

ESSAYS ON EDUCATION AND THE FAMILY

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This dissertation explores issues related to children's schooling and well-being in Africa and Asia. Using household level data, I empirically assess the effectiveness of a policy that raised the minimum age of marriage for girls in Ethiopia; the effect of a large financial crisis on inequality in educational attainment between siblings in Indonesia in both the short and long-run; and the relationship among living away from home, school attendance, and work for children in West Africa.

BIOGRAPHICAL SKETCH

My life as an economist began during my first semester at Wellesley College in the Fall of 2004, when I realized with excitement that an academic discipline existed that mathematically represented and evaluated individuals' and families' private, communal, and social decisions, and often with an eye toward designing policy and improving people's lives. "How is that economics?" is a common refrain when I explain my research to non-economists. And yet it is! After years of learning theory and empirical methods, I am delighted to join the professional ranks of those who use these tools for good. I will continue to learn and my hope is that it makes some kind of difference.

I dedicate this dissertation to Jane, Bill, and Liz McGavock to whom I am so grateful for support of all kinds. And in loving memory to my grandfather, Captain John O. F. Dorsett, a scientist, discoverer, and lover of coffee. I barely knew you before you passed, but I'm told we would have been so very fond of one another, and that you would have been particularly proud of this accomplishment.

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CHAPTER 1

**CHILD BRIDES, BARGAINING POWER, AND REFORM OF ETHIOPIA'S
FAMILY LAW**

"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

1.1 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 ear-

lier 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 1.3 discusses the particular anthropological, ethnographic, and political environments in which my empirical approach is set. Section 1.4 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 1.5, I present

results from my estimation and I discuss their robustness in Section 1.6. Section 1.7 concludes and discusses areas for future work.

1.2 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 1981.¹ 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."² 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,

¹Jacobson (1992).

²Boyden et al. (2013).

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.³

1.2.1 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.

³However, 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 (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.

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.⁴ 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)⁵, 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 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 fam-

⁴Conditional 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.

⁵See, for example, Pankhurst (1992).

ily, 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.

1.2.2 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.⁶ 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

⁶Browning et al. (1994).

(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.

1.2.3 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 prop-

erty 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.

1.3 Anthropological, Political, and Empirical Contexts

1.3.1 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).⁷

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

⁷The majority of brides bring nothing to the marriage.

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.

1.3.2 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 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.⁸ 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

⁸See Federal Democratic Republic of Ethiopia (2000).

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 1.6, I directly address these other aspects.⁹

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

⁹Certainly 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.

until after 2005.¹⁰ Figure 1.1 presents a map of Ethiopia with the early reform regions boxed. Following Hallward-Driemeier & Gajigo (2011), I will henceforth refer to these eleven units - the nine regions plus the two municipalities - as regions.¹¹

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).

¹⁰These are: Afar, Somali, Gambela, Benishangul-Gumaz, Harari, and the region called Southern Nations, Nationalities, and Peoples (SNNP).

¹¹This is how they are defined in the DHS data as well.

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-Gumuz) (Pausewang et al., 2002; Smith, 2013). With respect to the Revised Family Code, Smith (2013) finds only one 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.¹² 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 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

¹²See Walters (2014).

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.

1.4 Empirical Strategy

1.4.1 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 2011.¹³ 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 1.2 shows the timing of these surveys relative to the reform and its occurrence in

¹³The 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.

the different regions.

The surveys were designed to be nationally and regionally representative for the purpose of policy planning.^{14,15} In addition to gathering more objective quantitative information from women on 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 1.1 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 1.3 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 hy-

¹⁴The 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.

¹⁵Due 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.

pothesis (at the 99% confidence level) that the distribution of marriage ages is identical before and after the policy was implemented.

1.4.2 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:

$$\begin{aligned} \text{Age at marriage}_{irt} = & \beta_1 \text{Reform Region}_r + \beta_2 \text{Post} \\ & + \beta_3 (\text{Reform Region}_r * \text{Post}) + \eta_t + \mu_r + \mu_r * t + \epsilon_{irt} \end{aligned} \quad (1.1)$$

where the difference-in-differences estimator β_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 η_t , region fixed effects μ_r , and region-specific linear time trends, $\mu_r * t$, in order to control for unobserved time or region-varying

characteristics.¹⁶

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.¹⁷ 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.¹⁸ Therefore, I adapt the specification above as follows:

$$\text{Age at marriage}_{irt} = \gamma \text{Policy} + \eta_t + \mu_r + \psi \mu_r * t + \epsilon_{irt} \quad (1.2)$$

Including the same fixed effects and controls as above, this specification captures the effect γ 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.¹⁹

The difference-in-differences estimator γ captures the causal impact of the reform only if the difference-in-differences assumptions are satisfied. These assumptions require that 1) in the absence of the reform, the regions that reformed

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

¹⁷Figure 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.

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

¹⁹If 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.

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 1.4 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 1.4.2 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:

$$\begin{aligned} \text{Age at Marriage}_{irt} = \sum_k \beta_k * \text{Reform has been in effect for } k \text{ periods} \\ + \eta_t + \mu_r + \psi_1 * t + \psi_2 \mu_r * t + \epsilon_{irt} \end{aligned} \quad (1.3)$$

In this way, the set of coefficients β_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.²⁰ 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 specifica-

²⁰The 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.

tion as the most conclusive available test of the common trends assumption and find that it holds.

The above approach captures the effect of the policy on the outcomes of marriage age and other dimensions of women's wellbeing but may be influenced by the endogeneity of the timing of marriage (before or after the policy). Therefore, I explore also the effect of the reform by considering birth cohorts that would be affected by the minimum age of marriage because they were under 18 at the time of the reform in their region. Specifically, I estimate the following as a supplementary analysis to the analysis described above and confirm the results:

$$\text{Married by Age 18}_{irt} = \beta \text{Exposed Cohort}_{irt} + \eta_t + \mu_r + \eta_t * \mu_r + \epsilon_{irt} \quad (1.4)$$

where the dependent variable is an indicator for whether a woman was married by age eighteen (or another age), "Exposed Cohort" is an indicator for whether a woman was under 18 at the time of the reform in her region, and a full set of year of birth (η_t) and region (μ_r) fixed effects, as well as their interactions, is included.

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.

1.5 Results

1.5.1 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 2011.²¹ Table 1.2 shows the estimates of the basic difference-in-differences specification which uses the *Policy* variable as described above in Equation 1.4.2. The estimate for γ 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.²² 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.

²¹The 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.

²²While 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.

Figure 1.5 shows results from estimating the dynamic effects of the reform (see Table 1.3). 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.²³ 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 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 1.6 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

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

probit model for the specification in Equation 1.4.2 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.²⁴ 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.

1.5.2 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

²⁴It 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.

concern, I plot the pre- and post-reform proportions of women whose marital status was “ever married,” “divorced,” and “widowed,” including 95% confidence intervals, by age (at survey date).²⁵ These are Figures 1.7, 1.8, 1.9, 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.

1.5.3 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.²⁶

²⁵For 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.

²⁶As described above in Section 1.4, 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.

Education and Employment. Table 1.4 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 1.4 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 result is significant at the 99 percent confidence level even using the wild bootstrap method.²⁷

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).²⁸ 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.²⁹ 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

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

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

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

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.³⁰ In fact, the quality of spouses is frequently cited as a concern for family members and a reason for early marriages for girls. Table 1.5 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).³¹ 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 1.6 reports results for specifications with outcomes related to fertility and family planning. Frequent and many child births are thought to be a cause of women’s lower status and limited economic

³⁰These men may also be better educated and have higher earnings.

³¹Although 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.

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 1.6 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 1.7 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.³² 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 1.8 suggest that marrying later as a result of the reform did not change these perceptions on the part of women and that

³²See Allen & Raghallaigh (2013).

women still perceive their husbands to have significant rights to control them, even with violence.

1.6 Robustness and Discussion

1.6.1 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 1.9 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 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:

1.6.2 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.³³ 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 1.12 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.³⁴ On the other hand, the government and some NGOs

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

³⁴In 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.

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 1.12 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 1.12 produces the same results. Overall, these results suggest that marrying later does improve knowledge of women's health and, hence, women who marry later have more knowledge with respect to their fertility processes.

1.6.3 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.³⁵

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

1.7 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 in marriage age among some women, there may be some heterogeneity in the relationship between age at marriage and

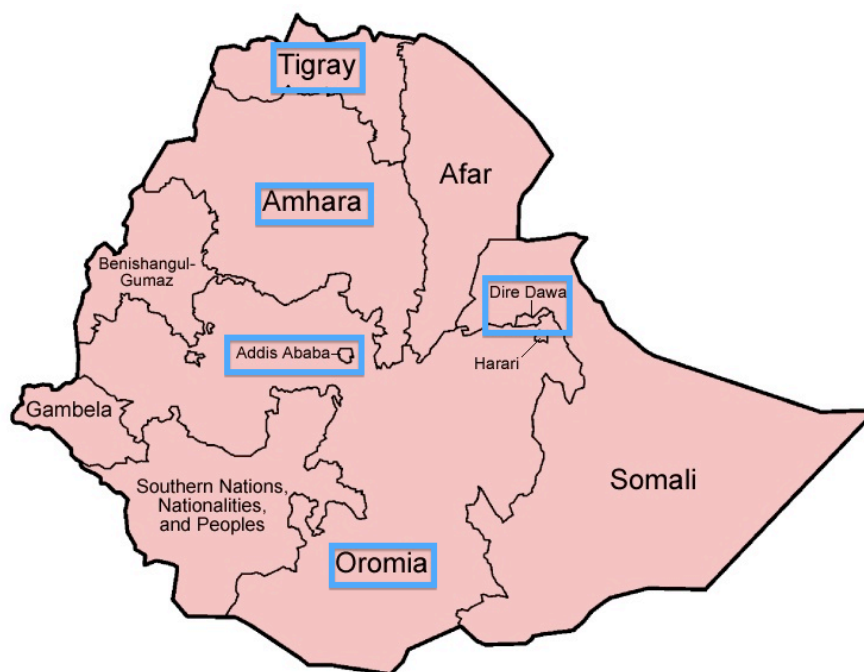
women's outcomes, and to the extent that poor enforcement of the minimum age at marriage fails to prevent early marriage among those girls for whom early marriage produces negative effects, my results have failed to capture the effect for those women. Households who were encouraged by the policy to raise 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.³⁶ 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.

³⁶According 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."

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.

Figure 1.1: 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 1.2: Timeline of Reform and DHS Data Collection

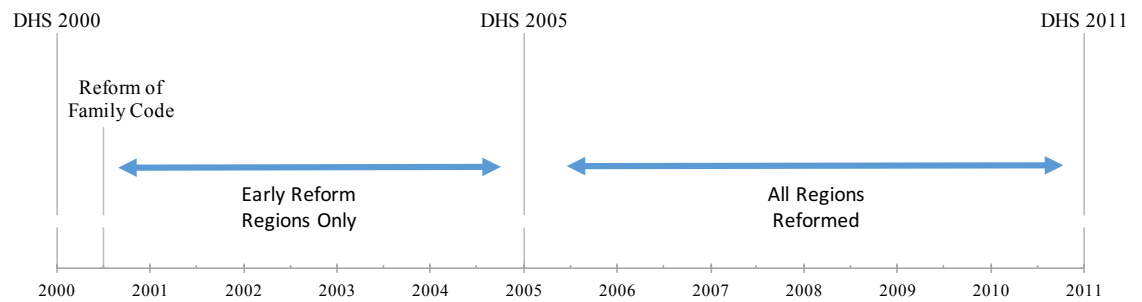
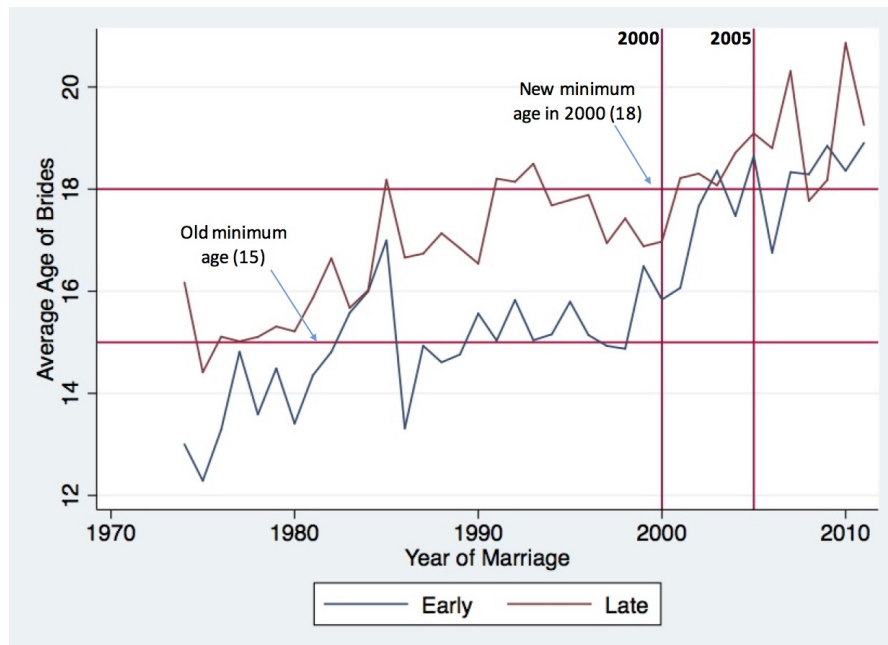


Figure 1.3:
Histogram of Age at Marriage Before and After Reform



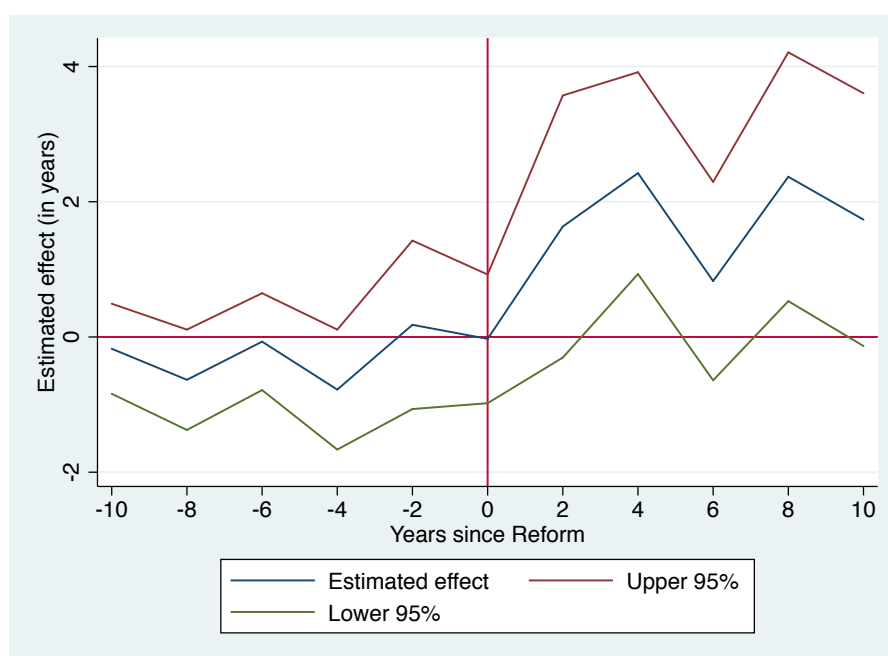
Source: Author's calculations from DHS data.

Figure 1.4: Average Ages of Marriage



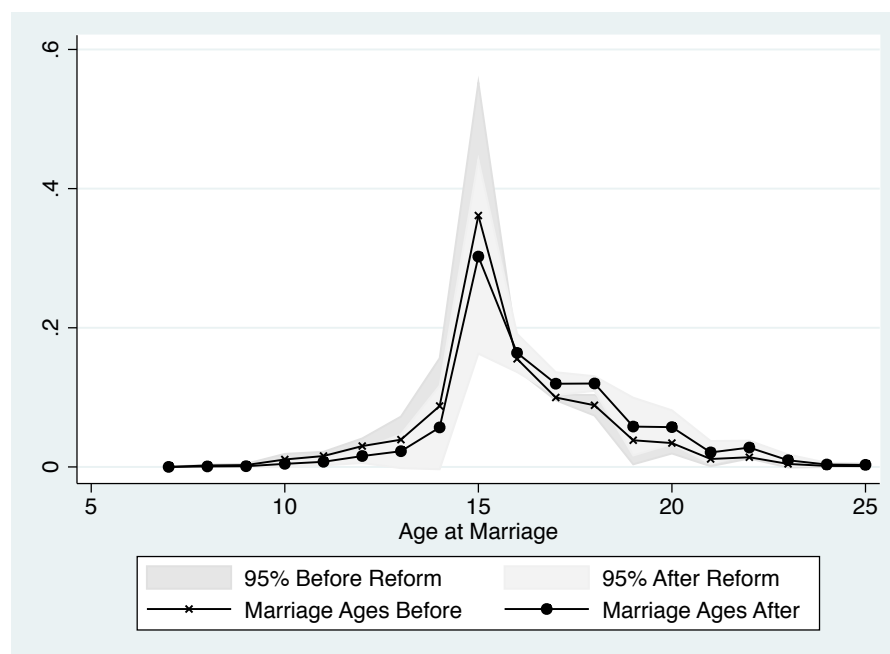
These averages are calculated from the raw data as the average age of marriages occurring in each year. Source: Author's calculations from DHS data.

Figure 1.5: Estimated Dynamic Effect of Reform



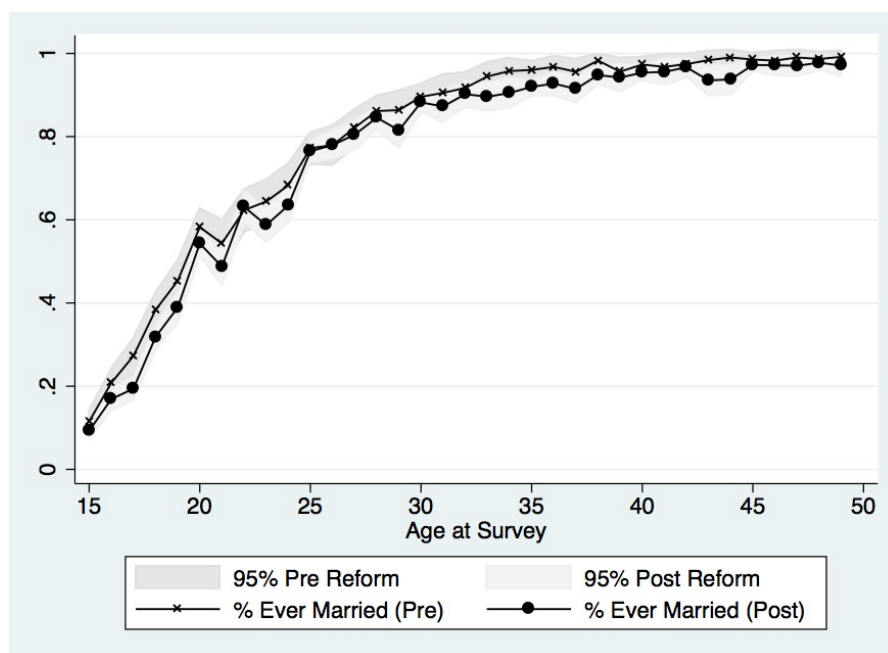
The figure shows estimated coefficients on leads and lags of the reform to show the dynamic effect of the reform and that the reform was both unanticipated and there were no trends prior to the reform that might be biasing the standard difference in differences coefficient. The estimated coefficients and standard errors are reported in Table 1.3. Source: Author's calculations from DHS data.

