across countries. This specification identifies the treatment effect by comparing within-country changes in the outcomes before and after the policy in the treated countries to countries without such a policy. Therefore, the model controls for differences in country characteristics that do not change over time. This essentially accounts for the country's political history, democracy/authoritarianism, and political systems. For individual covariates, I control for the area of residence (urban/rural), age at the time of the interview, and the sex of the household head. These variables were collected at the time of the survey and not at the time I expect the policy to impact individuals. Even though these variables were essentially collected post-treatment, I do not expect them to be influenced by gender quotas.

Considerable research recommends using Linear Probability Models (LPM) instead of Logit or Probit models to avoid the complications associated with estimating and interpreting the resulting coefficients (Ai and Norton, 2003; Buchmueller and DiNardo, 2002; Cantor et al., 2012). I use LPM to estimate the impact of political quotas on the probability of marriage and childbearing before 18. The LPM coefficients are consistent, direct impact estimates in large samples and are measured in terms of the percentage point change in the probability of an event. I conduct all analyses in STATA 14.2.

Aggregated analysis

I first use equation (1) to carry out an aggregated analysis on a complete set of six treated and thirteen control countries together. These results estimate the average effect of quota adoption across countries in Sub-Saharan Africa. I cluster the standard errors by country following the recommendations by Bertrand et al. (2004). However, considering the wide range of the years in which quotas were introduced by each country, from 1989 in Uganda to 2010 in Kenya, there is a strong possibility that treated and control countries might followed different trends in outcomes during different periods. It highlights the analytical challenge associated with considerably different birth cohorts constituting the pre- and post-policy years. The aggregated analysis might mask these individual effects.

Disaggregated individual country analyses

Estimating the effect of political quotas across all the treated countries within a single model specification is challenging. There is a wide variation in the timing of introductions of these quotas across countries. Also, while the model controls for country fixed effects, the treated countries are different from one another, and one set of control countries may not necessarily be a good comparison for all of them. I, therefore, now analyze each treated country individually. I choose comparison countries from the pool of countries without quotas that are similar in terms of the World Bank's income classification of low-income or lower-middle-income countries, GDP per capita at the time of quota adoption, and official development assistance (ODA) at the time of quota adoption. I include ten years of the pre-quota period for each country and ensure that the treated and control countries have similar trends in marriage and childbearing outcomes during the pre-quota years. I report bias corrected bootstrapped confidence intervals instead of using clustered standard errors because there are fewer country clusters in the disaggregated analyses (Bertrand et al., 2004).

Heterogeneity in effects

As evident from Table 4–1, a few countries introduced gender quotas at the national and sub-national levels in one go, while a few others introduced quotas in a staggered manner. Rwanda, Tanzania, and Uganda used a staggered approach and extended quotas from the national to sub-national level or increased quota threshold levels over time. For each of these three countries, I divide the post-quota period into different phases of quota diffusion, keeping in mind the available sample sizes. For Rwanda, I use two phases — phase I (1986–1992) with a 30–48% quota at the national level and phase II (1993–2000) with an additional 30% quota at the sub-national level. Similarly, for Tanzania, phase I (1977–1987) has a 15–20% quota at the national level and a 25–33% quota at the sub-national level, and phase II (1988–2000) sees an increment in the quota at the national level to 30%. Lastly, the post-quota period in Uganda is divided into phase I (1972–1977) with a national level quota of 13%, phase II (1978–1993) with a national level quota between 18 and 21%, and a sub-national level quota at 33%, and phase III (1994–2000) that increases the national level quota threshold to 31%. Using dummies for each of the quota phases within these countries, I investigate whether further quota diffusion impacts child marriage and early childbearing.

Model Assumption Tests

The identifying assumption of the DID method, popularly known as the common trends assumption, is that the treated and control countries would have exhibited similar trends in marriage and childbearing had the treated countries not introduced political reservations for women. While this assumption is essentially untestable, I test its tenability in several ways. First, I graphically check for any differences in the trends in these outcomes during the pre-policy period. Second, I statistically test for any significant difference in these trends between treated and control countries before the introduction of quotas for women. If these pre-policy trends are similar, one might assume that these parallel trends would have continued without the policy change. I also check for competing explanations for the resulting estimates. DID coefficients are susceptible to other developments within countries that change over the course of the study period. I document and analyze the timing of minimum age marriage laws and other significant changes with respect to the adoption of gender quota policy in all the countries in my analytical sample to evaluate the possibility of confounding of the impact estimates.

I also conduct an event-study analysis to test the parallel trends assumption. I replace the DID interaction term with a set of leads and lags with respect to the introduction of quotas, ranging from 10 or more years before quota adoption to 10 or more years after. The leads and lags were coded 0 for all respondents residing in control countries. I estimate equation (2) below:

$$Y_{ict} = \beta_0 + \beta_1 Country_c + \sum_{l=-2}^{-10+} \beta_{lead}(RS_{il}) + \sum_{l=0}^{10+} \beta_{lag}(RS_{il}) + \xi X_i + \delta_t + \varepsilon_{it} \quad (2)$$

where the subscript *l* refers to the event period in one-year increments. The variable RS_{il} denotes a series of binary indicators that take the value 1 if individual *i* was exposed to reserved seats quota in period *l*. The tenth lead and lag terms include all years greater than 10. The first lead is the omitted base category, and this model estimates the relative difference in outcomes for leads and lags of gender quota adoption relative to the reference year, $l = -1$. These relative differences are captured by β_{lead} and β_{lag}. The β_{lead} coefficients provide a means to investigate any potential violation of the parallel trends assumption. The coefficients are expected to be close to 0 and not statistically significant if the trends in the outcomes were similar across the treated and control countries before quota introduction. The β_{lag} coefficients, on the other hand, help investigate the timing of the impact of quotas and whether the treatment effects vary over time.

3. Results

Aggregated results

Table 4–3 shows the link between adopting political quotas for women and women's probability of child marriage and early childbearing across Sub-Saharan Africa. The reported estimates measure the difference in predicted probabilities in percentage point terms. Column (1) reports the estimates from the base model controlling for country fixed effects and birth year fixed effects, and column (2) builds on this base model and controls for rural/urban residence, women's age at the time of the survey, and the sex of the household head. I find that there is no connection between the adoption of political quotas and women's marriage and childbearing outcomes when all countries are analyzed together.

Figure 4–1 plots the event study estimates carried out using equation (2) for the probability of marriage before 18 (left) and the probability of childbearing before 18 (right). Each point on the graph represents the effect of quota adoption on the outcome for the specified birth cohort relative to the birth cohort that just missed the adoption of quotas. The lead coefficients of over ten years before the policy change suggest pre-existing differences in the outcome trends between the countries that adopted quotas and those that did not. On the other hand, the lag coefficients indicate that the treatment effects might vary over time, thus potentially biasing the treatment effects (Goodman-Bacon, 2021). Also see Figure 4–A that indicates the wide variation in the trend in the outcomes of interest over the study period as well as in the birth cohorts that constitute pre-policy years across the treated countries. These figures highlight the analytical challenge associated with the aggregated analysis.

