The Unintended Benefits of Women's Empowerment on Household Sanitation[*†]

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November 1, 2024

Abstract

Existing research shows that women benefit more from private toilets, but misperceptions on the net benefits from toilets and lack of women's decision-making power can hinder toilet adoption by households. In this paper, we explore a novel link between household sanitation and policies that empower women. We find that a policy aimed at improving women's property inheritance rights in India led to an increase in toilet adoption in the households of treated cohorts. Using a heterogeneity-robust event-study design, we find that this effect is concentrated in states where the policy increased women's education—plausibly reducing misperceptions about toilet benefits—and enhanced women's decision-making power. Together, these mechanisms led to higher toilet adoption and are consistent with a discrete choice model of household decision-making capturing the complementarity between women's education and decision-making power in toilet adoption. Our findings highlight that policies empowering women can yield unintended benefits, such as improving toilet coverage in regions struggling with open defecation, as women tend to benefit more from private toilets than men.
JEL Codes: I15, I18, J16, O15, O18
Keywords: Empowerment, Sanitation, Health, Gender

[*]For guidance on this paper we thank Laura Schechter, Jeff Smith, and Chris Taber. For helpful comments and discussions, we thank Jesse Gregory, Priya Mukherjee, Junfu Zhang, Jon Denton-Schneider and Sugato Dasgupta. We thank seminar participants at the University of Wisconsin-Madison, Indian Statistical Institute, and Jawaharlal Nehru University. All errors are our own.

[†]Earlier versions of this paper were circulated under the title "Female Inheritance Rights and Household Sanitation".

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1 Introduction

Open defecation is a widespread problem in low and middle income countries and has been linked to illnesses and developmental problems like diarrhea and stunting in children, among many others. The practice is particularly prevalent in India, which accounted for 60% of the world's open defecation in 2011 (Census 2011). The barriers to demand for toilets in India stem from deep-rooted cultural norms of religious purity, casteism, taboos surrounding menstruating women, and widespread lack of awareness about the health risks associated with improper sanitation. However, within the household, the absence of a toilet disproportionately impacts females. Women who go out to defecate, urinate, or manage their menstrual hygiene in the open are often at risk of non-partner sexual violence and are twice as likely to experience sexual harassment compared to those with access to household toilets (Aid Water 2013, Jadhav et al. 2016, Caruso et al. 2017, Saleem et al. 2019, Hossain et al. 2022). Despite such difficulties faced by females, there exist several deterrents in the demand for toilets. First, lack of education and health-based awareness about the importance of sanitation is an important factor behind the low demand for toilets (Coffey et al. 2014, Banerjee et al. 2017). Second, females are rarely the primary decision makers within their households (Coffey et al. 2014) and thus are likely to be at a disadvantage to advocate for their needs. These observations motivate the question we answer in this paper: do policies that are aimed at empowering women lead to an increase in the demand for toilets, a household public good that females value disproportionately more than males (Khanna & Das 2016, Augsburg, Malde, Olorenshaw & Wahhaj 2023)?

We study this question by exploiting variation in the legal amendments to inheritance rights in India. The Hindu Succession Act of 1956 (henceforth, HSA), governed the property inheritance rights for Hindus, Sikhs, Jains, and Buddhists, representing about 86% of the country's population.[1] However, the HSA was gender-unequal, granting sons an exclusive birthright to ancestral household property and leaving daughters with substantially lesser inheritance rights. In order to address the gender inequality in HSA, it was amended in five southern states of India, which equalized the inheritance rights of daughters to that of sons (Kerala amended the HSA in 1976, Andhra Pradesh in 1986, Tamil Nadu in 1989, followed by Karnataka and Maharashtra in 1994), before the national amendment in 2005, when all states eliminated the gender-inequality. Importantly, for the five states that passed the HSAA between 1976

[1] As with most personal laws, property inheritance laws in India are governed by religion. The Hindu Succession Act established rules for the division of household property among heirs, in the event of unwilled succession (or intestate succession).

and 1994, the amendments only applied to those females who were unmarried at the time of the passing of the amendment, thus creating variation in treatment within the treated states.

We leverage this within-state variation in exposure to the HSAA across marital cohorts of women along with it's staggered adoption across states to estimate the causal effect of HSAA on the likelihood of toilet adoption in marital households of women. Our identification assumption is that in the absence of the HSAA, the rate of toilet adoption in the treated states would evolve in parallel to the not-yet treated states, across marriage cohorts. Using data from the 2005-06 wave of the National Family Health Survey (NFHS), a nationally representative survey of households across India, we estimate the heterogeneous and dynamic treatment effect of the HSAA on the presence of a private toilet in households in an event study framework using a heterogeneity-robust estimator (Callaway & Sant'Anna 2021).[2]

We find that the HSAA led to an increase in the presence of a private toilet in the marital household of treated cohorts of women by at least 3.2-3.7 percentage points. This estimate corresponds to a minimum increase of 8.4-9.7% in toilet adoption relative to the comparison cohorts in untreated states, who had an average toilet coverage of 36%. Estimates of the group-wise heterogeneous treatment effects show that this effect was driven by the later adopting states of Maharashtra and Karnataka that adopted the HSAA in 1994 who were 4.7 percentage points (equivalently 12.4%) more likely to have a toilet in their marital household. Estimates of the dynamic treatment effects show that in these states the impacts are driven by cohorts of women who were young at the time of policy amendment and got married at least 6-7 years after the HSAA was adopted. We find neither statistically significant nor economically meaningful impacts in the early adopting states—reasons for which, along with suggestive evidence, are discussed later. Our pre-period event study estimates along with pre-trend tests provide no statistical evidence to suggest that the pre-treatment effects are statistically or economically different from zero, strengthening our identification assumption of conditional parallel trends. Our results also hold when we restrict our sample to rural India.

Contributions and mechanisms: Our paper adds to the literature on adoption of toilets. To the best of our knowledge, our paper is the first to explore whether female-empowering policies, specifically the HSAA, increase household toilet ownership rates, marking our primary contribution. In addition to cultural norms, primary deterrents

[2]We explain the choice of our estimator given our context and data in section 4 where we describe our empirical strategy.

to toilet adoption in India, include financial constraints (Guiteras, Levinsohn & Mobarak 2015),[3] and misperceptions regarding the costs and benefits of toilets (Augsburg, Malde, Olorenshaw & Wahhaj 2023). Combining financial incentives with information campaigns on the importance of sanitation, the national-level *Swacch Bharat Mission* (Clean India Mission) in 2014 proved to be successful in substantially increasing toilet adoption in India in recent years.[4] In terms of driving demand for toilets through marriage markets, Stopnitzky (2017) demonstrate that the "No Toilet, No Bride" campaign in Haryana, India, significantly increased toilet ownership rates highlighting the gender gap in preference for toilets. We differ from the existing literature by being the first to study a potentially unintended benefit of a female-empowering policy on sanitation, whereas other studies have focused on direct factors driving the demand for toilets and the deterrents to adoption in the context of sanitation-focused policies and interventions. Our contribution—female empowering policies can have unintended benefits by increasing toilet coverage—is particularly important from a policy perspective, as sanitation-focused policies are often considerably expensive (for e.g., the *Swacch Bharat Mission* cost approximately $20 billion).[5]

Our second contribution lies in shedding light on the heterogeneous treatment effects of HSAA, specifically on education and decision-making power, which were main outcomes in prior studies assuming treatment effect homogeneity,[6] but serve as mech-

[3]The cost of building a toilet can be as high as 50% of the average household income (Augsburg, Malde, Olorenshaw & Wahhaj 2023).

[4]The predecessor to the *Swacch Bharat Mission*, namely the Total Sanitation Campaign (TSC) which did not have financial incentives and information campaigns was less effective in increasing toilet ownership (Hueso & Bell 2013).

[5]Our paper is also related to the literature that studies how identity and/or gender of the beneficiary of a policy could affect household outcomes. See for e.g., Duflo (2003) who find increases in nutritional status of young girls when pensions are received by women, and finds no effect when pensions are received by men, suggesting that efficiency of public transfer programs may depend on the gender of the recipient.

[6]Prior studies provide mixed evidence on HSAA's direct impact on improving women's inheritance rights. Roy (2015) and Agarwal et al. (2021) find that the amendments were not successful in improving actual inheritance received by women. The documented reason behind parental reluctance in bequeathing land (the main form of ancestral property in India) to daughters are patrilocality (the norm of daughters moving to their husband's house post-marriage) and the related risk that the property ends up being controlled by the in-laws of the daughters (Agarwal 1994, Agarwal et al. 2021, Bhalotra et al. 2020). Only Deininger et al. (2013) find that the HSAA improved female inheritance. However, they consistently show that the policy led to alternative forms of parental investment, especially in education (Deininger et al. 2013, Roy 2015, Bose & Das 2021, Ajefu et al. 2022). The HSAA also led to increased dowries (Roy 2015), enhanced women's decision-making power (Deininger et al. 2019, Mookerjee 2019, Biswas et al. 2024, Bose & Das 2021, Ajefu et al. 2022), greater labor market participation (Heath & Tan 2014) improved nutrition and health outcomes for beneficiaries' children (Ajefu et al. 2022), but had no impact on their education levels (Bose & Das 2021). Unintended negative impacts, such as increased sex-selective abortion in son-preference areas (Rosenblum 2015, Bhalotra et al. 2020), and higher suicide rates (Anderson & Genicot 2015), have also been documented.

anisms in our paper. To achieve this, we use variation across marital cohorts and between treated and untreated states to allow for heterogeneous and dynamic treatment effects in the context of staggered policy adoption, following the recent literature on treatment effect estimation (De Chaisemartin & d'Haultfoeuille 2020, Callaway & Sant'Anna 2021). Our analysis reveals that the HSAA significantly boosted education and increased decision-making power for treated females in late-adopting states. Since we find positive impacts of the HSAA on decision-making power in other treated states as well, our results emphasize education as the primary driver behind the HSAA's success in increasing toilet adoption. Support for our mechanisms and their order of importance is found in Augsburg, Malde, Olorenshaw & Wahhaj (2023). They show that while women generally value toilets more than men, misperceptions about costs and benefits often hinder investment even when credit constraints are relaxed. Our results suggest that increased education likely mitigated these misperceptions. Augsburg, Malde, Olorenshaw & Wahhaj (2023) also show that when misperceptions are low, women's involvement in decision-making can influence households to build a toilet, aligning with our secondary mechanism of improved decision-making power—though only when accompanied by improved education. We provide theoretical foundation for our empirical results using a static model of household decision-making of building a toilet which delineates the primary role of education and the secondary role of decision-making power in the adoption of toilets, consistent with our empirical findings and the existing literature.

We discuss the underlying heterogeneity in treatment effects, particularly the absence of significant impacts in the early adopting states of Andhra Pradesh and Tamil Nadu. We provide suggestive evidence that this heterogeneity stems from systemic differences in age at marriage and caste composition across states. In Andhra Pradesh, women tend to marry at a younger age, which reduces their chances for attaining higher education—a key mechanism through which the policy is expected to have an impact. In Tamil Nadu, the much higher proportion of socio-economically disadvantaged caste groups (above 95% within HSAA religions across marital cohorts) has historically faced substantial social and economic barriers, especially in accessing education, and these groups are less likely to benefit from policies unless specifically targeted. Both factors likely contributed to the limited effectiveness of the HSAA in these states.