Figure 1.6: 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.

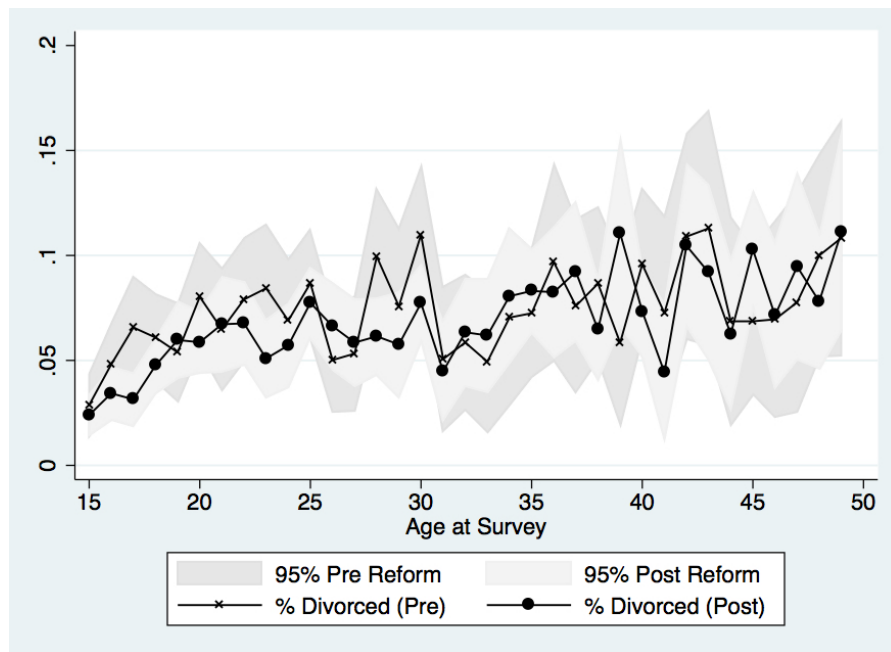
Figure 1.7: Proportion of Women Married by Age at Survey Date



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.

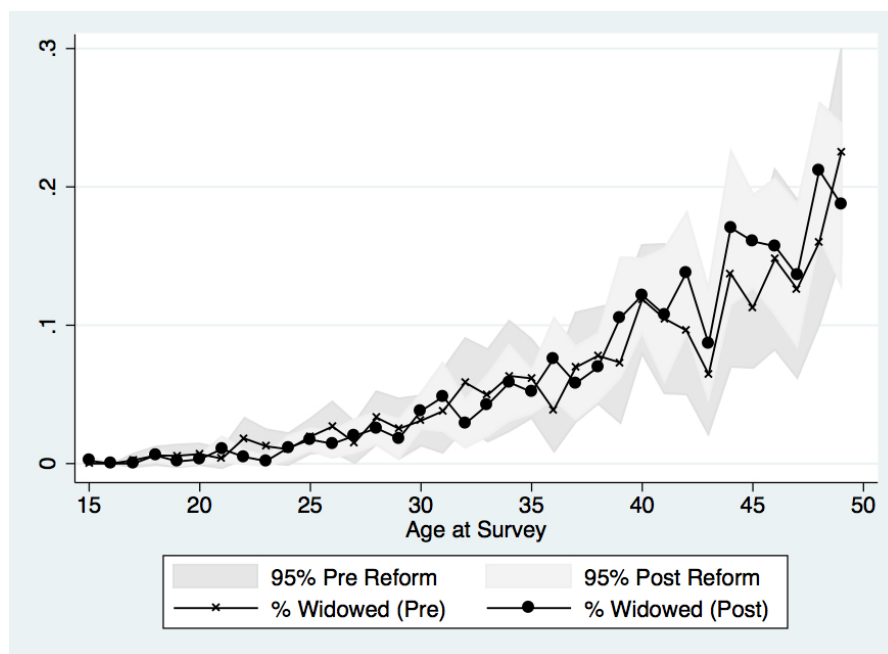
Source: Author’s calculations from DHS data.

Figure 1.8: 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 1.9:
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 1.1: Summary Statistics

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

Table 1.2: 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 1.3:
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 1.4: 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 1.2. 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 1.5: 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 1.2. 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 1.6: 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 1.2. 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 1.7: 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 1.2. 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 1.8: 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 1.2. 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 1.9:
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 1.10:
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 1.9. 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 1.11:
Bargaining Power and Domestic Violence Outcomes - Hetero-
geneous Results by Religion

VARIABLES	(1) Knows Ovulatory Cycle	(2) Husband Exclusively Controls His Earnings	(3) Beating Justified if 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 1.9. 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 1.8. 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 1.12: 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 1.2. 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.

CHAPTER 2

THE SHORT END OF THE DEAL: CRISIS, DROPOUT, AND SIBLING INEQUALITY

“Differences among siblings in socioeconomic outcomes are an important element in the structure of overall inequality.”

-Dahan & Gaviria (2003)

2.1 Introduction

Temporary school interruption is a common experience of many students in the developing world. During household, local, or national crises of various kinds, students may withdraw from school because of affordability concerns or changes to their opportunity costs of time. There is a significant literature on the effect of interruption on outcomes for students, but little attention has been paid to the long-term outcomes for siblings relative to one another when dropout may occur only for one sibling. This paper investigates these long-term outcomes using the panel Indonesian Family Life Survey (IFLS). I identify differential investments made during the 1997 financial crisis in children from the same family, which created inequality between siblings that would not otherwise have existed, and track siblings into adulthood seven years later.

Thomas et al. (2004) and Thomas & Frankenberg (2007) find that the effects of the 1998 Indonesian financial crisis were felt strongly by households as it reduced purchasing power by as much as 40% in the short run. They show that households responded to the crisis in the short run by drawing down their assets, by increasing female labor supply (particularly in self-employment and

family-run businesses, and by reducing household investments in education, reducing expenditures on education for some children (the intensive margin) and even withdrawing some children from school (the extensive margin). I draw on this work and show that educational attainment among some children was lower relative to that of their siblings as a result of experience of the crisis. The context of the Indonesian financial crisis and the Indonesian Family Life Survey (IFLS), a large panel dataset which follows individuals after they leave households, provides the best existing framework in which to study the question of sibling inequality in the long run: the panel allows me to track siblings even after they leave their parents' home. In my individual-level empirical analysis, I find evidence, consistent with Son (2013), that children who were scheduled to complete their final year of a schooling level (whether primary, junior high, or secondary school) during the 1998 crisis completed **more** schooling in the short run (as measured 3 years later in 2000) relative to children who were not in a final year of schooling. I expand on that evidence to show that such a decision rule *increases* short-term inequality between siblings in educational attainment. These differences among siblings persist even when controlling for age, child birth order, and gender. In 2007, ten years after the crisis, the *siblings* of those children whose schooling was preserved in 1998 to finish the final, critical year of a schooling level have *caught up* to them but are also *more likely to still be in school*.

I proceed by describing the context of Indonesia's educational system and the specific context of the 1998 Indonesian Financial Crisis; I then establish the context of this paper in the existing economics literature on unequal investments in siblings. In Section 2.3 I develop a theoretical model of differential education investments in the face of budget shocks and non-convexities in the returns

to education. Section 2.4 introduces the data I use and lays out my empirical strategy to econometrically identify the impact of the experience of the financial crisis on intra-sibling inequality in educational attainment. I then present my empirical results in Section 2.5 and conclude.

2.2 Economic and Institutional Context

2.2.1 Education in Indonesia

Education in Indonesia consists of three levels before age 18: primary (grades 1 to 6), junior high (grades 7 to 9), and secondary (grades 10 to 12), and students in their final year of secondary school are typically 17 or 18 years old. The Indonesian government made nine years of school compulsory in 1994, although enrollment information from household surveys (including those used in this paper) reveals that the compulsory schooling levels were neither universally enforced nor universally achieved in the mid-2000s.¹ The school year begins in July, with a semester break in December, and the second semester spans January to June. For the purpose of this paper, then, the Indonesian financial crisis struck in the middle of the 1997/8 school year.

School fees and other costs in Indonesia can be a large portion of household expenditures, especially for urban primary students, even in publicly funded schools. Additionally, significant social pressure exists to conform to standards of dress for uniforms, such that being able to afford school fees may still be in-

¹The government just made twelve years of schooling compulsory as of 2013. See <http://www.thejakartapost.com/news/2013/06/26/ri-kicks-12-year-compulsory-education-program.html>.

sufficient motivation for children to stay in school at all types of schools. Transportation costs (and a bump in them associated with the move to junior school or to secondary school from the previous level) may provide further deterrence for school attendance and in particular may pose additional costs to transferring to the next level; indeed, more than 80 percent of schools (of all levels) are in buildings on their own.

Students receive certificates of completion upon finishing the final year of each schooling level after taking school-administered examinations. At the end of grades nine (the end of junior high school) and twelve (the end of secondary school), the test results are indicated on the certificates of completion. Certificates are issued by individual schools and additional certificates are issued for individuals who pass the national examinations (based on a minimum score). Advancement to the next level of education requires passing these examinations and they are thus very important landmarks of accreditation.

Despite relatively high enrollment and completion rates relative to those of other developing nations, the quality of schooling in Indonesia is widely regarded as quite low, especially for primary school; UNICEF reported that only 27 percent of teachers were qualified in 2012. Teacher salaries are frequently reported to be insufficient to cover the needs of teachers and their families.² Corruption is also commonly cited as a significant problem in Indonesian schooling.

²From IFLS community survey information.

2.2.2 The 1998 Financial Crisis in Indonesia

The Indonesian financial crisis began in January 1998 with a sudden and unanticipated depreciation in the Rupiah following six months of strain on the financial sector.³ The macroeconomic effect of this was to reduce output by about 12% in 1998 relative to its 1997 level; it remained there well into 1999. The Rupiah remained unstable and depreciated well into the 2000s. Thomas et al. (2004) note that the collapse of the currency caused prices to rise, such that the CPI increased by about 80%; food prices and particularly rice prices rose by significantly more than that (20% and 50% more, respectively). The result was to temporarily but drastically affect the purchasing power of households that purchase food. In particular, poor non-rice producers (or net consumers) particularly in urban areas saw their real incomes plummet.⁴ On the other hand, as has been noted by the literature investigating the short term effects of the crisis, rural rice producing households were relatively sheltered due to the appreciation of rice prices and some even benefited from them. As I move forward with the analysis, then, I focus on non-agricultural households for whom I can be sure that the inflationary effects of the crisis were detrimental since in the data I have available I do not have information that would indicate the extent to which agricultural households might have been sheltered or even benefited.⁵

³The IMF, the World Bank, and then president of Indonesia Suharto were all surprised by the depth, magnitude, and duration of the crisis, particularly given popular opinion at the time that Indonesia's story was one of miracle.

⁴See Levinsohn et al. (2003).

⁵This also has the benefit of avoiding several other issues that might have proved confounding. First, there was significant drought in some areas of Indonesia during the year following the onset of the crisis, and I would have been unable to identify and separate the effects of this from those of the crisis; by excluding agricultural households, the effects of the drought will be small if they are even nonzero at all. Second, a public safety net program was implemented in some rural areas to provide partial scholarships to students who were deemed at risk of dropout from school; by excluding rural areas I exclude this obviously confounding factor. I check to be sure that restricting my analysis to urban households does not affect the results.

Thomas et al. (2004) note that the crisis, while unfortunate for those involved, provides an uncommon opportunity to identify the effects of an aggregate shock in a developing context unrelated to agricultural productivity.⁶ Households which could not completely smooth consumption by drawing down assets were likely to temporarily adjust their budgets. Browning & Crossley (2001) predict that households reduce spending on durables and other investment goods if a crisis is expected to be short lived, since utility from them derives from the flow of services provided by such goods and is unlikely to suffer greatly from temporary underinvestment in the durables themselves. Thomas et al. (2004) find evidence to support this in the context of Indonesia's financial crisis and suggest that human capital investments in young children on both the intensive (spending on school fees, uniforms, and books) and the extensive (enrollment and attendance) margins qualify as one such example. Temporarily resource-constrained households may have assumed that such short term adjustments to their budgets were unlikely to negatively impact children's lifetime human capital accumulation.

Using a special 25 percent subsample of the IFLS panel, they find that per capita education expenditures decreased substantially in the poorest households and that spending on the youngest children's education suffered the most.⁷ They use the household and national school enrollment data to support this finding, confirming that these poor households sought to protect the schooling of their older children⁸, and that declines in education spending were

⁶An extensive literature in development economics exists documenting the effects of income shocks, particularly those attributable to idiosyncratic changes in weather, on the livelihoods and choices of households.

⁷The authors use the IFLS2+, which was not publicly available at the time of this writing. My own efforts to gain access to these data were unsuccessful.

⁸Thomas et al. (2004) suggest that these decisions were made in order to protect the schooling of the children whose returns would be the greatest.

greatest among urban households and those that were initially poorest before the crisis.

Son (2013) uses the 1997 and 2000 IFLS data to investigate whether schooling investments can be described by a perceived “sheepskin” effect on the part of parents; she finds that in the short-term experience of shocks related to the financial crisis as well as those more idiosyncratic income or unemployment shocks, families’ short term enrollment decisions are consistent with preservation of schooling for those children slated to finish the final critical year of a schooling level. She does not, however, explore the inequality between siblings created by such decisions nor the long-run outcomes between them.

2.2.3 Literature: Parental resources and investments in children

Parental resources are widely regarded by the economics literature as being a key determinant of children’s human capital, the returns to schooling, and children’s adult earnings (Haveman & Wolfe, 1995; Acemoglu & Pischke, 2001; Cameron & Heckman, 2001; Altonji & Dunn, 1996; Behrman & Taubman, 1990; Peters, 1992; Solon, 1992; Rosenzweig & Wolpin, 1994). Evidence from the development economics literature in particular suggests that adverse shocks to household incomes may affect the nature and level of these investments. Whether adverse shocks to parents’ incomes increase or decrease schooling depends on the relationship between the income and substitution effects. This theoretical prediction of ambiguity is described by Basu & Van (1998) and others and has been empirically tested particularly in the context of the literature on child labor: the substitution effect operates by reducing the opportunity cost

of school when the child wage rate falls during aggregate shocks, thereby serving to increase schooling hours, whereas the income effect increases the relative importance (the marginal utility) of the child's contributions to earnings, which would decrease schooling hours and increase labor hours.⁹

Whether shocks increase or decrease schooling, then, is an empirical question. Ferreira & Schady (2009) review the existing empirical literature in both developing and developed country contexts and find that macroeconomic shocks which reduce incomes tend to decrease schooling outcomes in Asia but that the opposite is true for the US, the UK, and some Latin American countries. They propose that borrowing constraints and initial wealth levels in part determine these differences.

While the above evidence points to the role of parents in determining the human capital and earnings outcomes of their children, it does not explain the empirical existence of differences among adults who were children in the same household. In fact, previous work regarding long-term sibling differences in outcomes is sparse, in part due to the demands of these questions placed on the data. In the classic child quantity-quality tradeoff posed by Becker (1960), the scarcity of family resources predicts that the investments made in each child are decreasing in the number of children. However, a number of recent empirical studies show that parental resources are not evenly divided, and that the division may be both controllable and intentional on the part of the parents. Therefore, economy wide inequality in educational attainment and wages may

⁹Of course, there is not a one to one tradeoff between time spent in school and time spent working. On the one hand, work and school are not mutually exclusive: children can attend school and work during non-school hours as well. There is evidence also of the transition of children to and from "idleness" although the magnitudes are perhaps tainted by measurement error in the failure of documented "work" to reflect participation in income generating activities in household owned enterprises or in valuable but not income generating household production.

derive not only from the household of origin but also the status of treatment or investment within that household. Francesconi et al. (2015) point to evidence from the United States that inequality in bequests is increasing, and is in many families related to time spent with parents.

Children from the same parents may have different outcomes related to their genetics (which are likely random), from differences related to the order of their birth, or from differential investments on the part of parents, which may also prove to be related to birth order. Genetic differences are outside the scope of the current project, but the initial endowment level of a child in cognitive abilities, in health, and in behavior has been shown to be related to the birth order of the child in the psychology literature and, more recently, in the economics literature. For example, women who give birth too young or too old are more likely to have children with birth defects or of low birthweight, and thus birth order may influence children's outcomes as a mother ages.¹⁰ For child rearing reasons unrelated to investments made intentionally, there may be an advantage to being the first born (more alone time with parents) or the last born (parents have gained expertise in parenting).

Investments on the part of parents, then, are likely not only to be influenced by these initial endowments (a combination of genetics and of non-investment birth order effects) but also to depend on the preferences of parents and their objectives in the investment. There may be various and conflicting reasons that parents invest unequally in their children. On the one hand, parents may prefer equality in outcomes across their children for altruistic reasons, in which case they may invest more heavily in children with lower initial endowments. On the other hand, if parents view their children's outcomes as insurance for their own

¹⁰See Rosenzweig & Wolpin (1991) for a list.

retirement, they may invest more heavily in the children they think are more likely to succeed; in this case, they may simply choose one child to concentrate their investments, or they may - for cultural and legal reasons - choose the eldest child or the eldest son. Of course, the solution to a standard model of utility maximization on the part of households with preferences for maximizing their expected payoff from their investments in children's education would equate the present day marginal cost of schooling with the present marginal value of expected payoffs from that investment. Social or cultural reasons may dictate that the eldest child is the one whose expected payoff is highest.

These hypothetical reasons are supported by the empirical economics literature. In a seminal work, Behrman and Taubman (1986) show that, in a small sample of US young adults, individuals who are the eldest or near the eldest sibling have higher educational attainment and may have higher wages. They conclude the preference for first borns or children of lower birth order occurs either because these children have higher cognitive abilities related to their birth order or because parents are better able to sense a first born's probability of success relative to the more uncertain success of a (younger) higher order child.¹¹ Similarly, Horton (1988) uses sibling data from the Philippines to show that birth order has significant effects on both the short- and long-run nutritional status of siblings. Black et al. (2005) find negative family size and birth order effects on educational attainment and wages in a large sample from Norway, and find that the family size effects disappear when birth order is included as a control. Together this evidence suggests that elder children in all families have better educational attainment and wages than higher birth order children in larger families, and these studies provide some evidence against the classic

¹¹"Higher order" in this literature refers to the children who are born later, that is, child number 2, 3, 4, etc.

child quantity-quality tradeoff posited by Becker (1960). Child birth order, and not family size in general, appears to determine the outcomes of a particular child in these studies.

