Health Insurance and Infant Mortality: Evidence from India

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Abstract

In this paper, we estimate the causal impact of health insurance in reducing child mortality using exogeneous variation provided by the random rollout of a national-level at-scale public health insurance (RSBY) programme from India. Our data comes from two novel sources that have not been explored in the literature– 1. A unique administrative dataset that provides district level information on the programme's rollout, 2. The National Family Health Survey of 2015-16 which captures birth information and health service usage of around 300,000 children across the country. Using a difference in differences approach, we compare child mortality outcomes between cohorts who received the programme and those who did not, both within and across districts between 2010 and 2015. We find robust evidence that RSBY exposure reduces infant mortality by 1.8 per 1000 births and under-2 mortality rates by 3.4 per 1000 births. This translates into a mortality decline of 5 to 10 percent, thus saving close to 145,000 children annually. We also see that families in the lowest income quintile, girl children and children of higher birth parity experience the largest reductions in mortality rates. This finding is crucial for policy makers in countries like India where child deaths commonly happen when income constraints force poor households to either forgo access to child health care or trade-off health among their children. We show that access to RSBY also increases health investments in ante-natal and post-natal care for mothers. Hospital births and immunization rates of children increase in districts covered by RSBY. These, we propose, are the mechanisms that improve a child's health both in-utero and post birth and explain the reductions in mortality. Overall, our paper shows that health insurance, when implemented at-scale, leads to significant reductions in child mortality even in resource poor contexts like India.

Keywords: Health Insurance, Child Mortality, Reproductive and Child health, India

JEL Codes: I1, I13, I3, N35

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Introduction

In recent times many developing countries have made significant health investments towards reducing infant mortality rates (IMR). Low and middle income countries are leading the way in terms of IMR reductions, with declines of around 25 percentage points over the last decade.[1] Despite these improvements, there are about nine million (under-5) child deaths in developing countries every year. A populus like country like India accounts for around half of these deaths. Estimates suggest that nearly one-fourth of these deaths can be prevented by increasing access to reproductive health services for pregnant mothers, while an additional 40 percent by improving access to vaccines and other health care services that reduce diarrhea, pneumonia, tetanus, measles outbreaks.[2] The irony is that although the low cost technology and adequate health information exists to prevent such deaths exists, access to the same has been inequitable. Developing country populace, such as those in Asia and Sub-Saharan Africa, often forgo access to health services since they do not have the means to afford such life-saving technologies. In order to overcome this major affordability barrier, governments in developing countries have been making subsidized health insurance available especially among their poor and vulnerable populations over the last few decades.

The literature from across the world has show that the provision of health insurance increases access to health care (Dow & Schmeer, 2003; Chen & Jin, 2012; Camacho & Conover, 2013; Palmer, Mitra, Mont, & Groce, 2014; Fitzpatrick & Thornton, 2017; Aiyar & Venugopal, 2019). However evidence around it's impact on faciliating health improvements has not been as straight forward. In the area of child health, studies have produced mixed results. For example, evidence from Thailand, Taiwan, Mexico & United States strongly suggests that providing affordable health insurance reduces child mortality (Chou, Grossman, & Liu, 2014; Currie & Gruber, 1996; Goodman-Bacon, 2018b; Gruber, Hendren, & Townsend, 2014; Pfutze, 2014). However other studies based on data from China, Cost Rica & Colombia have shown limited effects on child mortality (Camacho & Conover, 2013; Chen & Jin, 2012; Dow & Schmeer, 2003). Embedded in this confusion is the lack of evidence on which channels drive gains made from having health insurance. Hence our understanding on why or how health insurance works in improving child health in some contexts and not in others has been limited.

[1] World bank data bank: https://data.worldbank.org/indicator/SP.DYN.IMRT.IN?locations=XO&view=chart
[2] WHO Website: https://www.who.int/pmnch/media/press_materials/fs/fs_mdg4_childmortality/en/

In this paper, we close this gap in two ways. First, we provide robust causal evidence that an at-scale national-level health insurance programme, provided at a very low cost, can bring reductions in child mortality. These health gains take place when health insurance effectively lowers affordability barriers for previously vulnerable groups. We also show that gains are larger in areas where there is a better provision of health services. Alternatively said, if the coverage is low or there is inadequate hospital coverage, gains from having health insurance can be low. This, we propose, sheds light on why health insurance has been effective in some countries and not in others. Second, we show that having health insurance is extremely effective in nudging households to increase health services usage, that improves a child's health, through two channels. Reproductive health services such as ante-natal visits (ANC), hospital deliveries and post-natal visits (PNC) are extremely crucial to diagnose and treat health issues of pregnant mothers. Health investments made through this indirect channel is known to improve early childhood health outcomes. We show that demand for these services increase when health insurance is introduced. Additionally, post-birth, health insurance stimulates demand for life-saving technologies such as vaccinations for children. These health investments are known to contribute to greater child health. Health investments through both these channels drive reductions in child mortality outcomes.[3]

In order to estimate the causal impact of health insurance scheme on child mortality and the corresponding channels, we focus our attention on India. In India, more than 0.8 million children still die before their first birthday, annually. The major causes of child deaths are pre-term deaths (29 percent). This is followed by death from health complications that develop post birth, such as pneumonia (14 percent), diarrhea (9 percent) and sepsis (8 percent).[4] Nearly all these deaths can be prevented by improving access to reproductive health services for mothers and increasing vaccination coverage of children. However, last mile challenges in improving access include lack of affordability of such services and low health service coverage. The average out of pocket health spending in India was around 65 percent of total health spending in 2019. The WHO estimated that an additional 600,000 doctors and 2 million nurses were needed to move India towards achieving their recommended doctor patient ratio of 1:1,000.[5] In order to overcome the problem of affordability, a national-level at scale health insurance programme was introduced in India in

[3] In appendix A, we provide some intuition on how these channels work using the Grossman health production function.
[4] UNICEF 2018
[5] https://economictimes.indiatimes.com/industry/healthcare/biotech/healthcare/india-facing-shortage-of-600000-doctors-2-million-nurses-study/articleshow/68875822.cms?from=mdr

2008. The Rashtriya Swasthya Bima Yojana (RSBY), provided families with Rs 30,000 worth of health coverage at an annual fee of just Rs 30. Thus, for less than 1 percent of the annual income guarantee of Rs 10,000, individuals could use secondary and tertiary health services worth three times this minimum income guarantee.

After it was announceed, between 2008 and 2015, the programme was rolled out in two-thirds of all districts across India. By 2016, this programme had covered 40 million families and 150 million individuals across the country (Karan et al 2017). In our methdology, we exploit the the spatial and temporal variation in its implementation in a difference-in-differences (DID) framework to estimate the causal of having health insurance on child mortality. We compare health outcomes and health investments of children who benefitted from the programme with those of children who were not exposed to this policy. The control group consists of children born before the policy was effective in a treatment district and children born in districts that never received RSBY. We also use this DID framework to identify which groups benefited and what channels determined the health gains that we find.

Our data comes from two novel datasets. The rollout data comes from a unique government maintained administrative dataset that collated district-level information on the RSBY's implementation. This dataset has not been used in prior evaluations of the impact of the RSBY on households. Our health and socio-economic data comes from the National Family Health Survey IV (NFHS) conducted in the year 2015-16.[6] This dataset provides the latest and most comprehensive health dataset and is representative at the district level. The dataset was made available for public use at the end of 2018 and hence research on child mortality from this dataset has been limited this far. Our identification strategy uses information on the start date of the programme in a district from the administrative data to assign policy exposure to children from the NFHS IV. We find causal evidence that after RSBY is implemented in a district, the IMR falls by an average of 5 percent a year. That is, an additional 1.8 infant deaths (per 1000 births) were averted in RSBY regions, as compared to cohorts that did not have access to this policy. We also find that by age two, mortality falls by an average of 3.4 deaths (per 1000 births), which translates to a 10 percent reduction in India's average under-2 mortality rate (U2MR).[7] These results are in line with findings from other contexts such as Taiwan & Thailand where studies have found a 10 to 20 percent

[6] This survey is more popularly known as the Demographic and Health Survey (DHS) across the world.
[7] In standard deviation terms, this translates to a 0.01SD and 0.02SD reduction in child mortality within the first and second year of her life.

reduction in IMR in response to the introduction of health insurance (Chou et al., 2014; Gruber et al., 2014; Pfutze, 2014).

Our confidence that the estimates presented are causal comes from their robustness to multiple checks that we implement. We show that there are no pre-trends in the data that suggest that treated and untreated districts are somehow different prior to the implementation of the RSBY. Additionally, prior to the intervention, we also see that there are no anticipatory effects. All the changes take effect after the programme is introduced in a district and not before. This is true for all districts across the country. We are able to show that there are no fertility responses that drive these changes. We can rule out concerns that there are household-level unobservables that drive selection into this programme since the point estimates are robust to estimation at the district level. We show that other flagship programmes that could explain our proposed mechanisms such as the Janani Suraksha Yojana were already available across the country prior to the RSBY's implementation. Hence we are not concerned that our effects are driven by another programme. We can also eliminate concerns that our estimates are impacted by a sample weighting issue that plagues staggered DID models such as the one used in this paper. We implement multiple robustness checks suggested by Goodman-Bacon, 2018a and find our results robust. Finally, we show that the results are not sensitive to measurement error in the birth date of children or potential implementation delays that have been documented in the literature. The least absolute shrinkage and selection operator (LASSO) model produces estimates are larger than our preferred estimate. Removing partially treated children also increases the size of our estimates. Thus we feel that the estimates presented here are causal and are at worst underestimates of the true programme effect.

In order to identify which groups benefited the most from the RSBY we present heterogenity analyses. We find that lowest wealth quintile households gain the most from this programme. Their mortalility reductions are between 16 to 19 percent by their first and second birthdays. Additonally, girls and higher birth order children gain over boys and lower birth order children respectively. These gains are significant since they show that health insurance effectively reduces income constraints. In many developing countries, when faced with income constraints, parents either forgo health investments in or trade-off care between children (Jayachandran and Pande 2017, Fitzpatrick & Thornton 2018). Thus having health insurance effectively subsidizes investments in health among previously disdvanatged groups. Additionally, we find that health improvments are the greatest for children from

urban areas. Urban areas in India are known to have more health providers per capita and better quality health infrastructure. We conclude that the effectiveness of an at-scale health insurance programme is moderated by the availability of health services. Though these results may seem straight forward to many, we propose, the empirically backed results explain why there is no consensus in the literature on the effectiveness of health insurance in improving child health. In terms of mechanisms that drive the observed effects, we show that reproductive health service usage, both during and after pregnancy, increase in response to RSBY. We find a rise in antenatal care (ANC) among mothers in RSBY districts (1.5 percent) with a simultaneous decrease in the rate of home births (1.3 percent). Women in treatment regions report being attended to by health care professionals (such as doctors) at a higher rate. Also, more women report having gone for post-natal checkups (1.2 percent) in RSBY districts. We also show that RSBY eligible children are 8 percent more likely to complete their vaccination schedules for measles (MV), Bacille Calmette-Guerin (BCG), Diphteria Pertusis-tentanus (DPT) and polio within the first year of life. This includes taking 4 doses of the polio vaccine, 3 doses of the DPT vaccine, 1 dose of the MV vaccine and 1 dose of the BCG vaccine. These health investments from in-utero to post birth explain the observed reductions in child mortality.

The rest of the paper is organized as follows. In section 2, we discuss and present more evidence on the RSBY's implementation. In section 3 we discuss the data sources used in the paper. In section 4, we discuss the empirical methodology used in the estimation and discuss how we address threats to our identification strategy. In section 5 we present the results, the heterogenity analysis and provide additional robustness checks. In section 6 discusses potential mechanism at play to explain these results. Section 7 concludes with a discussion on policy implications & the way forward.

Rashtriya Swasthya Bima Yojana (RSBY) programme

In an attempt to induce greater worker productivity through health investments, the Government of India announced a health insurance programme called the Rashtriya Swasthya Bima Yojana – RSBY - in 2007 (Virk & Atun, 2015). The goal was to provide Indian families Rs. 30,000 (430 USD[8]) worth of heath care coverage at very low cost. Households with RSBY could insure upto 5 of their members regardless of pre-existing health conditions.

[8] 1 USD ~= 70 Rupees. Anecdotal evidence from interviews with doctors suggest that this amount was sufficient to cover 60 to 70 percent of all inpatient health services that are provided in India.

Families enrolled in RSBY recevied comprehensive coverage for secondary and tertiary care services at empaneled hospitals across the country (Das and Leino 2011; Devadasan et al. 2013; Krishnaswamy and Ruchismita 2011; Palacios, Das, and Sun 2011). Reproductive health services, neonatal care services and family planning were also covered by the programme (Mozumdar, Aruldas, Jain, & Reichenbach, 2018). Families working in the informal sector as well below poverty line families became eligible to purchase this health insurance at the subsidized rate of Rs 30 (0.43 USD). The ultimate goal of RSBY was to be extended into a universal health insurance policy that could be purchased by all families across the country.[9] Under RSBY, care could be sought at both public and private hospitals across the country (Palacios et al., 2011; Virk & Atun, 2015). Details on empaneled hospitals was made public through a website – www.rsby.gov.in - that was developed to monitor the RSBY's implementation progress.

