The Impact of India's National Health Insurance Program on Adverse Pregnancy Outcomes

Caroline Mary Joyce

Department of Epidemiology, Biostatistics, and Occupational Health

School of Population and Global Health

McGill University, Montreal, QC

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Abstract

India has persistent socioeconomic inequalities in maternal and perinatal health outcomes, particularly in rates of stillbirth, abortion, and miscarriage. There is not universal health insurance, and households at or below the poverty line have high out-of-pocket healthcare costs. In 2008, the Government of India (GoI) launched a health insurance scheme, the Rashtriya Swasthya Bima Yojana (RSBY) health insurance program, to provide coverage for inpatient health services for families below the poverty line (BPL). The impact of the RSBY program on health outcomes has not been rigorously investigated. The overarching aim of this thesis was to evaluate the impact of RSBY on adverse pregnancy outcomes, and socioeconomic inequalities therein, using a novel combination of survey data with information on pregnancies from 2004 to 2019.

The first manuscript provides national estimates of changes in socioeconomic inequalities in pregnancy outcomes in India across 15 years. We observed persistent socioeconomic inequalities in rates of stillbirth and abortion from 2004 – 2019. Women who completed primary school and those at the top of the household wealth distribution were more likely to report an abortion and less likely to experience a stillbirth. There was less consistent evidence for socioeconomic inequalities in miscarriage, which increased for all groups over the study period. These results indicate that despite targeted investments in healthcare programs for the most socioeconomically disadvantaged, disparities in pregnancy outcomes persist.

In the second manuscript we evaluated the causal impact of access to RSBY on rates of stillbirth, abortion, and miscarriage. We leveraged the phased-in implementation of the policy across districts to estimate the impact of district-level access to RSBY using a difference-in-differences design. We accounted for the staggered timing of the policy roll-out by classifying

districts by when they received access to the policy. We did not observe an overall impact of access to RSBY on the probability of stillbirth, with a risk difference (RD) of 0.1 additional stillbirths per 1,000 pregnancies (95% CI: -1.5, 1.7), nor on abortion (RD = -1.7, [95% CI: -9.1, 5.7]). Access to RSBY was associated with 6.3 additional miscarriages per 1,000 pregnancies (95% CI: 0.9, 11.7). We conclude that access to RSBY was not associated with an overall improvement in pregnancy outcomes, and in fact was associated with an increased probability of miscarriage.

In the third manuscript, we examined the causal impact of household enrollment in RSBY on rates of stillbirth, abortion, or miscarriage. The instrumental variables (IV) approach uses the impact of district-level access to RSBY on household-level RSBY participation (the "first stage") to estimate the effect of household-level RSBY coverage on our primary outcomes of stillbirth, abortion, and miscarriage (the "second stage"). Average household enrollment remained low over the study period, with the highest level of enrollment seen in districts that received access between 2008 – 2010. Enrollment in RSBY was associated with a 1.8 percentage-point (95% CI: 1.1, 2.5) increase in the probability of a pregnancy ending in stillbirth. Enrollment was also associated with a -2.1 percentage-points (95% CI: -4.0, -0.2) decreased probability of abortion, and a 2.0 percentage-point (95% CI: 0.0, 3.9) increased likelihood of miscarriage. These findings contribute additional evidence on the lack of beneficial impact RSBY had on improving pregnancy outcomes in India.

This thesis provides a comprehensive evaluation of India's National Health Insurance Program on adverse pregnancy outcomes. We find that India has persistent socioeconomic disparities in rates of stillbirth, abortion, and miscarriage, and that the RSBY health insurance policy for BPL households did not decrease the probabilities of these adverse pregnancy outcomes.

Résumé

L'Inde connaît des inégalités socio-économiques persistantes dans les résultats de santé maternelle et périnatale, en particulier pour la mortinaissance, l'avortement et la fausse couche. L'absence d'assurance maladie universelle entraîne des coûts élevés pour les ménages vivant au-dessous du seuil de pauvreté (SDP). En 2008, le gouvernement indien a lancé le Rashtriya Swasthya Bima Yojana (RSBY), offrant une couverture hospitalière aux familles SDP. L'impact du RSBY sur la santé n'a pas été étudié rigoureusement. Cette thèse évalue son influence sur les grossesses de 2004 à 2019.

Le premier manuscrit décrit les inégalités socio-économiques persistantes dans les résultats de grossesse en Inde sur 15 ans. Les taux de mortinaissance et d'avortement restent disparates. Les femmes instruites et fortunées ont moins de risques d'avortement et de mortinaissance, indiquant des inégalités persistantes malgré les investissements ciblés.

Le deuxième manuscrit évalue l'impact causal de l'accès au RSBY sur la mortinaissance, l'avortement et la fausse couche. L'accès au RSBY n'améliore pas la probabilité de mortinaissance (RD de 0,1 pour 1 000 grossesses) ni d'avortement (RD = -1,7). L'accès est associé à 6,3 fausses couches supplémentaires pour 1 000 grossesses. Les résultats varient entre les districts, indiquant une absence d'amélioration globale et même une augmentation des fausses couches.

Le troisième manuscrit examine l'impact de l'inscription au RSBY sur la mortinaissance, l'avortement et la fausse couche. L'accès au RSBY au niveau du district augmente l'inscription au niveau du ménage. L'inscription est associée à une augmentation de 1,8 point de pourcentage de la probabilité de mortinaissance, à une diminution de 2,1 points de pourcentage de la probabilité d'avortement et à une augmentation de 2,0 points de pourcentage de la probabilité de fausse

couche. Ces résultats confirment le manque d'impact bénéfique du RSBY sur les résultats de grossesse en Inde.

Cette thèse offre une évaluation complète du programme d'assurance maladie nationale de l'Inde sur les résultats indésirables de la grossesse. Les inégalités persistent, et le RSBY n'a pas réduit les risques de mortinaissance, d'avortement et de fausse couche pour les ménages SDP.

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Contribution of authors

The manuscripts that comprise this thesis and the contributions of the co-authors are listed below. During my doctorate I was lead author on five research manuscripts, I present three of them as my thesis. The data used as part of this thesis come from three separate household surveys conducted in India. Further information on these survey datasets can be found in the methods chapter. I conceived of the research questions in this thesis and developed the analytic plan. I merged and harmonized all data used for this research, developed new variables, modified the statistical methods to correctly conduct the analyses, and generated the manuscripts.

I worked closely with my supervisor, Dr. Arijit Nandi, who provided substantive guidance through every stage of this research. He helped refine my research questions and build an analysis plan for all my aims. Dr. Nandi reviewed all manuscripts and gave detailed feedback.

My thesis committee, Dr. Arnab Mukherji, contributed substantive guidance to both the project overall and to each manuscript. Dr. Mukherji and Deepti Sharma contributed to the research design and gave important feedback on the interpretation of all results.

Manuscript 1: Joyce CM, Sharma D, Mukherji A, and Nandi A. Socioeconomic Inequalities in Adverse Pregnancy Outcomes in India: 2004 – 2019. Preparing for submission to The Lancet Global Health

Manuscript 2: Joyce CM, Sharma D, Mukherji A, and Nandi A. Impact of district-level access to RSBY on adverse pregnancy outcomes from 2004-2019: a difference-in-differences analysis. Being prepared for submission for publication.

Manuscript 3: Joyce CM, Sharma D, Mukherji A, and Nandi A. Impact of household enrollment into RSBY on rates of adverse pregnancy outcomes: an instrumental variable analysis. Being prepared for submission to publication.

Statement of originality

This thesis work presented is an original contribution on the impact of India's National Health Insurance Program on adverse pregnancy outcomes, presented across three research manuscripts. I am incredibly grateful to my supervisor and thesis advisory committee for their indispensable help in guiding my work. I was a co-investigator on the RSBY grant "The impact of the publicly-funded health insurance on social inequalities in use of sexual, reproductive, and maternal health services, and on maternal, newborn, and infant health: an evaluation of the Rashtriya Swasthya Bima Yojana scheme in India" and conceived and executed my specific thesis research questions, objectives, and analyses related to the broader project aims. I identified research gaps in the impact of this policy on maternal and reproductive health, and strove to address these gaps by: (1) applying empirical methods to quantify socioeconomic inequalities in adverse pregnancy outcomes and (2) using quasi-experimental designs to evaluate the impact of access to and enrollment in RSBY on these outcomes.

In my first manuscript, I quantified socioeconomic inequalities in rates of stillbirth, abortion, and miscarriage in India from 2004 – 2019. In manuscript 2, I used a difference-in-differences analysis that accounted for heterogeneous treatment timing to estimate the "intention-to-treat" effect of district-level access to RSBY on rates of these outcomes, as well as differences across socioeconomic strata. In my third manuscript, I extended these analyses by examining the effect of household-enrollment in RSBY on my primary outcomes using a two-stage least squares instrumental variable analysis. In these analyses, I extended the traditional two-stage least squares approach to account for heterogeneous treatment timing in the first-stage.

This thesis generated new evidence on trends in social inequalities, the impact of district-level access to RSBY, and the effects of household enrollment in RSBY on adverse pregnancy outcomes through the application of rigorous methods, including “frontier” causal inference methods for policy evaluation.

List of abbreviations

Government Of India (GoI)

Rashtriya Swasthya Bima Yojana (RSBY)

Below The Poverty Line (BPL)

Difference-in-Differences (DiD)

Instrumental Variable (IV)

Out-Of-Pocket (OOP)

Ayushman Bharat-Pradhan Mantri Jan Arogya Yojana (AB-PMJAY)

United Nations (UN)

Sustainable Development Goal (SDG)

World Health Organization (WHO)

Antenatal Care (ANC)

Skilled Birth Attendant (SBA)

Socioeconomic Status (SES)

Scheduled Caste (SC)

Scheduled Tribe (ST)

Empowered Action Group (EAG)

National Rural Health Mission (NRHM)

Janani Suraksha Yojana (JSY)

Accredited Social Health Activists (ASHAs)

National Family Health Survey (NFHS)

Demographic And Health Survey (DHS)

Ministry of Health And Family Welfare (MoHFW)

District Level Household Surveys (DLHS)

International Institute for Population Science (IIPS)

Annual Health Survey (AHS)

Primary Sampling Units (PSUs)

Census Enumeration Blocks (CEBs)

Average Treatment Effect On The Treated (ATT)

Two-Way Fixed Effects (TWFE)

Confidence Interval (CI)

Randomized Controlled Trial (RCT)

Extended Two-Way Fixed Effects (ETWFE)

Two-Stage Least Squares (2SLS)

Slope Index Of Inequality (SII)

Relative Index Of Inequality (RII)

Generalized Linear Models (GLMs)

Risk Difference (RD)

Risk Ratio (RR)

Complier Average Causal Effects (CACE)

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Chapter 1. Introduction

Stillbirth, abortion (sometimes referred to as induced miscarriage), and spontaneous miscarriage (hereafter just miscarriage), are often referred to as adverse pregnancy outcomes.[1] To define these terms for the purpose of this thesis: if a pregnancy ends in stillbirth or miscarriage, it was not viable due to either genetic abnormalities or poor health of the mother. Abortion is life-saving care that should be unconditionally supported, but it also reflects a pregnancy that was not viable or not wanted. Abortion is often also necessary as prevention or treatment for stillbirths and miscarriages. Tracking rates of these adverse pregnancy outcomes informs government and health officials about the state of maternal and reproductive health in their country.

India has high rates of stillbirth and miscarriage, which contribute to poor overall maternal and neonatal health.[2] Additionally, there are socioeconomic disparities in all of these outcomes, indicating that those of lower socioeconomic status may be more likely to experience stillbirth and miscarriage, and less likely to have access to an abortion when it is needed.[3,4] There are many potential explanations for this including differential access to, and utilization of, healthcare.[5] Government health facilities provide free healthcare for all Indian citizens, though these facilities are often lacking in basic necessities.[6] In order to access better healthcare, many households pay out-of-pocket (OOP) for private doctors and hospitals. India does not provide universal health coverage to help defray the costs of this private healthcare, with only approximately 37% of the population enrolled in either a commercial or public health insurance program.[7] Many households are therefore still responsible for the OOP costs for private healthcare, and high health costs have been shown to be a barrier to accessing maternal and reproductive healthcare.[8]

To reduce rates of all-cause morbidity and mortality, along with socioeconomic disparities in health, the Government of India (GoI) launched the Rashtriya Swasthya Bima Yojana (RSBY) in 2008. This government-sponsored insurance program aimed to provide coverage for healthcare services received in hospitals by households below the poverty line (BPL).[9] With its focus on socially disadvantaged groups, the introduction of this program had the potential to remove barriers to accessing healthcare – both directly and indirectly – with the ultimate goal of decreasing adverse health outcomes among those enrolled.[10] Healthcare services covered by this policy included those imperative for reducing rates of adverse pregnancy outcomes, such as hospital delivery, including delivery by caesarean section.[11]

Examining the impact of RSBY on population health, including levels and inequalities in pregnancy outcomes such as stillbirth, abortion, and miscarriage, is policy relevant, as the GoI expands health insurance access through a new health insurance program modeled after RSBY, Ayushman Bharat-Pradhan Mantri Jan Arogya Yojana (AB-PMJAY). While some studies have investigated the impact of RSBY on maternal and child health, most of the work has been descriptive, precluding inference regarding the causal impact of the program, and few studies have explored whether effects are heterogeneous across socioeconomic strata. In this thesis, I attempt to quantify trends in socioeconomic disparities in stillbirth, abortion, and miscarriage rates, and estimate the impact of RSBY and enrollment in the program on these outcomes.

1.1 Research Objectives

The goal of this doctoral thesis was to provide a rigorous evaluation of the impact of RSBY on levels and social inequalities in adverse pregnancy outcomes across India. The specific research objectives of this thesis are as follows:

1. To measure social inequalities in rates of stillbirth, abortion, and miscarriage, and how these inequalities vary over time (Manuscript 1).
2. To evaluate the causal impact of district-level access to RSBY on levels and social inequalities in rates of stillbirth, abortion, and miscarriage across India (Manuscript 2).
3. To evaluate the causal impact of household enrollment in RSBY on the probability of a pregnancy ending in stillbirth, abortion, or miscarriage among women that gained access to the RSBY program during the study period (Manuscript 3).

1.2 Structure

This manuscript-based thesis contains seven chapters. In chapter 2, I describe the epidemiologic profile of maternal and reproductive health in India and introduce historical health insurance reforms in India. I also present past evidence on the impact of RSBY, along with addressing the gaps in the extant literature. In chapter 3, I provide an overview of the data and analytical methods used to investigate my research objectives. In chapter 4, I quantify socioeconomic inequalities in rates of stillbirth, abortion, and miscarriage in India using the slope and relative indices of inequality. In chapter 5, I estimate the impact of RSBY on rates of stillbirth, abortion, and miscarriage, as well as stratified estimates by indicators of socioeconomic position, using a difference-in-differences design that accounts for the heterogeneous timing of policy implementation. In chapter 6, I present causal estimates of the impact of enrollment in RSBY on the probability of a pregnancy ending in stillbirth, abortion, or miscarriage among women that gained access to the health insurance policy using an instrumental variable analysis. In chapter 7, I summarize the key findings of this thesis and offer suggestions for further research on the evaluation of healthcare reforms in India.

Chapter 2. Literature Review

In this chapter I provide an epidemiologic profile of pregnancy outcomes and socioeconomic disparities in these outcomes in India. I then summarize research on socioeconomic inequalities in healthcare access, including government-sponsored programs aimed at decreasing health disparities. Finally, I introduce the Rashtriya Swasthya Bima Yojana (RSBY) health insurance program and summarize the existing literature that examines the impact of this scheme on health outcomes in India.

2.1 Maternal and Reproductive Health in India

India has made historic gains in improving maternal and reproductive health over the last 20 years,[12] with an estimated 70% decrease in the maternal mortality ratio (MMR) between 1997 and 2020.[13] Despite these improvements, rates of maternal death remain high with an estimated maternal mortality ratio of 103 deaths per 100,000 live births in 2020.[14] India is not on track to meet the 2030 Sustainable Development Goal (SDG) 3.1: "to reduce maternal mortality ratio to less than 70 per 100,000 live births".[15] India is also not on track to meet SDG 3.7: "to ensure universal access to sexual and reproductive healthcare services",[16] which is integral for lowering rates of maternal and infant mortality. Non-live birth pregnancy outcomes – specifically stillbirth, abortion, and miscarriage – are an important metric to consider in the context of maternal and neonatal mortality. Stillbirth and recurrent miscarriage are risk factors for maternal morbidity and mortality and must be addressed to achieve SDG 3.1.[17]

India has disproportionately high rates of stillbirth and miscarriage relative to live births — it accounts for 19% of births worldwide but 22.6% of the global burden of stillbirths, more than any other country.[18] India also has higher rates of first and recurrent miscarriage than the global average, including miscarriage occurring after the first trimester. [19-22] Recurrent pregnancy loss – both stillbirth and miscarriage – is associated with an increased risk of maternal death and serious health complications, and a history of recurrent miscarriage is associated with a higher risk of neonatal morbidity and mortality. [23,24] Reducing rates of stillbirth and miscarriage is a key strategy to improve maternal and neonatal mortality in India.

Access to abortion is one of the key determinants of stillbirth and miscarriage. According to the World Health Organization (WHO), access to modern contraception and safe induced abortion is critical for preventing unplanned pregnancies, and lowering rates of miscarriage and stillbirth.[25] Additionally, the medical procedure used for abortion (both surgery and medication) is also often used as a treatment for miscarriage.[26] India legalized abortion in 1971[27] and granted widespread access to medication-induced abortion in 2003,[28] and therefore has comparatively liberal abortion laws. However, legality and availability does not guarantee Indian women will be able to access and avail themselves of these procedures and treatments.

There are additional preventable risk factors for stillbirth and miscarriage. Lack of antenatal care (ANC) and giving birth without a skilled birth attendant (SBA) are associated with higher risk of adverse pregnancy outcomes, and maternal morbidity and mortality.[29] Poor nutrition during pregnancy, including iron deficiency,[30] also increases the risk of experiencing adverse pregnancy outcomes.[31] Additional risk factors include living far from medical care, having an unmet need for contraception and/or unwanted pregnancy, and maternal age younger than 20 or older than 35.[32-34] These risks are exacerbated among people of low socioeconomic status (SES).[35] The conceptual framework for the hypothetical relationship between socioeconomic characteristics and adverse pregnancy outcomes is shown in **Figure 2.1**.

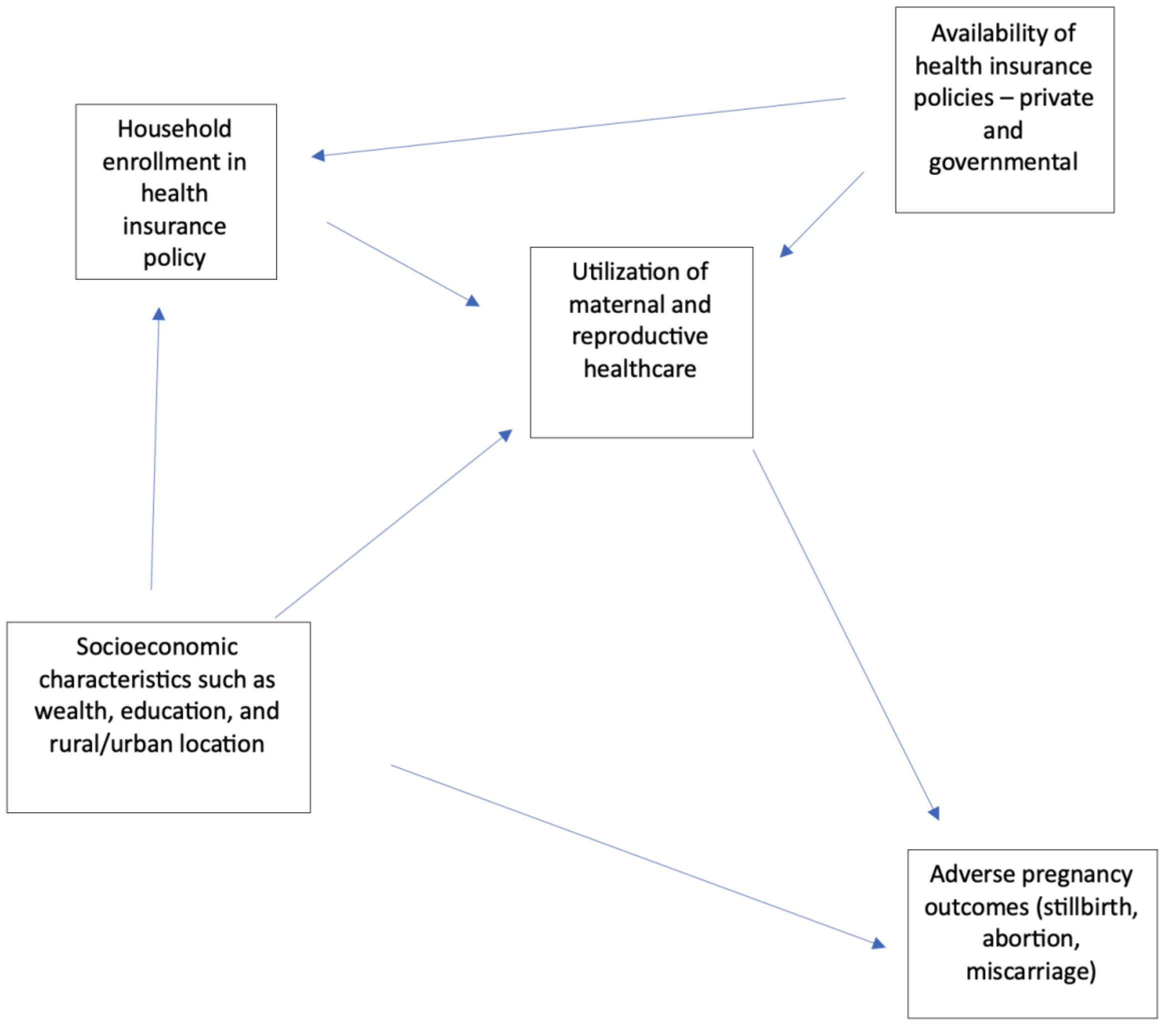


Figure 2.1 Conceptual framework illustrating the relation between socioeconomic characteristics and adverse pregnancy outcomes

2.2 Socioeconomic inequalities in India and their impact on health

Although India has experienced remarkable economic growth and urbanization in recent years, recent estimates show that approximately 64% of the Indian population still lives in a rural location.[36] Rural regions often lack access to essential resources for growth and development, such as clean water, healthcare, and education, contributing to lower life expectancy and limited economic opportunities compared to urban areas. Rural areas have lower access to healthcare and higher rates of adverse health outcomes, including maternal and infant mortality.[37] While various targeted programs from the central and state governments have attempted to address these systematic inequalities, disparities in health outcomes continue to persist across the rural/urban divide in India.[38]

India also has persistent wealth inequalities. Despite an overall reduction in poverty, wealth inequality has been increasing due to gains in wealth among the very rich.[39] Recent estimates show that the top 10% of the wealthiest households in India control 77% of total national wealth.[40] The poor in India have less access to health care services and often face high OOP expenditures to utilize care.[41] The most socioeconomically disadvantaged states in India contribute the majority of all-cause morbidity and mortality in India, and have rates of maternal and infant death that are disproportionately high relative to their population size.[42]

Another dimension for examining social inequalities is by membership in a Caste or Tribe. Scheduled Castes and Scheduled Tribes are groups of people that the UN defines as enduring, both historically and currently, systemic social, economic, and political discrimination that impacts all levels of life.[43] Approximately 25% of Indians identify as a member of Scheduled Caste (SC), with 9% identifying as Scheduled Tribe (ST).[44] Poverty rates remain high in these groups, particularly among members of Scheduled Tribes.[45] Members of these groups face additional barriers to accessing essential healthcare, and can face bias in their interactions with the healthcare system.[46] Additionally, women in these groups face the "triple burden" of discrimination due to their gender, socioeconomic status, and caste status, which has been shown to influence their ability to access maternal and reproductive health services.[47,48] Women in these groups are at higher risk of maternal morbidity and mortality.[49,50]

Previous work has examined socioeconomic disparities in adverse pregnancy outcomes. Socioeconomically disadvantaged women have less access to modern contraceptives and safe abortion services,[4] often due to financial barriers that prevent women from accessing this type of care. Socioeconomically disadvantaged women in India are also at greater risk for stillbirth and miscarriage because they are less likely to have access to ANC, deliver with a SBA,[51,52] and have adequate nutrition.[53] Women with less formal schooling, including illiterate women, are also more likely to experience stillbirth and miscarriage.[3,12]

These barriers can interact when women attempt to access healthcare, including abortion services. While surgical abortion is technically free at public facilities, women may be subject to other costs,[54] including the direct costs of the anesthesia and medication necessary for the procedure,[55] along with indirect costs such as missing work or transportation to the facility.[56] The drugs required for medication-induced abortion are not free, and women must pay OOP to access this care.[54,57] Without access to quality, affordable, maternal and reproductive healthcare, including abortion, women of lower socioeconomic status are more likely to experience unplanned pregnancies and receive inadequate ANC.

In part to address these socioeconomic inequalities, in 2001 the GoI designated the eight states with the worst socioeconomic profiles and that contribute most of the all-cause morbidity and mortality in India as the Empowered Action Group (EAG) states. These states have been the focus of targeted interventions to address poor health outcomes. In 2005 the GoI launched the National Rural Health Mission (NRHM) in eighteen states (including EAG states) which aimed to increase access to affordable, quality healthcare, including maternal and infant care.[58] One example of this initiative, Janani Suraksha Yojana (JSY), provided cash incentives for pregnant women to deliver in health centers, and in 2011 expanded to make delivery free at a health center for all pregnant women.[59] Reductions in maternal and infant mortality were observed after the introduction of these initiatives.[60,61] However, program implementation was variable by state, with poorer women more unlikely to receive these payments,[62] and further research suggests that these reductions subsequently plateaued.[63] Another aspect of the NRHM was deployment of Accredited Social Health Activists (ASHAs), which served as a link between the community, particularly women and children, and the health system.[64] Overall the program was considered to have achieved its goal of reaching women and children in the community.[65] However, as is not uncommon with programs of this scale, implementation across India was not uniform and evaluations of the effectiveness of ASHAs varied substantially by location, mandate, and year.[66]

2.3 Health Insurance in India

India does not have a universal health insurance scheme. Health costs are high, with those at or below the poverty line spending a disproportionate percentage of their income on healthcare expenditures.[67,68] Historically, health insurance has been offered through a combination of private (i.e. employer-sponsored) and public (state and central government) schemes. Large segments of the population, particularly BPL households, work in the informal or unorganized labor sector and are therefore unable to receive private health insurance.[69] Although the national government has offered the Central Government Health Scheme for federal government workers since 1954, it was not until the 2000s that state governments began to offer health insurance for secondary and tertiary care that aimed to prevent catastrophic health expenditures. These schemes were only introduced in a few states: Karnataka, Andhra Pradesh, and Tamil Nadu. Coverage across India therefore remained quite uneven, with some states taking the step to offer robust coverage, particularly for BPL households, and other states offering no health insurance policy.

In part to close this gap in coverage for BPL families, the GoI launched the RSBY program in 2008.[70] Similar to these state-level programs, RSBY covered tertiary care, with the goal of increasing access to needed inpatient hospital-based services. Primary and secondary care, i.e., outpatient services, were not included. Conceived by the central government, states were instructed to administer the program and share in the financing. Under the program, eligible households of up to five family members were covered for up to INR 30,000 annually (roughly USD $500 in 2008). The list of services covered was determined by the central government and is therefore standardized across states. Covered services included those the government recognized as imperative for lowering maternal and neonatal morbidity and mortality. These included in-hospital delivery, including delivery by caesarean section. Public and private hospitals that satisfied requirements set by the state for care then entered into contracts (empaneling) with insurance companies. Hospitals invoiced insurance companies directly.

