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

Monica Agarwal†
Northwestern University

Md Moshi Ul Alam‡
Clark University

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Abstract

Existing research shows that women benefit more from private toilets, but misperceptions about 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 show 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 by at least 10%. Prior literature shows mixed evidence on whether the policy increased women's inheritance, but shows that the policy had significant indirect effects, such as improving women's education. To generate empirical tests for the mechanisms driving our main results, we build a discrete choice model with idiosyncratic household preference shocks that produces policy-relevant complementarity between women's education and decision-making power in adoption of a household public good valued more by women. Using a heterogeneity-robust event-study design, we find that, consistent with our model, the increase in toilet adoption is concentrated in states where the policy boosted women's education—plausibly reducing misperceptions about the benefits of toilets—and increased women's decision-making power. Our findings highlight that policies empowering women can yield unintended benefits beyond their original scope—while we document improvements in toilet coverage, the implications extend to other household investments where women's preferences are stronger, but various frictions limit adoption.

JEL Codes: I15, I18, J16, O15, O18
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†Corresponding author. Email: monica.agarwal@northwestern.edu

‡Email: econmoshi@gmail.com

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 toilet adoption 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 to the adoption of toilets. First, lack of education and health-based awareness about the importance of sanitation is an important factor behind the low adoption of 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 adoption of toilets, a household public good that females value disproportionately more than males (Khanna & Das 2016, Augsburg, Malde, Olorenshaw & Wahhaj 2023, Chaturvedi et al. 2024)?

We study this question by exploiting variation in the legal amendments to inheritance rights in India designed to empower women. 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, in the four out of five states barring Kerala, that passed the HSA-Amendments (henceforth, **HSAA**) between 1986 and 1994, namely Andhra Pradesh, Tamil Nadu, Karnataka, and Maharashtra, the

[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).

HSAA only applied to those females who were unmarried at the time of the passing of the amendment, thus creating identifying variation in treatment status of individuals within these treated states.

We leverage this within-state variation in exposure to the HSAA across marital cohorts of women along with its 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). Beyond being robust to heterogeneous treatment effects, the estimator weighs the outcome model with propensity scores conditional on observables. This accounts for selection into treatment based on potential outcomes and captures differences in counterfactual outcome patterns between treatment and control states, conditional on observables. This alleviates concerns about observable differences between treated and untreated states confounding effects. Additionally, the estimator has a doubly-robust property (Sant'Anna & Zhao 2020) which requires weaker assumptions on potential mis-specification bias.[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 which had an average toilet coverage of 36%. Estimates of the group-wise heterogeneous treatment effects show that this effect was driven by cohorts in the states of Maharashtra and Karnataka who were on average 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 the impacts in these states 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. Our pre-period event study estimates along with pre-trend tests provide no statistical evidence to suggest that the pre-treatment differences were statistically or economically different from zero, strengthening our identification assumption of conditional parallel trends. We find neither statistically significant nor economically meaningful impacts in the states of Andhra Pradesh and Tamil Nadu—reasons for which, along with suggestive evidence, are discussed later.[3] Next, we build a static discrete choice

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

[3]We find similar results when we restrict our sample to rural India.

model of the household to provide a theoretical framework to generate empirical tests of mechanisms, and link them to the reasons behind the treatment effect heterogeneity of our estimated effects across different states.

In our model, households make a discrete choice about adopting a household public good (toilets), where the utility from adoption is subject to preference shocks, following a random utility maximization framework (McFadden 1973). These preference shocks capture unobserved idiosyncratic factors, such as misconceptions about health impacts, adoption costs, or cultural taboos, all of which deviate households' valuation of goods from observable characteristics alone. We allow for household members to have different preferences over the public good, with women valuing it more than men. By assuming that increase in education reduces the variance of the preference shock distribution as motivated by a long-standing literature,[4] our model generates a natural complementarity between education and decision-making power. Specifically reducing the variance of preference shocks through education makes household choices more responsive to changes in decision-making power. Consequently, our model implies that policies which either directly or indirectly reduce the variance of preference shocks through education gains can be more effective at increasing the likelihood of adoption than policies that only target decision-making power, since high variance in preference shocks can swamp the effect of any changes in decision-making power.[5] We also allow for reduction in preference shock variance through increased education of both spouses in the household, remaining agnostic about the relative magnitude of these effects. This flexibility is important in our context since the policy could induce marriage market equilibrium changes, where empowered women may 'marry up' by partnering with more educated spouses. These marriage market effects provide additional mechanisms for empirical investigation, beyond the primary mechanism of education and the secondary mechanism of decision-making power. We end this section with a discussion on our modelling choices, their implications, potential extensions and which among them can be tested with the variation in our data.

Consistent with the predictions of our model, using a heterogeneity-robust event-study design, we find that the increase in toilet adoption is concentrated in the states where the HSAA significantly boosted women's education and enhanced their decision-making power. Specifically, we exploit variation across marital cohorts and between each group of treated states relative to untreated states to estimate heterogeneous and dynamic treatment effects under staggered policy adoption following Callaway &

[4]The role of education in reducing preference uncertainty dates back to the 1970s. Schultz (1975) documents that education enhances individuals' ability to optimally respond to new events and disequilibria. Grossman (2006) shows that education improves information processing and decision-making capabilities through "knowledge-capital".

[5]Further our model can be extended to allow for uncertainty in both costs and benefits. However, in that model, since education plausibly affects both types of variance parameters, our simpler specification focusing on preference shocks is sufficient to capture the key mechanisms while maintaining tractability.

Sant'Anna (2021). Since we find positive impacts of the HSAA on decision-making power in other treated states, our empirical results—consistent with our model—emphasize education as the primary driver behind the HSAA's unintended success in increasing toilet adoption. Support for our mechanisms and their order of importance is also 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. Our results do not fully rule out changes in marriage market equilibrium (e.g., spousal education) as additional mechanisms. We end this section with a discussion on other plausible mechanisms, the flexibility of our model to allow for them with simple extensions, and their relation to the HSAA policy.

In alignment with our model, we discuss the underlying heterogeneity in treatment effects, particularly the absence of policy impacts in the 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 which could hinder educational attainment—the primary mechanism of our model. In Andhra Pradesh, women tend to marry younger, which reduces their chances of attaining higher education, since educational attainment typically terminates at marriage in this context. In Tamil Nadu, high proportion of socio-economically disadvantaged caste groups (above 95% in the sample) that have historically faced substantial social and economic barriers, especially in accessing education, are less likely to benefit from policies unless specifically targeted (for example the Right to Education Act of 2009).[6] Both factors likely contributed to the limited effectiveness of the HSAA in these states in increasing toilet adoption.

Our full set of results are robust to various potential concerns. The Total Sanitation Campaign (TSC) implemented in 1999—a government led program aimed at eliminating open defecation and improving toilet coverage—could potentially confound our results, especially since our treated states ranked high in TSC implementation intensity. However, restricting the comparison group to states with comparable TSC performance,[7] our main results and mechanisms remain consistent at the 90% confi-

[6] Consistent with our model's prediction of complementarity, decision-making power alone was insufficient for toilet adoption because baseline education levels in Tamil Nadu were low. Since the policy did not result in education gains, misperceptions regarding sanitation potentially remained high.

[7] We thank an anonymous referee for this suggestion.

dence level despite over-burdening the estimator by reducing statistical power. Another potential concern is whether our findings hold in rural households, which face additional cultural and infrastructural constraints. We find that our results are similar (and marginally stronger) in rural areas. Additionally, endogenous selection into or out of treatment through strategic marriage timing could threaten our identification, yet examination of marriage timing distributions reveal no such patterns. Finally, the low prevalence of inter-state marital migration (below 5%) and inter-religious marriages (2-3%) in India rule out concerns of misclassification. We discuss each of these in detail in the paper.

Our paper contributes to various strands of the literature. First, we directly contribute to the literature on health economics, specifically on adoption of toilets. To the best of our knowledge, our paper is the first to explore whether women-empowering policies (in our context, the HSAA) increase household toilet ownership rates, marking our primary contribution. Female empowering policies and household sanitation could seem unrelated, but we use the insight that females are disproportionately affected by the absence of toilets to examine and show that policies that empower women could in turn lead to higher adoption of private toilets. In addition to cultural norms, other documented deterrents to toilet adoption include financial constraints (Guiteras, Levinsohn & Mobarak 2015),[8] and misperceptions about their costs and benefits (Augsburg, Malde, Olorenshaw & Wahhaj 2023). Interventions like the *Swacch Bharat Mission* (Clean India Mission) in 2014, which combined financial incentives and information campaigns, successfully increased toilet adoption.[9] Recent work by Chaturvedi et al. (2024) documents that political reservations for women with a major push towards sanitation campaigns have been effective in increasing toilet provision in areas where the gender gap in preferences for toilets is large (in the state of Uttar Pradesh, India), while Stopnitzky (2017) shows that gender-specific campaigns like "No Toilet, No Bride" in Haryana, India significantly increased toilet ownership in households having men of marriageable age. This literature has primarily focused on direct factors driving adoption of toilets and their deterrents in the context of sanitation-focused policies and interventions. We differ from the existing literature by being the first to study a potentially unintended benefit of a large-scale female-empowering policy on sanitation. Additionally, we show both theoretically and empirically the complementarity between women's education and their decision-making power that is policy-relevant to improve toilet adoption. Our findings are particularly relevant given the high costs of sanitation-focused policies (e.g., the *Swacch Bharat Mission* cost approximately $20 billion).

[8]The average cost of building a toilet for rural households in India can be as high as 50% of their annual income (Augsburg, Malde, Olorenshaw & Wahhaj 2023).

[9]The predecessor to the *Swacch Bharat Mission*, namely the Total Sanitation Campaign (TSC) lacked such features and was less effective in increasing toilet ownership (Hueso & Bell 2013).

Second, our paper contributes to the literature on household decision-making models. Building on the evidence of non-unitary household frameworks (Chiappori & Donni 2009), we develop a static discrete choice model that introduces preference shocks (McFadden 1973) in household utility functions and gender-specific preferences for a household public good.[10] Our framework where education reduces the dispersion of the preference shocks introduces a new complementarity between education and decision-making power of household members. Standard collective models that assume perfect information about the benefits of public goods will predict that decision-making power alone will govern adoption of the household public good, but our model shows its effectiveness is constrained by the dispersion of the preference shocks making education and decision-making power theoretical complements. Our contribution is particularly relevant in developing country contexts where information frictions are substantial (Conley & Udry 2010, Foster & Rosenzweig 2010, Augsburg, Malde, Olorenshaw & Wahhaj 2023). Our model demonstrates that reduction in the dispersion of shocks through education can be more effective in increasing the adoption of household public goods (such as toilets), than increasing women's decision-making power alone. The theoretical insights from our model extend beyond toilet adoption to other household public goods in developing countries where women's preferences are stronger but information frictions exist. Our framework suggests that women's empowerment policies are most effective when they enhance both education and decision-making power, as the impact of increased decision-making power depends crucially on reduced dispersion of preference shocks.

Third, our paper contributes to the empirical literature on how the identity of a policy beneficiary within the household affects household outcomes. For example, Thomas (1990) find that transfers to mothers relative to fathers are more effective in improving children health outcomes. Similarly, Lundberg et al. (1997) show that a policy change in the UK that transferred resources from fathers to mothers led to increased expenditure on children's and women's clothing relative to men's clothing. Duflo (2003) find increases in nutritional status of young girls when pensions are received by women, and found no effect when pensions are received by men. Qian (2008) finds that increasing female income improves children's education and girls' survival rates, while increasing male income has either negative or no effects on these outcomes. Conditional cash transfers to mothers relative to fathers increase expenditure shares on food (Armand et al. 2020). These suggest that efficiency of public transfer programs may crucially depend on the gender of the recipient. While most of this literature examines gender-specific targeting of transfer programs after household formation, our empirical setting provides a unique context where women's empowerment through inher-

[10] Unlike other collective models that focus on sharing rules for private resources, we abstract away from these considerations to focus on pure public goods. We also present an isomorphic model with cost shocks in Appendix D.

itance rights occurs in their natal household, demonstrating how pre-marital women empowerment can generate (unintended) benefits in post-marital households.