The actual implementation of the programme took place between 2010 and 2015. During this time whether or not the RSBY was made available in a district was determined by the involvement of the local government and the insurance company in administering the programme. Anecdotal evidence suggests that the central government did not provide strict norms on how to implement RSBY. For example, though the Ministry of Labor was the main funding agency of RSBY at the central government, state governments had the freedom to choose which of their departments would implement the policy. Most of the states delegated this to the Department of Labor, but some states delegated it to the Department of Health. Private and public insurance companies were allowed to participate in the program (Virk & Atun, 2015). Insurance companies were selected through a bidding process by each state government. They were expected to enroll families and provide them with the identification cards (Palacios et al., 2011). Enrollment into RSBY was meant to occur annually and insurance companies contracted third party administrators to work with local panchayat members in order to implement the program on the ground. Thus district level implementation depended on the ability of the local government and the insurance company to coordinate amongst each other rather than the health needs of the district or the areas in which it was implemented.[10] Hospitals were mandated to provide `free' services to RSBY beneficiaries. Any additional fees beyond the RSBY approved amount was to be absorbed by

[9] In 2015, with the change in political parties at the central government, this program lost focus. In 2018, a new health insurance program called the Ayushman Bharat was launched in its place. The coverage increased from Rs 30,000 (USD 430) per households to Rs 3,00,000 (USD 4300) per household.

[10] Palacios, Das, and Sun 2011 discuss the role of the insurance companies in implementing the program as being one of the program's unique features.

the hospital and not passed on to the consumer.[11] In practice however, individuals did complain that there were asked to pay out of pocket when they tried to use their RSBY identification card at hospitals.

Evaluations of the RSBY have revealed that, as expected, it increased enrolment (Devadasan et al., 2013; Rajasekhar, Berg, Ghatak, Manjula, & Roy, 2011; Rathi, Mukherji, & Sen, 2012). Karan et al 2017 show that nearly 41 million RSBY cards had been issued which covered more than 150 million individuals across the country. While some papers have found that the RSBY increased hospital care access, others have shown that RSBY was ineffective in reducing out of pocket health spending or in improving health (Azam, 2018; Das & Leino, 2011; Devadasan et al., 2013; Johnson & Krishnaswamy, 2012; Karan, Yip, & Mahal, 2017; Ravi & Bergkvist, 2014; Selvaraj & Karan, 2012). A drawback of the prior studies has been that, due to the lack of national-level health data, they have relied on primary household small-scale surveys to derive implications for research. Even for those who use nationally-repressentative panel surveys such as the integrated health and demographic surveys, authors cannot asses the programme's causal impact of health either due to the lack of data or low RSBY coverage during the survey years. This has prevented authors from examining the programme's causal impact, at scale, on health. In this paper, we plug this gap in the literature. We assess pan-national health effects of the RSBY policy using exogenous variation whose use has not been explored before in this context. We describe the data used and the corresponding methdology in the next few sections.

Data

Program Rollout Information

District level information on the programme's start date, the number of targeted families, numbers enrolled, number of private and public hospital empanelled were updated into the site. In addition, information on the implementing agencies such as the contact government department, the third party administrator and the insurance company was made available on the government website - www.rsby.gov.in. This website updated information on a monthly basis after the programme was announced. After the Bharatiya Janata Party was

[11] The approved amount was set by the insurance company – this depended on the diagnostic related grouping set for any given procedure. Both public and private would receive the same amount and public hospitals were given the option on whether to absorb the entire reimbursement or come up with a cost sharing formula with their health care providers. In some states like Karnataka, hospitals decided to share 50 % of the payments with health care providers at the public hospitals. The key idea behind was to improve quality of care and foster competition in the public sector (Virk & Atun, 2015).

voted in to the central government in 2014, and in anticipation of changes to the programme, all data on the available on the programme as of May 2014 was downloaded from the website by one of the authors.[12] This data was cleaned and census information on the districts was merged into the data set. This district level administrative dataset has been used by Nandi et al., 2013 in an evaluation they conducted on the programme's impact on health insurance premiums. For this impact evaluation, we use one important aspect of the data - district-level information on programme start date – for the main source of identifying variation. In Figure 1, we present an overview of how the programme was implemented across the country. As indicated in figure 1a (left panel), prior to 2012, less than one percent of all districts across the country had the programme. Between 2012 and 2014, RSBY became available in two-thirds of all the districts across the country. Given that this programme was implemented at scale, in the second most populous country, this was a fairly fast rate of implementation. The data shows us that by 2014, more than 30 million families became eligible across these 452 districts. Of those enrolled, 54% reported having received some sort of hospitalization care. Figure 1b (right panel) shows us there is randomness in the start date of the programme. Glancing at this figure, one can also see that there is no observable spatial pattern in the rollout of the RSBY. In appendix table B1 we show that the RSBY's roll out after 2012, when the bulk of the implementation took pace, is not correlated with any district level measures of development.[13]

Health-related outcomes

The National Family Health Survey (NFHS) is a nationally representative survey that collects detailed demographic and health information on a variety of topics pertaining to women, men and children (under five years) in Indian households. The NFHS-IV, which was implemented in India in 2015-16, is the first NFHS survey to be representative at the district (county) level.[14] The NFHS-IV, released at the end of 2018-19, is currently the largest and

[12] After 2014, due to change in the government, the information on the website was not updated. Thus the information on the rollout of the program was only collected up to 2014.

[13] To check for randomness in the implementation we regress RSBY availability in a district in a year on various indicators of socio-economic development at the district-level. We include controls such as night lights composite, sex ratio, literacy and population density while controlling for state fixed effects in the district level regressions. In table B1 in appendix B, we show that the program prior to 2013 the programme was rolled out to areas with greater night lights and better sex ratios. This is consistent with the findings of Nandi et al., 2013, who find that better governed states, states with opposition parties (to the central government) and states with higher fiscal deficits were more likely to participate in the program and have higher rates of uptake. However this does not concern us as the bias it creates works against us finding effects.

[14] Within a district, a two stage sampling method is used. In the first stage, the number of rural and urban clusters (primary sampling units, PSU) that would be surveyed is determined by using population distributions available in census data. Within each cluster 20-25 households were randomly selected and surveyed for basic demographic information. Within households,

most comprehensive dataset available on reproductive and child health outcomes in India. The survey spans 601,509 households across 640 districts in 29 states of India.

Detailed information on birth histories is collected from 699,686 women. This birth history includes information on abortions, miscarriages, number of children ever born and child deaths that a woman may have experienced over her reproductive life course. This provides the key outcome of the analysis – child health measured by - whether the child was a neo natal death (within first month after birth), whether the child died during the first year (infant mortality rate), and whether the child died before age 2 (under-2 mortality rate). We restrict this sample to pregnancy outcomes after 2010. For each child conceived, we collate information on whether the mother reported if she had ante-natal care visits (ANC), the location of the child's delivery and if she had at least one post natal care visit (PNC). We also use information on whether a trained medical professional, such as a doctor, attended to the mother during the visits. We refer to these visits more broadly as reproductive health services. Child vaccination data is collected from health cards made available to mothers when they start the vaccination course. These cards are updated by the health care professional administering the vaccine. During these visits, mothers are also advised when to come back for the next round of immunizations. In our analysis, we examine impacts on whether RSBY children are more likely to complete basic vaccinations before their first birthday. This includes whether the child completed one dose of the measles vaccine (MV), one dose of Bacille Calmette Guerin (BCG) vaccine, three doses of the Diphtheria, Pertussis and Tetanus (DPT) vaccine and three doses of the Polio vaccine within the first two to three months of a child's birth. More details on how the variables are collected in the survey and then used in this analysis can be found in appendix C.

Other Covariates

In our empirical specifications we account for various individual-level covariates including month of birth, year of birth, mother's education, literacy, mother's age at child birth, and mother's body mass index. Given that there may be differences in how mothers access care for a single birth vis-à-vis twins and in some cases cultural or social reasons may dictate differences in access, we control for single birth (dummy), gender of the child and birth order. We control for household-level covariates such as wealth score, whether the

children under the age of five and an eligible women between the ages of 15-49 years is selected for a more detailed survey on health, fertility, biomarker and anthropometric outcomes.

household is in an urban cluster, religion, household's access to piped water and private toilet facilities. These factors may impact the household's ability to use care provided by the RSBY and their child's overall health. In addition, we include month fixed effects in all our specifications to account for month-specific economic factors that drive health care access and use. [15] Heavy rainfalls can also have an impact on transportation costs and income, especially in a rain-fed agrarian regions in India. To account for this, we include the deviation of rainfall from its long term mean[16] at the district-month level. In table 1, we summarize the outcomes and control variables that are used in our analysis.

For each district in the final NFHS-IV sample, we merge the district-level start date of the RSBY using district & state names provided by each of the datasets. We were able to match all districts surveyed in the NFHS 2015-16 with districts-level information from the RSBY administrative datasets. We use this match to merge any other individual and district-level datasets that are used in supplementary analysis. In the preferred specifications, exposure to RSBY is defined as whether the child had access to the RSBY while they were in-utero. This is constructed by comparing the child's birth date with the RSBY district-level start date.

Methods

Empirical methodology

In the difference in difference methodology we use, we exploit variation in the timing and the spatial rollout of the policy across districts. In the main specifications, we compare outcomes of children born after the program was implemented in treatment regions with both children born before the program in the same districts and with children from the same birth cohorts in non-RSBY districts. Our preferred specification for the reduced form model is as follows:

$$Y_{ibd} = \beta * RSBY_{bd} + \pi_{cd} + \emptyset_m + \sum_{k=1}^{k} \gamma_k * X_{ibd}^{k} + \sum_{j=1}^{j} \partial_j * W_{bd}^{j} + \varepsilon_{ibd} \qquad (1)$$

[15] It might be the case that health care access varies by the month of year. For example, there are large demands on women's times in certain times of the year (example - harvesting season), then they may be less likely to access health services due to the inconvenience associated with wage loss or higher transportation costs.

[16] We calculate district- month-specific rainfall shocks as the logarithm of rainfall in the district in the twelve months preceding the interview minus the logarithm of the long-term average monthly district rainfall. The long-term rainfall is constructed as average monthly rainfall between 1980 and 2016, leaving out the twelve months preceding the interview. This definition has been used in other work (e.g.,(Björkman-Nyqvist, 2013; Levine & Yang, 2014; Maccini & Yang, 2009)) and has a simple interpretation as a percentage deviation from the long-term mean.

In this specification, i is the index for the child, b represents the birth year and d is the district of residence. Y_{ibd} denotes the outcomes of interest. This includes both morality outcomes of children and health services sought by the mother for child i born in birth year b in district d. Health services usage includes measures of reproductive health care use as well as the probability that children complete their vaccinations over their life course. In this model, π_{cd} represents cluster fixed effects that account for time-invariant factors at the cluster (sub-district) level, $\emptyset_m$ represents birth-month fixed effects, W_{ibd} represents the household controls and X_{ibd} refers to the child level controls.[17] Unless specified, the standard errors in all regression specifications are clustered at the level of the primary sampling unit (PSU/cluster) of the survey.[18]

The variable of interest is $RSBY_{bmd}$. We combine information on birth month/year of the child, and the date of RSBY implementation in each district to create a categorical variable that takes a value of one if the child was born after the implementation of the program in their district. In other words, this variable takes a value of one for children who had *some* in-utero exposure to the RSBY policy. The coefficient of interest is β, which measures how the change in insurance availability within a district affects the outcome of interest. The identifying assumption in our model is that after controlling for individual, household and cluster-level factors, children in the treatment group are similar to those in the control group in almost all aspects except their exposure to RSBY. Since we assume that all children born in RSBY regions after the implementation of the policy are part of the treatment group, our methodology estimates the intent to treat treatment effects of RSBY. If in practice, the policy targeting was imperfect or there was incomplete take-up across groups eligible, then our estimates would represent a lower bound on the true causal effects.

Threats to identification

There are three threats to identification that we address in this section. First, we show whether or not RSBY was implemented in the district is independent of the baseline outcomes. Second, we show that parents do not change their behaviors in anticipation of the programme's introduction. Third, we eliminate concerns that other flagship programmes of the government implemented prior to the RSBY's introduction are driving these results.

[17] See table notes for the full list of controls used. Results are robust to the use of district level fixed effects as well.
[18] Results are robust to clustering estimates at the district level.

For the first concern, we compare our preferred mortality outcomes between ever-RSBY and non-RSBY districts using the District Level Health Surveys (DLHS) of 1999, 2004 and 2007. These surveys provide us information on changes happening in the pre-intervention period. We assign districts as RSBY districts if they were to ever receive the programme in the future. We construct measures of mortality using the definitions from the previous section. In figure 2, we present the results from the analyses. Overall, we see that RSBY districts have higher mortality. However in panel 2a (left side), we see that RSBY districts and non RSBY districts have the same trends in child mortality outcomes in the pre-intervention period. In panel 2b (right side), we see that the differences are not statistically different from zero. This is true regardless of the outcome we choose. Thus we are convinced that the chosen RSBY availability in a district is independent of our baseline outcomes.

For the second concern, we present results from an event study analyses in figures 3 to 5. The study uses the empirical strategy presented in equation 1. In all the figures, we can see that prior to the introduction of the programme (T<1), there are no differences in mortality rates between children exposed or not to RSBY. Figure 3 shows that there were limited effects on NNMR after the programme was introduced as well. In figures 4 and 5, we see that the mortality effects become signifcant after the programme is implemented in the district (T=0). Over time, we can also see a significant and sustained drop in mortality with size of the effect increasing with increasing exposure to the RSBY. In appendix E, we present additional event study analyses that show that the results are robust to 1. Restricting the sample to the district level (Figure E1) & adding district fixed effects (Figure E2), 2. Restricting the sample to the district level effects for RSBY covered districts (Figure E3) & adding district fixed effects (Figure E4). Taken together with the lack of pre-trends, we can conclude that there are no anticipatory effects that are driving the changes in mortality reductions prior to introduction of the programme. In the robustness checks section, we run additional tests to eliminate concerns that changes in fertility or household-level selection based on unobservables are driving these effets.