India's districts are administrative subdivisions within states and union territories, serving as the basic units for local governance and the implementation of government policies. Each district is headed by an administrative officer responsible for overseeing various aspects of governance and development within that geographical area. States chose whether to offer RSBY, secured insurance companies, and set the eligibility criteria to empanel hospitals. Districts were then responsible for identifying and enrolling households that were eligible for the policy. States were instructed to begin offering RSBY in approximately 20% of districts in 2008, with additional districts added each subsequent year.[71] Some states had preexisting insurance policies that either met or exceeded the benefits offered by RSBY. Consequently, enrollment was low in some states despite offering the program to its districts.[69] Other states elected to offer RSBY, but either delayed enrollment until past 2015 (i.e., Rajasthan) or stopped enrolling after the first few districts (i.e. Karnataka and Tamil Nadu).[10] This contributed to variation in access and coverage across the country, in particular with regards to timing of when districts, and therefore households, could receive access and enroll. In 2018, RSBY was subsumed under the expanded umbrella of AB-PMJAY, which offered expanded coverage and services to its members.[72]

2.4 Evaluation of RSBY and gaps in the literature

The primary aspects of RSBY that have been evaluated are household enrollment and use of the health insurance policy. Research indicates low-knowledge of the policy among eligible households – both on the policy in general and what was covered once enrolled.[73-75] Other work has focused on the impact of enrollment into the RSBY program on OOP healthcare expenses, with some research showing that while the policy has not reduced the burden of OOP expenses in poorer households there does seem to be greater use of healthcare utilization and treatment.[9] Some research has shown that women enrolled in the RSBY program do not use it for sexual and reproductive health services as they were not aware that those services were covered.[76] Other work has shown RSBY to be less effective at increasing reproductive health service utilization than other factors, such as maternal empowerment and household wealth.[77] Prior work has also examined the supply-side of the policy and found that states deferred service implementation to the insurance companies, leading to poor regulation of the contracts between the hospitals, insurance companies, and states themselves.[78,79]

Few studies have examined the impact of RSBY on maternal and reproductive health outcomes. Research examining the impact of RSBY on rates of caesarean delivery and in-hospital delivery suggest that it was likely societal changes more generally, and not access to RSBY specifically, that increased rates of delivery in a medical facility.[80] However, research on maternal health and pregnancy outcomes, such as infant and maternal morbidity and mortality, is lacking. Other research has found that there was low utilization of the program across the health spectrum, not just in maternal and reproductive health.[81] Further research concurred, showing that access to RSBY did not increase utilization of hospital services or decrease a household's likelihood of experiencing catastrophic health expenditures.[82]

It is also important to note the substantive and methodological limitations of the existing research evaluating the impact of RSBY. First, the National Family Health Survey (NFHS) is the Demographic and Health Survey (DHS) for India. It is commonly used, with researchers often combining multiple waves of data to achieve a quasi-longitudinal dataset.[83] However, it is rarely combined with other survey datasets collected at complementary timepoints to give a more comprehensive pan-India estimate. Additionally, while the survey datasets commonly used are collected over multiple years, most of the published research collapses across years to provide overall estimates for a single period.[84] Doing so ignores variation or trends in outcomes over time, making it harder to evaluate year-over-year changes in outcomes after RSBY was introduced. Research that does not use DHS data often focuses on just one state's experience with the policy.[85,86] Of the published research on RSBY, many studies examine its impact on the health system more broadly, and not population health outcomes.[10,87]

Finally, it is important to note that most of the studies reviewed in this chapter used observational methods that require stronger assumptions to draw causal conclusions. By relying on cross-sectional and/or descriptive data estimates may be biased by unaccounted for secular trends. In particular, districts that choose to offer RSBY and households that choose to enroll in the program may differ in unmeasurable ways from those that did not. This can lead to bias in estimates if these unmeasurable covariates also impact the outcomes of interest. For these reasons, investigating the impact of RSBY requires careful evaluation and modeling in order to provide precise and accurate estimates.

2.5 Conclusions

Extant research has not estimated the impact of RSBY on maternal and reproductive health outcomes, including pregnancy outcomes, or whether these effects vary across indicators of socioeconomic status. As the GoI continues to expand health insurance access through AB-PMJAY, understanding the population health effects of RSBY is increasingly policy-relevant.

This findings from this thesis aim to inform future policy decisions. By drawing on pan-India data across a substantial time span this research provides rigorous causal estimation for government stakeholders. My research aims to be the first step in further rigorous evaluation of India’s evolving government-sponsored health insurance landscape.

Chapter 3. Overview of Data and Methods

In this section I introduce the data sources and methodological approaches used in subsequent chapters evaluating the effects of access to and enrollment in RSBY.

3.1 Data Sources

To create the analytic sample, I used three district-representative health surveys that had been administered by the Ministry of Health and Family Welfare (MoHFW):

1. District Level Household Surveys (DLHS) rounds 3 (2007 – 20009) and 4 (2012 – 2014)
2. National Family Health Survey (NFHS) rounds 4 (2015 – 2016) and 5 (2019 – 2021)
3. Annual Health Survey (AHS) of India (2010 – 2013)

It is possible to merge information from these surveys to generate a district-representative, pan-India household sample that spans a longer time period, as they have similar sampling designs, target survey populations, and survey instruments (**Table 3.1.1**). All surveys asked female respondents for a reproductive history, including all pregnancies over the last three to five years. We used this information to create a panel of respondents' most recent pregnancies with corresponding outcomes (live birth, stillbirth, abortion, miscarriage) (**Figure 3.1.1**). Further information on each survey's sampling design, procedures, and instruments is provided below.

3.1.1 The Annual Health Survey (AHS) of India

The AHS is a prospective cohort study conducted by the Office of the Registrar General of India on behalf of the MoHFW. The AHS covers 284 districts in the 8 states designated by the

MoHFW as EAG states (i.e., Bihar, Chhattisgarh, Jharkhand, Madhya Pradesh, Orissa, Rajasthan, Uttaranchal, and Uttar Pradesh) and Assam. These 9 states are considered high-focus states by the GoI; they account for roughly one-half of the national population, but 61% of births, 71% of infant deaths, and 62% of maternal deaths.[88] The baseline survey was conducted in July 2010-March 2011, with follow-up surveys conducted in October 2011-April 2012 and November 2012-May 2013.

The AHS sample was selected using a stratified, single-stage sampling design that generates population-representative estimates at the district level. The primary sampling units (PSUs) are Census Enumeration Blocks (CEBs) in urban areas and villages in rural areas. In rural areas, larger villages with 2000 or more residents were subdivided into mutually exclusive areas, one of which was randomly selected to represent the village. The number of PSUs selected per district was proportional to its population size. In both urban and rural areas, PSUs were ordered by their female literacy rate and divided into three substrata of equal size, followed by simple random sampling of PSUs within each substratum without replacement. This sampling scheme ensures equal representation of the three strata across urban and rural areas and renders the design as self-weighting. A household head and eligible women aged 15-49 from all households within the 20,594 PSUs sampled were selected for interviews. A total sample size of 4,142,942 households and 20,113,607 individuals (adults and children household residents) was selected for the baseline survey. This exceeded the target of 4.1 million households based on sample size calculations designed to provide accurate and precise estimates of the infant mortality rates at the district-level. This makes the AHS the largest household sample survey in the world. Further details on the sampling scheme are available elsewhere.[88]

The AHS survey is comprised of 4 modules, specifically the (1) household schedule; (2) household-listing schedule; (3) woman schedule; and (4) mortality schedule. This thesis used the information collected as part of the woman schedule. During the baseline survey, the house-listing schedule mapped and listed all houses and households in a PSU and collected information on household characteristics (type and ownership), basic amenities, and assets. For the household schedule, basic identifying and demographic information was collected for all “usual” residents of the household from the head of the household (effective January 1st in 2010, 2011, and 2012 for the baseline, wave 2, and wave 3 surveys, respectively). The woman schedule included two sections. The first section, administered to all ever-married women 15-49 years of age, collected information on: the outcomes of pregnancies occurring in the last three years, including live births, stillbirths, and abortions. The second section was administered to all currently married women aged 15-49 years and asked about: pregnancy; family planning methods and sources; and contraceptive use/unmet need. In the second and third waves, only pregnancies that occurred since the last wave were recorded. The longitudinal design of the AHS therefore provides a 5-year record for many health services and health-related indicators.

Questionnaires were administered by trained interviewers. The coordination, supervision, and monitoring of fieldwork in the states was coordinated by the state Directorate of Census Operations; this included checks and backchecks by supervisors to ensure data quality. Additionally, a third-party independent audit was conducted in 20 randomly selected PSUs per district to assess data reliability.

To create the pregnancy panel, we used the woman schedule survey. The woman schedule asked women to report all pregnancies, duration of pregnancy, outcome of pregnancy, and date of pregnancy outcome. We restricted to the most recent pregnancy across the three survey waves to match the cross-sectional sampling of the DLHS and NFHS surveys. This resulted in a sample of the 1,351,276 most recent pregnancies, which were linked to the woman's demographic and socioeconomic covariates.

3.1.2 The District Level Household Survey (DLHS), rounds 3 and 4

The DLHS is sponsored by the MoHFW and conducted by the International Institute for Population Science (IIPS). The third round of the DLHS was conducted in 2007-8 in 602 districts from 34 states and union territories. The fourth round was designed to complement the coverage of the AHS and includes 336 districts from 18 states and 5 union territories. The survey was designed to provide district-representative estimates for most indicators collected.

The DLHS uses a multi-stage, stratified sampling design, with PSUs selected proportional to population size. PSUs were CEBs in urban areas and villages in rural areas. A household listing was conducted in all PSUs, followed by systematic random sampling of households. Approximately 720,320 households were selected in DLHS-3 and 350,000 in DLHS-4, with household response rates exceeding 94%. Further details are available elsewhere.[89]

All ever-married eligible women aged 15-49 who were regular residents of the household (defined by staying there the night prior to the survey) were eligible for interviews. All women were asked to list all pregnancies in the last three years, including the length of each pregnancy, the outcome, and the dates of the outcome. For each woman, the most recent pregnancy and date of outcome was selected to create the DLHS pregnancy panel. The numbers of women selected for interviews was roughly 644,000 for DLHS-3 and 320,000 for DLHS-4. When restricting to the most recent pregnancies, this gave 210,798 pregnancies from DLHS-3 and 87,297 pregnancies from DLHS-4. All pregnancies were linked to the woman's demographic and socioeconomic covariates.

3.1.3 The National Family Health Survey (NFHS), rounds 4 and 5

The National Family Health Surveys are sponsored by the MoHFW, which designated IIPS as the nodal agency for all NFHS surveys, and technical support is provided by ICF International through the Demographic and Health Survey (DHS) program—the NFHS are the equivalent of the DHS, which are conducted in many low- and middle-income countries throughout the world. The fourth round of the NFHS was fielded in 2015-6 and the fifth and most recent round was conducted in 2018-9. Both rounds were conducted in all 29 states and 7 union territories and are designed to provide district-representative estimates of most indicators for all 640 districts at the time of the 2011 Census.

Like the AHS and DLHS4, the NFHS uses a multi-stage, stratified sampling design, with PSUs selected proportional to population size. PSUs were CEBs in urban areas and villages in rural areas. A household listing was conducted in all PSUs, followed by systematic random sampling of households to select 22 households per PSU in NFHS-4. Nearly 630,000 households were selected in NFHS-4, with a household response rate of 98%. Women who stayed in the household the night prior to the survey were considered residents and were eligible for interview. Questions are asked of the household head as well as all women of reproductive age (15-49 years) in the household, with a response rate of 97% for women. For both NFHS-4 and NFHS-5 this included approximately 700,000 ever-married women between 15 and 49 years of age.

The NFHS uses a unique questionnaire to get a detailed reproductive health history. Called the contraceptive calendar, the interviewer asks the surveyed woman to go through every month of the last five years and report all births, pregnancies, terminations (stillbirth or miscarriage), and contraceptive use.[90] If a surveyed woman had an abortion in the fourth month of pregnancy, the calendar would show three months of pregnancy (i.e. May, June, and July of 2008), and an abortion in the fourth month (August, 2008). Every month must be filled out in the contraceptive calendar, giving incredibly rich data. From this dataset, the most recent pregnancy that occurred, the length of the pregnancy, the pregnancy outcome, and the date of the outcome were selected to create the pregnancy panel. This gave 192,549 pregnancies in NFHS-4 and 163,380 pregnancies in NFHS-5. All pregnancies were linked to the woman's demographic and socioeconomic covariates.

Table 3.1.1 Comparison of the District Level Household Survey (DLHS) rounds III and IV, Annual Health Survey (AHS), and National Family Health Survey (NFHS) rounds IV and V

	DLHS		AHS	NFHS	
	DLHS-3	DLHS-4		NFHS-4	5
Administration	International Institute for Population Sciences on behalf of the MoHFW		Office of the Registrar General on behalf of the MoHFW	International Institute for Population Sciences on behalf of the MoHFW	
Survey years	2007-09	2012-14	2010-2013	2015-16	2018-21
Coverage	34 states and union territories (n=601 districts)	18 (non-AHS) states 5 and union territories (n=336 districts)	8 Empowered Action Group states[1] + Assam (n=284 districts)	All 29 states and 7 union territories (n=640 districts)	All 29 states and 7 union territories (n=640 districts)
Study design	Repeated cross-sectional		Cohort (3 rounds)	Repeated cross-sectional	
Primary sampling units (PSUs)	Wards (urban) and villages (rural)	Census Enumeration Blocks (urban) and villages (rural)			
Sampling design	Multi-stage, stratified sampling, with PSUs sampled proportional to population size				
Households selected	720,320	378,487	4,149,307 (baseline)	601,509	636,699
Respondents (women's survey)	All ever married women 15-49 years of age in the household		All ever married women 15-49 years of age in the household	All women 15-49 years of age in the household	All women 15-49 years of age in the household
Number of ever married women 15-49 years of age	643,944	319,695	3,809,392 Surveyed in three waves	699,686	724,115
Pregnancy and birth outcome data	Collected for all pregnancies since January 2004	Collected for all pregnancies since January 2008	Collected for all pregnancies since January 2007	Collected for all pregnancies after since 2010. Outcomes only for most recent pregnancy	Collected for all pregnancies after since 2014. Outcomes only for most recent pregnancy

[1] Bihar, Chhattisgarh, Jharkhand, Madhya Pradesh, Orissa, Rajasthan, Uttaranchal, and Uttar Pradesh
Abbreviations: AHS=Annual Health Survey; DLHS=District Level Household Survey; Ministry of Health and Family Welfare; NFHS=National Family and Health Survey

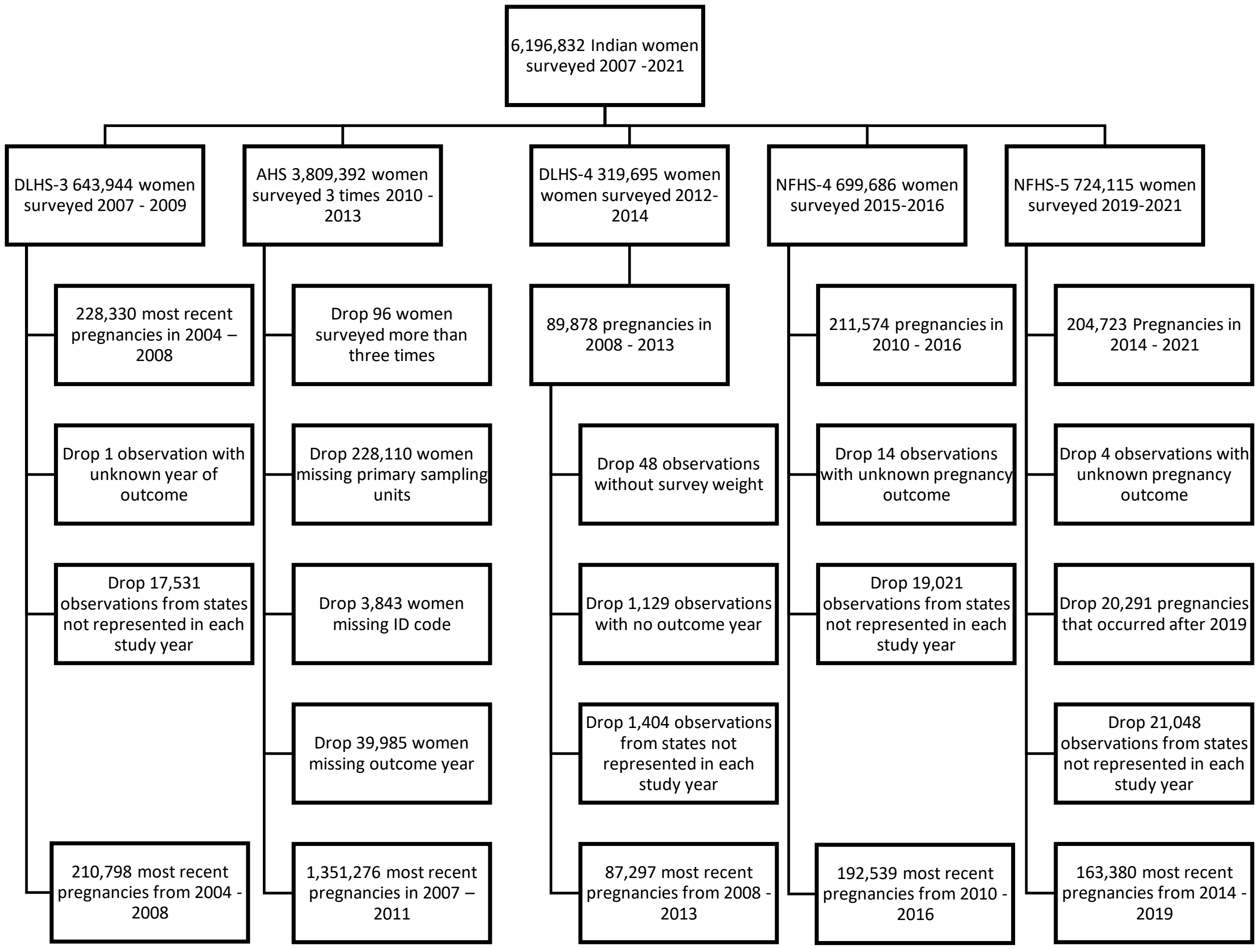


Figure 3.1.1 Panel creation diagram

3.1.4 Weight denormalization

All surveys included survey-specific sampling weights. The DLHS-3 and NFHS were pan-India surveys, whereas the DLHS-4 surveyed 26 states and Union Territories, and the AHS surveyed nine states with the highest fertility and mortality rates. While the DLHS and NFHS had somewhat similar sample sizes (ranging from 319,695 – 724,115 women), the AHS was a longitudinal study that surveyed ~3.5 million women in each of the three rounds. When restricting to most recent pregnancy, we had many more observations from AHS compared to all other surveys (**Table 3.1.2**).

Table 3.1.2 Sample size of women aged 15-49 and their most recent pregnancies by each survey

Survey	DLHS-3	DLHS4	AHS	NFHS-4	NFHS-5
n	210,798	87,297	1,351,276	192,539	163,380

Due to the varying sampling fractions applied across surveys, we de-normalized the weights prior to merging information. We followed established procedures from the DHS Program for denormalizing the sampling weights.[91] First, from India's 2001 and 2011 censuses, we collected information on the number of ever-married women ages 15-49 for each state. Counts from the 2001 census were used for pregnancies from the DLHS-3 that occurred in 2004 and 2005. All other outcome years and surveys used counts from the 2011 census. Second, we calculated survey and state specific sampling fractions, which represent the ratio of ever-married women aged 15-49 interviewed in each state to the total number of ever-married women aged 15-49 residing in each state around the time the survey was completed. Third, this sampling fraction was then used to de-normalize the sampling weight by multiplying each survey-specific sampling weight by the sampling fraction. For the NFHS data, the weight was first divided by 1,000,000, per DHS guidelines:

$$Denormalized\ Weight = Survey\ Sample\ Weight \times \frac{(Total\ females\ aged\ 15-49\ in\ the\ country\ at\ time\ of\ survey)}{(Number\ of\ women\ aged\ 15-49\ interviewed\ in\ the\ survey)}$$

3.1.5 Harmonizing Districts and States Across Surveys

India created a new state and new districts between survey administrations. The state of Telangana was created in 2014, and approximately 50 new districts were created between the 2001 and 2011 censuses.[92,93] Existing states and districts were divided to create these new areas. Districts were also reassigned to new states to accommodate the shifting boundaries. Due to these changes, households that lived in the same geographic area may have been assigned to different districts in different surveys. In order to accurately compare across survey years we recoded all states and districts to match those from the 2001 census. This was done by identifying all new districts and states and recoding to match the district and state they were part of as of the 2001 census. For example, the Tengnoupal district in the state of Manipur was created in 2016 by dividing the district of Chandel.[94] All households listed as residing in Tengnoupal were therefore recoded as residing in Chandel. Districts that were created by aggregating existing districts were recoded to the district that contributed the most sub-administrative districts. All spellings of districts were standardized across the surveys.

3.1.6 Analytical Sample for Chapters 5 and 6

The sample created for Chapter 4 served as the basis for the analytic samples used in Chapter 5 and Chapter 6 (**Figure 3.1.2**). For Chapter 5, where we examined the effect of district-level access to RSBY, we limited to districts represented in each study year that had data on whether they offered RSBY. For Chapter 6, this sample was further limited to households that did not have missing data for RSBY enrollment or maternal age. For the sensitivity analyses, households that reported enrollment in RSBY but lived in districts coded as never-treated were excluded.

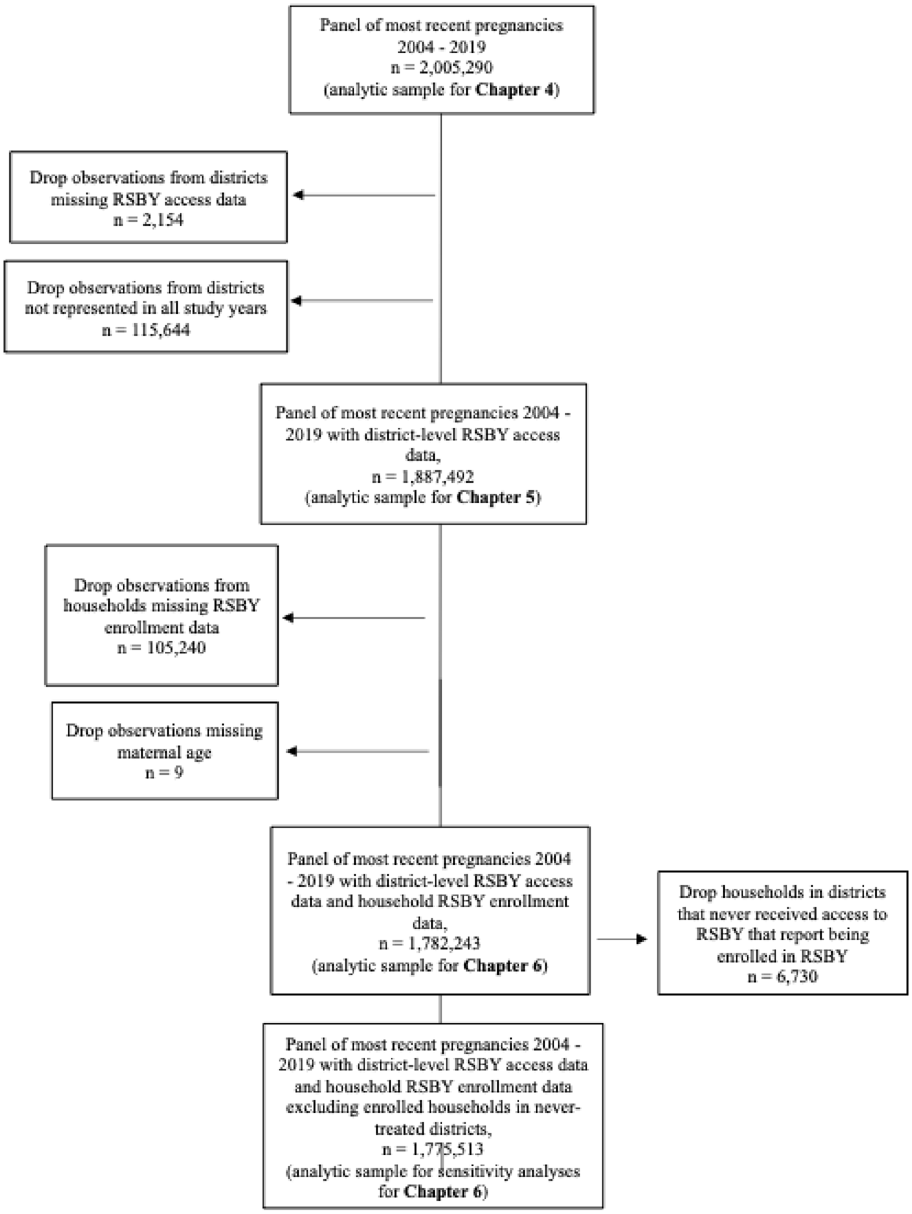


Figure 3.1.2 Extension of panel from **Figure 3.1.1** to create analytic sample for chapters 5 and 6

3.2 Measures

3.2.1 Creation of a Wealth Index

We adapted the approach applied by the DHS program for measuring asset-based household wealth to produce a household wealth index. We applied Principal Component Analysis (PCA). PCA is a data reduction technique used to reduce the dimensionality among correlated variables,[95] in this case by reducing items that measure household assets into a principal component or domain, or set of domains. The intuition is to identify the smallest number of components that explains substantial variation among the set of variables.[96] PCA works best when the variables are highly correlated, which is generally the case for socioeconomic indicators, including the asset measures used to generate the wealth index.[97] The DHS program uses the first principal component to estimate a continuous household wealth score for each household.[98]

To create the wealth index, the asset indices for all five surveys were compared. Variables that matched across the surveys were selected for creation of the index. These variables include whether the household owned a: radio; fridge; bicycle; motorcycle; animal cart; car; television; computer; or land. We also included: if the household drinking water is treated; whether the water source is from an improved facility; the source of water for the household; the type of toilet; if the toilet is shared; and the type of cooking fuel. Given substantial missing data on the mobile phone asset question in NFHS-5, mobile phone ownership was not included in the calculation of the wealth index. In contrast to the DHS approach of dichtomizing categorical variables, we followed recent guidance to order categorical variables sequentially by amount of resources used.[99] Ordering these categorical variables allows for greater discrimination among those of the lowest socioeconomic status.[100] For these categorical variables (e.g. type of toilet), the response items were matched and ordered according to the World Health Organization (WHO) criteria of improved vs. unimproved water and sanitation systems (**Table 3.2.1**).[101] The binary variables 0 category included both missing and those who did not endorse the item.[102]

Once these items had been selected, squared multiple correlations was used to examine variation. Variables explaining less than 5% of the variance were dropped from the asset index; this included owning an animal cart and whether water source was improved. The remaining variables were included in a principal component analysis with polychoric correlation. Mean missing substitution was used to impute missing categorical variables with the mean value.[102] The associated eigenvalues of the PCA can be seen in **Figure 3.2.1**. The first component explained 44% of the total variance of the included variables, and was therefore used to proxy household wealth. The continuous wealth score was used to rank each observation within the distribution of 0 – 1 for each survey year. Observations that had the same score were given the same rank. Scores were then divided into quintiles ranging from 0.2 (poorest) to 1 (richest) for descriptive figures.

Table 3.2.1. Ordered items for asset index

Variable	Ordered Items
Water Source	1 piped water into dwelling yard/plot 2 public tap stand pipe 3 tube well or borehole 4 protected dugwell 5 tanker/truck/card 6 unprotected dug well 7 surface water 8 other
Improved Water	1 no facilities 2 unimproved facilities 3 improved facilities
Toilet Type	1 pour/flush latrine: connected to piped sewer system 2 pour/flush latrine: connected to septic tank 3 pour/flush latrine: connected to pit latrine 4 pour/flush latrine: connected to something else 5 pit latrine: ventilated improved pit 6 pit latrine: with slab 7 pit latrine: open or without slab 8 service latrine /dry toilet 9 open defecation/no facility/open space or field
Cooking Fuel	1 electricity 2 lpg/ natural gas 3 biogas 4 kerosene 5 coal / ignite / charcoal 6 wood 7 straw / shrubs / grass / agricultural crop waste 8 dung cakes

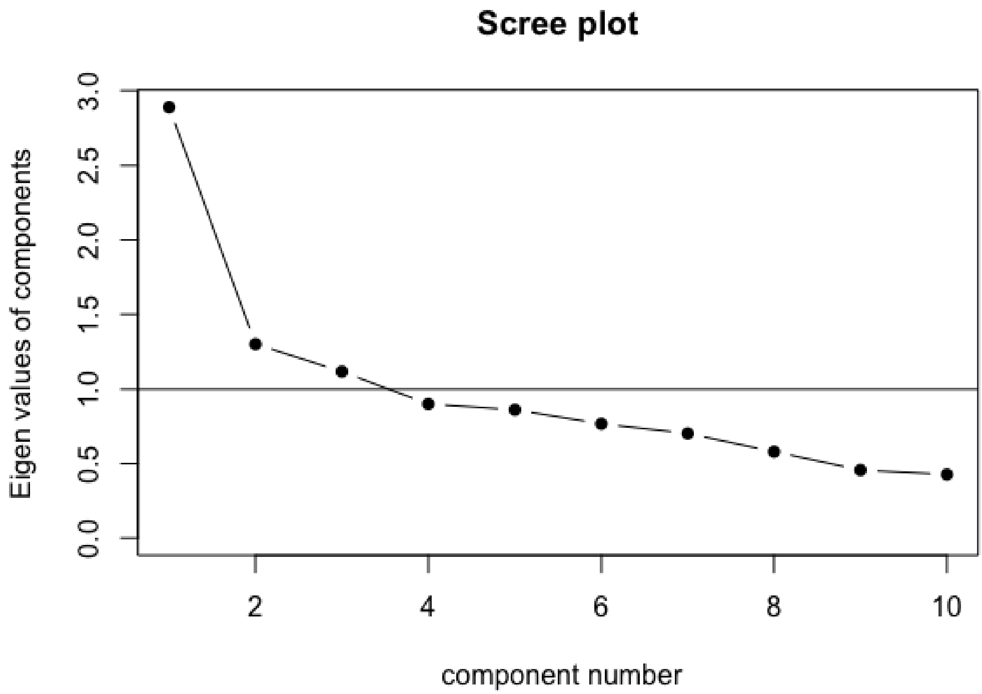


Figure 3.2.1. Scree plot from PCA for wealth index creation

3.2.2 District-Level Access to RSBY

District-level access to RSBY was assigned based on enrollment data from the GoI district-level enrollment portal, which was accessed via the internet archive since the original site was not maintained.[71] Total enrolled and eligible households were collected for each district from each year available. The earliest archived date of the webpage was 2010. We constructed a panel of districts with total enrollment percentages for each district and year. We compared our estimates of enrolled families to previously published work that examined district-level access to RSBY.[10] Our estimates were comparable, and we therefore felt confident in the district-level enrollment data. We did note that due to state-level variation in eligibility and enrollment there were families enrolled in RSBY that spanned the constructed wealth index, despite it ostensibly being solely for the most socioeconomically disadvantaged families. Of note, there was not enrollment data available for every district in each state.