Disaggregated individual country analyses

Tables 4–4 and 4–5 present the effect of quota adoption on the probabilities of marriage and childbearing before 18 for each treated country analyzed separately. Table 4–4 shows the results for Burundi, Kenya, and Niger that rolled out gender quotas in one go at the national and sub-national levels of governance simultaneously. The coefficient estimates indicate that quota adoption did not affect the probability of marriage or childbearing before 18 in any country.

Table 4–5, on the other hand, shows the results for Rwanda, Tanzania, and Uganda that implemented gender quotas in a staggered manner. The single post-quota DID coefficients suggest that gender quotas increased the probability of child marriage in Rwanda by 2.4 percentage points, did not impact either marriage or childbearing in Tanzania, and decreased the probability of marriage and childbearing before 18 in Uganda by 4.2 and 3.3

percentage points, respectively. For reference, see table 4–A that indicates the pre-policy probabilities of marriage and childbearing before age 18 within each of the treated countries. Because these countries increased quota thresholds subsequently, I further examine the timing of the results by including separate dummy variables signifying different phases of quota rollouts.

I find that gender quotas had no impact in Rwanda during the first six years of quota adoption, i.e., during phase I. However, during phase II, when Rwanda extended gender quotas to the sub-national level, I find a sharp increase in the probability of child marriage by 6.5 percentage points and that of early childbearing by 4.4 percentage points. In Tanzania, gender quotas seem to have no impact on child marriage at any time, but they significantly reduce the probability of early childbearing by 1.9 percentage points during the second phase of quota adoption. In Uganda, during the first five years following quota adoption, there seems to be an uptick in the probability of child marriage by 2.0 percentage points and no effect on early childbearing. However, during the subsequent phases II and III, gender quotas drastically reduce the probability of child marriage and childbearing by 6.5–7.0 percentage points and 4.1–6.1 percentage points, respectively.

Model Assumption Tests

Figure 4–2 shows the probability of marriage and childbearing trends before 18 within each set of the treated and control countries during the pre-policy years. The pre-policy trends are similar in the control countries and the treated countries before any quota adoption. None of the coefficients on the interaction term between the time trend and treated/control countries was significant either. These findings support the assumption that these trends would have continued to remain similar afterward had there been no change in the policy. Furthermore, the results are essentially[1] robust to the choice of pre-policy years and the standard errors. As shown in table 4–7, the results remain stable if I include only five years of pre-policy data instead of the initial choice of ten years of pre-policy data and use Huber-White robust standard errors instead of the bootstrapped standard errors.

To examine the role of other coincident policy shifts, I closely study the presence and the timing of minimum age marriage laws in all the study countries. Of the six treated countries, I find Burundi and Rwanda to be early adopters of child marriage laws such that all girls included in the study period were subject to the laws. Uganda outlawed marriages before 18 for girls during the study period. Moreover, Kenya, Niger, and Tanzania did not change the minimum age of marriage during the study period. Comparing girls across countries with similar exposure to child marriage laws did not change the results.

Further, the wide variation in the results examining staggered policy rollout led me to investigate other critical events that might have coincided with the exposure to gender quotas in these countries. Rwanda witnessed a civil war in 1994, and Tanzania and Uganda abolished tuition fees that might have impacted girls born after 1989 and 1985, respectively. Research shows that the armed conflicts impact marriage and childbearing alongside other relevant outcomes such as intimate partner violence, sex ratio, and the age of sexual debut in Rwanda and beyond (Kraehnert et al., 2019; La Mattina, 2012; Neal et al., 2016; Staveteig, 2011). Moreover, I show in chapter 2 that tuition-free policies impact child marriage and early childbearing. Therefore, I restrict the analytical sample to girls born before any of these changes took place in these countries and re-estimate the policy effects to parse out the effect of these changes from those of gender quotas. The coefficients presented in Table 4–6 confirm that child marriage increased in Rwanda for girls born after the civil conflict. Also,

[1] Rwanda is the only exception where the coefficient for the effect on the probability of childbearing before 18 turns significant if only 5-years of pre-policy data is included in the analysis.

child marriage and early childbearing rates did not reduce after gender quota adoption until Tanzania and Uganda abolished tuition fees.

4. Discussion

This work examines the impact of political quotas for women on the probability of girls marrying or bearing children before 18 years of age. Taking advantage of the variation in the timing of quota adoption across various countries in Sub-Saharan Africa, I use quasi-experimental methods to compare the change in the probability of marriage and childbearing outcomes within and across countries over the study period. I further explore whether the policy effects vary across the treated countries, whether the level of quota diffusion matters, and whether other concurrent changes during the study period can explain these effects. Methodologically, this work is similar to many others that exploit the variation in the timing of policy adoption across geographical units—districts, states, or countries—using a difference-in-differences analytical strategy to study policy effects (Besley and Burgess, 2004; Bhalotra et al., 2021; Castilla, 2018; Wolfers, 2006).

The aggregated analyses, considering all the countries that adopted gender quotas together versus similar countries in the region that did not, suggest that gender quotas alone did not impact child marriage and early childbearing. However, significant individual country-level effects might still be masked within these overall results. The wide variation in the pre-policy trends and the timing and duration of pre-policy years across individual countries illustrate the complexity of an aggregated analysis. The disaggregated analyses considering each treated country one at a time provide more robust evidence on the effect of gender quotas on girls' marriage and childbearing outcomes. Here, I pair each treated country with a set of control countries that did not adopt gender quotas and that have a similar trend in the probability of marriage and childbearing before age 18 during the pre-policy period.

I find no effect of gender quotas on the probability of girls marrying and bearing children before 18 in Burundi, Kenya, and Niger that had a one-time abrupt change in the gender quota mandates. The results for countries that had a staggered rollout of gender quotas are more nuanced. On average, gender quotas increased the probability of child marriage in Rwanda, had no impact on these outcomes in Tanzania, and reduced the probability of child marriage and early childbearing in Uganda. Upon further examining the timing of these effects using different phases of quota diffusion and other concurrent events, I conclude that gender quotas do not seem to impact child marriage and early childbearing in short to medium term (approximately 7–12 years). It might not be possible to separate the effect of gender quotas from other critical policy changes in the longer run. These other policy changes might likely drive the results, especially since gender quotas seem to have had no impact in other countries. However, it deserves further investigation to draw any definitive conclusions.

