# Essays in Development Economics with a Focus on Gender, Health, and the Environment

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# Abstract

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This thesis comprises three chapters on topics in development economics. The first chapter studies access to maternal healthcare in markets with vertically differentiated public and private providers. The second chapter studies the efficacy of induction stoves in reducing indoor air pollution in rural households when faced with erratic power supply. Finally, the third chapter studies the role of financial incentives in correcting disparities in sex ratios. All three chapters study the context of India but are representative of important development issues in low-income countries.

The first chapter titled "Equilibrium Effects of Subsidizing Public Services" studies one of India's largest welfare schemes *Janani Suraksha Yojana* (JSY) that incentivized pregnant women in India to access institutional maternal care at public hospitals. We argue that governments can make complementary investments to improve welfare gains from large scale policies.

JSY did not improve health outcomes despite a substantial increase in the take-up of institutional care. We document three equilibrium responses that explain this policy failure. First, JSY led to a mismatch of risk across health facilities – high-risk mothers sorted out of highest quality care at private facilities. Second, in line with the literature, public sector quality deteriorated as a result of congestion. This resulted in lower quality care for both marginal as well as infra-marginal patients at public hospitals. We show that only mothers with high socio-economic status adapted to the worsening quality of care at public hospitals by sorting into more expensive private hospitals. Third, despite increased competition, private hospitals maintained high prices, crowding out riskier and poorer mothers. We do not find evidence that private hospitals improved healthcare quality to justify higher prices.

The second chapter titled "Electric Stoves as a Solution for Household Air Pollution" is an

interdisciplinary field-based research study that studies the role of reliable electricity in inducing rural Indian households to switch away from dirty cooking fuels towards a clean cooking technology, induction cookstoves, thereby reducing the exposure to high levels of indoor air pollution. We collected minute-by-minute data on electricity availability, electric induction stove use, and kitchen and outdoor particulate pollution in a sample of rural Indian households for one year. Using within household-month variation generated by unpredictable outages, we estimate the effects of electricity availability and electric induction stove use on kitchen PM2.5 concentration at each hour of the day. Electricity availability reduces kitchen PM2.5 by up to 50 $\mu g/m^3$, which is between 10 and 20 percent of peak concentrations during cooking hours. Induction stove use instrumented by electricity availability reduces PM2.5 in kitchens by 200-450 $\mu g/m^3$ during cooking hours.

The final chapter titled "Can Large-Scale Conditional Cash Transfers Resolve the Fertility-Sex Ratio Trade-off? Evidence from India" studies a large-scale conditional cash transfer (CCT) scheme *Ladli Laxmi Yojana* that offered cash incentives to households upon the birth of girl children. The policy also offered substantial incentive for investing in girls' education. In my evaluation of the *Ladli Laxmi Yojana* in Madhya Pradesh, India. I find that financial incentives aimed at the girl child increased average fertility by about 0.15 children per household (on baseline average of 0.93 children) children per household and improved sex-ratio by 3%. This points to the well known fertility-sex ratio trade-off. Moreover, these effects are quite opposite to a similar CCT scheme in Haryana (Anukriti, 2018) suggesting context dependence of such policies.

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To Shreya, my wife.

# Chapter 1: Equilibrium Effects of Subsidizing Public Services

We study the equilibrium effects of subsidizing public services in the presence of vertically differentiated public and private suppliers. We evaluate one of India's largest welfare schemes, *Janani Suraksha Yojana* (JSY), which subsidized childbirth at public health institutions. JSY did not improve health outcomes despite a substantial increase in take-up of institutional care. We document three equilibrium responses that explain this policy failure. First, JSY led to a mismatch in patient risk across health facilities. High-risk mothers sorted out of the highest-quality care at private facilities and into lower-quality public facilities. Second, in response to congestion and deterioration of care at public hospitals, only mothers with high socio-economic status sorted out of congested public facilities into more expensive private facilities. Third, private hospitals increased prices without improvements in healthcare quality in a specific subset of states, further crowding out high-risk and poor mothers. These findings point to the need for complementary public policies in addition to JSY, in particular, capacity improvements at public facilities and targeted vouchers for poor mothers to access healthcare at private facilities.

## 1.1 Introduction

Effectively designing large-scale public policies is crucial given limited government funds. A growing literature has emphasized the importance of equilibrium considerations in the design of public policies at scale (Acemoglu, 2010; Egger et al., 2022; Cunha et al., 2019; Khanna, 2023). Studies have shown that equilibrium responses can either amplify (Barahona et al., 2020; Jiménez-

[0]This chapter is co-authored with Parijat Lal, and was my job market paper. We would like to thank Eric Verhoogen, Gautam Gowrisankaran, Jack Willis and Michael Best for their invaluable mentorship, support and guidance. We also thank Christian Pop-Eleches, Miguel Urquiola, Bentley MacLeod, Ashley Swanson, Ashley Langer, Laura Boudreau, Tomasso Porzio, Pietro Tebaldi, Doug Almond, Suresh Naidu, Aprajit Mahajan, Edward Miguel, Sebastian Otero, Nano Barahona, Andrew Olenski, Szymon Sacher, Shreya Chandra, Florian Grosset, Palaash Bhargava, Patrick Farell for valuable comments and suggestions. I gratefully acknowledge financial support from Program for Economic Research, Columbia University.

Hernández and Seira, 2021), attenuate (Andrew and Vera-Hernández, 2022), or redistribute (Khanna, 2023; Atal et al., 2022) the benefits of such policies. We study the equilibrium effects of large-scale subsidies for public services in the presence of vertically differentiated public and private suppliers. Theoretically, on the one hand, subsidies for the public option can discipline the market by restricting private suppliers' market power. On the other hand, they can induce distortions in demand by incentivizing take-up of lower-quality services. We offer an empirical investigation of these claims in the context of India's maternal healthcare system, which features a lower-quality public option along with private providers.

We study India's *Janani Suraksha Yojana* (JSY), a program that offered subsidies to pregnant women conditional on adopting institutional care for deliveries at India's public facilities. Around the launch of JSY in 2005, over 70% of pregnant women in India gave birth at home, presumably under severely inadequate healthcare expertise and facilities. Concurrently, India accounted for almost a third of all neonatal deaths and a fifth of all maternal deaths around the world (Lim et al., 2010). The key objective of JSY was to reduce maternal and perinatal mortality by encouraging pregnant women to give birth in public healthcare facilities instead of delivering at home. Previous evaluations of JSY have documented that even though mothers sorted from home to institutional facilities, perinatal mortality did not decline (Powell-Jackson et al., 2015). Moreover, Andrew and Vera-Hernández (2022) show that increased demand for public hospitals resulted in congestion, resulting in higher mortality in districts with low public sector healthcare capacity. We document that India's public and private healthcare systems are vertically differentiated. Furthermore, we demonstrate that equilibrium interactions between public and private healthcare sectors contributed to the failure of JSY in reducing perinatal mortality. First, high-risk mothers sorted out of the highest-quality care at private facilities and into lower-quality public facilities. Second, as congestion led to deterioration of care at public hospitals, only mothers with high socio-economic status adapted by sorting out of congested public facilities into more expensive private facilities. Finally, JSY led to an increase in prices at private facilities in a specific subset of states without improvements in healthcare quality, further restricting access to the highest-quality facilities.

JSY had two main components. First, pregnant women were offered significant cash incentives conditional on delivering at a public healthcare facility. And second, the government appointed personnel in each village to assist pregnant women with various stages of motherhood. These Accredited Social Health Workers (ASHA) were financially incentivized to encourage women to deliver at public healthcare facilities. Eligibility for benefits under JSY was determined based on prevailing rates of maternal and perinatal mortality across Indian states. The ten worst-performing Indian states were designated low-performing states (LPS) and the remaining were designated high-performing states (HPS). All mothers in LPS were eligible to receive benefits under the scheme whereas only poor and/or socially backward groups were eligible in HPS. The scheme was rolled out rapidly starting in the second quarter of 2005 and was present in all Indian districts in our sample by 2009. Crucially, in its effort to reduce mortality, the Indian government neither subsidized births in private facilities nor prioritized investments in public sector capacity.

Two features of JSY enable us to make empirical progress on our research question. First, this policy provided a large demand stimulus in a market with vertically differentiated public and private suppliers that was able to affect market equilibrium. And second, because JSY was a flagship policy under a larger healthcare agenda of the Indian government, special efforts were made to collect data on household choices, out-of-pocket costs, health infrastructure and health outcomes.

The data for this study come from three rounds of India's District-level Household Surveys (DLHS). This nationally representative dataset contains detailed retrospective information on the most recent childbirth for each woman in the household[1], including the outcome of delivery, place (private facility, public facility or home) and type of delivery, out-of-pocket costs for healthcare, receipt of government assistance, individual and household demographics, and socio-economic status. Importantly, the survey also asked women several questions about previous pregnancies (for example, previous birthing complications, still-births, and fertility), which helps us in assessing the ex-ante risk level of a mother before her last delivery, following Ash et al. (2012). Our data allow us

[1]Because DLHS only surveyed women within the households, the data does not have information on the 0.25% mothers that suffered maternal mortality.

to study women's choice of healthcare facility conditional on their socio-economic status and ex-ante risk level. We infer prices using reported out-of-pocket costs of delivering at various facilities, and healthcare quality from information on perinatal mortality and health inputs. The DLHS also provides information on existing public sector capacity (doctors, nurses and beds) that allows us to compare outcomes across districts with different levels of capacity. Overall, the data provide uniquely rich information on several variables that together characterize the market equilibrium.

We begin by demonstrating that public and private healthcare facilities in India are vertically differentiated. First, private facilities are on average higher-quality than public facilities, which in turn provide better quality care than delivering at home. We show: (i) controlling for a mother's pre-determined risk, the likelihood of perinatal mortality is smallest at private facilities followed by public facilities, (ii) more educated and economically better-off mothers are on average more likely to deliver at private facilities, followed by public facilities, and are least likely to deliver at home and (iii) private facilities provide higher quantity and quality of health inputs (pre-natal check ups) relative to public facilities and home. Second, median out-of-pocket costs for deliveries at private facilities are approximately four times larger than median costs at public facilities.

To study the causal effects of JSY, we use a staggered difference-in-differences research design where we exploit the gradual roll-out of JSY across Indian districts. Borusyak et al. (2022) show that, in cases with very few never-treated units, as in the case of JSY, the two-way fixed effects model suffers from multi-collinearity[2] and negative weighting. We therefore use the imputation method recommended by Borusyak et al. (2022) as our primary specification. The identification assumption behind our results is the parallel trends assumption i.e., treated and untreated districts would have the same trends in outcome variables in the absence of JSY. We present evidence in support of this assumption using event studies with pre-trends.

Using a larger sample of rural as well as urban mothers, we confirm previous findings that JSY resulted in a significant increase in institutional births but failed to lower perinatal mortality. The average effect on the probability of institutional births was a sizable 8% increase in treated districts

[2]Specifically, dynamic treatment effects are not point identified in cases with no or few never-treated units.

relative to untreated districts in quarters after the policy. Over the following two years, the effect size grew to 27%. JSY was effective at targeting: poorer mothers were more likely to receive JSY incentives. We also show suggestive evidence that JSY achieved higher rates of institutional births by not only reducing costs but also by relaxing norms and information frictions around the take-up of institutional care. However, despite a significant increase in institutional deliveries, we do not find any evidence of a decline in perinatal mortality as a result of JSY. This is surprising because our descriptive evidence showed that institutional facilities provided higher-quality of care than home. Our interpretation of this result is that while mothers took up institutional care, the average quality of healthcare received did not improve.

We present evidence on three equilibrium mechanisms that contribute to the failure of JSY in reducing perinatal mortality. First, we show that JSY led to a mismatch of patient risk across facilities. From the perspective of reducing mortality, the ideal match would be where higher-risk patients get treated at the highest-quality facilities (private facilities in the case of India). Although JSY resulted in fewer deliveries at home, we find that financial incentives under JSY diverted high-risk mothers away from private facilities (highest-quality but costly care) into public facilities (lower-quality but subsidized care). While mothers saved money, they increased the risk of mortality by moving away from private facilities. Strikingly, we find that the primary intended targets of JSY, poor and high-risk mothers, experienced an 18.81% decline in the likelihood of delivering at a private facility.

Second, we show that only richer mothers adapted to deteriorating quality of care due to increased congestion at public facilities by sorting into costly private facilities. Specifically, in districts with low public sector capacity, richer (particularly, those who were ineligible for incentives under JSY) sorted out of low-cost public facilities into high-cost private facilities as a response to JSY. This finding complements Andrew and Vera-Hernández (2022), which documents that congestion from increased demand due to JSY resulted in an increase in perinatal mortality among high-risk rural mothers in districts with below median public sector capacity. We confirm their results using the entire population as opposed to a select sample of rural patients in low-performing states, and add that the quality of healthcare inputs (ante-natal checkups) received by patients declined in low public capacity districts as a result of JSY.

Finally, we show that private facilities responded to increased competition from public facilities due to JSY by increasing prices (out-of-pocket costs) without improvements in quality (as measured by the likelihood of perinatal mortality). This further restricted access to highest-quality healthcare in India. An important econometric challenge with this analysis is that JSY changed patient characteristics across public facilities, private facilities and home. We present our results using a range of specifications flexibly controlling for ex-ante patient risk and patients' socio-economic status. We show that despite an 18% decline in net prices at public facilities (due to subsidies under JSY), average private sector prices increased by a statistically insignificant 1%. Our dynamic specification shows that JSY led to a sharp decline in private sector prices in the first two quarters after treatment followed by a sharp reversion and significant increase thereafter. Consistent with the theoretical finding from Chen and Riordan (2008), we find that this increase in price was likely a result of a dominant *price sensitivity effect* (steeper residual demand) over *market share effect* (downward pressure on prices from loss of market share). Prices increased only in high-performing states by a statistically significant 4.6%, where women from high socio-economic groups were not incentivized under JSY to deliver at public facilities, implying that the incentive to lower prices due to loss in market share was weaker in high-performing states. Crucially, we find that prices also increased by 3.72% for mothers from low socio-economic groups (*below poverty line*, abbreviated as BPL).[3]

Increase in prices might have been welfare improving if private facilities had simultaneously improved healthcare quality. However, we do not find any impact of JSY on private healthcare quality as proxied by perinatal mortality across our range of specifications, despite a less risky patient composition. Another possibility is that private facilities improved amenities. We find that the increase in prices at private facilities is at least partly driven by an increase in the rate of c-sections, even for BPL mothers. While we cannot rule out that this increase in c-sections for BPL

[3]This was despite the ability to price discriminate based on mothers' socio-economic status. Our data suggests BPL mothers pay 16% lower average prices at private facilities than non-BPL mothers.

mothers is demand driven, we provide back-of-the-envelope calculations that suggest it is unlikely for BPL mothers to demand higher rates of c-sections unless medically necessary. Specifically, our data suggest that BPL mothers would have to spend about 42% of their annual household income to afford a c-section at a private facility.

It is clear from our findings that policymakers must consider equilibrium responses while designing large-scale public policies. In the case of JSY, despite being one of India's largest efforts to improve health outcomes, the intended reduction in perinatal mortality did not materialize. Our results suggest that unintended interactions between public and private facilities played an important role: high-risk patients moved from high-quality private facilities to congested public facilities and the design of JSY led to an increase in prices at private facilities in a specific subset of states making them even harder to access. Our results suggest two potential avenues of complementary policy intervention: (i) investments in public sector capacity and (ii) improving access to private sector healthcare for India's poor, potentially via targeted vouchers.

Our paper contributes to several strands of the economics literature. First, this paper reiterates the need to incorporate general equilibrium considerations in program evaluations (Acemoglu, 2010). In this instance, simply measuring the effect of JSY on increase in take-up of institutional care without a deeper study of how JSY adversely affected the quality of care received (namely via mismatch of risk across health facilities, congestion at public facilities and higher prices at private facilities) would have been of little value to understand health outcomes. Existing literature in development economics has highlighted the importance of general equilibrium considerations in transfer programs (Cunha et al., 2019; Egger et al., 2022), large-scale education reforms (Khanna, 2023), and public employment programs (Muralidharan et al., 2018). We add to this literature in the context of healthcare services in markets where public and private suppliers co-exist and are vertically differentiated.

Second, we contribute to the research on healthcare quality in developing country contexts. Previous research has emphasized the supply side of healthcare quality. Das et al. (2016) study India's primary healthcare context and show how quality of healthcare varies for public and informal private providers in rural India. Andrew and Vera-Hernández (2022) highlight the role of public sector capacity in deteriorating healthcare quality via congestion. Mohanan et al. (2021) study how input versus output based incentives for care providers affect patient outcomes in the presence of heterogeneity in doctors' skill levels. We contribute by studying the demand side: particularly, the role of incentives in accessing high-quality care. Our finding that JSY led to high-risk and poorer mothers moving away from high-quality private facilities into lower-quality public facilities shows that demand for healthcare quality can be quite elastic. Moreover, our finding that richer ("ineligible") mothers adapted to congestion at public facilities by choosing private facilities despite high prices highlights inequities in access to high-quality life-saving healthcare services. Complementary to our findings, Dupas and Jain (2023) show in the context of health insurance that patient-driven accountability can improve public service delivery.

The third strand of literature relates to the competitive effects of public sector firms. A small and recent empirical literature has explored consequences of entry of public firms on incumbent private firms. Jiménez-Hernández and Seira (2021) show that entry of public milk stores in Mexico lowered prices at private stores despite the government milk being perceived as lower-quality. On the other hand, Atal et al. (2022) study competitive effects of public entry in pharmaceuticals market and show that entry of low-quality government providers segmented the market, increasing prices at private firms. Our paper explores the price response for maternal healthcare services at private facilities in markets where the incumbent public provider lowers prices, a much subtler intervention. We find that prices at private facilities increased as a result of increased competition from public sector. The private price response in our setting is mediated by the extent to which the subsidy applied to the overall market, consistent with the theoretical findings in Chen and Riordan (2008). Cunha et al. (2019) is a somewhat related exception in studying public-private interaction in a developing country. They show that entry of public suppliers in the form of in-kind transfers reduced market prices as a result of increased supply of food.

The rest of the paper proceeds as follows. Section 1.2 briefly discusses our setting and important policy details. Section 3.3 presents details about the data, important definitions for analysis and descriptive facts. Section 1.4 and section 1.5 present empirical strategy for evaluation of JSY and results respectively. Finally, section 1.6 concludes.

## 1.2 Setting and Policy Details

### 1.2.1 Maternal healthcare system in India

Pregnant mothers in India can choose to receive maternal care at public facilities, private facilities or at home. Public sector provides two levels of care at low administratively set prices (Almeida et al., 2017). Primary public healthcare system provides basic health services via primary health centers (PHCs) which are ubiquitous but lack sophisticated infrastructure and trained doctors to deal with medical complications. The secondary public healthcare system provides advanced care through community health centers (CHCs) and large district hospitals (DHs) which are better quality but more remote. Both levels of the public system severely lack capacity.[4]

Private sector functions unregulated and is characterized by private practitioners that run for-profit health facilities. Private facilities are mostly situated in urban areas, are more remote than PHCs but less remote relative to secondary public healthcare facilities (CHCs and DHs), charge very high prices and vary widely in the level of care they provide (Das et al., 2016). To date, very little is known about private healthcare system in India; official data and balance sheets of private hospitals are plagued with widespread misreporting. In this study, we shall utilize objective information on patient-facility interaction as reported by mothers and illuminate the economics of India's private healthcare system.[5] Several statistics in our data (as demonstrated later) suggest that private facilities provide higher-quality care than public facilities on average and home births receive the lowest-quality of care.[6]

During the time period of this study, take-up of health insurance was extremely low in India

[4]India has one of the lowest rates of investment in public healthcare. Only 1.3% GDP in recent years (Narain, 2019). Further, public sector facilities are below capacity even in 2017.

[5]In on-going work, our structural analysis provides first estimates of average mark-ups at India's private hospitals, a recent policy focus in India.

[6]Note that it is conceivable that under certain circumstances, delivering at home may indeed be the highest-quality option for a mother. For instance, sudden on-set of labor may make traveling to an institutional facility more unsafe than simply delivering at home.

(close to 4% in 2005 (DLHS)). This meant that pregnant mothers faced a trade-off between receiving higher-quality care and bearing the burden of out-of-pocket costs associated with the level of care. Accessing any institutional facility (public or private) required incurring significant additional expense on transport, lodging and other indirect healthcare costs all while navigating a difficult problem of matching with ideal health facilities.

Beyond financial concerns, several features of the Indian society prevented pregnant mothers from accessing institutional healthcare (over 70% of Indian mothers reported delivering at home (DLHS)). Figure A2 presents reasons for not going to a health facility as reported by mothers prior to JSY. Other than high costs, belief that delivering at a facility was not necessary, customs, lack of family permission to visit hospitals and lack of information were important reasons for delivering at home. Other (supply-side) reasons for delivering at home included mothers reporting poor quality service at health facilities, distance as well as inadequate infrastructure at government facilities including absence of doctors or lack of beds.

As a consequence of these frictions India suffered from a high fatality rate among mothers as well as off-springs. World Bank data in Figure A1 shows that India had among the highest rates of neonatal mortality among emerging and low-income countries.

### 1.2.2 Janani Suraksha Yojana (JSY) 2005

In 2005, neonatal mortality rate per 1,000 live births was 38 in India, compared to 33 in Nepal, 27 in Bhutan, and 6 in Sri Lanka[7]. India's maternal mortality ratio per 100,000 live births in that year was 286, eclipsing Pakistan's 237 and Sri Lanka's 45[8]. In absolute terms, the country accounted for almost a third of all neonatal deaths and a fifth of all maternal deaths around the world at the time (Lim et al., 2010). Against this backdrop, the central government launched the National Rural Health Mission (NRHM) in 2005, with the stated goal of providing accessible, affordable, and quality healthcare to Indian women, especially vulnerable socioeconomic and caste groups. The *Janani Suraksha Yojana* (JSY), or the "Safe Motherhood Scheme" is one of the

[7]See https://data.worldbank.org/indicator/SH.DYN.NMRT?
[8]See https://data.worldbank.org/indicator/SH.STA.MMRT?

flagship NRHM initiatives launched in April 2005.

The main objective of JSY was to reduce maternal and infant mortality by incentivizing institutional births. Specifically, implementation of JSY had two main components: first, eligible mothers were offered a substantial cash transfer conditional on delivering at public facilities [9] and second, the government appointed and incentivized Accredited Social Health Workers (ASHA workers) for every village with a population of at least 1,000 to encourage pregnant mothers to take-up institutional care. ASHA workers were trained female community health workers, preferably between 25 to 45 years of age, who were selected by community groups and public officials from the pool of literate women in a village. They underwent training to serve as promoters of good public health practices on issues ranging from nutrition to immunization in their village[10]. Importantly, under JSY, ASHA workers also received a financial incentive for every delivery they facilitated at a public facility.

In terms of targeting, the government identified a group of ten "low-performing" states (LPS), where rates of institutional deliveries were relatively lower.[11] All women in these states were eligible to receive cash payments under JSY. The rest of India's 18 states were designated as "High Performing" (HPS) where only women meeting certain criteria were eligible for cash assistance under JSY. Only mothers that belonged to the historically disadvantaged Scheduled Castes (SC) or Scheduled Tribes (ST), *or* were older than 18 years *and* possessed a "Below Poverty Line" (BPL) card were eligible to receive cash assistance in HPS.[12] Even after these criteria were met, the benefits in HPS can only be received by mothers for their first two live births. Figure A3 shows fraction of mothers that were eligible across *high* and low-performing states. In all cases, the policy mandated that the cash be disbursed to eligible women in a single installment at the health facility

[9] While the policy guidelines allowed for JSY disbursal at accredited private hospitals too, a 2008 government assessment of the policy in rural parts of five states found that relatively little effort was made towards the accreditation of private practitioners. According to the report, just over 1% of surveyed mothers in these states had delivered in accredited private facilities, and less than 30% of women were aware of the JSY provision for accredited private hospitals (https://nhm.gov.in/WriteReadData/l892s/78619790621474872646.pdf). Therefore, our discussion of JSY eligibility and primary measure of policy coverage is restricted to births at public institutions.

[10] For information on ASHAs, see https://nhm.gov.in/index1.php?lang=1&level=1&sublinkid=150&lid=226.

[11] The LPS included Uttar Pradesh, Uttarakhand, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Assam, Rajasthan, Orissa, and Jammu and Kashmir.

[12] Ownership of a BPL card is the most important determinant of eligibility for welfare assistance in India.

itself, no later than a week after delivery. Table 1.1 presents relevant details on cash incentives for pregnant mothers and ASHAs under JSY. As a benchmark, the cash incentive under JSY was roughly equal to the average reported out-of-cost for a normal (vaginal) birth at a public health facility.

Overall, the policy provided a significant demand stimulus by reducing out-of-pocket costs as well as by reducing information barriers and weakening norms against women's use of maternal healthcare through the work of ASHA workers. However, this large demand stimulus was largely unmatched from the supply side: public hospitals continued providing sub-standard quality of treatment and severely lacked capacity in terms of physical infrastructure (number of beds per 10,000 people) as well as medical expertise (number of obstetrician-gynecologists (OBGYNs) and nursing staff). Pandey and Sharma (2017) show that increasing experts at India's public facilities has been exceptionally difficult. Between 2005-2010, the number of OBGYNS at public facilities increased by just 2.7%. Section 3.3 presents evidence that no additional effort was made to enhance public infrastructure to accommodate increased demand. Consequently, as we shall demonstrate, the quality of overall healthcare services received at public facilities indeed declined. Lastly, JSY (and Indian government's larger healthcare agenda) largely ignored private healthcare sector despite a heavy concentration of skill and infrastructure at private facilities.

## 1.3 Data, definitions and descriptives

### 1.3.1 Data sources

Data for our analysis primarily comes from repeated cross-sections of the District Level Household Survey (DLHS), which is a nationally representative survey designed to provide indicators of maternal and child health, as well as access to public healthcare services, across India. We use data from the second, third, and fourth rounds of the DLHS, which were conducted in 2002-04, 2007-08, and 2012-14[13], respectively. In each round, women were surveyed about their overall birth history but detailed information was collected only for the last birth for each mother. We use

[13]The fourth round of DLHS only collected data from *high-performing states*.

detailed information on the last birth for our main analysis and utilize information on outcomes of previous births as supplemental information to assess the ex-ante riskiness of a mother. Note that, because DLHS surveyed mothers within households, we do not have information for 0.25% of the mothers that suffered maternal mortality in our period of analysis.

Crucially, for a mother's last birth, we have information on the outcome of birth (whether live birth, still birth or induced/spontaneous abortion), birth order, year and month of birth, place of birth (whether a public facility, private facility or home)[14], whether mother received JSY cash incentive or ASHA assistance, type of procedure (vaginal or cesarean section), quality of ante-natal and post-natal care, detailed information on pre and post labor birth related complications as well as whether a child is alive or dead (in case of death, we observe the age at death in number of days). Additionally, we observe socio-economic as well as demographic information (age, education status, religious group, and caste affiliation) for these households. We infer prices at facilities from reported out-of-pocket expenditure which we normalize to constant 2010 Indian rupees using IMF's consumer price index data. Our main measure of socio-economic status is whether a mother possessed a below poverty line (BPL) card[15]. Ownership of a BPL card is a major determinant of eligibility for social assistance in India.

To create our final sample, we first assign each mother in DLHS 3 and DLHS 4 to the district they would have been in if district boundaries had not changed over the years. Districts in our sample correspond to the boundaries as given in the 2001 census of India. Districts in DLHS 2 were found to be exactly the same as in the 2001 census of India. We stack data from all rounds of the DLHS. This gives us a full sample of 289,544 "most recent births," with each observation

[14] We classify each institutional birth as either: (i) public facility birth that includes deliveries at anganwadis, sub centers (SCs), primary health centers (PHCs), community health centers (CHCs), urban health centers (UHCs), district hospitals, and public university medical centers, or (ii) private facility birth that includes deliveries at private clinics, private hospitals, and private university medical centers.

[15] The second round of DLHS does not ask whether respondents possessed a BPL card. For this round, we use housing quality as a proxy for socio-economic status. In DLHS 2, enumerators classify each respondent's dwelling as either *kaccha*, *semi-pucca*, or *pucca* (in increasing order of quality). This categorization takes into account the materials used to construct the roof, wall, and floor of the housing. Roughly, a *kaccha* dwelling is built using mud, clay, and straw/bamboo, *semi-pucca* places rely on wood and metal sheets, whereas *pucca* houses are constructed using concrete. Owing to our finding that kaccha household was most likely to possess a BPL card in later rounds of DLHS, we classify such households as BPL households and the rest as non-BPL households.

corresponding to a unique mother. This set of observations spans 592 unique districts across 34 states and union territories.

Each round of DLHS contains a survey of village characteristics that can be linked to the data on households and mothers. Specifically, we have information on distances to nearest town, railway station, bus station, and a variety of public and private health facilities. In addition, the survey records distance to the district headquarters and whether the village has access to an all-weather road.

DLHS also features information on the public healthcare infrastructure in each district. The information includes the number of beds, nursing staff and doctors on government health facilities at the district level in rounds 2 and 3 for a subset of the sample. We modify this information using district level population from the 2001 and 2011 census. We calculate interpolated population for years 2002 (DLHS 2) and 2008 (DLHS 3) for districts as in census 2001. We normalize each of our three capacity variables by 10,000 persons in each district.

Table A1 presents descriptive information on our final sample across Indian districts. Three observations are worth noting. First, public capacity is severely lacking. India's median district in our sample has 16.5 beds, 0.1 OBGYN and only 2.1 nursing staffs per 100,000 persons. Second, average out-of-pocket cost at private facilities are about 4 times larger than average costs at public facilities and 12 times larger than delivering at home. Third, for the median district, district hospitals (highest level of public sector care) are twice as far from the nearest private facility. Acharya and McNamee (2009) show that a significant fraction of maternal deaths happened while in transit to far away district hospitals.

### 1.3.2 Definitions

For our analysis, we need to define three key variables that are not directly observed in our data. Using data-driven methods, we define a discrete treatment status at the level of a district-quarter, a pre-JSY capacity measure at the district level and an ex-ante risk level for each mother. We discuss each of our definitions in detail and suggest robustness checks where appropriate.

**Treatment status**

To construct our primary treatment variable, we rely on responses to a question asking whether mothers received any financial assistance from the government for delivery care under JSY or an existing related state scheme. Following Andrew and Vera-Hernández (2022), we define the quarter of treatment for a district under JSY if the following criteria are met: at least 25% of eligible women[16] must report receiving financial assistance in the given quarter and the same fraction of women must report receiving financial assistance over the following year.[17] We force the latter requirement that 25% women must receive cash assistance over the following year in order to avoid falsely assigning treatment status to a district owing simply to sampling errors. Once the district meets this criteria, we consider that district treated under JSY for all following quarters. That is, the treatment status is absorbing. One advantage of this classification is that while JSY was announced in the second quarter of 2005, the actual roll-out happened overtime as necessary personnel and public frameworks were put in place. Our measure considers the ground-truth about the actual roll-out of JSY and is not affected by incentives to inflate measures of roll-out at the administrative level. Secondly, this classification provides us with discrete treatment status that allows for clean comparisons of treated and untreated districts overtime (Borusyak et al., 2022; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). Balance Table A3 shows statistical differences between districts that were treated early (among first 50% districts to be treated) vs. districts that were treated later. The statistics are largely balanced with some evidence that districts with lower education levels and higher fraction of BPL households were treated early.

We test robustness of our results by: (i) redefining treatment status by different cut-offs (15%, 20% and 30%) and (ii) defining a continuous treatment variable, following Powell-Jackson et al. (2015), called "JSY *intensity*" as the proportion of all *eligible* women delivering in public facilities in a district-year who reported receiving government cash assistance. Zero intensity implies that

[16] Eligibility only matters for high-performing States (HPS)

[17] For example, if 25% women in a district report receiving financial assistance in the fourth quarter after the official announcement of JSY, in order to be considered treated, at least 25% women must also report receiving cash incentive on average over quarters fifth through eighth.

there were no JSY recipients in that district-year, while an intensity of one means that all eligible women who gave birth in a government facility in that district-year were beneficiaries of the policy. In order to isolate the effect of JSY specifically, we set the *intensity* measure to zero prior to the launch of JSY in the second quarter of 2005.

Figure A4 presents a visualization of rollout of JSY across Indian districts using our continuous intensity variable. Reassuringly, we find that our two measures, discrete and continuous, are very strongly correlated: a regression of our discrete treatment variable on the continuous measure gives a coefficient of $0.78^{***}$ (F-statistic: 4911).

**District level public capacity**

To assess the effects of JSY by district level public sector capacity, we use the three available measures in our data: number of OBGYNs, number of nurses and number of beds. Andrew and Vera-Hernández (2022) show that a large fraction of Indian districts fell short on the Indian Public Health Standards (IPHS) of public hospital capacity on all three of our measures. Since Andrew and Vera-Hernández (2022) show that effects of JSY varied only by the capacity at secondary health care facilities, we restrict our analysis to only the number of beds, doctors and nursing staff at secondary healthcare facilities normalized by 10,000 persons.

Our primary measure of pre-JSY secondary level public healthcare capacity in a district is the number of obstetrician gynecologists (OBGYNs) per 10,000 persons in a district in DLHS 2. Our choice is based on several facts. First, as mentioned earlier, India's public sector facilities severely lack medical experts: the median district has 0.1 OBGYNs for every 100,000 persons. Second, lack of medical expertise at public hospitals is a highly cited reason for lack of quality at public hospitals.[18]. Third, Pandey and Sharma (2017) show that increasing experts at India's public facilities has been exceptionally difficult. Between 2005-2010, while the number of CHCs (secondary level public health care facilities) increased by 35%, the number of OBGYNS at public facilities increased by just 2.7%. Reassuringly, all three of our variables on public hospital capacity

[18]See, for example https://www.indiaspend.com/83-shortage-of-specialists-in-community-health-centres-26127/

(OBGYNs, beds and nurses) are highly correlated.

For our regression analysis, we discretize our continuous measure of public sector capacity (number of OBGYNs per 10,000 persons) based on whether a district has above (or below) median value of capacity as reported in DLHS 2. Balance Table A4 presents evidence on balance on observables in low-capacity vs. high-capacity districts. We see that high capacity districts have higher overall rates of institutional births overall (higher rates of public facility births along with lower rates of private facility and home births). High capacity districts also offer higher quantity (whether mother received at least 3 ANC tests) and quality (whether at least 6 out of 8 tests were conducted during ANC) of health inputs than low capacity districts.

For robustness checks, we use all three variables on capacity to create a district level capacity index based on the first principle component of the number of beds, number of doctors as well as nursing staff at secondary care facilities. Table A5 presents the factor loadings from our principle component analysis.

Lastly, we show evidence using our defined JSY treatment variable that there was no differential increase in public capacity for treated vs. control districts using our two cross-sections from the DLHS 2 and DLHS 3 (see Table A6). Using a simple difference-in-differences specification, we find that treated districts did not receive additional capacity improvements relative to untreated districts. Thus, it appears that the government essentially rolled out a large-scale incentive scheme without investing in healthcare capacity.

**Ex-ante risk level**

Presence of various kinds of healthcare facilities offering different quality of care makes it inevitable that heterogenous patients will sort into different facilities. An important factor to consider in our context is an individual's ex-ante risk level. We build a measure of a mother's ex-ante risk levels. We extract detailed information about patient characteristics that are plausibly exogenously given by the time a patient decides to avail medical care for her most recent delivery. Specifically, we enlist 20 such health related variables including pre-labor complications [19], history of complications in previous deliveries[20] as well as age dummies and birth-order of the current pregnancy. In order to estimate the risk level of a patient, we run a linear regression of perinatal mortality on our health indicators and assign each patient a predicted mortality risk. Table A2 presents regression results. For our regression analysis, we define a high-risk patient as one with above median predicted mortality risk.

#### 1.3.3 Descriptive facts

We present three descriptive patterns in our data that are most relevant to our analysis. In our presentation of the facts, we define four different types of patients based on their socio-economic status (as captured by whether a mother owns a *below poverty line* - BPL card) and ex-ante risk level (whether a mother is above or below the median level of risk). This gives us the following four types of patients: BPL/High-Risk, Non-BPL/High-Risk, BPL/Low-Risk and Non-BPL/Low-Risk.

**Fact 1: mothers sort into institutional care by SES and risk level** Figure 1.1 presents a snapshot of sorting patterns across healthcare facilities before and after JSY by patient types. Strikingly, over 70% mothers in India chose to deliver at home prior to JSY. This proportion fell precipitously after the introduction of JSY.[21] Moreover, we see that our classification of the sample into four types does appear to be relevant for patient sorting. We observe that conditional on socio-economic status, high-risk mothers are more likely to take-up institutional care and conditional on ex-ante risk, richer mothers are more likely to take up institutional care.

**Fact 2: Average quality of care is highest at private facilities followed by public facilities and home** We first show that patients' choice of where to deliver matters for perinatal mortality. Columns (1)-(5) in Table A7 show results from a linear regression of a dummy for perinatal

[19] For example, swelling, paleness, visual disturbance, fatigue, convulsion, abnormal position etc.

[20] For example, previous abortions or still-births.

[21] It is worth noting that this figure does not necessarily present treatment effect of JSY but likely a combination of time-trends and treatment effects.

mortality on place of birth controlling for different sets of explanatory variables including pre-determined risk for a mother. The home option is the omitted category. Columns (1)-(3) show that controlling for pre-determined risk, likelihood of perinatal mortality is lowest at private facilities, followed by public facilities. Columns (4)-(5) show that this reduction in likelihood of perinatal death is coming from high-risk mothers.

Moreover, several statistics in our data suggest that average quality of treatment is highest at private facilities, followed by public facilities whereas home deliveries receive the lowest-quality of care.[22] This is in line with the findings in Das et al. (2016). Table 1.2 presents raw statistics from our data that capture patient sorting across facilities. Firstly, richer, urban and higher educated households prefer private facilities, followed by public facilities and lastly home. Secondly, average quantity and quality of treatment also varies across facilities. We see that the likelihood of receiving at least three ante-natal checkups and the likelihood that at least six out of eight tests were conducted in each of the ante-natal checkups is highest for private facility births followed by public facility births and lastly followed by home births.

**Fact 3: Median out-of-pocket costs are very high at private facilities** Private healthcare sector in India is largely unregulated and consists of privately operated facilities that set prices and quality to maximize profit. In contrast, public sector quality and prices are set "administratively" and "outside the market" (see Almeida et al 2017). Given this market setup, we observe two main differences in prices across public and private sectors (shown in Figure 1.3). First, median out-of-pocket costs at private sector are about 4 times larger than public sector. Second, we see that out-of-pocket costs for private sector differ by patient type suggesting price discrimination[23], whereas this is not the case at public sector hospitals.

[22]Unfortunately, we do not have healthcare quality indicators at individual hospitals therefore, we conduct our analysis in an environment where a patient can choose of one of the three broad buckets of facilities (private, public or home).

[23]Some of the difference in prices are driven by procedures. For instance, high-risk mothers are more likely to receive the more expensive c-section procedures.

## 1.4 Main econometric specification

The roll-out of JSY across Indian districts over-time naturally motivates a staggered difference-in-differences (DiD) research design. Several features of our setting require us to deviate from the usual two-way fixed effects specification estimated using OLS with some lags and leads of treatment. In addition to concerns about treatment effect heterogeneity (Borusyak et al., 2022; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021), our setting also has no never-treated units (districts) leading to under-identification in the usual event study specification. Figure 1.4 shows cumulative density of treated districts over-time. We see that by 2009, all districts in our sample were treated under JSY.

Owing to these, we follow the imputation based estimation procedure proposed by Borusyak et al. (2022). We begin our analysis with the following (assumed) true causal model for our outcomes of interest:

$$Y_{ibdt} = \alpha_d + \beta_b + \gamma_t + \tau_{it}.JSY_{dt} + \epsilon_{ibdt} \tag{1.1}$$

Here, $Y_{ibdt}$ represents the outcome variable of interest that varies at the level of an individual $i$, birth order $b$, district $d$ and quarter of birth $t$. $\alpha_d$ and $\gamma_t$ represent district and quarter of birth fixed effects respectively. Since our data only has detailed information for a mother's last birth, we also include a birth order fixed effect, represented by $\beta_b$, to account for unobservables specific to the birth order. $JSY_{dt}$ is an indicator variable that takes a value 1 if a district is treated (adopts JSY) and 0 otherwise. Once a district is treated, it remains treated for all the following periods. That is, treatment is an absorbing state. Our model shall compare treated districts with yet-to-be treated districts, before and after JSY. $\tau_{it}$ captures the heterogenous treatment effect of JSY that can vary by individual and quarter. Finally, $\epsilon_{ibdt}$ captures idiosyncratic error that satisfies: $E[\epsilon_{ibdt}|\alpha_d, \beta_b, \gamma_t, JSY_{dt}] = 0$. We cluster standard errors at the district level, our unit of treatment.

We construct the 'imputation estimator' in three steps. First, we estimate Equation 3.1 using OLS on the untreated sample, that is, those with $JSY_{dt} = 0$. This gives us the estimates of expected counterfactual outcome in the absence of treatment, conditional on the birth order, $E[Y_{ibdt}(0)|\beta_b]$, given by $\hat{\alpha}_d + \hat{\gamma}_t + \hat{\beta}_b$. Second, for all treated observations, we build an estimate of $\tau_{it}$ given by: $\hat{\tau}_{it} = Y_{ibdt} - (\hat{\alpha}_d + \hat{\gamma}_t + \hat{\beta}_b)$. Finally, we average these unbiased estimates of heterogenous treatment effects following Borusyak et al. (2022). This final step gives us consistent estimates of the average treatment effect. We present average treatment effect over the entire sample as well as over horizons (quarters) weighting each observation equally. For dynamic effects of JSY over different horizons ($h$) after treatment, we compare treated districts against untreated districts in a given $h$ relative to periods before treatment and present averages across all observations in $h$ weighted equally.

The interpretation of our results relies on the parallel trends assumption: absent JSY, treated and un-treated districts have the same trends in outcome variables. We provide support for this assumption by testing pre-trends as recommended in Borusyak et al. (2022). We estimate the following regression on all untreated observations for five quarters before the roll-out of JSY:

$$Y_{ibdt} = \alpha_d + \beta_b + \gamma_t + \sum_{h=-5}^{-1} \tau_h.1[t = E_d + h] + \epsilon_{ibdt} \tag{1.2}$$

Here, $E_d$ represents the quarter of treatment for district $d$ and $1[t = E_d + h]$ represents dummy variables that takes a value 1 for districts $h$ periods after treatment. The comparison group includes all quarters before five quarters to the treatment. Finally, a joint-test of all $\tau_h = 0$ suggests absence of differential pre-trends across treated and untreated districts.

## 1.5 Reduced-form results

### 1.5.1 Impact of JSY on healthcare take up and mortality

We begin by first presenting evidence on take-up of institutional care and perinatal mortality. To study the effect of JSY on take-up of institutional care, we use a dummy variable that takes value 1 if mother $i$ delivered at an institutional facility (either public or private): $Y_{ibdt} = 1[\text{Institutional Delivery}]$ as our dependent variable in Equation 3.1. In Table 1.3, we present the average treatment effect of JSY. We find that overall, JSY led to an 8.1% increase in the probability of delivering at a medical facility (Column 1 in Panel A of Table 1.3).

Figure 1.5 shows dynamic effects of JSY on take-up of institutional care over twelve quarters post roll-out. We find that the effect of JSY gradually increased overtime and by the end of two years, mothers in treated districts were nearly 10 percentage points (27% higher than pre-JSY) more likely to deliver at an institutional facility relative to mothers in yet-to-be-treated districts. Our estimated effect is slightly smaller than other evaluations of JSY (Powell-Jackson et al., 2015; Andrew and Vera-Hernández, 2022) primarily because these papers limit their sample to only rural mothers whereas our results are average effects over the entire population, since we are interested in equilibrium effects. We find suggestive evidence that in addition to lowering costs, JSY achieved the increase in institutional births by relaxing customs, norms, family restrictions and knowledge gaps against accessing institutional healthcare. Figure A5 presents results from difference-in-differences regressions using several reported reasons for delivering at home as dependent variables on a district's treatment status under JSY for a sub-sample of women that delivered at home. We find that in treated districts, women delivering at home were less likely to report high costs, restrictive customs, lack of knowledge or lack of family permission as reasons for delivering at home.

We also find evidence that JSY was able to effectively target mothers with lower socio-economic status. Columns (2)-(3) in Panel A of Table 1.3 show that the average effect of JSY for BPL and non-BPL mothers was 16% and 4% respectively. Event studies in Figure A6 confirm this heterogeneity. Among BPL households, the effect was larger for high-risk mothers relative to low-risk mothers (columns (1)-(2) of Table A8 and panels (a) and (b) in Figure A7) suggesting that high-risk BPL mothers responded to the subsidy more than low-risk BPL mothers. The story is different for non-BPL mothers where low-risk mothers responded to JSY more than high-risk mothers who were already significantly more likely to give birth at a health facility (columns (3)-(4) of Table A8 and panels (c) and (d) in Figure A7).

Next, we present results on perinatal mortality. We use a dummy variable that takes value 1 if mother $i$ experienced perinatal mortality: $Y_{ibdt} = 1[Perinatal\ Mortality]$ as our dependent variable in Equation 3.1. In line with the literature, we find that JSY did not significantly affect likelihood of perinatal mortality (column (1) in Panel B of Table 1.3). Figure 1.6 presents dynamic effects of JSY on perinatal mortality: all quarterly coefficients are statistically indistinguishable from zero. We find no effect of JSY on either the BPL or non-BPL sub-samples (columns (2)-(3) in Panel B of Table 1.3 and Figure A8). We also find no effect of JSY on either the high-risk or low-risk sub-samples (columns (4)-(5) and Panel B of Table 1.3 and Figure A9).

Finally, we study the effects of JSY on out-of-pocket (OOP) costs across our sample. We use reported OOP costs in constant Indian rupees as our dependent variable in Equation 3.1. Intuitively, the effect of JSY on OOP costs depends on the overall sorting of patients across our three groups of facilities. Recall, our descriptive statistics in Table A1 showed that, on average, private facilities charged the highest prices followed by public facilities and finally followed by home. Since JSY incentivized deliveries at public facilities, moving from home to a public facility would, on average, imply higher net prices whereas moving out of private facilities and staying at public facilities would imply lower prices as a result of the substantial subsidy under JSY.[24] Panel C of Table 1.3 presents our results on average OOP costs paid by patients. Column (1) shows that, on average, JSY did not have a significant effect on average out of pocket costs for consumers. Figure A10 presents results from our dynamic specification and confirms our null result. Splitting the sample by BPL status reveals that out-of-pocket costs remained unchanged for both BPL and non-BPL households (Columns (2)-(3) in panel C of Table 1.3 and Figure A11).

Overall, our results suggest that while JSY was effective in targeting and inducing pregnant mothers to take-up institutional healthcare, it failed to lower the incidence of perinatal mortality. It is worth emphasising that JSY increased take-up of institutional care without increasing average OOP costs. Recall, Table A7 shows that likelihood of perinatal mortality is lower for institutional births despite higher levels of average patient risk. In light of this, our null result on perinatal mortality suggests an overall worsening of healthcare quality received by mothers at institutional

[24]We later show that JSY did not induce a substantial price reduction at private facilities despite increased competition.

facilities.

### 1.5.2 Equilibrium responses that explain the failure of JSY

Given that JSY was one of the largest public health schemes around 2005, its failure presents a policy conundrum for Indian policy-makers. We next propose three equilibrium responses that contribute to this failure. We show that JSY: (1) resulted in a mis-match of patient risk across facilities, (2) in response to congestion and deterioration of care at public facilities (Andrew and Vera-Hernández (2022)), only mothers with high socio-economic status sorted out of congested public facilities into more expensive private facilities and (3) induced price increase at private facilities without quality improvements despite a substantial increase in competition from public hospitals. This increase in price made private facilities even less accessible.

**JSY resulted in mismatch of risk across facilities**

Presumably, an ideal match would be where higher risk patients get treated at highest-quality facilities (private facilities in the case of India). We find evidence that financial incentives under JSY diverted high-risk mothers out of private facilities (highest-quality care) into public facilities (lower-quality care).

In our exposition, we use three dummy variables as our dependent variables that take value 1 if mother $i$ delivered at either of the three choices available: $Y_{ibdt} = 1[Choice = c]$ where $c \in (Private, Public, Home)$ in Equation 3.1.[25] Since in this context patients necessarily substitute from one choice to another, our results should be interpreted as relative changes in equilibrium choices.

We begin by presenting patient sorting across private facilities, public facilities and home (presented in Figure 1.7). Overall we find that as a result of JSY, public facilities gained market share at the expense of private facilities and home. Public facilities received a net increase in market share i.e., a 22% increase over the baseline 18% market share (see column (1) of Table 1.4) while

[25]Note that our results for $c = Home$ are mirror images for our results on institutional deliveries presented earlier (see Figure A7a).

the market share of home and private facility births fell by 4.5% and 6.7%, respectively, over their respective baseline shares of 64% (see column (1) of Table 1.4) and 17% (see column (1) of Table 1.4).

Our interpretation of this finding is that while sorting out of the home choice improves health-care quality on average, a significant fraction of mothers that sorted out of private facilities which on average provide highest-quality of care, received worse quality of care.

Next, we explore the characteristics of patients that sorted out of private facilities due to JSY. Intuitively, if low-risk mothers who anyway did not require high-quality private sector services sorted out of private facilities, their reallocation might not adversely affect health outcomes. Instead, upon splitting our sample between high and low-risk mothers, we find that decline in private facility births was driven by high-risk mothers: 6.4% for high-risk mothers compared to 1.7% for low-risk mothers (see Figure 1.8 and columns (4)-(5) in panel B of Table 1.4)

Finally, we explore the socio-economic characteristics of the high-risk patients that switched out of private facilities. Column (2) in Table A9 shows that BPL and high-risk mothers were most likely (nearly 19% over baseline mean) to move out of private facilities among our four types of patients. This confirms that the primary intended targets of JSY, poor and high-risk mothers, lost out on highest-quality private healthcare.

One caveat with the discussion of quality is that private sector healthcare quality varies wildly across private facilities (Das et al., 2016) and we cannot confirm that the private facilities accessed by BPL mothers were indeed better quality than the public facilities they moved to as a response to JSY. One reassuring fact in our data is that BPL mothers' choice of private facilities were much more expensive than public facilities. This suggests an intent to find higher-quality care by paying more for private facilities (see Figure 1.3).

#### JSY caused congestion at public facilities

Next, we present evidence that quality of treatment at public facilities deteriorated as a result of congestion using revealed-preference from mothers' sorting behaviour. Andrew and Vera-

Hernández (2022) specifically highlight the role of congestion in the failure of JSY to reduce perinatal mortality. They show that JSY led to an increase in perinatal mortality among high-risk rural mothers in districts with below median public sector capacity in low-performing states (LPS). Our paper complements the findings from Andrew and Vera-Hernández (2022). First, we replicate their evidence of congestion (declining healthcare quality) using the entire population as opposed to a select sample of rural patients in LPS. Secondly, we show that richer mothers were able to adapt to worsening public sector quality by sorting out of public facilities and into more expensive private facilities in districts with low public sector capacity.

We start by first showing that public sector capacity was consequential for the impact of JSY on institutional births. Figure A13 shows that JSY led to a higher dynamic increase in institutional births in high capacity districts relative to low capacity districts. Columns (1)-(2) of Table 1.5 presents average treatment effects. We see that JSY lead to a 14% and 4% increase in the likelihood of institutional births in high and low capacity districts respectively.

Next, we replicate the results from Andrew and Vera-Hernández (2022) using our larger sample. Columns (1)-(2) of Table 1.6 show the effect on mortality for the high-risk mothers across low and high public capacity districts. We see that high-risk mothers in low capacity district experienced a statistically significant increase in the likelihood of perinatal mortality, while the likelihood of perinatal death remained unchanged in high capacity districts. Moreover, columns (3)-(8) of Table 1.6 present evidence that mothers in low capacity districts received worse level of care. Specifically, mothers in low capacity districts experienced a statistically significant decline in the quality of ante-natal checkups as measured by a dummy variable that takes a value 1 if a mother received at least 6 out of 8 tests reported in DLHS during each ante-natal check-up (see columns (7)-(8) of Table 1.6).

Finally, we present evidence that richer mothers in low capacity districts adapted to declining quality in public facilities by opting out of less-expensive public facilities in favor of more expensive private facilities. We begin by pointing out pertinent facts that suggest that sorting across facilities reflects a mother's (demand-side) trade-off between perceived quality (or utility) of treatment at a given facility and the cost of treatment, rather than a supply-side phenomena where facilities turn down patients. First, there are no hard quantity cut-offs at public facilities. In our data, only 0.5% of women not delivering at public hospitals reported being referred (DLHS 2). Second, anecdotal evidence shows that patients often wait in long lines at public facilities but are not refused treatment.