On the other hand, Quisumbing et al. (2003) note that households may exhibit behavior consistent with preferences for equality among their children. In particular, they posit that inequality averse parents may increase educational investment in their lower ability children in order to equalize them with their higher ability siblings. Relevant to the present topic, Quisumbing et al. (2003) also show that households in the Philippines invest more heavily in their daughters' education since they know that the eldest sons will receive inheritance in the form of land. This suggests that households which favor equality of their children may view different types of investments as interchangeable, particularly in the face of institutional or credit constraints which prohibit or restrict their investments.

While these studies do show that parents' preferences and their children's initial endowments explain differences in investment on the parts of parents, they do not address the dynamic nature of the investment decision, an issue which is important in considering the effect of a shock on differences in parental investments in education. Yamauchi (2008) uses panel data from South Africa to examine the dynamic complementarities of investments in human capital and health and their effects on inequality of educational attainment in later life. He notes that investments in early childhood affect the optimal level of investments in later childhood. For example, early nutritional or health investments in young children will affect the optimal level of schooling for high school aged children. Thai & Falaris (2014) find evidence of this behavior in Vietnam, where

rainfall shocks during early childhood (age three) are shown to negatively affect children's school entry, progression, and completion later in life, suggesting that perceived returns to schooling, and therefore its attainment, depend on the building up of human capital early in life. In this way, inequalities that present themselves early in a child's life may affect the equity with which they are treated in later childhood.

Dahan & Gaviria (2003) present a simple model of intrahousehold resource allocation to show that parents may generate inequalities among siblings over time in the presence of non convexities in human capital returns and borrowing constraints such that parents cannot borrow to finance all of their children's education.¹² Using data from three Latin American countries, the paper finds behavior consistent with the predictions of the model.

This prediction is the most relevant to the current question of this proposal. In the context of shocks, households may be prompted to remove their children from school temporarily; the question of which child the household keeps in school is likely to depend on the interplay of preferences and objectives described above as well as the relative contributions each child could make to household production or earnings if removed from school. One possible outcome – which would appear consistent with the empirical evidence from Indonesia that younger children were withdrawn while older children remained in school– is that parents might choose to withdraw the younger child, who would have a longer period in which to recover from the shock itself. In this explanation, borrowing constrained parents would choose their younger children to withdraw temporarily from school, hoping that the duration of the shock

¹²In a separate paper, Gaviria (2002) develops an extension to the Becker-Tomes model of parental investment and uses the PSID to support the prediction that, contrary to the original model, sibling earnings inequality later in life is the same for both rich and poor families.

would be short and the child could catch up later. This would be consistent with altruistic behavior on the part of borrowing or resource constrained households. On the other hand, even non-altruistic utility maximizing parents could be predicted to make the same choice: the expected marginal return to an additional year of school for a child who has completed all but a year of secondary school is likely much larger than that of a child who has completed only some of primary school. Provided that the parents are sufficiently forward looking that they internalize this expected future payoff even as they consider the current opportunity cost of the child's time (an older child is likely to yield higher wages from working during the shock than a younger child), they will likely keep the older child in school at the expense of the younger children. In fact, Son (2013) finds evidence from Indonesia in particular that households invest according to these perceived "sheepskin" effects.¹³

Sadly, evidence on schooling interruption in the US and in the context of Indonesia's financial shock suggests that children do not catch up later and that even if they do, such that their lifetime educational attainment is equal to that of their peers, their wages suffer from these interruptions (Light, 1995; Mejía-mantilla, 2012). The question I pose in this study is more nuanced than whether children are adversely affected by a shock in the long-term. I seek to delve into the within-household school removal behaviors to identify whether among siblings the shock generated inequality that would not have otherwise existed and, if so, what the educational differences are between them nine years later.

In the next section, before turning to empirical estimation of inequality created between siblings during the financial crisis, I develop a model of unequal

¹³Sheepskin effects are defined in the literature as additional returns to particular years of education beyond the marginal effects per year in a linear Mincer equation, owing their name to the fact that diplomas used to be written on sheepskin. See Card (1999).

school investment in the presence of irregularities in the returns to schooling that generate perceived sheepskin effects.

2.3 A Model of Unequal Schooling Investments

Consider a household consisting of a parent (or two parents acting as a unit) and two children.¹⁴ A standard model of human capital investments (such as Becker & Tomes (1976)) would suggest that self-serving parents invest in children's schooling to equalize the marginal return for a particular child and the marginal costs of schooling for that child (which can depend not only on direct schooling costs but also learning ability, heterogeneous opportunity costs, etc.).

If the simple investment model is extended to allow for transfers (either inter-vivos or bequested) made on the part of parents as well as direct human capital investments, parents view financial transfers and direct schooling expenditures as substitutes over the lifetime of a child. Following Behrman et al. (1982), the parent maximizes a two-period household welfare function that takes as arguments lifetime per capita consumption of food and other expenses for the household in each period (C_1 and C_2),

schooling of the children during the first period of life (S_1 and S_2), and transfers made to the children in the second period of their life (T_1 and T_2):

$$\max_{C, S_1, S_2, T_1, T_2} W(C, u(S_1, T_1), u_2(S_2, T_2)) \quad (2.1)$$

¹⁴The assumption that two parents act as a single unit implies a unitary or collective (and not a competitive bargaining) model of the household, which has been rejected elsewhere in the development literature in a number of contexts. However, Lafave & Thomas (2014) provide a direct test in the Indonesian context (in fact, also using the IFLS data) and they fail to reject that members of households in the Indonesian context operate cooperatively to attain the Pareto efficient level of resources.

The parent has budget constraints for each period as follows:

$$p_1 C_1 + d(S_1 + S_2) = Y \quad (2.2)$$

$$p_2 C_2 + T_1 + T_2 = Y \quad (2.3)$$

where schooling costs are equal across children (d does not vary by child), transfers can be made without transaction costs, per-period income is predetermined, and per capita consumption in period t has a price p_t .

Suppose for simplicity that $C_1 = C_2 = \bar{C}$ which is the minimum subsistence level of consumption.¹⁵ This simplifies the parent's problem to one of choosing schooling and transfer amounts for each of two children. If we assume that parents care about the utility of their children, this is altruism in the Beckerian sense, in which the (altruistic) parent's utility function incorporates the utility of her child. I assign each of the children utilities $U_i(S_i, T_i) = V(y_i(s_i) + T_i)$ so that the children have utility from their adult income which is the sum of earnings (with a child-specific earnings function relating schooling levels to earnings) and transfers from the parent. The parent solves for the optimal level of schooling for each child by setting:

$$\frac{\partial W}{\partial V_1} \frac{\partial y_1}{\partial S_1} = \frac{\partial W}{\partial V_2} \frac{\partial y_2}{\partial S_2} \quad (2.4)$$

If the parent has equal concern for the children (so that the children's utility weights in the parent's welfare function are equivalent), then this simplifies to

$$\frac{\partial y_1}{\partial S_1} = \frac{\partial y_2}{\partial S_2} \quad (2.5)$$

and suggests the familiar result that parents invest in children to equalize the marginal returns of the investment.¹⁶ For example, if child 1 has earnings function $y_1 = \ln(S_1)$ and child 2 has earnings function $y_2 = 2\ln(S_2)$ (so that she turns

¹⁵In any case, if the parent's welfare function is separable in C and in child investments, this is an innocuous simplifying assumption.

¹⁶I note here, as others have, that this occurs even if the parent is operating out of altruism.

each year of schooling into twice as much as child 1 does), then the solution suggests that the family should provide twice as much schooling for child 2 as for child 1.

Continuing to the second period of the children's lives, when they are grown they receive transfers from their parents to equalize their marginal utilities of their earnings (since their weights in the parent's welfare functions are the same) which means that $T_1 > T_2$ since child 2's earnings from schooling are so much larger (because their investment in her schooling was larger and her earnings function has a higher return).

2.3.1 Irregular Returns to School - "Sheepskin" Effect

Suppose that the earnings functions of children are not continuous. In particular, suppose that there are large and significant returns to finish a particular year of schooling (perhaps the very end of secondary school or the end of junior high school).¹⁷ Suppose in this case that each child's earnings function is as follows (see Figure 2.1):

$$y(S) = \begin{cases} f(S) & \text{if } S < \bar{S} \\ g(S) + R + f(\bar{S}) & \text{if } S \geq \bar{S} \end{cases} \quad (2.6)$$

where $f'(S) > 0$, $g'(S) > 0$, $f''(0) < 0$, and $g''(0) < 0$. Such a returns function can be graphically described as in the figure.

¹⁷Here I draw on the results of Son (2013) who found that human capital investments in Indonesia are consistent with a "sheepskin effect."

A more simple version (a special case) of the above is:

$$y(S) = \begin{cases} \ln(S) & \text{if } S < \bar{S} \\ \ln(S) + B & \text{if } S \geq \bar{S} \end{cases} \quad (2.7)$$

where $B > 0$ is a positive shifter (the return to finishing the school year \bar{S}).

What are the implications of this return structure for parental investments in education? The non-convexity of the earnings function suggests that we need to consider several cases.

Case 1. Recall that if the household consumes the minimum subsistence consumption level \bar{C} then the household's utility maximization problem is:¹⁸

$$\max_{S_1, S_2, T_1, T_2} W(u(S_1, T_1), u_2(S_2, T_2)) = W(V(\ln(S_1) + \mathbb{1}(S_1 > \bar{S})B + T_1), V(\ln(S_2) + \mathbb{1}(S_2 > \bar{S})B + T_2)) \quad (2.8)$$

$$d(S_1 + S_2) = Y - p_1 \bar{C} \quad (2.9)$$

$$T_1 + T_2 = Y - p_2 \bar{C} \quad (2.10)$$

Suppose that the family can afford to reach \bar{S} for both children; that is, $2d\bar{S} < Y - p_1 \bar{C}$. In this case, it can be shown that if parents have equal concern for their children, both children will achieve at least \bar{S} and any additional possible schooling for the children will be divided equally among them.

Case 2. The more interesting and relevant case for the example of Indonesian families experiencing a shock to their incomes occurs when parents *cannot* achieve \bar{S} for both children. In this case, the parents choose the schooling choice combination that maximizes utility.

¹⁸Or if the household's utility over consumption is additively separable from its utility over its children's consumptions, this result still holds.

It can be shown that in the event that a family must decide whether to allocate school years among children in this case, the child who is closest to finishing \bar{S} will finish that year and remaining funds for schooling will be divided according to the traditional marginal benefit rule. Depending on how much is left over after finishing \bar{S} for that child, the gap in schooling achieved by children may widen if additional funds are not made available for schooling investments.

2.4 Empirical Strategy

2.4.1 The Indonesian Family Life Survey

The Indonesian Family Life Survey (IFLS) is a joint project of the RAND corporation and the Indonesian government to document the socioeconomic circumstances of Indonesia's people at the community, household, and individual levels. Designed as a multi-round panel survey, the project began in 1993 in half of Indonesia's 27 provinces and was designed to be representative of 83% of country in 1993.¹⁹ The original sample size consisted of 7,224 households for a total of more than 30,000 individuals. Followup surveys were collected in 1997, 2000, and 2007 in which all original survey households as well as their offshoots were tracked and interviewed when possible.²⁰ Extensive cost and effort were made to ensure that attrition was as low as possible in order to maximize the usefulness of the survey in analyzing long term outcomes and socioeconomic

¹⁹The sampling frame was designed to minimize cost and maximize representativeness: the 13 provinces were selected and then enumeration areas (EAs) were randomly selected among them.

²⁰An offshoot household is established whenever a survey respondent (household member) of the original survey household moves permanently away.

patterns. The project boasts an impressively high reinterview rate of 94.5% between 1993 and 1997.²¹

In addition to the advantages of using panel data for controlling for time invariant unobservables within households or individuals, the timing of the 1997 survey collection (IFLS2) being completed immediately before the financial crisis of 1998 is particularly convenient because pre-crisis conditions can be controlled for with relatively little measurement error. More than 95% of the IFLS2 interviews were completed before December 1997, one month before the inflationary crisis hit Indonesia.²²

The panel nature of this dataset combined with the large and unanticipated shocks to households experienced during the Indonesian financial crisis make the Indonesian setting and the IFLS data in particular the best existing scenario in which to study creation of sibling inequality in the short and longer run. As I move forward with the analysis, I restrict my sample in the following ways: 1) Non-agricultural households: As described above, previous work has recorded potential gains from the inflationary crisis by rural households who produced food (for which prices rose dramatically during the crisis) and showed that urban households were hardest hit by the crisis. Unfortunately, the IFLS does not have information in the 1997 data (such as land area planted) which would allow me to measure the extent to which agricultural households were insulated or even benefited. Therefore, I restrict my sample to children living in non-agricultural households. 2) School-age children with siblings: I include all

²¹It should be noted, of course, that characteristics of households and individuals lost to attrition are likely to differ systematically from those that are retained in fundamentally unobservable ways which might be correlated with the outcomes of interest. However, the tracking methodology of the IFLS significantly reduces this attrition bias and provides a significant improvement relative to most other surveys in the developing world which do not track, locate, and interview households or their members upon moving.

²²Frankenberg and Thomas 2001.

children who were under 18 or still enrolled in high school in 1997 and were living with at least one other biological sibling in 1997. and 3) Continuity of sample: I restrict the sample to those children for whom survey responses were available in 1997, 2000, and 2007 for them as well as all the children with whom they resided in 1997.²³

The sample I construct in this way has 2,925 children, who were living in 1,482 unique non-agricultural households.

2.4.2 Identifying the Impact of the Crisis

Educational outcomes for children who were finishing a schooling level

I begin my empirical work in this paper by estimating the extent to which children who were in their final year of a schooling level were affected differentially by the crisis relative to those who were not. That is, setting aside the question of intra-household or sibling effects, I look for evidence along the lines of Son (2013) that households made school investment decisions in the short term during the crisis that were consistent with a “sheepskin” effect for those children who were close to finishing.²⁴ To do this, I first identify children who were in their final year of a schooling level at the time of the crisis. Since the crisis struck in January of 1998, I classify all children who, if they continued their schooling as normal during the crisis, would be in their final year of a schooling

²³I do this in order to avoid the selection bias that might occur from some households having some children available for survey or tracked effectively and others not. Of course, these households may also be selected in non-random ways.

²⁴A so-called “sheepskin” effect refers to additional benefits attributable to completion of a particular year of schooling associated with a diploma. The labor economics literature on human capital is divided as to whether the final capstone year is important in itself for the skills and topics mastered or whether the diplomas matter only as a signal.

level in the school year that started in July 1998, in order to capture differential enrollment. These children were enrolled in their fifth, eighth, or eleventh year of school by the 1997 survey and would continue on to their final year of primary, junior high, or high school (sixth, ninth, or twelfth year of school), respectively, in the middle of the crisis. I call these children “Final” children. I explore the possibility that these children had different school advancement in the short run aftermath of the crisis relative to those children who were not in such a final year (measured when I see them the next time, in 2000) and, possibly, different total years of education attained in the long run (when I observe them again, nine years later in 2007).²⁵

Specifically, I estimate the following:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + X'_{ihp} \gamma + \eta_p + \epsilon_{ihp} \quad (2.11)$$

where Final_{ihp} is an indicator for whether child i in household h in province p is in the year just prior to the final year of a schooling level in 1997 (meaning that 7 months after the crisis struck she enters the final year of that schooling level if she continues in school and does not withdraw), and X_{ihp} is a set of individual and household level controls: child’s age, gender, birth order, and household level controls (educational attainment of the household head, household size, number of siblings, and log per capita expenditures). I also include province fixed effects (η_p) to capture any province-specific time-invariant differences in average educational enrollment or other factors.²⁶ Thus, a positive and statis-

²⁵This approach assumes that any enrollment effects attributable to the crisis were most salient when it came time to re-enroll in school (or advance to the next grade and/or schooling level) as opposed to during the middle of the school year.

²⁶Sample weights are used in all analyses and standard errors are clustered at the *Kecamatan* (district) level. There are 35 districts in the sample. Results are forthcoming for using a bootstrap method to correct for the small number of clusters, but results are not substantively different if the standard errors are clustered at the community (enumeration area) level, of which there are 292.

tically significant estimate of β_1 would indicate that children who were in their final year of a schooling level at the time of the crisis had higher educational outcomes relative to children who were not. I explore the effects on the following educational outcomes: grade progression between 1997 and 2000 (the next time I see them in the data), grades completed between 2000 and 2007, junior high school graduation in 2007, high school graduation in 2007, and total years of education in 2007 (when most children are older than 18).

This child-specific identification strategy relies on the assumption that children in their final year of a schooling level *at the time of the crisis* and children who were not in such a year are not systematically different from one another but that the timing of the crisis was such that one child experienced a different “treatment” from others as a result of the crisis; that is, that the timing and experience of the shock was as good as random vis à vis particular children. Of particular concern with this approach is that being in the final year of a schooling level might not only reflect that a child is of a particular age but that a child is also of a particular ability level. That is, if children of different abilities do not all start primary school at the same age or they do not progress through school at the same rate (i.e. do not skip or repeat grades), the “Final” status as described above would confound the effect of being in a final year of a schooling level with the reasons for a particular child’s schooling progress. Table 2.1 shows the ages of children who we would normally expect to be in certain grade levels during the crisis conditional on their starting primary school at age 6 (when most Indonesian children start) and progressing normally through school up to the time of the crisis.

However, some children in the sample are not in their age-appropriate grade

in school: some of them are ahead and some of them are behind. I therefore identify children who are ahead (have completed more grades than normal given their age) and those who are behind (have completed fewer grades given their age). I define the dummy variables “Behind” and “Ahead” and include them in the estimation:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + \beta_2 \text{Behind}_{ihp} + \beta_3 \text{Ahead}_{ihp} + \mathbf{X}'_{ihp} \gamma + \eta_p + \epsilon_{ihp} \quad (2.12)$$

Figure 2.3 graphically shows results from this estimation. Consistent with the findings of Son (2013), children who were in their final year of a schooling level complete about 0.9 additional years of schooling beyond those who were not “Final” children between 1997 and 2000. They are thus more educated in 2000 (column 2), and since they complete no more or fewer grades between 2000 and 2007 (column 3), they are more educated by the time I see them as adults, in 2007 (column 4). These effects are statistically significant and suggest that during the crisis “Final” children were given priority in households’ human capital investment decisions. Since I have controlled for whether a child is “Final” because of age or because of differential school starting/progress prior to 1997, these can be seen as the causal effect of the crisis on these children relative to their peers given the unanticipated and large effects to household purchasing power of the financial crisis. In results not shown here, these children are more likely than their peers to have finished junior high and high school by 2007, when most individuals are over 18, but are no more or less likely to have completed some college or to be college graduates. Figures 4 and 5 suggest that this decision rule – to preserve the schooling of “Final” children in the short term

aftermath of the crisis – is more salient than birth order (oldest or younger) in determining longer term educational outcomes among children.