Finally, we address a typical data caveat in the literature estimating the effects of the HSAA which marks our final contribution. An obstacle in estimating the treatment effects of the HSAA is that the treatment group is not perfectly observed in most pub-

licly available datasets. One of the eligibility criteria under the HSAA required that the natal household property of the female must have remained undivided at the time the HSAA was adopted in her state.[7] To the best of our knowledge, survey data on the timing of property division in India does not exist.[8] Hence, a major share of studies in this literature have ignored this data caveat.[9] We address this common data caveat by formally showing that one can identify and estimate lower bounds of the true average treatment effect on the treated within an event-study design, even while allowing for heterogeneous and dynamic treatment effects in a staggered policy adoption setting. This result is broadly related to similar ideas in the literature on partial compliance in randomized control trials (See for e.g., Bloom (1984) and Heckman, Smith & Taber (1998)).

The rest of the paper is organized as follows: Section 2 describes the institutional background of the original HSA of 1956 and the HSAA and describes the data. Section 3 outlines the empirical strategy and section 4 presents the main results. Next we introduce a simple static model of household decision-making to guide the empirical investigation of our underlying mechanisms in section 5 followed by section 6 where we present causal evidence for the mechanisms and discuss the importance of education as the primary mechanism. In section 7, we show our results to be robust to potential concerns, discuss suggestive evidence on the underlying heterogeneous treatment effects of our main results, and conduct a back of the envelope calculation to discuss the economic value of the unintended benefit of the HSAA on increased toilet coverage. Section 8 concludes.

[7] In the context of India, "natal household property" refers to the property owned by a woman's family of birth, typically including assets such as land, which may be subject to inheritance laws. The reason why the HSAA required natal household property to remain undivided was because the HSAA did not apply retrospectively. If a household's property was already divided before the amendment was passed in the state, then the daughters of that household were not eligible to receive their notional share of the property even if they satisfied all other eligibility criteria.

[8] One reason is that marriages in India are *patrilocal*, which involve women moving to their husband's household after marriage. As a result, most representative survey datasets collect limited data on the natal household characteristics of married women.

[9] Notable exceptions are Roy (2015) and Deininger et al. (2013) who use timing of death of grandfather and father, respectively, as a proxy for timing of household property division using the REDS data. However, REDS is not useful for our study since it lacks information on whether married daughters have a toilet in their marital households, our outcome of interest.

2 Institutional Details and data

2.1 The Hindu Succession Act of 1956 (HSA)

Inheritance rights in India vary by religion. The original HSA of 1956 governed the property rights of Hindus, Sikhs, Buddhists and Jains. It established the rules of division of household property in the aftermath of the death of the patriarch of the family in absence of a will.[10] Two major legal doctrines governing Hindu inheritance are the *Mitakshara* and *Dayabhaga* schools. The HSA governed the property rights following the *Mitakshara* system which distinguishes a person's individual property from joint ancestral property. Joint ancestral property typically includes ancestral land. It could also include any property that was inherited patrilineally, or any property that was merged into the ancestral property, or property acquired by the joint family (Agarwal 1994, Rosenblum 2015). Under the HSA, only the male heirs (sons, grandsons, great-grandsons) were entitled to a share in this joint ancestral property. Separate property could be accumulated separately, and the owner had the freedom to bequeath it to whomever they wished. Under the original rules, daughters of a male dying intestate (i.e., without writing a will) were equal inheritors, along with sons, only of their father's separate property. But the daughters had no share in the joint ancestral property. Rights to the joint property were limited to the *coparceners*[11] that only constituted male members of the family. Since joint property typically takes the form of land that is generally family owned, females were at a significant disadvantage under the gender unequal inheritance rules of the original HSA.

2.2 Amendments to Hindu Succession Act (HSAA)

Five states in southern India enacted legislation to amend the HSA at the state level– Kerala in 1976, Andhra Pradesh in 1986, Tamil Nadu in 1989 followed by Karnataka and Maharashtra in 1994–to redress the gender inequality in the original HSA. Under these amendments, daughters were granted equal inheritance rights as sons in the joint household property. This was conditional on daughters satisfying some eligibility criteria. First, she had to reside in one of the five reform states at the time of the amendment. Second, she had to be unmarried at the time when the amendment was passed in her state. Third, she had to hail from one of the HSA religions

[10] According to field studies, more than 65 percent of people who die each year do so without making wills, and this proportion is much higher in rural areas, suggesting the importance and applicability of HSA in governing inheritances for individuals (Agarwal 1994, Deininger et al. 2013).

[11] In the context of Indian inheritance laws, "coparceners" are family members who command equal shares in the inheritance of undivided ancestral property.

(Hinduism, Jainism, Sikhism or Buddhism). Finally, the household property of the woman's parental household 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.

2.3 Treatment definition

We define treatment status of a household based on whether *any* married woman in a given household was exposed to HSAA. Using the year of the latest marriage in the household, this definition assigns those household as treated if the latest marriage took place after the HSAA was adopted in their state. This approach removes potential measurement error that would arise if we used the marriage year of any earlier cohorts in the household.[12]

2.4 Data

We use data from the third (2005-06) wave of National Family Health Survey (or NFHS-III), a large scale, cross-sectional and nationally representative survey of households across 29 states in India. It collects detailed information about the socioeconomic status of households, educational attainment for all household members, and an additional questionnaire for women aged 15-49 years. The questionnaire covers a variety of questions on the marital status of women, years of education, year of marriage, as well as questions regarding women's autonomy and decision-making across various dimensions. The data also has information on private toilet ownership in the marital household of women, which is our main outcome of interest.[13,14]

Following previous papers in the HSA literature, we restrict our analysis and sample to women belonging to one of the HSA-eligible religions—Hinduism, Sikhism, Jainism, and Buddhism—in order to restrict comparisons across treated and control groups within the eligible religions. We drop the households belonging to the state

[12]For example, if the oldest woman in the household was married before the HSAA adoption, but her son was married after the HSAA adoption, using the oldest woman's marriage year would misclassify the household as untreated, when in fact the daughter-in-law in the household is exposed to the HSAA.

[13]The NFHS-III has information on whether the household has access to a toilet facility, type of toilet (with or without flush, pit latrines, composting toilet etc.), and whether the household shares the toilet with other households. For our analysis our main outcome of interest is whether the household has access to a private toilet.

[14]This is an eventual outcome recorded at a point in time in the year of the survey in 2005. Although we are unable to observe the exact year in which toilets were constructed, to the extent our parallel trends assumption holds, this is not a concern.

of Kerala (one of the five states to pass the HSAA) because the amendment in Kerala abolished joint family property altogether (Kerala Joint Hindu Family System Abolition Act), and the reform applied to all daughters regardless of their marital status (Agarwal et al. 2021, Deininger et al. 2013, Rosenblum 2015). This would leave no variation within the state to identify the impact of the HSAA on any outcome of households in Kerala. Given staggered adoption of HSAA across states and after removing Kerala, Andhra Pradesh was the first state to pass the amendment in 1986, followed by Karnataka in 1989, followed by Tamil Nadu and Maharashtra which were the last two states to pass the amendment in 1994 before the national ratification in 2005. We drop all households where marriages occurred after 2005 because of no variation in treatment status post 2005. We drop all households belonging to the state of Jammu & Kashmir, since the Hindu Succession Act does not apply in that state. This leaves us with a total of 27 states in our main analysis.

2.4.1 Summary Statistics

Table 1 reports summary statistics of key variables in our sample divided by treatment groups of states that passed the HSAA and the comparison group of states that did not pass the HSAA by the year 2005. The table shows some notable differences not only between treatment and control groups but also across treatment groups. Treated households across all groups are more likely to be in urban areas compared to the comparison group. The state of Tamil Nadu (treated in 1989) has the highest proportion of individuals from Other Backward Classes (0.695), significantly higher than the other treatment groups (0.398 and 0.515) and the comparison group (0.304). Additionally, the comparison group has a larger proportion of individuals from the General Caste (0.375), compared to state of Tamil Nadu (0.025) while being fairly similar to other treatment groups, reflecting caste-based differences in the population composition across these groups. Individuals in the state of Andhra Pradesh (treated in 1986) tend to marry at a younger age (around 17 years) compared to the other groups (in between 18.5 and 19.3 years). This difference reflects different social and cultural practices across treatment groups. While the wealth distribution looks fairly similar across all groups, most notable difference is in the state of Tamil Nadu where the proportion of the richest is 22% compared to 28% in the comparison group cohort. More households in Tamil Nadu belong to the 3rd and 4th wealth quintiles compared to the comparison group. This distinction in the wealth distribution is reflective of the higher proportion of individuals from historically socio-economically disadvantaged castes in the state.

3 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 even of repeated cross-section data. What enables us to allow for heterogeneous effects across groups, 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.[15] 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 their 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, Callaway & Sant'Anna 2021). Hence, we estimate the average treatment effect on the treated using the estimator proposed by Callaway & Sant'Anna (2021) which allow for heterogeneous treatment effects. For inference, we use wild bootstrap standard errors clustered at the state level allowing for arbitrary correlation between the unobservables within a state.

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 group in which i belongs that represents the year of policy implementation in states where HSA was amended. G_i takes a value of zero for any i who belongs to the non-HSAA states (i.e., states that did not amend HSA before the national ratification of the Act in 2005), representing that these individuals were never treated.[16]

The outcome of interest is $Toilet_{igt}$ which equals 1 if woman i married in year t belonging to group $g \in \mathcal{G} \equiv \{1986, 1989, 1994\} \cup \{0\}$ has a toilet in her household at the time of the survey.[17] We report estimates using the never treated as the comparison group in our main analysis. Results are robust to using not-yet treated units as the

[15] In our case, a group refers a given year of policy implementation. Hence each group comprises the set of states which pass the amendment in a given year.

[16] The notation in Callaway & Sant'Anna (2021) for never treated units i is $G_i = \infty$ denoting that these units are treated at time infinity.

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

comparison group instead.

3.1 Assumptions for identification

We make the standard identifying assumptions outlined 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. We rely on conditional parallel trends assumptions for the purpose of identification of the parameter of interest. This assumption implies that in absence of HSAA, the evolution of toilet ownership in the amendment states would be parallel to the evolution of toilet ownership in never-treated states, for households with similar characteristics which are relevant for toilet ownership. To ensure this, we condition on the following household characteristics in our estimations: indicators of wealth quintile, caste, and urban residence. Therefore, we impose the parallel trends assumption conditional on these characteristics and is described in equation (1) as a statement on the counterfactual: the differences in average potential outcomes (toilet ownership in absence of policy) for any two cohorts of women that got married at any two years (t, t') in any amendment state would be the same as the difference in average outcomes for the same two marital cohorts in the non-amendment states.

$$\mathbb{E}\left[Y_{it}(0) - Y_{it'}(0) \mid X_i, G_i = g\right] = \mathbb{E}\left[Y_{it}(0) - Y_{it'}(0) \mid X_i, G_i = 0\right] \tag{1}$$

for all $t, t' \geqslant g_{\min} - 1$, where $g_{\min}$ is the first period where any married woman is treated (1986 in our case), and X_i denote time-invariant covariates of woman i. Equation (1) specifies that in absence of the policy, for each group the potential outcomes between treated and never-treated cohorts would evolve in parallel on average.