To present clean results on adaptation behaviour of richer mothers, we use the eligibility criteria as a measure of SES instead of whether a mother was above or below the poverty line (BPL status). This is because, even non-BPL mothers were incentivized under JSY in low-performing states whereas “ineligible" mothers (only in HPS) were not incentivized under JSY. First, we find that JSY led to an increase in public facility births for the “eligible" mothers by 33% and a decrease in public facility births among richer “ineligible" mothers by 7.5% (columns (1)-(2) of Table 1.7 and panels (a)-(b) in Figure 1.9). Second, majority (63%) of the “ineligible" mothers displaced from public facilities sorted into private facilities (column (4) of Table 1.7) while almost all the decline in private sector's market share was driven by “eligible" mothers (see panels (c)-(d) in Figure 1.9). Finally, columns (5)-(6) of Table 1.7 and Figure 1.10 show that the movement out of public facilities by “ineligible" mothers was driven by districts with low public sector capacity. This confirms that ineligible mothers experienced a dis-utility from delivering at public facilities post JSY especially in districts with low public sector capacity. This *crowding-out* could either imply a behavioural response to JSY by “ineligible" mothers[26] or a response to declining quality at public facilities. Our data provides support for the latter in two ways: first, our previous results from Table 1.6 show that mothers received worse quality of care in low public capacity districts and second, we show in Figure A14 and columns (7)-(8) in Table 1.7 that “ineligible" mothers that sorted out of public facilities were more likely to be high-risk mothers.

[26] For example, dis-utility from being surrounded by poor mothers

### Private sector quality and prices

Next, we evaluate the private sector's response to JSY. Private sector plays a crucial role in India's healthcare infrastructure for two reasons: first, private hospitals provide the highest-quality of care on average and second, anecdotally, private hospitals comprise a large fraction of OBGYNs and maternity beds in India.[27]

We evaluate the private sector's response on prices (out-of-pocket costs in Constant INR), and quality as measured by the likelihood of perinatal mortality and several health inputs in our data. One important challenge with this analysis is that JSY changed patient characteristics across facilities. Unlike the case of goods (for instance, milk in Jiménez-Hernández and Seira (2021)), delivery of (medical) services can be heterogenous across patients thereby making patient-patient comparison difficult in the presence of selection. To overcome selection concerns, we present regression results for a range of specifications increasingly and flexibly controlling for ex-ante patient risk and mother's socio-economic status. Moreover, we augment our main difference-in-differences specification laid out in section 1.4 with a third difference taken over the home option (the outside option) to capture relative changes in prices and quality.

We start by presenting our triple difference results on prices as measured by reported out-of-pocket costs expressed in constant Indian rupees. Table 1.8 presents our results on the effect of JSY on prices while increasingly and flexibly controlling for patient's ex-ante risk and BPL status. As expected, we find a sharp and stable decline in out-of-pocket costs at public facilities. As columns (2),(4) and (6) in Panel A of Table 1.8 show, JSY reduced prices at public facilities on average by 18%. This finding is confirmed in our event studies shown in panel (b) in Figure 1.11. Our results on consumer sorting from subsubsection 1.5.2 showed that incentives under JSY reduced demand for private facilities. These two combined, suggest that private hospitals faced significant competitive pressure from public facilities. If this increase in competitive pressure could successfully lower private sector prices while maintaining quality of treatment at private facilities, JSY would have

[27] No official figures are available for the time period of this study. Recent surveys claim that about 60% OBGYNs in India have a private practice.

indirectly improved access to high-quality care. On the contrary, columns (1),(3) and (5) in Panel A of Table 1.8 consistently show JSY led to a statistically insignificant increase in private hospital prices by approximately 1% on average. To explore the dynamics of private sector's response to JSY, we present event studies of our triple difference estimates in panel (a) in Figure 1.11. We find a significant price decline in the initial two quarters after the roll-out of JSY (6%), but a sharp reversion and increase in prices thereafter.

We test whether JSY affected quality of service at private hospitals. Using perinatal mortality as a measure of quality, we show our triple difference estimates in Table 1.9. We see that JSY did not have a significant effect on perinatal mortality at private facilities. Event studies in Figure 1.13 provide visual support for this finding. Note that this result is interesting in light of our finding that JSY led to high-risk patients leaving the private option.[28] We further probe healthcare inputs (quantity and quality of ANC checkups) at private facilities in columns (4)-(6) in Table 1.9. We find mixed evidence: while number of ANC checkups increased, the quality of these ANC checkups (measured by whether the patient received at least 6 out of 8 tests during ANC) declined. Overall, we see no clear evidence of an improvement in healthcare quality at private facilities.

Next, we present evidence on forces that explain this increase in price. Theoretically, Chen and Riordan (2008) (see section C for a discussion) show that increased competition can lead to an increase in price if the *price sensitivity effect* (steeper residual demand) dominates the *market share effect* (downward pressure on prices from loss of market share). This is consistent with our findings. There are two features of JSY that can potentially give rise to *price sensitivity effect* dominating *market share effect*. First, variation in the coverage of incentives across markets. Specifically, the fact that high SES mothers in high-performing states were not offered cash incentives. And second, quality deterioration due to congestion at public facilities.

While we do not find any evidence of a differential price increase in low capacity districts compared to high capacity districts (see columns (1)-(2) of Table 1.10 and Figure A16), we find that

[28] Therefore, if quality of service remained unchanged at private facilities, perinatal mortality should have declined simply as a result of a safer patient composition. Our finding that perinatal mortality remained unchanged at private facilities could either mean a decline in healthcare quality at private facilities or that the decline in overall level of risk was not enough to change perinatal mortality.

the increase in price is largely driven by high-performing states (see columns (3)-(4) of Table 1.10 and Figure 1.12). Mothers in high-performing states experienced a 4.6% increase in price at the private option. This is consistent with a dominant *price sensitivity effect* for private facilities in high-performing states as a result of weak *market share effect* due to lack of incentives for high SES mothers under JSY. We also find that private facilities increased prices for BPL mothers in high-performing states by 3.72% despite an ability to price discriminate based on mothers' socio-economic status. [29]. As far as providing access to high-quality healthcare is concerned, this could potentially further deter poorer women from accessing private facilities.

Prices at private facilities could increase as a result of improvements in amenities. In Table A11, we show that price increase at private facilities is at least in-part driven by an increase in c-sections even for BPL mothers. Our data suggests that increase in c-sections for BPL mothers, while possible, is unlikely to be driven by demand as opposed to medical necessity: BPL mothers will have to spend 42% of their annual household income to pay for a c-section at a private facility on average.

Overall, we find that JSY led to an increase in out-of-pocket costs at private facilities without improving healthcare quality at private facilities, ultimately reducing welfare for mothers choosing the private option but also deterring access to the highest-quality of care.

### 1.5.3 Robustness of our results

section B presents extensive evidence that our main results are robust to several alternate definitions of a district's treatment status and a district's public sector capacity. For a district's treatment status, we use two kinds of alternate definitions: (i) we define three discrete treatment variables for JSY (as in subsection 1.3.2) using cutoff values of 15%, 20% and 30% and (ii) we construct a continuous variable called *JSY intensity* (following Powell-Jackson et al. (2015)) defined as the fraction of all *eligible* mothers who reported receiving government assistance under JSY. For a district's public sector capacity, we use a measure of public facility capacity index created using

[29]Our data suggests BPL mothers pay 16% lower average prices at private facilities than non-BPL mothers.

first principal components of the three capacity variables observed in our data (OBGYNs, nurses and beds), each normalized by 10,000 persons. We show that across all our definitions JSY increased the likelihood of institutional births but failed to lower likelihood of perinatal death. We then present evidence of robustness of results for our three equilibrium mechanisms that lowered quality of healthcare that mothers received at institutional facilities.

### 1.6 Conclusion and policy implications

In this paper, we study the equilibrium effects of incentivizing public services in the presence of both, public and private suppliers. We study one of India's largest welfare schemes, *Janani Suraksha Yojana* (JSY), which offered subsidies to pregnant women in India to avail themselves of institutional healthcare at public facilities with a goal to lower maternal and perinatal mortality. Using staggered roll-out of JSY across Indian districts, we confirm the prior findings that despite a large increase in a mother's probability of delivering at an institutional facility (almost 27% increase two years after roll-out), JSY was unable to lower perinatal mortality.

Given the scale of this policy, its failure poses a conundrum for Indian policymakers. This paper highlights the role of interactions between public and private suppliers in shaping important economics outcomes with an aim to improve our understanding of effectively designing public policies at-scale. We provide evidence of three equilibrium responses that contribute to this policy failure. First, we show that JSY resulted in a mismatch between patient risk and healthcare facilities. We use several statistics in our data to argue, first, that private facilities offered the highest-quality healthcare. We then show that JSY induced high-risk mothers to sort away from private facilities into lower-quality options.

Second, we show that the Indian government's negligence towards improving public sector healthcare capacity alongside the roll-out of JSY resulted in lower healthcare quality due to congestion at public facilities. We complement the findings in Andrew and Vera-Hernández (2022). First, we replicate their finding that perinatal mortality increased in low public capacity districts using a larger sample. Second, we show that high SES mothers (i.e who were not eligible for JSY) in high-performing states adapted to worsening quality care at public facilities in low capacity districts by moving towards private facilities. This is revealed-preference evidence for deteriorating public sector quality.

Finally, private facilities increased prices without any evidence of improvement in quality of healthcare despite increased competition from public facilities. This reduced access to high-quality healthcare for Indian mothers. Furthermore, we find that the price increase was primarily driven by high-performing states where high SES mothers were not incentivized under JSY. This finding is consistent with Chen and Riordan (2008) where the *price sensitivity effect* (steeper residual demand resulting in higher prices) dominates the *market share effect* (loss of market share putting downward pressure on prices) as high SES mothers did not receive incentives to choose public facilities.

Overall, we see that the success of large-scale public policies crucially depends on equilibrium responses in the market. More research is needed in exploring potential channels that can steer outcomes of public policies in the direction of intended outcomes. Ultimately, policymakers will need to foresee equilibrium responses and incorporate complementary mechanisms while designing public policies to improve important development outcomes. In ongoing work, we develop a structural model of demand and supply of maternal healthcare in India and evaluate two counterfactual policies that could complement JSY with a goal to reduce perinatal mortality: (i) improvement in public healthcare capacity and (ii) targeted vouchers to low SES mothers to access private facilities.

## 1.7 Figures and Tables

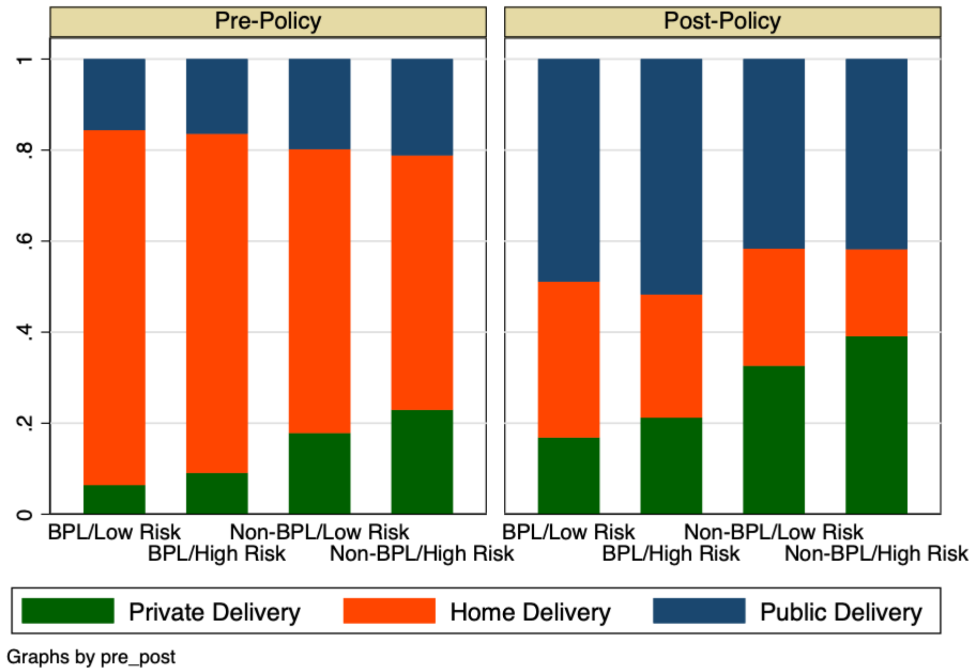



Figure 1.1: Patient sorting by types

*Notes:* Figure displays sorting of mothers across private facilities, public facilities and home by types (combinations of SES and ex-ante risk). The left (right) figure shows snapshot of patient sorting before (after) the announcement of JSY. Pre-policy period captures births before March 2005 and post-policy period captures births after March 2008 in districts that have had JSY for at least 6 months.

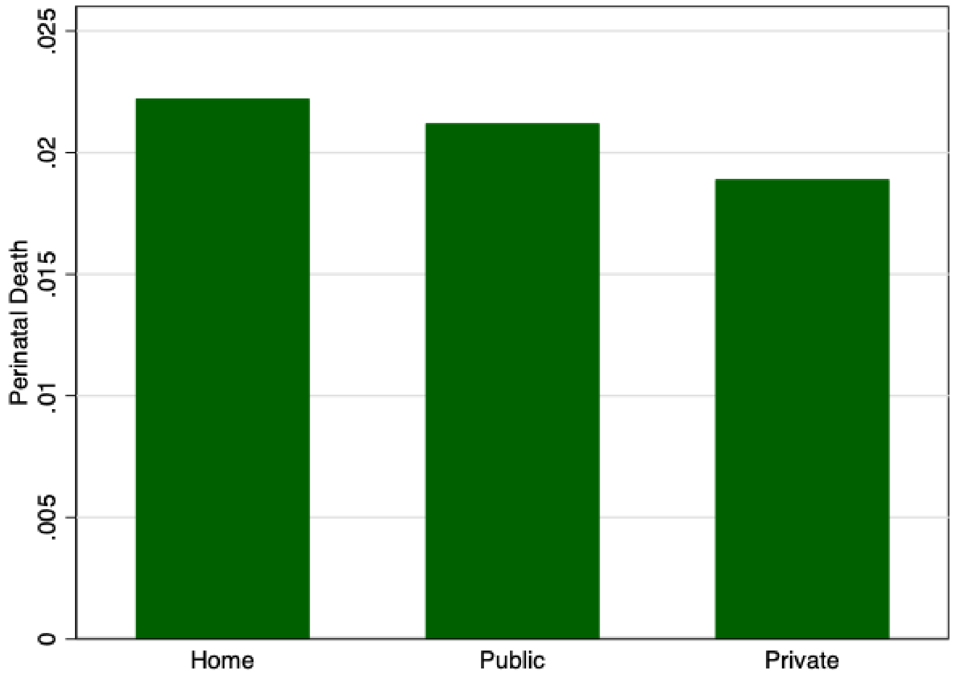
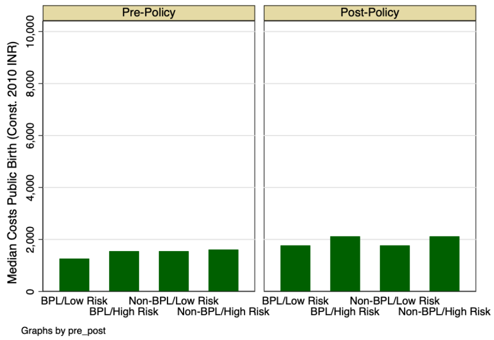
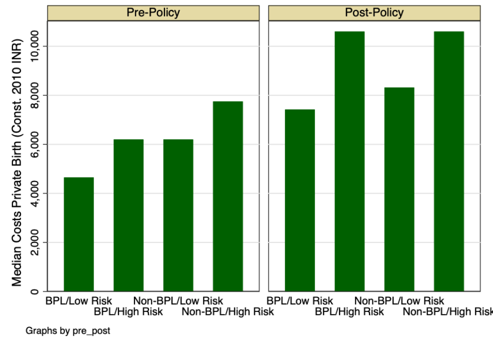



Figure 1.2: Perinatal Death by facility

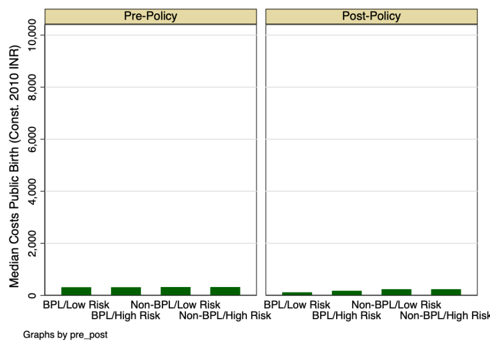
*Notes:* Figure displays perinatal mortality rates across private facilities, public facilities and home. The figure shows snapshot of perinatal mortality rates.




(a) Costs at Public Facilities




(b) Costs at Private Facilities




(c) Costs at Home Facilities

Figure 1.3: Median Out-of-pocket costs across facilities (INR)

*Notes:* Figure displays out-of-pocket costs (in constant Indian rupees) across public facilities (Panel a), private facilities (Panel b) and home (Panel c) by patient types (combinations of SES and ex-ante risk level). The left (right) figure in each panel shows snapshot of median out-of-pocket costs before (after) the announcement of JSY. Pre-policy period captures births before March 2005 and post-policy period captures births after March 2008 in districts that have had JSY for at least 6 months.

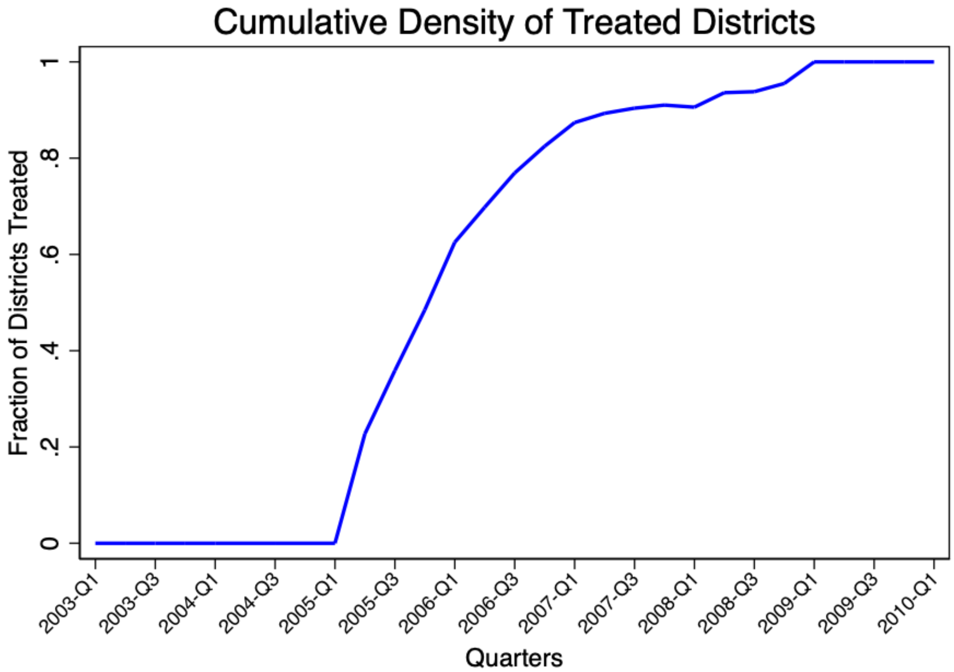



Figure 1.4: Cumulative density of roll-out of JSY across districts

*Notes:* Figure displays the cumulative density of treated districts under JSY over-time. This shows the fraction of treated and untreated districts in each quarter after the announcement of JSY in 2005 Q1.

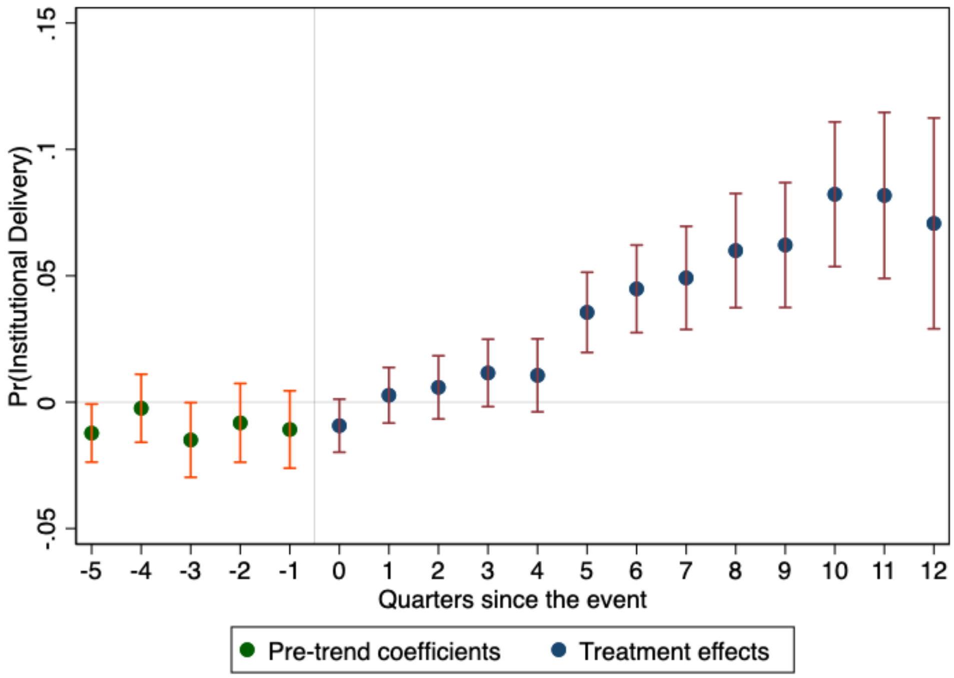



Figure 1.5: Effect of JSY on Institutional Delivery

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of institutional deliveries, following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

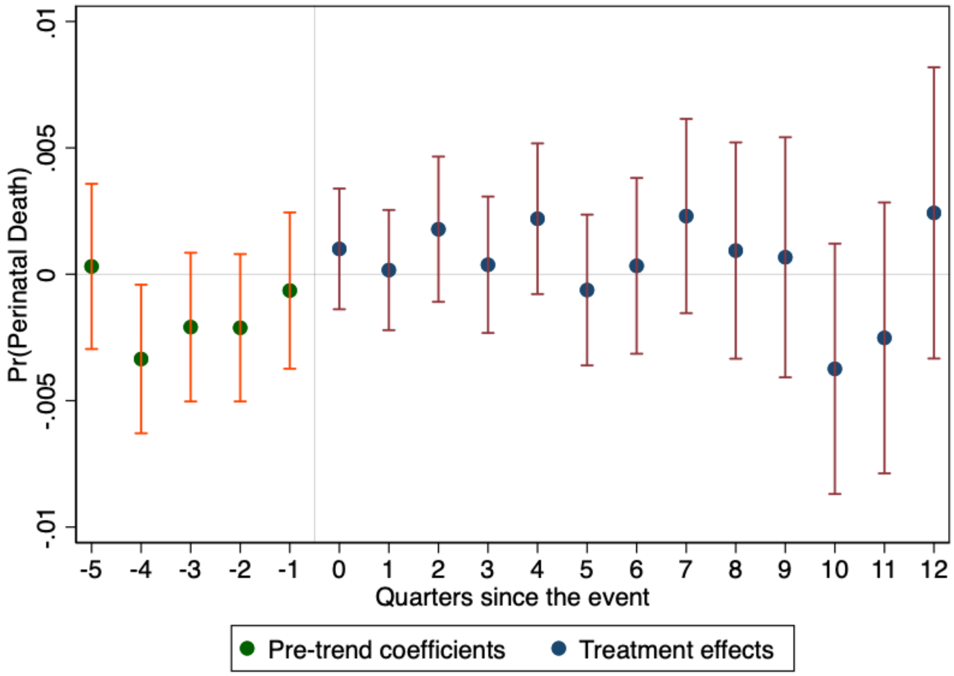
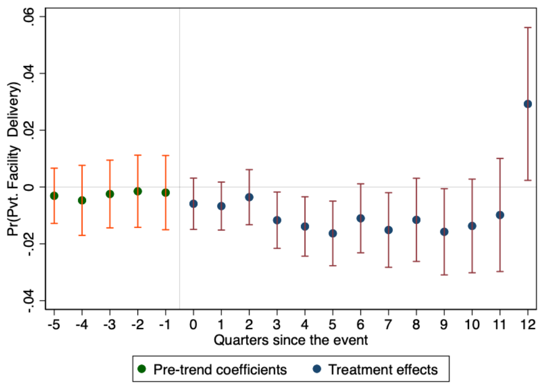
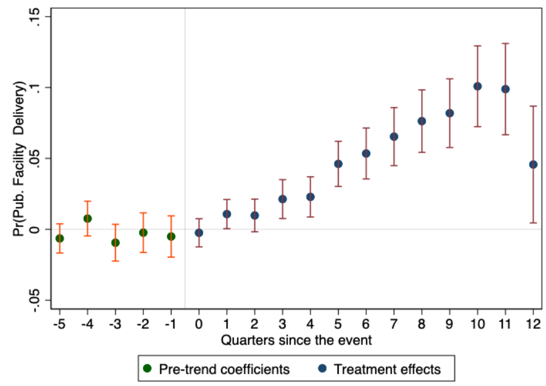



Figure 1.6: Effect of JSY on Perinatal Mortality

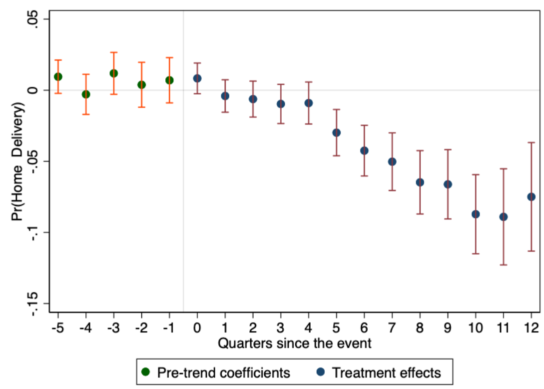
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of perinatal mortality, following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.




(a) Private Facility



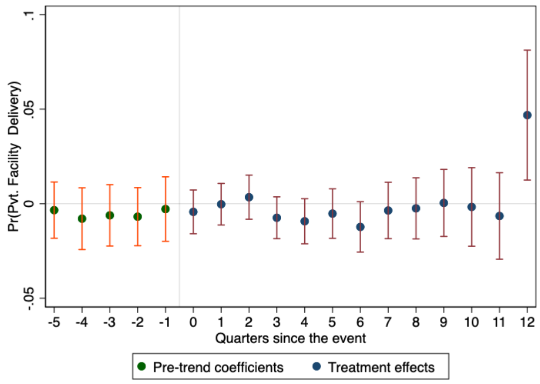

(b) Public Facility



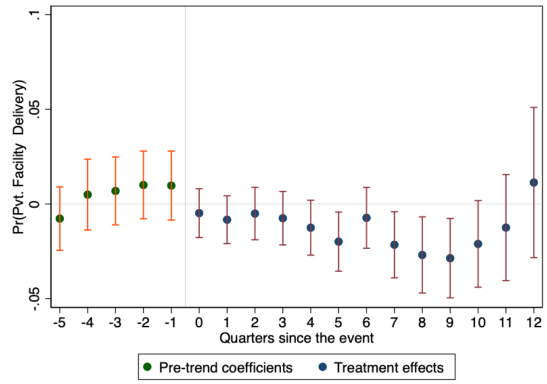

(c) Home Facility

Figure 1.7: Effect of JSY on sorting across facilities

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of deliveries across different healthcare facilities, following our empirical strategy in section 1.4. Panel A presents change in likelihood at private facilities. Panel B and Panel C present change in likelihood at public facilities and home, respectively. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



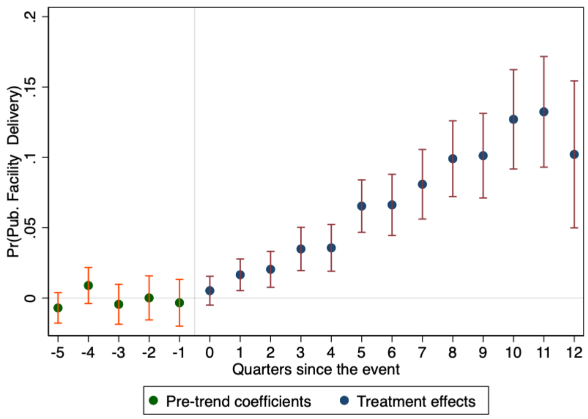

(a) Private Facility (Low-Risk)



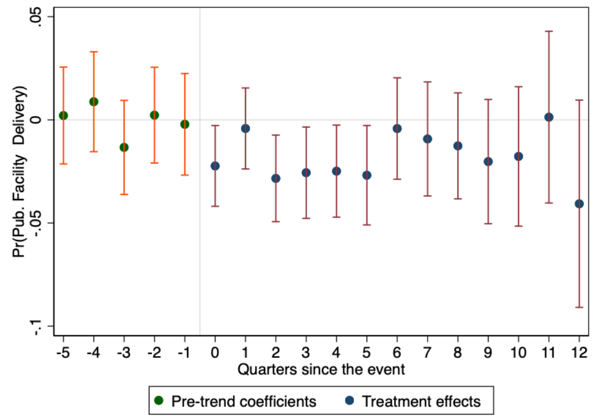

(b) Private Facility (High-Risk)

Figure 1.8: Effect of JSY on sorting into private facilities by risk

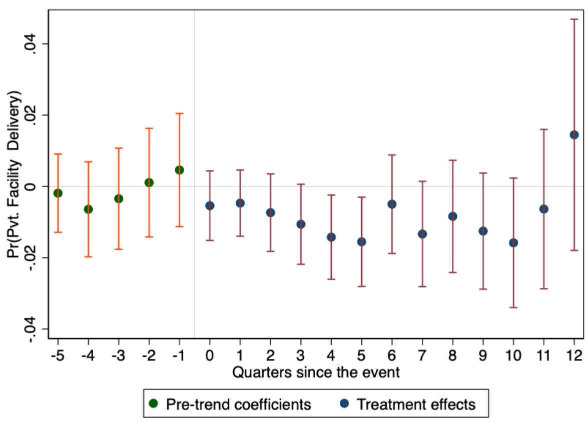
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of deliveries at private facilities by patients' ex-ante risk levels, following our empirical strategy in section 1.4. Panel A presents results for low-risk sample. Panel B presents results for high-risk sample. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



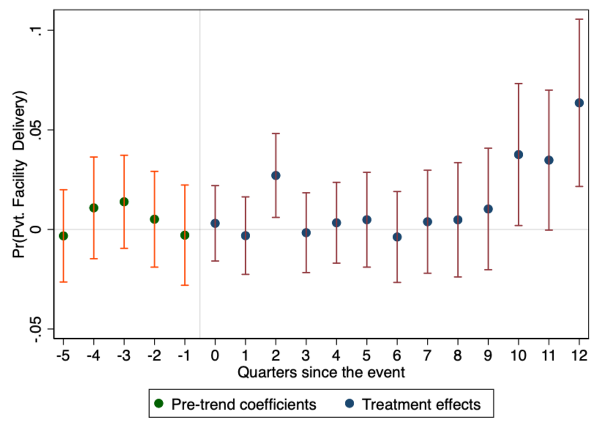

(a) Public Facility (Eligible Sample)




(b) Public Facility (Ineligible Sample)



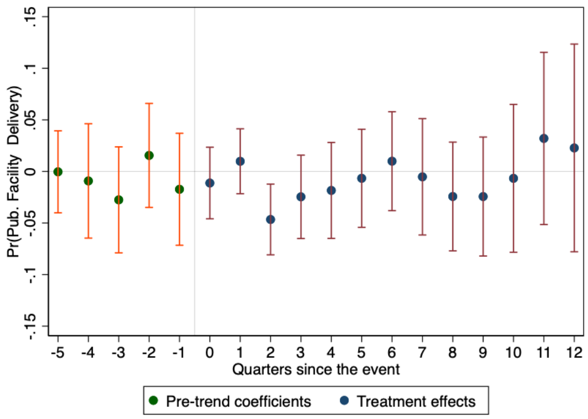

(c) Private Facility (Eligible Sample)



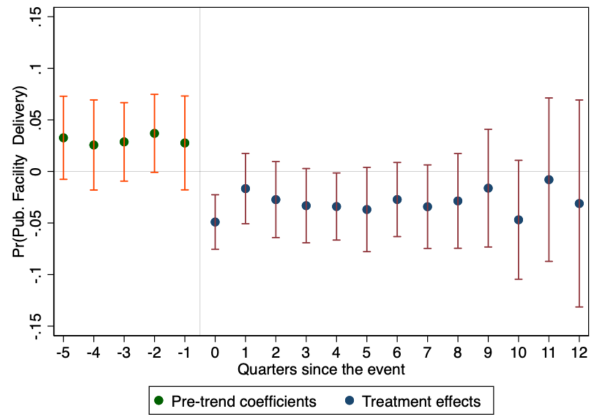

(d) Private Facility (Ineligible Sample)

Figure 1.9: Sorting across facilities by eligibility

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of delivery at a public and private facilities separately by eligibility for JSY, following our empirical strategy in section 1.4. Panel A and Panel C present results for the eligible mothers. Panel B and Panel D present results for the ineligible mothers. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



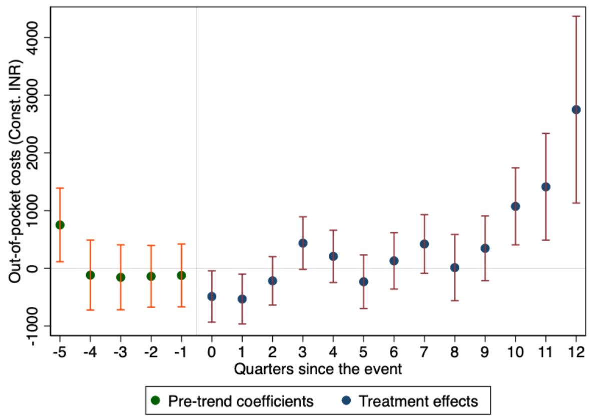

(a) Public Facilities (Ineligible, High Capacity)



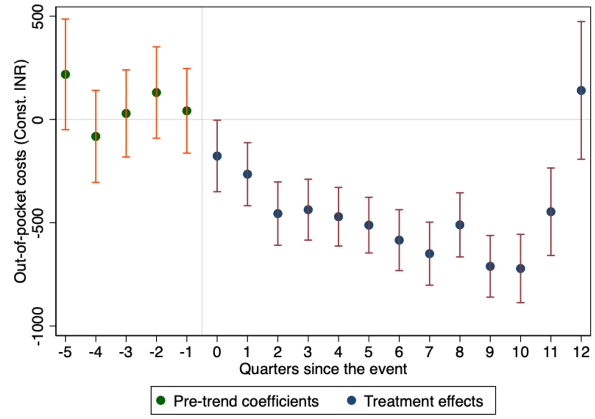

(b) Public Facilities (Ineligible, Low Capacity)

Figure 1.10: Sorting into public facilities for ineligible mothers by capacity

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of delivery at public facilities for ineligible mothers separately by district's public sector capacity, following our empirical strategy in section 1.4. Panel A presents results for the high capacity districts. Panel B presents results for the low capacity districts. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



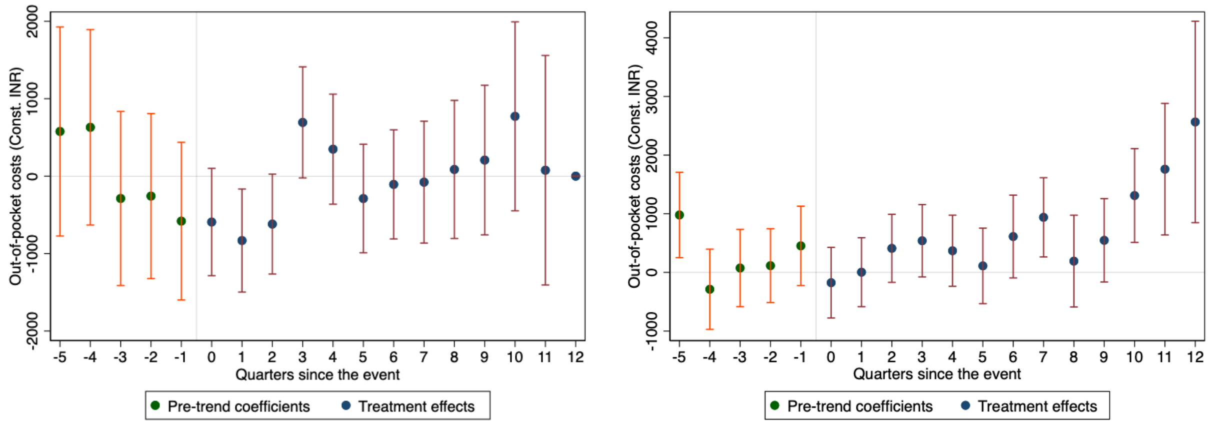

(a) Trip. Diff.: Private Costs (All Controls)




(b) Trip. Diff.: Public Costs (All Controls)

Figure 1.11: Triple Difference results on OOP Costs (Cont. INR)

*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in constant Indian rupees) at private and public facilities, following our empirical strategy in section 1.4 with an additional difference taken over the home option. Panel A presents results for deliveries at private facilities. Panel B presents results for deliveries at public facilities. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Additionally, the regressions include dummy variables for ex-ante risk-deciles and BPL status of mothers. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.




(a) Low-Performing States

(b) High-Performing States

Figure 1.12: Private facility price effect

*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in constant Indian rupees) at private facilities, following our empirical strategy in section 1.4 with an additional difference taken over the home option. Panel A presents results for deliveries at private facilities in LPS. Panel B presents results for deliveries at private facilities in HPS. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Additionally, the regressions include dummy variables for ex-ante risk-deciles and BPL status of mothers. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

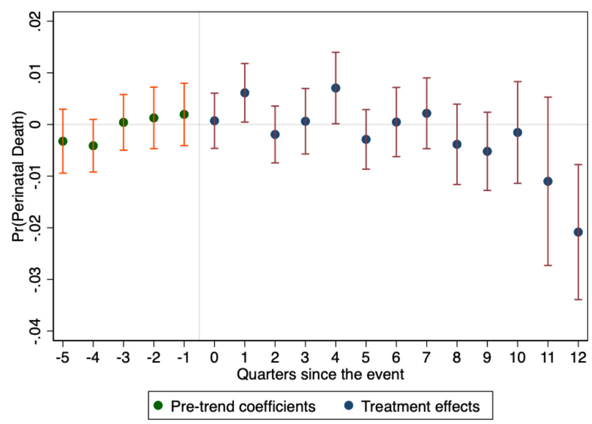



Figure 1.13: Trip. Diff.: Private Facilities Perinatal Death (All Controls)

*Notes:* This figure presents event study evidence of the effect of JSY on perinatal death at private facilities, following our empirical strategy in section 1.4 with an additional difference taken over the home option. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Additionally, the regressions include dummy variables for ex-ante risk-deciles and BPL status of mothers. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

Table 1.1: Cash incentives under JSY in Indian rupees

| State category | Rural areas | | Urban areas | |
|---|---|---|---|---|
| | Mother incentive | ASHA incentive | Mother incentive | ASHA incentive |
| Low-Performing | 1400 | 600 | 1000 | 400 |
| High performing | 700 | 600 | 600 | 400 |

*Notes:* Table depicts cash incentives under JSY for pregnant mothers as well as ASHA workers in urban and rural areas of high and low-performing states as listed in policy documents from April 2005.

Table 1.2: Snapshot of data before and after JSY

| | Pre-Policy | | | Post Policy | | |
|---|---|---|---|---|---|---|
| | Home Birth | Public Birth | Private Birth | Home Birth | Public Birth | Private Birth |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Mother Characteristics** | | | | | | |
| Caste - SC | 0.210 | 0.200 | 0.114 | 0.186 | 0.242 | 0.169 |
| | (0.41) | (0.40) | (0.32) | (0.39) | (0.43) | (0.37) |
| Caste - ST | 0.197 | 0.132 | 0.041 | 0.288 | 0.191 | 0.081 |
| | (0.40) | (0.34) | (0.20) | (0.45) | (0.39) | (0.27) |
| Mom's age at birth' | 25.659 | 24.121 | 24.729 | 25.305 | 24.415 | 24.932 |
| | (5.74) | (4.71) | (4.70) | (5.46) | (4.75) | (4.70) |
| Whether under 18 | 0.076 | 0.084 | 0.064 | 0.065 | 0.076 | 0.055 |
| | (0.27) | (0.28) | (0.24) | (0.25) | (0.27) | (0.23) |
| Whether above 35 | 0.084 | 0.032 | 0.037 | 0.076 | 0.035 | 0.034 |
| | (0.28) | (0.18) | (0.19) | (0.26) | (0.18) | (0.18) |
| Mother's Schooling | 6.813 | 8.425 | 10.072 | 7.531 | 8.703 | 10.639 |
| | (3.11) | (3.43) | (3.71) | (3.25) | (3.34) | (3.73) |
| Father Schooling | 8.049 | 9.208 | 10.797 | 8.202 | 9.337 | 10.637 |
| | (3.42) | (3.70) | (3.72) | (3.26) | (3.32) | (3.59) |
| Below Poverty Line | 0.363 | 0.246 | 0.138 | 0.272 | 0.258 | 0.129 |
| | (0.48) | (0.43) | (0.34) | (0.45) | (0.44) | (0.34) |
| Rural | 0.896 | 0.729 | 0.615 | 0.838 | 0.655 | 0.489 |
| | (0.31) | (0.44) | (0.49) | (0.37) | (0.48) | (0.50) |
| Hindu | 0.833 | 0.833 | 0.795 | 0.641 | 0.732 | 0.774 |
| | (0.37) | (0.37) | (0.40) | (0.48) | (0.44) | (0.42) |
| Muslim | 0.121 | 0.092 | 0.136 | 0.211 | 0.143 | 0.118 |
| | (0.33) | (0.29) | (0.34) | (0.41) | (0.35) | (0.32) |
| **Facility Quality** | | | | | | |
| Atleast 3 ANC | 0.260 | 0.692 | 0.762 | 0.364 | 0.780 | 0.847 |
| | (0.44) | (0.46) | (0.43) | (0.48) | (0.41) | (0.36) |
| Atleast 6 tests in ANC | 0.111 | 0.512 | 0.668 | 0.183 | 0.528 | 0.660 |
| | (0.31) | (0.50) | (0.47) | (0.39) | (0.50) | (0.47) |
| Delivery Cost (Const. INR) | 633 | 2688 | 9966 | 537 | 2673 | 11152 |
| | (942) | (3353) | (9301) | (1447) | (2982) | (9083) |
| **Village Characteristics** | | | | | | |
| Distance Nearest Town | 15.524 | 14.713 | 12.159 | 17.065 | 14.442 | 13.293 |
| | (14.83) | (14.63) | (13.77) | (16.92) | (13.02) | (11.27) |
| Distance Government CHC | 18.939 | 16.248 | 16.205 | 17.572 | 16.669 | 14.096 |
| | (9.36) | (9.40) | (8.95) | (9.59) | (10.18) | (6.34) |
| Distance Government Hospital | 33.969 | 34.992 | 32.734 | 38.312 | 37.521 | 37.189 |
| | (14.10) | (15.01) | (13.77) | (18.39) | (18.97) | (18.51) |
| Distance Private Hospital | 20.207 | 18.571 | 13.613 | 23.463 | 19.576 | 12.308 |
| | (10.38) | (11.97) | (8.53) | (21.32) | (20.47) | (8.87) |
| Observations | 9205 | 2512 | 2391 | 3870 | 4542 | 3167 |

*Notes:* The table presents patterns of patient sorting across various facilities by patient characteristics. The table shows a snapshot of our data across facilities (private, public and home), and before and after the implementation of JSY in the district. We present statistics for the pre-JSY period (2004-05) and post-JSY period (2008-09 and at least three quarters after JSY).

Table 1.3: Effect of JSY on Inst. Births, Perinatal Death and OOP Costs (Const. INR)

| | | SES | | Ex-ante Risk | |
|---|---|---|---|---|---|
| | Full Sample | BPL | Non-BPL | High-Risk | Low-Risk |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Probability of Institutional Birth* | | | | | |
| JSY | 0.029*** | 0.035*** | 0.018** | 0.037*** | 0.039*** |
| | [0.007] | [0.011] | [0.007] | [0.008] | [0.008] |
| Dependent Var. Mean (2004-05) | .36 | .21 | .44 | .39 | .33 |
| Treatment Effect (%) | 8.08% | 16.55% | 4.07% | 9.44% | 11.89% |
| Number of Districts | 587 | 586 | 587 | 577 | 577 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274964 | 78853 | 196108 | 111864 | 112122 |
| *Panel B: Probability of Perinatal Death* | | | | | |
| JSY | 0.001 | 0.001 | 0.001 | 0.001 | 0.000** |
| | [0.001] | [0.002] | [0.001] | [0.002] | [0.000] |
| Dependent Var. Mean (2004-05) | .02 | .03 | .02 | .02 | 0 |
| Treatment Effect (%) | 3.72% | 3.22% | 4.87% | 8.63% | .% |
| Number of Districts | 587 | 586 | 587 | 577 | 577 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 282540 | 80404 | 202133 | 111976 | 112233 |
| *Panel C: OOP Costs (Const. INR)* | | | | | |
| JSY | 31.376 | 7.736 | 26.638 | 81.514 | 40.077 |
| | [62.530] | [86.659] | [75.730] | [98.801] | [72.318] |
| Dependent Var. Mean (2004-05) | 2526.07 | 1429.04 | 2970.22 | 3063.8 | 2106.34 |
| Treatment Effect (%) | 1.24% | .54% | .9% | 2.66% | 1.9% |
| Number of Districts | 574 | 562 | 571 | 569 | 569 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 191950 | 51552 | 140337 | 95961 | 95860 |

*Notes:*

*Notes:* This table presents our estimates of the impact of JSY on the likelihood of delivering at an institutional facility (panel A), the likelihood of perinatal mortality (panel B) and average out-of-pocket costs expressed in constant Indian rupees (panel C). Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In column (1), we present average effect of JSY for the entire sample. Columns (2)-(3) present average effect of JSY by mothers' SES status (BPL Status). Columns (4)-(5) present average effect of JSY by a mother's ex-ante risk level (whether a mother was above median level of risk). Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.4: Average effect of JSY on Deliveries at Various Facilities

| | | SES | | Ex-ante Risk | |
|---|---|---|---|---|---|
| | Full Sample | BPL | Non-BPL | High-Risk | Low-Risk |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Public Faciltiy Births* | | | | | |
| JSY | 0.040*** | 0.047*** | 0.033*** | 0.049*** | 0.041*** |
| | [0.007] | [0.010] | [0.007] | [0.008] | [0.007] |
| Dependent Var. Mean (2004-05) | .18 | .14 | .21 | .2 | .18 |
| Treatment Effect (%) | 21.94% | 32.61% | 15.77% | 24.55% | 22.1% |
| Number of Districts | 587 | 586 | 587 | 577 | 577 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274964 | 78853 | 196108 | 111864 | 112122 |
| *Panel B: Private Faciltiy Births* | | | | | |
| JSY | -0.012** | -0.012* | -0.015** | -0.012* | -0.002 |
| | [0.005] | [0.007] | [0.006] | [0.006] | [0.005] |
| Dependent Var. Mean (2004-05) | .17 | .07 | .23 | .19 | .14 |
| Treatment Effect (%) | -6.68% | -18.05% | -6.28% | -6.42% | -1.11% |
| Number of Districts | 587 | 586 | 587 | 577 | 577 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274964 | 78853 | 196108 | 111864 | 112122 |
| *Panel C: Home Births* | | | | | |
| JSY | -0.029*** | -0.035*** | -0.018** | -0.037*** | -0.039*** |
| | [0.007] | [0.011] | [0.007] | [0.008] | [0.008] |
| Dependent Var. Mean (2004-05) | .64 | .79 | .56 | .61 | .67 |
| Treatment Effect (%) | -4.49% | -4.41% | -3.23% | -6.04% | -5.82% |
| Number of Districts | 587 | 586 | 587 | 577 | 577 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274964 | 78853 | 196108 | 111864 | 112122 |

*Notes:* This table presents our estimates of the impact of JSY on the likelihood of delivering at: (i) public facility (panel A), (ii) home births (panel B), and (iii) private facility (panel C). Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In column (1), we present average effect of JSY for the entire sample. Columns (2)-(3) present average effect of JSY by mothers' SES status (BPL Status). Columns (4)-(5) present average effect of JSY by a mother's ex-ante risk level (whether a mother was above median level of risk). Standard errors are displayed in parentheses and are clustered at district level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table 1.5: Institutional births, deaths and costs by public sector capacity

| | Y = {Inst. Birth} | | Y = {Perinatal Death} | | OOP Costs | |
|---|---|---|---|---|---|---|
| | High Pub. Capacity | Low Pub. Capacity | High Pub. Capacity | Low Pub. Capacity | High Pub. Capacity | Low Pub. Capacity |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| JSY | 0.053*** | 0.012 | 0.001 | 0.002 | -105.514 | -45.545 |
| | [0.016] | [0.012] | [0.002] | [0.002] | [80.320] | [62.847] |
| Dependent Var. Mean (2004-05) | .39 | .3 | .02 | .02 | 1714.09 | 1374.77 |
| Treatment Effect (%) | 13.85% | 3.96% | 5.65% | 9.95% | -6.16% | -3.31% |
| Number of Districts | 174 | 175 | 174 | 175 | 170 | 173 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 75892 | 95847 | 77976 | 98737 | 53464 | 69972 |

*Notes:* This table presents our estimates of the impact of JSY by public sector capacity. Districts with above median number of OBGYNs per 10,000 persons at public hospitals are high capacity districts. Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(2), we present average effect of JSY on likelihood of institutional births by public sector capacity. In columns (3)-(4), we present average effect of JSY on likelihood of perinatal death by public sector capacity. In columns (5)-(6), we present average effect of JSY on out-of-pocket costs (expressed in constant Indian rupees) by public sector capacity. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.6: Effects on real health inputs by public sector capacity

| | Y = {Death (High Risk)} | | Y = {Received ANC} | | Y = Number of ANC | | Y = {Atleast 6 tests ANC)} | |
|---|---|---|---|---|---|---|---|---|
| | High Pub. Capacity | Low Pub. Capacity | High Pub. Capacity | Low Pub. Capacity | High Pub. Capacity | Low Pub. Capacity | High Pub. Capacity | Low Pub. Capacity |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| JSY | 0.000 | 0.007* | 0.020 | 0.004 | -0.036 | -0.042 | -0.009 | -0.017* |
| | [0.004] | [0.004] | [0.015] | [0.015] | [0.109] | [0.078] | [0.011] | [0.010] |
| Dependent Var. Mean (2004-05) | .02 | .02 | .7 | .63 | 3.82 | 3.54 | .3 | .23 |
| Treatment Effect (%) | 2.31% | 46.26% | 2.91% | .67% | -.95% | -1.2% | -3.02% | -7.22% |
| Number of Districts | 171 | 174 | 174 | 175 | 174 | 175 | 174 | 175 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 31108 | 39894 | 75912 | 95845 | 54505 | 64790 | 77976 | 98737 |

*Notes:* This table presents our estimates of the impact of JSY by public sector capacity. Districts with above median number of OBGYNs per 10,000 persons at public hospitals are high capacity districts. Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(2), we present average effect of JSY on likelihood of perinatal death for high-Risk mothers by public sector capacity. In columns (3)-(4), we present average effect of JSY on likelihood of receiving ante-natal care (ANC) by public sector capacity. In columns (5)-(6), we present average effect of JSY on number of ante-natal check-ups received by public sector capacity. In columns (7)-(8), we present average effect of JSY on whether a mother was administered at least 6 out of 8 listed tests in ante-natal check-ups, by public sector capacity. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.7: Richer individuals adapt to worsening public sector quality

| | Y = {Birth: Public Fac.} | | Y = {Birth: Private Fac.} | | Y = {Birth: Public Fac.} | | Y = {Birth: Public Fac.} | |
|---|---|---|---|---|---|---|---|---|
| | Eligible | Ineligible | Eligible | Ineligible | Ineligible Low Pub. Cap. | Ineligible High Pub. Cap. | Ineligible High Risk | Ineligible Low Risk |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| JSY | $0.057^{***}$ | $-0.019^{*}$ | $-0.012^{**}$ | 0.012 | -0.030 | 0.004 | -0.012 | -0.005 |
| | [0.008] | [0.010] | [0.005] | [0.009] | [0.019] | [0.022] | [0.014] | [0.014] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .23 | .22 | .27 | .26 |
| Treatment Effect (%) | 32.5% | -7.5% | -7.56% | 4.44% | -13.05% | 1.76% | -4.52% | -1.74% |
| Number of Districts | 586 | 289 | 586 | 289 | 71 | 64 | 271 | 279 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 208890 | 66037 | 208890 | 66037 | 17557 | 14844 | 26223 | 33084 |

*Notes:* This table presents our estimates of the impact of JSY on patient sorting across facilities by public sector capacity, and patients' eligibility and risk level. We divide our sample by a mother's eligibility for benefits under the JSY. Under JSY, all mothers in low-performing districts were eligible whereas richer mothers were not eligible in high-performing states. Districts with above median number of OBGYNs per 10,000 persons at public hospitals are high capacity districts. Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(2), we present average effect of JSY on likelihood of delivery at a public facility by mothers' eligibility status. In columns (3)-(4), we present average effect of JSY on likelihood of delivery at a private facility by mothers' eligibility status. In columns (5)-(6), we present average effect of JSY on likelihood of delivery at a public facility for ineligible mothers in districts with low/high public sector capacity. In columns (7)-(8), we present average effect of JSY on likelihood of delivery at a public facility for ineligible mothers in districts by mothers' risk level. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.8: Triple Difference: Effect of JSY on Out-of-pocket Costs relative to Home

| | Y = Delivery Cost (Const. INR) | | | | | |
|---|---|---|---|---|---|---|
| | Place of Birth | | | | | |
| | Private | Public | Private | Public | Private | Public |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: OOP Costs (Const. INR)* | | | | | | |
| JSY | 122.9 | -500.1*** | 115.7 | -501.4*** | 115.5 | -498.9*** |
| | [150.4] | [56.0] | [150.5] | [56.0] | [150.4] | [56.0] |
| Dependent Var. Mean (2004-05) | 9922.5 | 2677.3 | 9925.0 | 2678.8 | 9925.0 | 2678.8 |
| Treatment Effect (%) | 1.24% | -18.68% | 1.17% | -18.72% | 1.16% | -18.63% |
| Number of Districts | 473 | 478 | 473 | 478 | 473 | 478 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | N | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | N | N | Y | Y |
| Procedure Fixed Effect | N | N | N | N | N | N |
| Observations | 112108 | 120806 | 112078 | 120775 | 112078 | 120775 |
| *Panel B: OOP Costs (Const. INR)* | | | | | | |
| JSY | -223.7* | -413.1*** | -227.3** | -414.3*** | -227.8** | -412.2*** |
| | [115.8] | [49.5] | [115.8] | [49.6] | [115.8] | [49.6] |
| Dependent Var. Mean (2004-05) | 9922.5 | 2678.7 | 9925.0 | 2680.2 | 9925.0 | 2680.2 |
| Treatment Effect (%) | -2.25% | -15.42% | -2.29% | -15.46% | -2.3% | -15.38% |
| Number of Districts | 473 | 478 | 473 | 478 | 473 | 478 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | N | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | N | N | Y | Y |
| Procedure Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 112074 | 120765 | 112044 | 120734 | 112044 | 120734 |

*Notes:* This table presents our estimates of the impact of JSY on out-of-pocket costs (expressed in constant Indian rupees) at public and private facilities. Estimates are from the triple difference specification similar to Equation 3.1 but with a third difference taken against the home option. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(2), we present average effect of JSY on out-of-pocket costs at private and public facilities respectively. In columns (3)-(4), we present average effect of JSY on out-of-pocket costs at private and public facilities respectively and additionally controlling for dummies of risk deciles in our regression specification. In columns (5)-(6), we present average effect of JSY on out-of-pocket costs at private and public facilities respectively, and additionally controlling for dummies of risk deciles and BPL status in our regression specification. Panel (A) does not control for procedure of birth and panel (B) controls for procedure of birth. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.9: Triple Difference: Effect of JSY on Perinatal Death relative to Home

| | Birth at a Private Facility | | | | | |
|---|---|---|---|---|---|---|
| | {Perinatal Death} | | | {Received ANC} | {Number of ANC} | {Atleast 6 tests} |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| JSY | 0.001 | -0.000 | -0.000 | -0.009 | 0.087** | -0.024*** |
| | [0.002] | [0.002] | [0.002] | [0.008] | [0.040] | [0.007] |
| Dependent Var. Mean (2004-05) | .02 | .01 | .01 | .92 | 5.64 | .7 |
| Treatment Effect (%) | 7.54% | -1.99% | -2.01% | -.95% | 1.54% | -3.43% |
| Number of Districts | 496 | 496 | 496 | 496 | 494 | 496 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | Y | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | Y | Y | Y | Y |
| Observations | 150711 | 128266 | 128266 | 128248 | 85590 | 128266 |

*Notes:* This table presents our estimates of the impact of JSY on likelihood of perinatal death at private facilities along with effects on various healthcare inputs. Estimates are from the triple difference specification similar to Equation 3.1 but with a third difference taken against the home option. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(3), we present average effect of JSY on perinatal death at private facilities increasingly and flexibly controlling for risk levels and BPL status. In column (4), we present average effect of JSY on whether a mother received an ante-natal check-up additionally controlling for dummies of risk deciles in our regression specification. In column (5), we present average effect of JSY on number of ANC check-ups a mother received additionally controlling for dummies of risk deciles in our regression specification. In column (6), we present average effect of JSY on number of tests done during ANC check-ups additionally controlling for dummies of risk deciles in our regression specification. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 1.10: Triple Difference: JSY and private sector market power

| | Y = Delivery Cost (Const. INR) | | | | | |
|---|---|---|---|---|---|---|
| | Private Facility Birth | | | | | |
| | High Cap. | Low Cap. | LPS | HPS | HPS/Non-BPL | HPS/BPL |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| JSY | 73.823 | -41.500 | -91.272 | 490.893** | 574.720** | 347.934 |
| | [276.219] | [262.506] | [242.342] | [217.857] | [230.318] | [327.582] |
| Dependent Var. Mean (2004-05) | 9623.24 | 9114.04 | 8855.19 | 10669.39 | 10917.18 | 9347.1 |
| Treatment Effect (%) | .77% | -.46% | -1.03% | 4.6% | 5.26% | 3.72% |
| Number of Districts | 146 | 142 | 260 | 213 | 213 | 203 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | Y | Y | Y | Y | Y | Y |
| BPL Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 30337 | 43153 | 78261 | 33817 | 24980 | 8814 |

*Notes:* This table presents our estimates of the impact of JSY on out-of-pocket costs (expressed in constant Indian rupees) at private facilities. Estimates are from the triple difference specification similar to Equation 3.1 but with a third difference taken against the home option. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(2), we present average effect of JSY on out-of-pocket costs at private facilities in high and low capacity districts respectively. In columns (3)-(4), we present average effect of JSY on out-of-pocket costs at private facilities in low and high-performing states respectively. In columns (5)-(6), we present average effect of JSY on out-of-pocket costs at private facilities in high-performing states by mothers' SES. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

# Chapter 2: Electric Stoves as a Solution for Household Air Pollution

We collected minute-by-minute data on electricity availability, electric induction stove use, and kitchen and outdoor particulate pollution in a sample of rural Indian households for one year. Using within household-month variation generated by unpredictable outages, we estimate the effects of electricity availability and electric induction stove use on kitchen PM2.5 concentration at each hour of the day. Electricity availability reduces kitchen PM2.5 by up to 50 $\mu g/m^3$, which is between 10 and 20 percent of peak concentrations during cooking hours. Induction stove use instrumented by electricity availability reduces PM2.5 in kitchens by 200-450 $\mu g/m^3$ during cooking hours.

