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ABSTRACT

Public Insurance and Demand for Private Healthcare*

Establishment of public–private partnerships is an emerging model in health care delivery. This study evaluates a pioneering social health insurance program in India that enables eligible households to access private hospitals for tertiary care services free of cost, but does not build more facilities. Leveraging policy discontinuities at state borders, we identify the program's causal effects on utilization of private facilities and associated out-of-pocket expenditures. The results indicate a pronounced substitution effect induced by relative price changes: the program substantially increases the incidence of deliveries in private hospitals while significantly reducing out-of-pocket spending. However, we find no statistically significant effects on fertility or a key health outcome, infant mortality.

JEL Classification: I13, I18, J18

Keywords: public health insurance, public-private substitution, maternal and child health

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1 Introduction

Publicly subsidized health insurance schemes with a private-public partnership (PPP) model are a popular way to improve the delivery of health and welfare services around the world. In many of these programs, low-income households are covered by health insurance that allows them to access health services at various government and private hospitals, having received a fully or partially subsidized premium (Hsiao and Shaw, 2007). However, given the actual and perceived disparity in quality between public and private facilities, it is ambiguous how such a framework would affect the demand for private healthcare when its relative price is lowered by subsidization. We address these questions in the context of a pioneering social insurance scheme in India. There are two main research questions. First, we examine whether subsidization of tertiary private care, having lowered its relative price compared to government care, increases the use of private facilities for reproductive services. Although private care is generally perceived as superior in India (Swain, 2019), recent evidence indicates a higher infant mortality rate in private compared to government health facilities in some states (Franz, 2025). Therefore, the substitution effect between public and private care after a change in relative price through subsidization is not obvious. Additionally, most private facilities are built in urban areas meaning that a large section of the population faced transportation and inconvenience costs (such as follow-up or family visits), limiting the potential use of such facilities (Debnath and Jain, 2020). Second, we examine whether the program led to improvements in key outcome variables that may have resulted from increased access to private care or a broader expansion of institutional care.

The Rajiv Aarogyasri Scheme (RAS), was introduced by the Indian state of Andhra Pradesh (AP) in 2007. At the time of its inception, the scheme was designed to provide no-cost tertiary care in public and private hospitals for low-income households up to a certain limit. In this paper we focus on reproductive health behavior, OOP costs, and infant mortality outcomes for which there exists very little evidence regarding the scheme's impacts. At 27.7 deaths per 1000 births (UNICEF, 2025), India has one of the highest infant mortality rates and from a policy perspective it is important to understand how it is affected by a major shift in the health delivery system. This assumes particular significance in the context of recent evidence pointing to a higher infant mortality rate in private compared to government health facilities in India (Franz, 2025). With more than 50%, India had one of the highest rates of OOP healthcare expenditure in the world in the first half of the 2000s (World Health Organization). High OOP may have catastrophic financial consequences, such as reducing consumption or incurring high debt levels.

We use pooled cross-section data from three waves of the District Level Household and Facility Survey (DLHS) in India. The survey collects detailed information on reproductive care, such as use of private health care, associated OOP costs, and mortality outcomes. Using birth history records, we created annual birth cohorts from 2001 to 2013 and combined it with information on the RAS rollout. The insurance program was implemented exclusively in the erstwhile state of AP, which encompassed present-day AP and Telangana; Telangana was formed in 2014 from the northwestern region of the undivided state.

Leveraging this historical event, our main empirical strategy uses a neighboring-units (or contiguous-units) methodology common in a wide range of literature (Boone et al., 2021; Muralidharan and Prakash, 2017). We start with the largest geographic units - neighboring states - as this sample provides the greatest statistical power. In this setup, we form our control group by combining the geographically-neighboring states. This is a plausible control group for several reasons. First, AP is a southeastern state that shares more cultural traits with other bordering southern states like Karnataka and Tamil Nadu and eastern states like Odisha than northern or western states. For example, Hindi is a dominant language in the rest of India but not in the southern states. Second, there is wide-ranging stylized evidence that health access and outcomes are better in southern states than in their northern counterparts (Kasthuri, 2018).

Due to the geographic and cultural proximities among these states, the geographic contiguity offers us a quasi-experiment. To bolster our strategy, we use a closer control group in our main specification, comparing each district in erstwhile AP with those districts that share a boundary with the district in AP but belong to a neighboring state. This is similar to the border-county-pair strategy common in the context of the US, where the outcomes of neighboring counties located on opposite sides of state borders are compared (Boone et al., 2021; Dube et al., 2010).

Combined with the policy timing, and the year of birth, we employ a difference-in-differences strategy to identify the effects of RAS on the residents of erstwhile AP. The first threat to this strategy is pre-existing trends. If AP had different trends in outcome variables compared to the control states in the years prior to the program, conditional on observed covariates, then any difference we see in the outcomes after the program may be a result of such differential pre-existing trends, and not a result of the program. We perform several tests to rule these out, as described in section 5.2. Another threat comes from concurrent health programs. The national government had also introduced two programs, the Janani Suraksha Yojana (JSY) and Anganwadi schemes, in 2005 and 1975, respectively, targeting maternal healthcare specifically (De and Timilsina, 2020). Although these programs were available in both the treatment and control states, their implementation could vary depending on local governance. Hence, we explicitly control for the presence of these programs in our specifications.

Our primary findings can be summarized as follows: we observe an increase in deliveries at private facilities and a decline in OOP delivery cost following the implementation of RAS for the overall sample. In more nuanced results, we find that the benefits of RAS were distributed across different users in different ways. Those who were using almost-free public healthcare before are not switching to subsidized-private care. Those who were paying out of pocket to access private care in pre-program periods are benefiting from the subsidy leading to lower OOP expenses. These results also vary based on access to hospital networks. The magnitude of the effect is close to zero for households who do not report any private or government healthcare facilities near their residence. These patterns are also observed in

OOP costs. Finally, we do not find evidence that the RAS significantly reduced infant mortality rates in the treated states. We also find no impacts on fertility - ruling out any possibility of deferred pregnancy due to program-anticipation.

We contribute to three strands of the literature. First, rigorous evaluations of pioneering social health insurance programs such as the RAS remain surprisingly limited. While a small number of descriptive studies and evaluations based on limited samples exist(Reshmi et al., 2021; Singh and Powell, 2022), quasi-experimental evaluations are rare (see Rao et al., 2014 and Reddy and Mary, 2013). One notable exception is Dupas and Jain (2024), who study a conceptually similar social health insurance program in Rajasthan; however, their analysis focuses on gender disparities in program utilization rather than on the causal impact of the program on access to care and health outcomes. Since the introduction of the RAS, several states have adopted related social insurance schemes, and the central government has launched large-scale national health insurance programs. A careful evaluation of this early initiative is therefore essential for understanding the effects of public health insurance on healthcare access and health outcomes. This study is among the first to provide such evidence.

Second, the relative roles of the public and private sectors in healthcare delivery are evolving not only in India but also globally, yet the empirical evidence remains sparse and, in many cases, dated. Existing studies suggest that this shift is influenced by supply-side factors such as public-sector quality—for example, demand for private insurance in the UK increases with the availability of senior public-sector physicians (Propper, 2001)—as well as by cultural preferences and norms (Eugster et al., 2011). Our study contributes to this literature by evaluating whether leveraging existing private-sector infrastructure, by lowering its relative price, improves healthcare access and health outcomes. Finally, we add to the literature on the impacts of insurance expansion on health outcomes, where such expansion has been based on fee-for-service model, such as the Medicaid in the Unites States (as discussed below in section 2).

In the rest of the paper, we provide a detailed background of the healthcare sector in India and the structure of the program in section 2. In section 3 we summarize the various data sources and variables used for this study. Next we turn to the identification strategy in section 4. Section 5 presents the results along with discussions on the validity of the identification assumptions. Sections 6 and 7 contextualize our findings in the broader policy discussion and conclude.

2 Background

2.1 Health Care Provisions in India

In India, publicly provided healthcare has existed since its independence from British rule. However, like most developing countries, it has suffered from overcrowding, crumbling infrastructure, staff shortages, chronic funding shortages, lack of equipment and medicines, among others (Mavalankar and Rosenfield, 2005). While the private market co-exists, the high OOP expenditure for private healthcare makes it difficult to access for poor households. The private health insurance market was underdeveloped during the time preceeding the RAS. Even when available, the high cost of insurance products mostly rendered them unaffordable for poor households. In the first half of the 2000s, less than 10% of the Indian population was covered by any form of health insurance (Ranson et al., 2007). AP was no different (see Figure 1).

In summary, for more than 50 years after India gained independence in 1947, two mutually exclusive segments of the health care system coexisted, where low-income families could only access overburdened but free inpatient and outpatient care, and families with resources to pay full price access better-attended private facilities (Powell-Jackson et al., 2015). This unequal delivery system led to substantial and rising levels of health inequity that have been widely documented (Joe et al., 2008; Balarajan et al., 2011). During the last two decades, successive state and national governments have introduced free health insurance for poor households in an attempt to increase access to healthcare. One defining feature of such a program is the PPP. Public engagement in health care has moved away from direct provision since the construction of physical infrastructure is costly and can be saddled with various issues such as bureaucratic delays, political favoritism, and budget constraints. Instead, governments at various levels have tried to implement social health insurance where low-income families receive care in private facilities at a substantial or fully subsidized price (Hooley et al., 2022). The RAS adopted this model. A trust was established to calculate insurance premiums and hospital reimbursement rates. Qualifying families received full subsidization of the premium and access to tertiary care.

2.2 Public Private Partnership and health access and outcome

Although there is no universal definition of Public-Private partnerships (PPP), the RAS is aligned to what Koppenjan (2005) defined as a structured arrangement between public and private actors for the planning, construction, and operation of infrastructure, in which risks, costs, benefits, resources, and responsibilities are allocated between the parties. Such partnerships have expanded rapidly in India and across many developing countries as a strategy to address persistent gaps in healthcare access Bhargawa and Neelima (2022). The most common form of PPP in India has been the delivery of secondary and tertiary health care through collaboration with private providers (Baru and Nundy, 2020).

However, the evidence on the impact of access to health insurance is mixed in the context of developing countries. Levine et al. (2016) do not find any impact on health outcomes or behaviors in Cambodia where free insurance was provided. But Helmsmüller and Landmann (2021) found in Pakistan that insured households more often chose private hospitals, indicating a shift towards higher perceived quality of care. In Nicaragua, individuals with insurance turned to services at covered facilities, resulting in a decrease in total out-of-pocket costs. However, there was no indication of increased healthcare usage among those who were newly insured (Thornton et al., 2010).

2.3 The RAS and related previous findings

The principal objective of the RAS was to provide health services for low income families, up to a value of Rs 2,0000 (roughly $300 at that time) per year for tertiary surgical and medical treatment of severe medical conditions, including childbirth [5]. The program was conceived against the backdrop of at least two recent developments. First, there were many reports of distressed farmers, some committing suicide due to debt traps. This unfortunate phenomenon brought the lack of healthcare access in rural AP to the fore (Ghosh, 2015). The second was a rapid proliferation of private healthcare facilities limited to urban areas (Shukla et al., 2011). Rao et al. (2012) provide a detailed description of the program. Here, we outline the salient features relevant to this study. First, private hospitals, government medical colleges, district hospitals, and area hospitals were eligible to enroll, provided that the private facilities were established chains and/or had at least 50 beds. Second, the scheme was implemented and supervised by a public-private partnership called the Aarogyasri Health Care Trust (AHCT) between state government bodies and insurance agencies Star Health and Allied Insurance. Finally, on the demand side, although the program was meant for BPL population, the eligibility cutoff was more lenient than the national definition making almost 90% of the population eligible (Debnath and Jain, 2020).

Although a few similar programs in later years emulated RAS, it was the only social health insurance program of its kind from 2007-2009. In 2008, the Rashtriya Swasthya Bima Yojana (RSBY) was launched at the national level, with cost sharing implemented between central government (75%) and respective state governments (25%). The roll out of RSBY took place over the period 2008-2010. However, the adoption of RSBY was extremely poor and the scheme was fraught with operational challenges, severely limiting its effectiveness (Rajasekhar et al., 2011). Amongst the neighboring states of AP, RSBY was introduced in 2008 in Maharashtra, in 2009 in the states of Chattisgarh and Odisha, and in the year 2010 in Karnataka. AP and its neighboring state Tamil Nadu did not introduce RSBY,

[5] Childbirth in a hospital was treated similar to other emergency procedures (Govt Schemes India).

instead introducing health insurance schemes- RAS and Chief Minister's Comprehensive Health Insurance Scheme (CMCHIS) respectively, which were financed by the state government. Despite being one of the early adopters of RSBY, Maharashtra began to replace RSBY with state government financed health insurance scheme Rajiv Gandhi Jeevandayee Arogya Yojana (RGJAY), eventually phasing out RSBY in 2014 (Thakur, 2016). While RSBY was targeted towards the BPL population, the scheme was expanded in Chattisgarh under Mukhyamantri Swasthya Bima Yojana (MSBY) to offer universal coverage (Nandi, 2017). Karnataka had a state government financed scheme- Vajpayee Arogyashree scheme (VAS), operating simultaneously in districts not yet covered by RSBY (Rajasekhar and Manjula, 2012). While Karnataka had earlier introduced another state government financed health insurance scheme, namely Yeshasvini Co-operative Farmers Health Scheme (YCFHS) in 2003, the eligibility was limited to Rural co-operative members only and is not comparable to RAS. In Table A.1 we present a brief description of these social health insurance programs which were in operation in the neighboring states of AP during our study period 2001-2013.

2.4 Access to insurance and maternal and child health outcomes

The RAS was structured in a novel way in the Indian context, where a private trust managed taxpayer funds to reimburse private hospitals and there is no direct comparison of the scheme globally (Nagulapalli and Rokkam, 2015). However, there are other instances where low-income families receive premium subsidies from the government, such as the Medicaid program in the USA. Research on Medicaid expansions has documented positive impacts on maternal and child health. Overall, the evidence suggests that Medicaid expansions increased insurance coverage among low-income pregnant women and children (Currie and Duque, 2019), contributed to reductions in infant mortality (Currie and Gruber, 1996), and were associated with improved long-term outcomes such as fewer hospitalizations in adulthood for those with longer childhood eligibility (Wherry et al., 2018). Additionally, children whose mothers gained access to antenatal coverage through Medicaid are found to have lower rates of obesity in adulthood (Miller and Wherry, 2019). Prior studies also indicate that prenatal coverage may yield both short- and long-term health benefits (Yan, 2017; Conway and Kutinova, 2006). Complementing this evidence, findings from the Mother and Infant Health Project in Ukraine—a program aimed at improving the quality of maternal healthcare—show that it led to reductions in various pregnancy complications (Nizalova and Vyshnya, 2010).

