Pennies for Pads: Subsidized Menstrual Health Products and Adoption in India*

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Abstract

We estimate the effect of India's Suvidha program, which sells sanitary napkins at ₹1 per pad through the Jan Aushadhi Kendra pharmacies. Exploiting the staggered rollout of Kendras across subdistricts, we apply a doubly robust difference-in-differences estimator to nearly 500,000 women aged 15–24 from two National Family Health Survey rounds. Subsidized availability raises sanitary napkin use by 9.6 percentage points, a 16% gain over a 61% baseline. A falsification test using Kendra openings before subsidized pads existed is consistent with the subsidized product, rather than Kendra presence per se, as the operative channel. Effects are larger where a Kendra opens near hospitals or supermarkets, and at woman-owned outlets.

Keywords: menstrual health, sanitary napkins, price subsidies, health product adoption
JEL Codes: I12, I15, I18, H51, J16

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1 Introduction

Around the world, governments have made menstrual hygiene products a focus of public policy. Over the past two decades they have moved on two fronts to make commercial sanitary napkins more affordable: scrapping the sales and value-added taxes that classify the products as non-essential, and distributing them free or at subsidized prices.[1] These policies share a single premise—that lowering the price will raise use—yet whether subsidizing sanitary napkins actually increases their adoption has attracted remarkably little direct evidence. This paper provides such evidence: we estimate the effect of a deep, national price subsidy for sanitary napkins on whether women adopt them in place of cloth, exploiting India's *Jan Aushadhi–Suvidha* program.

Closing gaps in menstrual product use is widely viewed as consequential, and the most recent causal estimates are large. Free sanitary-pad dispensers in schools in India lowered girls' dropout and raised their attendance by 24 percentage points (Agarwal, Chia, and Ghosh, 2024); free pads cut school absenteeism by roughly half in Kenya (Benshaul-Tolonen, Zulaika, Nyothach, Oduor, Mason, Obor, Alexander, Laserson, and Phillips-Howard, 2021); a school program pairing products with stigma reduction raised learning by 0.15 standard deviations in Madagascar (Macours, Vera Rueda, and Webb, 2024); and disposable pads cut women's work absenteeism by about 24 percentage points in Burkina Faso (Krenz and Strulik, 2021). Product access is also linked to better reproductive health (Phillips-Howard et al., 2016).

Adoption nonetheless remains low where the products are unaffordable: the monthly cost of commercial pads is prohibitive for the tens of millions of Indians living on little more than $2 a day (The Borgen Project, 2022), and non-pecuniary stigma around menstruation compounds the barrier (Sivakami et al., 2019). Roughly two-thirds of Indian women aged 15–24 rely on cloth rather than commercial napkins, and pad-use gaps between above- and below-median wealth households exceed thirty percentage points (NFHS-4). This paper studies the question: whether subsidizing sanitary napkins and providing them through a network of generic pharmacies increase their adoption. We study this question through the *Jan Aushadhi–Suvidha* program, which sells subsidized *Suvidha* sanitary napkins at ₹1 per pad—against a prevailing commercial price of ₹3–

[1] Kenya removed value-added tax on menstrual products in 2004, the first country to do so; the United Kingdom abolished its tax in 2021; Scotland in 2021 became the first country to provide products free to anyone who needs them; and free or subsidized school distribution now operates in England, New Zealand, France, Kenya, and elsewhere. India exempted menstrual products from the Goods and Services Tax in 2018, adopted a national Menstrual Hygiene Policy for School-Going Girls in 2024, and in 2026 its Supreme Court held menstrual hygiene to be protected under Article 21 and directed all schools, government and private, to provide free sanitary napkins to girls in Classes 6–12.

8—through the government's nationwide *Jan Aushadhi Kendra* generic-pharmacy network (Press Information Bureau, 2019).

The *Jan Aushadhi – Suvidha* program differs from India's earlier menstrual-health interventions in its delivery channel. Past programs, which relied on schools and health-workers distributed free or token-priced napkins to enrolled adolescent girls and largely reached girls already in school. *Jan Aushadhi Kendras*, by contrast, operate on commercial principles—operators retain a margin, keep standard retail hours, and serve any walk-in customer of any age—and so reach populations that school-based distribution does not: out-of-school adolescents, young working women, and rural residents. The setting therefore offers a rare opportunity to estimate the adoption response to a deep, national price subsidy for a recurring necessity good delivered through existing commercial retail infrastructure.

We assemble a novel dataset linking administrative records on the universe of *Jan Aushadhi Kendras*—establishment dates, precise geolocations, and owner characteristics—with the National Family Health Survey, which provides geocoded data on menstrual hygiene practices for 489,379 women aged 15–24 across two rounds bracketing the program. Identification exploits the staggered rollout of *Kendras* across roughly 7,000 subdistricts combined with the national availability of subsidized pads at ₹1. Because fieldwork within each round is spread across multiple years and the date on which subsidized pads reach a subdistrict varies with the staggered rollout, women are observed across a wide range of times relative to local availability. We estimate the adoption effect with the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020), conditioning on the observable characteristics that predict where and when *Kendras* open, and we use the estimator of Callaway and Sant'Anna (2021) to trace dynamic effects across event time.

Our central finding is that access to subsidized napkins through *Kendras* raises sanitary napkin adoption by 9.6 percentage points, a 16% increase over the 61% baseline. Event-study estimates show adoption rising sharply in the year subsidized pads become available, with no comparable movement beforehand. The response is sizable but far from complete, consistent with the substantial demand responses to price documented for other inexpensive welfare-enhancing products, take-up of which stays incomplete even at a zero price (Cohen and Dupas, 2010; Dupas, 2014).

These conclusions withstand a battery of robustness checks. One supporting check, available because the *Kendra* network long predates the subsidized product, is a falsification test: *Kendra* openings in the years before subsidized pads existed produce no adoption response, consistent with the subsidy rather than the channels—health information, retail density, or correlated local development—through which pharmacy presence might independently have raised pad use. The estimate survives further checks. It is stable when states running concurrent sanitary-napkin programs are excluded one at a time, and remains positive statistically significant when all such states are dropped together, so contemporaneous interventions do not account for it. A Rambachan and

Roth (2023) sensitivity analysis shows the effect persists under violations of parallel trends as large as three-quarters of the largest pre-treatment deviation. And the estimated effect rises monotonically with exposure duration—as expected if the subsidy, rather than some coincident shock, drives adoption.

The paper contributes to four strands of literature. Most directly, it speaks to a growing policy and research interest in subsidizing menstrual products. A large experimental literature provides products directly and traces their effects on schooling, health, and labor, with notably mixed results. In Nepal, Oster and Thornton (2011) find that menstruation costs girls under half a day of school per year and that free sanitary products do not close that small gap, and the Kenyan feasibility trial of Phillips-Howard, Nyothach, ter Kuile, Omoto, Wang, Zeh, Onyango, Mason, Alexander, Odhiambo, Eleveld, Mohammed, van Eijk, Edwards, Vulule, Faragher, and Laserson (2016) likewise detects no effect of cups or pads on school dropout; where effects do emerge, they tend to run through attendance and health, as in the sizable reductions in menstruation-related absenteeism that Benshaul-Tolonen, Zulaika, Nyothach, Oduor, Mason, Obor, Alexander, Laserson, and Phillips-Howard (2021) estimate from pad provision in western Kenya, the attendance gains reported by the quasi-randomized pad-and-puberty-education trial of Montgomery, Hennegan, Dolan, Wu, Steinfield, and Scott (2016) in Uganda, and the lower dropout that Agarwal, Chia, and Ghosh (2024) link to school pad provision.

Second, a complementary body of work shifts attention from access to information and social norms: Castro and Mang (2024) show that structured group discussions reduce menstrual stigma and raise both willingness to pay for and adoption of menstrual products, Czura, Menzel, and Miotto (2024) find among Bangladeshi garment workers that free pads and hygiene information improve menstrual practices and health, and Macours, Vera Rueda, and Webb (2024), in a field experiment across 140 Malagasy schools, show that bundling sanitation, products, and teacher-led sensitization shifts hygiene behavior and reduces stigma. We build on this work by estimating the adoption response to subsidized provision through an existing commercial-retail channel at national scale. Third, far less is known about the question these studies presuppose—whether lowering the price of menstrual products raises their use. The closest causal evidence comes from "tampon tax" reforms in high-income countries, where studies using retail and household scanner data find that VAT cuts pass through fully—often more than fully—into consumer prices, but that demand is price-inelastic: quantities move little, and where households respond they tend to trade up to higher-quality products rather than buy more, with lower-income households gaining most from the price decline (Büttner, Hechtner, and Madzharova, 2025; Frey and Haucap, 2024; Kinnl and Wohak, 2026). Yet these studies observe aggregate market sales in settings where pad use is already near-universal, leaving open whether subsidized provision draws in prior non-users—the margin we estimate, using a large national subsidy in a setting where baseline use is far from universal.

Fourth, and more broadly, the paper speaks to the provision and pricing of inexpensive health products. Demand for such goods is typically highly price-sensitive, with even small fees sharply cutting take-up of one-off or preventive products like bednets and water treatment (Cohen and Dupas, 2010; Kremer and Miguel, 2007; Ashraf, Berry, and Shapiro, 2010; Dupas, 2014; Dupas and Miguel, 2017); menstrual products extend the question to a recurring necessity, where the relevant margin is sustained repeat use. Because a program's delivery channel also shapes its reach (Muralidharan, Niehaus, and Sukhtankar, 2016; Ashraf, Bandiera, and Jack, 2014; Duflo, Dupas, and Kremer, 2015; Finkelstein and Notowidigdo, 2019), our setting is doubly distinctive: rather than the schools and frontline health workers that carried India's earlier menstrual programs—and reached mainly enrolled girls—the subsidy travels through a commercial-retail network serving any walk-in customer at national scale, reaching the out-of-school adolescents, young working women, and rural residents the earlier model missed.

These findings carry important policy implications. First, the price cut drives adoption, but its impact is amplified where the surrounding context lowers the non-pecuniary cost of obtaining a pad—where the purchase is normalized as a health good sold alongside medicines and near clinics, and where a same-gender transaction eases discomfort—so where outlets are placed and who runs them shape the return, not the price alone. Second, this heterogeneity creates an efficiency–equity tension for the planned scale-up to 25,000 outlets: adoption responds most strongly where unmet need is smallest, so maximizing adoption per rupee would divert the subsidy from the most deprived areas. Third, these adoption gains come at low public cost. Routed through a pre-existing commercial-retail network, the model should travel to other low- and middle-income settings that share prior retail infrastructure, operator margins that sustain stocking, and low-transaction-cost walk-in access.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting; Section 3 the data; Section 4 the empirical strategy; Section 5 the results and mechanisms; Section 6 robustness; and Section 7 concludes.