Sibling Inequality Caused by the Crisis

In the previous section, I established that children who were in the final year of a level were given preferential treatment and advanced through school more than their peers during the crisis, resulting in higher levels of education in 2007 when they are adults. While, given the interest of this paper, it may seem natural to wonder what happened to siblings of those children - was their schooling reduced relative to their siblings? - very few papers have ever raised the question, nor have they been able to address it given the limitations of available data. I move forward by exploring this question; specifically, I classify the children who were living with children in their final year of a schooling level at the time of the crisis as “Siblings of Final” children.

A first attempt to address this question would estimate the following:

$$\text{Educ Outcome}_{ihp} = \beta_0 + \beta_1 \text{Final}_{ihp} + \beta_2 \text{Sibling of Final}_{ihp} + X'_{ihp} \gamma + \eta_p + \epsilon_{ihp} \quad (2.13)$$

where the controls, fixed effects, and treatments of standard errors are as described above in the previous section. A positive effect on “Final” children relative to their siblings would be reflected by a positive and significant β_1 . Given that I have shown “control” children to have been disadvantaged by the crisis, such a coefficient implies that “Sibling of Final” children as well as “control” children are disadvantaged, whereas “Final” children are sheltered by their status at the time the crisis struck.

As above, this child-specific identification strategy relies on the assumption that children in their final year of a schooling level *at the time of the crisis* are

not systematically different from other children or *from their siblings* but that the timing of the crisis is such that one child experiences a different “treatment” from the other as a result of the crisis. That is, that the timing and experience of the shock was as good as random vis á vis particular children relative to their siblings as well as to children living in other households. In addition, the strategy requires that those households in which a “Final” child resides are not systematically different from those in which no such child resides in terms of their educational plans for their children and the extent to which they are inclined toward sharing via transfers. If parents were to invest in their best child up to the point where that child completed a level of schooling and only then do they invest in the second best child, this empirical strategy would be violated; being a “final” child at the time of the crisis would reflect not only the exogenous timing of the crisis but also the previous preferential and intentional investment decisions on the part of parents.

In order to address this concern, I again include indicators for whether children are ahead or behind relative to where they would be in school given their age and an assumed primary school starting age of 6. Results on short- and long-term schooling differences are presented in the next section.

2.5 Empirical Results

2.5.1 First Stage Results: Estimates of the Effect of Crisis on Education of Siblings

In what follows, I estimate Equation 19 for various outcomes related to educational attainment; that is, I use multivariate regressions and control for various individual and household characteristics to identify the effect of the crisis and its timing on own- and sibling- education levels and adult educational achievement. As described before, children are separated into three categories: 1) children who, if continuing school during the crisis, would have been enrolled in a grade that completed a schooling level, whether primary, junior high, or secondary school (these siblings carry an indicator “Final” in what follows); 2) children who had a sibling who was in a schooling level completion grade (“Sibling of Final”); and 3) “Control” children who were neither in a completion grade nor had a sibling who was (but, by virtue of the sample I have chosen, had co-resident siblings under 18 in 1997). These “control” children are the omitted reference category in the regression results which follow. For each outcome, I explore the role that controlling for the number of siblings, child birth order, and ability (indicated by whether children were ahead or behind in school grades relative to where they would have been if they had started at age six and progressed normally).

Table 2.3 shows that “Final” children progress through more grades between 1997 (just before the crisis) and 2000 relative to their siblings and relative to the controls. Columns 1 through 3 show that this effect persists even when con-

trolling for the sibship size (number of siblings) and birth order, although birth order also significantly affects grade progression. Higher birth order children (i.e. the second child and younger) progress through more grades in the short-term aftermath of the crisis, but the coefficient on “Final” is twice as large as that on birth order. Recall that child age is controlled for. The positive effect for “Final” children also persists when controlling for whether children were ahead or behind. Notably, children who were behind also progressed more through school during the crisis, while children who were ahead progressed through fewer grades (column 4). However, the effect of being “Final” does not vary among children who were behind, ahead, or on track since interaction terms for ahead and behind status with “Final” are not significant (column 5).

Turning to longer term schooling progression in Table 2.4, “Final” children do not progress through more grades during the second period (2000 to 2007) relative to the controls. However, their siblings *do* progress through more grades, indicating a level of catch up relative to their siblings and suggesting that having a “Final” child allowed families to shift resources to ride the crisis. This effect persists, but decreases in magnitude, when controlling for number of siblings, birth order, and ability (including ahead and behind dummies).

Table 2.5 shows that, as of 2007 (nine years after the crisis), “Final” children are more likely to have completed junior high school relative to control children (by 12 to 18 percentage points).²⁷ Their siblings are also more likely to have completed junior high relative to the controls, but relative to “Final” children, “Siblings of Final” children are less likely to have completed (by nine percentage points in Column 5). However, the difference in their completion rates is not

²⁷Note: marginal effects from a probit regression are shown for tables considering binary outcomes.

statistically significant. This perhaps suggests that the decision rule of keeping children in school to finish a final year and then catching their siblings up after the crisis passed allowed families to ride the wave of the crisis better than those who had no such clear decision to make. Table 2.6 shows a similar outcome for “Final” children and their siblings in the probability of high school completion.

Table 2.7 shows that total educational attainment is higher for final children relative to control children, by about half a year (column 5). Note also that children who were behind in 1997 have lower educational attainment in 2007 and those who were ahead in 1997 have higher educational attainment (column 5). Educational attainment is also higher for “Sibling of Final” children relative to the controls. Without controlling for ability it looks like “Final” children have an advantage in terms of total years attained relative to their siblings by about a third of a year, but controlling for ability reduces this advantage. It also appears that “Final” children who were behind in 1997 have an advantage of about 0.4 years as well.

To summarize, the results presented above (and shown in Tables 3 to 7) on educational attainment suggest that

1. Children in their final year of a schooling level during the crisis were more likely to remain in school during the crisis (between 1997 and 2000) relative to **both** their siblings and to children living in other families where no “Final” child was present (recall that these are the omitted reference category of “control” children).
2. After the crisis subsided, between 2000 and 2007 children who were “Siblings of Final” children were back in school and completed more grades than **both** their “Final” siblings who did not experience the temporary

dropout and the control children.

3. Combined, these 1997 to 2000 and 2000 to 2007 grade progression patterns result in *higher* rates of junior high and high school completion among **both** “Final” children *and* their siblings relative to control children. Average years of education among “Final” children and their siblings are also higher than the control group. The point estimates suggest that “Final” children are better educated relative to their siblings, but the confidence intervals overlap such that there are no statistically significant differences among these siblings.
4. The effects above persist when controlling for sibship size, birth order, gender, age, and ability (as proxied by whether children were ahead or behind in school when the crisis struck). Together these results suggest that a clear decision rule of preserving schooling mid-crisis of those children who were about to finish the final critical year of a schooling level helped families to adjust their budgets in the medium term in order to maintain higher levels of schooling for *all* of their children in the long run.
5. Finally, information from 2007 on the primary activities of respondents suggests that “Sibling of Final” children are more likely to still be in school, suggesting not only that they have caught up to their siblings who did not experience temporary schooling interruptions during the crisis but that their schooling attainment may indeed surpass that of their siblings. See Tables 2.8 and 2.9. The next wave of the panel (set to be released later in 2016) will allow me to explore these even longer-term educational attainment outcomes.

2.5.2 Second Stage Results: Estimates of the Effect of the Crisis on Wages and Coresidency

As we might predict, these educational differences translate into wage advantages. In a reduced form approach in Table 2.10, final children have a wage advantage that persists when controlling for sibship size, birth order, and ability (as well as per capita expenditures of their families just prior to the crisis). “Siblings of Final” children also have an advantage but this becomes insignificant at standard levels when controlling for ability. In Table 2.11 I estimate a two-stage regression with years of education instrumented with the siblings category variables (as in Table 2.7, column 5) and the second stage as a Mincer style log wage equation. The results suggest a 15% increase in wages per year of education. The F-statistic for a joint significance test of the excluded instruments is 15.34.

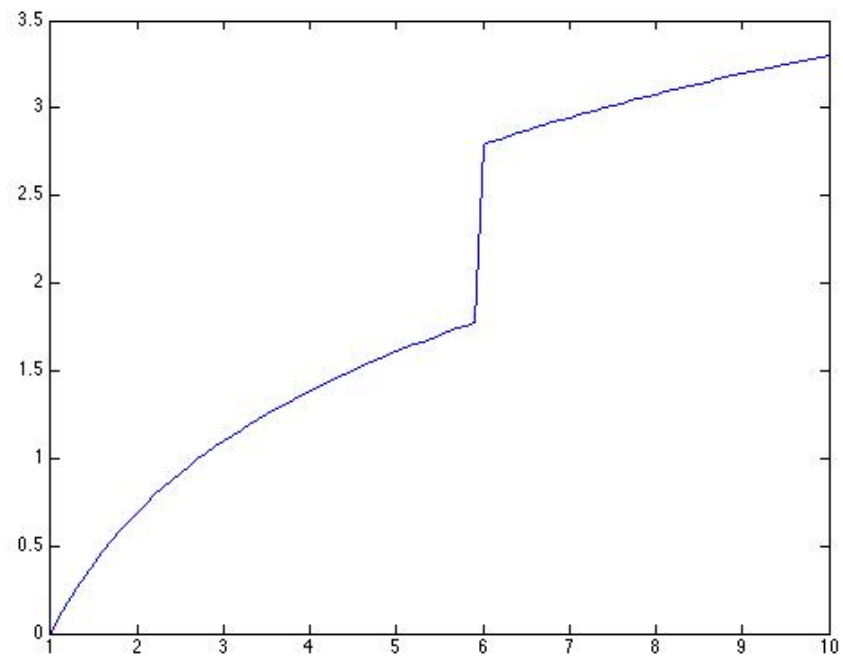
Tables 2.12 through 2.14 show results for coresidency status in 2007. I look at probability of being married, living in the same household as in 1997, living with one’s parents, and having siblings living elsewhere. Table 2.12 uses the sibship size and birth order controls, Table 2.13 adds the Behind and Ahead dummies, and Table 2.14 adds the interactions of Behind and Ahead with “Final” status. The results suggest that “Sibling of Final” children are slightly less likely to be married than controls and “Final” children. Children who were “Behind” in 1997 are more likely to be living in the same household and with their parents in 2007, whereas the opposite is true for children who were Ahead in 1997: they are less likely to live with their parents.

2.6 Conclusion

I find evidence that families preserved schooling of children during the Indonesian financial crisis if they were set to finish the final, critical year of a schooling level, but that the crisis and this decision rule created short-term inequality in educational attainment among siblings. Years later, when most of these siblings are older than 18, there do not remain any statistically significant differences in educational attainment (years completed) among these siblings. However, those siblings who experienced short-term dropout relative to their siblings are more likely to still be enrolled in school, suggesting that they may be even more educated than their siblings in the longer term. Using the IFLS 5 round of the panel will allow me to provide further evidence on this.

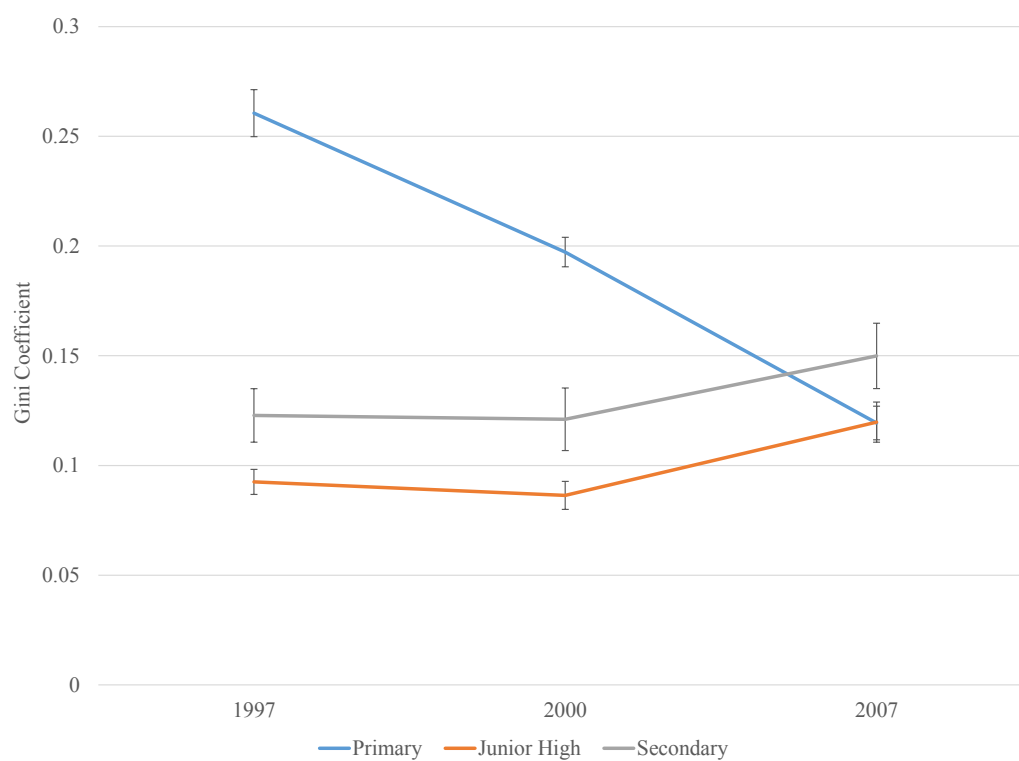
Relative to children living in households where *no* child was in the final year of a schooling level at the time of the 1997 financial crisis, children living in households where the decision rule was clear are more educated, whether or not they were in their final year at the time. These preliminary results suggest that preserving the final year of a schooling level allowed families to adjust temporarily in ways that maintained the schooling of *all* of their children over the long run.

Figure 2.1: Irregular Returns to Schooling



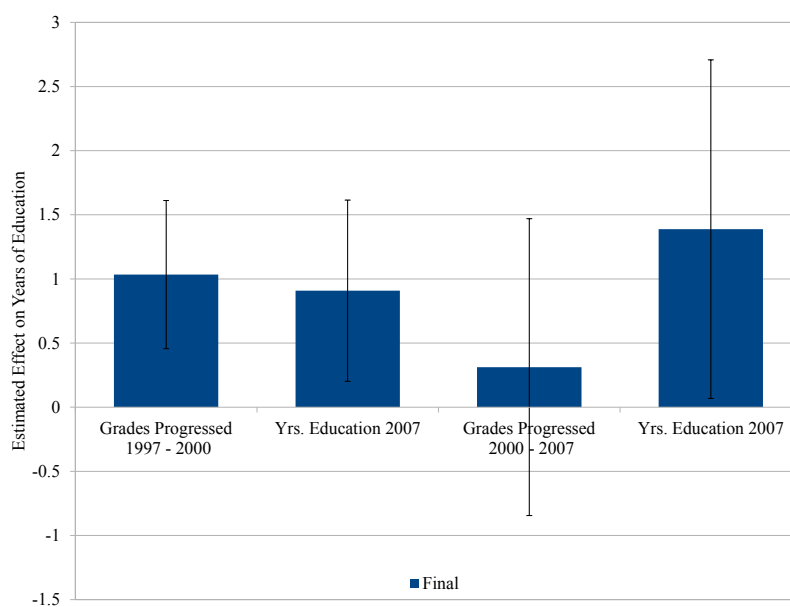
The figure shows a particular return associated with completion of a school level.

Figure 2.2: Gini Coefficient of Educational Attainment



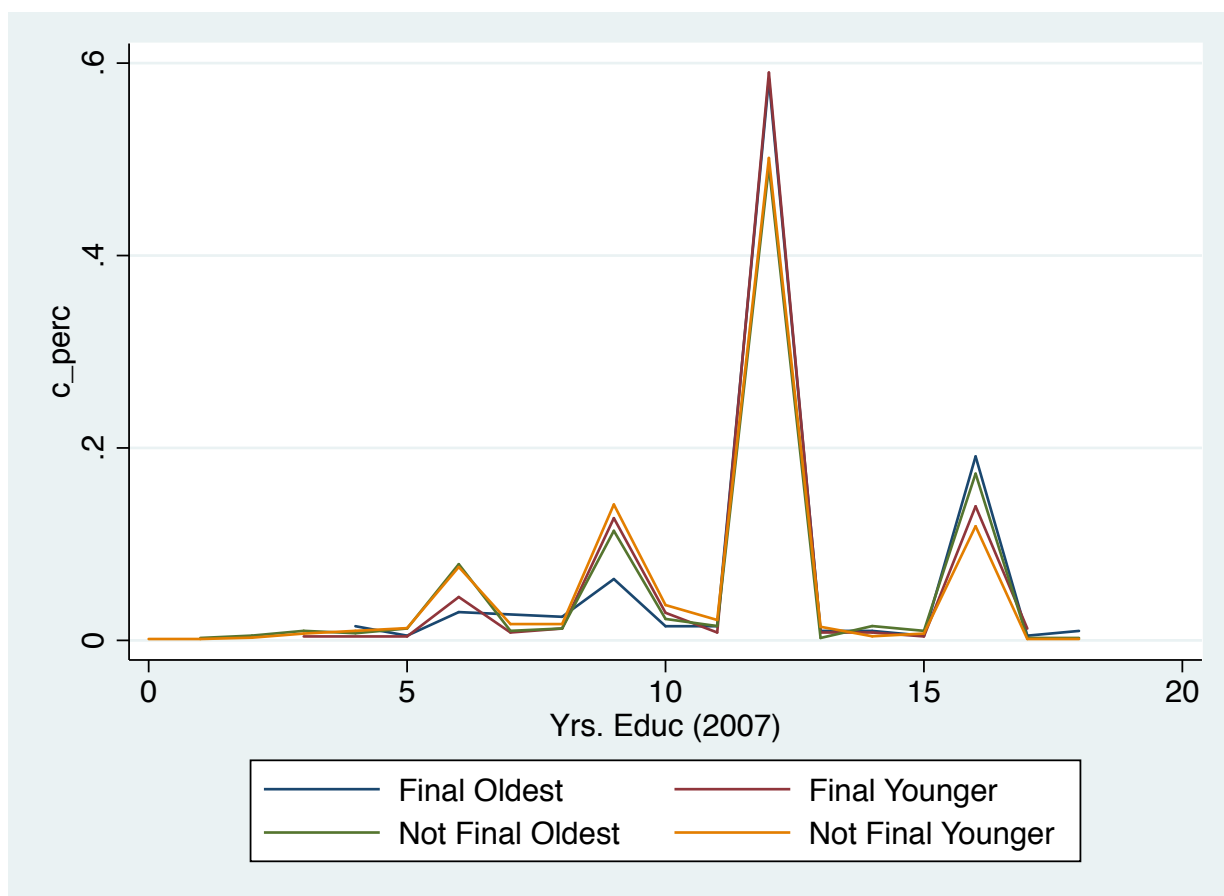
Gini coefficients of educational attainment by age groups in 1997. Error bars are 95% confidence intervals using jackknife standard errors.

