

# Child Brides, Bargaining Power, and Reform of Ethiopia's Family Law

Job Market Paper

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## **Abstract**

This paper seeks to identify the causal relationship between a woman's age of marriage and the wellbeing of women. I leverage a novel source of identification, the staggered rollout of a legal reform in Ethiopia that raised the minimum age of marriage for girls, to estimate the effect of delaying marriage on women's outcomes. Using a difference-in-differences strategy, I establish that the reform increased the average age of marriage for women by one year and also shifted the distribution of marriage ages up, making marriage at ages 15 or younger less common and nearly- or fully-compliant marriage more common. Next, using the reform as an instrument, I find that a one year delay in marriage increases educational attainment for women by one year and literacy by 18 percentage points, a result consistent with the only previous causal finding. I expand on that evidence to consider other outcomes for women and find mixed results. In particular, I find that women who marry later have more accurate knowledge of when children are conceived and are more likely to manage household finances jointly with their husbands. On the other hand, they are no less likely to think that domestic violence from their husbands is justified, which is a widespread belief among Ethiopian women. My estimates provide the first empirical evaluation of age of consent laws. The results suggest that policies and activist groups aiming to end child marriage will attain restricted improvements in women's lives unless other action is simultaneously pursued.

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\*Please do not cite or circulate without permission. E-mail: [tjm276@cornell.edu](mailto:tjm276@cornell.edu). I am grateful to my graduate committee – Francine Blau, Kaushik Basu, James Berry, and Victoria Prowse – for their direction and support. Thanks to John Abowd, Larry Blume, Andrew Simons, Andrew Green, Kalyani Raghunathan, and seminar participants at Cornell and the Eastern Economics Association for helpful comments. All errors are my own. For more information about my research and teaching, please visit my website: [www.tamaramcavock.com](http://www.tamaramcavock.com).

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*“It is a vicious circle. Women are deprived of rights because of their lack of education, and their lack of education results from their lack of rights. We must not forget that the subjection of women is so widespread and so old that we often refuse to recognize the abyss that separates [women] from [men].”*

-Leo Tolstoy (in *Anna Karenina*)

*“With marriage, a girl’s childhood abruptly ends. Her health and future prospects immediately fall in jeopardy. Tens of millions of girls in the developing world are at risk of being forced to wed as part of this deeply entrenched practice that significantly impedes progress on human rights, education, global health, and economic development.”*

-International Center for Research on Women

## **1 Introduction**

In 2010, 35% of women ages 20-24 in the developing world had been married before their 18th birthday, many of them well before their 16th (UNICEF, 2013). Leading NGOs in the field of development – including the UN Human Rights, UNICEF, the International Center for Research on Women, and the Population Council, buttressed by a photo campaign by National Geographic – assert that early marriage generates health risks and social and economic costs, the majority of which are borne by women. In particular, early marriage for girls is credited with reduced schooling and employment outcomes, higher and earlier fertility and likelihood of mortality and complications during childbirth, greater incidence of HIV and domestic violence, and low bargaining power among women (Warner et al., 2014; Jensen & Thornton, 2003; Edmeades, 2013). In addition, children born to mothers married as teenagers are thought to have lower health and human capital outcomes relative to their peers because of their mothers’ early marriage (Jensen & Thornton, 2003).

However, little empirical evidence establishes these observations as causal or separates them from what might simply be the effect of poverty and women’s low status on both child marriage and outcomes. In addition, even as governments and development institutions enact legal reform and other policies aimed at decreasing child marriage, the effects of these programs, their costs as well as their benefits, have not been quantified.

The contribution of this paper is twofold. First, I provide quasi-experimental estimates of the effect of Ethiopia’s 2000 reform of its family law on age at marriage using a difference-in-differences

strategy. Second, this is only the second paper to investigate the causal nature of early marriage for girls on negative outcomes typically associated with the practice. With the exception of Field & Ambrus (2008), until now, these linkages have been made by researchers and NGOs by observing only correlations; my quasi-experiment specification allows me to examine the existence (or lack thereof) of causal effects of early marriage for girls on their education and employment outcomes and their status within their marriages, and in the context of a legal reform that aimed to raise the age of marriage.

My results suggest that, while the policy did increase the average age at marriage for affected women by about one year and shifted the distribution of marriage ages up, young marriage is still prevalent. Further, I find that as a result of the policy, women's education also increased by about one year, which represents a large gain given low average levels of education (2.5 years) prior to the policy. This increase in education corresponds with an 18 percentage point increase in literacy, the ability to read a full sentence. These findings are consistent with Field & Ambrus (2008), showing that in the context of Ethiopia, a legal change to the minimum age of marriage brings increases in age of marriage for girls and, along with it, in education.

Expanding beyond the outcomes considered by Field & Ambrus (2008), my estimates support mixed evidence for the causal impact of age at marriage on other outcomes for women within marriage. On the one hand women who marry later as a result of the reform are more likely to report jointly managing their husband's income and less likely to think they are unable to ask for protection in the event that he has an STI, but they do not appear to marry better quality spouses or to be more likely to perceive physical abuse by their husbands as being unacceptable. My findings suggest that claims of the negative effect of child marriage on women's bargaining power and the benefits of ending the practice should be tempered and that further efforts must be made if the position of women is to be improved in all dimensions.

This paper proceeds by first describing in more detail the theoretical framework and existing empirical evidence regarding child marriage and outcomes. Section 3 discusses the particular anthropological, ethnographic, and political environments in which my empirical approach is set. Section 4 presents the empirical strategy I use to investigate the effect of the 2000 Revised Family Code in Ethiopia and the data I use to estimate the results. In Section 5, I present results from my estimation and I discuss their robustness in Section 6. Section 7 concludes and discusses areas

for future work.

## 2 Literature and Motivation

Early marriage for girls is prevalent in much of the developing world and has been given attention in the United Nations' Convention on the Elimination of All Forms of Discrimination Against Women (CEDAW) as a violation of human rights since 1981.<sup>1</sup> Jensen & Thornton (2003) discuss patterns of early marriage for girls being associated with low education, early childbearing and related maternal health problems, and limited assertion and independence within marriage. In light of these concerns and observations, non-governmental organizations and governments have increasingly taken up the cause to end child marriage among other "harmful traditional practices."<sup>2</sup> UNICEF hosted a "Girl Summit" in 2014 aimed at mobilizing efforts to end child marriage within a generation in Africa, and the practice of child marriage has become a focus of the International Center for Research on Women (ICRW) and USAID, along with governmental interventions in India, Pakistan, and throughout sub-Saharan Africa (ICRW, 2013). Efforts are also being undertaken in the Americas: in Guatemala, a bill raising the minimum age of marriage became a debating point in the country's 2015 presidential election run-off, and a recent opinion piece in the New York Times highlighted the work of "Unchained at Last," a nonprofit seeking to end child marriage in the United States, which occurs with judicial approval for girls of immigrant families but also in "so-called American families" (Moloney, 2015; Reiss, 2015).

These efforts and concerns have been made despite a dearth of empirical evidence establishing the link between child marriage and outcomes as causal, perhaps owing to the difficulty of finding sources of plausible exogeneity. In the first and only other existing paper identifying these effects as causal, Field & Ambrus (2008) estimated the causal effect of age at marriage on education and literacy using age at first menarche as an instrument for age of marriage of girls. They showed that delaying marriage for girls by one year increases years of schooling by 0.22 years and literacy by 5.6 percent in Bangladesh.<sup>3</sup>

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<sup>1</sup>Jacobson (1992).

<sup>2</sup>Boyden et al. (2013).

<sup>3</sup>However, some concerns surrounding age at first menarche as a suitable instrument for age at first marriage exist. First, the variable is not available in many datasets and when it is may be associated with significant measurement error. Indeed, the variable is not available in any of the datasets I am using from Ethiopia. Second, there may be concerns about the exclusion restriction in using age at menarche to proxy for age at first marriage exogenously

## 2.1 The Household's Problem

Norms, debt or other shocks to household income, and aspirations or expectations of daughters' future prospects (educational, employment, or otherwise) are all likely to play a role in the decision of when to marry a young girl. To the extent that legal reform changes only some but not all of these factors, one is not likely to see a substantial change in outcomes for women unless these constraints or motivations are addressed in addition to the legal age at marriage. This is especially true in contexts where customary or religious marriage is legally acceptable or when laws are otherwise difficult or impossible to enforce.

With respect to outcomes as determined by marriage age, causal effects could theoretically go in either direction and therefore raising age at marriage may not necessarily lead to observed changes in outcomes. On the one hand, marrying later for girls may increase positive outcomes. If the alternative to marriage is staying in school, then education will necessarily increase if girls marry later, and along with it, all of the outcomes for which education is a determinant.<sup>4</sup> In addition, if age at marriage itself increases women's bargaining power because it increases girls' maturity and assertiveness in a decision making process within a marriage, later marriage for girls will improve women's welfare as their bargaining power improves. On the other hand, if even very early marriage occurs after schooling is complete, then delaying marriage may have no effect on school attainment. Even if education does increase, women's wellbeing in other dimensions may fail to improve unless other changes to women's status (stemming from norms and customs) are also improved.

Furthermore, families may marry their girls young because they believe, correctly or not, that it improves the quality of a match, the bride price received (an almost universal practice in Ethiopia according to the anthropological and sociological literature)<sup>5</sup>, or both. To the extent that these transfers help a family pay off debt, feed remaining members, or invest in other aspects of well-being (perhaps even for the girl who is being married), mandating later marriage without addressing these underlying causes and constraints that encourage it in the first place may have no effect or, worse, a

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(relative to existing effects of poverty and health), because poor nutrition and health can sometimes be related to age at menarche in the first place. See Rah et al. (2009), although Field & Ambrus (2008) do directly address this concern and find weak evidence to support it in their context.

<sup>4</sup>Conditional cash transfers – monetary payments to families when girls stay in school instead of marrying – in the Indian states of Bihar and Jharkhand have been designed to incentivize delay of marriage, but these efforts have not been rigorously evaluated.

<sup>5</sup>See, for example, Pankhurst (1992).

negative impact on women’s outcomes. One extreme example frequently cited is that families marry their daughters early to prevent abduction or rape, which may not only bring shame on a family, generate children outside of marriage, or spread HIV and other sexually transmitted diseases, but may destroy a girl’s marriage prospect, and with it, financial and social stability in the future (Boyden et al., 2013). Early marriage for girls may be a protective measure taken in the context of a highly discriminatory and even violent environment for women and, in the absence of other means by which families can mitigate these risks for their girls, a mandated later age at marriage might in fact be welfare-reducing not only for the families, but also for the women themselves.