A concern for those familiar with the Indian context could be that other programmes/policies implemented by the congress government at this time may have been driving this results.[19] Take for example the Janani Suraksha Yojana (JSY). Between 2002 and

[19] With regards to any other programme, unless there is data to show that its implementation systematically coincides with the introduction of the RSBY, we are not concerned. For example, if there is a concern that the effects are driven by the National Rural Employment Guarantee Scheme, then Azam 2011, Dasgupta 2012 and Zimmerman 2012 present evidence that NREGS implementation was completed by 2008. Unless there is data to show that the consequent labor market effects rolled out in the way that RSBY did, we are not concerned about its presence in the district.

2007, the JSY provided a cash incentive to women who delivered their children in government hospitals. This programme successfully reduced home births and increased deliveries in government hospitals across the country (Gupta et al., 2012; Joshi & Sivaram, 2014; Ng et al., 2014; Powell-Jackson, Mazumdar, & Mills, 2015; Randive, San Sebastian, De Costa, & Lindholm, 2014).[20] Since this programme incentivized deliveries in hospitals by reducing costs of the same, one could be worried that the results in this paper are being driven by the JSY rather than the RSBY. In order to alleviate this important concern first, we show that JSY was almost universally implemented by the time that RSBY was starting to be implemented. In figure 6, we see that almost 100 percent of the districts had received JSY benefits in 2010, at which time RSBY was just starting. Since there was no variation in JSY exposure in the time period of our study, we conclude that the implementation of the RSBY was orthogonal to the availability of the JSY. Hence the variation that we use to estimate the impact of health insurance is not conditional on JSY availability. Second, JSY beneficiaries had to deliver in the government health institutions in order to receive their cash transfers. Private providers were not eligible for the JSY.[21] The RSBY model, on the other hand, covered private providers. Thus we test if hospital deliveries in private institutions went up in response to the RSBY in appendix F. The positive result on private institutional deliveries points towards our effects being driven by RSBY, and not the JSY.

Results

Main results

In line with the theoretical model proposed in appendix A, we expect that effective health insurance reduces the price of health care and increases access to health services. As a result health of individuals improve. Child health, measured by the reduction in child mortality, would improve if mothers are able to invest in their own health and in the health of the child. In line with this argument, in this section, we present causal estimates of the impact of RSBY on different measures of child mortality (by cohort). The coefficients represent health gains for children who had some access to RSBY (through their mothers) when they were in-utero, as compared to children with no RSBY exposure. In table 2, results suggest

[20] Despite its success in getting women to hospitals, JSY evaluations have found limited effects on child mortality, either neo-natal or infant mortality, ante natal care usage and post-natal care usage (Joshi & Sivaram, 2014; Murray et al., 2012; Powell-Jackson et al., 2015). Further, research has found many problems with the JSY programme, including patchy implementation, leakage of funds, poor care quality, and out-of- pocket costs of institutional delivery (Das, Rao, & Hagopian, 2011; Mazumdar, Mills, & Powell-Jackson, 2011; Sukla, 2012).

[21] Powell-Jackson, Mazumdar, and Mills 2015 show that JSY was very successful in increasing deliveries solely in public institutions through whom the cash benefits were distributed

that there are no statistically significant differences in Neo-natal Mortality (NNMR) between treated and control cohorts. This is reiterated in the event study in figure 3. There we see that there are no impacts of the programme even with more exposure over time. However, gains are quickly made within the first year of the child's life. Results in column 2 suggest that IMR – mortality rate in the first year – drops by around 1.8 deaths for every 1000 births after RSBY implementation. This represents a drop of infant mortality by 5.4 percent from the mean value. By age two, children make further gains to their health. Child under-2 deaths reduce by 3.4 per 1000 births, a 10 percent reduction over the mean. In table 3 we drop districts that never received RSBY to increase comparability across treatment groups in the sample. This analysis uses within-district variation in RSBY exposure to estimate the effect of the policy on children. It compares mortality outcomes between children who benefitted from RSBY with other children from the same districts who were born before it was implemented. The results suggest that there was a statistically significant drop in infant mortality and under-2 mortality in districts after RSBY policy was introduced in order to magnitude similar to our preferred specification. Thus we feel our preferred specification is capturing the unbiased population impacts of the programme.

These reductions in child death that we find are in line with, but on the lower bound, compared to findings from other international experiences of health insurance expansions. Gruber, Hendren, and Townsend 2014 find a 13 to 30 percent reduction in child mortality in Thailand after the introduction of the 30 baht program. Chou, Grossman, and Liu 2014 also estimate a 20 to 40 percent decline in child mortality after the introduction of national health insurance program in Taiwan. Goodman-Bacon, 2018b finds that introduction of Medicaid reduced child mortality of nonwhite children by 20 percent across the United States. We propose that our results may be underestimates of the true population effect of the programme for the following reasons. In table 4 we drop all children who had *less than full* in-utero exposure from the sample. Here, we only consider those children to be part of treatment if their mothers had access to RSBY during their entire pregnancy. This strategy compares the outcomes of children who had access to the RSBY policy during their entire in-utero period, with those of children born in regions/periods with no RSBY. When we do so we see that the life savings due to RSBY go up for children with *full* in-utero RSBY exposure when compare to estimates from table 2. Estimates in table 4 suggest that IMR reduces by 2.7 per 1000 births, while Under-2 mortality falls by 4.8 per 1000 births, which represents a 1.5 fold increase over the corresponding effects with any RSBY exposure. Thus it would seem that

our preferred identification provides a more conservative estimate of the programme's effects. These findings are also consistent with the literature on the importance of protecting pregnant women against external stressors as a means to improve the unborn child's long term health (Almond & Currie, 2011a; Currie, 2000; Miller & Wherry, 2019; Wherry, Miller, Kaestner, & Meyer, 2015). In table 5, we consider births that occur in the year before RSBY implementation as also being part of the treatment group. Children born at least one year before RSBY can potentially access benefits of this policy in their first year after birth. However in the main specifications these children are part of the control group since they are considered as having *no* in-utero RSBY exposure. When we run the regressions by considering these children as treated, we find that RSBY effects increase. Thus, our preferred estimates seem to be under-estimates of the true population effect of the programme. Once we account for these, we see that the estimates become larger and more comparable to the international experiences of health insurance expansions.

Heterogenous effects

The effectiveness of health insurance is determined by both, its ability to release the income constraint that prevent health service access and whether adequate health services are available for individuals to use when they are covered. If the price effects is not large enough then there may be no gains made to health since individuals may find health insurance and health services too expensive relative to what they are willing to spend. If there are no health services available, then introducing health insurance may have no real effect since individuals have nowhere to spend their newly found gains. In this paper, we have shown that health insurance does lead to health gains, even in a country like India where the quality of health services is poor. Here, we provide additional evidence of its ability to ease the income constraint and improve health of groups for whom this affordability barrier is a major constraint for health. We also show that the RSBY is more effective in improving health in areas where there are more or better access to health providers.

Literature from India suggests that parents prefer to invest in lower birth order boy children when faced with income constraints. These intra-household disparities eventually translate into greater health gains this group over other children in the same household. The introduction of the RSBY is a positive income shock for households. Since it reduces prices and release the income constraint, we could expect to see parents invest more in groups that were previously disadvantaged due to the same. In figure 7, we explore whether there were

any differential impacts of the program on these sub-groups. First, we check if either boys or girls disproportionately benefited from this program. For both outcomes (IMR and U2MR), girls show higher benefits of the RSBY policy. The IMR for girls decreases by around 2 per 1000 births and U2MR by around 4 per 1000 births. For boys, there are no (statistically) significant gains in terms of IMR reduction, but by age two, they see a reduction in deaths by around 3 per 1000 births. In terms of differences by birth order, our data suggests that RSBY does not make much of a difference to first-born children, whereas children of higher parity benefit more from this policy. These results would indicate that parents who were trading off the health of girls and higher birth order children, in favor of boys prior to the RSBY, are less likely to do so. The RSBY also seems to provide a substantial release from the affordability constraint that drive choices to forgo health among the poor. Coefficient estimates in figure 7 suggest that those in the lowest income quintile of the population had the largest reductions in IMR (16 percent, 0.03 SD) and U2MR (19 percent, 0.05 SD) respectively. This translates into an additional 5 (per 1000 births) and 8 (per 1000 births) children living to their first and second birthdays respectively. These results are around 16 percent to 19 percent of the changes over the means and are closer to the international experiences of health insurance expansions among the poor.

In terms of access to services, once would expect that in areas with greater access to health services, there would be larger impacts of the programme. In urban areas in India, there are larger numbers of health services providers. Better roads and transportation also reduce the cost of accessing these health services. Hence one would expect to see larger impacts of the RSBY in urban areas. In line with this, estimates in figure 7 also show that children from urban areas experience larger gains as a result of this policy. IMR reduced by 4 deaths per 1000 births and by age two, this group had seen further reductions in child death by 5 per 1000 births. In rural areas, IMR reduced by 1 death per 1000 births and by age 2, child deaths (under 5) reduced by 3 per 1000 births. Thus our results support the conclusion that better access in combination with reducing affordability barriers to market access, can help children make larger gains to their health.

Robustness

In this section, we build confidence in the robustness of our casual estimates by testing various aspects of the identification strategy used in the estimation. The first three tests remove the concern that measurement error in the outcome variables or in the treatment variable are impacting the results. In the next three robustness checks, we rule out concerns

that selection effects are driving the results. Finally, we present evidence that we are not affected by the bias that arises from changing sampling variance and shares of observations across the treatment arms as raised by Goodman-Bacon, 2018a.

First, since information on mortality of a child is reported by mothers based on recall of their birth histories, there is potential for measurement error for prior births or deaths if they are further off than the survey year. To overcome this we only keep births and deaths reported from 2010 the surveys. Since the recall time is only five years, we would expect that recall errors would be low. If this residual error is random across RSBY and non RSBY districts, then not accounting for it will at best reduce precision by increasing standard errors. Even in cases where mothers are proportionally underreporting child deaths, the coefficients will be at best underestimates (Gruber et al., 2014). In both cases, the measurement error works against us finding a statistically significant effect, and thus implies that our findings here are possibly underestimates of the true effect of RSBY.

A second concern is the considerable number of missing values in the date of birth information of children in the NFHS. To overcome this we assign all children a birth date of 15th in order to assign treatment exposure. Agarwal et al. 2017 and Larsen, Headey, and Masters 2019 show that using birthdates & month of birth for identification from the NFHS may cause a non-random measurement error that biases estimations of shocks on child health. In order to make sure that our results are not subject to this bias, we re-examine results after assigning everyone a birth date or 1st or 28th in their birth month. The result from this robustness check can be found in table 6.[22] The coefficient estimates suggest that the effects retain their sign and significance and lie within one standard deviation of the main results. Therefore, we feel convninced that our results are not sensitive to missing data in birth dates.

Third, Azam, 2018 suggests that coordination delays on the ground meant that the programme really took effect around four months after the official date of implementation in a district. These delays would imply that even if technically a district would have implemented RSBY, children born within a few months after implementation may not have been exposed to the programme. To we account for this, we delay the treatment assignment by 4 months from the actual listed start date of the programme in table 7. After running the

[22] In an additional check we show that our results are not sensitive to assigning any randomly chosen day of the month as the birth date of the child. Also, we can show that our estimates are not sensitive the number of days of exposure since the program. That is, regardless of the number of days of exposure, the cohort level mortality rates remain quite stable. These results are available from the authors on request.

main specification with the new treatment exposure date we find that the main results are still robust. Thus there is no sensitivity of the estimate to the implementation delays.

With regards to selection, we rule out that migration responses, fertilty responses or selection into the programme based on some household unobservable are driving the results. With regards to selection effects due to migration, readers maybe concerned that sickly mother may migrate towards districts that have greater (or any) RSBY exposure. If sickly mothers move into areas where RSBY is available, then the estimates presented here maybe be impacted by selection. We propose that this is not likely to be a concern for a few reasons. Women migrating in search of better health care access is almost non-prevalent in India. Kumar, Dansereau, & Murray, 2014 show that even in the presence of substantial financial benefits, distance is a major inhibitor to reproductive care access in India. This makes migrating for such services a very low probability event. Additionally, without adequate social networks or financial capital, it is unlikely that women would seek care very far from their homes during their pregnancy. Having said that, it could be argued that women may go back to their maternal homes during their pregnancies. If these homes are closer to RSBY districts, then the problem of selection may bias estimates. In India however, women's post-marital homes are on an average 20 miles from their pre-marital homes. Much of the migration for women that is marriage based is intra-district (Bloch, Rao, & Desai, 2004; Fulford, 2015; Singh, Kumar, Singh, & Yadava, 2011).[23] Since the exposure to the programme is assigned at the district level, this concern is thus not likely to affect our analysis.[24]

It can argued that the results we observe come from changing fertility responses to the RSBY. If increases in health care access induces mothers to have more children in RSBY districts then results may not reflect health gains from lower mortality but rather fertility responses to the programme. In a robustness check in table 8, we use the fertility recall data provided by women to create a monthly time series of births in a given district (cluster) over the years 2007 to 2016. We then regress monthly births in a district on a RSBY treatment variable while adding fixed effects for month, year and region fixed effects. The coefficient on the RSBY variable provides an estimate of the effect of RSBY on the total number of

[23] In this scenario, we argue that unhealthy mothers are less likely to be able to deliver healthy babies and hence estimates that we uncover are more likely to be underestimates in this case.

[24] Sood et al. 2014 also discuss that prospects of migration for seeking health care for women is not a major threat when evaluating the benefits of another health insurance program in Karnataka in India.

births within a district after it is introduced. Results suggest that there is no effect of RSBY on fertility.

