# Female Inheritance Rights and Household Sanitation*

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## Abstract

Existing research shows that females derive greater benefits from in-house toilets than males. Given this, we estimate the impact of a policy that increased inheritance rights of females on the presence of a toilet in their marital household in India. Daughters being usually married away to the household of the groom, available household level nationally representative data do not have all original (natal) household characteristics – which determines treatment eligibility. Under generic assumptions, we show that when the treatment is partially observed to the researcher, we can derive bounds on the average treatment effect in a difference-in-differences framework. We estimate that the policy increased the probability of the presence of a toilet in the household a woman marries into by at least 7.4-11.2 percentage points on average. Allowing for heterogeneous treatment effects, we show that the average treatment effect is primarily driven by larger effects in states that adopted the policy later compared to early adopters. In addition, allowing for dynamic effects, we find that the policy had its highest impact on the group of women who were the youngest at the time of policy implementation, thus having the longest exposure to the policy. Our results underscore that policies that empower can offer to be a seemingly unrelated, yet effective policy tool for improving sanitation coverage in regions grappling with open-defecation problems.

*JEL Codes:* I15, I18, J16, O15, O18
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# 1 Introduction

Open defecation is a menacing problem in low and middle income countries and has been linked to diseases like diarrhea and stunting in children, among many others. The practice is particularly prevalent in India, which accounts for 60% of world's open defecation (Census 2011). The barriers to demand for in-house toilets in India stem from notions of religious purity, casteism and lack of health-based awareness. However, within the household, the presence of a toilet disproportionately benefits females (Aid Water 2013, Jadhav et al. 2016). This is because females are often victims of sexual harassment when they go out in the open to defecate, urinate or attend to their menstrual hygiene (Jadhav et al. 2016).[1] In spite of such difficulties faced by females, an important deterrent in the demand for in-house toilets in India stems from the fact that females are rarely the primary decision makers within their households (Coffey et al. 2014). This leads us to ask the question: do policies targeted towards increasing women's access to economic resources lead to an increase in demand for in-house toilets, a household public good that females value disproportionately more than males (Augsburg, Malde, Olorenshaw & Wahhaj 2023, Coffey et al. 2014, Khanna & Das 2016, Stopnitzky 2017) ?

In theory, policies that increase women's access to economic resources can improve their marital outcomes through multiple channels. At the extensive margin, such policies might change the marriage market equilibrium by altering who marries whom. Prior research has shown that this might change sorting patterns (citation) and thereby lead women to marry into households with more desirable qualities (for example, households with higher education and thus higher preference for sanitation). Otherwise, if the nature of the marriage market is unchanged, that is they are marrying into similar households as before, then the policy might improve their intra-household bargaining power which results in higher toilet coverage. We test for these mechanisms and find evidence that the policy changes the nature of the marriage market whereby treated women are more likely to marry men with higher years of education.

We study this in the context of India's Hindu Succession Act of 1956 (HSA) which governed property inheritance rights of Hindus, Sikhs, Jains, and Buddhists. The HSA was gender-unequal in the sense that it devolved substantially higher inheritance rights to sons relative to

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[1]Also see https://www.huffpost.com/entry/open-defecation-india_b_7898834

daughters. We leverage the staggered adoption of amendments to the HSA across states in India before the year 2005 (HSA was nationally amended in 2005), under which states abolished gender-inequality in inheritance rights by equalizing the rights of daughters and sons. Our focus is to study whether an increase in access to economic resources for females – as measured by an increase in their property inheritance rights – increases the likelihood of there being a toilet in their marital household.

Kerela in 1976, Andhra Pradesh in 1986, Tamil Nadu in 1989 followed by Karnataka and Maharashtra in 1994 were the five states that redressed the gender-inequality inherent in HSA, before the amendements were nationally ratified in 2005. The amendment, however, did not apply to all the females in these states. It only applied to those who were unmarried at the time of passing of the amendment in their state, and did not apply to those who were already married, thus creating variation in treatment within the treated states. This variation allows us to estimate the causal impact of the HSA amendment in a difference-in-differences framework. We follow Callaway & Sant'Anna (2021) and compare toilet ownership rates in marital households of females in treated states relative to untreated states, across females who were unmarried at the time of passing of the amendment in their state (and thus benefited under the amendment), relative to those who were married at the time of passing of the amendment (and thus did not benefit under the amendment). Our identification assumption is that in absence of the HSA amendment, on average the likelihood of the presence of a toilet in treated states would evolve in parallel to the states where HSA was not amended, across marriage cohorts.

An obstacle in estimating the impact of the HSA amendments is that the treatment group is not perfectly observed in the data. While one of the eligibility criteria was that one should have been unmarried at the time of passing of the amendment in her state, another condition was that the natal household property must have been undivided at the time of the passing of the amendment in her state. The second condition is unobserved in nationally representative survey datasets since data on such natal household characteristics of married women in household are typically not asked in survey datasets.[2] To address this common data caveat, we

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[2]One reason for this is that marriages in India are patrilocal, a marital system where females leave their natal household and migrate to their husband's natal household after marriage. Consequently, survey datasets tend to primarily capture information about the household characteristics of their marital homes, while having very limited data on the natal household characteristics of married women. Notably, the Rural Economic and Demographic Survey (REDS) is a partial exception. REDS has retrospective information on all the members of a household, including daughters who have married and left the household. An advantage of using this dataset is that it has information on the timing of the death of the grandfather of daughters which has been used as a proxy for the

formally show that under generic assumptions, we can identify and estimate lower bounds of the true average treatment effect on the treated within a difference-in-differences framework, even while allowing for heterogeneous treatment effects in a staggered policy adoption setting.

We find that on average, the amendment to the Hindu Succession Act significantly increased the likelihood of the presence of a toilet in the marital household of women. In particular, we find that following the amendment, females who were eligible under the amendment were on average at least 10 percentage points more likely to have a toilet in their marital household relative to females who remained untreated as per the amendment. We conduct pre-trend tests which imply that there is no statistical evidence to suggest that the pre-treatment effects are different from zero, strengthening our identification assumption.

Allowing for heterogeneous treatment effects, we find that this average effect is primarily driven by effects in latter adopting states with little to no effect in early adopting states. Exploiting variation across marriage cohorts over time, we explore dynamic treatment effects of the policy. We find that the policy primarily impacted cohorts of females who were young at the time of policy implementation in their state, consistent with the existing literature on the effects of the policy amendment on other outcomes of females (Roy 2015). Finally, our event-study estimates indicate no statistical or economically significant effect of the policy in the years preceding its implementation, thereby supporting our identifying assumption of parallel trends.

We explore mechanisms that could plausibly drive the main result, which is that the policy led to an increase in the likelihood of women's marital household having a toilet. We find that the policy changed the marriage market equilibrium, and find strong evidence that it led to treated women marrying men with higher education levels. Given the existing literature on the positive association between education and sanitation, this change in the marriage market equilibrium through the policy could have increased the propensity for the existence of a toilet. Our evidence shows that women marrying husbands with higher education is plausibly channelled through the policy's effect on increasing dowries (Roy 2015) and/or increasing inheritance (Deininger et al. 2013). This evidence is consistent with other findings suggesting

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timing of the division of the household's joint ancestral property Roy (2015). However, for the purpose of our question, REDS is not useful as it does not provide information on whether daughters who have married and left the natal household, have a toilet in their marital household (which is our outcome of interest). To the best of our knowledge, survey data on the timing of property division in India does not exist.

that some policies could inadvertently change marital patterns Augsburg, Baquero, Gautam & Rodriguez-Lesmes (2023). We also explore whether the policy improved women's education and intra-household decision-making among treated women, but find no evidence to support those hypotheses.