3 Data and Sample Selection

3.1 Data source and sample selection

Data for our primary analysis come from the District Level Household and Facility Survey (DLHS) and the Annual Health Survey (AHS) of India. The DLHS was conducted between 1998-99 and 2012-13 over four rounds, consisting of repeated cross-sections, while the AHS was conducted between 2010-11 and 2012-13 over three rounds. Both surveys focus on reproductive and child health. The fourth round of DLHS (DLHS-IV) and AHS can be considered complementary in nature, as the former excluded the nine states that constituted the majority of neonatal deaths, and these states were covered in the latter. We use three rounds of DLHS: DLHS-II (2002-04), DLHS-III (2007-08), DLHS-IV (2012-13), and two rounds of AHS: AHS 1st update (October 2011-April, 2012) and AHS 2nd update (November, 2012- May, 2013). We elaborate on the choice of the survey rounds later in this section. Figure 2 presents a snapshot of shares of childbirth in private and government facilities, in undivided AP across the three survey rounds during the period 2002-2013.

We obtain our main outcome and control variables from the woman's module of the questionnaires in DLHS and AHS which collect data on childbirths experienced by 'eligible' women. In DLHS-II, the eligible women are currently-married women aged 15-49, while in DLHS-III and DLHS-IV, the eligibility criteria include ever-married women in the same age range. DLHS-II provides preliminary information on all births a woman has experienced during her lifetime, whether the child is alive or deceased. It also includes detailed data on the most recent birth (live birth, stillbirth, or abortion) that occurred since January 1, 2001. The preliminary data includes whether the birth was a single or multiple birth, the sex of the child, the birth date, the woman's age at the time of birth, the child's survival status at the time of the interview, and, if the child has died, the age of the child (in days, months or years) at the time of death. In DLHS-III, this preliminary information covers all live births, stillbirths, and abortions since January 1, 2004. DLHS-IV provides similar data for all live births, stillbirths, and abortions since January 1, 2008. As in DLHS-II, DLHS-III and DLHS-IV also collect more detailed information on the last live or stillbirth that occurred within their respective reference periods [6] .

AP and Telengana are our treatment states [7], and the neighboring control states are Odisha, Chattisgarh, Tamil Nadu, Karnataka, and Maharashtra, as explained below in our empirical specification. The states of Odisha and Chattisgarh were excluded from DLHS-IV, instead being surveyed in AHS. While the AHS has three rounds- Baseline (July, 2010-March, 2011), 1st update (October, 2011-April, 2012), and 2nd update (November, 2012-May, 2013), we consider the 1st update and 2nd update to be complementary to DLHS-IV and combine with the latter [8].

We link the birth history of the eligible women with their individual specific characteristics using the woman module, and use the household module to further link to household characteristics. For supplementary analysis, we link this data to village level characteristics, such as availability of health infrastructure and services in villages, using the village module, to create our data for main estimation. The estimation sample is a repeated cross-section

[6] Reference period for a survey is the time period for which survey respondents are asked to report activities or experiences of interest (Lavrakas, 2008).

[7] The state of Telengana was formed out of the state of AP on 2 June 2014 through the Andhra Pradesh Reorganisation Act,2014

[8] the baseline survey collected information on live births, stillbirths, and abortions that occurred between January 1, 2007, and December 31, 2009. In contrast, the 1st and 2nd updates covered the periods January 1, 2010–December 31, 2010, and January 1, 2011–December 31, 2011, respectively. As such, the reference periods for the 1st and 2nd updates align more closely with the reference period of DLHS-IV, while the Baseline survey's reference period begins before that of DLHS-IV. Note that DLHS-II provides preliminary information on all births in the lifetime of an eligible woman, while DLHS-III and DLHS-IV focus on births that occurred within the respective reference periods. Accordingly, we ensure consistency by considering only those births in DLHS-II that took place during its corresponding reference period.

where the cross-section unit is a childbirth experienced by an eligible woman in the reference period and the temporal variation arises from the month and year of childbirth.

We conduct supplementary analysis using data from the National Family Health Survey (NFHS) as a robustness check. The NFHS is a nationally representative, multi-round survey conducted across India, focusing on reproductive and child health. Since its inception in 1992-93, five rounds of the survey have been completed, with the most recent round covering 2019-21. We focus on births that occurred between 2001 and 2013 to maintain consistency with our main analysis, therefore, using data from three survey rounds: Round 3 (2005-06), Round 4 (2015-16), and Round 5 (2019-21), and restrict our analysis to the treatment and control states described above. Similar to the DLHS, we construct a repeated cross-section at the childbirth level by linking birth information for eligible women to their individual- and household-level characteristics.

3.2 Variable construction

Our main outcome variables are a binary dependent variable indicating whether the relevant childbirth took place in a private or non-private facility and the logarithm of associated out-of-pocket expenses. A birth is considered to have occurred in a private facility if it took place in a private dispensary, clinic, hospital, or traditional private hospital/clinic (equals one) as opposed to delivery in any other place, i.e. government facilities, NGO-run facilities, delivery in home or workplace or on the way to hospital. OOP expenses for childbirth include transportation, hospital stay, tests, medications, expenses due to complications, and other costs that are not reimbursed.

The RAS was introduced in erstwhile undivided AP in April 2007, and its rollout was completed by July 2008. Consequently, mothers who gave birth in AP (parts of which became Telengana in 2014) after April 2007 would be able to avail the benefits of the scheme. Similarly, children born after April 2007 would be able to avail the benefits of RAS. Thus, for natal and postnatal variables, our treatment group consists of births that have taken place after April 2007 in AP and Telengana. The control group consists of all births in the neighboring states of Odisha, Chattisgarh, Tamil Nadu, Karnataka, and Maharashtra, and births in or before April 2007 in AP and Telengana. All childbirths in undivided AP or Telengana and divided AP after April 2007 are considered *treated*.

To estimate the impact of RAS on our variables of interest, we further control for characteristics that vary across households or individuals. We include measures of mother's education, their age at childbirth, marital status, husband's age, as well as birth order. At the household level, we include measures of urbanity, religion, caste, and wealth. Urbanity and religion are indicator variables for whether a household is located in an urban area and whether the household is Hindu. Caste is an indicator variable for whether the household belongs to deprived social groups, Scheduled Caste, or Scheduled Tribe. Household wealth is calculated as an index based on the common amenities that a household has access to. Mother's education is an indicator variable for whether the mother ever attended school, while marital status is an indicator variable for whether the woman is currently married. Including all control variables in our first specification (contiguous states) results in an estimation sample of 128,703 births for deliveries in private facilities and 83,023 births for (log) OOP expenses.

The sample size for OOP expenses is smaller than that for deliveries in private facilities, as the former is available only for rounds 3 and 4 of DLHS, while the latter is available for all rounds. Finally, we use additional variables depending on the specification or sensitivity checks we perform later in the paper, such as vaccination and routine treatments, parts of primary care that were not covered by RAS.

4 Estimation and Identification

The RAS was implemented in the current-day Indian states of AP and Telangana. The program was rolled out in quick succession across districts over a short span of fifteen months between April 2007 and July 2008. We assume that the entire state was treated after April, 2007 and, any child born in or after April 2007 is considered to have access to the insurance plan. Accordingly, we estimate an intent-to-treat effect of RAS, comparing the relevant outcomes across cohorts in erstwhile AP with those of the neighboring states of Odisha, Maharashtra, Chattisgarh and Tamil Nadu before and after the RAS implementation. [9]

Figure 4 shows the spatial distribution of the control and treatment regions used in this estimation. The darker shades indicate districts in the treatment state of erstwhile AP. The districts with lighter shades belong to the four neighboring states.

4.1 Difference-in-differences approach - Neighboring States

Our first specification, based on the comparison groups shown in Figure 4, is a straightforward difference-in-difference model, estimated using Equation 1.

$$\mathrm{Y}_{ist} = \beta_1 + \beta_2 \mathrm{Birth}_{i,Post} * \mathrm{AP}_{is} + \alpha_s + \tau_t + X_{ist} + u_{ist} \tag{1}$$

Equation 1 is estimated for two outcomes denoting access to private healthcare - delivery of child in private health facility and OOP expenditure incurred during the birth of child. Y is observed for each woman i, residing in state s, in the child's birth month-year t. α are the state fixed effects, τ are birth-year fixed effects, and X is a set of characteristics that vary across households or individuals. Specifically, we include an indicator for whether the household resides in the rural or urban region, household religion, household wealth[10] and years of education completed by woman i. Standard errors are clustered at the state-birth year level. β_2 is the DID estimator of interest. It shows the difference in outcome Y between

[9] This quick rollout makes it difficult to exploit the staggered implementation for identification purposes. There was a concerted effort on part of the state government to increase enrollment of households into the program so that nearly 58% of the households had RAS insurance cards by the 2012-2014 survey year of DLHS. Further, the rollout prioritized vulnerable districts even within the short span of the phased implementation. Since selection of the districts into the program was not random, the effect of RAS cannot be identified through the variations across districts.

[10] We include the number of various amenities that a household has from a fixed list of 9 amenities covered in the survey. Since different rounds of the survey have used slightly different lists of amenities, we consider the household amenities covered in DLHS-II as the benchmark.

children born in the treatment state of AP (AP_{is}) and those born in the control states, after RAS was launched in April 2007 ($\text{Birth}_{i,POST}$), after having eliminated the baseline differences in Y between the treatment and control groups based on children who were born before April 2007. Table 1 presents summary statistics of the set of covariates before introduction of RAS.

4.2 Difference-in-differences approach - Neighboring Districts

States are large and populous in India and districts in neighboring states that are farther away from the border may not be precise controls. In India, healthcare is constitutionally designated as a subject under the jurisdiction of state governments (Government of India, 1950). As a result, there are wide variations in healthcare infrastructure across various states in India. While state fixed effects eliminate time-invariant differences across states, differential growth in infrastructure could lead to differences in access to healthcare and health outcomes. High travel costs between far apart treatment and control regions could imply differences in access to healthcare infrastructure even without the RAS program.

Neighboring districts in control states, on the other hand, constitute precise controls in the quasi-experimental setting. Although women from these districts can come and seek care in hospitals in AP, they could not have the RAS card or identification (Govt Schemes India). This setting closely mimics the ideal experiment where RAS cards would be distributed randomly. Accordingly, in a second specification, we compare districts of treatment and control states that share a common border, as depicted in Figure 5, after eliminating the baseline differences between them. Although households residing in districts of control states can have different healthcare infrastructure, the shorter travel distance means that people in control districts can access healthcare infrastructure in adjacent districts belonging to the treatment state (and vice versa), but they lack the publicly provided health insurance that is only provided to residents of AP.[11]. This specification is estimated using Equation 2.

[11] RAS, like most other state level health insurance program in India, requires residency in the state as

$$\mathrm{Y}_{idst} = \beta_1 + \beta_2 \mathrm{Birth}_{i,POST} * \text{AP-District}_{ids} + \alpha_d + \tau_t + X_{idst} + u_{idst} \quad (2)$$

Here, α are the district fixed effects and the other terms are defined exactly as in Equation 1. Standard errors are clustered at the level of district-birth year and month. This is our preferred specification as provides a more precise approximate to the ideal randomization without losing the sample size or statistical power to a large extent. Table 2 presents a summary statistics of the set of covariates before introduction of Arogyasri.

Our identifying assumption in equation 2 is that any differences in the outcomes between women living in AP and neighboring states would have remained constant over time had the program not occurred. This would be violated if there are pre-existing differences in trends of Y between the treatment and control states and other concurrent programs that could affect Y differently across treatment and control states. We address these points in Section 5.2.1. We use wild-cluster bootstraps in all our specifications, to account for correlations across error terms within a district-cohort cell.

5 Results

5.1 Neighboring states and Districts

We start by estimating the baseline model presented in Equation 1. It compares all births that took place after April 2007 in erstwhile AP, to those that happened before in AP and in other adjoining states. Panel A of Table 3 reports the results. The outcome variable in Columns 1 and 3 is an indicator whether the observed child was born in a private facility. The omitted reference category includes delivery at home and government healthcare facilities.

Column 1 accounts for state and birth-year fixed effects. Column 3 additionally controls for the birth order, mother's age at the time of birth, mother's education, husband's educa-

an eligibility criteria (Anfaz and Drèze, 2025). Finding proof of residency is a complicated bureaucratic process which requires long term residence in a state (Govt Schemes India) tion, husband's age, mother's marital status, whether the household lives in a rural or urban region, the household's religion, caste, index for family wealth. The coefficient estimates are very similar across the two specifications. The difference in differences (DID) estimate indicates that children born in AP, the treatment state, were approximately 10 percentage points more likely to be delivered in a private hospital compared to being born at a government facility or at home. At the sample mean this translates to a 25% increase in utilization of private care for child-birth.

While the RAS is designed to cover the cost of utilizing private care, like any insurance, households may still face an OOP payment burden (Jain, 2021). These expenses can arise from uncovered portions of the hospital bill or from additional costs such as travel and other associated expenditures that are typically not reimbursed by insurance. Therefore, it is not immediately clear how the increased utilization of private facilities—observed in columns 1 and 3—would affect a household's out-of-pocket expenses, associated with the observed delivery. In columns 2 and 4, we report the intent to treat effect of the RAS on the logarithmic transformation of OOP expenses borne by households towards child birth. Despite the rise in private healthcare utilization, we find that the RAS led to a reduction in OOP expenses of approximately 35% at the mean.

The state fixed effects in Panel A account for baseline differences in the utilization of private healthcare and OOP expenses between AP and the neighboring states. However, the difference in utilization of private hospitals might arise from variations in healthcare infrastructure across the treatment and control states. Given its focus on healthcare, AP government might have invested in enabling private healthcare infrastructure to flourish, much more than the neighboring states. To address this challenge, in Panel B, we present the DID estimates from Equation 2 which compares outcomes across the bordering districts of neighboring states, after accounting for all time invariant differences across the districts.[12]

[12] An alternative would be to adopt a fuzzy regression discontinuity approach by comparing villages across the bordering districts. While NFHS does not provide precise village identifiers, they provide a rough identification with 5km of error margin. However, the critical constraint for us is observing the birth cohorts after the introduction of the RAS and before the introduction of similar programs in neighboring

The underlying assumption is that households living in border districts of neighboring states would be able to access the health infrastructure of AP but will not have access to the insurance cover offered by RAS. The estimates imply magnitude changes similar to what we found in Panel A. Roughly, the RAS led to a 23% increase in utilization of private institutional healthcare and a 35% fall in OOP expenses related to child birth.

Next, we conduct a range of checks to address the remaining concerns related to the causal interpretation of our estimates in Table 3.