Even though our main specifications control for household-level observables, it can be argued that households are selecting into the programme based on unobservables in their health status. Hence the within-cluster estimates are being driven by selection into the programme. If such was the case, then we would not expect to see the impact of the programme remain if the preferred specifications are estimated at the district level. In table 9, we show that this is not true for our model. The outcomes at the district level are robust and larger than the individual level effects. Hence we feel confident that are estimates are not subject to selection effects. This is reiterated in figure E1 to E4 in appendix E where the event study analyses show that the programme's effects are not subject to the level of analysis and we are at best under-estimating the true effects. In table 10, we use a LASSO estimation to drop additional covariates which play little or no role in explaining the model of choice. When we use this procedure, our estimates increase in size and effects remain robust. This too strengthens our confidence in the estimates that we have presented.

Goodman-Bacon, 2018a cautions that difference in difference estimates which are outcomes of staggered timing in program roll outs maybe biased. This comes from OLS procedure which induces a weighting bias when formerly control units become treated over time. Based on his recommendations, we present three pieces that provide support that our estimates are not impacted by this bias. First, we show that the results are larger when we estimate remove the impact of untreated districts from the sample. Here, we compare only within district estimates for children born with and without RSBY exposure. The results are presented in table 3. Next, in order to reduce the bias that comes from the sampling variance, we also run the regressions at a more aggregate level. In table 9 we estimate the aggregated outcomes at the district level. In figures 8 and 9, we present the results from the event study analysis. In figure 8, we see that (1) there are no differential trends in the outcomes of interest across treated and untreated districts, and (2) all the improvement in the outcomes occur after policy implementation. Also, these effects seem to increase with greater programme exposure. In figure 9, we run the analysis for RSBY treated districts only. Here too, we find that the main results are robust and are at best under-estimates of the true population effects once these checks are added. This strengthens our confidence in the estimates that we present in the main specifications as our preferred causal impact of the programme.

Mechanisms

When health insurance reduces the relative prices of health care goods, investments in child health can increase through direct and indirect channels (Currie & Gruber, 1996; Dow & Schmeer, 2003). First, if ante-natal care (ANC) is a complementary good, and insurance reduces the total costs of health services in the household, then demand for the same can increase. Once the insurance effect induces an ANC visit, follow ups can be scheduled thus increasing demand.[25] Prior literature shows that ANC visits early in the pregnancy are extremely important towards diagnosing, monitoring and managing health symptoms that can later impact both the mother's and the child's health (Hogan et al., 2010; Lassi et al., 2014). During these visits, health professionals monitor fetal development, the mother's health, provide basic care such as tetanus injections, iron tablets. They use this as a time to educate woman on their nutritional health as well.[26] These health investments are essential for fetal development and hence long term health of the child (Almond & Currie, 2011a).

Second, in many developing countries, women still give birth in their homes, which are often low quality health environments compared to hospitals. Traditional birth attendants (TBA) are also preferred as they are often known to the family and charge less. However, if complications develop during delivery, TBAs are not able to provide necessary care since they are not professionally trained. In the event that the household is not able to reach the mother to the hospital in time, risks of mortality to both the mother and her child can increase. Hence reducing the cost of health care through provision of insurance can play a vital role in bringing women to hospitals for their deliveries. Third, reduction in the cost of pregnancy can stimulate demand for other complementary health services such as post-natal health visits. In these check-ups, health of women and children are assessed to identify any risks to health that can develop post the pregnancy and within the first year of the child's health. Additionally, mothers are educated and encouraged to bring children back for immunizations. These sustained health investments along the lifecycle of the child, from in-utero post birth, due to health insurance increase long term health. For example, evidence from the US suggests that medicaid expansions improved short term health of children exposed at early ages (Goodman-Bacon, 2018b; Wherry et al., 2015) and educational outcomes of children exposed in-utero (Miller & Wherry, 2019). Fourth, when programmes such as these are implemented at scale, there maybe supply side effects in response to the

[25] A minimum of three ANC visits during a pregnancy is the recommended norm.https://www.bloomberg.org/program/public-health/maternal-health/#overview

[26] https://www.worldbank.org/en/topic/reproductivematernalchildhealth,

change that drive the health gains. Since India has a robust private health sector, the programme's presence could have helped hospitals and health care centers advertise reproductive and child health services to new patients. Patients could have also demanded more and better quality services from providers. Finally, it is possible that the number of services provided increased either due to health professionals offering more hours to see patients or through the increase in the number of health centers.

Thus far health insurance impact evaluations have not been able to pin down the mechanisms driving the improvements in child mortality. Even in at-scale evaluations of insurance on child mortality, the lack of data, either due to the survey timing or the survey quality, has prevented authors from presenting empirically backed arguments on what channels drive these impacts. In this paper, we bring the first evidence on the mechanisms that drive improvement in the health of children. We specifically assess the programme's impact on access to reproductive and child health services. We show that there maybe a supply side response as well.

In table 11 and 12, we present the analysis of the impact of the RSBY on ANC, births, PNC and imminizations. In line with the hypothesis of complemetary demand effects, we find that pregnant women with access to RSBY were more likely to access ANC services. Results in table 11 column 1 suggest that women with any access to RSBY during their pregnancy are 1.5 percent more likely to have some ante natal care compared to mothers with no RSBY access. Additionally, RSBY eligible mothers are more likely to be examined by a health care professional (col 2) on these visits. This indicates that there is a potential supply side response to the programme as well. The RSBY policy also had a positive impact on hospital births. In table 11, cols 3 & 4, we see that RSBY has a small but statistically significant effect on the reduction of home births (column 1). In addition, nearly all those moving to hospitals report being attended to by a trained health professional (like a doctor) during their delivery.[27] One could reasonably expect that this change should have an effect on NNMR. The lack of any such significant effects suggests that this channel may not be as important as other factors. Another important follow-up in the reproductive services offered to pregnant women and their children are post-natal care checkups and immunization services. During these visits, vaccinations are also administered to children in order to protect

[27] One potential concern is that if more sickly women are using RSBY as a means to access health services and such women maybe more likely to get C-sections. Thus, if this was true emergency caesarian sections (C sections) in RSBY districts may also increase in response to the program. We find that this is not the case – prevalence of C-sections is lower among mother covered by RSBY. Results can be provided on request.

them from preventable communicable diseases. In RSBY regions, mothers are more likely to report having followed-up for their two month post-natal checkup, a 1 percent increase over the non-RSBY districts (See table 11, cols 5 & 6). The results in table 12 show large and significant effects on reported immunization completion as well. There is an 8% increase in the probability of completing the basic vaccine schedule, which involves receiving the MV, DPT, polio and BCG vaccine within the first year of a child's birth. This amounts to an effect size of 0.2 standard deviations. In RSBY regions, an additional 10 percent of children complete their MV schedule (0.2 SD), an additional 5% complete their polio (0.1SD) and DPT (0.1SD) vaccine schedules and an additional 0.4% complete their BCG (0.01SD) schedule. Put together with the PNC access results, this implies that insurance is an extremely effective tool, even in a resource poor context like India, to facilitate a last mile push for increasing access to complemetary health services such as reproductive care and child vaccinations. Given the internationally documented evidence of life saving effects of childhood vaccines, we can conclude that insurance plays an important role in reducing child mortality this channel as well.

In appendix G, we present event study analyses of the above mechanisms. We show that prior to the introduction of the programme, there are no anticipatory trends. All effects become evident after the programme is introduced in the district. These effects also seem to sustain themselves over the short term.

Discussion and Conclusion

Health insurance is an important tool that can increase health care access. Even though one would expect that having access would engender better child health outcomes – measured by lower mortality - empirical evidence from across the world suggests that this conclusion is not straightforward. While in places like Thailand, Taiwan and the United States, there is strong evidence to suggest that it reduces child mortality, in China and Costa Rica the evidence is not as clear. In this paper, we bring new causal evidence to the literature on the effectiveness of, an at-scale, national-level health insurance programme in reducing child mortality. We use a generalized difference-in-differences strategy that exploits randomness in the spatial and temporal roll out of a national-level health insurance programme implemented in India to present the causal impact of the programme. This roll out data comes from a unique government maintained administrative dataset that has not been used in the literature before. Through this data, we know that more than 150 million

individuals received health insurance between 2010 and 2015. Using the latest round of the National Family Health Surveys (NFHS) of 2015-16, we show robust evidence that IMR reduced by two per 1000 births and U2MR decreased by three per 1000 births every year in districts where the programme was introduced. These estimates imply a reduction of around 5 to 10 percent over the mean IMR and U2MR rates across the country. Our methodology thus provides causal evidence that an at-scale health insurance, even with less than 100 percent take up, can lead to significant reductions in child mortality.

We also show that health insurance is more effective in reducing mortality in a developing country if 1. It reduces the affordability barrier such that individuals can access previously unaffordable health care services, 2. If it is introduced in areas where health services are better provisioned. If either of these are not substantial, health gains may not be measureable even in at-scale evaluations. These contextual differences, we posit, explain the contradictory results on health gains made by children when health insurance is introduced in developing countries. Additionally, we find that health insurance increases demand for reproductive health services for mothers and increases vaccination coverage for children. These are the channels are important drivers of early child health and adult life outcomes. In a country like India increasing last mile health access is often the greatest challenge for policy makers, this paper provides evidence that health insurance can nudge households to increase their use of these essential health services.

One possible channel that may drive these health gains is the supply side response to the introduction of the programme. If the insurance drives greater numbers of people to hospitals, then hospitals can use this as an opportunity to advertise their reproductive health services thus generating demand. Alternatively, providers may switch out of the public sector and provide more services through the private sector increasing supply of such care. They may also increase hours offered to see such patients. In this paper, we provide some evidence that mothers who are eligible for the health insurance report more contact-visits with health care professionals such as doctors. Thus, even in a situation where there exists a large dearth of health care provides, an at-scale health insurance policy can induce a supply response that translates into better health. However, in the absence of better data on health service providers, services, or quality of services, we are not able to test the importance of the supply side mechanism vis-à-vis the demand side nudge provided by health insurance.

The findings of our paper have important implications for health insurance provision across the world. We show that health insurance can be extremely effective in reducing

income constraints and providing a demand side push to increase health care access for vulnerable groups. When health infrastructure and quality health service providers are available, policy health insurance can further improve child health. Given that mothers are more likely to visit doctors, these improvements will most likely translate into large gains for maternal health in the long term as well. Moving forward, we believe that further investigation into the impact of insurance on women's health is required. Additionally, experimental research to understand which of the proposed channels is the most cost effective needs to be conducted. Finally, in order to truly measure the cost-effectiveness of health insurance as a policy tool for improving health in a developing country, analysis on its impacts on anthropometric outcomes, cognition, adolescent health and adult health and productivity is required. Randomized control trials that compare of the cost effectiveness of insurance type interventions vis-à-vis in-kind transfers, cash transfers or conditional cash transfers are also needed to determine its cost effectiveness from a public finance perspective. These research questions, we feel, are important to assess in future work to derive meaningful implications for the cost effectiveness of health insurance interventions in developing countries.

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Figures

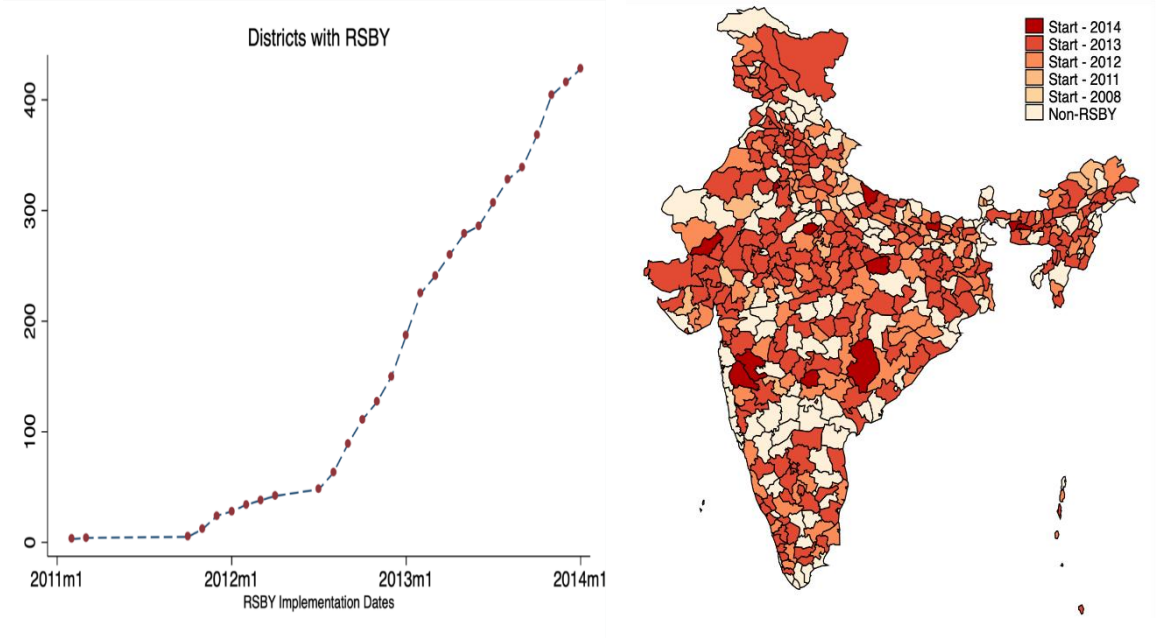

Figure 1 RSBY implementation – The graph shows the rate implementation of RSBY across different districts over time. The graph indicates that the bulk of the implementation happened in the years 2012 and 2013, thus showing that there was limited spatial heterogeneity in timing of RSBY implementation. The figure on the right shows the start date of the programme across districts in India. Here we can see that there is randomness in the start date both within and across states. Across states, implementation was conditional on the political climate of the state. Within states, district level implementation was determined efficiency of the insurance company to roll out the smart cards, enroll providers, facilitate health camps for enrollment and in recruitment of local leaders & health workers in promoting the program.