2 Institutional Background

India has pursued menstrual-hygiene policy through several channels over the past fifteen years. We situate the *Jan Aushadhi–Suvidha* program against the earlier school- and health-worker-based programs it departs from, the generic-pharmacy network through which it is delivered, and the contemporaneous state schemes that operated alongside it.

2.1 Earlier Menstrual-Hygiene Programs

Since 2011, the central government's Menstrual Hygiene Scheme and the 2014 *Rashtriya Kishor Swasthya Karyakram*, together with state schemes in Kerala, Odisha, Maharashtra, and elsewhere, distributed free or token-priced napkins to enrolled adolescent girls through schools and front-line health workers (National Health Mission, 2011, 2014). Evaluations document two recurring weaknesses of this model. It reached girls already in school while systematically excluding out-of-school girls and urban slum residents, and it suffered chronic stock-outs that eroded user confidence (Desai, 2017; US AID, 2022). These limitations—a narrow target population and unreliable supply—motivate interest in a delivery channel that serves any buyer through ordinary retail.

2.2 The *Jan Aushadhi Kendra* Network

Jan Aushadhi Kendras are government-sponsored generic pharmacies that have operated since 2008, roughly a decade before subsidized napkins joined their product line. They run on commercial principles rather than through public-sector frontline workers: operators retain a 20% margin on sales and receive enhanced support in aspirational districts and for women, SC/ST, and disabled entrepreneurs, who also pay no application fee. Because a *Kendra* serves any walk-in customer, it reaches the out-of-school adolescents, young working women, and rural residents that school- and health-worker-based programs miss.

Placement is administratively governed rather than random. Operators apply to the Department of Pharmaceuticals and undergo a verification and approval process that introduces variable delays in when an outlet opens, and a distance rule requires a new *Kendra* to maintain a minimum separation—ordinarily one kilometer, 1.5 kilometers in some areas—from an existing one. Openings concentrate in more developed subdistricts, a selection pattern we document in Section 3 and accommodate through the doubly robust estimator of Section 4; the idiosyncratic variation in establishment timing is what we exploit for identification. The network expanded sharply over the study period (Figure A1).

2.3 The *Suvidha* Sanitary Napkin

Suvidha is a government-branded, steeply subsidized sanitary napkin sold over the counter at *Kendras* alongside other health commodities—a setting in which the purchase may carry less stigma than collection through a specialized distribution point. Table A1 lists the product portfolio; the core product, regular-size with wings, retails at ₹1 per pad (₹4 for a pack of four), between one-third and one-eighth of the prevailing commercial price of ₹3–8 per pad for comparable products (Press Information Bureau, 2020).

The deep subsidy that defines our treatment took effect on August 27, 2019, when the government cut the retail price to ₹1 per pad, fulfilling a 2019 general-election commitment (Press Information Bureau, 2019). *Suvidha* had been available before this date at a higher, only lightly subsidized price but reached consumers in small volume; monthly sales rose several-fold once the ₹1 price took effect (Press Information Bureau, 2020). We therefore date treatment to the August 2019 price cut, the point at which subsidized napkins became widely available, and confirm in Section 6 that the estimate is robust to excluding the earlier, low-volume period.

2.4 Concurrent State Programs

Several states operated their own menstrual-hygiene schemes during our study window—among them Kerala's *She Pad*, Odisha's *Khushi*, Maharashtra's *Asmita Yojana*, and free-distribution programs in Tamil Nadu, Karnataka, Haryana, Punjab, Rajasthan, and Andhra Pradesh. Like India's earlier interventions, these distribute napkins free or nearly free through schools and *anganwadi* centers to targeted groups, primarily enrolled adolescent girls, and so differ from *Suvidha*'s sale to any buyer through retail outlets. Appendix Table A6 summarizes each program's timing, target population, and delivery channel. Because such schemes also raise pad use, we verify in Section 6.6 that they do not account for our results, excluding the affected states one at a time and jointly.

3 Data and Measurement

Our empirical analysis integrates four primary data sources: (i) administrative records on the universe of *Jan Aushadhi Kendras*, (ii) women's health outcomes from the National Family Health Survey (NFHS), (iii) spatial amenity measures from the Geoapify Places API, and (iv) baseline subdistrict characteristics from the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG).

3.1 Baseline Socioeconomic Characteristics

We construct pre-policy subdistrict characteristics from SHRUG (Asher, Lunt, Matsuura, and Novosad, 2021), which integrates the 2011 Population Census Abstract, 2011 Village Directory, and 2012 VIIRS nighttime lights. Female literacy rate, female labor force participation, sanitation infrastructure, school availability, and nighttime light intensity predate *Kendra* establishment and capture the development gradient along which the program expanded.

Table A2 documents substantial baseline heterogeneity. Sixty-eight percent of communities lack organized waste collection; the Total Sanitation Campaign covers roughly one-third of subdistricts. Female literacy averages 52%; female labor force participation, 33%. Nighttime light intensity ranges from 0 to 111 (mean 1.11), reflecting the rural-urban development divide. These covariates absorb confounding variation correlated with *Kendra* placement in our empirical strategy.

3.2 Spatial Amenity Data

Using the Geoapify Places API, we identify amenities within a five-minute driving radius of each *Kendra*: hospitals (potential sources of health information and referrals), non-generic pharmacies (alternative sources for menstrual products), and supermarkets (indicators of retail development). These measures enable our heterogeneity analysis of infrastructure complementarities in Section 5. Table A3 summarizes the share of *Kendras* that co-locate with these amenities, which we discuss below.

3.3 Administrative Data on *Jan Aushadhi Kendras*

Administrative records from the Department of Pharmaceuticals document the precise location, establishment date, and owner name for all *Jan Aushadhi Kendras*. We infer owner gender through a rule-based classification algorithm implemented using a large language model (Claude 3.5) and validated against a publicly available dataset of Indian female names (Bejda, 2015).

Table A3 summarizes *Kendra* characteristics: 25% are woman-owned, and infrastructure co-location is substantial (63% near hospitals, 26% near existing pharmacies, 37% near supermarkets).

Table A4 documents the selection pattern: *Suvidha* becomes available earlier in subdistricts with higher female literacy, higher nighttime lights, and lower female labor force participation (a counterintuitive pattern we interpret as reflecting India's paradoxical female labor supply, where higher participation captures low-wage informal work rather than economic advancement). This selection motivates our conditional parallel trends approach (Section 4).

3.4 Health Survey Data

The NFHS administers menstrual hygiene modules to women aged 15–24, eliciting information on use of cloth, sanitary napkins, locally-produced pads, tampons, and cups (International Institute for Population Sciences and ICF, 2017, 2021). We use two rounds bracketing the *Suvidha* launch: NFHS-4 (January 2015–December 2016) and NFHS-5 (June 2019–May 2021). The survey's two-stage sampling design and geocoding lead us to define treatment at the subdistrict level (The DHS Program, 2023).

Table A5 documents aggregate shifts in menstrual hygiene practices between rounds: sanitary napkin usage rose from 42% in NFHS-4 to 63% in NFHS-5, a striking aggregate trend that our empirical strategy decomposes into the causal *Suvidha* effect and contemporaneous secular change. Table I further shows a steep socioeconomic gradient in who uses sanitary napkins: relative to non-users, pad users are 35 percentage points more likely to be above-median wealth (62% versus 27%) and 29 percentage points more likely to be above-median educated (47% versus 19%), consistent with affordability shaping commercial pad use, though wealth here also tracks education and urban residence.

TABLE I: Descriptive Statistics by Treatment Status and Sanitary Napkin Usage (NFHS Rounds 4 and 5)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mean	Kendra ever opens			Woman uses pad		
		No	Yes	Difference	No	Yes	Difference
Household							
Religion: Hindu	0.74	0.75	0.74	-0.01^{***}	0.76	0.73	-0.03^{***}
Religion: Muslim	0.15	0.12	0.17	0.05^{***}	0.17	0.14	-0.03^{***}
Caste: SC	0.20	0.18	0.21	0.03^{***}	0.20	0.19	-0.01^{***}
Caste: ST	0.18	0.26	0.12	-0.13^{***}	0.21	0.16	-0.05^{***}
Caste: OBC	0.40	0.37	0.41	0.04^{***}	0.41	0.39	-0.02^{***}
Above-median Wealth	0.50	0.38	0.59	0.21^{***}	0.27	0.62	0.35^{***}
Neighborhood Urban	0.25	0.13	0.34	0.21^{***}	0.14	0.31	0.17^{***}
Individual							
Age: 20-24	0.49	0.49	0.50	0.01^{***}	0.50	0.49	-0.01^{***}
Above-median Education	0.37	0.31	0.42	0.11^{***}	0.19	0.47	0.29^{***}
Menstruation							
Menarche: 13 or later	0.81	0.80	0.83	0.03^{***}	0.82	0.81	-0.00
Uses sanitary napkins	0.65	0.59	0.68	0.09^{***}			
Kendra ever opens	0.58				0.52	0.62	0.10^{***}
Observations							489,379

Note: Columns 2–4 split the sample by whether a *Kendra* ever opens in the subdistrict; columns 5–7 by whether the woman uses sanitary napkins. Difference columns report the coefficient from a regression of the row variable on the split indicator, with robust standard errors; *, **, *** denote 10%, 5%, 1% significance. Cells are left blank where the row variable is itself the column-split variable.
Data source: NFHS Rounds 4 and 5; Department of Pharmaceuticals; SHRUG.

Table I documents the cross-sectional selection into treatment: women in subdistricts where *Suvidha* ever becomes available differ systematically from those in never-treated subdistricts on wealth, urban residence, education, and caste. Addressing this selection motivates our doubly robust estimator.

NFHS data link at the subdistrict level to administrative *Kendra* records and SHRUG baseline characteristics, yielding a sample of 489,379 women aged 15–24.

4 Empirical Strategy

We exploit two sources of variation: the staggered rollout of *Kendras* across India's roughly 7,000 subdistricts and the national date, August 27, 2019, on which subsidized pads reached ₹1. Although the NFHS comprises only two rounds, the design is not a single before/after comparison around a common date. Fieldwork within each round is spread over several years and local availability dates are staggered, so women are observed across a wide range of times relative to local availability rather than at two fixed points. Our headline estimate comes from the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020), which conditions on the covariates that predict *Kendra* placement; we use the estimator of Callaway and Sant'Anna (2021) to trace dynamic effects (Section 5). Both estimate the effect from comparisons of treated against never- and not-yet-treated units rather than from a pooled two-way fixed-effects regression, avoiding the negative-weighting biases that arise under treatment-effect heterogeneity (Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2024).

