ABSTRACT

Title of Dissertation: INHERITANCE REFORM, FEMALE EMPOWERMENT, AND INTERGENERATIONAL EFFECTS: THEORY AND EVIDENCE FROM INDIA

Fabliha Ibnat
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Dissertation Directed by: Professor Kenneth Leonard
Department of Agricultural and Resource Economics

Land ownership is an important determinant of intra-household bargaining power in low-income countries, yet women are systematically barred from inheriting land. Granting equal access to land tenuring has the potential to improve women's ability to make decisions within the household, particularly regarding their children. This dissertation examines the effect of women's land inheritance rights on fertility and child mortality in India. I explore this topic theoretically and empirically in three main chapters.

In the first chapter I develop a household bargaining model in which granting mothers inheritance rights may affect child mortality and fertility through a direct land channel and an indirect human capital channel. The model shows how an exogenous change in inheritance rights decreases fertility and has an ambiguous effect on child mortality for some households due to two competing effects. One is an empowerment effect that results from an increase to women's bargaining power and reduces child mortality. The second is an income effect that increases child

mortality and results from an increase in the pooled unearned income of the household. Which effect dominates is an empirical question.

In the second chapter I empirically estimate the effect of the reforms as they operate through each channel using quasi-random variation from a natural experiment in which four Indian states enacted equal rights for women to inherit joint family property between 1986 and 1994. I construct difference-in-differences estimators using variation in eligibility across marriage cohorts and religions. Using retrospective life history and fertility history data, hazard model estimates show that the reforms reduced child mortality through the land channel and reduced fertility through the human capital channel. Children with eligible mothers have a 57% lower hazard of dying before age five. Eligible women are more likely to delay their first birth and have a 32% lower hazard of having more than two children. The results correspond to 344,169 children who were saved between the reform passage years and 2005, the survey collection year.

In the third chapter I use a different dataset to identify the specific subset of households for which the theoretical model generates an ambiguous prediction. I directly test the prediction using an event study difference-in-differences model that exploits variation in eligibility across states and multiple pre- and post-reform marriage cohorts. I find that household level child mortality decreases by 2.2 percentage points, indicating that the empowerment effect dominates the income effect.

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by

Fabliha Ibnat

Dissertation submitted to the Faculty of the Graduate School of the
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Advisory Committee:
Professor Kenneth Leonard, Chair
Professor Louis Preonas
Professor Jing Cai
Professor Cory Smith
Professor Susan Parker
Professor Reeve Vanneman

Dedication

For my grandmothers, Rafiun Nessa and Rahima Khatun.

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Chapter 1: Introduction

1.1 Dissertation overview

Land is one of the most valuable forms of property in low income countries and often one of the few productive assets owned by the rural poor (Barbier & Hochard, 2018). Inheritance is a significant means of acquiring land, yet legal frameworks bar women from inheritance, not only depriving them of a source of wealth and social capital,[1] but also of a critical outside resource that can mitigate economic vulnerability in the event of divorce, separation, or widowhood (World Bank, 2021). The intra-household bargaining literature conceptualizes such assets as important determinants of women's bargaining power, and there is empirical evidence that the level of bargaining power affects women's ability to make decisions within the household, particularly regarding their children (Doss, 2006; Duflo, 2003; Quisumbing & Maluccio, 2003; Schultz, 2001; Thomas, 1990). Thus, the gender imbalance in land ownership may have important implications for intra-household bargaining, women's decision making, and child well-being.

However, the land rights literature largely estimates the effects of land tenuring at the household level, ignoring the possibility that land in the hands of women may have very different implications for welfare than the pooled land ownership of households (Banerjee et al., 2002;

[1] In the South Asian context, women's land ownership is associated with social capital in the form of better treatment in their marital home, improved social standing in their community, and increased decision-making within local social and political institutions (Agarwal, 1995; Kelkar & Krishnaraj, 2020).

Besley, 1995; Bhalotra et al., 2019; Congdon Fors et al., 2019; Field, 2007; Galiani & Schargrodsky, 2010; Goldstein & Udry, 2008). This dissertation is among the first studies to demonstrate an intergenerational link between gender parity in land inheritance rights, female empowerment, and demographic outcomes. I explore this topic theoretically and empirically in Chapters 2-4.

In Chapter 2, I motivate my analysis using an intra-household Nash bargaining model of fertility and children's health investment decisions adapted from Eswaran (2002), in which children ensure economic security for parents in old age. Parents face a trade off between consumption in the present, which is reduced by health investments in children, and consumption in the future, which is higher if more children survive and remit a fraction of their income to their parents. Although both parents benefit equally from children, mothers bear disproportionately higher costs from having children. As a result, mothers prefer fewer children and higher health investments per child than fathers, resulting in lower child mortality.

An exogenous change in women's inheritance rights may affect these outcomes by improving bargaining power[2] through two main channels: a land channel and a human capital channel. The land channel manifests through the right to inherit land itself, and may affect two types of households in different ways. In one type of household, the mother experiences an increase in land inheritance and the father experiences a decrease in land inheritance because he has sisters who are able to inherit land. The household's pooled unearned income remains the same, and the only real change is a shock to the mother's bargaining power. The model predicts that fertility and child mortality will decrease for these households. In the second type of household, only the mother experiences an increase in land inheritance and the father is unaffected. In this case, there

[2]I use the terms "bargaining power" and "empowerment" interchangeably throughout this paper. Both refer to power within the household.

are two competing effects. The shock to the mother's unearned income increases her bargaining power, causing investments in children to increase through an "empowerment effect". At the same time, land inheritance increases the total pooled household unearned income, resulting in an "income effect" that causes investments in children to decrease. Which effect will dominate is an empirical question. The human capital channel manifests through the improvements in education that may result from gaining the right to inherit at a young age.[3] The model predicts decreases in both child mortality and fertility through this channel.

This chapter makes two main contributions. First, it formally establishes women's empowerment induced by land rights as an important factor in the demographic transition of developing countries from high to low fertility and child mortality. It also generates a set of testable predictions for how land inheritance rights may affect demographic outcomes through two distinct channels, which helps guide the empirical analyses in Chapters 3 and 4.

In Chapter 3, I test the theoretical predictions using quasi-experimental variation in women's bargaining power induced by land inheritance reforms in India. I exploit India's Hindu Succession Act Amendments (HSAA), a series of reforms that required fathers to bequeath joint family property equally to sons and previously excluded daughters. I focus on four states that passed these reforms: Andhra Pradesh in 1986, Tamil Nadu in 1989, Maharashtra in 1994, and Karnataka in 1994. The reforms only applied to women who were unmarried at the time of the reform, whose fathers were alive at the time of the reform, and who were Hindu, Buddhist, Sikh, or Jain.

I construct retrospective right-censored child-level life history and mother-level fertility history datasets from data collected in 2005. I estimate a Cox-proportional hazards model and

[3] There is evidence that improved land inheritance rights results in increased education for women (Bose & Das, 2017; Deininger et al., 2013; Harari, 2019; Roy, 2015) and an increase in female labor force participation (Heath & Tan, 2019).

construct a difference-in-differences estimator using variation in eligibility across unmarried vs. married cohorts and eligible vs non-eligible religions to isolate the effect of the reforms through the land channel. I use variation in younger vs. older cohorts and eligible vs. non-eligible religions to estimate the effect through the human capital channel. A necessary assumption is that any non-HSAA driven differences between the two cohorts in each analysis must be the same for eligible religions and non-eligible religions. To interpret my effects as causal, I invoke parallel trends across religions in counterfactual child health and fertility outcomes and provide strong evidence that pre-HSAA trends were parallel.

I find that child mortality decreases through the land channel and fertility decreases through the human capital channel. Children who have a mother eligible to inherit land have a 57% lower hazard of dying before age five. The improvement is driven by a decrease in mortality for boys, and sex-ratios at birth remain the same. These mothers are more likely to have a say in how to spend their own earnings and in decisions regarding daily purchases. Examining the human capital channel, I find that women who were younger than age six when the amendment passed, and therefore had parents who made human capital investment decisions in response to the amendment throughout their childhood, have lower fertility than women who were already adults when the amendment passed. I find that they are more likely to delay their first birth, and if they already have children, have a 32% lower hazard of having more than two. This reduction is driven by women who have already had at least one son; women with only daughters remain just as likely to have more than two children, which is consistent with strong son preferences in India. I find that these women have more education as well as a higher probability of labor force participation, providing some evidence for the human capital mechanism.

This chapter makes three key contributions. First, it presents one of the first studies to causally identify women's land inheritance rights as an important driver of demographic change. Existing studies on the HSAA focus on the reform's direct effects on the eligible women themselves and find that they have higher labor force participation (Heath & Tan, 2019), lower rates of poverty in old age (Calvi, 2020), and higher levels of education (Bose & Das, 2017; Deininger et al., 2013; Roy, 2015). This chapter adds to this literature by showing that women are able to leverage these human capital gains as increased bargaining power within the household, and exert more influence over fertility and child health investment decisions. Second, it is also the first study to distinguish between the two channels that land rights may operate through. Third, the results from the empirical analysis in this chapter allow me to calculate the first estimate of the number of children who were saved as a result of these reforms.

In Chapter 4, I conduct a deeper investigation of the underlying mechanism for the significant effect found in Chapter 3. I use a second dataset that allows me to observe whether fathers experienced disinheritance due to having HSAA-eligible sisters. This information allows me to directly test the ambiguous prediction on child mortality generated by the theoretical model and determine whether the empowerment effect or income effect dominates in the case when mothers experience inheritance and fathers are unaffected. I restrict the data to this sample and estimate an event study difference-in-differences model using variation in eligibility across multiple pre- and post-amendment marriage cohorts and reform versus non-reform states. I find that child mortality decreases by between 2 to 3.8 percentage points for children whose mothers were eligible for the reform, indicating that the empowerment effect dominates the income effect.

It is possible that a part of the effects I identify may result from a marriage market channel, in which the inheritance reforms cause the conditions of women's marriages to be more favorable for child well-being. Although I am unable to disentangle this channel from the land and human capital channels, I use this dataset to explore whether it may be influencing my results by examining the effect of the reforms on husband characteristics, marriage characteristics, and women's dowry. I find that the reforms do not affect husband and marriage characteristics, but do increase women's dowries, which may improve their decision making ability and status in their marital household. This indicates that the effects I identify are inclusive of a marriage market mechanism.

This chapter makes two main contributions. First, it presents the first analysis of these reforms that observes and holds constant the effect of fathers' disinheritance. Existing papers that examine intergenerational effects in terms of children's education (Bose & Das, 2017) and short term health outcomes (Calvi et al., 2019; Hossain & Nikolov, 2021) are not able to account for fathers' disinheritance. Second, it identifies a possible marriage market channel that is operating alongside the land channel. This highlights an interesting consequence of the HSAA policy: it results in improved bargaining power for women through mechanisms beyond the land itself. Even if households are able to circumvent the law, the results in this chapter suggest that parents may be compensating daughters by increasing their dowry, leading to improved decision making and intergenerational effects. Overall, this chapter provides a richer understanding of the underlying mechanisms at play in the effects that are observed in Chapter 3.

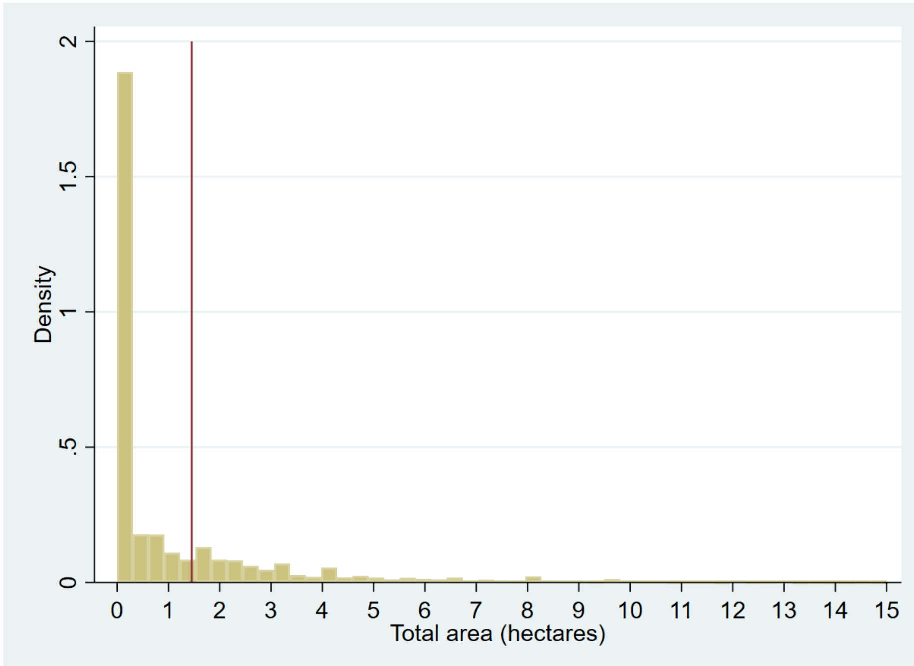
I find that the results in Chapters 3 and 4 are robust to falsification tests as well as various restrictions of the estimation sample to account for different sources of potential bias. Taken together, the results imply that the right to inherit ancestral property and the bargaining power it brings is a crucial determinant of child mortality and fertility decisions. Both reductions are aspects of the optimal strategy of more empowered women to ensure security in old age for themselves and their spouses. The reduction in the hazard of death amounts to .4% of live births saved in the reform states, resulting in 6,567 lives saved per year in Andhra Pradesh, 4,777 lives per year in Karnataka, 8,374 per year in Maharashtra, and 46,71 per year in Tamil Nadu between each state's reform passage year and 2005, the survey collection year. This totals about 344,169 children in reform states who were saved in that time frame. The results imply a reduction in under-five mortality in reform states from about 63 deaths per 1000 live births to 59 deaths. For comparison, India's national under-five mortality rate fell from 142 deaths per 1000 live births to 75 deaths between 1986, the year of the first reform, and 2005.

1.2 Land ownership in India

To give some context for the research question explored in this dissertation, I outline the prevalence of land ownership in India and characterize landed households around 2005, the year the data used in this study were collected. Overall, 98% of households report owning some form of land, and although the average land holding size is very small, it is still valuable as it accounts for a large percentage of household wealth.

Figure 1.1 shows the distribution of landholding size for households, using a nationally representative sample of Indian households from the 2003/04 round of India's National Sample Survey (NSS) data. Of all the households in this data, 98% report owning some form of land, including both urban homesteads and agricultural land. Both types of land are relevant in the context of this study, as they can provide women with a place to go outside of marriage or with a wealth creating resource. The figure shows the distribution for 99% of households, who all report owning less than 15 hectares of land. Only 1% of the population own land larger than 15 hectares. The majority of households, 85%, own less than 3 hectares of land. The vertical line indicates that the average household owns 1.45 hectares of land.

Figure 1.1: Distribution of household-level land



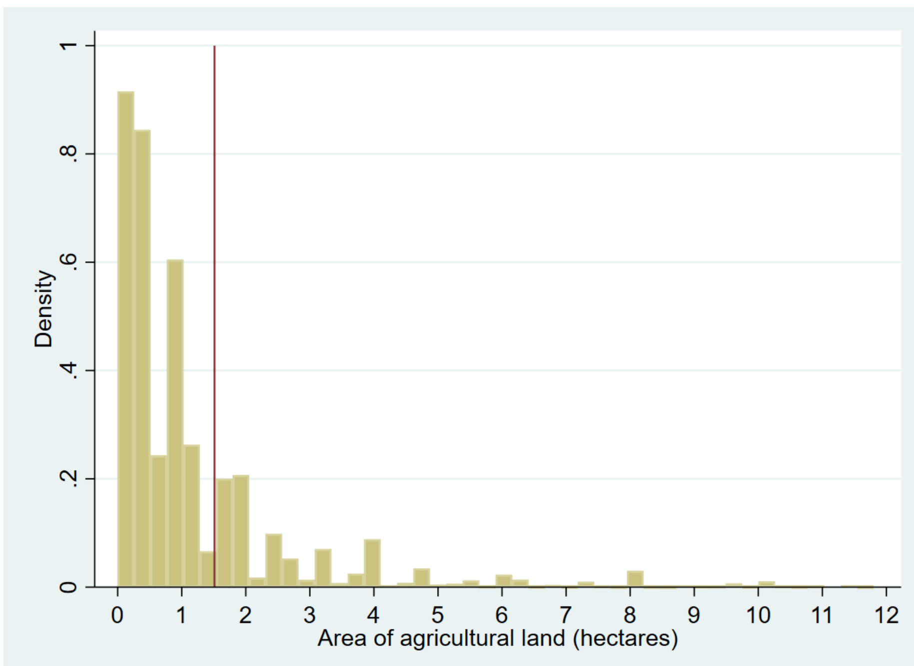
Sample comes from the 2003/04 NSS data. For ease of interpretation, the histogram shows the land distribution of 99% of households, who all report owning less than 15 hectares of land. The vertical line indicates the average household's total land.

Columns 1 and 2 in Table 1.1 describe these households in terms of their caste and urban/rural make-up as well as the average amount of land each subgroup holds. The average household from a Scheduled Caste, which is one of the most historically disadvantaged groups in India and accounts for 17% of the population, owns the smallest amount of land. Rural households, which are 64% of the sample, hold on average more land than urban households: 2.08 hectares compared to .35 hectares.

Although all types of land are relevant for this study, Figure 1.2 shows the distribution of landholding size for households that report owning agricultural land, which account for 52% of the sample. I use the nationally representative sample of households from the 2005 round of the

Demographic and Health Survey (DHS) data, which is used for the analysis in Chapter 3. The figure shows the distribution for 99% of households who report having agricultural land, all of whom own 12 hectares or less. Only 1% of these households own agricultural land greater than 12 hectares. As is the case for the full sample, the majority of households, 89%, own less than 3 hectares of land, which is sufficient for subsistence farming. The vertical line indicates that the average household owns 1.5 hectares of agricultural land.

Figure 1.2: Distribution of household-level agricultural land



Sample comes from the 2005 DHS data. For ease of interpretation, the histogram shows the land distribution of 99% of households, who all report owning less than 12 hectares of agricultural land. The vertical line indicates the average household's total agricultural land.

Columns 3 and 4 in Table 1.1 characterize households that own agricultural land in terms of their caste, location, and wealth index. Like the households in the full sample, Schedule Caste households own the least amount of land on average, and the most historically privileged

General Class owns the most. However, the representation of castes is generally preserved among agricultural land-owning households compared to the general population. Scheduled Castes, Other Backwards Castes, and the General Class are slightly underrepresented, and Scheduled Tribes are over-represented. Of all the households that own agricultural land, 78% are rural and 22% are urban, and rural households own on average more land. Finally, the households in the poorest, poorer, and middle quintiles of the wealth index are over-represented relative to the total population,[4] indicating that agricultural land is a particularly important resource for the bottom 60% of the population. However, households in the wealthier indices own more land on average.

Taken together, these statistics show that the majority of households own some form of land, but the households with the most land on average are rural and of a higher caste and wealth index. Agricultural land is a particularly important resource for the rural poor, though average land holdings increase by wealth quintile. Although land ownership is extremely unequal in India and the average household owns a small amount of land, it is important to note that land is still an extremely valuable resource for households in terms of its value as a percentage of total household wealth. Land accounts for 60% of total wealth for the wealthiest 10% of the population, 55% for the middle 40% of the population, and 40% for the bottom 50% of the population (Bharti, 2019). Land is the most important asset for the average household, indicating that the land inheritance reforms likely had a large impact on households across the wealth distribution in India.

[4]In the full sample, 13% of households are classified as "poorest", 15% as "poorer", 19% are "middle", 23% are "richer", and 29% are "richest".

Table 1.1: Characteristics of landed households

	2003/04 NSS data (all households, 98% have land)		2005 DHS data (households with agricultural land)	
	(1)	(2)	(3)	(4)
	% of sample	Average land area (hectares)	% of sample	Average land area (hectares)
Caste				
Scheduled tribe	11.04	1.62	19.36	1.86
Scheduled caste	16.67	0.72	13.4	0.85
Other backwards class	36.95	1.55	34.66	1.44
General class	35.34	1.7	32.08	1.74
Location				
Rural	63.72	2.08	77.76	2.01
Urban	36.28	0.35	22.24	1.37
Wealth index				
Poorest			17.42	0.84
Poorer			19.52	1.07
Middle			22.13	1.36
Richer			21.47	1.66
Richest			19.46	2.57
N	82,160	82,160	43,892	43,892

Columns 1 and 2 show the percentage of each sub-group and the average size of their landholdings respectively for a nationally representative sample of households from the NSS, 98% of whom report owning some form of land. Columns 3 and 5 show the percentage of each sub-group and the average size of their agricultural land holdings respectively among all households who reporting having agricultural land. These households are 52% of the total sample. The wealth index, available only in the DHS data, is a composite measure of a household's cumulative living standard and is calculated using data on a household's ownership of selected assets. This score is used to rank households by wealth, and the population is then subdivided into five quintiles to produce a relative indicator of socio-economic status within the country.

Chapter 2: Theory

2.1 Introduction

I motivate the empirical analysis using a household bargaining model of fertility and children's health investment decisions adapted from Eswaran (2002). In Eswaran's original model, couples are motivated to have children for security in old age. This is a salient factor in India, where incomplete credit markets and a patchwork of social security schemes makes the number of children (and the sex composition) a crucial determinant of comfort and security in old age.[1] (Lamb, 2013) Decisions regarding fertility and allocation of resources towards children's healthcare are determined by the relative bargaining powers of the mother and father. The more health investments are made, the lower the chances of child mortality. A key feature of this model is that the costs associated with having children are distributed disproportionately between mothers and fathers, with mothers bearing a larger share of the burden due to the vast number of illnesses and health problems associated with childbirth that reduce the quality of life for the mother, particularly in a developing country setting like India (Geller et al., 2018). In the context of this study, men exercise a disproportionate degree of control over family decisions relative to the extent to which they bear the costs of these decisions. Thus, there is a fundamental externality

[1] In reality there are of course many different reasons why couples choose to have children. However, these are additive to the model and do not interact with land rights.

within the family which renders fertility and child mortality excessive relative to what would be deemed optimal when the interests of the mother and the father are weighted equally.