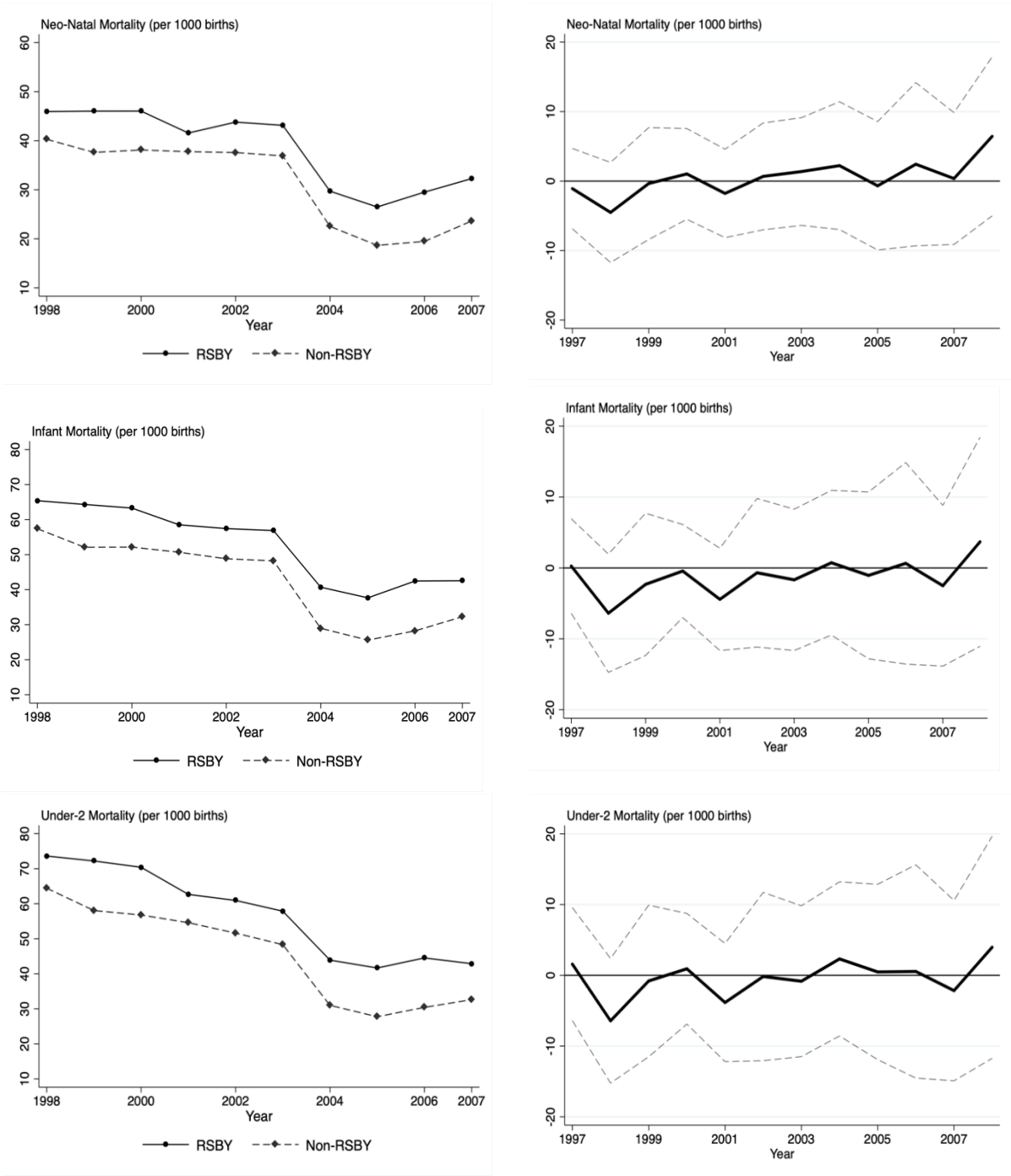

Figure 2 - Testing common trends assumption and a falsification check - This figure tests a critical assumption of the Difference-in-Differences method –Parallel trends in the pre-treatment period (left panel). A placebo event-study, on children born before the programme was implemented is conducted as well (right panel). Since RSBY implementation started in 2008, we use data on births between 1997 and 2007 in the DLHS 2 and 3. We assign treatment to districts who we know eventually get the programme. Here we can see that there are null effects when we compare districts with and without RSBY.

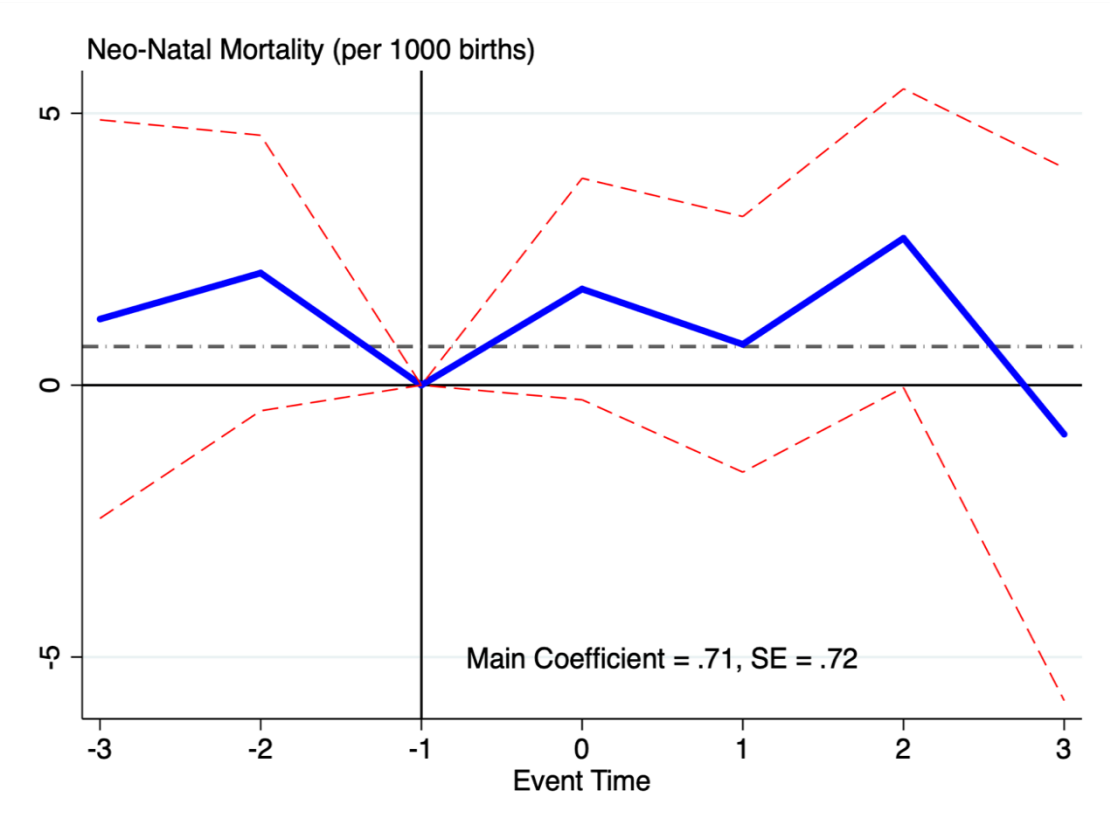

Figure 3 - ***Event Study Analysis - Neonatal Mortality -*** Sample is based on birth-level data from DHS 2015. The figure presents the effects of RSBY on neo-natal mortality (0/1). The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and PSU fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered at the PSU level.

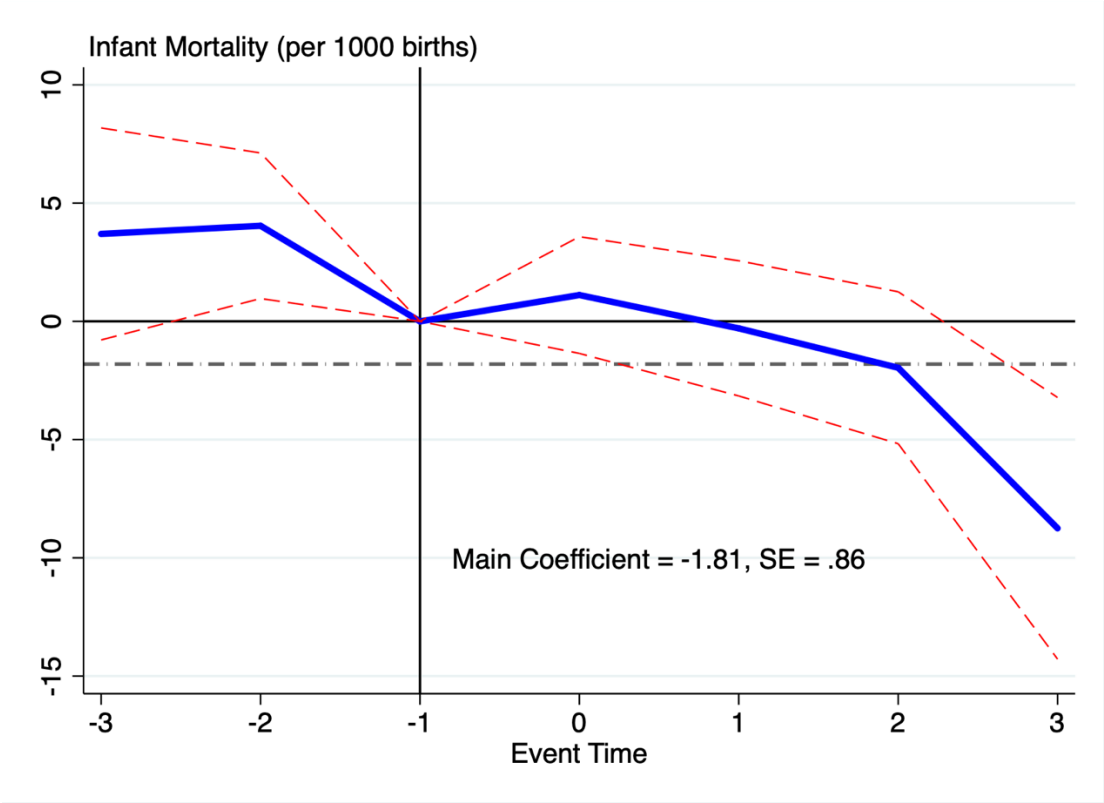

Figure 4 - ***Event Study Analysis - Infant Mortality*** - Sample is based on birth-level data from DHS 2015. The figure presents the effects of RSBY on infant mortality (0/1). The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and PSU fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered at the PSU level.

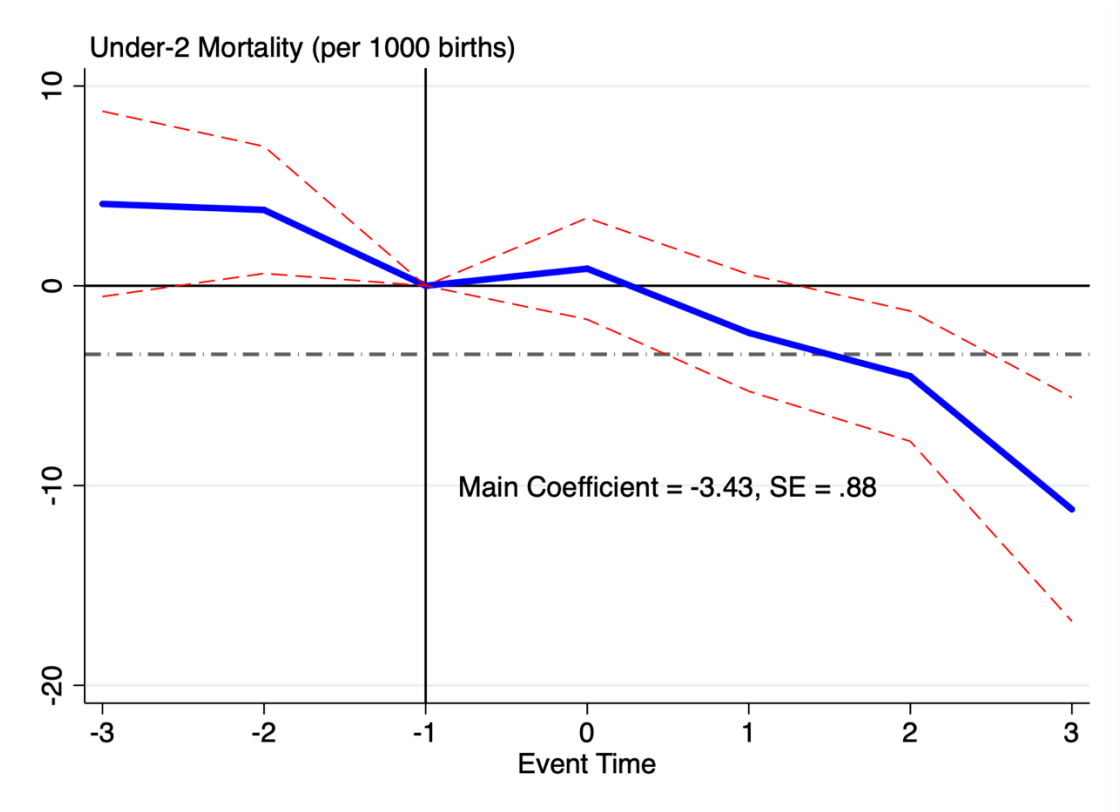

Figure 5 - ***Event Study Analysis – Under-2 Mortality*** - Sample is based on birth-level data from DHS 2015. The figure presents the effects of RSBY on Under-2 mortality (0/1). The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and PSU fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered at the PSU level.

