

Women's Inheritance Rights, Household Allocation and Gender Bias

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Abstract

This paper analyzes the impact of women's empowerment through improved land inheritance rights in India on their own education as well as the intergenerational effect of this reform. Using the Indian Human Development Survey data and a difference-in-differences strategy, we find that the amendment to the Hindu Succession Act is associated with a significant increase of 0.40 years in women's education, especially for those from landed households. Comparing educational outcomes of children whose mothers were affected by the policy change to the control group, we find a significant decrease in boys' education, but no impact on girls' education. We attribute this decrease to treated mothers who are more educated and are better able to assess the higher opportunity cost of education for boys in rural households than less educated control group mothers.

JEL Codes: D13, H31, I38, J18, J13, K36

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1 Introduction

Gender disparity exists in much of the developing world regarding education, health, income, asset ownership, labor markets, and participation in public and private decision-making (World Development Report, 2012). As a result, promoting gender equality and empowering women is of prime importance and features as one of the eight Millennium Development Goals. Over the years, several programs and policies have been implemented to increase women’s access to “rights, resources, and voice” (World Bank, 2001). Apart from the direct benefits to women, studies have shown that increasing women’s control over household resources can positively impact children’s welfare. This results from mothers directing more family resources towards improving children’s health and education (Qian (2008), Duflo and Udry (2004), Jensen and Miller (2010)). Further, studies find that daughters benefit more than sons when their mother’s unearned income increases (Lundberg et al. (1997), Blumberg (1988), Thomas (1990), Hoddinott and Haddad (1995), Thomas et al. (2002)). Duflo (2003) finds that pension income in the hands of women can significantly improve a girl’s anthropomorphic status. Additionally, an increase in a woman’s education has a positive impact on child education, especially that of a girl child (Afridi (2010)). Rosenzweig and Schultz (1982) and Barrera (1990) also find that maternal education reduces child mortality. We extend this literature by assessing an Indian reform that increased women’s access to property to measure its impact on women’s empowerment and the subsequent impact on her children’s educational outcome, specifically focusing on the girl child. Our paper focuses on analyzing how an increase in women’s education through the channel of improved property rights (unearned income) can improve her bargaining power over household resources, reflected through the educational attainment of her children.

The 1956 Hindu Succession Act heavily discriminated against women to be able to inherit ancestral property (usually land). Daughters were equal coparceners with respect to property acquired directly by their fathers, however, any property passed down generations could only be inherited by sons. The 2005 Hindu Succession (Amendment) Act changed these

rules, allowing for women to ask for equal share in ancestral property. This was a significant movement towards gender equality since land tenure rights are heavily biased against women in India (Agarwal, 1994). While this amendment was effective for the entire country of India (except for Jammu & Kashmir), five states of Andhra Pradesh, Kerala, Karnataka, Maharashtra, and Tamil Nadu, had already amended the law (between the 1970s and 1990s), to allow for women to be able to inherit ancestral property. This change in property rights policy is the subject of the paper.

Documented evidence suggests that even with the amendment to the Hindu Succession Act (HSA) of 1956, families did not give daughters their rightful land inheritance (Agarwal, 1994). Families with strong preferences towards sons inheriting ancestral property and who want to avoid giving daughters land, done primarily to keep the land under the family name, may have chosen to compensate daughters with greater human capital by providing them with more education. Other families may have chosen to compensate daughters with greater dowry at the time of her wedding. A growing literature has studied the various impacts the amendment to the HSA has had on women's well-being. Roy (2015) finds that women who were below the age of 10 at the time of the HSA reform, have on average 1.5 more years of education relative to those unaffected by the reform. However, the author also finds an increase in dowry payments for women close to marriageable age. Similar to Roy (2015), Deninger et al. (2013) find that a daughter's educational attainment significantly increases post HSAA implementation. The authors also note that while women's likelihood to inherit land has increased post reform, the bias between sons and daughters inheriting land is not completely erased. The amendments have also improved women's bargaining power and their labor force participation (Heath and Tan, 2015).

However, not all effects of the reform have been positive in nature. Rosenblum (2015) notes that female child mortality has increased post reform since daughters are now considered to be more costly. Anderson and Genicot (2014) find that the reform has increased the number of male and female suicides attributing this to a possible increase in marital discord

in the households. The authors further find that domestic violence increases in households with women who have improved access to property. Our paper adds to this literature by analyzing the intergenerational impact of the property rights reform. We argue that improving a woman's right to property could improve her bargaining power in the household she is married into and could impact the welfare of her children, specifically that of her daughter's. To our knowledge, we are the first paper to look at the intergenerational effects of the HSA reform by examining the impact of women being exposed to the reform on her children's educational outcomes.

To identify the impact of the amended Hindu Succession Law on women's education and household resource allocation, we take advantage of the state-cohort variation of women's exposure to the reform. The first source of variation is the timing of a woman's marriage. Women who were unmarried before the year of reform in their states form our treatment group, while women who were already married at the time of the reform are in our control group. The other source of variation comes from the states that had passed the reform before the national reform in 2005. Thus, our treatment group of states are Andhra Pradesh, Kerala, Karnataka, Tamil Nadu and Maharashtra, while the rest of the country form our control group. This allows us to use a difference-in-differences strategy to evaluate the causal impact of the reform. We perform a series of falsification tests to validate our empirical strategy and thus provide evidence that any differences in women's outcome can be attributed to the HSA amendment.

Using the 2004-05 round of the nationally representative data, Indian Human Development Survey (IHDS), we find a significant increase of approximately 0.40 years of education for women who were married after the year of the reform in a treated state. We then focus on the education outcomes of the children of mothers treated by the reform. While we find no significant impact on girls' education, our results indicate a decrease of approximately 0.15 standard deviation for boys' educational attainment in treated households compared to those households that weren't treated. To better understand these results, we divide the

sample of two children households by the gender composition: homogenous households with either only boys or only girls, and heterogenous households with one boy and one girl. We find that the decrease in boys' education is corroborated in this estimation as well.

Since the timing of marriage could be endogenous, we conduct a robustness check by using the year of birth of the women. We argue that women who were 18 years or younger at the time of reform in a reform state would be our treated group, while those above 18 years of age are the control group.¹ We use the 2001- 02, 2003, and 2004-05 rounds of the National Sample Survey Data, a nationally representative data to conduct this analysis. Our results are robust to this alternate definition of the treatment variable and the use of a new dataset.