I modify Eswaran's model in two key ways. First, the original model conceptualizes parents' income as joint income. I make a distinction between earned and unearned income for the mother and the father, which allows me to generate predictions for the two possible channels that women's land inheritance rights may operate through to improve bargaining power: a land channel and a human capital channel. Second, the original model assumes that all surviving children of a couple will provide old age support regardless of gender and that parents have no preference for children of a given sex. However, in India, old age security is expected mostly from sons. Therefore, I relax this assumption and model remittances from sons as higher than from daughters.

2.2 Basic model

In the basic model, each parent maximizes utility over two periods. In period 1, they decide on the number of children to have (n), the health investment per boy (h_b), and the health investment per girl (h_g). I assume parents have full control over the choice of n, and take it to be a continuous variable, as in Eswaran (2002) and Cigno (1998). In period 2, the parents are in old age and rely on income from their surviving children. The probability that children survive into adulthood is determined by the amount of health investments parents made in period 1. Thus, there is a trade-off between consumption in the present and consumption in the future. Equations 2.1 and 2.2 show the utility functions of the husband and the wife respectively.

$$U^H(n, h_g, h_b) = U_1(c_1) + U_2(c_2) \tag{2.1}$$

$$U^W(n, h_g, h_b) = [U_1(c_1) + U_2(c_2)]D(n) \tag{2.2}$$

$D(n)$ gives the maternal depletion effect, which is decreasing in n and reduces the wife's utility. I assume here that $D(0) = 1$, $D(n) > 0$, and $D'(n) < 0$. Thus, the wife's utility is simply an attenuated version of the husband's utility. Consumption in periods 1 and 2 are given by c_1 and c_2, which I assume to be jointly consumed and non-rivalrous to simplify the analysis as in Eswaran (2002).[2]

The utility functions are subject to a budget constraint for period 1 and a constraint for period 2:

$$c_1 \leq \underbrace{y^H + y^W}_{\text{earned income}} + \underbrace{r_1^H + r_1^W}_{\text{unearned income}} - \underbrace{\theta n(h_b + x)}_{\text{total investment in boys}} - \underbrace{(1-\theta)n(h_g + x)}_{\text{total investment in girls}} \tag{2.3}$$

$$c_2 \leq \underbrace{r_2^H + r_2^W}_{\text{unearned income}} + \underbrace{\alpha_b \theta n q(h_b)}_{\text{total income from surviving boys}} + \underbrace{\alpha_g (1-\theta) n q(h_g)}_{\text{total income from surviving girls}} \tag{2.4}$$

In period 1, parents decide on levels of n, h_g, and h_b and, as is shown in equation 2.3, consume the sum of their combined earned and unearned incomes minus the total amount they choose to investment in boys' health and girls' health. The share of sons in the household is exogenously given by θ, thus θn gives the total number of boys and $(1-\theta)n$ gives the total number of girls. It is assumed the household pools its income. The exogenous parameter x represents non-discretionary spending on each child's health that parents always invest, such as the mother's forgone wages during child-rearing. Discretionary health expenditures per boy and

[2]This obviates the need to model how the resources available for the couple's own consumption is split between the husband and the wife.

per girl, two key choice variables, are given by h_b and h_g respectively.

Equation 2.4 shows that in period 2, parents do not have any earned income and consume the sum of their period 2 unearned income as well as the income remitted to them from surviving boys and surviving girls. The probabilities of survival, $q(h_b)$ and $q(h_g)$, are increasing in health investments and concave. Thus, child mortality is, to an extent, endogenous in this model. I assume children have a positive probability of surviving even if health investments are zero (i.e. $q(0) > 0$).

The expected remittance per surviving girl, given by α_g, is assumed to be lower than the expected remittance per surviving boy, given by α_b. Thus, I assume that $0 < \alpha_g < \alpha_b$. This captures a key feature of Indian society, in which parents rely more on boys for income in old age, due to both economic and cultural factors.[3] I assume no saving or borrowing between periods, no-sex selective abortion, and full commitment by parents.

2.3 Equilibrium in the extreme case

I first derive a set of propositions under the extreme case, in which all decision making power is vested with either the father or the mother. If this is the case, he/she would choose fertility and health care expenditures per child to maximize (1)/(2). Propositions 1.a - 1.c can be derived for this scenario. They closely follow Proposition 1 in Eswaran (2002) and are extended to include results stemming from gender differences in children's income remittance rates.

PROPOSITION 1.A. *At the optimum of either parent, fertility and healthcare expenditure per child are perceived as substitutes, i.e.* $\frac{\partial h_g^*}{\partial n} < 0$ *and* $\frac{\partial h_b^*}{\partial n} < 0$. *Since* $\alpha_g < \alpha_b$, *the elasticity for*

[3]In fact, it is possible that if a household's daughter's dowry at the time of her marriage is large enough, α_g will become negative. In this analysis however, I keep this parameter positive.

girls is larger.

Proof. See Appendix A.

Health care expenditures safeguard future consumption at the expense of present consumption. An increase in fertility lowers the marginal utility of healthcare expenditure. Thus, fertility and health expenditures per child are perceived to be (imperfect) substitutes in the provision of a more secure future: if one is parametrically increased, the other decreases. Because $\alpha_g < \alpha_b$, the marginal benefit from a boy's health investment decreases faster with h_b than does that from a girl's health investment. Therefore, the adjustment in h necessary to restore equilibrium is less for boys than for girls.

PROPOSITION 1.B. *The optimal fertility from the wife's perspective is less than that from the husband's perspective. The optimal health care expenditure from the wife's perspective is greater than that from the husband's perspective.*

Proof. See Appendix A.

This follows immediately from the fact that although the benefits to higher fertility are equally shared by both parents, the costs to the mother increase disproportionately. Since the father bears a negligible share of the cost of bearing children, he would prefer to have more children than would the mother. Furthermore, he would prefer to allocate less resources than the mother would for the healthcare of these children because the costs of higher fertility in anticipation of child mortality are largely borne by the mother.

PROPOSITION 1.C. *Investments in girls will be less than investments in boys for either parent's perspective.*

Proof. See Appendix A.

This follows from the first order conditions, which show that parents will invest in the health of boys and girls up to the point at which the marginal benefit of boy's health investments equals the marginal benefit of girl's health investments:

$$U_2'\alpha_b q'(h_b) = U_2'\alpha_g q'(h_g).$$

If $\alpha_g < \alpha_b$, the left hand side is greater than the right hand side when evaluated at the same levels of h_b and h_g. Since the marginal benefit functions are decreasing in h_b and h_g, this equation is satisfied at a point at which $h_b > h_g$.

2.4 Impacts of a bargaining power shock through inheritance reform

Next, I consider the general case, where decisions are the outcome of bargaining and given by the solution to the following Nash bargaining problem:

$$\max_{n,h_g,h_b} U^T = [U^W(n, h_g, h_b) - \bar{U}^W]^\gamma [U^H(n, h_g, h_b) - \bar{U}^H]^{(1-\gamma)} \tag{2.5}$$

subject to

$$c_1 \leq (y^W + y^H) + (r_1^W + r_1^H) - \theta n(h_b + x) - (1-\theta)n(h_g + x)$$

$$c_2 \leq (r_2^W + r_2^H) + \alpha_b \theta n q(h_b) + \alpha_g (1-\theta) n q(h_g)$$

This problem is maximizing a weighted average of parent's utilities, with weights given by γ and $1-\gamma$. There are two ways to conceptualize bargaining power in this model. One way is by using the weights, which determine the household's optimal allocation from the set of possible solutions along the Pareto frontier. Bargaining power is also captured by the relative threat points of the wife and the husband, given by $\bar{U}^W$ and $\bar{U}^H$, which determine the set of possible solutions. In this model, I take the threat points to be the utilities of each spouse outside of the marriage, in which $n = 0$, $h_g = 0$, and $h_b = 0$ for each individual. These can be expressed as indirect utility functions that depend solely on the individual earned and unearned income of each parent: $\bar{U}^W = V^W(y^W, r_1^W, r_2^W)$ and $\bar{U}^H = V^H(y^H, r_1^H, r_2^H)$. Therefore, optimal fertility and healthcare expenditure per child will in general depend on the relative bargaining powers of the mother and the father: $n^*(\gamma, \bar{U}^W, \bar{U}^H), h_g^*(\gamma, \bar{U}^W, \bar{U}^H), h_b^*(\gamma, \bar{U}^W, \bar{U}^H)$.

Inheritance reforms can exogenously change women's bargaining power through two channels: a direct land channel and an indirect human capital channel. Sections 2.4.1 and 2.4.2 develop a set of testable predictions for each of these channels.

2.4.1 Direct land channel

First, the land channel manifests through the direct effect of having the right to inherit land itself. Inheritance is triggered at the time of the woman's father's death, which is more likely to happen later in her life (in period 2 rather than period 1). Therefore, I conceptualize this channel as a shock to r_2^W, women's unearned income in period 2, which changes her threat point $\bar{U}^W$. Although the land itself is bequeathed in period 2, households affected by the reforms make maximization decisions in anticipation of land, even if they have not earned it yet. Proposition 2 shows how a change in the wife's threat point through the land channel changes fertility and health investment decisions in two different cases.[4]

Proposition 2.a considers an increase in the wife's unearned income when husband's unearned

[4] The results I find generally hold for changes in γ as well, but they are not the focus of this chapter since it makes more sense to conceptualize increased land as an increase in unearned income rather than an increase in the wife's Pareto weight.

income, r_2^H, remains constant. In this case, the household's total pooled unearned income in period 2, $r_2^H + r_2^W$, increases. This is consistent with the impact of the HSAA on households whose treatment status is defined by the wife's exposure.

PROPOSITION 2.A. *When the threat point of the mother in the household increases through an increase in r_2^W, keeping r_2^H constant, the couple's fertility decreases ($\frac{\partial n}{\partial r_2^W} < 0$) and effects on investments in boys and girls is ambiguous.*

Proof. See Appendix A.

The ambiguous effect on health investments is driven by two competing effects. On the one hand, the mother's threat point $\bar{U}^W$ has increased due to an increase in her r_2^W, allowing her to invest more in children and reduce child mortality as is in line with her preferences. I call this the "empowerment effect". On the other hand, the total pooled household unearned income in period 2 has also increased, giving parents more income in old age and causing them to become less reliant on their children. Parents will want to smooth this increased future income by transferring it to the present. Since the model assumes parents cannot borrow against future income, they instead reduce expenditure on children in period 1 while increasing spending on own consumption c_1.[5] This "income effect" causes investments in children to decrease. The direction of change in the resulting child mortality depends on which effect will dominate, which is an empirical question

Besides increasing the wife's unearned income, the HSAA could also decrease the husband's unearned income if he has sisters who also gained inheritance rights. Proposition 2.b considers an increase in wife's period 2 unearned income with an equivalent decrease in the husband's period

[5]There is empirical evidence that parents use children as a means for smoothing consumption in India and a variety of other contexts (Kim & Prskawetz, 2010).

2 unearned income, so that the household's total unearned income remains the same. This is consistent with the impact of the HSAA on the society overall. The total amount of land remains the same and the HSAA changes the allocation rule.

PROPOSITION 2.B. *When the threat point of the mother in the household increases through an increase in r_2^W, accompanied by an equivalent decrease in r_2^H so $r_2^H + r_2^W$ remains constant, the couple's fertility decreases ($\frac{\partial n}{\partial r_2^W} < 0$) and health investments in boys and girls increases ($\frac{\partial h_b}{\partial r_2^W} > 0$ and $\frac{\partial h_g}{\partial r_2^W} > 0$).*

Proof. See Appendix A.

Since the household's pooled unearned income is constant, there is no income effect. The empowerment effect predicts an increase in investments and reduction in fertility. The elasticity for investment in girls should be larger than that for boys.

2.4.2 Indirect human capital channel

The second channel that the HSAA can operate through is the human capital channel. This channel manifests through the improvements in education that may result from gaining the right to inherit at a young age. Women who were very young when the amendment passed in their state grow up with parents who made different education and marriage decisions for their daughters, all of which later influence her bargaining power within her marriage beyond the land she can inherit.[6] Evidence shows that this may manifest as increased labor force participation within the marriage (Heath & Tan, 2019). Therefore, I conceptualize this channel as a shock to y^W, women's earned income in period 1, which changes her threat point $\bar{U}^W$. Proposition 3 considers

[6] For example, previous studies find that the HSAA increases girls' level of education (Bose & Das, 2017; Deininger et al., 2013; Roy, 2015) and increases their age at marriage (Deininger et al., 2013).

the effect of this change.

PROPOSITION 3. *When the threat point of the mother in the household increases through an increase in y^W, the couple's fertility decreases ($\frac{\partial n}{\partial y^W} < 0$) and and health investments in boys and girls increases ($\frac{\partial h_b}{\partial r_2^W} > 0$ and $\frac{\partial h_g}{\partial r_2^W} > 0$).*

Proof. See Appendix A.

An increase in women's earned income carries with it two effects, each moving fertility and child mortality in the same direction. As in Proposition 2, there is an empowerment effect, which decreases fertility and child mortality. There is also an earned income effect, which is different from the income effect in Proposition 2 in that it is an increase in income due to the rising value of women's time. This not only expands household income but also raises the effective price of children to the family. It is empirically observed that higher values of women's time are associated with lower levels of fertility, implying that the price effect of women's wages outweighs its income effect on fertility (Averett et al., 2018). The opportunity cost of children to parents has increased. The increase in earned income will also either decrease child mortality or cause it to remain unchanged.

2.5 Conclusion

In general, Propositions 2 and 3 show the differences in the strategies that are optimal for mothers and fathers in arranging for old age security. Mothers and fathers prefer to trade off quantity of children versus quality of children differently: mothers put more emphasis on quality, and fathers more on quantity. As noted by Eswaran (2002), this result formally establishes the claim that the empowerment of women is a factor that could hasten the demographic transition in developing countries. Table 2.1 summarizes the main testable predictions from Propositions 2 and 3.[7]

Table 2.1: Testable predictions

	r_2^W increase, r_2^H constant	r_2^W increase, r_2^H+r_2^W constant	y^W increase
n	decrease	decrease	decrease
h_g	ambiguous	increase	increase
h_b	ambiguous	increase	increase

[7]There is a third case in which the husband experiences disinheritance and the wife is unaffected. This is possible if the couple married pre-HSAA but the husband has sisters who married post-HSAA. I do not model this effect since it does not directly change the wife's absolute bargaining power.

Chapter 3: Empirically estimating the effect of inheritance rights

3.1 Introduction

This chapter uses the 2005/06 round of India Demographic and Health Survey data to empirically test whether land inheritance affects fertility and child mortality through the two main channels outlined in the theoretical model in Chapter 2. I first discuss the details of the Hindu Succession Act Amendments, which I use as a natural experiment to estimate the impact of land inheritance. I then use hazard models to disentangle the two channels, and find that the reforms decrease child mortality through the direct land channel and decrease fertility through the indirect human capital channel. The results are robust to a series of falsification tests, and to specifications using restricted samples that remove various sources of potential bias. This chapter makes three key contributions: it establishes that a causal effect exists, it disentangles the effect as it operates through two main channels, and the results allow me to provide the first estimate of the number of children who were saved as a result of these reforms.

3.2 Background on the Hindu Succession Act Amendments

Under India's Hindu Succession Act of 1956, sons, but not daughters, had a direct birth right to inherit joint family property (e.g. ancestral land). Daughters could inherit ancestral property from their fathers or late husbands only in the absence of male heirs. However, five Indian states subsequently enacted amendments to explicitly grant equal inheritance rights for daughters: Kerala in 1976, Andhra Pradesh in 1986, Tamil Nadu in 1989, Maharashtra in 1994, and Karnataka in 1994. These reforms only applied to women who were not yet married at the time of the reform; whose fathers were alive at the time the amendments were passed; and who were Hindu, Buddhist, Sikh or Jain.[1] I exclude Kerala from my analysis because it has various castes and tribes that historically followed matrilineal kinship systems, leaving me with four reform states.

Table 3.1 shows the percentage of women in each major religious group by reform state. About 85% of women are of an eligible religion in the four reform states, comprising mostly Hindus, and about 15% of women are of a non-eligible religion, comprising mostly Muslims and Christians. The inheritance rights of Muslim women, who form the majority (75%) of women in non-eligible religions, are governed by the Muslim Personal Law (Shariat) Application Act of 1937, which strongly favors male heirs. Under this law, women can inherit land only if there are no male heirs and sons inherit double the share of the daughters if they inherit jointly. The inheritance of Indian Christians (who represent 24% of women in non-eligible religions) has traditionally been governed by either local customary law or English law (Khan, 2000).

Approximately 86% of India's arable land is privately owned (Agarwal, 2003), and of this privately owned land, joint property is the vast majority (97%) of total property (Roy, 2015). Therefore, laws governing inheritance of joint family property are extremely important determinants of asset ownership. The Hindu Succession Act Amendments (HSAA) had large

[1] Currently, India does not have a uniform civil code and distinguishes personal law from public law. Personal law covers marriage, divorce, inheritance, adoption, and alimony and is based on the scriptures and customs of each major religious community in India.

Table 3.1: Mothers' religion breakdown by state (%)

	Maharashtra	Andhra Pradesh	Karnataka	Tamil Nadu	Total
HSAA Eligible					
Hindu	76.0	76.9	86.6	88.6	81.2
Buddhist	8.7	0.0	0.0	0.0	2.7
Jain	1.4	0.0	0.6	0.2	0.6
Sikh	0.3	0.0	0.0	0.0	0.1
Not HSAA Eligible					
Muslim	12.0	18.1	10.0	4.2	11.5
Christian	1.4	5.0	2.5	7.0	3.7
Other	0.2	0.0	0.3	0.0	0.1
Total	100.0	100.0	100.0	100.0	100.0
N	(6807)	(5599)	(4725)	(4608)	(21739)

Data: 2005 India DHS. Religion breakdown by state of ever married women between ages 15-49. The "Other" category includes women who reported being Jewish, Parsi/Zoroastrian, or no religion.

effects on women's ability to inherit land: they caused the proportion of eligible women who have inherited land to increase by 15 percentage points relative to the baseline level of 6 percent in non-reform states (Deininger et al., 2013). Importantly, the reforms ensure that joint family property devolves to all children by birthright, and fathers cannot disinherit daughters through a will. However, it is possible for families to circumvent the law by pressuring daughters to voluntarily sign away their rights or by preventing them from having any real access or control of land even if it is in their name. In 2005, the amendment was ratified nationally and applied to all women whose fathers were still alive at the time of ratification, regardless of marital status.

3.3 Data

I use the 2005/2006 round of India Demographic and Health Survey (DHS) data to obtain the results for this analysis, which relies on within-state variation. The data contains 21,750 ever married women ages 15-49 from the reform states, with information on their marriage date and religion, which I use to proxy for eligibility. The key advantage of the DHS data is that it contains records for each child from the full birth history for each ever married woman, with 51,784 children in the four reform states. I use this information to construct a right-censored child-level retrospective life history dataset. The dataset contains each child's lifespan and a dummy variable that equals one if the lifespan ended before age five and zero if it ended as a censored observation. For children under five who survived past the date the data was collected, we know that their lifespan is above their reported age, but it is unknown by how much. I also use the detailed birth history information to construct a mother-level retrospective fertility history dataset. The dataset contains the time between each birth and a dummy variable that equals one if the duration spell was ended by a new birth and zero if it ended as a censored observation.

Panel A of Table 3.2 summarizes the average values for mother-level variables by eligible and non-eligible religion women in reform states. Eligible women have a significantly higher under-five mortality rate and a lower average number of children than non-eligible women. Non-eligible women also tend to have significantly more education and a higher age at marriage. Panel B of summarizes the relevant child-level characteristics. The higher mortality and lower fertility among children of eligible religion mothers is again reflected in the higher percentage of children who died before age five and in the lower average birth order.

Table 3.2: DHS data descriptive statistics

	Eligible Religion	Non-Eligible Religion	Difference
Panel A: Mother-level variables			
Under-five mortality (%)	0.06	0.04	-0.02***
	(0.15)	(0.12)	(0.00)
Number of children	2.32	2.71	0.39***
	(1.45)	(1.82)	(0.00)
Years of education	5.66	6.36	0.70***
	(5.10)	(4.82)	(0.00)
Age at marriage	17.63	17.96	0.33***
	(3.87)	(3.93)	(0.00)
Age at first birth	19.50	19.62	0.11
	(3.85)	(3.92)	(0.14)
Current age	32.46	32.20	-0.27
	(8.40)	(8.36)	(0.09)
Percent male children	0.53	0.52	-0.01
	(0.34)	(0.33)	(0.06)
Scheduled caste (%)	0.20	0.09	-0.11***
	(0.40)	(0.28)	(0.00)
Scheduled tribe (%)	0.07	0.01	-0.06***
	(0.25)	(0.08)	(0.00)
Other backward class (%)	0.49	0.37	-0.12***
	(0.50)	(0.48)	(0.00)
General class (%)	0.23	0.51	0.28***
	(0.42)	(0.50)	(0.00)
Rural (%)	0.62	0.34	-0.28***
	(0.50)	(0.41)	(0.00)
Spousal age gap	6.55	6.33	-0.22**
	(4.24)	(4.26)	(0.01)
Spousal education gap	1.56	0.99	-0.57***
	(4.04)	(4.07)	(0.00)
Has say in daily purchases (%)	0.69	0.67	-0.01
	(0.46)	(0.47)	(0.14)
Has say in how to spend own earnings (%)	0.80	0.87	0.07***
	(0.40)	(0.34)	(0.00)
Worked outside the home in the last year (%)	0.49	0.30	-0.19***
	(0.50)	(0.46)	(0.00)
N	18401	3349	21750
Panel B: Child-level variables			
Died before age five (%)	0.07	0.05	-0.03***
	(0.26)	(0.22)	(0.00)
Percent female	0.48	0.49	0.00
	(0.50)	(0.50)	(0.41)
Child age	12.55	12.09	-0.45***
	(7.77)	(7.53)	(0.00)
Birth order	2.12	2.46	0.35***
	(1.25)	(1.57)	(0.00)
N	42701	9083	51784

Columns 1 and 2 list average values for all variables with the standard deviation in parenthesis. Column 3 gives the difference between the two groups with the p-value from the t-test of means in parenthesis.

3.4 Empirical strategy

The DHS life history data allows me to go beyond a basic household level measure of child mortality and use Cox proportional hazards models[2] at the child level to handle right censoring and evaluate the effect of the amendment on the probability of death. Although the data does not report measures of health investments per child (h_g and h_b in the theoretical model), the predictions for how bargaining power may change h_g and h_b extend to how bargaining power changes child mortality, as mortality is decreasing in investments. Therefore, I use the child level life history data to examine the effect of having an HSAA eligible mother on child mortality directly.