We created a binary variable for each year in the study period indicating whether each district had access to the RSBY program or not. We assigned the year a district gained access to RSBY as the first year any household was enrolled, under the assumption that once the district received access it maintained access for the rest of the study time period, which is consistent with the design of the policy.[103] In other words, once a district gained access to RSBY it was assumed to remain treated for the duration of the study period. We compared this list of districts to the sampled districts in our dataset and matched on name and state. Districts that were created after the 2001 census were mapped to their previous district name to facilitate the matching. We then assigned treatment to each pregnancy in our sample based on district and year. We were able to assign treatment to 94.1% of our study sample (1,887,492 / 2,005,290). Pregnancies that did not have enrollment data were dropped.

While RSBY enrollment began in 2008, the archived reports only provided information on district-level enrollment from 2010 onwards. Given the staggered roll-out of the program and potential heterogeneity in treatment effects over time,[104] we created three enrollment groups: 1) district that received access between 2008-2010 (early-adopters); 2) access between 2011-2012 (mid-adopters); and 3) access from 2013-2014 (late-adopters).

3.2.3 Pregnancy Outcomes

Pregnancy outcomes were measured from retrospective, self-reported, reproductive health histories. In the DLHS and AHS surveys, surveyed women were asked to list all pregnancies, their outcomes, and the date of the outcome that occurred over the last three to five years. From this we created a panel of most recent pregnancies

In the NFHS surveys, women complete the contraceptive calendar with the surveyor. In this, they report for every single month in the last five years whether they were pregnant, gave birth, or using contraception.[90] Through this it is possible to capture all pregnancies, their outcomes, and the year and month of outcome. There is one unique aspect of the NFHS data that must be noted. The most recent non-live birth outcome is differentiated as a stillbirth or miscarriage, but if they had had a non-live birth preceding the most recent one it was coded simply as a termination. Due to this, we limited our dataset to the most recent pregnancy in all of the surveys.

Pregnancies that were recorded as stillbirth but occurred before the 28th week of gestation were re-coded as miscarriages, and the reverse was done for pregnancies occurring after 28 weeks but coded as miscarriages. All reported abortions were left as is. All pregnancies and their outcomes were linked to the mother's socioeconomic characteristics.

3.3 Study Designs

3.3.1 Difference-in-differences (DiD) design

The first manuscript (**Chapter 4**) of this thesis uses a descriptive design to examine trends in social inequalities in rates of stillbirth, abortion, and miscarriage over 15 years. In the second manuscript (**Chapter 5**), I was interested in whether the introduction of RSBY impacted these existing inequalities. Therefore, to evaluate the causal impact of providing district-level access to RSBY on pregnancy outcomes, I applied a difference-in differences (DiD) design that accommodates staggered treatments (i.e., heterogeneity in the adoption of RSBY across districts). DiD is a commonly applied design for evaluating the effects of interventions because it accounts for underlying secular trends in an outcome using a control group that substitutes for the counterfactual, specifically what would have been observed in the treated group had it not been treated. The standard approach for estimating the Average Treatment Effect on the Treated (ATT) is the two-way fixed effects (TWFE) regression model. However, recent work shows that applying the standard TWFE model when treatment times are staggered can result in biased estimates of the ATT if effects are heterogeneous across treatment groups and/or over time; this is related to the TWFE regression making "forbidden comparisons" (i.e., using already treated units as controls for subsequently treated units).[104,105] Even the dynamic effects or event study specification accounts for heterogeneity over time since treatment, but not across groups or "cohorts". The stronger assumptions required for unbiased estimation of the TWFE model when treatments are staggered has led to the introduction of newer methods,[104,106] which focus on "clean comparisons" of treated groups with not-yet and/or never treated units. **Figure 3.3.1** illustrates the concept of staggered treatments. In this, groups are treated at years t = 0, t = 3, and t = 6. When calculating the ATT at year 2, groups that were treated at t = 0 (early) would be compared to the groups were never, mid-, and late-treated.

We applied the approach to DiD proposed by Callaway and Sant'Anna to account for staggered adoption time of the treatment.[104] This non-parametric method compares changes in rates of stillbirth, abortion, and miscarriage in treated districts before and after they received access to RSBY to corresponding differences in districts that had either not yet received or never received access to the program (control groups). Importantly, this method allows for heterogeneous effects across treatment groups (e.g., "early" vs. "late" adopters) and time since treated.

Assumptions

A fundamental assumption for the DiD to be valid is that, in the absence of the intervention, trends of the outcome for the treatment and control groups would be parallel. That is, differences in outcome rates for treated and control units prior to the intervention would remain constant in post-intervention period, had the program not been implemented.[103] We examined evidence for pre-policy parallel trends in the time before the first group was treated both by examining trends visually and statistically, using the Cramér-von Mises test[107] with districts that had not-yet-been treated as the control group.[104]

For our causal interpretation of the DiD estimate we assume no reverse causality or unmeasured time-varying confounding.[108] We also assume no anticipation, specifically that the treatment, district-level access to RSBY, had no impact on pregnancy outcomes before the program's implementation. In this case, we assume that households did not hold-off on utilizing healthcare and alter their family planning decisions knowing that they would soon be receiving access to RSBY. Due to the staggered implementation of RSBY, with approximately 20% of districts receiving access each year, we assume quasi-random implementation of the program and no anticipation.

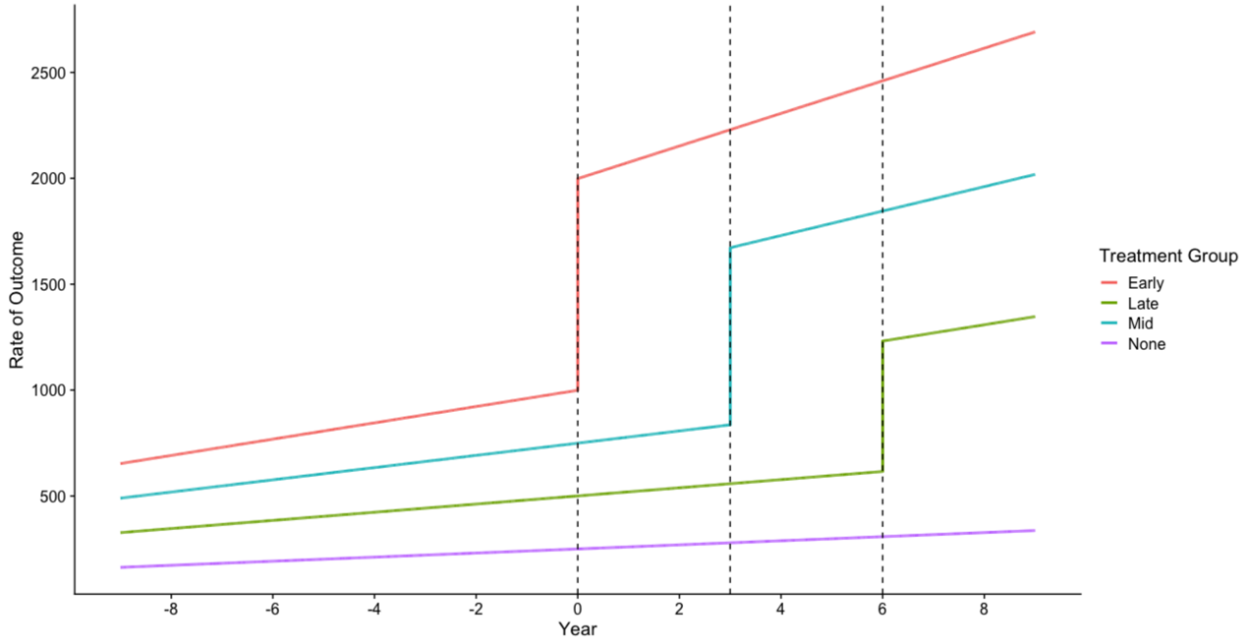


Figure 3.3.1 Graphical description of a simulated difference-in-differences design showing rates of outcome for each group. Year 0 is the first year of policy implementation, year -2 is two years before policy is implemented, +2 is two years after policy is implemented, etc. In this example, the early treatment group receives access to a hypothetical policy in year 0, the mid group in year +3, and the late group in year +6. Changes in outcome correspond to year the group received access to the policy. The parallel trends assumption is visible in the pre-period.

3.3.2 Instrumental Variable (IV) design

The second manuscript of this thesis (**Chapter 5**) examines the "intention-to-treat" effect of district-level access to the RSBY program, but does not tell us about the impact of household enrollment in the health insurance program. In **Chapter 6**, I was interested in estimating the effect of household enrollment on pregnancy outcomes. An inherent challenge when estimating the impact of enrollment in a voluntary health insurance program is the non-random "selection" into the program, or unmeasured confounding, since households that enroll in the program differ from those who do not in ways that affect their reproductive health outcomes. For this reason, observational comparisons of health outcomes among enrollees and non-enrollees are likely biased. Instrumental variable analysis (IV) is one approach for addressing this confounding bias. IV analyses can be used to exploit the arguably unconfounded variation in enrollment due to the staggered roll-out of the RSBY program to estimate its impact on pregnancy outcomes. In this section I will introduce the IV design, along with the assumptions necessary for causal identification.

IV analysis can be thought of as a quasi-experimental analogue to a randomized controlled trial (RCT) since it leverages the exogenous variation in treatment related to an "instrument" to recover the causal effect of the treatment. The "as-if random" variation in RSBY coverage induced by the district-level roll-out of RSBY provides a rare opportunity to evaluate the impact of insurance coverage on the utilization of health services and health outcomes. We treat the district-level roll-out of RSBY as an instrument to examine the impact of household-level RSBY coverage on likelihood of a pregnancy ending in stillbirth, abortion, and miscarriage. The IV approach uses the impact of district-level access to RSBY on household-level RSBY participation to understand the effect of household-level RSBY coverage on our primary outcomes of stillbirth, abortion, and miscarriage.[109-111]

For the variation in enrollment related to district-level access to the program to be plausibly unconfounded, we must again account for the staggered roll-out of the RSBY program. This was done in **Chapter 5** using methods developed by Callaway and Sant'Anna.[104] An alternative approach has been developed by Wooldridge [106] and, in **Chapter 6**, we used the extended two-way fixed effects (ETWFE) DiD model to estimate the first-stage of the two-stage least squares (2SLS) IV model. Predictions from the first stage were then used to estimate the impact of household-level coverage on adverse pregnancy outcomes in the second stage.

Assumptions

There are several assumptions that must be met for IV analysis to inform unbiased inference.[112] The first is the exclusion condition, which states that the only way the instrument can impact the outcome is through the instrument's impact on the treatment. We therefore assume that the only way access to RSBY can impact the stated pregnancy outcomes is if households choose to enroll in the policy. Another way of thinking about this is we must assume that households not enrolled in RSBY cannot use the policy benefits to pay for healthcare. Second, we must assume exchangeability, and the absence of backdoor paths linking the instrument to our outcomes. Therefore, we must assume that the instrument of district-level access to RSBY is not correlated with other unobserved variables that also predict the outcome (**Figure 3.3.2**). Additionally, for an instrument to be valid it must meet the criteria of "relevance", that is district-level access to RSBY is correlated with household-level enrollment in the policy. This is commonly assessed using the F-statistic.[113]

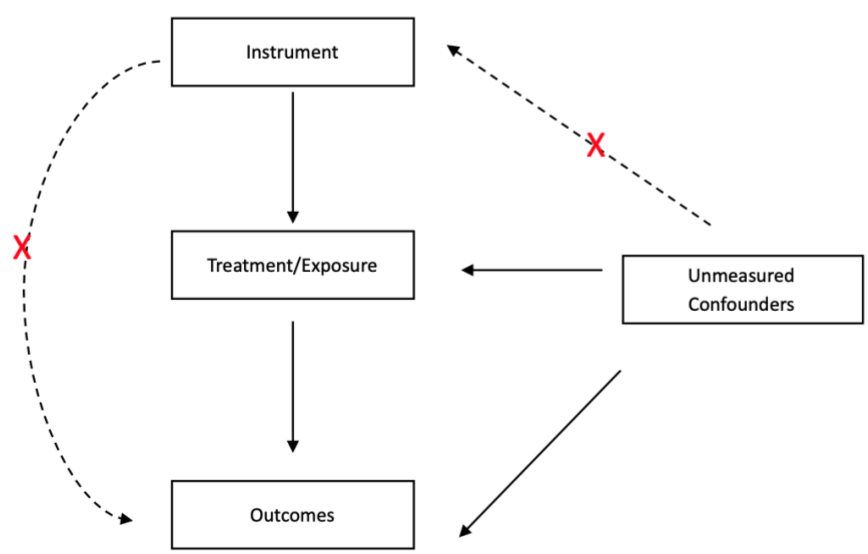


Figure 3.3.2 Graphical representation of the assumptions necessary for IV analyses. The arrows showing the blocked paths represent the exclusion restriction.

3.4 Code availability

All analyses in this dissertation were conducted using R.[114] Analytical code is publicly available for reproducibility at https://github.com/cmjoyce/cihr_rsby.

Chapter 4. Socioeconomic Inequalities in Adverse Pregnancy Outcomes in India: 2004 – 2019

4.1 Preface: Manuscript 1

Before examining the impact RSBY had on households below the poverty line, I was first interested in identifying and quantifying socioeconomic inequalities in the pregnancy outcomes of interest. The manuscript presented in this section does so by providing estimates of 1) overall rates of these outcomes, and 2) socioeconomic inequalities therein. Using the slope index of inequality (SII) and relative index of inequality (RII) we quantified the social inequalities in stillbirth, abortion, and miscarriage across bi-yearly time periods. The RII and SII are regression based measures that account for the relation between a socioeconomic indicator and outcome across the entire distribution of the data, and are recommended for examining inequalities across groups or time.

This work was presented as a poster at the Population Association for America (PAA) Annual meeting (April 2023) and is being prepared for submission to The Lancet Global Health.

4.2 Manuscript 1

Socioeconomic Inequalities in Adverse Pregnancy Outcomes in India: 2004 - 2019

Caroline M. Joyce[1], Deepti Sharma[2], Arnab Mukherji[2], Arijit Nandi[1,3]

Affiliations:

[1] Department of Epidemiology, Biostatistics, and Occupational Health, School of Population and Global Health, McGill University

[2] Center for Public Policy, Indian Institute of Management Bangalore

[3] Institute for Health and Social Policy, McGill University

Corresponding Author:

Caroline Joyce

2001 McGill College Avenue

Montreal, QC H3A 1G1

Canada

Email: caroline.joyce@mail.mcgill.ca

Abstract

Background: Although India has made substantial improvements in public health, it accounted for one-fifth of global maternal and neonatal deaths in 2015. Stillbirth, abortion, and miscarriage contribute to maternal and infant morbidity and mortality. There are known socioeconomic inequalities in adverse pregnancy outcomes. This study estimated changes in socioeconomic inequalities in pregnancy outcomes in India across 15 years.

Methods: We combined data from three nationally representative health surveys. Absolute inequalities were estimated using the slope index of inequality and risk differences, and relative inequalities were estimated using the relative index of inequalities and risk ratios. We used household wealth, maternal education, and Scheduled Caste and Scheduled Tribe membership as socioeconomic indicators.

Results: We observed persistent socioeconomic inequalities in rates of abortion and stillbirth from 2004 – 2019. Women who completed primary school and those at the top of the household wealth distribution were more likely to report an abortion and less likely to experience a stillbirth compared to women who did not complete primary school and those at the bottom of the wealth distribution. Women belonging to a Scheduled Caste or Scheduled Tribe were less likely to report an abortion compared to other women, although these inequalities diminished by the end of the study period. There was less consistent evidence for socioeconomic inequalities in miscarriage, which increased for all groups over the study period.

Conclusion: Despite targeted investments to improve access to health services for socioeconomically disadvantaged groups in India, disparities in pregnancy outcomes persist.

Keywords: Maternal health, stillbirth, abortion, miscarriage, pregnancy loss, pregnancy outcomes

Introduction

India has made substantial improvements in public health over the last few decades, including significant reductions in neonatal and maternal mortality.[12] Despite these improvements, India accounted for an estimated one-fifth of all global maternal and early neonatal deaths in 2015.[2,115] India is not on track to meet the United Nations (UN) Sustainable Development Goals (SDGs) by 2030,[2,16] including goals to lower rates of maternal and infant mortality.[16] Adverse pregnancy outcomes such as stillbirth and miscarriage contribute to these high rates of maternal and neonatal mortality. Non-live births that occur at or after the 28th week of pregnancy are typically defined as stillbirths, while pregnancy losses that occur before 28 weeks are defined as miscarriages.[20] India accounts for 19% of births worldwide but 22.6% of the global burden of stillbirths, more than any other country.[18] India also has high rates of miscarriage,[20] with recent research showing that rates of recurrent miscarriage are higher in India compared to high-income countries.[22] Recurrent miscarriage is associated with higher likelihood of neonatal death,[23] and India's miscarriage rates – particularly those that occur after the first trimester – are higher than the global average.[19-21] Reducing spontaneous pregnancy losses is critical to accelerating decreases in rates of maternal and neonatal mortality,[116] and achieving India's SDG targets.

The World Health Organization (WHO) states that access to quality family planning – including modern contraception and safe induced abortion – is critical to preventing unplanned pregnancies, and lowering rates of miscarriage and stillbirth.[25] The medical procedure used for abortion (both surgery and medication) is also often a necessary treatment for miscarriage.[26] India legalized abortion in 1971[27] and granted widespread access to medication-induced abortion in 2003,[28] and therefore has comparatively liberal abortion laws. However, availability does not guarantee access, and lack of access to quality and safe abortion is a primary risk factor for miscarriage and stillbirth. Other preventable risk factors for experiencing a stillbirth or miscarriage include lack of antenatal care (ANC), giving birth without a skilled birth attendant (SBA), and receiving inadequate nutrition during pregnancy.[29] Approximately 40% of stillbirths occur during prolonged labor, which is more common among women who did not receive quality ANC or live far from emergency obstetric care.[117]

Previous research has shown that low socioeconomic status is associated with adverse pregnancy outcomes, including stillbirth, abortion, and miscarriage.[73-75] Poorer women in India are at greater risk for stillbirth and miscarriage because they are less likely to receive ANC, deliver with a SBA,[51,52] and have adequate nutrition.[53] Women with less formal schooling, including illiterate women, are also more likely to experience stillbirth and miscarriage.[3,12] Additionally, women belonging to designated Scheduled Castes and Scheduled Tribes, defined by the UN as groups of people that, both historically and currently, endure systemic discrimination impacting all levels of life – especially health –[118] have been shown to have higher rates of adverse pregnancy and neonatal outcomes.[116]

Socioeconomically disadvantaged women also have less access to modern contraceptives and safe abortion services,[4] often due to financial barriers that prevent women from accessing this type of care. While surgical abortion is technically free at public facilities, women may be subject to other costs,[54] including the direct costs of the anesthesia and medication necessary for the procedure,[55] along with indirect costs such as missing work or transportation to the facility.[56] The drugs required for medication-induced abortion are not free, and women must pay out-of-pocket to access this care.[54,57] Without access to quality family planning, including abortion, women of lower socioeconomic status are more likely to experience unplanned pregnancies and receive inadequate ANC.

There are limitations to existing research for measuring socioeconomic disparities in rates of stillbirth, abortion, and miscarriage in India. Most research has been cross-sectional,[3,20,23,116,119] including research using Demographic Health Survey (DHS) data,[120-122] with few studies examining trends or multiple pregnancy outcomes nationally.[123] Similarly, few studies have examined socioeconomic disparities from a pan-Indian perspective.[3,116] A comprehensive national analysis of trends in levels and disparities in pregnancy outcomes is needed to measure progress towards the SDGs.

This study aims to provide robust, country-wide estimates of trends in socioeconomic inequalities in adverse pregnancy outcomes. In particular, we used three nationally representative surveys of ever married women in India to estimate the association between socioeconomic position, measured by the household wealth index, women's educational attainment, and membership in a Scheduled Caste or Scheduled Tribe, and rates of stillbirth, abortion, and miscarriage from 2004 – 2019.

Methods

Data Sources

Data were derived from three district-representative household surveys with similar target populations, sampling designs, and survey instruments that were conducted by the Ministry of Health and Family Welfare (MoHFW): (1) District Level Household Surveys (DLHS) rounds 3 and 4; (2) National Family Health Survey (NFHS) rounds 4 and 5; and (3) the Annual Health Survey (AHS) of India. These data have been pooled previously to facilitate cross-sectional and longitudinal analyses.[124-131] The DLHS, AHS, and NFHS provide district-representative snapshots of the target population of ever-married women between 15 and 49 years of age in nearly all states and territories during the study period from 2004-2019.

We used the information provided by women in each survey on their reproductive histories to construct a panel of pregnancies and corresponding pregnancy outcomes (i.e., live birth, miscarriage, abortion, or stillbirth) over the study period. For comparability across surveys, we included only the most recent reported pregnancy. In total, we recorded 2,005,290 pregnancies that resulted in live births, still births, miscarriages, or abortions between 2004 and 2019 (**Supplementary Figure 4.1**). Technical details for each survey are available elsewhere,[89,132-134] and in **Chapter 3**.

Measures

Our primary outcomes of interest were self-reported stillbirth, abortion, and miscarriage. In the DLHS and AHS surveys women were asked to report all pregnancies and their outcomes in the last three years. In the NFHS, the contraceptive calendar was used to self-report reproductive health outcomes (contraception, pregnancies, births) for each month of the past five years. Following DHS guidelines, spontaneous abortions before the seventh month (28 weeks) of pregnancy were defined as miscarriages, and those that occurred during or after the seventh month (28 weeks) of pregnancy were defined as stillbirths.[135,136] We used this information to define three binary outcome variables for each participant's most recent pregnancy: 1) stillbirth; 2) induced abortion; or 3) miscarriage.

Household and respondent-level socioeconomic characteristics were used to measure social inequalities. We used information on reported household assets[137] to create a continuous wealth index. This index was used to create a continuous rank measure of the woman's socioeconomic position relative to others in the same survey year. Further details on the creation of the wealth index are available in **Chapter 3**. Education was dichotomized based on whether the respondent completed a primary education (≥ 8 years of schooling) or not. Two dichotomous variables were created for whether the respondent reported belonged to a Scheduled Caste or a Scheduled Tribe. The comparison group for these analyses were women who did not belong to a Scheduled Caste or Scheduled Tribe. Age was included as a covariate, and state of residence and year of outcome assessment were included as fixed effects. To examine changes over time, we estimated rates by two-year intervals.

Statistical Analyses

We estimated social inequalities in rates of stillbirth, abortion, and miscarriage. For binary socioeconomic factors (i.e., education and belonging to a Scheduled Caste or Scheduled Tribe) we used generalized linear models (GLMs) with a Gaussian distribution and an identity link to estimate associations on the risk difference (RD) scale and with a Poisson distribution and a log link to estimate associations on the risk ratio (RR) scale.[138]

For the continuous household wealth index, we measured social inequalities on the relative scale using the relative index of inequality (RII) and on the absolute scale using the slope index of inequality (SII). The RII and SII are regression based measures that account for the relation between a socioeconomic indicator and outcome across the entire distribution of the socioeconomic gradient, and are recommended for examining inequalities across groups or time.[139,140] The RII and SII are defined as the ratio and difference of the risk of the health outcome, respectively, comparing those in the highest vs. the lowest socioeconomic position.

Pregnancies were assigned the respondent's wealth score and ranked from lowest to highest position in the socioeconomic gradient. For descriptive statistics, these rankings were then divided into quintiles. For regression estimates, each observation was assigned a ranking, r_{it}, based on its position in the cumulative distribution of the socioeconomic indicator.

The RII and SII were estimated using the following GLM:

$$f(\pi_{it}) = \beta_0 + \beta_1 r_{it} + \Sigma_k \delta_k X_{it} + \varepsilon, \quad (1)$$

where $\pi = E(Y_{it}) = P(Y_{it} = 1)$ for the binary outcome of interest (i.e., stillbirth, abortion, miscarriage), r_{it} is the fractional rank for observation i in year t, and X_{it} is a vector of k covariates. A linear probability model with an identity link function [i.e., $f(\pi) = \pi$)] was used to calculate the SII, presented as the difference in the risk of the outcome per 1,000 pregnancies. To calculate the RII, we fitted a Poisson regression model with a log link function [i.e., $f(\pi) = \log(\pi)$] and robust variance estimators.[141] The RII and SII are defined as the ratio and difference of the risk of the health outcome, respectively, comparing those at the top ($r_i = 1$) and bottom ($r_i = 0$) of the socioeconomic gradient.[142]

To enhance the comparability of our estimates, we adjusted for a vector of k covariates, X_k , that may vary across groups and time, including age of the mother, state of residence, and urban vs. rural residence. Results were stratified by year. As survey-specific sampling weights are relative weights and do not provide valid estimates when surveys are pooled, the weights from each survey were de-normalized.[91] We report inequality measures with corresponding robust 95% confidence intervals. All analyses were run using the survey package[143] in R Version 4.1.3 to account for the complex survey design.[114]

Results

Respondent characteristics are shown in **Table 4.1**. The sample included the 2,005,290 most recent pregnancies from surveyed women. The median age of women at time of survey was 25.3 (SD = 5.5) years, with 50% having completed primary school (≥ 8 years of school). 81% of participants lived in a rural location and 34% reported belonging to either a Scheduled Caste or Scheduled Tribe.

Table 4.1. Socio-demographic information on respondents

Variable	**N = 2,005,290**[1]
Age	25.3 (5.5)
Missing	6,490
Rural / Urban	
Urban	375,453 (19%)
Rural	1,629,837 (81%)
Member of Scheduled Caste or Scheduled Tribe	
None	1,305,093 (66%)
Scheduled Caste	382,551 (19%)
Scheduled Tribe	292,667 (15%)
Missing	24,979
Completed Primary School	954,921 (50%)
Missing	108,309

[1] Mean (SD); n (%)

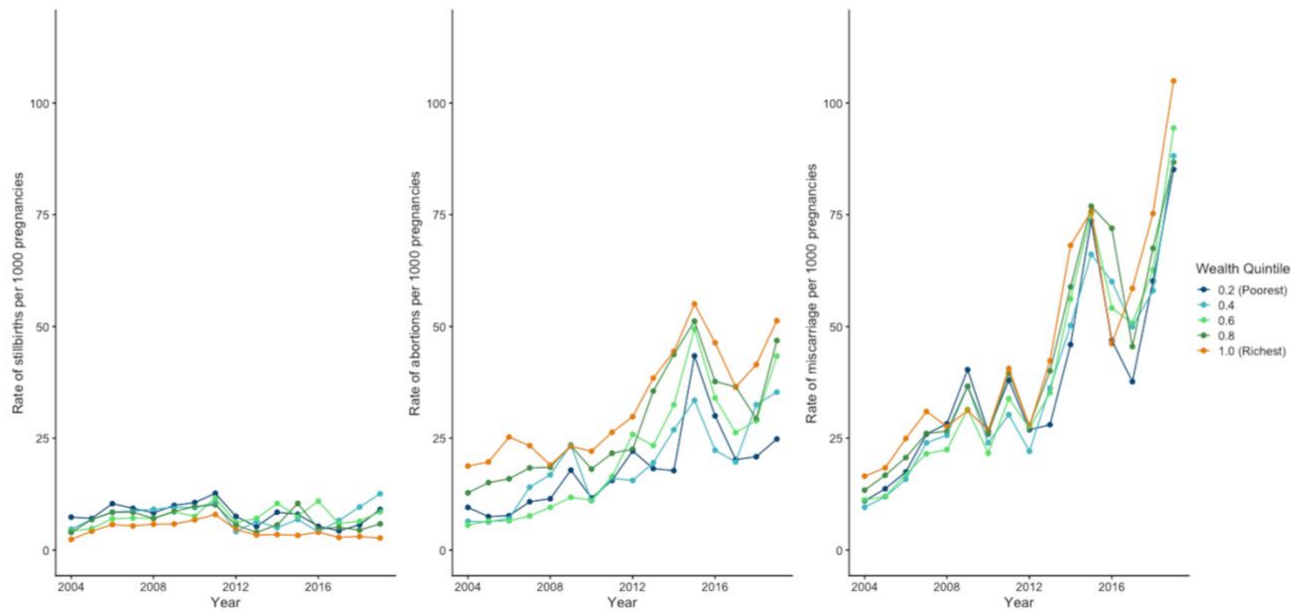


Figure 4.1. Rates of stillbirths, abortions, and miscarriages per 1,000 pregnancies by wealth quintile, 2004-2019

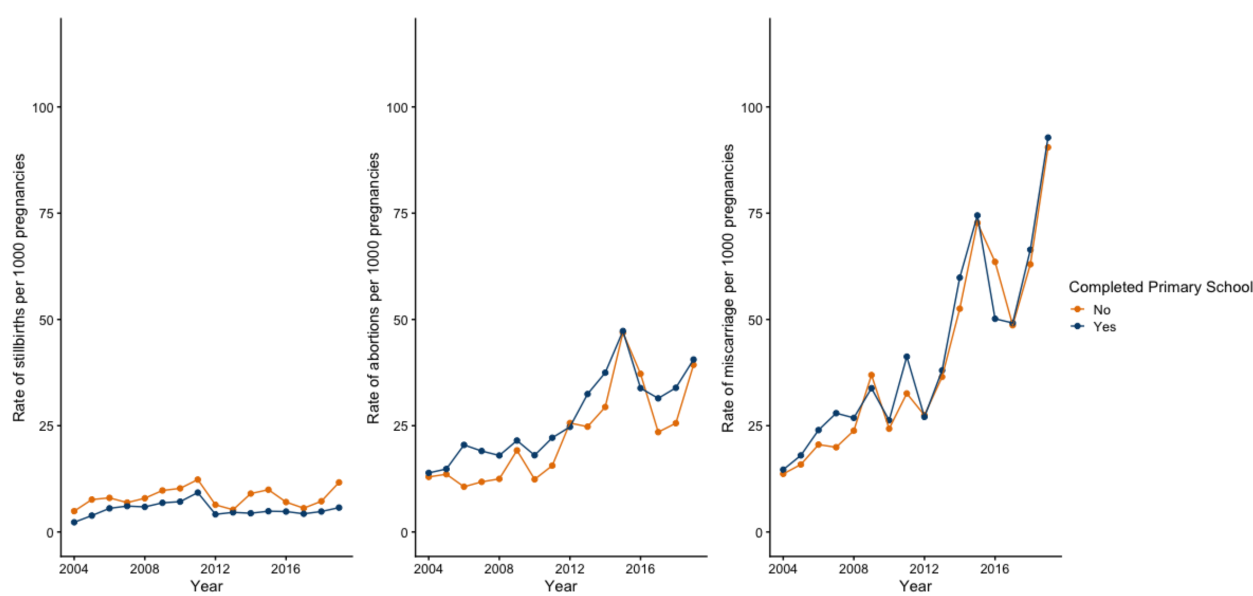


Figure 4.2. Rates of stillbirths, abortions, and miscarriages per 1,000 pregnancies by educational level, 2004-2019

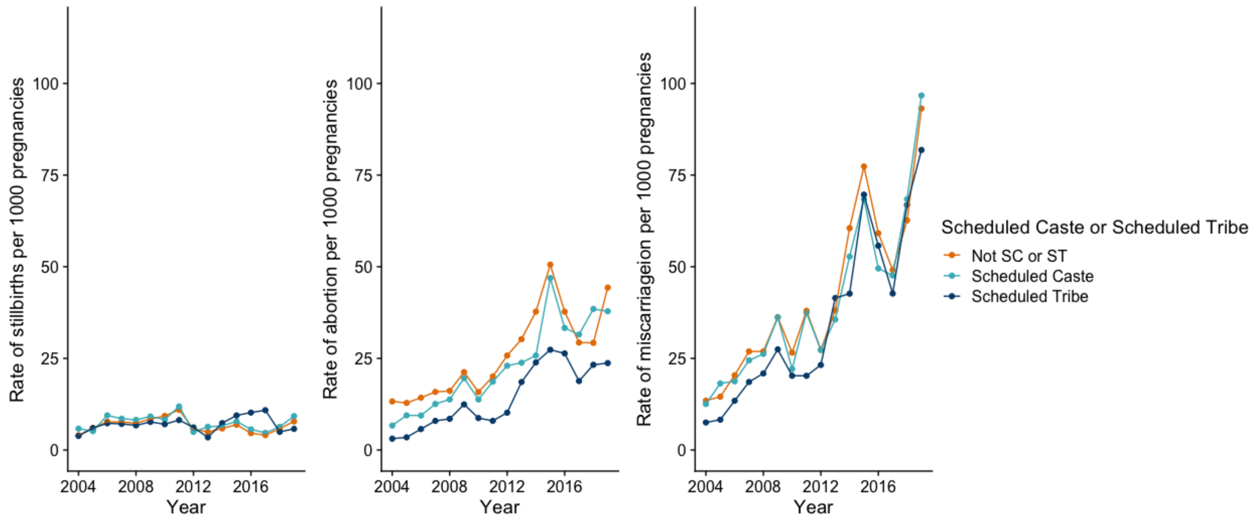


Figure 4.3 Rates of stillbirth, abortion, and miscarriage per 1,000 pregnancies by Schedule Caste/Scheduled Tribe group, 2004-2019

Descriptive Trends

Across all socioeconomic characteristics, there were increases in rates of abortion and miscarriage after 2008. **Figure 4.1** shows annual rates of stillbirth, abortion, and miscarriage per 1,000 pregnancies for each wealth quintile. There was a consistent social gradient in rates of stillbirth, with the lowest rates among those in the highest wealth quintiles across almost all study years. Conversely, rates of abortion were consistently higher in the highest wealth quintile, although in later years we observed increases in the lower wealth quintiles. There was no discernible wealth-based gradient for miscarriage, although we observed a larger increase in rates of miscarriage among women in the lowest wealth quintile. This trend was seen across states in the study sample (**Supplementary Figure 4.5**).

