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An Impact Evaluation of RSBY on Hospitalization and Total Out-of-pocket Expenditure

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Abstract

The Rashtriya Swasthya Bima Yojana (RSBY, literally "National Health Insurance Programme") was introduced in 2008 in India to provide financial protection for below poverty line population (BPL) and increase access to health care. RSBY is one of the largest health insurance schemes in the world and this paper introduces the first longer term impact analysis of RSBY on health care utilization, as well as evaluate differing effects on rural and urban households separately. We use a triple difference approach in order to estimate causal "intention to treat effects". The analysis was conducted on recently published data from the India Human Development Survey (IHDS), which provides better identification of BPL households than previous surveys. Finally, we validate previous research on the impact of RSBY on total morbidity expenditures and likelihood of catastrophic expenditure using the IHDS dataset. We find very different effects of RSBY on rural and urban households. While there is suggestive, but not statistically significant, evidence that RSBY seems to decrease total morbidity expenditure by as much as 30% for rural households, there is no effect at all on urban households. There's evidence that RSBY induces longer stays at healthcare facilities for urban households, statistically significant at the 10% level, while there is no effect on rural households. Our research suggests that RSBY partly achieves its goals: it alleviates morbidity expenditures for the rural poor, and the scheme increases health care utilization, but only among urban households.

Keywords: India, RSBY, Out-of-pocket expenditure, Health insurance, Impact evaluation
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List of abbreviations

Table 1: List of abbreviations

Abbreviation	Definition
BC	Backward caste
BPL	Below poverty line
CBHI	Central Bureau of Health Intelligence
CEM	Coarsened exact matching
CES	Consumer expenditure survey
DiD	Differences-in-differences
DiDiD	Differences-in-differences-in-differences
HH	Household
HDPI	Human development profile of India
IHDS	India human development survey
INR	Indian Rupees
ITT	Intention to treat effect
LMICs	Low and middle income countries
OOP	Out-of-pocket
PSM	Propensity score matching
RSBY	Rashtriya Swasthya Bima Yojana
USD	United States Dollars

1 Introduction

The poor in low and middle income countries (LMICs) are consistently at a disadvantage regarding access to healthcare (Peters et al., 2008).[1] To mitigate this pressing issue, both the World Bank and the World Health Organization has turned their focus towards health financing solutions in order to increase health care access to the poor (Ruger, 2014 and WHO, 2010).

There is however a sizable knowledge gap on the impacts of insurance schemes on actual health outcomes, health utilization and out-of-pocket (OOP) expenditure on health, as research and policy evaluations have primarily focused on how insurance can reduce financial risk. Researchers often face challenges with lacking detailed data on health care utilization as well as longer term health outcomes (Escobar et al., 2010). Furthermore, the circumstances in which insurance policies are typically implemented often do not allow for a rigorous evaluation. For instance, national schemes do not offer adequate control groups and there are endogeneity problems with health insurance schemes based on voluntary enrollment (Escobar et al., 2010).

We will contribute to diminishing this knowledge gap by evaluating one of the largest health insurance schemes in the world, the Indian Rashtriya Swasthya Bima Yojana (RSBY), both on its impact on health care utilization and on morbidity expenditures. RSBY is a national health insurance scheme for below poverty line (BPL) households, which was gradually rolled out district by district since its introduction in 2008. As the roll out has been slow and about a third of selected districts still had not implemented the policy in July 2012 (Indian Government, 2016), the setting allows for a difference in differences analysis with promising control groups.

The current literature available on RSBY mostly consists of short term impact studies. Overall, research has been focused on enrollment and utilization (Hou and Palacios, 2011 and Sun, 2011) and differences-in-differences studies on the impact of RSBY on morbidity expenditures. The latter capture only short term impacts of the scheme as the average district only had RSBY implemented 6 months before the data used in these studies was collected. (Selvaraj & Karan, 2012 and Johnson & Krishnaswamy, 2012). There is one recent study (Karan et al., 2015) which has examined longer term data for 2011-2012, up to three years after the scheme started being implemented in some of the districts but it has only analyzed impacts on OOP expenditure, not hospital utilization. It also has not considered evaluating differing effects between urban and rural households separately.

[1] The World Bank uses gross national income (GNI) per capita estimates. Low income economies are defined as those with a GNI per capita, calculated using the World Bank Atlas method, of USD 1,045 or less in 2014; middle income economies are those with a GNI per capita of more than USD 1,045 but less than USD 12,736 (World Bank, 2016a).

This paper introduces the first longer term analysis on RSBYs impact on healthcare utilization, using data collected 3-4 years after the scheme started being implemented. It also evaluates if the findings on morbidity expenditures in Karan et al. (2015) can be reproduced with the India Human Development Survey, which has never been used to evaluate RSBY.

Our contribution to the current literature will be to answer the following questions:

> *Did the implementation of RSBY have an effect on households usage of hospitalization?*
>
> *Can we confirm the findings of Karan et al. (2015) that RSBY had a small and not statistically significant effect on households' out-of-pocket morbidity expenditures?*
>
> *Can we confirm the findings of Karan et al. (2015) that RSBY had a small and not statistically significant effect on households likelihood of incurring catastrophic out-of-pocket morbidity expenditures?*

In order to answer these questions we measure the effect of the RSBY scheme on four outcome variables: (a) households' OOP expenditure on health, (b) their likelihood of facing catastrophic morbidity expenditures, measured as OOP expenditures being 10% or more of the households' annual consumption, (c) hospitalization rate as well as (d) hospitalization days. Furthermore we analyze if there are significant differences once we divide the sample between rural and urban households.

We use a triple difference approach in order to estimate the causal 'intention to treat' (ITT) effects for these variables. As the provision of RSBY is limited to households with BPL cards, the control group consists of both (1) households with BPL-cards in districts where RSBY has not yet been implemented and (2) poor households, defined as belonging to the two bottom income quintiles, which do not have a BPL-card. The first difference, between years, allows us to control for all time-invariant effects. The second difference allows us to control for all trends within BPL-card households that are the same for districts where RSBY was implemented (treated districts) and not implemented (untreated districts), for example other national health policies targeted towards BPL households. Finally, the third difference enables us to control for all trends that are specific to relatively poor and BPL-card households within the same treated districts, for example district level health interventions. The only bias we cannot account for is time-varying variables that only affect households with BPL-cards in treated districts.