I restrict the analysis to children within the four reform states only. I use variation in eligibility by mother's marital status at the time the amendment passed in her state[3] and mother's religion to construct a difference-in-differences (DID) estimator similar to Duflo (2001). Note that this strategy estimates the effect of the legal right to inherit land, and not the effect of actually inheriting land itself. In addition, since I do not observe the dates of death for the women's fathers, and the reforms only apply to women whose fathers were still alive at the time the amendment passed in their state, some untreated women will be incorrectly defined as treated in my specification. This will bias estimates towards zero. Sections 3.4.1 and 3.4.2 outline the specifications I use to isolate the effect of the HSAA as it operates through the two channels discussed in Chapter 2: the land channel and the human capital channel.

[2] Schoenfeld residual tests for proportional hazards hold.

[3] Note that while I observe the state the woman lives in in 2005, I do not observe the state she lived in with her natal family before marriage. However migration upon marriage rarely occurs between states. In the 2001 Census of India, only 0.9 percent of women were interstate migrants (Castaldo et al., 2012). I cannot fully rule out that some amount of selective migration contributes to the results, but the impact is likely small.

3.4.1 Impact of the HSAA through the land channel

To isolate the direct effect of the land channel, which is the right to inherit itself, I only include children whose mothers were over the age of 18 in the unmarried treatment group. These women were not young enough for human capital decisions to be made by parents under exposure to the HSAA, ruling out the human capital channel. The following Cox-proportional hazards model[4] estimates the effect of the HSAA through the land channel:

$$
\begin{aligned}
ln(h_{isjr}(t)) = {} & \alpha(t) + \beta_1 EligibleReligion_r + \beta_2 Unmarried_{sj} \\
& + \beta_3 Unmarried_{sj} \times EligibleReligion_r + \beta_4' X_{isjr} + \gamma_s + \epsilon_{isjr}, \qquad (3.1)
\end{aligned}
$$

where $h_{isjr}(t)$ is the probability that child i with a mother in state s, of religion r, and marriage year j dies in month t conditional on the lifetime lasting to month t. The baseline hazard function, $\alpha(t) = ln(h_0(t))$, is left unspecified and fluctuates over time. $Unmarried_{sj}$ is a dummy variable that equals 1 if the child's mother was unmarried and at least 18 years old during the year the reform passed in her state and 0 if she was married at least one year before the reform passed in her state. $EligibleReligion_r$ is a dummy variable that equals 1 if the mother was in an eligible religion and 0 otherwise. X_{isjr} is a vector of controls including child's sex, birth order, sex composition of siblings at time of birth, mother's age, mother's age at first birth, caste, a rural/urban dummy, parents' age gap, and parents' education gap. Finally, γ_s controls for state.

[4] Clotfelter et al. (2008) use a similar DID framework within a Cox model. In Appendix Figure B.1, I investigate whether using a DID framework in a non-linear model is problematic in my case by reconstructing the hazard function by estimating sixty linear probability models of the probability of dying for children at months $t = 0$ to $t = 60$ conditional on having been alive up to month t. This pins down the hazard function without imposing any structure between each month t. I find a significantly negative DID effect on the cumulative probability of dying before age 5, as is presented in Table B.1. Therefore, although my main empirical strategy uses DID in a non-linear model, I find that the results from such models are empirically similar to those from a more flexible framework.

The parameter of interest, β_3, represents the differential hazard (mortality) rate of children whose mothers were both an eligible religion and unmarried at the time of the reform. This is the DID estimate of the impact of the ability to inherit land on the mortality hazard. To focus on child mortality, the latest that a subject can be in the pool and still at risk is five years. This model achieves a "like to like" comparison between eligible religion women who were unmarried and women who were married at the time of the reform under the assumption that same comparison for non-eligible religion women captures and nets out any non-HSAA driven differences between the two cohorts of women.

I use a model identical to equation 3.1 to evaluate the effect of the HSAA on women's fertility through the land channel. I construct three separate hazard functions for women who have had no births, one birth, and two births.[5] In general, factors that affect the transition from having never given birth to giving birth for the first time may not play any role in the transition from the first birth to the second birth, or the transition from the second birth to the third birth. (Miranda & Trivedi, 2020). Therefore, estimating separate hazard functions by birth number accounts for the fact that dynamics are an essential feature of how a fertility history is generated, and allows me to pinpoint which birth transitions are influenced more strongly by the HSAA. In these models, $h_{isjr}(t)$ is the probability that woman i in state s with marriage year j of religion r gives birth to an additional child and therefore transitions from having given birth x times to $x + 1$ times at a given instant t, given that birth $x + 1$ has not yet happened. I estimate three such models for births $x = 0, 1, 2$. The vector of controls X_{isjr} includes mother's age, mother's age at first birth, caste, a rural/urban dummy, spousal age gap, spousal education gap, and sex

[5]"Births" is defined as the number of times a woman has given birth to a fetus with a gestational age of 24 weeks or more, regardless of whether the child was born alive or was stillborn.

composition of previous children. The parameter of interest, β_3, gives the impact of the ability to inherit land on the hazard of having an additional birth.

In addition to estimating the effect of the ability to inherit land on mortality and fertility, I also estimate a logistic model version of equation 3.1 in which the dependent variables are two binary measures of women's decision making ability within the marital household. One is a variable for whether the woman is able to make decisions about her own earnings, and another is a variable for whether women are able to make decisions about daily purchases within the household. These specifications allow me to test whether improved decision making via the ability to inherit land is in fact a key mechanism driving the fertility and mortality results.

3.4.2 Impact of the HSAA through the human capital channel

Next, to isolate the effect of the HSAA on mortality as it operates through the human capital channel, I compare children with mothers who were unmarried and younger than age 6 when the HSAA passed in their state to children with mothers who were unmarried and older than 18 when the HSAA passed in their state. This comparison rules out the land channel because both groups of mothers are eligible for land, but the former group of mothers were young enough for her parents to make schooling, health, and other investment decisions after the amendment passed.[6] I compare this difference in children with eligible religion mothers to the difference in children

[6]The average age of mothers in the *young* group at the time the amendment passed in their state is 3 and the average age of mothers not in this group is 21.

with non-eligible religion mothers in the following regression:

$$
\begin{aligned}
ln(h_{isjr}(t)) = \alpha(t) + \lambda_1 EligibleReligion_r + \lambda_2 Young_{sj} \\
+ \lambda_3 Young_{sj} \times EligibleReligion_r + \lambda_4' X_{isjr} + \gamma_s + \epsilon_{isjr},
\end{aligned} \tag{3.2}
$$

where $h_{isjr}(t), \alpha(t)$, $EligibleReligion_r$, and X_{isjr} are defined as before, with j indexing the mother's birth year rather than marriage year. $Young_{sj}$ is a dummy that equals 1 if the mother was unmarried and younger than 6 years in the year the amendment was passed in her state, and equals 0 if she was unmarried and older than 18 years in the year the amendment was passed in her state. The parameter of interest, λ_3, gives the impact of human capital gains on the mortality hazard. This model achieves a ”like to like” comparison between eligible religion women who were young and eligible religion women who were older at the time of the reform under the assumption that same comparison for non-eligible religion women captures and nets out any non-HSAA driven differences between the two cohorts of women.

I use a model identical to equation 3.2 to evaluate the effect of the HSAA on women's fertility outcomes as they operate through the human capital channel. I again construct separate hazard functions for women who have had 0, 1, and 2 births. In these models, $h_{isjr}(t)$ and X_{isjr} are defined as they were in the previous fertility model and the parameter of interest, λ_3, gives the impact of human capital gains on the hazard of having an additional birth.

In addition to estimating the effect of long-term exposure to the reforms on mortality and fertility, I estimate an OLS model version of equation 3.2 in which the dependent variables are age at marriage and years of education. I also estimate a logistic version with a binary variable for whether the woman worked outside of the home in the last 12 months as the dependent variable.

These outcomes allow me to measure whether improved human capital, in the form of a later age at marriage, improved education, and subsequent higher likelihood of working outside of the home, is indeed the mechanism driving the mortality and fertility results.

3.5 Validity of identification

3.5.1 Common trends

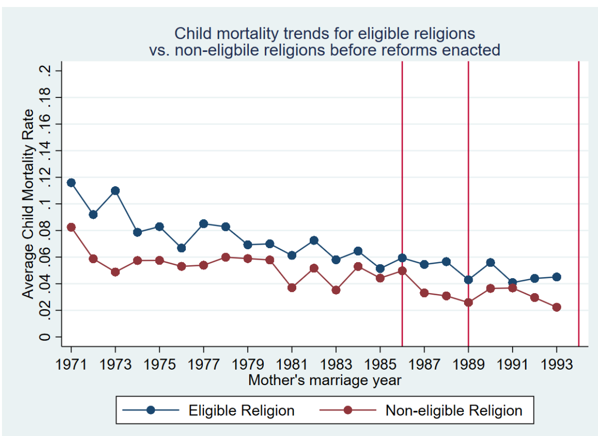
To estimate an unbiased DID effect, the evolution of mortality and fertility outcomes must be the same for eligible religions and non-eligible religions before the amendments were enacted. To give a sense of these pre-reform trends, Figure 3.1 shows the trajectories of child mortality and fertility for eligible and non-eligible women using only those who were married before the amendment passed in their state. This data includes years between 1971 and 1994, and drops any women who were married after the amendment passed in their state. Therefore, all women who were married between 1971 and 1986 are included, women from Andhra Pradesh who were married after 1986 are dropped, women from Tamil Nadu who were married after 1989 are dropped, and any women married after 1994 are dropped.[7] Panel A shows that the children of eligible religion women consistently have higher mortality rates than children of non-eligible women, which is driven by lower mortality rates among Muslims compared to Hindus.[8] This trend works in favor of my identification, as Hindus comprise the majority of individuals in the eligible religion group, and are starting at a higher level of child mortality at baseline. Both

[7] Hossain & Nikolov (2021) use a similar approach to show common trends.

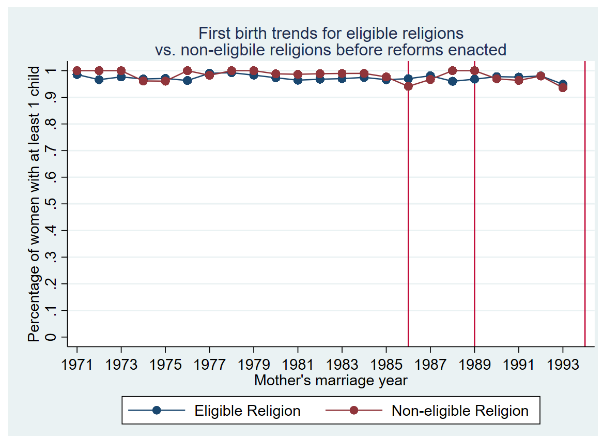
[8] This trend is well documented in the literature and known as the "puzzle" of Muslim advantage in child survival in India. Although the socioeconomic status of Indian Muslims is, on average, considerably lower than that of Hindus, Muslims nevertheless exhibit substantially higher child survival rates and have for decades (Bhalotra et al., 2010).

religion groups trend downward over time with some divergence from year to year. Panels B and C show that the trends in the probability of having first and second births are very similar between the two groups. Panel D shows that both groups follow a general downward trend in the probability of having a third birth, though there is some divergence in the trajectories from year to year.

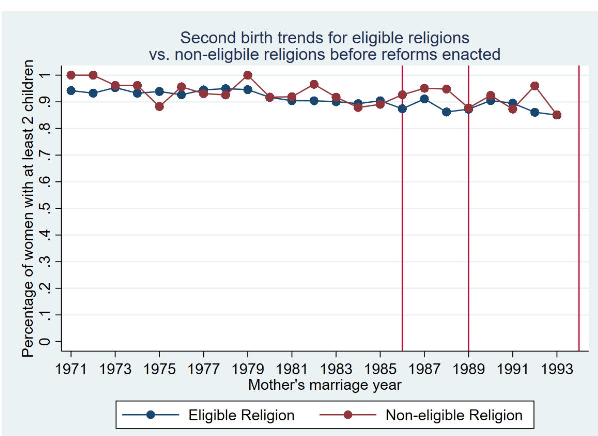
Figure 3.1: Pre-amendment trends in child mortality and fertility



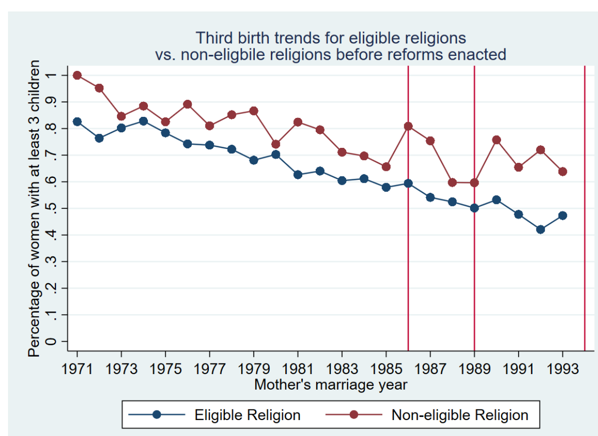
A



B



C



D

Sample is restricted to pre-reform observations, i.e. women who were married before the reforms passed in their state. Vertical lines at 1986, 1989, and 1994 indicate the year the HSAA passed in Andhra Pradesh, Tamil Nadu, and Maharashtra & Karnatika respectively.

I formally test for common trends in mortality and fertility by restricting the data to women who were married prior to the amendment passage year in their state and regressing the outcomes of interest on the interaction between a linear trend of the mother's year of marriage and an indicator variable for being in an eligible religion:

$$
\begin{aligned}
ln(h_{isjr}(t)) = \alpha(t) + \delta_1 EligibleReligion_r + \delta_2 MarriageCohort_{sj} \\
+ \delta_3 MarriageCohort_{sj} \times EligibleReligion_r + \delta_4' X_{isjr} + \gamma_s + \epsilon_{isjr},
\end{aligned}
\tag{3.3}
$$

where $h_{isjr}(t)$ is the mortality hazard for child i born to a mother in state s, of religion r, who married in year j. For fertility models, $h_{isjr}(t)$ is the hazard of having one more child for woman i in state s, who married in year j and is religion r. The variable $MarriageCohort$ is a linear trend of the mother's year of marriage, which is defined relative to the year the reform passed in their state. X_{isjr} includes the same vector of mother and household-level controls used in equation 3.1. The coefficient δ_3 captures the differential trend in the outcomes between eligible religions and non-eligible religions prior to the HSAA.

To test for common trends in the human capital channel specification, I estimate a similar model in which I restrict the data to women who were born at least 18 years before the reform passed in their state, and therefore were too old to be exposed to any changes in human capital investments due to the reforms. I regress the outcomes of interest on the interaction between the mother's age at the time the amendment passed in her state and a dummy for whether she is in an eligible religion.

$$ln(h_{isjr}(t)) = \alpha(t) + \mu_1 EligibleReligion_r + \mu_2 AgeAtAmendment_{sj}$$
$$+ \mu_3 AgeAtAmendment_{sj} \times EligibleReligion_r + \mu_4' X_{isjr} + \gamma_s + \epsilon_{isjr}, \quad (3.4)$$

where $h_{isjr}(t)$ and X_{isjr} are defined as in equation 3.3, with j indexing the mother's birth year rather than marriage year. The variable $AgeAtAmendment$ is a linear trend of the mother's age at the time of the reform year and the coefficient μ_3 captures the differential trend in the outcomes between eligible and non-eligible religions prior to among observations not exposed to human capital investments induced by the HSAA. The results are presented in Table 3.3, where Panel A reports coefficients from equation 3.3 and Panel B corresponds to equation 3.4. The hazard ratios are not significantly different from 1, implying that there were no differential trends in outcomes between eligible religions and non-eligible religions prior to the amendments.

Table C.8 in the Appendix reports results from similar specifications using key mediating variables as the outcomes of interest. Specifically, it reports the odds ratios from a logistic version of equation 3.3 for measures of women's decision making in the household, key variables that mediate mortality and fertility outcomes through the land channel. It also reports the odds ratios from a logistic version of equation 3.4 for an indicator of whether the woman worked outside of the home in the last 12 months, and coefficients from an OLS version of the same equation for women's years of education and age at marriage, the key mediating variables that influence demographic outcomes through the human capital channel. All coefficients are statistically insignificant, providing further evidence for common trends.

Table 3.3: Test of common trends in mortality and fertility outcomes

	(1) Mortality	(2) First birth	(3) Second birth	(4) Third birth
Panel A: Land channel				
Hindu × Marriage cohort	0.991	1.005	0.993	1.005
	(0.0218)	(0.0107)	(0.00772)	(0.00844)
N	26915	8764	8575	8082
Panel B: Human capital channel				
Hindu × Age at amendment	1.150	0.960	1.052	1.178
	(0.160)	(0.0315)	(0.0430)	(0.131)
N	2584	1370	1240	925

Robust standard errors in parentheses. All coefficients are from Cox proportional hazards models, and hazard ratios are reported. The data is limited to pre-reform observations; in Panel A this includes women who were married before the reform passed in their state and in Panel B this includes women who were over the age of 18 and unmarried at the amendment. Column 1 corresponds to specifications that uses child-level data, measures the hazard of dying before age 5, and controls for child gender, birth order, sex composition of siblings, mother's age, mother's age at first birth, state, caste, rural/urban dummy, parents' age gap, and parents' education gap. Columns 2-4 correspond to specifications that use mother-level data and measure the hazard of having a first child, a second child, and a third child respectively. Controls exclude child-level variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote hazard ratios significantly different from 1.

3.5.2 Accounting for concurrent policies

A second concern is the possibility that a concurrent policy in India, called the Dowry Prohibition Act Amendments (DPAA), may result in biased estimates. The DPAA went into effect in 1986, and strengthened the original Dowry Prohibition Act of 1961, which prohibited both the giving or receiving of a dowry.[9] Similar to the HSAA, the DPAA applied to all non-Muslims who were unmarried at the time of the reform, and excluded Muslims and those who were already married. Importantly for this analysis, Calvi & Keskar (2021b) find that the act decreased dowry payments, reduced women's decision making power in their marital households, increased domestic violence, and increased education.[10] This has several implications for the direction of bias in my estimates. First, it is possible that the effect of the DPAA is captured in the effect I estimate through the land channel in the form of reduced decision making in the household. Since the HSAA should affect demographic outcomes by increasing women's decision making ability, the DPAA biases this estimate towards zero. Second, it is possible that the effect of the DPAA is captured in the effect I estimate through the human capital channel in the form of increased education for women. This may inflate the magnitude of the estimate, as the HSAA is also expected to impact outcomes through increased education.

I address the first of these concerns by replicating the core results using a restricted sample of women who were married after 1986, the year the DPAA was passed. This effectively removes Andhra Pradesh from the analysis, and restricts the sample to women who were all affected by the

[9]The original act lacked strong provisions and enforcement, and did not significantly reduce dowries (Chiplunkar & Weaver, 2021). The subsequent 1986 amendment tightened the existing legislation in several ways and increased the minimum punishment for taking or abetting dowry.

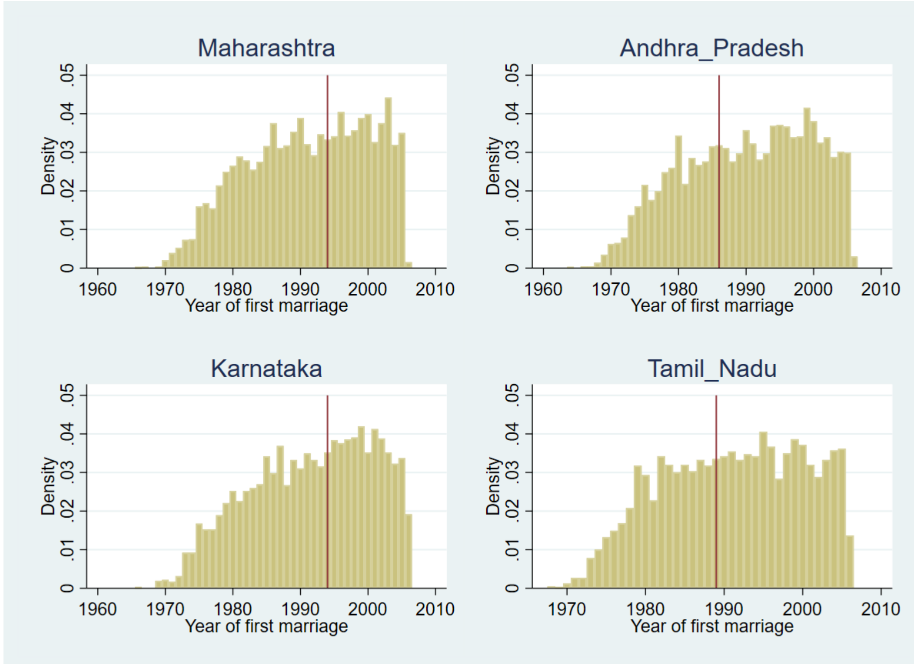
[10]Calvi & Keskar (2021b) argue that the increase in education may be a result of parents compensating for the decrease in dowry payments in order to maintain the attractiveness of the bride in the marriage market.

DPAA regardless of their marriage year, allowing the DPAA's effect on the outcomes of interest to be differenced out. The results, presented in Panel A of Table C.9 in the Appendix, are not statistically significantly different from the results using the full sample. Unfortunately a similar sample cannot be constructed for the human capital channel analysis and those results should be interpreted with caution.

3.5.3 Accounting for selection

A third threat to identification is that eligibility for the HSAA is partially determined by marital status, which is endogenous. This would be an issue if parents anticipated the reforms and married off their daughters just before the amendment passed in order to prevent them from being eligible, or if parents delayed the marriage date of their daughters to after the amendment passed in order to ensure that they were eligible to inherit land, depending on their preferences. I check for evidence for this type of selection in the data by examining the distribution of marriages in the months preceding and following the reform in each state, presented in Figure 3.2. There is no spike in marriages directly before or after the amendment passed in each state, providing some visual evidence that selection bias is likely not impacting my analysis. Nevertheless, as is reported in Panel B of Table C.9 and Table C.10 in the Appendix, the main results of my analysis are robust to dropping mothers who were married one year before and one year after the amendment passed in their state.

Figure 3.2: Distribution of mothers' year of first marriage by reform state for eligible religions



Vertical lines indicate the year the HSAA was passed in each state.