## 2.1 Introduction

Most developing countries have achieved or have targeted universal electricity access, and technical progress has made electric cooking appliances affordable for many developing-country households. However, many of these countries also suffer from a highly unreliable power supply, and air pollution from cooking with solid fuels continues to be a major public health problem. We examine the effect of electric power reliability on household air pollution in a sample of households in rural India, most of whom have electric induction stoves. We collected minute-by-minute data on electricity availability, induction stove use, and PM2.5 (a measure of particulate pollution concentration), in 50 households in rural north India for one year. We find that these households have a highly unreliable power supply with frequent and unpredictable outages. Using day-to-day variation for each hour of the day within households that own induction stoves, and controlling for outdoor pollution, we show that electricity availability reduces PM2.5 in kitchens by up to 50

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$\mu g/m^3$ (10 to 20%) during morning and evening cooking hours. The effect of induction stove use, when instrumented by electricity availability, is an order of magnitude larger. To put the effect sizes in context, we note that the World Health Organization recently reduced the safe limit for average daily exposure from 25 to 15 $\mu g/m^3$.

Our paper makes two contributions. First, we contribute to the literature on potential solutions to household air pollution. Most of this literature has been on improved solid fuel stoves and gas, but, as detailed below, improved stoves have largely failed to reduce pollution while gas is limited by issues of cost and scalability in rural areas. Much of the literature measures outcomes other than air pollution, such as stove adoption or firewood use. The few papers that measure air pollution usually do so for just 24 hours or less and are constrained to rely on between-household comparisons (Shupler et al., 2018). Since we have a vastly greater temporal resolution, we can use within-household variation to identify effects of electricity availability. There are very few papers on electric cooking, and these have focused on the effects of electricity access (Barron and Torero, 2017; Dendup, 2021). The quality of supply varies a great deal, so access by itself is a limited indicator of electricity services (Lee et al., 2020). Instead of access, our data allow us to examine the effect of electricity reliability.

Second, we contribute to the literature on electricity and economic development. Most studies surveyed by Lee et al. (2020) examine outcomes such as income or employment. Taking a different tack, our study suggests that air pollution is an important outcome that should be considered in this literature. This literature has also concentrated on the extensive margin, that is, the effect of electrification[1] while our paper looks at the intensive margin, examining the impact of electricity reliability in electrified households.

Air pollution is the leading killer among all environmental problems worldwide (Cohen et al., 2017), with an impact on life expectancy that resembles that of tobacco smoking and that exceeds all forms of violence by an order of magnitude (Lelieveld et al., 2020). Cooking with solid fuels leads to very high exposure to air pollution in developing countries (Shupler et al., 2018). For example, households are the most important source of ambient air pollution in India (Venkataraman et al., 2018) and the largest contributor to air-pollution related mortality in China (Yun et al., 2020). Moreover, household air pollution remains an intractable problem in all but the richest

[1]A notable exception being Allcott et al. (2016) on the impacts of shortages on industry.

countries. Decades of efforts to develop improved solid-fuel stoves have had only small impacts due to technological limitations (Venkataraman et al., 2010; Sambandam et al., 2015), low adoption rates (Venkataraman et al., 2010; Mobarak et al., 2012), and infrequent use of the stoves even among adopters (Hanna et al., 2016; Sambandam et al., 2015; Venkataraman et al., 2010). Liquefied Petroleum Gas or LPG, though available in many poor regions, remains expensive given prevailing low incomes.[2] As a result, many people continue to cook with solid fuels and are, therefore, exposed to very high levels of air pollution. Here we examine a third possible source of cooking energy, electricity, and ask whether its reliability reduces air pollution.

Universal electricity access has recently become a major policy goal for developing country governments. Almost concurrently, electric induction stoves have become relatively cheap to buy and operate.[3] Electric cooking, therefore, could become an important part of the solution to the so-far intractable household air pollution problem (Smith, 2014; Smith and Sagar, 2014; Banerjee et al., 2016; Panagariya and Jain, 2016). Dendup (2021) shows that rural electrification in Bhutan led to widespread purchase of electric cooking appliances. Yet the limited success of past clean cooking interventions has naturally engendered skepticism about this potential. Prior experiences have highlighted the deeply cultural aspects of traditional cooking preferences and practices (Pattanayak et al., 2019; Jeuland et al., 2015). Given inequities in electricity access and the unreliability of power supply in many developing countries, it is also unclear if poor households will use electric stoves extensively. Even if they do, it is possible that households will use electricity only to displace other expensive clean fuels like LPG, rather than substituting for dirty solid fuels. Any assessment of the potential of electric cooking to make a substantial dent in household air pollution must address these possibilities. We find that many rural households who own electric induction stoves do in fact use them to cook meals and a substantial fraction of them use induction stoves to

[2] In India, LPG is sold in metal cylinders marketed by state-owned oil companies at a price that is subsidized by the government. Even the subsidized price of about 500 rupees per cylinder (a quantity that would last 4-6 weeks if used as the primary household cooking fuel) could exceed 10% of monthly income for many rural households in our study site in northern India. The price has risen since 2018-19 when our data was collected

[3] In India, a single-plate stove costs about 1400-2100 rupees (20-30 USD) with a set of compatible utensils costing 700 rupees (10 USD) and up at the time of data collection in 2018-19. Operating costs are a potential concern, but may be zero if electricity is not metered, as was the case in our study area. In places where electricity use is metered, the cost of cooking exclusively with an induction stove is unlikely to exceed 100 rupees per month (about 1.50 USD) for poor households that have no other major electrical appliances, due to widespread use of increasing block pricing with low prices for the first block. Recent field data suggest a growing and meaningful market demand for electric stoves (Pattanayak et al., 2019), including demand for induction stoves in India (Krishnapriya et al., 2021).

cook items that are often thought to be cooked only on open flames. Rural households are more adaptable and less tradition-bound than they are sometimes depicted to be.

Most existing research on electric cooking and air pollution relies on between-household comparisons carried out over a single day or two, which makes the findings vulnerable to confounding by unobserved household characteristics that might be related to cooking preferences and behaviors (Gould et al., 2020; Shupler et al., 2018). Since our data contains hundreds of thousands of hourly observations, we can identify the effect of electricity availability on indoor air pollution in households that own electric induction stoves using the within-household variation generated by unpredictable outages. We are able to rule out channels other than induction stove use through which outages can affect indoor air pollution in a subset of the sample households. Using this exclusion restriction to instrument induction stove use by electricity availability, we find that induction stove use reduces PM2.5 by up to 450 $\mu g/m^3$ during cooking hours. Our study area is not atypical for much of northern India, which of course has its own special characteristics. Still, the behaviors and responses that we observe are likely relevant for many developing countries where households face unreliable power supply.

### 2.2 Data

The study was conducted in the Sultanpur district of the state of Uttar Pradesh in northern India. When the study began in 2018, only 1% of rural households in Uttar Pradesh had an induction stove (Mani et al., 2018) because this technology had primarily been marketed in urban areas.[4] Dharma Life, a social enterprise that sells induction stoves, gave us access to their customer base. About 70% of their customers in 4 districts of Uttar Pradesh, when contacted by phone, reported that they used their induction stoves for cooking full meals and not just for making tea. We chose Sultanpur district because preliminary visits suggested that it had variable electricity availability and sufficiently many of Dharma Life's customers. We shortlisted villages that had at least 3-4 customers who used induction stoves for cooking full meals. This gave us 50 users in 8 villages. We also monitored 16 nearby households from the same villages that did not possess an induction stove.

[4] Percentage obtained from the ACCESS 2018 survey of rural households. Data available at `https://dataverse.harvard.edu/dataverse/IndiaAccess`

We recorded the availability of electricity by installing two voltage monitors for each of the ten power lines that reached sample households. The monitors were provided by the Prayas Energy Group and were in place from 1 September 2018 to 19 September 2019. Reliability is low. During much of the day, there is a better than even chance that the power is out. Outages are more frequent during the day, since electricity demand is higher due to industrial, commercial and cooling demand (see figure A15 in the Supplementary Materials). The quality of the power supply is also poor: Though the prescribed voltage is 220V, the mean voltage is only 204.6V, with a standard deviation of 27.3. Furthermore, 92% of households said that they could not predict the outages before they occurred. This allows us to use exogenous day-to-day variation in electricity supply to help identify the effect of electric cooking on household air pollution.

We measured PM2.5 in all household kitchens in the sample using optical particle sensors developed by the Bergin group at Duke University.[5] We measured ambient PM2.5 with two sensors installed outdoors in each village. We logged use of induction stoves via an ammeter installed in each induction stove owner's home. A member of the research team visited each household once a week while the equipment was in place to upload the data from the SD cards in the air pollution sensors and the ammeters, and to detect and resolve any problems with the equipment. Data from the voltage monitors was transmitted automatically to a server via the mobile phone network. We surveyed households about their cooking habits and electrical appliances in August 2018, in February 2019, and finally in September 2019.

While nearly all induction-stove-owners in our sample also had LPG, critically most also had a *chulha*, the traditional mud stove in which firewood or other solid fuels are burnt. Among the other sample households, some used both LPG and a *chulha* while a few used only a *chulha* (Table 2.1). No household in our sample cooked exclusively with electricity, as would be expected with a highly unreliable power supply. The use of both traditional and modern cooking stoves in the same household, a practice known as "fuel stacking", is widely observed in developing countries (Ruiz-Mercado and Masera, 2015). The proportion of relatively rich households in our sample is higher than in rural Sultanpur and Uttar Pradesh.

[5] Further details are in Supplementary Materials.

## 2.3 Descriptive Evidence

Figure 2.1 shows the percentage of induction-stove-owning households that reported cooking various foods – *rotis* (unleavened bread), lentils, rice, vegetables, other items, and none of the above (NA), using an induction stove (top panel), LPG (middle panel), and a *chulha* or traditional solid fuel stove (bottom panel). Three of the four staple foods – rice, lentils, and vegetables, were cooked more frequently on induction stoves than on LPG stoves or *chulhas*. This frequent use suggests that induction stoves may substitute for the use of smoky *chulhas* and thus reduce pollution. Only *rotis* or unleavened wheat bread, were cooked less frequently on induction stoves. In India, it is frequently asserted that induction stoves are not as versatile as stoves with an open flame, and in particular that *rotis* cannot be cooked on an induction stoves for this reason; our data show this is untrue. However, it does seem that many households prefer to cook *rotis* using LPG or a *chulha* which do have open flames.

Figure 2.2 shows average kitchen PM2.5 concentrations for each of the three subsamples of households identified in the first column of Table 2.1, along with outdoor average PM2.5 in the sample villages. Households in the primary subsample, those having both induction stoves and *chulhas* and possibly LPG as well, have lower kitchen PM2.5 than the subsample of households without induction stoves, especially during the morning and evening cooking hours. (All households surveyed reported that they cooked twice a day, in the morning and the evening.) Households using only 'clean' stoves – induction stoves and LPG, have much lower kitchen PM2.5 concentrations than the other two subsamples that are completely or partially dependent on solid fuels.

These findings are against a backdrop of extremely high ambient concentration of PM2.5 even during non-cooking hours in the early afternoon and at night. The World Health Organization recommends that the annual average concentration of PM2.5 to which people are exposed should not exceed 5 $\mu g/m^3$, and that the 24-hour average should not exceed 15 $\mu g/m^3$ on any day. The average *outdoor* concentration of PM2.5 in the study villages was 127 $\mu g/m^3$. Furthermore, there are large spikes in kitchen concentrations during cooking hours. In most households and on many days, these levels rise to more than 1000 $\mu g/m^3$ (Figure A13), though Figure 2.2 shows these spikes in gentler fashion due to averaging. Ambient concentration also rises during cooking hours. This is clearly driven by cooking activity. Indian rural houses are very well-ventilated, so PM2.5

concentrations indoors and outdoors quickly equilibrate by diffusion unless one or the other has an active source. This is why kitchen concentrations closely track the high ambient concentrations during non-cooking hours; it is also why household air pollution that includes short-lived climate pollutants like black carbon has attracted concern from climate scientists (Dasgupta and Ramanathan, 2014).

## 2.4 Effect of Electricity Availability on Kitchen PM2.5

The finding that households with induction stoves have lower PM2.5 concentrations than those without could be due to induction stove use substituting for *chulha* use, thus reducing pollution. It could also be that these households also use more LPG, or cook less than households without induction stoves. In order to remove the effects of such possible confounders, we use the long time dimension of our data to examine how pollution in each household varies from day to day.

### 2.4.1 Econometric Specification and Main Results

To estimate the causal effect of electricity availability on kitchen PM2.5 during morning and evening cooking hours as well as non-cooking times, we aggregate the minute-level data to the hourly frequency. This removes minute-level noise and is better suited to account for the gradual response of PM2.5 concentration in the kitchen to the lighting or dousing of a cooking fire. For each hour, and within each household and month, we compare kitchen PM2.5 across days with varying shares of electricity availability in that hour while controlling for ambient PM2.5. We do this by estimating the following equation using data from the primary subsample of interest, the 45 induction-stove-owning households that also had a *chulha* (and possibly LPG as well).[6] The effects of electricity availability on PM2.5 are given by the coefficients $\mu_j$ in the equation

$$Kitchen\_PM2.5_{hljt} = a_{hj} + b_{mj} + c_w + \gamma Ambient\_PM2.5_{ljt} + \sum_{j=0}^{23} \mu_j Elec\_share_{ljt} * hour_j + \epsilon_{hljt} \quad (2.1)$$

where $Kitchen\_PM2.5_{hljt}$ is the average PM2.5 concentration in household $h$ on electricity

[6]There were 45 unique households that had an induction stove and a solid-fuel stove, and possibly also LPG at some point in the study period. 3 households dropped out about three months into the study and 3 others were recruited to replace them.

line $l$ on day $t$ in hour $j$, $a_{hj}$ is a household-hour fixed effect, $b_{mj}$ is a month-hour fixed effect, $c_w$ is a day-of-the-week fixed effect, $Ambient\_PM2.5_{ljt}$ is the average ambient PM2.5 concentration in the village with electricity line $l$ on day $t$ in hour $j$ (two villages had more than one electricity line), $Elec\_share_{ljt}$ is the share of time in hour $j$ on day $t$ for which electricity was supplied in line $l$, $hour_j$ is a dummy variable for hour $j$, and $\epsilon_{hljt}$ is the residual error term for household $h$ on day $t$ in hour $j$ on line $l$. The household-hour and month-hour fixed effects ensure that the only variation being used to estimate the effect of the electricity share in each hour is day-to-day variation within households and months in that hour of the day.

We find that electricity availability reduces kitchen PM2.5 by up to 50 $\mu g/m^3$ during the morning and evening cooking hours (see Figure 2.3) which is between 10% and 20% of the evening and morning mean peak concentrations seen in Figure 2.2.

A placebo test conducted by running this regression on the subsample of 6 households with only clean stoves (those with induction stoves and LPG but no *chulhas*), showed no negative statistically significant effects of electricity availability on PM2.5 (Sections G.5). When the LASSO estimator was used on this subsample, none of the electricity shares were selected for inclusion in the model, while ambient PM2.5 was G.6. This placebo test suggests that the pollution reduction from electricity availability in the primary subsample is largely driven by a reduction in the use of smoke-emitting *chulhas*.

### 2.4.2 Reverse Causality

A concern here might be that electricity shares are endogenous if outages happen as a result of induction use. However, as noted above, the proportion of induction stove users in the state of Uttar Pradesh was only 1% in 2018. The villages in our data could have a higher proportion of induction users as a result of the presence of Dharma Life, but the sales of induction stoves even in these villages do not exceed 7% of the total households with the median village sales equal to 0.7% so we can rule out reverse causality [7].

[7] Induction stoves sales data is from Dharma Life until December 2017 and village population data is from Census of India, 2011

### 2.4.3 LASSO Estimation

Since we estimated a large number of coefficients of electricity shares (24), the probability that a few of them are negative under a zero null is much greater than 0.05. To guard against this possibility, we re-estimate Equation 2.1 using the LASSO estimator (Tibshirani, 1996; Ahrens et al., 2020; Chernozhukov et al., 2021) that adds a penalty term – the sum of the absolute values of the regression coefficients – to the usual least-squares minimization problem. LASSO drops variables that fail to contribute much to predicting the dependent variable. Since we want to ensure that our estimates are not confounded by seasonal, hourly, and household-specific variation, we do not penalize these fixed effects in the LASSO procedure. We find that only the electricity shares during morning and evening cooking hours with statistically significant coefficients seen in Figure 2.3 are selected for inclusion by the LASSO estimator except for the share in hour 20. Ambient PM2.5 was also selected for inclusion. Moreover, the coefficient estimates from the least-squares model after dropping the non-selected coefficients are almost identical to those from the original model (Section G.1). Our conclusion that electricity availability reduces PM2.5 during morning and evening cooking hours is robust.

### 2.4.4 Does Electricity Availability Reduce Aggregate PM2.5?

The results are also robust to inclusion of electricity shares lagged by one hour (Section G.2) and by one day (Section G.3). We noted above that most households said that they could not predict the timing of power outages. This would make it difficult for households to shift the timing of their use of induction stoves and solid-fuel stoves to match power availability. If they were able to do so, then it is possible that even though electricity is associated with reduced pollution at each hour, overall pollution is not reduced by electricity availability, rather its timing is shifted to match electricity supply. If such a substitution across hours or days was actually happening, then the coefficients of electricity shares lagged by an hour or by a day would be positive during cooking hours. However, we find that this is not the case. We can, therefore, conclude that the negative effect of electricity availability on kitchen pollution is, in fact, an aggregate effect, and not just a matter of timing.

## 2.5 Extent to which Induction Stove Use reduces Kitchen PM2.5

If the dominant channel for the effect of electricity availability on kitchen pollution is indeed the substitution of induction stove use for solid fuel stoves, then we should expect to see little or no effect among households that did not own induction stoves. Running the regression in Equation 2.1 on the subsample of 15 households without induction stoves, we see that the result depicted in Figure A21 confirms this expectation. Just as we did for the primary subsample, we run the lasso estimator for this subsample. In contrast to the results for the primary subsample, we find that none of the electricity shares are selected. Only ambient PM2.5 is selected for inclusion in the model suggesting that electricity availability does not have much of an effect on kitchen PM2.5 during cooking hours for households that do not own induction stoves.

We now turn to our second major question of policy interest. By how much does induction stove use reduce kitchen PM2.5 in households that also have a *chulha*, controlling for ambient PM2.5?

### 2.5.1 Econometric Specification

For each hour, and within each household and month, we compare kitchen PM2.5 on days with varying shares of induction stove use in that hour, while controlling for ambient PM2.5. We use electricity availability as an instrument for induction stove use (Equations 2.2 and 2.3).

For ease of computation, the following regressions are run separately for each hour $j$.

$$Kitchen\_PM2.5_{hljt} = a_{hj} + d_{mj} + c_w + \beta_j Ambient\_PM2.5_{hljt} + \gamma_j Induction\_\hat{use}\_share_{hljt} + \epsilon_{hljt} \quad (2.2)$$

$$Induction\_use\_share_{hljt} = a_{hj} + d_{mj} + c_w + \eta_j Ambient\_PM2.5_{hljt} + \nu_j Elec\_share_{ljt} + \epsilon_{hljt} \quad (2.3)$$

The identifying assumption being made is that the only channel for the effect of electricity on kitchen pollution after controlling for ambient pollution, is through the use of an induction stove. Due to the ambient control, any other channel must involve either an indoor source that varies with electricity availability, or dispersal of *chulha* smoke that varies with electricity availability.

### 2.5.2 Exclusion Restriction

We consider a comprehensive list of such possibilities. First, we consider fan owners: all households in the sample owned electric table fans that are commonly used for cooling in India during hot weather. None owned exhaust fans. We asked households whether they used their fans in the kitchen during or after cooking hours.[8] Only five households reported doing so, and all five, in response to a follow-up question, said that they did so to clear smoke out of the kitchen, but only in summer and not in winter. We dropped these households from the sample used to estimate Equations 2.2 and 2.3. Figure A23 depicts estimates from Equation 2.1 for households who said they used fans in the kitchen during or after cooking, and those who said they did not. We see that households that use a fan in their kitchen during or after cooking do see larger reductions in kitchen PM2.5 when electricity is available than households that do not. However, we also see that the estimated coefficients for households that do not use fans are very close to those for the entire primary subsample shown in Figure 2.3. It seems that not enough households use fans to have a sizeable effect on the coefficients.

Second, we consider electric heaters because a *chulha* could be used as a source of warmth in winter to substitute for the heater when the power is out. We excluded two households who own electric heaters from the IV regressions.

Third, we consider backup lighting from a smoky source such as kerosene lamps or candles when the power fails. We classified a household as using a non-polluting backup lighting source if they did not have a kerosene lamp or a candle, *and* if they possessed some form of backup electric lighting such as a solar lamp, or an inverter and battery used to run a light. We excluded the 15 households that did not meet this condition from the IV regressions. Figure A24 shows that in fact, there is very little difference in the marginal effects of electricity availability between households with and without backup electric lighting, except possibly in the hours beginning at 9 and 10 a.m. However, the lighting channel cannot be in play at this time since sunrise occurs by 7 a.m. even in winter.[9]

[8]This question and the questions below on heaters, using a *chulha* for backup lighting, and mosquito deterrence were asked during a follow-up survey in early 2022.

[9]Battery backup does not appear to be sufficient to be used to backup induction stoves. When examining the impact of electricity availability on induction stove use in households with and without battery backup, we find no difference in the coefficients, in a specification with the same fixed effects. Results are available on request.

Fourth, we consider if a *chulha* could be used as a supplementary light source during a power failure, even if it is a poor light source since it is enclosed on at least three sides. If it were kept going longer, it would contribute to increased kitchen pollution during outages. We explicitly asked households if they used their *chulha* as a backup source of lighting, and excluded the 3 households that said they did from the IV regressions.

Fifth, we asked households a series of questions about their use of electric mosquito repellents and 'coils' that emit a little smoke and repel mosquitoes. We excluded from the IV regressions the two households that said they use these methods to repel mosquitoes in the kitchen.

#### 2.5.3 Results

After these exclusions that rule out any channel except induction stove use, we end up with a sample of 22 households with induction stoves and a *chulha* to estimate the IV regressions. As seen in Figure 2.4, the reductions in kitchen PM2.5 as a result of induction stove use during the morning from 6:00 to 9:00 and evening from 18:00 to 20:00 range from about 220 to 450 $\mu g/m^3$. These are very large effect sizes that are comparable to the average peaks in kitchen pollution during cooking hours that are seen in Figure 2.2.

First and second-stage coefficient estimates are reported in Section I. Since the regression for each hour has a single endogenous regressor and is exactly identified, Lee et al. (2021) recommend adjusting the confidence intervals for possibly weak instruments. Since the first-stage (HAC robust) F-statistics during the cooking hours given above are large, if we were to adjust the confidence intervals using their procedure, the ones from 6:00 to 8:00 and at 18:00 would increase by less than 0.5% while the one at 19:00 would increase by less than 5%.[10]

### 2.6 Conclusion

While electrification has received much attention in the development literature, the role of reliability has been studied less. We have presented new evidence on the effects of electricity reliability at the household level in a developing country. Our study was conducted in a setting of extremely high pollution in a sample of rural household kitchens in northern India that also

[10] We show the adjusted standard errors in I

contribute substantially to ambient pollution. We find that electricity availability substantially reduces air pollution during cooking hours in this setting, and that use of induction stoves greatly reduces air pollution. Thus, improvements in the reliability of electricity together with promotion of electric cooking appear to be promising policies for reducing household and also ambient air pollution.

It is important to note that households in the study area did not pay a per-unit charge for electricity. Instead, they faced a fixed monthly payment, making additional induction stove use essentially free. Increasing the reliability of the grid would certainly impose costs, requiring investment in both infrastructure and enhanced maintenance. To pay for such improvements, the government of UP has been moving in the direction of instituting metering and per-unit charges throughout the study region. So as not to deter households from adopting electric cooking, governments should consider reimbursing the poor for a reasonable portion of their monthly bills, enough to cover cooking and other basic needs.

Our data allow us to quantify the effect of electricity reliability on kitchen pollution at the intensive margin; that is, we examine the effect of greater use of electric induction stoves among households that already possess them. Although Figure 2.2 suggests an effect at the extensive margin (that is, the effect via more widespread acquisition of induction stoves), to rigorously identify this effect will require data of a different nature. As the market expands, multi-plate stoves and many other electric cooking appliances are likely to be marketed, in addition to the single-plate stoves that are already in use. While there is some research on demand for and supply of electric cooking (Pattanayak et al., 2019; Krishnapriya et al., 2021), these are still very early days for electric cooking in rural India as well as in many other developing countries. Thus, it remains to be seen if the results generalize to other locations. Even so, electric cooking appliances are making inroads in other regions where electricity supply is more reliable, suggesting that this technology can meet rural users' needs (Mani et al., 2018; Pattanayak et al., 2019). Our findings suggest that electricity reliability and electric cooking deserve greater policy attention as a way of tackling the household air pollution crisis in India and other developing countries.

### 2.7 Figures and Tables

Table 2.1: No.of households covered in the 3 surveys

| Stove Combination | | Baseline Survey (August 2018) | Midline Survey (February 2019) | Endline Survey (September 2019) |
|---|---|---|---|---|
| Induction stove owning households with *Chulha* | Induction, LPG, *Chulha* | 39 | 41 | 39 |
| | Induction, *Chulha* | 3 | 4 | 2 |
| Households with only clean stoves | Induction, LPG | 8 | 6 | 9 |
| Households without induction stoves | *Chulha*, LPG | 9 | 9 | 12 |
| | *Chulha* | 7 | 6 | 3 |

Notes: All induction-stove-owning households also had either *chulhas* (solid-fuel stoves) or LPG, or both.
All households without induction stoves had *chulhas*, among which some had LPG.

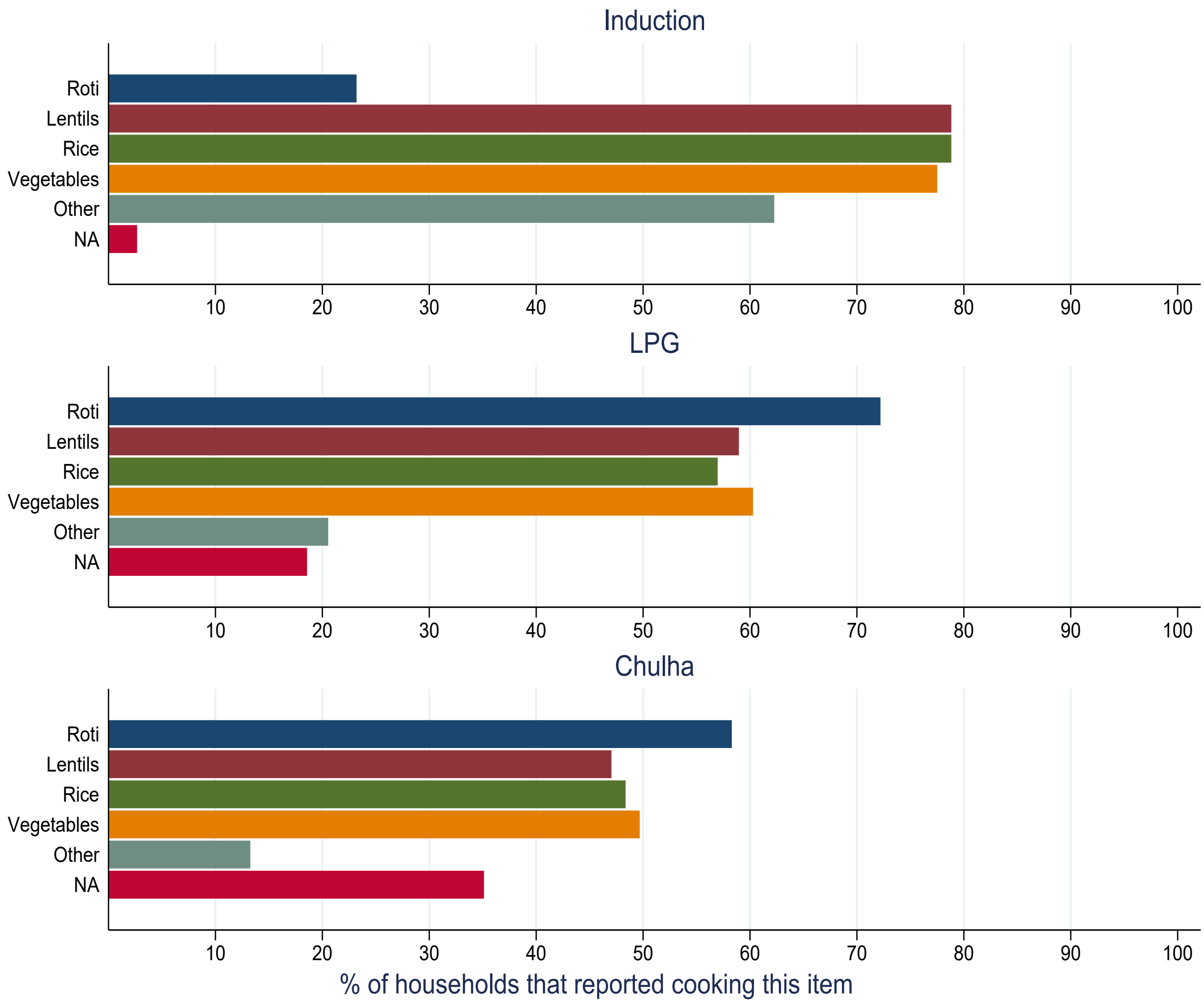



Figure 2.1: Food items cooked by induction-stove-owning households

Notes: Household survey data show that induction-stove-owning households reported cooking various foods for morning meals on an induction stove (top panel), LPG (middle panel), *chulha* or traditional solid-fuel stove (bottom panel). Data are pooled from the baseline (50 households, August 2018), midline (51 households, February 2019) and endline (50 households, September 2019) surveys. NA stands for None of the Above. In the top panel, the NA responses correspond to 3 households which reported that their induction stoves were under repair. The somewhat larger percentage of NA responses in the middle and bottom panel can be explained by the fact that several households did not possess an LPG stove or a *chulha*. Data for evening meals look similar and are not shown.

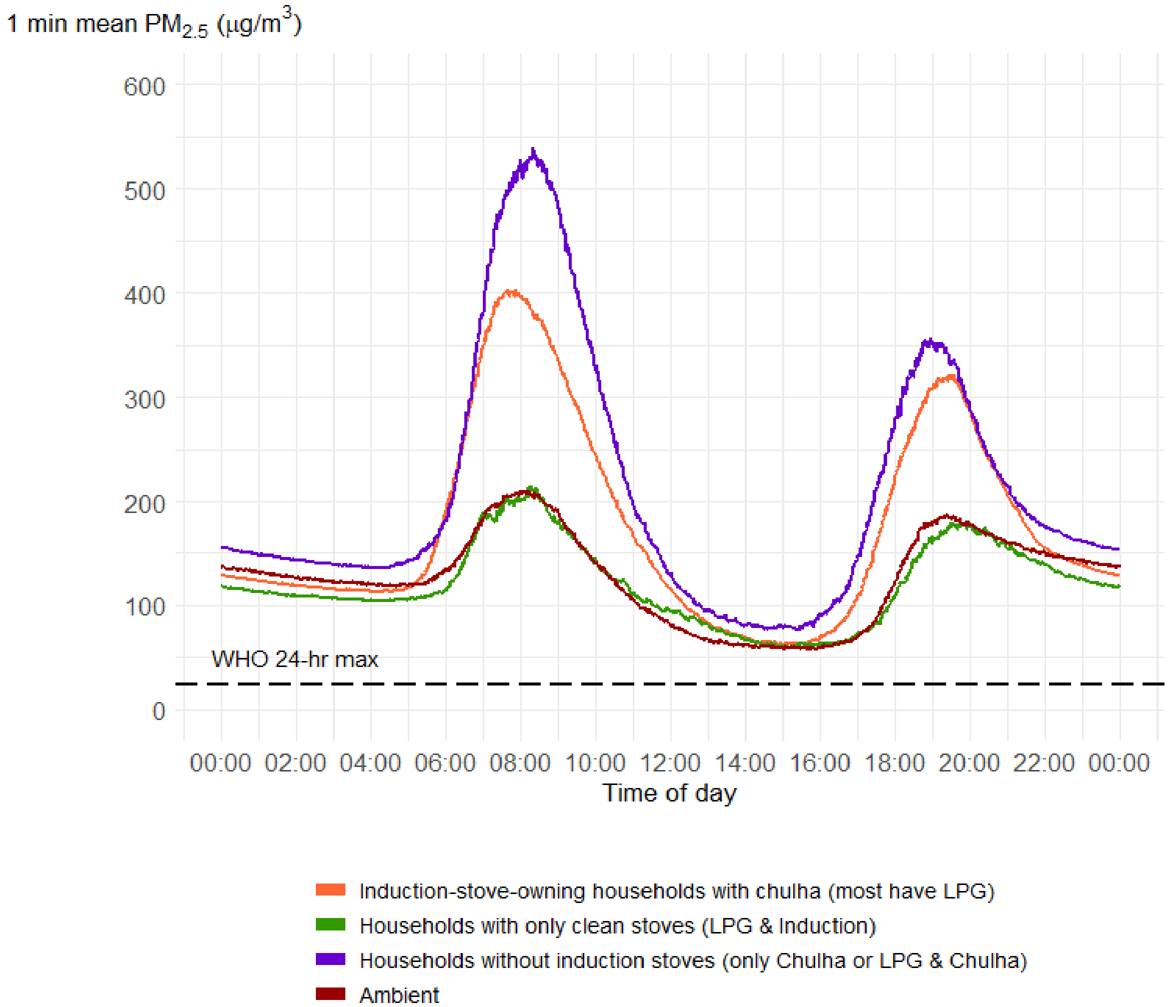



Figure 2.2: Mean PM2.5 ($\mu g/m^3$) in the sample villages and household kitchens during each minute of the day

Notes: A *chulha* is a traditional solid-fuel stove. PM2.5 ($\mu g/m^3$) for each minute of the day has been averaged over the twelve-month period 1 September 2018 to 19 September 2019. Ambient PM2.5 is averaged over the outdoor sensors in each of the 8 villages, while the others refer to measurements from sensors in kitchens of three different subsamples based on stove ownership. Table 2.1 shows the number of households within each subsample depicted in the figure. A more detailed plot of average PM2.5 for each stove combination in the second column of Table 2.1 is shown in Figure A14.

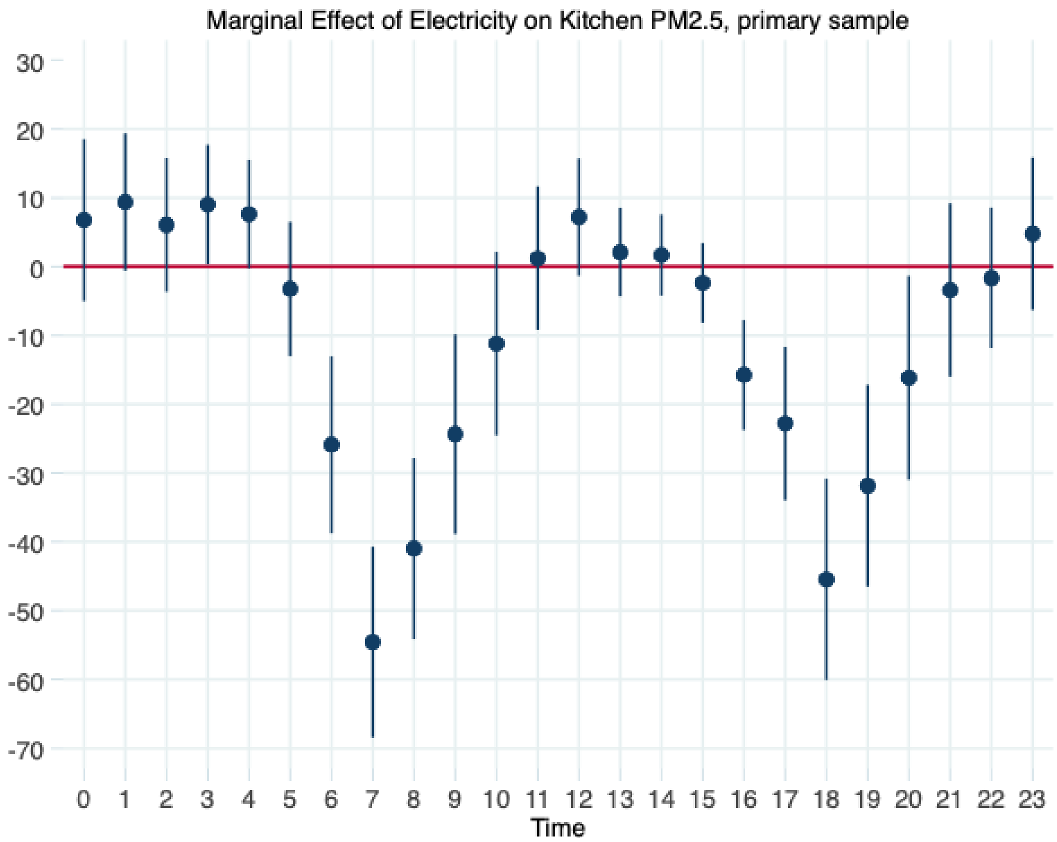



Figure 2.3: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for induction-stove-owning households with a *chulha* (solid-fuel stove)

Notes: The time labels on the horizontal axis refer to hours beginning with that particular time (eg. 6 refers to 6 AM - 6:59 AM). Plots depict the coefficients $\mu_j$ from Equation 2.1 with 95% confidence intervals computed using Driscoll-Kraay standard errors that are robust to cross-sectional and temporal dependence. Kitchen PM2.5 is the mean concentration in $\mu g/m^3$ in an hour. Electricity is measured as the share of an hour during which the power was not out. The regression uses 228,184 observations on 45 induction-stove-owning households who also had solid-fuel stoves over the one-year time span of the study. Household-hour, month-hour, and day-of-the-week fixed effects are included in the regression (Equation 2.1).

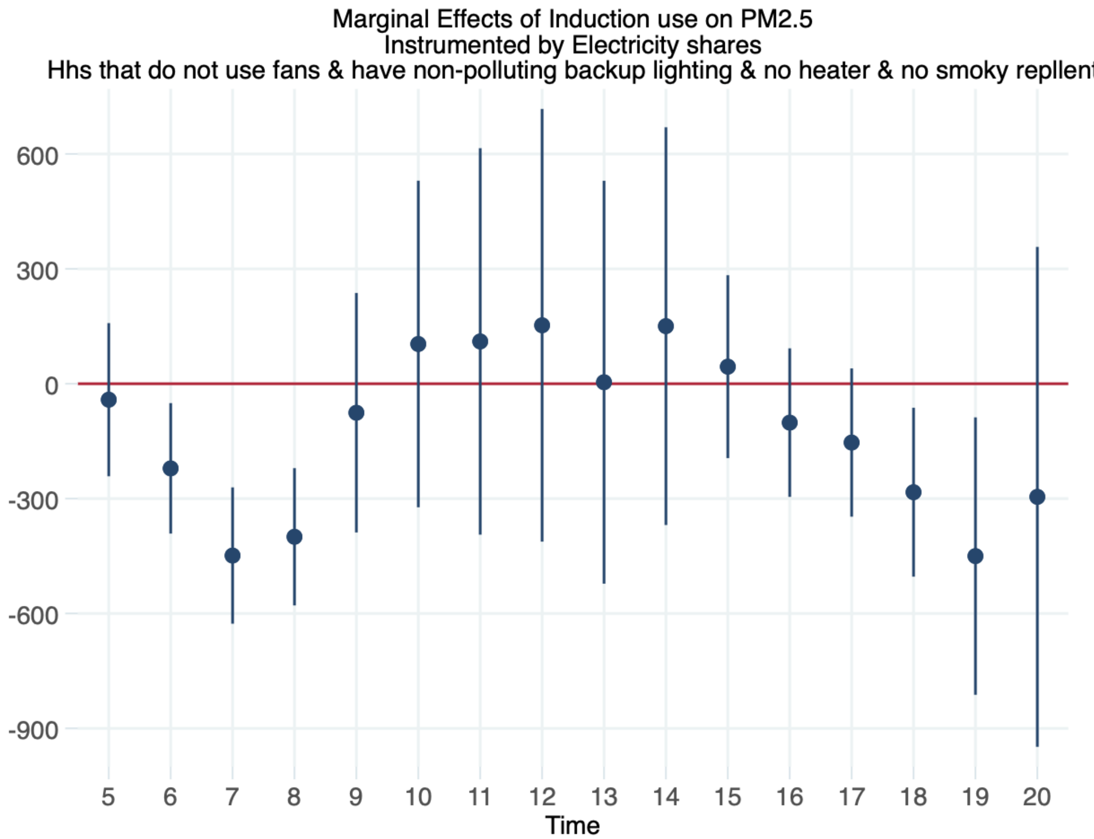



Figure 2.4: Hour-wise marginal effects of induction stove usage on kitchen PM2.5 for 22 induction-stove-owning households with a *chulha* (solid-fuel stove) who did not use fans, had clean backup power for lighting, did not use their  for additional lighting, did not have an electric heater, and did not use a smoky mosquito repellent.

Notes: Plots depict coefficient $\gamma_j$ from Equation 2.2. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence. The early morning and late night hours are excluded from the figure since some of the first-stage F-statistics are small and/or the effects are not statistically significant, and the confidence intervals are very wide. First and second-stage coefficient estimates are reported in Section  I. Figure A26 plots the first-stage coefficient estimates.

# Chapter 3: Can Large-Scale Conditional Cash Transfers Resolve the Fertility-Sex Ratio Trade-off? Evidence from India

Currently, there are at least 15 conditional cash transfer schemes in India that aim to correct persisting gender inequalities arising from a preference for sons among Indian families. Despite huge financial resources being spent on these schemes, there is a lack of field-level monitoring and useful redressal mechanisms which make their impact unclear. I evaluate a conditional cash transfer (CCT) scheme called *Ladli Laxmi Yojana* in Madhya Pradesh, India. I find that financial incentives aimed at the girl child increased average fertility by about 0.15 (on baseline average of 0.93 children) children per household and improved sex ratio by 3.4 percentage points. These results confirm the theorised fertility-sex ratio trade-off. Interestingly, these effects are quite opposite to a similar CCT scheme in Haryana Anukriti (2018) suggesting that the effect of such policies can be context dependent.

## 3.1 Introduction

Conditional Cash Transfer schemes targeting the girl child mark a significant shift in Indian policy from being supply-driven to demand-driven. These conditional financial incentives aim to correct gender-based discrimination that has existed in a majority of Indian households. Gender based discrimination in the form of son preference, has led to a significant divergence in outcomes between male and female populations in most domains of life – sex-selection, education, health-care, financial independence, child marriage as well as teenage pregnancy.

These norms, coupled with access to inexpensive sex determination technology, has resulted in significant sex selective abortions in India as well as in many other parts of the developing world leading to disturbingly skewed sex ratios at birth. Estimates suggest that nearly 4.8 million sex-selective abortions took place in India annually between 1995-2005 (Bhalotra and

[0]This chapter is based on my second year paper. I am grateful to Jack Willis and Eric Verhoogen for their generous guidance. All remaining errors are my own.

Cochrane, 2010). Data from around the same time also show that sex ratio at birth, defined as the number of male births for every female birth, was about 1.18 in China (2010) and about 1.11 in India (2008) suggesting a large imbalance in demographics. Consequently, the Indian government has made several attempts at regulating sex selective abortions mostly using command and control policies. These policies have remained unsuccessful as enforcement of bans in this context is difficult especially when ultrasounds are widespread and legal. The failure of these supply-side policies[1] motivated the introduction of demand-side policies where households were offered financial incentives to compensate for the supposed 'negative' utility they receive from girl children because of the existing son preference norms. From the policy maker's perspective, demand-side conditional cash transfer schemes are clearly a lot more expensive as compared to putting in place a law that was too difficult to implement. This necessitates their careful evaluation.

Ideally, in a country like India with a distorted boys-to-girls ratio and high levels of fertility, policymakers would want a reduction in sex ratio as well as a reduction in fertility. However, there is often a trade-off between these two goals in the presence of son preference norm as such a norm induces asymmetry in the utility parents derive from a child of either sex (Kashyap and Villavicencio, 2015; Jayachandran, 2017; Jayachandran and Pande, 2017). While conditional cash transfers may improve the value of having a daughter, their effect on fertility could be adverse depending upon the nature of the son preference norm. For example, in cultures where families desire a certain minimum number of boys (or at least one boy as in the case of India), it is highly likely that couples will have more children to make sure they have a son.

Additionally, Indian culture and traditions vary substantially across states. This renders external validity of any analysis of similar policies questionable. For instance, as I shall also show below, my study shows contrasting results compared to another recent study of a similar CCT scheme in Haryana (Anukriti, 2018). Haryana and Madhya Pradesh are both large states in India with significant divergence in both culture and the level of economic development. Haryana, at 1.16 (Census 2001) had the worst sex ratio in India while Madhya Pradesh had a much better but still disturbing figure of 1.09 in the same period. Similarly, Haryana has had a much higher fertility rate than Madhya Pradesh. While Anukriti (2018) finds that the incentives for the girl child

[1] China's command and control policy that restricted fertility to one child per couple had unintended consequence on sex ratio Ebenstein (2010)

resulted in lower fertility but worsening sex ratio in the case of Haryana, I find that fertility rose by about 0.15 children on average (from an average of 0.93 children per couple) for couples in Madhya Pradesh while sex ratio improved. The left panel of Figure 3.1 plots data from the year 2006 (National Sample Survey) on the proportion of girls by age in Madhya Pradesh. This figure in a way represents the evolution of the proportion of live persons of either sex over a twenty year period. It can be seen that there has been a steady improvement in the sex ratio but a lot remained to be achieved in the year *Ladli Laxmi Yojana* was introduced (2007).

I use a difference-in-differences strategy to study the causal effect of *Ladli Laxmi* (CCT scheme). In my difference-in-differences framework, I compare couples in Madhya Pradesh to couples in Chhattisgarh across a number of outcomes, including fertility and sex ratio. To control for baseline differences in observable characteristics across Madhya Pradesh and Chhattisgarh, I use control variables informed by the balance on observables in the pre-policy year, 2006. I find that the policy significantly increased fertility rates among the eligible women. Fertility rates increased by 0.15 children on average (from a baseline level of 0.93 children per eligible couple) for an average eligible woman. Moreover, the policy led to a significant improvement in sex ratio. The proportion of boys in an eligible household fell by 3.4 percentage points. I show that these findings are robust to the synthetic controls method, where I use a larger set of states as the control group.

To study the mechanisms behind these results on fertility and sex ratio, I examine the effect of the policy on: (i) households with varying child compositions before the announcement of the policy and (ii) child composition of couples, i.e. changes in the likelihood of observing any given child composition across treatment and control groups. I find evidence that the incentives under the policy indeed relaxed households' budget constraints on having more children – households with no children before the policy experienced the largest increase in fertility. Moreover, there is suggestive evidence for the son preference norm, specifically the one where households want at least one boy child. While for households with one boy before the policy, there is no change in fertility and sex ratio, for households with one girl before the policy, we see a significant increase in both fertility and sex ratio (proportion of boys).