5.2 Threats to Identification

The DID estimates in Panel B of Table 3 constitute our preferred specification. Identification in this framework relies on the assumption that there are no time-varying differences between treatment and control districts. This assumption could be violated if: (a) there are differential pre-treatment trends between treatment and control districts; (b) other contemporaneous changes coinciding with the rollout of the RAS program differentially affect treatment and control districts and are correlated with healthcare utilization and health outcomes; or (c) there are anticipation effects, whereby households delay pregnancy until after the introduction of RAS. Higher fertility post RAS could mechanically increase the share of private deliveries, given capacity constraints in public hospitals.

To address concern (a), we present a series of tests examining unconditional parallel trends between treatment and control districts. We address concern (b) in two ways. First, we test whether other health outcomes—those that depend on overall healthcare provision—changed differentially across treatment and control districts during this period. Second, we control for two flagship central government programs that targeted maternity care. With respect to concern (c), anticipation effects are less likely to be salient since RAS was a pioneering social health insurance program in India. Nevertheless, we investigate fertility decision of households to provide supporting evidence on anticipation of RAS in Section 5.6

states. The earliest round with village identifiers is 2015-2016 and the earliest cohort for whom we have the relevant information is 2011, much after the introduction of RAS.

5.2.1 Placebo and Pre-trend Tests

In Table 4 and Table A.2, we perform a formal placebo exercise to understand if the trends in the utilization of private care and OOP expenses varied between the bordering districts in AP and in the neighboring control-states even before the introduction of RAS. The analysis in Column 1 and Column 2 of Table 4 restricts to children born before the program inception, i.e till April 2007. For delivery in private facility, the earliest available birth year is 2001. Thus the time period available for placebo analysis is 2001-March 2007 [13]. We assume that the policy was implemented in 2003 and compare the utilization in private healthcare between control and treatment districts for cohorts born between 2001-2003 to those born between 2004-April 2007. This division of the placebo analysis period enables us to have 67% of births to be after the pseudo-policy year of 2003, which matches the share of post-policy births in our baseline analysis in Table 3. The results in column 1 indicate that the probability of birth at a private facility grew at similar rates between control and treatment districts. For OOP cost, the earliest available birth cohort is 2004, thus the time period available for placebo analysis is 2004-2007 March. We assume the policy was implemented in 2005 and compare the costs between control and treatment districts for cohorts born between 2004-2005 to those born between 2006-April 2007 [14]. This division of the pseudo-analysis period allows us to have an equal distribution of births before and after the pseudo-policy. The results in column 2 indicate that the trend of OOP was similar between control and treatment districts before program implementation.

In columns 1 and 2 of Table A.2 we allow the estimates to vary non-parametrically over birth-cohorts. For delivery in private facilities, the omitted birth year is 2001, while for OOP the omitted birth year is 2004. The results are re-assuring. While differential trends in utilization of private healthcare and OOP is close to zero for each year before the implementation of the program, compared to the respective base years, there is a significant

[13] We verify that our results are robust to using 2004 and 2005 as pseudo-policy year.
[14] Our results are robust to using 2006 as pseudo-policy year.

and large difference for every year after the implementation of the program. We visually represent the estimates reported in columns 1 and 2 using event-study figures. Figures 10 and 11 present the estimated time effects of the program on the respective outcome variables for the years before and after its implementation. These visual representations confirm our findings from columns 1 and 2 of Table A.2. First, we find that in the initial years following the program's launch, the utilization of private facilities increased annually, stabilizing after the fourth year. A similar trend is observed for OOP expenses. Second, the absence of treatment effects in the years preceding implementation supports the validity of the parallel trends assumption.

5.2.2 Synthetic Difference-in-differences

Although restricting the control group to neighboring districts improves the comparability of treatment and control districts in aspects other than the program, the choice may be criticized for being somewhat arbitrary. To address concerns regarding the selection of comparison units, we employ the synthetic control (SC) approach, which constructs a data-driven aggregate comparison unit for the single treated state by optimally weighting untreated states to match pre-treatment trends. Specifically, a SC unit is created based on the observable attributes of the neighboring units. The underlying assumption is that only the policy separates the synthetic control and treatment states, and any difference in outcomes between the two can be causally attributed to the policy change. Our setting is conceptually amenable to this framework, as AP is the only state where RAS was implemented.[15]

To implement the method, we first aggregate our key outcome and control variables at the state level to create state-level indicators. We cannot include OOP costs in this analysis as the question was not included in rounds before 2007 and synthetic control relies

[15] Unfortunately, data limitations prevent us from fully exploiting this methodology. The SC method requires that we have data on the treatment and the set of states that potentially contribute to the synthetic control state for several years before the treatment and on several key observed characteristics. In this case, we need information on the state GDP, population, healthcare spending, etc., to form a synthetic AP. We found no reliable source containing data for all relevant years (2001-2014) and all neighboring states.

on comparisons across pre and post 2007 waves. We focus only on delivery in private vs. non-private facilities. We use a more flexible version of the SC method - the Synthetic Difference in Differences (SDID) methodology developed in Arkhangelsky et al. (2021). This method combines the strengths of the SC method (no parallel trend assumptions needed) and the difference-in-differences method that we use (allowing some state-years to be missing). It allows for the construction of a SC group that can better match the pre-treatment trends of the treated unit (Arkhangelsky et al., 2021). The final estimand uses unit and time weights to help balance out unobserved unit-year factors alleviating bias arising out of non-randomized treatment. Consequently, SDID estimates of the effect of treatment are consistent under mild assumptions and robust.

Figure 6 plots the trend of private facility deliveries in AP as well as the weighted value of border districts of neighboring states. The figure shows that the trends of private facility deliveries in AP and synthetic (weighted) private facility deliveries in control states are parallel in the pre-treatment period, making the SDID results credible. Additionally, the effect of RAS on the dependent variable (solid vs. dashed lines) is clear as the treatment state increases more sharply after the policy passage suggesting the policy effect is large in magnitude. The weights used to average pre-treatment periods are shown as the area fills at the bottom of the figures.

The point estimates in Table 5 confirm these visual findings. We use two specifications. In the first column, where the model does not include any control variables, the policy shows an increase of 8 percentage points in the likelihood of delivery in a private facility, not very different from our baseline findings in Table 3. In column 2, we control for state-level (aggregated)predictors for infant mortality - as a measure overall state health status, family assets - as a measure of income, and urban locations - as a proxy for health infrastructure. The results indicate that despite the known data limitations, our primary finding is robust to using a different methodology.

5.2.3 Falsification Tests

Next, we conduct a falsification test by examining whether RAS had any significant impact on primary healthcare outcomes, which are not covered under the scheme. Vaccination is a common healthcare need for newborns and constitutes a primary care service. By contrast, childbirth is classified as a tertiary healthcare service and is covered by RAS. Since vaccination services are excluded from the insurance program, we do not expect RAS to affect vaccination outcomes. However, if the results in Table 3 are driven by correlated or confounding factors rather than the program itself, these factors would likely influence demand for other healthcare services, including vaccination.[16]

In column 1 of Table 6 we test if the probability that a child is fully vaccinated at the time of the survey is affected by the program. We define full vaccination as a measure of whether the child has completed three doses of polio, BCG vaccine, measles vaccine, three doses of DPT vaccine, and hepatitis B vaccine. We do not find any significant effect, even when the pre-treatment proportion of the extent of full vaccination is only 17%.

Column 2 conducts a similar test. In this case, the outcome measures whether children are more likely to be treated at a private facility if they experience illnesses that are largely considered to be in the gambit of primary healthcare - specifically, diarrhea and pneumonia. The effect of RAS on the treatment of diseases that typically do not require tertiary care is insignificant, although positive. This could indicate an imprecise measure of the outcome variable, since critical cases of diarrhea or pneumonia may still require hospitalization, in which case RAS may be used for the treatment of children in a private facility.

[16] There may be some income effects from the availability of free tertiary care. The resources freed up by the availability of free tertiary care could be used to spend on other health needs, including vaccination. However, vaccines are freely available at all government health clinics and hospitals. Hence, vaccination is unlikely to experience any positive income effects.

5.3 Sensitivity checks

In Table 7, we consider alternate specifications to address additional concerns about the validity of our results reported so far. Although border districts provide reasonable control groups, a more precise comparison group for a district in the treatment state would be a contiguous district in the neighboring state. We use the sample of border-districts to conduct a contiguous district pair analysis that compares a treatment district to only its adjacent districts, unlike in Equation 2 where the average of all districts in treatment states is compared to the average of all bordering districts in control states. By restricting comparisons to geographically contiguous districts, we minimize differences in access to health infrastructure, local labor markets, and unobserved regional characteristics that may vary at broader spatial scales. Consequently, the identifying assumption is that, absent the program, outcomes would have evolved similarly on either side of the border, and any post-treatment divergence can be attributed to the introduction of RAS. To understand the district-pair fixed effect specification, consider the following Equation 3:

$$\mathrm{Y}_{dps} = \sum_{1}^{n} \beta_i D_{P_i} + \gamma D_{\text{AP-District}} + u_{dps} \tag{3}$$

Here, there are n pairs of adjacent districts such that one of the districts in the pair belongs to the treatment state of AP, and the other belongs to a control state. One particular treatment (control) district can appear multiple times in different district-pairs if it shares its border with multiple control (treatment) districts. Then, γ is difference in Y between the treatment (AP) and control (non-AP) district in each district-pair, averaged over all district-pairs. This gives the first difference in a pair fixed effect model.

$$\mathrm{Y}_{idpst} = \beta_1 + \beta_2 \text{Birth}_{i,POST} * \text{AP-District}_{id} + \alpha_p + \tau_t + X_{idpst} + u_{idpst} \tag{4}$$

The difference in this measure computed before and after the program implementation provides the difference in differences estimate in the district-pair fixed effect model. Equation

4 outlines the difference in differences framework that we estimate. Here, α are the district-pair fixed effects.

Columns 1 and 2 report estimates from Equation 4 for delivery in private hospitals and out-of-pocket expenditures for childbirth, respectively. The resulting estimates closely mirror those reported in Panel B of Table 3, our preferred specification, and further strengthen the causal interpretation of the results.

A remaining concern, however, is the potential influence of other pre-existing programs targeting maternal and child health. In particular, during the study period, two nation-wide programs—Janani Suraksha Yojana (JSY) and the Anganwadi program—were already in operation with the aim of improving childbirth and maternal health outcomes. JSY, a conditional cash transfer scheme introduced in 2005, incentivizes institutional deliveries and also provides households with information on maternal and child healthcare (De and Timilsina, 2020). The Anganwadi program, introduced in 1975, provides nutritional support and primary healthcare services to pregnant and lactating women and to children under six years of age (Ministry of Women and Child Development, 2021). Although both are national programs, their implementation quality may vary across states.

To address this concern, we explicitly control for the incidence of JSY and Anganwadi in the study districts in Columns 3 and 4. Since information on these programs is available only for rural areas, we restrict this part of the analysis to the rural sample. The estimated effect of RAS on delivery in private hospitals remains positive and statistically significant. While the coefficient on out-of-pocket expenditures is statistically insignificant, the corresponding specification without controls for JSY and Anganwadi in the rural sample (Column 1 of Table A.13) yields similar results.

Finally, column 5 uses birth records from the National Family Health Surveys (NFHS) to conduct a similar analysis using an entirely different data. Unfortunately, the information on OOP expenses in NFHS is limited. It has OOP expense information only for 2015-16 and 2019-21, much after the implementation of RAS. Since the reference period for detailed birth information is the last five years, and information on OOP expense is collected only for the last birth in the reference period, we have OOP expense information for births in post-policy round only. In addition, the NFHS does not allow us to create continuous birth-year cohorts because of the long gap between the various rounds and birth related details being collected only for the last born child. Hence, we do not use this dataset for our main estimation.

However, this data offers an opportunity to perform a sensitivity check for our main results. Although limited in size, it is possible to observe the place of delivery for some birth cohorts both before and after the introduction of RAS. In addition to representing a different set of households, the NFHS also differs from the DLHS in terms of district coverage. As with the DLHS, not all districts are surveyed in the NFHS, but the list of included districts differs between the two surveys. The results are qualitatively similar. The main difference is that the estimated effect from the NFHS is larger in magnitude than the estimate reported in Table 3 using DLHS data. However, the standard errors are also larger, which is not surprising given the smaller sample size in the NFHS.

We present a few additional robustness checks in the appendix. Table A.3 reports DID results on a matched sample obtained by coarsened-exact-matching. As shown in Table A.1, the states of Tamil Nadu, Maharashtra, and Chattisgarh had introduced their own publicly funded health insurance in subsequent years. Thus, we further confirm the robustness of our estimates by excluding Tamil Nadu (in Table A.4), Maharashtra (in Table A.5), and Chattisgarh (in Table A.6). The results from these variations do not differ significantly from our baseline findings.

5.4 Mechanisms

The analyses in Sections 5.2 and 5.3 provide confidence that the estimated effects can be interpreted causally and indicate that the RAS increased the share of deliveries occurring in private facilities by 25% and reduced OOP expenditures by approximately 35%. The rise in private hospital utilization suggests that households are substituting toward private care, either from public facilities or from non-institutional deliveries. However, substitution from public facilities alone is unlikely to explain the observed decline in OOP expenditures, since public deliveries are provided at little or no cost and switching from free public care to only partially subsidized private care would not, by itself, reduce household spending. Instead, the decline in OOP expenditures is more plausibly driven by households that were already using private facilities prior to the introduction of RAS and now receive financial coverage under the program. To better understand the sources of this increase in private utilization, we next examine patterns of substitution across delivery settings.

5.4.1 Substitution from government care

Table 8 examines the sources of increased private healthcare utilization in greater detail. Column 1 reproduces our main estimate for comparison, where the dependent variable indicates delivery in a private healthcare facility relative to all other delivery locations. Column 2 restricts the sample to births that occurred in either private or government healthcare facilities, thereby isolating substitution within institutional care.[17] The results suggest that a substantial share of the increase in private deliveries observed in Column 1 is driven by substitution away from government facilities.

Column 3 explicitly estimates switching from government hospitals to private hospitals and indicates that RAS accounts for a meaningful share of this transition. Column 4 further shows that an even larger proportion of deliveries are shifting from public health centers to private hospitals. Together, Columns 2–4 highlight that the program primarily reallocated births within the institutional care sector, away from public facilities and toward private providers.

In contrast, Columns 5–7 examine whether RAS induced transitions from non-institutional

[17] Government healthcare facilities include primary health centers (PHCs). PHCs constitute the backbone of India's public healthcare system for primary care, particularly in rural areas. They are typically staffed by one physician and provide basic medical services, with limited capacity for minor procedures in some cases.

to institutional deliveries.[18] The estimates provide little evidence that RAS substantially increased institutional deliveries among households that previously relied on non-institutional care. A significant fraction of households continue to deliver outside institutional settings despite the availability of free public insurance. This pattern suggests that constraints such as geographic access to hospitals or other non-financial barriers may play an important role in determining program take-up. We examine this possibility in the next section.