3.5.4 Accounting for treated children

A final threat to identification is the issue of treated children. A subset of the mothers who are married at the time of the reform, and are therefore ineligible, had children before the reform was enacted in their state. Although these children would eventually be eligible for the reforms themselves, mothers would have already made health investment decisions that influenced their mortality before then. As a result, the mortality outcomes of these children are therefore not influenced by their own eligibility for the reforms. On the other hand, all mothers who are unmarried at the time of the reform marry and have children after the reform passes, and make health investment decisions for these children with the knowledge that they are eligible for the same reforms. The mortality outcomes of these children are influenced by their own eligibility for the reforms. This results in potentially biased estimates, as part of the effect on child mortality may be a result of the fact that some children may also be eligible for the reforms. I address this by constructing a restricted sample of children who were all born after the reform passed in their state, which removes any differential treatment by mother's marriage cohort. The results are presented in Panel C of Table C.9, and are robust to this restricted sample.

3.6 Results

3.6.1 Descriptive results

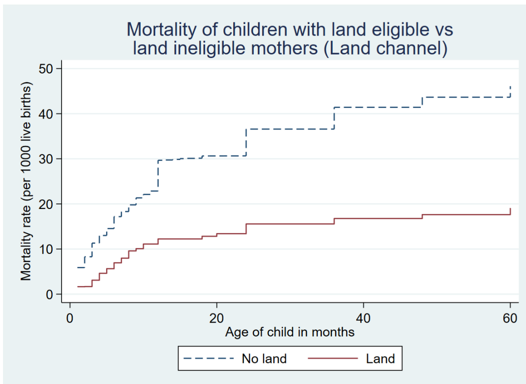
Figure 3.3 shows the cumulative mortality hazards and survival functions for two sets of HSAA comparisons. The graphs on the left compare land eligible mothers with non-eligible mothers, which captures the land channel effect. The graphs on the right compare mothers with childhood exposure, and therefore potential for more human capital, to mothers with adult exposure, which captures the human capital channel effect. Overall, the graphs show that the HSAA reduces child mortality through the land channel, extends the age at first birth for childless women through the human capital channel, and reduces the probability of having a third child for women who already have children through the human capital channel.

Panels A and B show the cumulative hazard estimates of child mortality expressed as number of deaths per 1,000 live births. Panel A shows that for children whose mothers are eligible to inherit land, the under-five mortality rate is about 45 deaths per 1,000 live births, compared to only 20 deaths for children whose mothers are not eligible. Panel B shows that there is no statistically significant difference between children with mothers who had childhood exposure and children with mothers who did not, indicating that mortality is not affected through the human capital channel.

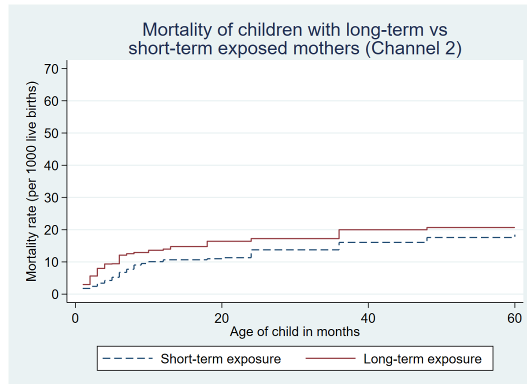
Panels C and D show the survival functions for women who have never given birth, which measures the probability of remaining childless past time t. Panel C shows that the HSAA does not affect this probability through the land channel, and Panel D shows that there may be a small effect through the human capital channel. Ultimately survival drops to zero for both groups of women after age 30, indicating that the HSAA does not reduce the probability of having children at the extensive margin, but rather increases the survival duration, which is the woman's age at her first birth.

Panels E and F show the survival functions for women who have given birth twice, which measures the probability of remaining at two births past time t. Panel E shows that the HSAA has no effect through the land channel, and Panel F shows that there is an effect through the human capital channel. Early exposure to the HSAA increases the probability of remaining at two births.

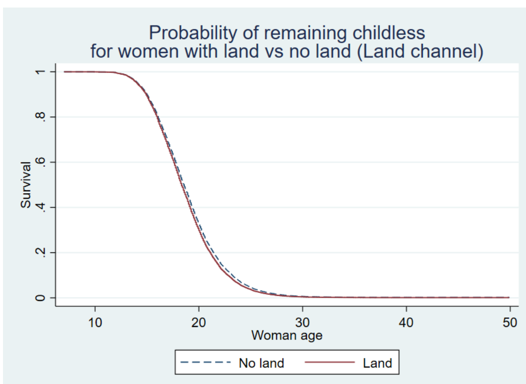
Figure 3.3: Cumulative mortality and survival functions



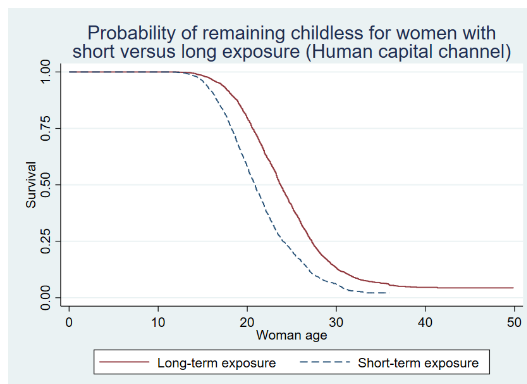
A



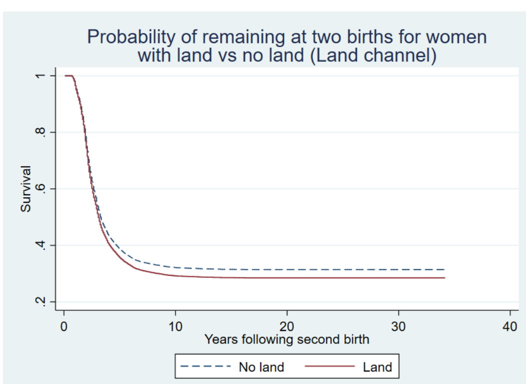
B



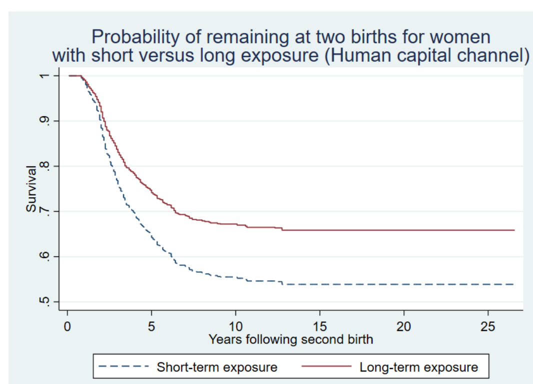
C



D



E



F

Mortality rates in Panels A and B are estimated using Nelson-Aalen cumulative hazard. Panels C-F show Kaplan-Meier survival curves. Covariates are held at their means. The birth 2 survival functions are excluded from this figure but reported in Appendix Figure C.1. They are identical for treated and untreated women in both channels and do not return significant regression results.

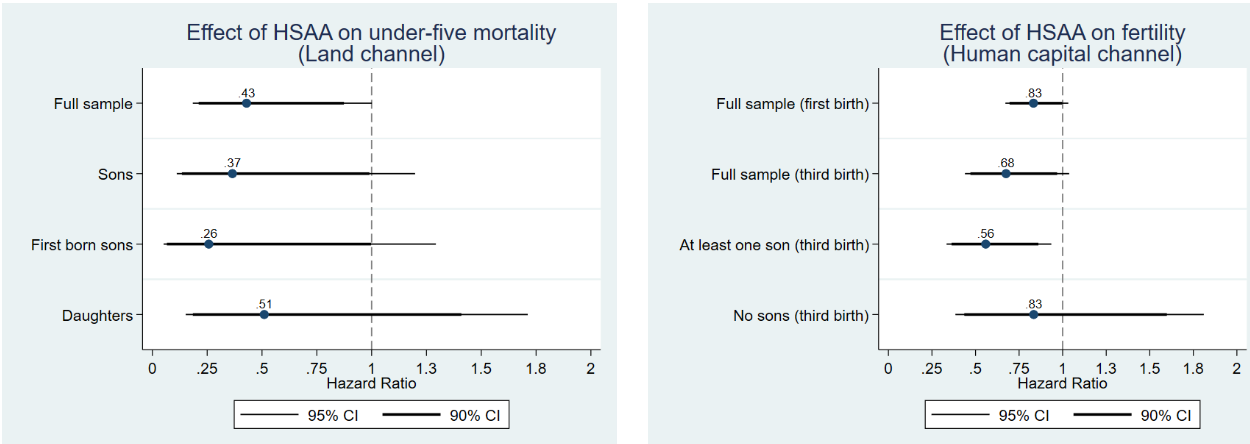
3.6.2 Model results

Hazard model estimates confirm that mortality decreases through the land channel and fertility decreases through the human capital channel. Figure 3.4 presents these results, along with heterogeneous effects on mortality by gender and heterogeneous effects on fertility by sex composition of previous births. The reported hazard ratios are the significant DID estimates from Cox proportional hazards models.

Panel A of Figure 3.4 shows that having a mother who is eligible for land significantly reduces the hazard of dying at any given point in time by 57% ((1-.43)$\times$100) for the full sample. These results are driven by a decrease in the mortality hazard for sons by 63% and particularly for first born sons by 74%, significant at the 10% level. However, mortality for daughters does not decrease. I find that the mothers of these children are more likely to have a say in how to spend their own earnings and in decisions regarding daily purchases, providing some evidence for an improvement in bargaining power within the household.[11]

[11] Effects on mothers' decision making are shown in Table C.6 in the Appendix.

Figure 3.4: Effect of HSAA on mortality and fertility



A B

Reported hazard ratios are the DID interaction terms from Cox proportional hazard models. In the models in Panel A, the hazard refers to the probability of dying after month t, conditional on surviving until month t. Controls include dummies for child gender, birth order, sex composition of prior siblings, mother's age, mother's age at first birth, state, caste, whether the household is rural, parents' age gap, and education gap. In the models in Panel B, the hazard refers to having birth number x+1, conditional on having x births until year t. Results for x = 0 and x=2 are shown. Controls include dummies for mother's age, state, caste, spousal age gap, spousal education gap, whether the household is rural, and sex composition of previous births. Tables C.1, C.2, and C.3 in the Appendix show the full results of the effect of each channel on all outcomes.

It is important to note that the hazard ratio should be interpreted as a reduction in the instantaneous risk of death at any given point in time, or a reduction in the rate of death, and not a reduction in the cumulative risk of death over a specified period of time. To get a sense of the durable benefit of the reform, I calculate the effect of the reform on the cumulative risk of dying before age five by calculating the cumulative probability of dying before age five for control individuals and comparing it to the cumulative probability of dying before age five if the hazards of those individuals decreased by 57%. The following is a representative equation:

$$\text{Percentage point decrease in under five mortality} = \int_0^5 h(t)s(t)dt - \int_0^5 .43h(t)s(t)dt,$$

where $h(t)$ is the hazard function of untreated children, $s(t)$ is the proportion of the population still alive at time t, t is years of life, and the hazard ratio is .43. I find that there is a .4 percentage point change in the cumulative probability of dying before age five from a baseline of 5%. Such a calculation is a useful way of summarizing the survival benefit in this particular setting, in which age five is an appropriate landmark time point beyond which the risk of under-five death is, by definition, impossible.

While the large reduction in child mortality has positive implications for the importance of women's inheritance rights, it is important to consider whether it is coming at the expense of a more male-biased sex ratio at birth, particularly since mortality seems to decrease only for boys. Since sex ratios cannot be defined at the individual level, I estimate the effect of the HSAA on the probability of being a boy using a logistic model. Table C.4 in the Appendix shows that the reforms do not have a significant effect on the gender at birth, suggesting that exposure to the HSAA is not simultaneously changing sex ratios at birth.

Next, examining the fertility results, the first hazard ratio in Panel B of Figure 3.4 indicates that the human capital channel reduces the instantaneous risk of having the first child at any given point in time by 17% ((1-.83) $\times$ 100) for the full sample, significant at the 10% level. Note that the survival function in Figure 3.3, Panel D shows that the probability of remaining childless becomes zero past age 30, indicating that the risk event eventually happens to the vast majority of women. Therefore, the concept of a reduction in the cumulative risk of having the first birth as calculated in the mortality analysis is not applicable. In this case, the reduction in the hazard rate means that time until first birth is prolonged, but not that the risk of having children has been averted. Thus, rather than a decrease in fertility at the extensive margin, I find that the reform causes women's age at first birth to increase by about 11 months.[12]

The second, third, and fourth hazard ratios in Panel B of Figure 3.4 show how the hazard of having more than two children is affected by the reform. Overall, the human capital channel reduces the instantaneous hazard of having more than two children by 32% ((1-.68) $\times$ 100). However, the reform is only effective at reducing fertility along this margin for women who have already given birth to at least one son, reducing their hazard of having more than two children by 44%, significant at the 5% level. Women with daughters remain just as likely to have more children, which reflects strong son preferences in India.

The full sample decrease in the hazard corresponds to a 11.2 percentage point decrease (from a baseline of 34%) in the probability of having a third child within 12 years post-birth of the second child. This was calculated by comparing the cumulative probability of having a third child for untreated women to the cumulative probability of having a third child if the hazards of those women decreased by 32%:

$$\text{Percentage point decrease in probability of third birth} = \int_0^{12} b(t)s(t)dt - \int_0^{12} .68b(t)s(t)dt,$$

where $b(t)$ is the hazard function of untreated women, $s(t)$ is the proportion of women with two births at time t, t is years after the previous birth, and the hazard ratio is .68. I use 12 years as the stopping point because Figure 3.3, Panel F shows that the probabilities of survival plateau after 12 years. This means that the hazard of having a third child past 12 years post-birth of the second child is very low compared to the initial 12 years. 12 years is therefore a reasonable time point at which to assess the durable benefit of the reform.

[12]This result is reported in Table C.6 in the Appendix.

Overall, the fertility results show that women who were young when the reform passed and delay their age at first birth and are less likely to have more than two children. There are no decreases in fertility for women at other birth orders.[13] Both reductions in fertility operate through the human capital channel. This implies that it is not the right to land itself that gives women the bargaining power to reduce fertility, but rather the human capital gains that childhood exposure to the reform brings. I find evidence for these human capital gains and how they manifest within women's marriages in Appendix Tables C.5 and C.6, where I show that mothers who were young when the reforms passed have about 10 more months of education, are 8 months older at the time of marriage, and have twice the odds of having worked outside of the home in the past year compared to mothers who were adults when the reform passed. These results are in line with Deininger et al. (2013), Roy (2015), and Bose & Das (2017) who also find increases in education and age at marriage.[14] The results of this chapter take the findings of these previous studies a step further and show that women are able to translate these pre-marital human capital gains into increased bargaining power within their marriage down the line, and reduce fertility to align more closely with their preferences.

I explore the possibility that these results are driven by changes in marriage market sorting that result in women marrying men with characteristics that may lead to more favorable child mortality and fertility outcomes. In Table E.1 I show that the reforms do not affect spousal age gap, spousal education gap, whether the husband works in a white collar job, or whether

[13]See Table C.2 for all birth order results.

[14]There is evidence that the inheritance reforms raised the costs of having daughters for the parents of the mothers in my sample, along with an increase in stated son preference in fertility post reform (Bhalotra et al., 2020). This resulted in an increase in female mortality, female foeticide, but no changes in overall fertility (Bhalotra et al., 2020; Rosenblum, 2015). This may mean that the women who survived (the treated mothers in my sample) may come from families that are more progressive and more likely to invest in their daughters' education. It is possible that this selection issue could be driving the increase in women's education, inflating the estimates found for the human capital channel analysis.

the husband is often drunk.[15] Nevertheless, there are other unobservable husband and marriage characteristics that may have changed in response to the reforms, so I cannot entirely rule out or disentangle the marriage market channel. Chapter 4 further explores this possibility with different data.

3.7 Robustness checks

I conduct three robustness tests to check the validity of the hazard model results. First, I conduct the same hazard model analysis using two placebo amendment passage dates, one five years before the actual amendment passed in each state and another ten years before. I restrict the data to children with mothers who married before the true amendment date in their state, so the samples consist of individuals unaffected by the true amendments. I find no significant effects, which provides further evidence that the results are not driven by differences between eligible religions and non-eligible religions. The results are reported in Table D.1 in the Appendix.

In the second robustness check, I conduct three versions of randomization inference tests following the approach taken by Bertrand et al. (2004) and Gubler et al. (2018). In one test, I randomly reassign individuals' eligibility status by religion and estimate the effect using the original hazard model specifications, repeating this process 50 times.[16] In another, I randomly assign a placebo amendment passage date between 1980 and 1995 to each state and estimate the effect, again repeating this process 50 times. Finally, I randomly assign a placebo passage date

[15] As Calvi (2020) argues, given that the reforms affected all women within a religious group, that interfaith marriages are extremely rare, and that the vast majority of marriages occur within states, it is possible that the amendments did not have a large impact on the relative ranking of women in their relevant marriage market. This rules out the possibility of changes in sorting due to the reforms.

[16] To preserve the original proportion of eligible religion vs non-eligible religion individuals in the data, I assign 85% of individuals as an eligible religion and therefore ”affected” by the placebo amendment and 15% non-eligible and therefore unaffected.

and scramble eligibility by religion. The placebo coefficients are shown in Figure D.1 in the Appendix. I find that the true coefficients are in the top 10% of estimates for all tests, which is expected given their significance levels.

In the third robustness check, I conduct the original hazard model analyses using four non-reform Indian states with similar baseline levels of child mortality as well as a similar balance of eligible religion and non-eligible religion individuals.[17] I run the analysis for each of the twenty-four possible ways to assign reform years to non-reform states. This replicates my original analysis but preserves any omitted differential trend. Figure D.2 in the Appendix shows that all coefficients are greater than zero, indicating that, if anything, there is some omitted variable that moves outcomes in the opposite direction of my estimated effect. This further strengthens my result.

3.8 Conclusion

The results in this chapter show that bargaining power induced through land inheritance rights allows women to reduce fertility and child mortality to better align with their optimal strategies for security in old age. Each outcome is reduced through a distinct channel. First, the reforms reduce child mortality through a land channel. Children with mothers who have the ability to inherit land itself have a 57% lower hazard of dying, which amounts to a .4 percentage point decline in the cumulative probability of dying before age five from a baseline of 5% . The decline is driven by boys, and particularly first born boys. Second, women's empowerment through inheritance reforms reduces fertility through a human capital channel.[18] Women who

[17] I use Gujarat, Arunachal Pradesh, Odisha, and Rajasthan for the non-reform states.

[18] These results may be biased upwards and should be interpreted with some caution, as I am unable to remove the effect of the Dowry Prohibition Act Amendment of 1986 from this analysis.

were young when the amendment passed in their state, and therefore had parents who made human capital investment decisions in response to the amendment throughout their childhood, delay their first birth by nearly a year, and if they already have children, have a 32% lower hazard of having more than two. This decrease is driven by women who have already had at least one son, which reflects strong son preferences in India. I find that these women have more education, an older age at marriage, as well as a higher probability of labor force participation, providing some evidence for the human capital mechanism and its consequences for women's bargaining power within her marriage.

Back-of-the-envelope calculations indicate that the impact of the reforms amounts to a decrease in overall under-five mortality from about 63 deaths per 1000 live births to 59 in reform states. This corresponds to 6,255 children saved per year in Andhra Pradesh, 4,549 children per year in Karnataka, 7,976 children per year in Maharashtra, and 4,448 children per year in Tamil Nadu. This amounts to a total of about 344,169 children in reform states who were saved in the years between the reform passage years and 2005, the survey collection year.[19] Thus, in addition to being an important human rights goal in and of itself, equal land inheritance rights also functions as a costless way to contribute to the important development goal of reducing child mortality. However, the lack of an effect on mortality for daughters implies that empowering mothers without also changing the norms and incentives that make investing in daughters less beneficial for households may not be enough to improve outcomes for girls in some contexts.

[19] See Appendix F for calculations.

Chapter 4: A deeper investigation of underlying mechanisms

4.1 Introduction

This chapter uses a new dataset to further explore the underlying mechanisms driving the effects found in Chapter 3. The 2006 round of the Rural Economic & Demographic Survey data allows me to identify fathers' disinheritance status using their sisters' eligibility status and directly test the ambiguous prediction of the reforms on child mortality given by Proposition 2.a.[1] This is the case in which mothers are eligible for inheritance and fathers are not affected, resulting in both an empowerment and income effect, which affect child mortality in opposite directions. I use an event study design and find that the reforms reduce child mortality for this particular subset of households, indicating that the empowerment effect dominates the income effect. The results are robust to falsification checks and to a series of tests using restricted samples that remove various sources of potential bias.

I also take advantage of the dowry information available in this dataset to test whether the there is an alternative marriage market channel that may also be affecting bargaining power and impacting child mortality. I find that the reforms increase dowry payments, indicating that my estimates are inclusive of this alternative mechanism.

This chapter makes two main contributions: it presents the first analysis of these reforms

[1] I am unable to test Proposition 2.b. directly due to the very low number of fathers affected by the HSAA.

that observes and holds constant the effect of fathers' disinheritance, and it identifies a possible marriage market channel.

4.2 Data

I use the 2006 round of the Rural Economic & Demographic Survey (REDS) data for an additional analysis using between-state variation. The REDS data is representative of the rural population in India, rather than the full population. The dataset used for analysis contains 4882 observations of ever-married women of eligible religions in the four reform states and nine non-reform states. Non-reform states include Bihar, Gujarat, Haryana, Himachal Pradesh, Madhya Pradesh, Odisha, Punjab, Rajasthan, and Uttar Pradesh.[2] The data contains information on women's state and marriage dates, which I use to proxy for eligibility.

Table 4.1 summarizes the average values for mother-level variables for women from reform states and women from non-reform states. Child mortality, the main outcome variable, is measured at the mother level as the percentage of children who have died and is significantly lower for mothers in reform states.[3] Fertility is lower for mothers in reform states, and they also tend to have higher education and ages at marriage.

A key advantage of this data over the DHS is that it also contains all the relevant HSAA eligibility information on the husbands' sisters, including their state, religion, marriage date, whether their fathers are alive and fathers' death years if not. I use this information to construct

[2]Following Hossain & Nikolov (2021), I exclude Union territories, West Bengal, Jammu and Kashmir, and Northeastern states from this analysis. Union territories are politically and administratively different from rest of India; West Bengal and Assam practice the Deyabhaga system of property right, which allows girls to inherit various types of property. Jammu and Kashmir was not subject to the Hindu Succession Act and Northeastern states are historically matrilineal kinship areas (Agarwal, 1988).