Figure 4.2 shows trends by whether the respondent completed primary school education. Rates of stillbirth were higher among women without a primary school education for each year surveyed. For abortion, rates were higher among women with a primary education between 2004-2011, however, the gap narrowed after 2012. Rates of miscarriage were similar across the two groups. Finally, **Figure 4.3** shows outcomes by whether the respondent belonged to a Scheduled Caste, a Scheduled Tribe, or neither. Rates of stillbirths and miscarriages were similar among all three groups, although rates of stillbirth increased among women belonging to a Scheduled Tribe relative to the other groups. Additionally, rates of abortion were lowest for women who belonged to a Scheduled Tribe.

Social Inequalities Measured by the Wealth Index

We observed wealth-based inequalities for all three outcomes. Estimates of the SII stratified by years are shown in **Figure 4.4**. Differences in rates of stillbirth between the top and bottom of the wealth distribution were observed in all years except 2004 – 2005, 2012 – 2013, and 2016 – 2017. Rates were consistent across the study period, with SII estimates of -1.4 (95% CI: -2.8, -0.6) in 2006 – 2007 and -5.3 (95% CI: -8.1, -2.5) in 2018 – 2019 (**Figure 4.4**). This indicates that over the study period women at the top of the wealth distribution experienced between 2 and 5 fewer stillbirths per 1,000 pregnancies compared to women at the bottom of the wealth distribution. A similar pattern was observed on the relative scale, with an RII of 0.8 (95% CI: 0.7, 0.9) in 2006 – 2007 and 0.4 (95% CI: 0.3, 0.6) in 2018 – 2019 (**Supplementary Figure 4.2**).

Absolute rates of abortion for women at the top of the wealth distribution were higher for all study years. The SII was 12.1 (95% CI: 8.2, 15.9) in 2004 – 2005 and 19.7 (95% CI: 11.4, 27.9) in 2018 – 2019, indicating that women at the top of the wealth distribution reported 12.1 and 19.7 more abortions per 1,000 pregnancies, respectively, compared to women at the bottom of the wealth distribution (**Figure 4.4**). On the relative scale, the RII was higher in 2004 – 2005 [RII = 3.5 (95% CI: 2.5, 4.9)] compared to 2018 – 2019 [RII = 1.6 (95% CI: 1.2, 1.9)], since abortion rates increased over the study period (**Supplementary Figure 4.2**). For miscarriage, women at the top of the wealth distribution reported 10.6 (95% CI: 6.7, 14.4) more miscarriages per 1,000 pregnancies in 2004 – 2005 and 19.8 (95% CI: 7.5, 32.0) more miscarriages per 1,000 pregnancies in 2018 – 2019, compared to those at the bottom of the wealth distribution (**Figure 4.4**). On the relative scale, the RII was 1.8 (95% CI: 1.4, 2.3) in 2004 – 2005 and 1.3 (95% CI: 1.1, 1.5) in 2018 – 2019 (**Supplementary Figure 4.2**). For all outcomes there was a significant interaction on the absolute scale between individual rank and year ($p < 0.05$), indicating heterogeneity in the magnitude of the inequality over the study period.

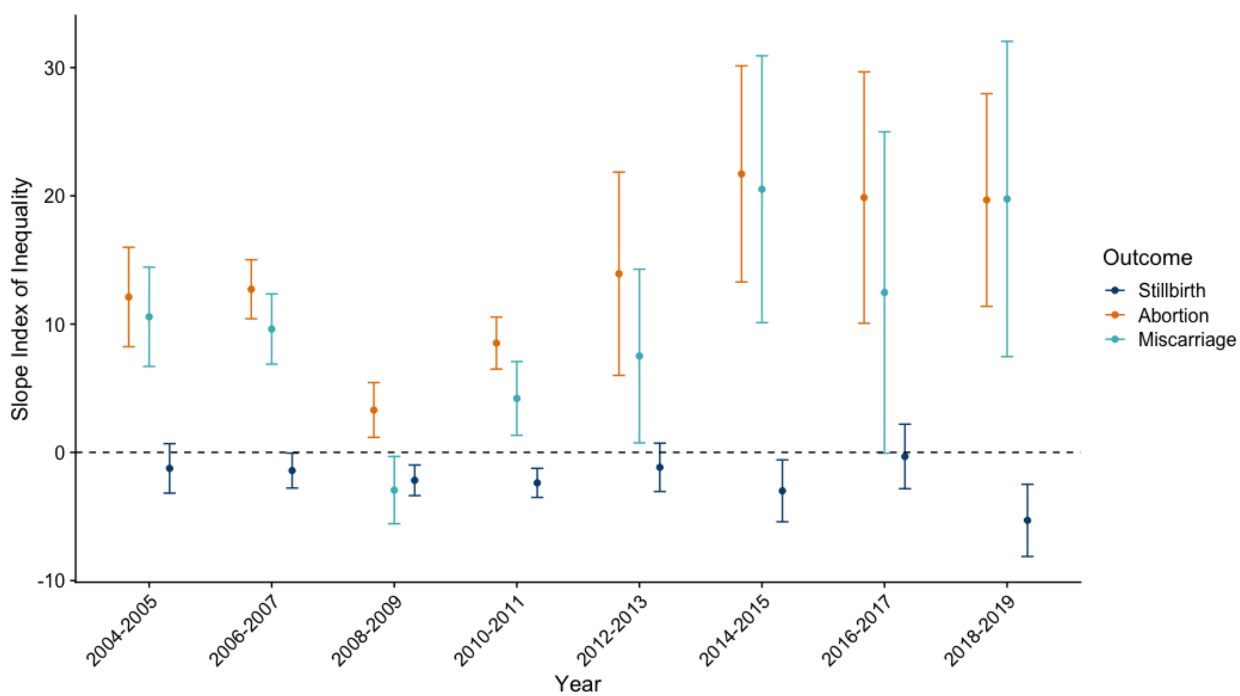


Figure 4.4 Slope index of inequality for measuring inequalities by household wealth in stillbirth, abortion, and miscarriage; India; 2004 - 2019

Education

Similar social inequalities in pregnancy outcomes were observed by educational attainment (**Figure 4.5**). Across most study years, rates of stillbirth were lower among women who completed primary school compared to those who did not. In 2004 – 2005, women who completed primary school had an absolute risk of -3.1 (95% CI: -4.9, -1.3) stillbirths per 1,000 pregnancies, compared to women who did not, which remained fairly consistent across study years. However, we did observe a significant interaction between education and year ($p < 0.05$) on the absolute scale, indicating heterogeneity in the magnitude of the inequality over the study period. On the relative scale, women who completed primary school had a relative risk for stillbirth of 0.5 (95% CI: 0.4, 0.7) in 2004 – 2005 and 0.7 in 2018 – 2019 (95% CI: 0.5, 0.9) (**Supplementary Figure 4.3**). Rates of abortion were higher among women who completed primary school compared to those who did not for many, but not all, years. In 2004 – 2005 there was no observed difference in the risk of abortion for women by educational attainment (RD = -0.3, 95% CI: -3.6, 2.9); however, the RD was positive in later years, with an estimate of 5.0 (95% CI: 0.2, 9.9) additional abortions per 1,000 pregnancies at the end of the study period in 2018 – 2019 (**Figure 4.5**). The corresponding estimates on the relative scale were RR = 1.0 (95% CI: 0.8, 1.3) in 2004 – 2005 and RR = 1.2 (95% CI: 1.0, 1.4) in 2018 – 2019 (**Supplementary Figure 4.3**). Education-based inequalities in miscarriage were less consistent over the study period (**Figure 4.5** and **Supplementary Figure 4.3**).

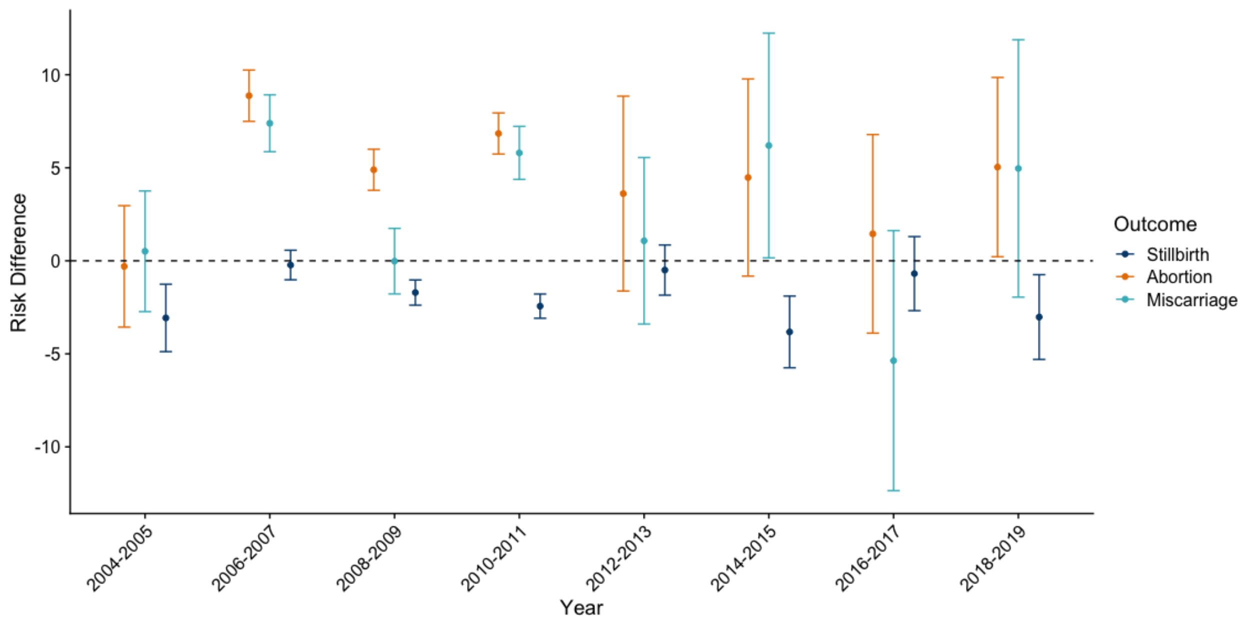


Figure 4.5. Risk Differences in stillbirth, abortion, and miscarriage by primary education attainment; India; 2004-2019

Scheduled Caste or Scheduled Tribe

Differences in pregnancy outcomes by membership in a Scheduled Caste or Scheduled Tribe were less apparent. For most years, there were little evidence of differences in the risk of stillbirth and miscarriage for women who belonged to Scheduled Caste on the absolute or relative scales (**Figure 4.6** and **Supplementary Figure 4.4**). In 2004 - 2005, women belonging to a Scheduled Caste reported 4.8 fewer abortions per 1,000 pregnancies (95% CI -7.2, -2.4) compared to women who did not belong to a Scheduled Caste or Scheduled Tribe. However, by 2018 – 2019 the RD was 2.1 (95% CI: -4.3, 8.4) (**Figure 4.6**), with similar patterns on the RR scale (**Supplementary Figure 4.4**). We did not observe consistent inequalities in risks of stillbirth or miscarriage for women who belonged to a Scheduled Tribe across study years

(**Figure 4.6** and **Supplementary Figure 4.4**). However, across all study years, the risk of abortion was lower for women belonging to Scheduled Tribe compared to women who were not in a Scheduled Caste or Scheduled Tribe, with an RD of 3.7 fewer abortions per 1,000 pregnancies (95% CI: -4.8, -2.6) in 2004 – 2005, and an RD of 5.7 fewer abortions per 1,000 pregnancies (95% CI: -9.2, 2.2) in 2018 - 2019, with relative estimates shown in **Supplementary Figure 4.4**.

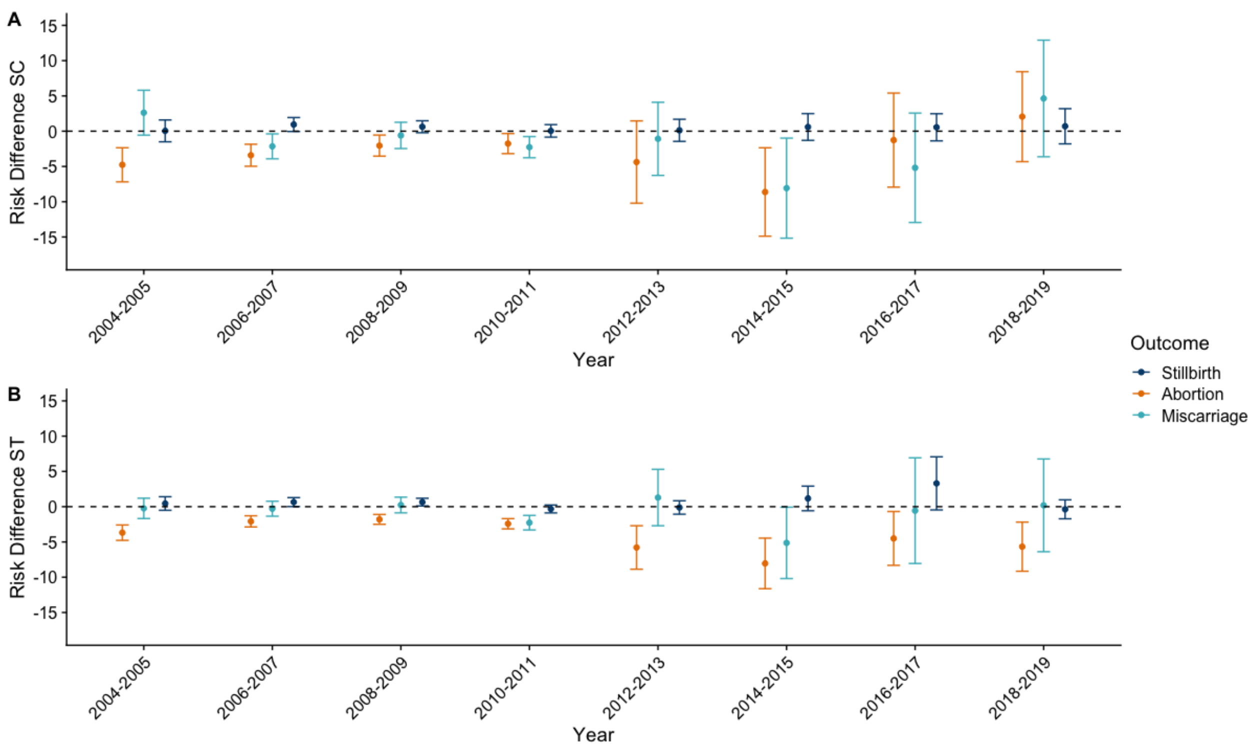


Figure 4.6. Risk Differences in stillbirth, abortion, and miscarriage by Scheduled Caste or Scheduled Tribe Status; India; 2004 -2019

Discussion

This study provides national estimates of socioeconomic inequalities in adverse pregnancy outcomes across a 15-year time span. We observed persistent socioeconomic inequalities in rates of abortion and stillbirth in India during the study period from 2004 – 2019. The magnitude of the inequalities varied according to the socioeconomic indicator and outcome. Across most study years, women at the top of the household wealth distribution and women who completed primary school were more likely to report an abortion and less likely to experience a stillbirth compared to women at the bottom of the wealth distribution and women who had not completed primary school. Women belonging to Scheduled Tribes were less likely to have an abortion compared to women who did not belong to either Scheduled Caste or Scheduled Tribe. There was less consistent evidence for socioeconomic inequalities in miscarriage, for which there were stronger positive trends for all groups over the study period and greater between year variability in rates. There were substantial increases in rates of abortion and miscarriage, with an acceleration in rates across all socioeconomic groups after 2008.

Our research follows prior work showing socioeconomic disparities in adverse pregnancy outcomes in India. Higher abortion rates among women of higher socioeconomic status have been documented by previous studies.[54,144] Our results are also in line with the national estimates obtained from National Family Health surveys, India's version of the Demographic Health Survey.[120-122] There were also results that differed from previous literature. Previous research has showed that higher education was associated with higher risk of miscarriage,[145] and that maternal education is the strongest predictor of induced abortion likely due to both access and resources for payment.[146] Prior work also shows disparities in access to, and utilization of, maternal and reproductive health services by women belonging to a Scheduled Caste or Scheduled Tribe.[147]

We observed disparities in miscarriage and abortion, though not in the later study years. We did not observe disparities in rates of stillbirth. We observed that belonging to the highest quintile of the household wealth distribution was a stronger and more consistent predictor of lower stillbirth and higher abortion rates than completing primary education. This may be because greater household wealth and attendant material resources facilitate access to essential reproductive services such as abortion or antenatal care, which are important mechanisms for preventing stillbirth.[54,148] Recent research has shown that rates of stillbirth and miscarriage in India are increasing, with a corresponding decrease in rates of livebirths.[149] Our analyses indicate that these observed increases are not equal across socioeconomic groups.

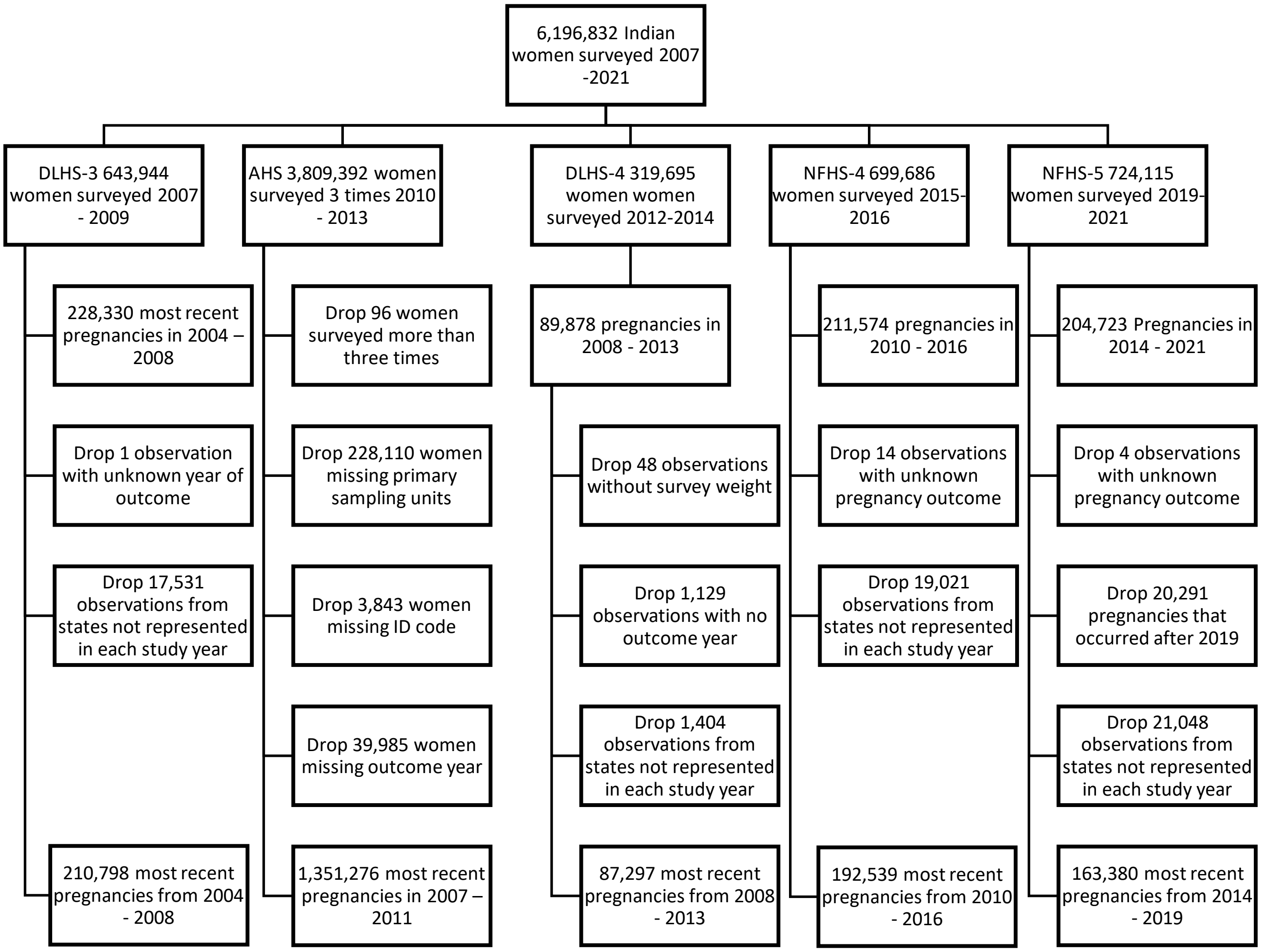
There are several limitations to the reported research. First, all outcomes were self-reported and subject to measurement error, including recall bias. This is a salient issue for stillbirth, which in self-report data is hard to differentiate from early neonatal death.[136] However, these surveys use well-validated instruments[150,151] and we restricted our sample to the most recent pregnancy, which should reduce recall bias.[152] Secondly, the AHS focused on nine states with disproportionately worse maternal and infant health outcomes, resulting in a larger number of observations from these states between 2007 – 2011. Additionally, pregnancy information from some years (e.g., 2007) was available from multiple surveys (e.g., AHS and DLHS-3), resulting in uneven sample sizes per year and higher levels of precision in the earlier part of the study period. However, before pooling data from multiple surveys, we de-normalized each survey's sampling weights based on the proportion of women aged 15-49 in each state, available from the Census,[92,153] who were sampled by the survey, using the de-normalization of standard weights approach described in the DHS Sampling and Household Listing Manual.[154] Thirdly, abortion may have been underreported or misclassified as miscarriage, and this error may have been differential by socioeconomic status. Due to social stigma and bias, it is likely that some abortions were self-reported as miscarriages, as abortion has been found to be underreported among Indian women of lower socioeconomic status.[54] This misclassification is common when measuring abortion – particularly for self-managed medication abortion,[155] and is a salient issue in India, where combating gender-biased sex selection by abortion is a top priority for the national government.[156,157] All else equal, differential underreporting among socially disadvantaged groups may have resulted in an overestimate of the disparities in reported abortion by household wealth and education reported above.

Conclusion

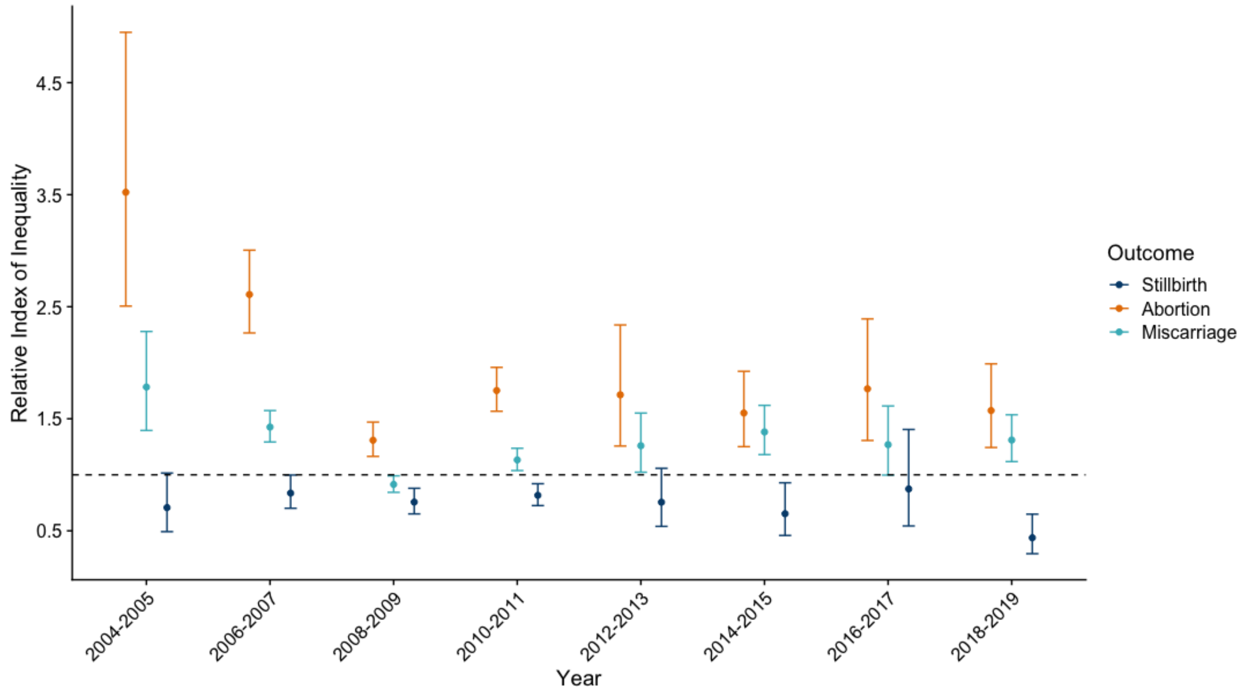
Our study provides evidence of persistent socioeconomic inequalities in rates of stillbirth and abortion, despite targeted investments by the GoI to improve access to healthcare among socioeconomically disadvantaged groups. Additionally, we observed pronounced increases in rates of miscarriage across all socioeconomic groups. Further research is needed to investigate these alarming trends, notably the increase in miscarriage rates, and understand the mechanisms underlying social inequalities in pregnancy outcomes, including higher rates of stillbirth and lower rates of abortion among socioeconomically disadvantaged compared to advantaged groups.