The rest of the paper is organized as follows. Section 2 details the Hindu Succession Act and the amendments to the Act. In Section 3, we describe a conceptual framework with a literature review and then describe our empirical strategy. We detail our data in Section 4. In Section 5, we present a falsification test to test the validity of the difference-in-differences strategy and discuss our results in Section 6. A series of robustness checks is presented in Section 7 and we conclude in Section 8.

2 Hindu Succession Act of 1956

Before the Hindu Succession Act of 1956, Hindu inheritance laws were governed by two systems of doctrines, *Dayabhaga* (prevalent in West Bengal and Assam) and *Mitakshara* (prevalent in the rest of the country) (Agarwal, 1994). The two systems differed in terms of how they categorized property. *Coparcenary* or joint family property includes property that has been inherited through generations; property (such as an ancestral house, and most importantly, land) that has been passed from great grandfather to grandfather to father and so on. Additionally, any other property that was acquired both jointly or separately

¹We acknowledge the prevalence of child marriage, especially in rural India, and so the cohort between 15-18 is an inherent falsification check in our difference-in-differences strategy. This method was also employed in Roy (2015).

and merged into joint property, was also considered joint property.² *Separate property*, on the other hand, is any property that is purchased outside of the patrilineally inherited property and also any thing inherited from people other than father and ancestors and self-acquired (Agarwal, 1994). The *Mitakshara* system specifically differentiated between these two properties; women were not considered coparceners to joint property, however sons had birth right to it. For separate property, since the father (or male head of the household) had absolute power over it, he could decide on how to distribute the property to his children. On the other hand, under the *Dayabhaga* system, there was no distinction between joint and separate property, allowing for the patriarch to divide the property according to his wishes. All heirs, including, daughters, had some right over property, although preference was given to sons in terms of inheritance. However, daughters still could not inherit land and preferences were given specifically to sons. Thus, under both systems, women were heavily biased against inheriting property, especially with respect to land.

The Hindu Succession Act of 1956 (HSA) sought to unite the two systems of inheritance systems and promote gender equality regarding ability to inherit property. The HSA governed inheritance laws for anyone who was considered Hindu, i.e., Hindus, Buddhists, Jains and Sikhs.³ The HSA of 1956 applied to the entire country except for Jammu and Kashmir, the state has its own version of the Act. The Act also made special provisions for communities that were considered matrilineal. Further, tribal communities in the northeastern states are excluded from the Act since they are ruled by local customs and are also matrilineal in nature (Agarwal, 1994). The 1956 law specifically applies to inheritance in the case of a male dying intestate (without a will or settlement)⁴ and mandated that daughters and sons were to be equal inheritors to their father's separate property. While sons continued

²Agarwal (1994) also notes that joint property does not require that the family cohabits in the same household.

³Inheritance laws in India are based on religious contexts. Muslims and Christians have their own set of laws pertaining to property. These laws are considered far more progressive than the Hindu laws, in spite of there being significant biases towards women.

⁴This is of particular interest since rural households in India very rarely have any formal wills. Goyal et al.(2013) report that 65% of the population in India die without wills, and this percentage is presumed to be higher for rural households.

to be entitled to both ancestral and separate property under HSA of 1956, the law did not give daughters the right to inherit joint property. The *Dayabhaga* system was not affected since it did not distinguish between the two types of property. However, for those that were under the *Mitakshara* system, gender inequality continued to exist because of the treatment of ancestral property.

Over the course of time, states started to amend the HSA of 1956 to make it more gender equal.⁵ Specifically, Kerala (in 1976), Andhra Pradesh (in 1986), Tamil Nadu (in 1989), Maharashtra and Karnataka (1994) had all amended the Act to allow for daughters to be able to inherit an equal share of ancestral property as their brothers, with rights by survivorship. A daughter was affected by these changes only if she were unmarried at the time the reform was passed in her state. Kerala, is particularly interesting, since its amendment to the HSA abolished the system of joint property altogether. These amendments were later extended to the rest of the country in 2005, in the Hindu Succession (Amendment) Act (HSAA).

3 Methodology

3.1 Conceptual Framework

The amendments to the Hindu Succession Act were only applicable to women who were unmarried at the time of the reform in that state. In this section, we explore the underlying mechanisms through which an increase in a woman's access to property rights can improve her own well-being and, more importantly, affect her bargaining power in the household she is married into.

For a treated mother (*Generation I*), there are multiple channels through which her parents (*Generation 0*) could have transferred wealth to her. First, the most important channel is that of land inheritance, where *Generation 0* did in fact, give their daughters

⁵Inheritance in India is a concurrent issue, i.e., states also have jurisdiction to amend the laws governing them.

a share of ancestral property. With the new amendment to the Hindu Succession Law, parents could switch away from other types of property transfers to ancestral (joint) property, assuming they put equal weight on all types of property and also that various property items are easily substitutable. However, post reform, Agarwal (1994) notes that many families avoided giving daughters their rightful land inheritance. Documented evidence suggests that sisters voluntarily gave up their claims to ancestral land in favor of their brothers.⁶ There is further evidence that instead of giving ancestral property to their daughters, sonless households often adopted sons to give them their property to keep the land under the family name (Agarwal, 1998). Deninger et al. (2013) also suggest that although a daughter's likelihood of inheriting joint property increased after the reform, substantial bias in the actual implementation of it still existed. Roy (2015) empirically finds that women who were treated by the reform were no more likely to receive land than those that were not treated.

The second channel is that of human capital formation or education. Parents could substitute away from giving daughters any joint property⁷, and instead invest in her education. Deninger et al. (2013) and Roy (2015) find that daughters that were treated by the reform did experience an increase in the years of education. Roy (2015) also points that daughters who were close to the marriageable age, however, did not enjoy this increase in educational attainment. The third channel in the form of dowry payments, is a more plausible explanation of wealth transfer, especially for daughters close to marriageable age. Roy (2015) finds that dowry payments for them increased by approximately 50%. However, for girls between 0-10 years of age, their dowry payments decreased by 28%.