## 2.2 Bargaining Power

Jensen & Thornton (2003) discuss the possibility that women who marry young or who marry men who are much older than they may lack status or power within a marriage and may be less capable of asserting themselves. For this reason, in addition to exploring education and related outcomes as Field & Ambrus (2008) do, I also explore outcomes associated with perceived or actual power within marriage. This insight and application grows out of empirical evidence on the microeconomics of households rejecting the standard unitary model of the household in favor of models which assume that household members are heterogeneous in their preferences and that household outcomes reflect internal strategic decision making that may be affected by members’ bargaining power.<sup>6</sup> Household bargaining power may be derived from assets brought in at the inception of marriage, dowries and bride prices, as well as levels of education and other forms of human, social, and physical capital that reflect outside opportunities and freedom as well as contemporaneous relative income levels (Fafchamps & Quisumbing, 2005; Goldstein & Udry, 2008; Chiappori et al., 2002; Quisumbing & Maluccio, 2003; Duflo, 2003; Hoddinott & Haddad, 1995; Qian, 2008; Harari, 2014). Recent experimental evidence from Ashraf et al. (2014) has suggested that increasing women’s ability to control contraceptive use and method decreases “unwanted” fertility in Zambia, in line with findings from Bailey (2010) among the US women’s population.

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<sup>6</sup>Browning et al. (1994).



### 2.3 Legal Reforms and Women’s Status

In this spirit, a growing literature estimates the effectiveness of legal changes that directly influence bargaining power on influencing outcomes for women. Most previous work has been confined to developed country settings, especially as the law relates to divorce. The U.S. state-specific adoption of no-fault divorce laws during the 1960s and 70s prompted a number of quasi-experimental studies addressing the theoretical prediction that a perceived change in “threat point” – the outside option or expected division of marital property upon divorce – would alter bargaining power and therefore outcomes within a marriage even if it never actually dissolved. Gray (1998) finds that women’s labor force participation increased and Chiappori et al. (2002) find that women’s labor supply hours fell when divorce laws became more favorable to women. Adam et al. (2003) used similar legal changes in Canada to assess the effect of changes to divorce laws on female suicide rates, finding that laws improving women’s rights to assets upon divorce reduced the suicide rate among married women, but had no effect on the rate of suicide among unmarried women.

Turning to a developing country setting, legal changes improving property rights and other measures of women’s status seem to have favorable effects. Harari (2014) finds that a 1981 reform in Kenya granting daughters and sons the same inheritance share increased women’s education and age at first marriage, and reduced fertility and the incidence of female genital mutilation. Hallward-Driemeier & Gajigo (2011) investigate the effects of the Ethiopia reform’s changes to inheritance law and property rights, and find that district-level female labor participation rates and employment in higher-paying occupations are higher, especially for young unmarried women, in 2005 after the early regions adopted the reform.

Kumar & Quisumbing (2015) find evidence suggestive of complementarities between Ethiopia’s family law reform and land allocation reform which predominantly favored female-headed households. Kumar & Quisumbing (2012) also find evidence suggestive that perceived allocation of assets upon divorce is associated with perceived control over one’s life, and that these perceptions are also correlated with children’s actual schooling outcomes: when women’s perceptions of equitable asset division upon divorce improve, their own self-reported well-being is unchanged but their children’s schooling outcomes improve. However, their analysis relies on panel data that asks women about their perceived control over assets during marriage and upon divorce, and cannot distinguish the

separate aspects of the family law reform or other contemporaneous trends on these perceptions. That is, they use panel data to estimate changes in perceptions of asset distribution during marriage and upon divorce and attribute the correlation between the two to the reform of the family law without addressing particular aspects of the legal reform or time trends.

This paper is the first study to investigate the causal relationship between age at marriage as a potential source of intra-household bargaining power and outcomes of that bargaining process within the context of a legal reform that raised the legal age of marriage for women. Beyond Field & Ambrus (2008), who used the instrumental variables approach described above, further evidence on the negative effects of child marriage for girls has thus far been confined to statements based on empirical observation that can only be described as correlational. By leveraging a legal reform in Ethiopia and its staggered timing across regions of the country, I provide the first quasi-experimental evidence on the causal effect of age of marriage on outcomes for women. In confirming the causal nature of these claims but also showing that some outcomes related to women’s well-being do not change, my work can inform the policy and aid community with respect to the optimal strategy for achieving desired outcomes associated with improving women’s status.

### **3 Anthropological, Political, and Empirical Contexts**

#### **3.1 Marriage in Ethiopia**

With more than 85 distinct ethnic groups and representation from many of the world’s major religions, Ethiopia represents an ethnically and religiously diverse environment (Webb et al., 1992). Across Ethiopia, marriage is most frequently arranged, with most brides and many grooms having no part in the choice of spouse; it is common for young girls not to know they are being married until the day of the wedding, on which the bride and groom meet for the first time (Pankhurst, 1992; Fafchamps & Quisumbing, 2002, 2005; Kumar & Quisumbing, 2015). Marriage celebrated in a religious setting is far less common than customary or traditional marriage ceremonies, and local marriage customs tend to be dominated by local customary practices rather than more universal religious practices (Pankhurst, 1992). Anthropological records from Pankhurst (1992) suggest that in rural areas, where nearly 80% of the population lived in 2000, girls are traditionally 10 to 14 years old at marriage while grooms are usually ten years older than brides on average. Parental

transfers at the time of marriage are most consistent with assortative matching (rich with rich, poor with poor) as opposed to strategic or compensatory motives, although the majority of assets brought into a marriage are from the bride and groom and not their parents, with grooms bringing on average 10 times as many assets into marriage as do brides (Fafchamps & Quisumbing, 2002, 2005).<sup>7</sup>

Land is the most commonly held and most valuable asset in Ethiopia, and the asset most frequently brought in upon marriage; however, the right of women to own land was established by the 1995 revision to the civil code, five years before the 2000 revision of the family code in Ethiopia (Gopal, 2001). Contrary to popular thought that grooms are granted family land upon the occasion of their engagement or marriage, grooms bringing land to a marriage typically have owned it prior to the celebration of marriage (Webb et al., 1992). While ethnic and religious norms affect customary practices, “location-specific norms are generally the best predictor of the disposition of assets upon marital dissolution” (Kumar & Quisumbing, 2015).

Polygamy is practiced with some frequency in Ethiopia across ethnic and religious groups, despite being officially prohibited in the Ethiopian Orthodox church. A draft constitution of Ethiopia’s revolutionary government in 1974 prohibited bigamy, which is officially prohibited by the Ethiopian Orthodox church, but the phrase was later dropped in deference to Islamic pressure. Divorce is also much more widespread than would be expected in a deeply traditional and religious setting (Pankhurst, 1992). In the results section, I investigate whether divorce rates appear to have increased following the reform to family law, but find no evidence to suggest this. The results section also explores the effects of the reform on women’s involvement in polygamous marriages.

### **3.2 Ethiopia’s Family Law Reform**

In July 2000, the Federal Democratic Republic of Ethiopia amended existing law on family relation in its “Revised Family Code” to “provide the legal basis which guarantees the equality of the spouses during the conclusion, duration, and dissolution of marriage” (Federal Democratic Republic of Ethiopia, 2000). Within a package of reforms to existing law, Articles 6 and 7 require “free and full consent” and raised the minimum age of marriage from 15 to 18 years for girls, granting an exception of not more than two years for “serious cause.” The law left untouched the

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<sup>7</sup>The majority of brides bring nothing to the marriage.

legal minimum age of marriage for men (at 18). The reform also made marrying children a criminal act, punishable by up to seven years in prison.<sup>8</sup> An important feature of marriage law in Ethiopia is that marriages that are performed as part of a traditional customary or religious ceremony are considered legal by the government, in addition to marriages carried out by an officer of civil status. Combined with the fact that enforcement of the legal minimum age – both before and after the reform – is weak, this means that persisting norms and customs have historically caused the median age at marriage in Ethiopia to be substantially lower than 18. In my sample from the nationally representative Demographic and Health Surveys, the median age at marriage among women ages 18 to 28 in 2000 was 16, with a large proportion of women married well before age 15. For the purposes of this paper, then, I view the change in the legal minimum age established by the law reform as a suggestion to families, communities, and religious and ethnic leaders, working perhaps slowly to change norms with the help of awareness campaigns and social pressures (North, 1991; Voigt & Kiwit, 1998).

The Revised Family Code of 2000 also includes a number of other changes to family law with the potential to affect women’s bargaining power, not simply changing the minimum age at marriage. The legal reform also gave women the authority to jointly administer common marital property, granted civil courts more authority in settling inheritance disputes (whereas previously substantial authority was held by local traditional arbitrators), and withdrew authority from husbands to deny permission of women to work outside the home. While such a concert of reforms might influence the outcomes I investigate here, my empirical strategy focuses on women who were likely to have been affected by the marriage age established by the the reform because they were married after the reform, although in Section 6, I directly address these other aspects.<sup>9</sup>

The Revised Family Code was approved by the federal government in 2000 but its implementation occurred at the regional government level and was staggered: the two chartered cities (Addis Ababa and Dire Dawa) and three additional regions (Tigray, Amhara, and Oromia) implemented the code immediately, but the remaining six regions of Ethiopia did not implement the reform until after 2005.<sup>10</sup> Figure 1 presents a map of Ethiopia with the early reform regions boxed. Following

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<sup>8</sup>See Federal Democratic Republic of Ethiopia (2000).

<sup>9</sup>Certainly later life inheritance would be a potential determinant of marriage quality and intra-marital bargaining power (to the extent that it is determined by real or promised assets brought in by members), but I cannot separate these effects using the data I have.

<sup>10</sup>These are: Afar, Somali, Gambela, Benishangul-Gumaz, Harari, and the region called Southern Nations, Na-

Hallward-Driemeier & Gajigo (2011), I will henceforth refer to these eleven units - the nine regions plus the two municipalities - as regions.<sup>11</sup>

My empirical strategy uses this staggered rollout of the reform to estimate the effect of the reform by comparing those regions where the reform was adopted early to those where it was adopted later. To be valid, this method treats the adoption of the reform as random relative to women and their marriage timing in particular regions. Several features of Ethiopia's political economy warrant this assumption. Following the Derg (1977 to 1991), the military dictatorship which ended the 43-year rule of Emperor Haile Selassie, Ethiopia's democratic socialist republic was designed to give importance and pay respect to Ethiopia's ethnic groups and "nationalities" (Kefale, 2013; Abebe, 2014). Establishing regional boundaries based on these ethnic groupings and granting significant autonomy to regional level governments, Ethiopia has been referred to by anthropologists and political scientists as an "ethnic federalist" state (Abebe, 2014). The federal government's immediate jurisdiction is within the two large municipalities, Addis Ababa and Dire Dawa, and its legislative proclamations take immediate effect there, but each region has political autonomy and responsibility to enact and approve its own constitution as well as civil, family, and land laws. In practice, this pays recognition to Ethiopia's customary, religious, and traditional laws in Ethiopia's complex ethnic and religious landscape, but ultimately establishes extremely similar laws throughout the country (Burgess, 2011).