Finally, we make a two-fold contribution to the literature that studies various impacts of the HSAA. First, we document heterogeneous treatment effects of HSAA, specifically on education and decision-making power, which were main outcomes in prior studies assuming treatment effect homogeneity,[11] but serve as mechanisms in our paper. Our analysis reveals that the HSAA significantly boosted education and increased decision-making power for treated females in some states. Second, we address a typical data caveat in the literature estimating the effects of the HSAA. An obstacle in estimating the treatment effects of the HSAA is that the treatment group is not perfectly observed in most publicly available datasets. One of the eligibility criteria under the HSAA required that the natal or birth household property of the female must have remained undivided at the time the HSAA was adopted in her state.[12] To the best of our knowledge, survey data on the timing of property division in India does not exist.[13] Hence, most studies in this literature have ignored this data caveat.[14] 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 under plausible assumptions. The intuition of 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, the HSAA and also describes the data. Section 3 outlines the empirical strategy and Section 4 presents the main results. In Section 5, we introduce a static discrete model of household decision-making to guide the em-

[11] 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). An exception is Deininger et al. (2013), who find that the HSAA improved female inheritance. All the studies however consistently find evidence 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). Other findings show that the HSAA 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 2020) and improved nutrition and health outcomes for beneficiaries' children (Ajefu et al. 2022), but had no impact on children's education levels (Bose & Das 2021). Unintended negative impacts have also been documented, such as increased sex-selective abortion in son-preference areas (Rosenblum 2015, Bhalotra et al. 2020), and higher suicide rates (Anderson & Genicot 2015).

[12] 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.

[13] One reason is that marriages in India are *patrilocal*, i.e., women move 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.

[14] 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.

pirical investigation of our underlying mechanisms. We present consistent empirical evidence on the mechanisms in Section 6 and discuss the importance of education as the primary mechanism. In Section 7, we discuss suggestive evidence on the underlying heterogeneous treatment effects of our main results, show our results to be robust to potential concerns, 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.

2 Institutional details and data

2.1 Sanitation policies in India

The Indian government's efforts to address sanitation challenges have evolved over several decades. The Central Rural Sanitation Programme (CRSP), launched in 1986, represented India's first nationwide rural sanitation initiative, focusing primarily on subsidy provision for toilet construction. However, the program proved largely ineffective due to its emphasis on infrastructure over behavior change and awareness (Kumar 2022). By 1999, the government replaced the CRSP with the Total Sanitation Campaign (TSC), which aimed at implementing information and sanitation based education campaigns alongside improving sanitation coverage. However, the TSC fell short of its goals of improving sanitation based awareness because of under-investments in information based campaigns with implementation varying significantly across states (Hueso & Bell 2013, WSP 2011). The limited effectiveness of these early programs led to the launch of the *Nirmal Bharat Abhiyan* (NBA) in 2012, which evolved into the *Swachh Bharat Mission* (SBM or Clean India Mission) in 2014 targeting behavior-change based information campaigns on sanitation awareness combined with financial incentives. Although it was the most successful sanitation program in India, it cost approximately INR 1.34 trillion (US$20 billion) between 2014-2019 (SBM, 2019).

Thus, despite dedicated efforts spanning decades before the SBM was launched in 2014, progress in improving sanitation coverage saw limited success due to deeply entrenched cultural norms, widespread lack of awareness about health impacts, and persistent financial constraints. The difficulty in achieving improvements in sanitation coverage through direct interventions highlights the importance of understanding how other policies might indirectly influence sanitation coverage by reducing some of these underlying frictions.

2.2 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, applicable to all members of these religions except Scheduled Tribes. It established the rules of division of household property in the aftermath of the death of the patriarch of the family in the absence of a will.[15] 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. Such 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*[16] 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.

2.3 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 specified eligibility criteria. The amendments specified four eligibility criteria: residence in a reform state at the time of amendment, unmarried status at the time of state-level reform, membership in HSA-governed religions (Hinduism, Jainism, Sikhism, or Buddhism), and the existence of undivided household property in her natal household at the time of the state amendment. Kerala's policy differed from the other four states. Specifically, Kerala's amendment abolished joint family property altogether (Kerala

[15] 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).

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

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). 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.

2.4 Treatment definition

We define treatment status of a household based on whether at least one married woman in a given household was exposed to HSAA. Using the year of the latest marriage in the household, our definition assigns those households as treated if the latest marriage took place after the HSAA was adopted in their state.[17] This approach removes potential measurement error that would arise if we used the marriage year of any earlier cohorts in the household.[18] Additionally, treatment eligibility requires that woman's state of birth be a HSAA state, however, we define treatment based on state of residence of woman because state of birth is unobserved in the data. Note that this not a concern because inter-state marital migration is very low in India (less than 5%).[19,20]

2.5 Data

We use data from the third (2005-06) wave of National Family Health Survey (or NFHS-III), a large-scale, cross-sectional, 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. The survey includes a questionnaire for women aged 15-49 years, covering marital status, educational attainment, year of marriage, and various dimensions of women's autonomy and decision-making, and has information on private toilet ownership in the marital household of women, which is our main outcome of interest.[21,22]

[17] Since the treatment is at the household level, for a household to be treated, we need at least one woman in the household to have married after HSAA was adopted in the state.

[18] Consider a household where the oldest woman married before HSAA adoption, but her daughter-in-law married her son after HSAA adoption. Using the oldest woman's marriage year would incorrectly classify this household as untreated despite the daughter-in-law's HSAA exposure.

[19] In a 2011 report by the Indian Economic Service, inter-state migration because of marriage is estimated to be 4.6% in between 2001-2011 (Kumar 2021). This is computed by using the inter-state migration rate of 11% (Table 1) multiplied by share of individuals who migrate because of marriage, which is 0.42 (Table 4).

[20] Reviewing both the existing literature on HSAA and relevant legal documentation, it is unclear whether the HSAA applied to Scheduled Tribes (ST). See for example, Civil Appeal No. 13516 of 2024. Accordingly, we re-estimate our specifications excluding ST from our sample as a robustness check. Our results remain robust to this exclusion (see Online Appendix Table A8).

[21] The NFHS-III records 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. Our main outcome of interest is whether the household has access to a private toilet.

[22] 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.

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. As described earlier, Kerala's amendment applied to all daughters regardless of their marital status. This eliminates within-state variation to identify the impact of the HSAA on household outcomes in Kerala. Hence, we drop all households belonging to the state of Kerala from our sample.

After Kerala's removal from the sample, the remaining HSAA adoptions occurred in Andhra Pradesh (1986), Tamil Nadu (1989), and Maharashtra and Karnataka (1994), prior to national ratification in 2005. We exclude households with post-2005 marriages due to the absence of treatment variation after national implementation. We also exclude households from Jammu & Kashmir where the Hindu Succession Act does not apply, leaving 27 states in our main analysis.

2.5.1 Summary Statistics

Table 1 presents summary statistics for key variables, comparing HSAA-adopting states with non-adopting states through 2005. The data reveal significant heterogeneity both between treatment and control states and among treatment states themselves. Treatment states consistently show higher urbanization rates than control states. Andhra Pradesh (treated in 1986) displays distinct marriage patterns, with a mean marriage age of 17 years compared to 18.5-19.3 years in other states, reflecting its systemic regional sociocultural differences. Tamil Nadu (treated in 1989) exhibits distinctive demographic characteristics, with the highest proportion of Other Backward Classes (69.5%), substantially exceeding other treatment states (39.8% and 51.5%) and control states (30.4%). Conversely, control states show a higher proportion of General Caste individuals (37.5%) compared to Tamil Nadu (2.5%), while remaining comparable to other treatment states. While wealth distributions are broadly similar, Tamil Nadu shows notable differences, with 22% in the highest wealth quintile compared to 28% in control states, and higher concentrations in the third and fourth quintiles—a pattern consistent with its larger proportion of historically disadvantaged castes and higher levels of inequality.

3 Empirical Strategy

We begin by explaining how our cross-sectional data structure permits estimation of heterogeneous average treatment effects on the treated. While the absence of panel or repeated cross-sectional data might appear to preclude a difference-in-differences strategy, the eligibility criteria based on marriage year relative to state-specific policy implementation provides the necessary temporal variation.[23] 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 methodological advances demonstrate that two-way fixed effects estimation in staggered adoption designs can produce misleading results when treatment effects are heterogeneous across groups 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 to account for small number of states and allowing for arbitrary correlation between the unobservables within a state.[24]

Following Callaway & Sant'Anna (2021), we estimate the group-time average treatment effects of the policy on the treated. Let i index women, t denote marriage year (cohort), and G_i represent the policy implementation year in HSAA-amended state where i belongs. For individuals in non-HSAA states G_i takes a value of zero, representing that these individuals were never treated.[25] Let Y_{it} be an indicator variable equal to 1 if woman i married in year t has a private toilet in her household at the time of survey, where $g \in \mathcal{G} \equiv \{1986, 1989, 1994\} \cup \{0\}$.[26] We report estimates using the never treated as the comparison group in our main analysis. Our results remain robust when using not-yet-treated units as the comparison group instead of never-treated units.

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 the conditional parallel trends assumption for identification: absent HSAA, trends in toilet ownership would be parallel between amendment and never-treated states, conditional on relevant household characteristics. We condition on key household characteristics: wealth quintile indicators, caste categories, and urban residence status. Therefore, we impose the parallel trends assumption conditional on these char-

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

[24] For comparison, we present two-way fixed effects estimates in Section 4.3.

[25] 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.

[26] Unlike standard difference-in-differences settings where outcomes vary over time t, our outcome is a point-in-time realization.

acteristics which is described in equation (1) as a statement on the counterfactual: in the absence of policy, 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.

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

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) formalizes the parallel trends assumption: in the absence of treatment, the average difference in potential outcomes between any two cohorts would be identical for treated and never-treated groups, conditional on covariates X_i.

The estimator has a doubly robust property that combines propensity score adjustments with outcome regression models, achieving consistency if either component is correctly specified (see Appendix E). This property is particularly valuable in our context, as it addresses concerns about observable differences between treated and untreated states by capturing observable differences in counterfactual outcome patterns between treatment and control states, conditional on observables through their propensity scores.

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 employ the doubly robust estimator of Callaway & Sant'Anna (2021), which extends the two-period, two-group framework of Sant'Anna & Zhao (2020) to accommodate multiple periods and groups in estimating $ATT(g, t)$. The doubly robust estimator exhibits better performance relative to inverse probability weighting, particularly with unbalanced data structures like ours. See Callaway & Sant'Anna (2021) and Sant'Anna & Zhao (2020) for more details.[27]

3.3 Bounds on the true parameter

A key eligibility criterion under the amendment stipulated that the woman's natal household property must have been undivided when the amendment was enacted in her state. To the best of our knowledge this data does not exist in any survey of Indian households. Due to this data limitation, our treatment assignment likely includes some individuals who should be classified as controls, introducing potential measurement error in treatment status. To address this measurement concern, we derive analytical bounds on the true parameter under such treatment misclassification. Under reasonable assumptions, our analysis demonstrates that our estimates constitute a lower bound on the true average treatment effect on the treated (ATT), thus providing conservative estimates of the policy's impact.

We formally show 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 Appendix Section B. 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 the timing of property division. 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 average effect remains unchanged, as the true effect for these misclassified individuals is zero. Consequently the estimated effect will be understated.

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 B, noting that the average outcome for the treated group is a mix of zero and non-zero treatment effects. This further strengthens our argument.

[27] We list the details on the Callaway & Sant'Anna (2021) estimator in Appendix E. It combines outcome regression and propensity score weighting approaches, providing consistent estimates even if one of these components is misspecified. These identification strategies share theoretical foundations with double/debiased machine learning approaches (Chernozhukov et al. 2018).

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 high 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.[28] 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 a 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.

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 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

[28] We thank Jeff Smith for this suggestion.

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 of the three 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 reported in Appendix Table A6, shows that the HSAA led to an increase in toilet adoption by 2.7 pp (p-value < 0.10) on average.[29] The TWFE estimate is 15.6% lower than the estimates from the heterogeneity-robust estimator that imposes weaker identifying assumptions than the TWFE estimator. [30]

5 Theoretical framework to guide mechanisms

In this section, we present a static discrete choice model to provide a theoretical foundation to guide the empirical investigation of the mechanisms which drives our main empirical result of a woman empowering policy (the HSAA, in our case) increasing 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:

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

[29] Specifically, we estimate the following equation: $Y_i = \alpha + \delta_{s(i)} + \delta_{c(i)} + \beta D_{i,c(i)} + X_i'\gamma + \epsilon_i$, where Y_i is the indicator of the presence of a toilet in the household of individual i in state $s(i)$ who belongs to the marital cohort $c(i)$; $\delta_{s(i)}, \delta_{c(i)}$ 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.

[30] Additionally, Bacon decomposition (Goodman-Bacon 2021) shows that 3.8% of the total weight in the TWFE estimate comes from forbidden comparisons of already treated units with not-yet treated units.

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 and C_h is the cost of toilet known to the households. $\beta_{i,h}$ represents the valuation of the presence of a toilet by individual i of household h.