While Deninger et al.(2013) and Roy(2015) have looked at the direct effects of the HSAA on women who were affected by it, other studies have looked at the intra-household bargaining power of these women. Heath and Tan(2015) suggest that these women have an increased

⁶Apart from cultural norms which discourage women from claiming their rights, the main reason for this is that women want to maintain access to their natal homes in case of "economic, social, and physical vulnerability in case of marital discord, ill-treatment, marriage break-up or widowhood." (Agarwal, 1994).

⁷Since women leave their natal house for that of their husband's, her maternal family might be reluctant for the property to leave the family.

say in their house which in turn increases their labor supply participation. Anderson and Genicot (2015) show that post reform, increased bargaining power leads to greater marital discord in a household, further increasing suicide rates for both women and men. Our paper extends this literature on intra-household bargaining by specifically looking at how children are affected when their mothers have improved access to property rights through the amendments to the HSA.

Existing literature on household decision making has rejected that households pool income and instead established that a woman's bargaining power in household decision making increases when her contribution to household wealth increases (Attanasio and Lechene (2002), Duflo and Udry (2004), and Jensen and Miller (2010)). Thomas (1990) finds that in Brazil, an increase in unearned income for women improves the nutritional status of the girl child. Qian (2008) finds that increasing women's income increases overall education levels irrespective of the gender of the child, and significantly raises the survival rates of girls. Duflo (2000) finds in the case of South Africa that only when women are recipient of government cash transfers that nutritional status improves for children. Specifically, the anthropomorphic status of girls' increases, while there is no impact on boys. Our paper adds to this literature by studying the impact of mothers with greater access to property rights (and as a result greater bargaining power) on her children's (*Generation II*) educational outcome. If mothers place greater value on education, then they would use their bargaining power to increase her children's educational attainment. However, if mothers in rural areas deem the opportunity cost of education to be high, then they may use their bargaining power to decrease the number of years of children's education. For example, if mothers realize that quality of education is low, possibly due to teacher absenteeism and poor teacher quality (Banerjee, 2006), then they may consider it better to take their sons out of school to train them in farming or non-farming related education skills, thus increasing their potential future earnings.

3.2 Empirical Strategy

To be eligible for the amendment, a woman should have been unmarried at the time of the reform. Thus, we exploit the woman’s year of marriage as our first source of variation.⁸ Women who were married after the year of the reform in their state are considered treated by the policy, and those married before the reform form the control group. Our second source of variation comes from the states that passed the amendment prior to the 2005 national amendment in 2005. The treated group of states are Andhra Pradesh (1986), Karnataka (1994), Kerala (1976), Maharashtra (1994) and Tamil Nadu (1989).⁹ The rest of the states in our dataset form our control states. One concern could be that women might migrate from a control state to a treated state after marriage. However, as Roy (2015) notes, migration to a state that has a reform is not a problem since the percentage of migration to different states is close to being negligible. We first focus on *Generation I* women and estimate the effects of the amendment on their years of education using the following regression equation:

$$educ_{ist} = \alpha + \beta_1(HSAA_{ist}) + \lambda_s + \delta_t + X_{ist}\Pi + \epsilon_{ist} \quad (1)$$

where $educ_{ist}$ is the completed years of schooling of *Generation I* mother i in state s married in the year t . $HSAA_{ist}$ is a dummy variable that takes on the value 1 if a woman i belongs to reform state s and was unmarried in the reform year t in her state, and the value 0 if she was not exposed to the reform. X_{ist} is a vector of individual and household characteristics including religion, caste, average age of men in the households she married into, average age of women in the household she married into, if the family owns land or not, total income of the household, total farm income of the household, if they are below poverty line (BPL), family occupation type, if the wife is the same caste as her husband,

⁸A caveat to using this method of identifying the treated cohort is that parents could delay (those who are gender progressive) or prepone (those who do not want to devolve property to their daughters) their daughters’ marriage. Hence, we also control for age at marriage to correct for this bias.

⁹We exclude Kerala from our main estimation since Kerala’s reform was different than the rest of the country and was much before the rest of the country. We include Kerala in our robustness check later on and our results are largely unchanged.

their economic status as their husband’s and land dowry practice. λ_s are state fixed effects and δ_t are year of marriage fixed effects. Finally ϵ_{ist} is the error term. β_1 is the difference-in-differences estimate that gives us the difference in education outcomes between a treated woman and a control woman due to the amendments to HSA. All errors are clustered at the district level, and we restrict our sample to all landed Hindu households.¹⁰

We then shift our focus to the children (*Generation II*) of the women from *Generation I* to understand the intergenerational effects of the HSAA reforms. A household with a mother exposed to the reform is likely to have more bargaining power than those who were not affected by the HSAA. It is of interest to analyze whether this increased bargaining power translates to an increase in her children’s educational attainment, especially that of her daughters. Our first equation to analyze the intergenerational outcomes is the following:

$$y_{ist} = \alpha + \beta_2(HSAA_{st}) + \lambda_s + \delta_t + X_i\Pi + \epsilon_{ist} \quad (2)$$

where y_{ist} is the measure of educational outcome of the child i belonging to a mother in state s who was married in the year t . Following Quisumbing and Maluccio (2003) and Afridi (2010), educational attainment is measured as the deviation of the child’s highest grade completed from the average grade attainment. This is done to account for incompleteness of years of schooling caused by any reasons, providing a consistent measurement of grade attainment relative to the child’s comparable group. $HSAA_{st}$ is a dummy variable that takes on the value 1 if the child’s mother belongs to a reform state s and was unmarried in the year of the reform t and 0 otherwise. λ_s are the state fixed effects, δ_t are the year of marriage fixed effects. X_i is a vector of individual characteristics similar to the ones in Equation (1) and ϵ_{ist} is the error term. The coefficient of interest is β_2 , which gives us the difference in children’s education outcomes between treated and control households.

Given the persistence of significant gender inequality in access to education and edu-

¹⁰To account for differences among regions with similar economic and social characteristics, we include district fixed effects. Since districts are administrative divisions of states, accounting for district fixed effects, controls for state level unobservable variations. This is our preferred specification of estimation.

cational attainment, we analyze the additional impact of the policy on the girl child by estimating the following equation:

$$y_{ist} = \alpha + \beta_3(HSAA_{st} * female_i) + \beta_4(HSAA_{st}) + \beta_5(female_i) + \lambda_s + \delta_t + X_i\Pi + \epsilon_{ist} \quad (3)$$

where $female_i$ is a dummy variable that takes on the value 1 if the child i is female and 0 otherwise. β_4 gives the impact of the property rights reform on boys' education. β_3 gives the differential impact of the amendment on girls.