It is striking that the null effects of gender quotas on child marriage and early childbearing hold across different levels of quota thresholds, political systems, and levels of democracy and economic development. I study countries with quota thresholds as low as 10% in Niger to as high as 48% in Rwanda. The follow-up years post-quota adoption also vary from 7 years in Kenya to almost 29 years in Uganda. I examine the quality of quota implementation by studying the provision of sanctions for noncompliance and the abrupt increase in the percentage of women parliamentarians following quota adoption. All the countries except Kenya had strong sanctions if gender quotas were not complied with. The percentage of women parliamentarians jumped by approximately 9 percentage points in Kenya to nearly 23 percentage points in Uganda during the election cycle following quota adoption. The way gender quotas are enforced also differs across countries. Women are elected through a district-wide electoral college of men and women in Uganda or by women in each district as in the case of Rwanda (Tripp and Kang, 2008). In contrast, Tanzania appoints preselected women on party lists to the reserved seats.

My findings are in contrast with previous work in this area. While Castilla (2018) found a reduction in child marriage and an increase in the age at marriage in response to gender quotas for the position of the village council head (Pradhan) within districts in India, I find no impact of gender quotas at the national and sub-national levels on girls' marriage and childbearing outcomes. Castilla (2018) examined the effect of a constitutional amendment in India in which the federal government devolved power to the district and village level governing bodies. Castilla (2018) draws upon the previous findings of Chattopadhyay and Duflo (2004) on the same constitutional amendment to attribute the results to the role model effect that girls and their families experience when they see women leaders heading the village councils. The reservations for women in village councils were considered a major success of the women's movement, which dramatically increased the involvement of women in decision-making bodies at the grassroots level (Bhalotra and Clots-Figueras, 2014).

These studies examine the effect of women's village-level council leadership and not women's representation in governing bodies. Despite some cross-sectional evidence on role modeling effect of women's representation in national legislatures in the form of greater women's civic and political engagement and political activism (Barnes and Burchard, 2013; High-Pippert and Comer, 1998; Wolbrecht and Campbell, 2007), there is no causal evidence on whether the greater presence of women in parliaments has a role-modeling effect on girls and women in general. Even if women's representation raises girls' aspirations, girls and their families might see those aspirations as conflicting with marriage.

It could also be the case that the subsequent rise in aspirations is not enough to challenge community and social norms around the timing of marriage and childbearing. Beaman et al. (2009) findings are especially pertinent in this regard. They find that although previous exposure to women leadership in village councils can change views regarding the effectiveness of women leaders and change voting decisions over time, eroding deep preferences and social norms remains challenging. They also find evidence for a 'backlash effect' where men were more likely to have an explicit distaste for female leadership when forced to elect women leaders. This has important implications for girls' marriage and childbearing outcomes as these are typically entrenched in deep-rooted cultural and social norms that dictate the role of women in society.

While there is definitive evidence for the prioritization of women and children's health by women policymakers, the effect of women's representation on other policy areas such as education and social welfare is not apparent (Clayton and Zetterberg, 2018; Clots-Figueras, 2012). Many studies show positive effects of women's representation on health outcomes and healthcare utilization (Bhalotra and Clots-Figueras, 2014; Bhalotra et al., 2021; Macmillan et al., 2018; Quamruzzaman and Lange, 2016; Swiss et al., 2012). This may result from an increase in healthcare spending (Clayton and Zetterberg, 2018) or a shift within pre-existing healthcare spending levels toward the health of women and children (Bhalotra and Clots-Figueras, 2014). However, research shows that gender may interact with other identities to shape education and social redistribution decisions. Many studies find that reserved seats for women held by the elites do not lead to the adoption of redistributive and social welfare policies in the interest of women (Clots-Figueras, 2011; Halim et al., 2016; Rai, 2002). Therefore, it is possible that greater women's representation by itself might not lead to the passage of laws and policies that influence the timing of girls' marriage and childbearing outcomes.

This work has several limitations. I restricted the analysis to nineteen countries in Sub-Saharan Africa that met my policy and outcomes data requirements. I included only those treated countries that introduced gender quotas in the form of reserved seats for women and compared them to control countries that did not have any form of gender quota in place. This excludes many countries that adopted other gender quotas, such as legislated candidate quotas and voluntary party quotas. Clayton and Zetterberg (2018) report that globally 15 countries have adopted reserved seat quotas for women in their national parliaments, another 43 have candidate list quotas, and 7 others have voluntary political party quotas. It might be worthwhile for future work in this area to examine the impact of these different types of gender quotas on child marriage and early childbearing.

Second, it is difficult to determine the age at which girls and their families start making decisions regarding marriage and childbearing. It is also challenging to establish the ideal age at which girls could be exposed to more women representatives in a way that influences their decisions and behaviors. Together, these weaken the identification of girls expected to be exposed to gender quotas and alter their behaviors related to marriage and childbearing. In the absence of more precise information on exposure, I consider all girls who are yet to attain the threshold age of 18 at the time of quota adoption as those who could still alter their behaviors such that their probability of marrying or bearing children before 18 could be affected. This might undermine the ability to precisely estimate the policy effect on the probability of marrying and bearing children before 18 years of age.

Moreover, it might take a long time for policy effects to show up on these downstream outcomes instead of more proximal outcomes of spending and budgetary allocations. The analyses provide convincing and robust evidence that gender quotas alone do not influence girls' marriage and childbearing outcomes in short- to medium-run. Further research is needed to evaluate the long-run effects of gender quotas in combination with other policies on the timing of girls' marriage and childbearing outcomes and on the potential mechanisms through which they might work. Qualitative research using in-depth interviews and focus groups might be particularly helpful in understanding the implementation and workings of gender quotas.

Tables and Figures

Table 4–1: Countries with reserved seats for women, the year and level of national and sub-national quotas, first birth cohorts expected to be exposed to the gender quotas.

	National		Sub-national	
Country	Reserved Seats (Year, Level)	Earliest Birth Cohort Exposed	Reserved Seats (Year, Level)	Earliest Birth Cohort Exposed
Burundi	2005, 30%	1988	2005, 30%	1988
Kenya [a]	2010, 33%	1993	2010, 33%	1993
Niger	2000, 10%	1983	2000, 10%	1983
Rwanda [b]	(2003, 30%); (2007, 48%)	1986	2010, 30%	1993
Tanzania [b]	(1995, 15%); (2000, 20%); (2005, 30%)	1978	(1994, 25%); (2000, 33%)	1977
Uganda [b]	(1989, 13%); (1995, 18%); (2001, 21%); (2011, 31%)	1972	(1995, 33%)	1978

[a] Kenya first introduced reserved 6 seats (1.7%) for women in the national parliament out of a total of 350 seats in 1997.
[b] Rwanda, Tanzania, and Uganda increased the threshold levels of quota for women in a staggered way.
Notes: The pool of control countries includes Benin, Cameroon, Central African Republic, Chad, Comoros, Ethiopia, Gambia, Ghana, Madagascar, Malawi, Mozambique, Nigeria, and Zambia.