4.1 Specification

Consider woman i in subdistrict s surveyed at time t, and let $Y_{ist} \in \{0,1\}$ indicate sanitary napkin use. The treatment indicator D_s equals one if a *Kendra* ever opens in s; the post indicator T_{ist} equals one for interviews conducted after subsidized pads become available locally—in treated subdistricts, after a *Kendra* has opened and the ₹1 price has taken effect (August 27, 2019); in never-treated subdistricts, after August 27, 2019. A woman interviewed before local availability is coded pre-treatment regardless of *Kendra* presence. Our parameter of interest is the average treatment effect on the treated,

$$\tau = \mathbb{E}[Y_{i,\text{post}}(1) - Y_{i,\text{post}}(0) \mid D_s = 1]. \tag{1}$$

We estimate τ with the doubly robust estimator of Sant'Anna and Zhao (2020) for repeated cross-sections, which combines a propensity-score model and outcome regressions, each conditioning on a covariate vector X_{ist}: individual demographics (religion, caste, urban residence, age), district and interview month-by-year fixed effects, and baseline subdistrict characteristics (sanitation infrastructure, anganwadi and government-school availability, female literacy and workforce shares, and nighttime lights). The propensity score

$$\Pr(D_s = 1 \mid X_{ist}) = \frac{\exp(X'_{ist}\gamma)}{1 + \exp(X'_{ist}\gamma)} \tag{2}$$

is estimated by inverse probability tilting (Graham, de Xavier Pinto, and Egel, 2012) and the outcome regressions by weighted least squares, the combination that delivers double robustness and local efficiency (Sant'Anna and Zhao, 2020): the estimator is consistent if *either* the propensity score or the outcome regression is correctly specified. These enter the doubly robust moment for repeated cross-sections,

$$\begin{aligned}\widehat{\tau}^{\mathrm{DR}} =& \mathbb{E}_n\left[\frac{T}{\mathbb{E}_n[T]}\cdot\frac{D}{\mathbb{E}_n[D]}\left(Y-\widehat{\mu}_{0,1}(X)\right)\right]-\mathbb{E}_n\left[\frac{1-T}{\mathbb{E}_n[1-T]}\cdot\frac{D}{\mathbb{E}_n[D]}\left(Y-\widehat{\mu}_{0,0}(X)\right)\right]\\ &-\mathbb{E}_n\left[\frac{T}{\mathbb{E}_n[T]}\cdot\frac{\widehat{p}(X)(1-D)}{(1-\widehat{p}(X))\,\mathbb{E}_n\left[\frac{\widehat{p}(X)(1-D)}{1-\widehat{p}(X)}\right]}\left(Y-\widehat{\mu}_{0,1}(X)\right)\right]\\ &+\mathbb{E}_n\left[\frac{1-T}{\mathbb{E}_n[1-T]}\cdot\frac{\widehat{p}(X)(1-D)}{(1-\widehat{p}(X))\,\mathbb{E}_n\left[\frac{\widehat{p}(X)(1-D)}{1-\widehat{p}(X)}\right]}\left(Y-\widehat{\mu}_{0,0}(X)\right)\right],\end{aligned} \tag{3}$$

which differences the treated-group residuals across periods and reweights the control-group residuals by the propensity-score odds.

Interview month-by-year fixed effects play a specific role. The COVID-19 suspension of NFHS-5 fieldwork (Section 6.4) pushed a disproportionate share of treated-subdistrict interviews into the later, resumed window, so without controls for interview timing the treatment indicator would partly capture longer accumulated exposure rather than the subsidy; conditioning on month-by-year absorbs this (Appendix Figure A2).

4.2 Identification

Identification rests on conditional parallel trends: absent *Suvidha*, adoption in subdistricts that gain access would have followed the same path as in those that do not, conditional on X_{ist}. The threat is that *Kendra* placement is not random—operators apply to the Department of Pharmaceuticals, and outlets concentrate in more developed subdistricts with higher female literacy and nighttime lights (Table A4). Three features of the setting make the design credible.

First, selection operates largely on observables. The covariates that predict placement are measured and conditioned on, so the doubly robust estimator compares treated subdistricts to controls with similar literacy, infrastructure, and development—precisely the margin on which they differ.

Second, the residual variation in *when* access arrives is administratively generated. Openings proceed through an application and verification process, are constrained by a minimum-distance rule between outlets, and are subject to approval delays that introduce idiosyncratic variation in establishment timing—plausibly unrelated, conditional on the placement covariates, to a subdistrict's underlying adoption trajectory.

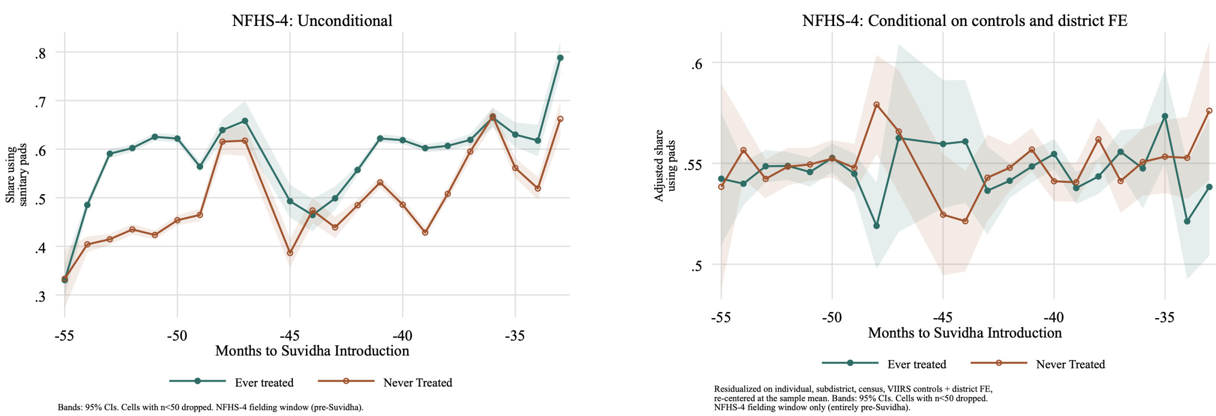
Third, the *Kendra* network predates the subsidized product by roughly a decade, furnishing a built-in placebo: we hold the network fixed and ask whether *Kendra* entry alone, before any ₹1 pad, moved adoption. It does not (Panel (b) of Figure II). This supports the design, because the confounds conditioning cannot rule out—a subdistrict already trending upward, anticipatory siting ahead of rising demand, or coincident local development—would each produce an adoption response at *Kendra* opening, and none appears.

Two further pieces of evidence support the assumption. Within the pre-*Suvidha* NFHS-4 window the level gap between ever- and never-treated subdistricts disappears once we condition, leaving the two series statistically indistinguishable (Figure I); and the event study, which we read as a diagnostic against differential pre-trends rather than a formal pre-test (Roth, 2022; Kahn-Lang and Lang, 2020), shows no movement before local availability. For violations beyond the reach of any pre-period check, a Rambachan and Roth (2023) sensitivity analysis quantifies how large a departure from parallel trends would have to be to overturn the estimate. We take up the two remaining concerns—concurrent state programs and the COVID-19 fieldwork interruption—in Sections 6.6 and 6.4.

FIGURE I: Pre-*Suvidha* Pad-Use Trends by Treatment Status, NFHS-4 Fielding Window

(a) Unconditional

(b) Conditional on controls and district fixed effects



Note: Both panels plot the share of women aged 15–24 using sanitary pads against months to *Suvidha* introduction (the ₹1 date), separately for subdistricts that ever versus never receive a *Kendra*, using only NFHS-4 interviews—a window lying entirely before *Suvidha* became available. Panel (a) plots raw shares. Panel (b) residualizes pad use on the full set of individual, subdistrict, census, and VIIRS controls and district fixed effects, re-centered at the sample mean. Cells with fewer than 50 observations are dropped. Shaded bands are 95% confidence intervals.

Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Round 4; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

5 Results

This section presents our findings on the impact of subsidized sanitary napkin provision through the *Jan Aushadhi Kendra* network. We begin by documenting the program's effect on menstrual hygiene product adoption, showing that access to the highly subsidized *Suvidha* pads increases sanitary napkin usage by 9.6 percentage points—a 16% improvement from baseline. We complement the headline estimate with event-study evidence, which shows adoption rising sharply in the year *Suvidha* becomes available with no comparable movement beforehand.

We then explore heterogeneity in treatment effects across supply-side and institutional characteristics, revealing that impacts are largest in areas with complementary healthcare and retail infrastructure and when outlets are woman-owned. We also show that the effect builds over the first six to eight months of *Suvidha* availability rather than appearing immediately, consistent with gradual diffusion.

5.1 Effect on Menstrual Health Management

Table II presents estimates of the impact of *Suvidha* availability through *Jan Aushadhi Kendras* on sanitary napkin adoption. We implement the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020), as described in Section 4, exploiting the staggered rollout of *Kendras* across subdistricts. Identification draws on two sources of variation: a cross-sectional contrast between subdistricts that ever receive a *Kendra* and those that never do, and a temporal contrast based on whether a woman is interviewed before or after *Suvidha* pads become available to her. In treated subdistricts, this temporal contrast is defined by the subdistrict's own *Kendra* opening date; in never-treated subdistricts, which have no opening date of their own, it is anchored to the national launch date, which provides the natural counterfactual clock. The design thus mirrors a canonical 2×2 difference-in-differences, with the treatment date varying across subdistricts. We also estimate group-time average treatment effects using the doubly robust estimator of Callaway and Sant'Anna (2021), presented below.

The specification includes individual-level characteristics (religion, caste, urban/rural status, and age), and controls for baseline subdistrict characteristics that may predict both *Kendra* placement and sanitary napkin usage: female literacy rates, female labor force participation, nighttime luminosity, and sanitation infrastructure coverage. We also control for district and survey month-year fixed effects.

Table II presents the impact of *Suvidha* availability on menstrual hygiene product choice for 489,379 women aged 15 to 24 from NFHS-4 and NFHS-5; only this age group was administered the menstruation module in the NFHS. The program increased sanitary napkin usage by 9.6 percentage points (SE = 0.01) from a baseline mean of 61 percent, a 16% improvement. The baseline mean is the average sanitary napkin usage among comparison observations: women in subdistricts where *Suvidha* pads never become available, and women interviewed before *Suvidha* pads become available in their subdistrict.