3.2 Average treatment effect on the treated

Under the assumptions described in the previous section, we use variation in treatment timing relative to the year of marriage to identify the average treatment effect on the treated for each group g (year of policy implementation) and time period (marriage cohort) t denoted by $ATT(g,t)$. Intuitively, we can identify $ATT(g,t)$ for each group g married in year t, by comparing the expected change in outcome between cohorts in a given group g that were married in year t and those that were married in year $g-1$ (the year prior to policy amendment for the group) to the same difference for control states (never-treated or not-yet treated). Formally, under the conditional parallel trends assumption, using any comparison group $\mathcal{G}_{\text{comp}}$, the average treatment

effect on the treated for each group g and time period t is given by:

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid X_i, G_i = g\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid X_i, G_i \in \mathcal{G}_{\text{comp}}\right] \quad (2)$$

We use the doubly robust estimator proposed in Callaway & Sant'Anna (2021) who extend estimators for two-period, two groups setup developed by Sant'Anna & Zhao (2020) to multiple periods and groups, to estimate the $ATT(g,t)$'s. The doubly robust estimator performs better than alternative estimands such as inverse probability weighting, especially when the data are not a balanced panel, which is our case. See Callaway & Sant'Anna (2021) and Sant'Anna & Zhao (2020) for more details.[18]

3.3 Bounds on the true parameter

One of the eligibility criteria under the amendment was that the woman's natal household property should have been undivided at the time the amendment was passed in her state. To the best of our knowledge this data does not exist in any survey of Indian households. Consequently, in our empirical model, the treatment group is likely to be mis-measured as some individuals who should ideally be in the control group would end up in the treatment group instead. This mis-measurement could lead to a bias in the estimated average treatment effect. We address this by deriving bounds on the true parameter when the treatment group is mis-measured and find that our estimates serve as the lower bound on the true ATT.

Appendix Section A.3 shows that not observing one of the eligibility criteria defining individual treatment status can allow us to identify bounds of the treatment effect if the unobservable criterion is independent of other variables and only affects the outcome through treatment. In our context, since we allow for heterogenous treatment effects, this would require the assumption that for each group g, the timing of division of property is independent of other variables. We support this assumption following Roy (2015) who uses the year of death of the grandfather–a plausibly random event–as a proxy for when property division occurs. The intuition of this result is simple. If a researcher observes all but one one eligibility criterion, some individuals who truly belong to the control group (meeting all but the one unobserved criterion) are mistakenly classified as treated. Since the treatment effect for these individuals should be zero, their inclusion in the treatment group increases the size of the treated sample and thus reduces the average treatment effect. Although the control group shrinks, its

[18]Similar ideas guide identification in double/debiased machine learning methods (Chernozhukov et al. 2018).

average effect remains unchanged, as the true effect for these misclassified individuals is zero. Consequently, if the true treatment effect is positive, the estimated effect will be understated, as is the case here.

Our approach bears an analogy to the literature on partial compliance in randomized controlled trials, such as the work by Heckman et al. (1998). In fact, the 'original' Bloom (1984) paper motivated a rescaled estimator similar to what we show in the Appendix Section A.3, noting that the average outcome for the treated group is a mix of zero and non-zero treatment effects. This parallel further strengthens our argument.

3.4 Pairwise pooling of consecutive marital cohorts to improve precision of estimates

Even though we show identification of parameter(s) of interest with our cross-sectional data, the lack of a panel data negatively impacts the precision of our estimates. This is because in the cross-section, the number of households belonging to each marital cohort is small, and hence the number of treated households in each group-cohort cell is also small. This would lead to higher standard errors of our estimates, since the estimator estimates each group-cohort ATT and then aggregates them to estimate the overall ATT for each group.

To improve the precision of our estimates, we pairwise pool two consecutive marital cohorts to estimate the group-time ATTs.[19] Specifically, keeping the first treated marital cohorts of 1986, 1989 and 1994 unchanged, we pool all other pairs of consecutive marital cohorts t and $t+1$ to improve the precision of our estimates. In doing so, we make a weak assumption of unobserved differences between the treated and control groups, and cohort-specific effects to remain constant between two consecutive marital cohorts. Note that this is much weaker than implementing a group-wise two-by-two comparison of treated and untreated groups before and after treatment where all cohorts after treatment, and all cohorts before treatment are pooled together. By pooling two consecutive cohorts, for each pooled group-cohort we are estimating an equally weighted average of the treatment effects of the two group-cohorts since equation 2 holds for each group and each cohort. Additionally, this pooling also makes the no-anticipation assumption weaker as we are now imposing no anticipation assumption for two cohorts before treatment instead of one.

[19] We thank Jeff Smith for this suggestion.

4 Results

In this section we report and discuss the results from our estimation of the effect of the HSAA allowing for heterogeneous and dynamic treatment effects. As discussed above, we interpret our estimates as a lower bound of the true treatment effect.

4.1 Heterogeneous Treatment Effects

We find evidence of heterogeneous treatment effects of the policy across the states that adopted the HSAA in different years. We report the group-wise (defined by year of policy implementation) and the aggregated average treatment effects of the policy on the treated in Table 2. In the states of Maharashtra and Karnataka that adopted the HSAA in 1994, toilet coverage is estimated to have increased by 4.75 percentage points on average than it would have been had it not adopted the HSAA. This is a substantial increase and compared to the never-treated group which had an average toilet coverage of 38.3%, this is a 12.4% increase. We find that the policy did not have a statistically significant impact on the likelihood of women's marital households having a toilet for the early adopting states in our sample—Andhra Pradesh, in 1986 and Tamil Nadu in 1989—with the corresponding estimates being very close to zero. The corresponding weighted average of the group-wise treatment effects gives us the estimate of the aggregate treatment effect of 3.2pp. Finally, a pre-treatment test of the null hypothesis of no differential pre-trends between treated and untreated groups across all marriage cohorts produces a chi-squared test statistic estimate of 21.32-20.48 (p-value ranging in between 0.5 and 0.55). Hence, 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.

4.2 Average treatment effects on the treated over time

We estimate dynamic treatment effects of the HSAA in an event-study design to investigate the group-wise average treatment effects of the policy on the treated over time by comparing average outcomes of different marital cohorts across treated and untreated groups. This exercise is useful in shedding light on how the policy impacted different cohorts of women. In particular, for each treated group and time period, 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 design 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 allowing us to conduct a falsification test of the identification assumption of conditional parallel trends. We plot the event study estimates in Figure 1 containing 4 subplots for each group of states that adopted the HSAA in different years, and an aggregated event study plot that plots the weighted average of the group-period-specific treatment effects.

In the pre-treatment period, that is for households where women who were married before the HSAA was adopted and thus were not exposed to the HSAA, the event study plots show that, there are no statistical differences between the treated and untreated states in the average likelihood of the presence of a toilet, for all treated groups. This supports our conditional parallel trends assumption—in the absence of the policy, the evolution of toilet presence in households in treated states would have evolved in parallel to those in untreated states. Furthermore, our event study estimations take into account long differences to estimate pre-policy estimates, to address concerns surrounding pre-trends and pre-trend testing using short-differences (Roth 2013).

In the post-treatment periods, the event study plots show upward trends in toilet adoption for cohorts that got married at least 2 years after adoption of the HSAA in the states of Maharashtra and Karnataka (adopted HSAA in 1994) with the largest effects for cohorts who got married at least 6 years after policy adoption. Consistent with the results on the heterogenous treatment effects across groups we find no evidence of statistically significant and economically substantial dynamic treatment effects in the early adopting states of Andhra Pradesh (adopted HSAA in 1986) and Tamil Nadu (adopted HSAA in 1989).

4.3 Two-way Fixed Effects Estimates

Estimates using a two-way fixed effects (TWFE) difference-in-differences model including a state fixed effect and a year of marriage fixed effect, reported in Appendix Table A5, shows that the HSAA led to an increase in toilet adoption by 2.2 pp (p-value < 0.001) on average.[20] Pooling all groups together, the TWFE estimator unsurprisingly improves the precision of the ATT estimate by increasing power. However,

[20]Specifically, we estimate the following equation: $Y_{isc} = \alpha + \delta_{s(i)} + \delta_{c(i)} + \beta D_{i,c(i)} + X_i'\gamma + \epsilon_{isc}$, where Y_{isc} is the indicator of the presence of a toilet in the household of individual i in state s who belongs to the marital cohort c; $\delta_{s(i)}, \delta_{c(i)}$ respectively represent the state and the year of marriage or marital cohort fixed effects, and $D_{i,c(i)}$ is an indicator whether individual i belonging to the marital cohort $c(i)$ was married after the HSAA was adopted in her state, and X_i denotes household level controls. The estimate of the average treatment effect on the treated of the HSAA is given by β.

the estimate using TWFE is 31% lower than the estimates from the heterogeneity-robust estimator. In light of the evidence on heterogeneous and dynamic treatment effects presented earlier, this suggests that the TWFE estimator could be using a non-convex weighted average of the underlying heterogeneous and/or dynamic treatment effects.

5 Theoretical framework to guide mechanisms

In this section, we present a model to provide a theoretical foundation to guide the empirical investigation of the mechanisms through which we find the main empirical result of a woman empowering policy—the HSAA—increases toilet adoption.

5.1 Primitives and Assumptions

We consider a static model of a population of households indexed by $h \in \mathcal{H}$ with individuals indexed by i. Each household consists of a man ($i = m$) and a woman ($i = w$). Each individual i in household h derives utility from consumption and the presence of a toilet:

$$U_{i,h}(X_h, T_h) = u(X_h) + \beta_{i,h} T_h, \quad i = \{m, w\}, \quad s.t. \quad X_h = Y_h - C_h \cdot T_h \tag{3}$$

where, $u_{i,h}(X_h)$ is the utility from consumption for individual i, assumed to be strictly increasing and weakly concave in X_h, which is the amount of a numéraire household consumption good, $T_h \in \{0, 1\}$ is the indicator of the presence of a toilet in the household. $\beta_{i,h}$ represents the valuation of the presence of a toilet by individual i of household h.

For simplicity, we assume that all individuals value consumption equally. Without loss of generality, and for simplicity we assume that consumption utility is linear, i.e., $u(X) = X$.[21] Following existing literature that shows that women value toilet more than men, we assume that $\beta_{w,h} > \beta_{m,h}$ for all h.

The cost of having a toilet can be thought of as the monetary cost of making the toilet net of how much the household saves by not incurring additional healthcare costs resulting from open defecation, or in general from not having a toilet in the household. Individuals do not observe this true net cost of a toilet in the household denoted by

[21] Note that model implications hold for any functional form of $u()$ as long as it is strictly increasing and weakly concave.

${C_h}^*$. Instead, they observe a net perceived cost C_h which enters their budget constraint and is modeled as,

$$C_h = {C_h}^* + \eta_h \quad \text{where} \quad \eta_h \sim \mathcal{N}(0, \sigma_h^2) \tag{4}$$

where, η_h is a noise term representing uncertainty. This uncertainty could be thought to consist of the uncertainties in the true monetary cost of building a toilet net of the uncertainty in the health costs of not having a toilet. We assume that the variance of the noise σ_h^2 decreases with increased education of either the woman or man, or both:

$$\sigma_h^2 = f_h\left(E_{w,h}, E_{m,h}\right), \quad f_h'(.) < 0 \quad \forall h \tag{5}$$

Given that we find no empirical evidence on the man's education (See Section 7.3), in the remaining discussion of the model, we focus on the case where there are exogenous changes to the woman's education level.[22]

The household's total utility is a weighted sum of the individuals' utilities:

$$\begin{aligned} U_h(T_h) &= \theta_{m,h} U_{m,h}(X_h, T_h) + \theta_{w,h} U_{w,h}(X_h, T_h) \\ &= \theta_{m,h} U_{m,h}(Y_h - C_h \cdot T_h, T_h) + \theta_{w,h} U_{w,h}(Y_h - C_h \cdot T_h, T_h) \end{aligned} \tag{6}$$

where $\theta_{w,h} \in [0,1]$ is the woman's decision-making power, and $\theta_{m,h} = 1 - \theta_{w,h}$ is the man's decision-making power.[23]

Household Decision

The utility difference between building and not building a toilet for household h is:

$$\begin{aligned} U_h(T_h = 1) - U_h(T_h = 0) &= -C_h + \theta_{m,h}\beta_{m,h} + \theta_{w,h}\beta_{w,h} \\ &= -(C_h^* + \eta_h) + \theta_{m,h}\beta_{m,h} + \theta_{w,h}\beta_{w,h} \\ &= \Delta_h - \eta_h, \end{aligned}$$

where $\Delta_h \equiv \theta_{m,h}\beta_{m,h} + \theta_{w,h}\beta_{w,h} - C_h^*$ represents the household valuation of a toilet net

[22] Alternatively, further generalization can be made wherein the variance of the noise decreases more with the education of the individual who values the toilet more. For example consider $\sigma_h^2 \equiv \sigma^2 - k(\beta_w E_{w,h} + \beta_m E_{m,h})$ where $k > 0$ is a proportionality constant. Thus, if the husband's utility from having a toilet is very low i.e., $\beta_m \approx 0$ then the variance can only be reduced through increasing woman's education. The results would hold in such generalizations.