We provide additional evidence to rule out the potential concerns on (a) self-selection into treatment in anticipation of the policy and (b) toilets resulting from post-marital outcomes which increase intra-household bargaining power, instead of the pre-marital increased female inheritance policy. To address the first concern of self-selection into the policy, we find no evidence of discontinuous changes in either the marriage rate or age at marriage around the year of policy implementation. For the latter, we use sex of the first-born child to investigate whether there are differences between households where the mother had a boy as her first-born child relative to those where the first-born was a girl. We find no evidence of this, substantiating our evidence of increased likelihood of toilet presence because of the policy and not because of post-marital outcomes which also could have increased the intra-household bargaining power of females.

Our work relates to two strands of literature. First, the literature on inheritance rights and their impact on female outcomes. Second, the literature on sanitation and how policies that are targeted towards improving women's access to economic resources can lead to unintended benefits, such as increasing demand for sanitation. Several papers have studied the impact of amendments to the Hindu Succession Act on female outcomes. Roy (2015) finds that while the amendments were unsuccessful in improving the likelihood of inheritance for women, however, the amendment led to a statistically significant increase in the years of schooling for young girls of school going age and an increase in dowry payments for girls of marriageable age, suggesting an alternate channel of wealth transfer. [3] Deininger et al. (2013) find similar results on education attainment of daughters as a result of the HSA amendments, however, find that the amendments increased the likelihood of intergenerational land transfers for women.

The rest of the paper is organized as follows: Section 2 describes the institutional back-

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[3]The author uses data from the Rural Economic Demographic Survey (REDS) which has retrospective information on all the members of a household, including daughters who have married and left the household. An advantage of using this dataset is that it has information on the timing of the death of the grandfather of daughters which is then used as a proxy for the timing of the division of the household's joint ancestral property. For the purpose of our question, this dataset would not be of use as for our purposes, we require information on the sanitation outcome for females in their marital families rather than their birth/natal families.

ground of the Hindu inheritance law in India. Section 3 outlines the data. Section 4 outlines the empirical strategy. Section 5 presents results followed by Section 6 which talks about mechanisms. Section 7 talks about robustness checks and Section 8 concludes.

## 2 Institutional Details

### 2.1 The Hindu Succession Act of 1956 (HSA)

Inheritance rights in India vary by religion. There are two major legal doctrines regarding Hindu inheritance namely the *Mitakshara* and *Dayabhaga* schools. The Hindu Succession law of 1956 governs the property rights of Hindus, Sikhs, Buddhists and Jains following the *Mitakshara* system. The *Mitakshara* system distinguishes individual property from joint ancestral property which includes land (Agarwal 1994). Joint household property was any property that was accumulated by the patriarch of the family and jointly held by the members of the house. Separate property was accumulated separately by the father. Following the act, daughters of a Hindu male dying intestate (i.e., without writing a will) were equal inheritors, along with sons, only of their father's separate property but had no share in the joint property. Rights to the joint property were limited to the *coparceners* that only constituted male members of a family. Thus, under the original HSA, females were only entitled to inherit their share in the property of their father whereas males in addition to the father's property were *coparceners* or joint heirs in the joint household property by birth. Since joint property takes the form of land that is typically family owned, females were at a disadvantage under the inheritance rules and HSA was by no means a gender neutral law.

### 2.2 State Amendments to Hindu Succession Act (HSAA)

Five states in southern India enacted legislation to amend the law at the state level, before the amendments were nationally ratified in 2005. Kerela in 1976, Andhra Pradesh in 1986, Tamil Nadu in 1989 followed by Karnataka and Maharashtra in 1994 were the five states that took measures to redress the gender inequality inherent in the original HSA. Under these amendments, daughters were granted equal inheritance rights as sons in the family property but this was conditional on daughters satisfying some eligibility criteria. The followingsconditions

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needed to be satisfied by a woman to be eligible under the HSAA. First, the woman had to reside in one of the five reform states. Second, the woman had to be unmarried at the time when the amendment was passed in her state. Third, the woman had to hail from one of the religions of Hinduism, Jainism, Sikhism or Buddhism. Finally, the household property of the woman's house must have been undivided at the time of the passing of the amendment in her state. On September 9, 2005, all the eligibility criteria were removed, and the amendment was implemented at the national level granting equal claims to the joint household property to daughters and sons.

## 3 Data

We use data from the 2004-05 wave of Indian Human Development Survey (IHDS), a nationally representative survey collected by the National Council for Applied Economic Research. For the purpose of our analysis, we focus on the wife of the household head for every household and our goal is to estimate the impact of the HSA amendment on the presence of a toilet in her marital household.

Following Roy (2015) we restrict our analysis to women belonging to one of the HSA-eligible religions—Hinduism, Sikhism, Jainism, and Buddhism. In another restriction, we drop the households belonging to the state of Kerala (one of the five states to pass the HSA amendment). We do this because Kerala, being one of the most progressive states, has almost universal in-house toilet coverage in the year prior to the amendment. In figure 1 we plot the unconditional likelihood of the presence of a toilet in the household against the year of marriages that happened in Kerala within a 15-year window of the state's policy implementation in 1976. Women who got married in 1975—the year before HSAA was implemented in Kerala—we see 100% of toilet coverage providing us with no variation in outcome of women who got married in the baseline year in Kerala. With no variation in toilet coverage in marital households of women who got married in the baseline year, even slightly smaller percentage of toilet coverage in households of cohorts that got married after the policy would result in a highly negative effect of the policy in Kerala which could result in misinterpretation of the effects of the policy.

Following a similar logic, we also drop the north-eastern states of India which also have

Figure 1: Toilet prevalence by year of marriage in Kerala
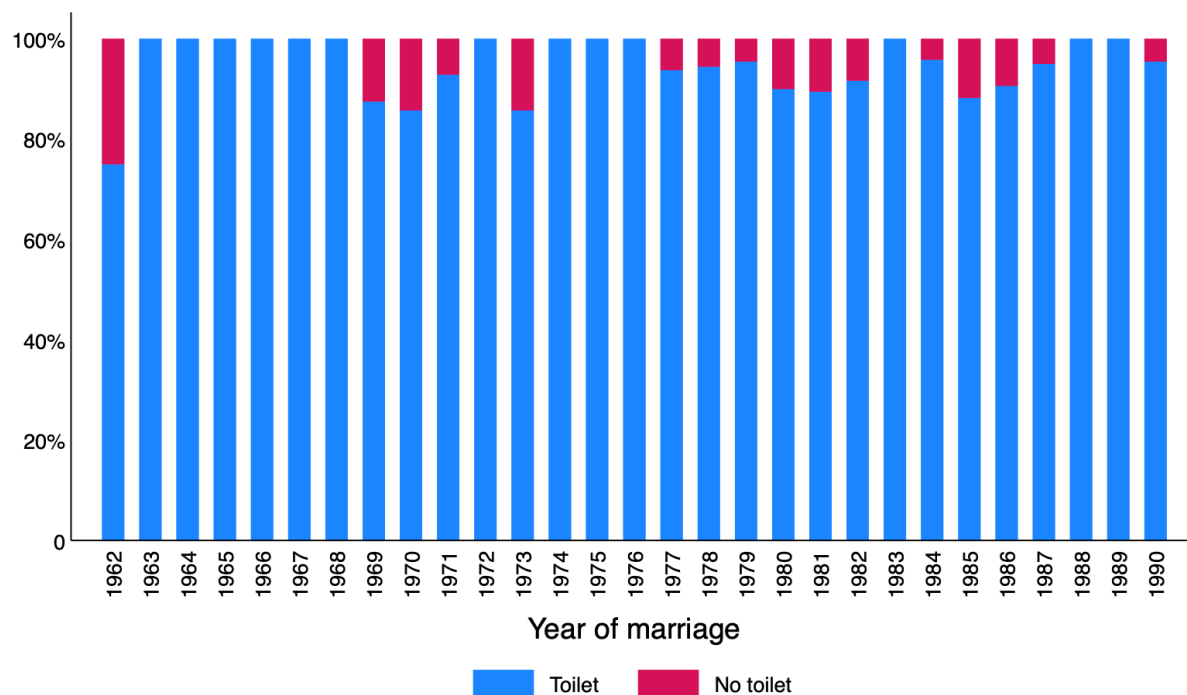
*Notes:* Blue bars show the unconditional likelihood of the presence of a toilet in the household against the year of marriages that happened in Kerala within a 15-year window of the policy implementation in 1976. The corresponding red bars correspond to the unconditional likelihood of the absence of a toilet. The year 1975 is the year before the HSA amendment was implemented in Kerala. We see that in 1975, the year before the HSAA was implemented, 100% of the households had a toilet. Note that our data can only tell us whether the household has a toilet in 2005, and it does not give any information about the year in which the toilet was constructed. So the plots here represents whether these households had a toilet in 2005.