TABLE II: Treatment Effect of Provision of Subsidized Suvidha Sanitary Napkins via Jan Aushadhi *Kendras* on Period Poverty

	(1)
$\hat{\tau}$	0.096
	(0.011)
Mean	0.606
Obs	489,379

Note: This table reports the estimate from Sant'Anna and Zhao (2020)'s doubly robust difference-in-differences framework. Covariates include baseline sub-district aggregates of village amenities, female literacy, female labor force participation, and mean nightlights, as well as individual-level characteristics (religion, caste, urban/rural status, and age), district fixed effects, and survey month-year fixed effects. Standard errors, clustered at the subdistrict level, are reported in parentheses. *Data sources:* Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

Interpreting the magnitude. The outcome is the use of any sanitary napkin, not *Suvidha* specifically: the NFHS records whether a woman uses sanitary napkins but not the brand, so the estimate captures the effect of subsidized availability on overall pad adoption rather than on *Suvidha* purchases alone. Read this way, a 9.6 percentage point increase need not reflect a one-for-one substitution of *Suvidha* pads for cloth. The more plausible reading is that access to a deeply subsidized pad induces women to try sanitary napkins and to keep using them—whether the *Suvidha* pad itself or a commercial alternative—so that the subsidized product acts as a catalyst for pad adoption more broadly. Two patterns in the results below are consistent with this interpretation. First, effects are larger, not smaller, where a *Kendra* opens near hospitals, pharmacies, and supermarkets (Table III)—settings where commercial pads are also readily available, so that uptake can spill onto non-*Suvidha* products. Second, the estimate falls when states with long-running concurrent distribution schemes are removed (Section 6.6), indicating *larger* effects where free or subsidized supply coexists with *Suvidha*—consistent with the two sources reinforcing one another in raising overall pad use rather than with confounding. We return to both patterns when discussing mechanisms below.

Dynamic effects. The spread of interview dates within each round, combined with the staggered dates at which *Suvidha* reaches each subdistrict, generates event-time variation spanning several years before and after local availability. Appendix Figure A3 plots the distribution of interview timing relative to *Kendra* establishment and *Suvidha* availability, confirming that the estimation sample contains women interviewed both before and after each reference date. We present the resulting dynamic estimates in Section 6 alongside the falsification and exclusion plots (Figure II). In brief, adoption rises sharply in the year *Suvidha* becomes available—an event-time-zero estimate of 0.092 (SE = 0.013), close to the headline—while every pre-treatment coefficient is non-positive.

5.2 Heterogeneous Effects

We next examine whether program impacts vary with (i) local supply-side infrastructure and (ii) the gender of the *Kendra* owner. Supply-side characteristics capture potential complementarities between the *Kendra* and existing health and retail infrastructure, while owner gender captures whether the comfort of transacting with a woman shopkeeper shapes adoption.

To characterize heterogeneous effects, we estimate the baseline specification separately across mutually exclusive subsamples defined by the characteristic of interest and compare the resulting treatment effects. For each characteristic we partition treated subdistricts into two mutually exclusive regimes—for example, subdistricts where a *Kendra* opened within five minutes' driving distance of a hospital versus those where it opened without a nearby hospital—and estimate the baseline specification on each regime against a common comparison group of never-treated subdistricts. Treatment is whether a subdistrict ever experienced a *Kendra* opening of the relevant type, and the post indicator equals one for women interviewed after the corresponding opening; each regime excludes the complementary opening type. Figure A4 illustrates the construction for hospital proximity, and the pharmacy, supermarket, and owner-gender cuts follow the same template.

Health and Retail Infrastructure Table III reports the estimates. Point estimates are larger where *Kendras* co-locate with existing infrastructure: 12.8 versus 7.9 percentage points near a hospital ($t = 2.10$ on the difference), 8.9 versus 6.2 percentage points near a non-generic pharmacy ($t = 1.18$), and 15.5 versus 5.0 percentage points near a supermarket ($t = 4.56$). The hospital and supermarket gaps are statistically distinguishable, whereas the pharmacy gap is not. Adoption thus responds most strongly where a *Kendra* opens amid existing health and retail infrastructure, and least in underserved areas—a pattern we return to when discussing mechanisms below.

Gender of *Kendra* Owner Columns 7–8 examine how ownership structure affects outcomes. Woman-owned *Kendras* raise adoption by 11.0 percentage points, versus 5.0 percentage points for male-owned outlets—a 6.0 percentage point differential ($t = 2.41$). Three channels could contribute to this gap. First, woman-owned *Kendras* may locate where underlying gender norms are more favorable. Second, because the program offers explicit incentives to women entrepreneurs,

Table III: Treatment Effect of Provision of Subsidized Suvidha Sanitary Napkins via Jan Aushadhi *Kendras* on Period Poverty by Local Amenities and Owner Gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hospital		Pharmacy		Supermarket		Woman-owned	
	Yes	No	Yes	No	Yes	No	Yes	No
$\hat{\tau}$	0.128	0.079	0.089	0.062	0.155	0.050	0.110	0.050
	(0.012)	(0.020)	(0.019)	(0.013)	(0.019)	(0.014)	(0.021)	(0.013)
t		2.097		1.179		4.555		2.409
Mean	0.618	0.583	0.628	0.585	0.627	0.582	0.627	0.586
Obs	416,279	277,614	312,521	381,372	334,551	359,342	336,168	357,725

Note: This table reports estimates from Sant'Anna and Zhao (2020)'s doubly robust difference-in-differences framework. Covariates include baseline sub-district aggregates of village amenities, female literacy, female labor force participation, and mean nightlights, as well as individual-level characteristics (religion, caste, urban/rural status, and age), district fixed effects, and survey month-year fixed effects. Standard errors, clustered at the subdistrict level, are reported in parentheses. The t statistic reported in each pair tests the equality of the two subsample treatment effects.

Columns (1) and (2) compare outcomes when a *Kendra* opened near versus not near a hospital. Columns (3) and (4) present analogous comparisons for a non-generic pharmacy; columns (5) and (6), for a supermarket; and columns (7) and (8), for whether the *Kendra* is woman-owned.

Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights. Geoapify Places API; Claude AI; dataset of Indian female names (Bejda, 2015).

woman-owned status may proxy for a bundle of program support that improves outlet operations. Third, the gender of the shopkeeper may shape the comfort of purchasing an intimate product, so that placing women behind the counter lowers a social barrier the subsidy alone does not address. This last channel is consistent with evidence that patient–provider gender concordance shapes women's health-seeking (Cabral and Dillender, 2024) and with qualitative accounts of discomfort around menstrual purchases (Castro and Mang, 2024).

Effects by Time Since Availability A final source of heterogeneity is exposure duration: how long *Suvidha* pads have been available to a woman at the time she is interviewed. Table IV re-estimates the baseline doubly robust specification while progressively widening the post-treatment window, restricting treated post-period observations to women interviewed within 60, 120, 240, and 360 days of *Suvidha* becoming available in their subdistrict. The estimated effect rises monotonically with exposure duration, from 3.1 percentage points within 60 days to 5.3 percentage points within 120 days, 10.3 within 240 days, and 10.4 within 360 days, each estimated precisely. The effect builds over roughly the first six to eight months of availability and then levels off; the 240- and 360-day estimates sit marginally above the full-sample headline of 9.6 percentage points in Table II because the full sample additionally includes the longest-exposure women—interviewed latest, and thus deepest into the COVID-19 fieldwork window—among whom the estimated effect is somewhat attenuated (Section 6.4). This ramp-up is consistent with gradual diffusion of information about the subsidized product and with adoption occurring at successive menstrual cycles rather than immediately upon availability, a pattern in keeping with the social-learning dynamics documented for other new technologies (BenYishay and Mobarak, 2019; Conley and Udry, 2010).

TABLE IV: Treatment Effect of Subsidized Suvidha Sanitary Napkins by Time Since Availability

	(1)	(2)	(3)	(4)
	60	120	240	360
$\hat{\tau}$	0.031	0.053	0.103	0.104
	(0.012)	(0.012)	(0.011)	(0.011)
Mean	0.606	0.606	0.606	0.606
Obs	405,234	416,279	446,029	446,751

Note: This table reports estimates from Sant'Anna and Zhao (2020)'s doubly robust difference-in-differences framework for the use of sanitary pads. Each column re-estimates the baseline specification restricting treated post-period observations to women interviewed within 60, 120, 240, or 360 days of *Suvidha* availability; never-treated subdistricts are retained throughout. Covariates include baseline sub-district aggregates of village amenities, female literacy, female labor force participation, and mean nightlights, as well as individual-level characteristics (religion, caste, urban/rural status, and age), district fixed effects, and survey month-year fixed effects. Standard errors, clustered at the subdistrict level, are reported in parentheses.
Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

5.3 Mechanisms and the Efficiency–Equity Tradeoff

Four facts organize the discussion. First, subsidized *Suvidha* provision through *Jan Aushadhi Kendras* raised sanitary napkin adoption by 9.6 percentage points overall (Table II). Second, the effect is larger where a *Kendra* opens amid existing infrastructure: 12.8 versus 7.9 percentage points near a hospital and 15.5 versus 5.0 near a supermarket, with the pharmacy cut the lone exception (8.9 versus 6.2, not statistically distinguishable; Table III). Third, effects are larger at woman-owned outlets (11.0 versus 5.0 percentage points). Fourth, adoption builds over roughly the first six to eight months of availability rather than jumping immediately (Table IV). We read this heterogeneity as identifying *where* the program works rather than *why*, but it narrows the set of plausible mechanisms.

The treatment bundles several channels at once: a new *Kendra* can improve *physical access* where no pad was obtainable nearby, spread *information* about the subsidized product, lower the *transaction cost* of obtaining pads, reduce the *stigma* of an intimate purchase, or relax an *affordability* constraint through the ₹1 price. The infrastructure pattern helps adjudicate among them. Where a *Kendra* opens near hospitals and supermarkets, foot traffic is higher, so information about the subsidized product may diffuse faster (BenYishay and Mobarak, 2019); women already visiting these areas can bundle a pad purchase with routine errands or clinical visits at low additional cost; and a purchase made alongside other health and retail goods may carry less stigma. Two supply-side channels point the same way: operators in higher-traffic locations may hold deeper inventory and obtain more reliable resupply, and co-located commercial retail provides a fallback—if *Suvidha* stock runs out or a woman prefers another product, a nearby alternative sustains *any*-pad use, the outcome we measure.