[23] The model can be easily extended to a dynamic set-up where if the household decides to build a toilet by incurring a one time cost, but enjoys the benefits of the toilet in all consequent periods. All the results shown below extend into the dynamic set-up where we would work with the present discounted value of future utilities of having a toilet relative to not having a toilet.

of the true cost for household h. Household h decides to build the toilet if $\Delta U_h(T_h) \geq 0$, i.e., if $\eta_h \leq \Delta_h$. The probability that household h builds a toilet is:

$$\begin{aligned} \Pr(T_h = 1) &= \Pr(U_h(T_h = 1) - U_h(T_h = 0) \geq 0) \\ &= \Phi\left(\frac{\Delta_h}{\sigma_h}\right) \end{aligned}$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function.

The proportion of households building toilets in the population is:

$$P = \int_{h \in \mathcal{H}} \Pr(T_h = 1)\, dF(h), \tag{7}$$

where $F(h)$ is the distribution of households over the characteristics $\{\Delta_h, \sigma_h\}$.

The propositions that follow from the model are:

Proposition 1 *Increasing women's education on average increases the proportion of households building toilets by reducing the noise σ_h in perceived costs.*

Proof: See Appendix A.4.1.

Proposition 2: *Increasing women's decision-making power across households has a positive effect on the proportion of households building toilets. This effect is substantial only when the noise σ_h is low (high education).*

Proof: See Appendix A.4.2.

Proposition 3: *Increasing women's education and decision-making power has a combined positive effect on the proportion of households building toilets, due to the combined effect resulting from the above two propositions.*

Proof: See Appendix A.4.3.

The primary channel through which the model operates is that increasing education reduces the uncertainty in costs of having a toilet net of the benefits of having a toilet. This reduction in uncertainty leads households to realise the true net benefit of having a toilet. As long as there are more households in the population who truly would benefit from having a toilet–through reduced healthcare costs, increased safety of women, etc.–the proportion of households building toilets increases as a result of increased education.

The model also shows that as long as women value toilets more than men, increased

decision-making power of women can only increase toilet adoption when the level of noise in perceived costs is low due to increased education. By itself, increased decision-making power of women does not have a substantial impact on toilet adoption if education levels are low and consequently the noise in perceived net costs is high.

Finally, the model shows that the combined effect of increased education and decision-making power is positive on toilet adoption. This is results intuitively from the positive effect of increased education on toilet adoption being amplified along with increased decision-making power of women, given that women value toilets more than men.

6 Mechanisms

Our data allows us to test for two mechanisms that could plausibly drive our main results on toilet ownership: women's years of educational attainment and their intra-household decision-making power within their marital household. [24] Increased education could increase toilet coverage through increase in health and sanitation based awareness and reducing misperceptions regarding costs and benefits of toilets. It could also empower women to question pre-existing gender unequal social and religious norms which hinder toilet adoption. With women preferring toilets more than men, an increment in their intra-household decision-making power could also increase toilet coverage if the HSAA enhanced their decision-making power. We use the same estimation strategy as before but with different outcomes, to test whether these factors are affected by HSAA and whether they align with our main results on toilet ownership.

6.1 Years of educational attainment

We report the estimates of heterogenous treatment effects of the HSAA on the women's years of education in Table 3. Consistent with our main results, we find that exposure to HSAA causes an increase in the years of educational attainment predominantly in the states of Maharashtra and Karnataka that adopted the HSAA in 1994 by 0.45 years and is statistically significant at the 95% confidence level. Over a control group average of 4.9 years of education, this estimate represents a 9.18% average increase in

[24] The HSAA could increase years of education of treated women if parents use education as alternate forms of investments in their daughters instead of property (for e.g. see, Roy (2015)). The HSAA could increase women's decision making power through either increased inheritance or, increased dowries (for e.g. see Deininger et al. (2019), Bose & Das (2021), Mookerjee (2019), and Biswas et al. (2024)).

years of education in these treated states.[25] These impacts in the late-adopting states are strong enough to drive an overall average treatment effect of the HSAA on years of educational attainment. Consistent with our main results, we find little to no effect of the HSAA on years of education in other states.

Allowing for dynamic treatment effects, we plot the corresponding event study estimates in Figure 2 which corroborate the results described in the previous paragraph. Here too we find an upward trend in education attainment for cohorts who married at least 3 years after the HSAA implementation in the states of Maharashtra and Karnataka, with the strongest effects observed for cohorts who were married at least 6-7 years after HSAA adoption. This implies that the policy had the strongest affect on cohorts that were relatively young at the time of policy implementation in Maharashtra and Karnataka. This finding is similar to Roy (2015) and Deininger et al. (2013), but we provide an additional insight that this result is primarily concentrated in the late adopting states with little to no effect in the early adopting states.

6.2 Intra-household decision-making power

We use individual survey questions on women's household decision making and code answers to each question as 1 to denote higher empowerment, and 0 otherwise. Then we use PCA to create an overall decision-making index, and standardize it using moments from the control group distribution to create z-scores of decision-making power of women in the household.[26]

We report the estimates of heterogenous treatment effects of the HSAA on intra-household decision-making power of women in Table 4.[27] The estimates reported in Table 4 show statistically significant effect of the HSAA on the decision-making power of women in the treated states of Maharashtra and Karnataka (adopted HSAA in 1994) where overall ATT increases by 0.112 s.d. units for treated women significant at the 95% level, and the event study estimates for the same group in Figure 3 provides support in favor of this mechanism depicting a gradual upward trend in the decision-making

[25] Our estimate on the impact of HSAA on years of education is similar to Roy (2015).

[26] The household decision-making index is constructed by making use of the following survey questions: indicators for whether the woman makes decisions about her health care, major household purchases, purchases for daily household needs, and visiting family and relatives.

[27] The parallel trends assumption for intra-household decision-making power is conditional on an additional indicator variable for whether the household belongs to any of the matrilineal states in the North-East, allowing for differential distribution of bargaining power between matrilineal and patrilineal states. Although this additional conditioning is necessary when intra-household decision-making power is the outcome of interest, our full set of other results remains robust to the inclusion of this dummy variable.

power of treated women. For cohorts in the state of Tamil Nadu (adopted HSAA in 1989), while there appears to be a substantial increase in decision-making power following the HSAA, however, that does not translate into higher toilet ownership rates for this group as per our main results.[28] This suggests that higher-decision making power alone could not translate into advocating for building a toilet, unless education is also increased thereby plausibly increasing sanitation based awareness.

6.3 On the importance of increased education as the primary mechanism

Augsburg, Malde, Olorenshaw & Wahhaj (2023), using random variation in access to sanitation-based credits, demonstrate that although women generally perceive toilets as more beneficial than men, the primary barrier to investing in private toilets often stems from misperceptions about their costs and benefits. This finding supports our results, where increased education emerges as the key mechanism driving the HSAA's impact on increasing toilet coverage. Education not only improves sanitation awareness but also reduces these misperceptions, making the benefits of toilets clearer to households.

Moreover, Augsburg, Malde, Olorenshaw & Wahhaj (2023) show that when misperceptions are low and women participate in household decision-making, their views on the costs and benefits of sanitation significantly influence whether the household takes out a sanitation loan and ultimately builds a toilet. This evidence aligns with our secondary mechanism of improved decision-making power, but only in the context of low misperceptions. This suggests that the primary and necessary mechanism through which the HSAA improved toilet coverage was increased education. Without education to mitigate misperceptions, an increase in women's decision-making power alone would have been unlikely to drive toilet adoption. Hence, across all our results, we do not find evidence that increased bargaining power without an accompanying increase in education led to improvements in toilet adoption.

[28] Roy (2015) finds evidence of increased dowries as a result of homogenous treatment effects of the HSAA. Increased dowries themselves could have led to increased decision-making power. Indeed, with some documented evidence of Tamil Nadu having one of the highest rates of dowry practices in India (Upadhyay 2012) (and Maharashtra having one of the lowest), it is plausible that increased decision-making power in Tamil Nadu could be driven by increased dowries. Future research focused on the heterogeneous treatment effects of the HSAA on dowries could provide further insights.

7 Additional exercises, discussions and robustness checks

In this section, we discuss the underlying reasons of the estimated heterogeneous effects of the HSAA on different states. We also outline potential concerns that could threaten the identification of our parameter of interest and provide evidence demonstrating that our results are robust to these concerns.

7.1 Discussion on the heterogeneity of treatment effects

In this section, we discuss the underlying heterogeneity in treatment effects across the treated states. Specifically, we discuss the systemic differences between the treated states to explain some plausible suggestive evidence on why the HSAA did not have a significant impact on toilet ownership in the early-adopting states of Andhra Pradesh and Tamil Nadu.

First, in Andhra Pradesh (which adopted the HSAA in 1986), we observe that women systematically marry earlier than in the other treated states. In Figure 5, we plot the average age at marriage by treated groups across marital cohorts. We find that the average age at marriage for women from Andhra Pradesh is significantly and consistently lower than that of women in the other treated states. This suggests that women in Andhra Pradesh were less likely to pursue higher levels of education before marriage, which is a key mechanism through which the HSAA increases toilet ownership. We find evidence consistent with this claim: the average years of education for women in Andhra Pradesh is significantly lower than in Maharashtra and Karnataka, regardless of whether the marital cohort was exposed to the HSAA or not.

Second, in Tamil Nadu (which adopted the HSAA in 1989), we document that a significant proportion of the population belongs to any one of the socio-economically disadvantaged caste group of either schedule caste, or schedule tribe or OBC ("Other Backward Class"). In Fig 4 we plot the proportion of individuals who do not belong to the general caste group (equivalently belong to either schedule caste, schedule tribe or the other backward class group) and find that it is above 95% in Tamil Nadu across marital cohorts.[29] In contrast, in other treated states, a larger share of the population does not belong to any one of the disadvantaged caste groups. A vast literature

[29] In Appendix Figure B5, we further disaggregate this by different disadvantaged caste groups and plot their proportion across marital cohorts in states that adopted the HSAA in different years. We find that the proportion of OBCs in Tamil Nadu is significantly higher than in the other treated states. The OBCs are one of the most socio-economically disadvantaged groups among all caste groups. They can be further subcategorized into "Backward Class" (BC) and "Most Backward Class" (MBC). Their proportion is close to 70% in Tamil Nadu, according to the Tamil Nadu Household Panel Survey's Pre-Baseline Survey (TNHPS-PBS) 2018-19. See discussions here and here.

on caste, documents how socio-economically disadvantgaed caste groups face significantly higher social and economic barriers in economic mobility, and thus to pursue education. These groups have systematically lower education levels because of such frictions and various government affirmative action programs specifically target these groups in various capacities.[30] This is plausibly one of the suggestive reasons why we do not find significant treatment effects of the HSAA on toilet adoption in Tamil Nadu.

7.2 Impact on rural households

We restrict our sample to rural households to examine the impact of the HSAA on toilet ownership in rural India. We report the main results in Appendix Table A1.[31] Similar to our main results, we find that the HSAA led to an increase in the rate of toilet ownership in rural India, with the effect being driven by the impact of the HSAA in the states of Maharashtra and Karnataka by 3.88 pp (p-value = 0.07).[32] This estimate corresponds to a 16.24% increase in toilet coverage compared to rural households in untreated states where the average toilet coverage was 23.89%.