5.4.2 Role of physical infrastructure

In general, subsidizing private care is expected to increase demand for private healthcare across households. However, the results in Table 8 show that the observed increase in the utilization of subsidized private care for childbirth is driven primarily by households that previously used government institutional facilities. This pattern suggests that reducing the price of private institutional care alone may not be sufficient to induce a response from all segments of the population. One explanation is the presence of sticky preferences or non-financial barriers. Alternatively, the cost of accessing tertiary private care may remain prohibitively high for some households, such that the subsidy does not fully offset these costs. While we cannot directly identify preferences, we investigate whether high access costs to tertiary healthcare can account for the heterogeneous responses observed in Table 8.

We proxy the cost of accessing private healthcare using proximity to hospitals. Because private hospitals are disproportionately located in urban areas (Chaudhuri and Datta, 2020), while a large share of India's population resides in rural regions, distance is likely to be an important determinant of access costs. Even with full financial coverage, reaching a private hospital in a timely manner for delivery may be infeasible for many households.

We examine this hypothesis in Figure 7 and Table A.11. A substantial portion of the aggregate effect reported in Table 3 is driven by households residing near a hospital, consistent with better physical access to healthcare infrastructure. In contrast, households living

[18] Appendix Tables A.7, A.8, A.9, and A.10 present additional comparisons across private, government, and non-institutional delivery categories.

in more remote areas—who constitute the majority of the sample, as reflected in the number of observations—exhibit markedly smaller effect sizes. These findings suggest that while the subsidy lowers the financial cost of private care, it does not fully compensate for the higher travel and access costs faced by households located far from hospitals.

5.5 Heterogeneity

In addition to geographic access, the effective cost reduction associated with accessing private healthcare under RAS may also differ across subpopulations that vary in socioeconomic or spatial vulnerability. For example, disparities in hospital accessibility between rural and urban households may substantially shape the extent to which different groups benefit from the program. We therefore examine heterogeneity in the estimated effects on private delivery and OOP expenditures. Specifically, we study heterogeneity along three dimensions: (i) gender, (ii) caste, and (iii) location (rural versus urban). Figures 8 and 9 graphically present the heterogeneous effects on private facility deliveries and OOP expenditures, respectively. Tables A.12, A.13, A.14, and A.15 report the corresponding estimates.

Overall, subpopulations with lower access to private healthcare in the pre-treatment period experience larger shifts toward private care following the introduction of free insurance. In particular, the increase in private hospital use is greater in rural areas than in urban areas. However, the reduction in OOP expenditures is substantially larger in urban areas. This pattern reinforces the mechanism discussed in Section 5.4. Households in urban areas were more likely to use private hospitals even prior to the program and therefore benefit primarily through subsidization of existing private care. By contrast, in rural areas—where baseline private hospital use was low—the program enables more households to switch to private facilities, but OOP expenditures decline less because many of these households previously relied on free public hospitals.

Columns 3 and 4 present results by social group. Scheduled Castes and Scheduled Tribes (SC–ST), historically disadvantaged groups, exhibit both a larger increase in private hospital utilization and a greater reduction in OOP expenditures compared to other social categories. One plausible explanation is program eligibility: households belonging to general castes are more likely to be economically better off and therefore less likely to qualify for coverage under RAS.

Finally, we examine whether the effect of RAS on private healthcare utilization differs by the sex of the child. Son preference is a well-documented and persistent feature of fertility and healthcare decisions in India (Chakraborty and Kim, 2010). Historically manifested through postnatal discrimination, son preference has increasingly operated at the prenatal stage with the diffusion of ultrasound technology (Pörtner, 2022; Mudur, 2002). These preferences imply that households may be more willing to incur costs—or more responsive to cost reductions—when the expected child is male. Consequently, the effective price subsidy introduced by RAS may generate stronger increases in private healthcare utilization for male births than for female births. Although prenatal sex determination has been illegal in India since the mid-1990s under the Pre-Conception and Pre-Natal Diagnostic Techniques (PNDT) Act, substantial evidence suggests continued violations, particularly outside urban areas. A government of India press release reports that in 2017 alone nearly 4,000 court cases were filed and approximately 2,000 ultrasound machines were seized across various states within a single quarter (Ministry of Health and Family Welfare , 2018).

These results imply that parents are perhaps more likely to seek private healthcare, which is perceived to be of higher quality (Powell-Jackson et al., 2015; Franz, 2025), if they expect a boy to be born compared to when they expect a girl to be born, when healthcare is costly. When healthcare is costless, they are more likely to be indifferent between using the facilities based on the expected sex of the child. In our context, this would mean that households are also more likely to use private facilities for birth of boys relative to girls at baseline. However, once RAS reduces the cost of private healthcare, households appear more likely to utilize these facilities for the birth of girls as well. We find that while RAS participation increases private healthcare utilization for all births, the point estimates are larger for female births than for male births. Notably, the reduction in OOP expenditure is greater for male births, reflecting the fact that households were already more likely to use private hospitals for male children prior to RAS, and therefore experienced a larger cost reduction following its implementation.

5.6 Health Outcomes

Beyond its direct effects on delivery location and OOP expenditures, a health insurance program may also indirectly influence fertility behavior. By lowering the OOP cost of childbirth—as documented in our main results—the RAS program can generate an income effect at the household level, which may in turn affect age-specific fertility decisions (Grossman, 2019). The relationship between household wealth and fertility, however, is theoretically and empirically ambiguous, as fertility responses may differ between women and their male partners (Becker, 1960; Schaller, 2016). Changes in age-specific fertility can also mechanically affect childhood mortality if fertility responses differ across socioeconomically advantaged and disadvantaged households (Grossman, 2019). This channel is particularly relevant in our context because the reduction in OOP expenditures under RAS is highly heterogeneous across social and economic dimensions, including child sex, social group, and urban residence. To the extent that household economic status is correlated with fertility choices, such heterogeneity could mechanically influence childhood mortality outcomes.

In addition, as documented in our main results, RAS induced substitution from government to private facilities for childbirth. If quality differs between private and public providers—as suggested by Franz (2025)—this reallocation across delivery settings could further affect childhood mortality. Given that childhood mortality is a critical indicator of child health in developing countries and a commonly used measure of overall development (WHO, 2000), it is important to examine whether RAS, through reduced OOP costs and shifts in delivery location, influenced child survival outcomes. Accordingly, we estimate the impact of RAS on infant mortality—defined as whether a child died on or before the first birthday—and on age-specific fertility, measured by the total number of live births to a woman conditional on age.

Table 9 reports the results of these estimations. Column 1 presents difference-in-differences estimates for infant mortality and shows no statistically significant effect. This finding suggests that the shift from government to private hospitals did not translate into measurable changes in overall infant mortality.

Column 2 reports difference-in-differences estimates for age-specific fertility. We find no significant effect of RAS on the total number of live births to women of a given age. Notably, the average number of live births in our sample is 2.3, close to the replacement fertility level of 2.1. This result also provides support for the absence of anticipation effects: if households had postponed pregnancies in anticipation of the program, we would expect a decline in fertility immediately prior to implementation followed by a corresponding increase afterward. We observe no such pattern in the data.

6 Discussion

A central question in health-care policy is how to structure effective PPPs. Recent reviews argue that universal health coverage requires public financing while also leveraging private provision to expand access (Reich et al., 2016; McPake and Hanson, 2016). One common modality is publicly funded, means-tested insurance schemes with nominal or zero premiums and co-payments. Yet evidence on publicly financed programs that operate through private providers remains limited, particularly in low- and middle-income settings.

This paper provides one of the first quasi-experimental evaluations of the RAS, a pioneering public–private partnership that subsidizes tertiary care in both government and private hospitals. Prior to its 2007 launch, India's health-care system was largely bifurcated: public hospitals and clinics provided free care financed by state and national governments, while private providers relied almost entirely on OOP payments. RAS altered this structure within

AP and catalyzed similar reforms across India.

Using neighboring-state borders, state-based eligibility rules, and birth-level data from multiple rounds of the DLHS and AHS, we document clear substitution toward private facilities and substantial reductions in OOP expenditures. Two mechanisms clarify these patterns. First, most of the increased private use reflects substitution away from government facilities, including public health clinics, rather than an expansion in overall institutional deliveries. Second, behavioral inertia appears shaped by constraints in physical access: proximity to hospitals strongly predicts the increase in private facility use, suggesting that infrastructure gaps limit the effects of financial subsidies on utilization.

Recent evidence underscores that even highly subsidized or near-cashless insurance schemes do not eliminate structural and behavioral barriers to care. Dupas and Jain (2024) show in Rajasthan's BSBY program that such barriers—although manifested in gender gaps—affect all patients. In these contexts, nominally free care still entails unofficial fees, travel costs, and substantial time burdens.

Public subsidization of private care was partly justified by a widespread perception that private providers offer superior quality. However, we find no statistically significant impacts on infant mortality and no spillover effects on fertility behavior. These results align with two emerging strands of evidence. First, Dieye (2025) show that community-based health insurance in Senegal produced modest gains in coverage, limited improvements in selected dimensions of delivery care, and no detectable reductions in pregnancy loss or neonatal mortality. Second, recent research suggests that private facilities may not deliver higher clinical quality. Franz (2025), examining Uttar Pradesh and Bihar, finds that public facilities—despite serving poorer patients at far lower cost—achieve substantially lower neonatal mortality rates than nearby private clinics. Our finding of increased private use without corresponding improvements in infant mortality is consistent with this pattern.

These results should be interpreted with standard caveats. The use of neighboring spatial units as control groups, although common in the literature, remains somewhat arbitrary.

Eligibility for RAS is determined by geographic location rather than individual characteristics, allowing potential misclassifications. Finally, all outcomes are self-reported. While these limitations are inherent to survey-based secondary data, the consistency of our findings across specifications strengthens our conclusions.

7 Conclusion

This study illustrates both the potential and the limits of PPP–based insurance expansions. Subsidizing access to private tertiary hospitals reduces financial burdens and alters care-seeking behavior but does not fully overcome structural and behavioral barriers, nor does it necessarily improve health outcomes. Three broader insights emerge. First, reducing financial costs alone is insufficient: gender norms, mobility constraints, and mistrust continue to shape care-seeking decisions. Second, supply-side quality is central. Without strong regulatory oversight, aligned incentives, and effective monitoring, expanded access to private providers may shift utilization patterns without improving clinical outcomes. Third, improvements in health do not automatically follow from increased private provision. For PPPs to yield meaningful health gains, they must address both demand-side constraints and quality deficiencies within the health system.

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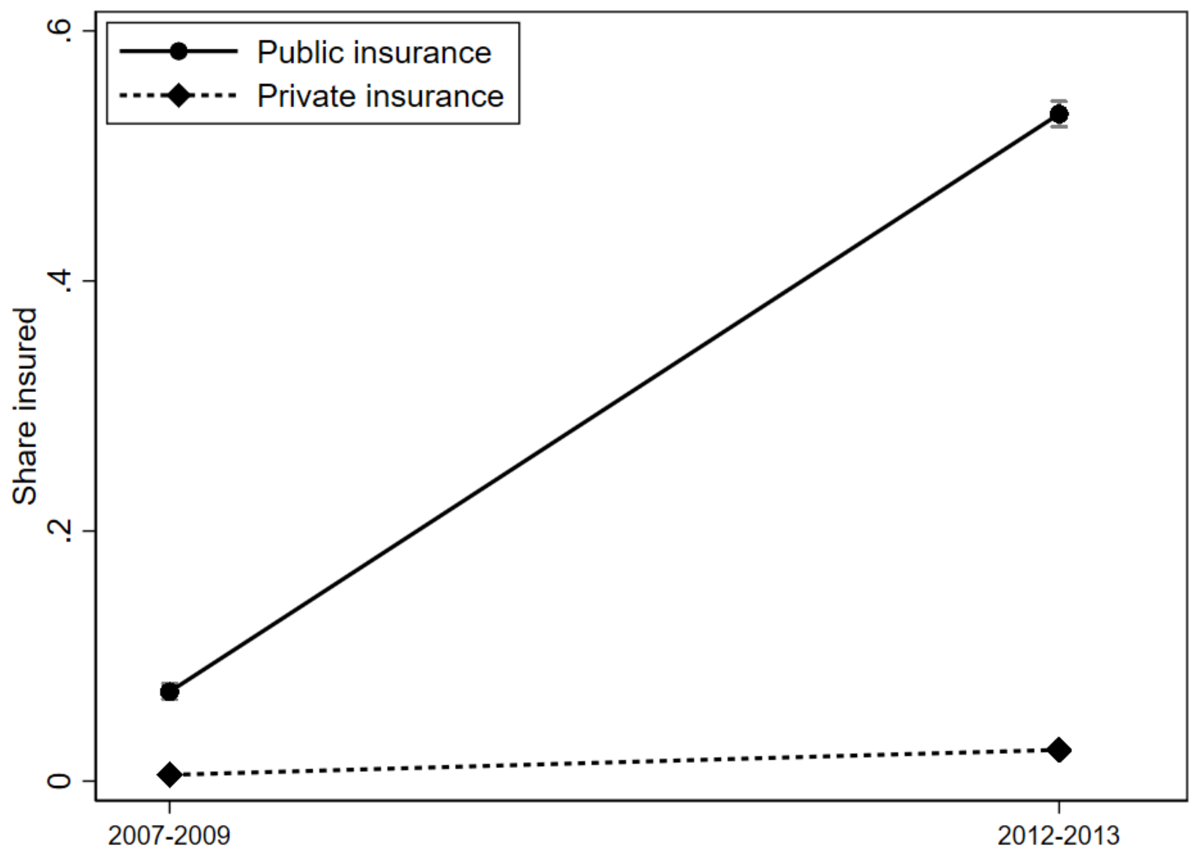
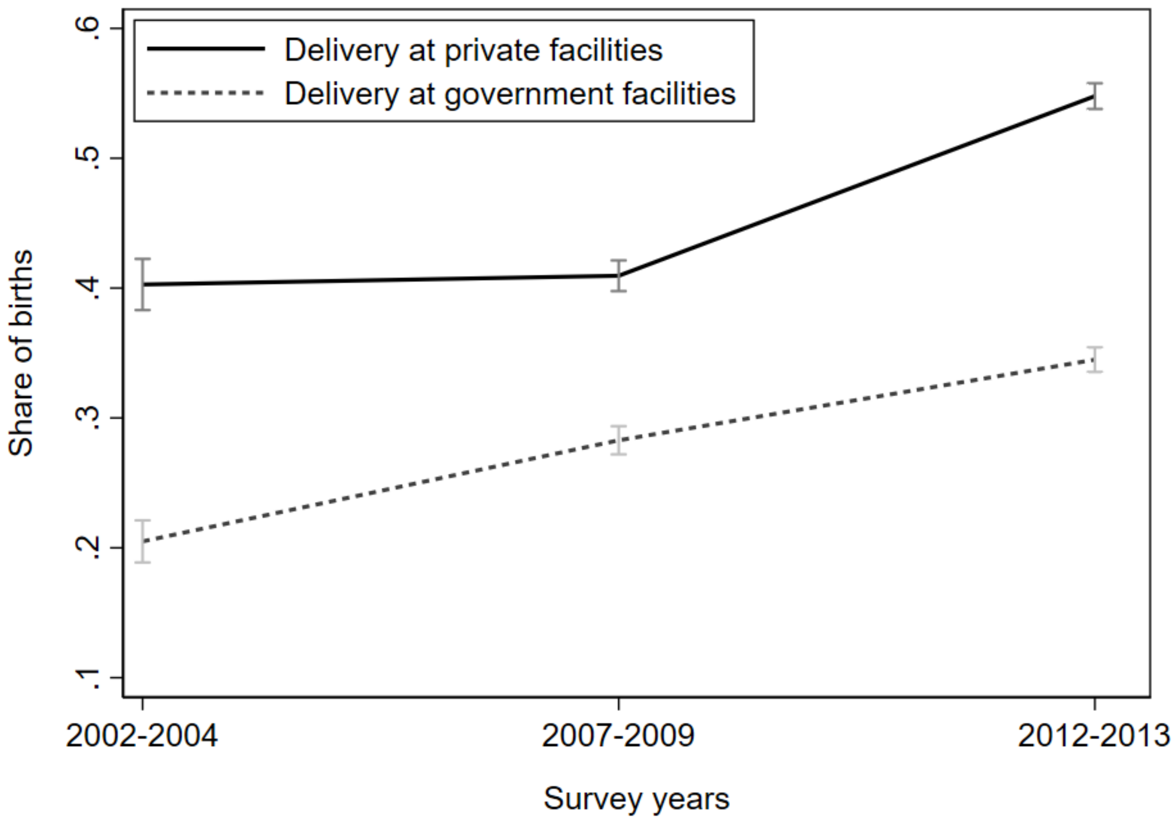
Figure 1: Trend of health insurance coverage