4 Data

For our analysis, we use the 2004-2005 wave of the Indian Human Development Survey (IHDS), a nationally representative sample consisting of 41,554 households from over 25 states and union territories of India. The survey covers 1503 villages and 971 urban neighborhoods. We restrict the sample to landed Hindu households in rural areas, reducing our main sample to 13,803 households. The states in our treatment and control groups are: Andhra Pradesh, Bihar, Chhattisgarh, Gujarat, Haryana, Himachal Pradesh, Jharkhand, Karnataka, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttaranchal, and Uttar Pradesh. Jammu and Kashmir, West Bengal, Kerala, and the Northeastern states have not been included in our data.¹¹

The IHDS contains detailed information on household characteristics such as religion, caste, size of land holding (converted to hectares), main household occupation, and household

¹¹Jammu and Kashmir is not in the sample since it has always been excluded from the HSA of 1956. In the late 1970s, West Bengal passed a successful and highly ambitious redistributive land reform measure, which most likely affected the amount of land that women inherited in the 1970s and 1980s. Since land redistributive measures in West Bengal were during the time the 5 treatment states amended property rights, we exclude West Bengal from the control group. Also, West Bengal and Assam followed the Dayabhaga system of property rights, which allowed daughters to inherit all types of property, thus making them unsuitable as control groups. Further, Kerala, in 1975, passed a different amendment to the Hindu Succession Act which impacted property inheritance by women in the state. We omit households from the state of Kerala to ensure that our treatment group consists of households experiencing a similar change in property rights. Union Territories are also not considered in our sample since politically and administratively they differ from the rest of the states in India. Additionally, we exclude Delhi as we are only focusing on the rural areas in this paper.

income. The survey has a special section on education and contains data on the years of schooling and the current spending on education for all individuals. Additionally, the survey also collects data on *eligible women*, women who are currently married and are between the ages of 15 and 49. This section contains information on their year of marriage (and consequently, age at marriage), allowing us to identify women who were exposed to the reform versus those who were not. We are, therefore, able to create the treatment and control group in the following manner. Unmarried women in, for example, Andhra Pradesh (that passed the reform in 1986) were exposed to the reform and were eligible to inherit land. These women form our treatment group. Women who were already married by 1986 were excluded from inheriting property and thus are in our control group. We use this method for all 16 states to form our treatment and control group. Since we use year of marriage to identify the treatment group, we only focus on women who are interviewed in the “eligible women” section of the questionnaire.

Table 1 shows that the 5 states that amended the HSA and the 12 states that did not, have a comparable percentage of Hindu and Muslim households in the sample. Among the Hindus, the percentage of high caste Hindus and the lower caste Hindus are similar in the reform states and non-reform states. When we focus on the percentage of households that are landed, we find that, in the reform states, 54.7% of households have some land holding while only 35.5% of non-reform state households have land holding. The average size of the land holding in the non-reform states is 1.8 hectares and this is higher than the average of 1.37 hectares for land holdings in the reform states. However, the difference is not statistically significant.

In Column 4 and 5 we only look at the reform states. In Column 4, we focus on women affected by the reform (treated women) and in Column 5, on women who were not affected by the reform but resided in the reform states. It is important to note that marriage age for treated women is significantly higher than women who were not affected by the reform in these states. Also, the number of years of schooling attained by women impacted by the

reform is greater than women not affected by the reform. However, the average income in a household with a treated woman is lower than the mean income in a household where there is no treated woman. This would likely explain the higher spending on education for both boys and girls in households with no treated women.

5 Falsification Test

The main identifying assumption in a difference-in-differences analysis is that the parallel trend assumption holds true; that the educational outcomes of the treatment and control group of women would have been the same in the absence of the reform. The estimated coefficients will capture the true causal effect of the reform only if the variation in the reform’s timing and setting is not related to unobserved shocks and trends that differentially affected women who were unmarried at the time the reform was introduced in her state. In our study, one challenge to our difference-in-differences analysis is the substantial differences that exist between the reform and non-reform states (Table 1). Women in the reform states tend to be married at a later age compared to those in the non-reform states, which could have influenced her additional time in school. With respect to household characteristics, a household’s average landholding in reform states is 1.8 hectares, while in the non-reform states the average landholding is 1.37 hectares. However, surprisingly, 35.5% of households in the reform states have some form of land holding while compared to the 54.7% in the non-reform states. Also, average household income in reform states is Rs. 49975, while the average household income in non-reform states is Rs. 45007. Due to these baseline differences, it is important for us to test the validity of the difference-in-differences strategy.

We conduct a falsification test by examining the patterns in women’s educational attainment in the pre-reform period when educational attainment of *Generation I* women should not have been affected by the changes to property rights. We first focus on the reform state Andhra Pradesh (AP) and use 1981 as the false start for property rights reform.¹² Specifi-

¹²Excluding Kerala from our analysis, makes AP the first state to have passed the reform. Hence, we

cally, we estimate an equation analogous to equation 1 but with data for AP till 1985, i.e., a year before the reform was actually implemented in the state. This exercise allows us to see whether women in the reform states and non-reform states had different pre-trends in educational attainment.

The results of this exercise are presented in Table 3. In Columns 1 and 2, we first focus on the reform state Andhra Pradesh (AP) and use 1981 as the false start for property rights reform. Specifically, we estimate an equation analogous to Equation (1) but with data for AP till 1985, that is, the year before the reform was actually implemented in the state. In Column 1, we include the control group used in our earlier analysis. In Column 2, we further include the three other reform states (Maharashtra, Karnataka, and Tamil Nadu) to our control group. The reform was introduced in Karnataka in 1989 and in Tamil Nadu and Maharashtra 1994. Since the reform was enacted in the states in different years, we restrict the data for these states to before the reform set in. The point estimates of β_1 are close to zero and statistically insignificant, indicating a common pre-trend and providing some support for the identification strategy. In Column 3, include all the reform states in our treatment group but give them all a false start date of 1981. Again we restrict the data for these four states till before the start of the property rights reform. The point estimate of β_1 is small and positive but not statistically significant.