Before running our main analysis we run a parallel trends test on two pre-treatment data points (Reggio & Mora, 2012 and Duflo, 2000). In accordance with previous literature (Selvaraj & Karan, 2012, Johnson & Krishnaswamy, 2012 and Karan et al., 2015), we also perform coarsened exact matching as a robustness check in order to further mitigate possible selection biases.

We find very different effects of RSBY on rural and urban households. While there is suggestive, but not statistically significant, evidence that RSBY seems to decrease total morbidity expenditure by as much as 30% for rural households, there is no effect at all on urban households. There's evidence that RSBY induces longer stays at healthcare facilities for urban households, statistically significant at the 10% level, while there is no effect on rural households. Our research suggests that RSBY partly achieves its goals: it alleviates morbidity expenditures only for the rural poor, and the scheme increases health care utilization, but only among urban households.

The rest of the paper is organized as follows. We start by presenting our literature review in Section 2, then move on to Section 3 which is focused on strengths and weaknesses of our data. Section 4 describes our methodology in detail. We close the paper with a section presenting and discussing results, followed by a discussion on policy implications and extensions for future research.

2 Background and literature review

Our background and literature review section is divided into three parts. We start by analyzing the broader context of health insurance schemes in low and middle income countries. Then we move on to the Indian health care context and details about the RSBY scheme. Finally we review the literature on impact evaluation of health insurance schemes.

2.1 Health insurance in low and middle income countries

Being able to finance health care is a key indicator of a country's development (WHO, 2010). Health expenditure usually rockets with economic growth and therefore there is extreme inequality in health expenditure across richer countries and poorer countries (Escobar et al., 2010). According to the World Development Indicators, as of 2014 health expenditure per capita in OECD countries (USD 4746.50) was on average 53 times higher than average health expenditure per capita in LMICs (USD 89) (World Bank, 2016b).

The poor in low and middle income countries are consistently at a disadvantage regarding access to healthcare (Peters et al., 2008). In order to mitigate this pressing issue, greater focus has been extended to health financing solutions to increase health care access and ultimately achieve universal coverage both by the World Bank (Ruger, 2014) and the WHO (WHO, 2010).

The WHO argues that one of the key barriers to universal health coverage is over-reliance on direct health payments such as over-the-counter payments for medicines, consultation fees and procedures. A large payment at the moment of need makes it harder to spread health costs over the life-cycle and means that health care is virtually inaccessible to a large part of the population. Furthermore, direct payments increase the risk of impoverishment and make achieving universal health coverage unlikely.

With a large share of direct payments, people pay the same regardless of social background, creating a health system that favours the rich. This is a pressing problem especially in LMICs where households bear a much higher share of the health expenditure. In OECD countries on average 60% of health care is covered by government (central and local) budgets, external borrowings and grants (including donations from international agencies and non-governmental organizationations) while this figure is much lower in LMICs, where around 35% of health care expenditure is covered by the same sources (World Bank, 2016b).

Governments around the world are exploring financing options for their health care system and most countries that are closest to reaching universal health coverage have chosen to reduce the share of direct payments by risk-pooling mechanisms such as social insurance and taxes and prepayment mechanisms such as wage-based contributions (WHO, 2010). For instance, Germany finances a large part of health care with wage-based insurance contributions (WHO, 2010) and Carrin & James (2005) claim that up to twenty-seven countries have approached universal coverage by expanding social health insurance schemes.

LMICs have also implemented public health insurance schemes in order to reduce inequity in access to health (Escobar et al., 2010). China reduced inequities in utilization of both outpatient and inpatient care significantly in recent years through increased insurance coverage and access to primary care (Zhou et al. 2013). A randomized controlled trial also shows evidence that Mexico's Seguro Popular reduced catastrophic expenditure and average expenditure for the poor (King et al., 2009). India, Ghana, Indonesia and Peru are other examples of countries that have implemented health insurance policies in the past twenty years (Escobar et al., 2010).

There are multiple reasons why social health insurance schemes are largely implemented. People without insurance are more prone to have higher shares of out-of-pocket spending, more likely to self-medicate or go without treatment and incur catastrophic financial loss (Escobar et al.,2010). Insurance is argued to increase welfare as it allows the insured to pool their risk (Ruger, 2014) and prevents them from taking loans or reducing their consumption of other essential goods to afford health care (Escobar et al., 2010).

The government gains an additional financing source in the form of the premium paid by the insured, which can be compulsory (WHO, 2010). Consequently, expenses on health care can be pooled between the government and the insured rather than financing public health care solely with taxation money. In addition, the government can partly outsource the administrative burden of public health care to insurance companies and private hospitals through public-private partnerships. This is especially relevant in the context of LMICs where governments have less revenues in terms of taxes, less administrative capabilities and households bear a larger share of the health care expenditure (World Bank, 2016b).

Moreover, there is a growing literature and guidelines from international organizations arguing that health insurance can improve equity (Dercon et al., 2005, WHO, 2010 and Ruger, 2014). The poor have to manage risk much more actively as they are subject to income shocks related to floods, fires, illness, crime and unemployment to a larger extent. Illness shocks raise more concern because in addition to the short term impoverishment, they can lead to long term disability or job loss, eliminating income sources. Since the poor cannot afford insurance premiums, there is a strong argument for subsidies and social insurance schemes in order to improve equity.

On the other hand, concerns have been raised with regards to monitoring, coverage and awareness of insurance schemes. For instance, hospitals and doctors may be tempted to overperform procedures or prescribe unnecessary medicines to maximize their kickbacks. In the Indian state of Gujarat, Desai et al. (2011) suggest that insured women may be overly inclined to have a histerectomy. There is also potential for fraudulent claims (Nandi et al., 2015)

Even in countries where funding is largely prepaid and pooled, there are tradeoffs between the proportion of the population to be covered, the range of services made available and the limit of coverage per person, so governments need to reach coverage compromises to make the scheme financially viable (WHO, 2010). Some countries may choose to prioritize a limited set of services for a larger part of the population and gradually expand available services as the country grows. Others may choose a large set of services targeted at specific vulnerable groups that can gradually be expanded. These choices largely affect the impact of such a policy in financial protection and equitable access for health and therefore in-depth impact analysis studies are needed to understand how these choices affect outcomes.