Figure 2.3:
Effect of Crisis on Final Children's Educational Attainment.



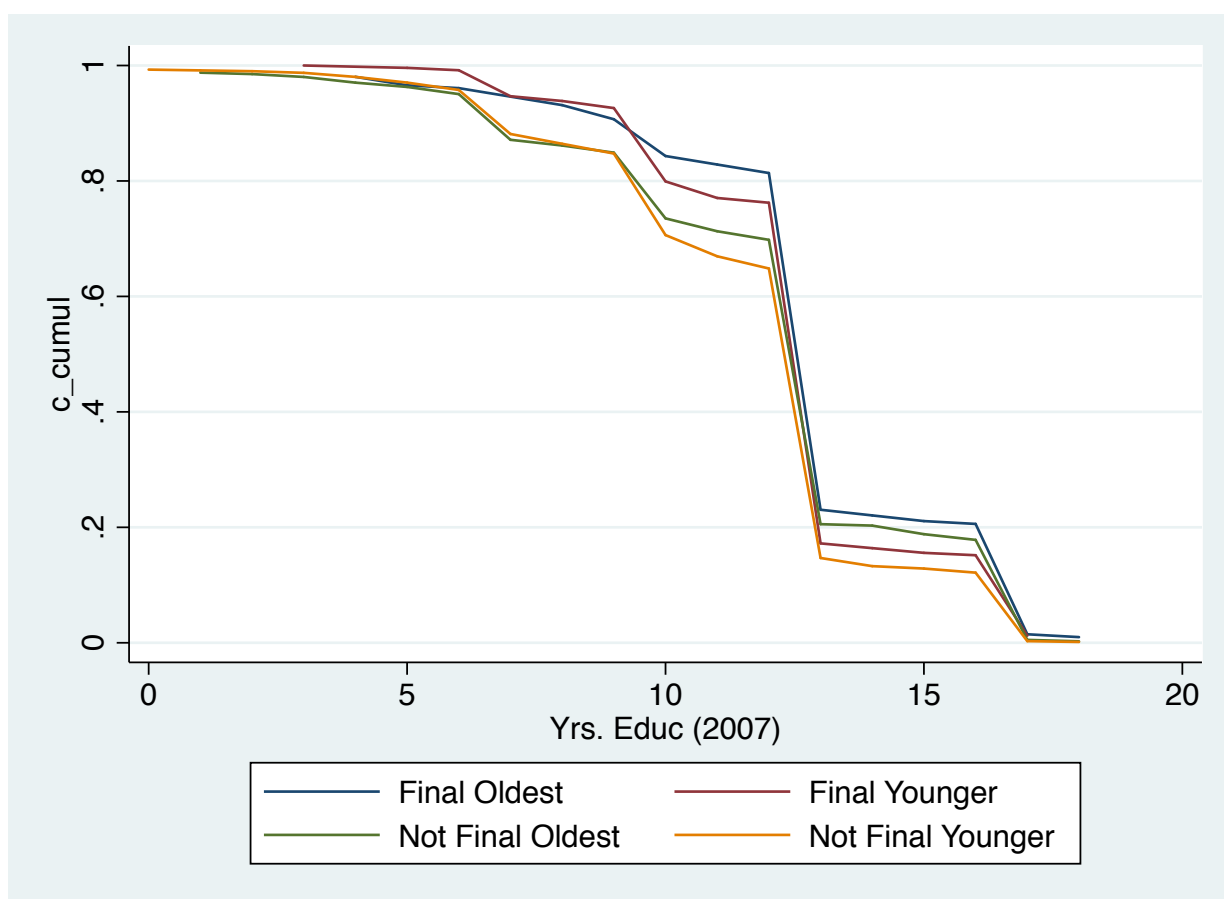
Estimated coefficients with 95% confidence intervals. Effects are relative to other children who are not in their final year of a schooling level at the time of the crisis. Controls as described in equation.

Figure 2.4:
School Completion by Birth Order and Final Status -
PDF



Probability density of school completion by birth order and “final” status.

Figure 2.5:
School Completion by Birth Order and Final Status -
Cumulative Grade Completion



Cumulative density of school completion by birth order and “final” status.

Table 2.1: Would be Final Status

Age in 1997	Grade in 1997 Survey	Grade Mid-crisis	Would be Final?
6	1	2	
7	2	3	
8	3	4	
9	4	5	
10	5	6	Y
11	6	7	
12	7	8	
13	8	9	Y
14	9	10	
15	10	11	
16	11	12	Y

Table 2.2: On Track, Ahead, and Behind Status

Age and Schooling Levels				
	Age 9	Age 10	Age 11	
Grade 4	On Track	Behind	Behind	
Grade 5	Ahead	On Track	Behind	
Grade 6	Ahead	Ahead	On Track	

Table 2.3:
Effect of Crisis on Grade Progression (by 2000) for Final
Year Children and Siblings

Dependent variable: Grades Progressed 1997 to 2000					
VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.233*** (0.0681)	0.231*** (0.0705)	0.216*** (0.0702)	0.304*** (0.0692)	0.327*** (0.118)
Behind				0.166*** (0.0466)	0.176** (0.0659)
Ahead				-0.515*** (0.0513)	-0.509*** (0.0601)
Final*Behind					-0.0414 (0.136)
Final*Ahead					-0.0251 (0.182)
Final Sib	0.0941 (0.0689)	0.0885 (0.0758)	0.0660 (0.0744)	0.0983 (0.0721)	0.0986 (0.0718)
Log Expenditures (percap)	0.0886 (0.0819)	0.0886 (0.0817)	0.0891 (0.0815)	0.111 (0.0786)	0.111 (0.0785)
Female	0.00909 (0.0564)	0.00925 (0.0564)	0.0115 (0.0567)	0.0419 (0.0572)	0.0422 (0.0573)
Number of Siblings		0.0145 (0.0377)	-0.0187 (0.0406)	-0.0189 (0.0387)	-0.0190 (0.0391)
Birth Order			0.129*** (0.0436)	0.101** (0.0409)	0.101** (0.0407)
Age (1997)	-0.160*** (0.00710)	-0.161*** (0.00669)	-0.143*** (0.00995)	-0.174*** (0.0103)	-0.174*** (0.0102)
Constant	3.208*** (0.857)	3.221*** (0.837)	2.821*** (0.843)	2.992*** (0.822)	2.991*** (0.822)
Observations	2,960	2,960	2,960	2,960	2,960
R-squared	0.159	0.159	0.162	0.190	0.190
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.4:
Effect of Crisis on Grade Progression (2000 to 2007) for
Final Year Children and Siblings

Dependent variable: Grades Progressed 2000 to 2007					
VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.0288 (0.0916)	0.0250 (0.0903)	0.0248 (0.0949)	0.0125 (0.101)	-0.107 (0.128)
Behind				-0.0674 (0.131)	-0.0823 (0.143)
Ahead				0.0162 (0.117)	-0.0648 (0.144)
Final*Behind					0.0598 (0.213)
Final*Ahead					0.330 (0.231)
Final Sib	0.373*** (0.116)	0.361*** (0.112)	0.361*** (0.118)	0.357*** (0.118)	0.359*** (0.117)
Log Expenditures (percap)	0.320*** (0.0532)	0.320*** (0.0528)	0.320*** (0.0529)	0.318*** (0.0528)	0.319*** (0.0531)
Female	0.00221 (0.0808)	0.00279 (0.0811)	0.00282 (0.0814)	-0.00176 (0.0812)	0.000951 (0.0807)
Number of Siblings		0.0317 (0.0413)	0.0312 (0.0467)	0.0317 (0.0466)	0.0335 (0.0468)
Birth Order			0.00164 (0.0887)	0.00364 (0.0889)	0.00457 (0.0891)
Age (1997)	-0.507*** (0.0104)	-0.508*** (0.0104)	-0.508*** (0.0157)	-0.503*** (0.0191)	-0.503*** (0.0191)
Constant	5.253*** (0.547)	5.276*** (0.541)	5.271*** (0.632)	5.275*** (0.631)	5.275*** (0.632)
Observations	2,925	2,925	2,925	2,925	2,925
R-squared	0.475	0.475	0.475	0.475	0.476
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.5:
Effect of Crisis on Junior High Graduation (by 2007) for
Final Year Children and Siblings

VARIABLES	Dependent variable: Junior High Graduate				
	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.189*** (0.0152)	0.190*** (0.0156)	0.186*** (0.0165)	0.124*** (0.0170)	0.131*** (0.0270)
Behind				-0.211*** (0.0145)	-0.209*** (0.0179)
Ahead				0.0835*** (0.0211)	0.0864*** (0.0252)
Final*Behind					-0.00875 (0.0405)
Final*Ahead					-0.0123 (0.0404)
Final Sib	0.0639*** (0.0139)	0.0671*** (0.0158)	0.0598*** (0.0162)	0.0443*** (0.0157)	0.0443*** (0.0157)
Log Expenditures (percap)	0.0245** (0.0109)	0.0247** (0.0110)	0.0250** (0.0110)	0.0114 (0.00817)	0.0114 (0.00813)
Female	0.0488** (0.0198)	0.0487** (0.0197)	0.0493** (0.0200)	0.0271 (0.0174)	0.0270 (0.0175)
Number of Siblings		-0.00903 (0.0136)	-0.0207 (0.0128)	-0.0173 (0.0123)	-0.0173 (0.0122)
Birth Order			0.0449*** (0.0149)	0.0489*** (0.0117)	0.0489*** (0.0117)
Age (1997)	0.0424*** (0.00285)	0.0427*** (0.00267)	0.0489*** (0.00368)	0.0613*** (0.00327)	0.0613*** (0.00326)
Constant				-0.264** (0.119)	-0.265** (0.119)
Observations	3,085	3,085	3,085	3,085	3,085
R-squared				0.248	0.248
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.6:
Effect of Crisis on High School Graduation (by 2007) for
Final Year Children and Siblings

VARIABLES	Dependent variable: High School Graduate				
	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.188*** (0.0225)	0.188*** (0.0224)	0.184*** (0.0232)	0.109*** (0.0203)	0.0978*** (0.0246)
Behind				-0.250*** (0.0168)	-0.251*** (0.0184)
Ahead				0.113*** (0.0145)	0.105*** (0.0168)
Final*Behind					0.00519 (0.0417)
Final*Ahead					0.0316 (0.0344)
Final Sib	0.0945*** (0.0224)	0.0932*** (0.0240)	0.0860*** (0.0237)	0.0606*** (0.0193)	0.0608*** (0.0192)
Log Expenditures (percap)	0.0508*** (0.0165)	0.0508*** (0.0165)	0.0513*** (0.0164)	0.0310** (0.0128)	0.0311** (0.0128)
Female	0.0187 (0.0229)	0.0188 (0.0229)	0.0191 (0.0231)	-0.00280 (0.0189)	-0.00263 (0.0191)
Number of Siblings		0.00346 (0.0140)	-0.00822 (0.0167)	-0.00533 (0.0126)	-0.00511 (0.0126)
Birth Order			0.0425 (0.0260)	0.0474** (0.0185)	0.0475** (0.0185)
Age (1997)	0.0433*** (0.00277)	0.0431*** (0.00256)	0.0490*** (0.00446)	0.0618*** (0.00286)	0.0618*** (0.00286)
Constant				-0.517*** (0.169)	-0.517*** (0.169)
Observations	3,058	3,058	3,058	3,058	3,058
R-squared				0.272	0.272
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.7:
Effect of Crisis on Education Attained (by 2007) for Final
Year Children and Siblings

VARIABLES	Dependent variable: Years of Education in 2007				
	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.991*** (0.135)	0.990*** (0.136)	0.980*** (0.139)	0.667*** (0.130)	0.460*** (0.161)
Behind				-1.573*** (0.149)	-1.667*** (0.163)
Ahead				0.558*** (0.118)	0.517*** (0.131)
Final*Behind					0.411* (0.224)
Final*Ahead					0.187 (0.221)
Final Sib	0.648*** (0.132)	0.647*** (0.135)	0.629*** (0.138)	0.519*** (0.126)	0.515*** (0.125)
Log Expenditures (percap)	0.504*** (0.0904)	0.504*** (0.0903)	0.504*** (0.0902)	0.446*** (0.0856)	0.448*** (0.0862)
Female	0.160 (0.0948)	0.160* (0.0945)	0.161* (0.0944)	0.0507 (0.0866)	0.0474 (0.0873)
Number of Siblings		0.00281 (0.0464)	-0.0263 (0.0580)	-0.0120 (0.0570)	-0.0119 (0.0570)
Birth Order			0.107 (0.125)	0.164 (0.105)	0.163 (0.105)
Age (1997)	0.104*** (0.0139)	0.103*** (0.0139)	0.118*** (0.0199)	0.229*** (0.0185)	0.230*** (0.0186)
Constant	3.057*** (1.119)	3.059*** (1.121)	2.740** (1.247)	2.751** (1.107)	2.772** (1.115)
Observations	3,058	3,058	3,058	3,058	3,058
R-squared	0.225	0.225	0.226	0.296	0.297
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.8:
Effect of Crisis on 2007 Primary Activity for Final Year
Children and Siblings

VARIABLES	Primary Activity in 2007			
	(1) Working	(2) In School	(3) House Work	(4) Looking for Work
Final	0.0206 (0.0304)	-0.0234* (0.0125)	-0.0126 (0.0243)	0.00610 (0.0175)
Behind	-0.0415 (0.0330)	0.0200 (0.0146)	-0.0447* (0.0268)	0.00192 (0.0166)
Ahead	0.0176 (0.0286)	0.0102 (0.0130)	-0.0275 (0.0237)	0.00446 (0.0191)
Final Sib	-0.0342 (0.0209)	0.0288** (0.0121)	-0.0223 (0.0317)	-0.0203 (0.0137)
Log Expenditures (percap)	-0.0323** (0.0134)	0.0395*** (0.00788)	-0.00982 (0.0135)	-0.0112 (0.00902)
Female	-0.206*** (0.0269)	0.00325 (0.0114)	0.514*** (0.0163)	0.350*** (0.0176)
Number of Siblings	-7.21e-06 (0.00942)	-0.00626 (0.00646)	0.0235* (0.0124)	-0.00518 (0.00832)
Birth Order	0.0121 (0.0160)	-0.00792 (0.0108)	-0.0276** (0.0126)	-0.00633 (0.00991)
Age (1997)	0.0518*** (0.00358)	-0.0479*** (0.00214)	0.0205*** (0.00468)	0.0127*** (0.00248)
Observations	3,086	3,086	3,086	3,087
Province FE?	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.9:
Effect of Crisis on 2007 Primary Activity for Final Year
Children and Siblings, Ability Interacted

VARIABLES	Primary Activity in 2007, Ability Interacted			
	(1) Working	(2) In School	(3) House Work	(4) Looking for Work
Final	-0.00339 (0.0470)	-0.0597*** (0.0222)	-0.0348 (0.0348)	0.000322 (0.0230)
Behind	-0.0509 (0.0367)	0.0169 (0.0151)	-0.0512 (0.0315)	-0.000771 (0.0193)
Ahead	0.0108 (0.0311)	-0.00353 (0.0137)	-0.0378 (0.0287)	0.00331 (0.0221)
Final*Behind	0.0418 (0.0538)	0.0145 (0.0526)	0.0275 (0.0635)	0.0115 (0.0291)
Final*Ahead	0.0294 (0.0440)	0.108** (0.0535)	0.0435 (0.0706)	0.00506 (0.0378)
Final Sib	-0.0345 (0.0213)	0.0283** (0.0118)	-0.0221 (0.0317)	-0.0204 (0.0138)
Log Expenditures (percap)	-0.0321** (0.0134)	0.0394*** (0.00788)	-0.00966 (0.0135)	-0.0111 (0.00896)
Female	-0.206*** (0.0268)	0.00437 (0.0114)	0.514*** (0.0163)	0.350*** (0.0175)
Number of Siblings	0.000107 (0.00938)	-0.00569 (0.00644)	0.0237* (0.0124)	-0.00517 (0.00843)
Birth Order	0.0119 (0.0161)	-0.00739 (0.0104)	-0.0275** (0.0126)	-0.00635 (0.01000)
Age (1997)	0.0519*** (0.00355)	-0.0466*** (0.00216)	0.0206*** (0.00465)	0.0127*** (0.00249)
Observations	3,086	3,086	3,086	3,087
Province FE?	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.10:
Effect of Crisis on 2007 Earnings for Final Year Children
and Siblings

Dependent variable: Log monthly earnings in 2007					
VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Final	0.133** (0.0535)	0.129** (0.0529)	0.126** (0.0543)	0.0930* (0.0522)	0.137 (0.0970)
Behind				-0.285*** (0.0808)	-0.263** (0.105)
Ahead				0.0780 (0.0608)	0.0902 (0.0773)
Final*Behind					-0.0799 (0.160)
Final*Ahead					-0.0454 (0.134)
Final Sib	0.0911* (0.0498)	0.0837* (0.0449)	0.0800* (0.0464)	0.0701 (0.0489)	0.0702 (0.0489)
Log Expenditures (percap)	0.156*** (0.0414)	0.157*** (0.0419)	0.156*** (0.0420)	0.141*** (0.0424)	0.142*** (0.0426)
Female	-0.121* (0.0623)	-0.122* (0.0624)	-0.122* (0.0624)	-0.150** (0.0628)	-0.149** (0.0626)
Number of Siblings		0.0172 (0.0258)	0.0114 (0.0290)	0.0111 (0.0288)	0.0112 (0.0288)
Birth Order			0.0204 (0.0439)	0.0283 (0.0406)	0.0276 (0.0409)
Age (1997)	0.0616*** (0.00855)	0.0609*** (0.00864)	0.0639*** (0.0133)	0.0850*** (0.0135)	0.0848*** (0.0137)
Constant	10.91*** (0.394)	10.92*** (0.387)	10.85*** (0.387)	10.80*** (0.351)	10.79*** (0.358)
Observations	1,309	1,309	1,309	1,309	1,309
R-squared	0.157	0.157	0.157	0.180	0.181
Province FE?	Y	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.11:
Returns to Education in 2007 (Instrumenting with Final
Year and Sibling Status)