near-universal toilet coverage (over 95%) across marriage cohorts. This drops 567 households from the sample. Finally, we drop all households where the marriages took place after September 9, 2005 which is when the HSA was amended nationwide.

Removing Kerala, we are left with four states which passed the amendment before the policy was nationally amended 2005. Andhra Pradesh passed it in 1986, followed by Karnataka in 1989, making Tamil Nadu and Maharashtra the last two states to pass the amendment in 1994. This leaves us with 11 years after Tamil Nadu and Maharashtra passed the amendment, and before the national ratification in 2005. Thus, in our estimation we restrict comparisons to married women in treated with married women in control states such that these marriages happened within the time window of 11 years before and after the policy implementation for each state. This brings our final sample size to 20,413 households.

## 4  Empirical Strategy

We begin by discussing how—in our case with cross-sectional data—we are able to estimate the average treatment effect on the treated while allowing for heterogeneous treatment effects. At first glance, the limitation in implementing a difference-in-differences strategy in our setting arises from the lack of a panel, or a repeated cross-section data. What enables us to allow for heterogeneous effects across groups and time, in spite of this seeming limitation, comes from the year of marriage component of the eligibility criteria, relative to the year of policy implementation across states. This brings the dimension of time into our analysis and allows us to compare treated and untreated cohorts of women within a given state (as defined by whether they were unmarried or married by the year of policy implementation in the state).

Recent advances in the literature on treatment effects estimation in a staggered policy adoption design using two-way fixed effects have been documented to produce potentially misleading results when the treatment effects are heterogeneous across groups and/or over time (Borusyak & Jaravel 2018, De Chaisemartin & d'Haultfoeuille 2020, Goodman-Bacon 2021). In particular, the estimates resulting from the TWFE specification produces a non-convex weighted average of the heterogeneous treatment effects. Hence, if the treatment effect varies with group and time, some weights could be negative for some groups and time periods even if all the weights sum to 1 resulting in the estimate of the ATT to be of a different sign than

that of each of the individual treatment effects. We estimate the average treatment effect of the HSAA policy on the treated which is robust to heterogeneous treatment effects across groups and time. We conduct diagnostic tests proposed by De Chaisemartin & d'Haultfoeuille (2020) and find evidence for negative weighting as reported in Table [insert]. Hence, we estimate the average treatment effect on the treated using methods proposed by Callaway & Sant'Anna (2021) which is robust to heterogeneous treatment effects. For inference, we use wild bootstrap clustered at the state level allowing for arbitrary correlation between the unobservables within a state.

Another point to note is that the outcome of interest - presence of a toilet in the marital household of a female - is an eventual outcome observed in the year 2005 (i.e., the year of survey). Thus, we are unable to observe the year in which toilet was constructed and whether it was before or after the women in our sample got married into these households.

Following Callaway & Sant'Anna (2021), we estimate the group-time average treatment effects of the policy on the treated. Let $i$ denote a woman and let $t$ denote the year of marriage of the woman (thus representing the cohort). Let $G_i$ denote the time period when a woman $i$ in our sample gets treated. This would be the year when the policy got implemented in their state and if the woman was unmarried in that year and belonged to one of the HSA religions. Let $G_i = \infty$ denote the never-treated group whom we will use as our control group. These consist of women in states where HSA was not implemented before 2005. Thus, the outcome of interest is $Toilet_{igt}$ which equals 1 if woman $i$ married in year $t$ belonging to group $g$ has a toilet in her household at the time of the survey.[4] Our results are also robust to using not-yet treated groups as controls.

We make the standard assumptions in Callaway & Sant'Anna (2021), namely, random sampling, sharp design, no treatment anticipation and conditional parallel trends in post-treatment periods based on the never-treated group.[5] The conditional parallel trends assumption in our case specifies that in absence of treatment, the probability of the presence of a toilet in the amendment states would have evolved in parallel across years of marriages relative to the non-amendment states. In particular, denoting $Y_{i,t}(\infty)$ as the potential outcome of woman $i$

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[4]This is unlike standard outcomes in a difference-in-differences settings where outcome is a realization at time period $t$. In our case the outcome is an eventual realization.

[5]For details on the assumptions and identification of group-time average effects and their aggregations devoid of negative weighting problems, see Callaway & Sant'Anna (2021).

married in year $t$ in the absence of the policy implementation, we have for all groups $g \in \mathcal{G}$ where the policy was implemented,

$$\mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid X_i, G_i = g\right] = \mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid X_i, G_i = \infty\right]$$

for all $t, t' \geqslant g_{\min} - 1$, where $g_{\min} = \min \mathcal{G}$ is the first period where a unit is treated, and conditional on time-invariant covariates $X_i$.[6]

Under these assumptions, variation in treatment timing relative to the year of marriage can be used to identify the average treatment effect on the treated for each group $g$ and time period $t$ denoted by $ATT(g,t)$.

Intuitively, under the staggered versions of the parallel trends and no anticipation assumptions, we can identify $ATT(g,t)$ by comparing the expected change in outcome for cohort $g$ between periods $g - 1$ and $t$ to that for a control group (not-yet treated) at period $t$. Formally, under the conditional parallel trends assumption, we have

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g'\right], \text{ for any } g' > t$$

Since this holds for any comparison group $g' > t$, it also holds if we average over some set of comparisons $\mathcal{G}_{\text{comp}}$ such that $g' > t$ for all $g' \in \mathcal{G}_{\text{comp}}$,

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}\right]$$

As discussed earlier, one of the eligibility criteria under the amendment was that the woman's maternal household property should have been undivided at the time of passing of the amendment in her state. However, our dataset does not have information on this condition. Roy (2015) studies the impact of the HSA amendment on educational attainment and dowries for daughters, but these are pre-marriage outcomes and the data used—the Rural Economic and Demographic Survey (REDS)—has retrospective information on all the members of the household including daughters who have married and left the household. REDS does not have

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[6]The conditional parallel trends assumption in post-treatment periods, is a weaker version than conditional parallel trends assumption for all time periods i.e., for all $t \neq t'$ and $g \neq g'$,

$$\mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid X_i, G_i = g\right] = \mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,t'}(\infty) \mid X_i, G_i = g'\right]$$

post-marriage data for daughters who have left the household and hence it is not useful for the purpose of our analysis. The author uses data on the timing of a daughter's grandfather's death as indicative of whether the household property was undivided at the time of amendment in her state. This is because in Indian households, property typically gets divided when the patriarch of the family dies. However, we do not observe this as most publicly available surveys, including the one we use, do not ask this question for the married females of the household. If one could observe this in data, the causal effect of HSA on the outcome of interest could have been estimated using a standard difference-in-differences framework. However, in our model, the treatment group is likely to be mis-measured because of which some individuals who should ideally be in the control group fall might end up in the treatment group. This mis-measurement would lead to a bias in the estimated average treatment effect. In spite of this, we can find a way of estimating the direction and magnitude of the bias. In the following proposition we derive bounds on the true parameter, when the treatment group is mis-measured.