In this paper we propose to analyz a social health insurance scheme introduced in India in 2008, to understand its effect on financial protection and utilization rates. RSBY is one of the largest and most innovative health insurance schemes implemented in a developing country. By evaluating it in light of the most prominent methods within current literature and using the most recent available data, we aim to set a methodological standard for health insurance evaluation in poor countries and to provide policy recommendations that can be extrapolated beyond the boundaries of the Indian experience with public health insurance. In the next section we will introduce the context of Indian health care policies before moving on to an in-depth explanation of RSBY.

2.2 An overview of Indian health care

The Government of India has announced that it would like to achieve universal health coverage by 2022 (Ghosh, 2014). Although the pursuit of equity in access to health and healthcare has been reiterated in health policy documents in India since 1946 this seems to be an ambitious goal for the country. India has been struggling with challenges regarding income inequality and availability of health infrastructure for the past 80 years (Ghosh, 2014).

India's health outcomes are among the world's worst with infant mortality rate of 41 per 1,000 live births and maternal mortality rate of 190 per 100,000 (Nandi et al., 2015). Public health care is supposedly provided free of charge or with nominal fees, however, relative public spending on health care hasn't increased significantly over the last 30 years, which means the same funding level around 1.25% of GDP had to support a growing base of facilities and staff (World Bank, 2016b). Consequently, shortages in supplies and staff are common and public health care is widely perceived to be of poor quality (Berman, 1998). Moreover, India's growing and enriching population creates a rising burden of non-communicable diseases, and OOP medical expenditure associated with chronic and hospital care will continue to increase (Nandi et al., 2015).

By 2015, only around 18% of the population was covered by any kind of health insurance of which 72% are covered by government sponsored schemes (CBHI of the Government of India, 2015). Aside from leading to a high burden of private health care expenditure, India's low health insurance coverage and debilitated public health care system have remarkably increased household burden of OOP expenditure on health. 69% of total public and private health spending is being financed through OOP expenditures (Shahrawat & Rao 2012) and between 39 and 62 million people are pushed into poverty every year in India due to health payments (Balarajan et al., 2011 and Nandi et al., 2015). For instance, illness is typically the second most common cause for impoverishment of rural households, before crop failure (Dercon, 2002).

Figure 1 reiterates India's (and its neighbors and LMICs) high shares of out-of-pocket expenditure in comparison to OECD countries where a larger part of private health care expenditure is covered by insurance:

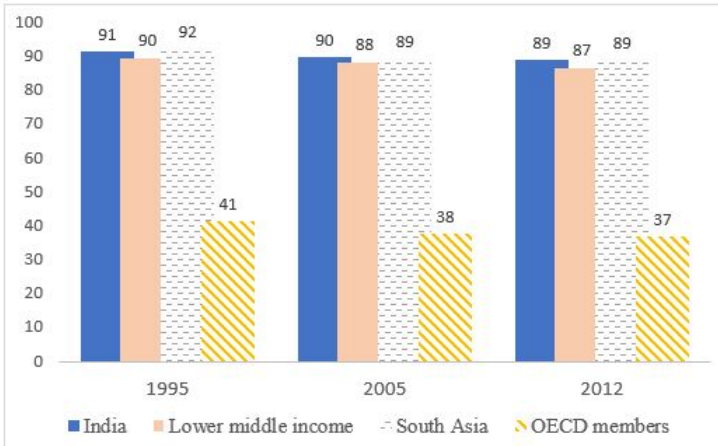

Figure 1: Out- of-pocket expenditure, (% of private health expenditure)
Source: World Development Indicators, The World Bank

Furthermore, there are substantial differences in the distribution of available health care facilities between urban and rural areas. In India 73% of people live in rural areas, yet only 20% of the health care facilities are situated in those areas, while the 27% of Indians living in urban areas have access to the other 80% of health care facilities (Parambath & Sekher, 2013). For instance, urban areas in the low income agrarian state of Madhya Pradesh had 272 hospital beds per 100,000 pop., 54.9% of which in the public sector while rural areas only had 14, 88.8% of which in the public sector. Set-ups operated by a single provider and polyclinics are predominantly in rural areas (90%) while most hospitals are situated in urban areas (60%) (De Costa & Diwan, 2007).

Likewise the distribution of doctors and health manpower between urban and rural areas is extremely asymmetric. On average Madhya Pradesh has 41 qualified doctors per 100,000 inhabitants, a level comparably higher than other southeast Asian low income countries like Thailand and Indonesia. However the distribution works out as 120/100,000 doctors in urban areas (comparable to Singapore) and 12/100,000 in rural areas (comparable to low income countries Djibouti and Guinea-Bissau)(De Costa & Diwan, 2007).

Rural households are therefore less likely to utilize health care services in general, but especially inpatient (hospitalization) services. The same reasoning applies to poor households as transportation costs, marginalization and low literacy are likely to hinder their access to health care services. Consumer Expenditure Survey data from Selvaraj & Karan (2012) in Figure 2.2 below divides India into income quintiles and rural and urban population:

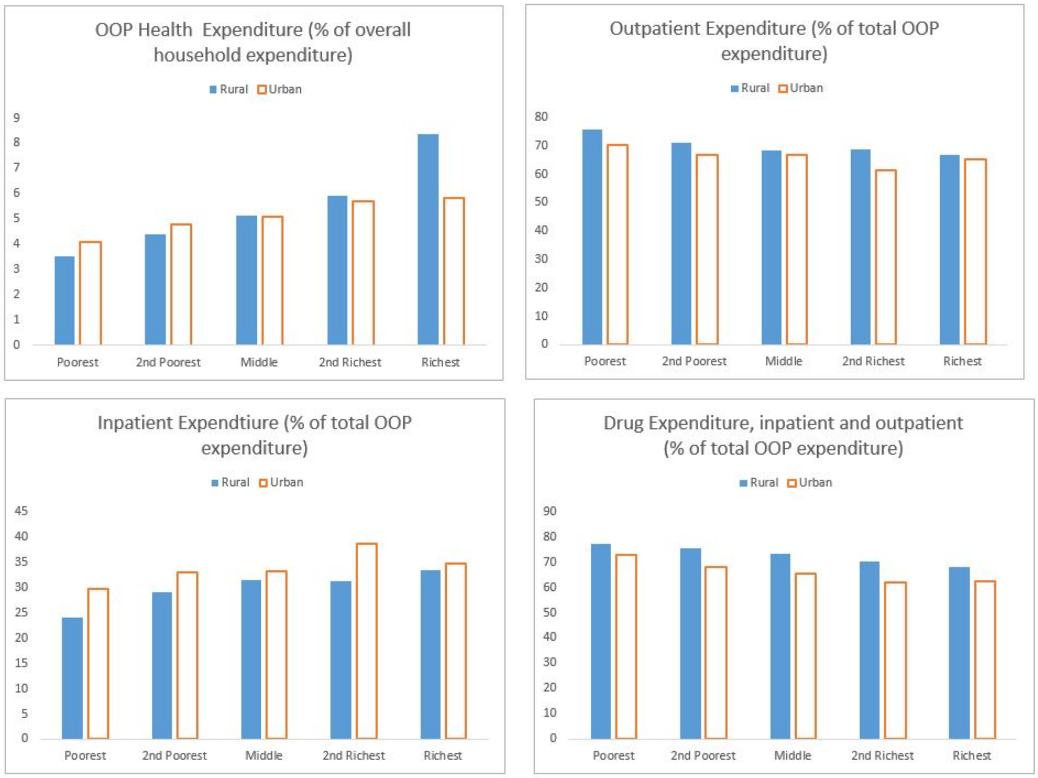

Figure 2: Out- of-pocket expenditure, (% of private health expenditure)
Source: Selvaraj & Karan, 2012

As expected, richer households spend a larger a share of their income on health in general, which can be seen on the top left graph, with rural rich households spending the most. Moreover, it is noteworthy that rural households spend a higher share of their total health care expenditure on outpatient care than urban households and a lower share on inpatient care than urban households across all income quintiles which can be seen by comparing the bottom left and top right graphs. This could indicate that that rural households are less likely to utilize hospitals than urban households as aforementioned studies have shown. Finally, the bottom right graph highlights the fact that the majority of overall OOP expenditure on health is actually on medicines.

Zhou (2013) also found substantial differences in utilization rates between rural and urban households. The average outpatient utilization rate in urban areas in 16 representative Indian states was around 9% while it was approximately 7,5% in rural areas. For hospitalization rates the differences were also significant, the national average was 2.40% and the rural-urban differential was substantial, with hospitalization rate of 2.20% among the rural population and 3.10% in urban population.

Rural households are particularly likely to choose informal health care because of convenience and cost (Sudhinaraset et al., 2013). For instance, Sudhinaraset et al., (2013) mention a survey in different states of India which found that 80% of the professionals people referred to as doctors were not legally qualified. Inequality in access and skepticism towards public health care could be other reasons why informal providers persist despite their low quality.

Therefore, a successful health policy in India hinges on reaching out to the rural and poor population. Before the National Health Policy in 2002, health policies in India were very sporadic and focused on individual diseases or conditions (Nandi et al., 2015). A unified framework and strategy became an explicit priority for the government in 2005 when the National Rural Health Mission (NRHM) was launched. The NHRM integrated stand-alone health policies while focusing on rural and poor population, by targeting lower performing states, enhancing community engagement (e.g. accredited social health activists, so-called ASHAs), focusing on women and infant health (e.g. the Janani Suraksha Yojana (JSY) policy to increase the share of institutional deliveries) (Nandan, 2010).

RSBY is one of the policies under the umbrella of the National Rural Health Mission with specific targets to reach below poverty line population and provide financial protection through means of social insurance. In the next section we will describe RSBY in more detail.

2.3 Rashtriya Swasthya Bima Yojana

Rashtriya Swasthya Bima Yojana (RSBY) is one of the largest health insurance schemes in the world. There are currently 40 million households enrolled in the scheme (Indian Ministry of Health, 2016). It began enrolling households in February 2008 and in 2015 the Indian government announced an ambitious plan to make RSBY their platform for a gradual implementation of universal health coverage. RSBY is an integral part of the Health Ministry's new policy agenda and its aim is to improve access to medical care for below poverty line households by providing virtually free health insurance. In 2011, 21.9% of India's population lived below national poverty line and reducing out-of-pocket (OOP) expenditure on health is a pressing problem (World Bank, 2016b).

2.3.1 RSBY's strengths and weaknesses

RSBY and the idea of public heath insurance are not new concepts in India. An earlier policy, called the Universal Health Insurance Scheme attempted to target subsidized health insurance for the poor already in 2003. However, core flaws of the program such as lack of awareness and difficulties identifying the target market led to its failure and abandonment (Rao, 2004). RSBY is an attempt to address and improve on these shortcomings by leveraging smart card technology and creating incentive structures that reward insurance companies for reaching out to the eligible population proactively.

RSBY is targeted at BPL population and other vulnerable groups and is virtually free of charge. The scheme is fully funded by central and state government, with respective shares of 75% and 25% for the majority of states. It covers hospitalization expenses up to 30,000 Indian rupees (INR), equivalent to USD 450 per family of five per year (Karan et al., 2015) and requires no deductible or copayment. Families are only required to pay a cost of INR 30 (USD 0.50) for issuance of renewal of the smart card. BPL households are identified according to census conducted by the states.

RSBY covers a wide range of procedures associated with inpatient care, which generally means the patient is required to stay at least one night at the hospital. Under RSBY guidelines inpatient care includes medical and/or surgical procedures that require hospitalization (e.g. Appendectomy, Tumour Excision), a number of daycare surgeries and procedures (e.g. Cataract surgery – Bilateral and Unilateral, Dislocation - Shoulder), maternity procedures, pre-existing diseases, transportation costs up to INR 1,000 (USD 15), pre-hospitalization costs up to 1 day before hospitalization and post up to 5 days after discharge. On the other hand, RSBY does not cover specialized tertiary care (e.g. cancer treatment) or outpatient costs such as post-operative medicines, outpatient diagnostics and any other conditions that do not require hospitalization (USAID, 2010).

To increase outreach and reduce the burden on the government, the scheme relies on public-private partnerships. Private and public insurance companies are selected through a competitive bidding process and become responsible for enrolling eligible beneficiaries and settling hospital claims. In order to provide an incentive for insurance companies to enroll as many eligible households as possible, the premium is dependent on the total number of households enrolled during a four-month period for a particular district. One insurance company is chosen to be responsible in each district and is monitored thereafter by a Nodal Officer, who is a government officer in charge of collecting monthly reports (Sun, 2011).