Table 4–2: Countries with reserved seats for women, DHS waves used, youngest birth cohort interviewed by latest DHS and included in the sample, and years of exposure to the national and sub-national gender quotas.

Country	DHS Waves	Youngest birth cohort in the analytical sample [a]	Years of exposure to quota at the national level	Years of exposure to quota at the sub-national level
Burundi	1987, 2010, 2017	2000	13	13
Kenya	1989, 1993, 1998, 2003, 2008, 2014	1999	7	7
Niger	1992, 1998, 2006, 2012	1997	15	15
Rwanda	1992, 2000, 2005, 2010, 2015	2000	15	8
Tanzania	1991, 1996, 1999, 2005, 2010, 2016	2000	24	25
Uganda	1989, 1995, 2000, 2006, 2011, 2016	2000	29	23

[a] The DHS surveys in Burundi, Tanzania, and Uganda interviewed girls born up to the years 2003, 2001, and 2001 respectively. However, I restrict the sample to girls born up to the year 2000 considering limited sample sizes in the treated and control countries after the year 2000.

Table 4–3: The effect of introducing reserved seats for women on marital and fertility outcomes of women in all countries.

Outcomes	(1) Model 1 (Full sample) Policy Effect (95% CI [a])	(2) Model 2 = Model 1 + Controls Policy Effect (95% CI [a])
All countries (N=575,222)		
Marriage before 18	–0.13 (–3.08, 2.81)	–0.16 (–2.93, 2.60)
Child birth before 18	0.45 (–1.69, 2.59)	0.42 (–1.66, 2.49)
Country fixed effects	Yes	Yes
Birth year fixed effects	Yes	Yes

[a] 95% confidence intervals (CI). Standard errors clustered at the country level.
Notes: Model 2 controls for rural/urban residence, sex of the household head, and woman's age at the time of survey.

Table 4–4: The effect of introducing reserved seats for women on marital and fertility outcomes of women in countries with simultaneous rollout of gender quotas at the national and sub-national levels of governance.

Outcomes	Control Countries	(1) Marriage before 18 Policy Effect (95% CI [a])	(2) Child birth before 18 Policy Effect (95% CI [a])
Simultaneous Rollout			
Burundi (N=48,726)	Chad, Gambia, Madagascar	–1.37 (–3.05, 0.32)	–0.51 (–2.06, 1.07)
Kenya (N=85,368)	Benin, Ghana, Nigeria, Zambia	–0.28 (–1.90, 1.61)	0.08 (–1.50, 1.75)
Niger (N=160,451)	Chad, Ethiopia, Gambia, Madagascar, Malawi, Mozambique	0.08 (–1.39, 1.60)	0.88 (–0.65, 2.36)

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).
Notes: All models control for rural/urban residence, sex of the household head, and woman's age at the time of survey, country fixed-effects, and birth-year fixed effects.

Table 4–5: The effect of introducing reserved seats for women on marital and fertility outcomes of women in countries with staggered rollout of gender quotas at the national and sub-national levels of governance.

Treated country	Control countries and Quota threshold N=National, S=Sub-national	(1) Marriage before 18 Policy Effect (95% CI [a])	(2) Child birth before 18 Policy Effect (95% CI [a])
Staggered rollout			
Rwanda (N=*32,370; 101,102)*[b]	Comoros, Gambia, Nigeria, Zambia	2.44 (0.12, 4.70)	0.78 (–0.05, 1.82)
I (1986–1992)	N=30–48%	1.27 (–1.17, 3.67)	–0.01 (–0.99, 0.91)
II (1993–2000)	N=48%, S=30%	6.47 (3.01, 9.63)	4.36 (2.57, 5.93)
Reference: (1976–1985)			
Tanzania (N=162,913)	Cameroon, Nigeria, Zambia	0.46 (–0.56, 1.70)	–1.07 (–2.15, 0.00)
I (1977–1987)	N=15–20%, S=25–33%	0.30 (–1.01, 1.61)	–0.67 (–1.91, 0.57)
II (1988–2000)	N=30%, S=33%	0.80 (–0.78, 2.37)	–1.89 (–3.36, –0.42)
Reference: (1967–1976)			
Uganda (N=157,348)	Benin, Chad, Comoros, Gambia, Madagascar	–4.15 (–5.64, –2.79)	–3.29 (–4.61, –2.08)
I (1972–1977)	N=13%	2.04 (0.19, 3.62)	–0.56 (–2.34, 0.99)
II (1978–1993)	N=18–21%, S=33%	–6.51 (–7.86, –4.97)	–4.08 (–5.47, –2.62)
III (1994–2000)	N=31%, S=33%	–6.99 (–9.13, –4.77)	–6.13 (–8.43, –3.96)
Reference: (1962–1971)			

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).
[b] Rwanda was compared only with Gambia for marriage before 18 to satisfy the parallel trends assumption. Therefore, the sample size for marriage before 18 is 32,370 and for childbearing before 18 is 101,102.
Notes: All models control for rural/urban residence, sex of the household head, age at the time of survey, country fixed effects, and birth-year fixed effects.

Table 4–6: The effect of introducing reserved seats for women on marital and fertility outcomes of women considering only those cohorts who were not exposed to other competing changes in the treated countries.

Treated Country	Truncated Sample	(1) Marriage before 18 Policy Effect (95% CI [a])	(2) Child birth before 18 Policy Effect (95% CI [a])
Rwanda (N=(29,946; 94,672)[b]	Up to 1993 [c]	1.59 (–0.79, 4.23)	0.16 (–0.88, 1.03)
Tanzania (N=131,866)	Up to 1988 [d]	0.05 (–1.15, 1.37)	–0.78 (–1.84, 0.38)
Uganda (N=107,450)	Up to 1984 [e]	0.09 (–1.33, 1.54)	–0.67 (–2.14, 0.71)

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).
[b] Rwanda was compared only with Gambia for marriage before 18 to satisfy the parallel trends assumption. Therefore, the sample size for marriage before 18 is 29,946 and for childbearing before 18 is 94,672.
[c] Girls born during or after the civil war of 1994 are not part of the analytical sample.
[d] Girls exposed to tuition-free primary schooling are not part of the analytical sample.
[e] Girls exposed to tuition-free primary and secondary schooling are not part of the analytical sample.
Notes: All models control for rural/urban residence, sex of the household head, and woman's age at the time of survey, country fixed effects, and birth-year fixed effects.

Table 4–7: Sensitivity analyses showing the effect of introducing reserved seats for women at the national and sub-national levels of governance on marital and fertility outcomes of women in individual countries.