For each group $g$ we will assume that the division of property is independent of other variables motivated by Roy (2015) who uses the death of the grandfather as the time which defines division of property which could be plausibly assumed to be a random event. This will ensure identification of lower bounds of group-time treatment effects. Thus any aggregation of the group-time ATTs will result in a lower bound of the overall aggregated ATT. In particular, we can show that not observing one of the eligibility criterion of treatment can allow us to identify lower bounds of the treatment effect if the unobservable criterion is independent of other variables and can only affect outcome through treatment. Formally, we can state this as follows:

**Proposition 1.** *Suppose for each unit i we only observe its group identity $G_i$ but we do not observe one criterion that determines treatment eligibility. Let us denote this unobserved treatment eligibility criterion as a dummy variable $b_i$ which takes a value 1 if unit i is eligible for treatment. We continue to maintain standard assumptions of random sampling, no anticipation and parallel trends based on a comparison group $\mathcal{G}_{comp}$ (not-yet treated or never-treated) which identifies $ATT(g, t)$ for all groups $g \in \mathcal{G} \setminus \mathcal{G}_{comp}$ and all time periods t when all criterion of treatment eligibility are observed. Under an additional assumption that $b_i$ only affects potential outcomes of unit i through treatment only and is*

*independent of other group identity, the $ATT(g,t)$ identified under this data limitation is a lower-bound on the true $ATT(g,t)$ for all groups $g \in \mathcal{G}$ and all time periods $t$. This also extends to the case where we condition on a set of covariates $X_i$ which are independent of $b_i$ and only affect potential outcomes through treatment.*

*Proof.* We start by re-iterating that over some set of comparison groups $\mathcal{G}_{\text{comp}}$ such that $g' > t$ for all $g' \in \mathcal{G}_{\text{comp}}$, the above assumptions identify the true group-time treatment effects if both the group identity $G_i$ and the treatment eligibility $b_i$ are observed. In this case the true $ATT(g,t)$ is given by

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right]$$

However, since we do not observe $b_i$ for all units $i$, we can identify (and estimate) the following expression, which we denote as $ATT^*(g,t)$

$$ATT^*(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}\right]$$

Now using the Law of Iterated Expectations, we rewrite the above identified expression as,

$$ATT^*(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] \mathbb{P}(b_i = 1 \mid G_i = g)$$
$$- \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right] \mathbb{P}(b_i = 1 \mid G_i \in \mathcal{G}_{\text{comp}})$$

By our assumption that the event $b_i$ is independent of group indicators, we have

$$ATT^*(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] \mathbb{P}(b_i = 1) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right] \mathbb{P}(b_i = 1)$$
$$= \mathbb{P}(b_i = 1) \left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right]\right)$$
$$= \mathbb{P}(b_i = 1) ATT(g,t)$$

since $\mathbb{P}(b_i = 1) \in [0,1]$, we have that

$$\mid ATT^*(g,t) \mid \leq \mid ATT(g,t) \mid$$

Hence, if the true treatment effect $ATT(g,t)$ is positive then $ATT^*(g,t) \leq ATT(g,t)$.

This proof can be easily extended to a case where we also condition on other covariates $X_i$ which are independent of $b_i$ and $G_i$. In this case, under the assumption of conditional parallel trends based on comparison group $\mathcal{G}_{comp}$, along with the assumptions on random sampling and no anticipation, we can write the true $ATT(g,t)$ as

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{comp}, b_i = 1, X_i\right]$$

and the identified $ATT^*(g,t)$ given the data limitation as

$$ATT^*(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, X_i\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{comp}, X_i\right]$$

Using the Law of Iterated Expectations, we can write the above identified expression as,

$$ATT^*(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid G_i = g, X_i)$$
$$- \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{comp}, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid G_i \in \mathcal{G}_{comp}, X_i)$$

By our assumption that the event $b_i$ is independent of other covariates and group indicators, we have

$ATT^*(g,t)$
$$= \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid X_i) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{comp}, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid X_i)$$
$$= \mathbb{P}(b_i = 1 \mid X_i) \left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{comp}, b_i = 1, X_i\right]\right)$$
$$= \mathbb{P}(b_i = 1 \mid X_i) ATT(g,t)$$
$$\leq ATT(g,t)$$

Since $\mathbb{P}(b_i = 1 \mid X_i) \in [0,1]$, we have that

$$\mid ATT^*(g,t) \mid \ \leq \ \mid ATT(g,t) \mid$$

Hence, if the true treatment effect $ATT(g,t)$ is positive then $ATT^*(g,t) \leq ATT(g,t)$

Now let $\widehat{ATT(g,t)}$ be a consistent estimator of the true treatment effect $ATT(g,t)$. Hence if $ATT(g,t) \sim \mathcal{N}\left(\mu_g, \sigma_g^2\right)$, we have $\sqrt{n}\left(\widehat{ATT(g,t)} - \mu_g\right) \xrightarrow{d} \mathcal{N}\left(0, \sigma_g^2\right)$.

Now let $\widehat{p_x}$ be a consistent estimator of $\mathbb{P}(b_i = 1 \mid X_i)$. Using the Delta method, we have

$$\sqrt{n}\left(\widehat{p_x}\widehat{ATT(g,t)}\right) \xrightarrow{d} \mathcal{N}\left(\mathbb{P}(b_i = 1 \mid X_i)\mu_g, \left(\mathbb{P}(b_i = 1 \mid X_i)\sigma_g\right)^2\right)$$

Using the continuous mapping theorem, $\widehat{p_x}\widehat{ATT(g,t)}$ is a consistent estimator of $ATT^*(g,t)$. Thus,

$$ATT^*(g,t) \sim \mathcal{N}\left(\mathbb{P}(b_i = 1 \mid X_i)\mu_g, \left(\mathbb{P}(b_i = 1 \mid X_i)\sigma_g\right)^2\right)$$

It is straightforward to derive the asymptotic distribution of the average treatment effect $\hat{\beta}_1$ which is the parameter of interest.

$$ATT(g,t) \sim \mathcal{N}\left(\mu_g, \sigma_g^2\right)$$
$$\Rightarrow \sqrt{n}\left(\widehat{ATT(g,t)} - \mu_g\right) \xrightarrow{d} \mathcal{N}\left(0, \sigma_g^2\right)$$

Using the delta method, and that $ATT^*(g,t) = \mathbb{P}(b_i = 1 \mid X_i)ATT(g,t)$ we have

$$\sqrt{n}\left(\frac{\widehat{ATT(g,t)}}{\Pr(b_i = 1 \mid X_i)} - \frac{\mu_g}{\Pr(b_i = 1 \mid X_i)}\right) \xrightarrow{d} \mathcal{N}\left(0, \frac{\sigma^2}{\Pr(b_i = 1 \mid X_i)}\right)$$

Observe that the function $g(y) = \frac{y}{Pr(p=1|X)}$ is continuous and differentiable $\forall y \in \mathcal{R}$.