In most legal matters, the regions of Amhara, Oromia, Tigray, and SNNP, which comprise the majority of Ethiopia's population, follow suit with the federal government rather quickly in implementation; for example, the Productive Safety Net Program (PSNP), Ethiopia's government and NGO-sponsored large-scale food security plan laid out in 2005, was begun first in those regions (Hoddinott et al., 2011). In the case of the reform of the family law, however, Amhara, Oromia, and Tigray adopted nearly identical family codes soon after it was implemented by the federal government, but in SNNP, where security concerns associated with the 2000 election delayed legislative action, the family code was not reformed until after 2005, during the same time frame as the remaining regions (Afar, Gambela, Harari, Somali, and Benishangul-Gumaz) (Pausewang et al., 2002; Smith, 2013). With respect to the Revised Family Code, Smith (2013) finds only one

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nationalities, and Peoples (SNNP).

<sup>11</sup>This is how they are defined in the DHS data as well.

instance of region-level conflict with respect to ratification of the family law reform: in Oromia, one of the early adopters. Traditions of polygamy in the dominant Oromo ethnic group prompted some local leaders to favor revision of the reform to legally recognize polygamous marriages, but in the end the reform was adopted nearly unaltered in Oromia, as in other regions.

The reform's effectiveness also relies on knowledge of the reform in the public sphere where families and communities would be informed about the reform's impact on families' decisions regarding marriage of their girls. Indeed, the Ethiopian government, international NGOs, and local community organizations were active in raising awareness of the reform, especially with respect to its stipulations on marriage age and consent. Newspaper, radio, and community plays encouraged the dissemination of the reform's implications and the "dangers" and human rights violations of early marriage for girls, and local government, health, education, and religious leaders were trained in order to spread knowledge of it (Pankhurst, 2015). One Ethiopian Orthodox priest says that knowledge of the rules and benefits of delayed marriage among religious leaders is critical in the improvement of women's lives (Cosier, 2015). Instances of enforcement of the law have come in the form of both fines and imprisonment, but seem to be few (Pankhurst, 2015). Some local community organizers say that some villagers may not be aware that punishment can be incurred even after the marriage ceremony, while still others cite the stigma associated with divorce as a reason for remaining married despite the law's implications.<sup>12</sup> In the extreme, Mekonnen & Aspen (2009) note that during fieldwork in the Maqet *woreda* (district) of Amhara, the region where the prevalence of early marriage is highest, most individuals consulted during interviews were aware of, but opposed to, the new law.

Since the data I use come from 2000, 2005, and 2011, I have surveys of individuals who were affected by the reform at different points in time, depending on their region of residence. I use the natural experiment provided by the reform and its geographically staggered timing to investigate its effects. While one concern might be that the regions that adopted the reform early differed substantially and systematically in ways that might be related to age at marriage and other outcomes, in what follows, I address this concern with an analysis of pre-trends, and I always control for time-invariant region-level characteristics and region-specific time trends. Finally, despite significant efforts to track them down, I do not know the exact dates of adoption of the reform in

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<sup>12</sup>See Walters (2014).

each region, only the five-year period in which they were known to have adopted the reform. I adopt the conservative approach of treating regions as having reformed in 2000 if their adoption occurred in the five years between 2000 and 2005, and I treat regions as having reformed in 2005 if their adoption of the reform occurred between 2005 and 2011, though my results are robust to random adjustment of the reform dates. This measurement may bias my results toward zero since I may misattribute certain years as post-reform for some women in certain regions when in fact the reform had not yet taken place.

## 4 Empirical Strategy

### 4.1 Data

I use household surveys from the Demographic and Health Surveys (DHS), conducted in partnership with the Central Statistical Agency in Ethiopia, which provide cross-sections of women ages 15-49 and men ages 15-59 in 2000, 2005, and 2011. The timing of the survey data collection and the fact that I can retroactively observe marriages that occurred well before the period (based on women's stated age at marriage) allows me to construct a panel of women whose marriages occurred between 1974 and 2011.<sup>13</sup> That is, even though I have responses to questions in the survey (whether 2000, 2005, or 2011), the age ranges and marriage ages of women span the decades before. Figure 2 shows the timing of these surveys relative to the reform and its occurrence in the different regions.

The surveys were designed to be nationally and regionally representative for the purpose of policy planning.<sup>14,15</sup> In addition to gathering more objective quantitative information from women on

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<sup>13</sup>The question that the DHS uses for age at marriage is "age at first cohabitation"; this does not allow for the possibility that, as is reportedly common in Ethiopia, *de jure* marriages are celebrated when women are very young but not consummated until a few years later, even though the bride lives with her husband's family. However, in these data, the correlation of age at first cohabitation and age at first intercourse is very strong at 0.88. Eighty percent of women report their age at first intercourse to be the same as their age at first marriage (cohabitation). Among the 10 percent of women who report their first intercourse as occurring in a year later than their first marriage, the average gap is 2 years; among the other 10 percent of women who report their first intercourse as occurring before their first marriage, the average gap is 3.5 years.

<sup>14</sup>The sampling procedure was two-stage: in the first stage, census enumeration areas were randomly selected at the regional level and households were then sampled at random within each enumeration area. All of my analysis which follows uses the appropriate sampling weights provided in the data.

<sup>15</sup>Due to some security concerns, some enumeration areas in the Somali region were not surveyed for the 2005 and 2011 rounds, and similarly in Afar for the 2005 round. My estimates are robust to excluding these regions from the analysis for the entire sample period.

their education, births, health, and contraceptive knowledge and use, the DHS have been designed also to gauge women’s perceptions of various intra-marital bargaining outcomes for comparison with surveys in other countries. However, some standard DHS questions are not answered in Ethiopia at all and some are not answered in all of the available survey years. Therefore, from the list of possible questions related to perceived bargaining power of women, I use those for which the questions were asked in each of the Ethiopia survey years (2000, 2005, and 2010).

Table 1 reports summary statistics of the variables of interest in this paper separately for child brides (those married before age 18) and for adult brides, including p-values for t-tests of the difference. In keeping with the concerns raised by the UN and various NGOs, these statistics indicate that child brides are on average less educated and have less say in their marriages, and the differences are statistically significant for most of these measures. Figure 3 provides a histogram of age at marriage for women married before and after the reform; the reform does appear to have shifted the distribution toward a higher age at marriage, although without controlling for pre-existing trends in the age at marriage, this cannot be confirmed as causal in this simple comparison. However, Kolmogorov-Smirnov test for equality of distributions rejects the null hypothesis (at the 99% confidence level) that the distribution of marriage ages is identical before and after the policy was implemented.

## 4.2 Empirical Specification

My empirical strategy proceeds in two steps: first, I estimate the effect of the reform on the marriage ages of women who were married after the reform, using a difference-in-differences strategy leveraging the staggered implementation of the Revised Family Code that raised the minimum age of marriage from 15 to 18. Second, I use that analysis as the first stage in a two-step instrumental variables procedure identifying the causal relationship between delayed marriage for girls and outcomes among women in their marriages.

If the reform occurred only in some regions and not in others, a standard difference-in-differences specification would capture the effect of the reform on the age at marriage among women in the reformed regions (provided the difference-in-differences assumptions held, which I explore below). The following specification would capture the effect of the reform on the age at marriage of those



who were married after the reform:

$$\text{Age at marriage}_{irt} = \beta_1 \text{Reform Region}_r + \beta_2 \text{Post} + \beta_3 (\text{Reform Region}_r * \text{Post}) + \eta_t + \mu_r + \mu_r * t + \epsilon_{irt} \quad (1)$$

where the difference-in-differences estimator  $\beta_3$  shows the difference in average age at marriage (or later I consider other outcomes), before versus after the reform, in the reform regions relative to the non-reform regions. Here, I include year of marriage fixed effects  $\eta_t$ , region fixed effects  $\mu_r$ , and region-specific linear time trends,  $\mu_r * t$ , in order to control for unobserved time or region-varying characteristics.<sup>16</sup>

However, the reform did occur in the other regions, five years after they occurred in the early reform regions, so I use a different specification to capture the full effect of the reform.<sup>17</sup> Following Wolfers (2006) and Bailey (2010), I use a more flexible difference-in-differences specification that allows for the estimated effect of the reform to capture the full effects of the reform in both sets of regions. Specifically, I include a *Policy* variable which takes the value 1 if the reform was in effect in the region at the time of the woman’s marriage and zero otherwise.<sup>18</sup> Therefore, I adapt the specification above as follows:

$$\text{Age at marriage}_{irt} = \gamma \text{Policy} + \eta_t + \mu_r + \psi \mu_r * t + \epsilon_{irt} \quad (2)$$

Including the same fixed effects and controls as above, this specification captures the effect  $\gamma$  of the reform on the dependent variable. This is the overall difference-in-differences specification which determines the effect of the policy on age at marriage. In this way, women whose marriages occurred before 2000 or between 2000 and 2005 in the late reform regions are used as “controls” for those women whose marriages occurred when *Policy* is equal to one.<sup>19</sup>

The difference-in-differences estimator  $\gamma$  captures the causal impact of the reform only if the

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<sup>16</sup>I also include a control for whether the respondent lives in a rural area and, in estimating additional outcomes, the age at survey date.

<sup>17</sup>Figure 2 presents a map of the 11 regions of Ethiopia - the blue boxes indicate those regions that adopted the reform early, between 2000 and 2005.

<sup>18</sup>Wolfers (2006) uses exactly this specification for states’ adoption of no-fault divorce laws during the 1970s in the United States.