In terms of changes in child compositions, we find a significant decline in the proportion of families with no children and a significant increase in families with at most two children.[2] Inter-

[2]As would be expected from the design of the policy, since families with more than two children were ineligible

estingly, the only category within the set of households with at most two children that did not see an increase was families with two girl children.

My overall conclusion is that incentives in any such conditional cash transfer scheme must be calibrated to match the contextual requirements in order to have the intended effects. It is clear that the current incentives under *Ladli Laxmi* do increase the relative 'value' of a girl child but not to the extent that would nullify son preference. This leads to an unintended increase in fertility among couples even though sex ratio improves.

The CCT scheme also provides financial incentives towards girls' enrollment at various levels of schooling. I briefly discuss some preliminary results and my future plan on using variation in financial incentives for schooling once more data becomes available. The right panel in Figure 3.1 shows enrollment for girls and boys separately by age (NSS 2006). The figure shows two facts. First, enrollment in primary schools is high. Second, there is a clear divergence in the proportion of girls and boys enrolled in school once the children hit secondary school age with many more girls dropping out of school than boys. This graph is in fact representative of the nature of school enrollments in India (Muralidharan and Prakash, 2017). *Ladli Laxmi* scheme also aimed to get girls to complete school as opposed to dropping out at higher schooling years. Under the *Ladli Laxmi* scheme, the beneficiaries were entitled to cash payments over the course of the girl's schooling years conditional upon entering certain grade levels.[3] In this version of the paper, I am only able to study enrollments at the primary level due to data limitations but hope to be able to study education outcomes for secondary schooling as more recent rounds of the National Sample Survey are made publicly available. As of now, my results for primary education outcome are as one would expect. I find no significant intent-to-treat effects on school enrollments for primary school going girls. This is expected because enrollment is already quite high for girls (about 85%) in the primary school going age group.

My study contributes to the literature that explores the fertility - sex ratio trade-off. My paper complements a significant literature on supply-side command and control policies. A number of studies have explored how these command and control policies affect fertility and investments in children (Pop-Eleches, 2006; Ananat et al., 2009). A similar study to mine, Anukriti (2018),

for the incentives under the scheme.

[3]See section 3.2 below for exact program details

evaluates another conditional cash transfer program in Haryana, India (with a somewhat different incentive structure to *Ladli Laxmi*[4]) and finds a decline in fertility and a worsening of sex ratio[5]. I find contrasting results to Anukriti (2018), suggesting context or path dependence of conditional cash transfer programs as well as their efficacy in overcoming long standing social and cultural norms centered at patriarchal underpinnings.

My study also contributes to the literature on health as well as human capital investment decisions within households. Studies have shown that differences in the value derived from the girl child versus the boy child can sometimes lead to different investments for children (Grossman and Joyce, 2017; Levine et al., 1996). I specifically study how financial incentives for a girl child alter parents' education investment decisions for girls vis-a-vis boys.

The rest of the paper is organized as follows: section 3.2 describes the policy in detail; section 3.3 describes the observational data I use in my evaluation of *Ladli Laxmi*; section 3.4 describes my empirical strategy and section 3.5, section 3.6, section 3.7 present my empirical results, heterogeneity in my findings and robustness of my results respectively. section 3.8 concludes.

### 3.2 *Ladli Laxmi Yojana*, Madhya Pradesh 2007

Madhya Pradesh, a state in central India, is one of the infamous "BIMARU" states – characterized by its poor performance on many human development indicators (NITI Aayog, 2019). As per the third round of the District Level Household Survey (2007-08), only about 12% girls of grade-appropriate age were enrolled in secondary school, as compared to 15-18% boys in the state. Sex ratios in the state fare slightly worse than the national average at 930 females per 1,000 males (Census of India, 2011), although there is substantial variation among districts within the state.

The Government of Madhya Pradesh announced *Ladli Laxmi Yojana* on May 2nd, 2007. The objective of this scheme was to improve health and education outcomes for girls, improve (reduce) sex ratios, prevent female feticide and child marriage.[6]

The scheme, although announced in 2007, benefited eligible families with girls born after January 1st, 2006. Eligibility towards these benefits was conditional on parents being non-income tax

[4]For instance, the program that Anukriti (2018) evaluates had financial incentives for both, girls and boys.
[5]Anukriti (2018) is, to the best of my knowledge, the only published paper that speaks to the fertility – sex ratio trade-off.
[6]https://ladlilaxmi.mp.gov.in

paying permanent residents of Madhya Pradesh, and upon adopting a terminal method of family planning (vasectomy or tubectomy) after two births. Further, families should not have more than two children[7] in order to be able to avail the benefits. Finally, a maximum of two girls per family could enroll in the policy.

Eligible families received Rupees 6,000 (USD 85) annually for the first five years after birth in the form of National Savings Certificates. Girls also received cash payments upon admission to various levels of schooling. They received Rupees 2000 (USD 28) upon admission to the 6th grade, Rupees 4000 (USD 56) upon admission to the 9th grade, Rupees 7500 (USD 105) upon admission to the 11th grade, and a monthly sum of Rupees 200 (USD 3) for 24 months while they finished grade 12th. Lastly, girls received a large sum of Rupees 100,000 (USD 1400) upon turning 21 years of age conditional on having taken the grade 12th examination and being unmarried at the age of 18. No benefits were given if the girl dropped out of school in between. Parents were required apply for the benefits within one year of birth. Lastly, families could not take up loans or credits on these benefits.

The take up of this policy was significant. In its very first year, about 200,000 girls were enrolled under the scheme and the cumulative take up by 2015 was about 2 million girls. Madhya Pradesh state government had budgeted about Rupees 7,370 million in *Ladli Laxmi* Yojana by 2012. Some anecdotal accounts have warned that while a lot of money is being spent, there is little information on implementation or grievance redressal, especially given the complicated eligibility criteria and application procedure, which may undo any potential benefits of the policy.

## 3.3 Data

### 3.3.1 Family Composition, Sex Ratio, and Fertility

I primarily use two rounds of the National Family Health Surveys from 2005-2006 (NFHS-3) and 2015-2016 (NFHS-4). These surveys are representative at the state level and have complete birth histories of the interviewed women. For each woman in the sample, following Anukriti (2018), I use retrospective birth histories to create an unbalanced panel for years 2000 through 2015. Each woman enters the panel in the year of first marriage and exits in the year of interview.

[7] With an exception for twins. Because conceiving a pair of twins is not under parents' control, I eventually drop all families who ever gave birth to twins.

I choose year 2000 as the starting year because I use Chhattisgarh as my control group, which was part of Madhya Pradesh till 2000 and therefore shared common institutional setup till then. The two rounds of NFHS surveys have, in addition to family composition, detailed data on socio-economic characteristics, demographics, health outcomes for women and each child including the child's weight at birth, current height of the child, size of the child at birth as well as investments in pre-natal care and vaccinations.

I use Madhya Pradesh as my treated state. I use two control groups in my analysis. The first control group is Chhattisgarh. This makes sense because Chhattisgarh was part of Madhya Pradesh until the year 2000 and therefore shared similar institutional settings. For Chhattisgarh, I use panel starting from the year 2005 because the Government of Chhattisgarh introduced a policy that provided bicycle subsidies for secondary school going girls in the state in the same year.[8] I also show that my results hold even if I extend my sample back to the year 2000. For robustness checks, I use a second control group comprising a set of states chosen based on certain criteria. Choice of these states is based on the prevalence of the son preference norm and similar sex ratio at birth according to census 2011. Figure 3.2 shows my treatment and two control groups on the map of India.

I apply a number of criteria to arrive at my final sample for analysis. My sample consists of married women between ages 15 years and 45 years, who are permanent residents of the household being surveyed. I use the last criteria since I do not have socio-economic characteristics for visitors, and since they might as well not be permanent residents of Madhya Pradesh. This range between ages 15 years and 45 years spans almost 97 percent of my sample and makes sure that women are in their active fertility period. I only include women with at most 4 children. Since the policy restricts fertility to at most 2 children, it is reasonable to assume that the incentives from the scheme are likely to not induce high fertility couples to reduce fertility by large numbers. As I shall show, my results do not change even if I include women with at most 6 children. Following Anukriti (2018), I drop the women who had their first child before the age of 14 and after the age of 30 as they are outliers in my sample. I also drop couples who ever gave birth to twins since giving birth to twins is largely out of one's control and does not necessarily factor into fertility decisions. Lastly, I incorporate the fact that sex determination technology was only introduced in India after 1995. In

[8]See Muralidharan and Prakash (2017) for an analysis of a similar policy in Bihar.

order to maintain consistency in access to sex determination technology across younger and older couples, I only include couples who had their first child in or after 1995.

### 3.3.2 Education Outcomes for Girls

*Ladli Laxmi* policy comprises an interesting incentive scheme for girls during their schooling years. Girls get varying cash payments upon admission to different grades as well as a significant lump-sum amount conditional on taking the grade 12th final exam. Due to data constraints, I am only able to study enrollment for primary school age girls. I use five rounds of National Sample Survey (NSS) data: round 62 (2005-06), round 64 (2007-08), round 66 (2009-10), round 68 (2011-12), round 71 (2014).

## 3.4 Empirical Strategy

To evaluate the *Ladli Laxmi* scheme, I primarily use quasi-experimental variation from the year and state where the scheme was implemented in a difference-in-differences framework. All of my regression estimates provide intent-to-treat effects.

An important thing to note is that the incentives varied depending upon the child composition just before the policy was introduced. Three kinds of birth histories made a couple eligible for the benefits: no children, one girl and one boy.[9] Out of these three birth histories, couples with no children could potentially earn benefits for two girl children whereas parents with one child of either sex could only avail benefits if they chose to have another child provided this child was a girl.

Before describing my econometric strategy, it is important to make a few points clear. I treat the year 2008 as the first year of the program. The program was introduced in April 2007 and completed one cycle of pregnancy (nine months) in January 2008. Therefore it is likely that any effects of the policy will start to show in year 2008. In my analysis, I only consider live children since it makes sense for parents to make fertility choices based on living household members. As mentioned above, in order to be eligible to enroll in the program, parents needed to meet three

[9] Some couples with two children could also claim benefits conditional on at least one of the two children being girls born after 2006. I ignore this minor point as decisions made before the policy announcement (April 2007) could not have influenced parents in 2006.

criteria in addition to being in one of the three birth history categories mentioned above. First, the couple must be a resident of Madhya Pradesh. Towards this requirement, I drop all surveyed women who were not permanent residents of the household that was interviewed.

Second, neither the husband nor the wife must be an income tax payer. The latter is not directly verifiable in my data as I do not have information on incomes. However, I appeal to the fact that very few people in India pay income taxes (less than 2 percent) due to several exemptions. In fact, Banerjee and Piketty (2005) find that incomes below top 1 percent are largely exempt from taxation in India.

Third, either one among the father or the mother must have adopted a terminal method of birth control after the birth of their second child. This last criteria is most often poorly enforced since parents can get fake certificates which are difficult to check for authenticity. Moreover, male sterilization is reversible. Therefore, it is unlikely that the condition on sterilization is binding in this context.

My regression framework is the difference-in-differences regression of the following form:

$$y_{ist} = \beta_0 + \beta_1(MP_s \times Post_t) + \delta_s + \gamma_t + \mathbf{X}_{is}^{'}\phi + \epsilon_{ist} \tag{3.1}$$

where $y_{ist}$ represents several outcome variables for a woman $i$ in state $s$ in year $t$. These outcome variables include fertility, proportion of boys (sex ratio), as well as dummy variables for various child compositions – no children, one girl only, one boy only, two girls only, two boys only, one girl and one boy, and others. I call these set of variables on child composition as 'stock' variables since they tell us about the child composition in any given period. For example, if $y_{ist}$ is a dummy variable for no children, for a given woman $i$, $y_{ist} = 1$ for years where the woman did not have any children and zero otherwise. Here, $Post_t = 1$ if $t \geq 2008$ and $MP$ is a dummy variable for the treated state, Madhya Pradesh. I use Chhattisgarh as my control state (see Figure 3.2 left panel). $\delta_s$ and $\gamma_t$ are state and year fixed effects, respectively. $\mathbf{X}_{is}$ is a vector of woman and state specific controls. $\beta_1$ is the coefficient of interest and is interpreted as the causal effect of *Ladli Laxmi* for women in Madhya Pradesh compared to Chhattisgarh, pre- and post- 2008. My identification assumption is the usual parallel trends assumption – women's outcomes would have followed similar trends in Madhya Pradesh and Chhattisgarh without the *Ladli Laxmi* scheme. I show visual support for this assumption using event-studies with pre-trends.

I also use a larger set of control states to assess the impact of *Ladli Laxmi* (right panel in Figure 3.2). The choice of the control states is based on the existence of the son preference norm and historical record of a skewed sex ratio in control states being similar to those in Madhya Pradesh. With this set of controls however, I am unable to support my assumption of parallel trends. Therefore, I instead use the synthetic controls approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to test the robustness of my results from the smaller control group in the difference-in-differences analysis. To run my synthetic controls analysis, I aggregate my individual level sample up to the state level using state-year means of my outcome as well as control variables. I then construct a 'synthetic' Madhya Pradesh using the algorithm in STATA's synth package.

The analysis so far considers the 'stock' of children. That is, my outcome variable $y_{ist}$ includes dummy variables for different compositions of children in any given year. In addition to looking at the 'stock' of children, I also examine the 'flow' of children – the probability of a marginal birth and it's sex in any period $t$ conditional on various possible child compositions in period $t$-1. Following Anukriti (2018), I restrict my sample to one year after the policy was implemented. That is, $t \leq 2008$. This restriction reduces my sample size but helps to make sure that the child composition assignment in period $t$-1 is not affected by exposure to the policy.

Lastly, I present some very preliminary results on education outcomes for girls. I use a similar difference-in-differences strategy for education outcomes.

## 3.5 Results and Mechanisms

### 3.5.1 Difference-in-Differences Results: Sex ratio and Fertility

In this section, I start with providing evidence in support my assumption of parallel trends. Following this, I elaborate on my findings on fertility, sex ratio, and changes in child composition. Studying the effect on the child composition helps understand parental responses to the incentives under the policy.

I test for parallel trends using a regression similar to Muralidharan and Prakash (2017) using years 2000 through 2006 as my pre-policy years. I show my regression results for both control groups on the full range of outcome variables that I use in this study while controlling for unbalanced variables from the balance Table 3.1. Specifically, I control for whether the household is urban or rural, number of members in the household, age and sex of the household head, whether the household is hindu, whether the household is muslim, wealth index of the household to control for economic characteristics and whether the household fell into a less privileged class: scheduled caste, scheduled tribe or other backward class. Table 3.2 and Table 3.3 show my most important results. Panel A of Table 3.2 rules out parallel trends between Madhya Pradesh and a group of other states with historically similar sex ratios and son preference. Panel B of Table 3.2 however, provides support for my assumption that Madhya Pradesh and Chhattisgarh were not on significantly different trajectories prior to the policy in terms of fertility and sex ratio (defined here as the proportion of boys).[10] I therefore proceed with Chhattisgarh as my control state in the difference-in-differences specification.

Table 3.3 shows results for the difference-in-differences specification for both, the entire sample as well as the eligible sample.[11] There is strong evidence of an increase in fertility in Madhya Pradesh compared to Chhattisgarh as a result of the policy. An average woman in Madhya Pradesh had 0.08 children more that an average woman in Chhattisgarh as a result of *Ladli Laxmi*. Among the eligible women, average fertility increased by 0.15 children (on baseline average of 0.93) in Madhya Pradesh compared to Chhattisgarh. The effect on sex ratio however is only detected in the eligible sample. I find a significant decrease in the proportion of boys – an average eligible woman in Madhya Pradesh had 3.4 percentage points fewer male children than an average eligible woman in Chhattisgarh. Overall, the results in Table 3.3 suggest that the policy failed to overcome the fertility - sex ratio trade-off.

### 3.5.2 Event Study Evidence

To present visual evidence in support of the parallel trends assumption, and study the effect of the policy overtime, we present our results in the form of event studies.

[10] A somewhat unconventional feature of my regressions shown in Table 3.2 is the way year is coded. I follow Muralidharan and Prakash (2017) who code year as taking increasing values overtime. For instance, in panel A of Table 3.2, I take 2000-07 as my pre-treatment years and create a year variable which goes from 1 through 8. I later show in my event study design that the parallel trends assumption holds even when I use year dummies.

[11] A woman is eligible for benefits if she has had at most 1 child before 2007 and at most two children overall.

Figure 3.3 presents results for an event study regression of the following form:

$$y_{ist} = \sum_{p=2005}^{2016} \beta_p (MP_s \times \mathbf{1}[Year_t = p]) + \delta_s + \gamma_t + \mathbf{X}'_{is}\phi + \epsilon_{ist} \tag{3.2}$$

We see that the event studies validate our assumption that Madhya Pradesh and Chhattisgarh were trending the same before the policy on our two main outcome variables: fertility and sex ratio. Moreover, we see that the increase in fertility as a result of the policy increases steadily over time while the effect of the policy on sex ratio is relatively small and remains consistent over years after the policy.

### 3.5.3 Mechanisms

To explore the mechanisms behind our findings, I unpack the eligibility criteria into three categories: eligible women with one boy before policy, eligible women with one girl before policy, and eligible women with no children before policy. This decomposition helps in studying the heterogenous responses to the policy, as well as parents' preferences on child compositions.

#### Suggestive Evidence for Son Preference

I find that an eligible couple with exactly one girl before the policy responded by significantly increasing fertility as well as the proportion of boys (Panel D of Table 3.3). Whereas, an eligible family that already had a boy (Panel C of Table 3.3), did not have any significant change in fertility as well as the proportion of boys.

One possible explanation for this finding is that couples that already had a boy before the policy satiated their requirement for children and were able to meet the son preference norm of at least one boy. Whereas, couples with one girl child before the policy were now less averse to having a daughter than they would have been otherwise.[12] This led to an increase in fertility. However, because the incentives under the policy were not strong enough to nullify the son preference norm of at least one boy, couples could have adopted illegal but widespread practice of sex-selection.

[12] To support this inference, Panel E of Table 3.3 shows that fertility rates increased among families with no children before the policy. This suggests that the incentives under the policy relaxed households' budget constraints on having children.

### Effect of the Policy on Child Composition

Next, I analyze the effect of the policy on child compositions for each of the eligible categories I used above. Table 3.4 provides evidence of parallel trends for each of my outcome variables. I run the exact same regression as the fertility - sex ratio regressions for pre-policy periods and find that Chhattisgarh was on a similar trajectory as Madhya Pradesh before the introduction of *Ladli Laxmi* (see Panel B of Table 3.4). Moreover, I shall demonstrate support for parallel trends using event studies. Therefore, I proceed with my difference-in-differences analysis using only Chhattisgarh as the control state as before.

Table 3.5 presents the main results on the effect of the policy on child compositions. Column 1 of Table 3.5 shows a robust decline in the likelihood of a couple with no children in Madhya Pradesh compared to Chhattisgarh as a result of the policy. This suggests that a large part of the increase in fertility (Column 1 of Table 3.3) was driven by couples who earlier had no children. This also suggests that the policy indeed relaxed household budgets on fertility by making it less expensive to have children.

Further, Panel A of Table 3.5 shows that most of the families with no children earlier now had at most two children – the coefficient on 'Other' child compositions is insignificant (see Column 7 of Panel A in Table 3.5). Among eligible couples (Panel B of Table 3.5), I find that the increase in fertility for couples with no children, resulted in a higher likelihood of observing families with child compositions with at most one girl (but never two girls).

Another thing worth noting from Table 3.5 is the result in Panel D. It is clear that families that already had a girl child before policy were less likely to have a second girl child but more likely to have a boy child. The likelihood of having a second girl child falls by 3 percentage points and the likelihood of a second child being a boy goes up by about 6 percentage points. This provides support for my earlier explanation on son preference and the adoption of illegal sex selection. This is also in stark contrast to the finding in Panel C of Table 3.5, where for families that already had one boy, the likelihood of giving birth to another boy goes up only by about 1.8 percentage points with no change in the likelihood of having a girl as the second child.

Figure 3.4, which shows results for the event study specification in Equation 3.2, provides visual support for the parallel trends assumption and confirms our findings on the effect of the policy on child compositions.

### 3.5.4 Robustness using the Synthetic Controls Method

Next, I provide support for my findings using the synthetic controls method with aggregated data at the state-year level (Moulton, 1986; Moulton, 1990) for a larger set of states that are similar to Madhya Pradesh. These 'donor' states are Rajasthan, Chhattisgarh, Gujarat, Maharashtra, Jharkhand, Orrissa and West Bengal.[13] To use this methodology, I first aggregate all of my outcome as well as control variables using averages at the state-year level. For instance, the dummy for zero children now becomes proportion of families with zero children in a given state-year combination, proportion of boys in a family-year now becomes average proportion of boys in a state-year. Similarly variables like hindu (muslim) now become proportion of families that are hindu (muslim) in any given state-year combination. My synthetic controls specification overall controls for a larger set of variables than my difference-in-differences specification. In addition to all the previously used controls, I also control for average years of education, ideal number of children (both boys and girls), average of partner's age and education. My results for the synthetic controls strategy are shown in Figure A3 below. The impact of the policy can be seen from the divergence between the solid line (Madhya Pradesh) and the dashed line (Synthetic Madhya Pradesh). The general pattern of my results does not change.

## 3.6 Heterogeneity

To explore differences in results across various groups of people, I present the difference-in-differences results while conditioning for various socio-economic and demographic criteria, specifically the age of the women, place of residence (urban or rural), religion of the household head, and whether the family belonged to disadvantaged castes i.e., scheduled castes, scheduled tribes, and other backward castes. For this section, I restrict my results to fertility and sex ratio only.

I create three categories for mother's age: 20 to 30 years old, 31 to 40 years old and below 20 years of age. Most of my sample is concentrated in the first category. I present my regression

[13] I do not include states like Haryana, Uttar Pradesh and Bihar that also have poor sex ratios because these states also implemented a conditional cash transfer scheme around the same time as Madhya Pradesh.

results in Table A2. I find that the increase in fertility is mainly driven by women below 30 years of age. Whereas, the effect on sex ratio is mainly driven by older mothers aged 21 years and above. This is intuitive as older mothers may be able to make better decisions and have more bargaining power within the household. Women between 20 and 30 years of age experience the largest decline in sex ratio post policy.[14] Panel C of the table presents results for women younger than 20 years of age. I find a very large increase in fertility as well as a large increase in the proportion of boys for these women.

Next, I divide my sample into urban and rural households. Most observations in my sample come from rural households. Table A3 presents my difference-in-differences regression results. I find a significant increase in fertility in both rural as well as urban areas. However, the effect on sex ratio is mainly driven by urban households. Among Hindu and Muslim populations, I find that the policy led to a decline in the proportion of boys among hindu couples whereas an increase in the proportion of boys for muslim families (Table A4). Lastly, I run my regression specification for people from different caste groups. I find that most reduction in proportion of boys (sex ratio) are concentrated among families from scheduled tribes and other backward caste communities (Table A5).

### 3.7 Robustness Checks

As mentioned in section 3.3, I restricted my sample to women who had a maximum of 4 live children. The reason for this was that couples with a large number of children may not find the incentives under the policy to be large enough to restrict the number of children to two children with at least one girl born after 2006. I relax this constraint and show my results in Table A6 for fertility and sex ratio for couples with at most 6 children. I find a robust increase in fertility for the entire sample, as well as the eligible sample. Additionally I find that for this group with higher average fertility preferences, an average family had a significantly larger proportion of boys after policy. However, my earlier result on sex ratio, the decline in the proportion of boys, for the eligible group still holds. Additionally, I find a fall in the proportion of boys for families who had only one boy in the pre-policy years and an increase in the proportion of boys for couples with only one girl before policy. Table A7 shows my results for child sex compositions. The general

[14] Recall overall sex ratio in Madhya Pradesh in year 2011 was about 1.09.

pattern of my results remains consistent – a decline in likelihood of a couple with no children and a sharp increase in the likelihood of having only one girl child. Along with that, I find a decline in the likelihood of two girl children. My data does not allow me to exactly point out how the probability is being distributed from one category to another. For example, the sharp increase in the likelihood of having only one girl child could either come from a decline in the likelihood of having no children or from a decline in the likelihood of having two girl children only. In any case, the main takeaway here is that my results do not rely on a arbitrarily chosen cut off of 4 live children.

In my analysis, I restricted my sample to years after 2005. The primary reason for doing this was to avoid any concurrent effects of a conditional kind transfer in Chhattisgarh where secondary school going girls were given bicycles. This restriction left me with only 3 pre-policy periods which may not be ideal to establish parallel trends. To address this issue, I present my results from my event study specification mentioned earlier using data from years 2000 through 2016. Figure A2 shows that the parallel trends assumption that is the basis of my analysis likely holds with this longer panel as well. Moreover, none of my results change with this longer panel of data.

### 3.8 Conclusion

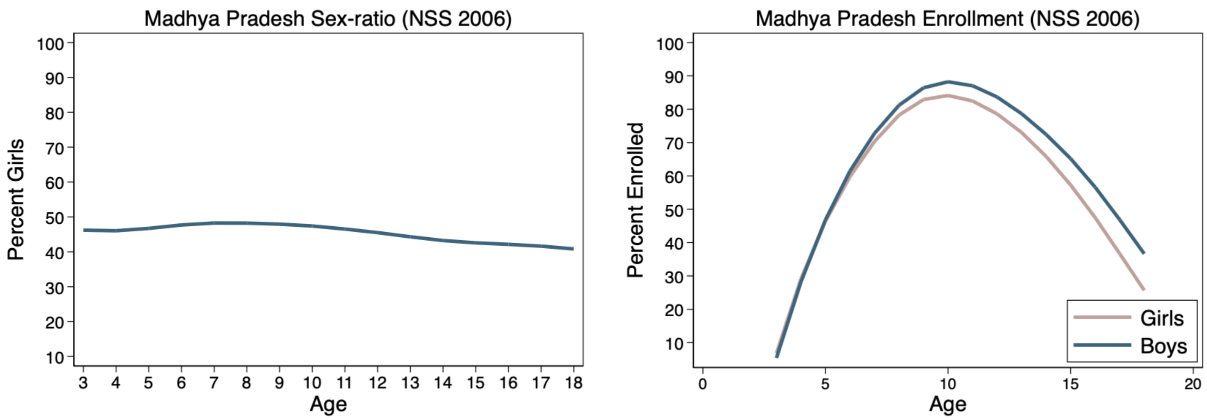
In this paper, I present evidence confirming the trade-off between fertility and sex ratio in societies where male children are preferred over female children. I evaluate a large scale conditional cash transfer program for girls, *Ladli Laxmi Yojana*, in Madhya Pradesh, India. I find robust effects of this policy on fertility and sex ratio (proportion of boys). Financial incentives offered under the program towards improving the status and prospects of a girl child led to an increase in fertility especially among couples that were less likely to have any children at all. I also find a fall in the proportion of boys (improvement in sex ratio) for eligible couples.

Overall I find that *Ladli Laxmi* was unable to achieve the dual objective of reducing fertility as well as sex ratio in Madhya Pradesh. My results are in contrast to another conditional cash transfer scheme in Haryana called *Devi Rupak* which provided higher financial incentives to a girl child than a boy. Anukriti (2018) finds that *Devi Rupak* led to a decline in fertility but an increase in sex ratio since the difference in incentives for girls and boys was not enough to nullify the asymmetry in value parents derived from girls compared to boys. While Madhya Pradesh has historically had better sex ratio than Haryana, I find that couples in Madhya Pradesh too show preference for boys which reflects in a higher likelihood of a male birth after a female birth.

My overall conclusion is that son preference is unlikely a cost-based norm and therefore requires a more holistic approach than mere financial incentives.[15] It may therefore be worthwhile to explore complementarities with behavioral approaches that target norms directly (Karing, 2018; Das Gupta et al., 2003).

[15] *Ladli Laxmi* Yojana, Madhya Pradesh offers financial incentives roughly close to average dowry amount in Madhya Pradesh

## 3.9 Figures and Tables

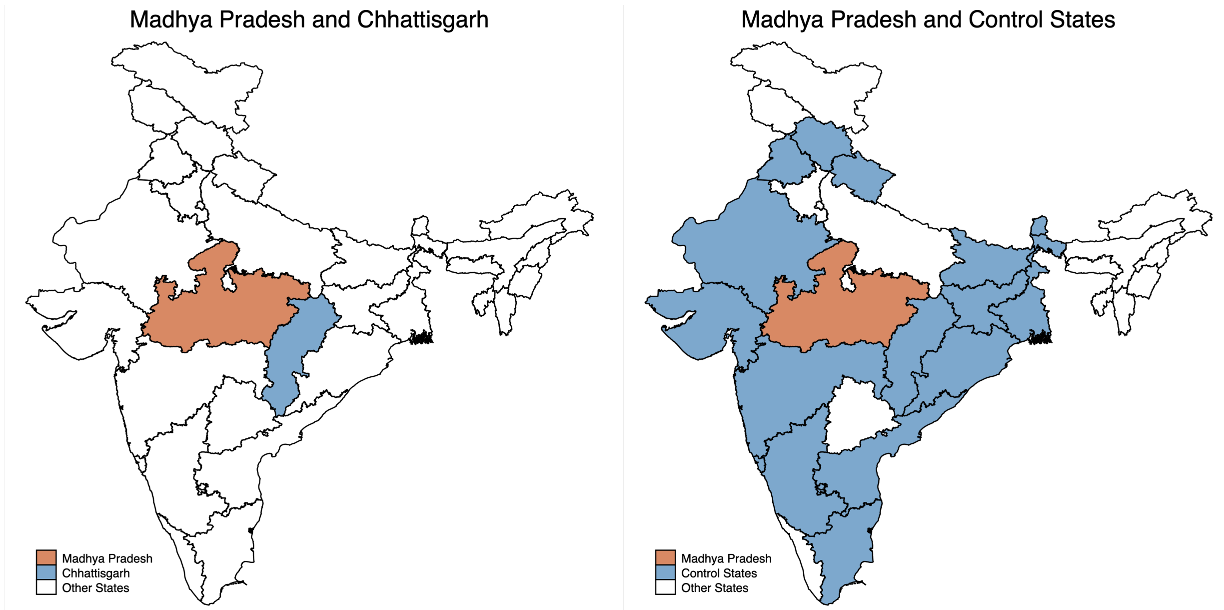



(a) Sex Ratio in Madhya Pradesh

(b) Madhya Pradesh Enrollment by Gender

Figure 3.1: Imbalance in Sex Ratio and Education

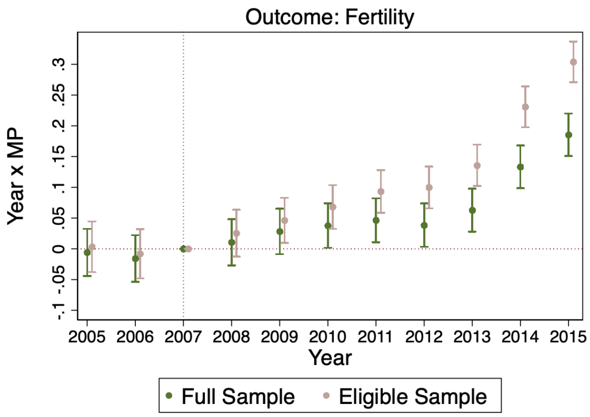
*Notes:* This figure presents raw statistics from our data. Panel (a) present sex ratio (fraction of girls) by different ages in Madhya Pradesh. Panel (b) presents raw data on divergence in enrolment by gender



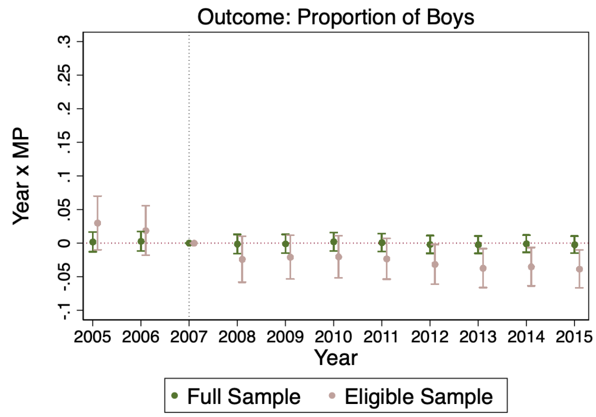

(a) Madhya Pradesh  Chhattisgarh          (b) Madhya Pradesh and Control States

Figure 3.2: Treatment and Control States

*Notes:* This figure shows Madhya Pradesh and the two sets of control states in our analysis. Panel (a) shows the treatment and control state used in our difference-in-differences analysis. Panel (b) shows the set of control states used in the synthetic controls method.



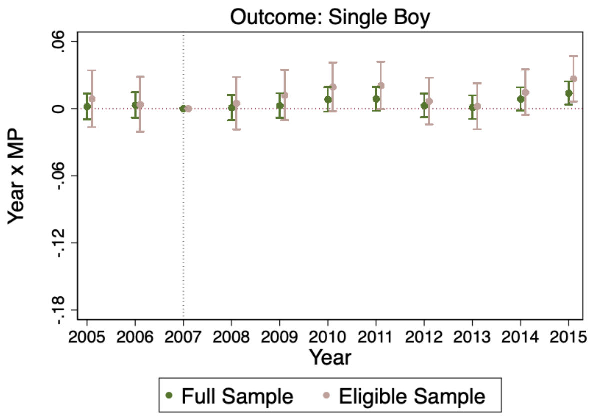

(a) Effect on Fertility




(b) Effect on Sex Ratio

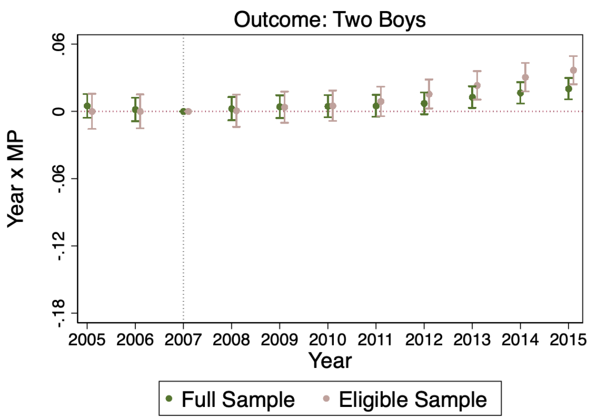
Figure 3.3: Event Study: Fertility and Sex Ratio

*Notes:* This figure present dynamic treatment effects of *Ladli Laxmi* using our difference-in-differences specification from Equation 3.2. Panel (a) present the treatment effect on fertility and panel (b) presents treatment effect on sex ratio. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



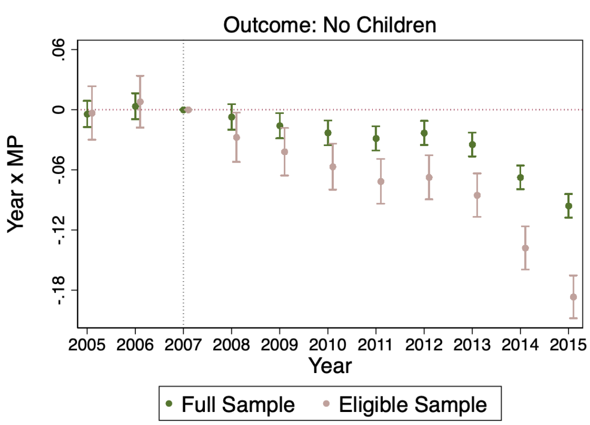

(a) Single Boy Families


(b) Single Girl Families

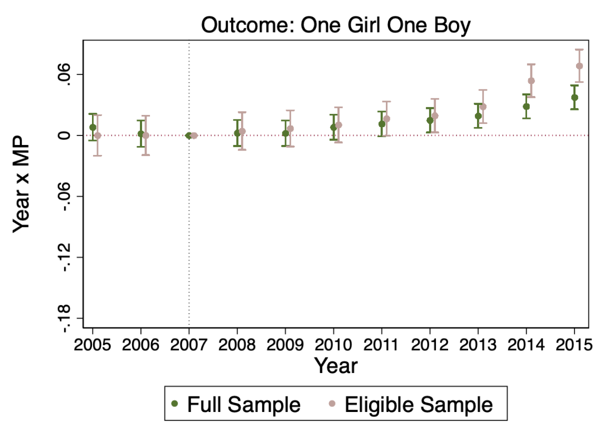



(c) Two Boys Families


(d) Two Girls Families




(e) Zero Child Families




(f) One Girl One Boy Families

Figure 3.4: Event Study: Child Sex Composition

*Notes:* This figure present dynamic treatment effects of *Ladli Laxmi* using our difference-in-differences specification from Equation 3.2. Panel (a) presents the treatment effect on likelihood of families with a single boy, panel (b) presents presents the treatment effect on likelihood of families with a single girl, panel (c) presents the treatment effect on likelihood of families with two boys, panel (d) presents the treatment effect on likelihood of families with two girls, panel (e) presents the treatment effect on likelihood of families with no children, and panel (f) presents the treatment effect on likelihood of families with one girl and one boy. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

Table 3.1: Balance Table (Madhya Pradesh vs Chhattisgarh)

| Variable | (1) Chhattisgarh | (2) Madhya Pradesh | (3) Difference |
|---|---|---|---|
| Urban | 0.222 | 0.287 | 0.065*** |
| | (0.017) | (0.007) | (0.007) |
| Household Size | 6.976 | 6.246 | -0.730*** |
| | (0.094) | (0.018) | (0.036) |
| Children Under 5 | 1.551 | 1.556 | 0.005 |
| | (0.041) | (0.039) | (0.021) |
| Sex of Household Head | 0.951 | 0.977 | 0.027*** |
| | (0.005) | (0.002) | (0.002) |
| Age of Household Head | 42.197 | 40.070 | -2.127*** |
| | (0.254) | (0.355) | (0.165) |
| Education Years (Woman) | 2.360 | 2.257 | -0.102 |
| | (0.145) | (0.153) | (0.080) |
| Total Children Born | 2.140 | 2.302 | 0.162* |
| | (0.164) | (0.171) | (0.090) |
| Age at 1st Birth | 19.234 | 19.402 | 0.168*** |
| | (0.037) | (0.087) | (0.036) |
| Ideal Number of Boys | 1.268 | 1.250 | -0.018 |
| | (0.031) | (0.026) | (0.015) |
| Ideal Number of Girls | 0.910 | 0.911 | 0.001 |
| | (0.015) | (0.011) | (0.007) |
| Partner's Age | 30.546 | 29.996 | -0.550 |
| | (0.678) | (0.617) | (0.347) |
| Hindu | 0.963 | 0.912 | -0.051*** |
| | (0.004) | (0.003) | (0.002) |
| Muslim | 0.022 | 0.071 | 0.049*** |
| | (0.004) | (0.003) | (0.002) |
| Scheduled Caste | 0.126 | 0.185 | 0.059*** |
| | (0.005) | (0.004) | (0.002) |
| Scheduled Tribe | 0.290 | 0.184 | -0.106*** |
| | (0.018) | (0.004) | (0.007) |
| Other Backward Class | 0.482 | 0.421 | -0.062*** |
| | (0.014) | (0.004) | (0.005) |
| Whether first birth was boy | 0.493 | 0.471 | -0.022*** |
| | (0.015) | (0.007) | (0.006) |
| Whether Sterilized | 0.152 | 0.192 | 0.039 |
| | (0.089) | (0.101) | (0.051) |
| Respondent's Age | 25.214 | 25.483 | 0.269 |
| | (0.644) | (0.577) | (0.327) |
| Wealth Index | 2.312 | 2.516 | 0.204*** |
| | (0.045) | (0.017) | (0.018) |

*Notes:* This table presents balance results on several socio-economic variables in Madhya Pradesh and Chattisgarh. Column (3) presents a t-test on difference between Madhya Pradesh and Chhattisgarh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 3.2: Parallel Trends: Fertility and Sex Ratio

| | (1)<br>Fertility | (2)<br>Proportion of Boys |
|---|---|---|
| *Panel A: (2000-07)* | | |
| *MP vs. Control States* | | |
| MP × Year | 0.003* | 0.002* |
| | [0.002] | [0.001] |
| Controls | X | X |
| Observations | 512993 | 357312 |
| *Panel B: (2005-07)* | | |
| *MP vs. Chhattisgarh* | | |
| MP × Year | 0.006 | 0.002 |
| | [0.014] | [0.006] |
| Controls | X | X |
| Observations | 69780 | 50965 |

*Notes:* This table presents evidence on parallel trends for the difference-in-differences model in Equation 3.1 on two outcome variables: fertility and proportion of boys (sex-ratio). Year is coded 1 to 8 in panel A and 1 through 3 in Panel B. MP denotes Madhya Pradesh. Panel A presents evidence for parallel trends comparing Madhya Pradesh and the set of states in panel (b) of Figure 3.2 and panel (b) presents evidence for parallel trends between Madhya Pradesh and Chhattisgarh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 3.3: Diff-in-diff: Fertility and Sex Ratio (MP vs. Chhattisgarh by eligibility)

| | (1) Fertility | (2) Proportion of Boys |
|---|---|---|
| *Panel A: Full Sample* | | |
| MP × Post | 0.077*** | 0.003 |
| | [0.013] | [0.005] |
| Controls | X | X |
| Observations | 318662 | 261296 |
| *Panel B: Eligible Sample* | | |
| MP × Post | 0.150*** | -0.034*** |
| | [0.010] | [0.013] |
| Controls | X | X |
| Observations | 148366 | 98947 |
| *Panel C: One Boy* | | |
| MP × Post | 0.024 | 0.003 |
| | [0.015] | [0.004] |
| Controls | X | X |
| Observations | 26982 | 25126 |
| *Panel D: One Girl* | | |
| MP × Post | 0.080*** | 0.031*** |
| | [0.017] | [0.006] |
| Controls | X | X |
| Observations | 16579 | 15347 |
| *Panel E: No Children* | | |
| MP × Post | 0.124*** | - |
| | [0.006] | - |
| Controls | X | - |
| Observations | 104805 | - |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio). Panel A presents results on entire sample, panel B presents results for the eligible sample, and panels C through E present results for the three configurations of child compositions eligible for the scheme. Year is coded 1 through 3. MP denotes Madhya Pradesh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 3.4: Parallel Trends: Stock Variables

| | (1) No Children | (2) 1 Boy | (3) 1 Girl | (4) 2 Girls | (5) 2 Boys | (6) 1 Girl & 1 Boy | (7) Other |
|---|---|---|---|---|---|---|---|
| *Panel A: (2000-07)* | | | | | | | |
| *MP vs. Control States* | | | | | | | |
| MP $\times$ Year | 0.002$^{**}$ | 0.000 | -0.001$^{*}$ | -0.001$^{**}$ | -0.001$^{***}$ | -0.003$^{***}$ | 0.004$^{***}$ |
| | [0.001] | [0.001] | [0.001] | [0.000] | [0.000] | [0.001] | [0.001] |
| Controls | X | X | X | X | X | X | X |
| Observations | 512993 | 512993 | 512993 | 512993 | 512993 | 512993 | 512993 |
| *Panel B: (2005-07)* | | | | | | | |
| *MP vs. Chhattisgarh* | | | | | | | |
| MP $\times$ Year | -0.003 | 0.003 | 0.001 | 0.001 | -0.002 | -0.003 | 0.004 |
| | [0.005] | [0.004] | [0.004] | [0.003] | [0.003] | [0.004] | [0.005] |
| Controls | X | X | X | X | X | X | X |
| Observations | 69780 | 69780 | 69780 | 69780 | 69780 | 69780 | 69780 |

*Notes:* This table presents evidence on parallel trends for the difference-in-differences model in Equation 3.1 on several different child compositions. Year is coded 1 to 8 in panel A and 1 through 3 in Panel B. MP denotes Madhya Pradesh. Panel A presents evidence for parallel trends comparing Madhya Pradesh and the set of states in panel (b) of Figure 3.2 and panel (b) presents evidence for parallel trends between Madhya Pradesh and Chhattisgarh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table 3.5: Diff-in-diff: Stock Variables (MP vs. Chhattisgarh by eligibility)

| | (1) No Children | (2) 1 Boy | (3) 1 Girl | (4) 2 Girls | (5) 2 Boys | (6) 1 Girl & 1 Boy | (7) Other |
|---|---|---|---|---|---|---|---|
| *Panel A: Full Sample* | | | | | | | |
| MP × Post | -0.045*** | 0.009** | 0.007* | 0.001 | 0.011*** | 0.016*** | 0.001 |
| | [0.005] | [0.004] | [0.004] | [0.002] | [0.003] | [0.004] | [0.005] |
| Controls | X | X | X | X | X | X | X |
| Observations | 318662 | 318662 | 318662 | 318662 | 318662 | 318662 | 318662 |
| *Panel B: Eligible Sample* | | | | | | | |
| MP × Post | -0.103*** | 0.022*** | 0.034*** | 0.001 | 0.018*** | 0.028*** | - |
| | [0.009] | [0.008] | [0.007] | [0.001] | [0.002] | [0.003] | - |
| Controls | X | X | X | X | X | X | - |
| Observations | 148366 | 148366 | 148366 | 148366 | 148366 | 148366 | - |
| *Panel C: One Boy* | | | | | | | |
| MP × Post | - | -0.003 | - | - | 0.018** | -0.004 | - |
| | - | [0.014] | - | - | [0.009] | [0.008] | - |
| Controls | - | X | - | - | X | X | - |
| Observations | - | 26982 | - | - | 26982 | 26982 | - |
| *Panel D: One Girl* | | | | | | | |
| MP × Post | - | - | 0.010 | -0.029*** | - | 0.064*** | - |
| | - | - | [0.017] | [0.009] | - | [0.011] | - |
| Controls | - | - | X | X | - | X | - |
| Observations | - | - | 16579 | 16579 | - | 16579 | - |
| *Panel E: No Children* | | | | | | | |
| MP × Post | -0.059*** | 0.010*** | -0.016*** | 0.009*** | 0.018*** | 0.037*** | - |
| | [0.004] | [0.004] | [0.003] | [0.001] | [0.002] | [0.003] | - |
| Controls | X | X | X | X | X | X | - |
| Observations | 104805 | 104805 | 104805 | 104805 | 104805 | 104805 | - |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on several different child compositions. Panel A presents results on entire sample, panel B presents results for the eligible sample, and panels C through E present results for the three configurations of child compositions eligible for the scheme. Year is coded 1 through 3. MP denotes Madhya Pradesh. *** $p < .01$, ** $p < .05$, * $p < .1$

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# Appendix A: Chapter 1: Equilibrium Effects of Subsidizing Public Services

## A Additional Tables and Figures

### A.1 Additional Figures

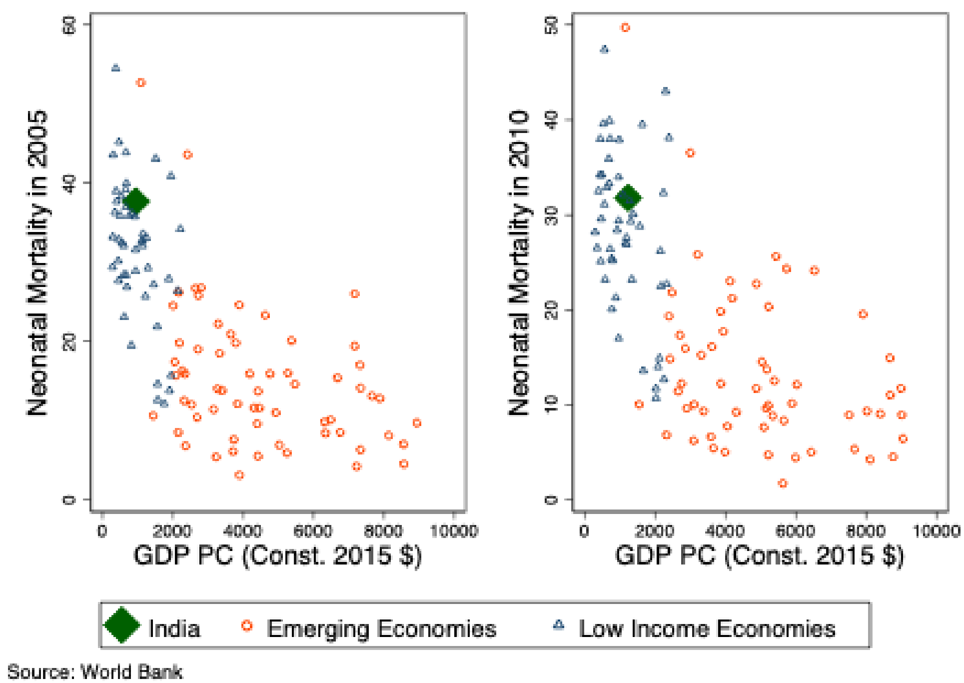



Figure A1: Neonatal Mortality across Countries

*Notes:* This figure displays rates of neonatal mortality and GDP per-capita across numerous low-income and emerging economies for years 2005 (left) and 2010 (right).

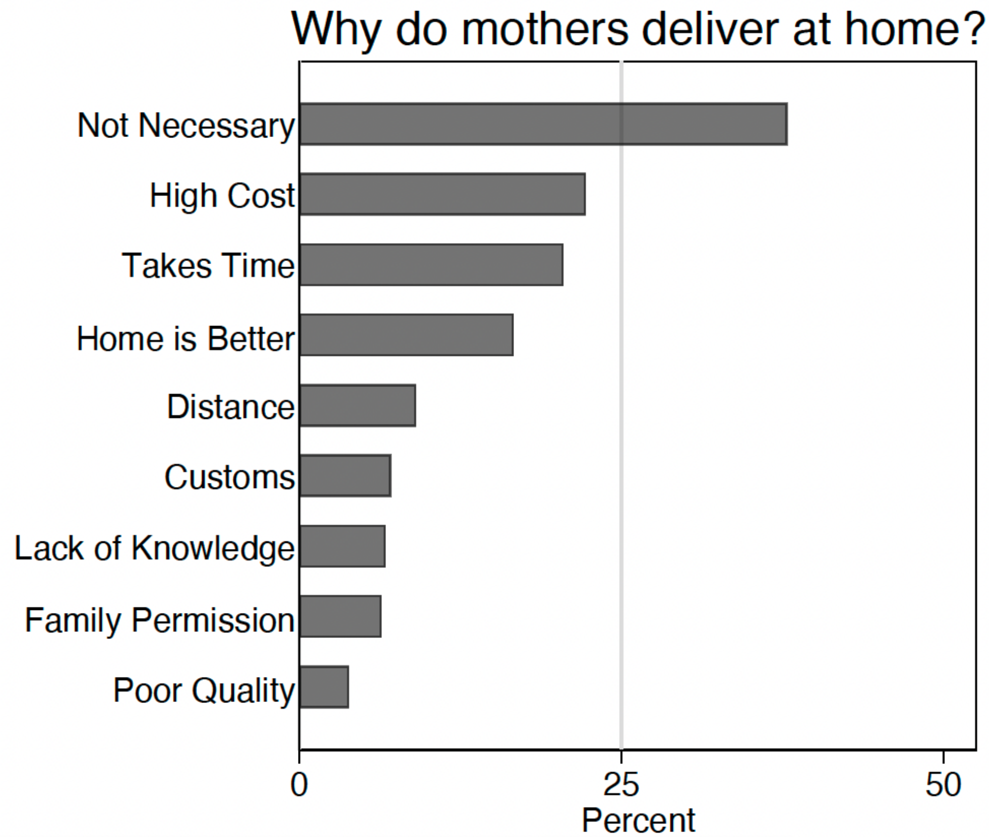
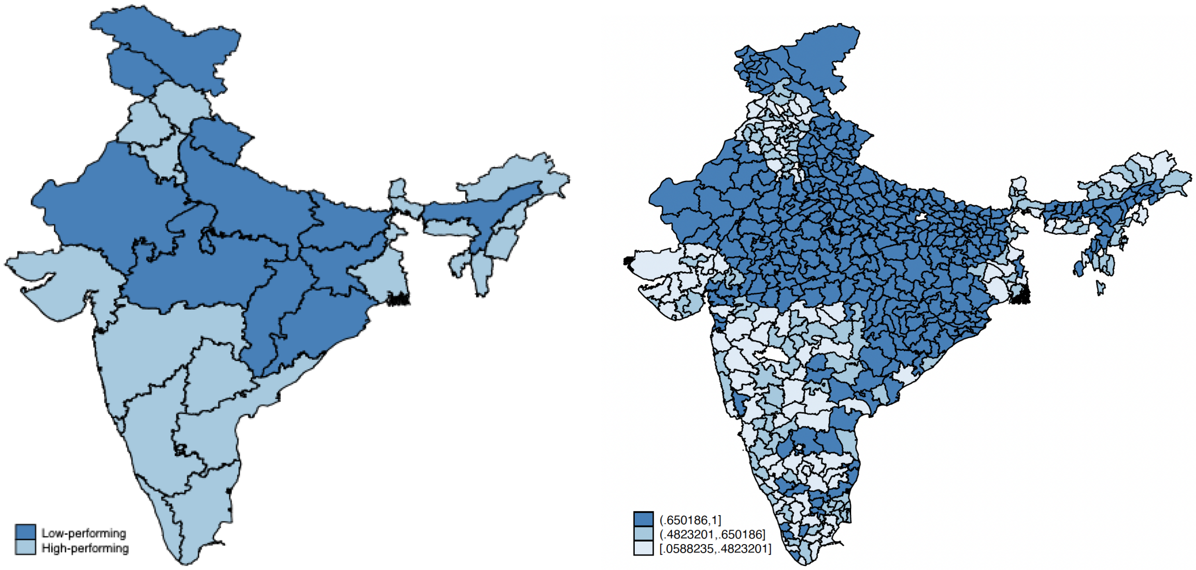



Figure A2: Reported Reasons for Home Births

*Notes:* This figure displays the share of mothers reporting various reasons for delivering at home in DLHS 2 (2002-03). The reported set of reasons is listed on the vertical axis on the left.