This effect is driven by the HSAA increasing the years of education by 0.875 years (p-value = 0.001), and decision-making power of women by 0.147 s.d. (p-value = 0.061) in these states on average. We report the results on these mechanisms in Appendix Tables A2 and A3. Notably, households in rural India face additional cultural constraints such as stronger societal norms surrounding religious purity, and infrastructural constraints such as the absence of piped water supply, which could explain the smaller impact of the HSAA on toilet ownership in rural India compared to the overall sample, despite the larger impact on the years of education.

7.3 Impact on husband's education

A policy improving women's inheritance rights could potentially impact the *observed* husband's education, through its impact on the marriage market equilibrium. Specifically, in equilibrium increased female education could result in increased demand for,

[30] For example the RTE (Right to Education Act) of 2009 specifically requires private schools in India to reserve 25% of their seats for children belonging to disadvantaged caste groups. See for e.g., Agarwal (2024).

[31] The respective event study plots of the main results and the mechanisms are in Appendix figures B1, B2 and B3.

[32] Note that given the data hungry nature of the heterogeneity-robust estimator, we loose precision in our estimates once we restrict the sample to rural households only.

and consequently increased matches with, more educated males.[33] In other words, increased female education could lead to higher rates of positive assortative matching in the marriage market. To the extent of its empirically validity in the data, it is important to note that such changes in the marriage market equilibrium is still the consequence of the HSAA—a woman empowering policy. As a result, this exercise should not be interpreted as a placebo test, but rather as more generic test of the underlying mechanisms.

According to our theoretical framework, the noise in the perceived net cost of having a toilet can be reduced by increasing the education of the wife, or the husband, or education of both. We have already shown that the HSAA increased women's education in the latter adopting states of Maharashtra and Karnataka. We report the estimates of heterogenous treatment effects of the HSAA on the husband's education in Table A4 We find no evidence that the policy significantly changed the education of the husband in any of the treated states.[34] This suggests that the increment in toilet adoption in the latter adopting states of Maharashtra and Karnataka is primarily driven by the increase in women's education, without significantly altering the marriage market equilibrium, and consequently the husband's education.

7.4 Endogenous selection into or out of the HSAA

There are two concerns regarding potential selection. First, if parents strongly prefer to pass family inheritance to sons over daughters, they may respond by marrying off their daughters before the state-level amendments. In this case, such individuals would be endogenously self-selecting out of the policy. Conversely, gender-progressive families or individuals might delay marriages to become eligible for increased inheritance in anticipation of the policy. If this were the case, it would result in individuals self-selecting into the treatment group. Either of these self-sections could

[33]Such changes in marriage market equilibrium is not only restricted to result from changes in women's education. This could also happen if the HSAA impacted factors such as dowries and inheritance which determine matches in the marriage market.

[34]The aggregate weighted average ATT does show statistical significance at the 90% significance level in spite of statistically insignificant group-wise effects. However, upon observing the event study graphs in Figure B4 we find that this is driven by significant estimates from the households treated at least 14 years (or 7 periods) after the policy in the states of Tamil Nadu and Andhra Pradesh. Additionally, the estimates of the last two periods (16 years after the policy) have confidence intervals twice as large as earlier periods resulting from small sample size only coming from the earliest adopting state of Andhra Pradesh. These are relatively longer run impacts when compared to the effects on toilet adoption that we document in the states of Maharashtra and Karnataka till 10 years (5 periods) after HSAA adoption. Hence, although we cannot reject such long run impacts on the marriage market, the statistical significance of the aggregate estimate should be interpreted with caution and our results show that these are not substantial enough to increase toilet adoption.

compromise clean comparisons in the event-study design.

Such patterns of self-selection would be visible in the data by examining the distribution of year of marriage and age at marriage. We plot the distribution of marriages relative to the HSAA adoption year in each of the treated states in Figures 6 and find no evidence of systemic jumps in marriages around the time of HSAA adoption. This suggests that substantial self-selection into or out of the policy is unlikely.

7.5 Post marital change in religion

We do not have data on females who changed their religion post-marriage. Failing to account for this could result in biased estimates, as religion is one of the criteria determining whether a woman benefited under the HSAA. However, this is not a significant concern, as inter-religious marriages are rare in India. Das et al. (2011) provides evidence that only about 2.1% of marriages in India are inter-religious, citing social stigma as a major hindrance. Roy (2015), in her analysis of the effect of the HSA on female education, finds that only 3% of marriages are inter-religious. Additionally, inter-caste marriages within a religion are also uncommon. For example, Banerjee et al. (2013) show strong preferences for marrying within the same caste, with individuals willing to trade off qualities like having a master's degree for caste compatibility. Therefore, the inability to observe these rare choices is unlikely to affect our results.

7.6 The Total Sanitation Campaign

In 1999, the Government of India introduced a nationwide campaign to improve sanitation practices called the Total Sanitation Campaign (TSC). The TSC focused on increasing awareness about sanitation. However, some studies show that on average it was not very successful in encouraging households to construct toilets (Hueso & Bell 2013, WSP 2011).[35] For the purpose of our identification, we assume that any impact of a national-level policy like the TSC, if any, led to the evolution of toilet adoption in parallel between treated and untreated states across marital cohorts. Support for this assumption is found in Augsburg, Baquero, Gautam & Rodriguez-Lesmes (2023), who show that any variation in TSC implementation across states had seen parallel evolution of toilet ownership until 2004 (see Fig. 5 and Section 3.1.1 in their paper). This covers all the cohorts in our analysis who were married after the TSC was implemented in 1999 until 2004, as we exclude any individuals married starting in 2005 when the HSAA was ratified nationally.

[35]Due to the lack of success of the TSC, it was later replaced by the *Nirmal Bharat Abhiyaan* policy in 2009, which provided monetary subsidies for toilet construction to households below the poverty line.

7.7 The costs of open defecation and the benefits of toilets

The economic and health costs of open defecation are profoundly high, making toilet access highly beneficial. Open defecation is linked to severe health issues, including diarrhea, cholera, typhoid, and intestinal worms, particularly affecting children. Economically, the costs stem from premature deaths, healthcare expenses, and lost productivity. A 2017 UNICEF report on sanitation and the *Swacch Bharat Mission* estimate that open defecation costs India 7.9% of its GDP, up from the 2014 World Bank estimate of 6.4%. The report concludes that achieving 100% toilet coverage could save up to 100,000 lives annually and reduce medical costs by approximately INR 17,622 per household ($872 in 2017 PPP), yielding national savings of INR 8.1 trillion (around $126 billion) from improved sanitation and productivity.[36] Geruso & Spears (2018) find that a reduction in open defecation by 10 percentage points is associated with a decrease in infant mortality by 6 per 1000 live births.

Though there are no studies estimating the cost of HSAA implementation, it is likely centered on administrative and legal processes related to property rights, not sanitation, which would be costly.[37] In 2004-05, the average toilet coverage in our sample was 36%. A 4.7pp increase in toilet coverage due to the unintended benefits of the HSAA corresponds to a 13.4% increase in toilet coverage. Using a simple back of the envelope calculation if we scale the above numbers given our estimates, the unintended benefits of the HSAA increasing toilet coverage could have reduced healthcare costs by approximately INR 10,655 per household yeilding a national saving of INR 4.76 trillion ($74 billion). Indeed these numbers are not directly comparable as these estimates are based on different baselines and different assumptions. However, they serve as conservative estimates because it is plausible to assume that benefits of increased toilet coverage are not only non-linear but also larger gains are expected at lower levels of toilet coverage.

This discussion does not account for the benefits of toilets in reducing sexual harassment against women. Increased access to toilets has been shown to lower the risk of non-partner sexual violence against women (Hossain et al. 2022). Thus, the unintended benefits of a female empowerment policy like the HSAA, through increased toilet coverage, extend beyond direct health and economic gains, enhancing women's

[36] Note that these estimates are based on a projected increase in toilet coverage from a 2017 baseline of 85%, corresponding to a 15 percentage point to achieve universal coverage, or 17.6%, increase.

[37] For context, India's investment in the *Swacch Bharat Mission* campaign to directly improve sanitation was considerable. The government allocated around INR 1.34 trillion (approximately $20 billion) between 2014 and 2019 to achieve its goals of eliminating open defecation and improving sanitation infrastructure across the country.

safety.

8 Conclusion

Open defecation is a significant public health crisis in low- and middle-income countries, with India accounting for a large share. Despite the barriers to toilet adoption—rooted in cultural norms, misperceptions and economic constraints—women suffer disproportionately from the lack of sanitation facilities. In this paper, we present evidence of an unintended significant impact of the the HSAA—a women-empowerment policy aimed at improving women's inheritance rights—on toilet adoption.

Using a heterogeneity-robust event-study design, we show that the HSAA led to an increase in toilet ownership, by at least 3-4 percentage points translating to a 9.6-11.2% increase in toilet coverage relative to marital cohorts that were not exposed to the HSAA. Our results indicate that increased education was the primary mechanism in driving this effect. Increased education plusibly mitigated documented misperceptions about sanitation, raising awareness and challenging cultural norms around open defecation. While decision-making power also played a role, it was only effective in conjunction with improved education. This finding aligns with Augsburg, Malde, Olorenshaw & Wahhaj (2023), who highlight that misperceptions hinder sanitation investment and that women's decision-making power becomes impactful only when these misperceptions are addressed. Using a heterogeneity-robust difference-in-differences estimator, we find that the impact of HSAA on toilet adoption being concentrated in the states of Maharashtra and Karnataka where we find the HSAA to have increased women's education and their intra-household decision-making power. The other treated states–Andhra Pradesh and Tamil Nadu experienceed no significant effects. This is likely due to systemic differences: younger marriage ages in Andhra Pradesh limited opportunities for women to attain the higher education required to reduce sanitation and toilet based misperceptions, while Tamil Nadu's large proportion of socio-economically disadvantaged caste groups, who have historically faced substantial barriers in benefiting from non-targeted policies, likely reduced the HSAA's impact.

From a policy perspective, our paper documents that women-empowerment policies such as the HSAA provides valuable insights through their unintended benefits. Sanitation-focused initiatives, like the Clean India Mission (*Swacch Bharat Mission*), are expensive and require addressing both financial and informational barriers. Overall, our results on the HSAA's positive impact on toilet adoption highlights how policies

empowering women can lead to broader household welfare improvements, beyond their intended scope.