[3]Unlike the DHS data, there is no child-level death information and I do not observe which children died before age five.

an indicator for whether the husband had a sister who was eligible for the HSAA and therefore experienced disinheritance. I define affected fathers as those who experience disinheritance because they are of an eligible religion, are from a reform state, had a father who was alive at the time the amendment passed in his state, and has at least one sister who was unmarried before the amendment. I define unaffected fathers as those who are of a non-eligible religion, are from a non-reform state, did not have any sisters, or had sisters who were all married before the amendment passed in their state. Table 4.1 shows that 19% of mothers in reform states are married to men who experienced disinheritance.

4.3 Empirical strategy

I estimate an event study DID model using variation in eligibility across multiple pre- and post-amendment marriage cohorts and reform versus non-reform states. I duplicate the observations in each control state four times to create four groups of control states. Each group is assigned one of the four "would have adopted" dates and matched with the corresponding treated state, creating four match groups.

I restrict the data to eligible-religion women who report coming from landed households[4] and whose husbands were not affected by the HSAA and estimate the following model:

$$ChildMortality_{isdtg} = \sum_{t=-20,t\neq-1}^{17} \beta_t(TreatedState_s \times 1_t) + \gamma_d + \lambda_{tg} + \delta' X_i + \epsilon_{isdtg}, \quad (4.1)$$

where $ChildMortality_{isdtg}$ is the percentage of children who died for woman i in state s and district d, who married in cohort t, and is in match group g. Marriage cohort t is measured relative

[4]The REDS data reports whether the woman's parents own irrigated or unirrigated land.

Table 4.1: REDS data descriptive statistics (mother-level)

	Reform State	Non-Reform State	Difference
Child mortality rate (%)	0.06	0.09	0.03***
	(0.08)	(0.10)	(0.00)
Number of children	2.72	3.67	0.95***
	(1.39)	(1.79)	(0.00)
Years of education	2.98	2.02	-0.95***
	(3.90)	(3.65)	(0.00)
Age at marriage	19.06	18.30	-0.76***
	(5.09)	(3.95)	(0.00)
Current age	41.60	42.24	0.64*
	(9.91)	(9.47)	(0.02)
Percentage male children	0.59	0.58	-0.01
	(0.32)	(0.28)	(0.21)
Scheduled caste (%)	0.17	0.18	0.01
	(0.37)	(0.39)	(0.27)
Scheduled tribe (%)	0.07	0.10	0.03***
	(0.26)	(0.30)	(0.00)
Other backward class (%)	0.52	0.45	-0.07***
	(0.50)	(0.50)	(0.00)
General class (%)	0.24	0.27	0.03*
	(0.43)	(0.44)	(0.03)
Spousal age gap	6.09	3.73	-2.35***
	(3.13)	(2.50)	(0.00)
Spousal education gap	2.15	4.08	1.93***
	(3.78)	(6.41)	(0.00)
Percentage of fathers affected by HSAA	0.19	0.00	-0.19***
	(0.39)	(0.00)	(0.00)
N	1928	2954	4882

Columns 1 and 2 average values for all variables with the standard deviation in parenthesis. Column 3 gives the difference between the two groups with the p-value from the t-test of means in parenthesis.

to the year the reform passed in each match group. $TreatedState_s$ is an indicator for states where the reform passed and γ_d are district fixed effects. The λ_{tg} indicates a full set of match group-by-cohort fixed effects, where the set of cohort indicators denote each of the possible marriage years relative to the year of the reform. This accounts for trends in outcomes across cohorts within each match group. X_i is a vector of mother-level controls including caste, religion, mother's age, spousal age gap, and spousal education gap. The coefficients of interest are given by β_t, and measure the differences in outcomes between those in treated versus control states in each marriage cohort t within each match group. These effects capture both the empowerment effect, which reduces child mortality, and the income effect, which increases it. Standard errors are clustered at the state $\times$ cohort level.[5]

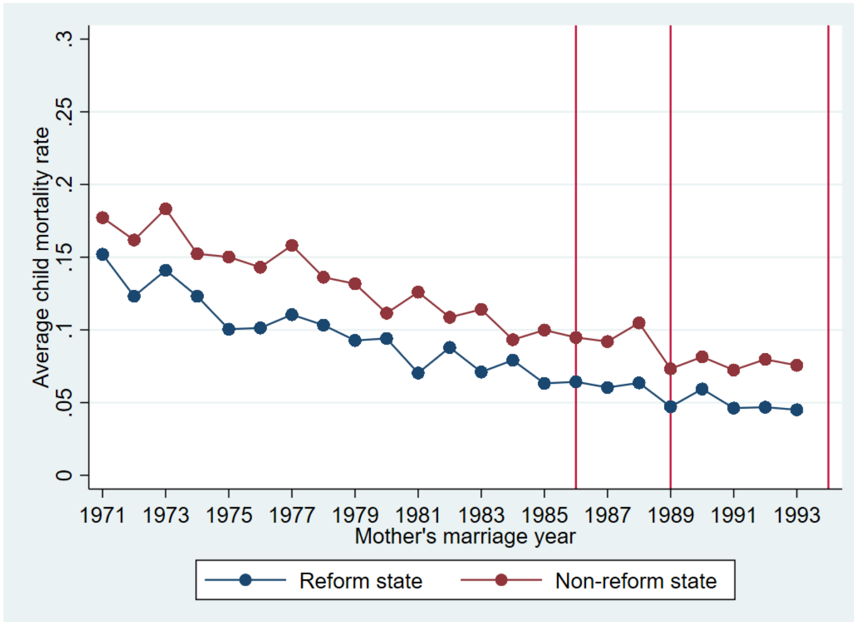
4.4 Validity of identification

4.4.1 Common trends

To give a sense of pre-reform trends, Figure 4.1 shows the trajectories of child mortality for eligible and non-eligible women using only those who were married before the amendment passed in their state. This data includes years between 1971 and 1994, and drops any women who were married after the amendment passed in their state. The figure shows fairly parallel trends in child mortality between women in reform and non-reform states.

[5] There are 4 reform states and 9 control states, for a total of 13 states. There are 37 marriage cohorts in this specification. This results in $13 \times 37 = 481$ clusters.

Figure 4.1: Pre-amendment trends in child mortality



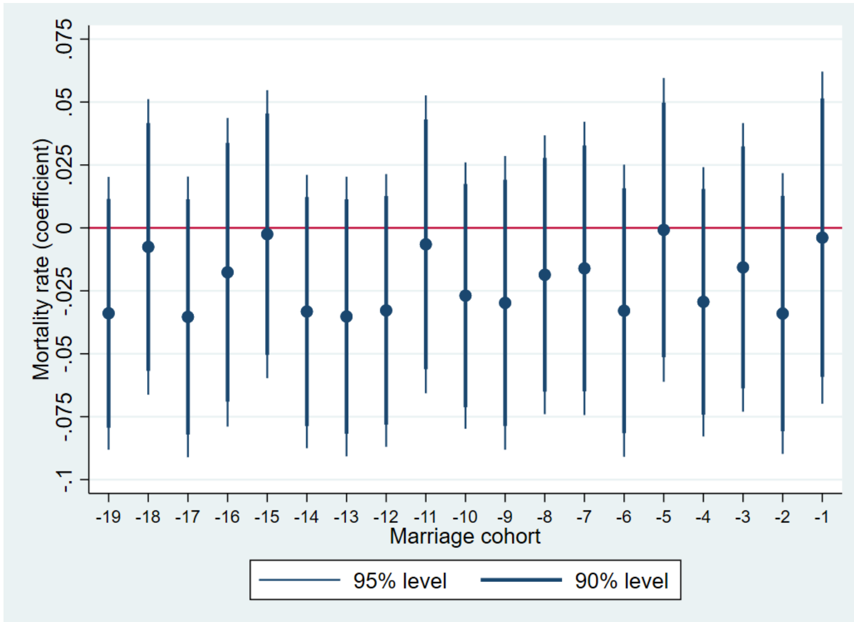
Sample is restricted to pre-reform observations, i.e. women who were married before the reforms passed in their state. Vertical lines at 1986, 1989, and 1994 indicate the year the HSAA passed in Andhra Pradesh, Tamil Nadu, and Maharashtra & Karnatika respectively.

I conduct a formal test of differential pre-trends by restricting the sample to the pre-reform period, which includes only women who married before the reform passed in their state, and estimate a model identical to the main specification given by equation 4.1:

$$ChildMortality_{isdtg} = \sum_{t=-19}^{-1} \mu_t(TreatedState_s \times 1_t) + \gamma_d + \lambda_{tg} + \delta' X_i + \epsilon_{isdtg}. \quad (4.2)$$

Figure 4.2 plots each interaction coefficient μ_t as well as their 90% and 95% confidence intervals, and shows that there are no significant divergence in mortality.

Figure 4.2: Test of differential pre-trends in mortality (REDS)



Coefficients from the interaction terms are shown. Sample is restricted to pre-reform observations, i.e. women who were married in cohorts -20 to -1, where cohorts are measured relative to the reform passage year. Controls include district fixed effects, group-by-cohort fixed effects, caste, religion, mother's age and spousal age and education gaps. The omitted cohort is -20. Standard errors are robust and clustered at the state by cohort level ($13 \times 20 = 260$ clusters).

4.4.2 Accounting for bias using restricted samples

As is outlined in Chapter 3, there are three additional threats to identification that I address using restricted samples. First, the results may be biased due to the Dowry Prohibition Act Amendment (DPAA) of 1986. Like the HSAA, the DPAA applied to women deferentially by marriage cohort. Although it was ratified nationally, Calvi & Keskar (2021b) find that the negative effect of the DPAA on women's decision making ability in the marital home is mitigated in states in south India compared to northern states. Therefore, the DPAA also simultaneously affected women differentially by treated versus control states. This may inflate my estimates. I check that the HSAA impacts child mortality independently of the DPAA by replicating the main results using a sample restricted to women who married after 1986, the year of the DPAA. All the women in this restricted sample are exposed to the DPAA regardless of their marriage year, removing its effect from the estimate.

Second, the results may be biased due to selection from endogenous marriage. Parents may have anticipated the reform and married their daughters off early in order to avoid inheritance eligibility or delayed marriage in order to ensure eligibility, depending on their preferences. I account for this by estimating the model on a restricted sample in which women who were married one year before and after the reforms are excluded.

Third, there is potentially bias that results from children who are themselves eligible for the reforms differentially by religion and their mothers' marital status. I account for this by replicating the results using a restricted sample of mothers who have at least one child born after the reforms.[6] This results in a subsample of children who are all eligible for the reform, regardless of their mothers' marital status, removing this effect from the estimate.

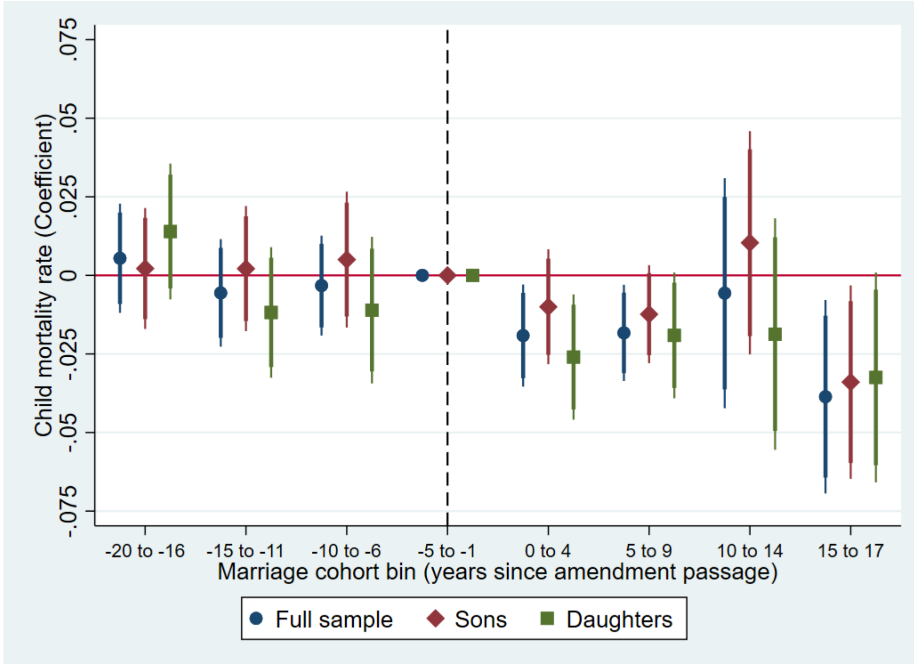
The results using the three restricted samples are reported in Appendix Table C.11 and are not statistically different from the estimates from the full sample.

4.5 Results

Figure 4.3 reports the DID estimates from the event study model that restricts the sample to households where mothers gain inheritance and fathers do not experience disinheritance. Separate coefficients are reported for the full sample of children, sons, and daughters. Cohorts

[6]The REDS data only allows me to identify the birth year of each mother's last child. It is possible that the women in this restricted sample still had older children who were born before the reform years, so the results should be interpreted with caution.

Figure 4.3: Event study DID coefficients (90% and 95% confidence intervals)



Coefficients from the interaction terms in the event study are shown. Controls include district fixed effects, group-by-cohort fixed effects, caste, religion, mother's age and spousal age and education gaps. Cohorts are binned into groups of five years. Standard errors are clustered at the state × binned cohort level (13 × 8 = 104 clusters). Table C.7 in the Appendix reports the full table of coefficients and their standard errors.

are binned into groups of five years. This model allows me to explicitly examine the possibility of differential cohort trends by estimating coefficients on placebo exposure for cohorts who were married before the reforms. I see no evidence of significant impacts of placebo exposure, providing support for the validity of this research design.

At the same time, I observe a significant decrease in child mortality rates for cohorts who were eligible for the reform, indicating that the empowerment effect dominates the income effect. Child mortality rates for the full sample significantly decrease by about 2 percentage points for women married between 0-4 and 5-9 years after the reform, and by about 3.8 percentage points for women married between 15-17 years after the reform. I find that the decrease for cohort groups 0-4 and 5-9 are driven more strongly by a decrease in mortality for daughters, and that the decrease for cohort group 15-17 is driven by a decrease in mortality for sons, indicating that the reforms are more salient in a rural context when it comes to improving female mortality rates. F-tests for joint significance using the full sample indicate that the pre-reform coefficients are not jointly significant, and the post-reform coefficients are jointly significant.[7]

4.6 Robustness checks

I test the validity of the event study result by conducting the same analysis on a placebo sample of women from non-eligible religions. I find null results for cohorts who were married after the reforms passed, indicating that there are no omitted variables that bias my results. Figure D.3 in the Appendix displays the results. In addition, I conduct a falsification test in which I restrict the data to pre-reform observations and run the same specification using placebo reform years 5, 10, and 15 years before the true reform. Table D.2 shows that there are no significant effects.

4.7 Marriage market outcomes

There is a third unexplored but potentially important mechanism through which the HSAA may be operating to affect fertility and child mortality: the amendment's effect on women's ability to find a better match in the marriage market and on the conditions of her marriage in general. Although I am unable to disentangle this channel from the land and human capital channels, I investigate whether there is evidence for it by observing the effect of the reforms on

[7] F-tests for joint significance return p values of .4111 and .0687 for the pre-reform coefficients and post-reform coefficients respectively.

various husband and marriage characteristics.

In Table E.2, I show that the reforms do not affect the husband's socio-economic class proxied by his father's level of education, the distance of the wife's marital home from her natal household, or the probability of marrying a relative.[8] However, as I show in Table E.4, they do increase net dowry by 4971.3 Rupees, which corresponds to an increase of about .2 standard deviations.[9] This finding is consistent with Roy (2015), who also finds that the HSAA increases dowry and argues that it is because families attempt to circumvent the reforms and compensate their daughters for disinheritance by giving them alternative transfers in the form of higher dowries.

What does this mean for women's bargaining power in their marital households? In the Indian context, dowries are controlled by the bride's marital family and not by the woman herself. Thus, the level of dowry payment does not represent a direct shock to the wealth the woman controls in the household. However, the dowry may still function as a source of protection for women, and a recent study finds that higher dowry payments are associated with larger shares of household resources allocated to Indian women and lower poverty rates of women relative to men (Calvi & Keskar, 2021a). In this way, increased dowry payments result in more bargaining power for women within the household, though not through the land shock as is conceptualized in the theoretical model.

Note that this channel does not necessarily amount to an omitted variable; rather, it is

[8] I use event study specifications with the same framework as in equation 4.1 to estimate the effect of the reforms on the husband and marriage characteristics.

[9] Although dowry is illegal in India, it is still widely practiced (Chiplunkar & Weaver, 2021). The survey question that obtains dowry information is, "Total value of gifts given at the time of marriage (Rs.)" To estimate the effect on dowry, I use an alternative dimension of the REDS data as well as new variation in eligibility by marital cohorts and father's death year. The data description, empirical specification, and results are summarized in Appendix E and Table E.4.

another mechanism through which the reforms may impact women's intra-household bargaining power. My effects are inclusive of this channel. Taken together, the increase in dowry and the lack of an effect on husband and marriage characteristics imply that while increased bargaining power through dowry is a possible mechanism for improved demographic outcomes, there is no evidence for changes in marriage market sorting.

4.8 Conclusion

This chapter directly tests Proposition 2.a. and finds that the empowerment effect dominates the income effect for households in which mothers experience inheritance and fathers are unaffected by the reform. In this particular sample of rural households, mortality decreases for both sons and daughters, indicating either that the mortality of daughters was below the optimal level from the mother's perspective before the reforms, or that the reforms allow parents to invest in sons up to a maximum threshold, and are then able to also invest in the health of daughters.

The positive effects of the reforms on dowry found in this chapter show that the land channel is likely inclusive of a marriage market channel as well. It also highlights an interesting consequence of the HSAA: even if parents of land-eligible women are able to circumvent the law (for example, by pressuring daughters to voluntarily sign away their rights), the policy still causes households to invest in their daughters through a different channel. The increased dowry still improves women's bargaining power in her marital household, a direct result of the policy, but not of land inheritance itself. Overall, this chapter provides a richer understanding of the underlying mechanisms at play in the effects that are observed in Chapter 3.

Chapter 5: Conclusion

5.1 Summary of findings

This study shows that women's inheritance reforms operate through a land channel and a human capital channel to reduce child mortality and fertility, and establishes women's land rights as an under-explored but powerful driver for demographic change. Chapter 2 formally establishes the links between land inheritance, bargaining power, and demographic outcomes and provides a set of testable predictions to guide the empirical strategy and interpretation of results. The empirical analyses provide three main sets of findings.

First, Chapter 3 shows that women's empowerment through inheritance reforms reduces child mortality through a land channel. Children with mothers who have the ability to inherit land itself have a 57%[1] lower hazard of dying before age five, which amounts to a .4 percentage point decline in the cumulative probability of dying before age five from a baseline of 5% . The decline is driven by boys, and particularly first born boys. The mothers of these children are more likely to have a say in how to spend their own earnings and in decisions regarding daily

[1] Note that the 57% reduction in the hazard of child mortality is large, but plausible. To put it in perspective, it is similar in size to other hazard ratio estimates of correlations between various measures of women's intra-household bargaining power and child mortality in South Asia. For example, increases in women's ability to participate in household decision making and freedom of movement in Bangladesh reduce the hazard of child death by 46% and 68% respectively (Hossain, 2015). Having a secondary school education reduces the hazard by 32% in India (Maitra, 2004). My study uses quasi-random variation in women's bargaining power through land rights to causally estimate an effect of a similar size.

purchases, providing some evidence for an increase in their intra-household bargaining power.

Second, Chapter 3 also shows that women's empowerment through inheritance reforms reduces fertility through a human capital channel. Women who were young when the amendment passed in their state, and therefore had parents who made human capital investment decisions in response to the amendment throughout their childhood, delay their first birth by nearly a year, and if they already have children, have a 32% lower hazard of having more than two. This decrease is driven by women who have already had at least one son, which reflects strong son preferences in India. I find that these women have more education, an older age at marriage, as well as a higher probability of labor force participation, providing some evidence for the human capital mechanism and its consequences for women's bargaining power within her marriage.

Third, Chapter 4 uses a different dataset to provide a deeper look at the underlying mechanisms driving the effects found in Chapter 3 and directly tests an ambiguous effect generated by the theoretical model in Chapter 2. I estimate a model that controls for father's disinheritance, which shows between a 2 to 3.8 percentage point reduction in household-level child mortality. In this particular sample, the results are driven by a decrease in mortality for both sons and daughters. This chapter also shows that eligible women have a higher dowry than non-eligible women. Since higher dowry is associated with better decision making and treatment in the marital household (Calvi & Keskar, 2021a), this result indicates that even if eligible women do not inherit land due to poor policy enforcement or pressure from families to sign away land rights, the reforms still improve women's bargaining power and have positive intergenerational effects through this alternative marriage market mechanism.

The results in Chapters 3 and 4 are robust to a series of falsification tests and to restricted samples that account for potential bias from a concurrent policy, selection due to endogenous marriage, and differentially treated children.

5.2 Policy implications

The reduction in the hazard of death found in this study amounts to a decrease in overall under-five mortality in reform states from about 63 deaths per 1000 live births to 59 deaths. This corresponds to .4% of live births in the reform states. Between each state's respective reform year and 2005, the survey collection year, a total of 124,778 children were saved in Andhra Pradesh, 52,542 were saved in Karnataka, 92,119 were saved in Maharashtra, and 74,731 were saved in Tamil Nadu. This amounts to a total of about 344,169 children in reform states who were saved as a result of the reforms in that time frame.