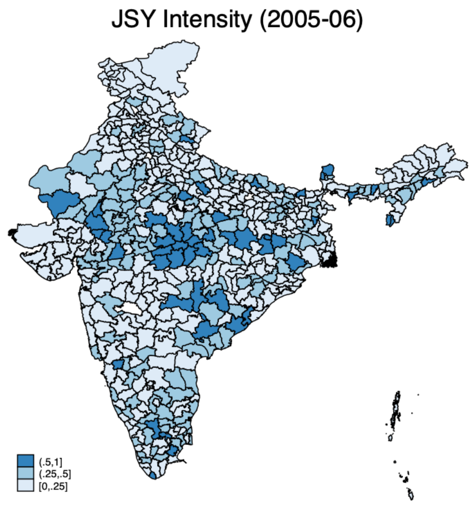



(a) Low-Performing States

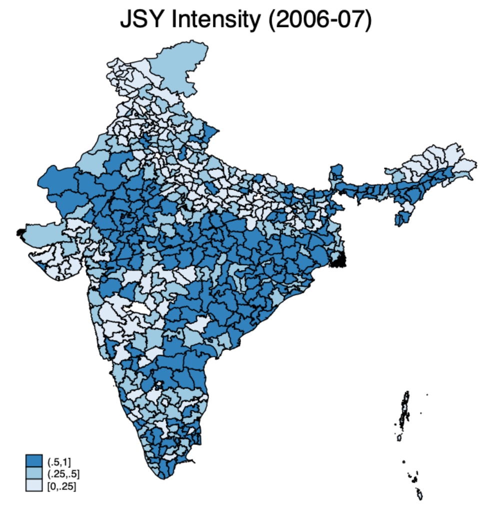
(b) Eligibility

Figure A3: Low-Performing States and Eligibility across Districts

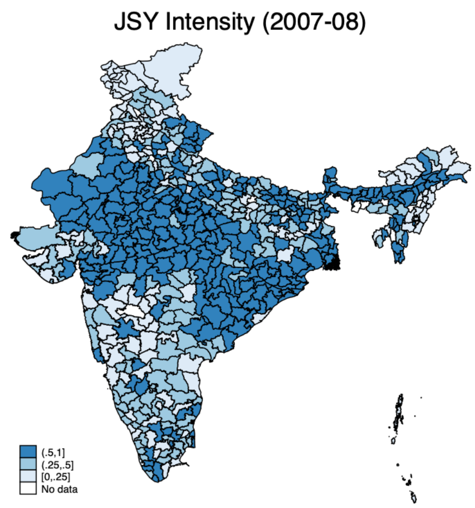
*Notes:* This figure displays low and high-performing states (left) and fraction of mothers eligible for JSY incentives in a district (right) as defined by the authors. Note, all mothers in low-performing states were eligible for JSY incentives.




(a) JSY 2005




(b) JSY 2006




(c) JSY 2007

Figure A4: Rollout of JSY across districts

*Notes:* This figure displays the gradual roll-out of JSY across Indian districts over three years (2005, 2006 and 2007). Each figure displays the fraction of eligible mothers in a district that actually received financial assistance under JSY in a given year. In other words, each figure captures the intensity of JSY in Indian districts over three years after the official announcement of JSY.

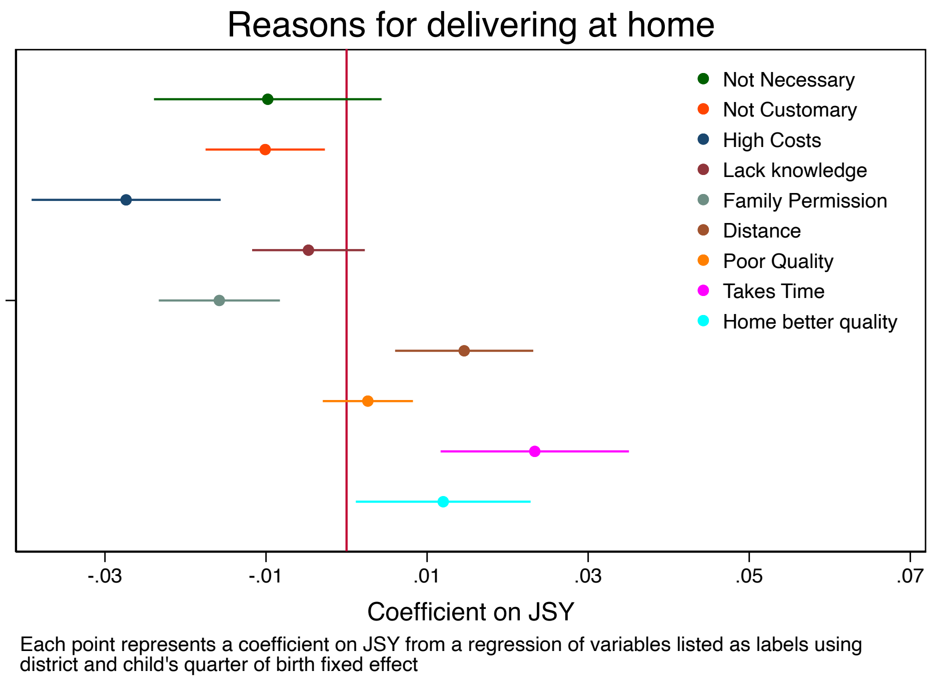
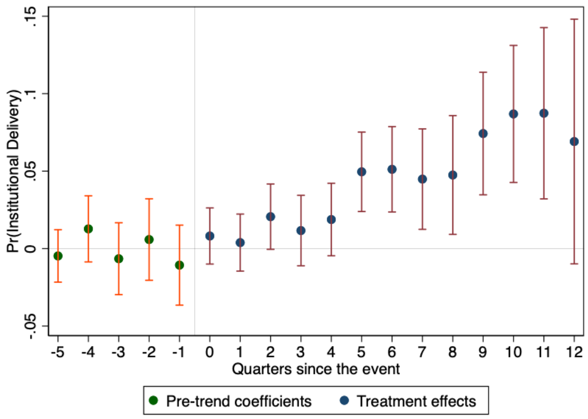
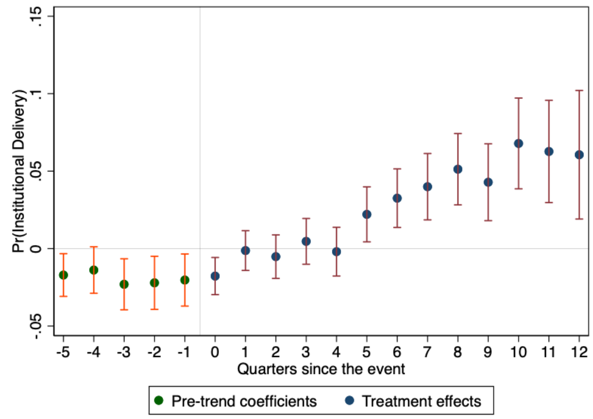



Figure A5: JSY and Reasons for Delivering at Home

*Notes:* This figure presents difference-in-difference estimates of JSY on stated reasons for delivering at home instead of an institutional facility. Each dot corresponds to an estimated coefficient for a dependent variable listed in the legend, and horizontal lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



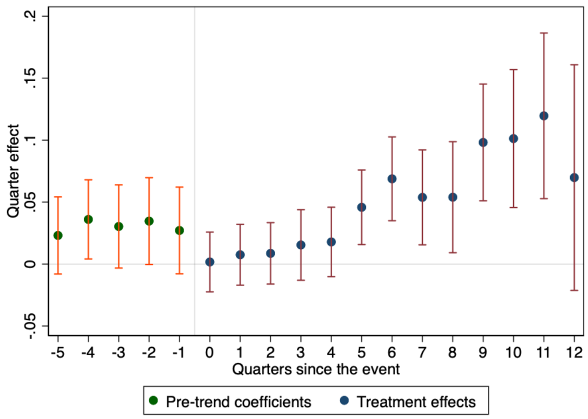

(a) BPL Sample



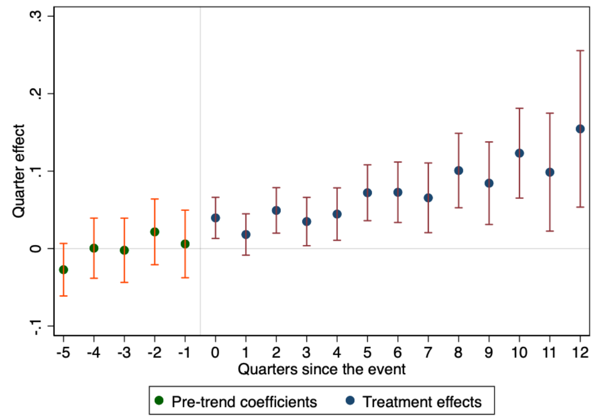

(b) Non-BPL Sample

Figure A6: Effect of JSY on Institutional Delivery by SES

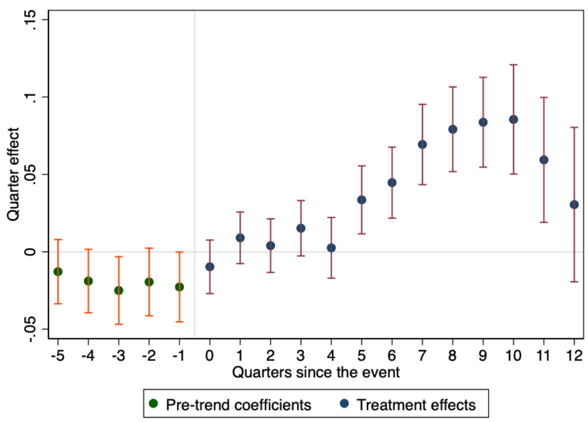
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of institutional deliveries by SES (BPL status), following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



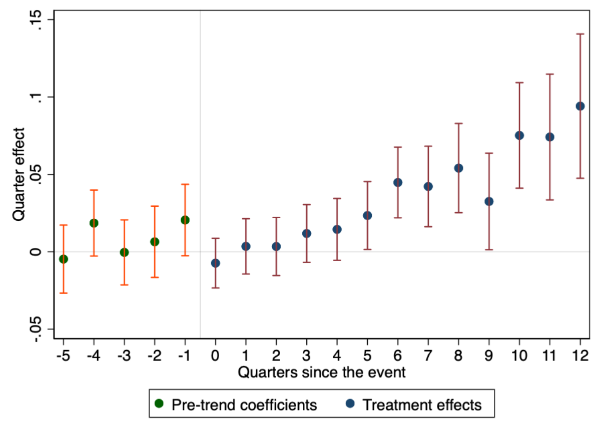

(a) BPL, Low-Risk Sample




(b) BPL, High-Risk Sample



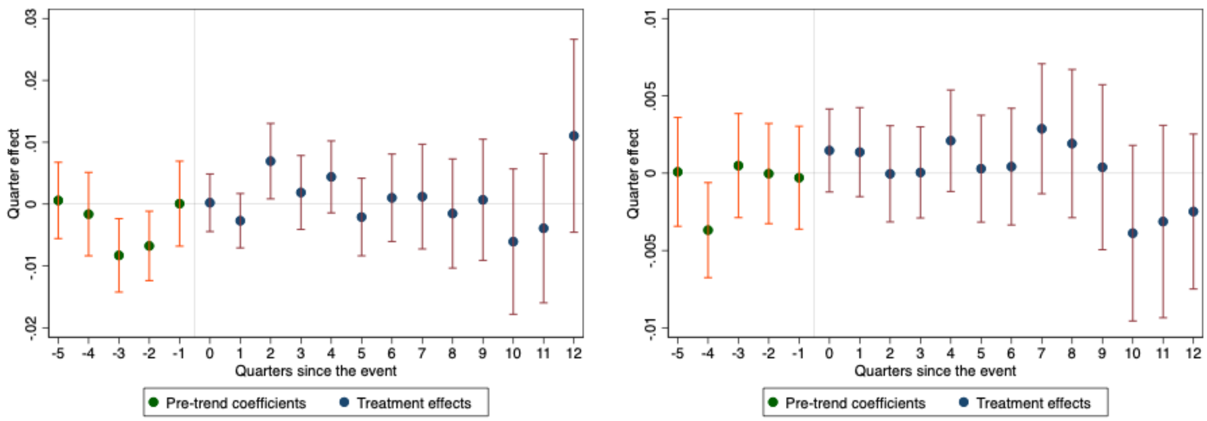

(c) NonBPL, Low-Risk Sample




(d) NonBPL, High-Risk Sample

Figure A7: Effect of JSY on Institutional Delivery by Types

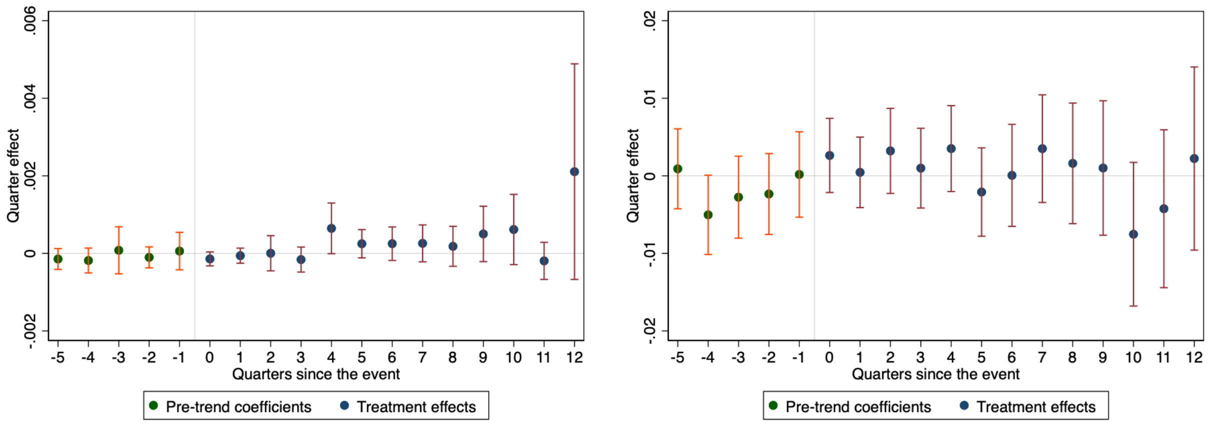
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of institutional deliveries for different types of patients (combinations of patients' SES and ex-ante risk), following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.




(a) BPL Sample (b) Non-BPL Sample

Figure A8: Effect of JSY on Perinatal Mortality by SES level

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of perinatal mortality by SES (BPL status), following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.




(a) Low-Risk Sample

(b) High-Risk Sample

Figure A9: Effect of JSY on Perinatal Mortality by Risk level

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of perinatal mortality by patient's ex-ante risk level, following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

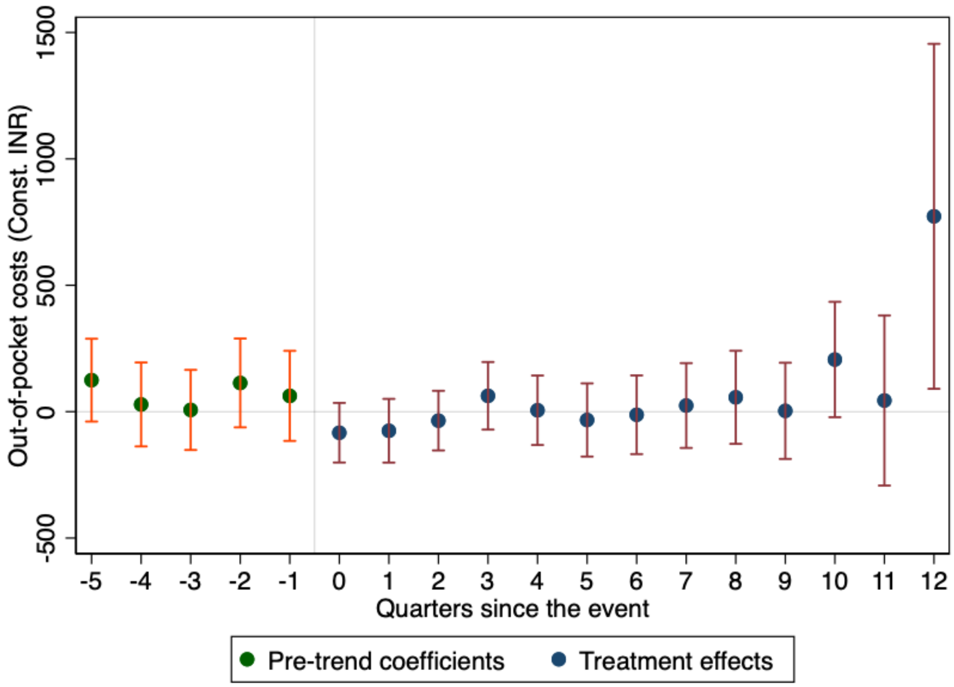
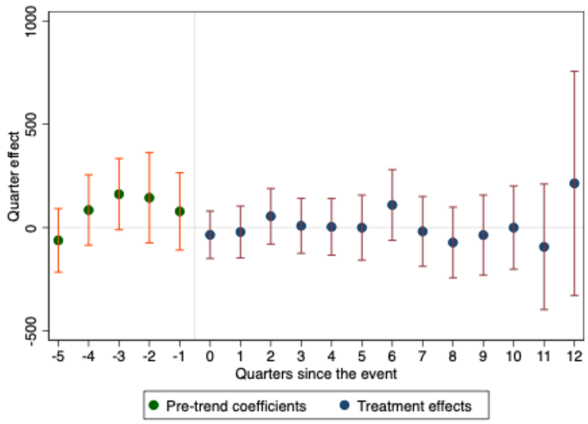
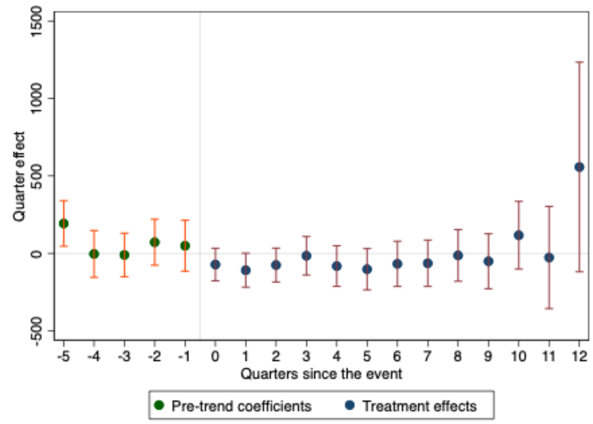



Figure A10: Effect of JSY on OOP Costs (Const. INR)

*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in Constant Indian Rupees), following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



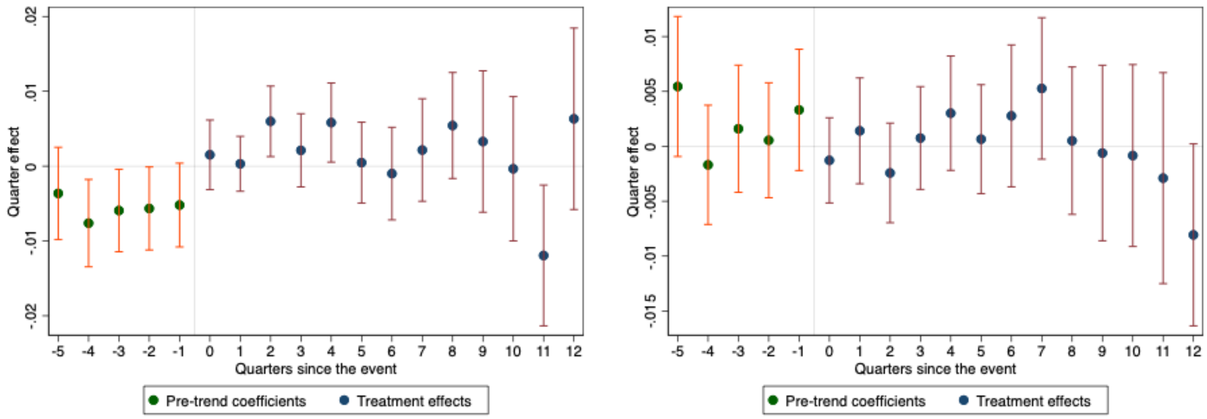

(a) BPL Sample




(b) Non-BPL Sample

Figure A11: Effect of JSY on OOP Costs by SES level (Const. INR)

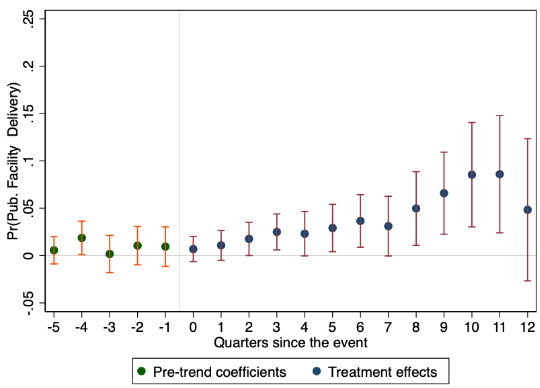
*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in Constant Indian Rupees) by SES (BPL status), following our empirical strategy in section 1.4. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



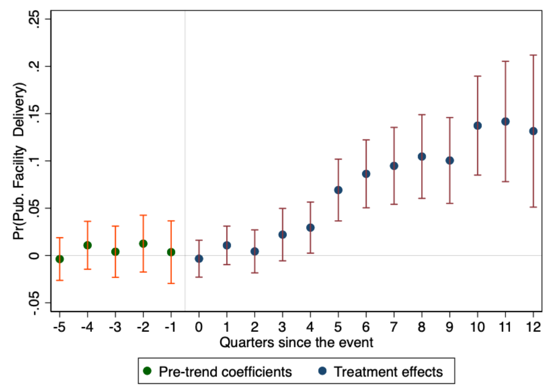

(a) Low Capacity (b) High Capacity

Figure A12: Effect of JSY on Perinatal Mortality by Public Sector Capacity

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of perinatal mortality separately by public sector healthcare capacity, following our empirical strategy in section 1.4. Panel A presents results for low-capacity districts. Panel B presents results for high-capacity districts. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



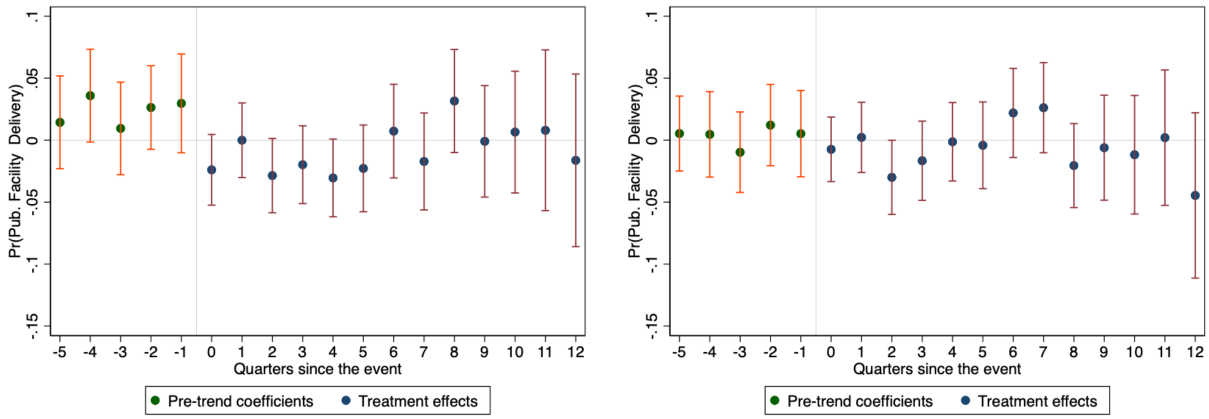

(a) Public Facility (Low Capacity)




(b) Public Facility (High Capacity)

Figure A13: Effect of JSY on sorting into public facilities by Public Capacity

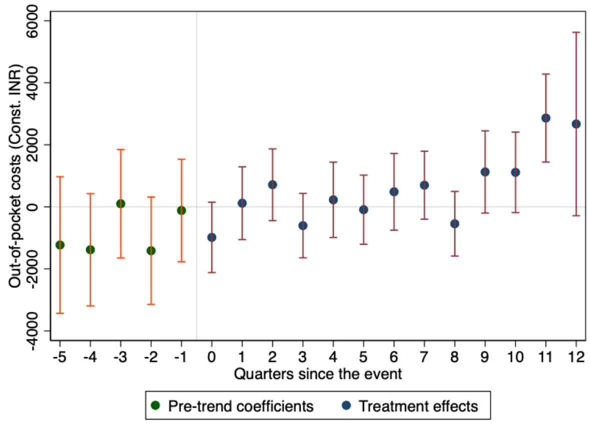
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of delivery at a public facility separately by public sector healthcare capacity, following our empirical strategy in section 1.4. Panel A presents results for low-capacity districts. Panel B presents results for high-capacity districts. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



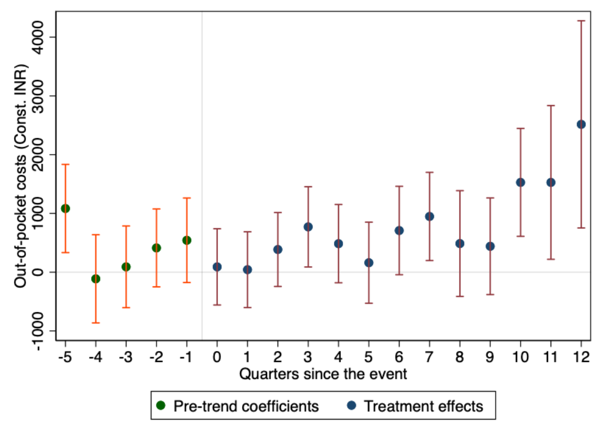

(a) Public Facilities (Ineligible, high-Risk) (b) Public Facilities (Ineligible, Low-Risk)

Figure A14: Sorting into public facilities for ineligible mothers by risk level

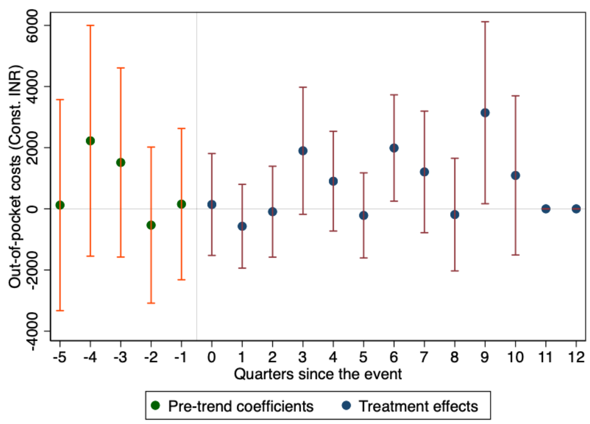
*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of delivery at public facilities for ineligible mothers separately by ex-ante risk level, following our empirical strategy in section 1.4. Panel A presents results for the high-risk mothers. Panel B presents results for the low-risk mothers. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



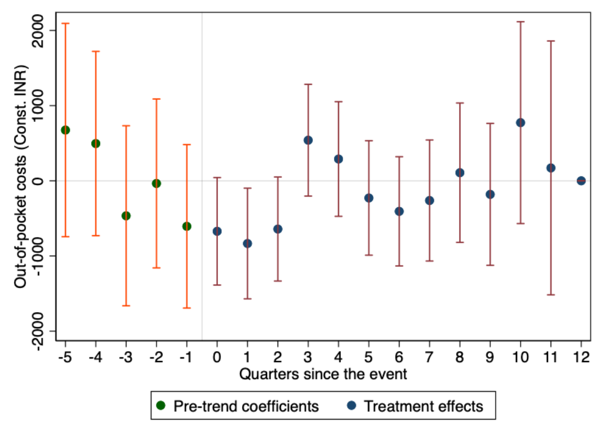

(a) High-Performing States/BPL




(b) High-Performing States/Non-BPL



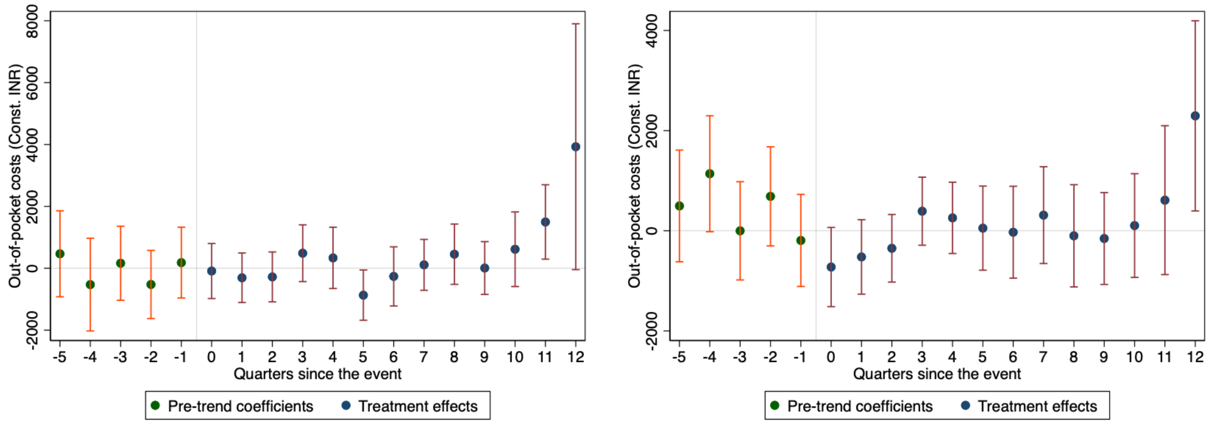

(c) Low-Performing States/BPL




(d) Low-Performing States/Non-BPL

Figure A15: Private facility price effect (by SES)

*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in constant Indian rupees) at private facilities, following our empirical strategy in section 1.4 with an additional difference taken over the home option. Panel A presents results for deliveries at private facilities in HPS for BPL sub-sample. Panel B presents results for deliveries at private facilities in HPS for Non-BPL sub-sample. Panel C presents results for deliveries at private facilities in LPS for BPL sub-sample. Panel D presents results for deliveries at private facilities in LPS for Non-BPL sub-sample. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Additionally, the regressions include dummy variables for ex-ante risk-deciles and BPL status of mothers. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

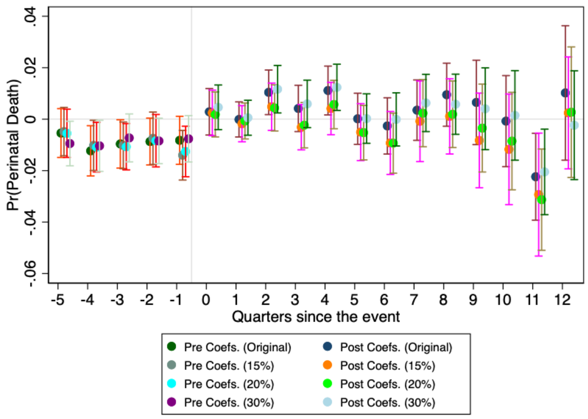



(a) High Capacity

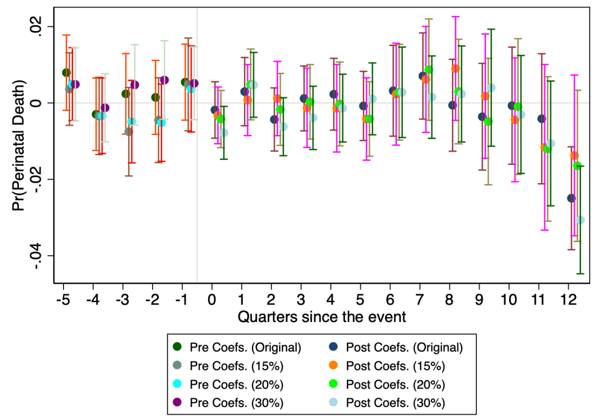
(b) Low Capacity

Figure A16: Private facility price effect (by Public Sector Capacity)

*Notes:* This figure presents event study evidence of the effect of JSY on out-of-pocket costs (in constant Indian rupees) at private facilities, following our empirical strategy in section 1.4 with an additional difference taken over the home option. Panel A presents results for deliveries at private facilities in districts with high public sector capacity. Panel B presents results for deliveries at private facilities in districts with high public sector capacity. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Additionally, the regressions include dummy variables for ex-ante risk-deciles and BPL status of mothers. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.




(a) Low Capacity Districts




(b) High Capacity Districts

Figure A17: Robustness: Effect of JSY on Perinatal Mortality by Capacity (Obgyns)

*Notes:* This figure presents event study evidence of the effect of JSY on likelihood of perinatal mortality for high-risk patients by a district's public sector capacity, following our empirical strategy in section 1.4 across the four discrete definitions of treatment under JSY including our original definition of treatment in subsection 1.3.2. The figure uses quarterly data on pregnant mothers in a time window of 5 quarters before and 12 quarters after the the district was treated under JSY, and exploits the gradual roll-out of JSY across Indian districts. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

## A.2 Additional Tables

Table A1: Descriptive Statistics

| | Mean | Std. Dev. | Bottom 10% | Median | Top 10% | Obs. |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Mother Characteristics** | | | | | | |
| Caste - SC | 0.190 | 0.10 | 0.05 | 0.19 | 0.31 | 592 |
| Caste - ST | 0.193 | 0.27 | 0.00 | 0.06 | 0.67 | 592 |
| Mom's age at birth' | 24.85 | 1.48 | 22.96 | 24.84 | 26.40 | 592 |
| Whether under 18 | 0.076 | 0.05 | 0.02 | 0.07 | 0.14 | 592 |
| Whether above 35 | 0.053 | 0.04 | 0.01 | 0.05 | 0.10 | 592 |
| Mother's Schooling | 8.297 | 1.17 | 6.85 | 8.23 | 9.86 | 592 |
| Father's Schooling | 8.984 | 1.00 | 7.62 | 9.05 | 10.17 | 574 |
| Below Poverty Line | 0.282 | 0.16 | 0.08 | 0.27 | 0.51 | 592 |
| Rural | 0.758 | 0.19 | 0.53 | 0.80 | 0.94 | 592 |
| Hindu | 0.754 | 0.26 | 0.33 | 0.86 | 0.97 | 592 |
| Muslim | 0.125 | 0.16 | 0.01 | 0.08 | 0.31 | 592 |
| Perinatal Death | 0.015 | 0.01 | 0.00 | 0.01 | 0.03 | 592 |
| **Facility Characteristics** | | | | | | |
| Pub. Beds (per 10k) | 2.536 | 3.06 | 0.49 | 1.65 | 5.10 | 353 |
| Pub. Nurses (per 10k) | 0.333 | 0.46 | 0.04 | 0.21 | 0.69 | 353 |
| Pub. OBGYNs (per 10k) | 0.025 | 0.05 | 0.00 | 0.01 | 0.05 | 353 |
| Av. Costs (Const. INR) | 2565.9 | 2031.2 | 758.5 | 1884.2 | 5175.7 | 591 |
| Private Price (Const. INR) | 9733.6 | 3945.9 | 5353.1 | 9076.1 | 14930.4 | 581 |
| Public Price (Const. INR) | 2428.7 | 1159.7 | 1251.60 | 2200.2 | 3879.7 | 590 |
| Home Price (Const. INR) | 681.2 | 428.5 | 246.1 | 600.9 | 1182.9 | 544 |
| **Village Characteristics** | | | | | | |
| Distance PHC (kms.) | 10.43 | 6.09 | 5.14 | 8.95 | 16.32 | 582 |
| Distance CHC (kms.) | 17.73 | 9.03 | 9.01 | 16.19 | 28.17 | 582 |
| Distance District Hosp. (kms.) | 34.45 | 16.97 | 16.87 | 33.75 | 52.01 | 583 |
| Distance Pvt. Hosp. (kms.) | 20.76 | 19.48 | 8.01 | 16.79 | 35.56 | 583 |

*Notes:* This table presents descriptive statistics for our final sample for analysis. The data comes from rounds 2, 3 and 4 of the DLHS. Mother characteristics come from the DLHS module for eligible women. Facility characteristics come from self-reported information on out-of-pocket costs (interpreted as prices and normalized to constant 2010 Indian rupees) and perinatal mortality as well as the DLHS facilities module. Finally, the village characteristics come from the village module of the DLHS.

Table A2: Ex-ante risks and perinatal mortality

| | Perinatal Death |
|---|---|
| Pre-labor Swelling | 0.003*** |
| | [0.001] |
| Pre-labor Paleness | 0.001 |
| | [0.001] |
| Pre-labor Visual Disturbance | -0.001 |
| | [0.001] |
| Pre-labor Fatigue | -0.001 |
| | [0.001] |
| Pre-labor Convulsion | 0.000 |
| | [0.001] |
| Pre-labor Foetus Movement | -0.002* |
| | [0.001] |
| Pre-labor Abnormal Position | 0.005*** |
| | [0.002] |
| Pre-labor Malaria | 0.003 |
| | [0.001] |
| Pre-labor Vomit | -0.002** |
| | [0.001] |
| Pre-labor Jaundice | 0.005* |
| | [0.002] |
| Pre-labor Bleeding | 0.007*** |
| | [0.002] |
| Pre-labor Blood Pressure | -0.001 |
| | [0.001] |
| Pre-labor Vaginal Discharge | 0.006*** |
| | [0.001] |
| Other Pre-labor Complication | 0.000 |
| | [0.001] |
| Multiple Births | 0.052*** |
| | [0.002] |
| Previous Abortions | -0.002 |
| | [0.001] |
| Previous Still-births | 0.006*** |
| | [0.001] |
| Previous Deaths | 0.093*** |
| | [0.001] |
| Age less than 18 | 0.002** |
| | [0.001] |
| Age above 35 | 0.011*** |
| | [0.001] |
| Birth Order | -0.010*** |
| | [0.000] |
| $R^2$ | 0.077 |
| Adjusted $R^2$ | 0.077 |
| Observations | 228610 |

*Notes:* The table presents regression results from a regression of perinatal mortality on our twenty enlisted measured of ex-ante risks for mothers in our sample. The results from this regression are used to create a predicted continuous measure of riskiness for each mother. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A3: Balance Table

| Variable | Early Treatment | Late Treatment | Difference |
|---|---|---|---|
| Birth at institutional facility | 0.431 | 0.450 | 0.019 |
| | (0.267) | (0.238) | (0.023) |
| Birth at private facility | 0.191 | 0.211 | 0.020 |
| | (0.182) | (0.166) | (0.016) |
| Birth at public facility | 0.224 | 0.221 | -0.004 |
| | (0.141) | (0.179) | (0.015) |
| Birth at home | 0.585 | 0.568 | -0.017 |
| | (0.263) | (0.237) | (0.023) |
| Perinatal Death | 0.016 | 0.015 | -0.001 |
| | (0.018) | (0.015) | (0.001) |
| Delivery Cost (Const. INR) | 2,952 | 3,116 | 164 |
| | (2,589) | (2,247) | (223) |
| SC | 0.177 | 0.200 | 0.023** |
| | (0.095) | (0.116) | (0.010) |
| ST | 0.183 | 0.152 | -0.030 |
| | (0.212) | (0.259) | (0.022) |
| Mother's age at birth | 25.074 | 25.646 | 0.572*** |
| | (1.557) | (1.631) | (0.145) |
| Mothers under 18 yrs | 0.070 | 0.052 | -0.018*** |
| | (0.058) | (0.053) | (0.005) |
| Mothers over 35 yrs | 0.058 | 0.065 | 0.008* |
| | (0.046) | (0.050) | (0.004) |
| Mothers Education | 8.133 | 8.460 | 0.327*** |
| | (1.158) | (1.223) | (0.109) |
| BPL | 0.345 | 0.297 | -0.049*** |
| | (0.208) | (0.198) | (0.018) |
| Rural | 0.774 | 0.742 | -0.032** |
| | (0.129) | (0.173) | (0.014) |
| Received at least 3 ANCs | 0.504 | 0.502 | -0.003 |
| | (0.277) | (0.259) | (0.024) |
| Received at least 6 ANC Tests | 0.372 | 0.346 | -0.026 |
| | (0.301) | (0.269) | (0.026) |
| Distance to CHC | 18.126 | 16.954 | -1.171 |
| | (7.787) | (9.577) | (0.802) |
| Distance to public Hosp. | 31.801 | 31.453 | -0.348 |
| | (12.935) | (15.305) | (1.301) |
| Distance to private Hosp. | 20.138 | 20.469 | 0.331 |
| | (10.591) | (22.297) | (1.627) |
| Number of Districts | 225 | 261 | 580 |

*Note:* The table presents summary statistics for several variables during the period before JSY was announced across districts that were treated early (among first 50% of the treated districts) vs districts that were treated later.

Table A4: Balance Table by Capacity

| Variable | Low-Capacity Districts | High-Capacity Districts | Difference |
|---|---|---|---|
| Birth at institutional facility | 0.374 | 0.423 | 0.048** |
| | (0.235) | (0.215) | (0.024) |
| Birth at private facility | 0.181 | 0.166 | -0.014 |
| | (0.151) | (0.156) | (0.017) |
| Birth at public facility | 0.175 | 0.240 | 0.065*** |
| | (0.152) | (0.157) | (0.017) |
| Birth at home | 0.644 | 0.594 | -0.050** |
| | (0.235) | (0.212) | (0.024) |
| Perinatal Death | 0.017 | 0.016 | -0.001 |
| | (0.016) | (0.021) | (0.002) |
| Delivery Cost (Const. INR) | 2,401 | 2,705 | 303 |
| | (1,797) | (1,934) | (204) |
| SC | 0.175 | 0.186 | 0.011 |
| | (0.097) | (0.111) | (0.011) |
| ST | 0.178 | 0.180 | 0.002 |
| | (0.272) | (0.257) | (0.028) |
| Mother's age at birth | 25.546 | 25.431 | -0.115 |
| | (1.547) | (1.702) | (0.175) |
| Mothers under 18 yrs | 0.062 | 0.060 | -0.002 |
| | (0.052) | (0.059) | (0.006) |
| Mothers over 35 yrs | 0.073 | 0.059 | -0.014*** |
| | (0.047) | (0.046) | (0.005) |
| Mothers Education | 8.115 | 8.280 | 0.165 |
| | (1.124) | (1.059) | (0.118) |
| BPL | 0.314 | 0.302 | -0.011 |
| | (0.179) | (0.207) | (0.021) |
| Rural | 0.790 | 0.771 | -0.019 |
| | (0.128) | (0.138) | (0.014) |
| Received at least 3 ANCs | 0.426 | 0.483 | 0.057** |
| | (0.257) | (0.246) | (0.027) |
| Received at least 6 ANC Tests | 0.287 | 0.316 | 0.029 |
| | (0.271) | (0.239) | (0.028) |
| Distance to CHC | 18.088 | 17.136 | -0.952 |
| | (9.249) | (7.934) | (0.930) |
| Distance to Public Hosp. | 32.098 | 31.693 | -0.404 |
| | (14.434) | (13.915) | (1.529) |
| Distance to Private Hosp. | 19.614 | 21.800 | 2.186 |
| | (17.261) | (17.894) | (1.898) |
| Observations | 173 | 172 | 580 |

*Note:* The table presents summary statistics for several variables during the period before JSY was announced across districts with above and below median capacity.

Table A5: First Principle Component

| | Eigenvector<br>(1)<br>Comp1 |
|---|---|
| OBGYN per 10,000 | .5406908 |
| STAFF per 10,000 | .6040319 |
| BEDS per 10,000 | .5854903 |

*Note:* The table presents loadings on the first principle component of three public sector capacity variables (OBGYNs, Nursing staff, beds) each normalized by 10,000 persons from DLHS 2 (before JSY was implemented). The results are used to create a continuous measure for district level public-sector capacity before JSY.

Table A6: Did Government Invest In Public Facilities in treated districts?

| | Obgyns/10K | Nurses/10K | Beds/10K |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treated | -0.000 | -0.000*** | -0.000 |
| | [0.000] | [0.000] | [0.000] |
| District FE | Y | Y | Y |
| Year FE | Y | Y | Y |
| Observations | 450 | 450 | 450 |

*Note:* The table presents evidence that government did not invest in public sector capacity alongside JSY. Columns (1)-(3) present results from a difference-in-difference regression of number of OBGYNs, Nursing staff, beds respectively on treatment status of a district using data from from DLHS 2 (before JSY) and DLHS 3 (after JSY). Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p <$ .01, $^{**}p < .05$, $^{*}$ $p < .1$

Table A7: Does place of birth matter for perinatal mortality?

| | Y = Perinatal Death | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Private Facility | 0.0000 | -0.0046*** | -0.0029*** | 0.0003* | -0.0037*** |
| | [0.0006] | [0.0006] | [0.0007] | [0.0001] | [0.0010] |
| Public Facility | -0.0010 | -0.0036*** | -0.0023*** | 0.0002 | -0.0025** |
| | [0.0006] | [0.0006] | [0.0006] | [0.0001] | [0.0009] |
| SES | Yes | Yes | Yes | Yes | Yes |
| Risk Controls | No | Yes | Yes | | |
| Quarter FE | No | No | Yes | | |
| District FE | No | No | Yes | | |
| High Risk Sample | | | | No | Yes |
| Observations | 289246 | 228610 | 225531 | 114870 | 174376 |

*Note:* This table shows the extent to which choice of a delivery facility (private, public or home) can explain perinatal mortality using several regressions of a dummy variable for perinatal death on choice of facility.