Hence, the estimated standard error is asymptotically an upper bound. Intuitively, this arises from the fact that the variance of the unobserved eligibility criterion remains as residual variance, thus reducing the precision of the estimator.

$\square$

# 5   Results

In this section we report and discuss the results from our estimation of the effect of the policy allowing for heterogeneous and dynamic treatment effects. As shown before in the proof, we interpret our estimates as a lower bound of the true treatment effect (since the estimate obtained is positive). Our baseline estimates use the never-treated units as the comparison group. As a robustness check, we also report the estimates obtained from using the not-yet treated units as comparison group, and they yield estimates of similar magnitudes.

Table 1: Estimates for Never Treated and Not-Yet Treated

| | Never treated as comparison group | | Not-yet treated as comparison group | |
|---|---|---|---|---|
| | Estimate | $p$-value | Estimate | $p$-value |
| ATT of units treated in 1986 | 0.0984 (0.0756) | 0.193 | 0.0773 (0.0747) | 0.301 |
| ATT of units treated in 1989 | 0.0815 (0.1023) | 0.426 | 0.0631 (0.1011) | 0.533 |
| ATT of units treated in 1994 | 0.1199 (0.0555) | 0.031 | 0.1199 (0.0555) | 0.031 |
| Aggregate ATT (GAverage) | 0.1054 (0.0417) | 0.011 | 0.0953 (0.0415) | 0.022 |
| Pre-trend Test ($\chi^2$) | 21.3560 | 0.8764 | 21.7578 | 0.8627 |

*Notes:* This table reports the coefficient estimates, standard errors and *p*-values of each treated group's average treatment effect on the treated parameter following Callaway & Sant'Anna (2021). We present estimates by two different comparison groups: never-treated as comparison and not-yet treated as comparison. Standard errors are computed using wild cluster bootstrap at the state level. The last panel of the table reports estimates of a chi-square test which tests the null hypothesis of no differential pre-trends between treated and untreated units.

## 5.1 Heterogeneous Treatment Effects

We report the group-wise and the aggregated average treatment effects of the policy on the treated in Table 1. We find evidence of heterogeneous treatment effects of the policy across the states that adopted the policy in different years. In particular, we find that the overall average effect of the policy is primarily driven by the treatment effect in the latter adopting states. The policy did not have a statistically significant impact on the likelihood of the household having a toilet for the earliest adopting states in our sample—Andhra Pradesh, in 1986 and Tamil Nadu in 1989 (even though the effect sizes are positive). The ATT estimate for the latter adopting states which include Maharashtra and Karnataka in 1994, drive the overall average effect of the policy.

We report the aggregated effects to get at an overall average of the heterogeneous treatment effects on the treated at the bottom of Table 1. This estimate is a weighted average of the group-wise average treatment effect on the treated, with the weights proportional to the sample size of each group. In Table 2 we report the sample size of each group which determines the weights in the aggregating the group-wise ATT estimates. The biggest effect and as well as the largest weight comes from the latest treated group in 1994, followed by those in 1986 and in 1994.

We find that on average, the policy increased the overall likelihood of the presence of a toilet in the household by at least 10.5 percentage points (p-value = 0.011). Finally we conduct a pre-treatment test with the null hypothesis of no differential pre-trends between treated and untreated groups across all marriage cohorts. The corresponding chi-squared test produces an estimate of 21.77 ($p$-value = 0.86). We fail to reject the null hypothesis, implying that there is no statistical evidence to suggest that the pre-treatment effects are different from zero.

## 5.2 Average treatment effects on the treated over time

We estimate an event-study framework to investigate the average treatment effects of the policy on the treated over time by comparing outcomes of marriage cohorts across states over time. In particular, for each group (state) and time period (year of marriage) the average treatment effect on the treated is estimated by comparing differences in average outcomes of the group in the given time period relative to its average outcome in the time period prior to policy implementation in that group, with that of the comparison group's differences in average outcomes for the same pair of time periods. The event study framework additionally provides estimates of the treatment effect of the policy for the cohorts that got married before the policy was implemented in their state, thus providing a test of the identification assumption of conditional parallel trends. We report the estimates of the event-study estimation in Table 3 and plot the estimates in Fig **??** and Fig **??**.

The event study figures show that there are no statistical differences between the treated and untreated states in the likelihood of the presence of a toilet in the households where women were married in years before the policy were implemented. This helps us justify the

Table 2: Group sizes

| Treatment group | N | Percent |
|---|---|---|
| Treated at 1986 | 1,460 | 24.36 |
| Treated at 1989 | 1,159 | 19.34 |
| Treated at 1994 | 3,375 | 56.31 |
| Total | 5,994 | 100.00 |

Notes: Sample size by treatment group where the groups are Andhra Pradesh (1986), Tamil Nadu (1989), Maharashtra and Karnataka (1994).
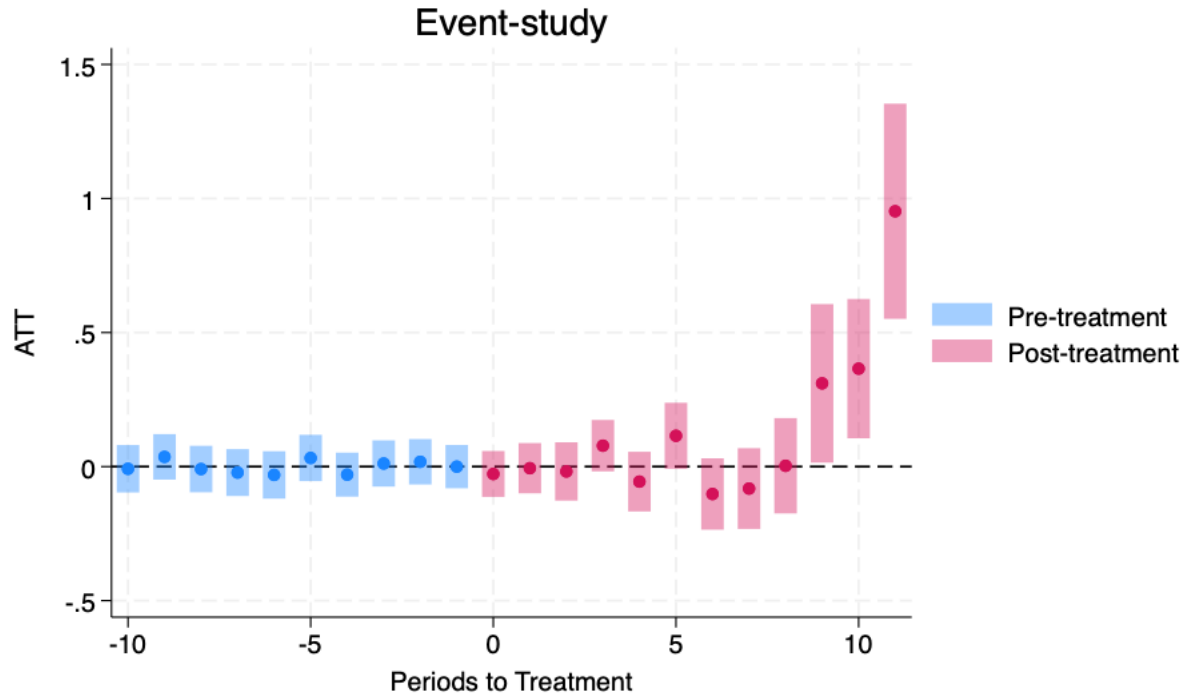
## Event-study

Figure 2: Notes: This figure plots event study estimates of the impact of the policy by comparing marriage cohorts over time, using never-treated states as comparison groups.

parallel trends assumption—in absence of the policy, the evolution of toilet presence in households in treated states would have evolved in parallel to those in untreated states.