The household's total utility is a weighted sum of the individuals' utilities given $T_h = t \in \{0,1\}$:

$$\begin{aligned} U_h(t) &= \theta_{m,h} U_{m,h}(X_h, t) + \theta_{w,h} U_{w,h}(X_h, t) + \epsilon_{t,h} \\ &= \theta_{m,h} U_{m,h}(Y_h - C_h \cdot t, t) + \theta_{w,h} U_{w,h}(Y_h - C_h \cdot t, t) + \epsilon_{t,h} \end{aligned} \tag{4}$$

where $\epsilon_{t,h}$ are unobserved idiosyncratic household preference shocks. Preference shocks capture unobserved idiosyncratic factors influencing household decision-making, including misconceptions about health effects, adoption costs, or cultural adjustments. These factors divert households' expected valuation of the household public good from what would be predicted by observable characteristics alone, and distinguish between otherwise identical households who make different decisions in equilibrium despite having identical observable characteristics. Thus, preference shocks themselves do not incorporate any observable household characteristics, although its distribution could depend on these characteristics as we discuss later.

The woman's decision-making power is represented by $\theta_{w,h} \in [0,1]$, and $\theta_{m,h} = 1 - \theta_{w,h}$ is the man's decision-making power.[31] If $\theta_{m,h} = 1$ then the decision of the man in the household is dictatorial, but still subject to household preference shocks.

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., $u_i(X) = \lambda_i X$ for some exogenous $\lambda_i > 0$.[32] Following the existing literature that shows that women value toilet more than men, we assume that $\beta_{w,h} > \beta_{m,h}$ for all h.[33] For simplicity, we assume $\beta_{i,h} = \beta_i$ for all i and for all h

[31] Our 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.

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

[33] Even though we motivate this using empirical evidence, we can relax this assumption to some degree. For example our results still hold as long as there are some but not all households with a strict gender gap in preference i.e., if $\beta_{w,h} > \beta_{m,h}$ for some h and for the rest $\beta_{w,h} = \beta_{m,h}$.

The difference in utility between building and not building a toilet is:

$$\begin{aligned}\Delta U_h &\equiv U_{1h} - U_{0h} \\ &= \theta_m[u_m(Y_h - C_h) - u_m(Y_h)] + \theta_w[u_w(Y_h - C_h) - u_w(Y_h)] + \theta_m\beta_m + \theta_w\beta_w + (\epsilon_{1h} - \epsilon_{0h}) \\ &= \theta_m\beta_m + \theta_w\beta_w - (\theta_m\lambda_m + \theta_w\lambda_w)C_h + (\epsilon_{1h} - \epsilon_{0h})\end{aligned}$$

Assuming that ϵ_{1h} and ϵ_{0h} follow Type-I Extreme Value distribution, the difference $\epsilon_h \equiv \epsilon_{1h} - \epsilon_{0h}$ follows a logistic distribution. Let its scale parameter be σ_h. Let $\Lambda \equiv \theta_m\lambda_m + \theta_w\lambda_w$, so:

$$\Delta U_h = \theta_m\beta_m + \theta_w\beta_w - \Lambda C_h + \epsilon_h$$

Relation to education: The role of education in reducing preference uncertainty has been well-documented (see for example, Schultz (1975), and Grossman (2006)). Motivated by this, we assume that the variance of the preference shock σ_h^2 decreases with an increase in education in the household. We allow this reduction in σ_h^2 through increase in education of either the woman or man, or both:[34]

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

This modelling choice allows for policy-induced changes in the marriage market equilibrium where empowered women could marry more educated husbands, which could serve as an additional mechanism.[35] Given that we do not find strong evidence that treated cohorts of women marry men with different education relative to control cohorts of women (See Section 6.3), in the remaining discussion of the model, we focus on the case where there are exogenous changes to the woman's education level.

Choice Probabilities:

Define the deterministic part of the utility difference that represents the true net valuation of a toilet in absence of any shock for household h by $\Delta_h \equiv \theta_m\beta_m + \theta_w\beta_w - \Lambda C_h$. We assume that the proportion of households that are harmed by choosing to build a toilet absent any preference shocks, (i.e. $\Delta_h < 0$) is negligible.[36]

[34] Alternatively, further generalization can be made wherein the variance of the shock 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. Our results would hold in such extensions.

[35] This could further be extended to allow for additional factors ξ_h by specifying $\sigma_h^2 = f_h\left(E_{w,h}, E_{m,h}\right) + \xi_h$. We discuss this further in Section 6.5.

[36] Even though this is a weak assumption given vast evidence on the health benefits of toilets, in the appendix we discuss a relaxation of this assumption by considering relative masses of households and their relative magnitude of benefit and cost.

The probability that household h builds a toilet is:

$$\begin{aligned} P_h &\equiv \Pr(\Delta U_h \geq 0) = Pr(\Delta_h + \epsilon_h \geq 0) \\ &= \frac{1}{1 + exp(-\frac{\Delta_h}{\sigma_h})} \end{aligned}$$

Correspondingly, the proportion of households building toilets in the population is:

$$P = \int_{h \in \mathcal{H}} P_h \, dF(h)$$

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 *A decrease in the variance of the preference shock σ_h (equivalently an increase in education) increases the probability of the household P_h choosing to build a toilet which leads to an increase in the proportion of households P building a toilet.*

Proof: See Appendix C.1.

Proposition 2: *An increase in women's decision-making power $\theta_{w,h}$ increases P_h and thus increases P proportional to the gender gap in preferences $(\beta_w - \beta_m)$. The magnitude of this effect depends on the ratio Δ_h/σ_h. When the variance of the preference shocks σ_h is large (relative to Δ_h), any effect of increasing $\theta_{w,h}$ is attenuated as household decisions are dominated by the noise component.*

Proof: See Appendix C.2.

Proposition 3: *The combined effect of decreasing σ_h and increasing $\theta_{w,h}$ on P_h and thus on P is positive.*

Proof: See Appendix C.3.

5.2 Discussion of model mechanisms

The primary channel through which the model operates is that increasing education reduces the variance in the preference shocks of having a toilet. This reduction makes the household choice to be less sensitive to unobserved idiosyncratic factors such as misconceptions about health effects, adoption costs, or cultural adjustments associated with changing traditional practices regarding sanitation. Correspondingly, as long as there are more households in the population who truly would benefit from having a toilet, the proportion of households building toilets increases as a result of increased education. When the variance of preference shocks is large, the probability of adoption becomes less sensitive to changes in decision-making power, as equilibrium decisions are dominated by the random component (McFadden 1973, Train 2009). This generates a natural complementarity between education and decision-making power: reducing the variance of preference shocks through education makes household choices more responsive to changes in decision-making power. While from the policy-maker's perspective, increasing decision-making power of women can have many benefits through the channel of reducing gender inequality within the household, our model suggests that adoption of household public goods like toilets which are primarily deterred by large misconceptions about their benefits, may not increase substantially with increased decision-making power alone despite women valuing toilets more than men.

In our model, high variance in the noise component of household utility make household decisions dominated by the noise component and swamp the effect of any gains in decision-making power, leading to low adoption rates. Thus, as long as women value toilets more than men, the marginal effect of increasing women's decision-making power on toilet adoption is attenuated when the variance of the preference shock is very large, and stronger when variance of the preference shock is lower. Finally, the combined effect of increased education and decision-making power is positive on toilet adoption. Intuitively, this results from the positive effect of increased education on toilet adoption potentially reducing misperceptions, combined along with increased decision-making power of women, given that women value toilets more than men.

5.3 Discussion of model choice, generalizability, and other channels

We discuss certain simplifications and generalizability of the implications of our model.

First, it is plausible that increased education because of the policy could itself directly enhance women's decision-making power. However, we do not have any variation to empirically test that hypothesis. If that were empirically valid, it still maintains the take-away of increased education as the primary mechanism. To that end, in our model, women's education and decision-making power are independent of one another and both can be exogenously changed through policy.[37]

Second, we do not incorporate externalities from the presence or absence of the household public good (toilet). This could understate the true benefits accrued to the household, which would strengthen the implications of our model.

[37] Unlike public goods provided by local or federal governments financed through taxation, where political representation and proximity to elected officials can determine provision (Besley et al. 2004), household toilets are private decisions made at the household level while being public to the members within the household. While generating community-level health externalities, some primary benefits of toilets (privacy, safety) accrue directly to household members, with women benefiting disproportionately more. This makes intra-household characteristics central to understanding toilet adoption.

Our model abstracts away from endogenizing private consumption shares to adoption of household public good.[38] The consistency of our estimated reduced form effect of the policy on toilet adoption does not depend on this assumption. If, in reality, toilet adoption induces changes in the allocation of private consumption across household members, our results would be interpreted as capturing the reduced-form effect of women's empowerment policy on household public good adoption, net of intra-household reallocation of private consumption, if any. However, welfare comparisons require additional structure. Welfare depends on the marginal rate of substitution between sanitation and private consumption for each household member, which governs how households trade off the public good against private goods at the margin. Identifying this requires supply side variation in the household public good that shifts adoption independent of the policy. Such variation is not present in our setting. Existing empirical evidence shows that private good expenditure shares primarily respond to who controls income or transfers within the household (Lundberg et al. 1997, Duflo 2003, Armand et al. 2020). To this extent, how household public good investments interact with intra-household private good consumption shares remains an open empirical question for future work.

Next, financial constraints have been documented to be a barrier to toilet adoption (see for example Yishay et al. (2017), Abramovsky et al. (2023)). The HSAA could increase household income through increased inheritance (Deininger et al. 2013) or increased dowries (Roy 2015) as compensating mechanisms, thereby relaxing household budget constraints and potentially increasing the probability of toilet adoption. However, within the context of India, Augsburg, Malde, Olorenshaw & Wahhaj (2023) find that relaxing financial constraints alone is not enough, and previous national government policies which only involved subsidizing the cost of toilet construction have not been effective (see discussion in Section 2). Nevertheless, our model can accommodate this channel with a simple extension. By allowing household members' utilities to be concave in consumption instead of linear, it is straightforward to show that relaxing the budget constraint by increasing Y_h increases the probability of a household choosing to build a toilet.

6 Empirical evidence on mechanisms

Our data allows us to test for three mechanisms that could plausibly drive our main results on toilet ownership: women's years of educational attainment, their intra-

[38] In standard separable collective household models private good consumption shares embedded in Pareto weights, are orthogonal to public good provision. They are typically identified through exogenous variation in wages, prices, or transfers under separability. Under this assumption, purchasing a household public good reduces total household consumption without affecting private shares in a static framework in absence of savings and fixed budget constraint. However, relaxing separability require even richer variation to identify the MRS beyond what is used in separable models (Alam et al. 2025).

household decision-making power within their marital household, and marriage market equilibrium changes through husband's observed education.[39] Increased education could increase toilet coverage through increase in health and sanitation based awareness, reducing misperceptions regarding costs and benefits of toilets, and combating rigid cultural norms. 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, any increase in their intra-household decision-making power could also increase toilet coverage if the HSAA increased 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 years of education in these treated states.[40] 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 educational 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 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.

[39] 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)).

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

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.[41]

We report the estimates of heterogenous treatment effects of the HSAA on intra-household decision-making power of women in Table 4.[42] 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 (that adopted HSAA in 1994) where overall ATT increases by 0.112 SD 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 power of treated women. For cohorts in the state of Tamil Nadu (that 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. We discuss this in more details in subsection 7.1.2. This suggests that higher-decision making power alone could not translate into advocating for building a toilet, unless education also increases thereby plausibly increasing sanitation based awareness.

6.3 Husband's education

A policy improving women's inheritance rights could potentially impact the *observed* education of husband, through its impact on the marriage market equilibrium. Specifically, in equilibrium, increased female education could lead to increased demand for, and consequently increased matches with, more educated males.[43] In other words, increased female education could lead to higher rates of positive assortative matching in the marriage market. It is important to note that such changes in the marriage market equilibrium are still the consequence of the HSAA—a woman empowering policy. As a result, any effect on the marriage market is not a threat to identification,

[41] 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.

[42] 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.

[43] Such changes in marriage market equilibrium are 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.

rather this exercise serves as an exploration of additional mechanisms supported by our theoretical framework.

We report the estimates of heterogenous treatment effects of the HSAA on husband's education in Appendix Table A5. The aggregate weighted average ATT does show statistical significance at the 95% significance level in spite of statistically insignificant group-wise effects, and should be interpreted with caution.[44] Hence, we do not fully rule out impacts on marriage market equilibirium and women marrying men with higher education. Overall, our findings on hetergenous treatment effects suggest 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 and their decision-making power.