4.3 Supplementary appendix: Manuscript 1

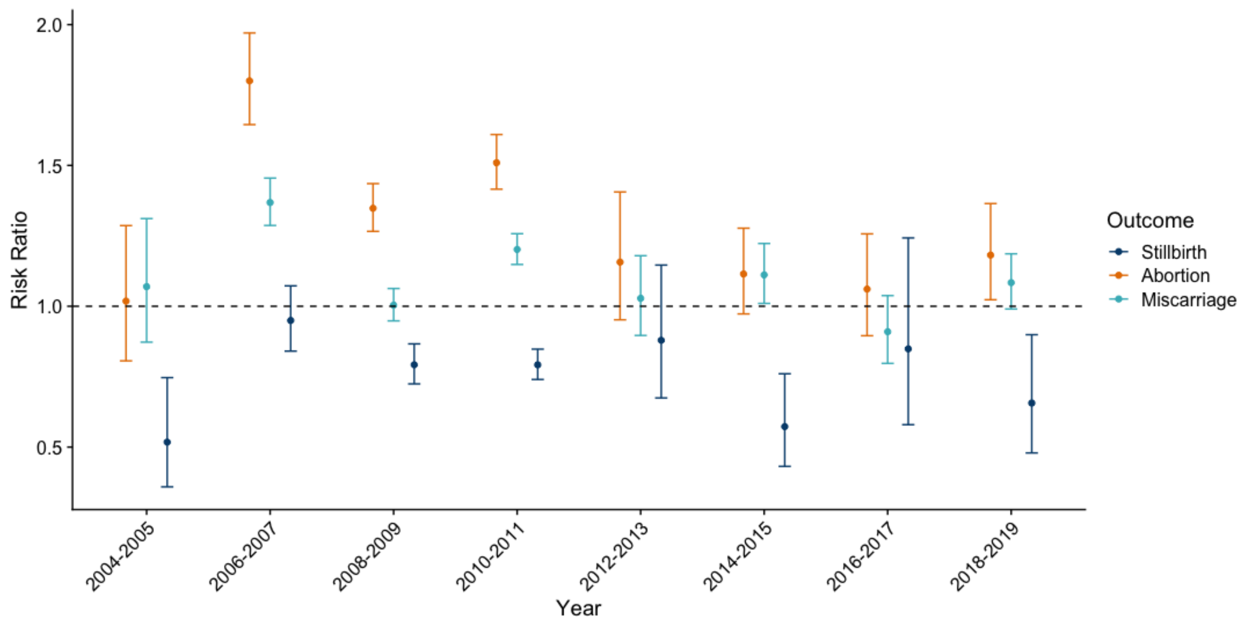
Supplementary Figures



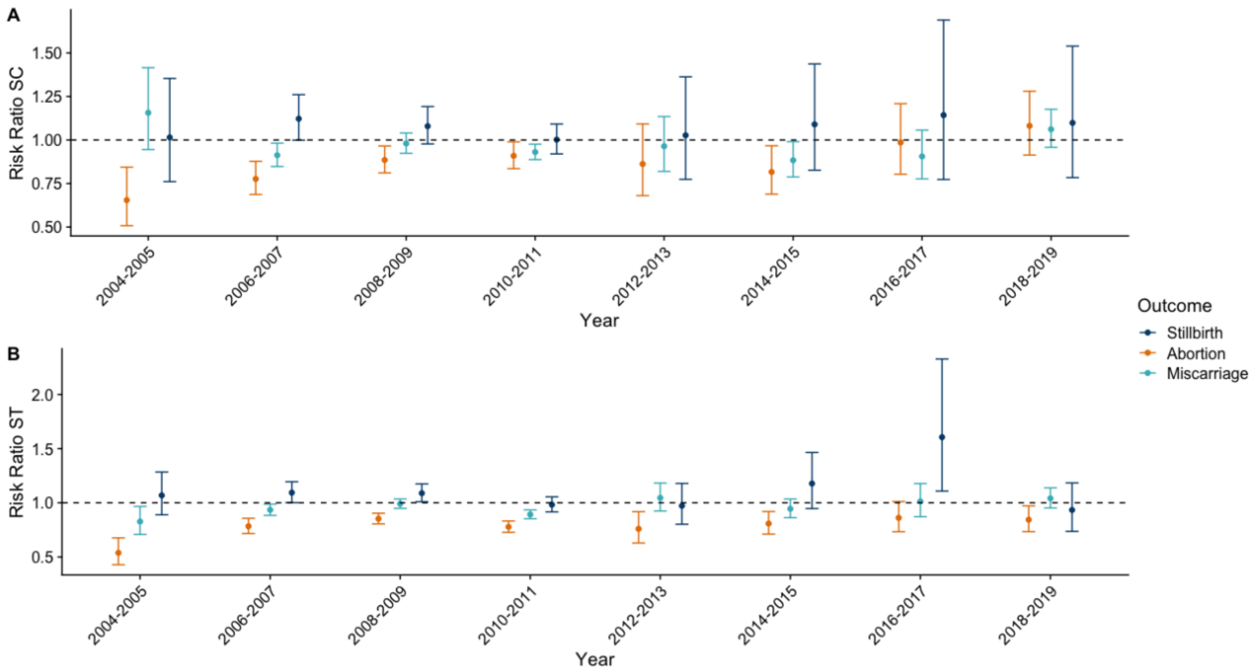
Supplementary Figure 4.1. Flowchart describing creation of analytic samples



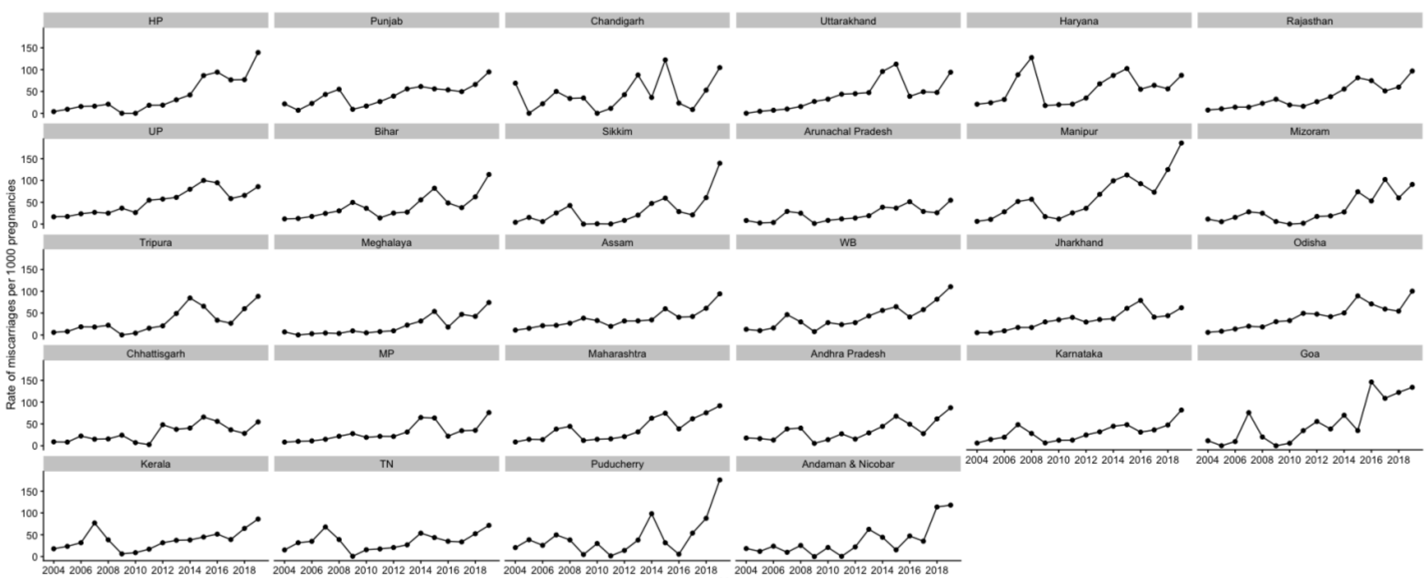
Supplementary Figure 4.2 Relative index of inequality for relationship between wealth index and stillbirth, abortion, and miscarriage; India; 2004 - 2019



Supplementary Figure 4.3 Risk ratio measuring the relationship between primary education attainment and stillbirth, abortion, and miscarriage; India; 2004 - 2019



Supplementary Figure 4.4. Risk ratio measuring relationship between Scheduled Caste or Scheduled Tribe status and stillbirth, abortion, and miscarriage; India; 2004-2019



Supplementary Figure 4.5. Rates of miscarriage per 1,000 pregnancies by year and state

Chapter 5. Impact of district-level access to RSBY on adverse pregnancy outcomes from 2004-2019: a difference-in-differences analysis

5.1 Preface: Manuscript 2

Most evidence on the impact of RSBY examines one or two states at two time points – before and after policy implementation. However, evidence from specific states may not be generalizable to India as a whole, and it is therefore hard to draw conclusions about the overall impact of the policy across India.

Chapter 4 demonstrates socioeconomic inequalities in adverse pregnancy outcomes. Following this, I felt it was important to examine whether these existing inequalities impacted the role RSBY had on adverse pregnancy outcomes.

This study uses a differences-in-differences analysis to examine the causal effect of RSBY on adverse pregnancy outcomes. As different districts received access to RSBY at different time points I utilized recent methodological advances in the difference-in-differences literature to account for the staggered roll-out of the program. This approach was described in **Chapter 3**.

This work was presented at the Society for Epidemiological Research (SER) Annual Meeting in June 2023. It is being prepared for submission for publication.

5.2 Manuscript 2

Impact of district-level access to RSBY on adverse pregnancy outcomes from 2004-2019: a difference-in-differences analysis

Caroline M. Joyce[1], Deepti Sharma[2], Arnab Mukherji[2], Arijit Nandi[1,3]

Affiliations:

[1] Department of Epidemiology, Biostatistics, and Occupational Health, School of Population and Global Health, McGill University

[2] Center for Public Policy, Indian Institute of Management Bangalore

[3] Institute for Health and Social Policy, McGill University

Corresponding Author:

Caroline Joyce

2001 McGill College Avenue

Montreal, QC H3A 1G1

Canada

Email: caroline.joyce@mail.mcgill.ca

Abstract

Background: India has high rates of adverse pregnancy outcomes, which are compounded by socioeconomic disparities. India does not have a universal health insurance scheme, and those at or below the poverty line pay a disproportionate percentage of their income on healthcare expenditures. The Rashtriya Swasthya Bima Yojana (RSBY) health insurance program was launched in 2008 with the aim of covering inpatient healthcare services for families at or below the poverty line. This study evaluates the causal impact of access to RSBY on probabilities of stillbirth, abortion, and miscarriage, and whether this varied by indicators of social position.

Methods: Using difference-in-differences analyses that account for heterogeneous treatment timing, we estimated the causal impact of district-level access to RSBY on the probability of a pregnancy ending in stillbirth, abortion, or miscarriage. We report estimates of the overall effect of access to RSBY, along with stratified estimates by socioeconomic group.

Results: We did not observe an overall impact of the policy on rates of stillbirth or abortion. However, district-level access to RSBY was associated with 6.3 additional miscarriages per 1,000 pregnancies compared to control areas. There was some variation across groups defined by when the district implemented RSBY. Additionally, RSBY was associated with higher rates of miscarriage among women who had completed primary school, and women below the median wealth index.

Conclusion: We observed no overall improvement in rates of stillbirth or abortion among women in districts that received access to RSBY relative to control districts. Access to RSBY was associated with an increase in rates of miscarriage compared to control districts. Challenges in the roll-out and administration of RSBY may have limited its impact.

Keywords: Maternal health; stillbirth; abortion; miscarriage; policy analysis; difference-in-differences analysis; causal inference

Introduction

Despite significant reductions in maternal and neonatal mortality, India continues to account for an estimated one-fifth of all global maternal and early neonatal deaths,[2,115] and is not on track to meet the United Nations (UN) Sustainable Development Goals (SDGs) to lower rates of maternal and infant mortality by 2030.[2,16] Adverse pregnancy outcomes such as stillbirth and miscarriage contribute to these high rates of maternal and neonatal mortality. India accounts for a disproportionate number of stillbirths relative to live births, contributing 19% of births worldwide but 22.6% of the global burden of stillbirths – more than any other country.[18] Recurrent miscarriage is associated with higher likelihood of neonatal death,[23] and India's miscarriage rates – particularly those that occur after the first trimester – are higher than the global average.[19-21] Recent research has also shown that rates of recurrent miscarriage are higher in India compared to high-income countries.[22] Reducing adverse pregnancy outcomes is critical to accelerating decreases in rates of maternal and neonatal mortality,[116] and achieving India's SDG targets.

In order to meet the SDG to lower rates of maternal mortality, the UN calls for ensuring universal access to sexual and reproductive healthcare.[16] The World Health Organization (WHO) expands upon this, stating that access to quality reproductive healthcare – including modern contraception and safe induced abortion – is critical for preventing unplanned pregnancies, and lowering rates of miscarriage and stillbirth.[25] While abortion has been legal in India since 1971,[27] availability does not guarantee access. Lack of access to quality and safe abortion is a primary risk factor for miscarriage and stillbirth. There are other preventable risk factors for experiencing a stillbirth or miscarriage such as: lack of antenatal care (ANC), giving birth without a skilled birth attendant (SBA), and receiving inadequate nutrition during pregnancy.[29] India does not have a universal health insurance scheme to help defray the costs of this care. Health costs are high, with those at or below the poverty line paying a disproportionate percentage of their income on healthcare expenditures.[67,68]

Socioeconomically disadvantaged women have less access to modern contraceptives and safe abortion services,[4] often due to financial barriers that prevent women from accessing this type of care. Poorer women in India are also at greater risk for stillbirth and miscarriage because they are less likely to receive ANC, deliver with a SBA,[51,52] and have adequate nutrition.[53] Women with less formal schooling, including illiterate women, are also more likely to experience stillbirth and miscarriage.[3,12] While surgical abortion is technically free at public facilities, women may be subject to other costs,[54] including the direct costs of the anesthesia and medication necessary for the procedure,[55] along with indirect costs such as missing work or costs of transportation to the facility.[56] The drugs required for medication-induced abortion are not free, and women must pay out-of-pocket to access this care.[54,57] Without access to quality, affordable, maternal and reproductive healthcare, including abortion services, women of lower socioeconomic status are more likely to experience unplanned pregnancies,[158] which are at higher risk for adverse outcomes.[159]

In part to address disparities in access to health services, the Government of India (GoI) launched the Rashtriya Swasthya Bima Yojana (RSBY) program in 2008, created specifically for families at or below the poverty line.[70] The goal of the RSBY program was to increase access to inpatient hospital services for those individuals who might not otherwise be able to afford them. The RSBY program was implemented by state governments, though the list of services covered came from the national level and is therefore standardized. The state governments secured insurance companies to administer the program and enroll beneficiaries through competitive bidding. The insurance companies were responsible for enrolling eligible families, who were then responsible for a low enrollment fee. Enrolled families received a RSBY identity card, which they showed at the time of hospital admission. Not all hospitals accepted the RSBY program; instead, private and public hospitals that satisfied government standards entered into a service agreement with insurance companies and provided services to enrolled families. Hospitals invoiced the insurance company directly and received reimbursement according to a set fee schedule. Eligible households were covered for up to INR 30,000 annually (roughly USD $500 in 2008), and coverage was limited to a maximum of five family members. States started enrolling households in 2008, with a goal of introducing the policy to 20% of districts in the state in each consecutive year. The number of districts with access to RSBY increased from 0 in 2007 to 487 (out of a total of 707) in 2016.

The RSBY program covered specific inpatient services identified as imperative for lowering maternal and neonatal morbidity and mortality. For example, it covered in-hospital sexual and reproductive health services, such as family planning services and childbirth – including delivery by caesarean section. By covering in-hospital expenses, it also freed up income that would otherwise have been spent on out-of-pocket healthcare costs. This income could then be used to cover other health services that weren't included under the RSBY program, such as contraception that is prescribed as part of an outpatient visit. Therefore, it has been hypothesized that enrollment into the RSBY program will have direct and indirect effects on reproductive health outcomes.

While the RSBY program has been previously studied, prior research has not investigated its impact on adverse pregnancy outcomes. Extant research has focused on the uptake and enrollment into the program after roll-out.[73-75] Other work has assessed the impact of enrollment into the RSBY program on out-of-pocket healthcare expenses, with some research showing that enrollment has not substantially decreased expenses, but is associated with greater use of healthcare and treatment.[9] Some research has shown that women enrolled in the RSBY program do not use it for sexual and reproductive health services as they were not aware that those services were covered.[76] Other work has shown RSBY to be less effective in increasing reproductive health care service utilization than other factors such as maternal empowerment and household wealth.[77] Rigorous evaluation of the impact of RSBY on levels and socioeconomic inequalities in stillbirth, abortion, and miscarriage is needed as India expands health insurance access through a new health insurance program modeled after RSBY, Ayushman Bharat-Pradhan Mantri Jan Arogya Yojana (AB-PMJAY).[79]

This study aims to provide a rigorous evaluation of the causal impact of access to RSBY, which was expanded across districts, on probabilities of stillbirth, abortion, and miscarriage, and whether this varied by social position. Using data from three nationally representative surveys with information on pregnancy outcomes from 2004 through 2019, we leveraged the phased-in implementation across districts to estimate the impact of district-level access to RSBY using a difference-in-differences design.

Methods

Data Sources

Data were derived from three district-representative household surveys with similar target populations, sampling designs, and survey instruments that were conducted by the Ministry of Health and Family Welfare (MoHFW): (1) District Level Household Surveys (DLHS) rounds III and IV; (2) National Family Health Survey (NFHS) rounds IV and V; and (3) the Annual Health

Survey (AHS) of India. These data have been pooled previously to facilitate cross-sectional and longitudinal analyses.[124-131] Together, the DLHS, AHS, and NFHS provide district-representative cross-sections of the target population of ever-married women between 15 and 49 years of age in nearly all states and territories during the study period from 2004-2019.

We used the information provided by women in each survey on their reproductive histories to construct a panel of pregnancies and corresponding pregnancy outcomes (i.e., live birth, miscarriage, abortion, or stillbirth) by district over the study period. The NFHS surveys only recorded the specific type of non-live birth outcome for the most recent occurrence of this. Therefore, for comparability across surveys, we included only the most recent pregnancy outcome for each respondent. Technical details for each survey are available elsewhere.[89,132-134] We limited our sample to districts that were represented in each year of the study, which reduced the number of districts from 555 to 463.

<u>Measures</u>

Our primary exposure was district-level access to RSBY. To measure district-level exposure to RSBY we identified the first year any household was enrolled in each district, which we accessed from the GoI RSBY website via the Internet Archive. The first year that districts could enroll households in the RSBY program was 2008, with district-level roll-out completed by 2014.[71] States were instructed to begin offering RSBY to approximately 20% of districts in 2008, with additional districts added each subsequent year. To account for variation in treatment timing, we assigned districts to four groups: 1) early adopters enrolled between 2008 – 2010; 2) mid-adopters enrolled in 2011 – 2012; 3) late-adopters enrolled 2013 – 2014; and 4) districts that had not adopted RSBY by 2017. From here forth we refer to these groups as early, mid, late, and never adopters respectively. We assigned exposure to each surveyed women based on the district they were residing in at the time of survey.

Our primary outcomes of interest were self-reported stillbirth, abortion, and miscarriage. These were obtained from the woman's questionnaire in each survey. In the DLHS and AHS, women were asked to report all pregnancies and their outcomes in the last three years. In the NFHS, as part of the woman's questionnaire, the contraceptive calendar was used to self-report reproductive health outcomes (contraception, pregnancies, births) for each month of the past five years. Following Demographic Health Survey (DHS) guidelines, spontaneous abortions before the seventh month (28 weeks) of pregnancy were defined as miscarriages, and those that occurred during or after the seventh month (28 weeks) of pregnancy were defined as stillbirths.[135,136] We used this information to define three binary outcome variables for each participant's most recent pregnancy: 1) stillbirth; 2) induced abortion; or 3) miscarriage.

Household and respondent-level socioeconomic characteristics were used to measure social inequalities in the impact of district-level access to RSBY. We used information on reported household assets[137] to create a continuous wealth index. Further details on the creation of the wealth index are available in the Supplement. We used the mother's home location and self-reported educational attainment to create indicators for urban vs. rural residence and whether the respondent completed primary school.

Statistical Analyses

We used a difference-in-differences (DiD) approach that accommodates staggered treatments (i.e., groups gaining access to the intervention at different times)[104] to estimate the impact of RSBY on rates of stillbirth, abortion, and miscarriage. The standard DiD design accounts for time-invariant characteristics that vary across treatment groups and secular trends in outcomes that are shared across treatment and control groups by including fixed effects.[160] Recently, various methods have been proposed for addressing heterogeneous treatment timing in DiD.[161] Compared to the scenario where all treated units receive the intervention at the same time, additional assumptions are necessary for unbiased estimation of the treatment effect when treatments are staggered. We assume treatment in RSBY is an absorbing state, that is once a district is treated it remains treated. We also assume no anticipation, that is knowledge of future treatment does not affect outcomes in the pre-treatment period.[103] We follow the method proposed by Callaway and Sant'Anna[104] for estimating the Average Treatment Effect on the Treated (ATT).

The approach of Callaway and Sant'Anna estimates the ATT by comparing changes in outcomes in the treated groups before and after the intervention to corresponding changes in a control group that was either not-yet or never treated. Specifically this approach compares outcomes among pregnancies at timepoints t and g – 1, the period directly before the year of implementation in treated districts denoted $G_i = g$, where g corresponds to the year the treated district gained access, to the analogous change in pregnancy outcomes in the control group. To estimate the overall ATT and group-time average treatment effects (ATT (g, t)) for each treatment group (g) and each year (t) we ran linear probability models separately for each outcome (stillbirth, abortion, miscarriage) using the Callaway Sant'Anna did package in R:[162]

$$Y = \alpha_1^{g,t} + \alpha_2^{g,t} \cdot G_i + \alpha_3^{g,t} \cdot 1\{T = t\} + \beta^{g,t} \cdot (G_i \times 1\{T = t\}) + \varepsilon^{g,t} \qquad \text{(Eq. 1)}$$

In this model: Y is the probability of each outcome; G_i indicates whether the district received access to RSBY in year t; $T = t$ indexes when, in years, the pregnancy outcome occurred since the start of the study period, $t = 1$; T is equal to the total number of years in the study period; and $\beta^{g,t}$ is the ATT for group g at time t. District and time t are treated as fixed effects.

We aggregated the group-time ATTs to assess how effects varied with the length of exposure to RSBY and across treatment groups. The dynamic effects for each year (duration) of exposure were calculated using the weighted average of ATT (g, t) estimates by year, in relation to when the program was introduced in the district.[104] Following the Callaway and Sant'Anna (2021) approach,[104] this was calculated as a cumulative average treatment effect for all groups in treated years t as:

$$\theta_c(t) = \sum_{g\epsilon\mathcal{G}} 1\,\{t \geq g\}\, P(G = g|G \leq t)\; ATT\,(g, t) \qquad \text{(Eq. 2)}$$

We also examined variation in treatment effects by group with the following equation:

$$\theta_{sel}(\tilde{g}) = \frac{1}{T-\tilde{g}+1} \sum_{t=\tilde{g}}^{T} ATT\,(\tilde{g}, t) \qquad \text{(Eq. 3)}$$

where $\theta_{sel}(\tilde{g})$ is the average effect of exposure to RSBY among all women who received access in group ($\tilde{g}$), across time after implementation.

We reported overall ATTs on the risk difference (RD) scale as the change in the number of events (e.g., stillbirths) per 1,000 pregnancies, along with stratified estimates by socioeconomic group. Standard errors were clustered at the district level using the multiplier bootstrap procedure.[104] To assess the credibility of our assumptions, we visually inspected pre-enrollment data to assess whether it met the parallel trends assumption. As a sensitivity analysis, in the Supplement we compared estimates of the overall ATTs to those estimated using the method proposed by Wooldridge.[106]

Results

Respondent characteristics are shown in **Table 5.1**. The sample included the 1,887,492 most recent pregnancies from respondents in 463 districts. 1,225,712 of pregnancies occurred in districts that were early-adopters of RSBY, with 189,382 in mid-adopter districts and 265,040 in late-adopter districts. Of the surveyed women, 207,358 lived in districts that never received access to RSBY. The mean age of surveyed women was 27 years. 29% of women in mid-adopter districts and 28% of women in districts that did not introduce RSBY lived in urban areas, compared to 20% of women in early-adopter districts and 18% of women in late-adopter districts. A lower proportion of women in early-adopter districts (5.2%) identified as members of a Scheduled Tribe compared to women in mid-adopter (16%), late-adopter (17%), and never-adopter (21%) districts. 45% of women in early-adopter districts completed primary school, compared to 55% in mid-adopter districts, 43% in late-adopter districts, and 52% in never-adopter districts.

Table 5.1. Socio-demographic information on respondents overall and stratified by district-level RSBY access; all pregnancies occurring 2004 - 2019.

		District-level Access				
Characteristic	**Missing (N)**	**Overall** (N = 1887492)[1]	**2010 - Early** (N = 1225712)[1]	**2012 - Mid** (N = 189382)[1]	**2014 - Late** (N = 265040)[1]	**None** (N = 207358)[1]
Age	21	27 (6)	28 (6)	26 (5)	27 (6)	26 (5)
Rural / Urban	0					
Urban		347,902 (21%)	213,177 (20%)	43,507 (29%)	37,239 (18%)	53,979 (28%)
Rural		1,539,590 (79%)	1,012,535 (80%)	145,875 (71%)	227,801 (82%)	153,379 (72%)
Member of Scheduled Caste or Scheduled Tribe	22,106					
None		1,232,679 (69%)	831,053 (72%)	110,439 (62%)	172,742 (66%)	118,445 (60%)
Scheduled Caste		360,104 (22%)	252,345 (23%)	36,544 (22%)	38,139 (18%)	33,076 (18%)
Scheduled Tribe		272,603 (9.0%)	132,959 (5.2%)	37,469 (16%)	47,919 (17%)	54,256 (21%)
Completed Primary School	96,958	891,083 (47%)	564,332 (45%)	107,552 (55%)	117,901 (43%)	101,298 (52%)

[1] Mean (SD); n (%)

Overall rates of the outcomes are seen in Figure 5.1. In 2004 there weas an estimated 4.7 stillbirths per 1,000 pregnancies, which increased to 8.1 stillbirths per 1,000 pregnancies in 2019. We observed an increase in rates of abortion across the study years from 10.9 per 1,000 pregnancies in 2004 to 38.9 per 1,000 pregnancies in 2019. Similarly, we observed an increase in rates of miscarriage across the study years, from 12.2 miscarriages per 1,000 pregnancies in 2004 to 94.9 miscarriages per 1,000 pregnancies in 2019.

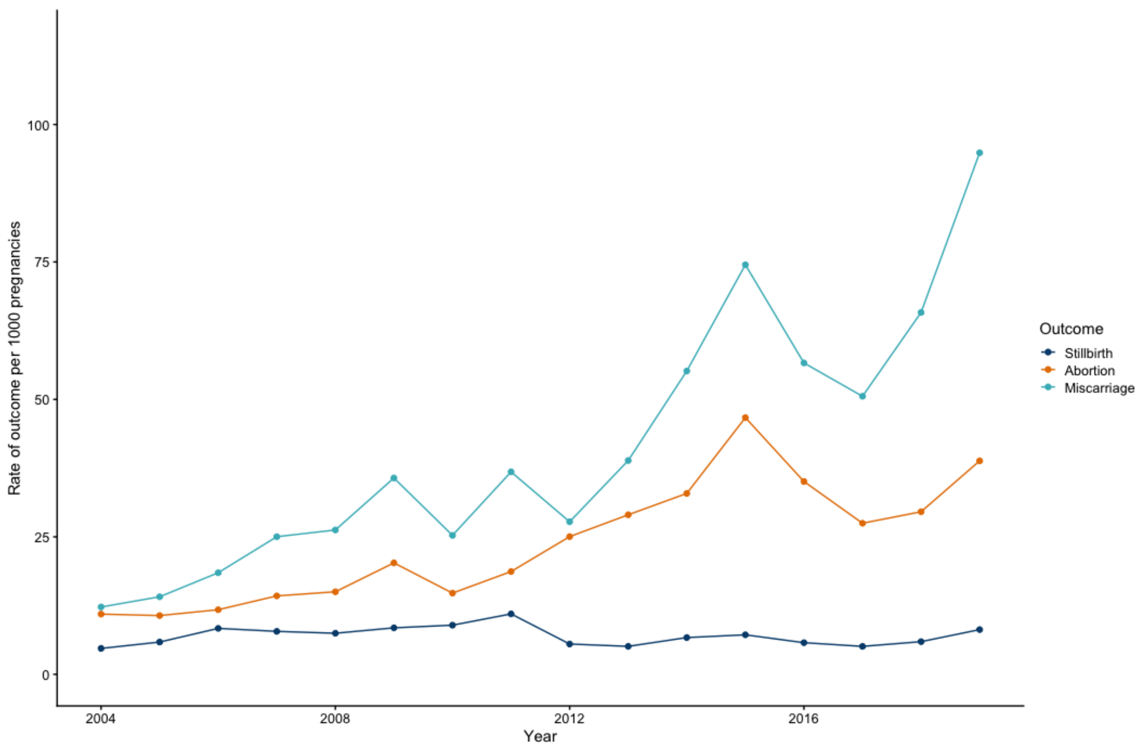


Figure 5.1. Rate of primary outcomes per 1,000 pregnancies across study years, 2004-2019

Overall Treatment Effect

We estimated the average treatment effect on the treated of receiving district-level access to RSBY on pregnancy outcomes, presented on the risk difference scale with corresponding 95% confidence intervals (CI). Access to RSBY was associated with 0.1 additional stillbirths per

1,000 pregnancies (95% CI: -1.5, 1.7) (**Figure 5.2**) and 1.7 fewer abortions per 1,000 pregnancies (95% CI: -9.1, 5.7). **Figure 5.3** shows the dynamic effects in relation to the time since a district introduced RSBY; we did not observe evidence of a consistent lagged effect. Finally, district-level access to RSBY was associated with 6.3 additional miscarriages per 1,000 pregnancies (95% CI: 0.9, 11.7) (**Figure 5.4**).