The event study plots show that the treatment effect is not immediate and it takes up to 9 years after the policy implementation for the effects to start showing. This suggests that the policy was not very effective for women who were close to marriageable age at the time of the policy implementation in their state. In fact, these results suggest that the policy was effective for women who were young at the time of policy implementation in their state and were thus most exposed to the policy. This finding is consistent with prior work of Roy (2015) who also finds strongest effects for the women who were youngest at the time of policy implementation. A simple back of the envelope calculation suggests that the policy's effect were the strongest for women who were in between 6 and 10 years on average at the time of policy implementation, given average age at marriage in our sample is 17.2 years.

Table 3: Event study estimates

|          | Coefficient | Std. err. | $p$-value |
|----------|-------------|-----------|-----------|
| Pre_avg  | 0.003       | 0.006     | 0.628     |
| Post_avg | 0.176       | 0.059     | 0.003     |
|          |             |           |           |
| $t-11$   | 0.013       | 0.043     | 0.758     |
| $t-10$   | 0.013       | 0.044     | 0.765     |
| $t-9$    | 0.032       | 0.041     | 0.443     |
| $t-8$    | -0.037      | 0.044     | 0.397     |
| $t-7$    | 0.004       | 0.044     | 0.936     |
| $t-6$    | -0.049      | 0.043     | 0.256     |
| $t-5$    | 0.007       | 0.041     | 0.863     |
| $t-4$    | -0.020      | 0.039     | 0.613     |
| $t-3$    | 0.008       | 0.042     | 0.856     |
| $t-2$    | 0.012       | 0.042     | 0.767     |
| $t-1$    | 0.021       | 0.039     | 0.591     |
| $t+0$    | -0.067      | 0.040     | 0.095     |
| $t+1$    | -0.014      | 0.045     | 0.764     |
| $t+2$    | -0.027      | 0.050     | 0.581     |
| $t+3$    | 0.012       | 0.048     | 0.801     |
| $t+4$    | -0.092      | 0.053     | 0.085     |
| $t+5$    | 0.061       | 0.064     | 0.342     |
| $t+6$    | -0.041      | 0.070     | 0.561     |
| $t+7$    | -0.059      | 0.078     | 0.446     |
| $t+8$    | -0.051      | 0.087     | 0.561     |
| $t+9$    | 0.228       | 0.182     | 0.210     |
| $t+10$   | 0.459       | 0.137     | 0.001     |
| $t+11$   | 0.783       | 0.170     | 0.000     |

Notes: This table provides the event study estimates. Sample is restricted to marriages that happen within the 11 year period before and after each treated state's policy implementation.
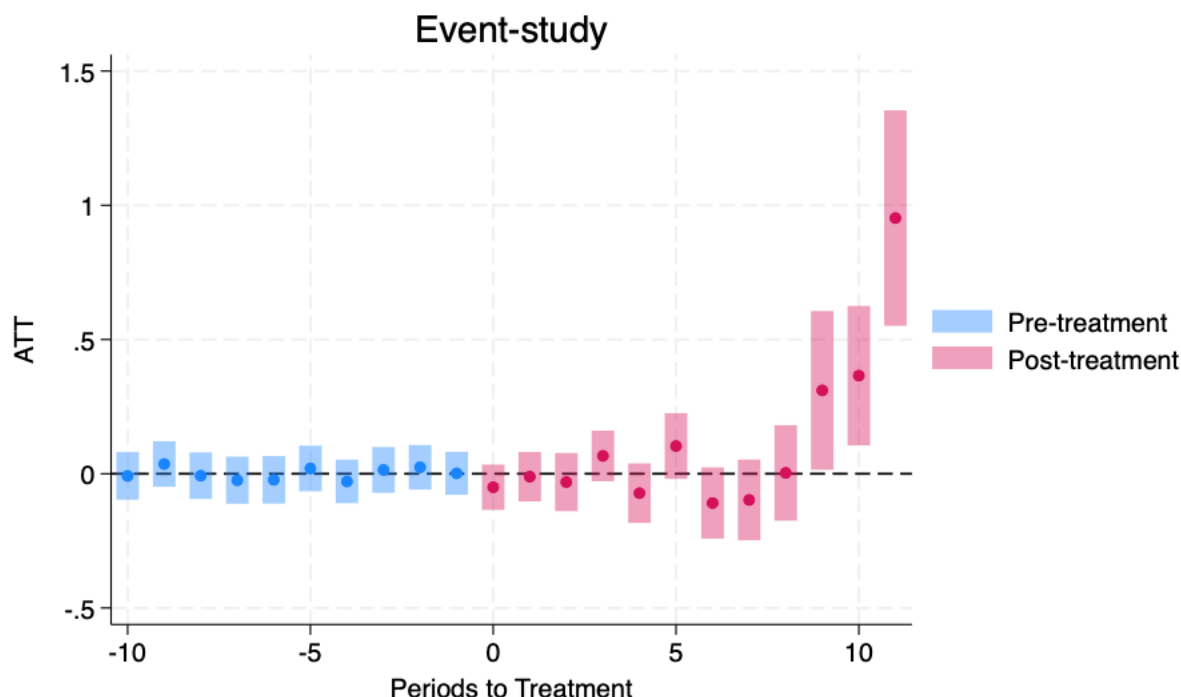
Figure 3: Notes: This figure plots event study estimates of the impact of the policy by comparing marriage cohorts over time, using not-yet-treated states as comparison groups.

## 6 Mechanisms

Our main result is that the increased female inheritance rights policy increased the likelihood of the presence of a toilet in the household. This effect is primarily driven by the latter adopting states, and cohorts of females who were most exposed to the policy before marriage. In this section, we explore plausible mechanisms that could explain these results.

We find that the policy induced women marrying men with higher education on average. Consistent with the evidence on the effect on toilets primarily driven by later adopting states, we also find that women marrying men with higher education on average is prominent in the later adopting states. We find little to no effect of the policy on women's education on average. This finding is consistent with the evidence documented by Roy (2015) who utilizes data on dowry payments and finds that earlier cohorts of females exposed to the policy received more dowries instead of higher education in rural India. We also explore the inequality within the household and estimate the impact of the policy on the difference in education between the husband and the wife within the household. We find that on average the policy reduced

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Table 4: Estimates for Never Treated and Not-Yet Treated on wife's education

| | Never treated as comparison group | | Not-yet treated as comparison group | |
|---|---|---|---|---|
| | Estimate | *p*-value | Estimate | *p*-value |
| ATT of units treated in 1986 | -0.5209 (0.4463) | 0.243 | -0.5677 (0.4377) | 0.195 |
| ATT of units treated in 1989 | -0.4097 (0.7775) | 0.598 | -0.4926 (0.7698) | 0.522 |
| ATT of units treated in 1994 | 0.5543 (0.4651) | 0.233 | 0.5543 (0.4651) | 0.233 |
| Aggregate ATT (GAverage) | 0.0294 (0.3096) | 0.924 | -0.0021 (0.3119) | 0.995 |
| Pre-trend Test ($\chi^2$) | 21.5476 | 0.8700 | 22.9608 | 0.8169 |

*Notes:* This table reports the coefficient estimates, standard errors and *p*-values of each treated group's average treatment effect on the treated parameter following Callaway & Sant'Anna (2021). The two sets of estimates presented in the column represent different comparison groups: never-treated and not-yet treated units. Standard errors are computed using wild cluster bootstrap at the state level. The last panel of this table reports estimates of a chi-square test which tests the null hypothesis of no differential pre-trends between treated and untreated units.

educational differences within the household by half a year.