Table A8: Average effect of JSY on Institutional Delivery by Types

| | Y = {Whether Institutional Birth} | | | |
|---|---|---|---|---|
| | BPL Less Risk | BPL High Risk | Non-BPL Less Risk | Non-BPL High Risk |
| | (1) | (2) | (3) | (4) |
| JSY | $0.045^{***}$ | $0.058^{***}$ | $0.033^{***}$ | $0.026^{***}$ |
| | [0.013] | [0.014] | [0.008] | [0.008] |
| Dependent Var. Mean (2004-05) | .22 | .26 | .38 | .45 |
| Treatment Effect (%) | 20.87% | 22.14% | 8.6% | 5.89% |
| Number of Districts | 566 | 552 | 577 | 576 |
| District Fixed Effect | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y |
| Observations | 29293 | 29595 | 82847 | 82189 |

*Note:* This table presents our estimates of the impact of JSY on the likelihood of delivering at an institutional facility by patient type. Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In column (1), we present average effect of JSY for the below poverty line and low-risk sub-sample. In column (2), we present average effect of JSY for the below poverty line and high-risk sub-sample. In column (3), we present average effect of JSY for the above poverty line and low-risk sub-sample. In column (4), we present average effect of JSY for the above poverty line and high-risk sub-sample. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A9: Average effect of JSY on Deliveries at Private Facilities by Types

| | Y = {Whether Delivery at Private Facility} | | | |
|---|---|---|---|---|
| | BPL Less Risk | BPL High Risk | Non-BPL Less Risk | Non-BPL High Risk |
| | (1) | (2) | (3) | (4) |
| JSY | -0.005 | -0.017 | -0.000 | -0.013$^{*}$ |
| | [0.009] | [0.011] | [0.006] | [0.008] |
| Dependent Var. Mean (2004-05) | .06 | .09 | .18 | .23 |
| Treatment Effect (%) | -7.99% | -18.81% | -.17% | -5.73% |
| Number of Districts | 565 | 552 | 577 | 576 |
| District Fixed Effect | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y |
| Observations | 29263 | 29578 | 82763 | 82094 |

*Notes:* This table presents our estimates of the impact of JSY on the likelihood of delivering at a private facility by patient type. Estimates are from the staggered DiD specification in Equation 3.1. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In column (1), we present average effect of JSY for the below poverty line and Low-Risk sub-sample. In column (2), we present average effect of JSY for the below poverty line and high-Risk sub-sample. In column (3), we present average effect of JSY for the above poverty line and Low-Risk sub-sample. In column (4), we present average effect of JSY for the above poverty line and high-Risk sub-sample. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A10: Distance and delivery place

| | Home Birth | Public Birth | Private Birth |
|---|---|---|---|
| | (1) | (2) | (3) |
| Distance to Pvt. Hospital | 0.0004 | 0.0005 | -0.0009$^{**}$ |
| | [0.0004] | [0.0004] | [0.0003] |
| Distance to Pub. Hospital | 0.0014$^{***}$ | -0.0018$^{***}$ | 0.0005 |
| | [0.0005] | [0.0005] | [0.0003] |
| District FE | Y | Y | Y |
| Year FE | Y | Y | Y |
| Birth Order | Y | Y | Y |
| Individual Conts. | Y | Y | Y |
| Risk Dummies | Y | Y | Y |
| Observations | 154780 | 154780 | 154780 |

*Note:* This table presents evidence that distance to a facility affects patient choice. Column (1) presents results from a fixed effects regression of a dummy variable for home birth on distance to nearest (secondary level) public and private facilities while controlling for district, year, birth order risk deciles fixed effects and individual level controls. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A11: Triple Difference: JSY and C-sections at private facilities

| | Y = Whether birth via C-section | | | |
|---|---|---|---|---|
| | | Private Facility Birth | | |
| | Full Sample | HPS | HPS/Non-BPL | HPS/BPL |
| | (1) | (2) | (3) | (4) |
| JSY | 0.029*** | 0.038*** | 0.035*** | 0.049** |
| | [0.007] | [0.009] | [0.009] | [0.019] |
| Dependent Var. Mean (2004-05) | .28 | .31 | .32 | .29 |
| Treatment Effect (%) | 10.43% | 12.04% | 11.19% | 17.22% |
| Number of Districts | 495 | 235 | 235 | 230 |
| District Fixed Effect | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | Y | Y | Y | Y |
| BPL Fixed Effect | Y | Y | Y | Y |
| Observations | 128160 | 42662 | 31819 | 10826 |

*Notes:* This table presents our estimates of the impact of JSY on likelihood of C-sections at private facilities. Estimates are from the triple difference specification similar to Equation 3.1 but with a third difference taken against the home option. The empirical analysis uses quarterly panel data for all districts in our sample period. We do not impose a time window for our results. In columns (1)-(4), we present average effect of JSY on perinatal death at private facilities controlling for risk levels and BPL status. In column (1), we present average effect of JSY on whether a mother received a c-section. In column (2), we present average effect of JSY on whether a mother received a c-section in HPS. In columns (3)-(4), we present average effect of JSY on whether a mother received a c-section in HPS by SES status. Standard errors are displayed in parentheses and are clustered at district level. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A12: Robustness: Effect of JSY on Institutional Delivery and Perinatal Mortality

| | 10% | 20% | 30% | JSY Intensity |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Probability of Institutional Birth* | | | | |
| JSY | 0.040*** | 0.037*** | 0.027*** | |
| | [0.008] | [0.008] | [0.007] | |
| JSY Intensity | | | | 0.015*** |
| | | | | [0.005] |
| Dependent Var. Mean (2004-05) | .36 | .36 | .36 | .36 |
| Treatment Effect (%) | 11.21% | 10.45% | 7.5% | 4.16% |
| Number of Districts | 585 | 585 | 588 | 592 |
| District Fixed Effect | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y |
| Observations | 274806 | 274806 | 275040 | 273430 |
| *Panel B: Probability of Perinatal Death* | | | | |
| JSY | -0.001 | 0.001 | 0.000 | |
| | [0.001] | [0.001] | [0.001] | |
| JSY Intensity | | | | 0.000 |
| | | | | [0.001] |
| Dependent Var. Mean (2004-05) | .02 | .02 | .02 | .02 |
| Treatment Effect (%) | -3.48% | 2.48% | .34% | .31% |
| Number of Districts | 585 | 585 | 588 | 592 |
| District Fixed Effect | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y |
| Observations | 282378 | 282378 | 282619 | 280956 |

*Note:* This table presents our estimates of the impact of JSY on the likelihood of delivering at an institutional facility (panel A) and perinatal mortality (panel B) using three discrete definitions of treatment status in Equation 3.1 in columns (1)-(3) and continuous treatment in specification described in Equation A1 in columns (4). Standard errors are displayed in parentheses and are clustered at district level. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A13: Robustness: JSY and Mis-match of risk across facilities

| | Y = {Whether Delivery at Private Facility} | | | | |
|---|---|---|---|---|---|
| | Full Sample | Low Risk | High Risk | High Risk/Non BPL | High Risk/BPL |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Treatment at 15% cutoff* | | | | | |
| JSY | -0.007 | -0.001 | -0.014 | -0.021** | -0.002 |
| | [0.006] | [0.006] | [0.009] | [0.010] | [0.010] |
| Dependent Var. Mean (2004-05) | .17 | .14 | .18 | .25 | .07 |
| Treatment Effect (%) | -4.12% | -.69% | -7.63% | -8.49% | -3.7% |
| Number of Districts | 585 | 573 | 585 | 585 | 573 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274806 | 111988 | 162221 | 112898 | 49205 |
| *Panel B: Treatment at 20% cutoff* | | | | | |
| JSY | -0.010* | -0.005 | -0.015* | -0.023** | -0.010 |
| | [0.006] | [0.006] | [0.008] | [0.009] | [0.009] |
| Dependent Var. Mean (2004-05) | .17 | .14 | .18 | .25 | .07 |
| Treatment Effect (%) | -5.58% | -3.41% | -8.24% | -9.01% | -15.55% |
| Number of Districts | 585 | 573 | 585 | 585 | 573 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 274806 | 111988 | 162221 | 112898 | 49205 |
| *Panel C: Treatment at 30% cutoff* | | | | | |
| JSY | -0.015*** | -0.004 | -0.025*** | -0.034*** | -0.021** |
| | [0.005] | [0.005] | [0.006] | [0.007] | [0.008] |
| Dependent Var. Mean (2004-05) | .17 | .14 | .18 | .25 | .07 |
| Treatment Effect (%) | -8.41% | -2.44% | -14.05% | -13.34% | -31.24% |
| Number of Districts | 588 | 581 | 588 | 588 | 578 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 275040 | 112295 | 162319 | 112975 | 49258 |
| *Panel D: Continuous Treatment* | | | | | |
| JSY Intensity | -0.022*** | -0.015*** | -0.030*** | -0.027*** | -0.037*** |
| | [0.003] | [0.004] | [0.005] | [0.006] | [0.007] |
| Dependent Var. Mean (2004-05) | .17 | .14 | .18 | .25 | .07 |
| Treatment Effect (%) | -12.72% | -10.23% | -16.44% | -10.9% | -54.56% |
| Number of Districts | 592 | 592 | 592 | 592 | 585 |
| District Fixed Effect | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y |
| Observations | 273430 | 111786 | 161642 | 112334 | 49304 |

*Note:* This table presents our estimates of the impact of JSY on patient sorting across healthcare facilities in India using three discrete definitions of treatment status in Equation 3.1 in Panels A through C and continuous treatment in specification described in Equation A1 in Panel D. In column (1), we present average effect of JSY on likelihood of delivering at private facilities. Columns (2)-(3) present average effect of JSY on likelihood of delivering at private facilities for low and high-risk patients. Columns (4)-(5) present likelihood of delivering at private facilities for high-risk mothers across non-BPL and BPL mothers. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A14: Robustness: Effect of JSY on Congestion (capacity measure: OBGYNs)

| | Y = {Pub. Facility} | | Y = {Pvt. Facility} | | Y = {Pub. Facility} | |
|---|---|---|---|---|---|---|
| | Elig | Inelig | Elig | Inelig | Inelig/High Cap | Inelig/Low Cap |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Treatment at 15% cutoff* | | | | | | |
| JSY | 0.066*** | -0.009 | -0.010 | 0.008 | 0.035 | -0.016 |
| | [0.009] | [0.012] | [0.007] | [0.013] | [0.025] | [0.018] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 37.79% | -3.54% | -6.33% | 3.04% | 16.02% | -7.13% |
| Number of Districts | 584 | 287 | 584 | 287 | 64 | 71 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208811 | 65958 | 208811 | 65958 | 14844 | 17554 |
| *Panel B: Treatment at 20% cutoff* | | | | | | |
| JSY | 0.065*** | -0.012 | -0.008 | 0.007 | 0.004 | -0.022 |
| | [0.008] | [0.010] | [0.006] | [0.010] | [0.020] | [0.017] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 37.39% | -4.77% | -5.36% | 2.68% | 2.07% | -9.61% |
| Number of Districts | 584 | 287 | 584 | 287 | 64 | 71 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208811 | 65958 | 208811 | 65958 | 14844 | 17554 |
| *Panel C: Treatment at 30% cutoff* | | | | | | |
| JSY | 0.054*** | -0.020** | -0.010* | 0.007 | 0.006 | -0.045*** |
| | [0.008] | [0.009] | [0.005] | [0.008] | [0.019] | [0.017] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 31.05% | -8.07% | -6.17% | 2.47% | 2.91% | -19.86% |
| Number of Districts | 587 | 290 | 587 | 290 | 65 | 71 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208928 | 66075 | 208928 | 66075 | 14882 | 17557 |
| *Panel D: Continuous Treatment* | | | | | | |
| JSY Intensity | 0.044*** | -0.003 | -0.024*** | 0.003 | 0.006 | 0.010 |
| | [0.006] | [0.006] | [0.004] | [0.006] | [0.011] | [0.012] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .27 | .22 | .23 |
| Treatment Effect (%) | 24.91% | -1.18% | -15.36% | 1.24% | 2.84% | 4.3% |
| Number of Districts | 592 | 293 | 592 | 293 | 67 | 71 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 209080 | 64349 | 209080 | 64349 | 14611 | 17114 |

*Note:* This table presents our estimates of the impact of JSY on congestion at public healthcare facilities in India using number of obgyns per 10,000 persons as our capacity measure, and three discrete definitions of treatment status in Equation 3.1 in Panels A through C and continuous treatment in specification described in Equation A1 in Panel D. In columns (1)-(2), we present average effect of JSY on likelihood of delivering at public facilities for "eligible" and "ineligible" mothers. Columns (3)-(4) present average effect of JSY on likelihood of delivering at private facilities for "eligible" and "ineligible" mothers Standard errors are displayed in parentheses and are clustered at district level. Columns (5)-(6) likelihood of delivering at public facilities for “ineligible” mothers across high and low capacity districts. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A15: Robustness: Effect of JSY on Congestion (capacity measure: Capacity Index)

| | Y = {Pub. Facility} | | Y = {Pvt. Facility} | | Y = {Pub. Facility} | |
|---|---|---|---|---|---|---|
| | Elig | Inelig | Elig | Inelig | Inelig/High Cap | Inelig/Low Cap |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Treatment at 15% cutoff* | | | | | | |
| JSY | 0.066$^{***}$ | -0.009 | -0.010 | 0.008 | -0.001 | -0.019 |
| | [0.009] | [0.012] | [0.007] | [0.013] | [0.017] | [0.012] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 37.79% | -3.54% | -6.33% | 3.04% | -.42% | -8.26% |
| Number of Districts | 584 | 287 | 584 | 287 | 93 | 42 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208811 | 65958 | 208811 | 65958 | 20292 | 10264 |
| *Panel B: Treatment at 20% cutoff* | | | | | | |
| JSY | 0.065$^{***}$ | -0.012 | -0.008 | 0.007 | -0.004 | -0.024 |
| | [0.008] | [0.010] | [0.006] | [0.010] | [0.015] | [0.026] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 37.39% | -4.77% | -5.36% | 2.68% | -1.85% | -10.34% |
| Number of Districts | 584 | 287 | 584 | 287 | 93 | 42 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208811 | 65958 | 208811 | 65958 | 20292 | 12110 |
| *Panel C: Treatment at 30% cutoff* | | | | | | |
| JSY | 0.054$^{***}$ | -0.020$^{**}$ | -0.010$^{*}$ | 0.007 | -0.006 | -0.055$^{***}$ |
| | [0.008] | [0.009] | [0.005] | [0.008] | [0.016] | [0.018] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .28 | .22 | .23 |
| Treatment Effect (%) | 31.05% | -8.07% | -6.17% | 2.47% | -2.97% | -23.69% |
| Number of Districts | 587 | 290 | 587 | 290 | 94 | 42 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 208928 | 66075 | 208928 | 66075 | 20330 | 12110 |
| *Panel D: Continuous Treatment* | | | | | | |
| JSY Intensity | 0.044$^{***}$ | -0.003 | -0.024$^{***}$ | 0.003 | 0.012 | 0.013 |
| | [0.006] | [0.006] | [0.004] | [0.006] | [0.011] | [0.012] |
| Dependent Var. Mean (2004-05) | .17 | .25 | .16 | .27 | .22 | .23 |
| Treatment Effect (%) | 24.91% | -1.18% | -15.36% | 1.24% | 5.34% | 5.74% |
| Number of Districts | 592 | 293 | 592 | 293 | 96 | 42 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y |
| Observations | 209080 | 64349 | 209080 | 64349 | 19979 | 11746 |

*Note:* This table presents our estimates of the impact of JSY on congestion at public healthcare facilities in India using capacity index as our capacity measure, and three discrete definitions of treatment status in Equation 3.1 in Panels A through C and continuous treatment in specification described in Equation A1 in

Panel D. In columns (1)-(2), we present average effect of JSY on likelihood of delivering at public facilities for "eligible" and "ineligible" mothers. Columns (3)-(4) present average effect of JSY on likelihood of delivering at private facilities for "eligible" and "ineligible" mothers Standard errors are displayed in parentheses and are clustered at district level. Columns (5)-(6) likelihood of delivering at public facilities for "ineligible" mothers across high and low capacity districts. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A16: Robustness: Triple Diff: Private Sector response to JSY

| | Y = OOP Cost in HPS (Const INR.) | | | Healthcare Quality | | | |
|---|---|---|---|---|---|---|---|
| | | | | Perinatal Death | Rec. ANC | Number ANC | At least 6 tests |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *Panel A: Treatment at 15% cutoff* | | | | | | | |
| JSY × Pvt | 228.690 | 222.570 | 213.359 | 0.000 | -0.018** | 0.083* | -0.024*** |
| | [234.499] | [233.155] | [232.856] | [0.002] | [0.007] | [0.044] | [0.008] |
| Dependent Var. Mean (2004-05) | 10669.39 | 10669.39 | 10669.39 | .01 | .92 | 5.64 | .7 |
| Treatment Effect (%) | 2.14% | 2.09% | 2% | 2.92% | -1.94% | 1.48% | -3.47% |
| Number of Districts | 211 | 211 | 211 | 496 | 496 | 494 | 496 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | Y | Y | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | Y | Y | Y | Y | Y |
| Observations | 33816 | 33810 | 33810 | 128266 | 128248 | 85590 | 128266 |
| *Panel B: Treatment at 20% cutoff* | | | | | | | |
| JSY × Pvt | 421.226* | 409.787* | 397.592* | 0.000 | -0.010 | 0.075* | -0.025*** |
| | [224.703] | [224.057] | [223.572] | [0.002] | [0.007] | [0.042] | [0.007] |
| Dependent Var. Mean (2004-05) | 10669.39 | 10669.39 | 10669.39 | .01 | .92 | 5.64 | .7 |
| Treatment Effect (%) | 3.95% | 3.84% | 3.73% | 7.27% | -1.09% | 1.34% | -3.54% |
| Number of Districts | 212 | 212 | 212 | 496 | 496 | 494 | 496 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | Y | Y | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | Y | Y | Y | Y | Y |
| Observations | 33821 | 33815 | 33815 | 128266 | 128248 | 85590 | 128266 |
| *Panel C: Treatment at 30% cutoff* | | | | | | | |
| JSY × Pvt | 583.370*** | 584.952*** | 574.918*** | -0.001 | -0.002 | 0.093** | -0.009 |
| | [223.073] | [222.132] | [221.907] | [0.001] | [0.008] | [0.039] | [0.007] |
| Dependent Var. Mean (2004-05) | 10669.39 | 10669.39 | 10669.39 | .01 | .92 | 5.64 | .7 |
| Treatment Effect (%) | 5.47% | 5.48% | 5.39% | -16.62% | -.26% | 1.65% | -1.26% |
| Number of Districts | 218 | 218 | 218 | 497 | 497 | 496 | 497 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | Y | Y | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | Y | Y | Y | Y | Y |
| Observations | 34601 | 34595 | 34595 | 128279 | 128261 | 85608 | 128279 |
| *Panel D: Continuous Treatment* | | | | | | | |
| JSY × Pvt | 523.621** | 521.927** | 481.547** | -0.001 | 0.001 | 0.045 | 0.013 |
| | [222.233] | [222.016] | [222.588] | [0.002] | [0.008] | [0.043] | [0.008] |
| Dependent Var. Mean (2004-05) | 10757.54 | 10757.54 | 10757.54 | .01 | .92 | 5.63 | .7 |
| Treatment Effect (%) | 4.87% | 4.85% | 4.48% | -17.9% | .07% | .8% | 1.89% |
| Number of Districts | 291 | 291 | 291 | 592 | 592 | 591 | 592 |
| District Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Quarter Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Birth Order Fixed Effect | Y | Y | Y | Y | Y | Y | Y |
| Risk Deciles Fixed Effect | N | Y | Y | Y | Y | Y | Y |
| BPL Fixed Effect | N | N | Y | Y | Y | Y | Y |
| Observations | 71173 | 71159 | 71159 | 223367 | 223336 | 161920 | 223367 |

*Note:* This table presents our triple difference estimates of the impact of JSY on out-of-pocket costs (in Const. INR) at HPS and healthcare quality at private facilities using three discrete definitions of treatment status in Equation 3.1 in Panels A through C and continuous treatment using specification described in Equation A2 in Panel D. The third difference is taken against the home option. In columns (1)-(3), we present average effect of JSY on out-of-pocket costs increasingly and flexibly controlling for risk and SES status. Columns (4)-(7) present triple difference results on healthcare quality at private facilities. Standard errors are displayed in parentheses and are clustered at district level. *** $p < .01$, ** $p < .05$, * $p < .1$

## B Robustness of reduced-form results

This appendix presents evidence on robustness of our main results to alternate definitions of important variables in our analysis. As discussed in subsection 1.3.2, we used somewhat arbitrary definitions of a district's treatment status under JSY and a measure of district's pre-existing public capacity.

We present robustness results using two kinds of alternate definitions for a district's treatment status under JSY. First, we define three alternate discrete treatment variables for JSY using cutoff values of 15%, 20% and 30%.[1] And second, we define a continuous variable *JSY intensity* as our measure of treatment for a district and is defined as the proportion of all eligible women delivering in public facilities in a district-year who reported receiving government cash assistance. Zero intensity implies that there were no JSY recipients in that district-year, while an intensity of one means that all eligible women who gave birth in a government facility in that district-year were beneficiaries of the policy.

We run the regression specification as in Equation 3.1 for the three discrete treatment variables and we run the following two-way fixed effects regression specification using the continuous measure, *JSY Intensity*:

$$Y_{ibdt} = \alpha_d + \beta_b + \gamma_t + \tau.JSYIntensity_{dt} + \epsilon_{ibdt} \tag{A1}$$

Here, $Y_{ibdt}$ represents the outcome variable of interest that varies at the level of an individual $i$, birth order $b$, district $d$ and quarter of birth $t$. $\alpha_d$ and $\gamma_t$ represent district and quarter of birth fixed effects respectively. Since our data only has detailed information for a mother's last birth, we also include a birth order fixed effect, represented by $\beta_b$, to account for un-observables specific to the birth order. $JSYIntensity_{dt}$ is a continuous measure that captures roll-out of JSY in Indian districts over quarters after its announcement. $\tau$ captures our targeted treatment effect of JSY that does not vary by individual and quarter. Finally, $\epsilon_{ibdt}$ captures idiosyncratic error that satisfies: $E[\epsilon_{ibdt}|\alpha_d, \beta_b, \gamma_t, JSYIntensity_{dt}] = 0$. We cluster standard errors at the district level, our unit of

[1] For instance, at the cut-off value of 15%, a district is said to be treated if two conditions are met: at least 15% of eligible women must report receiving financial assistance in the given quarter and the same fraction of women must report receiving financial assistance over the following year.

treatment.

It should be noted that this specification suffers from consequences of ignoring treatment effect heterogeneity as highlighted by (Borusyak et al., 2022; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). Nevertheless, this demonstrates that our results hold under the previously conventional difference-in-difference methods.

We also present robustness of our results to an alternate definition of pre-JSY district level public hospital capacity. We use a measure of public facility capacity index created using first principal components of the three capacity variables observed in our data (OBGYNs, nurses and beds), each normalized by 10,000 persons. Table A5 presents the first principal components from this analysis.

### B.1 Effect of JSY on Institutional Births and Mortality

Table A12 presents our results on the effect of JSY on likelihood of institutional delivery and perinatal mortality. Consistent with our main results, we find that JSY significantly increased the likelihood of institutional births across our four definitions of treatment (see Panel A in Table A12). Panel A in Table A12 shows that JSY did not have a significant effect on perinatal mortality.

### B.2 Effect of JSY on mismatch of patient risk across Facilities

Table A13 presents robustness results for our finding that JSY led to a mismatch in patient risk across health facilities in India across our four definitions of treatment in panels A through D. Specifically, we show that as a result of JSY, high-risk patients were less likely to deliver at the highest quality (private sector) facilities in India. Columns (2)-(3) in Table A13 across panels A through D show that JSY induced high-risk mothers to switch out of private facilities.

### B.3 Effect of JSY on Congestion at Public Facilities

First, we present robustness of our replication of the result in Andrew and Vera-Hernández (2022) that high-risk mothers experienced an increase in likelihood of perinatal death in low public capacity districts. We show, in Figure A17, that our results hold across the four discrete definitions of treatment under JSY including our original definition in subsection 1.3.2.

Table A14 presents robustness results for our finding that high SES "ineligible" mothers adapted to deteriorating healthcare capacity by moving away from public facilities in low capacity districts to private facilities. Panels A through C of Table A14 presents our results using the three alternate discrete measures of a district's treatment status using number of OBGYNs per 10,000 persons as a measure for public sector capacity. Panel D presents evidence of adaptation behavior by "ineligible" mothers using the continuous measure of *JSY Intensity*.

Table A15 replicates these results using a capacity index generated using principle components on three variables on public sector capacity in our data namely OBGYNs per 10,000 persons, nurses per 10,000 persons and beds per 10,000 persons. We find that our results are consistent across the two measures. We also find that our results remain stable across panels A through D.

### B.4 Private Facility response to JSY

In our robustness tests, we again present robustness results for our three alternate discrete definitions of treatment under JSY (using 15%, 20% and 30% as cut-offs) and our continuous variable *JSY intensity* as our measure of treatment for a district. We present triple difference results as in our main results with the third difference taken against the home option, the outside option. For our continuous treatment measure, we run the following triple difference regression specification, with the third difference taken against the home option:

$$Y_{ibdt} = \alpha_d + \beta_b + \gamma_t + \beta_1.JSYIntensity_{dt} + \beta_2.\mathbb{1}Pvt.Dvy._{dt} + \beta_3.\mathbb{1}Pub.Dvy._{dt} \tag{A2}$$

$$+\beta_4.JSYIntensity_{dt} \times \mathbb{1}Pvt.Dvy._{dt} + \beta_5.JSYIntensity_{dt} \times \mathbb{1}Pub.Dvy._{dt} + \epsilon_{ibdt} \tag{A3}$$

Here, $Y_{ibdt}$ represents the outcome variable of interest that varies at the level of an individual $i$, birth order $b$, district $d$ and quarter of birth $t$. $\alpha_d$ and $\gamma_t$ represent district and quarter of birth fixed effects respectively. We also include a birth order fixed effect, represented by $\beta_b$, to account for un-observables specific to the birth order. $JSYIntensity_{dt}$ is a continuous measure that captures roll-out of JSY in Indian districts over quarters after its announcement. $\beta_4$ captures our targeted triple difference treatment effect of JSY for outcomes at private facilities and does not vary by individual and quarter. We cluster standard errors at the district level, our unit of treatment.

Panels A through D in Table A16 present our triple difference estimates. Columns (1)-(3)

present the treatment effect of JSY on out-of-pocket costs (in Const. INR) at private facilities in high-performing states increasingly and flexibly controlling for risk deciles and BPL status. We find that JSY significantly increased out-of-pocket costs (prices) at private facilities. Columns (4)-(7) present the effect of JSY on a number of measures of healthcare quality at private facilities. We find that JSY did not affect the likelihood of perinatal mortality at private facilities.

## C Price increasing effects of public competition

In this appendix, we provide a theoretical basis for our finding that prices at private healthcare facilities in India increased as a response to increased competition from public facilities due to a substantial subsidy for eligible mothers. Chen and Riordan (2008) provides conditions under which increased market competition from an entrant can lead to an increase in incumbent's prices. While there is no entry in our context, the same forces are likely present in our case.

### C.1 Theory

We adopt the exposition from Atal et al. (2022). Consider a population of consumers of size one choosing which healthcare facility to access: private facilities (H), public facilities (G) and home (outside option, O). Consumer's utility is for each choice is given by:

$$u_{ic} = \begin{cases} v_{iH} - p_H & \text{c=H} \\ v_{iG} - p_G & \text{c=G} \\ 0 & \text{c=O} \end{cases}$$

where $v_{ic}$ is the value of option $c$ for consumer $i$ and $p_c$ is the price they pay for their choice. The option value follows a joint differentiable distribution $H(v)$. Consumers make a discrete choice over their three options and choose the one that provides them highest utility. The probability that consumer $i$ chooses $c$ is:

$$s_{ic} = Pr(u_{ic} \geq u_{ik} \; for \; each \; k)$$

Integrating this probability over the distribution of valuations gives us market shares for each option $c$: $s_c$.

Given these preferences, private suppliers choose prices $p_H$ to maximize $\pi_H = s_H(p_H - c_H)$. Public facilities on the other hand charge a low administratively set price $p_G$. Under JSY, the prices at public facilities are lowered exogenously to $p_G^{'}$. We want to understand the conditions under which this fall in competitor's (public facilities) price induces a price increase by private facilities.

Chen and Riordan (2008) show that private facilities' price response depends on two counteracting forces. While a loss of market share puts a downward pressure on private facilities' price, more inelastic residual demand induces upward pressure on prices. More formally, let $F(v_H)$ be the marginal distribution of valuation of the private option and let $G(v_G|v_H)$ be the conditional distribution of valuation for the public option conditional on valuation of the private option. Given these definitions, Chen and Riordan (2008) show that the incumbent's price increases if and only if the following condition holds:

$$\int_{p_H}^{\infty} [G(v|v) - G(p_H|v)] f(v) dv \leq (p_H - c_H) \int_{p_H}^{\infty} [g(p_H|v) - g(v|v)] f(v) dv$$

On the left, this condition captures the *market share effect* where the greater market share that private facilities lose, greater is their incentve to lower prices. The right side of this inequality captures the *price sensitivity effect* - the steeper the residual demand curve for private facilities after JSY (more inelastic residual demand), larger is the incentive for them to raise prices.

### C.2 Discussion

Our results on private sector's price response in subsubsection 1.5.2 are consistent with *price sensitivity effect* dominating the *market share effect* in high-performing states.

In subsubsection 1.5.2, we established that private facilities increased their price as a response to a reduction in prices at public facilities induced by JSY without an accompanied improvement in quality at private facilities. Moreover, we found that the increase in price was largely driven by private hospitals in high-performing states where high SES mothers were not offered incentives under JSY. We posit that complete coverage of JSY in low-performing states resulted in a dominant *market share effect* that put downward pressure on prices whereas incentivizing only low SES mothers in high-performing states led to a dominant *price sensitivity effect*.

# Appendix B: Chapter 2: Electric Stoves as a Solution for Household Air Pollution

## A Materials and Methods

The data used in the analysis were obtained using primary surveys and three types of monitoring devices. The devices - air quality sensors, voltage monitors, and ammeters were in place from 1 September 2018 to 19 September 2019.

### A.1 Sample selection and survey

For notational convenience, we define a village as a cluster of households in which a particular representative from Dharma Life (know as a Dharma Life Entrepreneur (DLE)) lived and had sold induction stoves. We have eight such 'villages' in the sample. One of these has households from a single village as defined in the Census of India, six have households from two Census villages, and one has households from four Census villages.

The first survey was conducted in August 2018, a second round in February 2019, and a final round at the conclusion of the study in September 2019. Respondents were asked about their ownership of different kinds of stoves and their preferred stove in each season. The households were also asked to recall the items cooked on each stove and the time at which they cooked their primary meals. A number of questions were asked about their electricity supply. Households were paid a monthly amount of 200 rupees (2.69 USD) for their permission to install monitors in their homes, to not switch them off, and for allowing our field staff to collect data from the devices periodically. Respondents were also paid 100 rupees (1.35 USD) for participation in each survey.

### A.2 Voltage monitors

The data on electricity supply were collected using voltage monitors (Figure A1 A) provided to us by the Prayas Energy Group (https://www.prayaspune.org/peg/). These monitors record voltage every minute and transmit it via the mobile phone network to Prayas's server. Monitors were placed on a total of 10 electricity lines, as 2 out of the 8 villages had more than one electricity line.

Poor connectivity to the mobile network in the villages led to some missing data (see Table A1), a problem that was reduced by installing a primary and backup monitor in 2 households in each village. We use the minute-level records to code the presence of electricity, with an indicator equal to 1 if the reading in a given minute is greater than 100 volts, and 0 otherwise.

Table A1: No. of non-missing observations (in millions) from minute-level electricity data used in Figure A15

| Voltage Monitor | Sep-Nov 2018 | Nov-Mar 2019 | Apr-June 2019 | Jul-Sep 2019 |
|---|---|---|---|---|
| Non-Missing Observations | 1.04<br>(95.5 %) | 1.93<br>(98.4%) | 1.31<br>(99.6%) | 1.12<br>(96.1%) |

Notes: The parentheses show these numbers as percentages of the total number of observations we would have if all voltage monitors functioned properly for every minute from 1 Sep 2018 to 19 Sep 2019.

### A.3 Air quality sensors

The air quality sensors (Figure A1 B) were developed by the Bergin group at Duke University (http://bergin.pratt.duke.edu/) and have been used previously in other relatively polluted environments (Barkjohn et al., 2019; Zheng et al., 2018). These sensors capture minute-level PM2.5 concentration, and were installed in the primary cooking space used by every study household about 1.5 to 2 meters above the ground. Each sensor was powered by a 6 V rechargeable lead acid battery, which was connected to a normal power source all through the day. Households were instructed not to disconnect the battery.

To capture ambient pollution levels, two sensors were installed in open spaces within the premises of some households in each village. To minimize data loss on ambient pollution, we inspected the time series from each ambient sensor and used the one that had less missing data for our regression analysis. Gaps in the data from the chosen ambient sensor were filled in by data from the other ambient sensor, if it was found to be recording data over the same period.

The sensors were intended to be on at all times, but gaps nonetheless occurred during periods with frequent or long duration outages, when the lead acid battery became drained. When the batteries were drained to the extent that they could no longer power the air quality sensor, the


A B C

Figure A1: Devices that were deployed on the field.

Notes: Figure A is the electricity supply monitor deployed in the field. These were obtained from the Prayas Energy Group. Figure B shows the air quality sensor developed in the Bergin lab at Duke University. Figure C represents the induction stove monitors that were developed by a local manufacturer in Delhi, India.

sensors would stop recording data (even after the batteries got recharged) until our field assistant restarted the sensor. The sensor batteries would not recharge if the voltage dropped much below the prescribed standard of 220V, and this accounts for most of the data losses. Table A2 records the number of non-missing observations in the sensor data for different types of households. Since about 37% of the kitchen sensor data for induction-stove-owning households is missing, it is important to check if this could bias our results.

One possibility is that data from air quality sensors is missing more often following long-duration outages, and households are also reluctant to stop using *chulhas* after such outages. This would tend to over-estimate the negative effect of electricity availability on air pollution in Equation 2.1. However, as seen in Figure A2, only a very small fraction of outages are greater than 36 hours, which is what it would take to drain the sensor batteries. Therefore, this source of bias is negligible.

Table A2: No. of non-missing observations (in millions) from minute-level PM2.5 data used in Figure 2.2.

| Sensor Type | Sep-Nov 2018 | Nov-Mar 2019 | Apr-June 2019 | Jul-Sep 2019 |
|---|---|---|---|---|
| Induction-stove-owning households | 2.86 (52.3%) | 7.93 (79.4%) | 4.49 (67.2%) | 2.58 (43.4%) |
| Households without induction stoves | 0.90 (51.5%) | 2.59 (82.7%) | 1.07 (54.5%) | 0.52 (30%) |
| Ambient sensors | 0.72 (81.8%) | 1.47 (94%) | 1.00 (95.2%) | 0.64 (69.1%) |

Notes: Non-missing observations as a percentage of the total that would have been observed if all air quality sensors were functioning for every minute from 1 Sep 2018 to 19 Sep 2019 are given in parentheses.

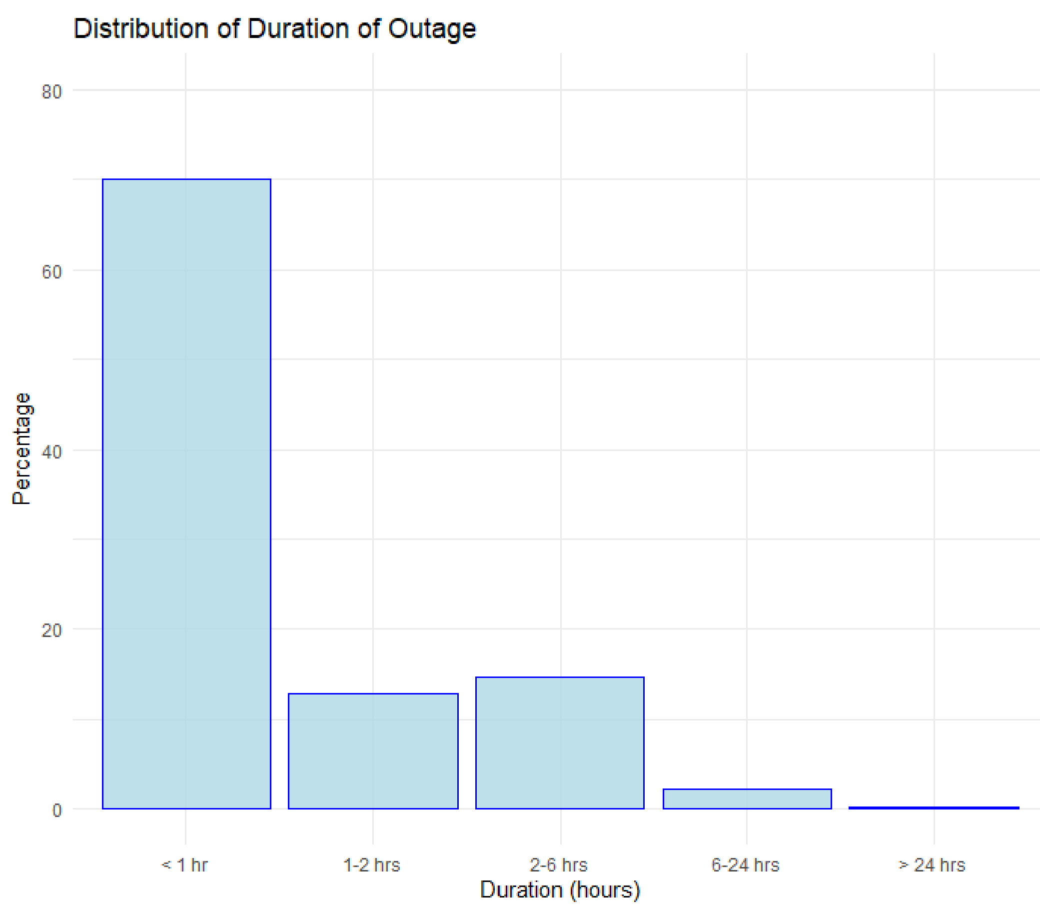



Figure A2: Distribution of duration of outages

Figure A3 shows distributions of voltages conditional on sensor data being missing and non-missing separately. It can be seen that a greater share of voltages lie in the lower range when PM2.5 is missing. This is in line with our expectations since low voltage electricity was one of the main reasons for sensor batteries not getting recharged, and for RTC resets. If low-voltage electricity leads to less use of induction stoves than near-normal voltage electricity, then the estimated effects of electricity in Equation 2.1 would apply to normal-voltage electricity but perhaps not to low-voltage electricity. We examine this by running a modified version of Equation 2.1 in which the share of the period electricity is available is replaced by two variables, the share of the period low-voltage electricity is available, and the share of the period that near-normal voltage electricity is available. Figure A25 shows that the effect sizes during cooking hours appear to be a little smaller for low-voltage electricity and about the same as in the original specifications for near-normal voltage electricity.

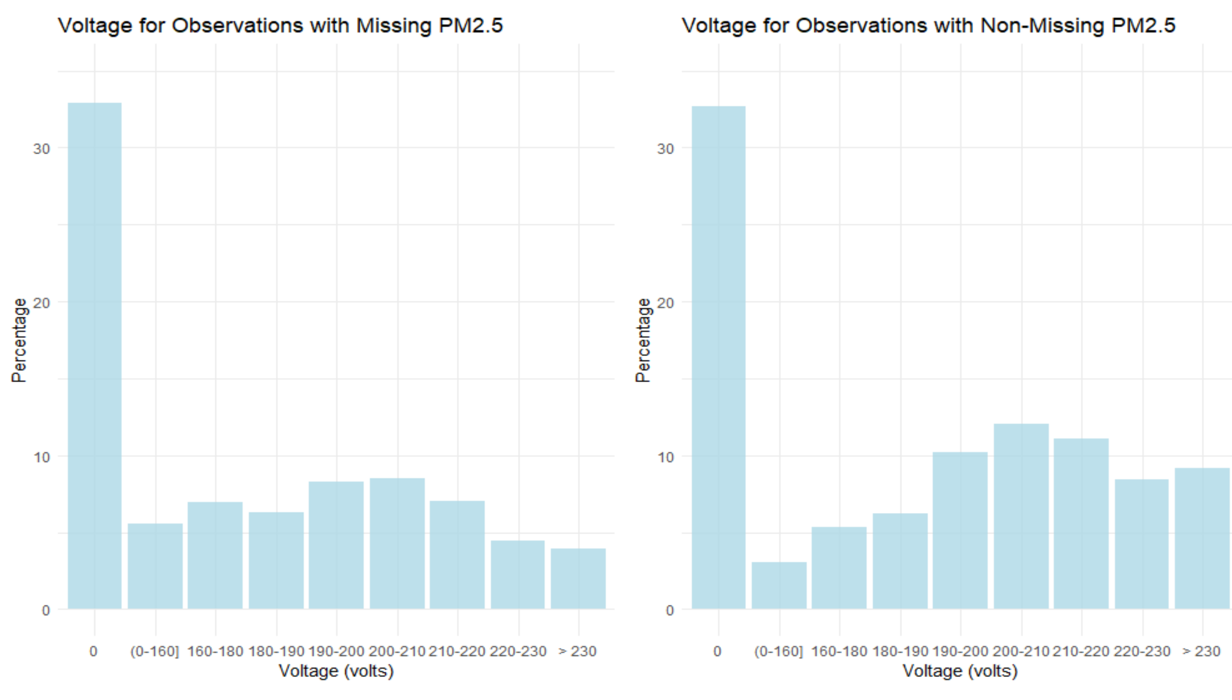



Figure A3: Voltage Distribution when PM2.5 is missing (left) and non-missing (right)

Two lesser causes of data losses were resets of the real-time clocks (RTCs) on the sensors, and particulate matter getting stuck in the intake to the light-scattering chamber. These events are likely to be unrelated to induction stove use in addition to being less frequent, and are, therefore, not likely to bias our regression results. The real time clock (RTC) in the air quality sensors sometimes spontaneously reset to 1/1/2000. This could happen once or multiple times between two successive data-collection visits. However, most of this data was recovered by mapping the incorrect times to the times at which the data was collected (recorded by our field assistant).[1]

If particulate matter gets stuck in the chamber where light-scattering by PM occurs, it can result in relatively stable but erroneous concentrations of PM2.5 readings. Depending on which component of the sensor is being obscured, these readings could be abnormally high or abnormally low. In order to overcome this problem and minimize data loss, compressed air was routinely used to clean the sensors. Outliers arising due to the aforementioned problem were identified by inspecting the plots of the sensor data and affected observations were dropped. These constituted about 15.7% of missing kitchen sensor data.

Data from the air quality sensors were adjusted to account for under-statement of PM2.5 at high levels ($> 200\mu g/m^3$) and over-statement at low levels of PM2.5. We contracted with the National Physical Laboratory (NPL), Delhi to calibrate the sensors in India. All the optical sensors were co-located with a Beta attenuation monitoring (BAM) sensor in ambient conditions (concentrations ranging between $50\mu g/m^3$-$200\mu g/m^3$) in the NPL lab in Delhi to simply check if there were any obvious defects in any sensors. A few malfunctioning sensors were replaced with new sensors. All sensors tracked BAM readings quite well and 5 were chosen randomly to act as reference sensors for our calibration process. Next, data were recorded for all sensors against two of our reference sensors at high PM2.5 concentrations ($> 500\mu g/m^3$) generated using incense sticks as well as low concentrations in an indoor laboratory ($30\mu g/m^3$-$50\mu g/m^3$). Sensors that did not show any defects were then deployed in the field.

In our final calibration step, we recorded PM2.5 readings from one of our reference sensors against an Aerodynamic Particle Sizer (model TSI 3321). Data from this process was used to fit a calibration equation which was then used to adjust data from all sensors in the field (See Figure A4). This adjustment is very close to one computed earlier in the Bergin lab at Duke University during a similar and independent calibration exercise which used a TSI Dustrak instrument as a reference sensor.

In February 2019, we examined whether there was any drift in our air quality sensor readings

[1]Occasionally, the RTC (real-time clock) had to be corrected by reprogramming the Arduino board in the sensor. In addition, the coin battery in the clock had to be replaced after a couple of months to avoid multiple resets.

that had been installed in the sample households' kitchens for six months. We chose 2 out of the 5 reference sensors and co-located them with one kitchen sensor in each village for about 24 hours. As can be seen in Figures A5 - A12, the kitchen sensors tracked our reference sensors well and we did not find any evidence of a drift in the readings. The reference sensor in village 2 got stuck after about 3.5 hours of the start of the co-location and showed unreasonably high concentrations, (the issue mentioned above) these data were dropped.

### Calibration Equation

$$APS\_PM2.5_t = \beta_1 Sensor\_PM2.5_t + \beta_2(Sensor\_PM2.5_t - 200) * D_t + \epsilon_t \tag{A1}$$

where $APS\_PM2.5_t$ is the PM2.5 value recorded by the Aerodynamic Particle Sizer (APS) at time $t$,
$Sensor\_PM2.5_t$ is the PM2.5 value recorded by our air quality sensor at time $t$,
$D_t$ is a dummy variable that takes value 0 when PM2.5 $\leq$ 200 and 1 when PM2.5 $>$ 200

The estimated coefficients are displayed in the following table. Intercepts have been forced to zero.

Table A3: Calibration Equation

| | Slope Coefficient |
|---|---|
| Sensor_PM2.5 | 0.8572$^{***}$ |
| | (0.0839) |
| (Sensor_PM2.5 - 200)D | 1.5950$^{***}$ |
| | (0.0599) |
| Obs | 107 |
| R-Sq | 0.982 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$
Standard errors in parentheses

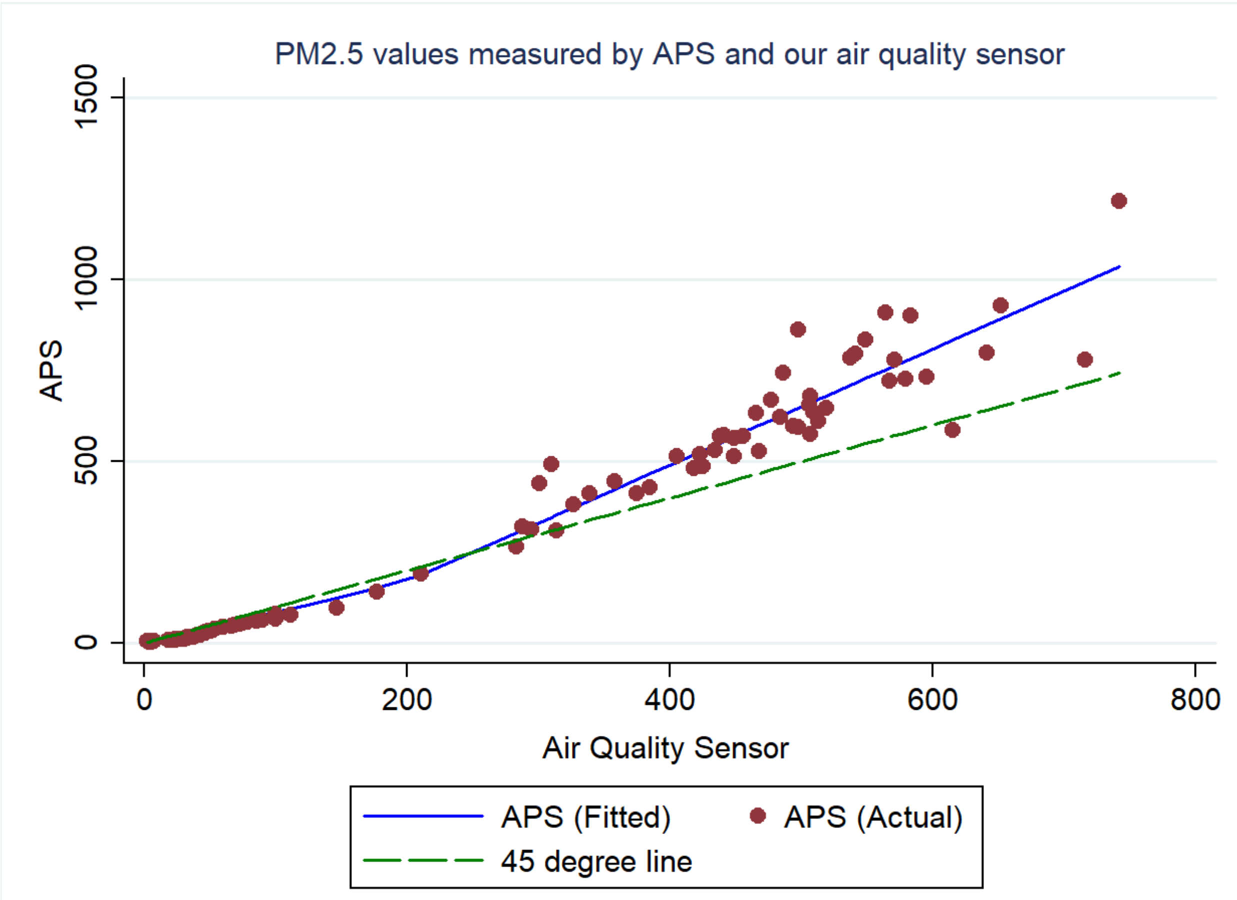



Figure A4: Piece wise regression used to estimate the calibration equation for the air quality sensors deployed in the field

Notes: Our sensor was tested against an Aerodynamic Particle Sizer (APS) and it was noted that our sensors underestimated pollution at higher concentrations and overestimated at lower concentrations of pollution. The relevant adjustments were made to the sensor readings.

## Co-location plots: One Sensor from each village was co-located with a reference sensor

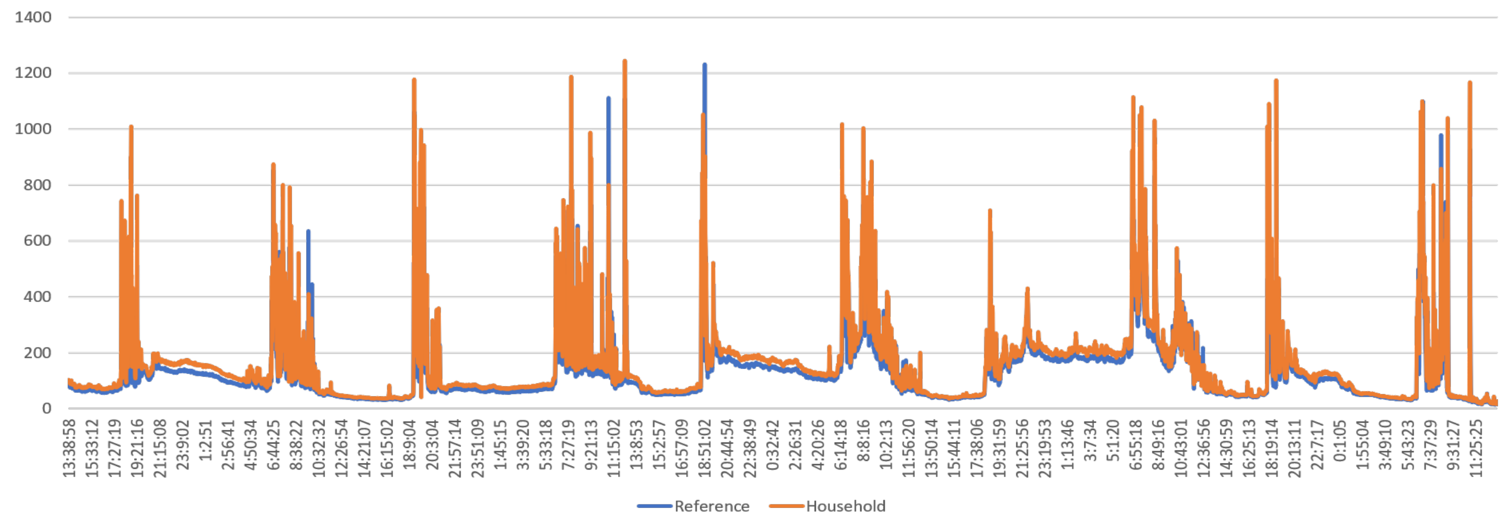



Figure A5: Village 1 : Reference Sensor - 88i, Household Sensor - 87i

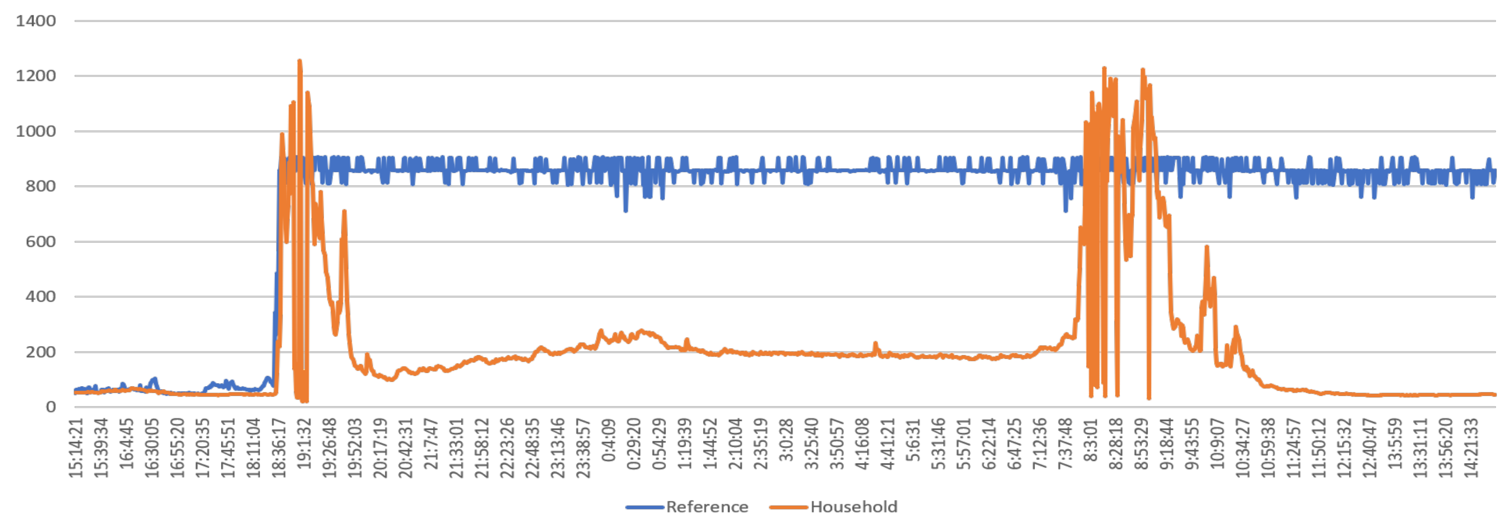



Figure A6: Village 2 : Reference Sensor - 37i, Household Sensor - 31i (Note: The stuck PM2.5 concentrations have been removed before conducting analysis)