These results indicate that in addition to being an important human rights goal in and of itself, equal land inheritance rights also functions as a costless way to contribute to the important policy goal of reducing child mortality. In the past few decades, developing countries have implemented costly conditional cash transfer (CCT) programs that provide women with regular money transfers under the condition that they use various health services, with the goal of reducing maternal and child mortality. India in particular has implemented two of the biggest CCT programs by number of beneficiaries in the world. In 2005, it implemented the *Janani Suraksha Yojana* program, which provides cash transfers to women who give birth in government and accredited private health facilities rather than at home, and has an annual expenditure of about 8.8 billion rupees, or US $207 million (Powell-Jackson et al., 2015). Studies show that the program caused hospital births to increase dramatically, but find mixed results for its impact on neonatal and infant mortality (Lim et al., 2010; Nandi & Laxminarayan, 2016; Powell-Jackson et al., 2015; Sengupta & Sinha, 2018) as well as an unintended increase in fertility (Javadekar & Saxena, 2019; Powell-Jackson et al., 2015). In 2011, India implemented a second maternal CCT program called the *Pradhan Mantri Matru Vandana Yojana* (PMMVY), which pays mothers to participate in infant health-promoting activities and has been successful in reducing child mortality. With a budget allocation in 2017/2018 of 27 billion rupees (US $400 million), PMMVY is India's most expensive CCT program ever, accounting for 12% of the Ministry of Women and Child Development's budget or 0.13% of all government expenditures (von Haaren & Klonner, 2021). Similar programs have been implemented in other countries, including Mexico (Barham, 2011; Gertler, 2004), Brazil (Guanais, 2015; Rasella et al., 2013; Shei, 2013), Bangladesh (Nguyen et al., 2012), and Honduras (Morris et al., 2004). Most programs are extremely costly but have been generally successful in improving child health and reducing child mortality to varying degrees.

Studies on the effects of these CCTs on child health generally conclude that the use of income transfers and conditional mechanisms alone may not be enough to mitigate health inequalities in the presence of poor access to health services and low quality of care (Cruz et al., 2017). The effectiveness of the HSAA also relies on easy access and quality care, but unlike cash transfers, it functions by empowering some of India's most vulnerable and marginalized women to make their own decisions regarding the health of their children, and does not constrain behavior by dictating cash transfer conditions. In addition, it does not require regular cash payments to each beneficiary, resulting in much lower costs and providing a potentially higher rate of return.[2]

The findings from this study also provide empirical evidence for how India's lack of a

[2] The main costs associated with the reforms are administrative costs, unintended consequences associated with decreasing the amount of land that widows inherit, and the unintended outcome of increased female foeticide for the mothers' generation. It is beyond the scope of this study to monetize these costs in a rate of return calculation.

uniform civil code perpetuates inequality. The establishment of a uniform civil code is an ongoing debate regarding the Indian constitution's mandate to replace personal laws based on the scriptures and customs of each major religious community in India with a common set of rules governing every citizen. Personal laws are distinguished from public law and cover marriage, divorce, inheritance, adoption, and alimony. Consequently, although it was ratified nationally in 2005, India's Hindu Succession Act Amendment still only applies to Hindus, Sikhs, Buddists, and Jains, disregarding nearly 20% of the population. Not only does this result in discrimination along religious lines, but my study suggests that it may also have negative consequences for fertility outcomes and the mortality of the next generation of children.

The effects estimated in this study can be interpreted as intention-to-treat effects. The right to inherit land does not guarantee land ownership, and land ownership does not necessarily translate into the ability to control land. Women in half of the countries in the world are still unable to exercise control over land, despite legal protections (World Bank, 2019). My study highlights the importance of investing in policies and interventions that improve women's legal literacy, promote women's land documentation, and establish paths of legal recourse in order for women and their children to fully benefit from existing inheritance rights. Given the promising effects of the right to inherit land found in this study, the intergenerational benefits of empowering women to fully assert ownership and control over land is likely to be substantial.

Appendix A: Proposition proofs

Proof of Proposition 1.a. I use the husband's utility function given by equation 1 in the paper to calculate the optimum. Since the wife's utility is an attenuated version of the husband's utility, the trade-off between present and future consumption are the same for the wife and the husband and the following results apply to both parents.

First substitute the budget constraints into equation 1:

$$\begin{aligned} U^H = &\, U_1((y^W + y^H) + (r_1^W + r_1^H) - \theta n(h_b + x) - (1-\theta)n(h_g + x)) \\ &+ U_2((r_2^W + r_2^H) + \alpha_b \theta n q(h_b) + \alpha_g (1-\theta) n q(h_g)) \end{aligned}$$

First order conditions are given by:

$$\frac{\partial U^H}{\partial h_g} = U_1'(-(1-\theta)n) + U_2'(\alpha_g(1-\theta)nq'(h_g)) = 0 \tag{A.1}$$

$$\frac{\partial U^H}{\partial h_b} = U_1'(-\theta n) + U_2'(\alpha_b \theta n q'(h_b)) = 0 \tag{A.2}$$

The second order partial derivatives are given by:

$$\frac{\partial^2 U^H}{\partial h_g^2} = U_1''(1-\theta)^2 n^2 + U_2''(\alpha_g(1-\theta)nq'(h_g))^2 + U_2'(\alpha_g(1-\theta)nq''(h_g)) < 0 \tag{A.3}$$

$$\frac{\partial^2 U^H}{\partial h_b^2} = U_1''(\theta n)^2 + U_2''(\alpha_b \theta n q'(h_b))^2 + U_2'(\alpha_b \theta n q''(h_b)) < 0 \tag{A.4}$$

$$\frac{\partial^2 U^H}{\partial h_b \partial h_g} = U_1''(-\theta n)(-(1-\theta)n) + U_2''(\alpha_b \theta n q'(h_b))(\alpha_g (1-\theta) n q'(h_g)) < 0 \tag{A.5}$$

I evaluate $\frac{\partial^2 U^H}{\partial h_g \partial n}$ and $\frac{\partial^2 U^H}{\partial h_b \partial n}$ and evaluate the result at the optimum $h_g = h_g^*(n)$ and $h_b = h_b^*(n)$ to obtain the following expressions:

$$\begin{aligned}\frac{\partial^2 U^H(n, h_g^*(n), h_b^*(n))}{\partial h_g \partial n} = &\Big\{U_1''(-\theta(h_b^* + x) - (1-\theta)(h_g^* + x))(-(1-\theta)n) \\ &+ U_2''(\alpha_b \theta q(h_b^*) + \alpha_g (1-\theta) q(h_g^*))(\alpha_g (1-\theta) n q'(h_g^*))\Big\} \\ &+ \Big\{U_1'(-(1-\theta)) + U_2'(\alpha_g (1-\theta) q'(h_g^*))\Big\} < 0\end{aligned}$$

The second term in the curly brackets is equal to $\frac{1}{n}\frac{\partial U^H}{\partial h_g}$, which is equal to 0 at the optimum. Thus the full expression is negative:

$$\frac{\partial^2 U^H(n, h_g^*(n), h_b^*(n))}{\partial h_g \partial n} < 0 \tag{A.6}$$

I can derive a similar expression for $\frac{\partial^2 U^H}{\partial h_b \partial n}$ and obtain:

$$\frac{\partial^2 U^H(n, h_g^*(n), h_b^*(n))}{\partial h_b \partial n} < 0 \tag{A.7}$$

Expressions A.6 and A.7 indicate that the marginal utility of health care for boys and girls should decline with fertility.

From equations A.3 - A.7 and applying Cramer's rule, I find the following:

$$\frac{\partial h_g^*}{\partial n} = -\frac{\begin{vmatrix} \frac{\partial^2 U^H}{\partial h_g \partial n} & \frac{\partial^2 U^H}{\partial h_g \partial h_b} \\ \frac{\partial^2 U^H}{\partial h_b \partial n} & \frac{\partial^2 U^H}{\partial h_b^2} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U^H}{\partial h_g^2} & \frac{\partial^2 U^H}{\partial h_g \partial h_b} \\ \frac{\partial^2 U^H}{\partial h_b \partial h_g} & \frac{\partial^2 U^H}{\partial h_b^2} \end{vmatrix}} = -\frac{\begin{vmatrix} - & - \\ - & - \end{vmatrix}}{\begin{vmatrix} - & - \\ - & - \end{vmatrix}} \tag{A.8}$$

Similarly,

$$\frac{\partial h_b^*}{\partial n} = -\frac{\begin{vmatrix} \frac{\partial^2 U^H}{\partial h_g^2} & \frac{\partial^2 U^H}{\partial h_g \partial n} \\ \frac{\partial^2 U^H}{\partial h_b \partial h_g} & \frac{\partial^2 U^H}{\partial h_b \partial n} \end{vmatrix}}{\begin{vmatrix} \frac{\partial^2 U^H}{\partial h_g^2} & \frac{\partial^2 U^H}{\partial h_g \partial h_b} \\ \frac{\partial^2 U^H}{\partial h_b \partial h_g} & \frac{\partial^2 U^H}{\partial h_b^2} \end{vmatrix}} = -\frac{\begin{vmatrix} - & - \\ - & - \end{vmatrix}}{\begin{vmatrix} - & - \\ - & - \end{vmatrix}} \tag{A.9}$$

The numerator of expression A.8 is positive if $\frac{\partial^2 U^H}{\partial h_g \partial n}\frac{\partial^2 U^H}{\partial h_b^2} > \frac{\partial^2 U^H}{\partial h_g \partial h_b}\frac{\partial^2 U^H}{\partial h_b \partial n}$. In equation A.9, the numerator is positive if $\frac{\partial^2 U^H}{\partial h_g^2}\frac{\partial^2 U^H}{\partial h_b \partial n} > \frac{\partial^2 U^H}{\partial h_g \partial n}\frac{\partial^2 U^H}{\partial h_b \partial h_g}$. The denominator in both expressions are positive if $\frac{\partial^2 U^H}{\partial h_g^2}\frac{\partial^2 U^H}{\partial h_b^2} > \frac{\partial^2 U^H}{\partial h_g \partial h_b}\frac{\partial^2 U^H}{\partial h_b \partial h_g}$. These conditions hold if α_g and α_b are large enough. Thus, we have that health investments and fertility are substitutes:

$$\frac{\partial h_g^*}{\partial n} < 0 \tag{A.10}$$

$$\frac{\partial h_b^*}{\partial n} < 0 \tag{A.11}$$

Since $\alpha_g < \alpha_b$, the expression for $\frac{\partial h_g^*}{\partial n}$ is greater than that for $\frac{\partial h_b^*}{\partial n}$.

Proof of Proposition 1.b.

For a given fertility, the maximized utility of the wife is given by:

$$\hat{U}^W(n) = D(n)U^H(n, h_g^*(n), h_b^*(n)) \tag{A.12}$$

Using the Envelope Theorem, the derivative of A.12 can be written as:

$$\frac{\hat{U}^W(n)}{\partial n} = D(n)\frac{\partial U^H(n, h_g^*(n), h_b^*(n))}{\partial(n)} + D'(n)U^H(n, h_g^*(n), h_b^*(n)) \tag{A.13}$$

Let n_W and n_H be the optimal fertility from the point of view of the wife and husband respectively. Evaluating A.13 at these points give:

$$\left.\frac{\hat{U}^W(n)}{\partial n}\right|_{n=n_H} = D'(n)U^H(n, h_g^*(n), h_b^*(n)) < 0 \tag{A.14}$$

$$\left.\frac{\hat{U}^W(n)}{\partial n}\right|_{n=n_w} = 0 \tag{A.15}$$

Given the concavity of $\hat{U}^W(n)$, we know that values of n greater than n_H will give increasingly negative values. Therefore, n_W must be smaller than n_H to satisfy equation A.15. Thus, $n_W < n_H$.

From A.10 and A.11, it follows that $h_g^*(n_W) > h_g^*(n_H)$ and $h_b^*(n_W) > h_b^*(n_H)$

Proof of Proposition 1.c.

The first order conditions given by A.1 and A.2 can be reduced to:

$$U_1' = U_2' \alpha_g q'(h_g)$$

$$U_1' = U_2'\alpha_b q'(h_b)$$

$$U_2'\alpha_b q'(h_b) = U_2'\alpha_g q'(h_g),$$

which indicates that parents invest health care for boys and girls up to the point at which the marginal benefit of boy's healthcare is equal to the marginal benefit of girl's health care. Since $\alpha_g < \alpha_b$, the left hand side is greater than the right hand side when evaluated at the same level of h_b and h_g. Since the marginal benefit functions are decreasing in h_b and h_g, this equation is satisfied at a point at which $h_b > h_g$.

Proof of Proposition 2.a.

The solution to equation 2.5 is also the solution to the maximization of the logarithm of the objective function in 2.5:

$$\max_{n,h_g,h_b} \gamma \ln[U^W(n, h_g, h_b) - \bar{U}^W] + (1-\gamma)\ln[U^H(n, h_g, h_b) - \bar{U}^H]$$

subject to the constraints given by equations 2.3 and 2.4. As in Proposition 1, it is convenient to do the optimization over n and h_b^* and h_g^* in two stages. First I maximize the objective function with respect to h_b^* and h_g^* for a given n, then I choose the optimal n.

For a given level of n, the first order conditions are given by:

$$\frac{\partial U^T}{\partial h_g} = \gamma \frac{\frac{\partial}{\partial h_g} U^W(n, h_g, h_b)}{U^W(n, h_g, h_b) - \bar{U}^W} + (1+\gamma) \frac{\frac{\partial}{\partial h_g} U^H(n, h_g, h_b)}{U^H(n, h_g, h_b) - \bar{U}^H} = 0 \tag{A.16}$$

$$\frac{\partial U^T}{\partial h_b} = \gamma \frac{\frac{\partial}{\partial h_b} U^W(n, h_g, h_b)}{U^W(n, h_g, h_b) - \bar{U}^W} + (1+\gamma) \frac{\frac{\partial}{\partial h_b} U^H(n, h_g, h_b)}{U^H(n, h_g, h_b) - \bar{U}^H} = 0 \tag{A.17}$$

Using the fact that $U^W = D(n)U^H$, we can rewrite A.16 and A.17 as:

$$\frac{\partial U^T}{\partial h_g} = \left\{ \gamma \frac{D(n)}{U^W(n,h_g,h_b) - \bar{U}^W} + (1+\gamma)\frac{1}{U^H(n,h_g,h_b) - \bar{U}^H} \right\} \frac{\partial}{\partial h_g} U^H(n,h_g,h_b) = 0 \tag{A.18}$$

$$\frac{\partial U^T}{\partial h_b} = \left\{ \gamma \frac{D(n)}{U^W(n,h_g,h_b) - \bar{U}^W} + (1+\gamma)\frac{1}{U^H(n,h_g,h_b) - \bar{U}^H} \right\} \frac{\partial}{\partial h_b} U^H(n,h_g,h_b) = 0 \tag{A.19}$$

Since the terms in the brackets are positive, it follows that the optimal expenditure on the health care of each child coincides with the solutions to A.1 and A.2 and are therefore given by $h_g^*(n)$ and $h_b^*(n)$, both of which are independent of γ and the threat points.

The first order condition for n in the maximization of the second stage objective function that obtains after h_b and h_g have been "maximized out" is given by:

$$\frac{\partial U^T(n,h_b^*,h_g^*)}{\partial n} = \gamma \frac{\frac{\partial}{\partial n}\hat{U}^W(n)}{\hat{U}^W(n) - \bar{U}^W} + (1+\gamma)\frac{\frac{\partial}{\partial n}\hat{U}^H(n)}{\hat{U}^H(n) - \bar{U}^H} = 0 \tag{A.20}$$

Let $n^*(\gamma, \bar{U}^W(r_2^W))$ be the solution to A.20. Plugging in the indirect utility function for $\bar{U}^W$ and totally differentiating A.20 with respect to r_2^W gives:

$$\gamma \frac{\frac{\partial}{\partial n}\hat{U}^W(n^*)}{(\hat{U}^W(n) - U_1(y^W + r_1^W) - U_2(r_2^W))^2} U_2' + \frac{\partial^2 U^T}{\partial n^2}\frac{dn^*}{dr_2^W} = 0 \tag{A.21}$$

At $n^*(\gamma, \bar{U}^W(r_2^W))$, $\frac{\partial}{\partial n}\hat{U}^W(n) < 0$, so the first term is negative. The second order condition for a maximum at n^*, which is assumed to be satisfied, is given by $\left.\frac{\partial^2 U^T}{\partial n^2}\right|_{n=n^*} < 0$ in the second term. Therefore, in order for the two terms to sum to zero , $\frac{dn^*}{dr_2^W}$ must be negative. Thus, we have that an increase in the woman's unearned income induced by the HSAA decreases fertility:

$$\frac{dn^*}{dr_2^W} < 0.$$

. Using the chain rule it follows that:

$$\frac{dh_g^*[n^*(\gamma, \bar{U}^W(r_2^W)), r_2^W + r_2^H]}{dr_2^W} = \underbrace{\frac{\partial h_g^*(n)}{\partial n} \frac{dn^*}{d\bar{U}^W} \frac{d\bar{U}^W}{dr_2^W}}_{\text{Empowerment effect (positive)}} + \underbrace{\frac{\partial h_g^*}{\partial r_2^W}}_{\text{Income effect (negative)}}$$

$$\frac{dh_b^*[n^*(\gamma, \bar{U}^W(r_2^W)), r_2^W + r_2^H]}{dr_2^W} = \underbrace{\frac{\partial h_b^*(n)}{\partial n} \frac{dn^*}{d\bar{U}^W} \frac{d\bar{U}^W}{dr_2^W}}_{\text{Empowerment effect (positive)}} + \underbrace{\frac{\partial h_b^*}{\partial r_2^W}}_{\text{Income effect (negative)}}$$

We know that $\frac{\partial h_g^*(n)}{\partial n} < 0$ and $\frac{\partial h_b^*(n)}{\partial n} < 0$ from A.10 and A.11 respectively. We also know that $\frac{dn^*}{d\bar{U}^W} < 0$ and $\frac{d\bar{U}^W}{dr_2^W} > 0$. This means the first term, which captures the empowerment effect, is positive. The empowerment effect is larger for girls, given that $\frac{\partial h_g^*(n)}{\partial n} > \frac{\partial h_b^*(n)}{\partial n}$ from Proposition 1.a.

Using the implicit function theorem and Cramer's rule, it can be shown that the second term, which captures the income effect, is negative. A similar analysis to Proposition 1.a. shows that the income effect for girls is also larger than that for boys. Thus, the signs for $\frac{dh_g^*}{dr_2^W}$ and $\frac{dh_b^*}{dr_2^W}$ are ambiguous.

Proof of Proposition 2.b.

When $r_1^H + r_2^W$ is held constant, the chain rule gives a pure empowerment effect, which as is shown in Proposition 2.a, is positive:

$$\frac{dh_g^*[n^*(\gamma, \bar{U}^W(r_2^W)), r_2^W + r_2^H]}{dr_2^W} = \frac{\partial h_g^*(n)}{\partial n}\frac{dn^*}{d\bar{U}^W}\frac{d\bar{U}^W}{dr_2^W} > 0$$

$$\frac{dh_b^*[n^*(\gamma, \bar{U}^W(r_2^W)), r_2^W + r_2^H]}{dr_2^W} = \frac{\partial h_b^*(n)}{\partial n}\frac{dn^*}{d\bar{U}^W}\frac{d\bar{U}^W}{dr_2^W} > 0$$

Proof of Proposition 3

Let $n^*(\gamma, \bar{U}^W(y^W))$ be the solution to A.20. Plugging in the indirect utility function for $\bar{U}^W$ and totally differentiating A.20 with respect to y^W gives:

$$\gamma \frac{\frac{\partial}{\partial n}\hat{U}^W(n^*)}{(\hat{U}^W(n) - U_1(y^W + r_1^W) - U_2(r_2^W))^2} U_1' + \frac{\partial^2 U^T}{\partial n^2}\frac{dn^*}{dy^W} = 0 \quad \text{(A.22)}$$

At $n^*(\gamma, \bar{U}^W(y^W))$, $\frac{\partial}{\partial n}\hat{U}^W(n) < 0$, so the first term is negative. The second order condition for a maximum at n^*, which is assumed to be satisfied, is given by $\left.\frac{\partial^2 U^T}{\partial n^2}\right|_{n=n^*} < 0$ in the second term. Therefore, in order for the two terms to sum to zero , $\frac{dn^*}{dy^W}$ must be negative. Thus, we have that an increase in the woman's earned income induced by the HSAA decreases fertility:

$$\frac{dn^*}{dy^W} < 0$$

. Using the chain rule, it follows that:

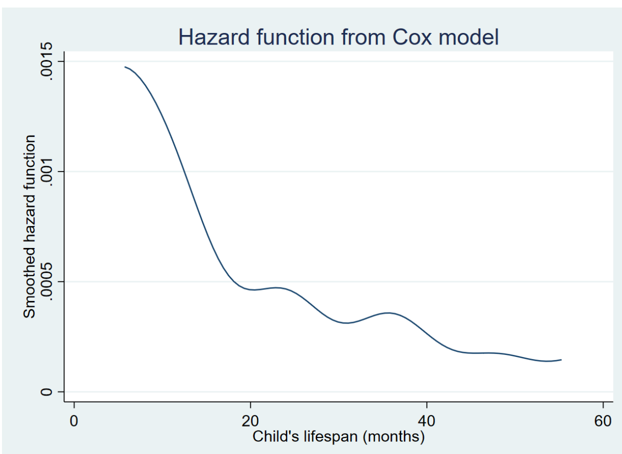
$$\frac{dh_g^*[n^*(\gamma, \bar{U}^W(y^W)), y^W]}{dy^W} = \underbrace{\frac{\partial h_g^*(n)}{\partial n}\frac{dn^*}{d\bar{U}^W}\frac{d\bar{U}^W}{dy^W}}_{\text{Empowerment effect (positive)}} + \underbrace{\frac{\partial h_g^*}{\partial y^W}}_{\text{Unearned income effect (positive)}} > 0$$

$$\frac{dh_b^*[n^*(\gamma, \bar{U}^W(y^W)), y^W]}{dy^W} = \underbrace{\frac{\partial h_b^*(n)}{\partial n}\frac{dn^*}{d\bar{U}^W}\frac{d\bar{U}^W}{dy^W}}_{\text{Empowerment effect (positive)}} + \underbrace{\frac{\partial h_b^*}{\partial y^W}}_{\text{Unearned income effect (positive)}} > 0$$

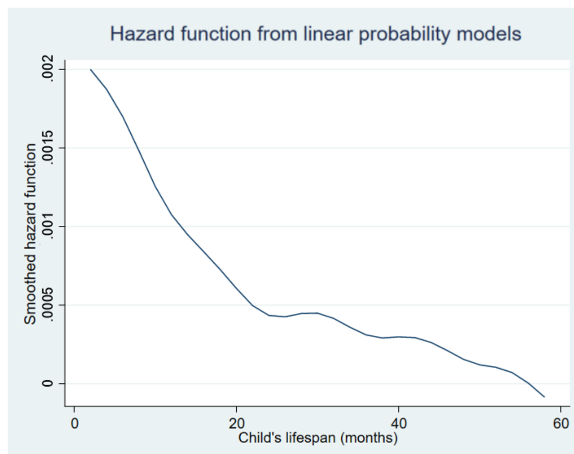
We know from Proposition 2.a. that the first term, which captures the empowerment effect, is positive. Using the implicit function theorem and Cramer's rule, it can be shown that the second term, which captures the earned income effect, is positive. Thus, the signs for $\frac{dh_g^*}{dy^W}$ and $\frac{dh_b^*}{dy^W}$ are positive.