# 7  Robustness

## 7.1  Using the *never-treated* as the comparison group

It might be argued that within the sample period, the never-treated group form a cleaner comparison group than the not-yet treated group. This is because identification with the never-treated as a comparison group, rests on weaker sets of assumptions than using the not-yet treated group. Additionally, using the never-treated group as a comparison group will imply that for each treated group we would use the same comparison group unlike when we would use the not-yet treated group, and as such requires the parallel trend assumption to be valid for only one comparison group. Consequently, the later adopting states will not be used as a control group for the early adopting states till the time they adopt the policy. We report the estimates of the effect of the policy on the treated using the never-treated as a comparison group. Once again we find that the conclusion of the main results remain unchanged.

Table 5: Estimates for Never Treated and Not-Yet Treated on husband's education

| | Never treated as comparison group | | Not-yet treated as comparison group | |
|---|---|---|---|---|
| | Estimate | $p$-value | Estimate | $p$-value |
| ATT of units treated in 1986 | -0.1256 (0.5419) | 0.817 | -0.1399 (0.5327) | 0.793 |
| ATT of units treated in 1989 | 0.6231 (0.6912) | 0.367 | 0.5607 (0.6847) | 0.413 |
| ATT of units treated in 1994 | 1.0889 (0.5005) | 0.030 | 1.0889 (0.5005) | 0.030 |
| Aggregate ATT (GAverage) | 0.6287 (0.3267) | 0.054 | 0.6112 (0.3289) | 0.063 |
| Pre-trend Test ($\chi^2$) | 27.3443 | 0.6051 | 26.9817 | 0.6242 |

*Notes:* This table reports the coefficient estimates, standard errors and $p$-values of each treated group's average treatment effect on the treated parameter following Callaway & Sant'Anna (2021). The two sets of estimates presented in the column represent different comparison groups: never-treated and not-yet treated units. Standard errors are computed using wild cluster bootstrap at the state level. The last panel of this table reports estimates of a chi-square test which tests the null hypothesis of no differential pre-trends between treated and untreated units.

Table 6: Estimates for Never Treated and Not-Yet Treated on gender gap in education

| | Never treated as comparison group | | Not-yet treated as comparison group | |
|---|---|---|---|---|
| | Estimate | $p$-value | Estimate | $p$-value |
| ATT of units treated in 1986 | -0.3953 (0.5062) | 0.435 | -0.4278 (0.4947) | 0.387 |
| ATT of units treated in 1989 | -1.0328 (0.5698) | 0.070 | -1.0534 (0.5623) | 0.061 |
| ATT of units treated in 1994 | -0.5345 (0.4690) | 0.254 | -0.5345 (0.4690) | 0.254 |
| Aggregate ATT (GAverage) | -0.5993 (0.2986) | 0.045 | -0.6133 (0.3001) | 0.041 |
| Pre-trend Test ($\chi^2$) | 34.7371 | 0.2523 | 35.5993 | 0.2215 |

*Notes:* This table reports the coefficient estimates, standard errors and $p$-values of each treated group's average treatment effect on the treated parameter following Callaway & Sant'Anna (2021). The two sets of estimates presented in the column represent different comparison groups: never-treated and not-yet treated units. Standard errors are computed using wild cluster bootstrap at the state level. The last panel of this table reports estimates of a chi-square test which tests the null hypothesis of no differential pre-trends between treated and untreated units.

## 7.2 Discussion on potential concerns

In this section we list various potential concerns which could threaten the identification of our parameter estimates. We discuss these concerns and provide evidence to show that our results are robust to these concerns.

### 7.2.1 Endogenous selection out of the policy

Another additional concern is whether information of implementation of this policy led families to pre-empt marriages of their sons and daughters. It could be imagined that if parents have strong preference to give the family inheritance to their sons, they would marry off their daughters sooner. If this were the case, then such individuals are endogenously self-selecting out of the policy and it would bias our results downward. This is because now the control group would consists of self-individuals who are less likely to be gender-progressive and possibly thus also less likely to have a toilet in the household. To address this concern, we investigate the distribution of the year of marriage. This is because such self-selection out of the policy, would generate a spike in the distribution of marriages right before the implementation of the policy. We do not find any evidence of this in the data. When we plot the distribution of marriages over time, we do not find any discontinuous change in marriage rates around the year of policy implementation as shown in Figure 4.

### 7.2.2 Endogenous selection into the policy

Gender-progressive families and individuals could potentially delay their marriage in order to be eligible for increased inheritance in anticipation of the policy. If this were the case, then this would lead such individuals to self-select into the treatment group and will not lead to clean comparisons in the difference-in-differences framework leading to potentially upward biased estimates. This is because if such individuals are systematically self-selecting into the policy then our estimate of the effect of the policy would be confounded with the unobserved selection of progressive individuals who in absence of the policy would have had a higher chance of having a toilet in the household. Such self-selection would be evident in the data by examining the distribution of age at marriage in the data. In Figure 5 we plot the average age of marriage over years for treated states. If there was a self-selection into the policy we would

Figure 4: Distribution of marriages over time



Y-axis measures the difference in year of marraige and year of policy implementation
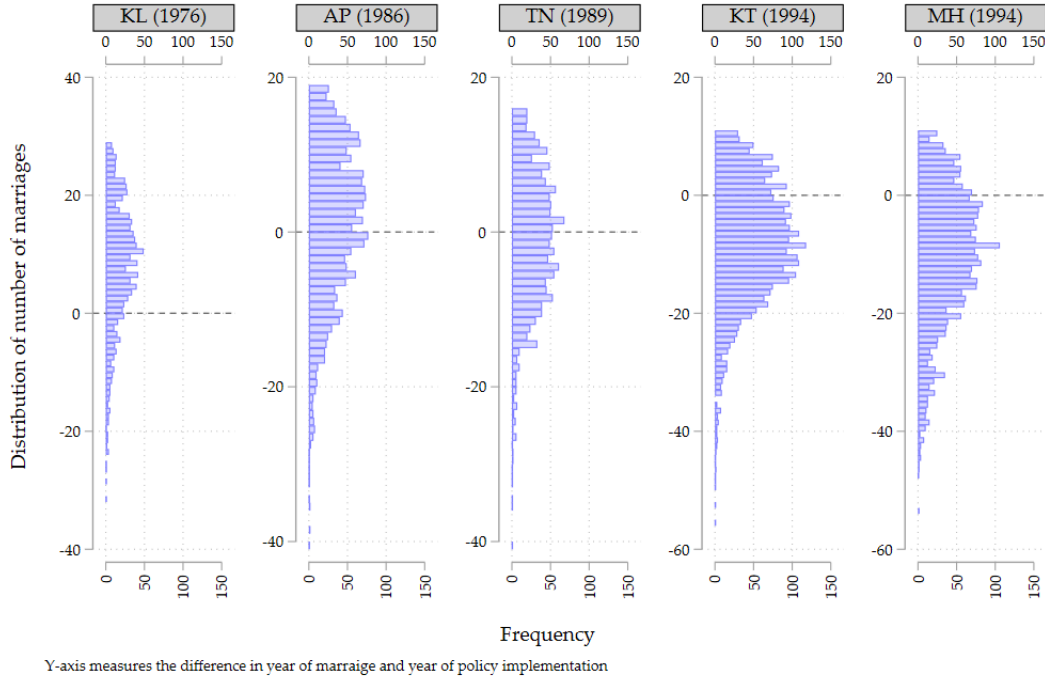
Notes: The figure plots the frequency distribution of the marriages over the difference in the year of marriage relative to the year of the implementation of the policy in each state. KL : Kerala, AP: Andhra Pradesh, TN: Tamil Nadu, KT: Karnataka and MH: Maharashtra.

have observed a spike in the average age at marriage in the years after the policy implementation in treated states. We do not find any such systemic jumps around the year of policy implementation as shown in Figure 5. This gives us evidence that it is unlikely that there was substantial self-selection into the policy to drive the observed results.