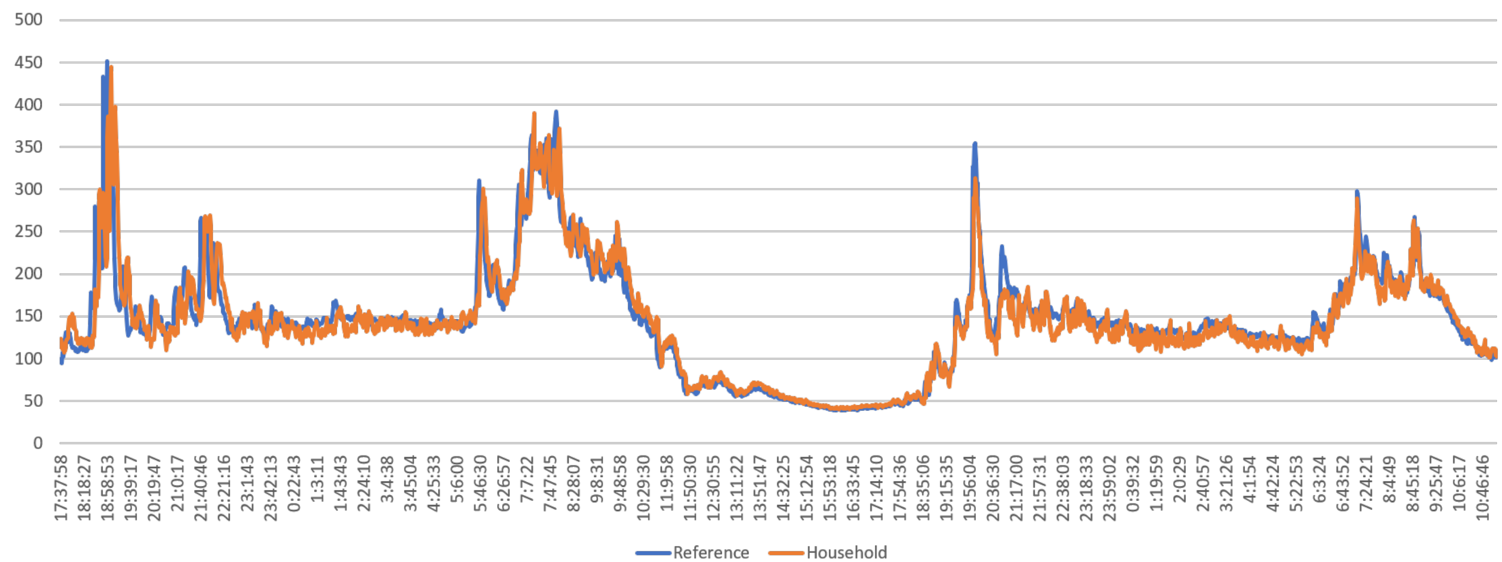



Figure A7: Village 3 : Reference Sensor - 88i, Household Sensor - 26i

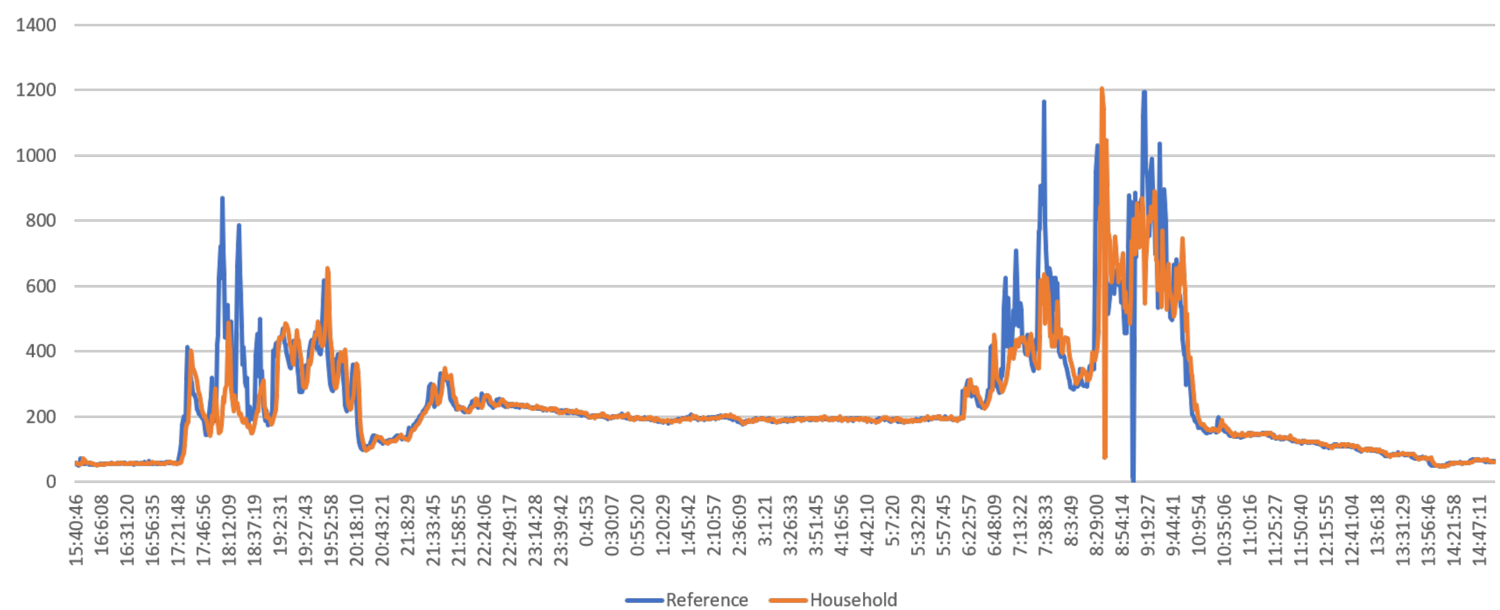



Figure A8: Village 4 : Reference Sensor - 37i, Household Sensor - 77i

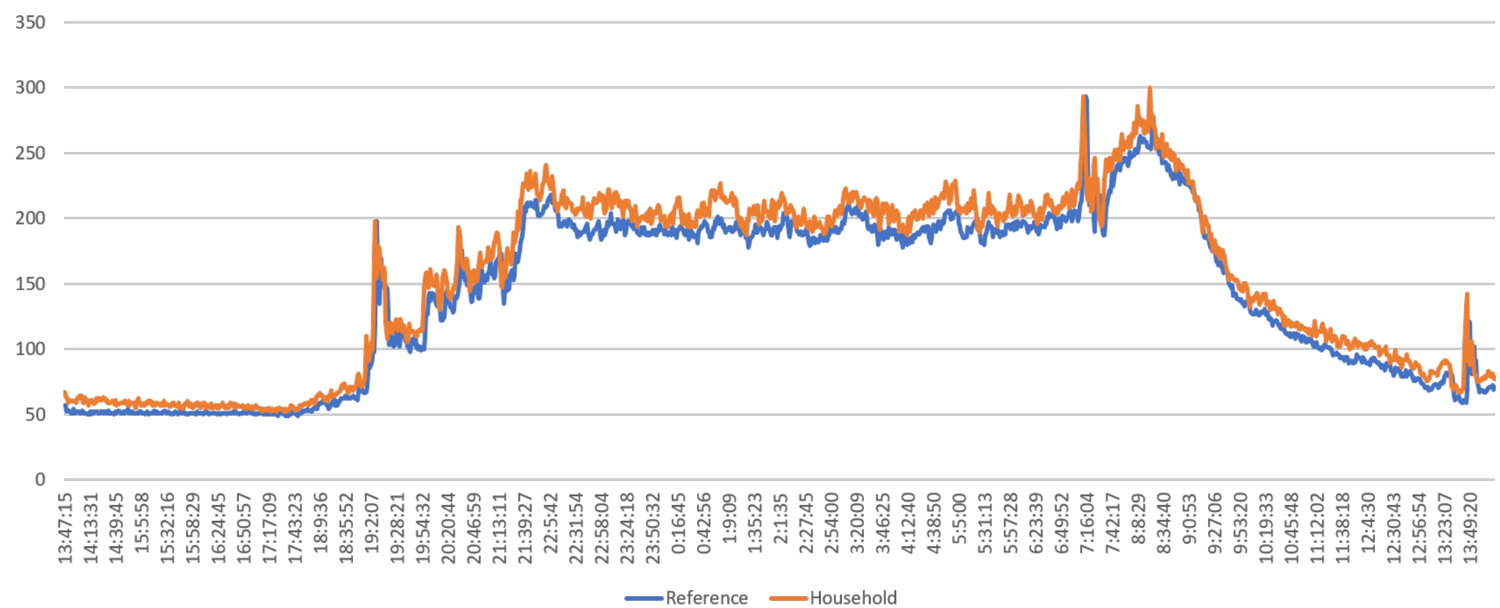



Figure A9: Village 5 : Reference Sensor - 88i, Household Sensor - 25i

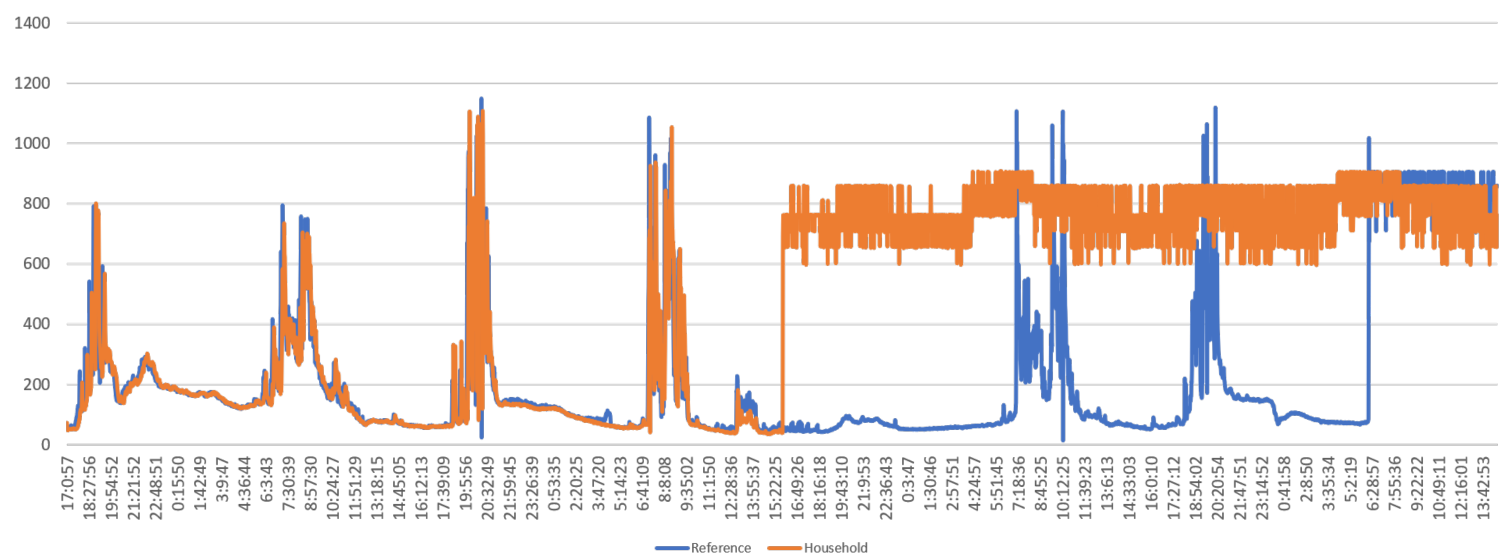



Figure A10: Village 6 : Reference Sensor - 37i, Household Sensor - 73i (Note: The stuck PM2.5 concentrations have been removed before conducting analysis)

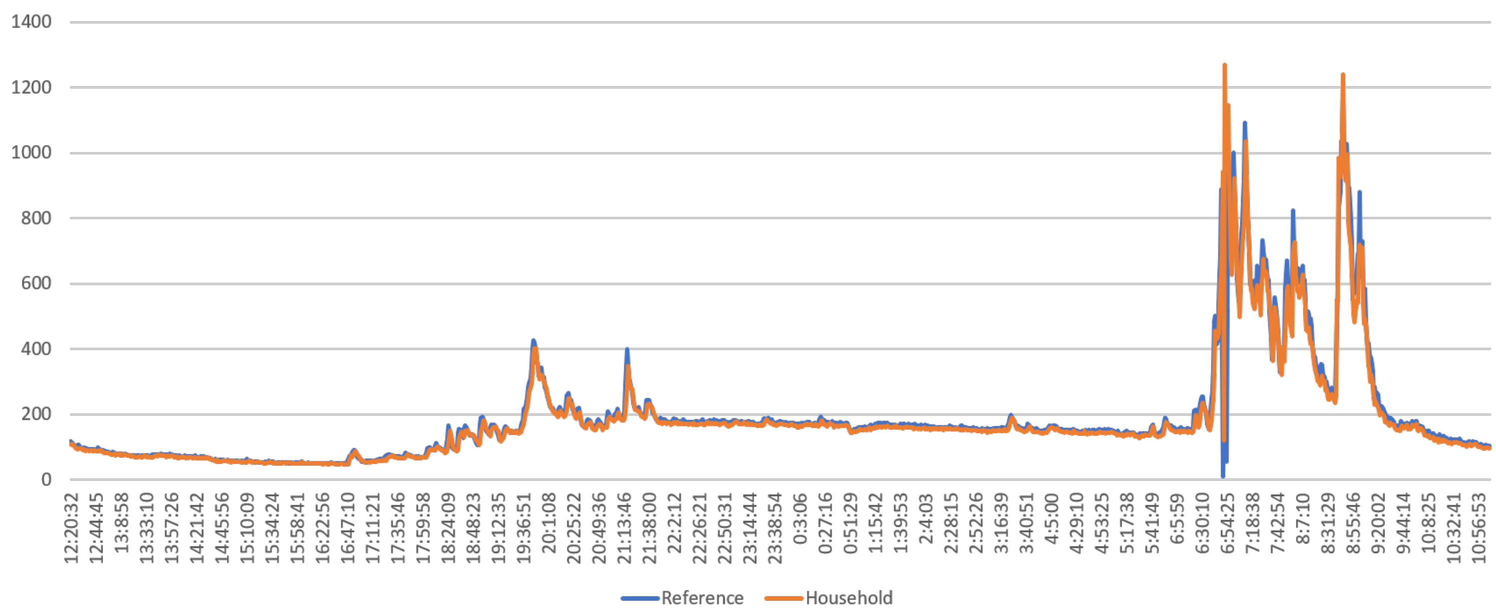



Figure A11: Village 7 : Reference Sensor - 88i, Household Sensor - 30i

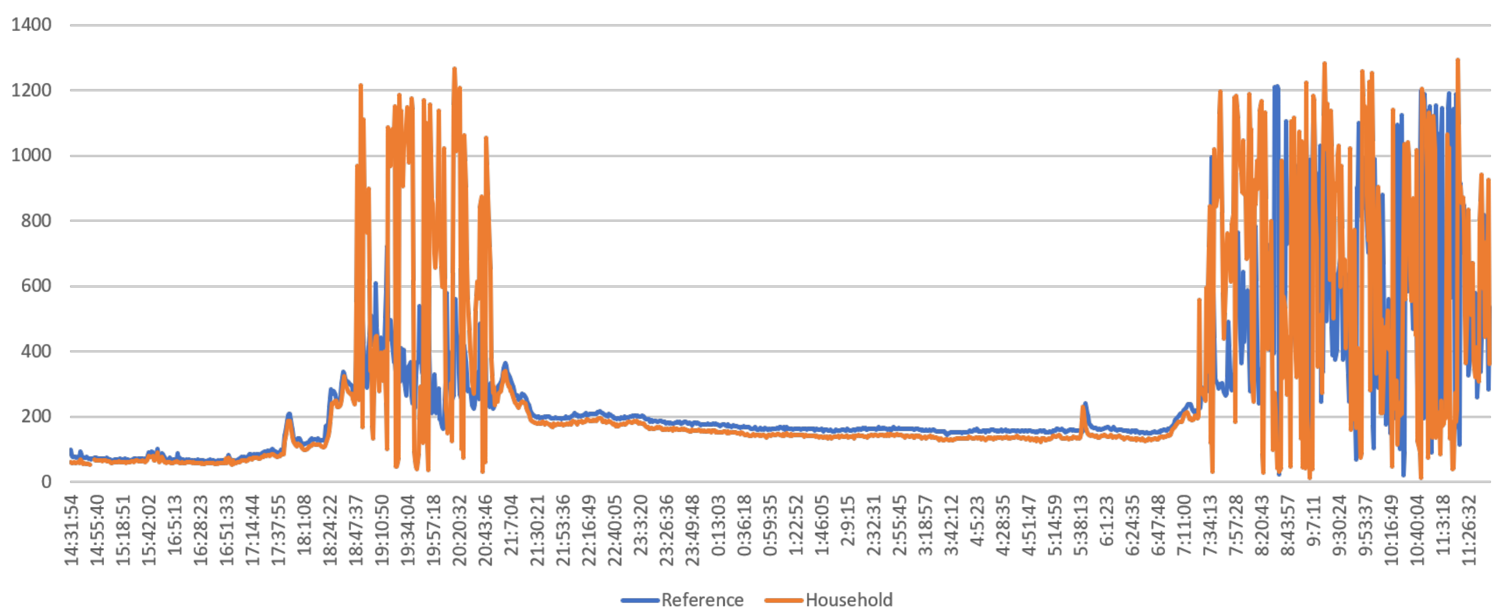



Figure A12: Village 8 : Reference Sensor - 37i, Household Sensor - 85i

## A.4 Ammeters

To measure induction stove use, we used ammeters with a data logger built to our specifications by a manufacturer in Mumbai, India (Figure A1 C). Each ammeter was connected to an induction stove on the line connecting the stove to the wall socket. It recorded a proxy for current flowing through the circuit whenever the induction stove was turned on, at minute-by-minute intervals. These data were stored on an SD card and collected by our field assistant on a weekly basis. No data were recorded when the stove was not being used.

The real-time clocks (RTCs) in the ammeters were subject to drift (a difference in device time and actual time), an issue which was first noted at the end of December 2018. This problem may have been caused by the low quality of electricity supply. The devices were removed for much of January for re-engineering to fix this problem, so ammeter data for these days were not obtained. Thereafter, in March 2019, the devices were modified again to allow our field assistant to update the time in the device if a drift was found during a data collection visit.[2] Where feasible, data were corrected to account for observed drifts in the RTCs. We corrected for drifts that arose prior to December 2018, assuming that the drifts occurred at a constant rate between the time the RTCs were reset for the first time on July 05, 2018, and the time of record of the discrepancy in January 2019. There were 5 ammeters in which the clocks had drifted by more than 3 hours. Data from these were dropped. Drifts observed after March 2019 were corrected using the same constant drift rate assumption and data for periods with drifts greater than or equal to 3 hours were dropped. These corrections were based on the drifts recorded by our field assistant during data collection visits. Drifts could be recorded only when the devices did not suffer from SD card issues and the RTC could be updated. Since problems with SD cards worsened over time, there were a number of devices with no drift records at the end of the study period. Such ammeters were assumed to have no drift in September 2019 if the last observed drift was less than an hour. If the last observed drift exceeded an hour, the subsequent data were dropped.

The SD cards in the ammeters sometimes had errors that prevented recording of data, evidently due to the card socket's exposure to cooking smoke. This problem got worse over time and is the major cause of missing data. To deal with this issue, we reformatted or replaced affected SD

[2] We are grateful to Vijay Rao for technical help with re-engineering and other ammeter issues.

cards during data collection visits. Table A4 shows the number of non-missing observations in the induction stove usage data.

Table A4: No. of non-missing observations (in millions) from minute-level induction stove usage data

| Ammeter | Sep-Nov 2018 | Nov-Mar 2019 | Apr-June 2019 | Jul-Sep 2019 |
|---|---|---|---|---|
| Non-Missing Observations | 4.56<br>(83.4%) | 6.22<br>(62.3%) | 4.39<br>(65.7%) | 2.68<br>(45%) |

Notes: The parentheses show these numbers as percentages of the total number of observations that would have been obtained if all ammeters functioned properly for every minute from 1 Sep 2018 to 19 Sep 2019.

Table A6: Ownership of Assets by Household Type

| | Induction-stove-owning households | Households without induction stoves |
|---|---|---|
| Car/Truck | 0.18 | 0 |
| Computer | 0.20 | 0.0625 |
| Cots | 1 | 1 |
| Livestock | 0.78 | 0.9375 |
| Bicycle | 0.84 | 1 |
| Electric Fan | 1 | 1 |
| Refrigerator | 0.4 | 0.1875 |
| Kachcha Floor | 0.5 | 0.5 |
| Kachcha Roof | 0.08 | 0 |
| Kachcha Walls | 0.24 | 0.25 |
| Cellular Phone | 1 | 1 |
| Mosquito Nets | 0.88 | 0.625 |
| Motorcycle | 0.72 | 0.6875 |
| Land | 0.94 | 0.9375 |
| Sewing Machine | 0.68 | 0.375 |
| Television | 0.76 | 0.375 |
| Tractor | 0.16 | 0.125 |
| Washing Machine | 0.14 | 0 |
| Water Pump | 0.4 | 0.3125 |
| Sample Size | 50 | 16 |

Note: The table shows the proportion of households that own one or more of the identified durable assets based on our baseline survey in 8 villages in Sultanpur. Column 1 represents households that reported owning induction stoves at the time of the baseline survey, and column 2 represents households that do not report induction stove ownership.

## C Kitchen and ambient PM2.5 concentrations in one household on one day

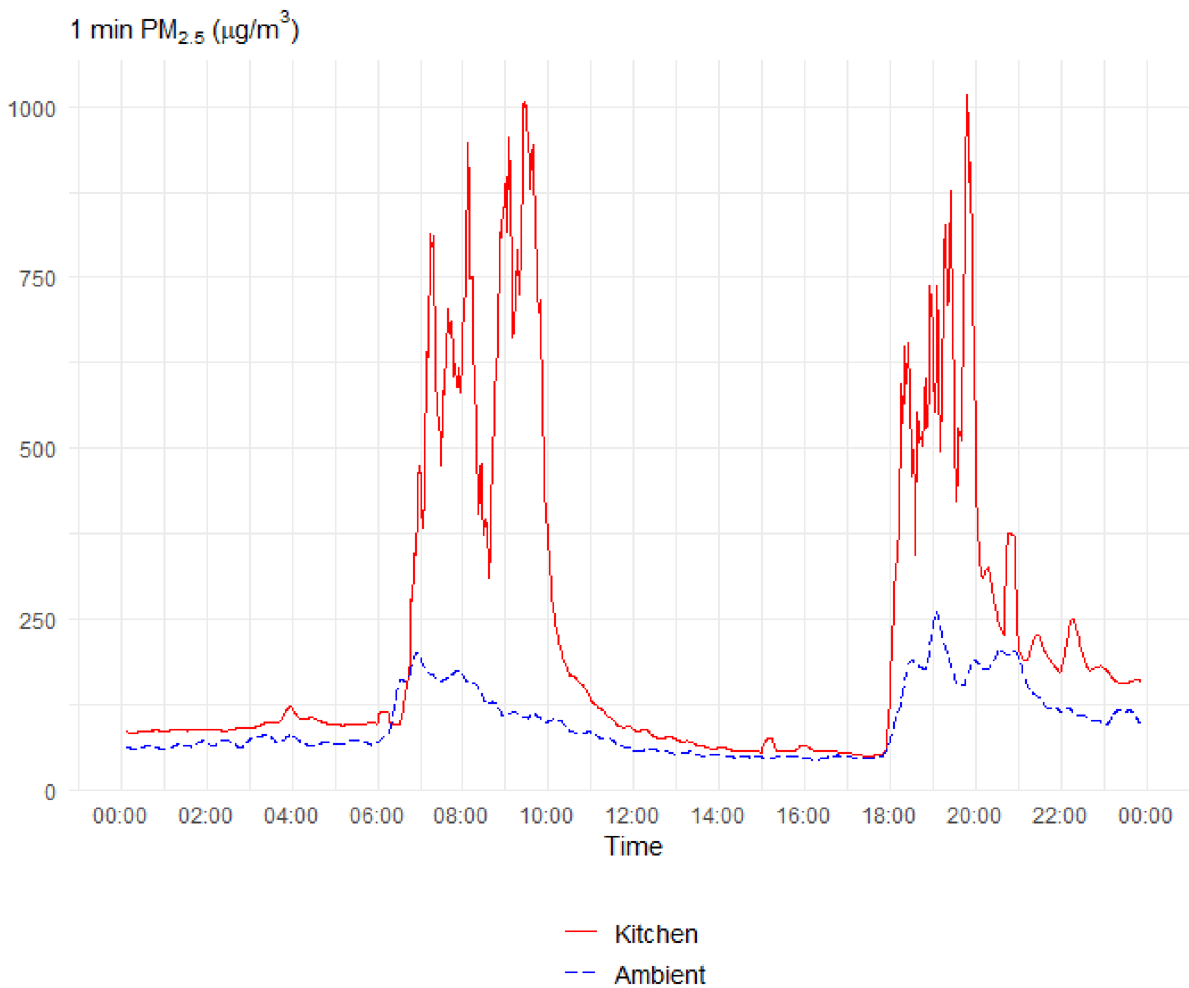



Figure A13: 15-minute moving averages of PM2.5 concentrations over a day in a household

Notes: The solid line plots 15-minute moving averages of PM2.5 ($\mu g/m^3$) concentrations over a day (10 February 2019) measured in the kitchen of a household that cooks with solid fuels. The dashed line shows data from an outdoor sensor in the same village on the same date.

# D Average PM2.5 in different household kitchen categories

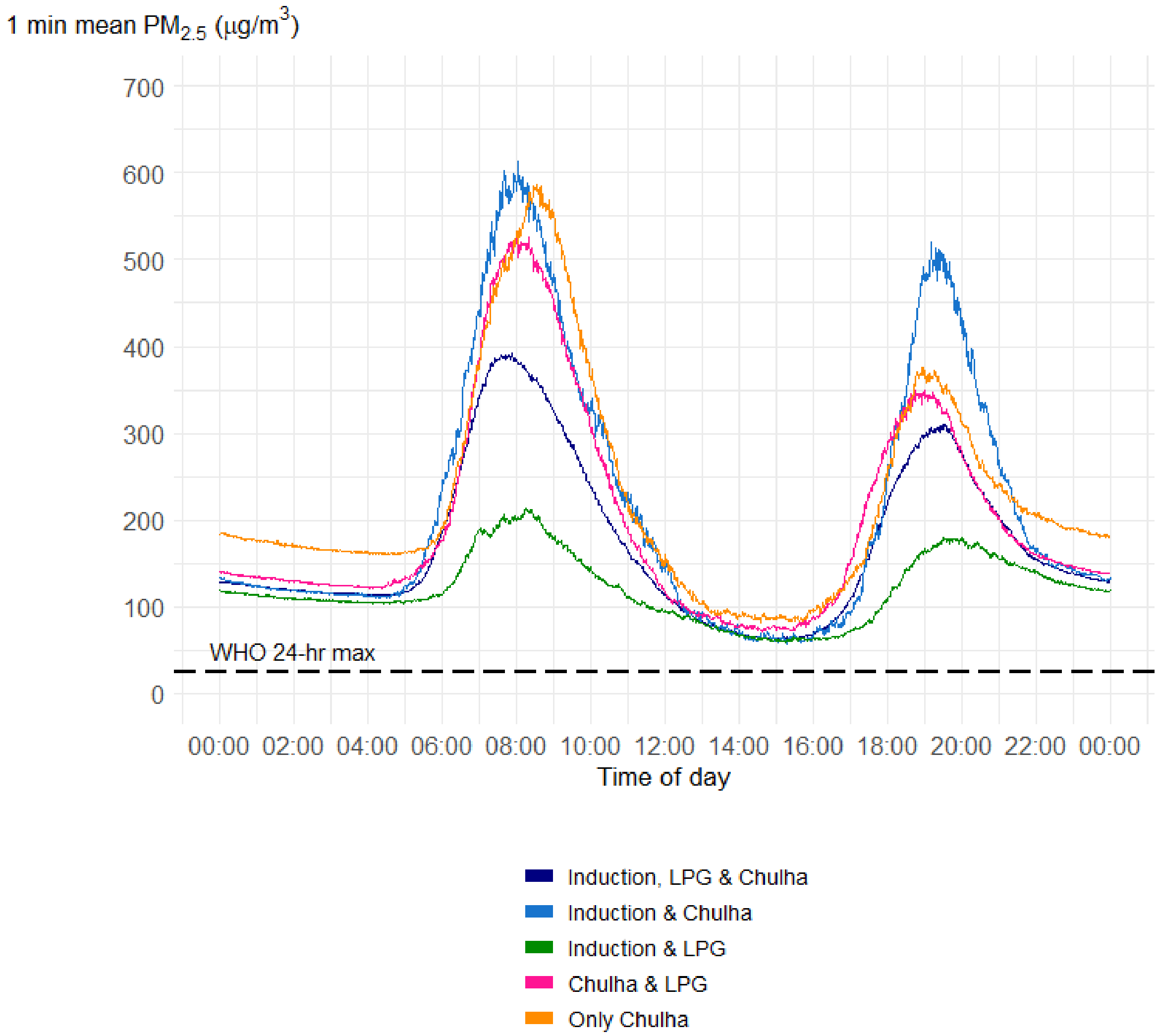



Figure A14: Mean PM2.5 $\mu g/m^3$ in the sample villages and various household kitchen categories during each minute of the day.

Notes: PM2.5 $\mu g/m^3$ for each minute of the day has been averaged over the twelve-month period 1 September 2018 to 19 September 2019. Ambient PM2.5 is averaged over the outdoor sensors in each of the 8 villages. Table 2.1 shows the number of households in each of the five categories depicted in this figure.

## E Electricity availability and outages

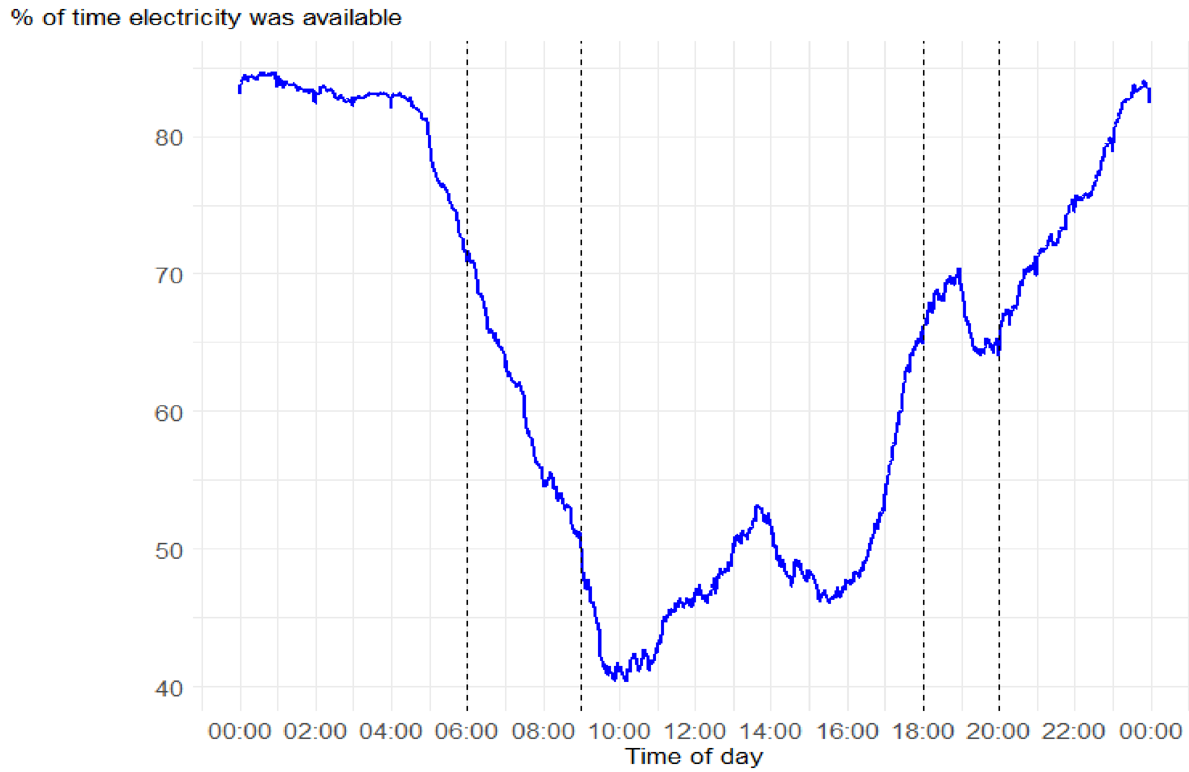



Figure A15: Percentage of days electricity was available for each minute of the day

Notes: This is an average from voltmeters on the ten lines from which the sample households drew their power from 1 September 2018 to 19 September 2019. The vertical dotted lines are the medians of start and end of morning and evening cooking times as reported from the household surveys.

# F Mean induction use share

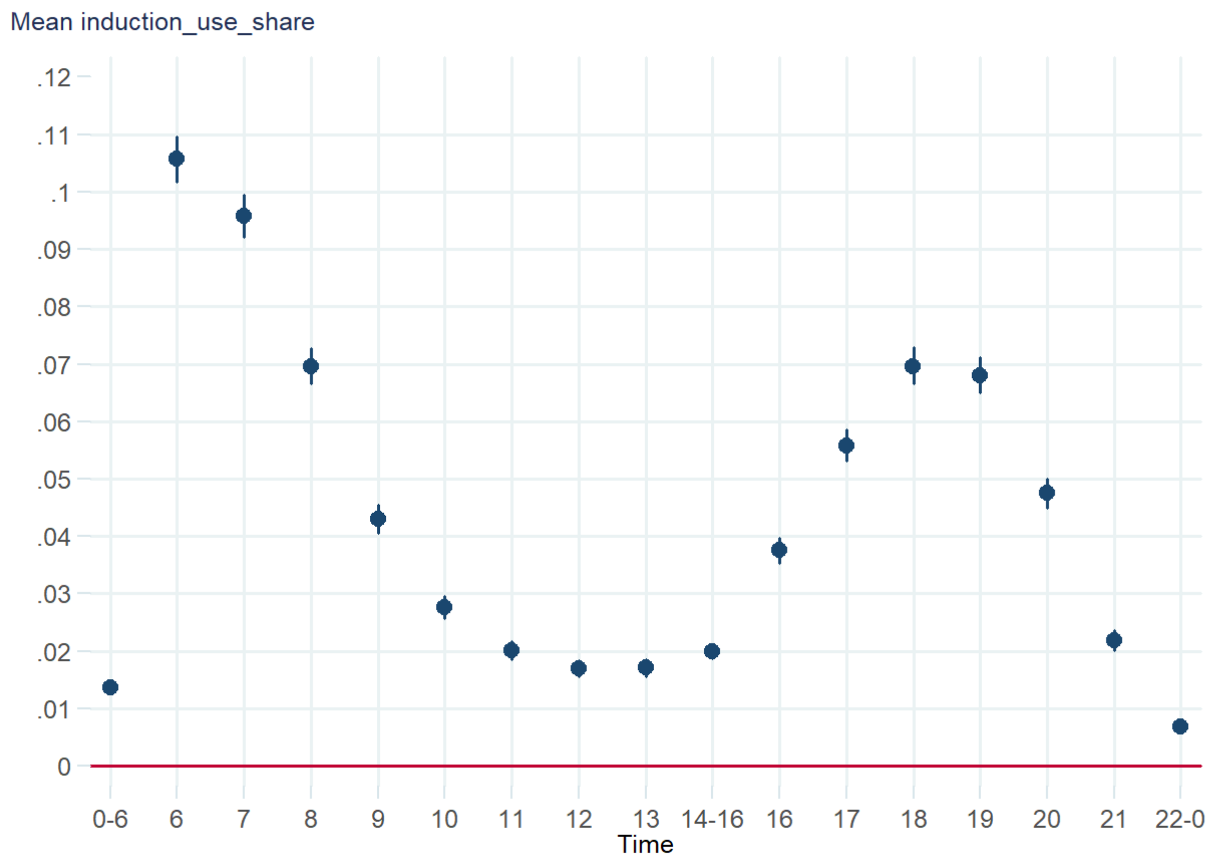



Figure A16: Period-wise shares of time in which induction stove has been used by households, averaged over all induction-stove owning households from 1 September 2018 to 19 September 2019.

Notes: The time labels on the x axis refer to periods beginning with that particular time (eg. 0-6 refers to 12 AM - 5:59 AM and 6 refers to 6 AM - 6:59 AM). Averages have been calculated using induction use data for all induction-owning households. Figure shows 95% confidence intervals of mean values.

## G Robustness checks

### G.1 LASSO estimation

Since we have 24 variables of interest in our regression models - the electricity shares during each hour of the day, it is possible that some of the coefficients will appear to be statistically significant by chance. We use the LASSO estimator to check whether any of the 24 electricity shares are poor predictors of the left-hand-side variables in our regression models.

We use the program rlasso available in the STATA package 'LASSOPACK' for estimation (Ahrens et al. (2020)). The LASSO estimator $\hat{\beta}$ solves the following problem.

$$\min_{\beta} \frac{1}{N} RSS + \frac{\lambda}{N} * ||\psi * \beta||_1 \tag{A2}$$

where $RSS = \sum_{i=1}^{N} (y_i - x_i'\beta)^2$ denotes the residual sum of squares,

$\beta$ is a p-dimensional parameter vector,

$\lambda$ is the overall penalty level,

$||.||_1$ denotes the L1-norm, i.e. $\sum_i |a_i|$,

$\psi$ is a p by p diagonal matrix of predictor-specific penalty loadings (rLASSO treats $\psi$ as a row vector),

N is the number of observations

We partial-out month-hour and household-hour variables prior to construction of penalty loadings since we want to use only between-day variation in electricity shares in each period to estimate effects on PM2.5. Heteroskedastic and autocorrelation-consistent (HAC) penalty loadings (Chernozhukov et al. (2021)) have been obtained using the bw() option with the robust option. The default Bartlett kernel with bandwidth 11 (order $T^{1/4}$) has been used.

**LASSO estimation of Equation 2.1 : Induction-stove-owning households with *chulha***

The variables selected for inclusion by the LASSO estimator are shown in the first column of Table A7. The second column shows the LASSO estimates and the third column lists the Ordinary Least Squares (OLS) coefficient estimates from the model estimated after dropping the non-selected coefficients.

Table A7: LASSO Estimation of Equation 2.1 with dependent variable kitchen PM2.5 on the primary subsample of induction-stove-owning households with *chulhas*

| Selected | LASSO | Post-est OLS |
|---|---|---|
| Ambient_Pollution | 0.2822 | 0.3006 |
| elec_6 | -2.9119 | -25.8859 |
| elec_7 | -29.2644 | -54.5689 |
| elec_8 | -16.5691 | -40.9609 |
| elec_16 | -1.7719 | -15.7593 |
| elec_17 | -1.7139 | -22.7862 |
| elec_18 | -19.2343 | -45.4534 |
| elec_19 | -6.4340 | -31.8523 |
| Obs | 228184 | |
| R-Sq | 0.046 | |

Notes: "elec_i" denotes the share of hour *i* during which electricity was available. Month-hour, household-hour, and day-of-the-week fixed effects partialled-out prior to LASSO estimation. Only ambient PM2.5 and electricity shares in each of the 24 hours were included in the set of variables to be penalized.

### G.2 Equation 2.1 with hour - lag of electricity share as an additional control variable

As a robustness check, we re-estimated Equation 2.1 after including electricity shares lagged by one hour as shown in Equation A3 below.

$$Kitchen\_PM2.5_{hljt} = a_{hj} + d_{mj} + \gamma Ambient\_PM2.5_{ljt} + \sum_{j=1}^{24} \mu_j Elec\_share_{ljt} * hour_j + \sum_{j=2}^{24} \eta_j Elec\_share_{lj-1t} * hour_j + \epsilon_{hljt} \quad \text{(A3)}$$

where $Kitchen\_PM2.5_{hljt}$ is the average PM2.5 concentration in household $h$ on electricity line $l$ on day $t$ in hour $j$, $a_{hj}$ is a household-period fixed effect, $d_{mj}$ is a month-period fixed effect, $Ambient\_PM2.5_{ljt}$ is the average ambient PM2.5 concentration in the area with electricity line $l$ on day $t$ in period $j$, $Elec\_share_{ljt}$ is the share of time in hour $j$ on day $t$ for which electricity was supplied in line $l$, $hour_j$ is a dummy variable for hour $j$, $\epsilon_{hljt}$ is the residual error term for household $h$ on day $t$ in hour $j$ on line $l$.

As seen in Figure A17, the coefficients on electricity shares show a pattern similar to the one depicted in Figure 2.3, although they are less precisely estimated. Figure A18 shows that electricity availability in the previous hour reduces pollution to a much lesser extent when compared with its contemporaneous effect. The effect of the one-period lag may be due to a decision to start cooking with an induction stove earlier, rather than with a *chulha*, when electricity is available, a shift that could carry over into the subsequent period.

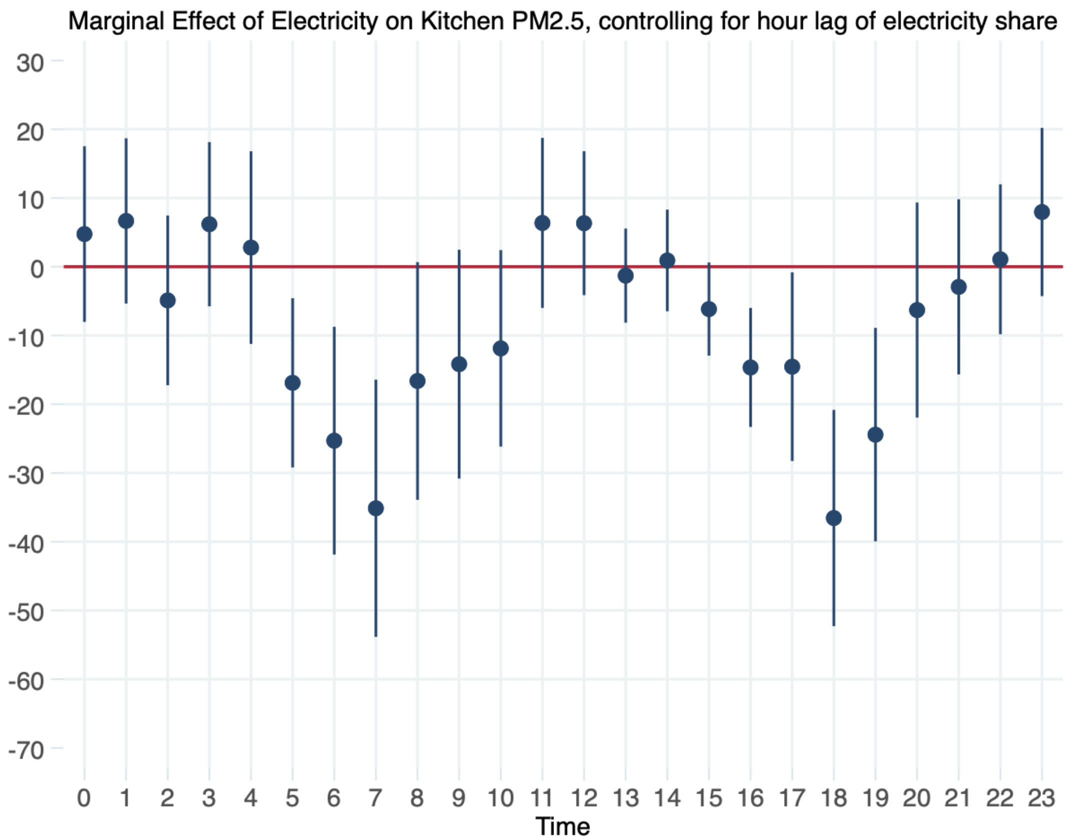



Figure A17: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for induction-owning households with *chulha*, controlling for electricity shares lagged by one hour

Notes: The time labels on the x axis refer to hours beginning with that particular time (eg. 6 refers to 6 AM - 6:59 AM). Plots depict coefficient $\mu_j$ from Equation A3. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

Electricity lagged by one hour coefficients from Equation A3

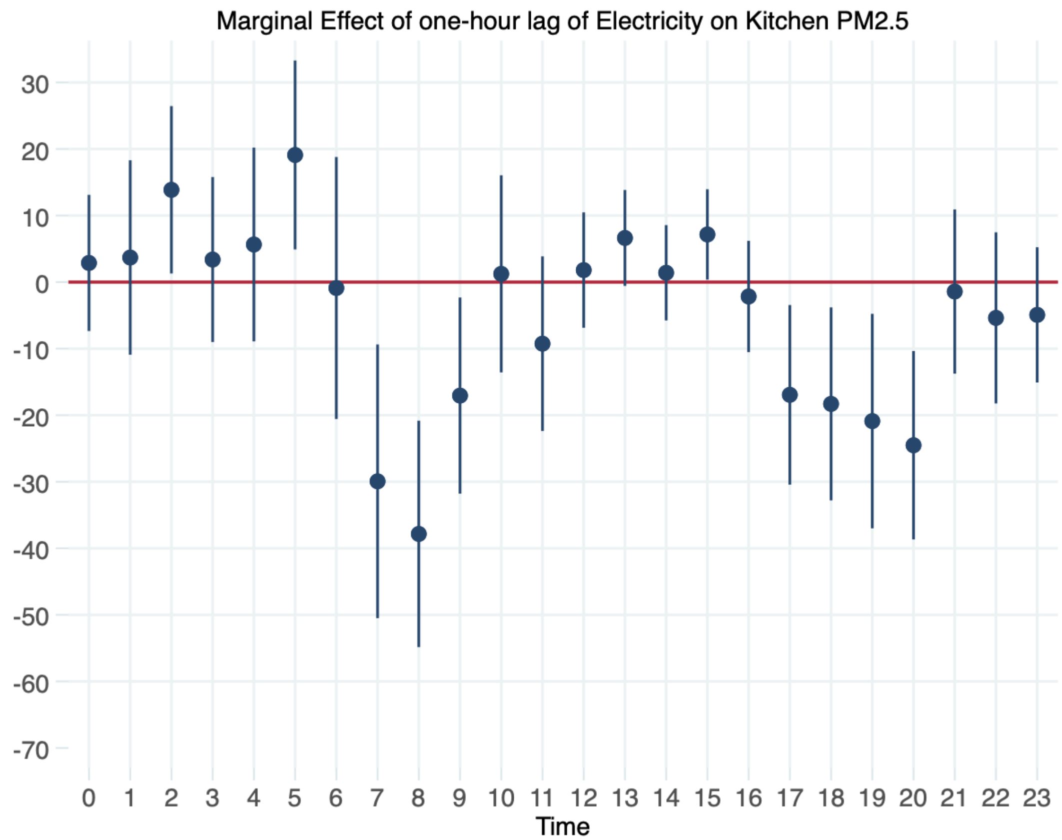



Figure A18: Hour-wise marginal effects of previous hour's electricity supply on kitchen PM2.5 for induction-owning households with *chulha*

Notes: The time labels on the x axis refer to periods beginning with that particular time (eg. 6 refers to 6 AM - 6:59 AM). The plots depict coefficient $\eta_j$ from Equation A3. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

### G.3 Equation 2.1 with day - lag of electricity share as an additional control variable

We ran a specification similar to Equation 2.1 after including electricity shares lagged by one day as shown in Equation A4 below.

$$Kitchen\_PM2.5_{hljt} = a_{hj} + d_{mj} + \gamma Ambient\_PM2.5_{ljt} + \sum_{j=1}^{24} \alpha_j Elec\_share_{ljt} * Period_j + \sum_{j=1}^{24} \theta_j Elec\_share_{ljt-1} * hour_j + \epsilon_{hljt} \quad \text{(A4)}$$

where $Kitchen\_PM2.5_{hljt}$ is the average PM2.5 concentration in household $h$ on electricity line $l$ on day $t$ in hour $j$, $a_{hj}$ is a household-hour fixed effect, $d_{mj}$ is a month-hour fixed effect, $Ambient\_PM2.5_{ljt}$ is the average ambient PM2.5 concentration in the area with electricity line $l$ on day $t$ in hour $j$, $Elec\_share_{ljt}$ is the share of time in hour $j$ on day $t$ for which electricity was supplied in line $l$, $hour_j$ is a dummy variable for hour $j$, $\epsilon_{hljt}$ is the residual error term for household $h$ on day $t$ in hour $j$ on line $l$

The pattern shown by coefficients on electricity shares in Figure A19 is similar to the one seen in Figure 2.3. However, Figure A20 shows no such pattern of effects of the previous day's electricity shares. This confirms that adjustments such as the decision to start cooking with an induction stove earlier, rather than with a *chulha*, when electricity is available, only occur within the same day.

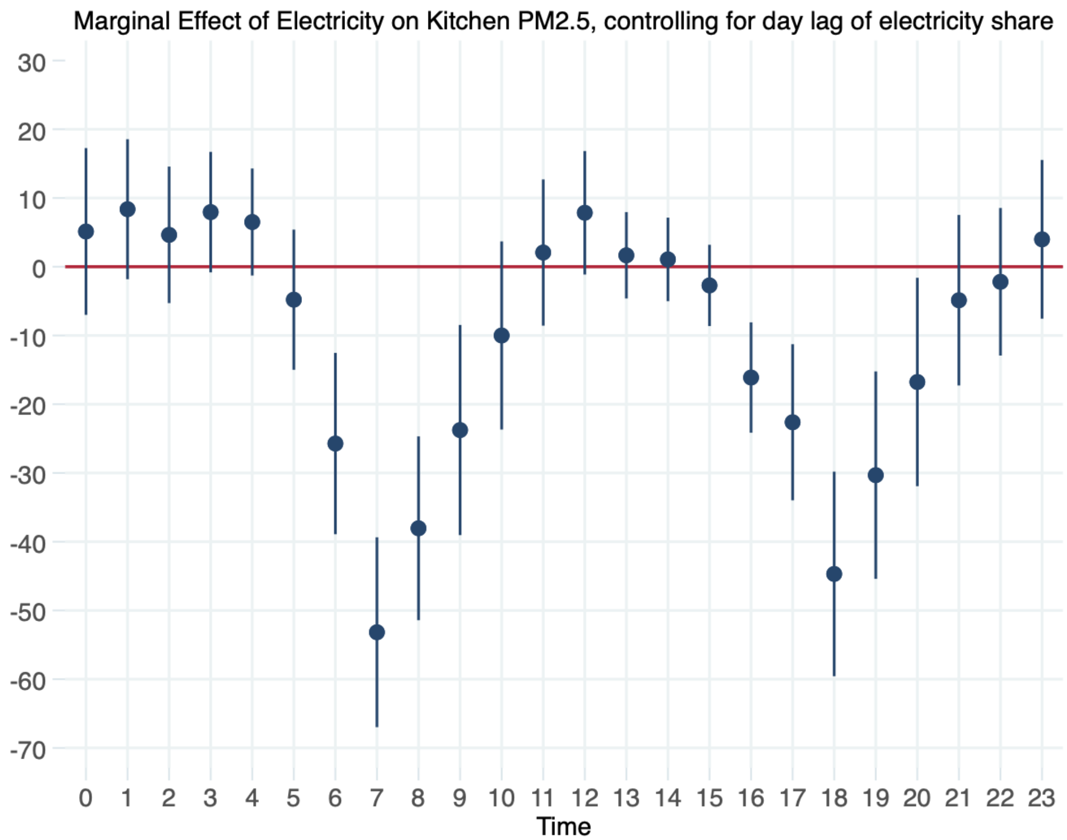



Figure A19: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for induction-owning households with *chulha*, controlling for electricity shares lagged by day.

Notes: The time labels on the x axis refer to periods beginning with that particular time (eg. 6 refers to 6 AM - 6:59 AM). The plots depict coefficient $\alpha_j$ from Equation A4. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

Day-lag electricity coefficients from Equation A4

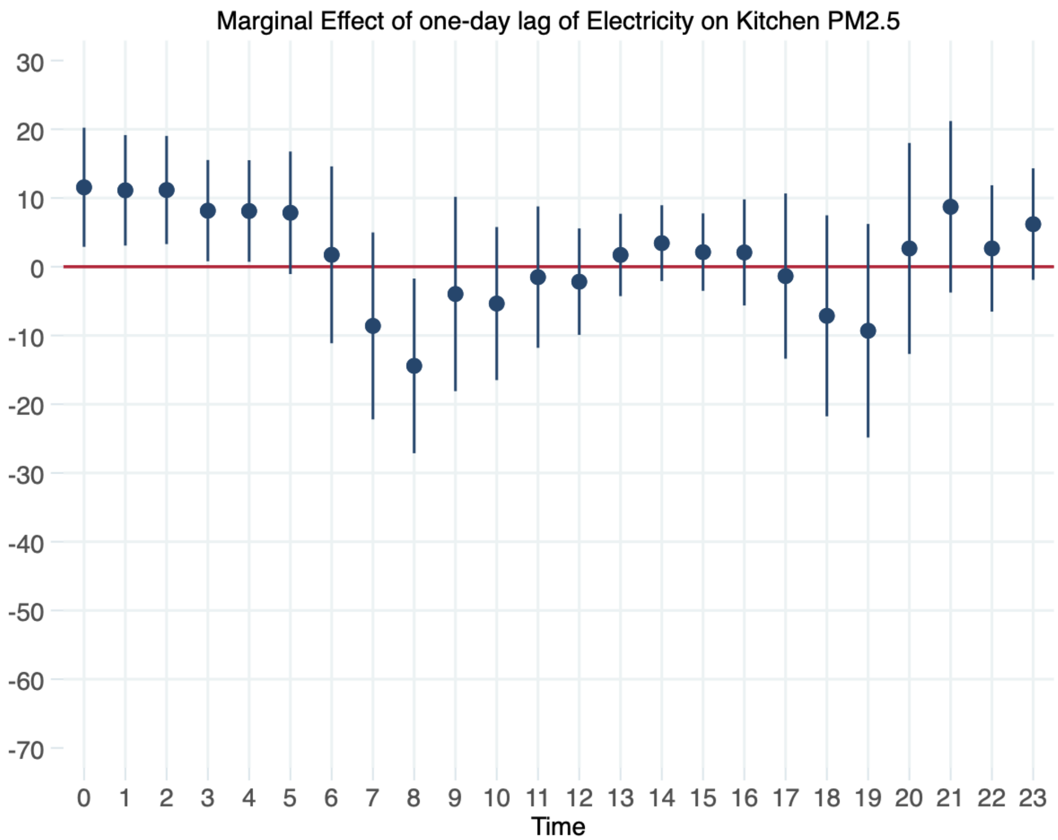



Figure A20: Hour-wise marginal effects of previous day's electricity supply on kitchen PM2.5 for induction-owning households with *chulha*.

Notes: The time labels on the x axis refer to periods beginning with that particular time (eg. 6 refers to 6 AM - 6:59 AM). The plots depict coefficient $\theta_j$ from Equation A4. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

## G.4 Equation 2.1 for the placebo subsample without induction stoves

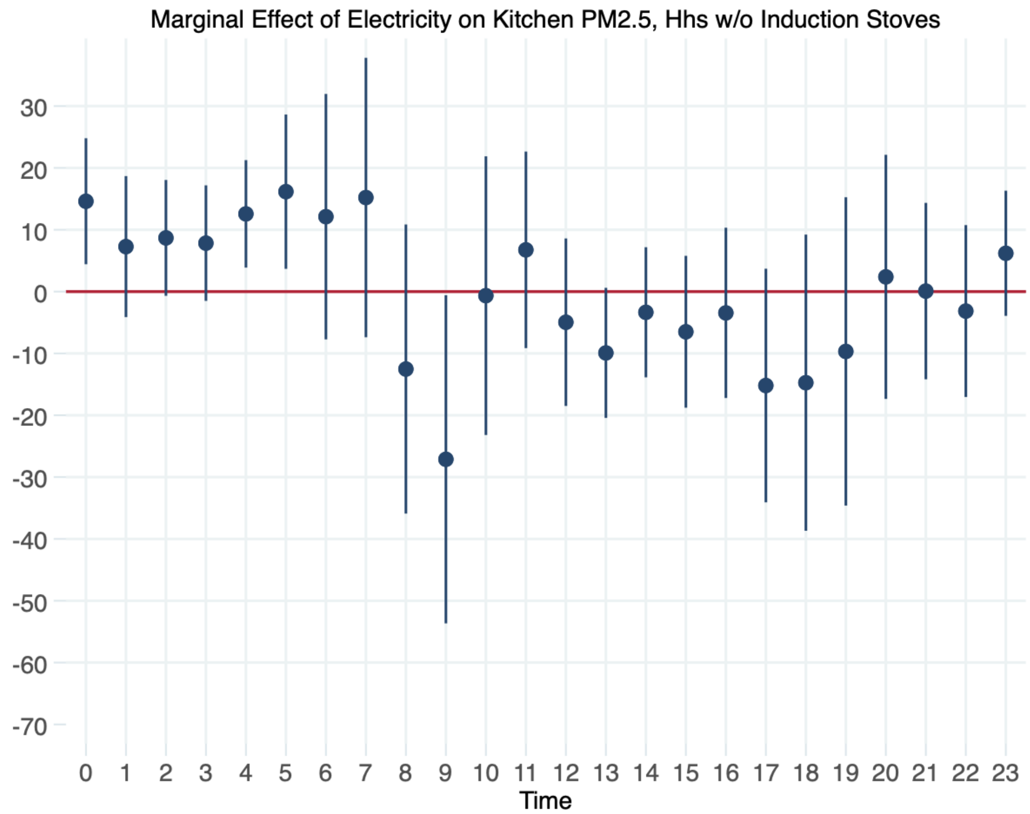



Figure A21: Period-wise marginal effects of electricity supply on kitchen PM2.5 for the 15 households with a *chulha* (solid-fuel stove) but without induction stoves

Notes: The plots depict coefficient $\mu_j$ from Equation 2.1. 95% confidence intervals have been computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

## G.5 Equation 2.1 for the placebo subsample with only clean stoves

Equation 2.1 was run on the subsample of households with only clean stoves as a placebo. As shown in Figure A22, the reductions in PM2.5 due to electricity availability are not only much smaller, but also insignificant in most periods in this subsample. Induction stove use in the clean-stove subsample responds in the same way to electricity availability as in the primary subsample (Figures A26, **??**), suggesting that it substitutes for LPG.