Appendix B: Recreating hazard function using linear probability models

Figure B.1: Cox model hazard function versus LPM hazard function



A



B

The smoothed hazard function in Panel A is generated directly from the survival model given by equation 3.1 with covariates held at their means. The smoothed hazard function in Panel B is generated by estimating sixty linear probability models of the probability of dying for children at months $t = 0$ to $t = 60$ conditional on having been alive up to month t using the same specification. This pins down the hazard function without imposing any structure between each month t.

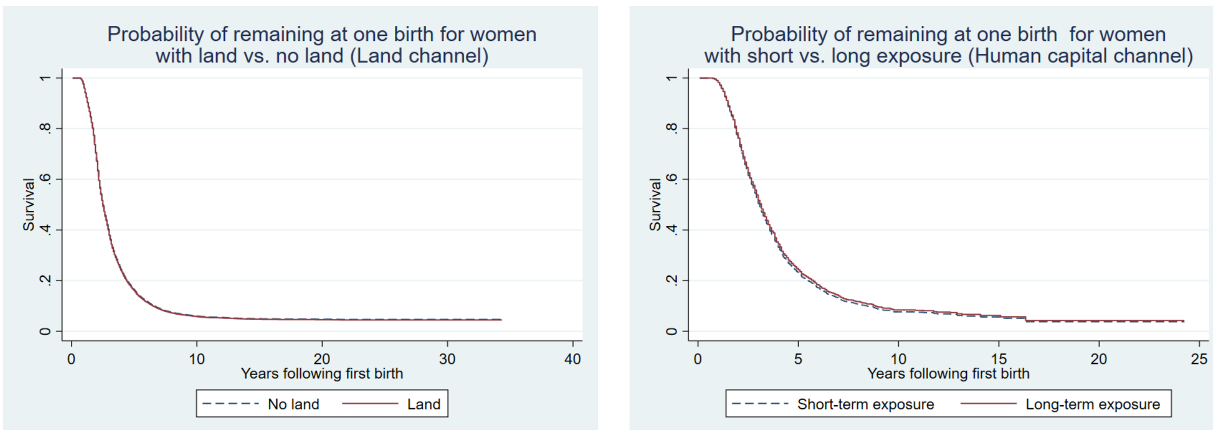
Table B.1: Effect of HSAA on probability of death (linear probability models)

	Under-5 death	Under-4 death	Under-3 death	Under-2 death	Under-1 death
Unmarried × Eligible Religion	-0.0211**	-0.0226**	-0.0206**	-0.0153	-0.0136
	(0.0105)	(0.0105)	(0.0102)	(0.00972)	(0.00935)
Unmarried	0.00121	0.00247	0.0000272	-0.00552	-0.00728
	(0.0102)	(0.0102)	(0.00990)	(0.00935)	(0.00901)
Eligible Religion	0.0177***	0.0182***	0.0174***	0.0157***	0.0136***
	(0.00388)	(0.00383)	(0.00379)	(0.00371)	(0.00357)
N	34844	34844	34844	34844	34844

Robust standard errors in parentheses. Coefficients are from linear probability models estimated using a specification similar to equation 3.1. Controls include dummies for child gender, birth order, sex composition of prior siblings, mother's age, mother's age at first birth, state, caste, a rural/urban dummy, parents' age gap, and parents' education gap. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from 0.

Appendix C: Additional tables and figures

Figure C.1: Survival functions for women at birth order 1



Graphs show Kaplan-Meier survival curves. Covariates are held at their means.

Table C.1: Effect of HSAA on under-five mortality (hazard ratios)

	Land channel				Human capital channel
	(1)	(2)	(3)	(4)	(5)
	Full sample	Sons	First born sons	Daughters	Full sample
Unmarried × Eligible religion	0.429*	0.366*	0.257*	0.514	
	(0.185)	(0.221)	(0.212)	(0.316)	
Young × Eligible religion					2.528
					(2.029)
Unmarried	1.667	1.840	4.607*	1.711	
	(0.673)	(1.029)	(3.611)	(0.965)	
Young					0.157
					(0.247)
Eligible religion	1.452***	1.428**	1.689*	1.379**	0.755
	(0.156)	(0.213)	(0.503)	(0.211)	(0.331)
N	33310	17093	5445	16217	4322

Robust standard errors in parentheses. The hazard refers to the probability of dying after month t, conditional on surviving until month t. To focus on child mortality, the latest that a subject can be in the pool and still at risk is 60 months (5 years). Columns 1 - 4 contain parameters of interest from equation 3.1, and column 5 contains parameters of interest from equation 3.2. Controls include dummies for child gender, birth order, sex composition of prior siblings, mother's age, mother's age at first birth, state, caste, a rural/urban dummy, parents' age gap, and parents' education gap. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote hazard ratios significantly different from 1.

Table C.2: Effect of HSAA on fertility (hazard ratios)

	First birth		Second birth		Third birth	
	(1)	(2)	(3)	(4)	(5)	(6)
	Land channel	Human capital channel	Land channel	Human capital channel	Land channel	Human capital channel
Unmarried × Eligible religion	1.080		1.007		1.084	
	(0.0746)		(0.106)		(0.204)	
Young × Eligible religion		0.833*		0.963		0.675*
		(0.0913)		(0.121)		(0.148)
Eligible religion	0.924*	0.966	0.904**	0.896	0.700***	0.727*
	(0.0376)	(0.0912)	(0.0362)	(0.101)	(0.0311)	(0.137)
Unmarried	0.277***		0.654***		0.408***	
	(0.0186)		(0.0637)		(0.0703)	
Young		1.599***		1.257		0.586
		(0.261)		(0.244)		(0.219)
N	10519	4926	10185	3871	9340	2519

Robust standard errors in parentheses. "Birth" is defined as the number of times a woman has given birth to a fetus with a gestational age of 24 weeks or more, regardless of whether the child was born alive or was stillborn. In columns 1 and 2, the hazard refers to the instantaneous risk of having a first child for women who have no children. In columns 3 and 4, the hazard refers to the instantaneous risk of having a second child, and in columns 5 and 6, the hazard refers to the instantaneous risk of having a third child. Controls include dummies for mother age, state, caste, spousal age gap, spousal education gap, a rural/urban dummy, and sex composition of previous births. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denotes hazard ratios significantly different from 1.

Table C.3: Heterogeneous effects on hazard of third birth by sex composition of previous births (hazard ratios)

	(1) At least one son	(2) No sons
Young × Eligible religion	0.559**	0.834
	(0.147)	(0.330)
Eligible religion	0.855	0.650
	(0.193)	(0.223)
Young	0.956	0.307*
	(0.420)	(0.211)
N	1425	1094

Robust standard errors in parentheses. The hazard refers to the probability of giving birth to the third child t years after the second birth, conditional on surviving until month t. Controls include mother's age, mother's age at first birth, caste, a rural/urban dummy, spousal age gap, and spousal education gap, and sex composition of previous children. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote a hazard ratio significantly different from 1.

Table C.4: Effect of HSAA on probability of being male (odds ratios)

	(1) Land channel	(2) Human capital channel
Unmarried × Eligible religion	0.868	
	(0.123)	
Young × Eligible religion		1.074
		(0.176)
Unmarried	1.215	
	(0.163)	
Eligible religion	1.008	0.885
	(0.0388)	(0.122)
young		1.039
		(0.216)
N	35027	8845

Robust standard errors in parentheses. Controls include dummies for child's birth order, sibling sex composition, mother's age, mother's age at first birth, state, parents' age and education gaps, and a rural/urban dummy. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denotes odds ratios significantly different from 1.

Table C.5: Effect of HSAA on mothers' human capital before marriage

	(1)	(2)
	Mothers' education (years)	Age at marriage (years)
Young × Eligible religion	0.821*	0.671*
	(0.444)	(0.355)
Young	-1.886***	-1.921***
	(0.594)	(0.445)
Eligible religion	-0.428	-0.0499
	(0.446)	(0.333)
N	5132	5133

Robust standard errors in parentheses. OLS regressions correspond to equation 3.2. Controls include dummies for woman's age, state, caste, and a rural/urban dummy. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from 0.

Table C.6: Effect of HSAA on mothers' outcomes within the marriage

	(1)	(2)	(3)	(4)
	Age at first birth (years)	Worked outside home	Decision own earning	Decision daily purchase
Young × Eligible religion	0.902***	2.146***		
	(0.326)	(0.436)		
Unmarried × Eligible religion			5.172**	1.707**
			(4.292)	(0.438)
Unmarried			0.273	0.817
			(0.224)	(0.204)
Young	-1.623***	1.329		
	(0.505)	(0.407)		
Eligible religion	-0.140	1.073	1.208	0.862*
	(0.294)	(0.181)	(0.244)	(0.0758)
N	4065	3909	4039	10223

Robust standard errors in parentheses. Column 1 estimates an OLS regression and columns 2-4 estimate logistic regressions and report odds ratios. Controls for column 1 include dummies for woman's age, state, caste, a rural/urban dummy, and spousal age and education gaps. Controls for columns 2-4 are the same and also include age at first birth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from 0 for column 1 and significantly different from 1 for columns 2-4.

Table C.7: Effect of having an HSAA mother on child mortality (sample restricted to households with non-HSAA fathers)

	(1) Mortality rate	(2) Son mortality rate	(3) Daughter mortality rate
Treated state×Cohort -20 to -16	0.00543	0.00218	0.0139
	(0.00876)	(0.00970)	(0.0109)
Treated state×Cohort -15 to -11	-0.00558	0.00214	-0.0118
	(0.00862)	(0.0100)	(0.0105)
Treated state×Cohort -10 to -6	-0.00325	0.00502	-0.0111
	(0.00801)	(0.0109)	(0.0118)
Treated state×Cohort 0 to 4	-0.0191**	-0.01000	-0.0260**
	(0.00818)	(0.00921)	(0.0100)
Treated state×Cohort 5 to 9	-0.0183**	-0.0124	-0.0191*
	(0.00772)	(0.00786)	(0.0101)
Treated state×Cohort 10 to 14	-0.00567	0.0104	-0.0187
	(0.0184)	(0.0179)	(0.0186)
Treated state×Cohort 15 to 17	-0.0386**	-0.0340**	-0.0325*
	(0.0155)	(0.0155)	(0.0168)
N	10342	9622	8210

Cohorts are binned into groups of five years. Standard errors are clustered at the state × binned cohort level (13 × 8 = 104 clusters). Controls include caste, religion, mother's age, spousal age gap, and spousal education gap. The reference cohort is the -5 to -1 category. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from from 0.

Table C.8: Test of common trends in other outcomes

	(1) Decision own earning	(2) Decision daily purchase	(3) Worked outside home	(4) Years of education	(5) Age at marriage	(6) Age at first birth
Eligible religion ×Marriage cohort	0.961	0.969				
	(0.0414)	(0.0189)				
Eligible religion ×Age at amendment			0.964	-0.0423	0.0239	0.167
			(0.0501)	(0.126)	(0.0811)	(0.128)
N	3507	8757	1370	1370	1370	1320

Robust standard errors in parentheses. The data is limited to pre-reform observations; for columns 1-2 this includes women who were married before the reform passed in their state and for columns 3-6 this includes women who were over the age of 18 and unmarried at the time of the amendment. Columns 1-3 report odds ratios from logistic regressions and control for woman's age, state, caste, a rural/urban dummy, spousal age gaps, spousal education gaps, and age at first birth. Columns 4 and 5 estimate OLS regressions and control for woman's age, state, caste, and a rural/urban dummy. Column 6 also estimates an OLS model and includes the same controls as well as spousal age and education gaps. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from from 1 for columns 1-3 and coefficients significantly different from 0 for columns 4-6.

Table C.9: Effect of HSAA through the land channel using restricted samples

	(1) Mortality	(2) Sons mortality	(3) First born sons mortality	(4) Daughters mortality	(5) Prob male	(6) Decision own earning	(7) Decision daily purchase	(8) Age gap	(9) Education gap
Panel A: Dowry Prohibition Act									
Unmarried × Eligible religion	0.408**	0.337*	0.206*	0.566	0.842	5.824**	1.761**	-0.283	0.162
	(0.175)	(0.207)	(0.168)	(0.357)	(0.122)	(4.901)	(0.482)	(0.474)	(0.353)
N	20226	10295	3527	9931	21210	2814	6909	7171	7840
Panel B: Endogenous marriage									
Unmarried × Eligible religion	0.243***	0.169***	0.112**	0.331	.813	4.885*	1.652*	-0.692	0.0521
	(0.124)	(0.111)	(0.0965)	(0.267)	(0.146)	(4.088)	(0.497)	(0.582)	(0.398)
N	29612	15241	4737	14371	31144	3469	8715	8980	10181
Panel C: Treated children									
Unmarried × Eligible religion	0.418**	0.321*	0.206*	0.568	0.867	5.048*	1.647*	0.0465	0.220
	(0.183)	(0.201)	(0.173)	(0.360)	(0.124)	(4.197)	(0.444)	(0.402)	(0.361)
N	29173	10172	3420	9801	30603	3897	9739	9776	10862

Robust standard errors in parentheses. The sample in Panel A is restricted to mothers who were married after 1986, removing the effect of the Dowry Prohibition Act Amendment of 1986. The sample in Panel B does not include mothers who were married one year before or after the reform passed in their state, removing a potential selection issue. The sample in Panel C is restricted to children who were born after the reforms passed in their state, removing the effect of treated children. Columns 1-4 report the hazard ratios from hazard models, 5-7 report odds ratios from logistic regressions, and 8-9 report coefficients from OLS regressions, all based on the model given by 3.1. Controls for column 1 include dummies for child gender, birth order, sex composition of prior siblings, mother's age, mother's age at first birth, state, caste, a rural/urban dummy, parents' age gap, and parents' education gap. Gender is dropped for columns 2 and 4 and gender and birth order are dropped for column 3. Controls for column 5 include dummies for child's birth order, sibling sex composition, mother's age, mother's age at first birth, state, parents' age and education gaps, and a rural/urban dummy. Controls for columns 6-9 include dummies for woman's age, age at first birth, state, caste, a rural/urban dummy, spousal age and education gaps. Controls for columns 8 and 9 are the same but drop age and education gaps. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote hazard ratios significantly different from 1 for columns 1-7 and coefficients significantly different from 0 for columns 8-9.

Table C.10: Effect of HSAA through the human capital channel using a restricted sample to account for endogenous marriage

	(1) First birth	(2) Second birth	(3) Third birth	(4) At least one son	(5) No sons	(6) Worked outside home	(7) Years of education	(8) Age at marriage	(9) Age at first birth
Young	0.824*	0.974	0.690	0.533**	0.987	2.437***	0.701*	0.521*	0.637*
× Eligible religion	(0.0944)	(0.132)	(0.167)	(0.154)	(0.429)	(0.519)	(0.361)	(0.280)	(0.377)
N	4784	3736	2396	1355	1041	3774	3775	3775	3775

Robust standard errors in parentheses. The sample does not include women who were married one year after the reform passed in their state, removing a potential selection issue. Columns 1-5 present hazard ratios, column 6 presents an odds ratio, and columns 7-8 presents coefficients, all given by hazard model, logistic model, and OLS model versions of 3.2. In columns 1-3, the hazard refers to the instantaneous risk of having a first child for women who have no children, of having a second child, and of having a third child respectively. In columns 4-5 the hazard refers to the instantaneous risk of having a third child for women who have already had at least one son and for women who have had no sons respectively. Columns 6-9 presents the impact of the reforms on the probability of having worked outside of the home in the last 12 months, on the mother's years of education, age at marriage, and age at first birth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote hazard ratios significantly different from 1 for columns 1-6 and coefficients significantly different from 0 for columns 7-9.

Table C.11: Event study results using restricted samples

	Dowry Prohibition Act			Endogenous marriage			Treated children		
	(1) Mortality rate	(2) Son mortality rate	(3) Daughter mortality rate	(4) Mortality rate	(5) Son mortality rate	(6) Daughter mortality rate	(7) Mortality rate	(8) Son mortality rate	(9) Daughter mortality rate
Treated state ×Cohort -20 to -16				0.00393	0.00471	0.0141	0.0246	0.0277	0.0163
				(0.00748)	(0.00954)	(0.0110)	(0.0166)	(0.0263)	(0.0183)
Treated state ×Cohort -15 to -11				-0.00336	0.00491	-0.0119	0.0274***	-0.0124	-0.0162
				(0.00737)	(0.00967)	(0.0106)	(0.00980)	(0.0120)	(0.0111)
Treated state ×Cohort -10 to -6	-0.0172	-0.000270	-0.0133	-0.00461	0.00786	-0.0110	-0.0112	0.00110	-0.0154
	(0.0126)	(0.0143)	(0.0110)	(0.00641)	(0.0106)	(0.0117)	(0.00876)	(0.0126)	(0.0124)
Treated state ×Cohort 0 to 4	-0.0340***	-0.0110	-0.0357***	-0.0199**	-0.00103	-0.0325***	-0.0276***	-0.0120	-0.0229**
	(0.0119)	(0.0128)	(0.0123)	(0.00808)	(0.0112)	(0.0106)	(0.00855)	(0.00986)	(0.00913)
Treated state ×Cohort 5 to 9	-0.0340***	-0.0135	-0.0257**	-0.0227***	-0.00976	-0.0193*	-0.0282***	-0.0135	-0.0180**
	(0.0117)	(0.0111)	(0.0120)	(0.00622)	(0.00770)	(0.0102)	(0.00856)	(0.00904)	(0.00893)
Treated state ×Cohort 10 to 14	-0.0256	0.00173	-0.0299*	-0.0103	0.0130	-0.0192	-0.000209	0.0384	0.0148
	(0.0170)	(0.0182)	(0.0163)	(0.0180)	(0.0179)	(0.0186)	(0.0347)	(0.0403)	(0.0410)
Treated state ×Cohort 15 to 17	-0.0675***	-0.0433**	-0.0635***	-0.0439***	-0.0316*	-0.0323*	-0.0493***	-0.0415**	-0.0323*
	(0.0177)	(0.0177)	(0.0174)	(0.0158)	(0.0162)	(0.0171)	(0.0178)	(0.0185)	(0.0172)
N	4012	3592	3180	9347	8693	7960	7618	7101	6449

The sample in columns 1-3 is restricted to women who were married after 1986, removing the effect of the Dowry Prohibition Act Amendment. The sample in columns 4-6 does not include any women who were married 1 year before or after the reform passed in her state, removing a potential selection issue. The sample in columns 7-9 only includes women who had at least one child born after the reform passed in her state, removing the effect of treated children. Cohorts are binned into groups of five years. Standard errors are clustered at the state × binned cohort level (13 × 6 = 78 clusters for the sample in columns 1-3 and 13 × 8 = 104 clusters for columns 4-9). Controls include caste, religion, mother's age, spousal age gap, and spousal education gap. The reference cohort is the -5 to -1 category. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from from 0.

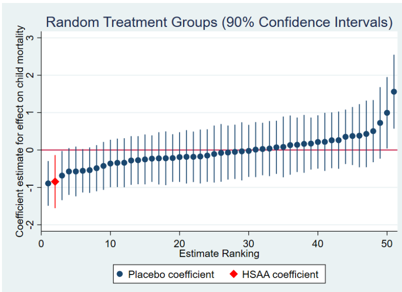
Appendix D: Robustness check tables and figures

Table D.1: Placebo treatment effects on mortality and fertility (DHS data)

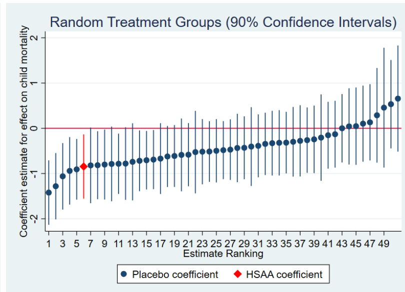
	Mortality		First birth		Second birth		Third birth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	-5 years	-10 years	-5 years	-10 years	-5 years	-10 years	-5 years	-10 years
Unmarried placebo	0.762	0.756						
×Eligible religion	(0.432)	(0.366)						
Young placebo			4.269	1.345	2.740	1.266	1.454	1.362
×Eligible religion			(4.614)	(0.562)	(4.174)	(0.336)	(0.886)	(0.367)
N	20636	12214	673	1015	655	990	553	887

Sample is restrict to pre-treatment observations, i.e. women who were married before the reforms passed in their state. Robust standard errors in parentheses. Columns 1 and 2 contain parameters of interest from specification 3.1, and columns 3-8 contain parameters of interest from specification 3.2. Controls for columns 1-2 include dummies for child gender, sex composition of prior siblings, birth order, mother's age, mother's age at first birth, state, caste, a rural/urban dummy, parents' age gap, and parents' and education gap. Controls for columns 3-8 exclude the child-level variables and include sex composition of prior births. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote hazard ratios significantly different from 1.