<sup>19</sup>If migration for marriage across regional boundaries were common in Ethiopia, this specification would be problematic because those hoping to avoid the policy’s jurisdiction in the early implementation regions could cross region lines to do so. However, due to the ethnic federal system and the delineation of regional boundaries based primarily on ethnic groupings, inter-regional migration for marriage is extremely uncommon in Ethiopia.

difference-in-differences assumptions are satisfied. These assumptions require that 1) in the absence of the reform, the regions that reformed early would have experienced the same trend in age at marriage and other outcomes as the regions that reformed late, such that the late-reform regions provide a sufficient proxy for the unobservable counterfactual of what would have occurred in early reform regions in the absence of the reform; and 2) there were no other significant interventions or policy implementations that might affect age at marriage or other outcomes and therefore confound the causal estimate. I consider these assumptions in turn.

The first difference-in-differences assumption (commonly referred to as the “common trends” assumption) can be justified by evaluating the trends in age at marriage prior to implementation of the reform. Figure 4 shows the average age of marriage for women married in each year since 1974. With some noise, the average ages of marriage in the Early and Late reform regions appear to have been relatively stable during the period 1988 to 2000, the twelve years prior to the reform. While the Early reform regions have lower average ages of marriage than the Late reform regions, the trends appear relatively parallel and even perhaps flat. Beginning after 2000, when the reform occurred in the Early regions, the average age of marriage increased, reaching the previous average in the Late reform regions. In 2005, when the reform began to be implemented in the Late regions, the average age of marriage there also increased (again, with some noise). This initial visual evidence suggests the tenability of the common trends assumption. The assumption can be more formally tested by including indicators for the pre-reform period, which I consider below.

The second difference-in-differences assumption is not possible to show empirically; it simply requires that we believe that the reform was the only policy implementation that might have affected age at marriage and other outcomes and that, in the absence of the reform, these outcomes would have remained unaffected. I consider this assumption below in the robustness section as I explore other possible interventions.

Even with the difference-in-differences assumptions, the specification above in Equation 4.2 may provide a biased measure of the causal impact of the reform because, as Wolfers (2006) argues, this approach may confound preexisting trends with the dynamic effects of a policy shock; that is, if we do not allow for the reform to change the age at marriage *over the course of the post-reform period*, the coefficient on such a unilateral *Policy* dummy variable may be a biased estimate of the true effect of the policy. To explore this possibility, I also estimate the above model with dummy

variables for each period after the reform. Specifically, following Wolfers (2006), I estimate the following:

$$\begin{aligned} \text{Age at Marriage}_{irt} = & \sum_k \beta_k * \text{Reform has been in effect for } k \text{ periods} \\ & + \eta_t + \mu_r + \psi_1 * t + \psi_2 \mu_r * t + \epsilon_{irt} \end{aligned} \quad (3)$$

In this way, the set of coefficients  $\beta_k$  (where  $k = 1, \dots, 10$ ), in place of the coefficient on the single *Policy* variable, captures the effect over time of the reform on the average age at marriage during each period following the reform. In the results which follow, I use  $k$  as two-year periods.<sup>20</sup> In order to formally test the pre-trends assumption, following Autor (2003), I can also include dummy variables for the years prior to the reform. If the pre-trends assumption is correct, only the coefficients on the  $k$  dummy variables for after the reform was implemented should be significant, while dummy variables for prior to the reform should be statistically indistinguishable from zero. Below, I test this specification as the most conclusive available test of the common trends assumption and find that it holds.

*Verifying unbiased standard errors.* Due to the small number of regions, which are the level of the policy and therefore the level of my identification strategy, according to Bertrand et al. (2004) and Cameron et al. (2008) the standard errors of the difference-in-differences estimates of these specifications may be biased downward, leading to greater likelihood of false rejection of the null (or reported levels of significance that are falsely deflated) in hypothesis tests. Following the suggestions of Cameron et al. (2008), I implement a wild bootstrap method for estimating the standard errors; the results of this method are p-values. I report both versions of significance levels (from standard clustered errors and the wild bootstrapped errors) in order to demonstrate the importance of this correction to proper hypothesis test conclusions, and rely on the more conservative wild bootstrap results in assessing my results.

Finally, establishing the effect of the reform raising the age of marriage as above, I use that flexible form difference-in-differences specification as the first stage, instrumenting for age of marriage, and explore the causal effect of age of marriage on other outcomes for women.

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<sup>20</sup>The number of marriages recorded for 2011 is extremely small since the survey occurred during that year. Therefore, I group these women with those who were married in 2010 in these dynamic specifications.

## 5 Results

### 5.1 Effect of Policy on Marriage Age of Women

Using the sample of repeated cross-sections from the DHS data of women ages 15 to 49 surveyed in 2000, 2005, or 2011, I estimate the effect of the reform on the age at marriage of women married between 1974 and 2011.<sup>21</sup> Table 2 shows the estimates of the basic difference-in-differences specification which uses the *Policy* variable as described above in Equation 4.2. The estimate for  $\gamma$  suggests that the reform increased the age of marriage for affected women by about 1 year and is significant at the 95 percent confidence level.<sup>22</sup> As described above, the minimum age of 15 was not universally enforced prior to the reform, so this estimated effect is an “intent to treat” effect of Ethiopia’s family law reform on the age at marriage.

Figure 5 shows results from estimating the dynamic effects of the reform (see Table 3). Each y-value on the graph is the coefficient on one of the  $k$  dummy variables indicating the time periods before and after the reform.<sup>23</sup> The results suggest that the effect of the reform on the age of marriage of women is increasing over the time of the post-reform period, which suggests that information and public opinion regarding norms may have spread gradually such that the behavior of families marrying their girls also changed gradually as a result of the reform (recall that time trends have been controlled for). The large magnitudes of the effects over time also suggest that, as Wolfers (2006) describes, the unilateral policy variable masks considerable differences in the effect over time and, in this case, represents a conservative estimate of the effect of the reform on the age of marriage since the single policy effect constrains the impact of the reform to be the same over all the post reform years. Thus, I conclude that the effect of the reform in increasing the age of marriage is between 0.97 and 2.4 years; that is, on the low end the estimated effect with the single *Policy* variable is to increase the age of marriage by 0.97 years, but when considering the fact that the reform increased the age of marriage by more in the later years than in the early years (as Figure

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<sup>21</sup>The earliest year of marriage in the sample is 1959 but I restrict the sample to those marriages during or after 1974 because the yearly numbers of marriages before 1974 are very small and likely not representative of those that occurred during those years. The results are not sensitive to this choice.

<sup>22</sup>While the estimated coefficient is significant at the 95 percent confidence level, estimating standard errors using the wild bootstrap method recommended by Cameron et al. (2008), the estimated coefficient is significant at the 90 percent confidence level.

<sup>23</sup>Bailey (2010) explores the effect of legal reforms on contraceptive use in the US during the 1960s a similar fashion.

4 shows), the upper end of the estimated effect of the reform is as high as 2.4 years. Note also that the estimates for the pre-reform periods (years before zero) are statistically indistinguishable from zero, which provides the required test for common trends suggested by Autor (2003).

Figure 6 shows that the distribution of age at marriage shifted to the right: marriage at age 15 and younger decreased in prevalence and marriage at ages 16 or higher increased. The figure plots predicted probabilities of marriage at each age associated with the pre-reform and post-reform periods using an ordered probit model for the specification in Equation 4.2 instead of an OLS regression for age at marriage. Although age 16 is still below the new minimum age of 18, it does appear that families who would have married their daughters at age 15 have responded to the reform by waiting until their girls are 16 instead.<sup>24</sup> The point estimates for marriage at ages 15 and younger are smaller with the *Policy* in place, suggesting that marriages earlier than 15 are also delayed when the *Policy* is in place, although these differences are not statistically significant (which may say more about statistical power than about the effect of the reform). Marriage at ages 16 or older is more likely after the reform than before, and the increase is statistically significant for ages 17 (at the 10 percent level), 18, and 22 (both at the 5 percent level). The reform increased not only the average age of marriage for women, but decreased very early marriage and increased the likelihood that women's ages were nearly or fully compliant with the legal minimum age when they were married.

## 5.2 Probability of Marriage, Divorce, and Widowhood

The analysis above has examined the effect of raising the legal age of marriage on the ages of marriage for those women who were married after the reform took place. Before I proceed to evaluate whether the change in marriage age affected women's outcomes, I address a potential concern of selection among the women who were married: did the reform prevent some women from getting married at all or in some way change the mix of which women were married, in ways that might be non-random or correlated with outcomes? To address this concern, I plot the pre- and post-reform proportions of women whose marital status was "ever married," "divorced," and

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<sup>24</sup>It would be helpful to be able to observe the families who make these decisions directly, but these data do not allow me to do so. In future work, I plan to address and directly estimate the household's problem of marriage age for their children.

“widowed,” including 95% confidence intervals, by age (at survey date).<sup>25</sup> These are Figures 7, 8, 9, respectively. Together with my other results, these figures suggests that, while the average age of marriage as well as the entire distribution of marriage ages shifted upward as a result of the reform to family law, the proportion of women who are married, divorced, and widowed did not change significantly with the reform, so I am little concerned about differential selection of women into or out of marital status.

### 5.3 Effect of Age at Marriage on Outcomes

Having established that the policy increased the age at marriage for affected cohorts of women, reducing the probability of being married very young and at age 15 and increasing the probability of being married at 16 or older, I now turn to examining the effect of age at marriage on women’s outcomes using the estimated of the effect of the policy described above as an instrument for age at marriage. Specifically, I investigate the effect of the policy on women’s education, employment, spousal quality, fertility and family planning, and perceptions of domestic violence.<sup>26</sup>

*Education and Employment.* Table 4 reports results for the effect of the reform on educational and employment outcomes of women. Just as has been asserted by the development community and as Field & Ambrus (2008) found, the increase in age at marriage associated with the reform corresponds to an increase in educational attainment of about one year, precisely the average delay in marriage that I predicted above. The result is also significant at the 95 percent confidence level, although only at the 92 percent confidence interval when correcting for the small number of clusters (the bottom row of these tables reports the p-values from the Cameron et al. (2008) wild bootstrap adjustment to standard errors). Given that the levels of education among the 2000 sample are so low, with a mean of 1.5 years completed and a median of zero years, this gain in educational attainment represents a large improvement relative to the mean. Further, Column 2 of Table 4 shows that women who married later are 18 percentage points more likely to be literate, which I define as having been able to read a full sentence in their native language during the survey; the

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<sup>25</sup>For convenience, I plot the proportions from the early reform regions only, but the results are similar when plotting the proportions in the later reform regions.