Dependent variable: Log monthly earnings in 2007					
VARIABLES	(1)	(2) With Num Sibs Control	(3) Birth Order Control	(4) Controls for Ability	(5) Controls for Ability and Interact
Yrs. Educ (2007)	0.154** (0.0748)	0.154** (0.0743)	0.155** (0.0776)	0.165*** (0.0320)	0.159*** (0.0325)
Log Expenditures (percap)	0.0702 (0.0530)	0.0705 (0.0547)	0.0702 (0.0549)	0.0639 (0.0580)	0.0676 (0.0587)
Female	-0.229*** (0.0758)	-0.229*** (0.0740)	-0.229*** (0.0745)	-0.236*** (0.0631)	-0.232*** (0.0624)
Number of Siblings		0.000682 (0.0251)	0.00172 (0.0291)	0.00120 (0.0314)	0.00150 (0.0312)
Birth Order			-0.00376 (0.0454)	-0.00667 (0.0409)	-0.00499 (0.0408)
Age (1997)	0.0379** (0.0176)	0.0380** (0.0174)	0.0373* (0.0221)	0.0351*** (0.0130)	0.0364*** (0.0126)
Constant	10.14*** (0.506)	10.14*** (0.501)	10.15*** (0.468)	10.14*** (0.428)	10.14*** (0.421)
Observations	1,298	1,298	1,298	1,298	1,298
R-squared	0.060	0.061	0.060	0.037	0.050
Province FE?	Y	Y	Y	Y	Y
IV?	Y	Y	Y		

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.12:
Effect of Crisis on Coresidency with Parents and Siblings (no ability controls)

VARIABLES	Coresidency Outcomes in 2007			
	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0120 (0.0240)	-0.0193 (0.0237)	-0.00299 (0.0329)	-0.0137 (0.0252)
Final Sib	-0.0483* (0.0262)	0.0321 (0.0261)	0.0314 (0.0349)	-0.0308 (0.0206)
Log Expenditures (percap)	-0.0208 (0.0132)	-0.00562 (0.0175)	-0.00544 (0.0158)	0.00292 (0.0130)
Female	0.311*** (0.0188)	-0.0768*** (0.0163)	-0.0865*** (0.0185)	0.0245** (0.0113)
Number of Siblings	-0.01000 (0.0149)	0.00822 (0.0131)	0.00442 (0.0139)	0.0372*** (0.0135)
Birth Order	-0.0216 (0.0181)	0.0180 (0.0148)	0.0198 (0.0143)	0.0888*** (0.0163)
Age (1997)	0.0743*** (0.00506)	-0.0317*** (0.00365)	-0.0367*** (0.00390)	0.0289*** (0.00408)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.13:
Effect of Crisis on Coresidency with Parents and Siblings (with ability controls)

Coresidency Outcomes in 2007 (Ability Controls)				
VARIABLES	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0194 (0.0241)	-0.00250 (0.0235)	0.0113 (0.0325)	-0.0135 (0.0244)
Behind	-0.0279 (0.0274)	0.0601*** (0.0188)	0.0547** (0.0246)	0.00397 (0.0189)
Ahead	0.0354 (0.0318)	-0.0709*** (0.0272)	-0.0562* (0.0287)	0.00348 (0.0190)
Final Sib	-0.0522** (0.0263)	0.0390 (0.0270)	0.0369 (0.0353)	-0.0308 (0.0204)
Log Expenditures (percap)	-0.0228* (0.0133)	-0.00187 (0.0179)	-0.00251 (0.0155)	0.00288 (0.0131)
Female	0.309*** (0.0192)	-0.0713*** (0.0171)	-0.0818*** (0.0191)	0.0245** (0.0111)
Number of Siblings	-0.00973 (0.0146)	0.00785 (0.0132)	0.00400 (0.0140)	0.0371*** (0.0135)
Birth Order	-0.0194 (0.0187)	0.0139 (0.0147)	0.0164 (0.0145)	0.0890*** (0.0167)
Age (1997)	0.0774*** (0.00577)	-0.0382*** (0.00430)	-0.0423*** (0.00446)	0.0289*** (0.00431)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.14:
Effect of Crisis on Coresidency with Parents and Siblings (with ability controls interacted with Final)

Coresidency Outcomes in 2007 (Ability Controls and Interacted)				
VARIABLES	(1) Ever Married	(2) Still in Same HH	(3) Lives with Parents	(4) Has Siblings Elsewhere
Final	-0.0196 (0.0354)	0.0366 (0.0402)	0.0237 (0.0584)	0.00201 (0.0349)
Behind	-0.0335 (0.0323)	0.0824*** (0.0235)	0.0588* (0.0313)	0.00469 (0.0205)
Ahead	0.0455 (0.0416)	-0.0684* (0.0364)	-0.0507 (0.0327)	0.0138 (0.0227)
Final*Behind	0.0231 (0.0490)	-0.0968** (0.0469)	-0.0169 (0.0688)	-0.00223 (0.0383)
Final*Ahead	-0.0321 (0.0488)	-0.0140 (0.0537)	-0.0213 (0.0554)	-0.0467 (0.0456)
Final Sib	-0.0527** (0.0259)	0.0403 (0.0269)	0.0369 (0.0353)	-0.0310 (0.0204)
Log Expenditures (percap)	-0.0227* (0.0133)	-0.00216 (0.0181)	-0.00261 (0.0156)	0.00264 (0.0130)
Female	0.308*** (0.0190)	-0.0705*** (0.0171)	-0.0819*** (0.0191)	0.0242** (0.0112)
Number of Siblings	-0.0100 (0.0147)	0.00797 (0.0134)	0.00390 (0.0141)	0.0368*** (0.0135)
Birth Order	-0.0197 (0.0188)	0.0143 (0.0151)	0.0164 (0.0146)	0.0889*** (0.0166)
Age (1997)	0.0774*** (0.00581)	-0.0384*** (0.00427)	-0.0423*** (0.00444)	0.0288*** (0.00429)
Observations	3,086	3,087	3,087	3,085
Province FE?	Y	Y	Y	Y
Lincom Final - Sibling	0.101	-0.0102	-0.0352	0.126
SE Final - Sibling	0.114	0.113	0.125	0.125

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

CHAPTER 3

**GENDER DIFFERENCES IN CHILD FOSTERING IN WEST AFRICA:
SCHOOLING AND WORK AWAY FROM HOME**

3.1 Introduction

Households in the developing world are larger and their size and structure tend to be more fluid than in the developed world. Choices of household composition can have significant implications for the well-being of household members: the composition of a household affects the availability and distribution of resources, from labor and investment choices to the receipt of public transfers that may be contingent on the eligibility of certain types of household members. Policymakers and academics have noted that, despite increasing awareness of the importance of household composition choices and their impacts, the topic remains difficult to measure and thus not well understood.

Child fostering is a widespread practice in West Africa, and to a lesser extent in other parts of the developing world, through which parents voluntarily and informally send children to spend a portion of their childhood living away from home while retaining familial ties to their biological households. Considering all of the children who are touched by the practice (fostered children, the siblings they leave behind at home, and the biological children of the households in which they reside), my estimates from West Africa suggest that more than 50 percent of children are potentially affected by the practice of fostering, a figure which warrants more attention than has been paid by economists in recent work.

Citing lower average school enrollment and higher average work rates among fostered children, several development and government organizations in West Africa have expressed the judgment that fostered children are little more than unpaid servants living in the households hosting them and that the practice arises out of parental selfishness. In the spirit of common arguments against child labor, several government and development agencies have called for a ban on fostering.¹ The United Nations' Committee on the Rights of the Child stated in 1993 that children should not be separated from their parents "except by competent authorities for their well-being" and in 2002 called for "a stop to the practice of 'fostering.'"² However, cross-sectional empirical work by economists has indicated that fostering may be associated with higher schooling rates, and that the decision to send a child to live elsewhere is related to financial and family network motives that may benefit the child into adulthood. Theoretical work by Serra (2009) demonstrates the possibility that schooling and labor motivations coexist in a single household's decision-making process to foster (out or in) and that fostered children may experience positive externalities in their host households even if they do not attend school. She cautions against judgments of fostering without full knowledge of the counterfactual experience of each child.

The contribution of this paper is two-fold. First, I provide recent evidence from eleven West African countries that fostering is still very prevalent and that it stands to affect a large number of children through siblinghood and coresidency even when they do not live away from home themselves. Second, despite the increased attention paid by social scientists to the potential benefits of fostering, no study has noted or explained what I observe in the data from the region: the observed relationship among fostered status, schooling, and work appears

¹See Simpson et al. (2012) and Pilon (2003).

²Vienna Declaration and Programme of Action (1993).

to vary significantly by the gender of the child. I show that foster girls work more and are less likely to be in school than all other children, while foster boys seem to fare better than the children they leave behind at home, and possibly even better than children in households that have not engaged in the fostering practice. In most of the eleven countries, the gender gap in schooling is wider among foster children than the overall gender gap in schooling in the region. This gender difference in the experience of foster children should be interpreted cautiously and not causally, and the empirical facts suggest that from a policy perspective, fostering of boys and fostering of girls should be considered entirely separately. Efforts to address low average school enrollment of girls more generally may be preferable to efforts directed at putting an end to fostering, even if foster girls seem to fare worse than other children.

This paper is organized as follows: Section 3.2 describes relevant previous work on fostering and household composition and the limitations of these studies' approaches. In Section 3.3, I describe the data I use in this descriptive study of eleven West African countries. Section 3.4 presents empirical stylized facts I observe regarding fostered children and the households that send and host them. Section 3.5 concludes and proposes areas for future work.

3.2 Literature Review

This paper contributes to a small but growing economic literature on child fostering, which has itself developed work by social scientists in other fields including sociology, anthropology, and ethnography. The practice of child fostering has been documented and discussed by social science disciplines outside of

economics for some time. It consists of a temporary and reversible transferring of child rearing responsibilities; children are typically sent away while remaining household members and the child's parents continue living at home, but the possibility that migrating parents may leave children behind with alternative caretakers has also been acknowledged. Most of these studies, most notably Isiugo-Abanihe (1985), highlight family network motivations and benefits of the practice: fostering has long been a socially acceptable and important means by which extended families strengthen their ties across geographic regions. In addition, these studies highlight but do not estimate the possible implications of the acceptability of fostering for national and regional demographic and fertility trends.

The limitations of available data regarding children's living arrangements have prevented many studies from identifying the practice at all. In addition, the household decision to send a child is likely to be inextricably linked to the household's expectations of outcomes such that outcomes and characteristics of households or specific children that determine sending decisions are endogenous. A household is not likely to make the decision to foster (out or in) or to send a specific child to be fostered independently of what its hopes or thoughts are as to what might happen to the child. Thus, OLS estimates of the coefficients on fostered status with child outcomes as the dependent variable will be inconsistent as to the true "treatment effects" of fostering due to this endogeneity of fostering at both the household and individual child levels. However, controlling for several observed characteristics, we can at least estimate cross-sectional correlations between fostering and child outcomes in the hopes of learning more about the practice and the children involved. OLS estimates can only hint at underlying correlations but cannot disentangle causality.

The seminal study of fostering by an economist, Ainsworth (1996), uses household survey data from Côte d'Ivoire to give evidence for the family level motivations for fostering exchanges. She estimates household-level Tobit regressions of household sending and hosting status on household income and demographic indicators. She finds that, consistent with explanations which relate child fostering to the demand for child labor in household production, households with more adults of both sexes are more likely to foster children in, girls or boys. Additionally, households with higher income and in urban areas are more likely to host foster children, especially girls; these relationships do not necessarily satisfy the child labor explanation in place of a schooling explanation, although she explicitly tests whether the availability and quality of local schooling affects household fostering (sending or hosting) decisions and finds that the evidence is inconclusive. Her study does not, however, investigate the child-level observations that others, including the current study, have, nor does she establish conclusive empirical evidence as to the sending motivations, although she does acknowledge that sending and hosting decisions are likely to depend asymmetrically on household characteristics.

More recent studies explore the possibility that household level correlations documented by Ainsworth (1996) mask heterogeneity in fostering sending and hosting decisions and their implications for schooling and labor outcomes at the individual child level. Zimmerman (2003) uses cross-sectional household survey data from South Africa to estimate the impact of fostering on the school enrollment status of children and on work arrangements, but he assumes that these motivations are mutually exclusive with respect to a particular child. Similarly, Akresh (2009) investigates cross-sectional evidence on the importance of exogenous income shocks, household demographic imbalances (among the

number of children of each gender), and extended family network quality in fostering hosting and sending decisions. In a separate paper in which he matched sending and receiving households in Burkina Faso, Akresh (2003) finds that foster children's schooling outcomes appear to be no worse than those of other children. He notes, but does not explore, a difference in the schooling outcomes of foster girls versus foster boys, but he also assumes that child labor motives are incompatible with motivations for human capital investment.

On the other hand, Gage (2005), Serra (2009), and Temin et al. (2013), acknowledge the possibility that human capital investment and labor demand and supply may coexist in a single fostering decision. Serra (2009) develops a theoretical model which seeks to understand this coexistence and to explain why households might simultaneously have sent and be hosting different children. However, she mentions but does not address the possibility that fostering out may comprise a household's ex-post consumption smoothing or risk insurance response to a negative income shock, nor does she consider the forward-looking nature of households in their decisions: schooling affects households' current period utility as it increases child "quality" but is not otherwise understood as a household investment in her model. While the model in Serra (2009) does acknowledge the possible existence of human capital externalities in host households that may improve fostered children's opportunities outside of schooling per se, she does not directly predict how these factors might affect the decisions to foster girls in particular.

Work by Coppoletta et al. (2012) and Beck et al. (2014) provides evidence based on adults in Senegal that being fostered as a child may improve later life outcomes such as employment status, educational attainment, and income for

men, as well as reducing the likelihood that women are involved in polygamous marriages (in which they are one of two or more wives and are thus more resource limited than those wives who do not share husbands).

In using data from comparable surveys from eleven West African countries, this paper provides a more comprehensive view of the prevalence of fostering in West Africa. In doing so, I provide new stylized facts giving evidence for a very gender-distinguished practice.

3.3 Data

In this section, I describe the data I use in this paper and the prevalence of fostering among children and households in the data from eleven West African countries.

3.3.1 Demographic and Health Surveys Data

The Demographic and Health Surveys (DHS) are conducted as a group of country-specific projects of the U.S. Agency for International Development (USAID) in cooperation with local national statistical offices. They provide household and individual-level survey data of men, women, and children using survey questions and designs that are comparable across countries to allow for cross-country comparisons.

In this paper, I use the most recent sample of data from each West African country available in the DHS. These eleven countries (with interview years in

parentheses) are: Benin (2011-2), Burkina Faso (2010), Cameroon (2011), Côte d'Ivoire (2011-2), Ghana (2014), Guinea (2012), Niger (2012), Nigeria (2013), Senegal (2014), Sierra Leone (2013), and Togo (2013-4).

For the purposes of this paper, I categorize children into four categories (which correspondingly classifies households into three categories):

1. **Foster children:** those whose parents are both alive but who live with neither parent. They live in *host households*.³
2. **Host siblings:** children living with their own biological parents in households that are hosting foster children.
3. **Biological siblings:** siblings living in households with their own parents, but who, according to mothers' birth records, have siblings living away from home. These children live in *sending households*.⁴
4. The fourth category of children is then defined as the remainder: children whose parents are alive and who live with their parents, in households where there are neither "missing" (fostered out) nor "extra" (fostered in) children. This category can be thought of as the reference (or base) group in the analysis which follows. These reference children live in *reference households* that are neither host nor sending households.

As previous studies have done, I restrict my analysis to children ages 5-16; younger children are likely to be fostered for reasons that might systematically differ from those for fostering older children, such as severe family crisis, and

³The household questionnaire asks, for each child residing in the household, whether her mother and father are alive and whether each of them resides in the household with the child.

⁴They are the biological siblings, who remain at home, of children who have been sent to live elsewhere as fosters. Note, however, that I cannot directly match these children to the foster children who left them behind; rather, they serve as representative proxies.

children older than 16 are considered adults in many family networks in West Africa, evidenced especially by the higher incidence of marriage among these children.⁵ In addition, because orphanhood arises from fundamentally different household situations and may lead to systematically different observed outcomes, I omit these children from my sample.

As mentioned above, it is important to note that while I have classified children as “foster” and as “biological siblings,” I cannot directly observe or describe these as *each other’s* siblings because the DHS have not intentionally matched them. I can only observe and define, for each child in a given household, which type of child she happens to be at the time of survey. Ideally, I would be able to control for time-varying or fixed household (or sibling) characteristics, but that would require tracking down the partner household in each fostering exchange. This is what Akresh (2009) did, but this understandably restricts his sample size to 358 households. However, assuming that the households selected to participate in the DHS surveys do not differ systematically from those that were not (as should be the case for nationally representative samples), and that the household weights have been computed correctly, this alone should not systematically bias the results that I observe.

The dataset I created containing children categorized as above from the eleven DHS samples contains 203,647 children ages 5 to 16. Table 3.1 provides sample size information for these surveys by country.

⁵This age restriction is consistent with that of Ainsworth (1996) and Akresh (2007), among others.

3.3.2 Summary Statistics: Prevalence of Fostering

Figure 3.1 shows the proportion of boys and girls in West Africa who were foster children at the time of survey. Across the eleven West African countries for which the DHS have data, the proportion of children fostered varies from between 3.5 percent (Niger and Nigeria) to more than 20 percent (Sierra Leone). In each country, girls are more likely than boys to be fostered, and the difference is always statistically significant. However, a sizable portion of boys are fostered in the region as well as girls. The overall fostering rate across the region is 9.2% (but 8.1% for boys and 10.3% for girls). Among foster boys and girls, the average age is 10 years old, and the difference in ages between foster boys and foster girls are *not* statistically significantly different from one another in all but one country, Senegal, where foster boys are on average 0.68 years older than foster girls.

Figure 3.2 shows that even when fostering rates are low (as in Nigeria and Niger, for example), the practice of fostering stands to affect a large proportion of children: including those children living in homes where fostered children are hosted (what I termed “host siblings”) and those children living in homes *from which* fostered children have come (what I termed “biological siblings”), at least 40 percent and as many as 80 percent of children in West African countries are touched by the practice of fostering. Together, these children live in 35 to 75 percent of households with children (which is most households) across the region, with sending households more prevalent than host households, as shown in Figure 3.3. Figures 3.4 and 3.5 confirm what others have observed with respect to wealth levels of households that host or send foster children.⁶ Figure

⁶The only wealth/income information available in all surveys is wealth quintile.

3.4 shows that among host households, those hosting girls are more likely to be in the higher wealth quintiles. Figure 3.5 shows that sending households are more likely to be in lower wealth quintiles, and there is not difference by the gender of the child sent.

3.4 Schooling and Work among Children

Having classified children and households in the manner described above, I turn to examining differences in schooling and work outcomes among fostered and non-fostered children to provide descriptive evidence of the realities that foster children face. Specifically, I show that a more naive comparison of simple average characteristics of foster children relative to non-fostered children masks significant and important heterogeneity of the experience of fostering by the gender of children.