Outcomes	(1) 5-years Pre-policy data Policy Effect (95% CI [a])	(2) Robust standard errors Policy Effect (95% CI [b])
	Panel A: Simultaneous rollout	
Burundi		
Marriage before 18	–1.16 (–3.35, 0.89)	–1.37 (–3.10, 0.35)
Child birth before 18	–0.22 (–2.01, 1.55)	–0.51 (–2.06, 1.05)
Sample Size	*35,112*	*48,726*
Kenya		
Marriage before 18	0.90 (–1.21, 2.88)	–0.28 (–2.04, 1.49)
Child birth before 18	0.54 (–1.33, 2.38)	0.08 (–1.64, 1.80)
Sample Size	*46,287*	*85,368*
Niger		
Marriage before 18	–0.18 (–1.87, 1.46)	0.08 (–1.37, 1.54)
Child birth before 18	0.13 (–1.45, 2.05)	0.88 (–0.73, 2.49)
Sample Size	*120,597*	*160,451*
	Panel B: Staggered rollout	
Rwanda		
Marriage before 18	1.91 (–0.98, 4.80)	2.44 (–0.00, 4.87)
Sample Size	*23,856*	*32,370*
Child birth before 18	1.45 (0.38, 2.52)	0.78 (–0.14, 1.70)
Sample Size	*73,558*	*101,102*
Tanzania		
Marriage before 18	0.84 (–0.66, 2.35)	0.46 (–0.72, 1.64)
Child birth before 18	–1.09 (–2.36, 0.29)	–1.07 (–2.16, 0.03)
Sample Size	*136,027*	*162,913*
Uganda		
Marriage before 18	–4.38 (–6.01, –2.77)	–4.15 (–5.51, –2.80)
Child birth before 18	–3.71 (–5.26, –2.23)	–3.29 (–4.61, –1.97)
Sample Size	*142,458*	*157,348*

[a] 95% Bias-corrected bootstrapped confidence intervals (CI).

Notes: All models control for rural/urban residence, sex of the household head, and woman's age at the time of survey, country fixed effects, and birth-year fixed effects.

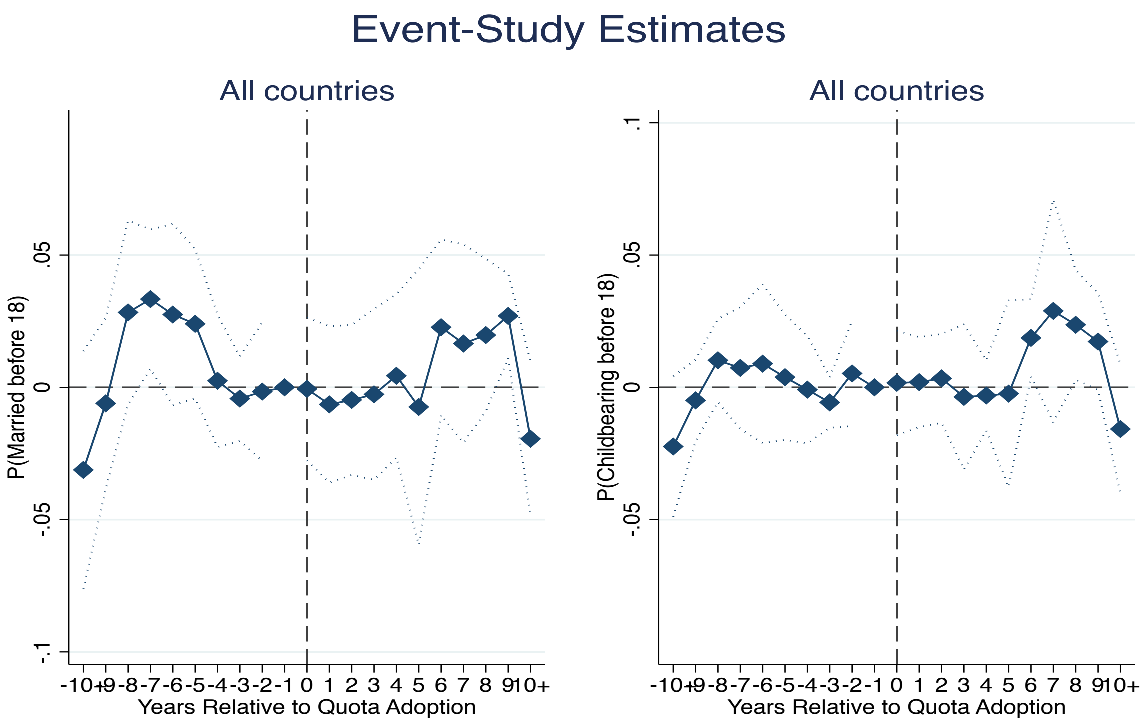


Figure 4–1: Event Study graphs for the probability of marriage before 18 (Left) and the probability of childbearing before 18 (Right).