<sup>26</sup>As described above in Section 4, I chose variables for women’s perceptions of power from the DHS by selecting those variables for which responses were recorded in each of the three survey years. There are some other questions related to husbands’ perceptions of family planning and women’s perceptions regarding needing permission from their husbands (or mothers-in-law) before seeking medical help for themselves, but these questions were not asked during all three survey years, so I have omitted them here.

result is significant at the 99 percent confidence level even using the wild bootstrap method.<sup>27</sup>

One perhaps surprising result is that the increase in the age of marriage decreases the probability of employment of married women by 21 percentage points (Column 3).<sup>28</sup> However, it should be noted that the data do not allow me to identify whether women's non-employment means women are unemployed and desiring work or simply not participating in the labor force.<sup>29</sup> If the reduction in employment is attributable to higher current school enrollment among women, which I cannot observe (I have only highest completed years for adult women in the surveys), then not only would the reduction in employment be a potential positive effect of the reform, but the increased educational attainment I estimate above would be a lower bound on the true long term effect for women whose marriage was delayed. Perhaps if we could interview those same women a few years later, they would have completed even more years of education and their employment rate would be higher than it would have been had they married younger.

*Spousal quality.* The quality of husbands may change depending on the age at marriage of women. On the one hand, women who are married later may somehow miss out on the suitable or higher quality mates because they waited too long and the "good ones" are gone. On the other hand, girls who marry later may find mates with whom they are better matched or they may marry men who are more progressive in their view of marriage.<sup>30</sup> In fact, the quality of spouses is frequently cited as a concern for family members and a reason for early marriages for girls. Table 5 shows that women married later due to the policy have not married significantly older or younger men as a result (Column 1). In addition, they have married men who do not have substantially more education than the spouses of their peers who marry younger (Column 2).<sup>31</sup> However, these women are 11 percentage points more likely to be in polygamous relationships (significant at the 10 percent level using the wild bootstrap adjustment), which may indicate a lower status of women in the marriage.

*Fertility timing and family planning.* Table 6 reports results for specifications with outcomes related to fertility and family planning. Frequent and many child births are thought to be a cause

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<sup>27</sup>I also find that they are more likely to have completed primary and secondary school.

<sup>28</sup>This is robust to inclusion of an age variable in addition to the year of marriage fixed effects.

<sup>29</sup>However, the employment rate among the women in the sample is very low relative to what is usually reported for Ethiopia, which I cannot explain.

<sup>30</sup>These men may also be better educated and have higher earnings.

<sup>31</sup>Although the estimated standard error is small enough that the confidence level is 99 percent, the wild bootstrap method returns a p-value of 0.13.

of women's lower status and limited economic opportunity in addition to the source of a number of health problems related to frequent child birth for both mothers and their children. If women who marry older are indeed more mature and able to increase control (either through knowledge or through bargaining power within the marriage) over fertility timing and frequency, then increasing age at marriage would be an important dimension of improving the well-being and status of women.

The results in Table 6 suggest that women who marry later as a result of the reform are 22 percentage points more likely to answer correctly questions about the ovulatory cycle (significant at the 5 percent level using the wild bootstrap method) relative to a low preexisting mean. The interval between their marriage and their first birth may be slightly longer, but the result is not statistically significant when using the wild bootstrap correction. They do have fewer children (about half a child) than those who were married before the reform (and younger on average); the result is highly significant even when using the wild bootstrap correction.

*Spousal control and domestic violence perceptions.* The final set of outcomes I consider here addresses the extent to which marrying later changes other aspects of decision making in the household beyond family planning. The DHS are particularly interested in these issues and they ask a large number of questions related to them. Table 7 shows results from a few of these questions. The results suggest some favorable improvements in the status of women in their marriages: women are less likely to believe they have *no* power in asking their husbands to use condoms if they have STIs (Column 2) and their husbands are less likely to have exclusive control over their own earnings (Column 3), suggesting that women increasingly believe they have voice in their relationships when they marry later (significant at the 1 and 5 percent levels, respectively when using the wild bootstrap corrected standard errors).

On the other hand, women's responses to questions related to domestic violence do not appear to have responded to later marriage for women. Women who are older when they marry are no less likely to answer that their husbands are justified in beating them for a number of reasons. The DHS questionnaire asks women (married or not) whether husbands are justified in beating their wives in certain situations: for burning the food, going out without the husband's permission, refusing to have sex with her husband, etc. While clearly it would be preferable to count actual domestic violence offenses rather than perceived justifiability, these data are not available in the nationally representative sample. However, the presence of other family members was noted for each individual



record, and most of the married women were not in the presence of their husbands or other male family members when they responded to these questions. Further, a cross-country report from the World Health Organization suggests that occurrence of violence by husbands toward their wives is more prevalent in Ethiopia than in any other Sub-Saharan African country in their study; they report that more than 70 percent of women have been victims of abuse, either physical or sexual, by their own husbands.<sup>32</sup> In any event, if later age at marriage (and the increased education that my results suggests came along with it) substantially increased a woman's bargaining position in a marriage, we would hope that increased marriage ages would correspond to more progressive views on the appropriateness of domestic violence in marriages. However, the results of Table 8 suggest that marrying later as a result of the reform did not change these perceptions on the part of women and that women still perceive their husbands to have significant rights to control them, even with violence.

## 6 Robustness and Discussion

### 6.1 Heterogeneous Results by Religion

Ethiopia's diverse religious environment, and the Islamic pressures associated with prohibiting bigamy in the draft revolutionary constitution in 1974, suggests the possibility that the family law reform in 2000 might have differential effects across religious groups. To explore this, I interact the *Policy* variable as above with indicator variables for Muslim or Ethiopian Orthodox background, and include the indicators separately for level effects. Another potential reason for heterogeneity by religious background is the manner in which news was spread in Ethiopia of the revised minimum age of marriage laws: Ethiopian Orthodox priests were recruited and gathered in discussion groups and were trained on the significant social and individual costs of early marriage, but there does not appear to have been a similar information campaign among Muslim religious leaders. Table 9 shows the effect of the reform on age of marriage by religious background. The results suggest that there are no baseline differences among Muslim and Orthodox women in their average marriage ages (the coefficients on the religion dummy variables are insignificant and not distinguishable from each other), but the effect of the reform on age at marriage was reduced for Muslim women, by

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<sup>32</sup>See Allen & Raghallaigh (2013).

eight tenths of a year. The education, literacy, and polygamy results are similarly different by religious background. Turning to bargaining outcomes, however, it appears that Muslim women who marry older as a result of the reform are the women who benefit the most:

## 6.2 Other Interventions in Ethiopia

### *Health Extension Workers and Family Planning Radio Campaigns*

In 2003, the Ethiopian Ministry of Health launched an extensive health service delivery program to train and send out health extension workers, with a special eye toward education of women on family planning.<sup>33</sup> In order to assess whether my results – which suggest that marrying later as a result of the reform increased the accuracy of women’s knowledge of how ovulation and conception works – are perhaps caused by the Health Extension Workers (HEW) program instead of the marrying later, I control for whether or not women report having been visited in the previous year by a family planning worker. The incidence of women having been visited by these workers rose from 2% to 18% between 2000 and 2011, consistent with government efforts to improve the scope of care. However, Column 1 of Table 12 shows that controlling for visits by these workers not only does not change the effect of marrying later on correct knowledge of the ovulatory system, but does not have a significant coefficient itself. This suggests that this program was perhaps not effective in increasing women’s awareness of the biology of family planning, or at least that its effect is imprecisely measured.<sup>34</sup> On the other hand, the government and some NGOs also supported a radio program discussing family planning in advertisements; over the 2000 to 2011 period the proportion of women who reported having heard these advertisement increased by 50% (from 20 percent to 30 percent). Controlling for this form of knowledge dissemination in the estimation in Column 2 of Table 12 does decrease the effect of age of marriage on knowledge of the ovulatory cycle, but only by about 2 percentage points. The radio program also seems to have been effective at increasing women’s knowledge, though the effect is much smaller (13 percentage points). Combining these two specifications, Column 3 of Table 12 produces the same results. Overall, these results suggest that marrying later does improve knowledge of women’s health and, hence, women who

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<sup>33</sup>See <http://cnhde.ei.columbia.edu/training/index.html>.

<sup>34</sup>In an interview with a former NGO worker in Ethiopia, it was suggested to me that the HEW disseminated contraceptives but not knowledge of the ovulatory cycle, owing to the taboo associated with discussing these matters.

marry later have more knowledge with respect to their fertility processes.

### 6.3 Additional Robustness Concerns

My results are also robust to exclusion of the two main urban centers (Addis Ababa and Dire Dawa), and the results maintain their magnitudes and significance levels if I consider each of the remaining early adopters individually (Amhara, Tigray, and Afar). Additionally, inclusion of ethnicity fixed effects or controls for altitude (Ethiopia’s landscape is quite varied and poverty is concentrated in the highland areas) does not affect the results.<sup>35</sup>

## 7 Conclusion and Discussion

This paper explores the effect of a reform to family law in Ethiopia on the age at marriage of young women and the effects of age at marriage on various outcomes. I find that early marriage for girls was delayed as a result of the reform; on average, marriage was delayed by 1 year, although the effect of the reform on the age at marriage increases during the years after the reform. The reform appears to have reduced the prevalence of very early marriage (15 and under) in favor of later teenage marriage (16 to 18) and early adult marriage. I also find that education increased by 1 year as a result of the reform and its effect on age at marriage, and that women are 18 percentage points more likely to be able to read a full sentence in their language. However, I find mixed empirical support for a causal relationship between age at marriage and women’s other outcomes related to fertility, spousal quality, or perceptions of the appropriateness of domestic violence. On the one hand, women married after the reform are more likely to have some control over their husbands’ income and to feel that they can stand up for themselves in sexual health, but their perceptions of the appropriateness of domestic violence are not changed as a result of later average marriage. My results suggest that while legal reform and other efforts to improve women’s status may be effective in some respects, and ending child marriage is certainly a human rights concern, there remains more work to be done to improve women’s lives in other dimensions.

Of course, it is possible that my estimation has failed to return evidence for the existence of a more complete causal relationship for a number of reasons. First, while I do observe an increase

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<sup>35</sup>As an additional robustness test, I consider a falsification test using a different country of data from the DHS. The results from this are forthcoming.

in marriage age among some women, there may be some heterogeneity in the relationship between age at marriage and women's outcomes, and to the extent that poor enforcement of the minimum age at marriage fails to prevent early marriage among those girls for whom early marriage produces negative effects, my results have failed to capture the effect for those women. Households who were encouraged by the policy to raise the age at marriage of their daughters may be already optimizing their daughters' well-being (through, e.g., education) in ways that contribute to positive outcomes. Another possible explanation for my insignificant results may be that the relationship between early marriage for girls and other outcomes may take time to take effect and that, with such a short time frame in which to follow up on outcomes, I have failed to observe these impacts. However, my estimates correspond to the time frame for observing the changes in outcomes that are frequently discussed by those in the policy community, so my results still suggest a moderation of expectations of legal reform's ability to prevent the negative effects of child marriage.