3.4.1 School Enrollment of Children

Figure 3.6 shows unconditional current school enrollment figures by country for non-fostered versus foster children (the p-value for a t-test of significance in the differences is reported above each column). The figure shows that school enrollment rates vary quite a bit across the region, with lows around 40% (Niger and Burkina Faso) and highs over 80% (Togo and Cameroon). The figure also confirms that in a number of countries, foster children are statistically significantly less likely to attend school than others (Benin, Côte d'Ivoire) but that in others, foster children's school enrollment rates are higher than those of other

children (Cameroon, Nigeria, Niger, Senegal), consistent with human capital motivations for fostering cited by others.

However, Figure 3.7 shows that boys and girls have different rates of school enrollment and, further, that the gender gap in school enrollment is *wider* for fostered children. In all countries except Sierra Leone and Senegal, boys' overall school attendance rates are higher than girls' among all children (the left, patterned bars). Among foster children, this gender gap in enrollment is even larger in several countries (Burkina Faso, Benin, Côte d'Ivoire, Niger, and Togo). In Senegal, where girls are more likely to attend school than boys on average, the opposite is true among fostered children: fostered girls are less likely than fostered boys to attend school even though school enrollment favors girls more generally.

To separate some of these effects and to look at "other children" by their status according to the classifications described earlier (their attachment to the fostering practice), I estimate an OLS regression as follows:

$$Enrolled_{ihc} = \beta_0 + \beta_1 Foster_{ihc} + \beta_2 HostSib_{ihc} + \beta_3 BioSib_{ihc} + X'_{ihc} \Gamma + \eta_c + \epsilon_{ihc} \quad (3.1)$$

where $Enrolled_{ihc}$ takes the value one if child i in household h in country c is enrolled in school at the time of survey, $Foster$, $HostSib$, and $BioSib$ are indicators for child status (their attachment to the fostering practice), X_{ihc} is controls containing the gender and age of child i and the wealth quintile of their household ($Wealth_h$), and country fixed effects and their interaction with child status are included, allowing for country-specific fostering schooling rates (and for host siblings and biological siblings as well). The regression results are not easily digested in table format, so instead I take the linear combination of child status and the country-specific interaction term with child status to give an aver-

age, regression-adjusted school enrollment rate of children relative to the omitted reference category (those children who live with their parents in neither a sending nor a hosting household).

The regression-adjusted differences in school enrollment are visually presented in Figure 3.8. These show that in most of the eleven sample countries, foster children are less likely to be enrolled in school relative to their host siblings (the children with whom they live), “biological siblings” (children who are living at home but who have siblings residing elsewhere), and children unattached to the fostering practice (the omitted reference category, indicated by the zero, the x-axis). In all countries, host siblings have higher school enrollment than all other children, and children living in sending households (bio siblings) are less likely than base children to attend. These naive comparisons of foster children relative to non-fostered children suggest that foster children fare worse than others.

However, as noted before in Figure 3.7, the school enrollment levels of foster boys and foster girls differ significantly in most countries in the sample. Therefore, I estimate a further regression that interacts gender with child status (and also with country fixed effects, allowing for country- and gender-specific regression adjusted average school enrollment for children of different types). The regression adjusted school enrollment rates (the linear combinations of the appropriate status and interaction terms) are shown graphically in Figures 3.9 and 3.10 (the predicted differences in these two figures are generated from the same, single regression described above but I have shown them in separate graphs for ease of presentation).

Figure 3.9 confirms that, controlling for child age and resident household in-

come quintile, foster girls are less likely to be enrolled in school than foster boys in every country (though the point estimates are significantly different from one another only in Burkina Faso, Benin, Côte d'Ivoire, Senegal, and Togo). Foster boys in Nigeria and Senegal are *more likely* than base children and than biological siblings (seen in the next figure) to be enrolled in school, and in most other countries they are equally likely (although their school enrollment is lower in some others).

Figure 3.10 shows regression adjusted relative school enrollment rates for host and biological siblings (relative to base children, those living in households not currently involved in the fostering practice). The enrollment rates show a pattern consistent with findings of others on fostering: families hosting foster children have high school enrollment rates among their own children (host siblings), especially for host sibling boys. Foster children appear to be hosted by households with high rates of human capital investment, conditional on income quintile. On the other hand, the siblings that have been left behind by foster children have low average school enrollment rates relative to base children, with bio sibling girls less likely than bio siblings boys to be enrolled. Looking at the two figures together, Figures 3.9 and 3.10 suggest the following stylized facts:

1. Foster children live in households where school enrollment is high for the household's own children, conditional on child age and family income quintile.
2. Foster girls are *less likely than any other child* to be enrolled in school.
3. Foster boys are, for the most part, no less likely to be enrolled in school than boys living in sending households, and sometimes they do better.

There are a number of possible explanations for higher enrollment rates among host siblings: hosting a foster child may make it easier for host households to send their own children to school, as there may be more hands to help with the household work. Host households may, all else equal, have a higher unobserved taste for human capital investment. The latter explanation may, in fact, be a factor in sending households' choices of where to send their children; Serra (2009) notes the plausible existence of positive externalities in host households which may benefit foster children even if they do not attend school. Most likely, given evidence and intuition from previous work by economists as well as the ethnographic and sociological literature, these motivations exist simultaneously, perhaps even in the same household, which would make treatment effects very difficult to disentangle. Moreover, without knowledge of each individual child's counterfactual experience, these correlations cannot be attributed as effects of fostering on children: the possibility remains that enrollment levels would be lower for those children from sending households (foster and bio-siblings) and that the practice of fostering improves schooling outcomes for both groups relative to their own counterfactuals, helping them to catch up with or surpass the base category of children.

It bears mentioning that quality of education need not improve merely with school enrollment; if foster children, particularly girls, are frequently working in their host households, even if they are enrolled in school they may frequently miss school or have less time to study than other children. One possible test for this hypothesis would be whether total educational attainment (or high school completion) differed for foster children (boys or girls). Results in Beck et al. (2014) and Coppoletta et al. (2011) regarding later life outcomes suggest that foster children's schooling is just as good as, if not better than, that of their

peers. On the other hand, Serra (2009) proposes that even when foster children are not enrolled in school, there may be positive externalities on child welfare and even human capital of those foster children by virtue of living in their host households.

3.4.2 Domestic Work of Children

Turning to the possible child labor explanations for and outcomes of fostering, I perform the same analysis as above using the question asked of children regarding the hours they spend working, whether working for pay, working on household income generating tasks (e.g. agriculture or family-run business), or doing domestic chores. In all of the surveys except Ghana and Senegal, children are asked whether they perform these types of work and how many hours they work. The examples provided to children for types of tasks are not comparable across countries (e.g. in some, time spent fetching water is a domestic chore and in others it is a chore different from domestic work). Therefore, I use the sum of hours spent on all types of tasks. Nearly two thirds of children report doing some kind of work; girls are significantly more likely to report doing work, which is particularly concentrated in domestic work. Market work is relatively uncommon (only twenty percent of children), and boys are only slightly more likely to report doing market work than girls are. Across all children, the average number of hours worked per week (including zero) is 8.45 hours. Conditional on doing any work, the average is 11.7 hours per week. The unconditional and conditional means of hours worked for girls is higher than for boys and the difference is statistically significant.

Similarly to Figure 3.8, Figure 3.11 shows the regression adjusted hours of work (including zeroes) for children of different types relative to “reference” children, where country fixed effects and wealth quintile of household of residence are controlled for. As the figure shows, host children typically do less work or the same amount of work as reference children, while biological siblings (those living in sending households) do more work in most cases. For foster children, the average hours of work is mixed: in some countries (Burkina Faso, Benin, and Niger) foster children work more on average than others, while in Cameroon, Guinea, and Togo, foster children work less. However, this mixed pattern becomes very clear when classifying children by gender as well as by household status. Figures 3.12 and 3.13 repeat the same regression adjustment but allowing for gender-specific child status (foster, host, or biological sibling). The figures show the linear combinations of the appropriate interaction terms (and they are estimated together, but separated into two figures for the purposes of presentation). The figures show that girls work more on average than boys for all child types, but that foster girls work more on average than any other child. Moreover, in Côte d’Ivoire, Cameroon, Guinea, and Togo, foster boys work less than all other children except sometimes the boys they live with (host boys). When it comes to work in the household of residence, the experience of fostering appears to be entirely different for foster girls than for foster boys.

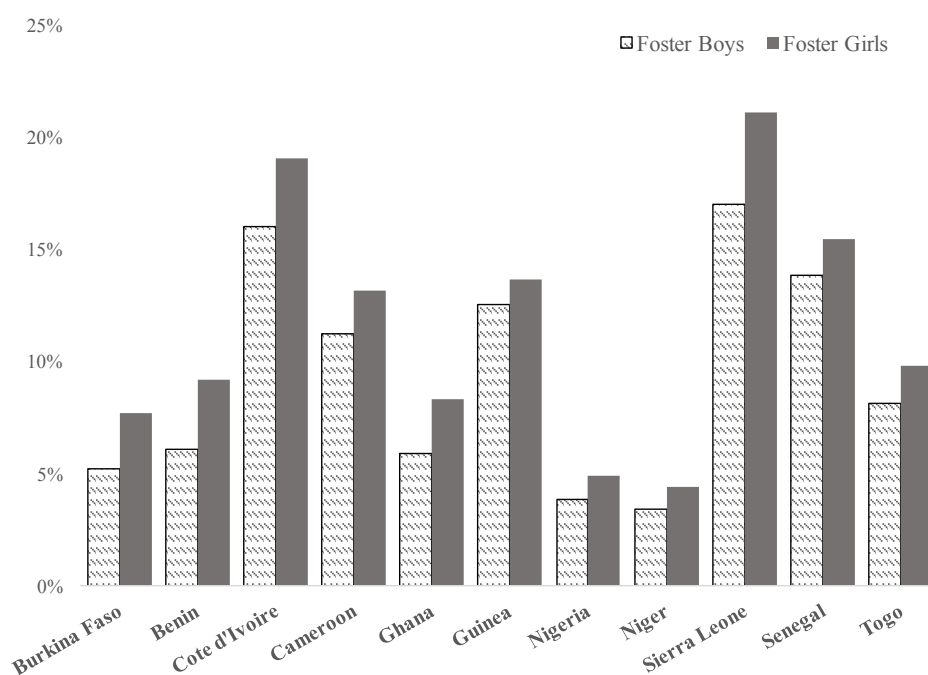
3.5 Conclusion

Child fostering is a widespread practice across the developing world and particularly in West Africa through which children are voluntarily and temporarily sent away from home and hosted by extended family or other households; due

to the large number of households involved, the practice has the potential to affect more than 50 percent of children by its impacts on the distribution of resources in households and communities and by the opportunities it may afford children for schooling and human capital investment. However, these effects remain difficult to measure and to predict, since fostering hosting and sending decisions and their impacts are endogenous at both the household and child levels. Previous work by economists as well as other social scientists has attempted to describe the potential outcomes and, in particular, the benefits that fostered children may experience, even into adulthood.

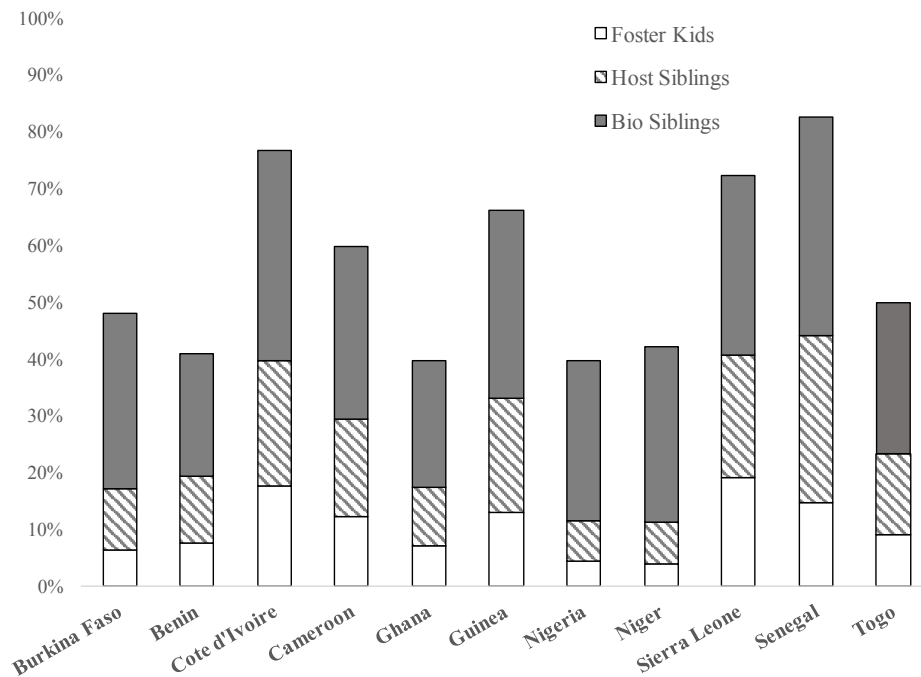
In this paper, I contribute to this line of research by providing cross-country evidence from across the region on the continued prevalence of the practice of fostering and previously unreported stylized facts: the reality of living as a foster child seems to be completely different for girls than for boys. Across the region, foster girls work more than all other children conditional on their age and they are less likely to be enrolled in school, while my evidence suggests that foster boys may fare better than most other children in some countries. Statements of causality as well as those calling for bans on the practice of fostering should be made cautiously, because the true counterfactual enrollment and work and more general well-being of foster children is unknowable.

Figure 3.1: Prevalence of Fostering by Country



The figure shows the proportion of boys and girls in each country that are living in a household that contains neither parent, but both parents are still alive. Source: Author's calculations from DHS data.

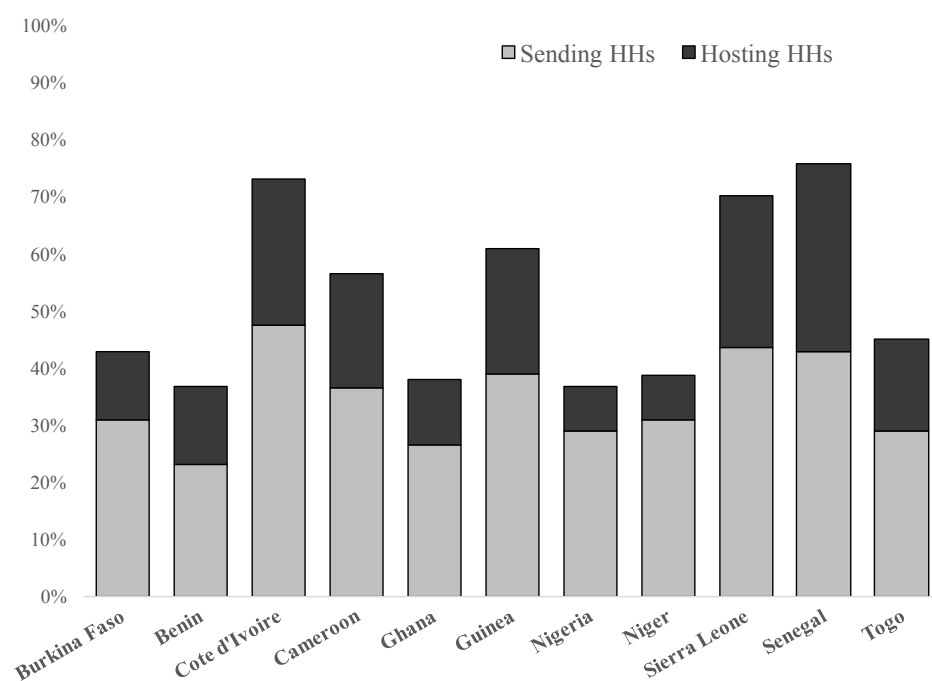
Figure 3.2: Proportion of Children Involved by Country



The figure shows the proportion of children in each country that are potentially affected by the practice of fostering: foster children themselves, as well as the children they live with (host siblings) and children whose siblings have gone to live elsewhere (biological siblings). Source:

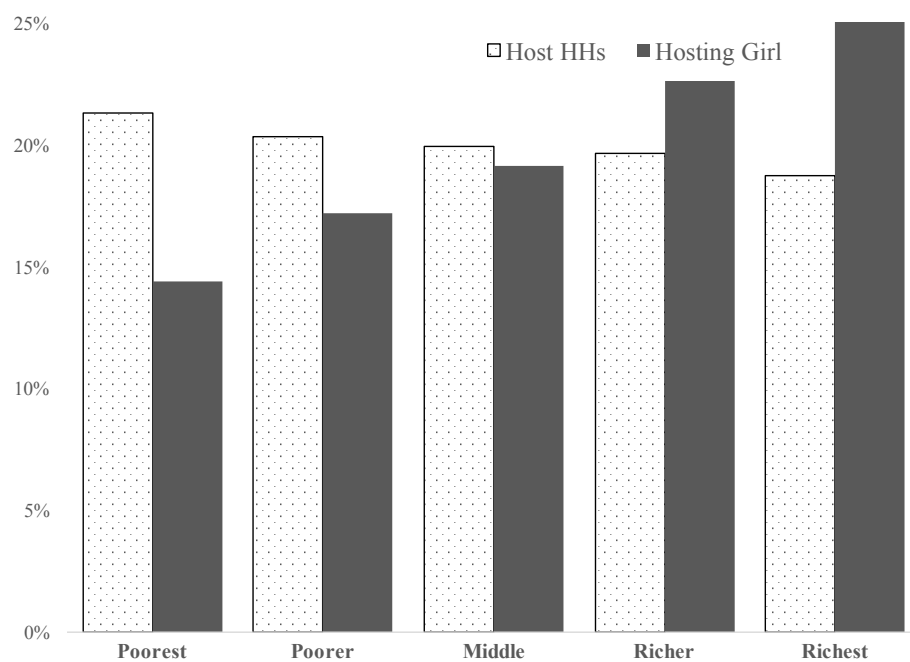
Author's calculations from DHS data.

Figure 3.3: Proportion of Households Involved by Country



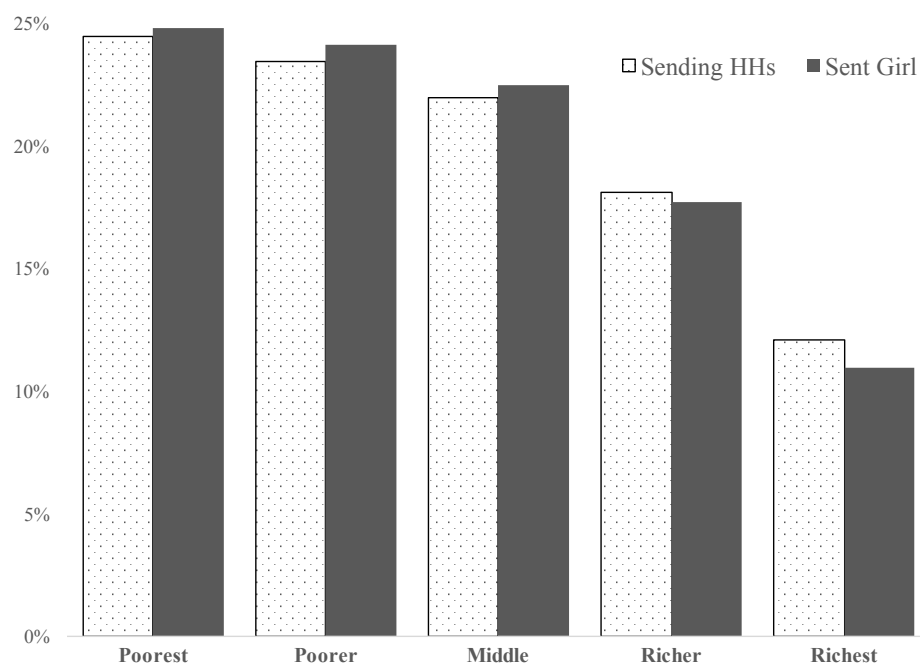
Source: Author's calculations from DHS data.