6.4 On the importance of women's increased education as the primary mechanism

Our theoretical framework and empirical evidence show that increased decision-making power alone may be insufficient—we find that its effectiveness crucially depends on reduced preference uncertainty through education. This complementarity between education and decision-making power can help explain why similar empowerment policies might yield different outcomes across contexts with varying educational gains.

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 con-

[44] Upon observing the event study graphs in Appendix 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.

text 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.

6.5 Other plausible mechanisms, and their connection to the model and the policy

The HSAA through its intended objective of reducing gender inequality in inheritance could have increased women's wealth and thereby their decision-making power, while simultaneously reducing men's wealth and thereby their decision-making power if they received smaller inheritance shares due to redistribution to sisters.[45] This redistribution of decision-making power could affect household decisions on adoption of household public goods like toilets. Even though we do not explicitly model inheritance due to lack of data, our model allows for this potential channel because decision-making power of men and women are linearly dependent, i.e., $\theta_{m,h} + \theta_{w,h} = 1$, for all h. Empirically this linear dependence implies that one cannot separately identify the policy's impact on women's and men's decision making powers, even though it is directly incorporated in our theoretical framework. Additionally, the empirical evidence on HSAA affecting inheritance for treated women is mixed (see Section 1 footnote 11).

Furthermore, such redistribution of transfer despite mixed evidence could impact who marries whom thereby altering the marriage market equilibrium. Even though we do not explicitly model the marriage market equilibrium, we allow for it to operate through the dependence of the variance of preference shocks on education of the husband (see equation 5). This could further be extended to allow for additional factors ξ_h that could impact the variance of the preference shock distribution by specifying $\sigma_h^2 = f_h\left(E_{w,h}, E_{m,h}\right) + \xi_h$. Such additional factors may or may not be impacted by the HSAA but allows for useful discussion for completeness. For example, even though by construction HSAA cannot impact age of an individual, the age of the household member could systematically be related to the preference shock variance with older members more likely to be influenced by cultural norms and misperceptions on sanitation benefits, thus have higher preference shock variance than younger members. Similarly, household members employed in the healthcare sector may be less likely to be influenced by misperceptions on sanitation benefits and thus have lower preference shock variance.

[45] We thank two anonymous referees for pointing this out.

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 that our results are robust to these concerns.

7.1 Discussion on the heterogeneity of treatment effects

Heterogeneous treatment effects across groups arise from idiosyncratic, group- and time-specific factors that cause different treated cohorts to experience different treated potential outcomes for the same policy. In staggered adoption settings, this is naturally captured by the group-time average treatment effect on the treated, $ATT(g,t) \equiv \mathbb{E}[Y_{it}(g) - Y_{it}(\infty) \mid G(i) = g]$ which by definition is a function of group identity $G(i)$ and time t (Callaway & Sant'Anna 2021). While such heterogeneity is identified, its underlying sources are generally not identified. We therefore discuss some differences in observable idiosyncratic group-level characteristics that could drive the differences in results we find. This approach, though suggestive and non-exhaustive, is feasible and informative in our setting due to the small number of groups and the limited number of states per group, allowing us to interpret group-specific effects rather than focusing solely on an aggregated ATT. Discussion of group-specific effects may be infeasible or uninformative when the number of groups is large or when many groups adopt treatment simultaneously (Roth et al. 2023).

One of the biggest constraints for toilet adoption is misperceptions about the benefits of sanitation (Augsburg, Baquero, Gautam & Rodriguez-Lesmes 2023). Such misperceptions are likely to be high when education levels are low. Our model links this through the inverse relationship between education and the variance of preference shocks, motivated by early literature documenting the role of education in reducing preference uncertainty (Schultz 1975, Grossman 2006).

7.1.1 Lack of effects on units treated in 1986

Units treated in 1986 (Andhra Pradesh) have systematically and significantly lower age at marriage than any other treated state (Appendix Figure B5b). Since marriage has been documented to causally reduce educational attainment (Sekhri & Debnath 2014, Chari et al. 2017, Kanji et al. 2024), this idiosyncratic cultural feature of Andhra Pradesh plausibly reduces the scope for education to respond to the policy.

7.1.2 Lack of effects on units treated in 1989

The average levels of education of pre-treatment units in Tamil Nadu (adopted policy in 1989) are very low—approximately 4 years of schooling on average, compared to 5.6 years for pre-treatment units in the group adopting the policy in 1994. This represents a 40% difference, with the latter group attaining above primary education on average before policy adoption. Both the low baseline level of education and the lack of education gains through the policy can potentially be explained by caste-based frictions. This is why affirmative action policies in Indian education sector specifically target these groups (for e.g. the Right to Education Act of 2009). Specific to Tamil Nadu, more than 95% of the sample across cohorts belongs to disadvantaged caste groups (Appendix Figures B5a and B6). This idiosyncratic feature of Tamil Nadu plausibly limits the scope for education to respond to the women's empowerment policy.

Given the low levels of education at baseline, as well as no education gains from the policy, misperceptions about sanitation plausibly remain high among these units. High misperceptions overpower any decision-making gains from the policy in this group.[46] Through the lens of our model, this implies that the variance of preference shocks remains high even after the policy. The intuition is that when misperceptions are high, even women with increased decision-making power may not prioritize toilet adoption because their misperceptions overpower any decision-making gains from the policy. This is one plausible reason why increased decision-making power does not translate into toilet adoption for units treated in 1989. Overall, this is consistent with documented evidence that disadvantaged caste groups on average are significantly less likely to use toilets than advantaged caste groups (Banerjee et al. 2017).

7.1.3 Concentration of effects for units treated in 1994

It is useful to think about why effects are largest for units treated in 1994 in comparison to the other treated groups through the differences in the impacts in the mechanism. For units treated in 1986 (Andhra Pradesh), early marriage truncates educational attainment. For units treated in 1989 (Tamil Nadu), caste-based frictions limit educational attainment. In contrast, units treated in 1994 (Maharashtra and Karnataka) do not have these idiosyncratic features limiting educational attainment. Finally, Deininger et al. (2013) discuss some evidence which suggest dynamic learning about the policy which could have impacted the effect sizes over time. Any dynamic learning about the policy, along with the above discussed idiosyncratic heterogeneity is consistent with our results where the largest effects are concentrated in the younger

[46] Roy (2015) documents that the HSAA led to increased dowries as a compensatory mechanism which Heath & Tan (2020) interpret as an increase in women's non-labor income, which in their model increases their intra-household decision-making power. This channel could have been particularly operative in Tamil Nadu given that it has among the highest prevalence of dowry practices in India (Srinivasan 2005, Upadhyay 2012), plausibly explaining the large decision-making power for this group.

cohorts of the latest adopting group.

7.2 The Total Sanitation Campaign

A potential concern for our identification strategy is that the Total Sanitation Campaign (TSC), launched by the Government of India in 1999, could confound the estimates of the HSAA's impact on toilet adoption. The TSC aimed to increase sanitation coverage in rural areas nationwide, but its implementation intensity varied across states. A World Bank report (WSP 2011) documents the variation in intensity of the TSC implementation across states. Maharashtra and Karnataka—where we find significant HSAA effects—rank particularly high in TSC implementation (2nd and 9th, respectively), and other two HSAA treated states of Andhra Pradesh and Tamil Nadu–where we do not find significant HSAA effects—rank 4th and 11th, respectively. To address this concern, we conduct a robustness check by restricting our control group to include only states with comparable TSC performance.[47] Specifically, we limit our comparison group to states in the top 50% of TSC performance rankings.

We report our results in Panels A and B of Appendix Table A7. Despite the resulting reduction in statistical power and the increased demands on our heterogeneity-robust estimator, our main results on toilet coverage remain robust at the 90% confidence level. Furthermore, our identified mechanisms of increased education and decision-making power, also remain consistent with our main results. Furthermore, we do not find statistical significance in the bargaining power mechanisms for states treated in 1994, strengthening support for women's education to be the main mechanism.[48] As before, we find overall aggregate significance in observed education of the husband but effects on individual group of treated states remain insignificant, suggesting that marital market equilibrium could have been affected by HSAA though we do not have robust evidence for this. Importantly, we observe that Tamil Nadu, despite ranking 4th in TSC implementation and thus being a high performer, shows no significant effects of the HSAA on toilet adoption. This pattern aligns with our earlier findings on the heterogeneity of treatment effects across states and supports our conclusion that the complementarity between education and decision-making power drives the observed improvements in toilet coverage, rather than confounding policies like the TSC.

Thus for the purpose of identification of parameters in our main results, it is safe to 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. Additional support for this assumption is found in Augsburg, Baquero, Gau-

[47] We thank an anonymous referee for this suggestion.

[48] Note that high standard errors of estimated effects in states treated in 1986 and 1989 renders the aggregate estimate insignificant.

tam & 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.

7.3 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 A2.[49] 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).[50] 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 SD (p-value = 0.061) in these states on average. We report the results on these mechanisms in Appendix Tables A3 and A4. Notably, households in rural India face stricter cultural constraints such as strong societal norms surrounding religious purity, cultural taboos surrounding menstruating women, 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.

7.4 Endogenous selection into or out of the HSAA

There are two concerns regarding potential selection. First, if parents have a strong preference 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-selections could compromise clean comparisons in the event-study design.

Such patterns of self-selection would be visible in the data by examining the distribu-

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

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

tion of year of marriage and age at marriage. We plot the distribution of age at marriage and marriages relative to the HSAA adoption year in each of the treated states in Figures B5b and B7 and find no evidence of systemic jumps in marriages around the time of HSAA adoption. This rules out concerns of self-selection into or out of the policy.

7.5 Marital migration of women

One of the eligibility conditions under HSAA was that woman's state of birth should be an HSAA-adopting state. However, most datasets in India do not collect data on women's state of birth and we only get to see women's current state of residence, i.e., the state of their marital residence.[51] Hence, we use the state of current residence to infer HSAA eligibility. Even though marriage is the leading cause of female migration in India, a long-standing literature documents that inter-state marital migration is very low in India. This rules out concerns surrounding misclassification of treatment exposure due to inter-marital migration. In a 2011 report by the Indian Economic Service, inter-state migration because of marriage is estimated to be 4.6% in between 2001-2011 (Kumar 2021).[52] Behrman et al. (1995) document that marital migration primarily occurs within districts, and often within *talukas* (sub-districts). Furthermore, Fulford (2013) finds that the average commute distance between birth location and marital location across India is three and a half hours, with longer-distance migration being more common in North India. In our case, the treatment states are in western and southern India which typically have even shorter commute distances.[53] Finally, based on this long-standing evidence on low inter-state migration and lack of retrospective data on women's state of birth, the major share of literature on the HSAA discussed in our introduction have credibly used the state of woman's residence and not the state of birth of the woman to infer HSAA eligibility (see for example, Calvi (2020), Heath & Tan (2020) among many others).

7.6 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 evi-

[51] The Rural Economics Demographics Survey and the recent Longitudinal Aging Survey of India are exceptions.

[52] This is computed by using the inter-state migration rate of 11% from Table 1 multiplied by share of migration because of marriage which is 0.42 from Table 4.

[53] Additionally, Rosenzweig & Stark (1989) document that long distance marital migration of women is not a norm rather it happens systemically to mitigate economic shocks and facilitate consumption smoothing. Thus, conditional on marital migration which is low to begin with, longer distance migration is systematically more common in households with higher exposure to economic shocks.

dence 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 HSAA 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.

7.7 Supply side responses

The HSAA did not target sanitation directly. Hence, it is unlikely that supply side changes with respect to toilet provision are endogenous to the policy and its time of implementation. Indeed in absence of price data, identification assumes that the average price of toilet adoption in treated groups would have evolved in parallel to those in the never (and not-yet) treated groups, in absence of the policy. This assumption is plausible since the HSAA did not target sanitation directly.

7.8 Sample differences and robustness of results

Our analysis of the decision-making power mechanism relies on data that are available only for women currently in a marital union, who constitute 95.06% of our sample. To ensure that our main results on toilet ownership are not driven by this subset, we re-estimate our baseline specifications restricting the sample to currently married women and find that our results are robust (see Online Appendix Table A9). We do not impose this restriction when estimating effects on women's own education or partner's education. Women's educational attainment is observed regardless of current marital status. For partner's education, formerly married women's partners may have influenced household toilet adoption decisions during the period of union, and so we retain these observations in the estimation.