Another concern to internal validity of my estimates is that, perhaps, the women whose outcomes would have improved the most from a change in age at marriage are "missing" from the data because instead of observing their changes they have simply passed away due to their low outcomes. While women and girls are discriminated against in a number of ways in Ethiopian society, sex-selective abortion, infanticide, or discrimination resulting in disproportionately more deaths among women and young girls do not appear to be an issue in Ethiopia.<sup>36</sup> Thus, I can be reasonably sure that my estimates are not biased downward by failing to include women whose inclusion would have caused an estimated favorable effect on outcomes.

This is only one study and requires additional validation, both external to Ethiopia as well as additional data for longer term outcomes. However, my results make clear that ending child marriage itself is not a panacea without additional efforts to increase the dimensions along which girls are made worse off. Multi-lateral development projects in addition to legal reform must continue to be pursued if true progress that benefits women and their families is to be made.

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<sup>36</sup>According to the CIA Factbook, the overall sex ratio (males divided by females) in 2013 was 0.99, which is lower even than other parts of Sub-Saharan Africa which has traditionally been thought to have very few "missing women."

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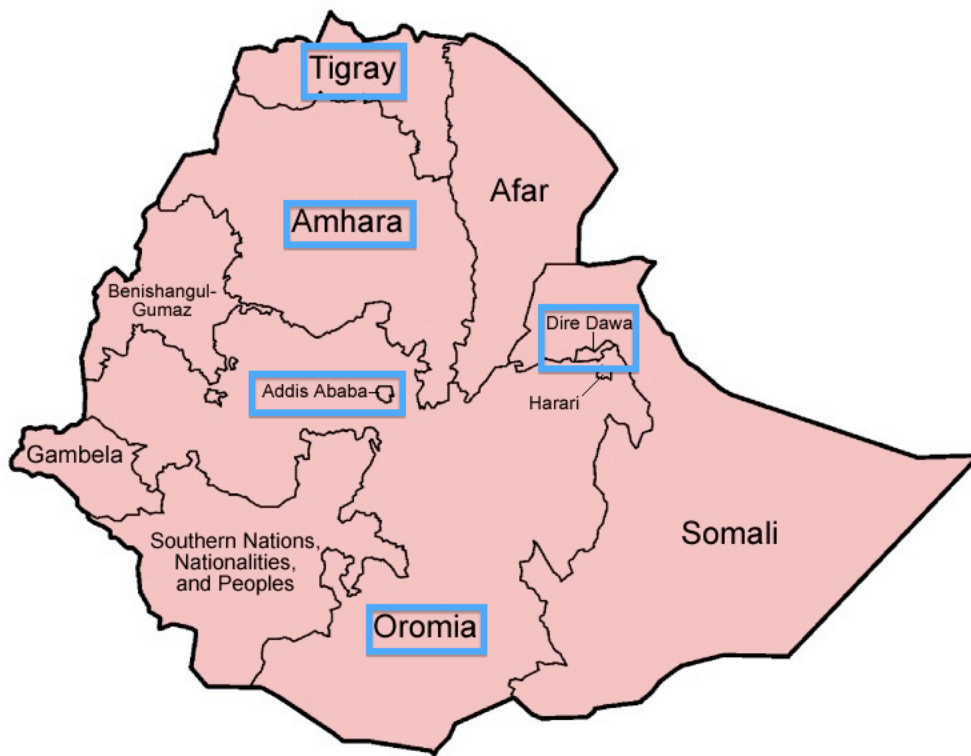
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Figure 1: Map of Ethiopia



Regional map of the Federal Democratic Republic of Ethiopia. Regions whose names are outlined adopted the reformed family law between 2000 and 2005, with the other regions adopting the law later.

Figure 2: Timeline of Reform and DHS Data Collection

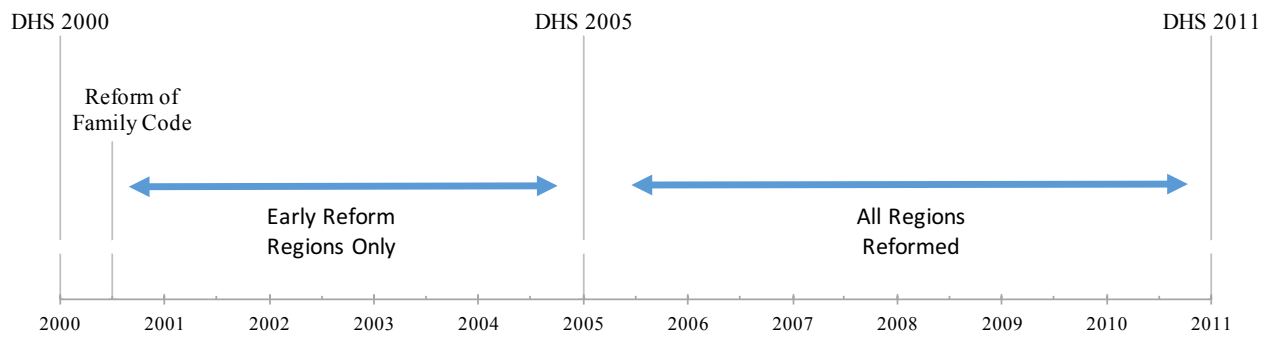
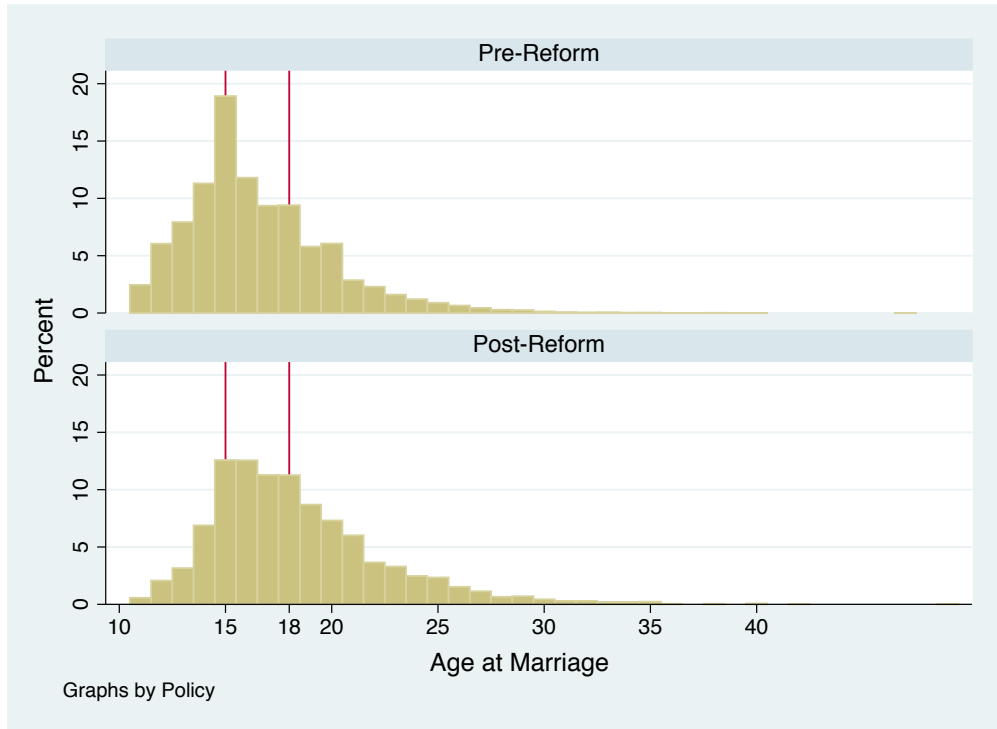
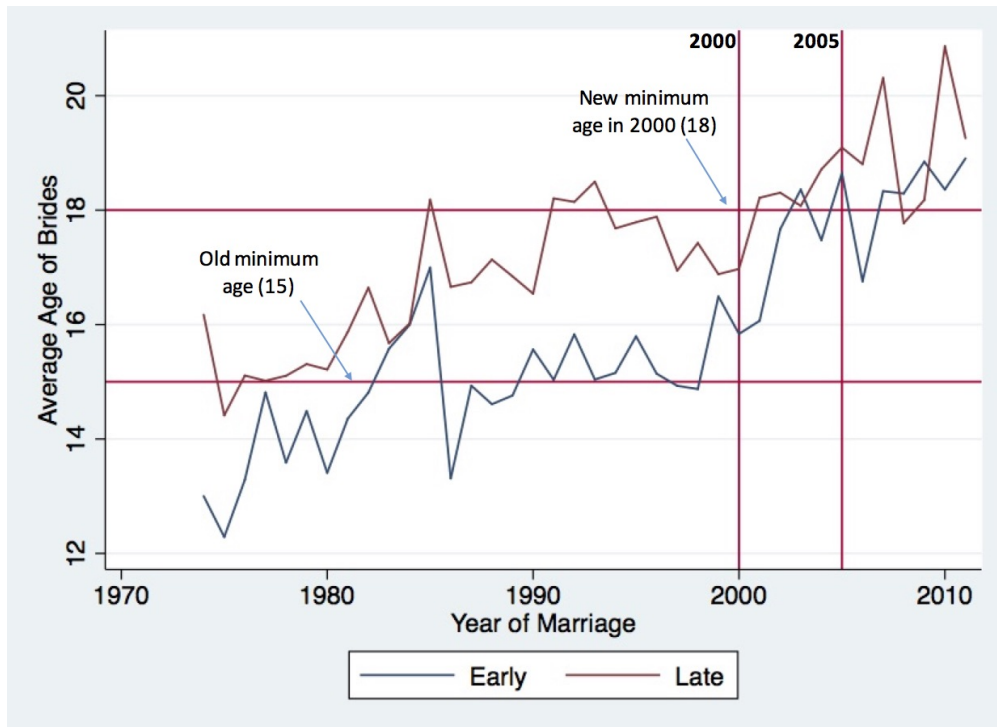