Finally in Column 4, we use the regression Equation (1) to conduct our analysis for the Muslim population. Since the Hindu Succession Act did not affect Muslims, we should not expect women in Muslim households to be impacted by the reform. In our data, we have 10.6% Muslim households in the non-reform states and approximately 7% Muslim households in the reform states. We find that the coefficient for the treatment variable HSA reform is not statistically significant. These findings lend support to make us confident of the validity of the difference-in-differences strategy used in the above section.

focus on AP to evaluate our falsification tests.

6 Results

With the introduction of the amendments to the Hindu succession law, women were legally entitled to a share of the ancestral property which they contribute to their “new” household after marriage. This increase in unearned income could potentially raise their bargaining power in households as seen in Duflo (2008), Qian (2008) and Jensen and Miller (2010). In the literature, improvements in women’s income are associated with increases in household well-being, especially for children. One way to measure this improvement is through examining the changes in educational outcomes. In Table 2, we focus on the impact of the Hindu Succession (Amendment) Act on a woman’s educational attainment (the effect on *Generation I*). Table 3 presents a series of falsification tests where we test the validity of our difference-in-differences strategy. Tables 4, 5 and 6 specifically look into the inter-generational effects of the reform on the children (*Generation II*) of mothers treated by the reform compared to those who were not. Finally, in Table 7, we present our robustness check where we look at an alternate definition of the treatment variable and also use a different data set.

6.1 Impact on Women’s Education (*Generation I*)

The amendments to the Hindu Succession Act were only applicable to women who were unmarried at the time the reform was introduced her state. If *Generation 0* preferred to maintain ancestral land under their family name by giving the land only to their male heirs, then they might have tried to compensate their daughters with human capital. In Table 2, we focus on the impact of HSAA reform on *Generation I* women in order to see if being eligible to inherit ancestral property created an incentive for her parents (*Generation 0*) to increase her human capital. We find that the HSAA significantly increased *Generation I* women’s education, on average, by 0.40 years.

Panel A of Table 2 presents results for the full sample. Column 1 reports the results

of estimating Equation 1 with the basic controls discussion in Section 5.¹³ We find that the HSA reform increased women’s education significantly by 0.39 years. Households that own land may react differently to the reform than non-land owning households. Since we do not have information on whether a woman’s parents owned land, we proxy for that (in Column 2) by including a control for whether the household, which the woman has married into, owns land. The coefficient decreases slightly but is still positive and significant at the 10% level. In Column 3, we strengthen the proxy for land ownership by adding more variables which control for whether women married men with a similar background. These variables control for whether women married men from the same caste and similar economic status. We also control for whether husbands and wives are blood relatives.¹⁴ Including these controls, the coefficient of interest decreases to 0.35 years of education, and our results are no longer significant. Finally, in Column 4, we add more controls for total land holding and the main occupation of household. Using the full range of controls, the coefficient remains unchanged.

We conduct a similar analysis in Panel B of Table 2 by restricting the sample to women who married into landed households, to proxy for same economic status and land ownership. This is particularly important to look at since the reform was especially significant for households with land ownership. Our results are still positive and significant, we find that women influenced by the reform saw a significant increase of approximately 0.50 years of education compared to those who weren’t exposed to the reform.

6.2 Impact on Children’s Education (*Generation II*)

Given strong evidence of inter-generational transmission of human capital (Black et al.(2005), Currie and Moretti (2007), Emerson and Souza (2003)), we extend our analy-

¹³These include age at marriage, year of marriage fixed effects, household total income, BPL, caste, average age of men in the household, average age of women in the household, and district fixed effects.

¹⁴Controlling for blood relatives also serves as a proxy of the economic status of the household that the woman married to. Marrying within the same family or blood relatives tends to be common to make sure that the property does not get devolved to the outside community.

sis by examining how the increase in women’s education through the HSA reform translates to the improvement of her children’s education. To answer this question, we first identify the children of the eligible women in our sample, allowing us to estimate the direct educational impact of the HSAA reform on a her children. Children with mothers who were impacted by the new policy are part of the treated household, while those whose mothers were not exposed to the reform are part of the control group of households. In India, children typically start primary schooling at age 5 and since the legal age to start work is at 14 and above, we drop anyone below the age of 5 and above the age of 14. Since landowning households are most affected by the new property rights reform, and that the increase in mothers’ education is most pronounced in the sample of only landed households, our analysis below will focus only on the sample of households that own some fraction of land. Employing a difference-in-differences strategy similar to the one above, we estimate Equation 2 and present the results in Table 4.

Panel A presents results for all households in our sample, i.e., we place no initial restriction on the number of children that the surveyed women have. In Column 1 of Panel A, our results show that overall, there is a 0.118 standard deviation reduction in the years of schooling of children of mothers impacted by the reform. One explanation behind the decrease in education in these treated households could be that they are, in fact, transferring land to daughters as is their right under the HSA reform. If families are engaging in this transfer, then households with son preference may reduce human capital investment in daughters since they are providing the daughters with land. To tease out the underlying mechanisms behind the results and to get a sense of any gender bias, we divide our sample into households with girls (Column 2) and households with boys (Column 3). We do not find a significant impact of the reform on girls’ education (Column 2), however, the main result is driven by a 0.145 standard deviation significant decrease in boys’ education (Column 3).

While the results above suggest declining education levels for boys, this could be a facet of larger families with more girls and less boys. Given strong son-preference in India, families

could be using stopping rules (benevolent sex selection) as a fertility mechanism. Stopping rules are used to achieve the desired number of sons, families following these rules are often larger and have more number of girls.¹⁵ An underlying mechanism behind our results from Panel A could be explained by the fact that in treated households more education resources are being substituted away from boys towards girls, resulting towards the girls and hence, being taken away from boys. To control for this, we restrict our sample to households with only 2 children. Our results largely mimic the ones presented in Panel A. Boys see a significant decrease in their years of education in households where the mothers were treated to better property rights.

The intergenerational transfer of mothers' education to her children could be heterogeneous based on the age of her children. We divide the sample into two age groups: 5-9 and 10-14. In both our sample specifications, the decline in education is statistically significant for children between the ages of 5-9. Further, the effects are most pronounced for boys of that age in the 2 children households. Recent reports from the Census of India indicate a rapid decline in education statistics (LiveMint, September 25, 2015). Based on the nationally representative data, National Sample Survey, approximately 39% of rural students in 2011-12 were no longer enrolled in school. The survey highlights that one of the primary reasons for children between 5-14 dropping out of school was that education was deemed unnecessary.¹⁶ This is in part due to the rising wages in India, making the opportunity cost of education higher, especially for boys in rural households. While we find no statistically significant results for girls, we do find an increase in their educational attainment. Anecdotal evidence suggests that, although households are weary of the quality of schools, they consider sending their daughters to schools to keep their daughters safe. Thus, although the results are not statistically significant, they hold some economic significance.