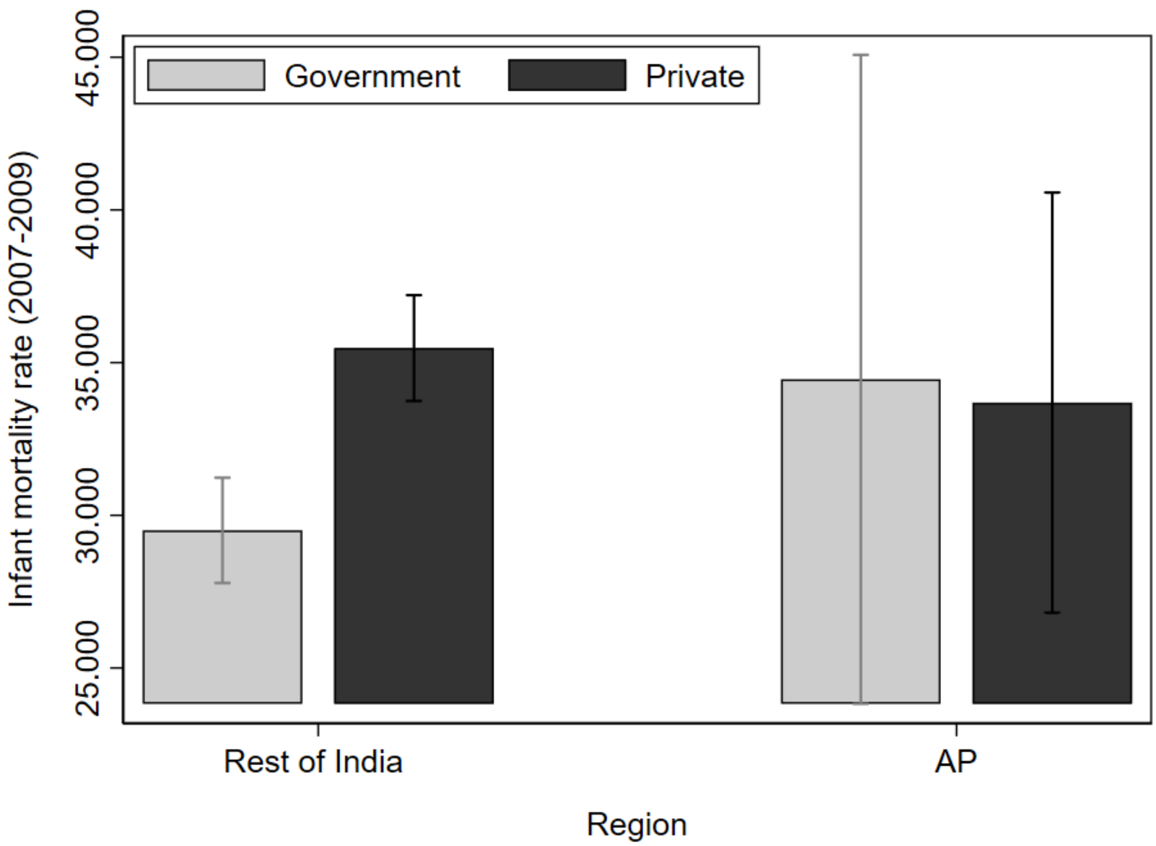
NOTES: This figure presents the coverage of public and private health insurance in undivided AP during the period 2007-2009 and 2012-2014. Further discussion located in Section 2.1. Source: Authors' own calculation from DLHS.

Figure 2: Delivery in private facilities



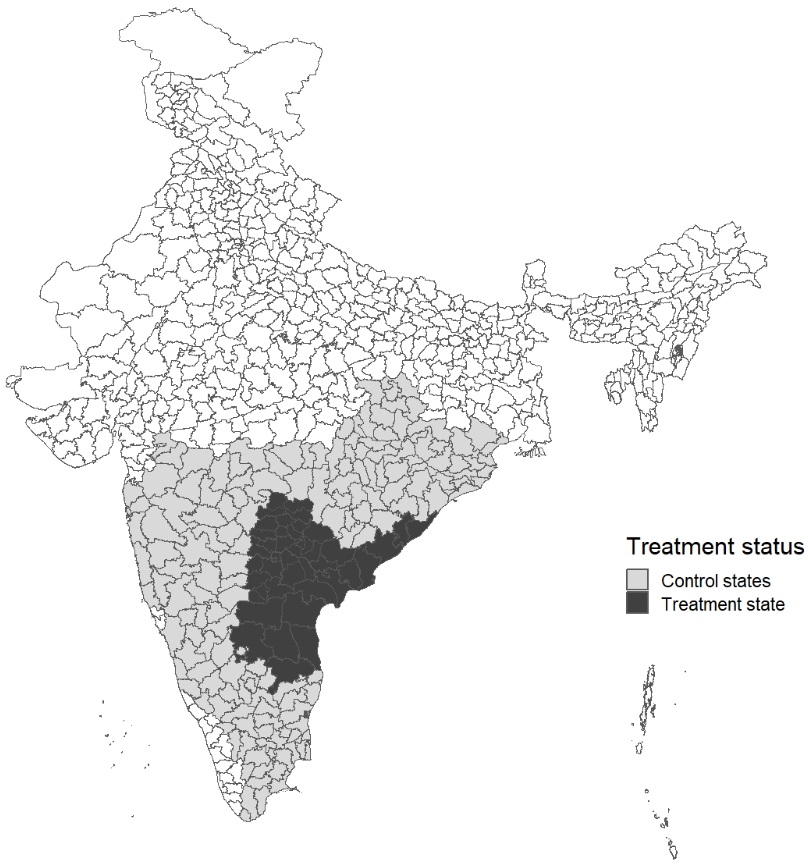
NOTES: This figure presents the shares of childbirth in private facilities and government facilities in undivided AP during the period 2002-2004 and 2012-2013. Section 3.1 refers to this figure while variable construction is discussed in Section 3.2. Source: Authors' own calculation from DLHS.

Figure 3: Infant mortality



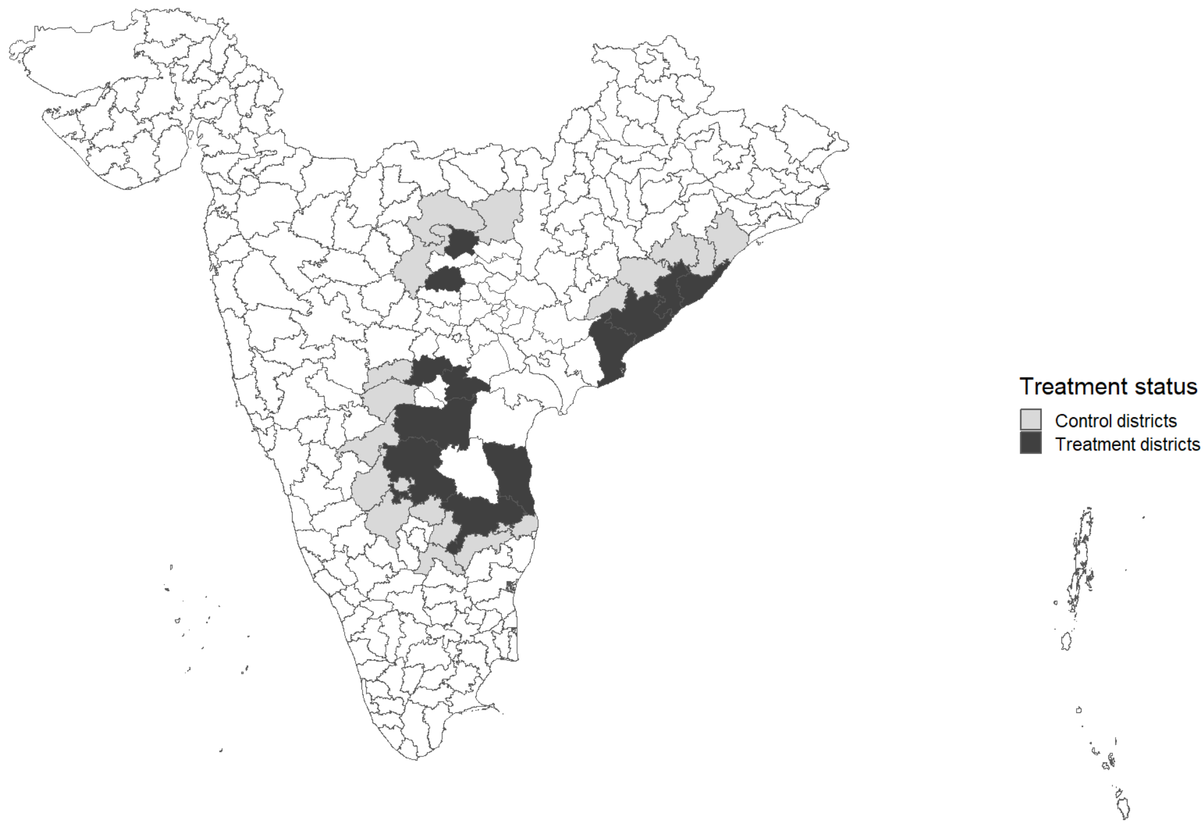
NOTES: This figure presents the infant mortality rate in private facilities and government facilities, in undivided AP and rest of India during survey year 2007-2009. Variable construction is discussed in Section 3.2. Source: Authors' own calculation from DLHS.

Figure 4: Treatment and Control areas



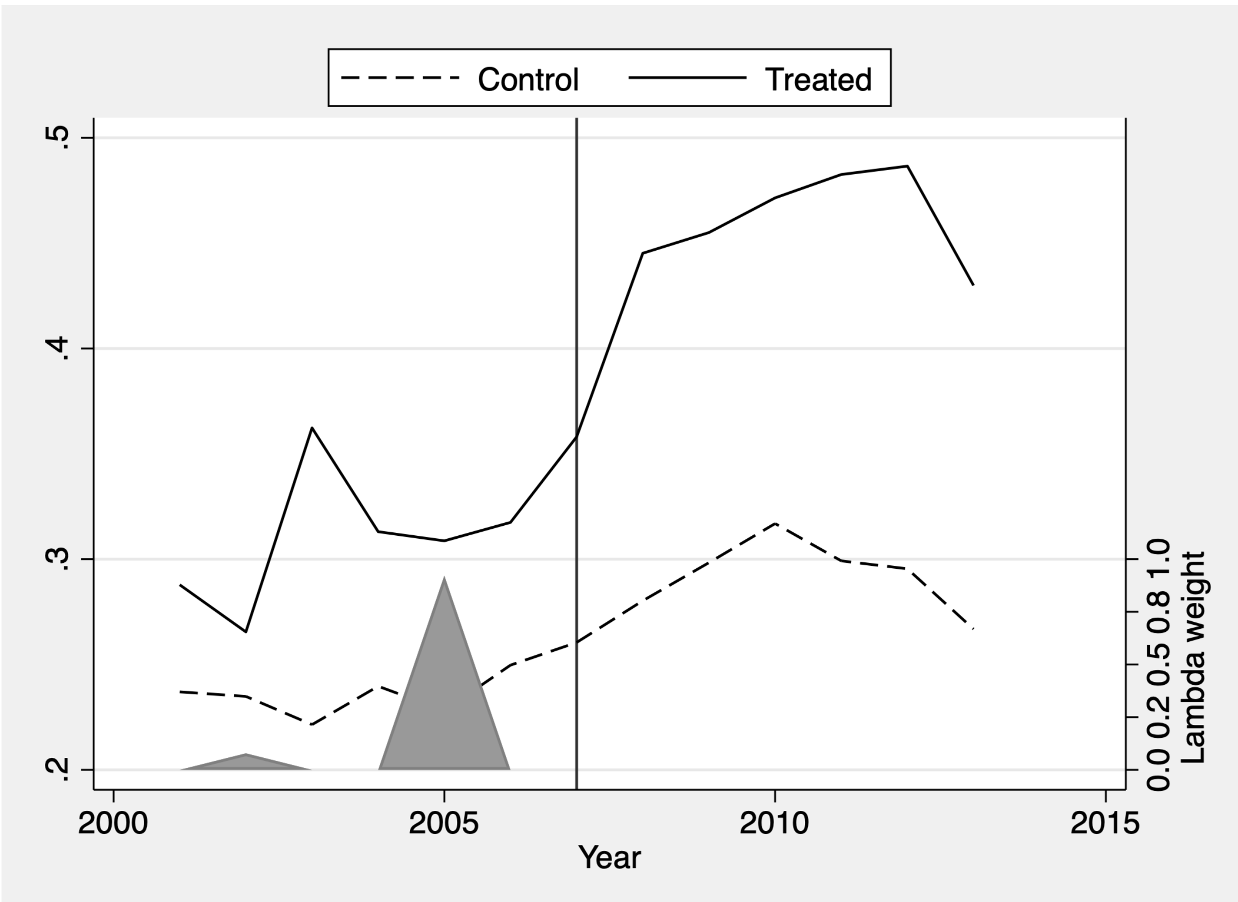
NOTES: This figure presents the district level map of the study area. The dark shaded region refers to the treatment state of undivided AP. The lighter shaded region indicates the control states of Chhattisgarh, Karnataka, Maharashtra, Odisha, and Tamil Nadu. Brief discussion can be found in Section 4, while Section 4.1 describes the estimation strategy using this division.