7.9 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* estimates that open defecation cost India 7.9% of its GDP, up from the 2014 World Bank estimate of 6.4%. The report concludes that increasing toilet coverage from 85% to 100% (a 17.6% increase) 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 (approximately $126 billion in 2017 PPP) from improved sanitation and productivity.[54] 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 1,000 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.[55] In 2004-05, the average toilet coverage in our sample was 36%. A 4.7 percentage point increase in toilet coverage due to the unintended benefits of the HSAA corresponds to a 13.1% increase in toilet coverage. Using a back-of-the-envelope calculation, we scale the UNICEF estimates to our findings under two scenarios of how health benefits relate to coverage gains. If benefits are proportional to the percentage increase in coverage as would be the case if marginal benefits are higher at lower baseline coverage, then the implied healthcare savings are approximately INR 13,120 per household ($649 in 2017 PPP), yielding potential national savings of INR 6.03 trillion ($93.8 billion in 2017 PPP).[56] If instead benefits scale linearly with the percentage-point increase in coverage, the implied savings are approximately INR 5,525 per household ($273 in 2017 PPP), yielding potential national savings of INR 2.54 trillion ($39.5 billion in 2017 PPP).[57] While neither estimate is directly comparable to the UNICEF projections due to different baselines and assumptions, they illustrate the potential magnitude of the unintended health benefits of the woman empowerment policy. Note that extrapolating our estimates to the national scale requires additional assumptions, particularly that state-specific idiosyncratic heterogeneity that generates treatment effect heterogeneity is independently and identically distributed across the country, similar to the states in the staggered roll-out.

In 2004-05, the average toilet coverage in our sample was 36%. A 4.7 percentage point increase in toilet coverage due to the unintended benefits of the HSAA corresponds to a 13.1% increase in toilet coverage. Using a back-of-the-envelope calculation, if we scale the UNICEF estimates proportionally to our findings, the unintended benefits of the HSAA increasing toilet coverage could have reduced healthcare costs by approximately INR 13,120 per household ($649 in 2017 PPP), yielding potential national

[54] 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 increase to achieve universal coverage, or 17.6% increase.

[55] 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 in 2017 exchange rates) between 2014 and 2019 to achieve its goals of eliminating open defecation and improving sanitation infrastructure across the country.

[56] Under proportional scaling savings are $(13.1/17.6) \times$ INR 17,622 $=$ INR 13,120 per household. National savings are $(13.1/17.6) \times$ INR 8.1 trillion $=$ INR 6.03 trillion. All monetary values are expressed in 2017 PPP for consistency with the UNICEF report.

[57] Under linear scaling, benefits are proportional to the percentage-point increase. So savings are $(4.7/15) \times$ INR 17,622 $=$ INR 5,525 per household, where 15 is the percentage-point increase in the UNICEF baseline (from 85% to 100%). National savings are $(4.7/15) \times$ INR 8.1 trillion $=$ INR 2.54 trillion.

savings of INR 6.03 trillion ($93.8 billion in 2017 PPP).[58] While these estimates are not directly comparable to the UNICEF projections due to different baselines and assumptions, they illustrate the potential magnitude of the unintended health benefits of the HSAA. Furthermore, the benefits of increased toilet coverage are plausibly non-linear, with larger gains expected at lower levels of coverage among states with a given idiosyncratic heterogeneity. Note that, extrapolating our estimates to the national scale requires additional assumptions, particularly that state-specific idiosyncratic heterogeneity that generates treatment effect hetergeneity is independently and identically distributed across the country, similar to the states in the staggered roll-out.

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 safety.

8 Summary and 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. Using this observation, in this paper, we present evidence of an unintended impact of the HSAA—a women-empowerment policy aimed at empowering women through improving their inheritance rights—on toilet adoption in India. 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.

Prior literature on this policy has documented mixed evidence on whether the policy increased women's inheritance, but has shown consistently that the policy had significant indirect effects, such as improving women's education. Coupled with other existing evidence that education could reduce informational frictions and that such frictions could be a major deterrent in toilet adoption, we build a theoretical framework to guide empirical tests of our mechanisms. Specifically, we build a discrete choice model of household decision-making with gender-specific preferences for a household pub-

[58] These calculations scale the UNICEF estimates (based on a 17.6% increase) to our observed 13.1% increase: (13.1/17.6) * INR 17,622 = INR 13,120 per household. The national savings are similarly scaled: (13.1/17.6) * INR 8.1 trillion = INR 6.03 trillion. All monetary values are expressed in 2017 PPP for consistency.

lic good (such as a toilet), where the household utility is subject to preference shocks whose dispersion are reduced by education. Our theoretical framework demonstrates that reduction in the variance of shocks makes household decisions less sensitive to idiosyncratic shocks such as misconceptions about health effects or cultural shocks and adjustments. Our model shows that when the variance of the shocks is high, increased decision-making power alone has limited impact on adoption, as choices are dominated by these random components. This generates a key insight: education and decision-making power are complementary, as reducing the dispersion of preference shocks and thus their importance through education makes household choices more responsive to women's decision-making power. Consequently, our model predicts that policies that increase women's education can be more effective at increasing toilet adoption than policies that target to increase only decision-making power.

Consistent with the predictions of our model, our empirical results indicate that increased education is the primary mechanism in increasing toilet adoption. Increased education plausibly mitigated documented misperceptions about sanitation, raising awareness and challenging cultural norms around open defecation. Increase in women's decision-making power was only effective in conjunction with increased women's education. This finding aligns with our model's predictions and 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 the impact of HSAA on toilet adoption being concentrated in the states of Maharashtra and Karnataka where the HSAA increased women's education and their intra-household decision-making power. The other treated states–Andhra Pradesh and Tamil Nadu experienced no significant effects. This is likely due to systemic differences: early marriages 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.

The theoretical insights from our model extend beyond toilet adoption to other household public goods in developing countries—such as clean cooking technology or preventive healthcare—where women's stronger preferences are coupled with substantial information frictions. Our framework suggests that the success of women's empowerment policies in increasing the adoption of such welfare-improving technologies depends crucially on their ability to simultaneously enhance education and decision-making power, explaining why similar policies might yield different outcomes across contexts based on their effectiveness in reducing dispersion of preference shocks.

From a sanitation policy perspective, our paper documents that women-empowerment policies such as the HSAA provide 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 highlight how policies empowering women can lead to broader household welfare improvements, beyond their intended scope.

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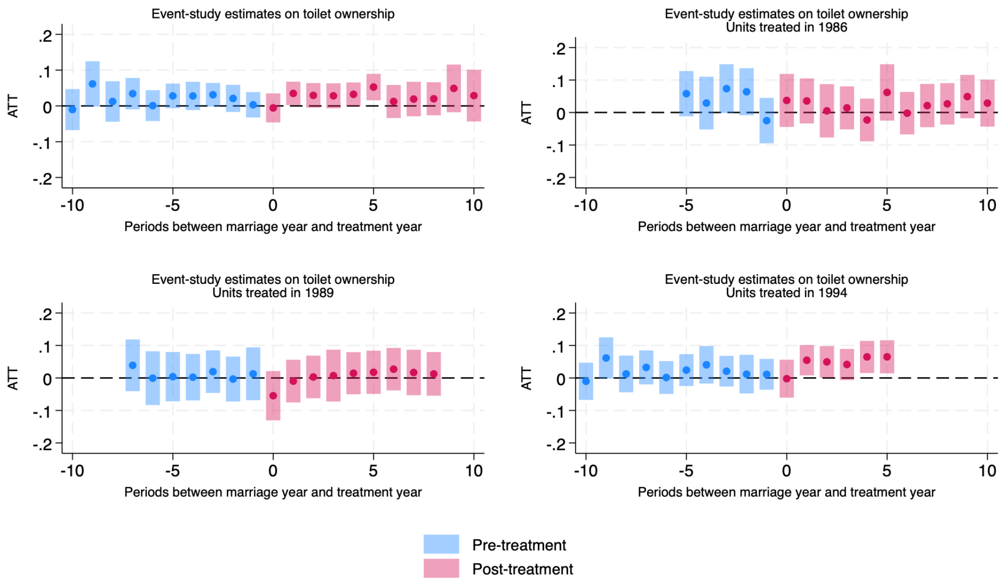
Tables and Figures

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 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

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 Karnataka (HSAA in 1994) and the never treated group respectively. 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

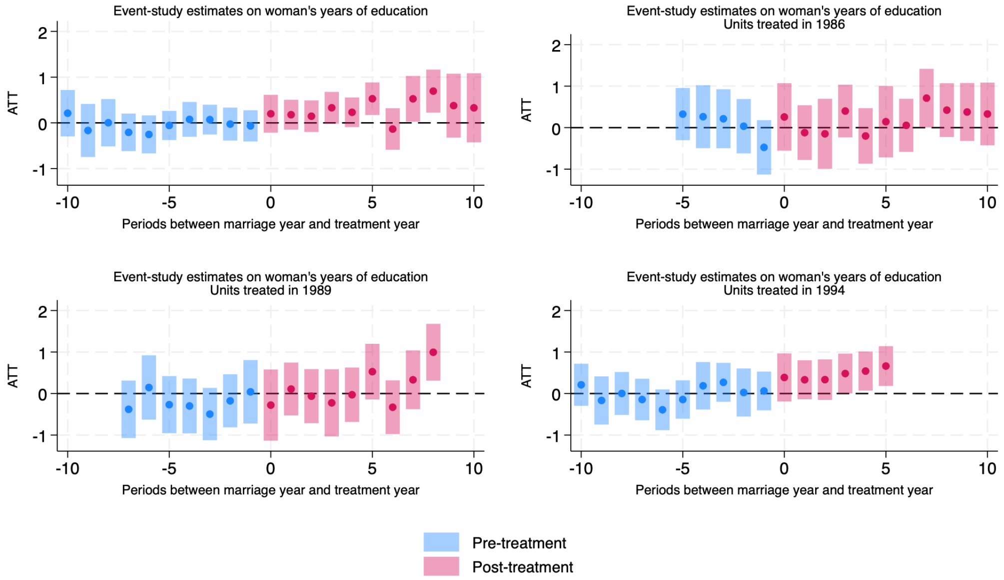
Notes: The table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT) parameter, followed by group-specific ATT estimates of the impact of the HSAA on household toilet ownership. These estimates are obtained under the conditional parallel trends assumption, 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Figure 2: Event study estimates on years of Education



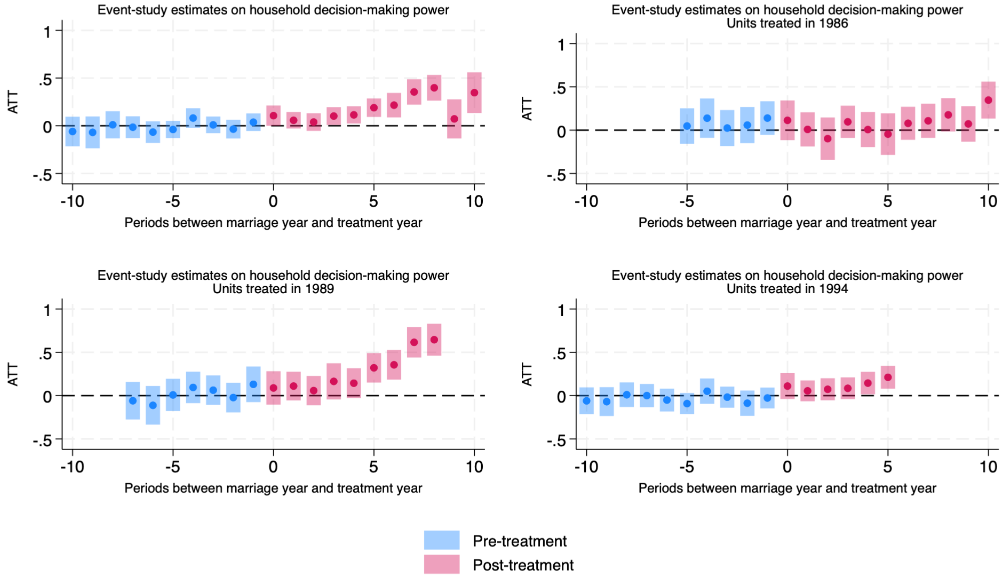
Notes: The effects of the HSAA on years of education estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group as the 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

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

	(1) Never treated	(2) 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

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 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

A Online Appendix

A.1 Tables

Table A1: Group-specific Pre-trend Test Results on Main Results and Mechanisms

χ^2 Pre-trend test	Toilet Ownership	Years of Education	Decision-making Power
Units treated in 1986	10.55	7.60	2.97
p-value (df=5)	[0.06]	[0.18]	[0.71]
Units treated in 1989	1.56	4.94	6.68
p-value (df=7)	[0.98]	[0.67]	[0.46]
Units treated in 1994	7.60	11.17	6.77
p-value (df=10)	[0.67]	[0.34]	[0.75]

Notes: This table reports pre-trend test statistics for each treatment group and outcome. The test evaluates the null hypothesis that all pre-treatment average treatment effects on the treated (ATT) are jointly equal to zero. Estimates are obtained using the doubly robust estimator described in Callaway & Sant'Anna (2021). The comparison group consists of households in states that did not adopt the HSAA until its national ratification in 2005. The χ^2 test statistic and corresponding p-value are reported, with degrees of freedom in parentheses. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Table A2: 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Table A3: 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Table A4: 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Table A5: 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. The unit of observation is a household, with treatment defined based on whether any woman in the household was exposed to the HSAA.