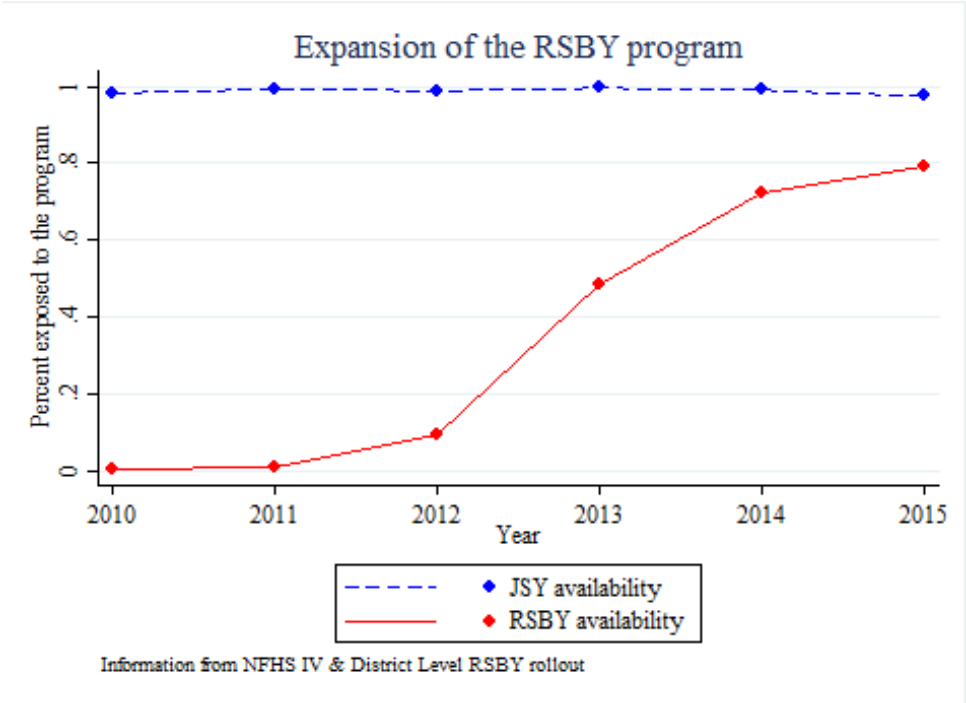

***Figure 6* – Presence of the JSY during RSBY implementation -** In this figure, the blue line represents the probability that the child was born in a district where the Janani Suraksha Yojana (JSY) programme was available. The red line represents the probability that the child born was covered by RSBY between 2010 and 2015. The data comes from the National Family Health Survey 2015-16. In the graph we see that the JSY was already available in most of the districts by the time the RSBY was rolled out. Hence any gains made in RSBY covered districts should not be attributed to JSY

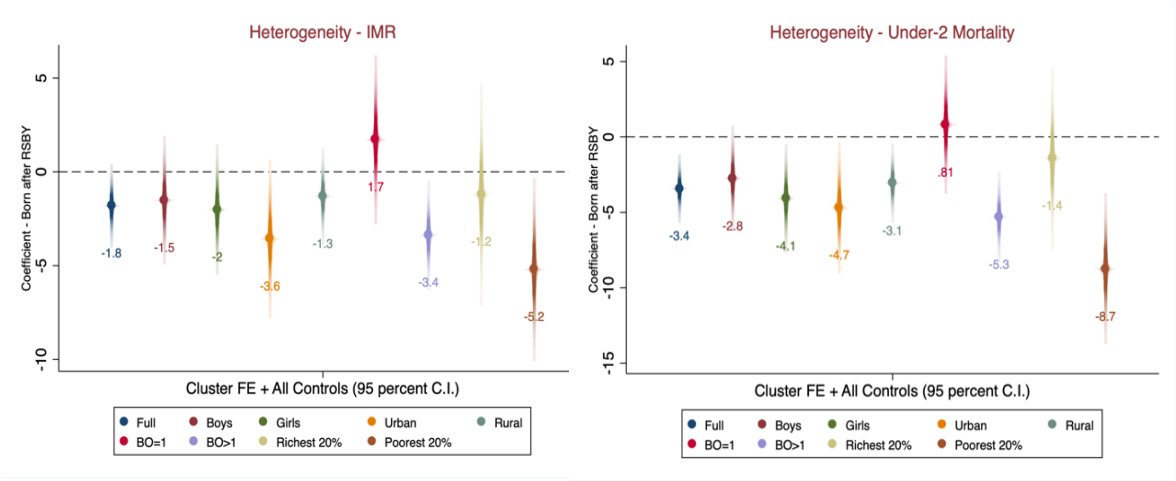

Figure 7 - Heterogeneity Analysis – In these graphs, we present the heterogeneous impact of RSBY on Infant mortality (graph on left) and Under-2 mortality (graph on right). Each estimate compares a sample of children born within a cluster with and without exposure to RSBY over time. In `Full', estimates from our preferred specification are presented. In `boys', we compare the mortality rates for the sub-sample of boy children born with and without exposure to the programme. For `girls', the sub-sample includes only girl children. In `urban', we restrict the sample to children born in urban areas. In `rural', the sample includes children born to mothers in rural areas only. In `BO=1', the sub-sample includes only first born children and in `BO>1', the sub-sample includes children who are in the second born or higher. In `richest20%', children born to mothers in the highest wealth quintile are compared to each other and in `poorest20%', outcomes of children born to mothers in the poorest quintile are compare to each other.

Tables

Table 1 Summary Statistics

	(1) Obs.	(2) Mean	(3) S.D.	(4) Min	(5) Max
Outcomes					
Neo-Natal Mortality rate (NNMR)	306,513	21.88	146.3	0	1,000
Infant Mortality Rate (IMR)	306,513	32.55	177.5	0	1,000
Died before age 2 (Under-2)	306,513	34.38	182.2	0	1,000
Child level Controls					
Born After RSBY[1] = 1	306,513	0.346	0.476	0	1
Birth Order of Child	306,513	2.303	1.512	1	17
Single Birth = 1	306,513	0.984	0.127	0	1
Female Child = 1	306,513	0.479	0.500	0	1
Month of Birth	306,513	6.320	3.482	1	12
Year of Birth	306,513	2,012	1.730	2,010	2,016
Mother Level Controls					
Year of Birth	306,513	6.004	5.119	0	20
Mother's BMI[2] (BMI*100)	306,513	2,113	343.1	1,202	3,343
Mother age at birth	306,513	24.60	5.052	15	49
Illiterate = 1	306,513	0.364	0.481	0	1
Household Level Controls					
Religion	306,513	0.714	0.452	0	1
Urban == 1	306,513	0.237	0.425	0	1
Wealth Score	306,513	-0.163	0.970	-2.319	2.910
Scheduled Caste = 1	306,513	0.187	0.390	0	1
Scheduled Tribe = 1	306,513	0.209	0.406	0	1
Other backward caste = 1	306,513	0.386	0.487	0	1
No Private Toilet = 1	306,513	0.422	0.494	0	1
Piped Water in Household = 1	306,513	0.238	0.426	0	1
Below Poverty Line Card = 1	306,513	0.357	0.479	0	1
Other Controls					
Any JSY[3] in district in birth year	306,513	0.986	0.119	0	1
Rainfall Deviation[4]	306,513	-0.0176	0.241	-1.754	0.987

[1]RSBY – Rashtriya Swasthya Bima Yojana, [2]BMI – Body Mass Index, [3]JSY – Janani Suraksha Yojana, [4]Rainfall deviation (Bjorkman definition) = (Log rainfall - log of long term mean), Long term mean rainfall = average monthly rainfall between 1980 and 2016, leaving out the twelve months preceding the interview (Maccini and Yang, 2009; Björkman-Nyqvist, 2013; Levine and Yang, 2014)

Table 2 Impact of RSBY on child health

	(1) NNMR	(2) IMR	(3) Under-2
Born After RSBY (=1)	0.71	-1.81**	-3.43***
	(0.72)	(0.86)	(0.88)
Observations	306,513	306,513	306,513
R-squared	0.121	0.130	0.132
Mean	22	33	34
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 3 Mortality effects for children born in RSBY districts

	(1) NNMR	(2) IMR	(3) Under-2
Born After RSBY (=1)	0.83	-1.72**	-3.33***
	(0.72)	(0.86)	(0.88)
Observations	225,492	225,492	225,492
R-squared	0.117	0.126	0.128
Mean	24	35	37
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 4 Impact of long term in-utero exposure

	(1) NNMR	(2) IMR	(3) Under-2
RSBY during full pregnancy (=1)	0.39	-2.72***	-4.76***
	(0.79)	(0.94)	(0.96)
Observations	278,985	278,985	278,985
R-squared	0.129	0.138	0.139
Mean	22	32	34
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 5 Alternative definition of RSBY exposure

	(1) NNMR	(2) IMR	(3) Under-2
RSBy (in-utero + year 1)	0.78	-2.39**	-4.03***
	(0.83)	(0.99)	(1.02)
Observations	258,484	258,484	258,484
R-squared	0.136	0.146	0.147
Mean	22	32	34
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 6 Sensitivity to birth date assignment

	(1)	(2)	(3)	(4)	(5)	(6)
	Birth Date = 1st			Birth Date = 28th		
	NNMR	IMR	Under-2	NNMR	IMR	Under-2
Born After RSBY (=1)	0.59	-1.95**	-3.55***	0.67	-1.82**	-3.46***
	(0.72)	(0.86)	(0.88)	(0.72)	(0.86)	(0.88)
Observations	306,513	306,513	306,513	306,513	306,513	306,513
R-squared	0.121	0.130	0.132	0.121	0.130	0.132
Mean	22	33	34	22	33	34
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 7 Sensitivity to implementation delays

	(1) NNMR	(2) IMR	(3) Under-2
Born After RSBY + 4 month lag (=1)	0.29	-2.38***	-4.22***
	(0.72)	(0.86)	(0.88)
Observations	306,513	306,513	306,513
R-squared	0.121	0.130	0.132
Mean	22	33	34
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 8 Fertility response to RSBY

	(1) Num Births	(2) Num Births	(3) Num Births	(4) Num Births	(5) Num Births	(6) Num Births
After RSBY (=1)	-0.001	-0.001	0.001	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	428,332	378,059	428,332	378,059	428,332	378,059
R-squared	0.006	0.006	0.011	0.011	0.075	0.082
Sample	YOB>=2007	YOB>=2008	YOB>=2007	YOB>=2008	YOB>=2007	YOB>=2008
Fixed Effect	State	State	District	District	Cluster	Cluster
S.E cluster	State	State	District	District	Cluster	Cluster
Controls	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, * p<0.1. Controls includes year and month of birth fixed effects. The data is organized at the cluster-month level. Num births - Monthly births in a given district (cluster) over the years 2007 to 2016

Table 9 District level program estimates

VARIABLES	(1) NNMR	(2) IMR	(3) U2MR
RSBY during full pregnancy (=1)	-0.23	-4.58***	-6.43***
	(0.90)	(1.02)	(1.03)
Observations	4,084	4,084	4,084
R-squared	0.298	0.348	0.359
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
District FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E cluster	District	District	District

*** p<0.01, ** p<0.05, * p<0.1. Controls include time and region fixed effects.

Table 10 Use of LASSO for identifying controls

VARIABLES	(1) NNMR	(2) IMR	(3) U2MR
Born After RSBY (=1)	0.43	-2.11***	-3.76***
	(0.68)	(0.82)	(0.84)
Observations	295,871	295,871	295,871
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010
Cluster FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
S.E cluster	Cluster	Cluster	Cluster

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 11 Usage of reproductive health care services

	(1) Any ANC	(2) ANC-Doctor	(3) Home Birth	(4) Health Professional	(5) Any PNC	(6) PNC-Doctor
Born After RSBY (=1)	0.015***	0.010**	-0.013***	0.012***	0.014***	0.006
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	244,623	182,576	244,623	244,623	182,091	182,091
R-squared	0.384	0.478	0.419	0.398	0.401	0.384
Mean	.61	.55	.24	.78	.36	.28
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB >= 2010
PSU FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Table 12 Vaccination rates of children

VARIABLES	(1) Measles	(2) BCG	(3) DPT-123	(4) Polio-0123	(5) All Vaccines
Born After RSBY (=1)	0.103***	0.004*	0.053***	0.048***	0.086***
	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
Observations	241,554	241,554	241,554	241,554	241,554
R-squared	0.419	0.319	0.372	0.348	0.354
Mean	.68	.88	.69	.5	.4
Sample	Full	Full	Full	Full	Full
DHS-Cluster FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
S.E cluster	PSU	PSU	PSU	PSU	PSU

*** p<0.01, ** p<0.05, * p<0.1. Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Appendix A: Conceptual Model

We model the relationship between health insurance, health outcomes and access to health services using a household utility maximizing model with a health production function and a benevolent dictator. In this model, a utility maximizing household derives greater value from being healthy and from consuming more of all other non-health related goods (Grossman, 1999). Health in this model is conceptualized as a production function that is concavely increasing in the time spent on creating better health as well as on the quantity of purchased health care goods. The budget constraint of the household is a function of the prices of health care goods, prices of non-medical goods (such as food etc) and wages of household members. The household utility function can be represented as follows:

$$U = u\ (H, Ha, C, L)$$

where H = health of the child, Ha = health of the adult, C = consumption goods other than health and L is leisure. Household utility is concave function.

Child health production function takes the form:

$$H = h(X_h, T_h; H_s\ (Ph), H_a)$$

Here health (H) is produced using health care investments (X_h) and time spent on health care access (T_h). These inputs are conditioned by Hs, the child-specific health stock and Ha, which is the mother's health stock. In this model, investments in mother's health improve the child health stock in-utero. Hence children with health insurance have better health stock at birth. Since health is condtional on the health stock, children with health insurance end up on higher health trajectory over time.

The child's budget constraint can be expressed as:

$$P_c*C + P_h*(X_h + X_a) = w*(1-L-Th) + A$$

where P_c is the price of the consumption good assumed to be numeraire. P_h is the relative price of the health good for the child, X_h is the health care investment in the child and Xa is the health care investment in the mother, w is the real wage and T_h is the time spent by household in producing the health good. A is the non-labor income of the household.