Figure 3: Histogram of Age at Marriage Before and After Reform



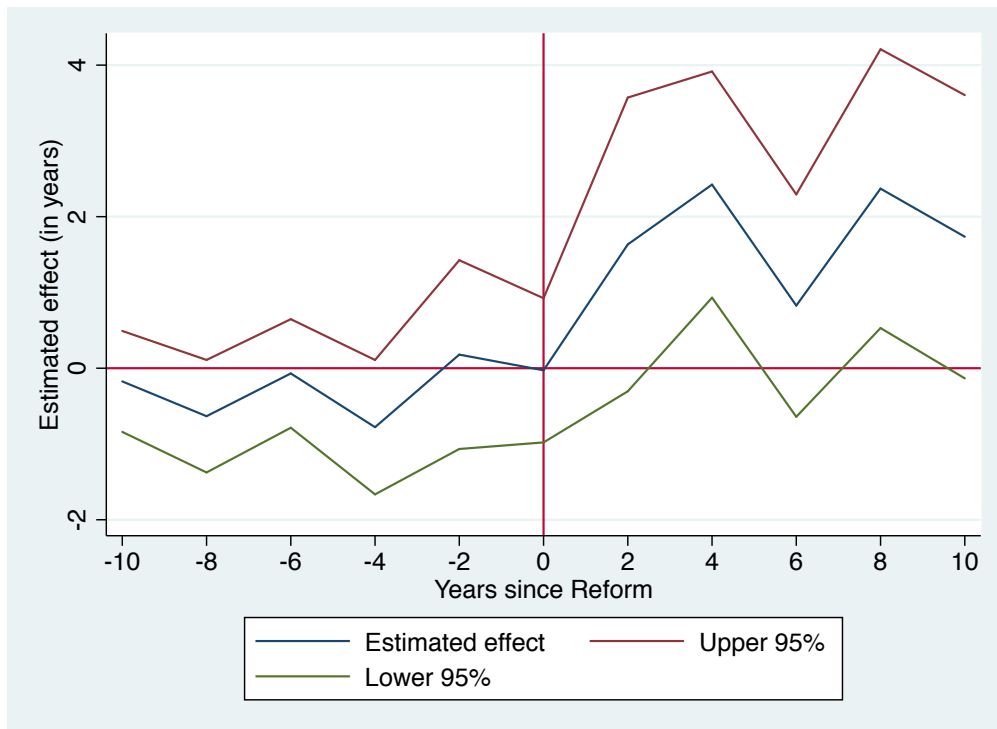
Source: Author's calculations from DHS data.

Figure 4: Average Ages of Marriage



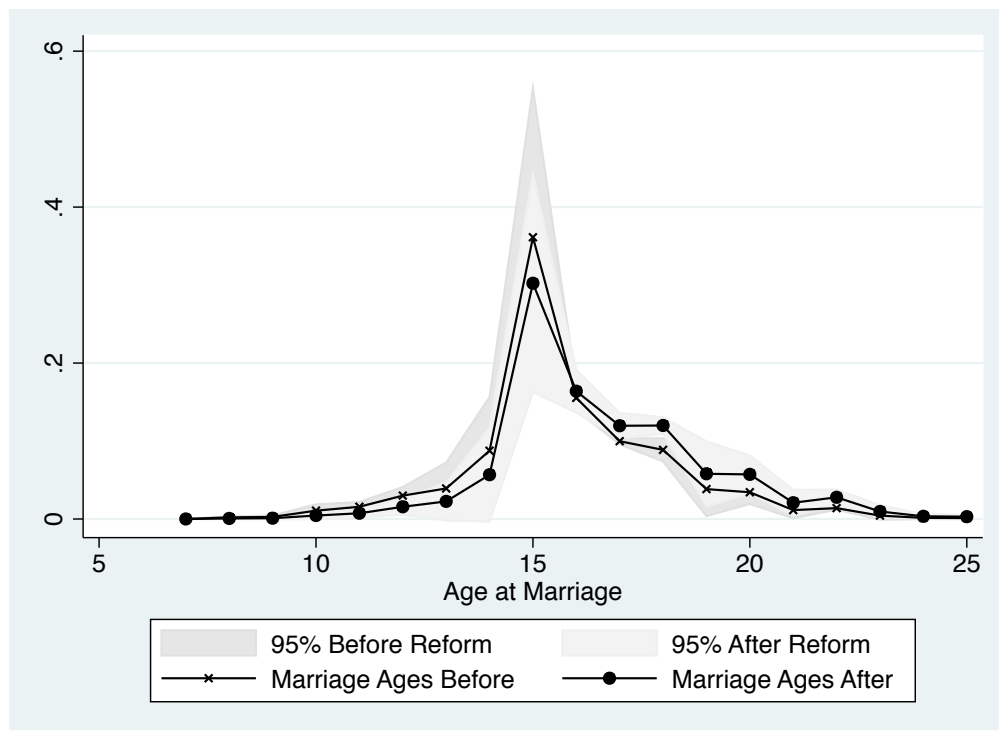
These averages are calculated from the raw data as the average age of marriages occurring in each year. Source: Author's calculations from DHS data.

Figure 5: Estimated Dynamic Effect of Reform



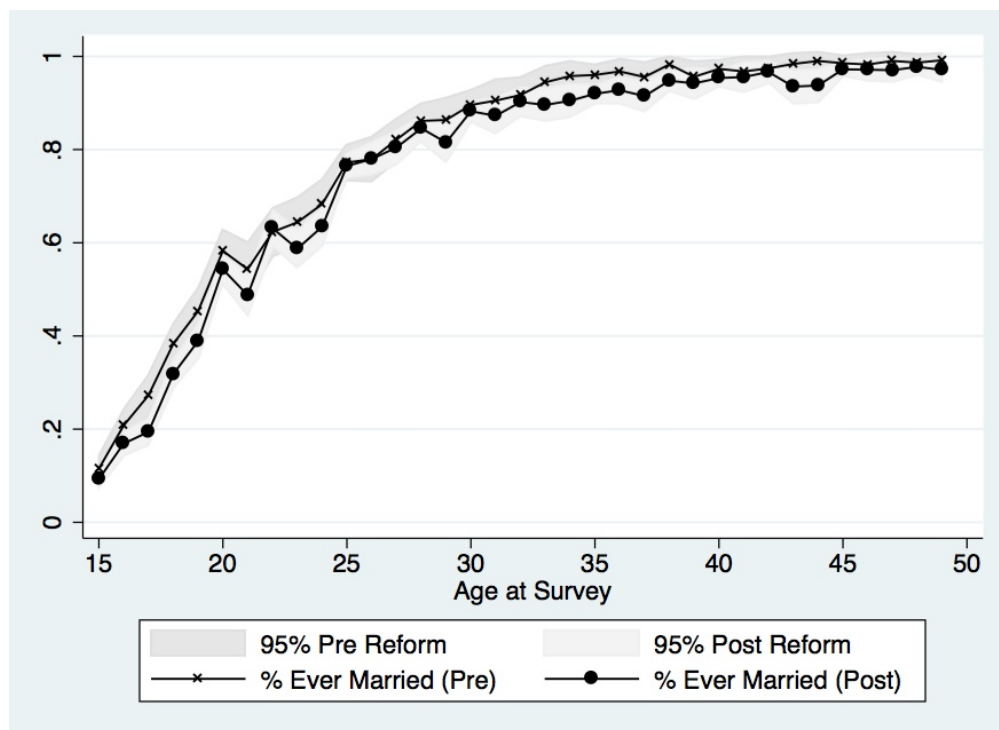
The figure shows estimated coefficients on leads and lags of the reform to show the dynamic effect of the reform and that the reform was both unanticipated and there were no trends prior to the reform that might be biasing the standard difference in differences coefficient. The estimated coefficients and standard errors are reported in Table 3. Source: Author's calculations from DHS data.

Figure 6: Effect of Reform on Distribution of Marriage Ages



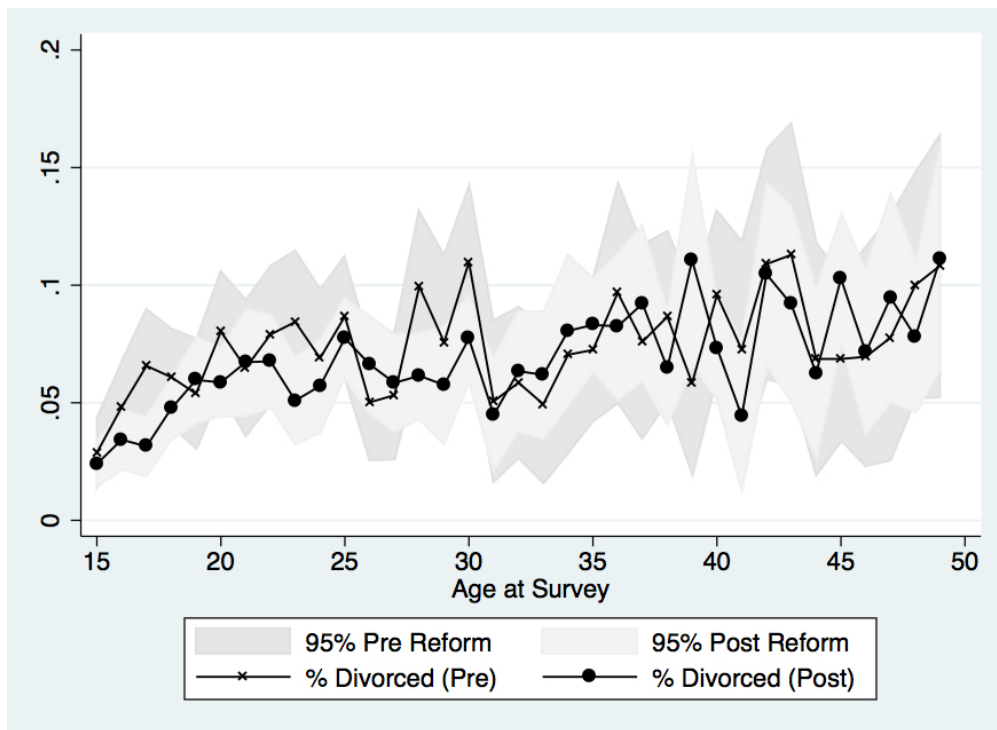
The figure shows the estimated distribution of marriage ages using an ordered probit and estimating marginal probabilities before and after the reform. Source: Author's calculations from DHS data.