### 7.2.3 Post marital change in religion

We do not have data on females who have changed their religion, post-marriage. Failing to take this into account could result in biased estimates as religion is one of the criteria determining whether a woman benefitted under the amendment. However, this is not much of a concern as inter-religious marriages are a rare occurrence in India. Das et al. (2011) provides evidence that only about 2.1% marriages in India are inter-religious.[7] Roy (2015) in her analyses of the effect of HSA on female education, finds only 3% of marriages to be inter-religious. Furthermore, the occurrence of inter-caste marriages within a religion is rare.[8] Thus, not being

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[7]Social stigma is one of the biggest hindrances.

[8]Banerjee et al. (2013) shows evidence of strong preference of marrying within the caste, to the extent that individuals are willing to trade off qualities like having a masters degree and no education.

# Figure 5: Average age at marriage over time
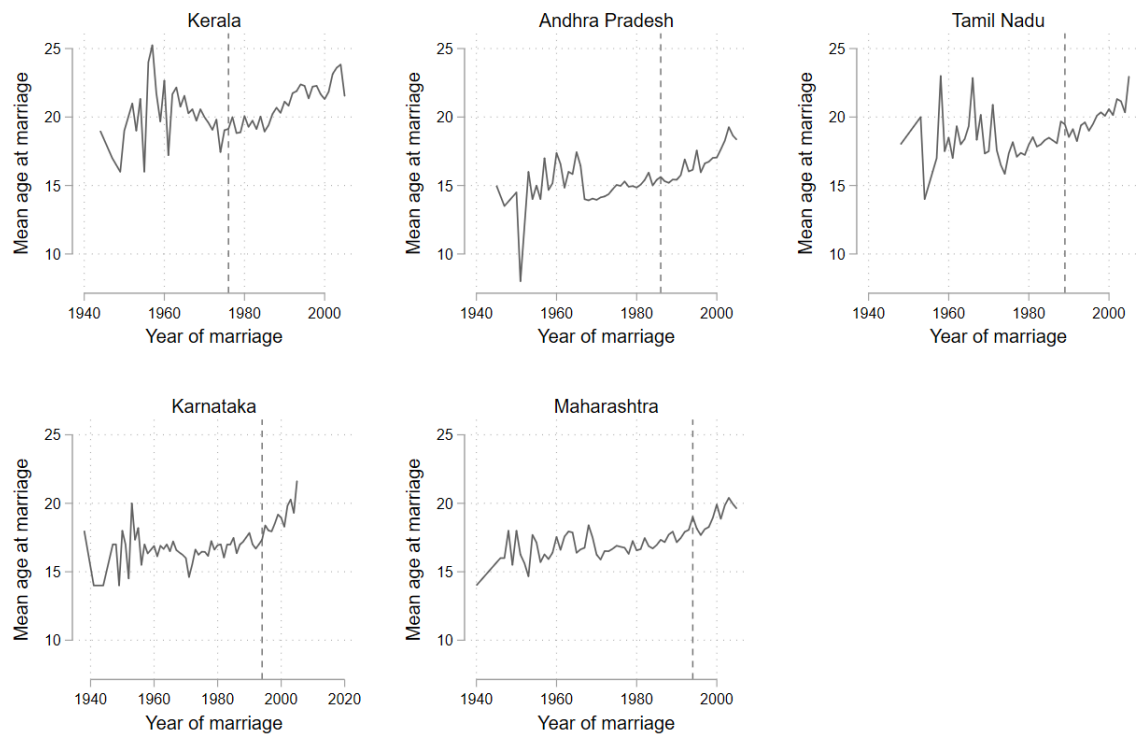


Notes: The figure plots the average age at marriage of females over years. The dotted line in each sub-graph points on the x-axis the year of policy implementation. The spikes in the earlier years of marriage stem from very small sample sizes.

able to observe the above events is unlikely to change the results.

## 8  Conclusion

Kofi Annan, speaking at the UN Headquarters, on the sixtieth anniversary of the Commission on the Status of females remarked[9]

*"...there is no tool for development more effective*

*than the empowerment of females and girls"* .

We examine a link between household sanitation and empowerment of females, as females disproportionately benefit from having access to private toilets, and on the other hand have lower bargaining power relative to males in the household. In this paper we estimate the causal impact of the Hindu Succession Act which improved female inheritance rights for women in India on the likelihood of the presence of a toilet in their households. We use a difference-in-differences framework with staggered adoption allowing for dynamic and heterogeneous treatment effects to estimate the impact of the reform on the presence of a toilet in the household. Given that one of the eligibility conditions is not observed in our, and in most datasets, we show that with minimal assumptions, the estimate given the data limitation, serves as a lower bound on the parameter of interest. We find a positive and significant impact of improved inheritance rights for females on the presence of a toilet in the household. We find that the results are primarily driven by significant effects on later adopting states. Additionally, we find that the effect of the policy was the strongest for women who were most exposed to the policy before being married. This is a key result, which in the context of females having little say in household decisions in rural India, goes to show how policies aimed at improving the status of females in the households—in our case by improving inheritance rights—can in turn positively impact their socioeconomic outcomes. This gives us suggestive evidence that the reform was successful in increasing the intra-household bargaining power of females which further enabled a change in household decisions from which they directly benefited such as access to a private toilet in the house.

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[9]Source: https://www.un.org/press/en/2006/wom1586.doc.htm

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# 9 Appendix

Table 7: Summary Statistics

|  | All states | Reform states | Non-Reform states |
|---|---|---|---|
| **I**(Toilet=1) | 0.46 | 0.46 | 0.46 |
|  | (0.50) | (0.50) | (0.50) |
| Years of education of head | 7.80 | 8.16 | 7.63 |
|  | (4.91) | (4.74) | (4.98) |
| Years of education of wife | 3.76 | 4.52 | 3.39 |
|  | (4.55) | (4.59) | (4.48) |
| Age of wife | 38.2 | 37.5 | 38.6 |
|  | (11.1) | (10.7) | (11.3) |
| **I**(HSA Religion=1) | 0.85 | 0.86 | 0.85 |
|  | (0.35) | (0.35) | (0.36) |
| log(income) | 10.5 | 10.5 | 10.5 |
|  | (0.97) | (0.95) | (0.98) |
| **I**(Urban=1) | 0.36 | 0.38 | 0.35 |
|  | (0.48) | (0.49) | (0.48) |
| #Teen-aged girls (15-20yrs) | 0.40 | 0.35 | 0.42 |
|  | (0.65) | (0.61) | (0.67) |
| #Girls (< 15yrs) | 0.92 | 0.76 | 1.00 |
|  | (1.05) | (0.93) | (1.09) |
| #Adult women (> 21yrs, <60 yrs) | 1.24 | 1.22 | 1.25 |
|  | (0.61) | (0.59) | (0.63) |
| #Senior citizens(> 60yrs) | 0.30 | 0.27 | 0.31 |
|  | (0.86) | (0.80) | (0.88) |
| **I**(Water within house=1) | 0.52 | 0.44 | 0.56 |
|  | (0.50) | (0.50) | (0.50) |
| Observations | 28708 | 9512 | 19196 |