Table A6: Impact of HSAA on toilet ownership: Two-way fixed effects estimates

	Toilet ownership
	(1)
ATT-TWFE	0.027*
	(0.015)
Forbidden comparisons (% weight) (Bacon decomposition)	3.8%
State FE	Yes
Year of marriage FE	Yes
Controls	Yes
Observations	55,833

Notes: The table reports estimates of the average treatment effect on the treated 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.

Table A7: Impact of HSAA on outcomes and mechanisms (Top 50% of TSC performing states)

	(1) Toilet ownership	(2) Women's education	(3) Decision-making power	(4) Husband's education
Aggregate ATT	0.0258*	0.2089	0.1141***	0.3462**
	(0.0141)	(0.1426)	(0.0372)	(0.1658)
ATT of units treated in 1986	-0.0102	0.0427	0.0523	0.3446
	(0.0283)	(0.2767)	(0.0772)	(0.3365)
ATT of units treated in 1989	0.0210	0.0189	0.2688***	0.3008
	(0.0283)	(0.2681)	(0.0714)	(0.3071)
ATT of units treated in 1994	0.0436**	0.3664*	0.0730	0.3672
	(0.0204)	(0.2121)	(0.0518)	(0.2436)
Pre-trend test (χ^2)	17.06	26.59	13.17	28.51
p-value	[0.65]	[0.15]	[0.78]	[0.07]

Notes: This 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 multiple outcomes. The comparison group consists of states that did not adopt the HSAA until its national ratification in 2005. The sample is restricted to states in the top 50% of performing states as per the World Bank Report (WSP 2011). 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 (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). The pre-trend test evaluates the null hypothesis that all pre-treatment ATT estimates are jointly equal to zero, with p-values reported in square brackets.

Table A8: Impact of HSAA on Outcomes (Excluding Schedule Tribes)

	(1) Toilet Ownership	(2) Years of Education	(3) Decision-making Power	(4) Husband's Education
Aggregate ATT	0.0290**	0.3289**	0.1459***	0.3524**
	(0.0136)	(0.1364)	(0.0356)	(0.1535)
ATT of units treated in 1986	0.0235	0.2089	0.0875	0.5192
	(0.0262)	(0.2640)	(0.0768)	(0.3248)
ATT of units treated in 1989	0.0054	0.1045	0.2731***	0.3310
	(0.0256)	(0.2503)	(0.0651)	(0.2845)
ATT of units treated in 1994	0.0428**	0.4898**	0.1121**	0.2900
	(0.0199)	(0.2012)	(0.0501)	(0.2171)
Pre-trend test (χ^2)	20.15	17.38	17.86	21.50
p-value	[0.57]	[0.74]	[0.71]	[0.49]

Notes: This table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT), followed by group-specific ATT estimates of the impact of the HSAA on various outcomes, excluding Schedule Tribes from the sample. 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. The comparison group consists of households in states that did not adopt the HSAA until its national ratification in 2005. The pre-trend test evaluates the null hypothesis that all pre-treatment ATT estimates are jointly equal to zero; failure to reject (high p-values) supports the conditional parallel trends assumption. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Impact of HSAA on Toilet Ownership: Robustness to Women in Union

	(1) In union	(2) In union Rural	(3) In union Non-S.T.	(4) In union Non-S.T., Rural
Aggregate ATT	0.0311**	0.0254	0.0283**	0.0286*
	(0.0134)	(0.0158)	(0.0139)	(0.0169)
ATT of units treated in 1986	0.0217	0.0346	0.0241	0.0367
	(0.0262)	(0.0349)	(0.0273)	(0.0376)
ATT of units treated in 1989	-0.0063	-0.0091	-0.0062	-0.0091
	(0.0265)	(0.0307)	(0.0266)	(0.0310)
ATT of units treated in 1994	0.0504***	0.0375*	0.0458**	0.0451*
	(0.0190)	(0.0217)	(0.0201)	(0.0238)
Pre-trend test (χ^2)	20.80	23.16	21.37	20.58
p-value	[0.53]	[0.39]	[0.50]	[0.55]

Notes: This table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT), followed by group-specific ATT estimates of the impact of the HSAA on toilet ownership under different sample restrictions. Column (1) restricts the sample to women currently in union. Column (2) further restricts to rural households. Column (3) excludes Scheduled Tribes from the in-union sample. Column (4) combines both restrictions. The comparison group consists of states that did not adopt the HSAA until its national ratification in 2005. 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 (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). The pre-trend test evaluates the null hypothesis that all pre-treatment ATT estimates are jointly equal to zero, with p-values reported in square brackets.

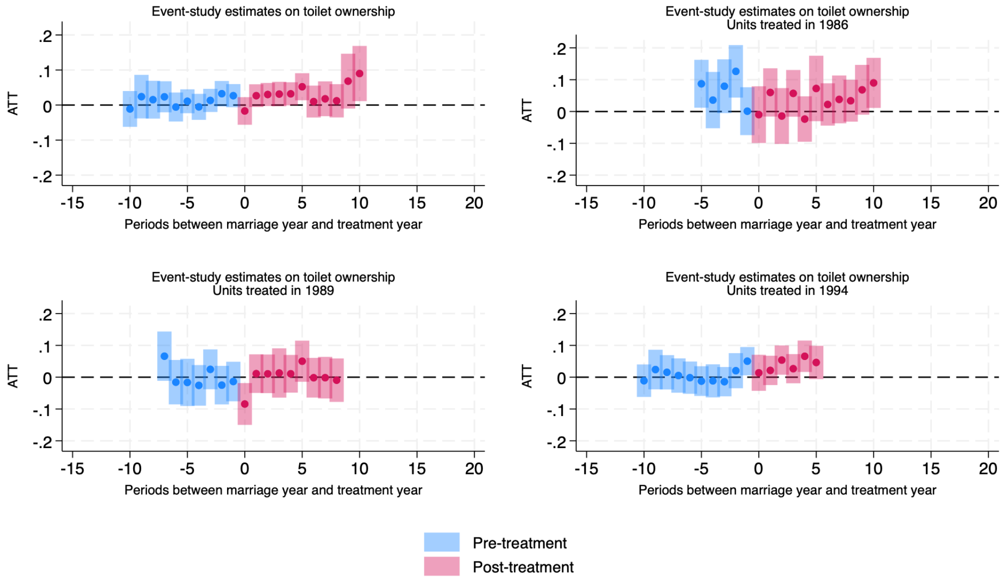
Table A10: NFHS-2 and NFHS-3 Appended: Impact of HSAA on Toilet Ownership

	Never Treated		Not Yet Treated	
	(1)	(2)	(3)	(4)
	Full	Removes S.T.	Full	Removes S.T.
Aggregate ATT	0.0263***	0.0257**	0.0265***	0.0261***
	(0.0096)	(0.0100)	(0.0096)	(0.0101)
ATT of units treated in 1986	-0.0144	-0.0155	-0.0120	-0.0128
	(0.0167)	(0.0175)	(0.0165)	(0.0173)
ATT of units treated in 1989	0.0268	0.0273	0.0257	0.0261
	(0.0183)	(0.0184)	(0.0181)	(0.0182)
ATT of units treated in 1994	0.0447***	0.0444***	0.0447***	0.0444***
	(0.0144)	(0.0153)	(0.0144)	(0.0153)
Pre-trend test (χ^2)	14.59	14.21	15.37	15.10
p-value	[0.88]	[0.89]	[0.85]	[0.86]

Notes: This table reports heterogeneity-robust estimates of the aggregated average treatment effect on the treated (ATT), followed by group-specific ATT estimates of the impact of the HSAA on household toilet ownership using appended NFHS-2 and NFHS-3 data. 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. The pre-trend test evaluates the null hypothesis that all pre-treatment ATT estimates are jointly equal to zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

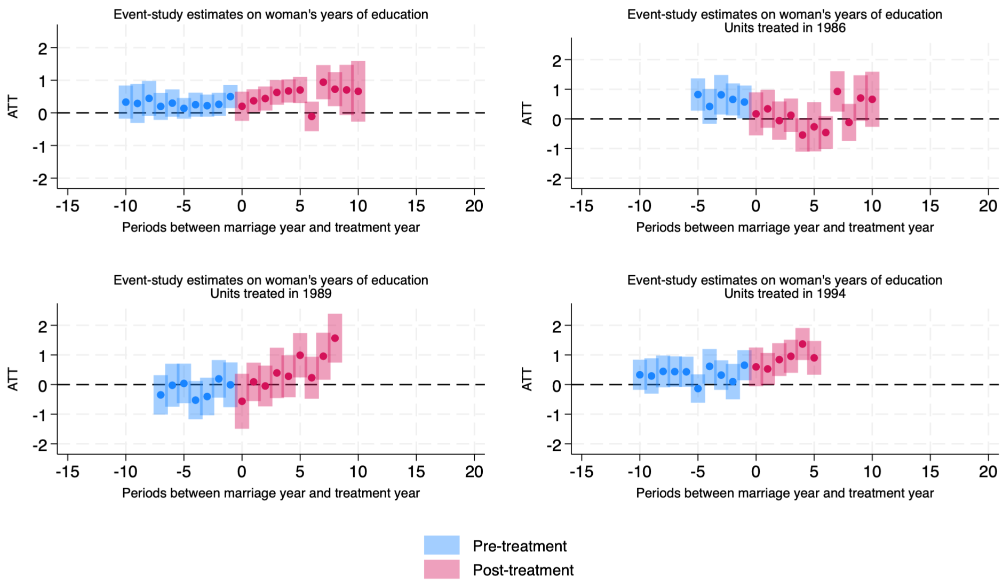
A.2 Figures

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



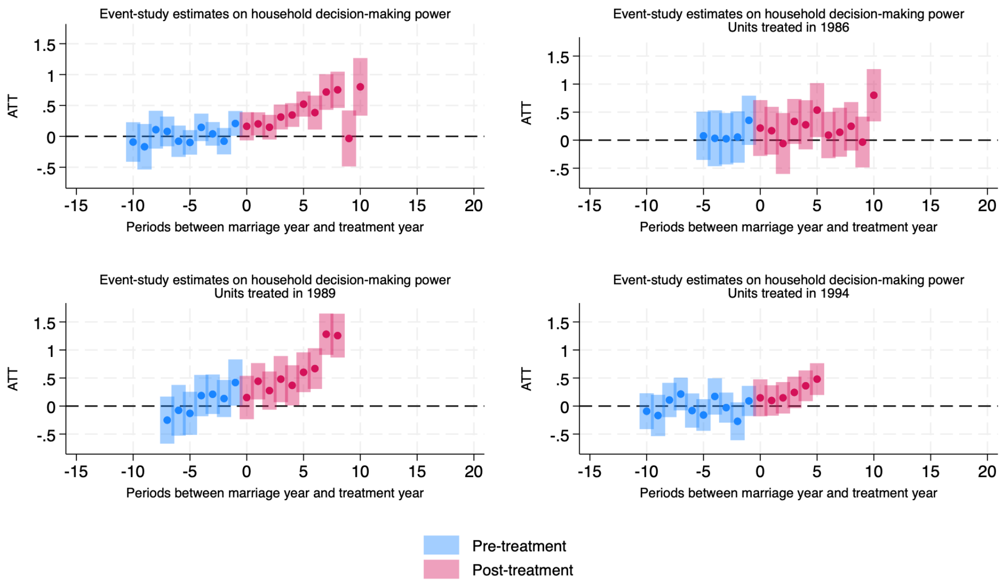
Notes: The effects of the HSAA on household toilet ownership in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