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Tables and Figures

Table 1: Summary statistics by treatment and comparison groups

Variable	**Treatment Group 1** *HSAA in 1986*	**Treatment Group 2** *HSAA in 1989*	**Treatment Group 3** *HSAA in 1994*	**Never treated** *Group*
Age at marriage	17.008	19.329	18.468	18.553
	(3.783)	(3.594)	(3.741)	(3.757)
Urban	0.559	0.501	0.527	0.406
	(0.497)	(0.500)	(0.499)	(0.491)
Caste:				
Schedule caste	0.162	0.269	0.183	0.218
	(0.368)	(0.444)	(0.386)	(0.413)
Schedule tribe	0.063	0.009	0.089	0.096
	(0.243)	(0.095)	(0.285)	(0.295)
Other backward class	0.515	0.695	0.398	0.304
	(0.500)	(0.460)	(0.489)	(0.460)
General caste	0.259	0.025	0.307	0.375
	(0.438)	(0.157)	(0.461)	(0.484)
Wealth Index Quintile:				
Wealth index (Q-1)	0.073	0.088	0.089	0.165
	(0.261)	(0.283)	(0.285)	(0.371)
Wealth index (Q-2)	0.126	0.137	0.169	0.162
	(0.332)	(0.344)	(0.375)	(0.369)
Wealth index (Q-3)	0.229	0.282	0.189	0.178
	(0.420)	(0.450)	(0.392)	(0.383)
Wealth index (Q-4)	0.277	0.273	0.230	0.206
	(0.447)	(0.445)	(0.421)	(0.405)
Wealth index (Q-5)	0.295	0.220	0.323	0.288
	(0.456)	(0.414)	(0.468)	(0.453)
N	3627	3508	7920	40778

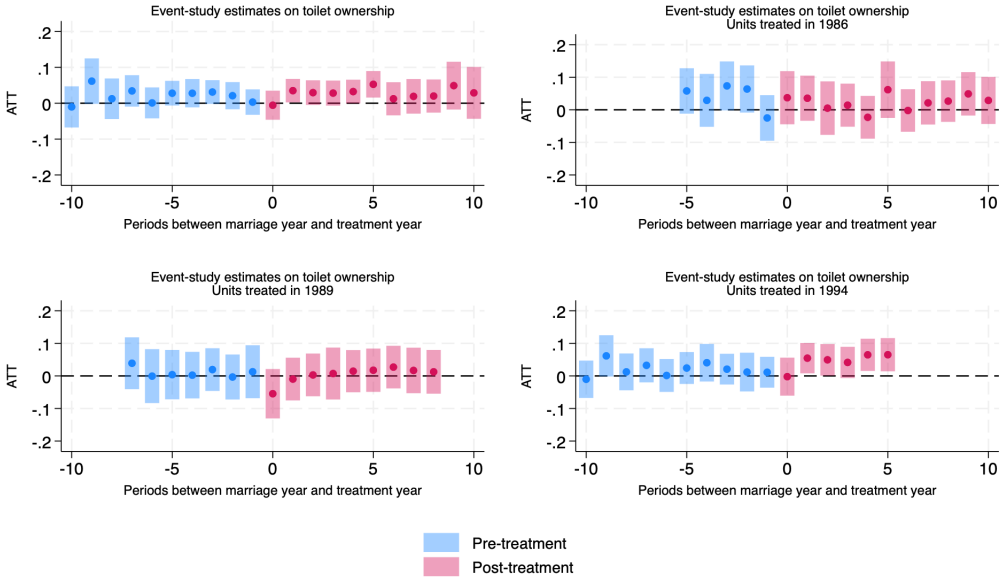
Notes: This table reports the summary statistics for the key variables by treated and never-tread groups, starting with Andhra Pradesh (HSAA in 1986), Tamil Nadu (HSAA in 1989), Maharashtra and Tamil Nadu (HSAA in 1994) and the never treated group respectively. Thus the respective marital cohorts are 1992 and 1993, 1987 and 1988, and 1984 and 1985. The data used come from the third wave of the National Family and Health Survey (2005). Households with marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Table 2: Impact of HSAA on toilet ownership

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.0318**	0.0319**
	(0.0131)	(0.0132)
ATT of units treated in 1986	0.0222	0.0226
	(0.0252)	(0.0248)
ATT of units treated in 1989	0.00538	0.00554
	(0.0254)	(0.0250)
ATT of units treated in 1994	0.0475**	0.0475**
	(0.0188)	(0.0188)
Pre-trend test (χ^2)	21.32	20.48
p-value	0.50	0.55

Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on household toilet ownership. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding *p*-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Figure 1: Event study estimates estimates on toilet ownership



Notes: The effects of the HSAA on household toilet ownership estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Table 3: Impact of HSAA on women's years of education

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.324**	0.316**
	(0.130)	(0.131)
ATT of units treated in 1986	0.212	0.187
	(0.251)	(0.248)
ATT of units treated in 1989	0.126	0.120
	(0.248)	(0.246)
ATT of units treated in 1994	0.458**	0.458**
	(0.188)	(0.188)
Pre-trend test (χ^2)	20.86	21.11
p-value	0.53	0.51

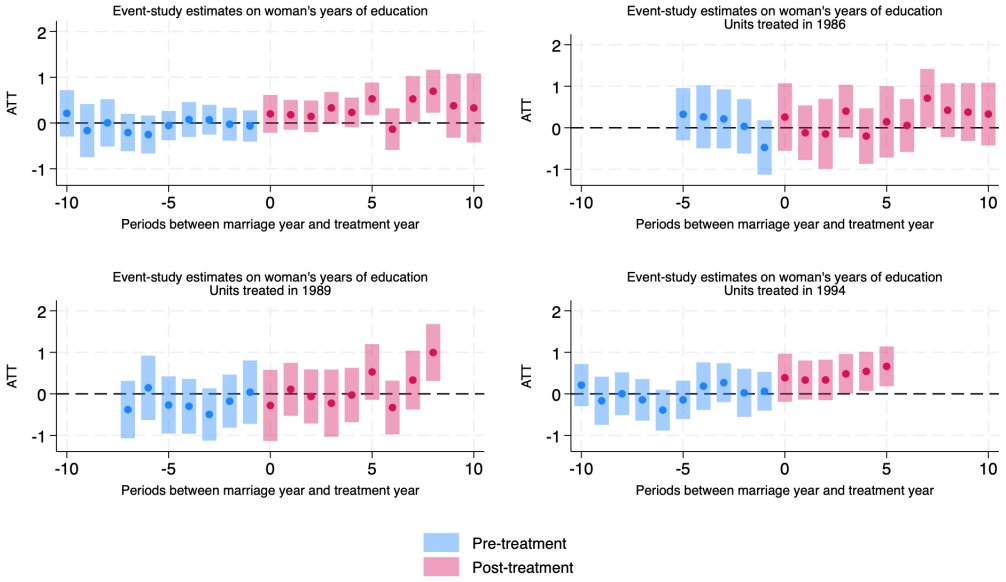
Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on years of education. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Table 4: Impact of HSAA on women's intra-household decision-making power

	(1)	(2)
	Never treated	Not yet treated
Aggregate ATT	0.145***	0.142***
	(0.0349)	(0.0351)
ATT of units treated in 1986	0.0839	0.0777
	(0.0752)	(0.0746)
ATT of units treated in 1989	0.282***	0.278***
	(0.0655)	(0.0647)
ATT of units treated in 1994	0.112**	0.112**
	(0.0485)	(0.0485)
Pre-trend test (χ^2)	15.70	17.59
p-value	0.83	0.73

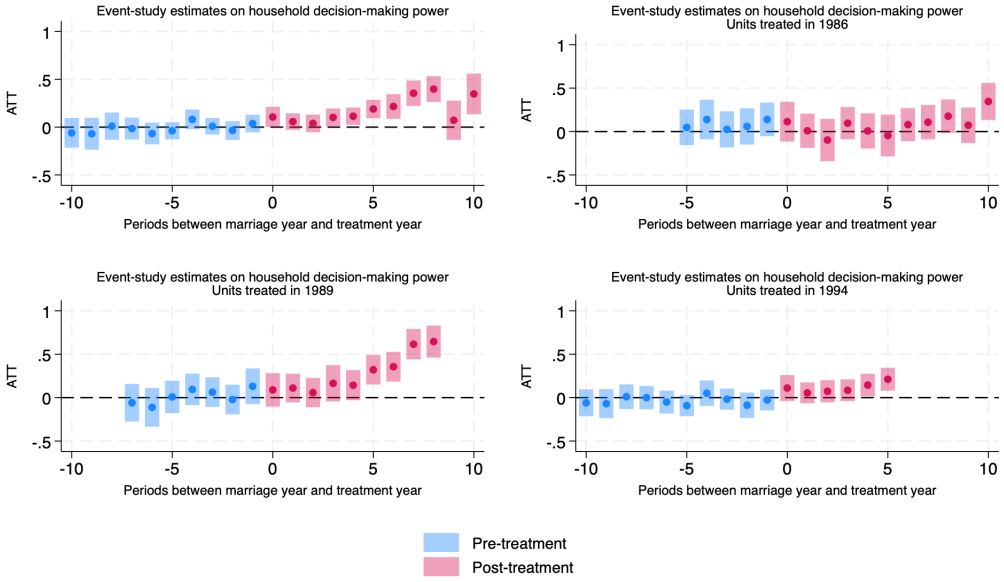
Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on women's intra-household decision-making power. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Figure 2: Event study estimates on years of Education



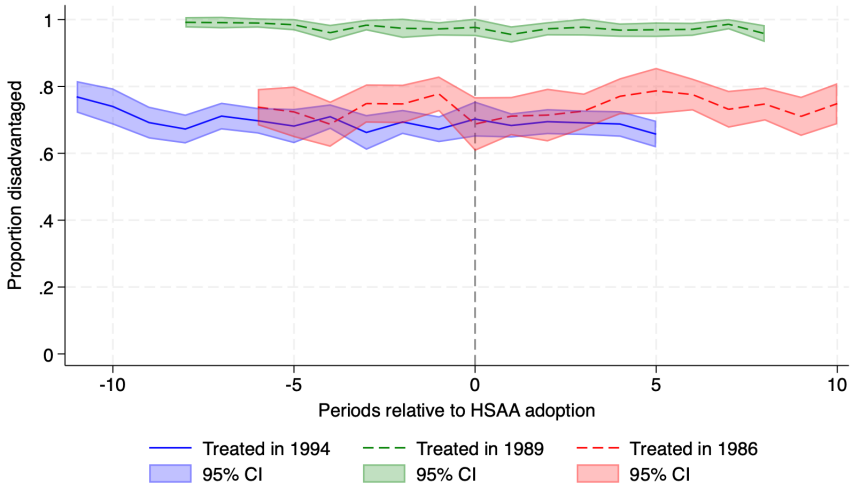
Notes: The effects of the HSAA on years of education estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure 3: Event study estimates on decision-making power



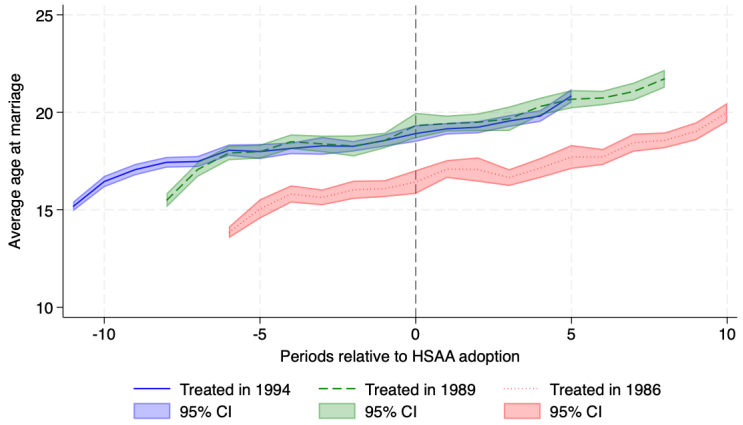
Notes: The effects of the HSAA on women's intra-household decision-making power estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure 4: Proportion of Disadvantaged Caste Groups



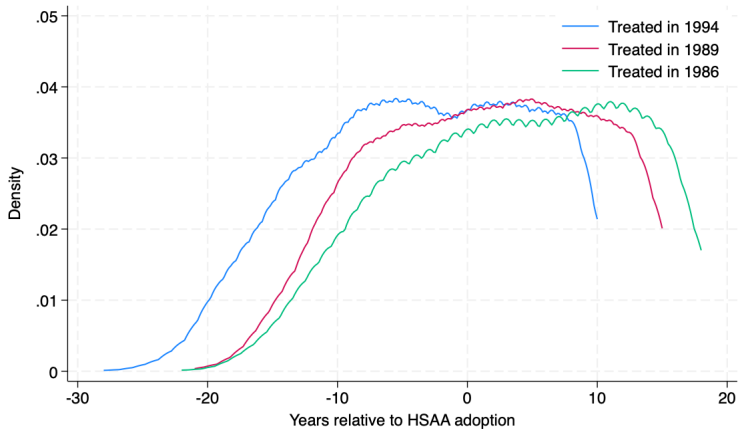
Notes: The figure plots the proportion of disadvantaged caste groups (defined as belonging to either schedule caste, or schedule tribe or OBC caste groups) across marital cohorts by states that adopted the HSAA in different years. The x-axis represents the number of periods relative to the year of policy implementation, and each period pools pairwise marital cohorts to increase precision.