This ordering is hard to reconcile with physical access being the binding constraint. Had the marginal *Kendra* primarily filled a pad desert, effects should have been largest in the underserved areas where commercial pads were least available; instead they are smallest there. The pharmacy cut sharpens the point. A non-generic pharmacy is itself an existing, if unsubsidized, source of commercial pads, so opening a *Kendra* nearby closes little physical-availability gap—yet adoption still rises by a magnitude statistically indistinguishable from areas without a pharmacy. That a *Kendra* works about as well where pads were already obtainable is consistent with the subsidized ₹1 price, rather than mere availability, doing much of the work. The steep wealth gradient in baseline pad use (Table I) reinforces that price plausibly binds for many women.

What the heterogeneity cannot do is separate the demand-side channels from one another, or from the supply-side complementarities. Information diffusion, lower transaction costs, reduced stigma, affordability, and richer commercial fallback all co-move with infrastructure density and all predict the same ordering. The owner-gender cut offers the most direct discriminating evidence: larger effects at woman-owned *Kendras* are consistent with the comfort of a same-gender transaction lowering a social barrier the subsidy alone does not address (Cabral and Dillender, 2024), pointing toward a stigma channel. This reading is not airtight, however. Under the program, women entrepreneurs receive a one-time setup grant and an application-fee waiver,[2] so woman-owned status may proxy for a better-equipped, better-run outlet rather than the gender of the person behind the counter *per se*. The timing pattern adds a final, consistent piece: gradual uptake over the first several months—rather than an immediate jump—fits information diffusion and adoption at successive menstrual cycles rather than instantaneous response to availability (Conley and Udry, 2010). On balance, the evidence argues against physical access as the binding constraint

[2]Women entrepreneurs (along with SC/ST, *Divyang*, and ex-servicemen applicants, and those in aspirational, Himalayan, island, and North-Eastern districts) receive a one-time special incentive of ₹2 lakh—₹1.5 lakh toward furniture and fixtures and ₹0.5 lakh toward computer and IT equipment—and are exempt from the ₹5,000 application fee.

and is consistent with information, transaction costs, stigma, and affordability operating together; cleanly separating their contributions would require a design with experimental price variation.

Where to Expand: A Planner's Siting Problem

The same heterogeneity that complicates the mechanism story has a clear implication for where to grow the network, exposing a tension between efficiency and equity. Because adoption responds most strongly where *Kendras* co-locate with existing infrastructure, a planner maximizing aggregate adoption per rupee would concentrate openings in already well-served regions—yet these are not the areas of greatest deprivation. We formalize the tradeoff as a constrained location-choice problem. Let $j = 1,\dots,N$ index subdistricts without a *Kendra*, each with n_j women of menstruating age, a predicted adoption gain $\hat{\tau}_j \in [0,1]$—obtained by mapping the heterogeneous effects of Section 5.2 to subdistrict characteristics—and an indicator $d_j \in \{0,1\}$ equal to one if the subdistrict is a healthcare desert. The planner chooses a set S of K subdistricts:

$$\max_{S\subseteq\{1,\dots,N\},\,|S|=K} \sum_{j\in S} n_j\left[(1-\lambda)\,\hat{\tau}_j + \lambda\, d_j\right], \tag{4}$$

where $\lambda \in [0,1]$ is the equity weight: at $\lambda = 0$ she maximizes aggregate predicted adoption, at $\lambda = 1$ the underserved population reached. Because the objective is additive, the solution ranks subdistricts by $s_j(\lambda) = n_j[(1-\lambda)\hat{\tau}_j + \lambda d_j]$ and takes the top K. The tension enters through the negative association between $\hat{\tau}_j$ and d_j: the underserved subdistricts where unmet need is greatest have the smaller predicted gains, since adoption responds most strongly where *Kendras* co-locate with existing infrastructure. Varying λ therefore traces an efficiency–equity frontier rather than reordering a stable ranking. An interactive tool[3] solves (4) for user-chosen K and λ: openings concentrate in well-connected urban subdistricts when λ is low and disperse into underserved regions as λ rises. The choice of expansion rule thus embeds an implicit distributional judgment rather than a purely technical optimization.

6 Robustness Checks

We first present the dynamic event-study estimates and then conduct five checks to validate our identification strategy and confirm that the documented effects stem from subsidized menstrual product availability rather than alternative mechanisms. First, a *falsification test* exploits the temporal gap between *Kendra* establishment and *Suvidha* pad availability, re-anchoring the event study to *Kendra* opening in the period before any subsidized pads existed. Second, we exclude interviews

[3]URL withheld for anonymous review.

conducted during *Suvidha*'s initial ₹2.50 price regime, before the price was reduced to ₹1. Third, we address the COVID-19 suspension of NFHS-5 fieldwork by re-estimating on interviews conducted before the suspension, so that differential post-suspension interview timing across treated and control units cannot drive the result. Fourth, we formally quantify the sensitivity of the estimate to violations of parallel trends following Rambachan and Roth (2023). Fifth, a *leave-one-out* exercise drops states with concurrent menstrual health programs one at a time.

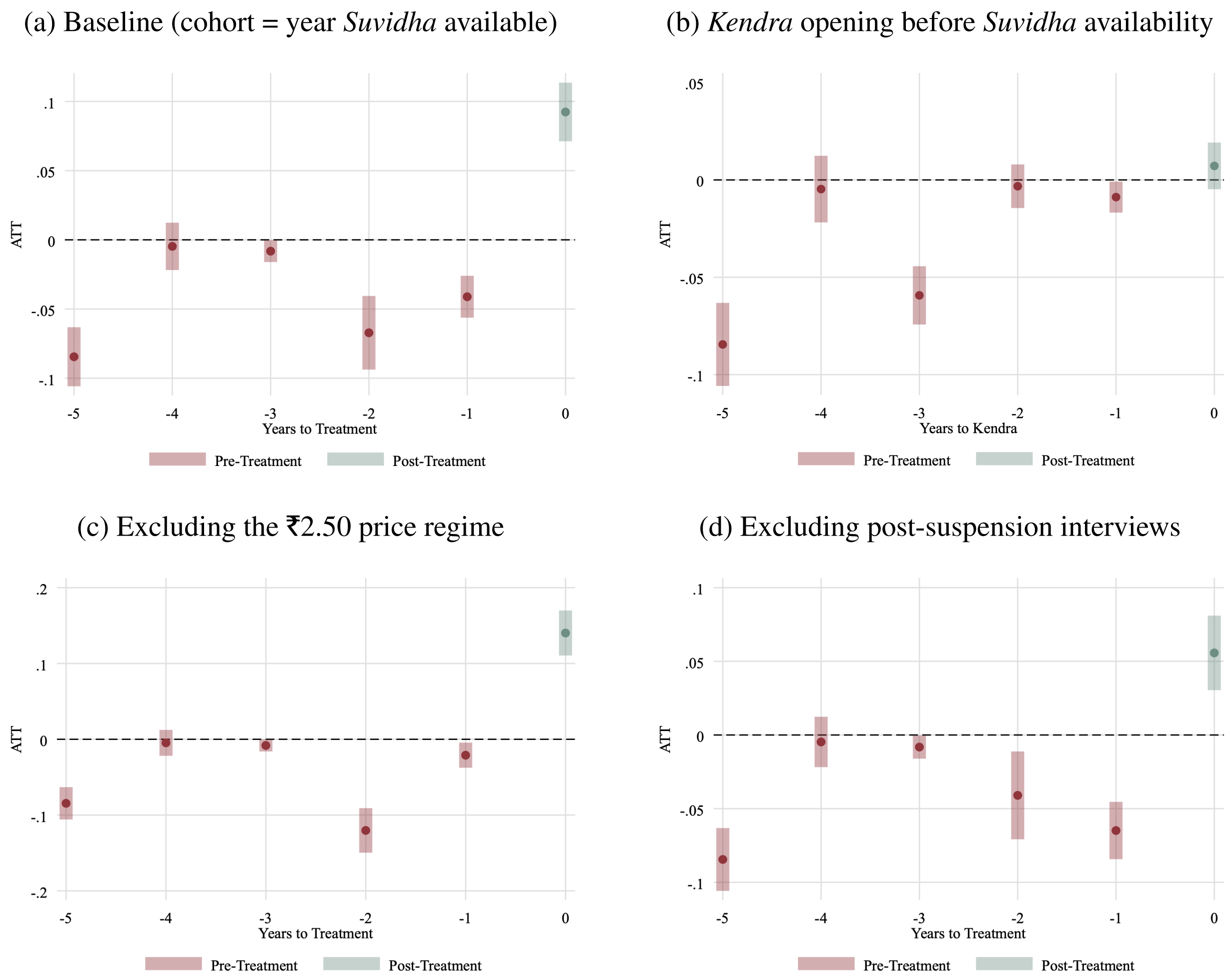
6.1 Dynamic Treatment Effects

Panel (a) of Figure II presents dynamic treatment effects from the Callaway and Sant'Anna (2021) estimator, where a subdistrict's treatment cohort is the calendar year in which *Suvidha* pads become available there and event time is measured in years to treatment. The estimate at event time zero (0.092, SE = 0.013) is a sharp, precisely estimated increase in pad use, closely in line with the headline estimate in Table II, while every pre-treatment coefficient is non-positive.

Two features of the NFHS shape how we estimate and read this figure. First, the event study exploits variation that the two survey rounds might seem to preclude: fieldwork within each round was spread over multiple years, and subdistricts gained access to subsidized pads on staggered dates, so women are observed across a wide range of event times relative to local availability rather than at two fixed points. Second, fieldwork was clustered in time within geographies: most interviews in a district occurred close together, leaving little within-district variation in interview timing. Our preferred Sant'Anna and Zhao (2020) specification can therefore absorb district fixed effects, while the event-study design cannot support them.

We do not read the pre-period as establishing exact parallel trends, but the negative pre-treatment coefficients do not threaten our conclusion. Each pre-period coefficient is identified from the change in adoption among cohorts not yet exposed to subsidized pads relative to comparison subdistricts, drawing on both survey rounds and on subdistricts whose *Kendras* open later; several are individually significantly negative. Three observations discipline their interpretation. First, the deviations are uniformly non-positive with no monotone build-up toward the treatment date, whereas adoption jumps discretely and positively exactly when *Suvidha* becomes available; if anything, a downward pre-trend would bias us against finding a positive effect. Second, the falsification event study below (Panel (b)), which anchors event time to *Kendra* opening before any *Suvidha* availability, reproduces a similar pre-period pattern but shows no jump at opening—the discontinuity is specific to the subsidized product, not to *Kendra* entry. Third, rather than treating the pre-period as dispositive, we quantify in Section 6.5 how large a violation of parallel trends would be required to overturn the result.