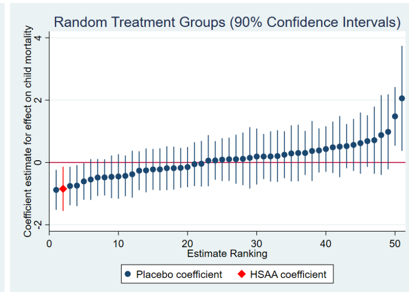
Figure D.1: Random treatment groups (90% confidence intervals)



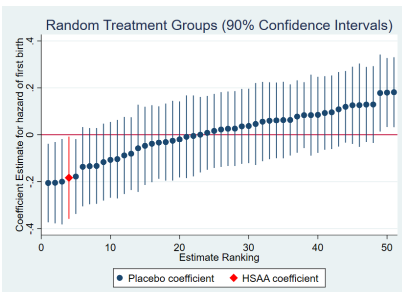
(a) Outcome: Mortality
Randomization: Religion



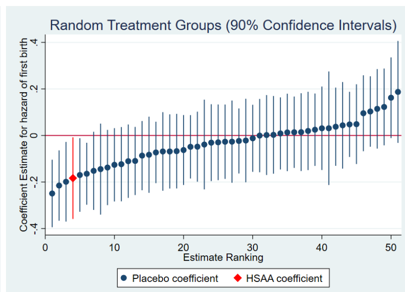
(b) Outcome: Mortality
Randomization: Year



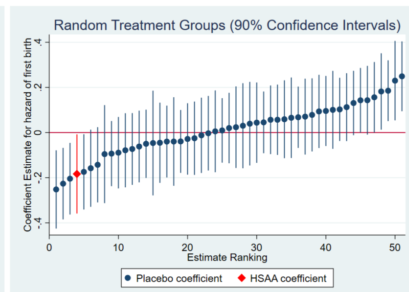
(c) Outcome: Mortality
Randomization: Religion and year



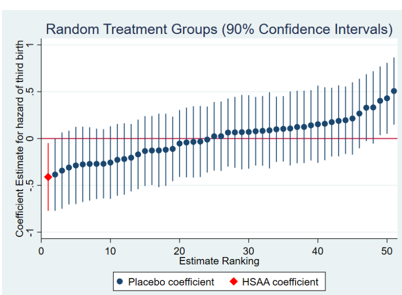
(d) Outcome: First birth
Randomization: Religion



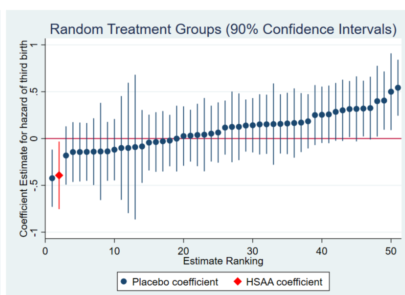
(e) Outcome: First birth
Randomization: Year



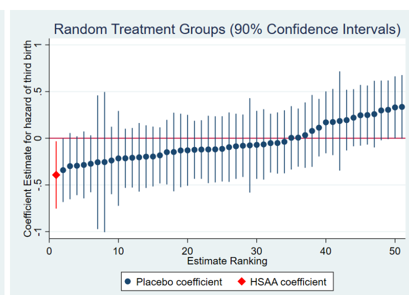
(f) Outcome: First birth
Randomization: Religion and year



(g) Outcome: Third birth
Randomization: Religion



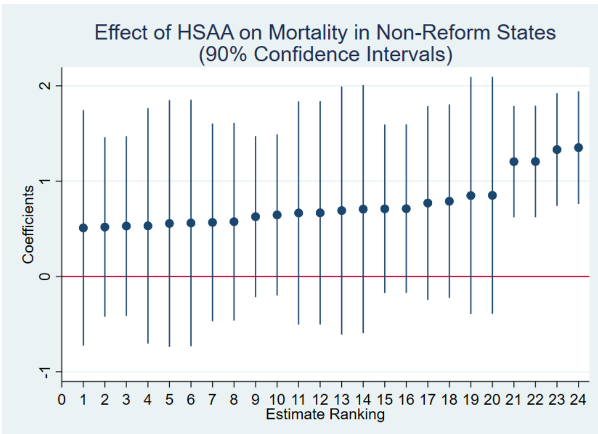
(h) Outcome: Third birth
Randomization: Year



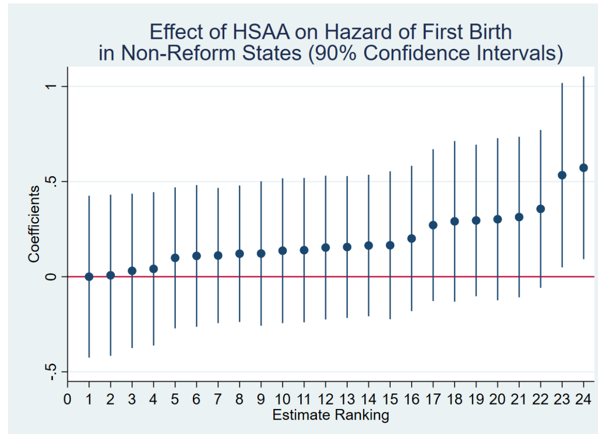
(i) Outcome: Third birth
Randomization: Religion and year

Random treatment groups are generated for each significant outcome: mortality through the land channel, first birth through the human capital channel, and third birth through the human capital channel. Coefficients from hazard models are reported. Placebo coefficients are reported in blue and true coefficients are in red. 90% confidence intervals correspond to the level of significance found for the true HSAA coefficients.

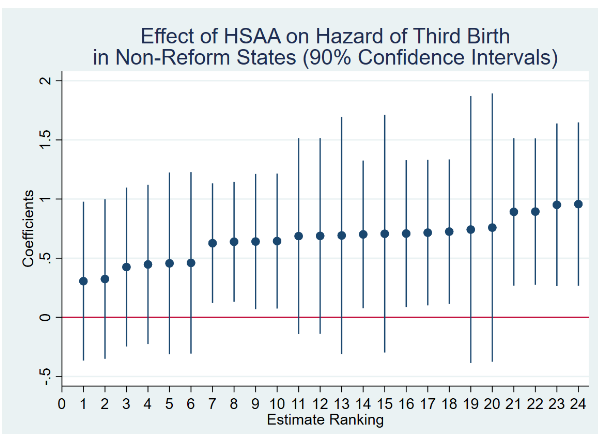
Figure D.2: Hazard model results for 24 placebo state-year combinations



A



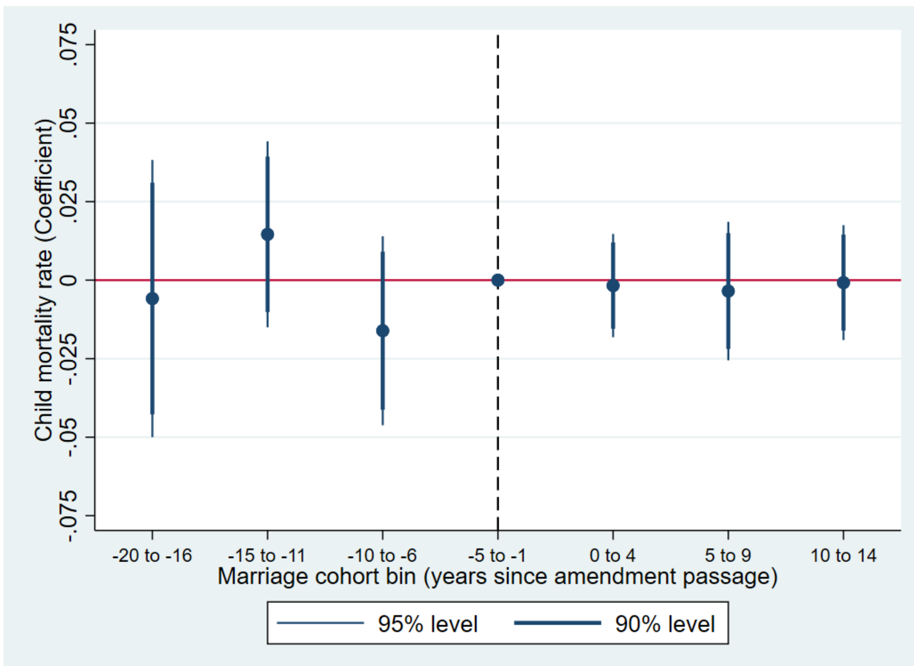
B



C

Each coefficient represents one of 24 possible combinations of reform year assignments to four non-reform states. Coefficients from hazard models are reported. 90% confidence intervals correspond to the level of significance found for the true HSAA coefficients.

Figure D.3: Event study DID coefficients using placebo religion sample



Sample is restricted to non-eligible religions. Coefficients from the interaction terms in equation 4.1 are shown. Controls include district fixed effects, group-by-cohort fixed effects, caste, religion, mother's age and spousal age and education gaps. Cohorts are binned into groups of five years. Standard errors are clustered at the state × cohort bin level (13 × 8 = 104 clusters).

Table D.2: Placebo treatment effects on mortality (REDS data)

	(1)	(2)	(3)
	-5 years	-10 years	-15 years
Treated state×Cohort -20 to -16	0.0111	0.00934	-0.0137
	(0.00759)	(0.00957)	(0.0189)
Treated state×Cohort -15 to -11	0.00816	0.0103	0.00199
	(0.00643)	(0.00957)	(0.00952)
Treated state×Cohort -10 to -6	0.00105	0.00736	0.00276
	(0.00688)	(0.00720)	(0.00809)
Treated state×Cohort 0 to 4	0.00795	-0.00101	-0.00730
	(0.00786)	(0.00659)	(0.00723)
Treated state×Cohort 5 to 9		0.00698	-0.00838
		(0.00875)	(0.00630)
Treated state×Cohort 10 to 14			-0.000334
			(0.00842)
N	8822	9543	9777

Sample is restricted to pre-treatment observations, i.e. women who were married before the reforms passed in their state. Columns 1-3 show the effect of placebo reforms if they had passed 5 years, 10 years, and 15 years before the true reform dates respectively. Cohorts are binned into groups of five years. Standard errors are clustered at the state × binned cohort level (13 × 5 = 65 clusters for column 1, 13 × 6 = 78 clusters for column 2, and 13 × 7 = 91 clusters for columns 3). Controls include caste, religion, mother's age, spousal age gap, and spousal education gap. The reference cohort is the -5 to -1 category. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from from 0.

Appendix E: Marriage market outcomes and dowry analysis

Table E.1: Marriage market outcomes (DHS data)

	Land channel				Human capital channel			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Age gap	Education gap	Husband white collar job	Husband often drunk	Age gap	Education gap	Husband white collar job	Husband often drunk
Unmarried × Eligible religion	-0.0784	0.157	1.099	0.967				
	(0.430)	(0.323)	(0.240)	(0.476)				
Young × Eligible religion					0.359	-0.145	1.057	1.649
					(0.416)	(0.337)	(0.315)	(1.062)
Unmarried	-2.112***	-1.383***	2.831***	0.265***				
	(0.406)	(0.295)	(0.576)	(0.122)				
Young					-0.200	0.136	0.747	0.926
					(0.528)	(0.454)	(0.332)	(0.939)
Eligible religion	0.0925	0.291**	0.887	1.276	-0.0365	0.357	0.867	1.073
	(0.165)	(0.135)	(0.0778)	(0.242)	(0.391)	(0.292)	(0.218)	(0.548)
N	10555	11865	7276	1637	4952	5098	2925	436

Robust standard errors in parentheses. Columns 1-2 and 5-6 report coefficients from OLS models and columsn 3-4 and 7-8 report odds ratios from logistic models. Controls include dummies for woman's age, state, caste, a rural/urban dummy. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from 0 for columns 1-2 and 5-6, and denote coefficients significantly different from 1 for columns 4-5 and 7-8.

Table E.2: Marriage market outcomes (REDS data)

	(1) Husband's father's education	(2) Distance from natal household (km.)	(3) Spouse relative
Treated state×Cohort -20 to -16	-0.242	-2.396	0.0525
	(0.368)	(3.249)	(0.268)
Treated state×Cohort -15 to -11	-0.617	-0.253	-0.0483
	(0.496)	(3.027)	(0.183)
Treated state×Cohort -10 to -6	-0.173	-4.723	-0.232
	(0.286)	(4.406)	(0.165)
Treated state×Cohort 0 to 4	-0.108	-0.873	-0.337
	(0.421)	(4.404)	(0.367)
Treated state×Cohort 5 to 9	0.438	-1.773	-0.0842
	(0.572)	(4.505)	(0.192)
Treated state×Cohort 10 to 14	0.0234	1.705	0.255
	(0.279)	(6.299)	(0.304)
Treated state×Cohort 15 to 17		-4.449	
		(5.261)	
N	2723	9980	10216

Cohorts are binned into groups of five years. Standard errors are clustered at the state × binned cohort level (13 × 8 = 104 clusters). Controls include caste, religion, mother's age, spousal age gap, and spousal education gap. The reference cohort is the -5 to -1 category. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from from 0.

Dowry analysis: In the REDS data, dowry information is collected only for the sisters of each male household head. I use this information to construct a new woman-level dataset, composed of the non-resident sisters of each male household head. The additional advantage of this data is that it contains information on all the relevant HSAA criteria for the sisters, including fathers' death year.

Table E.3: Sister-level data descriptive statistics

	Father alive on reform date	Father not alive on reform date	Difference
Number of children	2.60	2.46	-0.14*
	(1.55)	(1.80)	(0.01)
Years of education	3.44	1.95	-1.49***
	(3.78)	(2.90)	(0.00)
Age at marriage	20.54	20.09	-0.44**
	(3.71)	(3.64)	(0.00)
Current age	42.76	50.41	7.64***
	(10.51)	(11.96)	(0.00)
Scheduled caste (%)	0.17	0.10	-0.07***
	(0.38)	(0.30)	(0.00)
Scheduled tribe (%)	0.06	0.07	0.01
	(0.24)	(0.25)	(0.36)
Other backward class (%)	0.47	0.49	0.03
	(0.50)	(0.50)	(0.13)
General class (%)	0.28	0.32	0.03*
	(0.45)	(0.47)	(0.03)
Net dowry	9468.28	4437.22	-5031.06***
	(23136.53)	(11672.85)	(0.00)
Observations	2087	1438	3525

Columns 1 and 2 list average values for all variables with the standard deviation in parenthesis. Column 3 gives the difference between the two groups with the p-value from the t-test of means in parentheses.

Since dowry is not a prevalent practice among non-eligible religions, I restrict the data to women of eligible religions in reform states and estimate the effect of the HSAA in a DID framework using the woman's marital status and father's death:

$$
\begin{aligned}
Dowry_{isdj} &= \beta_0 + \beta_1 Unmarried_{sj} + \beta_2 FatherAlive_s \\
&\quad + \beta_3 Unmarried_{sj} \times FatherAlive_s + \beta_4' X_i + \lambda_d + \epsilon_{isdj},
\end{aligned}
\tag{E.1}
$$

where $Dowry_{isdj}$ is the net dowry (measured as the difference between gifts given to the groom's family by bride's family at the time of marriage minus the gifts received) for woman i of state s and district d with marriage year j. $Unmarried_{sj}$ is a dummy variable equal to one if the woman was unmarried at the time the reform passed in her state and zero otherwise, and $FatherAlive_s$ is a dummy equal to one if the woman's father was alive at the time the reform passed in her state and zero otherwise. District dummies are represented by λ_d and X_i includes controls for mother's age and caste. The coefficient of interest is β_3 and gives the impact of HSAA eligibility on dowry. Note that for women whose fathers died after the amendment passed and were not currently alive when the survey was taken, the actual inheritance should have been triggered. Thus, the treated group is a mix of both eligible women who have not yet inherited and eligible women who have.

I run two alternative specifications to account for two potential threats to identification. In the first, I restrict the sample to women who were married after 1986 in order to ensure that the Dowry Prohibition Act Amendment (DPAA) of 1986 does not apply differentially by religion. Note that the DPAA is less likely to be a threat to this identification than in my previous analyses. The DPAA affected women deferentially by marriage cohort, but there is no reason to expect that it also affected women differentially by the date of their father's death. Therefore, although any comparison between unmarried and married women whose fathers were alive at the time of the reform is biased by the DPAA, as long as the DPAA also affected unmarried and married women whose fathers were not alive at the time of the reform in the same way, this bias will be differenced out. Nevertheless, I run model using a restricted sample to ensure that the results are robust to removing the effect of the DPAA. In the second alternative specification, I account for possible selection due to endogenous marriage by estimating the model on a restricted sample in which women who were married one year before and after the reforms were dropped.

The results are reported in Table E.4, and show that the reforms resulted in an increase in dowry by Rs. 4971, which corresponds to an increase of about .2 standard deviations. The reforms are robust to accounting for the DPAA and selection due to endogenous marriage.

Table E.4: Effect of HSAA on net dowry (Rs.)

	(1) Full sample	(2) DPAA	(3) Endogenous marriage
Unmarried × Father alive	4971.3*	3322.9*	3206.0*
	(2892.5)	(1786.505)	(1723.6)
Unmarried	2737.2	4897.4*	4921.7**
	(1879.9)	(2516.2)	(2190.6)
Father alive	183.3	627.7	58.09
	(434.7)	(1254.4)	(428.2)
N	2308	518	2183

Robust standard errors in parentheses. Column 1 reports results from the full sample, column 2 reports results from a restricted sample of women who were married after 1986, and column 3 reports results from a restricted sample of women who were not married in the year before and after the reform passed in their state. Controls include mother's age, dummies for district, and caste. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$ denote coefficients significantly different from 0.

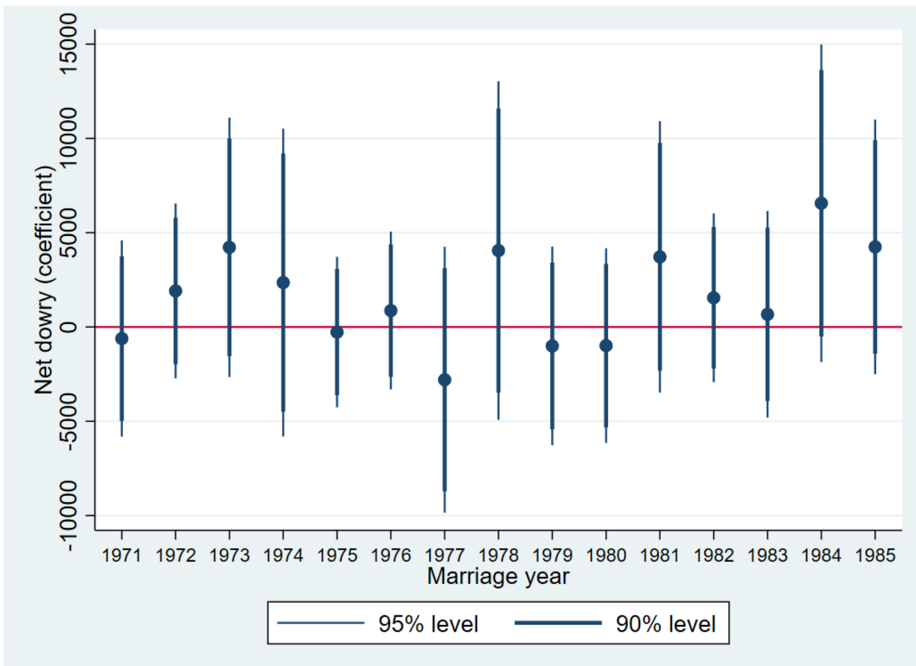
I test for common trends in three main ways. First, I limit the data to pre-reform observations (women married before the amendment passed in their state) and estimate a model identical to equation E.1, in which I regress net dowry on an indicator for having a father who was alive at the time of the reform, a linear trend of the mother's marriage year, and their interaction. Second, I conduct three falsification tests in which I assign a placebo reform year 5, 10, and 15 years prior to the true reform years and redefine the $Unmarried$ and $FatherAlive$ variables in equation E.1 accordingly. Third, I restrict the data to pre-reform observations (that is, to observations who had marriage years between 1971 and 1985, the year before the first reform passed) and regress net dowry on indicators for the year of marriage and for having a father alive at the time of the reform, and their interactions. Results for the first two tests are reported in Table E.5, and the coefficients on the interactions from the third test are reported in Figure E.1. The null coefficients indicate that there were no differential pre-trends.

Table E.5: Test of common trends for dowry

	(1) Net dowry (pre-reform with linear trend)	(2) Net dowry (-5 years placebo)	(3) Net dowry (-10 years placebo)	(4) Net dowry (-15 years placebo)
Marriage year× Father alive	-22.84			
	(48.56)			
Unmarried × Father alive		1397.2	684.3	-1733.2
		(2145.0)	(1610.8)	(1709.7)
N	2115	2018	2037	2055

Robust standard errors in parentheses. Sample is limited to pre-reform observations. Controls include mother's age, dummies for district, and caste. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ denote coefficients significantly different from 0.

Figure E.1: Test of differential pre-trends in net dowry (Rs.)



Coefficients of the interactions between marriage year and father's death indicator are shown. The omitted year is 1970. Sample is limited to women who married between 1970 and 1985. Controls include mother's age, dummies for district, and caste.

Appendix F: Magnitude of impact calculations

I first calculate the cumulative probabilities of dying before age five for two different samples of children within reform states: the counterfactual treated group and the control group.

First, my results indicate that having a mother who is eligible for land significantly reduces the instantaneous risk of dying at any given point in time by 57% (calculated from a hazard ratio of .43). I use this result to calculate the counterfactual cumulative probability of dying before age five for the treated population of children: the probability of dying if the reforms had not passed. I first calculate the hazard rates at each month of life t from 0 to 60 for the treated children and find, by taking the integral of the hazard function between 0 and 60, that the cumulative probability of dying before age five is about 1.9%. Since the hazard ratio of .43 gives the multiplicative effect of having an eligible mother, I calculate the counterfactual hazard rates by dividing each observed hazard for the treated group by .43. Taking the integral of this new counterfactual hazard function between months 0 and 60 gives me a counterfactual probability of dying before age five of 4.3%.

Second, I take the integral of the hazard function for the control children and find that their cumulative probability of dying before age 5 is 5%.

I then take the average of the counterfactual treated group probability and the control group probability, weighted by the number of live births in each group, to obtain a counterfactual probability of dying before age five for the overall population:

$$\frac{(.043 \times 15030) + (.05 \times 13439)}{15030 + 13439} = .047$$

I compare this to the true probability of dying before age which is found, by taking the integral of the full population hazard function, to be 4.3%. The difference indicates that the reforms reduced the overall probability of dying before age five in reform states by 4.7% - 4.3% = .4 percentage points.

I apply this to the mortality rate in reform states, which I calculate as the number of children who died before age five between the reform year and 2005, the survey collection year, divided by the number of live births in that period of time, expressed as the number of deaths per 1000 live births. I find an under-five mortality rate of 59 deaths per 1000 live births. My calculation suggests that in a counterfactual world with no reforms, this mortality rate would have been .4 percentage points higher, or 63 deaths per 1000 live births. This indicates that 4 children were saved per 1000 live births. I apply this to the total number of live births in each state between the reform year and 2005 to find the total number of children who were saved by the reforms:

$$\text{Total live births between the reform year and 2005} \times .004 = \text{Total children saved}$$

I obtain the values for the total lives births from Johnston (2014). I find that a total of 124,778 children were saved due to the reforms in Andhra Pradesh, 52,542 in Karnataka, 92,119 in Maharashtra, and 74,731 in Tamil Nadu between each state's respective reform year and 2005. This amounts to a total of about 344,169 children in reform states who were saved.

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