Under RSBY, all transactions are cashless and conducted through an electronic smart card that is personal covers up to five people per household (Karan et al., 2015). Cashless methods were introduced in order to facilitate reimbursement, fight corruption and possible problems with overcharging of fees. If in need of treatment, members of an enrolled household may visit any empanelled private or public hospital that validates the smart card. The patient receives treatment and the hospital gets reimbursed by the insurance company in accordance with fixed rates. The patient does not need to pay anything out-of-pocket as everything happens automatically by swiping the smart card. (Shahrawat & Rao, 2012)

Nevertheless, RSBY has shortcomings related to asymmetric implementation across districts, limited coverage, monitoring and incentive structures. Sun (2011) encountered district enrollment rates varying from 30% to 80% and even more volatile rates when comparing villages. The author attributes this variation to two main causes: 1) variability in the quality of public administration across districts and 2) strategic incentives for insurance companies to enroll in villages that are less isolated and have younger people.

Shahrawat & Rao (2012), found that the majority of OOP expenditures on health were on drugs, with a considerably higher share for outpatient doctor visits (82%) relative to inpatient doctor visits (42%). Additionally, specialized treatments for cancer or cardiac conditions are unaffordable under the INR 30,000 covered by RSBY. This raises concern as they were the cause for 60% of the claims for insurance in the state of Karnataka under their state-run scheme Vajpayee Arogyasri (Sood et al., 2014).

Monitoring of the scheme is costly which hinders quality control. Nine out of 39 empanelled hospitals in Karnataka had not treated any patient under RSBY due to the insufficient training in the operation of technology and incorrect or low quality information stored on the smart card (Dasgupta et al., 2013). Besides, hospitals and doctors may be tempted to overperform procedures to maximize their kickbacks as well as make fraudulent claims (Shahrawat et al., 2011).

Likewise the governance mechanisms of RSBY need to be improved. Research in Chhatisgarh showed that insurance companies often settle claims over six months late and reject up to 15% of them (Dasgupta et al., 2013). Providers surveyed by the authors met with the Nodal Officer once a month, but claimed that problems remain largely unresolved.

The ambitious design of RSBY has attracted the interest of researchers and we now move on to review and discuss previous evaluations conducted on the scheme. Furthermore we discuss how our study can contribute to move the discussion forward.

2.3.2 Previous impact evaluations of RSBY

The first studies that evaluated RSBY focused primarily on short term issues related to RSBY implementation. Sun (2011) surveyed enrollment issues and Hou & Palacios (2011) examined hospitalization patterns in the first two years of RSBY. These studies found that utilization and enrollment rates vary considerably across districts and differences are even sharper between villages. The authors attribute this result to strategic behavior by the insurance companies when conducting informational campaigns and increased awareness of the scheme for people living in proximity to hospitals. These studies were informative in regards to the implementation of RSBY, but they did not have enough data to evaluate longer term changes in OOP expenditure, patient satisfaction or health seeking behavior.

Thereafter, several states of India commissioned surveys to evaluate patient satisfaction with the RSBY scheme and the quality of service provided to RSBY patients of empanelled hospitals (Indian Ministry of Health, 2016). In Maharashtra, Rathi et al. (2012) found that empanelled hospitals were highly concentrated around district headquarters which means high transport costs for poor people living in rural areas. In Gujarat, Devadasan et al. (2013) concluded that while RSBY increased accessibility to health care for the poor, nearly 60% of insured and admitted patients still made OOP payments.

The aforementioned studies focused particularly on patients that were part of RSBY and never conducted a systematic evaluation of the impacts of the program with any kind of control groups. Selvaraj & Karan (2012) were the first to conduct an evaluation of RSBY on a national scale. They performed a differences-in-differences (DiD) analysis with consumer expenditure survey data from 2004-05 and 2009-10, comparing trends in BPL households between districts with and without RSBY. Although their results suggest that households in RSBY districts had a larger decrease in OOP over the studied period, the impact was not statistically significant.

It is also noteworthy that the average district only had implemented RSBY for 6 months when the data was collected and therefore unlikely that the RSBY scheme had a significant impact in such short term after its launch. For instance, Johnson & Kumar (2011) found that enrolled households did not increase health care utilization after RSBY enrollment visits were conducted in their villages, which indicates that it takes time for households to fully understand the purpose of the scheme. Therefore researchers might have to examine longer term data to adequately measure the effects of RSBY.

Johnson & Krishnaswamy (2012) evaluated the impact of RSBY on hospitalization and OOP expenditure combining coarsened exact matching and triple differences on the same data as Selvaraj & Karan (2012). They found suggestive evidence of endogeneity in the selection of districts, which is why they performed matching. This approach is also supported by research conducted in other countries, for instance Escobar et al. (2010) reviewed a number of evaluation studies focused on health schemes for the poor in South America as well as Asia and advocate for the matched DiD method.

Johnson & Krishnaswamy (2012) found that RSBY had increased hospital utilization rates by about 20% and decreased outpatient expenses by about 15%, with both coefficients not only being large, but also statistically significant. This rather massive impact is surprising as the average district had only implemented RSBY 6 months before the evaluations was conducted. One possible reason for this result is the fact the the authors had a model to predict if a household was BPL as they did not have a deterministic variable. Their model yielded a great number of false positives which means above poverty line households were also included in the treatment group.

The latest study to evaluate RSBY was Karan et al. (2015) and they also conducted a matched DiD analysis to estimate the causal effect of RSBY on measures of out-of-pocket expenditure. When comparing two pre-treatment data points, the authors also found suggestive evidence of endogeneity in the selection of implementation districts and therefore they performed propensity score matching to mitigate selection bias.

Karan et al. (2015) establish two treatment groups: early adopter districts and late adopters, in order to take into account differential effects of the policy for districts that have had longer time to implement RSBY. The authors found suggestive evidence that the probability of a household reporting catastrophic payment declined by 32% for the early adopter group, but their results were not statistically significant.