Although the restriction to two children attempts to control for gender preference, some

¹⁵Stopping Rules are termed *benevolent* selection methods and do not alter the sex-ratio. This is in contrast to *malevolent* selection methods which includes sex-selective abortions and female infanticide, negatively impacting the sex-ratio.

¹⁶The group of children between 5-9 in 2004-05 are 11-15 in 2011-12.

unobservable variables affecting the education of the children in the households could potentially bias our results. We categorize the two children households into three types, based on the gender composition of the children: households with two sons, households with two daughters, and households with one son and one daughter.¹⁷ We hypothesize that in gender homogenous households, we should see an increase or no significant effect in the educational outcomes of the children. For households with one boy and one girl child (B-G households), overall education for both children in treated households is likely to increase, since mothers in these households have more bargaining power. However, one could also hypothesize that the increase in education for girls might take away resources from the boys, and we could see a decrease in the average education, especially if the substitution away from the boys is significant. Similar to the results in Table 4, we see that in Table 5, the education decrease is significant for treated households with two boys households. We find no significant impact on children’s education in the other remaining categories.

Additionally, studies have shown that an improvement in women’s bargaining power and education positively impacts her children’s welfare, specifically that of her daughters’ (Afridi, 2010). To estimate the additional impact of the HSAA on girls’ education, we estimate equation 3 and present our results in Table 6. Column 1 presents estimates for households with at least one boy and one girl. While we see no additional impact on girls, we see a 0.161 standard deviation decrease in boys’ education. The sample in Column 2 is restricted to only one boy and one girl (2 children) households, and we find no significant impact of the policy on children’s education. Thus, in all of our specifications, we find that in households with educated mothers (through the channel of improved inheritance laws), boys are negatively impacted with a significant decrease in their years of schooling.

¹⁷Since the dataset covers a period of 2004-2005, and the national amendment was introduced in 2005, the only difference between the children in treated and control households stems from *Generation Imothers* who were exposed to the reform and those who were not exposed to the reform.

7 Robustness Checks: Year of Birth and NSSO data

Our analysis, thus far, has shown a significant increase in women’s education due to the HSAA. A potential endogeneity issue with using the year of marriage to identify our treated group is that the year of marriage could be impacted by the year of reform. On the one hand, parents could intentionally get their daughters married early (if they had anticipated a change to the succession law). On the other hand, they could have delayed their daughters’ marriage and increased their education levels, to avoid giving them property. Thus, although the policy was for women who were unmarried at the time of the reform, we identify the treatment group using another source of variation that is independent of the year in which the reform was passed. Also, since the national amendment to the HSA went into effect in 2005, and that coincides with the 2004-05 round of IHDS used in our study, we use a different data set to conduct this robustness check.

We assess the impact of Hindu Succession (Amendment) Act on women who were directly affected by the introduction of the Act by focusing on their educational attainment. For our analysis we use the Consumption Expenditure Survey conducted by the National Sample Survey Organization (NSSO) for 2001- 02, 2003, and 2004-05. The NSSO surveys are nationally representative household surveys and they cover nearly all the districts in India. The NSSO data set does not have the years of education; it only reports the level of education completed by each member of the household. Thus, we translate education level to the number of years of schooling completed by each individual in the household. We use the maximum number of years one spends in school at a particular education level without failing at any stage of the process. For example, the education level corresponding to high school, therefore, translates to 16 years in school. For our analysis, we use the 19 major states of India and restrict the sample to rural households and focus on the rural areas. The States are: Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Haryana, Himachal Pradesh, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab,

Rajasthan, Tamil Nadu, Uttaranchal, Uttar Pradesh and West Bengal.¹⁸ As in the above analysis, Jammu and Kashmir and the Northeastern states have not been included.

We hypothesize that women who were of school-going age at the time of the reform in one of the five states that enacted the amendment, could have experienced an increase in their education level.¹⁹ To do this, we first identify women (mothers in our data set) who were below 18 years of age at the time of the reform; these women form our treated cohort who are most likely to be affected by the change in the policy.²⁰ Our second source of variation stems from the five states that had already passed the amendment before 2005. The treatment states are the states of Andhra Pradesh (1986), Maharashtra (1994), Kerala (1976), Karnataka (1994) and Tamil Nadu (1989) and the other 14 states passed the amendment in 2005 and hence are our control group. Thus the treatment group constitutes the cohort of mothers who were under 18 at the time of the reform and lived in a reform state.²¹ We employ a difference-in-differences strategy to estimate the effects of the property rights on a treated cohorts' education. We further divide the mothers' ages into various cohorts. The primary equation we estimate is the following:

$$y_{ics} = \alpha_1 + \beta_1(HSAA_{ics}) + \lambda_s + \delta_c + \gamma_s * (cohort_c) + X_{ics}\Pi + \epsilon_{ics} \quad (4)$$

where y_{ics} is the education of the mother i who belongs to cohort c in state s . $HSAA_{ics}$ is the treatment variable that takes on the value 1 for treated cohorts c in the reform state s . The treated cohorts are the following: 0-5 (primary school), 6-10 (middle school), 11-15 (high school) and 16-18 (grades 11-12). The omitted group is the cohort above 18 at the time of the reform. λ_s controls for state trends, δ_c are cohort fixed effects. To account for any differential effects of the policy, we include the term $\gamma_s * (cohort_c)$ to control for state

¹⁸They account for nearly 96% of India's population in the 2001 Census.

¹⁹This approach has been employed by Roy (2015) and Goyal et al. (2013).

²⁰Although the legal age of marriage for women is set at 18, it is not uncommon for girls to get married before 18 years of age. This practice is particularly prevalent in rural areas in India. Thus, to check the robustness of our results, we include women between 16-18 years (at the time of the reform).