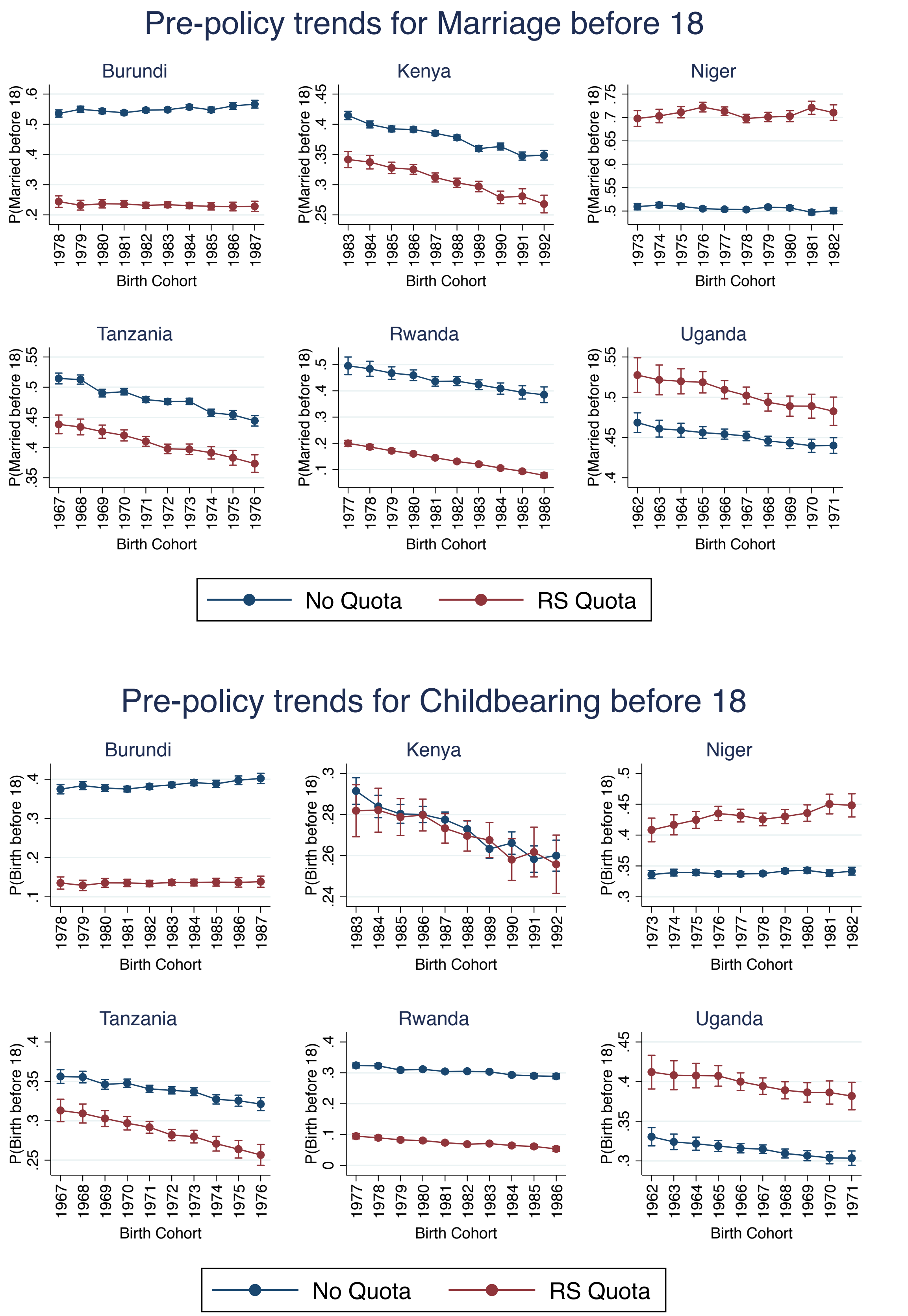


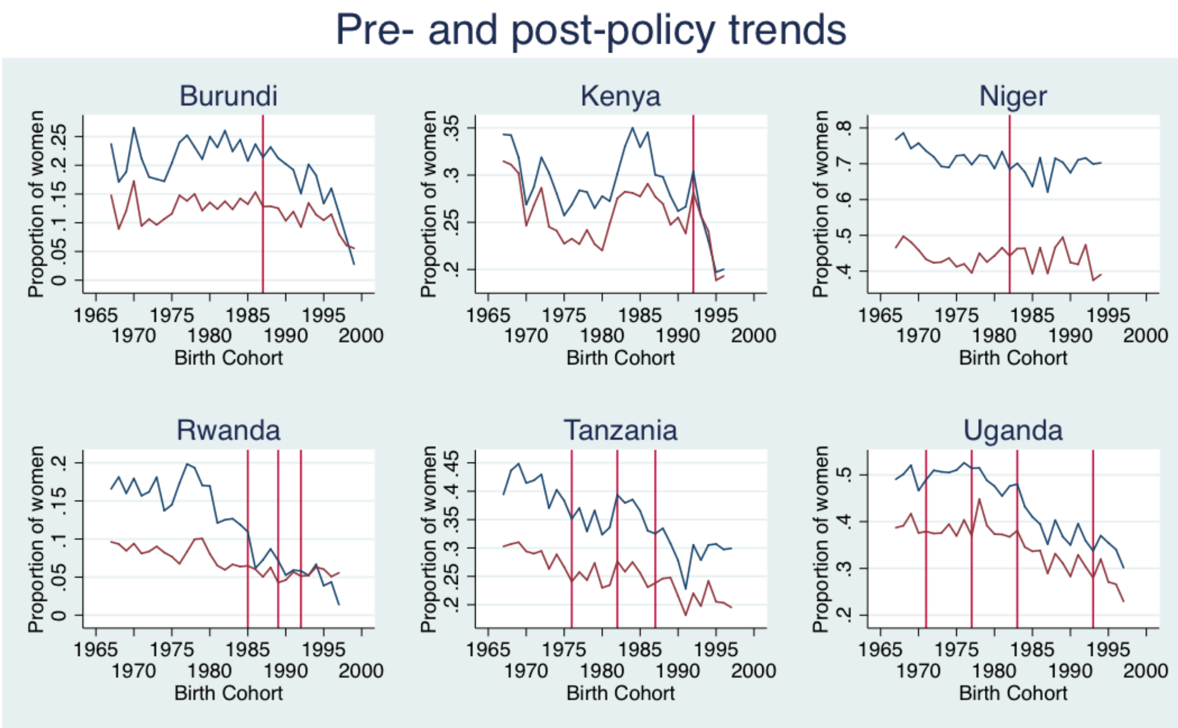
Figure 4–2: Pre-policy trends in the probability of marriage and childbearing before age 18 for each individual treated country compared to countries that did not have quotas.

APPENDIX

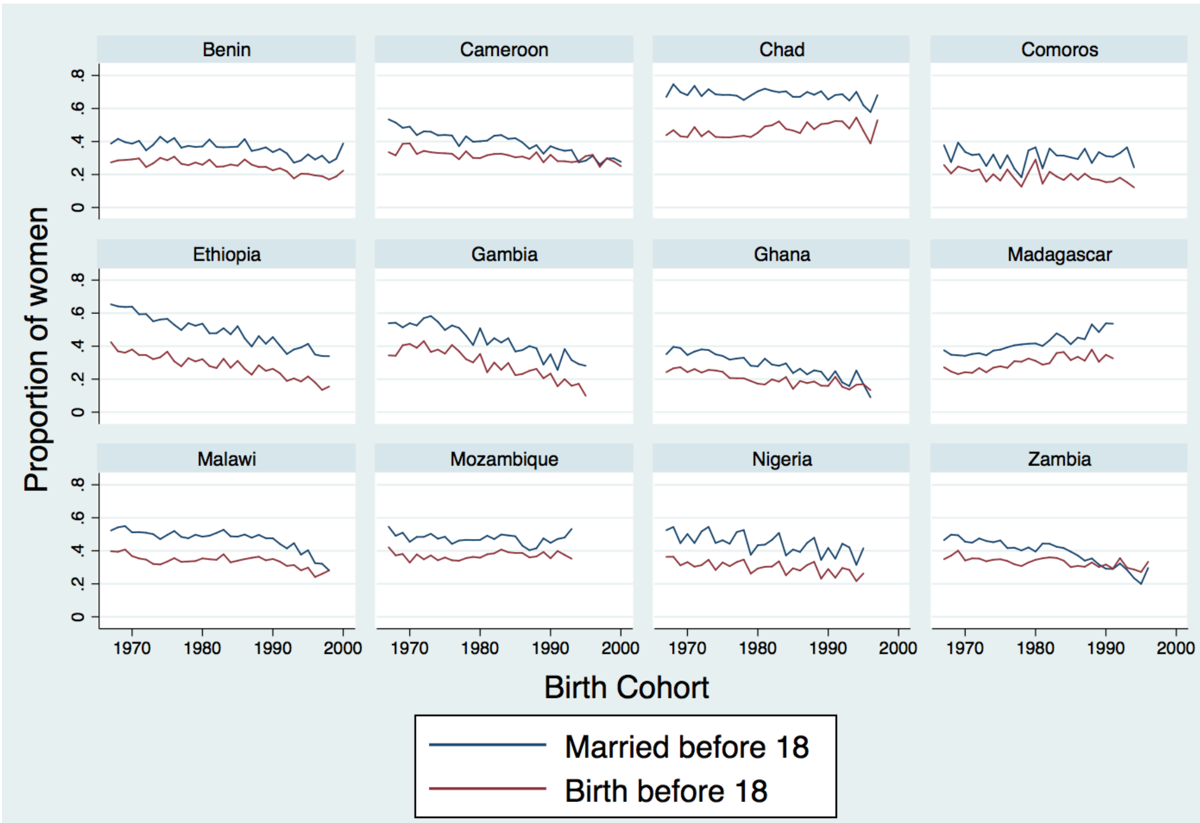
Table 4–A: The probability of marriage and childbearing before age 18 during the pre-policy years in the treated countries before gender quota adoption.