On solving, the reduced form, one derives that greater health and health investments is driven by changes in exogenous factors such as relative prices of health goods as well as wages of individuals conditional on health stock and income of the household.

$$H^* = c(P_c, P_h, T_h; H_s, H_a, Y)$$

In this model governments can play a role in increasing affordability of health care goods (P_h) by providing health insurance.[28] In such a scenario having health insurance reduces the relative prices of health care goods within the households. This can increase health investments and hence health through direct or indirect channels (Currie & Gruber, 1996; Dow & Schmeer, 2003).

The direct channel would include the increase in demand for health care that is covered by health insurance. This include increase in demand for child immunizations that improve child health. The indirect channels include visits to doctors during and after pregnancy and the increase in preference for delivering in hospitals over the home for mothers. Doctors or health professionals, who are now compensated for their time, may provide better quality services when mother's visit. These services can improve health stock of the child through health investments in their mother. Health investment in-utero and post-birth lead to improvements in health and reductions in child mortality.

Even in situations where health insurance does not cover either reproductive or child health services, insurance can still stimulate demand through a cross price effect. Falling costs of health services may lead to households reallocate savings towards the purchase of health services for pregnant women and children. In this case too, children who are in-utero benefit from their mother's health investments (Almond & Currie, 2011a). Even if child immunizations are provided for free, like they are in India, the cross price effects of the falling health care costs and increase contact visits to doctors can stimulate demand for the same. Here too, gains made to child health lead to reductions in mortality.

[28] In low income markets, (Aiyar & Venugopal, 2019) find evidence that government sponsored health insurance programs foster market inclusion and increase out of pocket spending for lower income populations.

Appendix B: Testing Randomness in program roll out

In the table below, we assess if district level development predictors predict the implementation of the RSBY. If the programme was initially made available in more developed districts, then this would bias us against finding any effect. If the programme was made available in less developed districts, then over time one would see gains that were larger than the true population effect. In table B1, the outcome variables take a value one if the district had RSBY in a year t and zero otherwise. The control variables include the night light composite of the district from the NDHS 2015-16. The sex ratio, literacy rates and the population density of the district is from the 2011-12 Indian census. The table shows us that higher night lights and a better sex ratio predict roll out in 2011 and 2012. This would imply that the bias for finding a true population effect would against us. In both 2013 and 2014, when most of the implementation was complete, whether or not a district had the programme was completely random with regards to these indicators.

Table B1: District Development Indicators and RSBY availability

	(1) RSBY in 2010	(2) RSBY in 2011	(3) RSBY in 2012	(4) RSBY in 2013	(5) RSBY in 2014
Night Lights (0-60)	0.00041	0.00174*	0.00148	0.00097	0.00145
	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)
Sex Ratio	0.00002	-0.00004	0.00041*	-0.00009	-0.00023
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Literacy	0.02118	0.16361	0.11336	-0.02604	-0.00690
	(0.022)	(0.217)	(0.183)	(0.066)	(0.072)
Population Density	-0.00000	-0.00000	-0.00001	-0.00000	-0.00000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	668	668	668	668	668
R-squared	0.062	0.556	0.644	0.752	0.807
State FE	Yes	Yes	Yes	Yes	Yes
S.E cluster	State	State	State	State	State

These are district-level regressions where the independent variable is a categorical variable taking a value of one if RSBY was implemented in the district in that particular year.

Appendix C: Data & Variable Information

Table C1: Data Sources & Description of Outcomes

VARIABLES	Dataset	Definition	Sample
Neo-Natal Mortality rate (NNMR)	NFHS IV	Child death reported within the first month	Full
Infant Mortality Rate (IMR)	NFHS IV	Child death reported within the first year	Full
Died before age 2 (Under-2)	NFHS IV	Child death reported within the first 2 years	Full
Any ANC (Ante Natal Care)	NFHS IV	Did the individual report any ANC	Upto 3 births between 2010 and 2015-16
ANC first Trimester	NFHS IV	If first visit was before 10 weeks	Upto 3 births between 2010 and 2015-16
ANC Second Trimester	NFHS IV	If first visit was 10-24 weeks	Upto 3 births between 2010 and 2015-16
ANC Doctor	NFHS IV	If a doctor attended the patient during any ANC visit	Upto 3 births between 2010 and 2015-16
Home Birth	NFHS IV	If the birth took place at home	Upto 3 births between 2010 and 2015-16
Delivery health professional	NFHS IV	If the birth was assisted by a doctor	Upto 3 births between 2010 and 2015-16
Any PNC (Post Natal Care)	NFHS IV	If mother visited doctor within the first two months	Most Recent birth only
PNC Doctor	NFHS IV	If a doctor attended the patient during the PNC visit	Most Recent birth only
Any JSY[3] in district in birth year	NFHS IV	Any mother reported being covered by JSY in district d in each year between 2010 & 2015-16	Upto 3 births between 2010 and 2015-16

Measles Vaccination	NFHS IV	Did the child receive MCV1	Children under five
DPT Vaccine	NFHS IV	Did the child receive 3 doses of DPT vaccine	Children under five
BCG Vaccine	NFHS IV	Did the child receive BCG1	Children under five
Polio Vaccine	NFHS IV	Did the child receive 4 doses of polio vaccine	Children under five
Any Vaccine	NFHS IV	Did the child receive MCV, BCG, DPT(all), Polio(all) vaccines	Children under five
Rainfall Deviation[4]	Univ. of Delaware Data	Log(rainfall in a given month) minus log(long term rainfall mean)	All districts

Appendix D: Other Datasets Used

Census 2011-12

The census 2011-12 provides district level information on outcomes such as population, population density, sex ratio, literacy and so on through their publically available website. We use this data as is after matching with the district level information from the RSBY programme datasets to test if there are any systematic patterns in rollout that are correlated with district level socio-economic indicators.

District Level Health Surveys (1997, 2002, 2007)

The district level health surveys (DLHS) is a publically available dataset that contains detailed information on child level health and mortality indicators for India. It is a repeated cross section that is representative at the district level. Child mortality outcomes are constructed using the birth histories of mothers who are interviewed during the surveys. We also use identifiers, state identifiers and birth year information to conduct the pre-trend analyses. Districts that have RSBY by 2014 are assigned a value 1 and all others 0 when comparing the child mortality outcomes from the DLHS surveys used in the analysis.

Appendix E: Additional Event Study Analyses of Outcomes

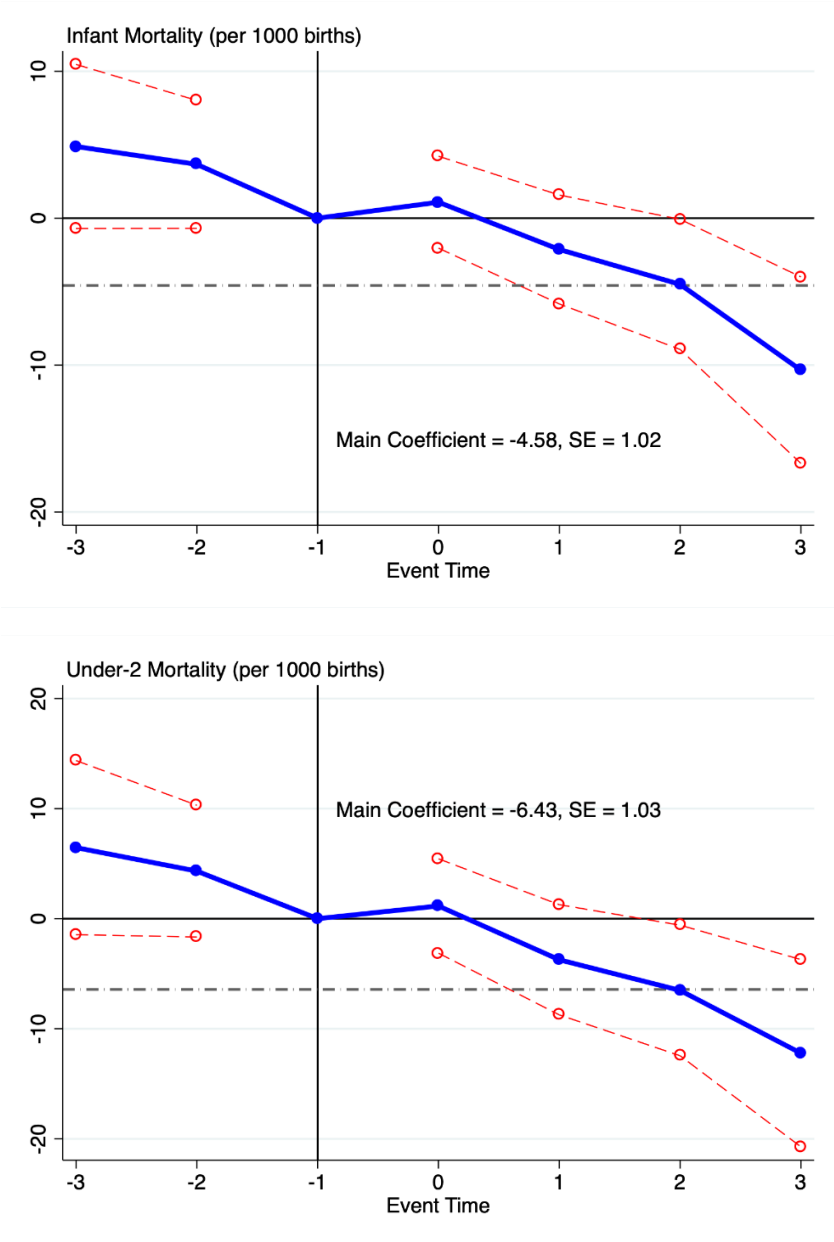

Figure E1 – Event Study at the District Level - The figure presents the effects of RSBY on Infant mortality (IMR) and Under-2 mortality (0/1) using district level regressions. The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes district fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered by district.

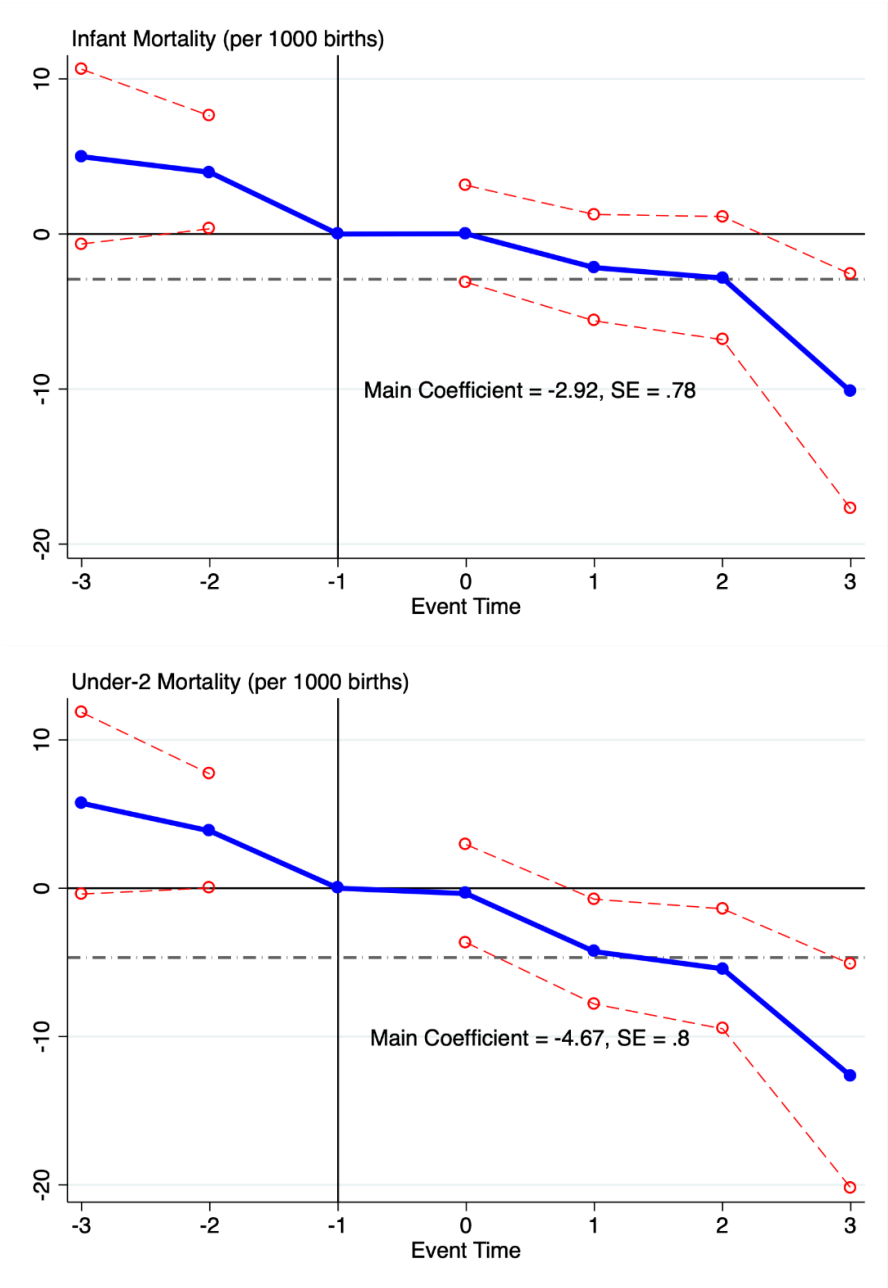

Figure E2 - Event Study Analysis - District Level with District FE The figure presents the effects of RSBY on Infant mortality (IMR) and Under-2 mortality (0/1) using district level regressions. The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and district fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered by district.