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



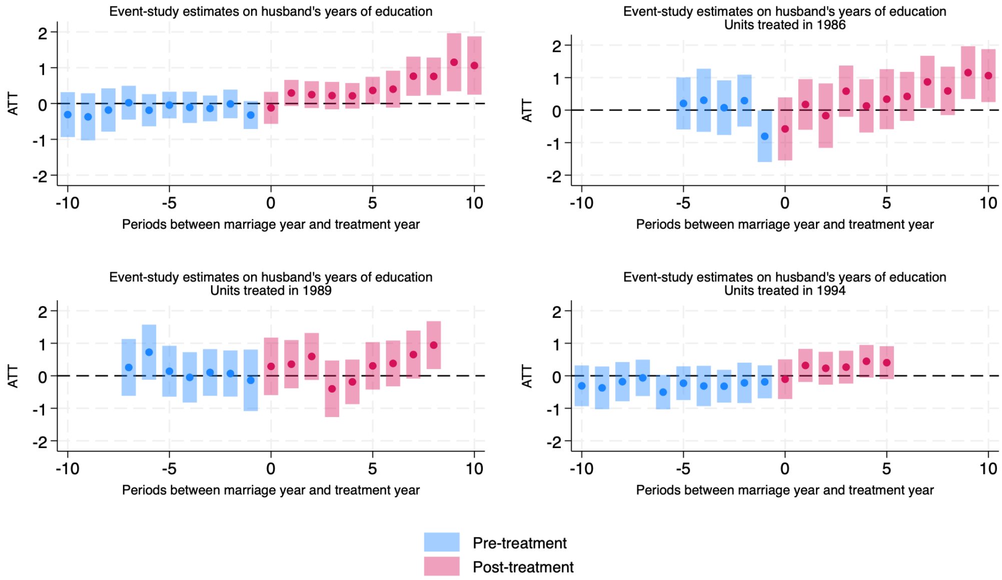
Notes: The effects of the HSAA on years of education in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

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



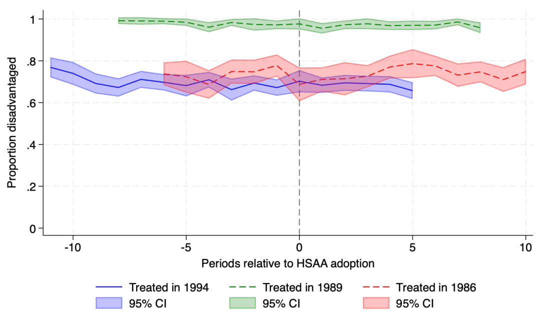
Notes: The effects of the HSAA on intra-household decision-making of women in rural areas estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

Figure B4: Event study estimates on husband's education

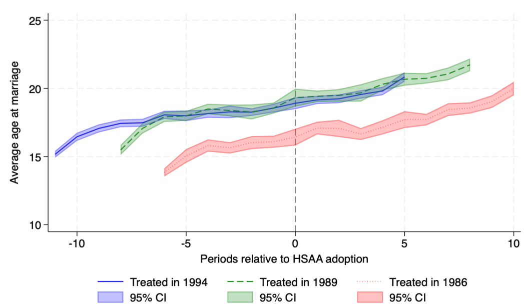


Notes: The effects of the HSAA on husband's education estimated under the conditional parallel trends assumption are plotted for each time period, first of the aggregated effect on all treated groups, followed by the group-specific effects on each treatment group, using the never-treated group as the 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 95% confidence bands for pre-treatment periods. Red lines provide point estimates and 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. The unit of observation is a household and treatment is defined based on whether any woman in the household was exposed to the HSAA.

Figure B5: Caste Composition and Average Age at Marriage Over Time



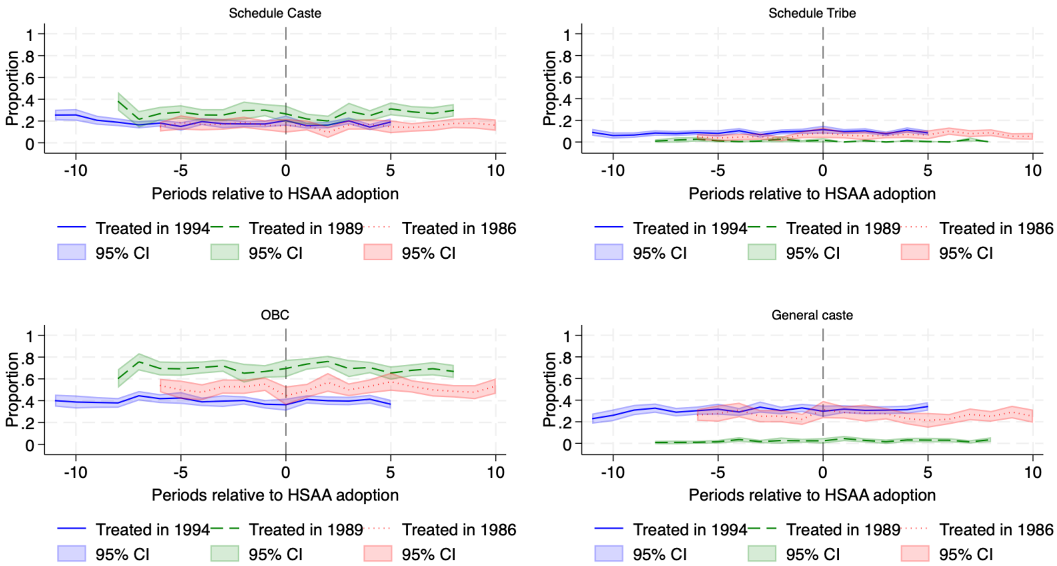
(a) Proportion of Disadvantaged Caste Groups



(b) Average Age at Marriage Over Time

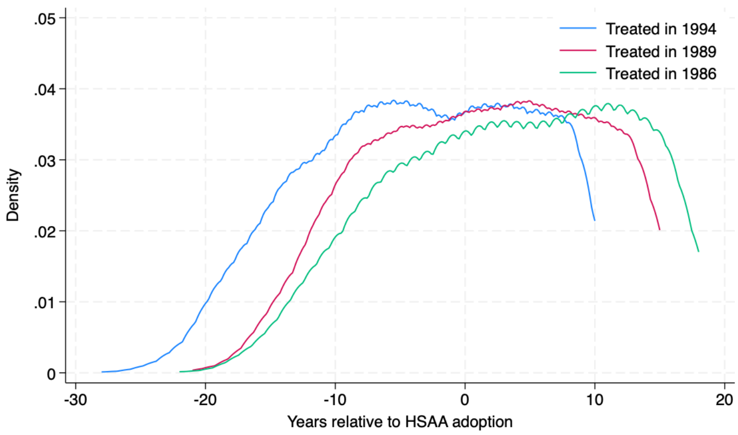
Notes: The figures plot caste composition and average age at marriage by states that adopted the HSAA in different years. The x-axis represents periods relative to policy implementation, pooling adjacent marital cohorts to increase precision.

Figure B6: 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.

Figure B7: 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.

B Identification of lower bounds on the ATT

Proposition B.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,

$$\begin{aligned} ATT^*(g,t) = {} & \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] \mathbb{P}(b_i = 1 \mid G_i = g) \\ & - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right] \mathbb{P}(b_i = 1 \mid G_i \in \mathcal{G}_{\text{comp}}) \\ & + \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0\right] \mathbb{P}(b_i = 0 \mid G_i = g) \\ & - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0\right] \mathbb{P}(b_i = 0 \mid G_i \in \mathcal{G}_{\text{comp}}) \end{aligned}$$

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

$$\begin{aligned} ATT^*(g,t) = {} & \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] \mathbb{P}(b_i = 1) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right] \mathbb{P}(b_i = 1) \\ & + \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0\right] \mathbb{P}(b_i = 0) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0\right] \mathbb{P}(b_i = 0) \\ = {} & \mathbb{P}(b_i = 1)\left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1\right]\right) \\ & + \mathbb{P}(b_i = 0)\left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0\right]\right) \\ = {} & \mathbb{P}(b_i = 1) ATT(g,t) \end{aligned}$$

where the last equality follows from (a) our independence assumption of b_i (b) b_i affects potential outcomes only through treatment. Hence, under parallel trends, the treatment effect for units with $b_i = 0$ is zero and the treatment effect for units with $b_i = 1$ is given by $ATT(g,t)$. 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,

$$\begin{aligned}
ATT^*(g,t) = {} & \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid G_i = g, X_i) \\
& - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid G_i \in \mathcal{G}_{\text{comp}}, X_i) \\
& + \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0, X_i\right] \mathbb{P}(b_i = 0 \mid G_i = g, X_i) \\
& - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0, X_i\right] \mathbb{P}(b_i = 0 \mid G_i \in \mathcal{G}_{\text{comp}}, X_i)
\end{aligned}$$

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

$$\begin{aligned}
& ATT^*(g,t) \\
& = \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid X_i) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1, X_i\right] \mathbb{P}(b_i = 1 \mid X_i) \\
& + \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0, X_i\right] \mathbb{P}(b_i = 0 \mid X_i) - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0, X_i\right] \mathbb{P}(b_i = 0 \mid X_i) \\
& = \mathbb{P}(b_i = 1 \mid X_i) \left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 1, X_i\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 1, X_i\right]\right) \\
& + \mathbb{P}(b_i = 0 \mid X_i) \left(\mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i = g, b_i = 0, X_i\right] - \mathbb{E}\left[Y_{i,t} - Y_{i,g-1} \mid G_i \in \mathcal{G}_{\text{comp}}, b_i = 0, X_i\right]\right) \\
& = \mathbb{P}(b_i = 1 \mid X_i) ATT(g,t) \\
& \leq ATT(g,t)
\end{aligned}$$

where the last equality follows from (a) our independence assumption of b_i (b) b_i affects potential outcomes only through treatment. Hence, under parallel trends, the treatment effect for units with $b_i = 0$ is zero and the treatment effect for units with $b_i = 1$ is given by $ATT(g,t)$. Since $\mathbb{P}(b_i = 1 \mid X_i) \in [0,1]$, we have that $\mid ATT^*(g,t) \mid \leq \mid ATT(g,t) \mid$. Hence, if the true treatment effect $ATT(g,t)$ is positive then $ATT^*(g,t) \leq ATT(g,t)$

Now, 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.

□

C Model: Comparative Statics

C.1 Proposition 1:

Proposition 1 *An increase in the variance of the preference shock σ_h (equivalently an increase in education) decreases the probability of the household P_h choosing to build a toilet which leads to an increase in the proportion of households P building a toilet.*

Proof: Compute the derivative of P_h with respect to σ_h:

$$\begin{aligned}
\frac{\partial P_h}{\partial \sigma_h} &= \frac{\partial P_h}{\partial\left(\frac{\Delta_h}{\sigma_h}\right)} \cdot \frac{\partial\left(\frac{\Delta_h}{\sigma_h}\right)}{\partial \sigma_h} \\
&= -\frac{\Delta_h}{\sigma_h^2} P_h(1 - P_h)
\end{aligned}$$

Since $P_h(1 - P_h) > 0$ because $0 < P_h < 1$ and $\sigma_h^2 > 0$, when $\Delta_h > 0$, we have $\frac{\partial P_h}{\partial \sigma_h} < 0$. Thus, when $\Delta_h > 0$, increasing σ_h decreases P_h. Assuming that the mass of households with $\Delta_h \leq 0$ is negligible, is sufficient to prove Proposition 1.

$$
\begin{aligned}
\frac{\partial P}{\partial \sigma_h} &= \int_{h\in\mathcal{H}} \frac{\partial P_h}{\partial \sigma_h} dF(h) \\
&= -\int_{h\in\mathcal{H}} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h) \\
&= -\left(\underbrace{\int_{\Delta_h>0} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h)}_{I_1} + \underbrace{\int_{\Delta_h\leq 0} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h)}_{I_2} \right)
\end{aligned}
$$

Assuming that the mass of households with $\Delta_h \leq 0$ i.e., the mass of households who are truly harmed by the presence of a toilet is negligible—implying $I_2 \approx 0$—is sufficient to prove Proposition 1.[59] Thus we have,

$$
\frac{\partial P}{\partial \sigma_h} \approx -\int_{h\in\mathcal{H}} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h) < 0
$$

Consequently, since $\frac{\partial \sigma_h}{\partial E_{w,h}} < 0$,

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

C.2 Proposition 2:

An increase in women's decision-making power $\theta_{w,h}$ increases P_h and thus increases P proportional to the gender gap in preferences $(\beta_w - \beta_m)$. The magnitude of this effect depends on the ratio Δ_h/σ_h. When the variance of the preference shocks σ_h is large (relative to Δ_h), any effect of increasing $\theta_{w,h}$ is attenuated as household decisions are dominated by the noise component.