FIGURE II: Dynamic Treatment Effects: Baseline, Falsification, and Exclusion Checks



Note: All four panels plot dynamic average treatment effects on the treated from the Callaway and Sant'Anna (2021) estimator with doubly robust estimation, using not-yet-treated subdistricts as controls; the outcome is an indicator for sanitary pad use among women aged 15–24. Panel (a) is the baseline event study: a subdistrict's cohort is the calendar year in which *Suvidha* pads become available. In Panel (b), the sample is restricted to women interviewed before *Suvidha* pads became available, and a subdistrict's cohort is the calendar year in which its *Kendra* opens. In Panel (c), the cohort is again the year *Suvidha* becomes available, and the sample excludes NFHS-5 interviews conducted while *Suvidha* retailed at ₹2.50, before the ₹1 price took effect. In Panel (d), the sample excludes all NFHS-5 interviews conducted after March 2020, removing the fieldwork resumed after the COVID-19 suspension so that identification rests only on interviews conducted before the suspension. Covariates include individual-level characteristics (religion, caste, urban/rural status, and age) and baseline sub-district aggregates of village amenities, female literacy, female labor force participation, and mean nightlights. Shaded bands are 90% confidence intervals.

Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

6.2 Falsification Test: *Kendras* Without *Suvidha* Availability

Our first check exploits the temporal gap between *Kendra* establishment and *Suvidha* pad availability. *Kendras* began operating in 2008, but subsidized *Suvidha* pads were not part of their product line until a decade later. If the adoption response in Panel (a) of Figure II reflects the subsidized product rather than some other feature of *Kendra* entry—health information, retail density, or correlated local development—then *Kendra* openings in the pre-*Suvidha* era should produce no movement in pad use.

Panel (b) of Figure II implements this test. We restrict the sample to interviews conducted entirely before *Suvidha* became available and re-estimate the Callaway and Sant'Anna (2021) event study, now defining a subdistrict's treatment cohort by the calendar year its *Kendra* opens. In sharp contrast to the discrete jump at *Suvidha* availability in Panel (a) of Figure II, the estimate at *Kendra* opening is small and statistically indistinguishable from zero. *Kendra* presence alone, absent the subsidized product, does not move menstrual product adoption—consistent with the ₹1 pad, rather than *Kendra* entry, as the operative channel.

6.3 Excluding the ₹2.50 Price Regime

Suvidha pads initially retailed at ₹2.50 per pad before the price was reduced to ₹1, effective August 27, 2019—the availability date that defines treatment throughout the paper. The initial ₹2.50 regime reached consumers in far smaller volume than the subsequent ₹1 rollout, which is why we anchor treatment to the ₹1 date rather than the initial launch. Women interviewed after the ₹2.50 launch but before the price reduction are coded as untreated in our baseline specification despite facing a partially subsidized product, which would attenuate the estimated effect. Panel (c) of Figure II re-estimates the event study excluding NFHS-5 interviews conducted during this transitional window. The post-treatment estimate is, if anything, larger than the baseline, and the pre-period pattern is unchanged. The headline effect is therefore not an artifact of the price transition; to the extent the transitional window matters, our baseline estimate is conservative.

6.4 COVID-19 Fieldwork Suspension and Interview Timing

NFHS-5 fieldwork was interrupted by the COVID-19 pandemic: interviewing was suspended in the spring of 2020 and resumed only several months later, leaving a multi-month gap in the field calendar. Because the ₹1 price took effect on August 27, 2019, women interviewed after fieldwork resumed had lived under subsidized availability substantially longer than those interviewed before the suspension, leaving more time for adoption to materialize. If treated (*Kendra*-served) women were disproportionately represented among these later interviews, the mechanical accumulation of exposure time—rather than the causal effect of the subsidy—could inflate the estimated effect.

Figure A2 plots the distribution of interview dates for treated and never-treated women in NFHS-5. The two distributions track each other closely throughout the pre-suspension window and both exhibit the characteristic gap during the suspension; in the resumed-fieldwork window, treated women are modestly over-represented relative to never-treated women. This imbalance motivates a direct check. Panel (d) of Figure II re-estimates the event study after dropping all interviews conducted after March 2020—the entire post-suspension window—so that identifying variation comes solely from the pre-suspension period, during which exposure time is bounded and comparable across groups. The post-treatment estimate remains positive and statistically significant, comparable to the baseline. Differential interview timing around the COVID-19 fieldwork suspension therefore does not account for the adoption effect.

6.5 Sensitivity to Parallel-Trends Violations

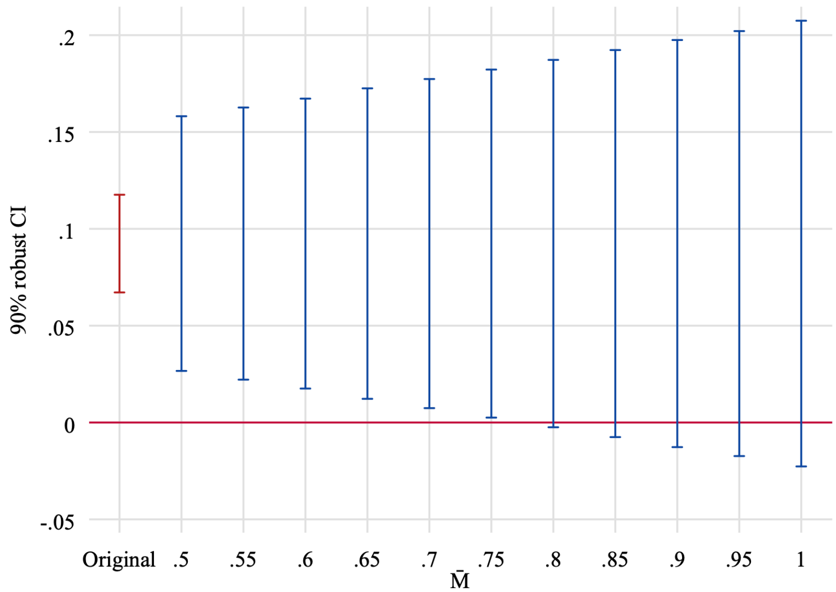
Because the pre-period of the event study in Panel (a) of Figure II exhibits some individually significant (but negative) coefficients, we assess how sensitive the adoption result is to violations of parallel trends using the sensitivity analysis of Rambachan and Roth (2023). We apply the relative-magnitudes restriction $\Delta^{RM}(\bar{M})$, which bounds the post-treatment violation of parallel trends by $\bar{M}$ times the largest pre-period violation, to the Callaway and Sant'Anna (2021) event-study estimates for pad use, and report 90% robust confidence intervals.

Figure III presents the results. The robust confidence interval excludes zero for $\bar{M} \leq 0.75$: the positive adoption effect survives post-treatment violations of parallel trends as large as three-quarters of the maximal pre-period deviation. The interval first includes zero at $\bar{M} = 0.80$, placing the breakdown value at approximately $\bar{M} \approx 0.75$. Given that the largest pre-period deviation in Panel (a) of Figure II is itself imprecisely estimated at distant event time, we read this as meaningful robustness: overturning the result requires a post-treatment confound roughly three-quarters the size of the most extreme—and least precisely identified—pre-period fluctuation, read alongside the other supporting evidence above.

6.6 Concurrent State Menstrual Health Programs

Several Indian states operated their own menstrual hygiene programs during our study window, raising the concern that the estimated effect of *Suvidha* availability partly reflects these concurrent interventions rather than the subsidy itself. Kerala's *She Pad* scheme (November 2017), Odisha's *Khushi* scheme, and Maharashtra's *Asmita Yojana* began distributing free or heavily subsidized napkins to school-going girls between the two NFHS waves, and Haryana introduced free napkins for below-poverty-line women and girls in 2020, during NFHS-5 fieldwork. Tamil Nadu and Kar-

FIGURE III: Sensitivity to Parallel-Trends Violations: Relative Magnitudes $\Delta^{RM}(\bar{M})$, Sanitary Pad Use



Note: 90% robust confidence intervals from Rambachan and Roth (2023) under the relative-magnitudes restriction $\Delta^{RM}(\bar{M})$, applied to the Callaway and Sant'Anna (2021) event-study estimates for sanitary pad use in Panel (a) of Figure II, over a grid of $\bar{M}$ from 0.5 to 1 in steps of 0.05. "Original" denotes the baseline confidence interval. The robust interval first includes zero at $\bar{M} = 0.80$.

Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

nataka have distributed free napkins to adolescent girls through schools and frontline health workers since 2011 and 2013–14, respectively, and Punjab, Rajasthan, and Andhra Pradesh launched free-distribution schemes in 2021, after most NFHS-5 fieldwork but potentially overlapping with late interviews. Table A6 describes each program; the Kerala *She Pad* scheme has itself been shown to reduce dropout among school-going girls (Agarwal, Chia, and Ghosh, 2024). These schemes differ from *Suvidha* in their delivery channel: they distribute napkins free of charge (or nearly so) through schools and *anganwadi* centers to targeted groups, primarily enrolled adolescent girls, whereas *Suvidha* sells napkins at ₹1 through retail outlets to any buyer. Nonetheless, because both raise pad adoption among young women, any correlation between *Kendra* rollout and the geography of these schemes could bias our estimates.

We address this concern by re-estimating our main specification while excluding, one at a time, each of the nine states with an active or recently launched distribution scheme, and then excluding all nine states jointly. Table V reports the results. The estimates are stable across single-state exclusions, ranging from 7.5 to 13.8 percentage points around the full-sample estimate of 9.6 percentage points, and every estimate is significant at the 1 percent level. No single state with a concurrent program drives the result. When all nine states are excluded simultaneously, removing roughly 30 percent of the sample, the estimate is 5.3 percentage points, or 9.4 percent of the baseline adoption rate of 56.1 percent in the remaining states. The smaller magnitude in this restricted sample admits two non-exclusive readings. First, the excluded states could be interviewed in systematically different months so part of the drop could reflect interview timing rather than program content; our survey month-by-year fixed effects are designed to absorb exactly this variation. Second, to the extent the reduction is substantive, it is consistent with complementarity rather than confounding: in states that also distributed free or subsidized napkins, *Suvidha* availability and the concurrent supply jointly raise overall pad use, so removing these states removes part of a reinforced response—in keeping with our broader reading that subsidized availability catalyzes pad adoption in general rather than merely substituting *Suvidha* for cloth. Either way, the effect remains positive and precisely estimated in every exclusion, indicating that subsidized availability through *Kendras* increases adoption even in states with no contemporaneous distribution program; we therefore conclude that concurrent state schemes do not, by themselves, account for our main finding.