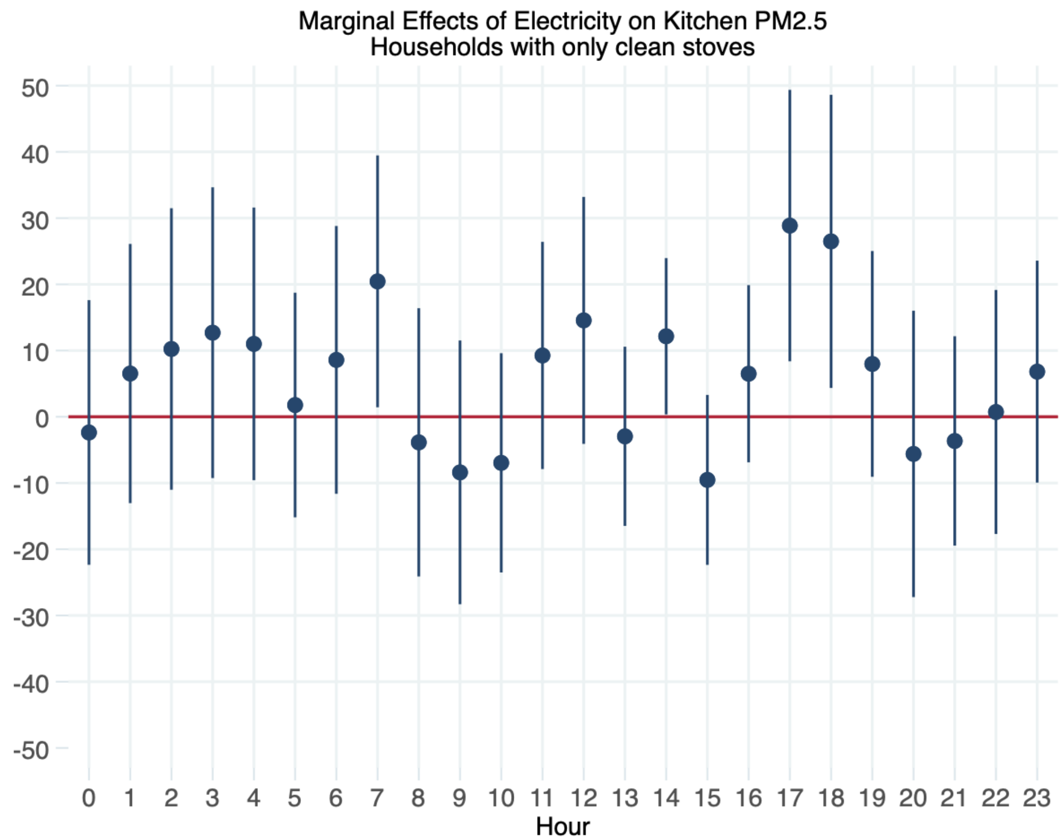



Figure A22: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for the 6 households with only clean stoves (induction and LPG, but no *chulha*)

Notes: The time labels refer to hours beginning with that particular time (e.g. 6 refers to 6 AM - 6:59 AM). The plots depict coefficient $\mu_j$ from Equation 2.1. 95% confidence intervals have been computed using Driscoll-Kraay standard errors robust to cross-sectional and temporal dependence.

### G.6 LASSO estimation of Equation 2.1 : Placebo subsample with only clean stoves

We re-estimated Equation 2.1 using the LASSO estimator for the placebo subsample of 6 households with only clean stoves. In line with our expectations, Table A8 shows that none of the electricity shares were selected for inclusion in the model indicating they were poor predictors of PM2.5.

Table A8: LASSO Estimation of Equation 2.1 (Subsample of 6 households with only clean stoves)

| Selected | LASSO | Post-est OLS |
|---|---|---|
| Ambient_PM2.5 | 0.2626 | 0.3032 |
| Obs | 30933 | |
| R-Sq | 0.071 | |

Notes: Month-hour and household-hour variables partialled-out prior to LASSO estimation. Only ambient PM2.5 and electricity share interacted with period variables were included in the set of variables to be penalized.

## G.7 LASSO estimation of Equation 2.1 : Placebo subsample of households without induction stoves

We re-estimated Equation 2.1 using the LASSO estimator for the placebo subsample of households without induction stoves. As seen in Table A9, all electricity shares were dropped from the model indicating that they had little predictive power.

Table A9: LASSO Estimation of Equation 2.1 with dependent variable kitchen PM2.5 on the placebo subsample of households without induction stoves

| Selected | LASSO | Post-est OLS |
|---|---|---|
| Ambient_PM2.5 | 0.5014 | 0.5452 |
| Obs | 56108 | |
| R-Sq | 0.126 | |

Notes: Month-hour and household-hour variables partialled-out prior to LASSO estimation. Only ambient PM2.5 and electricity share interacted with period variables were included in the set of variables to be penalized.

### G.8 Equation 2.1 for households that use and don't use a fan in the kitchen

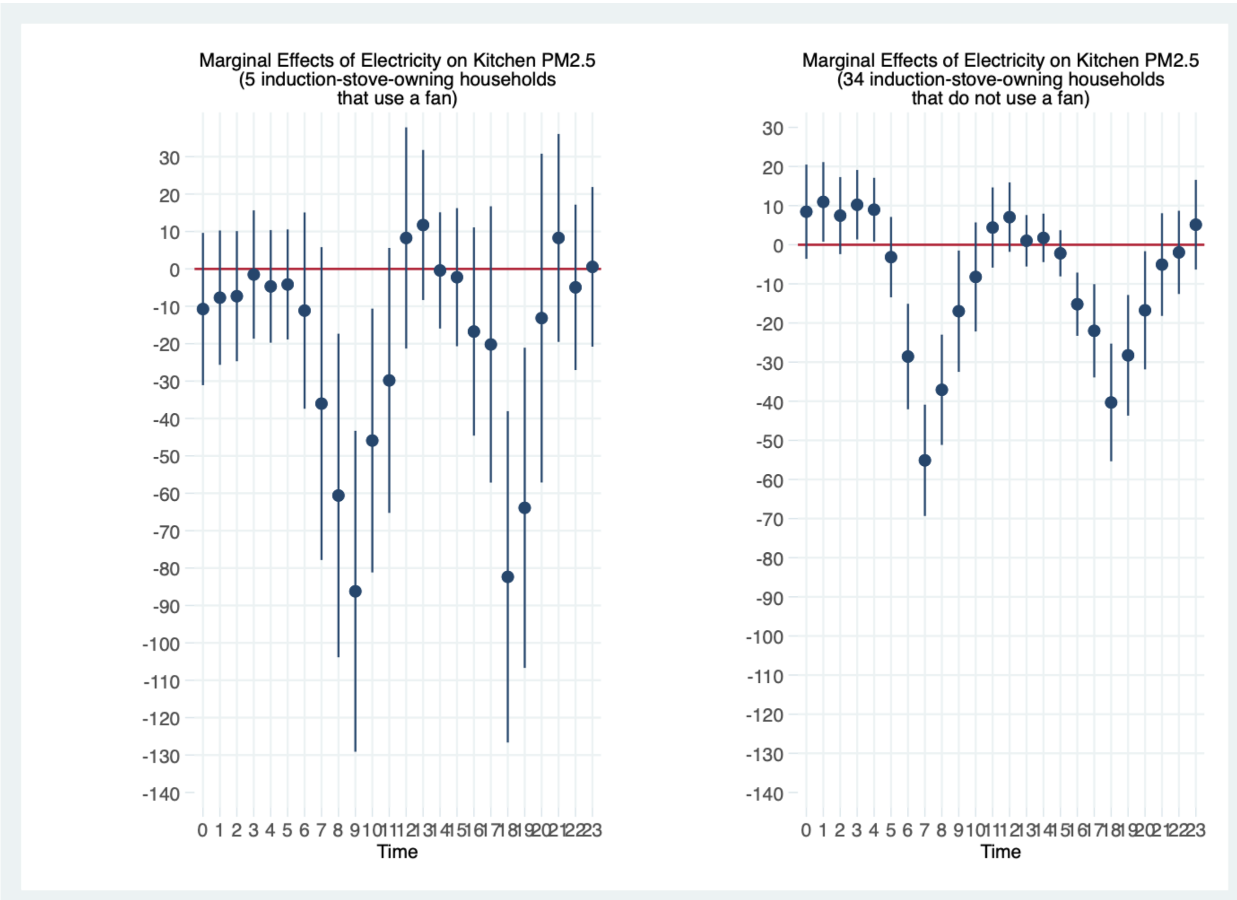



Figure A23: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for induction-stove-owning households with a *chulha* (solid-fuel stove) by use of fans in the kitchen

Notes: Plots depict the coefficients $\mu_j$ from Equation 2.1. Left panel: Households that use fans in the kitchen during or after cooking. Right panel: Households that do not do so. 95% confidence intervals computed using Driscoll-Kraay standard errors that are robust to cross-sectional and temporal dependence.

## G.9 Equation 2.1 for households that have and do not have power backup

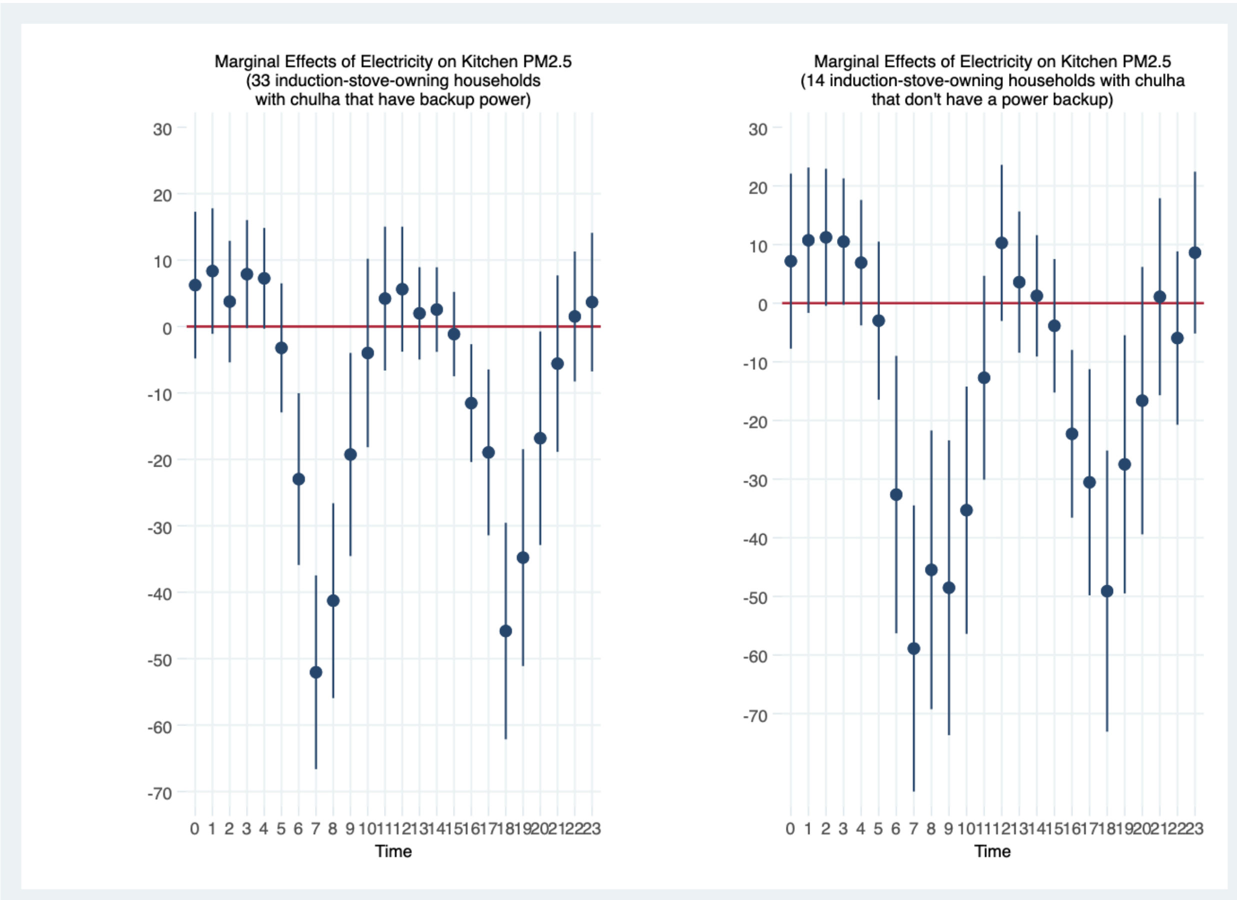



Figure A24: Hour-wise marginal effects of electricity supply on kitchen PM2.5 for induction-stove-owning households with a *chulha* (solid-fuel stove) by availability of backup power for lighting

Notes: Plots depict the coefficients $\mu_j$ from Equation 2.1. Left panel: Households with backup power. Right panel: Households without backup power. 95% confidence intervals computed using Driscoll-Kraay standard errors that are robust to cross-sectional and temporal dependence.

## G.10 Modified Equation 2.1 with low and normal-voltage electricity shares as control variables

We ran a modified version of Equation 2.1 in which the share of the period electricity is available is replaced by two variables, the share of the period low-voltage (100-200V) electricity is available, and the share of the period that near-normal-voltage (>200V) electricity is available as shown in Equation A5 below.

$$Kitchen\_PM2.5_{hljt} = a_{hj} + d_{sj} + \gamma Ambient\_PM2.5_{ljt} + \sum_{j=1}^{17} \alpha_j Low\_volt\_Elec\_share_{ljt} * Period_j + \sum_{j=1}^{17} \theta_j Normal\_volt\_Elec\_share_{ljt} * Period_j + \epsilon_{hljt} \quad \text{(A5)}$$

where $Kitchen\_PM2.5_{hljt}$ is the average PM2.5 concentration in household $h$ on electricity line $l$ on day $t$ in period $j$, $a_{hj}$ is a household-period fixed effect, $d_{sj}$ is a season-period fixed effect, $Ambient\_PM2.5_{ljt}$ is the average ambient PM2.5 concentration in the area with electricity line $l$ on day $t$ in period $j$, $Low\_volt\_Elec\_share_{ljt}$ is the share of time in period $j$ on day $t$ for which low-voltage electricity was supplied in line $l$, $Normal\_volt\_Elec\_share_{ljt}$ is the share of time in period $j$ on day $t$ for which normal-voltage electricity was supplied in line $l$, $Period_j$ is a dummy variable for period $j$, $\epsilon_{hljt}$ is the residual error term for household $h$ on day $t$ in period $j$ on line $l$

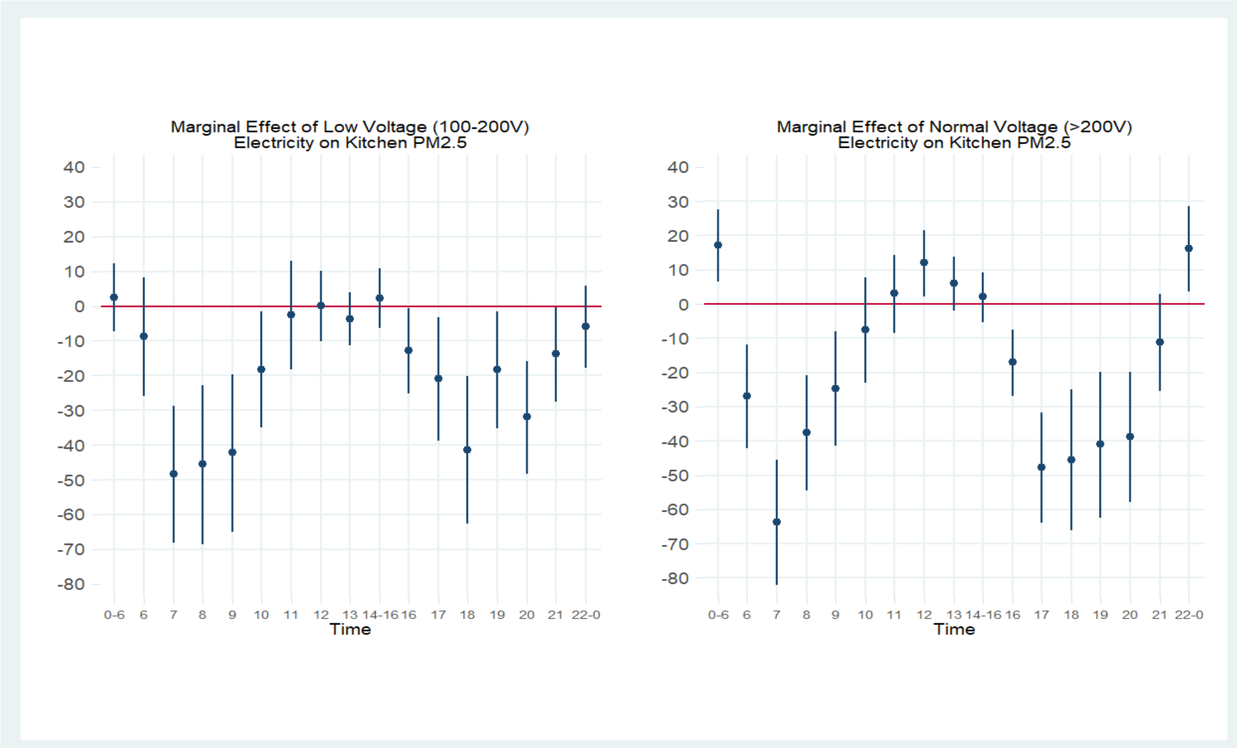



Figure A25: Period-wise marginal effects of low and normal voltage electricity on kitchen PM2.5 for induction-stove-owning households with *chulha*

Notes: The time labels on the x axis refer to periods beginning with that particular time (eg. 0-6 refers to midnight - 5:59 AM and 6 refers to 6 AM - 6:59 AM). The plots in the left panel depict coefficient $\alpha_j$ from Equation A5. The plots in the right panel depict coefficient $\theta_j$ from Equation A5. 95% confidence intervals computed using Driscoll-Kraay standard errors that allow for cross-sectional and temporal dependence.

## H IV Regressions - Detailed Results

## H.1 Estimates for Equation 2.2 (Second Stage of IV Regression)

Table A10: Equation (2.2)

| | 0 | 1 | 2 | 3 | 4 |
|---|---|---|---|---|---|
| induction_use_share | -3339.5309 | 2970.6680 | 561.6868 | -12284.5549 | 844.6813 |
| | (6634.9625) | (3252.6433) | (4851.4548) | (38563.5903) | (653.7015) |
| | [NA] | [NA] | [NA] | [NA] | [1049.892] |
| Ambient_Pollution | 0.3880*** | 0.4127*** | 0.3715*** | 0.3255** | 0.3247*** |
| | (0.0700) | (0.0680) | (0.0730) | (0.1393) | (0.0746) |
| Obs | 3189 | 3163 | 3153 | 3141 | 3149 |
| R-Sq | -0.362 | 0.026 | 0.573 | -6.213 | 0.293 |
| Kleibergen-Paap rk Wald F statistic | 0.325 | 2.535 | 2.664 | 0.118 | 11.883 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parenthesis, Lee et al. (2021) adjusted standard errors for in brackets

Table A11: Equation (2.2) contd.

| | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|
| induction_use_share | -41.7150 | -225.5968*** | -444.9707*** | -407.7511*** | -82.5507 |
| | (102.1713) | (87.0233) | (90.6950) | (91.1825) | (159.8459) |
| | [104.216] | [87.023] | [90.6950] | [91.1825] | [159.845] |
| Ambient_Pollution | 0.3870*** | 0.2562*** | 0.3051*** | 0.2657*** | 0.2464*** |
| | (0.0735) | (0.0537) | (0.0635) | (0.0614) | (0.0518) |
| Obs | 3166 | 3188 | 3187 | 3216 | 3218 |
| R-Sq | 0.316 | 0.431 | 0.376 | 0.377 | 0.375 |
| Kleibergen-Paap rk Wald F statistic | 87.206 | 106.105 | 172.024 | 130.978 | 108.169 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parenthesis, Lee et al. (2021) adjusted standard errors in brackets

Table A12: Equation (2.2) contd.

| | 10 | 11 | 12 | 13 | 14 |
|---|---|---|---|---|---|
| induction_use_share | 103.6628 | 108.8162 | 153.5697 | 4.7101 | 149.7756 |
| | (217.2444) | (257.4419) | (287.9337) | (268.2994) | (264.2224) |
| | [228.687] | [302.718] | [344.587] | [297.667] | [1101.578] |
| Ambient_Pollution | 0.2380*** | 0.1844*** | 0.1771*** | 0.0928** | 0.0887*** |
| | (0.0597) | (0.0558) | (0.0572) | (0.0407) | (0.0287) |
| Obs | 3196 | 3202 | 3245 | 3263 | 3266 |
| R-Sq | 0.332 | 0.316 | 0.249 | 0.144 | 0.124 |
| Kleibergen-Paap rk Wald F statistic | 80.302 | 33.086 | 30.386 | 45.960 | 48.585 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parenthesis, Lee et al. (2021) adjusted standard errors for in brackets

Table A13: Equation (2.2) contd.

| | 15 | 16 | 17 | 18 | 19 |
|---|---|---|---|---|---|
| induction_use_share | 43.4343 | -101.2363 | -150.7371 | -293.7736*** | -450.9189** |
| | (121.0090) | (98.9530) | (98.4872) | (112.1421) | (185.0617) |
| | [126.586] | [100.387] | [98.487] | [114.146] | [192.762] |
| Ambient_Pollution | ] 0.0882*** | 0.0816** | 0.2057*** | 0.2189*** | 0.2328*** |
| | (0.0285) | (0.0326) | (0.0518) | (0.0469) | (0.0486) |
| Obs | 3276 | 3308 | 3344 | 3341 | 3310 |
| R-Sq | 0.116 | 0.121 | 0.351 | 0.469 | 0.292 |
| Kleibergen-Paap rk Wald F statistic | 70.116 | 91.889 | 106.286 | 89.066 | 72.894 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parenthesis, Lee et al. (2021) adjusted standard errors for in brackets

Table A14: Equation (2.2) contd.

| | 20 | 21 | 22 | 23 |
|---|---|---|---|---|
| induction_use_share | -297.3164 | -790.2788 | -436.6537 | -132.7932 |
| | (333.4545) | (661.7319) | (1001.9221) | (1728.3976) |
| | [381.275] | [762.831] | [1561.456] | [NA] |
| Ambient_Pollution | 0.3617*** | 0.4033*** | 0.4364*** | 0.4054*** |
| | (0.0650) | (0.0688) | (0.0789) | (0.0655) |
| Obs | 3286 | 3259 | 3232 | 3221 |
| R-Sq | 0.340 | 0.407 | 0.576 | 0.604 |
| Kleibergen-Paap rk Wald F statistic | 38.421 | 36.756 | 12.731 | 0.848 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Standard errors in parenthesis, Lee et al. (2021) adjusted standard errors for induction_use_share

### H.2 Estimates for Equation 2.3 (First Stage of IV Regression)

Table A15: Equation (2.3)

| | 0 | 1 | 2 | 3 | 4 |
|---|---|---|---|---|---|
| electricity_supply_share | -0.0017 | 0.0027 | 0.0015 | -0.0006 | 0.0096*** |
| | (0.0029) | (0.0017) | (0.0009) | (0.0018) | (0.0028) |
| Obs | 3189 | 3163 | 3153 | 3141 | 3149 |
| F statistic | 0.325 | 2.535 | 2.664 | 0.118 | 11.883 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Equation (2.3) contd.

| | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|
| electricity_supply_share | 0.0700*** | 0.1277*** | 0.1444*** | 0.1219*** | 0.0765*** |
| | (0.0075) | (0.0124) | (0.0110) | (0.0106) | (0.0074) |
| Obs | 3166 | 3188 | 3187 | 3216 | 3218 |
| F statistic | 87.206 | 106.105 | 172.024 | 130.978 | 108.169 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: Equation (2.3) contd.

| | 10 | 11 | 12 | 13 | 14 |
|---|---|---|---|---|---|
| electricity_supply_share | 0.0499$^{***}$ | 0.0316$^{***}$ | 0.0304$^{***}$ | 0.0233$^{***}$ | 0.0237$^{***}$ |
| | (0.0056) | (0.0055) | (0.0055) | (0.0034) | (0.0034) |
| Obs | 3196 | 3202 | 3245 | 3263 | 3266 |
| F statistic | 80.302 | 33.086 | 30.386 | 45.960 | 48.585 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A18: Equation (2.3) contd.

| | 15 | 16 | 17 | 18 |
|---|---|---|---|---|
| electricity_supply_share | 0.0431$^{***}$ | 0.0658$^{***}$ | 0.1068$^{***}$ | 0.1088$^{***}$ |
| | (0.0051) | (0.0069) | (0.0104) | (0.0115) |
| Obs | 3276 | 3308 | 3344 | 3341 |
| F statistic | 70.116 | 91.889 | 106.286 | 89.066 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table A19: Equation (2.3) contd.

| | 19 | 20 | 21 | 22 | 23 |
|---|---|---|---|---|---|
| electricity_supply_share | 0.0771$^{***}$ | 0.0426$^{***}$ | 0.0153$^{***}$ | 0.0059$^{***}$ | -0.0037 |
| | (0.0090) | (0.0069) | (0.0025) | (0.0016) | (0.0040) |
| Obs | 3310 | 3286 | 3259 | 3232 | 3221 |
| F statistic | 72.894 | 38.421 | 36.756 | 12.731 | 0.848 |

$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

# I IV Regressions - Detailed Results

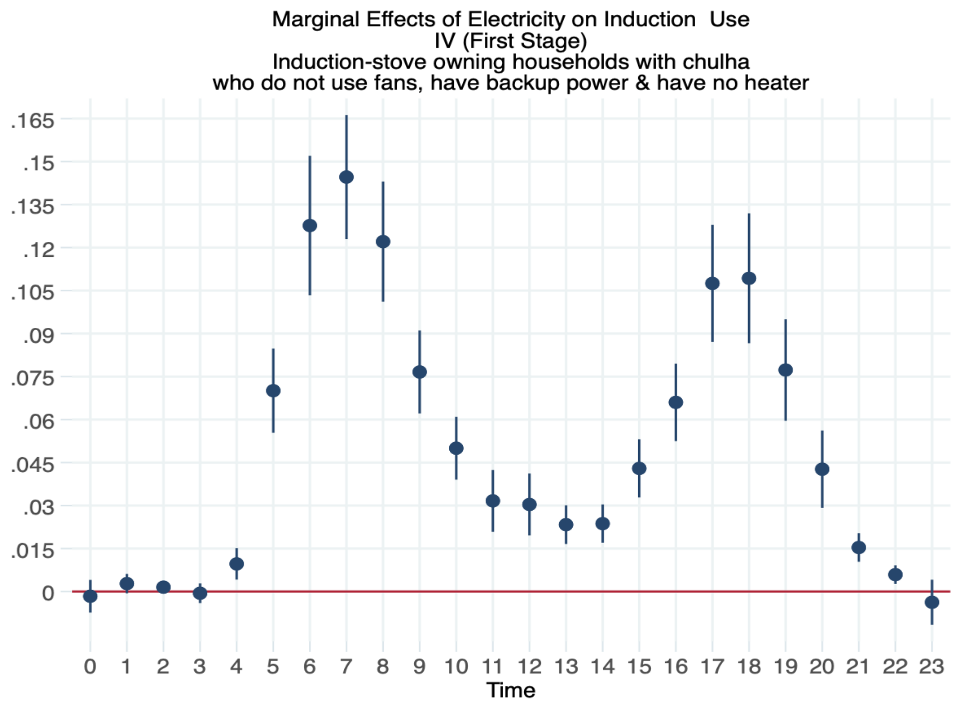
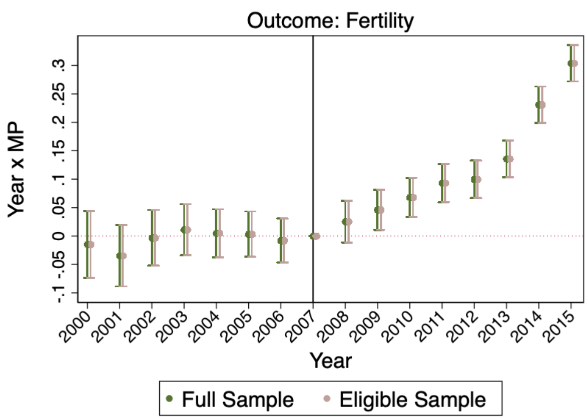



Figure A26: Marginal effects of electricity availability on induction stove use for induction-stove-owning households with a *chulha* (solid-fuel stove)

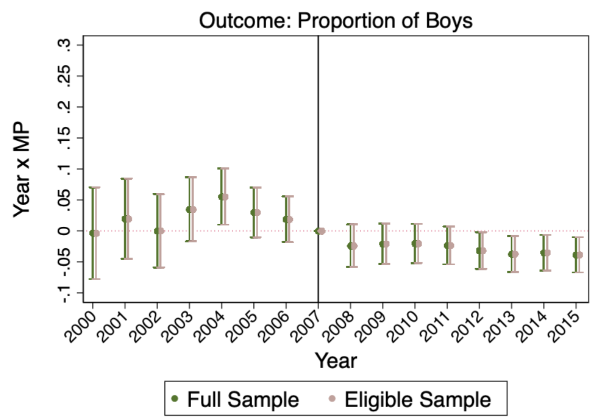
Notes: The sample includes 22 households that satisfied the exclusion restriction. Plots depict the coefficients $\nu_j$ from the first-stage Equation 2.3. 95% confidence intervals computed using Driscoll-Kraay standard errors robust to cross-sectional and temporal dependence.

# Appendix C: Chapter 3: Can Large-Scale Conditional Cash Transfers Resolve the Fertility-Sex Ratio Trade-off? Evidence from India

## A Additional Figures and Tables



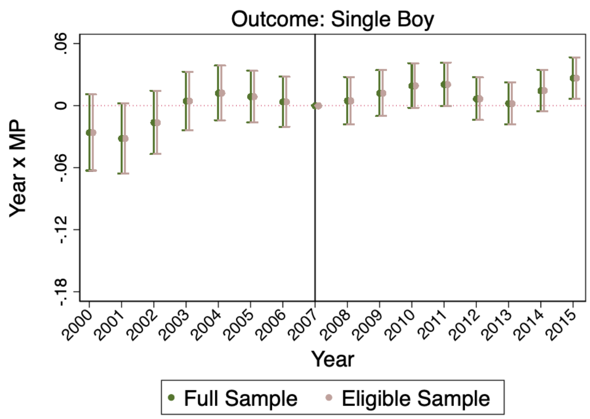

(a) Effect on Fertility



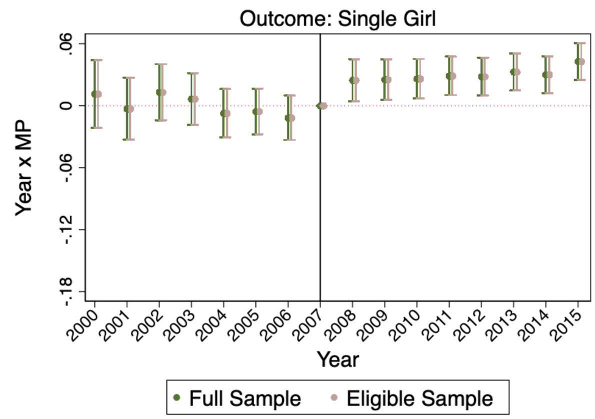

(b) Effect on Sex Ratio

Figure A1: Robustness: Effect of *Ladli Laxmi* on fertility and sex ratio using 2000-2016

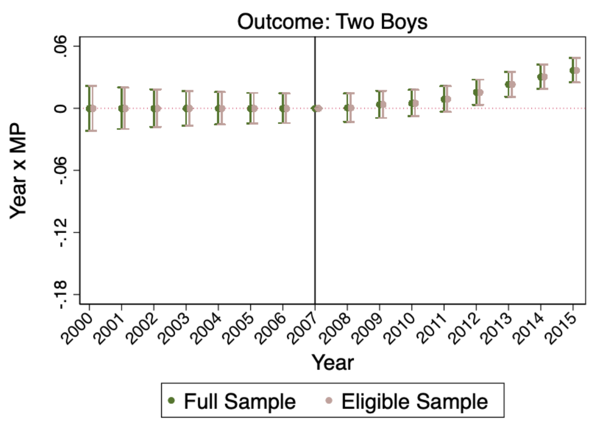
*Notes:* This figure present dynamic treatment effects of *Ladli Laxmi* using our difference-in-differences specification from Equation 3.2 and a larger panel from 2000-2015. Panel (a) present the treatment effect on fertility and panel (b) presents treatment effect on sex ratio. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.



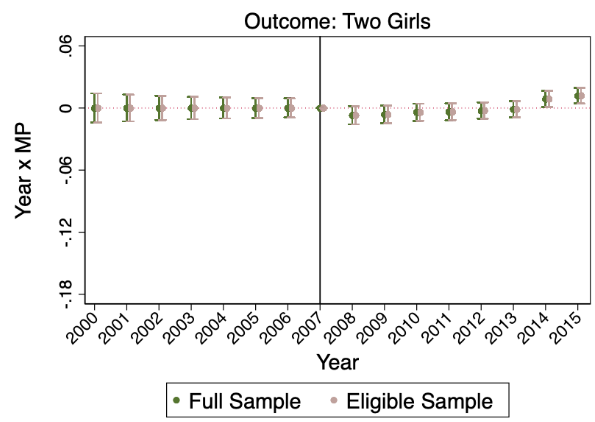

(a) Single Boy Families



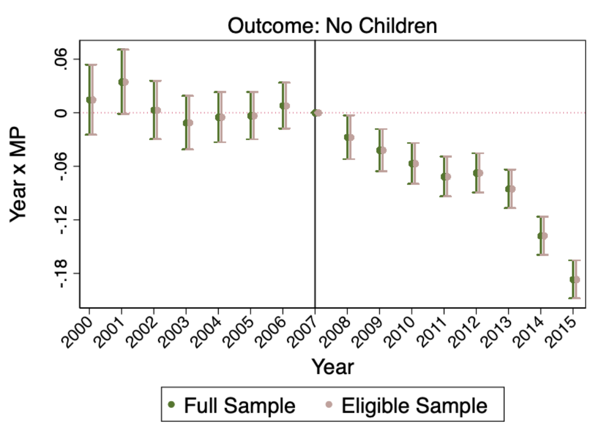

(b) Single Girl Families



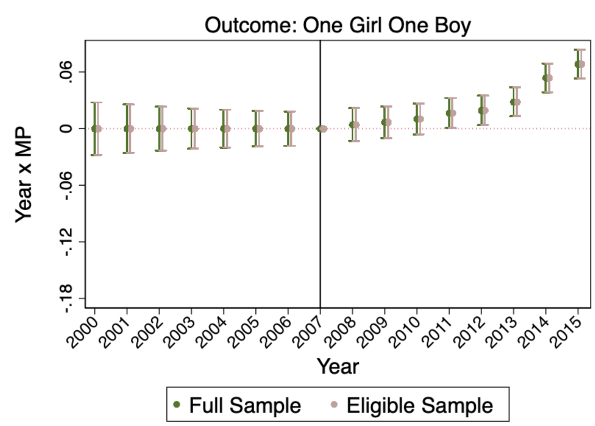

(c) Two Boys Families




(d) Two Girls Families




(e) Zero Child Families




(f) One Girl One Boy Families

Figure A2: Robustness: Child Sex Composition using 2000-2016

*Notes:* This figure present dynamic treatment effects of *Ladli Laxmi* using our difference-in-differences specification from Equation 3.2 and a larger panel from 2000-2015. Panel (a) presents the treatment effect on likelihood of families with a single boy, panel (b) presents presents the treatment effect on likelihood of families with a single girl, panel (c) presents the treatment effect on likelihood of families with two boys, panel (d) presents the treatment effect on likelihood of families with two girls, panel (e) presents the treatment effect on likelihood of families with no children, and panel (f) presents the treatment effect on likelihood of families with one girl and one boy. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.

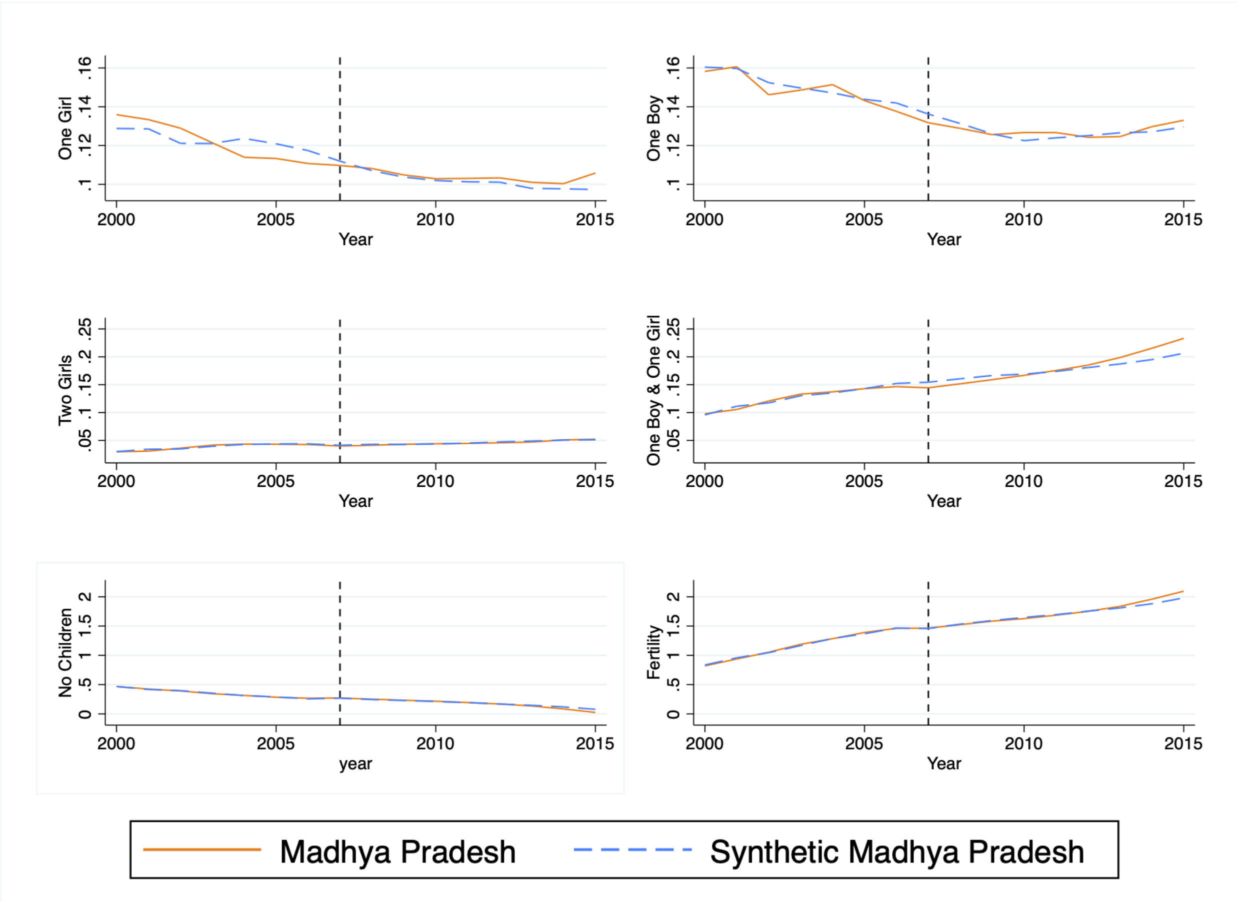



Figure A3: Synthetic Controls Method

*Notes:* This figure presents results from the synthetic controls method discussed in subsection 3.5.4.

Table A1: Flow Regressions (Birth), MP cs CG (Eligible Sample)

| | (1) No Children | (2) 1 Boy | (3) 1 Girl | (4) 2 Girls |
|---|---|---|---|---|
| *Panel A: Prob. of birth in period t* | | | | |
| MP × Post | 0.015*** | 0.058*** | 0.020 | 0.011 |
| | [0.016] | [0.019] | [0.016] | [0.019] |
| Controls | X | X | X | X |
| Observations | 10665 | 12764 | 5596 | 3381 |
| *Panel B: Prob. of male birth in period t* | | | | |
| MP × Post | 0.006 | 0.012 | 0.006 | 0.044*** |
| | [0.013] | [0.016] | [0.013] | [0.017] |
| Controls | X | X | X | X |
| Observations | 10665 | 12764 | 5596 | 3381 |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on marginal probability of any birth (Panel A) and probability of a male birth (panel B) in period $t$ conditional on various child compositions in period $t-1$. The sample consists of only eligible mothers and from years 2005 to 2008. MP denotes Madhya Pradesh. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A2: Heterogenous Effects of Age (Eligible Sample)

| | (1) Fertility | (2) Proportion of Boys |
|---|---|---|
| *Panel A: Ages 20 to 30* | | |
| MP × Post | 0.204*** | -0.029 |
| | [0.013] | [0.020] |
| Controls | X | X |
| Observations | 111264 | 70211 |
| *Panel B: Ages 31 to 40* | | |
| MP × Post | 0.039*** | -0.022 |
| | [0.017] | [0.019] |
| Controls | X | X |
| Observations | 30543 | 26270 |
| *Panel C: Ages below 20* | | |
| MP × Post | 0.557*** | 0.180** |
| | [0.043] | [0.072] |
| Controls | X | X |
| Observations | 6442 | 2726 |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio) - for three age brackets using sample of eligible mothers. Panel A presents results on mothers between 20-30 years old, panel B presents results on mothers between 31-40 years old, and panels C presents results on mothers below 20 years old. MP denotes Madhya Pradesh. *** $p < .01$, ** $p < .05$, * $p < .1$

Table A3: Heterogenous Effects by Urban/Rural Residence (Eligible Sample)

| | (1) Fertility | (2) Proportion of Boys |
|---|---|---|
| *Panel A: Urban* | | |
| MP × Post | 0.101*** | -0.050** |
| | [0.019] | [0.023] |
| Controls | X | X |
| Observations | 45800 | 33415 |
| *Panel B: Rural* | | |
| MP × Post | 0.186*** | -0.030* |
| | [0.012] | [0.016] |
| Controls | X | X |
| Observations | 99860 | 65532 |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio) - for urban and rural mothers using sample of eligible mothers. Panel A presents results on urban mothers, and panel B presents results on rural mothers. MP denotes Madhya Pradesh. *** $p$ < .01, **$p$ < .05, * $p$ < .1

Table A4: Heterogenous Effects by Religion (Eligible Sample)

| | (1) Fertility | (2) Proportion of Boys |
|---|---|---|
| *Panel A: Hindu* | | |
| MP × Post | 0.161*** | -0.041** |
| | [0.011] | [0.014] |
| Controls | X | X |
| Observations | 135629 | 91783 |
| *Panel B: Muslim* | | |
| MP × Post | 0.201*** | 0.194*** |
| | [0.063] | [0.062] |
| Controls | X | X |
| Observations | 7858 | 5531 |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio) - for two main religions using sample of eligible mothers. Panel A presents results on hindu mothers, and panel B presents results on muslim mothers. MP denotes Madhya Pradesh. *** $p < .01$, **$p < .05$, * $p < .1$

Table A5: Heterogenous Effects by Caste Type (Eligible Sample)

| | (1)<br>Fertility | (2)<br>Proportion of Boys |
|---|---|---|
| *Panel A: Scheduled Caste* | | |
| MP $\times$ Post | 0.124$^{***}$ | -0.018$^{**}$ |
| | [0.031] | [0.039] |
| Controls | X | X |
| Observations | 20237 | 13409 |
| *Panel B: Scheduled Caste* | | |
| MP $\times$ Post | 0.186$^{***}$ | -0.037 |
| | [0.019] | [0.027] |
| Controls | X | X |
| Observations | 34223 | 21935 |
| *Panel C: Oth. Backward Castes* | | |
| MP $\times$ Post | 0.161$^{***}$ | -0.047$^{**}$ |
| | [0.015] | [0.02] |
| Controls | X | X |
| Observations | 66293 | 45391 |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio) - for three caste groups using sample of eligible mothers. Panel A presents results on mothers belonging to scheduled castes, panel B presents results on mothers belonging to scheduled tribes, and panels C presents results on mothers belonging to other backward castes. MP denotes Madhya Pradesh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A6: Robustness: Fertility and Sex Ratio (MP vs. Chhattisgarh by eligibility)

| | (1) Fertility | (2) Proportion of Boys |
|---|---|---|
| *Panel A: Full Sample* | | |
| MP $\times$ Post | 0.087$^{***}$ | 0.007$^{***}$ |
| | [0.005] | [0.002] |
| Controls | X | X |
| Observations | 2433236 | 1988312 |
| *Panel B: Eligible Sample* | | |
| MP $\times$ Post | 0.135$^{***}$ | -0.027$^{***}$ |
| | [0.004] | [0.006] |
| Controls | X | X |
| Observations | 947783 | 652646 |
| *Panel C: One Boy* | | |
| MP $\times$ Post | 0.184$^{***}$ | -0.018$^{***}$ |
| | [0.007] | [0.002] |
| Controls | X | X |
| Observations | 229413 | 189073 |
| *Panel D: One Girl* | | |
| MP $\times$ Post | 0.176$^{***}$ | 0.060$^{***}$ |
| | [0.009] | [0.003] |
| Controls | X | X |
| Observations | 154759 | 125392 |
| *Panel E: No Children* | | |
| MP $\times$ Post | 0.049$^{***}$ | - |
| | [0.003] | - |
| Controls | X | - |
| Observations | 563611 | - |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on two outcome variables - fertility and proportion of boys (sex-ratio) using the sample of mothers with at most six children. Panel A presents results on entire sample, panel B presents results for the eligible sample, and panels C through E present results for the three configurations of child compositions eligible for the scheme. MP denotes Madhya Pradesh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

Table A7: Robustness: Stock Variables (MP vs. Chhattisgarh by eligibility)

| | (1)<br>No Children | (2)<br>1 Boy | (3)<br>1 Girl | (4)<br>2 Girls | (5)<br>2 Boys | (6)<br>1 Girl & 1 Boy | (7)<br>Other |
|---|---|---|---|---|---|---|---|
| *Panel A: Full Sample* | | | | | | | |
| MP × Post | -0.030*** | 0.002 | 0.004*** | -0.005*** | 0.008*** | 0.002 | 0.019*** |
| | [0.002] | [0.002] | [0.001] | [0.001] | [0.001] | [0.002] | [0.002] |
| Controls | X | X | X | X | X | X | X |
| Observations | 2433236 | 2433236 | 2433236 | 2433236 | 2433236 | 2433236 | 2433236 |
| *Panel B: Eligible Sample* | | | | | | | |
| MP × Post | -0.103*** | 0.031*** | 0.041*** | -0.005*** | 0.017*** | 0.020*** | - |
| | [0.004] | [0.003] | [0.003] | [0.001] | [0.001] | [0.002] | - |
| Controls | X | X | X | X | X | X | - |
| Observations | 947783 | 947783 | 947783 | 947783 | 947783 | 947783 | - |
| *Panel C: One Boy* | | | | | | | |
| MP × Post | -0.085*** | -0.013*** | - | - | 0.062*** | 0.037*** | - |
| | [0.005] | [0.007] | - | - | [0.005] | [0.004] | - |
| Controls | - | X | - | - | X | X | - |
| Observations | 229413 | 229413 | - | - | 229413 | 229413 | - |
| *Panel D: One Girl* | | | | | | | |
| MP × Post | -0.088*** | - | -0.0000 | -0.029*** | - | 0.118*** | - |
| | [0.006] | - | [0.009] | [0.005] | - | [0.007] | - |
| Controls | X | - | X | X | - | X | - |
| Observations | 154759 | - | 154759 | 154759 | - | 154759 | - |
| *Panel E: No Children* | | | | | | | |
| MP × Post | -0.010*** | -0.018*** | -0.012*** | 0.003*** | 0.013*** | 0.023*** | - |
| | [0.002] | [0.002] | [0.002] | [0.001] | [0.001] | [0.002] | - |
| Controls | X | X | X | X | X | X | - |
| Observations | 563611 | 563611 | 563611 | 563611 | 563611 | 563611 | - |

*Notes:* This table presents the difference-in-differences results from Equation 3.1 on several different child compositions using the sample of mothers with at most six children. Panel A presents results on entire sample, panel B presents results for the eligible sample, and panels C through E present results for the three configurations of child compositions eligible for the scheme. MP denotes Madhya Pradesh. $^{***}$ $p < .01$, $^{**}p < .05$, $^{*}$ $p < .1$

## B Dynamic Effects of *Ladli Laxmi*

Next, I present results for the dynamic effects of *Ladli Laxmi* Yojana in Table A1. In my regressions for this table, I regress a dummy variable for a birth in any period $t$ in my difference-in-differences specification conditional on households' child composition in period $t$-1. I restrict my data to just four years – three pre-policy years (2005-2007) and one post policy year (2008). I do not take years beyond 2008 as that would include fertility decisions made in year 2008 which are likely not independent of the *Ladli Laxmi* scheme. Panel A of Table A1 shows results for a dependent dummy variable for birth of any sex whereas the dependent variable in panel B takes a value equal to 1 whenever there is a male birth in period $t$. I find a 6 percentage point increase in the likelihood of giving birth to a child for couples with one boy as a result of the policy. However, among these couples, there is no change in likelihood of giving birth to a male child (Panel B). This makes sense because households who had only one child (boy) before policy were certainly eligible for benefits if they had a girl child in the period after policy. Having met their requirement for at least one boy, it is likely that these couples did not experience as high a dis-utility from a girl child as they would have without the policy. Whereas for couples with two girls in period $t$-1, I find an increase in the likelihood of a male child in period $t$ post the policy. Overall, these results are inconclusive and an alternate and better regression would include timing of the two girls' birth – whether before 2006 or after – since that would determine whether the household was eligible. I am unable to do that due to limited sample size.

## C Education Outcomes

Reducing gender gap in school enrollments has been one of the priorities of education policy in India. Governments in the developing world have used various policy tools to address the issue. On the demand side, such policies involve increasing benefits of sending girls to school in the form of conditional cash transfers as well as conditional kind transfers. Such policies have been found to be effective but costly fiszbein2009conditional, muralidharan2017cycling. On the other hand, supply side policies focus on improving access to school for example by building more schools duflo2001schooling. *Ladli Laxmi* Yojana, too, provides significant benefits towards girl's education.[1]

In this appendix, I present difference in difference results using multiple rounds of the NSS - round 62 (2005-06), round 64 (2007-08), round 66 (2009-10), round 68 (2011-12) and round 71 (2014). For my event study analysis, I estimate: (1) a double difference specification comparing enrollment for girls and boys within Madhya Pradesh and (2) a triple difference specification that compared pre-post difference between boys and girls in Madhya Pradesh with pre-post difference between boys and girls in Chhattisgarh. My treatment year in analysis for primary school age children is 2014 since only girls born after 2008 were exposed to this program. These girls reach primary school going age only in year 2014. Figures below present my event study results for primary school age children. I do not find any effect of *Ladli Laxmi* on enrollment for girls in primary school. One explanation for this could be that there is no enrollment gap between boys and girls at the primary school going age. As right panel in Figure 3.1 shows, this gap opens up only at secondary school going age.

My other strategy for analyzing education outcomes follows duflo2001schooling. I use NSS round 71 (2014) to create a treatment and control group for girls who were exposed to *Ladli Laxmi* and for girls who were born just before *Ladli Laxmi* was introduced and therefore just missed the benefits. I define my treatment group as girls aged 5-8 years and control group as girls aged 9-10 years. I then run double and triple difference regression specifications as described. My regressions confirm my findings from the event study that *Ladli Laxmi* did not lead to any significant change in likelihood of enrollment for primary school age girls.

Once more data becomes available, I hope to be able to extend the framework in duflo2001schooling to older age groups and study causal impact of *Ladli Laxmi* on schooling.

[1] section 3.2 provides detailed information about the policy.

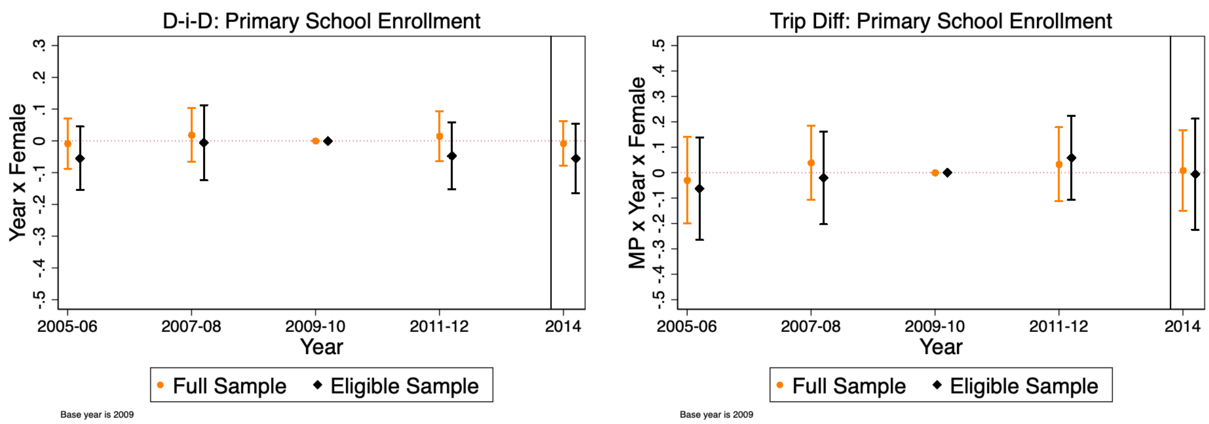



Figure A4: Education Gap by Gender

*Notes:* This figure presents results on the effect of *Ladli Laxmi* on likelihood of enrolment of the girl child using the specification described in section C. Panel A presents difference-in-difference results and panel B presents triple difference result. Each dot corresponds to an estimated coefficient, and vertical lines indicate the 95% confidence intervals. Standard errors are clustered at district level.