There was some evidence of heterogeneity in ATTs across the treatment groups (**Figure 5.5)**. In mid-adopter districts, the introduction of RSBY was associated with -3.2 (95% CI: -5.5, -0.7) fewer stillbirths per 1,000 pregnancies, compared to the control group. In early-adopter districts, we estimated that RSBY access was associated with 6.8 (95% CI: 0.7, 12.8) additional miscarriages per 1,000 pregnancies compared to women in control districts. The estimated effects of RSBY access in mid-adopter and late-adopter districts were 3.2 (95% CI: -5.8, 12.1) and 7.6 (95% CI: -2.9, 18.1) additional miscarriages per 1,000 pregnancies, respectively, although these effects were estimated with substantial imprecision.

Sensitivity analyses showing the comparison between the Callaway and Sant'Anna vs. Wooldridge estimators are showing in **Supplementary Table 5.1**. The estimated ATTs differed slightly, though all had overlapping confidence intervals.

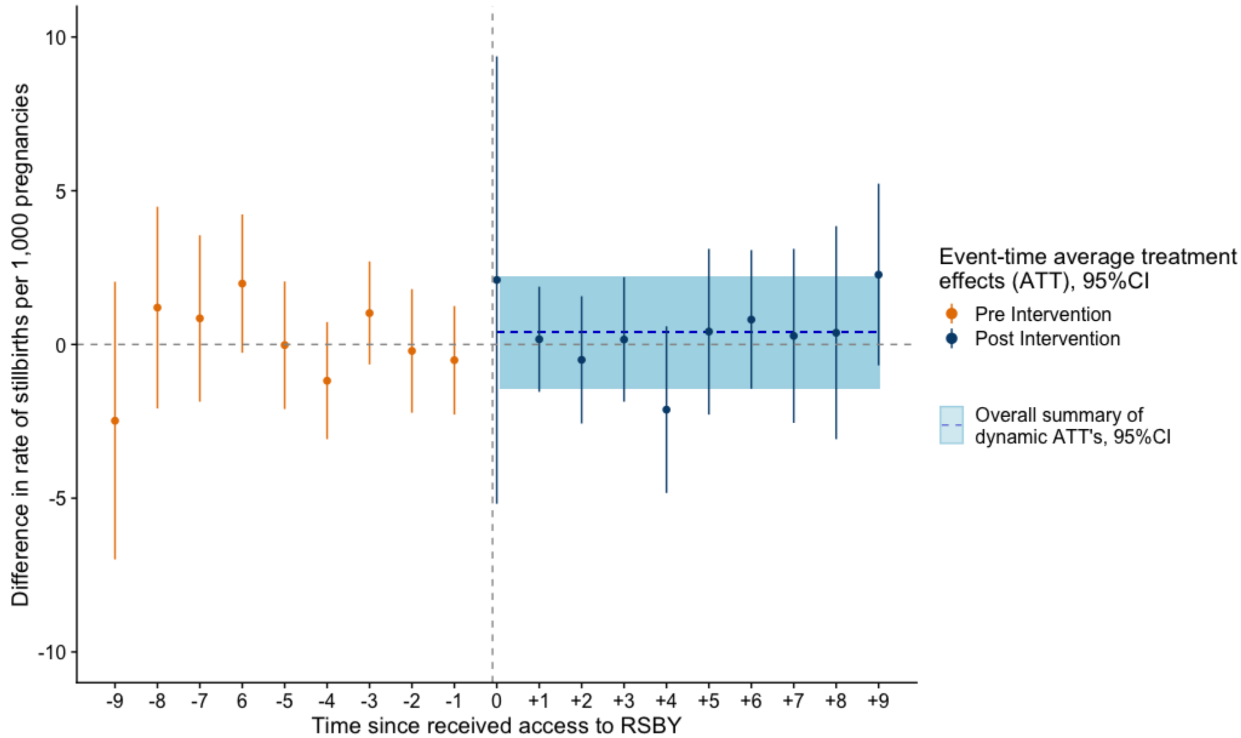


Figure 5.2 Effects of district-level RSBY access on stillbirths per 1,000 pregnancies, by time since the district gained access to RSBY; India, 2004-20019; risk differences with 95% confidence intervals

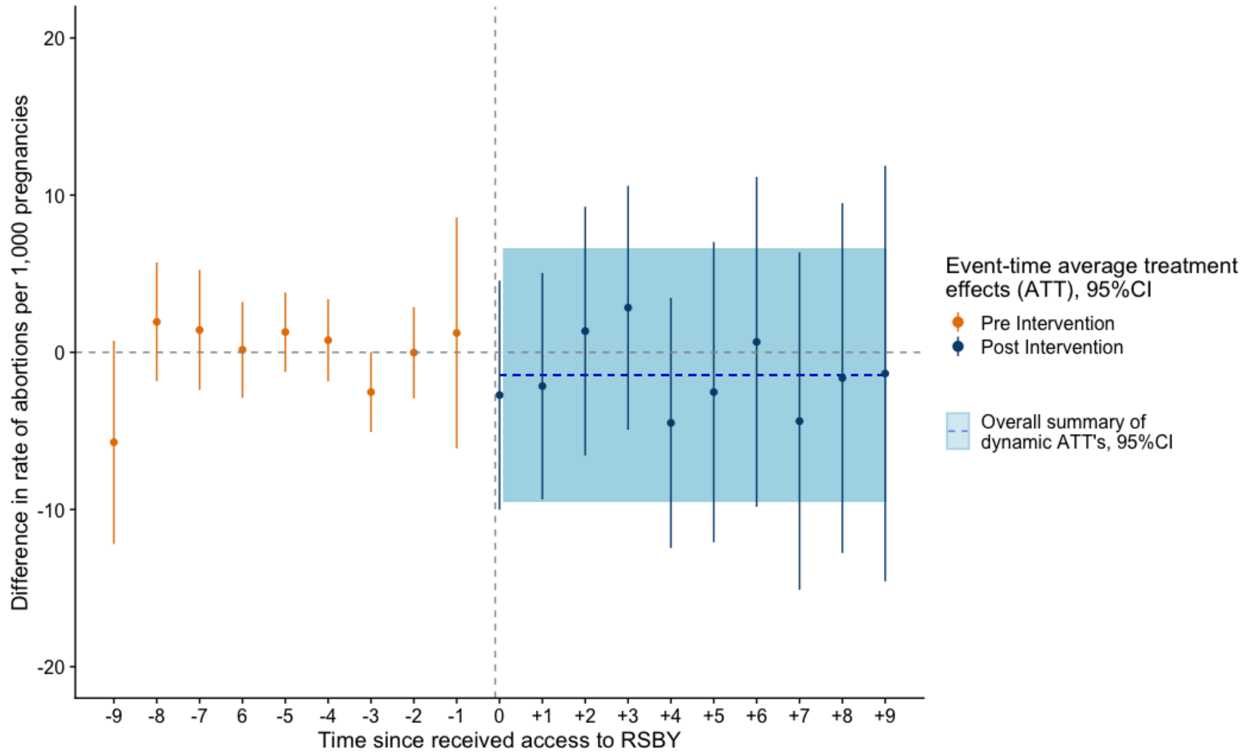


Figure 5.3 Effects of district-level RSBY access on abortions per 1,000 pregnancies, by time since the district gained access to RSBY; India, 2004-20019; risk differences with 95% confidence intervals

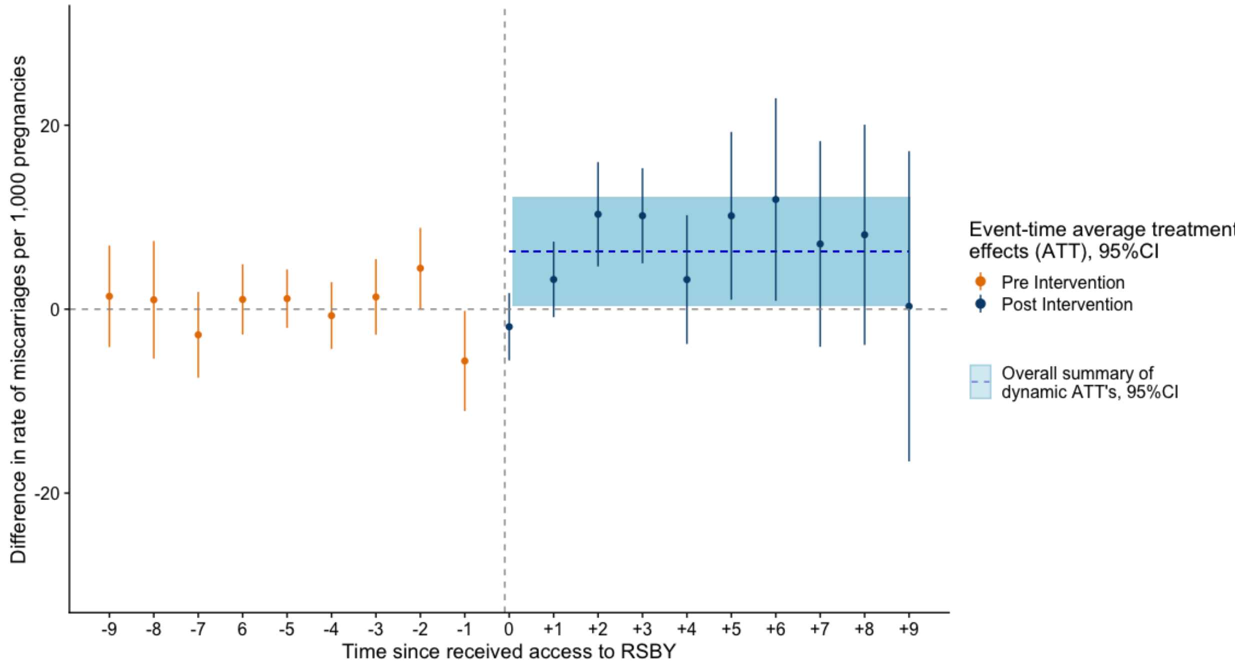


Figure 5.4 Effects of district-level RSBY access on miscarriages per 1,000 pregnancies, by time since the district gained access to RSBY; India, 2004-20019; risk differences with 95% confidence intervals

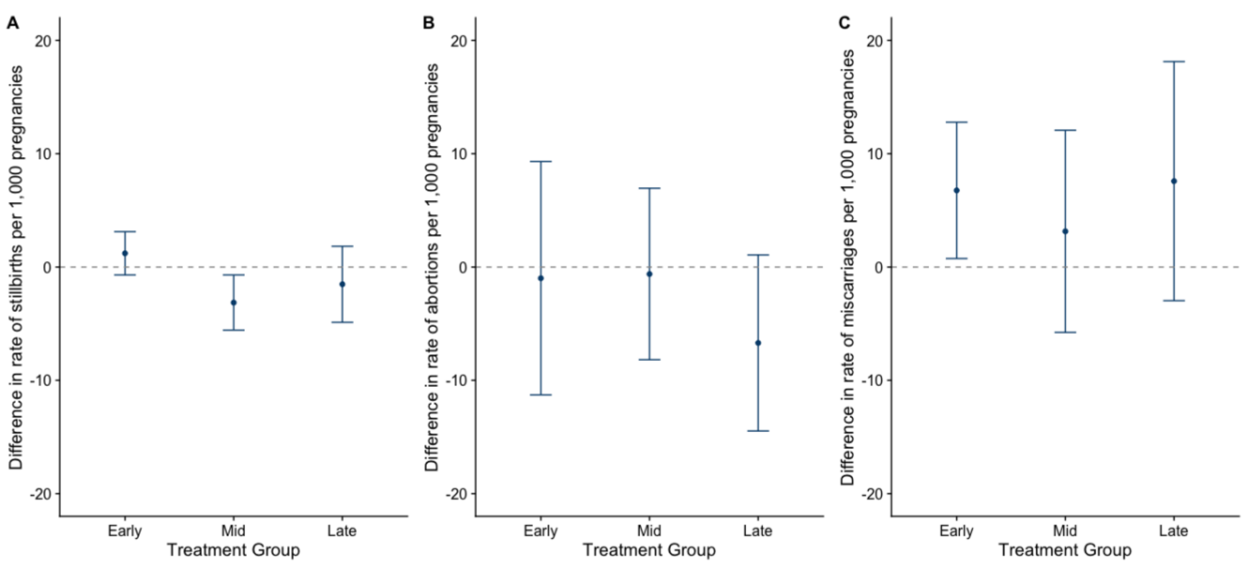


Figure 5.5 Effects of district-level RSBY access on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals

Stratified Results

Results were stratified by household wealth, maternal education attainment, and household location (rural vs. urban) (**Figure 5.6**). We did not observe an overall effect of RSBY on rates of stillbirth or abortion in any of the strata. However, we observed an overall effect of RSBY on miscarriage in each of the strata. Among women with a primary education, access to RSBY was associated with 6.9 (95% CI: 0.4, 13.4) additional miscarriages per 1,000 pregnancies compared to women in control districts. Among women living in rural districts, access to RSBY was associated with 6.3 (95% CI: 0.6, 12.1) additional miscarriages per 1,000 pregnancies. Finally, among women with household wealth below the median, access to RSBY was associated with 12.8 additional miscarriages (95% CI: 5.2, 20.4) per 1,000 pregnancies compared to women in control districts. Group-specific treatment effects for each stratum are shown in the **Supplement**.

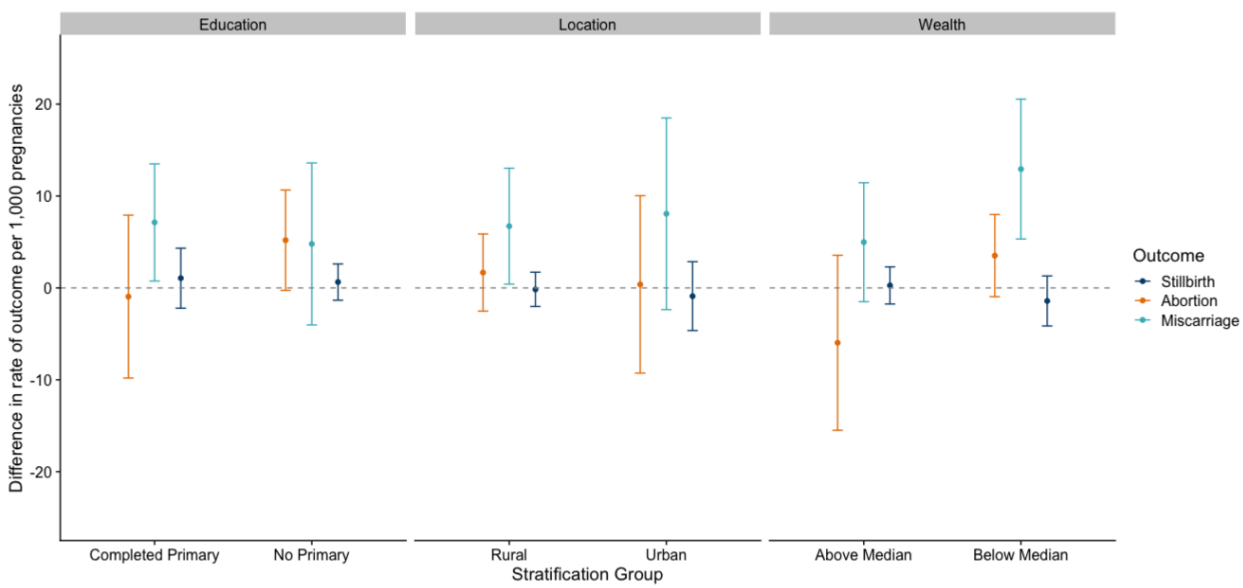


Figure 5.6. Stratified effects of overall impact of RSBY on each outcome by education, location, and wealth; India, 2004-2019; risk differences with 95% confidence intervals

Discussion

This study evaluates the impact of India's National Health Insurance Program on reproductive health outcomes spanning 11 years post-policy implementation. Previous research has not examined the impact of the policy on pregnancy outcomes. This research study leveraged the phased-in implementation of RSBY to estimate the impact of RSBY on levels and socioeconomic inequalities in adverse pregnancy outcomes. We did not observe an overall impact of the policy on rates of stillbirth or abortion. However, district-level access to RSBY was associated with 6.3 additional miscarriages per 1,000 pregnancies compared to control areas. There was some variation across groups defined by when the district implemented RSBY; among women in early-adopter districts, RSBY was associated with higher rates of miscarriage, whereas women in mid-adopter districts had lower rates of stillbirth after implementation when compared to control districts. The impact of access to RSBY also varied by socioeconomic group.

This research is consistent with previous work showing that the introduction of RSBY had little impact on maternal and reproductive health. Research examining maternal healthcare utilization from 2005 – 2012 found that the introduction of RSBY did not affect the utilization of maternal and child healthcare services.[77] This could be related to several factors, including the lack of awareness of the insurance policy. Additionally, recent research shows that men and women in the state of Uttar Pradesh were almost uniformly unaware that RSBY would cover the costs associated with family planning care and reproductive health services, with many seeking this care elsewhere despite being eligible to have the costs covered by RSBY.[76]

Forthcoming research provides estimates of persistent inequalities in these adverse pregnancy outcomes from 2004 to 2019.[163] Enrollment in RSBY may not have decreased subsequent healthcare spending, with evidence showing that RSBY did not reduce household out-of-pocket health expenditures for outpatient healthcare,[9] but in fact increased out-of-pocket costs for both outpatient and inpatient healthcare.[10] These increased costs would make accessing maternal and reproductive healthcare – both inpatient and outpatient – less likely. It is possible that lack of access and high out-of-pocket costs for healthcare contributed to the higher rates of miscarriage observed in districts that received access to RSBY, although we lack information on healthcare spending. It is unlikely that the introduction of RSBY could increase use of quality sexual and reproductive health services and reduce socioeconomic disparities in adverse pregnancy outcomes without affecting healthcare spending.

Research suggests that challenges in the roll-out of the program has limited enrollment at both the district and household level and limited the impact of RSBY on access to and utilization of healthcare, as well as relevant health outcomes. Districts with a higher concentration of poorer families were more likely to offer RSBY, but had lower enrolment ratios as families either did not know about the program or chose not to enroll.[164]Additionally, there were issues empaneling hospitals into the scheme, with some districts having a low number of hospitals that were empaneled with RSBY.[79] Empaneled hospitals were concentrated in urban areas, and some only designated a certain number of beds to be available for RSBY patients.[165] Private empaneled hospitals could also choose what services to cover under RSBY, which led to both decreased access for certain less-profitable services (e.g., treatment for multidrug-resistant tuberculosis), but higher rates of profitable medical procedures like hysterectomies – even if they were not medically necessary.[166] This impacted the ability of enrolled families to access care, and further increased out-of-pocket costs for healthcare.[10]

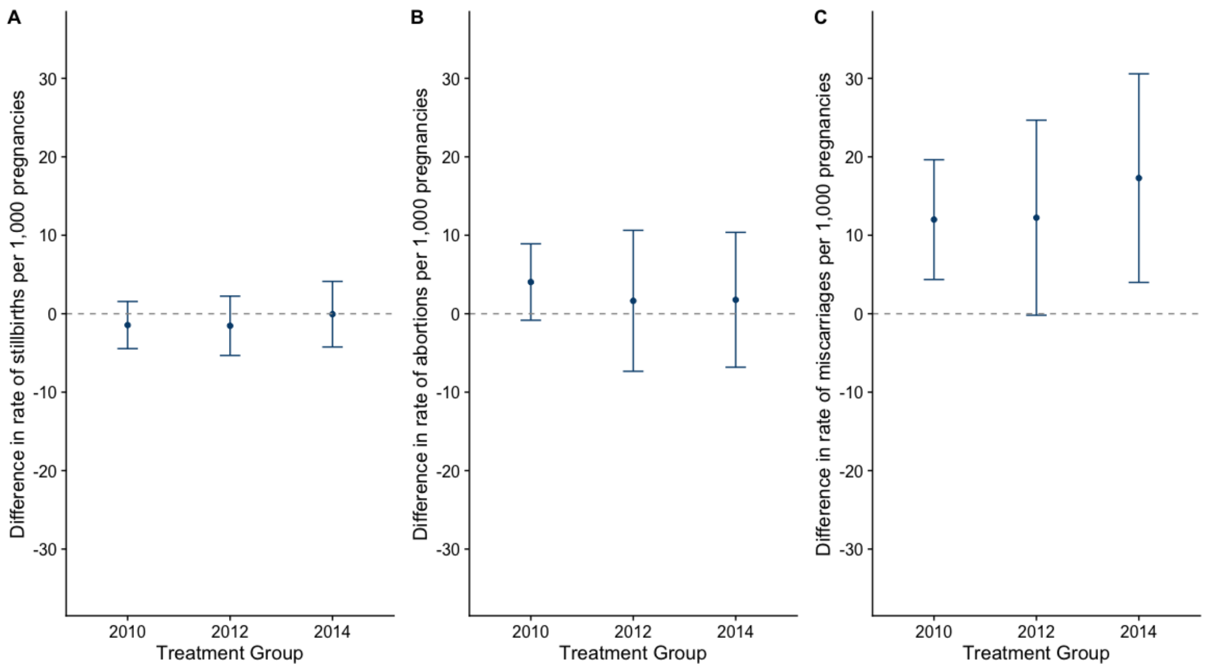
There are several limitations to this research. We did not have district enrollment data before 2010 and therefore could not assign each treated district the precise year when it received access to RSBY. We created treatment group bins instead, to compare early vs. mid vs. late adopting districts based on when they implemented the policy. By keeping these groups within a relatively narrow two-year grouping we aimed to reduce measurement bias while still accounting for the staggered implementation of the policy. It is possible that districts with worse health outcomes were prioritized for access by RSBY, which could introduce potential confounding. However, the DiD design accounts for time-invariant confounders and shared trends across treatment and control districts, which should mitigate potential confounding bias.[104] Additionally, the pregnancy outcomes used in this survey were from self-report and are therefore subject to recall bias. Stillbirth in particular has been found to be susceptible to measurement error in these self-report surveys, as it is often hard to differentiate between stillbirth and early neonatal death after delivery.[136] There is also the potential for measurement error as both treatment and covariates were measured at the time of survey and not the time of pregnancy. We restricted our panel to the most recent pregnancy within a relatively short time period, which should reduce both recall bias and measurement error.[152] Additionally, these surveys have been validated for reproductive health measurement.[150,151] There may have been differential misclassification or underreporting of abortion by socioeconomic status. It is likely that some abortions were self-reported as miscarriages, as abortion has been found to be underreported among Indian women of lower socioeconomic status due to social stigma and bias.[54] This is a common misclassification when measuring abortion,[155] and is a particularly relevant for India, where there is both historical and current gender-biased sex selection by abortion.[156,157] Finally, RSBY did not cover services with a direct impact on reproductive health outcomes such as abortion and miscarriage,[167,168] and was therefore only able to have an indirect impact on these outcomes.

Conclusion

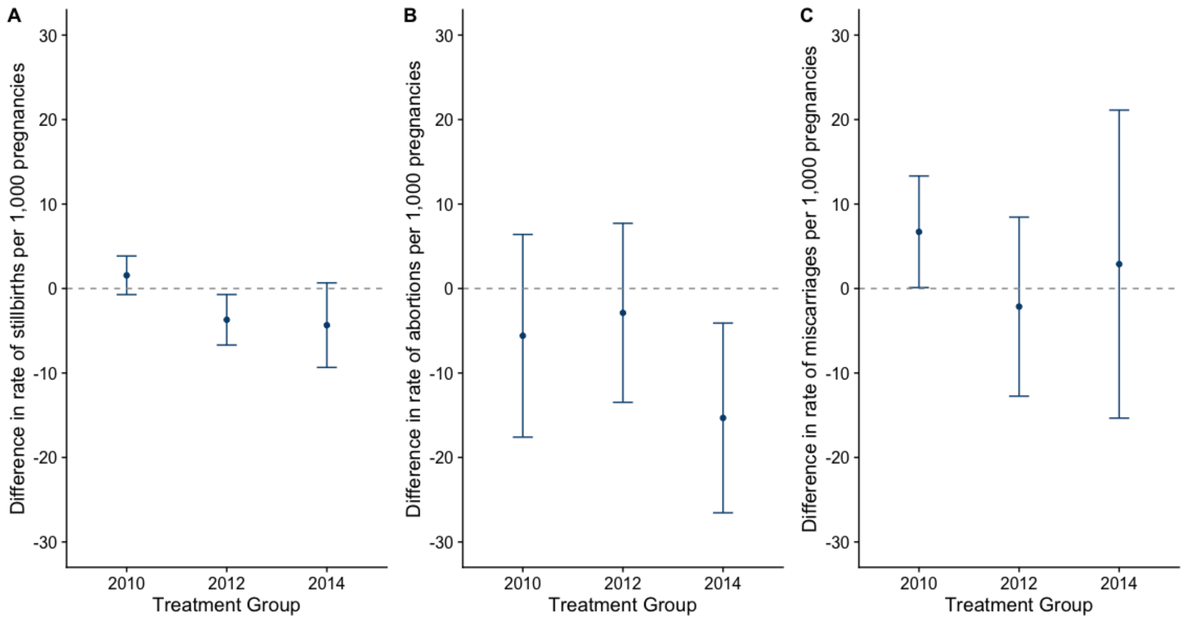
Our study adds evidence to the existing literature on the longer-term impact of RSBY on maternal and reproductive health outcomes using nationally representative household data. Despite targeted investment in the program, we observed no overall improvement in rates of stillbirth or abortion among women in districts that received access to RSBY relative to control districts. Access to RSBY was associated with an increase in rates of miscarriage compared to control districts. Stratifying by socioeconomic group showed variation in the impact of district-level access to the policy on adverse pregnancy outcomes. Recently, the GoI has expanded inpatient health insurance coverage to about a half billion people through a new health insurance program modeled after RSBY, AB-PMJAY. This policy expands insurance coverage to secondary and tertiary-level healthcare services, along with expanding the eligibility pool to encompass more families who may benefit. Further research should continue to investigate the impact of this expanded program on maternal and reproductive health.