²¹As noted in Roy(2015), migration to a state that has a reform is not a problem since the percentage of migration to different states is close to being negligible.

specific cohort trends. Finally, ϵ_{ics} is the error term. To control for serial correlation and heteroskedasticity, the standard errors are clustered at the district level. The coefficient of interest is β_1 which gives the differential impact of education for each of the cohorts compared to women above 18 years of age at the time of the reform.

The results of this estimation are presented in Table 7. Column 1 shows the impact on women aged 18 or less at the time of reform the compared to those aged 18 or more, controlling for state and cohort fixed effects. We find that the HSAA has a significant impact on education. The average increase in the years of education for all women exposed to the reform was 0.72 years. In Column 2, we focus on mothers who had been exposed to treatment by cohort (younger cohort were below 10 years during the time of reform and older cohort were above 10 years but below 18 at the time of reform), we find that only that there is a positive and significant effect only for the younger cohort. The number of years of education increases by 0.83 years for the younger cohort. We find similar results in Column 3, which further breaks down the treatment variable into different cohorts as specified in the empirical section. We find that the younger cohorts experience significant changes in educational achievement due to the HSAA, where as the older cohorts do not experience any boost in terms of education as a result of the reform. In fact, for Cohort 3 ((11-15) the effect is negative, although not significant.

Quite often, by age 16, most women in rural India are married, in spite of the legal marriage age for women being 18.²² The results presented for the oldest cohort likely stem from the fact that girls in that age group may have already been married and therefore not in school at the time of reform. For those girls who were in school, they were also likely to get married before the age of 18. Although this cohort is technically in the treated group, the practice of marrying daughters by the age of 18 allows us to use this last cohort to conduct a falsification test to ascertain if the impact of HSAA is restricted only to the unmarried girl

²²The 2013 UNICEF Statistics and Monitoring Section, Division of Policy and Strategy reported that India is the top country with child brides. The official number is 10063 (in thousands) woman aged 20-24 at the time of the survey who were married before 15.

at the time of reform or whether there are spillover effects in the control group. As we see in the table, the oldest cohort indeed does not benefit from the reform in terms of educational attainment. This gives confidence in our results.

8 Conclusion

This paper studies the impact of the property rights reforms mandated by the Hindu Succession (Amendment) Act of 2005. The amendment allowed for unmarried women to be equal shareholders as their brothers, specifically with respect to ancestral property. Since women in India are discriminated against with respect to tenure rights, this amendment attempted to move a step closer to gender equality. Five states in India had already passed the reform by the early 1990s, and we use this natural experiment to evaluate its impact on women's education. We further look at the intergenerational outcomes of the reform by examining the education outcomes of the second generation (children of women affected by the reform). This allows us to evaluate a mother's increased bargaining power (thus increased empowerment) and how it affects her children's well-being.

Using the 2004-05 round of the Indian Human Development Survey (IHDS), we exploit a difference-in-differences strategy where the treated group of individuals are women who were unmarried at the time of the reform and the five reform states are the treated states. We find that women directly impacted by the reform (*Generation I*) saw a significant increase in the number of years of schooling. To identify the intergenerational impact of the reform, we identify households that have treated mothers and those with mothers who weren't treated by the reform and examine the educational outcomes of children, separating the sample by the gender of the children. Our results indicate that educational attainment for boys decreases significantly in households with the treated mother.

The intergenerational results continue to show a trend towards transferring wealth to daughters through human capital formation. Although our results are from a reduced form estimation, it does provide some suggestive evidence towards the argument that families are

substituting away from devolving land share to daughters and instead are investing in their education (however, these results are not significant). Policymakers need to focus their efforts on understanding the factors behind the decrease in boys' education. One way to correct for this could be to make the opportunity cost of dropping out of school more expensive, by incentivizing their demand for education. For future research, we will turn our focus to include the age differences between siblings to understand if that has any impact on the education in these households. Further, we wish to explore fertility decisions of these women as a measure of the bargaining power within their households.

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Table 1:
Descriptive Statistics

	Full Sample	Non-Reform States	Reform States		
			All	Treated	Control
	(1)	(2)	(3)	(4)	(5)
Eligible Woman's Years of Education	3.69 (3.44)	2.36 (3.65)	3.29 (3.96)	4.40 (4.38)	2.20 (3.36)
Eligible Woman's Age	32.27 (8.12)	32.11 (8.16)	32.53 (8.06)	25.89 (4.74)	38.16 (5.65)
Age at marriage	16.51 (3.09)	16.32 (3.17)	16.86 (2.92)	17.53 (2.90)	16.29 (2.81)
Household Size	5.84 (2.36)	6.28 (2.47)	5.05 (1.89)	5.07 (1.96)	5.03 (1.83)
Female Headed Household	0.03 (0.18)	0.03 (0.18)	0.03 (0.18)	0.04 (0.19)	0.03 (0.17)
Hindus	0.92 (0.27)	0.91 (0.28)	0.94 (0.23)	0.95 (0.22)	0.94 (0.23)
SC/ST	0.76 (0.43)	0.74 (0.44)	0.78 (0.41)	0.79 (0.41)	0.77 (0.42)
Household Income	32580.57 (28235.05)	32188.82 (28314.78)	33301.04 (28076.51)	32886.31 (27471.62)	33648.97 (28574.94)
Household Farm Income	11036.09 (20182.21)	12713.22 (21284.58)	7951.66 (17568.16)	7474.78 (17534.60)	8351.74 (17589.83)
Land Owner	0.70 (0.46)	0.76 (0.43)	0.58 (0.49)	0.51 (0.50)	0.63 (0.48)
Monthly per capita Expenditure	653.91 (517.37)	607.17 (496.89)	739.89 (542.69)	728.27 (522.41)	749.63 (559.06)
Below Poverty Line	0.25 (0.43)	0.31 (0.46)	0.13 (0.34)	0.11 (0.32)	0.15 (0.35)
No. of Observations	13803	9073	4730	1922	2808

Notes: The data set is from the 2004-05 round of IHDS. Standard errors are in parentheses. Column 1 presents summary statistics for the full sample, Column 2 for the non-reform states. We then report the descriptive statistics for reform states, divided between the full sample (Column 3), for treated group of women (Column 4), and for control group of women (Column 5).