Outcomes	Pre-policy years	P (Marriage before 18) Mean (SD)	P (Child birth before 18) Mean (SD)
All treated countries (N=137,476)	Varying	35.4 (47.8)	25.0 (43.3)
Simultaneous Rollout			
Burundi (N=7,531)	1978–1987	24.4 (42.9)	13.6 (34.2)
Kenya (N=15,048)	1983–1992	26.6 (44.2)	27.2 (44.5)
Niger (N=8,999)	1973–1982	77.1 (42.0)	43.1 (49.5)
Staggered Rollout			
Rwanda (N=15,254)	1976–1985	15.4 (36.1)	7.5 (26.3)
Tanzania (N=15,143)	1967–1976	39.6 (48.9)	28.6 (45.2)
Uganda (N=8,604)	1962–1971	51.9 (50.0)	39.5 (48.9)

Notes: The individual country sample sizes do not add up to the sample size for all the treated countries. This is because the pre-policy years start in 1962 for all countries in the aggregate analysis whereas individual country analyses have ten years of pre-policy data calculated using the year of policy adoption within each of the treated countries separately.



Panel A



Panel B

Figure 4–A: Trends in the probability of marriage and childbearing before age 18 during the study period in treated countries (Panel A) and control countries (Panel B).

Notes: The red vertical lines in Panel A show birth years of girls in which changes in the quota policy took place in each individual country.

Chapter 5

Conclusion

Child marriage and early childbearing violate girls' fundamental human rights. These practices are harmful to the development of their full potential through the restrictions they impose upon girls' education, economic opportunities, and health and wellbeing. These practices affect young girls and further perpetuate the cycle of impeded development over to their children through intergenerational transmission of poverty, low levels of education, and ill-health. These negative consequences resign countries to a lower economic growth trajectory, and the estimated economic costs could run into trillions of dollars globally by 2030 (Wodon et al., 2017).

In 2015, 193 UN member states adopted the Sustainable Development Goals (SDG), centered on realizing human rights. SDGs 5 and 3 aim to eliminate child marriage and reduce births among adolescents by 2030, respectively. A UNICEF (2018) report posits that the global progress on child marriage needs to be twelve times faster than the rate observed over the previous decade to meet its elimination target. The COVID-19 pandemic has only made matters worse. Yukich et al. (2021) predict the pandemic to add 10 million child brides to an already projected 100 million girls who were at the risk of becoming child brides before the pandemic began (UNICEF, 2021). These trends highlight the dire need to identify interventions that effectively target these practices and can be taken to scale to accelerate progress.

Child marriage and early childbearing are direct manifestations of gender inequality and have severe long-term consequences on maternal and child health. These practices occur within the context of complex intersectionalities between gender and other overlapping deprivations. The WHO recognizes addressing the social determinants of health as critical to achieving better health and narrowing persistent health inequities. The Lancet Series on Gender Equality, Norms, and Health further highlights the importance of gender as a social determinant of health and the critical need for gender-equitable laws, policies, and programs to improve health for all. This dissertation examines the effectiveness of large-scale strategies that target the intersections between gender inequality and socioeconomic circumstances in delaying girls' marriage and childbearing outcomes. The comprising papers all employ a quasi-experimental study design on data representative at the national or state levels, building evidence that is likely to be generalizable to other places in similar settings.

Key Findings

In the first paper, I examine the impact of extending the tuition-free policy from the primary to the secondary level of schooling on the probability of girls getting married and bearing children before 15 and 18 years of age. The results suggest that tuition-free secondary policy significantly reduced the probability of marriage and childbearing before 15 and 18 years of age. I observed significantly larger effects associated with tuition-free secondary overexposure to tuition-free primary alone for all the outcomes. The study suggests that policies that aim to increase access to schooling beyond the primary level can achieve broader gains in child marriage and early childbearing.

The second paper evaluates the effectiveness of Apni Beti Apna Dhan (*ABAD*), a conditional cash transfer program, in achieving its program objective of delaying marriage until girls reach the age of 18. It was a statewide program launched in 1994 in Haryana, India that invested in government bonds upon the birth of the girl child. Girls and their families receive the amount upon maturity only if girls remain unmarried until they turn 18. The paper also explores the program's impact on related outcomes such as the probability of childbearing before 18, the number of months between marriage and childbearing, completion of primary and secondary schooling, and son preference. I find strong evidence of the program's impact on the probability of girls delaying marriage until 18 years of age. These results were robust to multiple model specifications. However, there was mixed evidence for the program's effect on the timing of childbearing and the interval between marriage and childbearing. Also, there was no evidence of its effect on schooling outcomes or son preference. These results suggest that while setting the timing of marriage as a condition to receive cash payments helps delay girls' marriage, such programs might have a limited ability to influence broader health and development outcomes.