TABLE V: Treatment Effect of Subsidized Suvidha Sanitary Napkins: Leave-One-Out by State

Excluded state	$\hat{\tau}$	Mean	Obs
Punjab	0.100***	0.601	477,426
	(0.012)		
Haryana	0.101***	0.601	474,725
	(0.012)		
Rajasthan	0.103***	0.603	456,748
	(0.011)		
Odisha	0.095***	0.608	469,371
	(0.011)		
Maharashtra	0.097***	0.603	469,710
	(0.011)		
Andhra Pradesh	0.138***	0.599	473,146
	(0.011)		
Kerala	0.075***	0.605	472,297
	(0.011)		
Tamil Nadu	0.076***	0.603	483,372
	(0.011)		
Karnataka	0.102***	0.598	474,017
	(0.011)		
All nine excluded	0.053***	0.561	335,780
	(0.015)		

Note: Each row reports the estimate from Sant'Anna and Zhao (2020)'s doubly robust difference-in-differences framework, re-estimated on the sample excluding the indicated state; the final row excludes all nine states simultaneously. Covariates include baseline sub-district aggregates of village amenities, female literacy, female labor force participation, and mean night-lights, as well as individual-level characteristics (religion, caste, urban/rural status, and age), district fixed effects, and survey month-year fixed effects. Standard errors, clustered at the subdistrict level, are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.
Data sources: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India; National Family Health Survey Rounds 4 and 5; SHRUG: 2011 Population Census Abstract and Village Directory; VIIRS Night Lights.

7 Conclusion

This paper asks whether subsidizing sanitary napkins and delivering them through a network of generic pharmacies raises their adoption, exploiting India's *Jan Aushadhi–Suvidha* program, which cut the retail price to ₹1 per pad in August 2019 across a network of more than 8,000 *Jan Aushadhi Kendras*. Linking administrative records on the universe of *Kendras* to two rounds of the National Family Health Survey, and estimating a doubly robust difference-in-differences specification that exploits the staggered dates at which subsidized pads reach each subdistrict together with the multi-year spread of survey fieldwork, we find that subsidized availability raises sanitary napkin use by 9.6 percentage points, a 16% increase over a 61% baseline. The response is sizable but well short of universal, in keeping with the incomplete take-up that even deep subsidies elicit for other inexpensive welfare-enhancing goods. A battery of checks—stability across single- and joint-state exclusions of concurrent programs, a Rambachan and Roth (2023) sensitivity analysis under which the effect survives parallel-trends violations as large as three-quarters of the largest pre-treatment deviation, and a falsification test finding no adoption response to *Kendra* entry before subsidized pads existed—is consistent with the subsidy, rather than coincident shocks or pharmacy presence per se, driving the result.

These findings speak to several literatures. Most of what is known about menstrual-product interventions comes from experiments that provide products directly and trace downstream effects on schooling, health, and labor; we estimate instead the first-order behavioral response—whether a deep price subsidy draws prior non-users into pad use—at national scale and through an ordinary retail channel rather than schools or frontline health workers. In doing so we extend the literature on the price-responsiveness of inexpensive, welfare-enhancing goods beyond the one-off and preventive products that have anchored it—bednets, water treatment—to a recurring necessity, where the relevant margin is sustained repeat use, and we supply evidence on the price response for menstrual products specifically, which prior work could observe only in high-income settings where pad use is already near-universal. The setting also underscores that the delivery channel shapes reach: a commercial network whose operators retain a margin and serve any walk-in customer reaches the out-of-school adolescents, young working women, and rural residents that India's earlier school- and health-worker-based programs missed, without adding to the load on schools or frontline staff.

The heterogeneity in the response carries the paper's central policy lesson. Adoption rises most where a *Kendra* opens amid existing health and retail infrastructure and at woman-owned outlets, and least in underserved areas—an ordering that argues against physical access as the binding constraint, though it cannot on its own separate the demand-side channels of information, transaction costs, stigma, and affordability that co-move with infrastructure density. Two implications follow.

The price cut drives adoption, but its return is amplified where the surrounding context lowers the non-pecuniary cost of obtaining a pad, so where outlets are sited and who runs them shape the return alongside the price itself. And because adoption responds most strongly where unmet need is smallest, the planned expansion to 25,000 outlets confronts an efficiency–equity tension: maximizing adoption per rupee would steer the subsidy away from the most deprived areas. Routed through a pre-existing commercial-retail network, the model delivers these gains at low public cost and should travel to other low- and middle-income settings that share prior retail density, operator margins that sustain stocking, and low-transaction-cost walk-in access; where such networks are thin, the returns to building one may be correspondingly high.

Two features of the design qualify these conclusions and mark out an agenda for future work. First, the data are a repeated cross-section observed in two calendar windows, so we cannot follow the same women over time; the staggered-availability design and sensitivity analysis address the resulting concerns, but a denser geocoded panel would permit sharper dynamic evidence and would allow the adoption gains documented here to be traced into schooling attainment, labor-force participation, and reproductive health as exposed cohorts mature. Second, the design bundles the channels through which subsidized availability raises adoption—information diffusion, lower transaction costs, reduced stigma, and improved affordability—and cannot cleanly separate them; a field experiment that varies the ₹1 price directly would isolate the affordability channel and quantify how much of the response the subsidy itself carries. A further question lies beyond our data: the general-equilibrium response of the commercial sanitary-napkin market to a deep subsidy delivered at national scale—whether it crowds out private supply, compresses prices, or expands the overall market, and with what net welfare consequences. We leave this to future research.

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Appendix

Pennies for Pads: Subsidized Menstrual Health Products and Adoption in India

Additional Tables and Figures

TABLE A1: Menstrual Products available at *Jan Aushadhi Kendras*

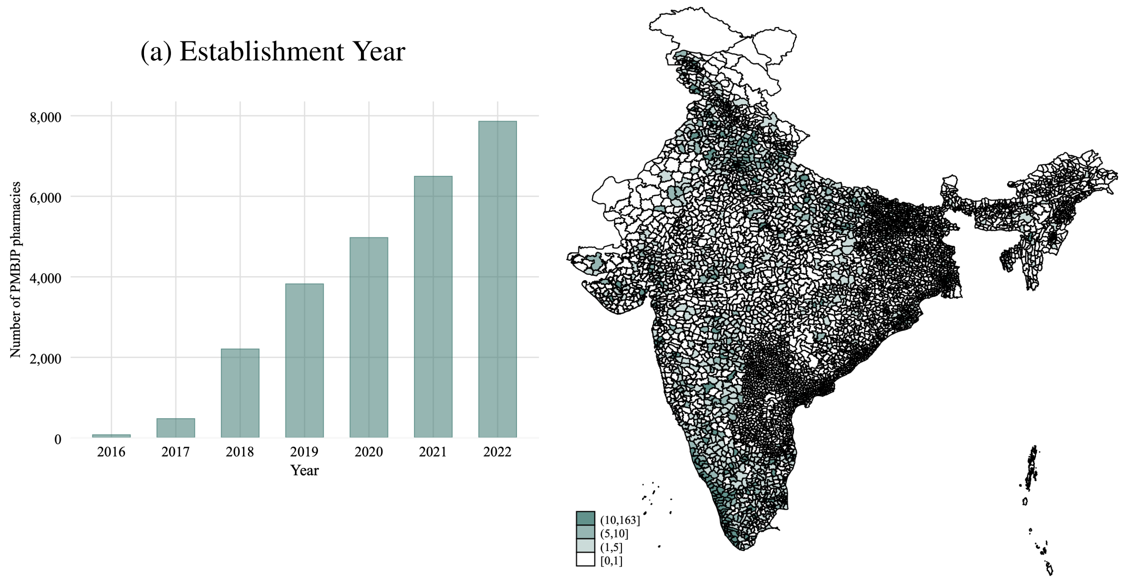
Drug Code	Generic Name	Unit Size	Retail Price (₹)
8140	Regular Size with Wings	Pack of 4	4.00
8141	Large Size with Wings	Pack of 4	12.00
8142	Extra Large Size with Wings	Pack of 4	15.00
8145	Regular Size with Wings	Pack of 10	10.00

Note: Prices are current retail prices in Indian rupees as set by the Department of Pharmaceuticals.
Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India.

FIGURE A1: Establishment Year and Location of *Jan Aushadhi Kendras*

(a) Establishment Year

(b) Establishment Location



Note: Panel (a) presents the cumulative number of *Jan Aushadhi Kendras* established by year. Panel (b) shows density across Indian subdistricts, with darker shading indicating higher concentrations.
Data source: Department of Pharmaceuticals, Ministry of Chemicals and Fertilizers, Government of India.

TABLE A2: Descriptive Statistics of Sub-district Characteristics (Baseline)

	(1)	(2)	(3)	(4)
	Mean	SD	Min	Max
Sanitation				
No waste collection	0.68	0.40	0.00	1.00
Community has waste disposal	0.04	0.12	0.00	1.00
Total Sanitation Campaign	0.36	0.41	0.00	1.00
School and Anganwadi				
Availability of govt middle schools	0.50	0.24	0.00	1.00
Availability of anganwadi	0.87	0.15	0.00	1.00
Night Lights				
Mean night lights	1.11	5.10	0.00	111.49
Female Literacy and LFP				
Share of literate women	0.52	0.13	0.07	0.90
Share of working women	0.33	0.14	0.04	0.79
Observations				5,993

Note: Baseline sub-district characteristics prior to the policy. Sanitation and educational infrastructure measures from the 2011 Population Census Village Directory; female literacy and labor force participation from the 2011 Population Census Abstract; nighttime light intensity from 2012 VIIRS data.
Data source: Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG).

TABLE A3: Descriptive Statistics of *Jan Aushadhi Kendra* Characteristics

	(1)	(2)
	Mean	SD
Established before Aug 27 2019	0.54	0.50
Owner is a woman	0.25	0.43
Hospital nearby	0.63	0.48
Pharmacy nearby	0.26	0.44
Supermarket nearby	0.37	0.48
Observations		8,264

Note: Characteristics of *Jan Aushadhi Kendras* across subdistricts. Amenities measured via Geoapify Places API (5-minute driving radius). Owner gender inferred via Claude AI rule-based classification validated against Bejda (2015).
Data source: Department of Pharmaceuticals; Geoapify Places API.