5.3 Supplementary appendix: Manuscript 2

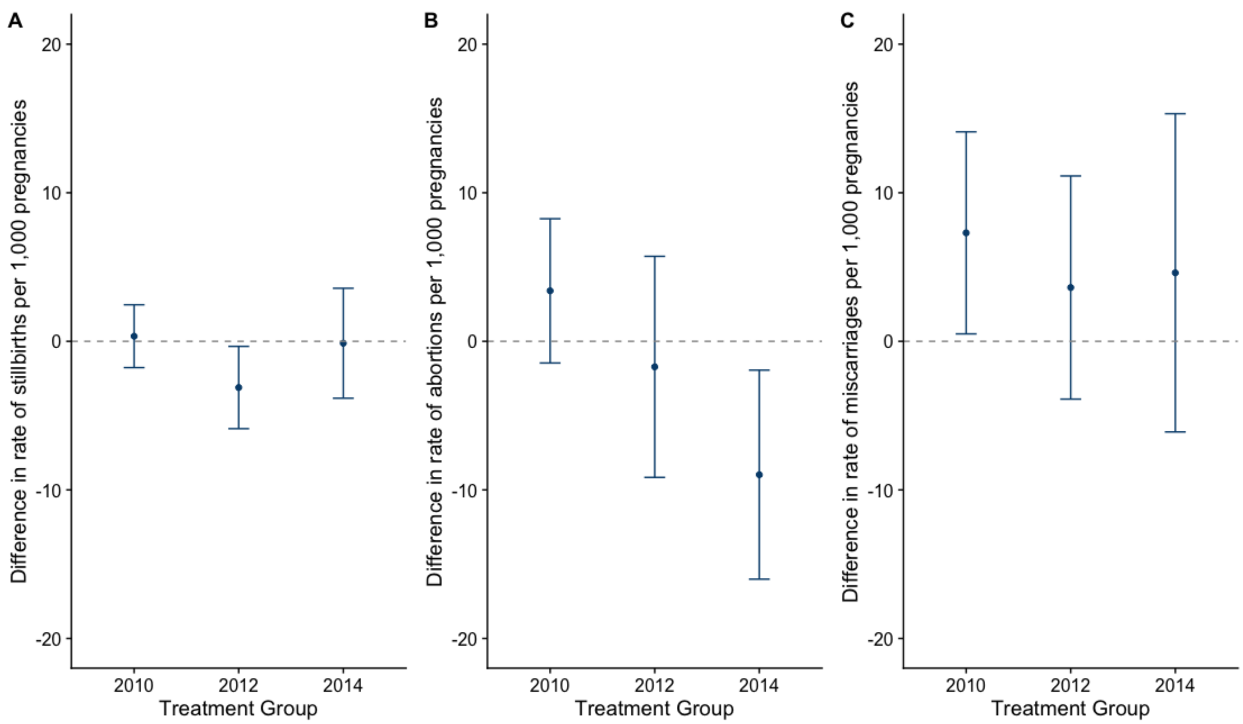
Supplementary figures and tables from analyses



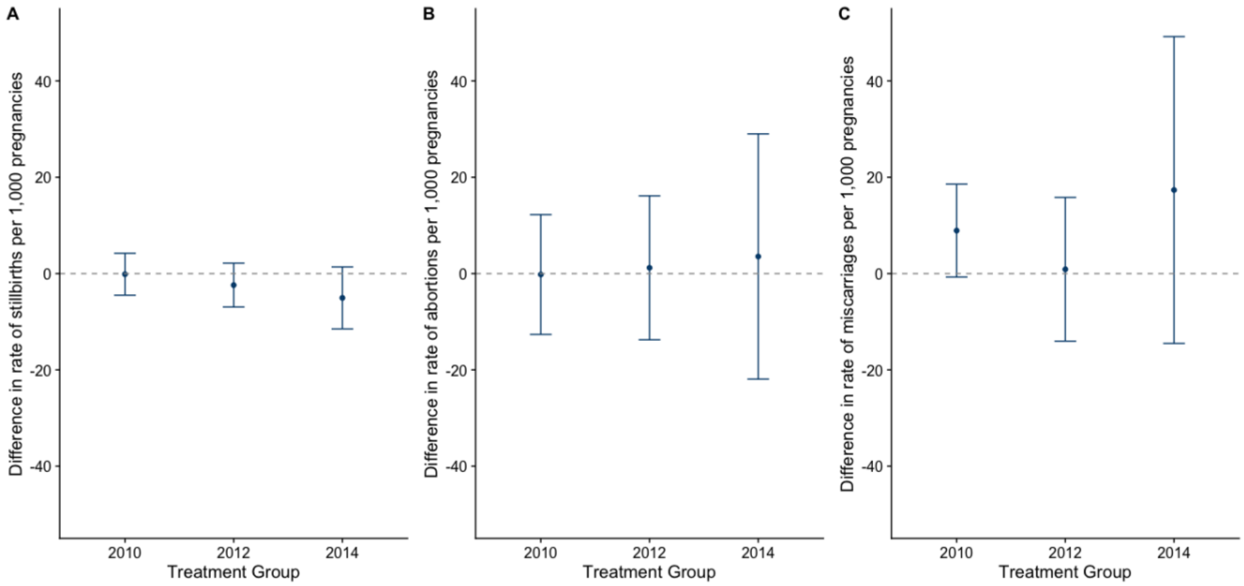
Supplementary Figure 5.1. Effects of district-level RSBY access among women below the median wealth index on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals



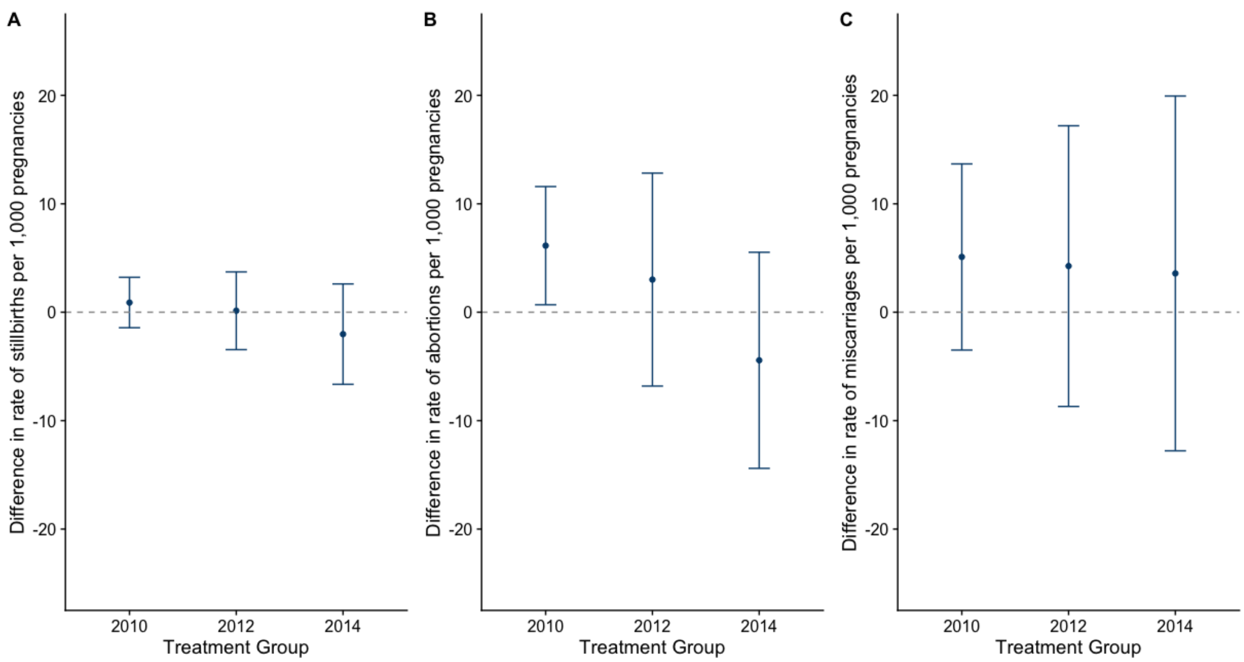
Supplementary Figure 5.2 Effects of district-level RSBY access among women above the median wealth index on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals



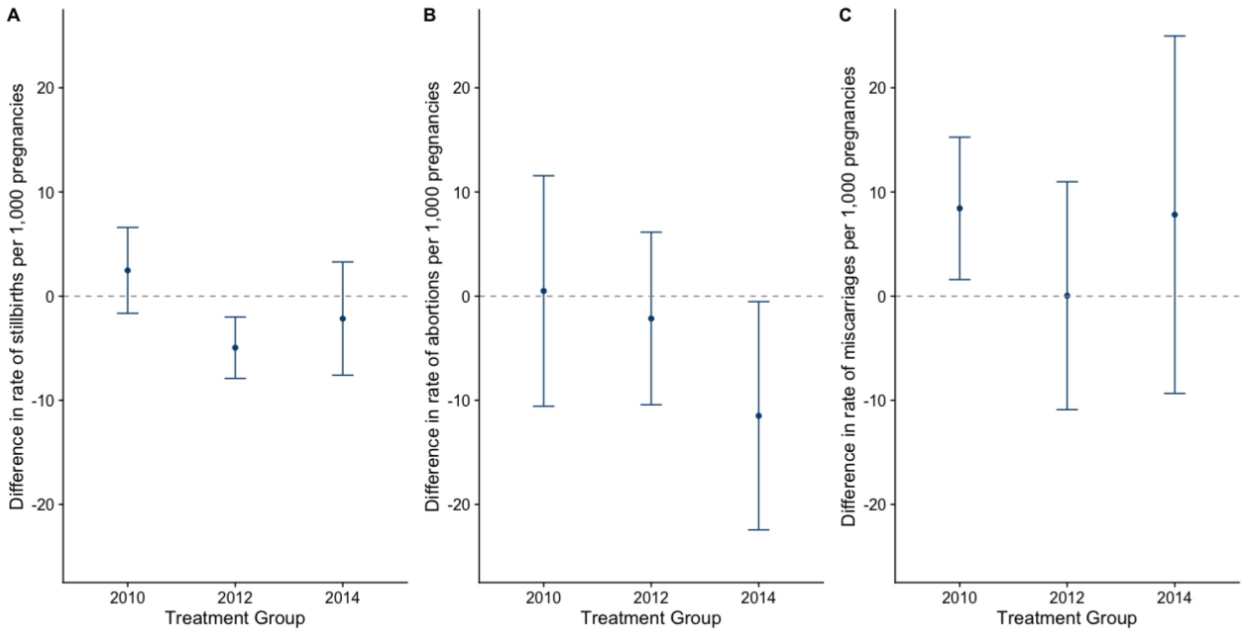
Supplementary Figure 5.3 Effects of district-level RSBY access among rural women on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals



Supplementary Figure 5.4 Effects of district-level RSBY access among urban women on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals



Supplementary Figure 5.5 Effects of district-level RSBY access among women without a primary education on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals



Supplementary Figure 5.6 Effects of district-level RSBY access among women with a primary education on stillbirths (A), abortions (B), and miscarriages (C) per 1,000 pregnancies by treatment group; India, 2004-2019; risk differences with 95% confidence intervals

Callaway & Sant'Anna vs. Wooldridge Estimator

The Callaway Sant'Anna method is explained above in the main text. We also estimated the overall ATTs using the Wooldridge approach. Briefly, we utilized the extended two-way fixed effect regression that has been proposed as a solution to differential effects by treatment groups in the difference-in-differences literature.[106] The two-way Mundlak regression extends the canonical two-way fixed effect model by accounting for interactions between treatment group and time periods.

$$\mathrm{Y_{igt}} = D_{igt} = \alpha_0 + \tau_g + \omega_t + \sum_{\ell=0}^{12} \beta_{early,\ell} R_{igt}^{early,\ell} + \sum_{\ell=0}^{9} \beta_{mid,\ell} R_{igt}^{mid,\ell} + \sum_{\ell=0}^{7} \beta_{late,\ell} R_{igt}^{late,\ell} + \Sigma_k \delta_k X_{igt} + \varepsilon_{igt}. \quad \text{(Eq. 1)}$$

In this Y represents whether the head of household reported household RSBY coverage in year *t* if the treatment began in t_g, τ_g are fixed effects for each group, and ω_t are year fixed effects; the fixed effects for group and year account for unmeasured time-fixed differences across groups and shared secular trends in the outcomes of interest, respectively. We control for the demeaned coefficient of mother's age. We estimated this using the R package etwfe.[169]

Aggregating the coefficients from each interaction to get the marginal effect gives the ATT(g, t) for overall impact of the policy on each outcome. We compared estimators obtained from the method described by Callaway & Sant'Anna (2021) to those in the method described by Wooldridge (2021). We found that the estimates differed by outcome, though all had overlapping confidence intervals.

Supplementary Table 5.1 Callaway & Sant'Anna vs. Wooldridge estimators

	Callaway & Sant'Anna ATT (95% CI)	Wooldridge ATT (95% CI)
Stillbirth	0.12	-0.5
	(-0.7, 3.1)	(-1.7, 0.7)
Abortion	-1.7	-0.6
	(-9.1, 5.7)	(-4.0, 2.9)
Miscarriage	6.3	4.9
	(0.9, 11.7)	(1.0, 8.9)

We present this comparison to show differences in estimates between the two methods. The growing literature in difference-in-differences with heterogeneous treatment timing has proposed various methods to deal with variation by treatment group. We show that these methods produce slightly different estimates of the ATT. Future research should continue to investigate methods to deal with variation between treatment groups.

Chapter 6. Impact of household enrollment in RSBY on adverse pregnancy outcomes: an instrumental variable analysis

6.1 Preface: Manuscript 3

For the last stage of this evaluation, we examined the impact of household-level enrollment in RSBY on adverse pregnancy outcomes. As households that choose to enroll in an insurance policy may differ in many ways (e.g., socioeconomic status, health conditions) from households that do not, it is important to use an analytic approach that accounts for these differences. This study uses instrumental variable analysis to examine the causal effect of household-level enrollment in RSBY on adverse pregnancy outcomes. I extend the updated DiD analyses discussed in **Chapter 5** as part of the instrumental variable analysis.

This work is being prepared for submission for publication.

6.2 Manuscript 3

Impact of household enrollment into RSBY on rates of adverse pregnancy outcomes: an instrumental variable analysis

Caroline M. Joyce[1], Deepti Sharma[2], Arnab Mukherji[2], Arijit Nandi[1,3]

Affiliations:

[1] Department of Epidemiology, Biostatistics, and Occupational Health, School of Population and Global Health, McGill University

[2] Center for Public Policy, Indian Institute of Management Bangalore

[3] Institute for Health and Social Policy, McGill University

Corresponding Author:

Caroline Joyce

2001 McGill College Avenue

Montreal, QC H3A 1G1

Canada

Email: caroline.joyce@mail.mcgill.ca

Abstract

Background: India does not have universal health coverage. Those at or below the poverty line (BPL) pay a disproportionately high percentage of their income on health expenditures. Due in part to inequitable allocation of healthcare resources, the very poor in India have high rates of adverse pregnancy outcomes. In 2008, the Rashtriya Swasthya Bima Yojana (RSBY) health insurance program was launched to cover inpatient healthcare services for BPL households. This research study provides a rigorous evaluation of the causal impact of household enrollment into RSBY on pregnancy outcomes such as stillbirth, abortion, and miscarriage.

Methods: Using two-stage least squares instrumental variable analysis we estimated the causal impact of household enrollment in RSBY on the probability of a pregnancy ending in stillbirth, abortion, or miscarriage. We leveraged the staggered roll-out of RSBY across districts in the first stage of the analyses. We estimated of the overall effect of enrollment in RSBY, along with stratified estimates for different socioeconomic groups.

Results: Enrollment in RSBY was not associated with a decreased probability of adverse pregnancy outcomes associated with maternal mortality such as stillbirth and miscarriage. Instead, analyses adjusted for individual-level covariates that may influence the outcomes of interest showed that enrollment was associated with an increased probability a pregnancy ending in stillbirth and miscarriage, and a decreased probability of abortion.

Conclusion: We found that access to RSBY did not reduce the prevalence of adverse pregnancy outcomes. Difficulties in district-level roll-out and household enrollment may explain the lack of a beneficial effect of the policy on pregnancy outcomes.

Keywords: Maternal health; stillbirth; abortion; miscarriage; policy analysis; household insurance coverage; instrumental variable analysis; causal inference

Introduction

India lacks a system that provides universal health coverage. Health costs in India are high, with those at or below the poverty line paying a disproportionate percentage of their income on healthcare expenditures.[67,68] Healthcare resources are often concentrated in wealthier, urban, areas, which creates inequalities in access to and use of health care services among poorer households.[41] Due in part to these disparities in cost and access, the eight most socioeconomically disadvantaged states in India contribute disproportionately high counts of maternal and infant mortality relative to their population size.[42] Adverse pregnancy outcomes, including stillbirth and miscarriage, contribute substantially to rates of maternal and neonatal mortality.[170,171]

Key to preventing stillbirth and miscarriage is access to quality maternal and reproductive healthcare. Risk factors for stillbirth and miscarriage include inability to access modern contraceptives and safe abortion services, lack of antenatal care, and absence of a skilled birth attendant at delivery.[4,25,29] These risk factors are more prevalent among poorer women and women without a primary school education,[163] who may have limited access to essential sexual and reproductive health services due to financial barriers and lack of health insurance.[51] Due to these existing disparities, state and national governments have attempted to improve access to and costs of healthcare through various public policy initiatives.

Health insurance in India is provided through a combination of private (i.e., from employment) and public (i.e., state and central government sponsored) programs. With a large proportion of workers employed in the informal economy, many are ineligible for private health insurance.[172] To close this gap, in the late 1990s some state governments began to offer health insurance to households below the poverty line (BPL) for secondary and tertiary healthcare.[79]

However, coverage was uneven across India, with some states offering robust health insurance programs to BPL households, and others offering none. Accordingly, there was a recognition of the need for a federally sponsored health insurance program to protect BPL households across India from catastrophic health expenditures.[69] The Government of India (GoI) subsequently introduced the Rashtriya Swasthya Bima Yojana (RSBY) program to provide coverage for hospital-based healthcare for BPL households.

Launched in 2008, the goal of RSBY was to increase access to inpatient hospital services for BPL households.[70] RSBY was funded and supported through a central and state government partnership. State governments were responsible for implementation and contracted with insurance companies to facilitate access to services. Hospitals deemed eligible by the state entered contracts with the insurance companies to provide coverage and insurance companies were responsible for enrolling eligible BPL households in the RSBY program. Inpatient hospital services specified by the central government were covered under the policy; this included services essential for lowering maternal and neonatal morbidity and mortality (e.g., in-hospital childbirth and caesarean sections). Eligible households were responsible for a low enrollment fee and were then covered for up to INR 30,000 worth of services annually (roughly USD $500 in 2008). It was hypothesized that the money saved by reducing out-of-pocket healthcare expenditures could then be used to pay for health services that would not be covered under RSBY, such as contraception and the medication used for an abortion. Enrollment into the RSBY program could therefore have both direct and indirect effects on reproductive health.

Previous studies on the effectiveness of RSBY have produced mixed results. The primary challenge to estimating the impact of enrollment in RSBY is unmeasured confounding, as households that are eligible for and choose to enroll in RSBY may differ from households that do not in ways related to their health status (i.e., by socioeconomic status, existing health issues, etc.). The non-random selection of households into the program makes it difficult to recover causal estimates of the impact of voluntary health insurance coverage, which is reflected in the literature on RSBY. Among studies that have attempted to estimate the causal impact of enrollment into the RSBY program, most have focused on out-of-pocket healthcare expenses; recent work shows that expenses did not significantly decrease, with some research indicating that out-of-pocket costs increased for RSBY households.[9,10,173] Other research has investigated the uptake of the program and enrollment of BPL households by state.[73-75] Few studies have examined the effect of RSBY on maternal and reproductive health outcomes, including use of health services. One study found that women enrolled in the RSBY program did not use it for sexual and reproductive health services, citing that they were not aware that these services were covered.[76]

The unique central and state government partnership to implement RSBY resulted in variation in the timing when various districts within states gained access to the program. The central government instructed states to first offer access to RSBY to about 20% of administrative districts in 2008, with up to an additional 20% of districts given access each subsequent year.[71] However, states decided which districts received access first, and what proportion of districts gained access the following years. Some states initially opted to offer the insurance program, but either delayed in starting enrollment until past 2015 (e.g., Rajasthan) or stopped enrolling after the first few districts (e.g., Karnataka and Tamil Nadu).[10] Additionally some states made an active effort to offer enroll eligible families, while others were more passive and, compared to other states, had lower enrollment rates even after the policy was available.[174] Despite these challenges, the number of districts with access to RSBY increased from 0 in 2007 to 487 (out of a total of 707) in 2016. These differences across districts in the roll-out of the program provided "as-if-random" variation in the timing of when districts gained access to RSBY. This presents a unique opportunity for using an instrumental variable (IV) approach for estimating the impact of household-level RSBY enrollment, which is posited to increase when a district receives access to the program, on health outcomes.

Research on the impact of RSBY on pregnancy outcomes is lacking. Rigorous evaluation of the causal impact of RSBY enrollment on pregnancy outcomes, including stillbirth, abortion, and miscarriage, is needed to provide a more comprehensive understanding of the effectiveness of the program. In this study, we leveraged the staggered introduction of RSBY across districts to estimate the causal effect of household enrollment on adverse pregnancy outcomes. Estimating the causal effect of RSBY is particularly important as the GoI expands health insurance access through a new health insurance program modeled after RSBY, Ayushman Bharat-Pradhan Mantri Jan Arogya Yojana (AB-PMJAY).

Methods

Study Design

IV analysis is often used to address the problem of unmeasured confounding of the effect of an endogenous treatment.[175] Analogous to randomizing a treatment in a randomized controlled trial (RCT), IV analysis exploits the as-if random variation in treatment assignment generated by the instrument (i.e., district-level RSBY access) to estimate the causal effect of the treatment (i.e., household-level RSBY enrollment) on outcomes of interest. As the instrument is not experimentally manipulated, there are additional assumptions necessary for the IV design to yield unbiased causal effect estimates. We assume that district-level access to RSBY is not correlated with other factors that would influence pregnancy outcomes[109,110] (the "exchangeability" condition), and that district-level access to RSBY only affects pregnancy outcomes through the treatment, household enrollment in RSBY, which is known as the "exclusion restriction" condition. Additionally, for an instrument to be valid it must meet the criteria of "relevance", specifically that district-level access to RSBY is associated with an increase in household-level enrollment in the policy. A graphical description of these assumptions is shown in **Figure 6.1**.

Data Sample

Data were derived from household surveys administered by the Ministry of Health and Family Welfare (MoHFW) that were designed to be district-representative: (1) District Level Household Surveys (DLHS) rounds 3 and 4; (2) National Family Health Survey (NFHS) rounds 4 and 5; and (3) the Annual Health Survey (AHS) of India. By merging information from these five household surveys, we captured pregnancy information among samples of ever-married women aged 15-49 between 2004 to 2019 in nearly every state and union territory in India. Researchers have previously combined these data for both cross-sectional and longitudinal analyses.[126-131] The sampling design of each survey is further detailed in **Chapter 3.** From the reproductive histories captured in each survey, we constructed a panel of the 2,005,290 most recent pregnancies reported by each woman in each district and year during the study period. Observations from households missing the health insurance variable used for measuring the treatment were excluded, giving a final sample of 1,775,513 most recent pregnancies. Further information, including a flowchart illustrating the selection of the sample, is provided in **Chapter 3**.

Measures

The primary exposure or treatment was self-reported household enrollment in RSBY. The head of the household was asked if the household held any type of health insurance. Among those who responded affirmatively, the head of the household was then asked to select the type of health insurance held from a set list of health insurance options in India (i.e., Employees' State Insurance Scheme, Central Government Health Scheme, RSBY). Households that reported no health insurance or health insurance of a different kind were classified as not enrolled in RSBY.

The instrumental variable was district-level access to RSBY. To measure district-level exposure to RSBY we assigned districts the year when the district first gained access to the program, which we accessed from the GoI RSBY website via the Internet Archive.[71] We limited our sample to districts that were represented in each year of the study which reduced the number of districts from 555 to 463. The first possible year districts could enroll in RSBY was 2008, with district-level roll-out completed by 2014. However, the GoI RSBY enrollment data started in 2010, with all household enrollment completed before this year included in the 2010 enrollment numbers. To account for this, we assigned districts to four groups: 1) early adopters enrolled between 2008 – 2010; 2) mid-adopters enrolled in 2011 – 2012; 3) late-adopters enrolled 2013 – 2014; and 4) districts that never adopted RSBY. Hereafter we refer to these groups as early, mid, late, and never adopters, respectively. We assigned the value for the district-level instrument to each surveyed women based on whether the district they were surveyed in had access during the year the pregnancy occurred.

Our primary outcomes of interest were self-reported stillbirth, abortion, and miscarriage. The contraceptive calendar from the NFHS surveys was used to identify all self-reported pregnancies from the past five years. For respondents from the DLHS and AHS surveys, all pregnancies and their outcomes from the last three years were self-reported. All respondents self-reported which non-live birth outcome their pregnancy ended as and the month of pregnancy it occurred during. For consistency, we defined miscarriages as spontaneous abortions before the seventh month (28 weeks) of pregnancy. Stillbirths were defined as spontaneous pregnancy losses occurring during or after the seventh month (28 weeks) of pregnancy.[135,136] All self-reported abortions were coded as an induced abortion. From this we defined three binary outcome variables for each participant's most recent pregnancy: 1) stillbirth; 2) induced abortion; or 3) miscarriage.

In each survey, the head of the household provided information on household and individual-level socioeconomic characteristics. A continuous wealth index was created from information gathered on reported household assets.[137] Additional details on the creation of the wealth index are included in **Chapter 3**. We also measured whether the household was coded as being in an urban or rural location, the age of the mother, the mother's educational attainment, and whether the household belonged to a Scheduled Caste or Scheduled Tribe.

Statistical Analyses

We measured the effect of household-level RSBY coverage using the two-stage least squares (2SLS) approach (**Figure 6.1**). In the first stage, we estimated the impact of the instrument, district-level access to RSBY, on the treatment, household-level RSBY coverage. Relevance of the instrument was assessed using the F-statistic.[113]

To account for the staggered roll-out of the RSBY across districts, we utilized the extended two-way fixed effects regression approach (ETWFE); this approach accommodates potential heterogeneity in treatment effects across groups and time relative to treatment by augmenting the "static" two-way fixed effects model with group and time interactions.[106] In the first-stage equation below, there are $G = 3$ treatment groups, defined by the initial year of RSBY access, t_g, (i.e., 2010 for the early group that implemented in 2008-2010, 2012 for the mid group that implemented in 2011-2012, and 2014 for the late group that implemented in 2013-2014):

$D_{igt} = D_{igt} = \alpha_0 + \tau_g + \omega_t + \sum_{\ell=0}^{10} \beta_{early,\ell} R_{igt}^{early,\ell} + \sum_{\ell=0}^{8} \beta_{mid,\ell} R_{igt}^{mid,\ell} + \sum_{\ell=0}^{6} \beta_{late,\ell} R_{igt}^{late,\ell} + \Sigma_k \delta_k X_{igt} + \varepsilon_{igt}$.**(Eq. 1)**

In **Eq.1**, the endogenous treatment in **Figure 6.1**, D_{igt}, represents whether the head of household reported household RSBY coverage in year *t* if the treatment began in t_g, τ_g are fixed effects for each group, and ω_t are year fixed effects; the fixed effects for group and year account for unmeasured time-fixed differences across groups and shared secular trends in the outcomes of interest, respectively. We adjusted for covariates, X_{igt}, that may have affected household enrollment in RSBY when granted access in the district, specifically the household wealth index and whether the household belonged to a Scheduled Caste or Scheduled Tribe.[164] For each of the treatment groups, we estimated the treatment effects, represented by β, for each event time ℓ after the group received access to RSBY, represented by the dummy variables, $R_{it}^{g,l}$. For the early group that received access starting in 2010, for example, there are 10 possible event times from 2010 ($\ell = 0$) to 2019 ($\ell = 9$). We estimated this using the R package etwfe.[169]

The coefficients from **Eq. 1** were used to predict, for each observation, the probability $\widehat{D}_{igt}$ of household-level RSBY coverage, which was added as a predictor in the second-stage equation:[176]

$$Y_{igt} = \lambda_0 + \gamma_1 \widehat{D}_{igt} + \tau_d + \omega_t + \Sigma_k \delta_k X_{igt} + \varepsilon_{igt} \qquad \textbf{(Eq. 2)},$$

where Y_{igt} is the outcome of interest. As in **Eq. 1** we included fixed effects for treatment group (τ_d) and year (ω_t). We compared the base model to a model adjusted for additional individual covariates that may influence the outcomes of interest, X_{igt} , but do not influence district-level access to RSBY. These included age of the mother and urban vs. rural residence.[177] As survey-specific sampling weights are relative weights and do not provide valid estimates when surveys are pooled, the weights from each survey were de-normalized, more information on this is in **Chapter 3**.[91] Robust standard errors were clustered at the district level and the 2SLS estimation was bootstrapped with 10,000 replications to yield complier average causal effects (CACE) for the impact of household enrollment in RSBY on the risk difference scale with corresponding 95% confidence intervals.[109,178]

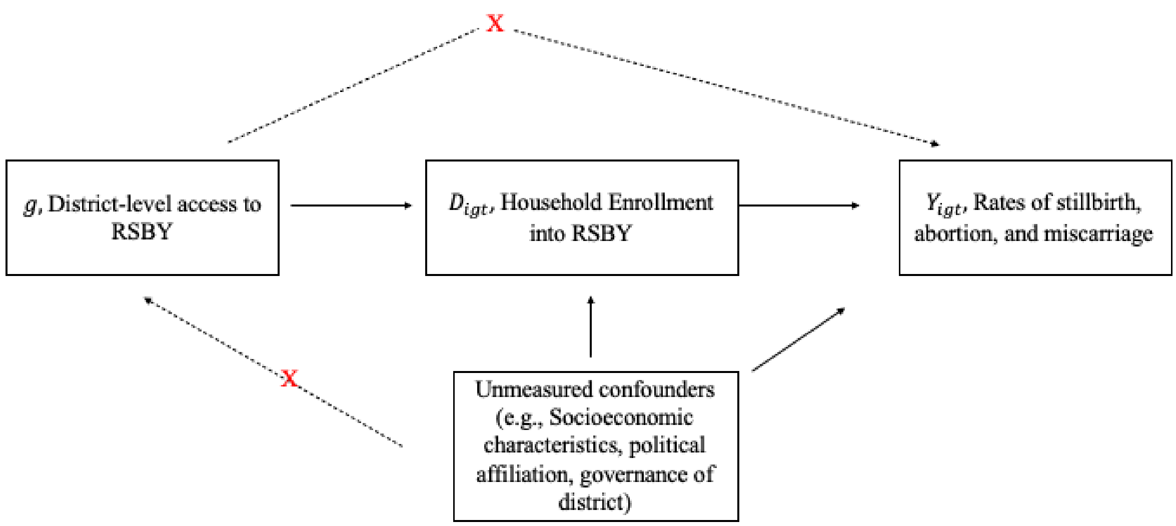


Figure 6.1 Graphical representation of the instrumental variable analysis. In the first-stage, district-level access to RSBY for each treatment group (g) is posited to increase household enrollment (D_{igt}) in RSBY ("relevance"). The exclusion restriction condition is shown through the blocked path between the instrument and our outcomes (Y_{igt}), which shows that district-level access can only influence the outcomes through household enrollment. The exchangeability condition assumes that the effect of the instrument on each outcome is unconfounded, as indicated by the blocked path between unmeasured confounders and the instrument.

Sensitivity Analyses

To account for possible bias stemming from having binary outcomes,[176] we re-ran the second-step of the 2SLS replacing the predicted values of household-level RSBY coverage with the residuals from the first step, $\hat{r}_{igt} = D_{igt} - \widehat{D}_{igt}$, along with an indicator for treatment status.[179] This method has been shown to be less biased than traditional 2SLS for binary outcomes.[180]

The sample included 6,730 pregnancies from households that reported enrollment in RSBY but resided in districts that administrative records indicated as not receiving access to the program. This may reflect error in the administrative data or in the self-reported enrollment information. It is also possible this relates to changes in the numbers and boundaries of from states and districts over time, as India created additional states and union territories during the study period. For example, the state and district that a household lived in in 2004 might be different from the state and district they resided in 2014, even if the household stayed in the exact same location. To ensure that geographic units were compared consistently across each year, observations from new states and territories were recoded to match the unit they were a part of as of the 2001 census. The same procedure was followed for newly created districts during the study period. It is possible that the district the respondent was living in at time of the survey had received access to RSBY, but the re-coded 2001 census-matched district did not. To account for this, the analyses were re-run excluding these participants. Further information on district harmonization is provided in **Chapter 3**.