Figure 5: Contiguous districts of AP and Neighboring states



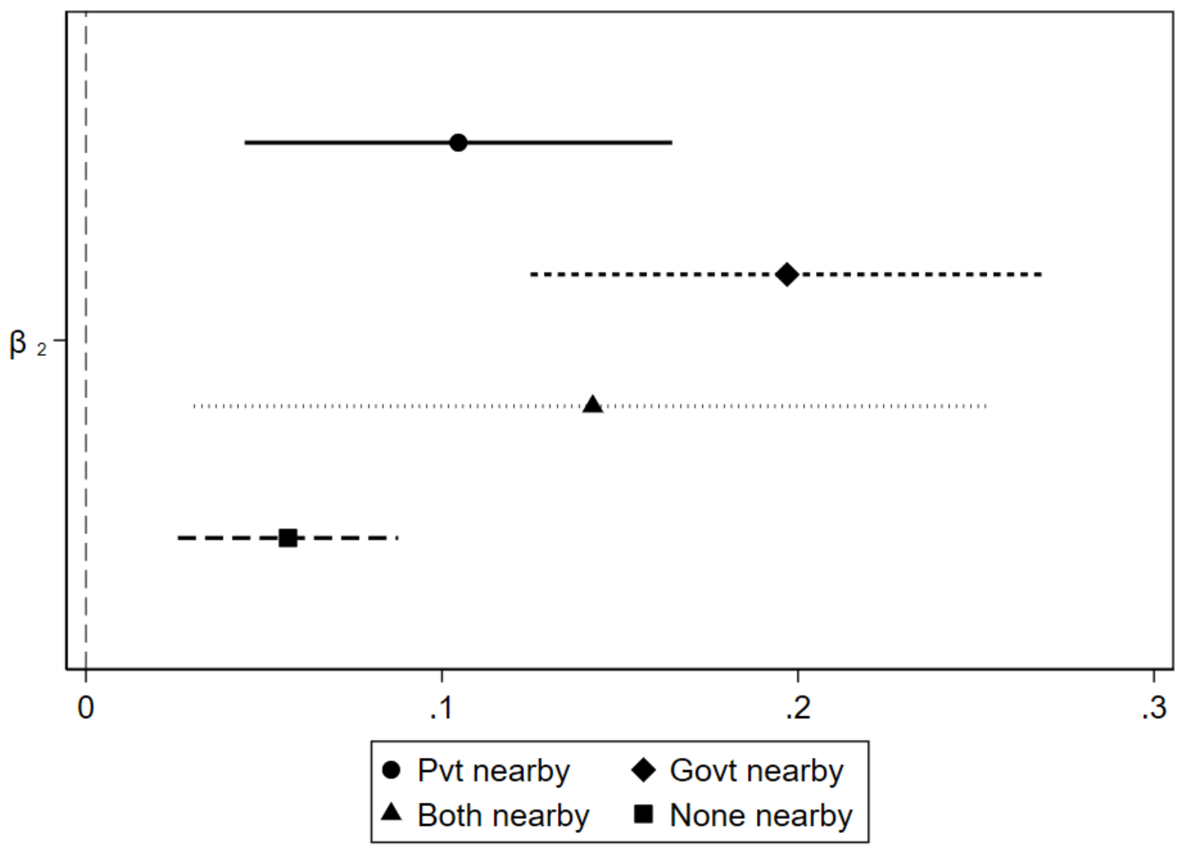
NOTES: This figure presents the district level map of the study area. The dark shaded region refers to the border districts of AP. The lighter shaded region indicates the border districts of Chhattisgarh, Karnataka, Maharashtra, Odisha, and Tamil Nadu that share state border with AP. Section 4.2 discusses the estimation strategy using this division.

Figure 6: Synthetic DID: Delivery in Private Facility



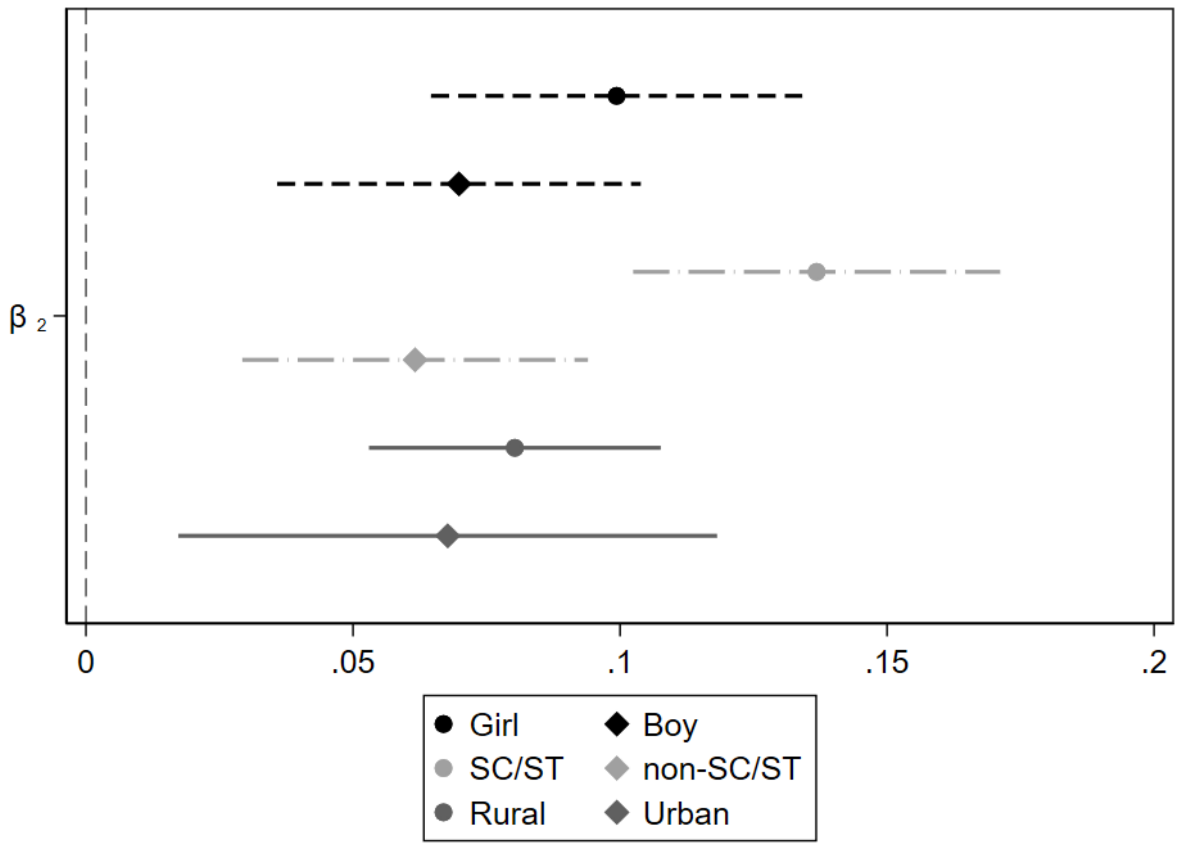
NOTES: This figure presents the Synthetic Difference-in differences analysis of effects of RAS on likelihood of delivery in a private facility. Section 5.2.2 discusses the methodology and estimates in Table 5 confirms the visual findings.

Figure 7: Mechanism: Delivery in Private Facility



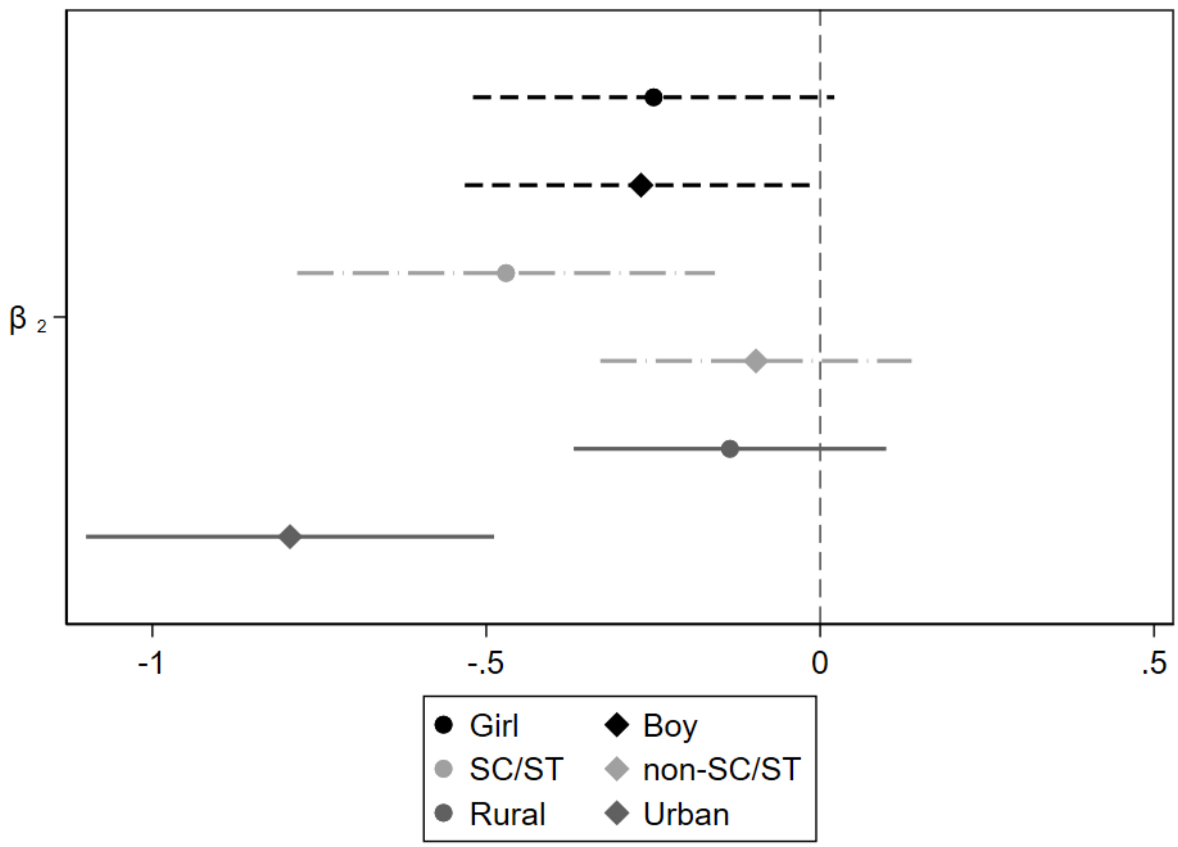
NOTES: This figure presents the heterogeneity analysis of delivery in private facility compared to govt facility with respect to proximity to types of hospital. The estimates are reported in Table A.11. Discussion is located in Section 5.4.2

Figure 8: Heterogeneity: Delivery in Private Facility



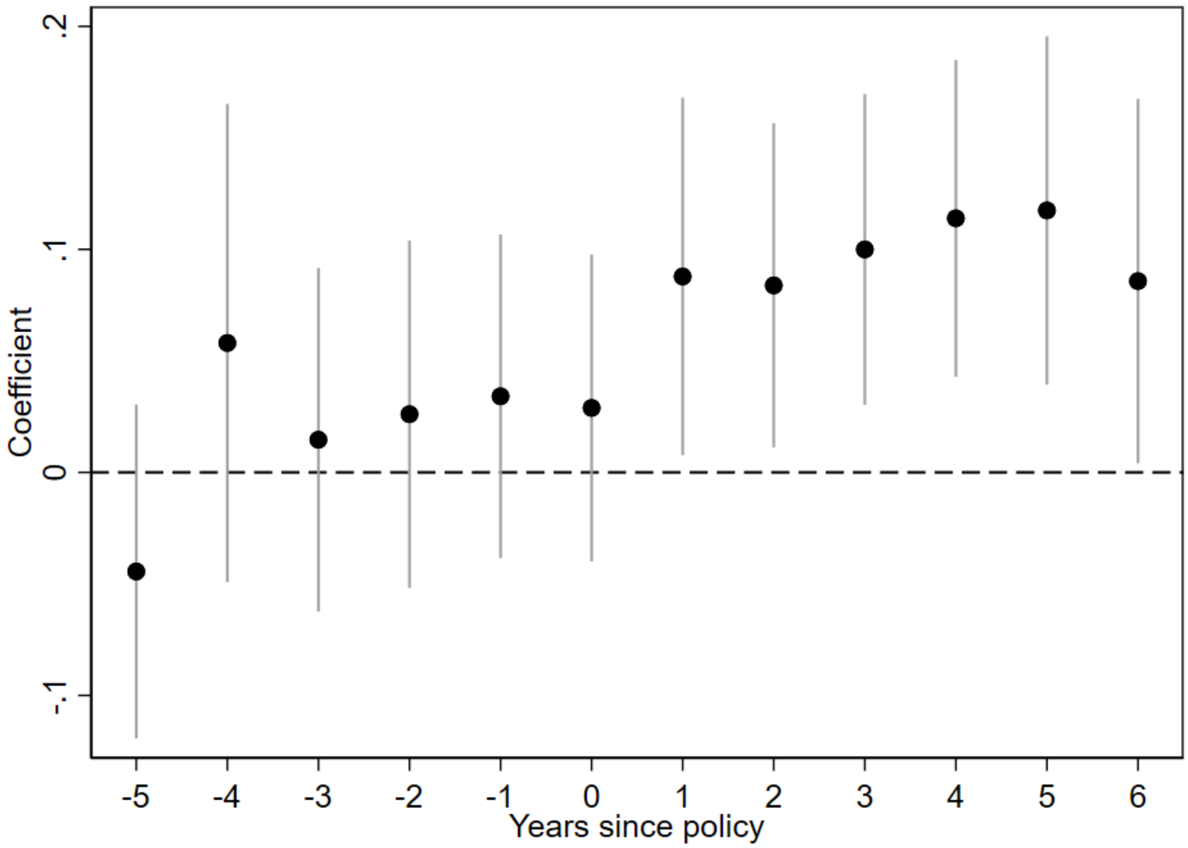
NOTES: This figure presents the heterogeneity analysis of delivery in private facility compared to govt facility with respect to sex of child, social group, and urbanity. Discussion is located in Section 5.5. Table A.12 presents the estimates.