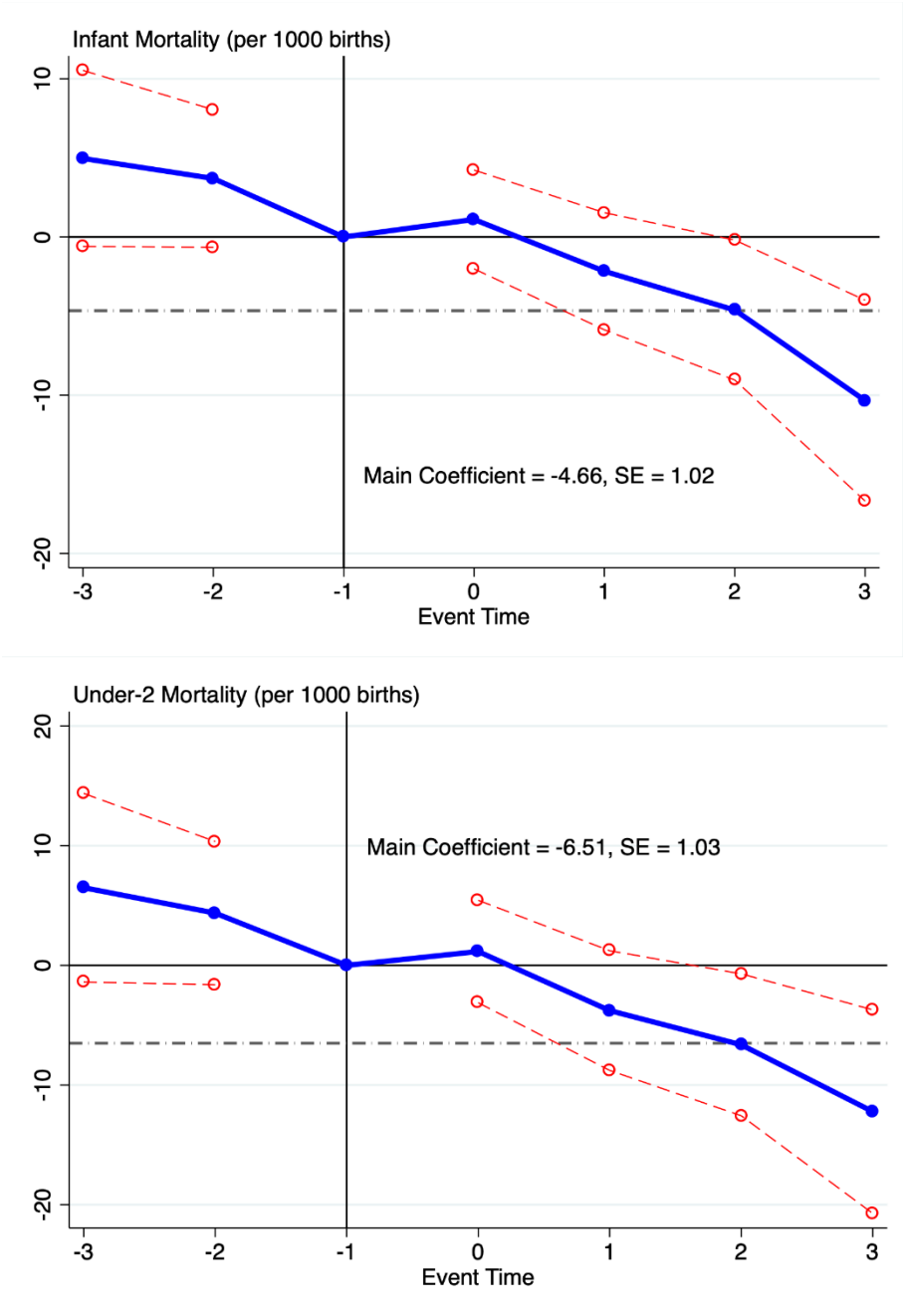

Figure E3 - Event Study Analysis - District Level - Only RSBY districts - The figure presents the effects of RSBY on Infant mortality (IMR) and Under-2 mortality (0/1) using district level regressions. The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes district fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered by district.

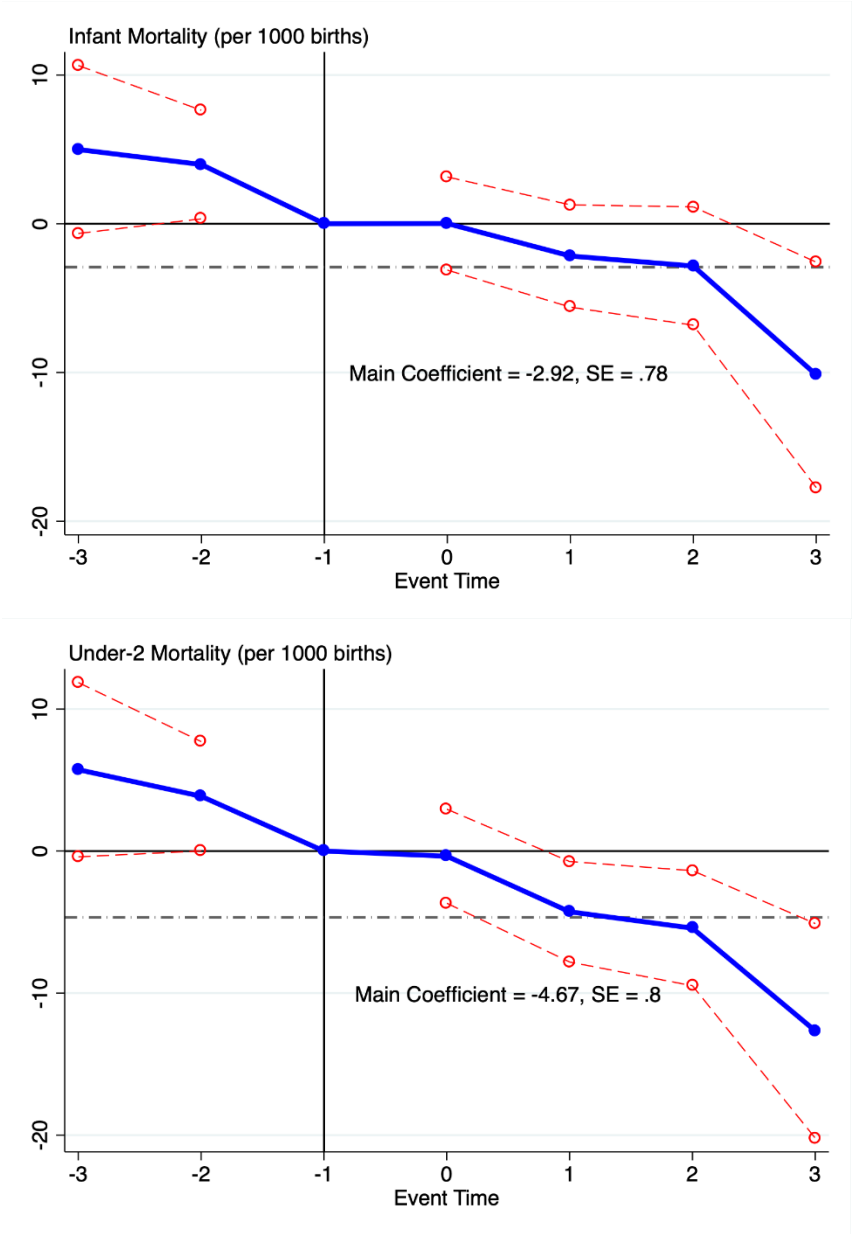

Figure E4 - Event Study Analysis - District Level with District FE – RSBY districts only - The figure presents the effects of RSBY on Infant mortality (IMR) and Under-2 mortality (0/1) using district level regressions. The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and district fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered by district.

Appendix F: Eliminating JSY driving these results

The Janani Suraksha Yojana (JSY) focused on increasing institutional deliveries through the provision of cash transfers. Women were eligible for cash bonuses if they delivered children in *public* health institutions. The RSBY, on the other hand, covered women for deliveries in both public and private health institutions. If the JSY was effective and not the RSBY, then one would see an increase in access of public health institutions. If it was the RSBY driving the results, then one would expect to see increase in births in either private or public facilities since the women had the choice available to them. In line with this hypothesis we show that births in private health institutions, measured by `any births in private hospitals' and `births in private hospitals conditional on institutional deliveries', increase during the study period (Table D1). This further supports our study's finding that it is the RSBY driving this result.

Table F1: Birth Place Preferences for Women

VARIABLES	(1) Home Birth	(2) Govt Hosp. Birth	(3) Pvt Hosp. Birth	(4) Pvt Hosp. Birth
Born After RSBY (=1)	-0.013***	0.003	0.010***	0.008**
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	244,623	244,623	244,623	185,268
R-squared	0.419	0.349	0.418	0.449
Mean	.24	.55	.21	.28
Sample	YOB >= 2010	YOB >= 2010	YOB >= 2010	YOB>=2010+Conditional on Institutional births
PSU FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
S.E clustering	PSU	PSU	PSU	PSU

Controls: Child birth order, whether child is a single birth, the gender of child. Mother related controls include, mother's age during the child's birth, education & BMI. Household level variables include urban dummy, wealth score, household access to piped water and toilet. In addition, all specifications include dummies for religion and caste. At the district level, presence of the Janani Suraksha Yojana (JSY) at the time of birth, district-level rainfall deviation in the month of birth, and cluster (PSU) fixed effects are included. Month fixed effects are also included.

Appendix G: Event Study Analyses of Mechanisms

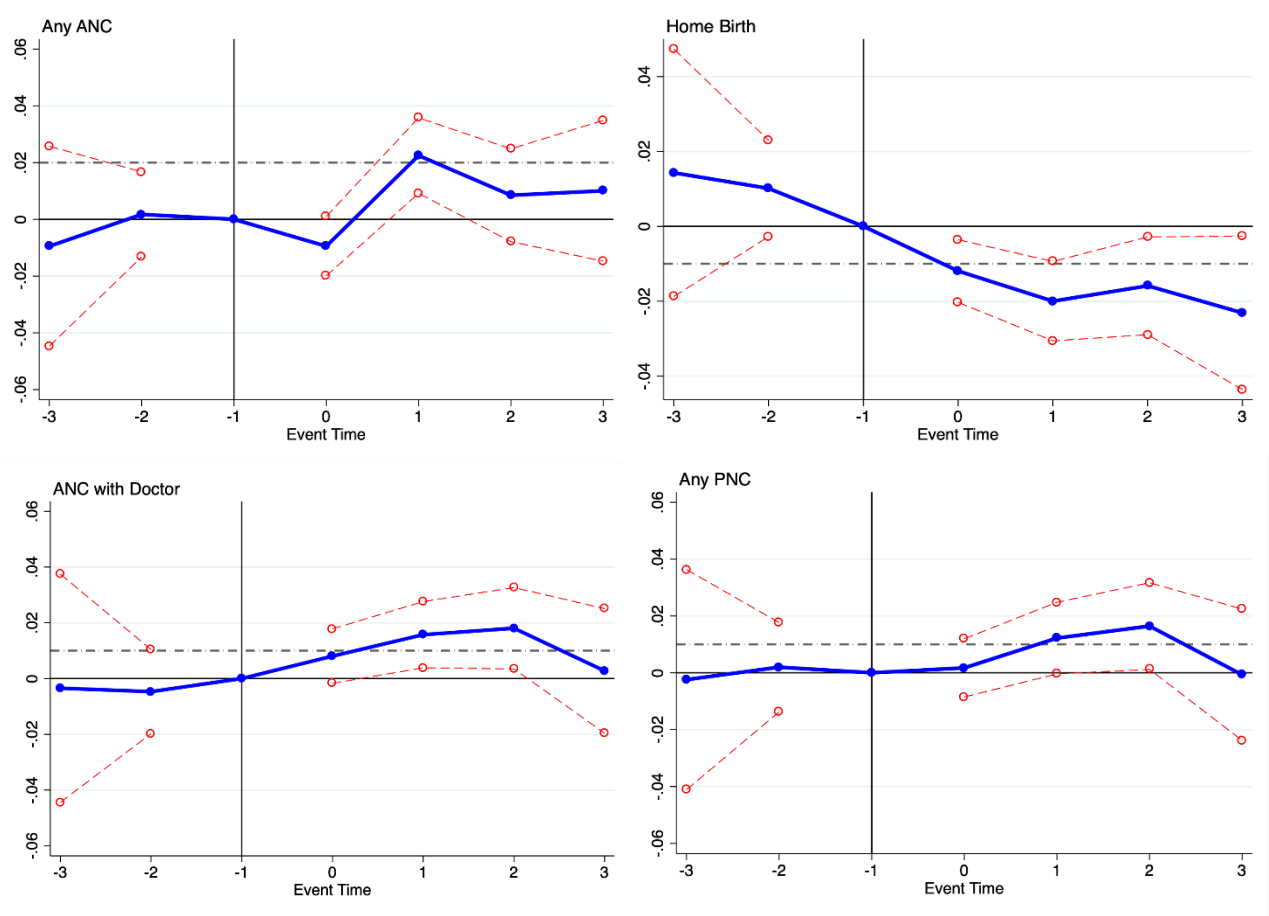

Figure G1 - Event Study Analysis -- ANC \& PNC Outcomes - Sample is based on birth-level data from DHS 2015. The figure presents the effects of RSBY on different antenatal and postnatal care outcomes. The time intervals are calculated based on the exact dates of implementation of the RSBY policy within each district. Event Time (T) = 0 is the year (365 days) after RSBY was implemented in the district. For instance, if the treatment district received the RSBY policy on 1 February 2012, then t = 0 represents the period between 1 February 2012 and 31 January 2013, while t = 1 represents the period between 1 February 2013 and 31 January 2014 and so on. The estimates presented here are intent-to-treat effects of RSBY relative to the year (365 days) before RSBY was implemented in the district (T = --1). The regression equation includes the full set of controls and PSU fixed effects. 95 percent confidence intervals are presented, and standard errors are clustered at the PSU level.