Considering the current literature, our unique contribution is three-fold. We are the first to measure the impact of RSBY on hospital utilization using longer term data up to 4 years after RSBY was implemented. Secondly, we are the first to nationally evaluate differing effects on rural and urban households. Considering the vast difference in livelihood and also health care infrastructure between rural and urban areas, it is likely that a simple dummy variable for being an urban household is not enough to account for differences between the groups. Third, we are the first to use the India Human Development Survey to evaluate the scheme. The data set provides a clear indication for BPL families and a multitude of socio-economic covariates that we can use to validate previous results in the literature under a more rigorous specification.

2.3.3 Other health insurance schemes

In this subsection, we would like to address the concern that the results in this study may be inflated by two other national insurance schemes running in parallel to RSBY, namely the Employee's State Insurance Scheme and the Central Government Health Scheme (Parambath & Sekher, 2013). However only contracted employees are eligible for these contributory schemes so it is unlikely that they overlap with the BPL families who are beneficiaries of RSBY.

The state-run health insurance schemes of Andhra Pradesh, Karnataka and Tamil Nadu are another possible source of bias. All three states have implemented independent health insurance schemes covering BPL population before the introduction of RSBY. In order to avoid bias in our estimates, we drop these three states from our sample.

Finally, the states of Kerala and Himachal Pradesh have extended RSBY further, including above poverty line households as well. For these two sates we keep the BPL part of the sample and remove the above poverty line households so we do not cause bias in our estimates, but are still able to keep both states in our sample.

We move on to a detailed explanation of the data we have used and the methodology we have chosen to estimate causal estimates of RSBY.

3 Data

The recently released data from the Indian Human Development Survey (IHDS) has a larger time span than data sets previously used in the literature, such as consumer expenditure survey (CES) data, namely the IHDS allows for a measure of effects of RSBY up to four years after its implementation. Our data comprises of a panel survey conducted at three points in time: the Human Development Profile of India (HDPI) from 1995, the India Human Development Survey round I (IHDS I) from 2005 and round II (IHDS II) from 2012 . The IHDS survey consists of a multitude of socio-economic variables concerning the livelihood of household and individuals such as household income and assets, education, occupation, health care, marriage and child birth, pre-natal and post natal care, family planning, housing quality and social/physical mobility.

The most relevant variables for our study are summarized in Table 2 and concern hospitalization and disabilities within the household, healthcare expenditures, income and other socio-economic variables such as having a literate family member.

The two IHDS survey rounds were carried out in a very similar fashion. They are an expanded version of the HDPI data set; the households were selected randomly within the stratified 1995 sampling design. The HDPI data includes 33,230 observations on household level of which 13,900 were re-interviewed in the latter IHDS rounds. The re-interviewed households were selected randomly within the stratified 1995 sampling design. Split households that remained within the village were re-interviewed. New households were added in four ways: 1) the IHDS surveys expanded to the 10 states and union territories that the HDPI did not survey, 2) urban samples were included, 3) households were replaced within the village if household records were insufficient in order to relocate households and 4) IHDS included 2 more villages in every district to increase the sample size.

The surveys include 41,554 (IHDS I) and 42,152 households (IHDS II) with 83% of households being the same in both surveys. They are nationally representative multi-topic surveys and the sample includes all 29 Indian states and 4 of 6 Union territories (the small populations living on the islands of Andaman & Nicobar and Lakshadweep were excluded).

Regarding households that could not be re-interviewed for IHDS-II (17% of the sample), an equal amount of households were randomly selected within the same strata, in order to cope with any attrition bias in the best way possible. As we are interested in changes on the district level, we are performing our analysis as a cross-sectional data set between 2005 and 2012.

Table 2: Descriptive data

	1995		2005		2012	
Variable	Obs	Mean	Obs	Mean	Obs	Mean
% of BPL HH	10775	31.6	32939	21.4	34088	17.6
% of HH with member hospitalized past year	10775	15.9	33000	7.35	34102	11.0
% of HH with member disabled in past year	10775	22.2	33000	17.8	34102	27.6
% of HH with catastrophic morbidity exp.	10775	9.58	33000	7.69	34088	5.55
% of urban HH			33000	34.9	34102	33.7
% with any literate in HH	10775	63.8	32909	79.9	34099	68.4
# HH members	10775	6.737	33000	5.354	34102	4.974
Disabled Days in HH	10775	9.500	33000	15.60	34102	21.26
Hospital Days in HH	10775	7.726	33000	1.093	34102	1.366
Total HH morbidity expenditures	10775	1097	33000	1580	34102	4292

A strength in our data compared to the CES data is that we have a clear indicator whether a household is considered having BPL status. It is a variable describing whether a household has a BPL ration card used to collect food subsidies from other governmental policies. CES did not include this variable for its later rounds and all previous papers that analyzed CES data had to perform prediction models to assign households into the treatment group.

There are two main limitations in our data set. Firstly, we are not able to divide morbidity costs into inpatient and outpatient categories, as previous studies that used the Indian CES data have been able to (Selvaraj & Karan, 2012, Johnson & Krishnaswamy, 2012 and Karan et al., 2015). This is relevant as RSBY mostly covers inpatient costs, however total morbidity expense can still be used to measure reduction in OOP expenditure and poverty alleviation. Secondly, when testing for pre-treatment parallel trends our analysis is also limited to the sub-sample of the HDPI survey that was re-interviewed in 2005 which only consists of rural households.

In order to track the progress in the implementation of the RSBY program we used Web Archive's Wayback Machine (Internet Archive, 2016), a database on past versions of internet websites, in combination with the publicly available implementation data presented at RSBY's official web page (Indian Ministry of Health, 2016). Tracking past versions of the published data allowed us to collect data on the gradual implementation of RSBY, which was crucial for our identification strategy, which will be covered in the following section.

4 Methodology

In this section we introduce our identification strategy, outcome variables and regression specification. Furthermore, we argue that triple differences analysis is able to control for most of the unobserved variation in the sample and therefore we use this approach in order to estimate causal ‘intention to treat’ effects for our outcome variables. In accordance with previous literature we also perform coarsened exact matching in order to further mitigate any potentital biases.

4.1 Identification strategy

Our identification strategy explores the fact that RSBY was implemented on a rolling basis across districts between 2008 and 2012. States were responsible for choosing which districts would implement RSBY each year, given that they fulfill a set of criteria by the Ministry of Labour and Employment. These criteria include having enough insurance companies, health providers and updating BPL lists. States are able to include up to 20% of their districts each year (Karan et al., 2015) and Table A2 in the appendix presents how many districts that have implemented RSBY over the last seven years in each of India’s states and union territories.