Figure 9: Heterogeneity: (log) Out-of-pocket Delivery Expense



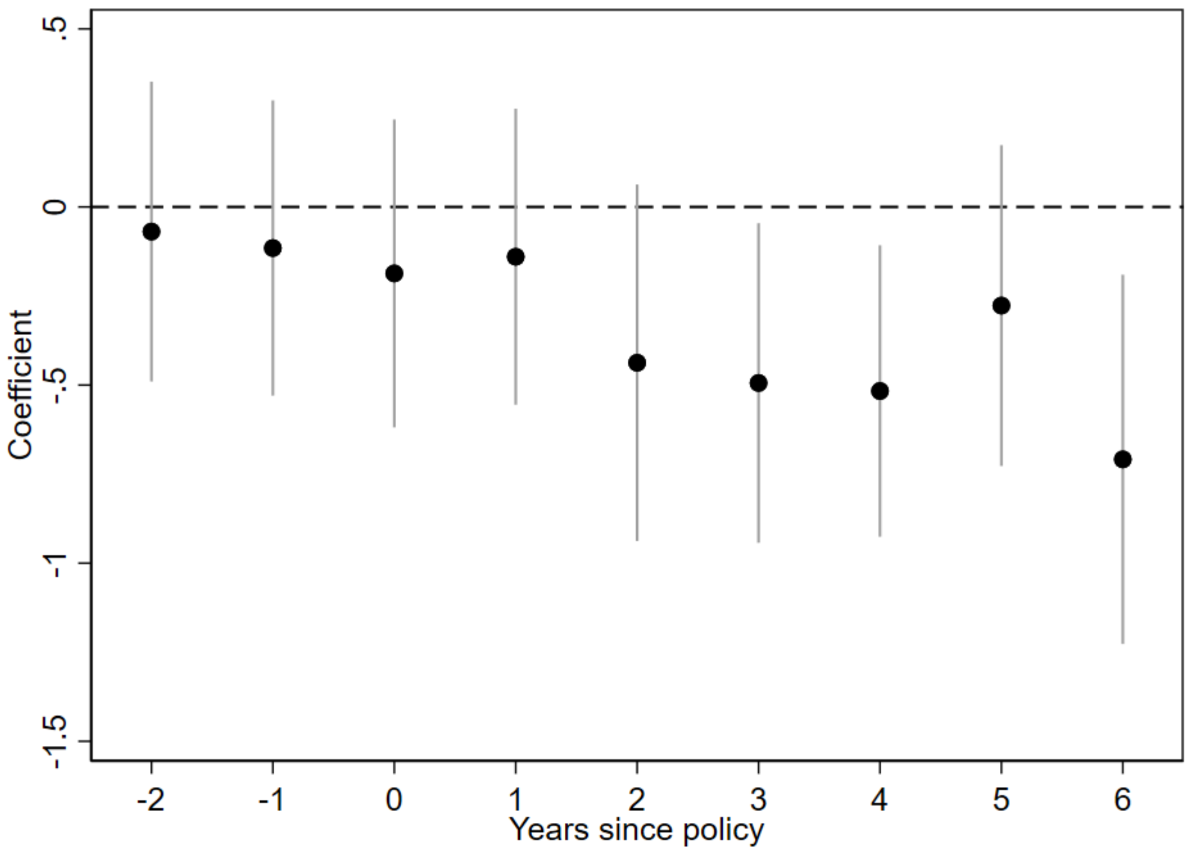
NOTES: This figure presents the heterogeneity analysis of OOP cost of delivery with respect to sex of child, social group, and urbanity.Discussion is located in Section 5.5. Table A.13 presents the estimates.

Figure 10: Event study: Delivery in private facility



NOTES: This figure presents the event-study graph of delivery in private facility compared to non-private facility. The point estimates for successive years since policy introduction are positive and significant. Discussion is located in 5.2.1.

Figure 11: Event study: Out-of-pocket expenses



NOTES: This figure presents the event-study graph of OOP cost of delivery. The point estimates for successive years since policy introduction are negative and significant. Unlike Figure 10, earliest coefficient estimates are available for two years before policy introduction. This is due to limitation of the data on OOP expenses. Discussion is located in 5.2.1.

Table 1: Pre-treatment summary statistics: neighboring states

Covariate		Treatment group mean	Control group mean	Difference
Woman characteristics				
	Mother has any schooling, %	57.39	65.20	-7.81***
	Mother 's age at birth	21.74	23.43	-1.69***
	Currently married,%	98.85	98.88	-0.03
Husband Charateristics				
	Husband has any schooling, %	68.77	77.68	-8.91***
	Husband's current age	29.50	31.69	-2.19***
Household characteristics				
	Amenities index	2.42	2.34	0.08***
	Urban,%	26.93	26.64	0.29
	Hindu,%	83.67	88.01	-4.34***
	Scheduled Caste/Scheduled Tribe,%	31.95	39.79	-7.84***

NOTES: This table presents a summary statistics of our set of covariates by comparing the means of AP and neighboring states before introduction of RAS. The covariates have been grouped in three categories: woman characteristics, husband characteristics, and household characteristics. Section 4.1 discusses the empirical strategy for this comparison and Figure 4 presents the study area.

Table 2: Pre-treatment summary statistics: border districts

Covariate		Treatment group mean	Control group mean	Difference
Woman characteristics				
	Mother has any schooling, %	52.35	53.25	-0.90
	Mother 's age at birth	21.84	23.01	-1.17***
	Currently married,%	98.71	99.16	-0.45*
Husband Charateristics				
	Husband has any schooling, %	63.57	65.45	-1.88*
	Husband's current age	29.66	31.76	-2.1***
Household characteristics				
	Amenities index	2.20	2.05	0.15***
	Urban,%	23.04	23.56	-0.52
	Hindu,%	87.30	87.21	0.09
	Scheduled Caste/Scheduled Tribe,%	32.83	49.62	-16.79***

NOTES: This table presents a summary statistics of our set of covariates by comparing the means of contiguous districts of AP and neighboring states before introduction of Arogyasri. The covariates have been grouped in three categories: woman characteristics, husband characteristics, and household characteristics. Section 4.2 discusses the empirical strategy for this comparison and Figure 5 presents the study area.

Table 3: Impact of RAS on delivery in private facilities and OOP cost of delivery: main results

Dependent variables	(1) Private Delivery	(2) OOP	(3) Private Delivery	(4) OOP
PANEL A				
Policy state X Born after	0.100***	-0.348***	0.094***	-0.363***
	(0.017)	(0.108)	(0.016)	(0.093)
Observations	212,753	89,590	128,703	83,023
R-squared	0.115	0.213	0.209	0.296
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.4	4335.875	.405	4331.833
PANEL B				
Border district X Born after	0.084***	-0.262**	0.081***	-0.278***
	(0.016)	(0.122)	(0.014)	(0.105)
Observations	25,266	16,837	23,504	15,757
R-squared	0.089	0.214	0.226	0.276
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.315	3538.527	.319	3509.647

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from estimations with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery. In panel A, the point estimates correspond to β_2 in Equation 1, where any resident of the erstwhile undivided state of AP is assumed to be treated. The study area is presented in Figure 4. Panel B, where the treatment group is restricted to only residents of the border districts, correspond to equation 2 and the study area is visually presented in Figure 5. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.1 discusses the results in this table.

Table 4: Impact of RAS on delivery in private facilities and OOP cost of delivery: Placebo analysis

Dependent variables	(1) Private Delivery	(2) OOP
Border district X Born after	0.029	-0.125
	(0.020)	(0.105)
Observations	9,769	6,051
R-squared	0.228	0.295
District FE	YES	YES
Cohort FE	YES	YES
Control	YES	YES
Baseline mean	.313	3479.806

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from pre-treatment trends analysis with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery. Column 1 presents the estimates for delivery in private facility, while column 2 presents the estimates for (log) OOP cost of delivery. Column 1 uses cohorts born between 2001-April 2007 and 2003 as the pseudo-policy year. We verify that our results are robust to using 2004 and 2005 as pseudo-policy year. Column 2 uses cohorts born between 2004-April 2007 and 2006 as the pseudo-policy year. Our results are robust to using 2006 as pseudo-policy year. All models control for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.2.1 discusses the results in this table.

Table 5: SDID: Impact of RAS on likelihood of delivery in a private facility.

	(1)	(2)
Dependent variables	Private Delivery	Private Delivery
=1 if treatment state	0.0874***	0.0733**
	(.010)	(.035)
Observations	52	52
Control	No	Yes

Robust standard errors in parentheses

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

NOTES: This table presents point estimates from Synthetic Difference-in differences analysis of effects of Arogyasri on likelihood of delivery in a private facility. Column (1) does not control for covariates, while Column (2) controls for (aggregated) state-level predictors – infant mortality as a measure overall state health status, family assets as a measure of income, and urban locations as a proxy for health infrastructure. Section 5.2.2 discusses the results in this table and Figure 6 presents the visual findings.

Table 6: Falsification tests

Dependent variables	(1) Falsification: Full vaccination	(2) Falsification: Illness treatment
Border district X Born After	-0.025	0.041
	(0.021)	(0.042)
Observations	23,504	2,767
R-squared	0.102	0.100
District FE	YES	YES
Cohort FE	YES	YES
Control	YES	YES
Baseline mean	.236	0.57

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from falsification tests. Column (1) is a regression with a binary dependent variable for whether a child has completed vaccination. We define full vaccination as a measure of whether the child has completed three doses of polio, BCG vaccine, measles vaccine, three doses of DPT vaccine, and hepatitis B vaccine. Column (2) is a regression with a binary dependent variable for whether a child has been treated for diarrhoea or fever in any private facility. These are illnesses that are typically considered to be in the gambit of primary healthcare. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, and an index for family wealth. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.2.3 discusses the results in this table.

Table 7: Sensitivity checks

Dependent variables	(1) Contiguous districts: Pvt delivery	(2) Contiguous districts: OOP	(3) SS-side policies Pvt delivery	(4) SS-side policies OOP	(5) NFHS data
Border district X Born after	0.081***	-0.278***	0.089***	-0.128	0.150***
	(0.014)	(0.105)	(0.017)	(0.135)	(0.025)
JSY			-0.014	-0.148	
			(0.015)	(0.132)	
Anganwadi			0.034**	-0.198	
			(0.016)	(0.135)	
Observations	23,504	15,757	14,004	10,850	6,495
R-squared	0.225	0.276	0.183	0.279	0.241
District FE			YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES
Contiguous-Pair FE	YES	YES			
Control	YES	YES	YES	YES	YES
Baseline mean	.319	3509.647	.266	3059.922	.17

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

NOTES: This table presents results from a series of robustness checks. Columns (1) and (2) estimate Equation 4 with binary dependent variable of whether respondent went to a private facility to give birth and (log) OOP cost of delivery. Columns (3) and (4) supplement equation 2 with local supply side conditions - availability of JSY and Anganwadi facility. Column (5) performs the main regression for delivery in private facility using the DHS-India (NFHS) data. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, and an index for family wealth. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.3 discusses the results in this table.

Table 8: Mechanism: Switch between facility types

Dependent variables	(1) Private delivery	(2) Pvt vs Govt	(3) Pvt hospital vs Govt hospital	(4) Pvt hospital vs PHC	(5) Pvt hospital vs Home	(6) Pvt vs Home	(7) Pvt vs Home/NGO
Border district X Born after	0.081***	0.073***	0.047**	0.111***	0.026	0.028	0.026
	(0.014)	(0.017)	(0.021)	(0.020)	(0.018)	(0.017)	(0.017)
Observations	23,504	16,146	11,969	8,974	13,612	13,868	13,927
R-squared	0.226	0.187	0.149	0.276	0.490	0.492	0.487
District FE	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES
Baseline mean	.319	.541	.627	.855	.447	.448	.445

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a private or non-private facility to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.1 discusses the results in this table. Appendix Tables A.7, A.8, A.9, and A.10 present additional comparisons across private, government, and non-institutional delivery categories.

Table 9: Impact of RAS on Infant mortality and live births

Dependent variables	(1) Infant mortality	(2) Total number of live births
Border district X Born after	-0.006	
	(0.005)	
Border district X Post policy		0.066
		(0.043)
Observations	23,504	66,528
R-squared	0.029	0.281
District FE	YES	YES
Cohort FE	YES	YES
Control	YES	YES
Baseline mean	.05	1.973

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from regressions with the binary dependent variable of whether a child died on or before its first birthday and the total number of live births to a woman. Column (1) controls for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order, while Column (2) controls for woman's age only. Standard errors are clustered at the district-year of birth level and reported in parentheses. Constants are not reported. Section 5.6 discusses the results in this table.

Appendix

Table A.1: Health insurance schemes in neighboring states

State	Year	Scheme	Eligibility
Chattisgarh	2009	Rashtriya Swasthya Bima Yojana (RSBY)	BPL families
	2012	Mukhyamantri Swasthya Bima Yojana (MSBY)	Universal coverage
Karnataka	2003	Yeshasvini Co-operative Farmers Health Scheme (YCFHS)	Rural co-op members
	2010	Rashtriya Swasthya Bima Yojana (RSBY)	BPL families
	2010	Vajpayee Arogyashree scheme (VAS)	BPL families
Maharashtra	2008	Rashtriya Swasthya Bima Yojana (RSBY)	BPL families
	2012	Rajiv Gandhi Jeevandayee Arogya Yojana (RGJAY)	BPL families
Odisha	2009	Rashtriya Swasthya Bima Yojana (RSBY)	BPL families
Tamil Nadu	2009	Chief Minister's Comprehensive Health Insurance Scheme (CMCHIS)	Poor, near-poor, other vulnerable

NOTES: This table presents a brief description of publicly financed health insurance schemes that were in operation during our study period 2001-2013 in the neighboring states of AP. RSBY was launched in 2008 at the national level, with cost sharing implemented between central government and respective state governments (Dwivedi and Pradhan, 2017). Amongst our sample of states, all but AP and Tamil Nadu had introduced RSBY in the period 2008-2010. In Chattisgarh, RSBY was expanded into MSBY (Nandi, 2017). In Karnataka, both RSBY and VAS were launched simultaneously, but in different districts (Rajasekhar and Manjula, 2012). Section 2.3 presents a brief discussion of the health insurance schemes. A visual representation of the neighboring states can be found in Figure 4.