Figure 7: Proportion of Women Married by Age at Survey Date



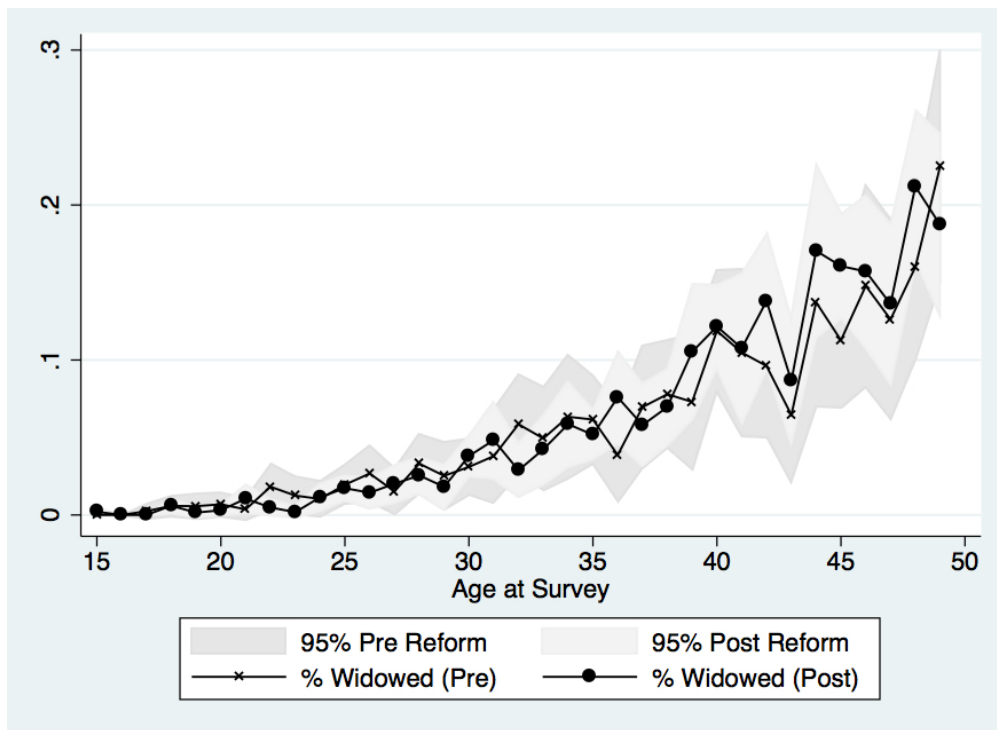
The figure shows the proportion of women by age at survey date who are “ever married” for the early reform regions before and after the reform took place, with 95% confidence intervals using the standard errors. The graph for late reform regions looks similar but is omitted here. Source: Author’s calculations from DHS data.

Figure 8: Proportion of Women Divorced by Age at Survey Date



The figure shows the proportion of women by age at survey date whose marital status is “divorced” for the early reform regions before and after the reform took place. The graph for late reform regions looks similar but is omitted here. Source: Author’s calculations from DHS data.

Figure 9: Proportion of Women Widowed by Age at Survey Date



The figure shows the proportion of women by age at survey date whose marital status is “widowed” for the early reform regions before and after the reform took place. The graph for late reform regions looks similar but is omitted here. Source: Author’s calculations from DHS data.



Table 1: Summary Statistics

	Child Brides	Adult Brides	P-value for Difference
Age at Marriage	14.43556	20.6339	0
Age at First Intercourse	14.57322	19.86294	0
Age at Survey	29.37768	31.18565	0
Divorced	.0833458	.0638402	1.66e-09
Separated or Divorced	.1156554	.1057476	.0098607
Widowed	.0499376	.0460198	.1340916
Ethiopian Orthodox	.4492884	.3819642	3.80e-29
Muslim	.3837703	.3940076	.0839239
Rural	.8084894	.6751199	0
Early Reform Regions	.5429214	.4993635	7.04e-13
Age difference of spouses	8.775576	7.557668	0
Educational attainment (in years)	1.220924	2.875453	0
Literate	.1339788	.2752166	0
Completed Primary School	.1798118	.306922	0
Completed Secondary School	.0571514	.1563083	0
Husband's education	2.4848	4.148974	0
Employed	.4984696	.4984255	.9942322
In Polygamous Marriage	.1272935	.1224182	.2677973
Knows Ovulatory Cycle	.1270911	.2038578	0
Marriage to First Birth (in months)	40.47028	26.07379	0
Not Justified in Asking for Condom use with STI	.2942381	.2700523	.0002581
Husband Exclusively Controls His Earnings	.3071332	.2752792	.0012947
<i>Beating wife is justified if wife...</i>			
Goes out without telling husband	.5658427	.506903	1.98e-22
Neglects the children	.6163296	.5634975	7.39e-19
Argues with husband	.5661423	.4945658	2.80e-32
Refuses sex	.4612235	.4172134	3.25e-13
Burns the food	.5661423	.4841868	0

Table 2: Effect on Age at Marriage

VARIABLES	(1) Age at Marriage
Policy	0.971** (0.393)
Observations	30,238
R-squared	0.304
Wild bootstrap p-value	0.0749*

Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 3: Dynamic Effect of the Reform

VARIABLES	(1) Age at Marriage
9 - 10 Years After	1.735* (0.934)
7 - 8 Years After	2.370** (0.920)
5 - 6 Years After	0.826 (0.734)
3 - 4 Years After	2.423*** (0.746)
1 - 2 Years After	1.633 (0.969)
Year of Reform	-0.0278 (0.476)
1 - 2 Years Before	0.179 (0.623)
3 - 4 Years Before	-0.778 (0.443)
5 - 6 Years Before	-0.0694 (0.358)
7 - 8 Years Before	-0.633 (0.371)
9 - 10 Years Before	-0.174 (0.333)
Observations	30,238
R-squared	0.276

Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 4: Effect on Education and Employment

VARIABLES	(1) Educational attainment (in years)	(2) Literate	(3) Employed
Age at Marriage	1.163*** (0.279)	0.176** (0.0742)	-0.210*** (0.0729)
Observations	30,238	29,770	30,091
Wild p-value	0.0796*	0.0010***	0.0025***

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 5: Effect on Spouse Quality

VARIABLES	(1) Age difference of spouses	(2) Husband's education	(3) In Polygamous Marriage
Age at Marriage	0.354 (1.436)	1.353*** (0.481)	0.146** (0.0677)
Observations	25,274	29,891	25,241
Wild p-value	0.7516	0.1323	0.0675

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 6: Effect on Fertility Outcomes

VARIABLES	(1) Knows Ovulatory Cycle	(2) Marriage to First Birth (in months)	(3) number of living children
Age at Marriage	0.235** (0.109)	5.066 (4.111)	-0.382 (0.256)
Observations	30,238	22,065	30,238
Wild p-value	0.0424	0.1448	0.0025

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 7: Effect on Bargaining Power in Marriage

VARIABLES	(1) Not Justified in Asking for Condom use with STI	(2) Husband Exclusively Controls His Earnings
Age at Marriage	-0.0710 (0.0460)	-0.142** (0.0699)
Observations	20,745	9,229
Wild p-value	0.003***	0.049**

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 8: Domestic Violence: Beating is justified if...

VARIABLES	(1) Goes out without telling husband	(2) Neglects the children	(3) Argues with husband	(4) Refuses sex	(5) Burns the food
Age at Marriage	-0.0613 (0.0702)	-0.0509 (0.0572)	-0.0336 (0.0720)	-0.0544 (0.0591)	-0.0374 (0.0766)
Observations	30,238	30,238	30,238	30,238	30,238
Wild p-value	0.574	0.504	0.769	0.557	0.784

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 9: Age at Marriage - Heterogeneous Results by Religion

VARIABLES	(1) Age at Marriage
Policy	1.387*** (0.312)
Orthodox*Policy	-0.356 (0.224)
Muslim*Policy	-0.844** (0.326)
Ethiopian Orthodox	-0.154 (0.393)
Muslim	-0.344 (0.415)
Observations	30,238
R-squared	0.307

Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 10: Education and Polygamy Outcomes - Heterogeneous Results by Religion

VARIABLES	(1) Educational attainment (in years)	(2) Literate	(3) In Polygamous Marriage
Age at Marriage	0.934*** (0.198)	0.128* (0.0656)	0.121** (0.0537)
Orthodox*Age at Marriage	0.391*** (0.130)	0.0818** (0.0387)	0.0561* (0.0309)
Muslim*Age at Marriage	0.249 (0.280)	0.0787 (0.0698)	0.0480 (0.0484)
Ethiopian Orthodox	-6.243** (2.659)	-1.353* (0.727)	-1.022** (0.514)
Muslim	-4.583 (4.943)	-1.327 (1.176)	-0.683 (0.821)
Observations	30,238	29,770	25,241
R-squared	-0.930	-2.238	-1.637

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 9. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 11: Bargaining Power and Domestic Violence Outcomes - Heterogeneous Results by Religion

VARIABLES	(1) Knows Ovulatory Cycle	(2) Husband Exclusively Controls His Earnings	(3) Neglects children
Age at Marriage	0.157* (0.0884)	-0.0882 (0.0718)	-0.0278 (0.0532)
Orthodox*Age at Marriage	0.114*** (0.0409)	-0.134 (0.0919)	-0.0211 (0.0249)
Muslim*Age at Marriage	0.165* (0.0908)	-0.143* (0.0863)	-0.0705*** (0.0265)
Ethiopian Orthodox	-1.910** (0.758)	2.387 (1.681)	0.288 (0.410)
Muslim	-2.695* (1.537)	2.423 (1.542)	1.121*** (0.415)
Observations	30,238	9,229	30,238
R-squared	-2.634	-0.571	0.166

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 9. The dependent variable in Column 3 is the response to the question "Is your husband justified in beating you if you neglect the children?" as in line with the questions from Table 8. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.

Table 12: Robustness: Family Planning Awareness Programs

VARIABLES	(1) Knows Ovulatory Cycle	(2) Knows Ovulatory Cycle	(3) Knows Ovulatory Cycle
Age at Marriage	0.232** (0.106)	0.209** (0.104)	0.209** (0.102)
Family Planning Worker Visit	0.0471 (0.0719)		0.0199 (0.0611)
Heard Family Planning on Radio		0.129** (0.0584)	0.128** (0.0581)
Observations	30,225	30,232	30,220
Wild p-value	0.0724	0.1723	0.1723

These are second stage effects where the first stage uses the policy as an instrument for age at marriage as in Table 2. Year fixed effects, region fixed effects, and region-specific time trends are included. Standard errors shown in parentheses are corrected for heteroskedasticity and clustering at the region level with \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$  and \*  $p < 0.1$ . P-values in the bottom row are for wild bootstrap standard errors following Cameron et al. (2008) for a null hypothesis of a zero effect. Source: Author's calculations from DHS Ethiopia 2000, 2005, and 2011 surveys of married women ages 15-49.