We use a triple difference (DiDiD) approach in order to estimate causal ‘intention to treat’ effects for our outcome variables (Johnson & Krishnaswamy, 2012). The underlying assumption in the DiDiD approach is that the treatment and control group follow parallel time trends with regards to the outcome variables (Angrist & Pischke, 2008).

Our treatment group comprises BPL households in districts that implemented RSBY until July 2012. As the provision of RSBY is limited to households with BPL cards, the control group consists of both (1) households with BPL-cards in districts where RSBY has not yet been implemented and (2) poor households, defined as belonging to the two bottom income quintiles, that do not have a BPL-card and reside in treated districts. The first difference, between pre- and post-treatment years, allows us to control for all time-invariant effects. The second difference allows us to control for all unobserved trends within BPL-card households that are the same for both districts that had implemented RSBY as well as districts that had not, for example other national policies targeted at BPL households. Finally, the third difference enables us to control for all unobserved trends that are specific to the relatively poor and BPL-card households within the same treated districts, for example district level health interventions.

Despite there being evidence that enrollment and utilization have been low and sporadic (Sun, 2011, Hou & Palacios, 2011) we implicitly assume that all households in a district are automatically treated once the district implements RSBY. In other words, what we measure in our study is not the average treatment effect on the treated, but rather the intention to treat (Angrist & Pischke, 2008). We have two reasons to estimate the ITT effect and not the average treatment effect on the treated. Firstly, our data does not allow us to determine if a households is actually enrolled in RSBY or not, rather we are able to identify households that are eligible. Secondly, since enrollment is voluntary, there could be a self-selection problem with sicker households being more likely to enroll and be part of the treatment group.

We also run the regression on separate rural and urban samples, as the literature points out to substantial differences in access to health care and health insurance between rural households and urban households (De Costa & Diwan, 2007, Parambath & Sekher, 2013 and Selvaraj & Karan 2012).

We argue that triple differences is able to control for most of the unobserved variation in the sample and we are able compose a comparison between control and treatment group, where the omitted variable bias will be greatly reduced. In this case, the DiDiD estimates can be argued to provide a causal estimate of the treatment (Angrist & Pischke, 2008).

4.1.1 Expected outcomes

We decided to focus on four outcome variables that we argue are most likely to be affected by RSBY in the short term, seen in Table 3. We analyze impacts on hospitalization rates in the extensive and intensive margin. For the extensive margin, meaning the probability of being hospitalized, we have a dummy with value 1 if anyone in the household was hospitalized in the last year. For the intensive margin we run a second regression only on households in which at least one member was hospitalized in the last year. In this second regression, the outcome is hospitalization days and we want to see the impact RSBY had on the decision to spend more days in the hospital. We use both of these variables as measures of utilization of health care. If RSBY reduces costs of health care and increases awareness, more people should be willing to utilize inpatient treatment. Hospitalization rate and hospitalization days should therefore increase in treated districts if RSBY was successful.

Table 3: Dependent variables

Variable	Definition
Catastrophic Expenditure	Total sickness expend. above 10% of annual expend. (= 1)
Hospitalization Rate	HH member being hospitalized at least 1 day in the last year (= 1)
Hospitalization Days (log)	Number of days HH members were hospitalized last year (log)
Total Expenditure on Morbidity	Total HH expenditure related to sickness (INR)

Total expenditure on morbidity is the sum of expenditures on short term morbidity (e.g. fever, diarrhea) and major morbidity (e.g. diabetes, heart conditions) measured in Indian Rupees. There are two contradictory effects on morbidity expenditures: as inpatient care is insured in RSBY districts, OOP morbidity expenditures related to inpatient care are expected to be lower. However, if the scheme increases usage of inpatient care, households might end up buying more medicines which increases the health care expenditure. Although it is likely that the reduction in inpatient costs constitutes the larger effect, the actual effect of the RSBY scheme on morbidity expenditures is an empirical question.

The catastrophic expenditure variable is a dummy defined as 1 if a household has had morbidity expenditure over a threshold of 10% of their annual expenditure (Selvaraj & Karan, 2012). As this variable is connected to total morbidity expenditure, the expected outcome is somewhat ambiguous. It is however unlikely that complementary expenditure due to increased access to health care would be large enough to offset the decrease in likelihood of catastrophic expenditure due to having inpatient care being insured. Conditional on RSBY being successful we therefore expect households in treated districts to be less likely to incur catastrophic expenditure.

4.1.2 Regression specification

In order to present our regression in a pedagogical manner, we first present it as a differences-in-differences (DiD) equation, then add the third difference on a second equation. We have also included clustered standard errors on state level as that is the level RSBY is implemented on. Equation 1 below describes the DiD specification:

$$Y_{it} = S_i + \beta_1 * treat_i + \beta_2 * POST_t + \beta_3 * POST_t * treat_i + \gamma_1 * X_{it} + \varepsilon_{it} \tag{1}$$

Where:

$POST_t$ is a time dummy (= 1) for year 2012, indicating it is post RSBY implementation

Y_{it} represents the outcome variables

S_i are state fixed effects

$treat_i$ indicates if the household is in a treated district (= 1)

X_{it} is a vector of control variables described below

ε_{it} is the unobserved variation on the household level

β_3 is the coefficient of interest, because it measures the difference-in-difference effect of RSBY on the outcome variables. We have also captured state fixed effects. Our triple difference specification adds a third difference estimator between households with below poverty line cards and other poor households, which do not have BPL cards, in the treated districts. This allows us to also account for time trends that happened simultaneously for all relatively poor households in treated districts.

$$\begin{aligned} Y_{it} = S_i + \beta_1 * treat_i + \beta_2 * POST_t + \beta_3 * POST_t * treat_i + \beta_4 * BPL_i + \\ \beta_5 * BPL_i * treat_i + \beta_6 * BPL_i * POST_t + \beta_7 * POST_t * BPL_i * treat_i + \\ \gamma_1 * X_{it} + \varepsilon_{it} \end{aligned} \tag{2}$$

The coefficients and variables are analogous to equation 1 with the addition of BPL_i indicating if the observation is a BPL household. Now we also have two additional interaction terms, $BPL_i * treat_i$, $BPL_i * POST_t$ as well as the triple interaction term $POST_t * BPL_{it} * treat$. Our coefficient of interest is β_6.