Table 2:
Difference-in-Differences Estimates of
Hindu Succession Amendment Act on Mothers' Education

	(1)	(2)	(3)	(4)
<u>Panel A: Full Sample</u>				
Unmarried at the time of Reform	0.39* (0.22)	0.37* (0.22)	0.35 (0.23)	0.35 (0.24)
No. of Observations	12789	12170	11560	11560
R^2	0.36	0.37	0.37	0.35
<u>Panel B: Landed Households</u>				
Unmarried at the time of Reform	0.49* (0.26)	0.52** (0.26)	0.48* (0.26)	0.51* (0.28)
No. of Observations	9159	8682	8469	8469
R^2	0.39	0.40	0.40	0.38

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. Standard errors are clustered at the district level and are reported in parentheses. The dependent variable is the level of education (measured in number of years) completed by the mother. In Panel A, the sample is restricted to rural areas and Hindu households and in Panel B we further restrict the sample to only landed households. Sample of states does not include Jammu & Kashmir and the North Eastern states. All regressions include district fixed effects and year of marriage fixed effects. Column 1 includes the basic controls such as age of the woman, caste, the household's total income, BPL, average age of men and women in the household and if the household owns land. Column 2 adds controls for land dowry practice, if the woman is the same caste as her husband, and the relative economic status compared to her husband's. Column 3 further adds the household's occupation type. Finally, Column 4 replaces the household's total income with the household's farm income.

Table 3:
Falsification Test

	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Unmarried at the time of Reform	-0.29 (0.40)	-0.30 (0.40)	0.08 (0.24)	0.51 (0.65)
No. of Observations	10990	9733	9733	808
R^2	0.37	0.34	0.34	0.53
Panel B: Landed Households				
Unmarried at the time of Reform	-0.16 (0.39)	-0.11 (0.37)	0.15 (0.37)	0.19 (0.94)
No. of Observations	8203	7412	7412	478
R^2	0.39	0.36	0.39	0.57

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. The dependent variable is the level of education (measured in number of years) completed by the mother. Column 1 has all 12 states as the control group compared to AP, Column 2 includes the 12 states and 3 reform states as the control group compared to AP. Column 3 includes all the reform states in our treatment group but give them all a false start date of 1981. Column 4 restricts the estimation to only Muslim population. The sample is restricted to rural areas and only Hindu households. Sample of states does not include Jammu & Kashmir and the North Eastern state. All regressions include district fixed effects, year of marriage fixed effects and all other control variables used in Table 2. Regressions include sample weights as well.

Table 4:
Effect of HSAA on Children's Years of Schooling

	Full Sample		Girls		Boys	
	(1)	Obs.	(2)	Obs.	(3)	Obs.
<u>Panel A: All Households</u>						
Full Sample	-0.118** (0.059)	18186	-0.103 (0.074)	8723	-0.145** (0.068)	9463
Ages 5-9	-0.117* (0.066)	8908	-0.162 (0.109)	4208	-0.132 (0.084)	4700
Ages 10-14	0.054 (0.085)	9278	0.139 (0.092)	4515	-0.020 (0.107)	4763
<u>Panel B: 2 Children Households</u>						
Full Sample	-0.109 (0.067)	5612	0.005 (0.091)	2354	-0.175** (0.086)	3258
Ages 5-9	-0.040 (0.088)	2572	0.217 (0.155)	1047	-0.241* (0.128)	1525
Ages 10-14	0.036 (0.124)	3040	0.209 (0.145)	1307	-0.080 (0.178)	1733

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. Standard errors are in parenthesis and are clustered at the district level. The dependent variable is the level of education measured in terms of z-scores. The sample is restricted to rural areas, and Hindu landowning households. Sample of states does not include Jammu & Kashmir, Kerala and the North Eastern states. Regression includes district fixed effects, year of marriage fixed effects and a full set of controls as described in Section 3. Regressions include household weights provided by IHDS

Table 5:
Effect of HSAA on Children's Years of Schooling
by Gender Composition of Children

	Only Sons (1)	Only Daughters (2)	Son & Daughter (3)
HSAA Household	-0.404** (0.191)	0.100 (0.246)	0.026 (0.122)
No. of Observations	1134	503	1866
R^2	0.39	0.60	0.41

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. Standard errors are in parenthesis and are clustered at the district level. The sample is restricted to rural areas, Hindu landowning households and only two children households. Sample of states does not include Jammu & Kashmir, Kerala and the North Eastern states. Regression includes district fixed effects, year of marriage fixed effects and a full set of controls as described in Section 3. Regressions include household weights provided by IHDS.

Table 6:
Effect of HSAA on Gender Differences Between Households

	All Households	Boy-Girl Households
	(1)	(2)
HSAA Household*Female	0.079 (0.085)	-0.022 (0.133)
HSAA Household	-0.161* (0.094)	0.039 (0.151)
Female	-0.039 (0.027)	0.135** (0.079)
No. of Observations	11352	1641
R^2	0.25	0.42

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. Standard errors are in parenthesis and are clustered at the district level. Column 1 reports results for all households that have at least one son and one daughter, where as in Column 2 we restrict the sample to two children households, with one boy and one girl. The sample is restricted to rural areas, Hindu landowning households and only households with one boy and one girl within the households. Sample of states does not include Jammu & Kashmir, Kerala and the North Eastern states. Regression includes district fixed effects, household fixed effects, year of marriage fixed effects and a full set of controls as described in Section 3 and sample weights as well.

Table 7: Robustness Check

Impact of HSAA on Mothers' Education: Year of Birth

	(1)	(2)	(3)
Reform State*Age \leq 18	0.720*** (0.200)		
Reform State*Age \leq 10		0.834*** (0.16)	
Reform State*Age $>$ 10		-0.015 (0.14)	
Reform State*Cohort 1			1.417*** (0.14)
Reform State*Cohort 2			0.710*** (0.18)
Reform State*Cohort 3			-0.105 (0.12)
Reform State*Cohort 4			0.073 (0.26)
No. of Observations	103792	103792	103792
R^2	0.234	0.232	0.232

Notes: ***, **, * denote significance at 1%, 5% and 10% respectively. Standard errors are in parenthesis and are clustered at the district level. We use the nationally representative data, NSSO for this analysis. The dependent variable is the level of education (measured in number of years) completed by the mother. The sample is restricted to rural areas and only Hindu households. Sample of states does not include Jammu & Kashmir and the North Eastern states. All regressions include state fixed effects, cohort fixed effects and state specific cohort trends. Regressions include sample weights as well.