Figure 5: Average Age at Marriage Over Time



Notes: The figure plots the average age at marriage of females over the years by states that adopted the HSAA in different years. The x-axis represents the number of periods relative to the year of policy implementation, and each period pools pairwise marital cohorts to increase precision.

Figure 6: Distribution of marriages over time



Notes: This figure plots the distribution of the marriages by the states that adopted the HSAA in different years. The x-axis represents the number of years relative to the year of policy implementation.

A Appendix

A.1 Appendix Tables

Table A1: Impact of HSAA on toilet ownership (Rural sample)

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.0288*	0.0286*
	(0.0152)	(0.0152)
ATT of units treated in 1986	0.0364	0.0339
	(0.0321)	(0.0317)
ATT of units treated in 1989	0.00171	0.00304
	(0.0287)	(0.0283)
ATT of units treated in 1994	0.0388*	0.0388*
	(0.0214)	(0.0214)

Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on household toilet ownership in rural areas. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Table A2: Impact of HSAA on years of educational attainment (Rural sample)

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.599***	0.598***
	(0.164)	(0.165)
ATT of units treated in 1986	0.127	0.0974
	(0.256)	(0.254)
ATT of units treated in 1989	0.430	0.455
	(0.296)	(0.294)
ATT of units treated in 1994	0.875***	0.875***
	(0.254)	(0.254)

Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on women's years of education in rural areas. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Table A3: Impact of HSAA on intra-household decision-making power (Rural sample)

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.200***	0.200***
	(0.0532)	(0.0535)
ATT of units treated in 1986	0.145	0.142
	(0.116)	(0.116)
ATT of units treated in 1989	0.363***	0.366***
	(0.0938)	(0.0928)
ATT of units treated in 1994	0.147*	0.147*
	(0.0757)	(0.0757)

Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on women's intra-household decision making power in rural areas. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

Table A4: Impact of HSAA on Husband's years of education

	(1) Never treated	(2) Not yet treated
Aggregate ATT	0.330**	0.323**
	(0.145)	(0.146)
ATT of units treated in 1986	0.449	0.448
	(0.306)	(0.303)
ATT of units treated in 1989	0.339	0.312
	(0.277)	(0.274)
ATT of units treated in 1994	0.275	0.275
	(0.203)	(0.203)
Pre-trend test (χ^2)	19.00	18.45
p-value	0.65	0.68

Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on husband's observed years of education. These estimates are obtained under the conditional parallel trends assumption, using the doubly robust estimator described in (Callaway & Sant'Anna 2021). Standard errors are computed using wild cluster bootstrap at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). We present estimates using two different comparison groups: (1) "never-treated" (column 1), which includes households in states that did not adopt the HSAA until its national ratification in 2005, and (2) "not-yet-treated" (column 2), which includes households in states that had not adopted the HSAA by the adoption year of the treated group being analyzed. The row "Aggregate ATT" reports the weighted average (by group size) of all estimated group-time ATT effects. The subsequent rows provide group-specific ATT estimates for households treated in 1986 (Andhra Pradesh), 1989 (Tamil Nadu), and 1994 (Karnataka and Maharashtra), respectively. The last two rows show the χ^2 test statistic estimate and its corresponding p-value that tests the null hypothesis of all pre-period ATT estimates being equal to zero. We use data from the third wave of the National Family and Health Survey (2005). The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

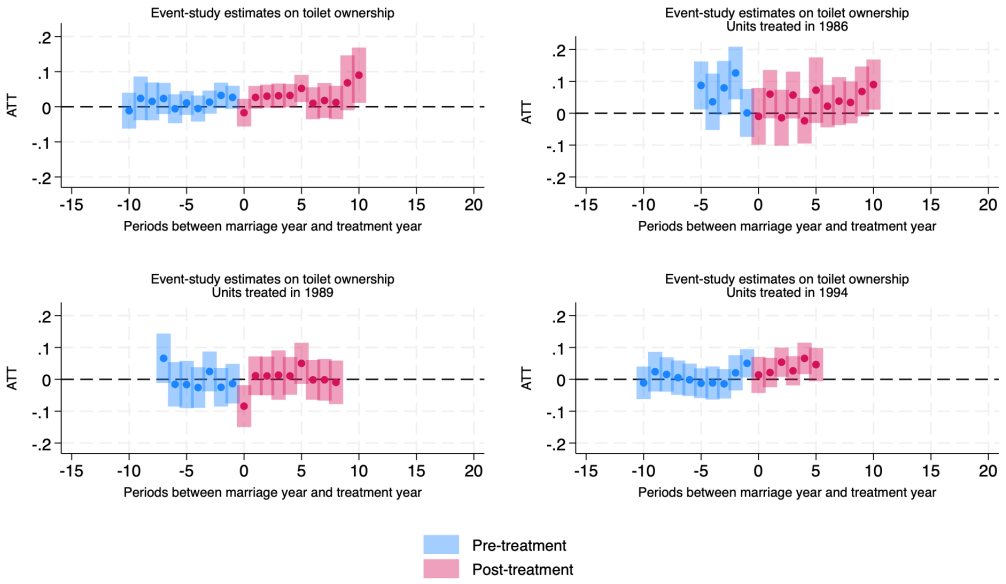
Table A5: Impact of HSAA on toilet ownership (Two-way fixed effects)

	Toilet ownership
	(1)
Treated	0.022***
	(0.009)
Observations	32,169
R^2	0.45
State FE	Yes
Year of marriage FE	Yes
Controls	Yes

Notes: The table reports estimates of the aggregated average treatment effect on the treated (ATT) parameter of the impact of the HSAA on husehold toilet ownership using a two-way fixed effects estimator. Standard errors are clustered at the state level and are reported in parentheses below each estimate (* p < 0.10, ** p < 0.05, *** p < 0.01). The data used come from the third wave of the National Family and Health Survey (2005). Households with marriages occurring after the national ratification of the HSAA in 2005 are excluded from the sample.

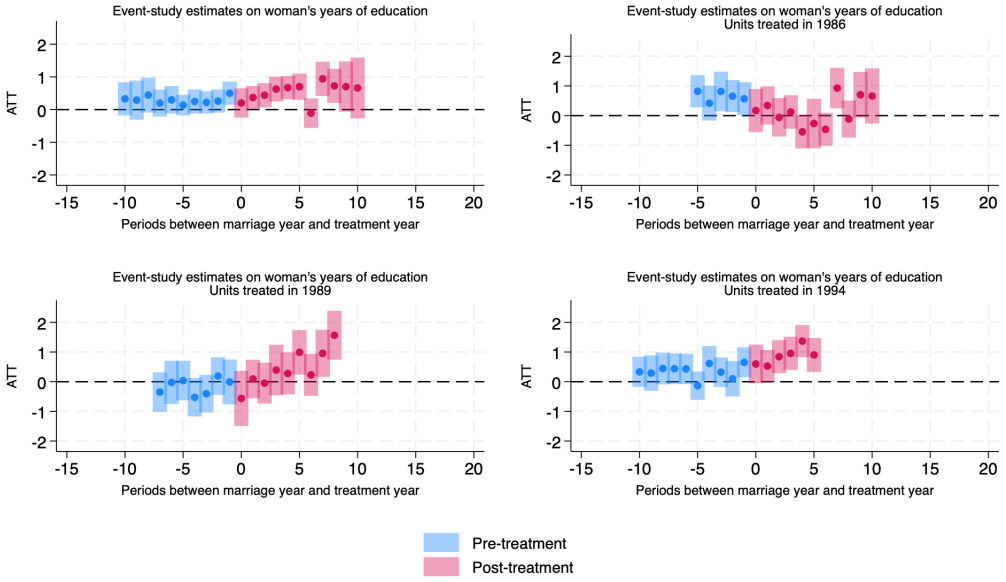
A.2 Appendix Figures

Figure B1: Event study estimates on toilet ownership (Rural sample)



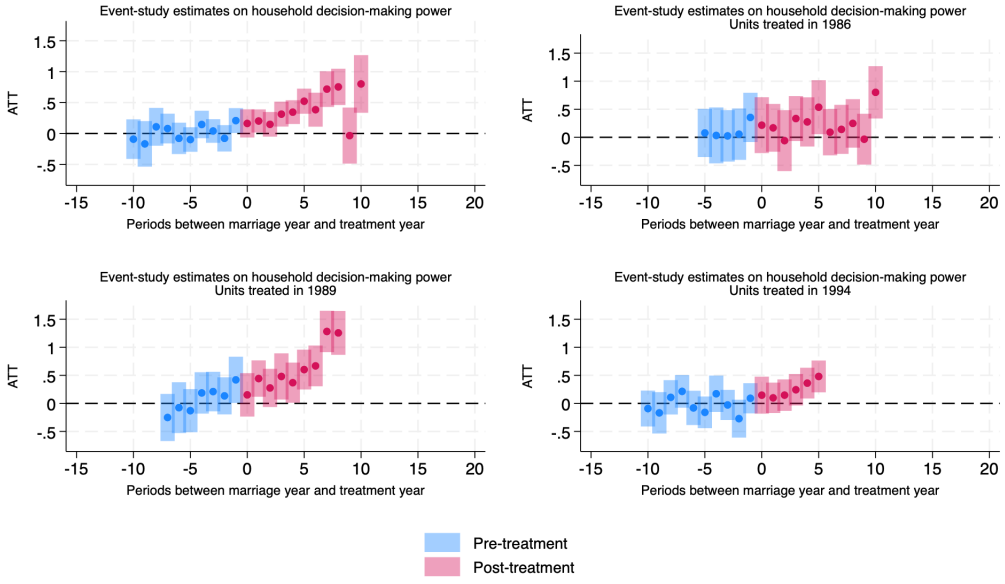
Notes: The effects of the HSAA on household toilet ownership in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure B2: Event study estimates on years of education (Rural sample)



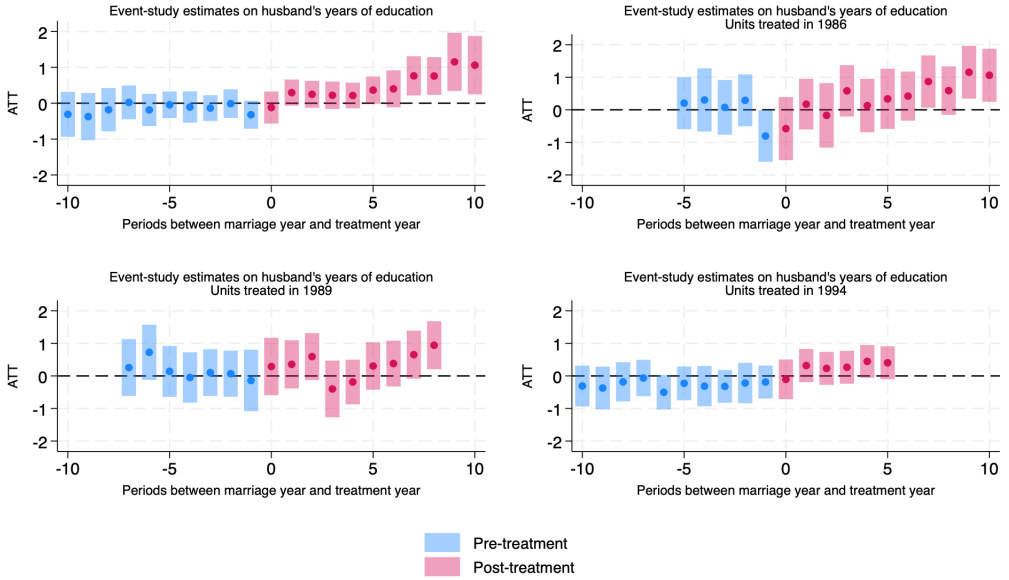
Notes: The effects of the HSAA on years of education in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure B3: Event study estimates on intra-household decision-making power (Rural sample)