TABLE A4: Correlation Between Sub-district Characteristics and Timing of *Suvidha* Availability

	Coef	S.E.
No waste collection	60.10	38.23
Community has waste disposal	-35.94	83.61
Total Sanitation Campaign	29.37	39.64
Availability of anganwadi	-16.78	58.31
Availability of govt middle schools	44.84	44.60
Share of literate women	-12.03***	1.00
Share of working women	7.57***	0.83
Mean night lights	-12.18***	3.30
Mean	1178.01	
R sq.	0.42	
Obs	5,897	

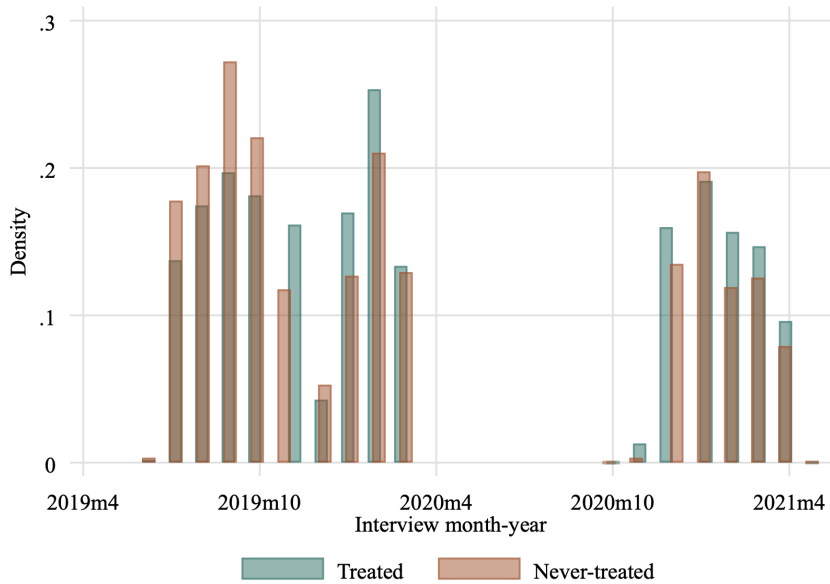
Note: Regression of the date on which *Suvidha* first becomes available in the subdistrict on baseline sub-district characteristics with state and district fixed effects. Subdistricts where *Suvidha* never becomes available are assigned the latest observed availability date. Positive coefficients indicate that higher values of the covariate are associated with later availability.
Data source: SHRUG; Department of Pharmaceuticals.

TABLE A5: Descriptive Statistics: Menstrual Hygiene Management Methods Among Women Aged 15–24

	(1)	(2)
	NFHS-4	NFHS-5
Woman uses cloth	0.657	0.519
	(0.475)	(0.500)
Woman uses locally prepared napkins	0.150	0.147
	(0.357)	(0.354)
Woman uses sanitary napkins	0.418	0.631
	(0.493)	(0.482)
Observations	247,833	241,180

Note: Means and standard deviations of menstrual hygiene practices by survey wave. *Data source*: National Family Health Survey Rounds 4 and 5.

FIGURE A2: Distribution of Interview Dates by Treatment Status, NFHS-5



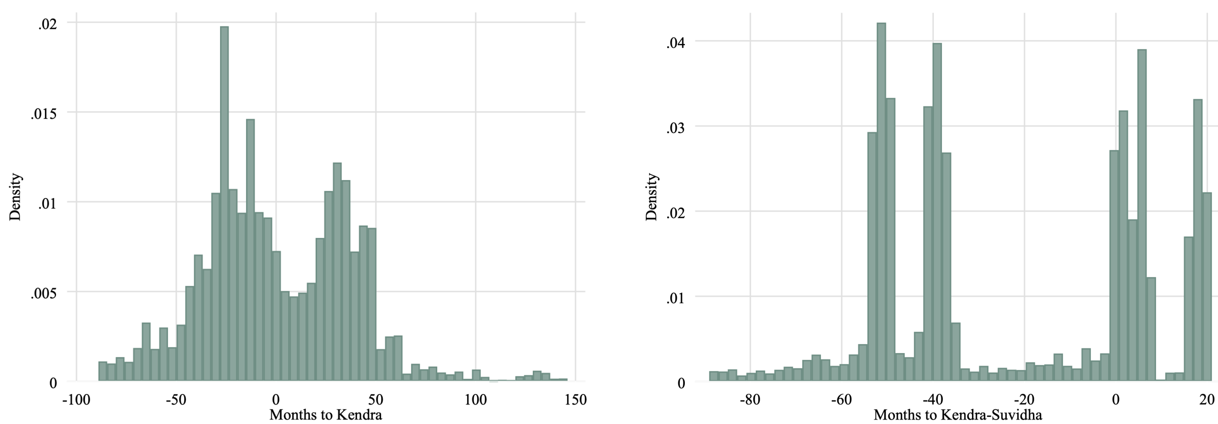
Note: Histograms of interview month-year for women aged 15–24 in NFHS-5, plotted separately for treated (*Kendra*-served) and never-treated subdistricts. The gap in both distributions corresponds to the suspension of NFHS-5 fieldwork during the COVID-19 pandemic. Treatment status is defined by *Kendra* service (`TREAT_suvidha_2019`), independent of interview date. Density is normalized within each group.

Data source: National Family Health Survey Round 5.

FIGURE A3: Distribution of Interview Timing Relative to *Kendra* Establishment and *Suvidha* Availability

(a) Months to *Kendra* establishment

(b) Months to *Suvidha* availability



Note: Histograms of the gap, in months, between a woman's NFHS interview month (`ym(v007,v006)`) and (a) the establishment date of the *Jan Aushadhi Kendra* serving her subdistrict and (b) the date *Suvidha* pads became available there. Negative values denote women interviewed before the reference date; positive values, after. In panel (a) the distribution spans roughly −90 to +150 months, reflecting *Kendras* established years before the survey windows; in panel (b) the right tail is bounded near +20 months because *Suvidha* availability falls late in the panel. The multimodal shape is consistent with pooling the NFHS-4 (2015–16) and NFHS-5 (2019–21) fielding windows.

Data source: National Family Health Survey Rounds 4 and 5; *Jan Aushadhi Kendra* administrative records.

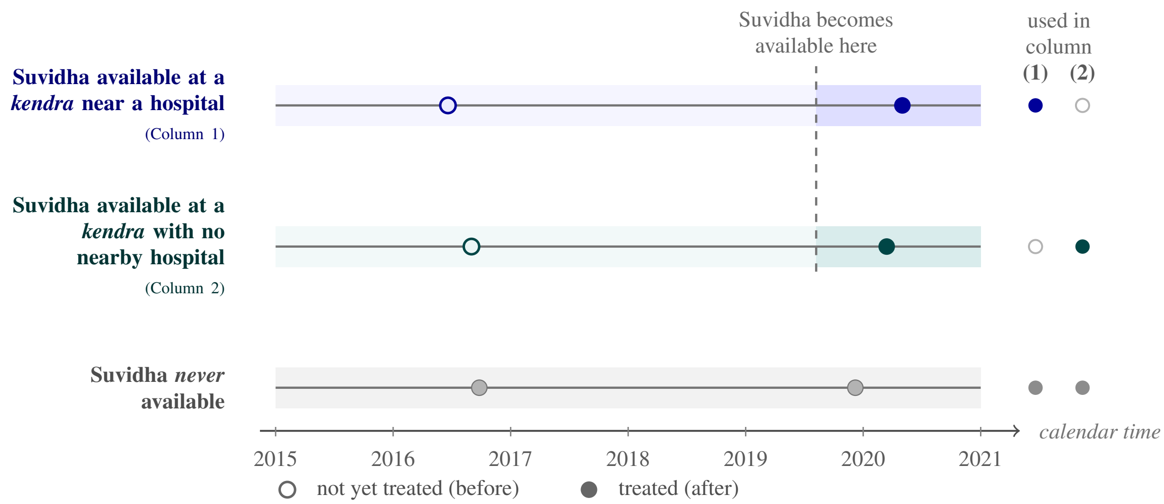


FIGURE A4: **Heterogeneity subsamples (hospital proximity).** Each lane is a representative subdistrict and each marker a single interview. Among subdistricts where Suvidha becomes available at a *Kendra*, the top lane holds openings near a hospital and the middle lane openings with no hospital nearby; within each, women interviewed before the local availability date are not yet treated and those interviewed after it are treated. The bottom lane—subdistricts where Suvidha never becomes available—is the common comparison group. As the right-hand indicator shows, column (1) of Table III is estimated on the near-hospital lane against the never-available lane, and column (2) on the no-hospital lane against the same never-available lane; each column excludes the other opening type. The pharmacy, supermarket, and owner-gender cuts follow the same template.

TABLE A6: State Sanitary Napkin Distribution Programs, 2011–2021

State	Scheme	Launch	Target population	Delivery channel and price
Tamil Nadu	Menstrual Hygiene Programme	2011	Rural adolescent girls (10–19), new mothers, women prisoners; extended to urban areas in 2020	Free; government hospitals, health centers, *anganwadis*, schools
Karnataka	Shuchi	2013–14	Girls aged 10–18 in government and aided schools and colleges	Free; distributed through schools (stalled c. 2019–2023, relaunched 2024)
Kerala	She Pad	Nov. 2017	Girls in government and aided schools	Free napkins, storage, and incinerators in schools
Odisha	Khushi	Feb. 2018	Girls in classes 6–12 in government and aided schools ($\sim$1.7 million)	Free; distributed through schools
Maharashtra	Asmita Yojana	Mar. 2018	Rural schoolgirls; women in self-help groups	Subsidized (₹5 per pack of 8 for schoolgirls); via SHGs (discontinued 2022)
Haryana	Mahila Avam Kishori Samman Yojana	Aug. 2020	Below-poverty-line women and girls ($\sim$2.25 million)	Free; one pack per month
Punjab	Udaan	May 2021	Out-of-school girls, BPL, slum-resident, and homeless women	Free; monthly through *anganwadi* centers
Rajasthan	Udaan	Nov. 2021	Women and girls aged 10–45	Free; schools, colleges, *anganwadis*
Andhra Pradesh	Swechha	Oct. 2021	Girls in classes 7–12 in government institutions ($\sim$1 million)	Free; 10 napkins per month through schools

Note: Launch dates refer to the initial statewide announcement. Compiled from state government announcements and press reports.