Proof: Since $P_h = \frac{1}{1+exp(-\frac{\Delta_h}{\sigma_h})}$, we have $\frac{\partial P_h}{\partial \theta_w} = \frac{\partial P_h}{\partial \Delta_h} \cdot \frac{\partial \Delta_h}{\partial \theta_w}$

[59] 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.

$$
I_1 \equiv \int_{\Delta_h>0} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h) > |I_2| \equiv \left| \int_{\Delta_h\leq 0} \frac{\Delta_h}{\sigma_h^2} P_h(1-P_h)\, dF(h) \right|
$$

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

Compute $\frac{\partial \Delta_h}{\partial \theta_w}$:

$$\begin{aligned}\frac{\partial \Delta_h}{\partial \theta_w} &= \frac{\partial}{\partial \theta_w}\left(-\Lambda C_h + \theta_m \beta_m + \theta_w \beta_w\right)\\ &= \frac{\partial}{\partial \theta_w}\left(-\Lambda C_h + \beta_m + \theta_w(\beta_w - \beta_m)\right)\\ &= \beta_w - \beta_m > 0\end{aligned}$$

Compute $\frac{\partial P_h}{\partial \Delta_h}$:

$$\frac{\partial P_h}{\partial \Delta_h} = \frac{1}{\sigma_h} P_h(1 - P_h)$$

Thus:

$$\frac{\partial P_h}{\partial \theta_w} = \frac{1}{\sigma_h} P_h(1 - P_h)(\beta_w - \beta_m)$$

Observe that, without a gender gap in preferences $(\beta_w - \beta_m)$ for the good, increasing decision-making power does not change the probability of adoption of the good. Now, since $P_h(1 - P_h) > 0$, $\beta_w - \beta_m > 0$, $\sigma_h > 0$ we have , $\frac{\partial P_h}{\partial \theta_w} > 0$. Consequently, we have,

$$\begin{aligned}\frac{\partial P}{\partial \theta_w} &= \int_{h \in \mathcal{H}} \frac{\partial P_h}{\partial \theta_w} dF(h)\\ &= (\beta_w - \beta_m) \int_{h \in \mathcal{H}} \frac{1}{\sigma_h} P_h(1 - P_h)\, dF(h) > 0\end{aligned}$$

While the sign of $\frac{\partial P}{\partial \theta_w}$ is positive, because $\beta_{w,h} > \beta_{m,h}$, its magnitude depends on the ratio Δ_h/σ_h through $P_h(1 - P_h)/\sigma_h$. Note that since P_h itself depends on σ_h, the term $P_h(1 - P_h)/\sigma_h$ does not exhibit a simple $1/\sigma_h$ scaling. Specifically, when σ_h is large relative to Δ_h, the household's choice is dominated by the noise component, and the $1/\sigma_h$ factor attenuates the marginal effect of decision-making power. As σ_h decreases from this high-noise regime, the marginal effect of θ_w initially increases as the deterministic component Δ_h becomes more influential relative to the noise. This is the empirically relevant regime in our context where misperceptions and cultural frictions imply high σ_h relative to Δ_h, and reducing variance through education moves households into the range where decision-making power becomes more effective at the margin.

Note, the limiting behavior though not empirically interesting but for theoretical completeness, shows that for any fixed $\Delta_h > 0$, as $\sigma_h \to 0$, the household's adoption decision becomes near-deterministic ($P_h \to 1$), and $P_h(1 - P_h) \to 0$ at an exponential rate that dominates $1/\sigma_h$, so the marginal effect also vanishes.[60] This is intuitive because

[60] For fixed $\Delta_h > 0$, we have $P_h(1 - P_h) \sim \exp(-\Delta_h/\sigma_h)$ as $\sigma_h \to 0$, and hence $P_h(1 - P_h)/\sigma_h \to 0$. The knife-edge case $\Delta_h = 0$ is the only case where $P_h(1 - P_h)/\sigma_h \to \infty$ as $\sigma_h \to 0$.

when σ_h is very low, that is in absence of any noise, the household's decision is near-certain and thus increasing decision-making power does not change the probability of adoption.

C.3 Proposition 3:

Proposition 3: *The combined effect of decreasing σ_h and increasing $\theta_{w,h}$ on P_h and thus on P is positive.*

Proof: From Proposition 1, decreasing σ_h increases P_h when $\Delta_h > 0$ and consequently increases P as long as mass of households who are truly harmed from having a toilet (i.e., $\Delta_h \leq 0$) is negligible. From Proposition 2, increasing θ_w increases P_h. Since both effects on P_h are positive, simultaneously decreasing σ_h and increasing θ_w results in a combined positive effect on P_h and thus on P. Moreover, in the empirically relevant regime where σ_h is large relative to Δ_h, the reduction in σ_h moves households into a range where decision-making power becomes more effective at the margin, generating complementarity between these two channels.

D Model with cost shocks

Here we present a model where households are uncertain about the costs of a toilet and do not have any preference shocks. This is isomorphic to the model we present in the main text with preference shocks, resulting in similar propositions.

D.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:

$$U_{i,h}(X_h, T_h) = u(X_h) + \beta_{i,h}T_h, \quad i = \{m, w\}, \quad s.t. \quad X_h = Y_h - C_h \cdot T_h \tag{6}$$

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., $u(X) = X$.[61] Following existing literature that shows that women value toilet more than men, we assume that $\beta_{w,h} > \beta_{m,h}$ for all h.

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

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

$$C_h = {C_h}^* + \eta_h \quad \text{where} \quad \eta_h \sim \mathcal{N}(0, \sigma_h^2) \tag{7}$$

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:

$$\sigma_h^2 = f_h\left(E_{w,h}, E_{m,h}\right), \quad f_h'(.) < 0 \quad \forall h \tag{8}$$

Given that we find limited empirical evidence on the man's education (See Section 6.3), in the remaining discussion of the model, we focus on the case where there are exogenous changes to the woman's education level.[62]

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

$$\begin{aligned} U_h(T_h) &= \theta_{m,h} U_{m,h}(X_h, T_h) + \theta_{w,h} U_{w,h}(X_h, T_h) \\ &= \theta_{m,h} U_{m,h}(Y_h - C_h \cdot T_h, T_h) + \theta_{w,h} U_{w,h}(Y_h - C_h \cdot T_h, T_h) \end{aligned} \tag{9}$$

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.[63]

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 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:

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

[62] 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.

[63] 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.

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

The proportion of households building toilets in the population is:

$$P = \int_{h \in \mathcal{H}} \Pr(T_h = 1)\, dF(h), \tag{10}$$

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 D.2.1.

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 D.2.2.

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 D.2.3.

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.

Our 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, our 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.

D.2 Model with cost shocks: Comparative Statics

D.2.1 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):

$$\frac{\partial P}{\partial \sigma_h} = \int_{h \in \mathcal{H}} \frac{\partial \Pr(T_h = 1)}{\partial \sigma_h} \, dF(h) \tag{11}$$

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.[64] This implies that,

$$\frac{\partial P}{\partial \sigma_h} \approx -\int_{\Delta_h > 0} \phi\left(\frac{\Delta_h}{\sigma_h}\right) \cdot \frac{\Delta_h}{\sigma_h^2} \, dF(h) < 0 \tag{12}$$

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

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

the proportion P of households building toilets, proving Proposition 1.

D.2.2 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.

D.2.3 Proposition 3:

Simultaneously increasing women's education and decision-making power has a combined positive effect on the proportion P of households building toilets. Where the noise σ_h is large *(relative to Δ_h), reducing σ_h increases the marginal effect of decision-making power and vice versa.*

Proof: The cross-partial derivative of $\Pr(T_h = 1)$ with respect to θ_w and σ_h:

From Proposition 2, we have

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

Differentiating with respect to σ_h, and letting $z_h \equiv \frac{\Delta_h}{\sigma_h}$

$$\frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \, \partial \sigma_h} = (\beta_{w,h} - \beta_{m,h}) \cdot \frac{\partial}{\partial \sigma_h} \left[\frac{1}{\sigma_h} \varphi(z_h)\right]$$

Applying the product rule and using $\varphi'(z) = -z\varphi(z)$ and $\frac{\partial z_h}{\partial \sigma_h} = -\frac{\Delta_h}{\sigma_h^2} = -\frac{z_h}{\sigma_h}$

$$\begin{aligned}
\frac{\partial}{\partial \sigma_h} \left[\frac{1}{\sigma_h} \varphi(z_h)\right] &= \frac{1}{\sigma_h} \varphi'(z_h) \frac{\partial z_h}{\partial \sigma_h} + \varphi(z_h) \left(-\frac{1}{\sigma_h^2}\right) \\
&= \frac{1}{\sigma_h} \left(-z_h \varphi(z_h)\right) \left(-\frac{z_h}{\sigma_h}\right) - \frac{\varphi(z_h)}{\sigma_h^2} \\
&= \frac{z_h^2 \, \varphi(z_h)}{\sigma_h^2} - \frac{\varphi(z_h)}{\sigma_h^2} \\
&= \frac{\varphi(z_h)}{\sigma_h^2} \left(z_h^2 - 1\right)
\end{aligned}$$

Thus,

$$\frac{\partial^2 \Pr(T_h = 1)}{\partial \theta_w \, \partial \sigma_h} = (\beta_{w,h} - \beta_{m,h}) \cdot \frac{\varphi(z_h)}{\sigma_h^2} \left(z_h^2 - 1\right)$$

Integrating over all households, assuming the mass of households with $\Delta_h \leq 0$ is negligible, we have,

$$\frac{\partial^2 P}{\partial \theta_w \, \partial \sigma_h} = \int_{h \in \mathcal{H}} (\beta_{w,h} - \beta_{m,h}) \cdot \frac{\varphi(z_h)}{\sigma_h^2} (z_h^2 - 1) \, dF(h)$$

The sign of the cross-partial depends on $(z_h^2 - 1)$ where $z_h \equiv \Delta_h / \sigma_h$. Specifically, it depends on the distribution of z_h across households. In our empirical context where large misperceptions, cultural frictions, and informational barriers imply high σ_h relative to Δ_h the majority of households have $z_h < 1$. The integral is therefore dominated by the $z_h^2 < 1$ region, giving $\frac{\partial^2 P}{\partial \theta_w \partial \sigma_h} < 0$.

E Estimation Details

This appendix provides details on the doubly robust (henceforth, DR) estimator employed in our analysis, following Callaway & Sant'Anna (2021). The estimator combines outcome regression and propensity score weighting approaches to estimate group-time average treatment effects while accounting for treatment effect heterogeneity and selection in a potential outcomes framework. Let $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes for household i under treatment and control conditions respectively. Let $D_i(g,t)$ indicate treatment status for unit i in group g at marriage cohort t. The observed outcome Y_i relates to potential outcomes as: $Y_i = D_i(g,t)Y_i(1) + (1 - D_i(g,t))Y_i(0)$. The doubly robust estimator for the group-time average treatment effect $ATT(g,t)$ takes the form:

$$ATT(g,t) = \mathbb{E}\left[\frac{D_i(g,t)}{p(g,t \mid X_i)}\left(Y_i - m_0(t,X_i)\right) - \frac{(1-D_i)p(g,t \mid X_i)}{1-p(g,t \mid X_i)}\left(Y_i - m_0(t,X_i)\right)\right] \tag{13}$$

where $p(g,t \mid X_i) = \mathbb{P}(G_i = g, T_i = t \mid X_i)$ is the estimated propensity score. $m_0(t,X_i) = \mathbb{E}[Y_i \mid G_i = 0, T_i = t, X_i]$ is the estimated outcome model for the never-treated group. X_i represents the vector of covariates conditional on which we assume parallel trends to hold.

Even though we use the package provided by Callaway and Sant'Anna, for completeness we lay out the steps in which their estimator is implemented:

1. Estimate the propensity score $\hat{p}(g,t \mid X_i)$ using a logit model

2. Estimate the outcome model $\hat{m}_0(t,X_i)$ for never-treated units using linear regression

3. Estimate the sample analogue of the weighted difference using equation 13 which yields a consistent estimate of the group-wise $ATT(g,t) = \mathbb{E}[Y_i(1) - Y_i(0) \mid G_i = g, T_i = t]$

4. Calculate standard errors using wild bootstrap clustered at the state level

The estimator achieves the DR property i.e., consistency under either of two conditions:

$$\begin{aligned}
&\widehat{ATT}(g,t) \xrightarrow{p} ATT(g,t) \text{ if either:}\\
&\qquad\hat{p}(g,t \mid X_i) \xrightarrow{p} p(g,t \mid X_i) \text{ (propensity score consistency)}\\
&\qquad\text{or } \hat{m}_0(t,X_i) \xrightarrow{p} m_0(t,X_i) \text{ (potential outcome model consistency)}
\end{aligned} \tag{14}$$

This property is particularly valuable in our context for two reasons. First, the propensity score component accounts for selection into treatment based on potential outcomes. This captures systematic differences in counterfactual toilet adoption patterns between treatment and control groups, conditional on observables. Second, the estimator accommodates heterogeneous treatment effects defined in terms of potential outcomes. This allows for varying policy effectiveness across groups and states.