Finally, while the district-level enrollment data was limited to 2010 onwards, enrollment itself began in 2008. In the main analyses, we therefore group districts by the year the available data shows access being offered. I.e. Early-adopting districts had all begun offering access by

2010, mid-adopting districts by 2012, and late-adopting by 2014. By grouping by the later year access was being offered, there is misclassification of treated districts being counted as un-treated. We therefore ran sensitivity analyses to account for the opposite case: grouping districts by the year RSBY could first possibly be offered. This swaps the misclassification – with districts that are not yet treated counted as treated. In this scenario, early adopters are districts that could begin receiving access in 2008, mid-adopters in 2011, and late-adopters in 2013. We report the results of these analyses in the **Supplement**.

Results

Descriptive Results

Respondent characteristics are shown in **Table 6.1**. The sample included the 1,782,243 most recent pregnancies from women in 463 districts across 24 states. The mean age of respondents was 27 years, with the majority (78%) residing in rural areas. 22% of women reported belonging to a Scheduled Caste and 9% to a Scheduled Tribe. Nearly one-half of respondents completed a primary education. 51.9% of respondents lived in early-adopter districts, with 39.8% living in districts that never received access.

We examined differences in sociodemographic characteristics by treatment group. There was a smaller proportion of women belonging to a Scheduled Tribe in the early adopter districts (5% compared to 9% overall), but a higher proportion belonging to the lowest wealth quintile (28% vs. 23% overall). Compared to early- and late-adopting districts, mid-adopter districts had a higher proportion of women who identified as members of a Scheduled Caste or Scheduled Tribe, a higher proportion of women who had completed primary school, and a lower proportion of women in the lowest wealth quintile. Women residing in early-adopter districts had the highest rates of stillbirth at 1% of live births, while mid-adopter districts had the highest proportion of abortions (2.8%), and late-adopter districts with the highest proportion of miscarriages (5.9%) of all live births.

Table 6.1 Socio-demographic information on respondents

		District-level Access			
Characteristic	**Overall** (N = 1665249)[1]	**2010 - Early** (N = 906996)[1]	**2012 - Mid** (N = 102938)[1]	**2014 - Late** (N = 31234)[1]	**None** (N = 624081)[1]
Age	27 (6)	28 (6)	26 (5)	27 (5)	27 (6)
Rural / Urban					
Urban	311,182 (22%)	158,923 (20%)	25,192 (31%)	4,974 (25%)	122,093 (22%)
Rural	1,354,067 (78%)	748,073 (80%)	77,746 (69%)	26,260 (75%)	501,988 (78%)
Member of Scheduled Caste or Scheduled Tribe					
None	1,107,742 (70%)	622,619 (72%)	59,594 (62%)	17,886 (65%)	407,643 (67%)
Scheduled Caste	319,026 (22%)	189,542 (23%)	20,657 (24%)	5,641 (21%)	103,186 (19%)
Scheduled Tribe	238,481 (8.6%)	94,835 (4.9%)	22,687 (15%)	7,707 (14%)	113,252 (14%)
Completed Primary School	828,837 (47%)	445,496 (46%)	63,614 (57%)	14,480 (47%)	305,247 (46%)
Lowest wealth quintile	339,383 (23%)	239,396 (28%)	11,465 (12%)	5,662 (17%)	82,860 (16%)
Stillbirths	12,447 (0.8%)	7,965 (1.0%)	782 (0.7%)	137 (0.4%)	3,563 (0.6%)
Abortions	30,946 (2.0%)	17,968 (2.3%)	3,279 (2.8%)	830 (2.5%)	8,869 (1.4%)
Miscarriages	54,028 (3.5%)	34,048 (4.1%)	4,442 (3.9%)	1,810 (5.9%)	13,728 (2.3%)

[1] Mean (SD); n (%)

Respondent-reported RSBY enrollment by each year and district-level access group is shown in **Figure 6.2**. The highest proportion of enrollment was observed in early-adopter districts between 2008 and 2011, with 16.5% of households enrolled at the peak. Households in late-adopter districts reported the lowest levels of enrollment, with a maximum of 3.4% of households enrolled in 2019. However, while the early- and mid-adopter enrollment groups had an initial peak and then drop in enrollment, the late-adopter districts had lower but steadier enrollment in each year.

As noted above, we did observe low levels of enrollment in districts that never received access to the policy, which is likely due to measurement error in assigning district-level access due to changing definitions of districts and boundaries during the study period.

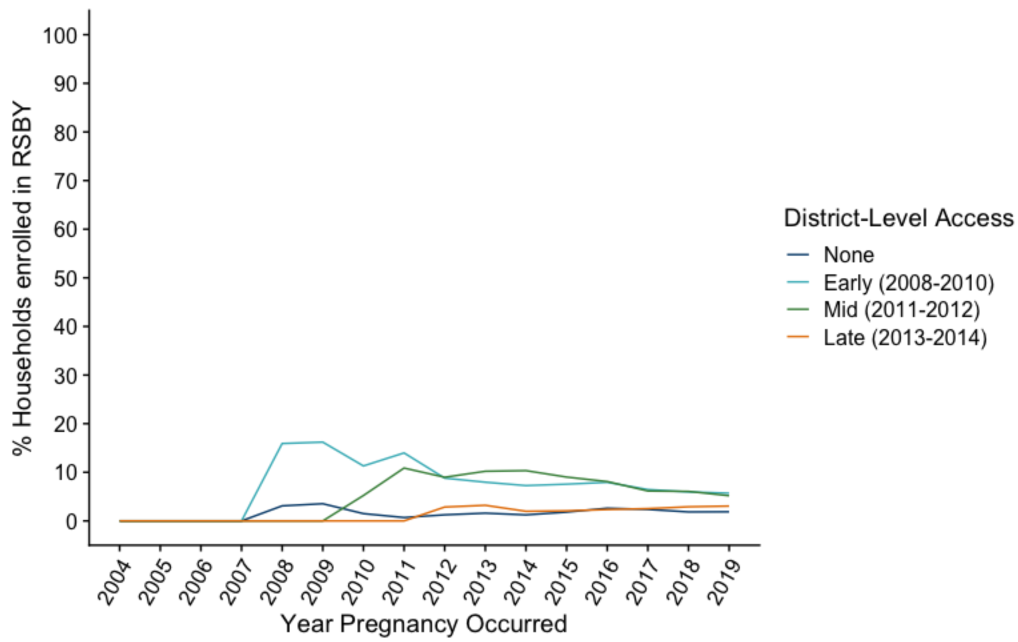


Figure 6.2 Proportion of households reporting enrollment in RSBY by year of pregnancy outcome and district-level access group.

Instrumental variable estimates for the impact of enrollment in RSBY on adverse pregnancy outcomes

Results from the first-stage of the 2SLS are in **Supplementary Table 6.1**.The F-statistic from the first-stage of the 2SLS was 40.1, suggesting that the instrument had a sufficiently strong effect on the treatment, enrollment in RSBY.[181] **Table 6.2** gives the CACE for the impact of RSBY enrollment on each pregnancy outcome for both unadjusted and adjusted models on the risk difference scale, interpreted as percentage-point differences. The unadjusted IV estimate of the impact of RSBY on stillbirth shows that enrollment in RSBY was associated with a 2.2 percentage-point increase in the risk of stillbirth (95% CI: 1.5, 2.9) across the study period.

When adjusting for urban residence and the age of the respondent, this decreased to a 1.8 percentage-point increase (95 % CI: 1.1, 2.5). Enrollment in RSBY in the unadjusted model was associated with a -4.9 percentage-point (95% CI: -7.0, -2.9) decrease in the prevalence of abortion, which decreased to -2.1 percentage-points (95% CI: -4.0, -0.2) in the adjusted model. Finally, enrollment in RSBY in the unadjusted model was associated with a -0.5 percentage-point decrease (95% CI: -2.6, 1.5) in the probability of miscarriage, which increased to a 2.0 percentage-point increase (95% CI: 0.0, 3.9) after adjusting for covariates.

Table 6.2. Unadjusted and adjusted effects of household-enrollment in RSBY on adverse pregnancy outcomes, 2004 – 2019; estimates on the risk difference scale interpreted as percentage-point differences with corresponding 95% confidence intervals.

Outcome	**Unadjusted model** *CACE* *(95% CI)*	**Adjusted for rural/urban + mother's age** *CACE* *(95% CI)*
Stillbirth	2.2 (1.5, 2.9)	1.8 (1.1, 2.5)
Abortion	-4.9 (-7.0, -2.9)	-2.1 (-4.0, -0.2)
Miscarriage	-0.5 (-2.6, 1.5)	2.0 (0.0, 3.9)

Sensitivity Analyses

Results from the sensitivity analyses for two-stage estimation with a binary outcome are shown in **Table 6.3**. Estimates were similar to the standard approach of using predicted values of the treatment, although standard errors differed slightly for analyses of abortion and miscarriage.

Estimates from the sensitivity analyses excluding enrolled participants in not-treated districts are shown in **Supplementary Table 6.2.** The only qualitative difference was for miscarriage. In the adjusted analyses, enrollment in RSBY was associated with a 2.0 percentage-point increase in the prevalence of miscarriage (95% CI: 0.0, 3.9). However, in the sensitivity analyses, enrollment in RSBY was associated with a -0.3 percentage-point decrease (95% CI: -2.1, 1.5) in the prevalence of miscarriage.

Results from the first-stage of the 2SLS sensitivity analyses adjusting the first year of district-level exposure to RSBY are seen in **Supplementary Table 6.3**. Estimates from the second-stage are seen in **Supplementary Table 6.4**. In these sensitivity analyses, the only observed difference from the main analyses was for the outcome of miscarriage. Enrollment in RSBY associated with a -2.9 percentage-point decrease (95% CI -5.4, 0.1) in the prevalence of miscarriage in the unadjusted sensitivity analyses, and a -0.3 percentage-point (95% CI -2.4, 1.8) decrease in the adjusted sensitivity analyses.

Table 6.3. Unadjusted and adjusted effects of household-enrollment in RSBY on adverse pregnancy outcomes, 2004 – 2019, from sensitivity analyses for alternative two-stage estimation for binary outcomes; estimates on the risk difference scale interpreted as percentage-point differences with corresponding 95% confidence intervals.

Outcome	**Substituting residuals adjusted for rural/urban + mother's age** *CACE (95% CI)*
Stillbirth	1.8 (1.1, 2.5)
Abortion	-2.2 (-4.1, -0.3)
Miscarriage	1.9 (-0.1, 3.9)

Discussion

We examined the impact of household-level enrollment in India's National Health Insurance Program on pregnancy outcomes spanning 11 years post-policy implementation. To account for unmeasured confounding we used an instrumental variable design that leveraged the district-level roll-out of RSBY as the instrument that generated variation in household-level enrollment in the program. Enrollment in RSBY was low throughout the study period, with the largest increases in enrollment observed in early-adopter districts in the first years of the roll-out. Levels of enrollment were lower in mid and late-adopter districts. Enrollment in RSBY was not associated with a decreased probability of adverse pregnancy outcomes associated with maternal mortality such as stillbirth and miscarriage. Instead, analyses adjusted for individual-level covariates that could influence the outcomes of interest showed that enrollment was associated with an increased probability a pregnancy ending in stillbirth and miscarriage, and a decreased probability of abortion. Multiple sensitivity analyses conducted to account for possible misclassification of household access to RSBY did not appreciably affect results for stillbirth or abortion, however, estimates on miscarriage were not as robust.

There are several policy characteristics to consider that may have affected the uptake of RSBY, and which may have influenced patterns of enrollment across districts. Although RSBY is a national initiative, with broad eligibility set by the Ministry of Labour and Employment, each state was responsible for enrolling eligible households in each district. National guidelines stated that each state had the discretion to decide if and when RSBY would be offered in each district.[10] Some states offered state health insurance program that provided more generous benefits than those offered by RSBY. Andhra Pradesh, for example, offered a generous state insurance policy and RSBY enrollment in the state was low.[69] Eligibility for RSBY also varied by state, as each state sets its own BPL threshold. Due to this, different states had different thresholds for BPL status than others, with some states giving access to RSBY to a higher proportion of residents than others did.[172] The survey data we used in our evaluation did not measure BPL status, and therefore we could not determine the number of eligible but unenrolled households in our sample.

The low enrollment in RSBY, and lack of a measurable positive impact on pregnancy outcomes, is consistent with prior research showing that RSBY did not influence health outcomes more broadly.[10,182] Previous research has highlighted the challenges of access to, and enrollment in, RSBY.[75] Poor knowledge of the policy benefits may have hampered its potential impact. Previous research has shown that households that were enrolled in RSBY still sought and paid for family planning services elsewhere because they were unaware it would be covered under their policy.[76] Overall OOP healthcare costs were also found to increase for enrolled households.[9,10,173] Other research found that even when RSBY-insured patients were admitted to the hospital they were still making additional OOP payments on top of their existing coverage.[86] Additionally, the care patients receive when using RSBY has been found to be sub-standard, which can lead to poor health outcomes.[183] The combination of supply and demand-side challenges, including the inability to access care through RSBY, low-quality healthcare received, and the additional fees for supplementary care on top of RSBY payments, may have contributed to a counterintuitive finding that enrollment in the program exacerbated rather than ameliorated pregnancy outcomes. It is possible that RSBY-enrolled households had pre-existing difficulties in accessing healthcare, which were made worse by enrollment in the program, although further research is needed in this area.

There are additional methodological considerations which may have biased estimates of the impact of RSBY. Primarily, states that offered health insurance with benefits that exceeded RSBY may have introduced the program to districts at a different schedule compared to states that did not offer similar programs.[10] Thus, unmeasured state-level characteristics could diminish the exchangeability of treatment groups. District-level participation and household-level enrollment were also impacted by socioeconomic characteristics, political affiliation, and the quality of governance, and it is possible this led to an open back-door path between confounders and the instrument.[164] The analyses presented should not be biased by individual or household-level characteristics, unless those characteristics influence the timing of the district gaining access to RSBY. This could be the case if, for example, a district with a worse health profile was prioritized to receive access first, or families migrated in order to receive access to the program sooner.

Due to these issues with the roll-out and implementation of RSBY, the GoI subsumed RSBY into a larger program, AB-PMJAY, in 2018.[79] AB-PMJAY expanded the number of households, healthcare services, and costs covered under the policy, with the goal of increasing beneficiaries and decreasing adverse health outcomes. The impact of the AB-PMJAY program on health outcomes and household health expenditure is still being evaluated.[79]

There are additional caveats to consider when interpreting the results of this research. First, we lacked data on the exact year districts gained access to RSBY. Our instrument was constructed from available administrative records data and did not include enrollment information pre-2010. As we could not assign districts an exact year when they gained access to the program, we created treatment group bins of early, mid, and late adopters of the program, which introduced error in the measurement of the instrument. Nonetheless, district-level access was strongly associated with household-level enrollment in the program, as indicated by the results from the first-stage regression. Second, all outcomes came from self-report, which is susceptible to recall bias and measurement error. This includes the measurement of stillbirth, which is thought to be underreported due to difficulty differentiating true stillbirth from early neonatal death.[136] However, the reliability and/or validity of using self-reported data, including contraceptive calendars, for reproductive health measurement has been supported in different contexts,[150,151] and we restricted our panel to the most recent pregnancy to reduce the impact of measurement error.[152] Third, measurement of abortion may have been impacted by differential misclassification or underreporting of abortion. The likelihood of this may have differed by socioeconomic status, as abortion has been found to be underreported among Indian women of lower socioeconomic status due to social stigma and bias.[54] This is a known problem when measuring abortion,[155] but the use of surveys with standardized training procedures are designed to limit corresponding measurement errors.[184,185] Fourth, general reproductive healthcare and antenatal care were not covered under RSBY. Access to this healthcare reduces the risk of abortion and miscarriage.[167,168] By not covering this care, RSBY is less likely to have a direct impact on these outcomes. Finally, the AHS did not ask about health insurance coverage in their first round of interviews. While it was collected for households in the second and third round of data collection, there was a concurrent data loss from the states of Assam and Odisha in the second round of interviews where it is assumed data was erroneously erased after it was collected.[186] It is possible enrollment in RSBY may therefore be higher in these states than is reflected in this analysis, which would bias our estimates towards the null. However, we did not observe differences in rates of household RSBY enrollment when we comparing states that lost data to the states that did not.

Conclusion

This study adds to existing evidence on the impact of RSBY. Rather than reducing the prevalence of adverse pregnancy outcomes, we observed that enrollment in the policy was associated with an increased probability of pregnancies ending in stillbirth and miscarriage, and a decreased probability of abortion. Sensitivity analyses to account for possible measurement error showed similar effects for stillbirth and abortion, but no impact on miscarriage. Difficulties in the district-level roll-out and household enrollment may habe limited any beneficial effect of the policy on pregnancy outcomes. As the GoI has expanded health insurance coverage to about a half billion people through a new health insurance program modeled after RSBY, AB-PMJAY, this research provides critical information for further research on the impact of this policy on maternal and reproductive health.

6.3 Supplementary appendix: Manuscript 3

Supplementary Table 6.1 First stage ETWFE models predicting district-level access to RSBY as an instrument of household-level enrollment into the policy. Estimates are reported as Average Treatment Effects among the Treated (ATT) on the risk difference scale

District-Level Access to RSBY	**ATT (95% CI)**
Early (2010)	-0.9 (-2.9, 0.9)
Middle (2012)	0.2 (-1.9, 2.3)
Late (2014)	2.4 (0.7, 4.1)

Supplementary Table 6.2 Unadjusted and adjusted effects of household-enrollment in RSBY on adverse pregnancy outcomes, 2004 – 2019, from sensitivity analyses for restricted sample excluding participants reporting enrollment in RSBY in districts that did not have access; estimates on the risk difference scale interpreted as percentage-point differences with corresponding 95% confidence intervals

Outcome	**Unadjusted model** *CACE* *(95% CI)*	**Adjusted for rural/urban + mother's age** *CACE* *(95% CI)*

Stillbirth	2.6 (1.9, 3.2)	2.1 (1.6, 2.7)
Abortion	-4.9 (-6.7, -2.9)	-4.2 (-5.9, -2.5)
Miscarriage	-2.1 (-3.9, -0.3)	-0.3 (-2.1, 1.5)

Supplementary Table 6.3 First stage ETWFE models predicting district-level access to RSBY as an instrument of household-level enrollment into the policy using the beginning of the district-level RSBY-availability window. Estimates are reported as Average Treatment Effects among the Treated (ATT) on the risk difference scale

District-Level Access to RSBY	**ATT** **(95% CI)**
Early (2008)	6.5 (4.9, 8.1)
Mid (2010)	1.8 (0.2, 3.4)
Late (2013)	2.5 (0.9, 4.0)

Supplementary Table 6.4 Unadjusted and adjusted effects of household-enrollment in RSBY on adverse pregnancy outcomes, 2004 – 2019, from sensitivity analyses accounting for first possible year districts could receive access; estimates on the risk difference scale interpreted as percentage-point differences with corresponding 95% confidence intervals

Outcome	**Unadjusted model** *CACE* *(95% CI)*	**Adjusted for rural/urban + mother's age** *CACE* *(95% CI)*
Stillbirth	1.9 (1.2, 2.7)	1.5 (0.7, 2.2)
Abortion	-7.5 (-9.6, -5.3)	-4.2 (-6.2, -2.2)
Miscarriage	-2.9 (-5.4, 0.1)	-0.3 (-2.4, 1.8)

Chapter 7. General Discussion

7.1 Summary of findings

This findings in this thesis provide detailed evidence of enduring socioeconomic inequalities in pregnancy outcomes throughout India. Despite significant investment into a health insurance program aimed at those at the lower end of the socioeconomic gradient, the RSBY program was not associated with meaningful change in rates of stillbirth, abortion, or miscarriage.

Chapter 4 (manuscript 1) provides national estimates of socioeconomic inequalities in adverse pregnancy outcomes across a 15-year time span. These socioeconomic inequalities in rates of abortion and stillbirth in India persisted throughout the study period from 2004 – 2019. The extent of disparities in various socioeconomic indicators and outcomes varied. We observed that women who completed primary education were more likely to report experiencing an abortion while being less likely to experience stillbirth. Additionally, women from a Scheduled Tribe had a lower probability of reporting abortion compared to women not affiliated with either a Scheduled Caste or Scheduled Tribe. For miscarriage, the evidence for socioeconomic inequalities was less conclusive, with more robust positive trends for all groups throughout the study period and greater variations in rates from year to year. There was a noticeable increase in abortion and miscarriage rates over the study period, with a marked acceleration in rates for all socioeconomic strata after the year 2008.

Using a difference-in-differences design, in **Chapter 5** (manuscript 2) we evaluated the impact of district-level access to India's National Health Insurance Program on adverse pregnancy outcomes up to 11 years post-implementation of the policy. We did not observe an overall impact of the policy on rates of stillbirth or abortion. We did observe an impact of RSBY on rates of miscarriage, with district-level access to RSBY associated with 6.3 additional miscarriages per 1,000 pregnancies compared to control areas. We also effects of the impact of RSBY also varied by treatment group. For women residing in early-adopter districts, RSBY was associated with higher rates of miscarriage, whereas women in mid-adopter districts had lower rates of stillbirth after implementation when compared to control districts. We stratified the sample by socioeconomic status to examine differences in estimated effects by these indicators. We observed that among women who had a primary school education and women below the median wealth index, district-level access to RSBY was associated with a higher risk of miscarriage.

Chapter 6 used instrumental variable analysis to examine the impact of household enrollment in RSBY on rates of adverse pregnancy outcomes. There was very low enrollment in RSBY across all study years. The largest increases in enrollment were observed among early-adopting districts. Household-level enrollment in RSBY was associated with an increased probability of stillbirth and miscarriage, and a decreased probability of abortion. We conducted additional sensitivity analyses to account for possible misclassification bias and alternative methods of IV estimation for binary outcome, but these analyses did not substantially change the interpretation of the estimates.

Chapter 4 shows persistent socioeconomic inequalities in adverse pregnancy outcomes. **Chapter 5** furthers this by showing that RSBY did not improve pregnancy outcomes overall, and this effect did not change by socioeconomic status. Additionally, as **Chapter 6** shows, RSBY-enrolled households did not experience improved pregnancy outcomes, suggesting that RSBY did not meet the goal of improving pregnancy outcomes among the most socioeconomically disadvantaged.

The impact of RSBY on adverse pregnancy outcomes may have been restricted by the combination of low household enrollment into RSBY and low utilization of maternal and reproductive health services conditional on enrolled. Although the program covered these essential services, many households were unaware of this and therefore did not avail themselves of healthcare services at empaneled hospitals. As the list of covered services originated at the central government, guidance on communication and how to encourage policy use should have been stronger.

One potential reason why RSBY did not affect pregnancy outcomes was because it did not cover primary healthcare. While childbirth was covered, antenatal care at empaneled hospitals was not, and it is possible that not covering these services received in the same facility care led to fragmented or missed care. Linking antenatal care to delivery with an SBA creates a continuum of care that leads to improved maternal and reproductive health outcomes.[187] By only covering hospital-based care, RSBY was likely unable to influence the abortion and miscarriage outcomes in particular. Expanding RSBY into AB-PMJAY with additional covered services has the possibility to move the needle on adverse pregnancy outcomes.

7.2 Methodological Strengths and Limitations

7.2.1 Household Survey Data

There are limitations to the use of this survey data. A panel of pregnancies was created from surveyed women, along with resulting outcome of the pregnancy (live birth, stillbirth, abortion, or miscarriage). However, while the surveys collected information on all pregnancies women experienced within the last three to five years, the NFHS surveys only differentiated the type of non-live birth for the most recent pregnancy. If the surveyed woman had had a previous pregnancy that resulted in a non-live birth outcome it was coded as “T” for termination. For this reason, we limited to the most recent pregnancy that occurred for all surveys. This also aimed to reduce the bias related to poor recall bias and measurement error. It is also important to note that other covariates and treatment assignment were measured at the time of survey, not the time of pregnancy. This is relevant for treatment assignment, which was done by where respondents were residing at the time of survey. However, the possibility of this resulting in significant bias is low as the majority of internal migration within India is within-district – with very little between-state migration.[188] We can therefore be relatively confident that the district of residence at survey matches the district of residence during pregnancy.

India added additional states and districts during the study period. As such, respondents in the NFHS-4 and 5 can live in states and districts that did not exist in the other surveys. Therefore, observations from new states and territories were recoded to match the unit they were part of as of the 2001 census. Observations that came from states and union territories that were not represented in each of the study years (i.e., Jammu & Kashmir, Delhi, Nagaland, Gujarat, Dadra & Nagar Haveli and Daman & Diu, and Lakshadweep) were dropped.

Finally, the AHS did not ask about health insurance coverage in their first round of interviews. While it was collected for households in the second and third round of data collection, there was concurrent large data loss from in the states of Assam and Odisha in the second round of interviews. Enrollment in RSBY may therefore be higher in these states than reflected in this research.

7.2.2 Measurement Error

Household survey reproductive histories have the potential for measurement error and misclassification. Reproductive histories are retrospective, spanning the last three to five years, making them particularly prone to recall error or unwillingness to report an outcome.[189] As previously addressed, limiting to the most recent pregnancy should mitigate potential bias from recall error.

Previous research has found that self-report surveys underreport true rates of stillbirth, as it's often confused for early neonatal death.[136] The surveys rely on the report of the mother, and often do not ask follow-up questions to differentiate between a true stillbirth and an early neonatal death right after delivery.[190] Additionally, abortion is particularly likely to be misclassified due to social stigma and bias.[191] This issue is exacerbated in India, which has a history of gender biased sex selection in favor of male babies. In 2011, India's sex ratio at birth (SRB) was 111 male births per 100 female births,[192] higher than the global average of 103 – 106 male births per 100 female births.[193] The SRB has stayed steady despite declining fertility levels, with recent research showing that – particularly among higher wealth and higher education couples – sex selection is used to meet societal norms of family size and composition. Due to the bias associated with gender-based sex selection, abortion is often self-reported as miscarriage– particularly for self-managed medication abortion.[155]

7.2.3 Statistical Inference and Bias

There are additional limitations to the statistical methods used in this thesis. In interpreting the SII and RII, it is not the socioeconomic group itself that is reflected, but rather the relative size and ranking within the group. It is also important to note the implicit value judgements inherent in these measurements, particularly when talking about potential policy implications and solutions.[194]

Differences-in-differences analyses assume that there are no unobserved time-varying confounders correlated with the introduction of RSBY that could impact rates of stillbirth, abortion, or miscarriage. While it is not possible to control for all potential sources of confounding, we did account for time-invariant district-level effects and secular time trends. To further control for potential time-varying confounding, we limited our sample to pregnancies that occurred before the onset of Covid-19, which was associated with increased rates of stillbirth and miscarriage. To address correlation between individuals in a cluster, we used cluster-robust standard errors. When the number of treatment groups is small and unbalanced, it is possible that doing so downwardly biases the errors.[195] Methods to address multiple treatment time periods are still relatively new. As a sensitivity analysis for our results, we compared estimates from the Callaway & Sant'Anna method to the Wooldridge method and found general agreement between the estimates.

IV analysis requires additional assumptions in order to draw causal conclusions. To meet the exchangeability restriction, we assume district-level access is not associated with other shocks or interventions that would influence pregnancy outcomes. The as-if-random roll-out of RSBY makes this plausible, as each state could choose what districts to offer access to first. In certain states, it is possible that districts with worse health outcomes may have been offered the policy earlier. To address this possible bias, we examined socioeconomic differences by treatment group and observed overall similarities across the groups. Variables that differed were controlled for in the first step of the 2SLS.

7.3 Future Directions

This is the first research I am aware of that has investigated the impact of RSBY on pregnancy outcomes. **Chapter 4** highlights socioeconomic disparities in adverse pregnancy outcomes, indicating a strong need for further evaluation of the role socioeconomic factors play in adverse pregnancy outcomes. This work is an imperative checkpoint to evaluate India's progress towards the SDGs.

It is particularly important for further research to examine whether AB-PMJAY improved upon these outcomes. With an expanded list of covered services and eligibility for enrollment, AB-PMJAY has the potential to impact maternal health in a way that RSBY did not. What will be particularly important is examination of household enrollment in the policy. As was seen in this research, there was very low household enrollment in RSBY which hampered its ability to impact health outcomes. Future research could delve further into this and examine whether particular factors of RSBY led to the lack of beneficial impact seen in this thesis. Further research should examine whether administration of AB-PMJAY improved upon this in order to reach more people. Further research could also examine if eligible households that did not enroll in RSBY chose to enroll in AB-PMJAY, and what the deciding factor on whether to enroll was. Research should also investigate if households in AB-PMJAY use it for maternal and reproductive health services. Rigorous research is needed to investigate whether the next iteration of government-sponsored health insurance programs in India are having a meaningful and equitable impact on pregnancy outcomes.

7.4 Conclusion

This thesis aimed to provide a thorough and robust evaluation of the impact of RSBY on rates of stillbirth, abortion, and miscarriage in India. We examined the impact of the policy stepwise, first, by presenting socioeconomic inequalities in these outcomes overall; second, by examining the role district-level access to RSBY had on these adverse pregnancy outcomes; and third and finally, by examining the impact of household-level enrollment in the policy on the rates of these outcomes. We did so by using a novel combination of household survey data that provided pan-Indian estimates from 2004 – 2019. We observed persistent socioeconomic inequalities in adverse pregnancy outcomes. We also observed that RSBY was not effective at improving pregnancy outcomes. This thesis sets the stage for subsequent work examining whether the expanded health insurance policy, AB-PMJAY, impacted population health outcomes.

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