Table A.2: Impact of RAS on delivery in private facilities and OOP cost of delivery: Pre-treatment trends analysis

Dependent variables	(1) Private Delivery	(2) OOP
Border district X Birth year=2002	-0.044	
	(0.038)	
Border district X Birth year=2003	0.058	
	(0.054)	
Border district X Birth year=2004	0.015	
	(0.039)	
Border district X Birth year=2005	0.026	-0.069
	(0.040)	(0.214)
Border district X Birth year=2006	0.034	-0.115
	(0.037)	(0.210)
Border district X Birth year=2007	0.029	-0.186
	(0.035)	(0.219)
Border district X Birth year=2008	0.088**	-0.140
	(0.041)	(0.211)
Border district X Birth year=2009	0.084**	-0.437*
	(0.037)	(0.254)
Border district X Birth year=2010	0.100***	-0.494**
	(0.035)	(0.228)
Border district X Birth year=2011	0.114***	-0.517**
	(0.036)	(0.208)
Border district X Birth year=2012	0.117***	-0.277
	(0.040)	(0.229)
Border district X Birth year=2013	0.086**	-0.708***
	(0.042)	(0.263)
Observations	23,504	15,757
R-squared	0.227	0.276
F-stat	1.64	0.26
District FE	YES	YES
Cohort FE	YES	YES
Control	YES	YES
Baseline mean	.319	3509.647

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from pre-treatment trends analysis with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery. Column 1 presents the estimates for delivery in private facility, while column 2 presents the estimates for (log) OOP cost of delivery. Column 1 omits cohorts born in 2001. Column 2 omits cohorts born in 2004. F-statistics for columns 1 and 2 present the results from join test of significance of pre-policy coefficients. All models control for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.2.1 discusses the results in this table.

Table A.3: Impact of RAS on delivery in private facilities and OOP cost of delivery: matched sample

Dependent variables	(1) Private Delivery	(2) OOP	(3) Private Delivery	(4) OOP
PANEL A				
Policy state X Born after	0.099***	-0.278**	0.088***	-0.332***
	(0.018)	(0.112)	(0.016)	(0.098)
Observations	100,781	65,837	98,351	64,372
R-squared	0.079	0.196	0.198	0.277
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.412	4383.827	.414	4363.391
PANEL B				
Border district X Born after	0.093***	-0.272**	0.077***	-0.365***
	(0.018)	(0.128)	(0.016)	(0.118)
Observations	19,641	13,429	19,128	13,102
R-squared	0.100	0.206	0.227	0.268
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.321	3561.66	.322	3513.634

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from estimations with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery using a matched sample. We use coarsened-exact-matching on our set of covariates- mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order, to create the matched sample. In panel A, the point estimates correspond to β_2 in Equation 1, where any resident of the erstwhile undivided state of AP is assumed to be treated. The study area is presented in Figure 4. Panel B, where the treatment group is restricted to only residents of the border districts, correspond to Equation 2 and the study area is visually presented in Figure 5. Section 5.3 briefly discusses the results in this table. We further control for our set of covariates- mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported.

Table A.4: Impact of RAS on delivery in private facilities and OOP cost of delivery: excluding Tamil Nadu

Dependent variables	(1) Private Delivery	(2) OOP	(3) Private Delivery	(4) OOP
PANEL A				
Policy state X Born after	0.082***	-0.676***	0.071***	-0.611***
	(0.018)	(0.110)	(0.016)	(0.093)
Observations	186,084	73,015	103,781	67,403
R-squared	0.126	0.280	0.224	0.364
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.4	4335.875	.405	4331.833
PANEL B				
Border district X Born after	0.068***	-0.538***	0.079***	-0.479***
	(0.016)	(0.113)	(0.014)	(0.102)
Observations	22,412	14,854	20,813	13,869
R-squared	0.101	0.250	0.235	0.314
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.315	3538.527	.319	3509.647

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from estimations with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery using a sub-set of the control states. In particular, we exclude the state of Tamil Nadu from our analysis to account for the presence of its own publicly financed health insurance scheme that was introduced in 2009. Brief information about this scheme can be found in Table A.1. In panel A, the point estimates correspond to β_2 in Equation 1, where any resident of the erstwhile undivided state of AP is assumed to be treated. The study area is presented in Figure 4. Panel B, where the treatment group is restricted to only residents of the border districts, correspond to Equation 2 and the study area is visually presented in Figure 5. Section 5.3 briefly discusses the results in this table. We further control for our set of covariates- mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported.

Table A.5: Impact of RAS on delivery in private facilities and OOP cost of delivery: excluding Maharashtra

Dependent variables	(1) Private Delivery	(2) OOP	(3) Private Delivery	(4) OOP
PANEL A				
Policy state X Born after	0.118***	-0.085	0.111***	-0.182*
	(0.018)	(0.115)	(0.016)	(0.101)
Observations	175,783	66,661	95,816	62,377
R-squared	0.131	0.223	0.228	0.303
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.4	4335.875	.405	4331.833
PANEL B				
Border district X Born after	0.086***	-0.216	0.072***	-0.285**
	(0.017)	(0.136)	(0.015)	(0.117)
Observations	22,091	14,833	20,667	13,976
R-squared	0.098	0.220	0.233	0.283
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.315	3538.527	.319	3509.647

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from estimations with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery using a sub-set of the control states. In particular, we exclude the state of Maharashtra from our analysis to account for the presence of publicly financed health insurance schemes that were introduced in 2008 and 2012. Brief information about this scheme can be found in Table A.1. In panel A, the point estimates correspond to β_2 in Equation 1, where any resident of the erstwhile undivided state of AP is assumed to be treated. The study area is presented in Figure 4. Panel B, where the treatment group is restricted to only residents of the border districts, correspond to Equation 2 and the study area is visually presented in Figure 5. Section 5.3 briefly discusses the results in this table. We further control for our set of covariates- mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported.

Table A.6: Impact of RAS on delivery in private facilities and OOP cost of delivery: excluding Chattisgarh

Dependent variables	(1) Private Delivery	(2) OOP	(3) Private Delivery	(4) OOP
PANEL A				
Policy state X Born after	0.097***	-0.365***	0.094***	-0.380***
	(0.018)	(0.108)	(0.016)	(0.093)
Observations	162,332	82,298	117,714	76,083
R-squared	0.092	0.129	0.192	0.216
State FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.4	4335.875	.405	4331.833
PANEL B				
Border district X Born after	0.084***	-0.262**	0.081***	-0.278***
	(0.016)	(0.122)	(0.014)	(0.105)
Observations	25,266	16,837	23,504	15,757
R-squared	0.089	0.214	0.226	0.276
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	NO	NO	YES	YES
Baseline mean	.315	3538.527	.319	3509.647

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

NOTES: This table presents results from estimations with a binary dependent variable for whether a respondent went to a private or non-private facility to give birth and (log) OOP cost of delivery using a sub-set of the control states. In particular, we exclude the state of Chattisgarh from our analysis to account for the presence of publicly financed health insurance schemes that were introduced in 2009 and 2012. Brief information about this scheme can be found in Table A.1. In panel A, the point estimates correspond to β_2 in Equation 1, where any resident of the erstwhile undivided state of AP is assumed to be treated. The study area is presented in Figure 4. Panel B, where the treatment group is restricted to only residents of the border districts, correspond to Equation 2 and the study area is visually presented in Figure 5. Panel B in this table is identical to that in main estimation in Table 3. This is because DLHS and AHS did not include any of the districts in Chattisgarh that share a border with AP. Section 5.3 briefly discusses the results in this table. We further control for our set of covariates- mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported.

Table A.7: Mechanism: Switch to any private facility

Dependent variables	(1) Private delivery	(2) Pvt vs Govt	(3) Pvt vs Home	(4) Pvt vs Home/NGO
Border district X Born after	0.081***	0.073***	0.028	0.026
	(0.014)	(0.017)	(0.017)	(0.017)
Observations	23,504	16,146	13,868	13,927
R-squared	0.226	0.187	0.492	0.487
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	YES	YES	YES	YES
Baseline mean	.319	.541	.448	.445

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a private facility to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.1 discusses the results in this table.

Table A.8: Mechanism: Switch to private hospital

Dependent variables	(1) Pvt hospital vs Govt hospital	(2) Pvt hospital vs PHC	(3) Pvt hospital vs Home	(4) Pvt hospital vs CHC	(5) Pvt hospital vs Govt dispensary	(6) Pvt hospital vs SC	(7) Pvt hospital vs UHC	(8) Pvt hospital vs CHC
Border district X Born after	0.047**	0.111***	0.026	0.013	-0.019***	0.004	0.075***	0.012
	(0.021)	(0.020)	(0.018)	(0.017)	(0.006)	(0.011)	(0.012)	(0.017)
Observations	11,969	8,974	13,612	7,758	6,825	6,917	7,207	7,969
R-squared	0.149	0.276	0.490	0.187	0.060	0.070	0.181	0.184
District FE	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES	YES
Baseline mean	.627	.855	.447	0.941	.996	.992	.989	0.941

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a private hospital to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.1 discusses the results in this table.

Table A.9: Mechanism: Switch to private hospital/dispensary

Dependent variables	(1) Pvt hospital/dispensary vs Govt dispensary	(2) Pvt hospital/dispensary vs PHC	(3) Pvt hospital/dispensary vs SC	(4) Pvt hospital/dispensary vs UHC	(5) Pvt hospital/dispensary vs Home
Border district X Born after	-0.019***	0.110***	0.003	0.071***	0.025
	(0.006)	(0.019)	(0.011)	(0.012)	(0.017)
Observations	7,036	9,185	7,128	7,418	13,823
R-squared	0.060	0.270	0.069	0.174	0.492
District FE	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES
Baseline mean	.996	.855	.992	.989	.447

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a private hospital/dispensary to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.1 discusses the results in this table.

Table A.10: Mechanism: Switch to any government facility

Dependent variables	(1) Govt hospital vs Home	(2) All Govt vs Home	(3) All Govt vs Home/NGO	(4) Govt vs Non-Govt	(5) CHC vs Home	(6) Govt dispensary vs Home	(7) PHC vs Home	(8) SC vs Home	(9) UHC vs Home
Border district X Born after	0.018	-0.025	-0.028	-0.118***	-0.057**	0.001	-0.122***	-0.058***	-0.159***
	(0.021)	(0.020)	(0.020)	(0.016)	(0.023)	(0.005)	(0.028)	(0.013)	(0.021)
Observations	12,075	15,996	16,055	23,504	7,864	6,931	9,080	7,023	7,313
R-squared	0.413	0.357	0.353	0.129	0.206	0.069	0.312	0.127	0.286
District FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES	YES	YES	YES
Baseline mean	.324	.407	.404	.27	.048	.003	.12	.007	.009

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a government facility to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.1 discusses the results in this table.

Table A.11: Mechanism: Role of proximity to health facility

Dependant variable: Private delivery	(1) Sample: near pvt	(2) Sample: near govt	(3) Sample: near both	(4) Sample: near none
Border district X Born after	0.132***	0.182***	0.138**	0.054***
	(0.028)	(0.039)	(0.057)	(0.016)
Observations	3,646	2,730	1,297	16,056
R-squared	0.206	0.225	0.244	0.213
District FE	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES
Control	YES	YES	YES	YES
Baseline mean	.336	.26	.314	.321

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: The table presents results from heterogeneity analysis of binary dependent variable for whether a respondent went to a private or non-private facility to give birth. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.4.2 discusses the results in this table. Visual representation of the estimates are in Figure 7.

Table A.12: Heterogeneity analysis: Delivery in private facility

Dependent variable: Private delivery	(1) Sample: Urban	(2) Sample: Rural	(3) Sample: non-SC/ST	(4) Sample: SC or ST	(5) Sample: Male child	(6) Sample: Female child
Border district X Born after	0.072**	0.088***	0.044**	0.130***	0.061***	0.101***
	(0.029)	(0.015)	(0.017)	(0.019)	(0.018)	(0.018)
Observations	7,059	16,445	13,743	9,761	12,184	11,311
R-squared	0.195	0.191	0.186	0.224	0.232	0.224
District FE	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES
Baseline mean	.486	.268	.389	.174	.343	.294

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

NOTES: This table presents results from heterogeneity analysis of delivery in private facility vs non-private facility. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.5 discusses the results in this table. Figure 8 presents the visual estimates.

Table A.13: Heterogeneity analysis: OOP cost of delivery

Dependent variable: (log) OOP	(1) Sample: Urban	(2) Sample: Rural	(3) Sample: Non-SC/ST	(4) Sample: SC or ST	(5) Sample: Male child	(6) Sample: Female child
Border district X Born after	-0.581***	-0.156	-0.100	-0.636***	-0.291**	-0.249*
	(0.150)	(0.131)	(0.127)	(0.180)	(0.137)	(0.133)
Observations	4,836	10,921	9,440	6,317	8,167	7,587
R-squared	0.189	0.277	0.172	0.345	0.262	0.297
District FE	YES	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES	YES
Control	YES	YES	YES	YES	YES	YES
Baseline mean	5280.322	3055.473	4117.704	2427.304	3592.742	3424.432

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from heterogeneity analysis of (log) OOP cost of delivery. The columns present results for the specific sub-sample indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.5 discusses the results in this table. Figure 9 presents the visual findings.

Table A.14: Heterogeneity analysis: Delivery in private facility

Dependent variable: Private delivery	(1) Girl child vs Boy child	(2) SC/ST vs Non-SC/ST	(3) Rural vs Urban
Border district X Born after X Girl child	0.094***		
	(0.017)		
Border district X Born after X Rural			0.093***
			(0.016)
Border district X Born after X SC/ST		0.151***	
		(0.020)	
Observations	23,495	23,504	23,504
R-squared	0.226	0.227	0.227
District FE	YES	YES	YES
Cohort FE	YES	YES	YES
Control	YES	YES	YES
Baseline mean	.319	.319	.319

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

NOTES: This table presents results from heterogeneity analysis of delivery in private facility vs non-private facility. The columns present results for the specific comparison indicated. All models control for the mother's age at birth, urbanity, religion, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.5 discusses the results in this table. Figure 8 presents the visual estimates.

Table A.15: Heterogeneity analysis: OOP cost of delivery

Dependent variable: (log) OOP	(1) Girl child vs Boy child	(2) SC/ST vs Non-SC/ST	(3) Rural vs Urban
Border district X Born after X Girl child	-0.188		
	(0.128)		
Border district X Born after X Rural			-0.175
			(0.133)
Border district X Born after X SC/ST		-0.541***	
		(0.196)	
Observations	15,754	15,757	15,757
R-squared	0.276	0.277	0.276
District FE	YES	YES	YES
Cohort FE	YES	YES	YES
Control	YES	YES	YES
Baseline mean	3509.647	3509.647	3509.647

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

NOTES: This table presents results from heterogeneity analysis of (log) OOP cost of delivery. The columns present results for the specific comparison indicated. All models control for the mother's age at birth, urbanity, religion, caste, mother's education, husband's education, husband's age, mother's marital status, index for family wealth, and birth order. Baseline mean is reported for OOP in INR. Standard errors, clustered at the district-cohort level, are reported in parentheses. Constants are not reported. Section 5.5 discusses the results in this table. Figure 9 presents the visual findings.