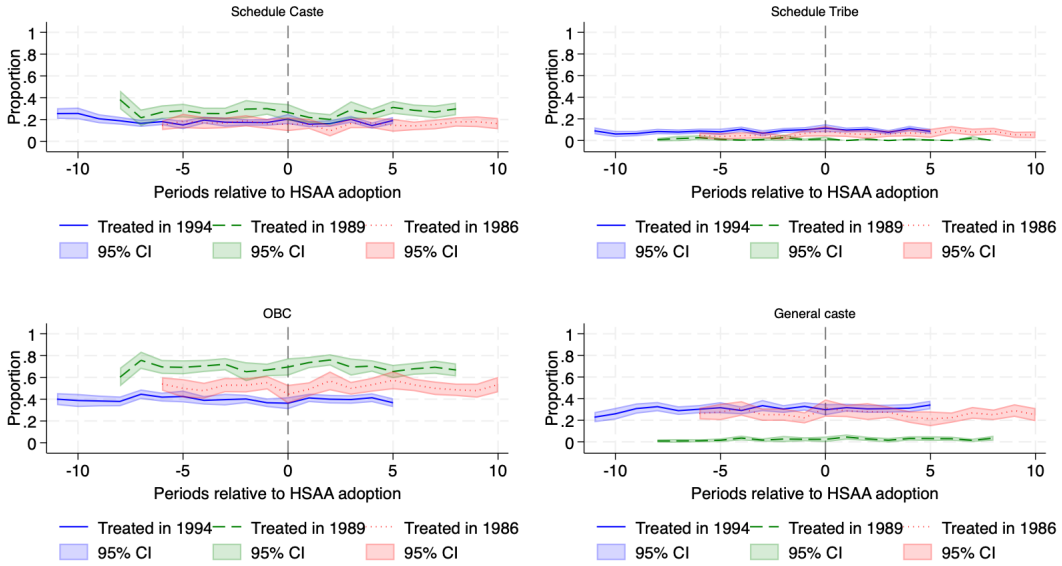
Notes: The effects of the HSAA on intra-household decision-making of women in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure B4: Event study estimates on husband's education



Notes: The effects of the HSAA on husband's education estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group (i.e., households in states that did not adopt the HSAA until it was nationally adopted in 2005) as the comparison group. The x-axis represents the number of periods relative to adoption of HSAA. Each period pools two consecutive marital cohorts as described in the text. Blue lines give point estimates and uniform 95% confidence bands for pre-treatment periods. Red lines provide point estimates and uniform 95% confidence bands for the treatment effect of the HSAA. These estimates are obtained under the conditional parallel trends assumptions using the doubly robust estimator described in (Callaway & Sant'Anna 2021) with standard errors computed using wild cluster bootstrap at the state level. We use data from the third wave of the National Family and Health Survey of 2005. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA. Non-HSA religion households, and marriages that happened after the national ratification of the HSAA are not a part of the sample.

Figure B5: Proportion of Caste Groups



Notes: The figure plots the proportion of different caste groups across marital cohorts by states that adopted the HSAA in different years. The x-axis represents the number of periods relative to the year of policy implementation, and each period pools pairwise marital cohorts to increase precision.

A.3 Identification of lower bounds on the ATT

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 criteria of treatment eligibility are observed. Under an additional assumption that b_i 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,

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

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

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

since $\mathbb{P}(b_i = 1) \in [0,1]$, we have $\mid ATT^*(g,t) \mid \leq \mid ATT(g,t) \mid$. 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

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

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

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

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

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

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

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

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, given a consistent estimator, let $\widehat{ATT(g,t)}$ be a consistent estimate 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 estimate 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 estimate 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.

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

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.

□

A.4 Model: Comparative Statics

A.4.1 Proposition 1:

Increasing women's education increases the proportion P of households building toilets by reducing the noise σ_h in perceived costs.

Proof: We consider the effect of reducing σ_h (through increased education $E_{w,h}$) on the proportion P. The derivative of P with respect to σ_h (assuming a uniform change in noise across households):

$$\frac{\partial P}{\partial \sigma_h} = \int_{h \in \mathcal{H}} \frac{\partial \Pr(T_h = 1)}{\partial \sigma_h} \, dF(h) \tag{8}$$

Now, $\frac{\partial \Pr(T_h=1)}{\partial \sigma_h} = -\phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2}$. Thus,

$$\begin{aligned}
\frac{\partial P}{\partial \sigma_h} &= -\int_{h \in \mathcal{H}} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2} \, dF(h) \\
&= -\left(\underbrace{\int_{\Delta_h > 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2} \, dF(h)}_{\equiv I_1} + \underbrace{\int_{\Delta_h \leq 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2} \, dF(h)}_{\equiv I_2} \right)
\end{aligned}$$

For households with $\Delta_h > 0$, $\frac{\Delta_h}{\sigma_h^2} > 0$. Since $\phi(.) > 0$, $I_1 > 0$. For households with $\Delta_h \leq 0$, $\frac{\Delta_h}{\sigma_h^2} < 0$. Since $\phi(.) > 0$, $I_2 \leq 0$. Assuming that the mass of households with $\Delta_h \leq 0$ is negligible, is sufficient to prove Proposition 1. This is because $I_2 \approx 0$ and the positive integral I_1 dominates.[38] This implies that,

$$\frac{\partial P}{\partial \sigma_h} \approx -\int_{\Delta_h > 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2} \, dF(h) < 0 \tag{9}$$

[38] If one does not find this to be a plausible assumption, then we need additional assumptions. In that case, to determine the sign of $\frac{\partial P}{\partial \sigma_h}$, we need to consider the relative magnitudes of the two integrals. Specifically, we need to assume that: The magnitudes of Δ_h for households with $\Delta_h > 0$ along with their mass $|| \, h : \Delta_h > 0 \, ||$ are sufficiently large compared to those with $\Delta_h \leq 0$ and their mass $|| \, h : \Delta_h \leq 0 \, ||$. Under this additional assumptions, the positive integral dominates.

$$I_1 \equiv \int_{\Delta_h > 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \frac{\Delta_h}{\sigma_h^2} \, dF(h) > |I_2| \equiv \left| \int_{\Delta_h \leq 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \frac{\Delta_h}{\sigma_h^2} \, dF(h) \right|$$

Therefore, $\frac{\partial P}{\partial \sigma_h} = -(I_1 + I_2) < 0$.

Since $\frac{\partial \sigma_h}{\partial E_{w,h}} < 0$, increasing education reduces σ_h, and thus:

$$\frac{\partial P}{\partial E_{w,h}} = \frac{\partial P}{\partial \sigma_h} \cdot \frac{\partial \sigma_h}{\partial E_{w,h}} > 0.$$

Under the assumption that households with positive net benefits dominate in the population, increasing women's education $E_{w,h}$ on average reduces noise σ_h and increases the proportion P of households building toilets, proving Proposition 1.

A.4.2 Proposition 2:

Increasing women's decision-making power has a significant positive effect on the proportion P of households building toilets only when the noise σ_h is reduced through increased education.

Proof: At the household level, the derivative of P with respect to θ_w and w.l.o.g. assuming $\theta_{w,h} = \theta_w$ for all h for simplicity s.t. $\frac{\partial \theta_{w,h}}{\partial \theta_w} = 1$, is:

$$\frac{\partial \Pr(T_h = 1)}{\partial \theta_w} = \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{1}{\sigma_h} \cdot (\beta_{w,h} - \beta_{m,h})$$

The above expression is positive because $\beta_{w,h} > \beta_{m,h}$ for all h and $\phi(.) > 0$ and $\sigma_h > 0$. Thus integrating over all households, we will have a positive effect of increasing θ_w on P.

$$\begin{aligned}
\frac{\partial P}{\partial \theta_w} &= \int_{h \in \mathcal{H}} \frac{\partial \Pr(T_h = 1)}{\partial \theta_{w,h}} \cdot \frac{\partial \theta_{w,h}}{\partial \theta_w} \, dF(h) \\
&\approx (\beta_{w,h} - \beta_{m,h}) \int_{\Delta_h > 0} \frac{1}{\sigma_h} \cdot \phi\left(\frac{\Delta_h}{\sigma_h}\right) dF(h) \\
&> 0
\end{aligned}$$

While the sign of $\frac{\partial P}{\partial \theta_w}$ is positive, because $\beta_{w,h} > \beta_{m,h}$, the effect of increasing θ_w on P is substantial only when σ_h is low due to increased education.

To see this, first note that for all values of $\{\Delta_h, \sigma_h\}$, $\phi\left(\frac{\Delta_h}{\sigma_h}\right)$ is bounded above by 1 and below by 0. Fixing Δ_h, observe that as $\sigma_h \to 0$, $\frac{\partial P}{\partial \theta_w} \to \infty$. On the other hand, as $\sigma_h \to \infty$, $\frac{\partial P}{\partial \theta_w} \to 0+$.

Since the effect is significant only when σ_h is low, and σ_h decreases with increased education, we conclude that when σ_h is low due to increased education, $\frac{\partial P}{\partial \theta_w}$ is significantly positive. Thus, increasing women's decision-making power across households significantly increases the proportion P of households building toilets, only when the noise σ_h is reduced through increased education, proving Proposition 2.

A.4.3 Proposition 3:

Simultaneously increasing women's education and decision-making power has a combined positive effect on the proportion P of households building toilets, due to the positive interaction between education and empowerment.

Proof: The cross-partial derivative of P with respect to θ_w and σ_h, assuming that the mass of households with $\Delta_h \leq 0$ is negligible:

$$\begin{aligned}
\frac{\partial^2 P}{\partial \theta_w \partial \sigma_h} &= \int_{h \in \mathcal{H}} \frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \partial \sigma_h} \, dF(h) \\
&\approx \int_{\Delta_h > 0} \frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \partial \sigma_h} \, dF(h) \\
&= -\int_{\Delta_h > 0} (\beta_{w,h} - \beta_{m,h}) \cdot \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \left(\frac{\Delta_h}{\sigma_h^3} + \frac{1}{\sigma_h^2}\right) dF(h)
\end{aligned}$$

The expression inside the parentheses $\left(\frac{\Delta_h}{\sigma_h^3} + \frac{1}{\sigma_h^2}\right) > 0$ for $\Delta_h > 0$. This along with $(\beta_{w,h} - \beta_{m,h}) > 0$ implies that $\frac{\partial^2 \Pr(T_h=1)}{\partial \theta_w \partial \sigma_h} < 0$.

Since $\frac{\partial \sigma_h}{\partial E_{w,h}} < 0$, we have:

$$\frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \partial E_{w,h}} = \frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \partial \sigma_h} \cdot \frac{\partial \sigma_h}{\partial E_{w,h}} > 0$$

Integrating over all households:

$$\frac{\partial^2 P}{\partial \theta_w \partial E_{w,h}} = \int_{h \in \mathcal{H}} \frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \partial E_{w,h}} \, dF(h) > 0.$$

Simultaneously increasing women's education and decision-making power leads to a combined positive effect on the proportion P of households building toilets, due to the positive interaction between reduced noise and increased empowerment, proving

Proposition 3. We should also note that if the variance of the noise is very large and we only have modest increases in education, this combined effect may not be substantial.