Figure 3.4: Income Distribution of Host Households



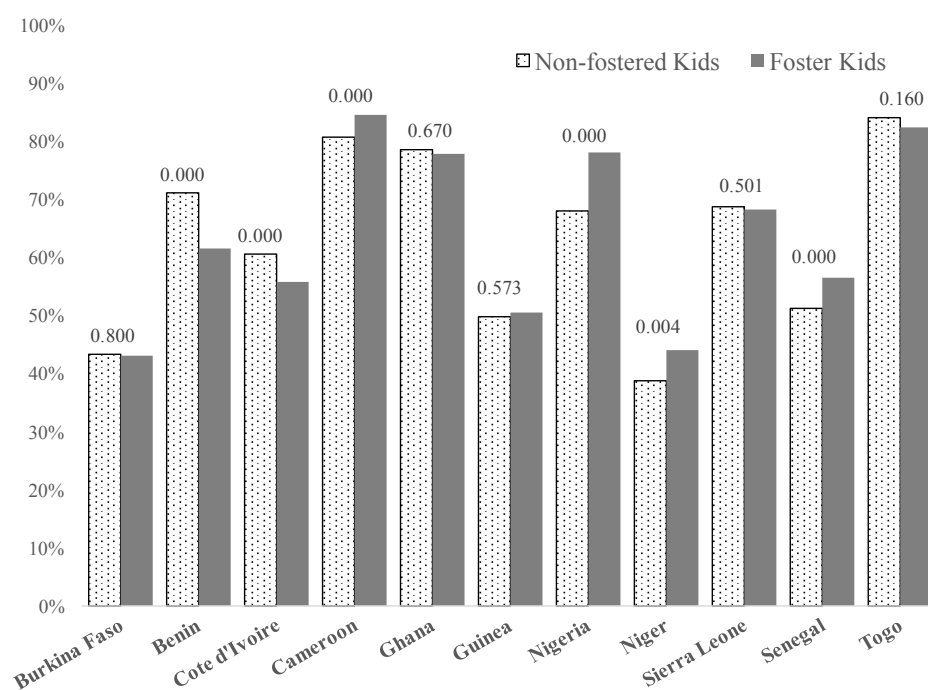
Source: Author's calculations from DHS data.

Figure 3.5: Income Distribution of Sending Households



Source: Author's calculations from DHS data.

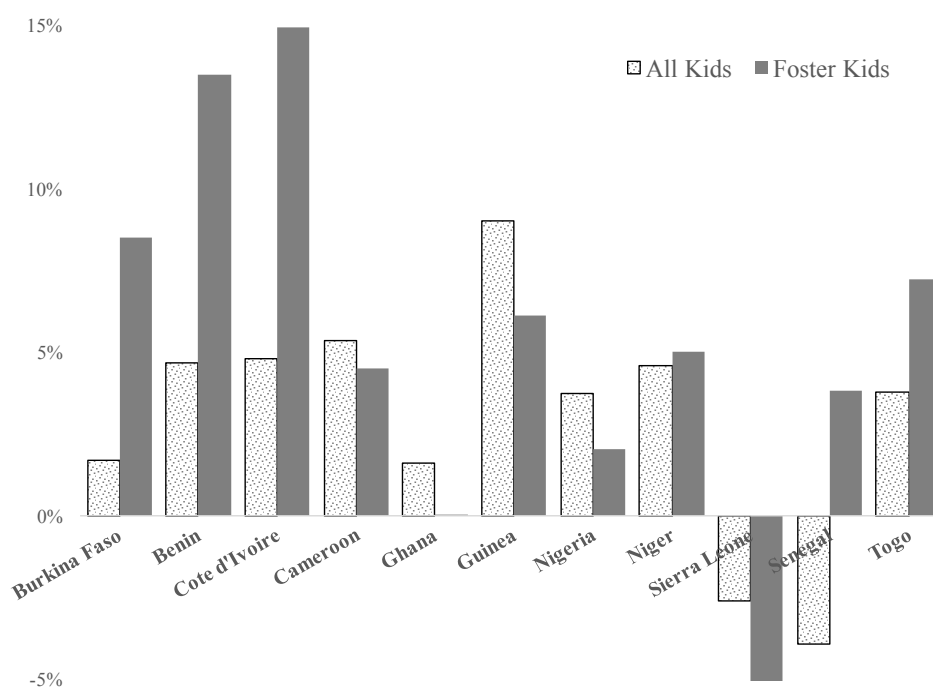
Figure 3.6: School Attendance by Country and Foster Status



The figure shows the overall average school attendance rates for non-fostered children and for fostered children. P-values for the t-test of the difference are reported above the columns.

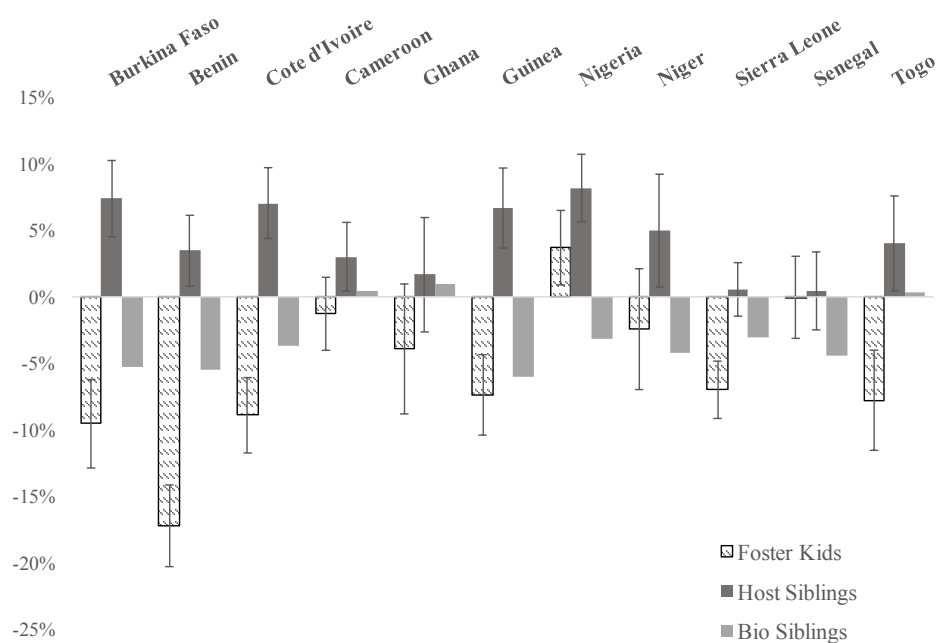
Source: Author's calculations from DHS data.

Figure 3.7:
Boy - Girl School Attendance by Country and Foster Status



The figure shows the raw gender differences (boys minus girls) in school attendance rates for all children and for fostered children. Source: Author's calculations from DHS data.

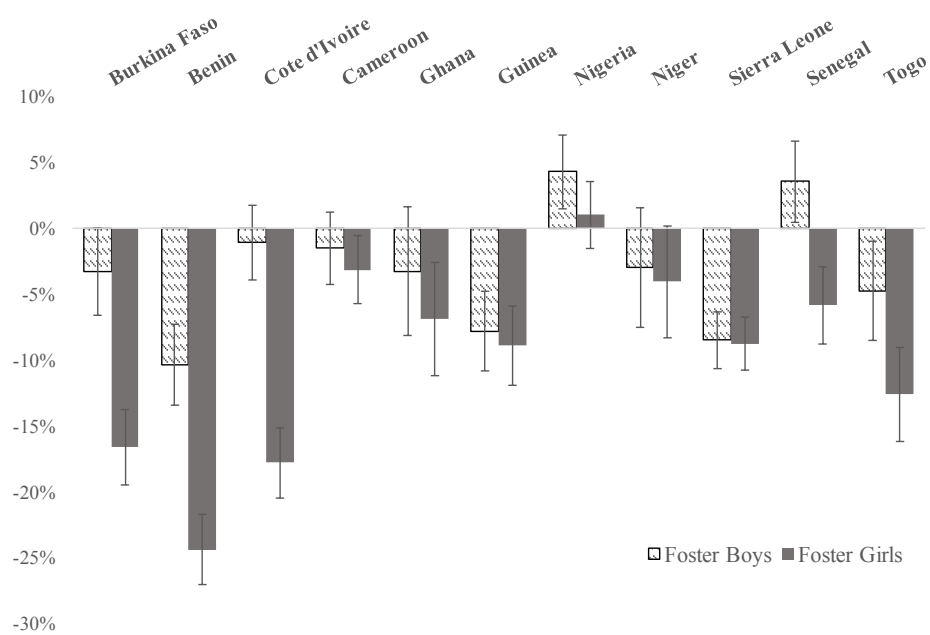
Figure 3.8: School Attendance by Country and Child Status



The figure shows the regression adjusted school attendance by country and child status.

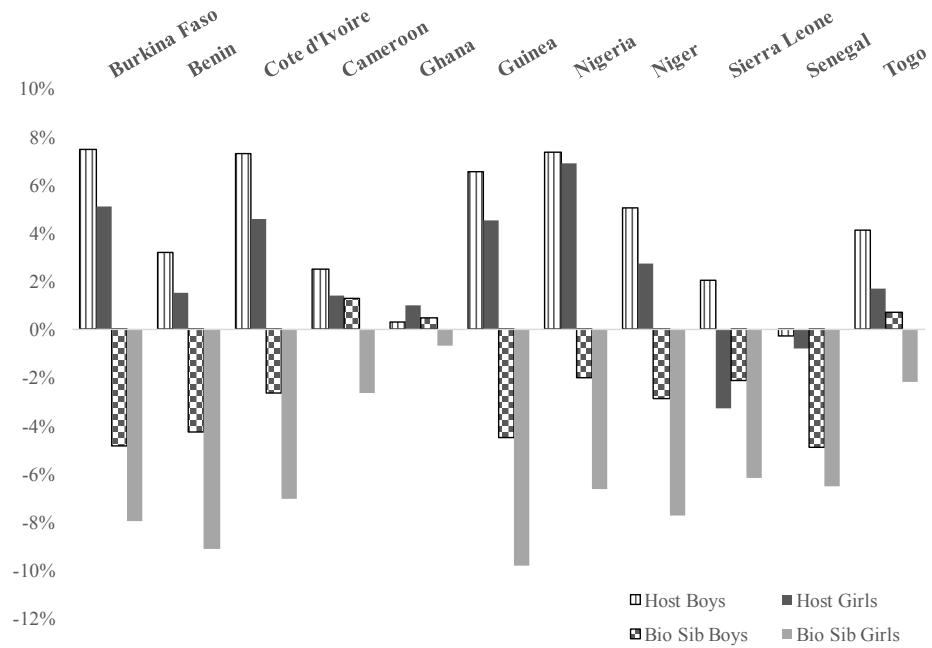
Source: Author's calculations from DHS data.

Figure 3.9: School Attendance for Foster Boys and Girls



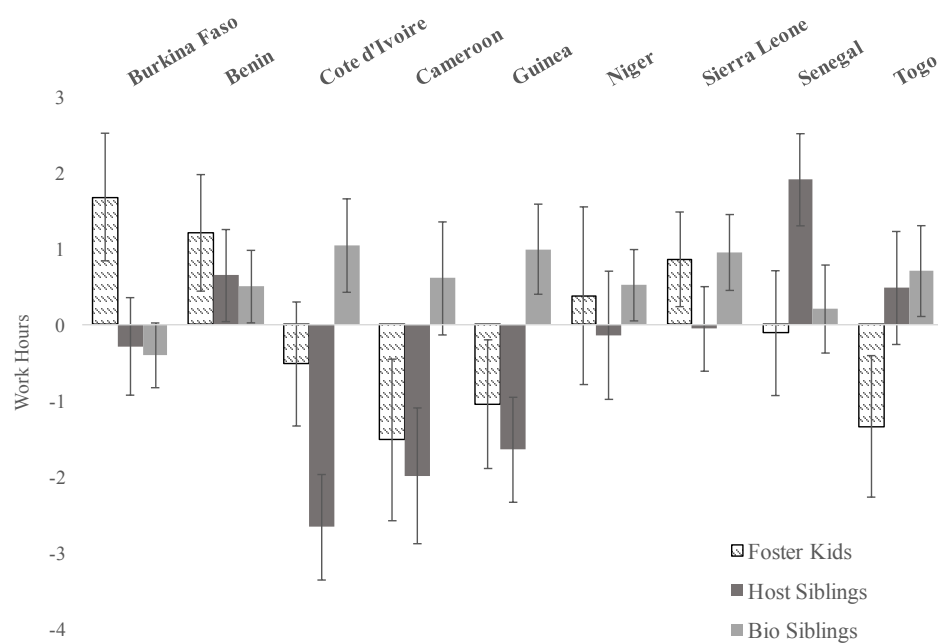
The figure shows the regression adjusted school attendance by country for foster boys and girls. Source: Author's calculations from DHS data.

Figure 3.10: School Attendance for Other Boys and Girls



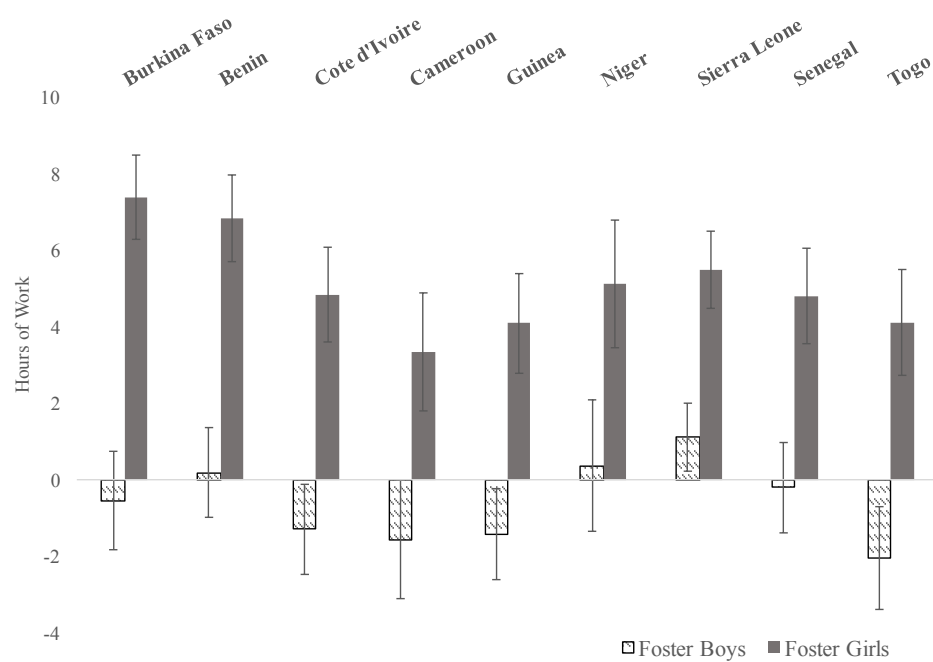
The figure shows the regression adjusted school attendance by country for host and bio sibling boys and girls. Source: Author's calculations from DHS data.

Figure 3.11: Hours Worked by Country and Child Status



The figure shows the regression adjusted hours worked (including zero) by country and child status. Work questions are missing from the Ghana and Senegal data, so only nine countries are shown. Source: Author's calculations from DHS data.

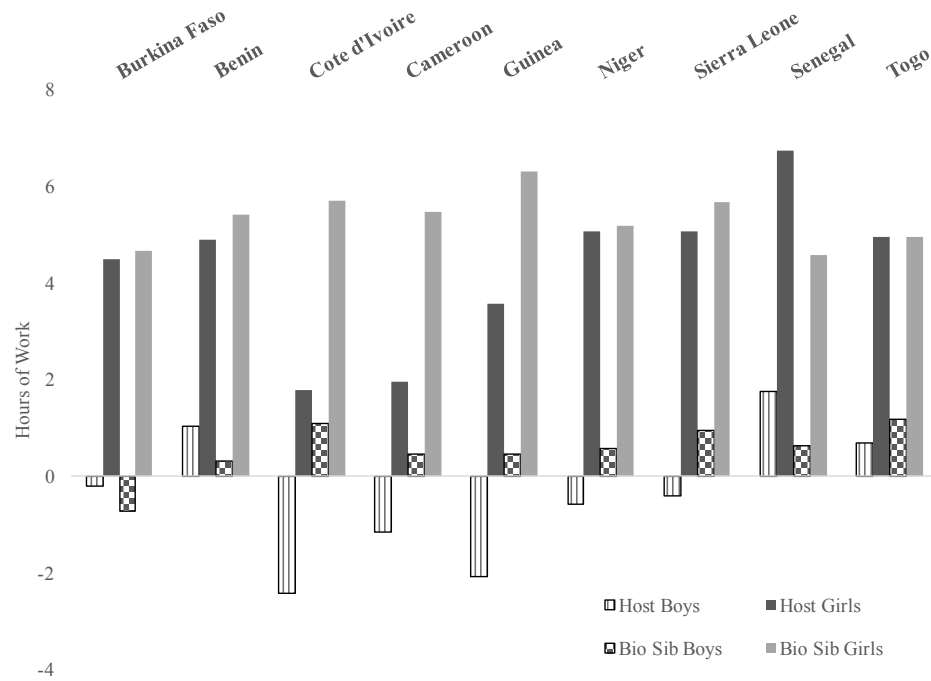
Figure 3.12: Hours Worked for Foster Boys and Girls



The figure shows the regression adjusted hours worked by country for foster boys and girls.

Source: Author's calculations from DHS data.

Figure 3.13: Hours Worked for Other Boys and Girls



The figure shows the regression hours worked by country for host and bio sibling boys and girls. Source: Author's calculations from DHS data.

Table 3.1: Sample Sizes by Country

Country	Number of Children
Burkina Faso	23,538
Benin	23,288
Cote d'Ivoire	11,540
Cameroon	17,055
Ghana	9,593
Guinea	12,403
Nigeria	45,446
Niger	18,629
Sierra Leone	18,919
Senegal	11,291
Togo	11,945

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