The third paper explores the role of political gender quotas in the form of reserved seats for women in national and sub-national governing bodies in influencing girls' marriage and childbearing outcomes. Beyond examining aggregate effects in countries that adopted these policies in Sub-Saharan Africa, I also test whether policy effects vary across the treated countries, whether the level of quota diffusion matters, and whether other concurrent changes during the study period explain these effects. The aggregated analysis suggests that gender quotas did not impact child marriage and early childbearing. The disaggregate analyses across individual countries provide a more robust picture. Countries that adopted national and sub-national quotas in one go did not experience delays in marriage and childbearing associated with quota adoption. Countries that adopted quotas and increased the threshold levels in a staggered way did not see any change in the short- to medium-run. While quotas likely did not impact child marriage and childbearing even in the long run, the effect of quotas in combination with other changes is unknown. Overall, the findings suggest that direct investment (tuition-free education) and direct benefits (conditional cash transfers) might work better than aspirational signals (political quotas).

Policy Implications

This dissertation has several implications for policies that aim to address child marriage and early childbearing. First and foremost, it demonstrates that secondary education can be a powerful tool to delay girls' marriage and childbearing outcomes. Most of the current evidence on secondary schooling and child marriage are either associational or small-scale randomized experiments. Until now, population-based causal evidence on the effect of national tuition-free policies on maternal and child health outcomes has only been at the primary level. Using quasi-experimental methods, this dissertation provides robust evidence in favor of the effectiveness of tuition-free secondary education policy in delaying marriage and childbearing among young girls.

This paper also makes an important distinction between tuition-free policy at the primary and secondary levels. While tuition-free primary delayed marriage and childbearing, tuition-free secondary consistently had significantly larger impacts. The greatest reductions in the probabilities of child marriage and early childbearing occur when countries make both primary and secondary schooling more accessible through tuition-free policies. Furthermore, the study used population-based large-scale surveys to estimate the effects of a policy adopted on a national scale and replicable at scale in other settings. This is particularly critical considering the large number of countries that continue to charge tuition fees at the secondary level.

Another implication relates to the effectiveness of income support programs such as conditional cash transfers in delaying marriage and childbearing. The robust and strong evidence points to how making the timing of marriage a condition to receive cash payments can incentivize girls to delay marriage. The *ABAD* program was different from most conditional cash transfers in its program design. It had long protracted payments given to girls and their families only if girls remained unmarried until 18 years of age. However, it is essential for policymakers to take into account its lack of a clear impact on a wider set of health and development outcomes. There is mixed evidence of the program's effect on delaying childbearing and the interval between marriage and first childbirth. Also, the program had no impact on schooling and women's preference for sons over daughters. These results demonstrate that conditional cash transfers alone are not enough for girls to realize their true potential. It is critical for CCT programs to imbibe a rights-based approach and develop synergies with other sectors such as education to have a sustained, holistic impact on girls.

As for fostering a favorable legal and policy environment through the adoption of political quotas for women in national and sub-national governing bodies, this dissertation shows that on its own, it might have a limited influence on girls' marriage and childbearing outcomes. While the results show that reserving seats for women in politics may not be a policy priority for addressing child marriage and early childbearing, it is not to say that substantive political representation and political empowerment of women do not play a role in shaping these outcomes. The results simply might indicate that unless a genuine transfer of political power occurs, a mere increase in numerical representation alone may not be sufficient to influence individuals to alter their behaviors.

Future Work

Despite examining many critical aspects of policies that might address child marriage and early childbearing among young girls, this dissertation opens up several outstanding questions for policymakers. I plan to continue working in this area to better inform future policy choices and advance evidence-based policymaking in this regard.

First, I plan to investigate the potential mechanisms through which tuition-free secondary education policy impacts child marriage and early childbearing. There are several potential explanations for why the extension of tuition-free policy from primary to secondary education reduces the probability of child marriage and early childbearing. First, girls are expected to attend secondary school during their teenage years when they are more vulnerable to early marriage and childbearing. Therefore, delays in marriage and childbearing could simply result from more time spent in school during these critical years. Second, schooling has the potential to empower girls with information, skills, and networks that can enable them to negotiate decision-making within their households. Third, labor market returns to secondary school are likely to be higher than those to primary school, encouraging parents to choose education over marriage as a better investment for girls. Fourth, education can play a powerful role in transforming gender norms and bringing about a change in girls' expectations of marriage. In my future work, I plan to examine the role of each of these pathways and help explain how tuition-free secondary delays girls' marriage and childbearing.

I also hope to understand better the workings of the conditional cash transfers program, *ABAD,* and other related income support programs in delaying girls' marriage and childbearing. Qualitative research, including interviews and focus groups, can be particularly informative on deeper social issues regarding community norms around gender, marriage, and childbearing. The currently available quantitative surveys in Haryana do not yet collect information on how beneficiary households used the cash benefits and whether it resulted in greater bargaining power among women or was simply used to cover marriage expenses. It is imperative to understand the community's perception of the program and its objectives to refine future program designs that are better suited to this context. As more survey data become available over the years, I also plan to examine the program's impact on other proxy measures of gender equality and gender norms.

In addition, I plan to study the full range of the program's impact on all cohorts of girls that received the program, with subsequent rounds of survey data being conducted and becoming available. Currently, available data has information on girls born only up to 32 months after the program rollout. Assuming better implementation and community awareness with time, the lack of data on later program beneficiaries might have underestimated the program's impact. Also, the expected program beneficiaries were too young at the time of the survey to enable the examination of a full range of marital and fertility outcomes that go beyond age 18 and other relevant long-run outcomes that measure intergenerational impact.

It is also necessary to study the prevalence of child marriage and early childbearing among households not targeted by the program. Current household survey data suggest that these rates remain high among households beyond those targeted by the program, suggesting income is not the only factor that leads girls to marry early. Rigid social and gender norms are often the driving force. I hope to study programs and policies that might target child marriage and early childbearing among this group. Importantly, I plan to comprehensively study other income support programs that are multipronged and holistic in their approach to identify effective complementary strategies that might enable girls to realize their human rights fully.

Furthermore, I hope to deepen our understanding of the effect of greater women's political representation and holding positions of power in governance on child marriage and early childbearing. In this dissertation, I explored one such policy that ensures higher women's political representation, i.e., reserving seats for women in governing bodies. Other gender quotas, such as legislated candidate and voluntary party quotas, function differently; therefore, their impacts may vary. I plan to study these policies in the future. I also hope to examine the effect of these policies on more proximal outcomes such as budgetary allocations, adoption of progressive policies, and role-modeling outcomes that could be potential pathways for influencing child marriage and early childbearing outcomes. Qualitative research using in-depth interviews and focus groups can also prove useful in understanding how gender quotas work within local contexts. Lastly, I plan to study the effect of women's political participation in and of itself and not the policies that help increase it. The impact of a woman leader who is elected on an unreserved seat against male candidates instead of those appointed by political parties will likely be different. One way to examine this would be to study close elections where male or female candidates win by a slim margin. This study design might help understand the difference in the impacts of mere numerical political representation versus truly empowering transfer of power to women.

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