Our control variables, presented in Table 6 consist of nine variables which control for socio-economic characteristics that might influence healthcare-seeking behavior and influence whether a district was selected to implement RSBY. There were many control variables to consider, we chose those most likely to impact healthcare usage and expenditures as well as the likelihood of being chosen for treatment: Households belonging to backward castes (BC) are marginalized in many ways in India, which makes it a good proxy for socio-economic status, having implications for health-status as well (Nayar, 2007). Household literacy is also relevant, as a active health-seeking behavior is more likely for literate households (Ahmed et al., 2000).

Residing in urban areas is likely to affect the probability of treatment, as the infrastructure for insurance and healthcare is considerably better. Take the example of Madhya Pradesh, were there is 20 times higher provenance of hospital beds per 100,000 population in urban areas De Costa & Diwan (2007). Maternal mortality is also much higher in rural areas (Nandi et al., 2015). We also include income, as richer households are more likely to utilize health care and have higher shares of OOP expenditure on health (Selvaraj & Karan, 2012). We control for number of household members as a large number of members in the household is correlated with rural and poor status and households with more people are more likely to have at least one sick person. We control for years of education as educated people are more likely to have health insurance, with households that have higher education being twice more likely than average (Parambath & Sekher, 2013). Religion is a proxy for cultural background and propensity to use Indian traditional medicine rather than alopathic medicine. Parambath & Sekher (2013) shows that Hindus are more likely to have health insurance than average, while and Muslims are less likely to have it.

Table 4: Relevant control variables

Variable
Predominantly backward caste HH
Anyone literate in HH
Urban HH
Mean age in HH
HH yearly income
Number of HH members
Years of education (HH head)
Predominantly Hinduist HH
Predominantly Muslim HH

4.2 Comparing districts

Aside from the minimum criteria for implementing RSBY stated above, we have found no formal indication that districts were prioritized by the governing states. However, concerns about selection bias have been raised in the literature (Karan et al., 2015 and Johnson & Krishnaswamy, 2012). Due to these concerns we conduct tests for selection bias before proceeding with our main analysis.

To test for selection bias, we start by comparing sample means for the treated and untreated districts. In Table 5 we can see several statistically significant differences such as treated districts having a higher fraction of BPL households and rural residents. The table is accompanied with respective p-value from Pearson chi2 test for the categorical variables and double sided t-tests for the interval variables. Having different means for control variables does not necessarily mean that our identifying assumption of parallel trends does not hold, although it indicates a risk that it does not. We do, however, note that many variables have statistically significant differences in mean values, an indicator that selection bias might be present.

Visual inspection is also a good starting point for evaluation, comparisons between the treatment groups for our relevant dependent variables are presented in Figure 3 and Figure 4 in the appendix. By looking at the graphs, there are indications that we might have a problem with parallel trends for hospitalization.

Because we found significantly different means and indications of non-parallel trends through visual inspection, we move on to perform a formal test for parallel trends using two pre-intervention points.

Table 5: Comparing treated and untreated districts, 2005

Variable	Untreated Mean	Treated Mean	Pearson chi2 p-value
Any literate in HH (%)	78.6	81.2	0.000
Any HH member disabled in past year (%)	18.4	17.1	0.003
BPL HH (%)	24.7	26.4	0.000
# HH members	5.40	5.30	0.0003
Hinduist HH (%)	78.5	79.3	0.078
Years of education (HH head)	7.46	7.85	0.000
Urban HH (%)	33.6	36.3	0.000
			T-test, p-value
HH morbidity expenditures (INR)	1597	1561	0.6851
HH monthly consumption per capita (INR)	911.3	972.3	0.0000
HH total yearly income (INR)	54455	56696	0.0174
Mean age in HH	28.88	29.05	0.1998

4.3 Testing for parallel trends

By running a "fake" differences-in-differences regression on two pre-treatment data points, it is possible to check if there is a significant coefficient for the treatment variable, even in the absence of treatment (Duflo, 2000 and Reggio & Mora, 2012).

If we run a pre-treatment regression using the same outcome variables and the coefficient for the treatment variable is significantly large and different from zero, we have evidence that the treatment and control group are not following parallel trends. Differential trends between BPL households in the treated districts and the non-treated districts even in the absence of treatment might create bias in our DiDiD analysis on pre- and post-treatment. The "fake" impact evaluation regression equation 3 is represented below:

$$\begin{aligned} Y_{it} = S_i + \beta_1 * treat_i + \beta_2 * Y2005_t + \beta_3 * Y2005_t * treat_i + \beta_4 * BPL_i + \beta_5 * BPL_i * treat_i \\ + \beta_6 * BPL_i * Y2005_t + \beta_7 * Y2005_t * BPL_i * treat_i + \gamma_1 * X_{it} + \varepsilon_{it} \end{aligned} \quad (3)$$

This regression is analogous to equation 2, but being run on two pre-treatment data points. Instead of a post-treatment dummy we have $Y2005_t$, an indicator which takes value 1 if the observation was collected in 2005 and 0 if it was collected in 1995.

Because we can only run this test using pre-treatment data points, we argue that if the treated BPL households and the non-treated BPL households were following parallel trends between 1995 and 2005, it is strong evidence that they continue follow parallel trends between 2005 and 2012, the periods of our main analysis. We have three arguments for this: 1) The ten years span between 1995 and 2005 is long enough to make short term fluctuations smoother, 2) Government healthcare expenditure as a percentage of India's GDP has not changed in the last 20 years and 3) Interventions that could have affected the health outcome are accounted for by our control groups.

On the other hand, if we do not find parallel trends between 1995 and 2005 we have reason to believe there is selection bias when districts are chosen to implement RSBY. This is why we conduct a complementary matching method on pre-treatment data before we run the DiDiD as a further robustness check against selection bias which we will describe in the next section (Escobar et al., 2010, Karan et al., 2015 and Johnson & Krishnaswamy, 2012) .

4.4 Matching

The goal of matching is to find a sub-sample in observational data which provides a treatment and control group that approximate the outcome of a randomized treatment. To do so a researcher creates a measure of imbalance between pairs of observations in the control group and treatment group, which represents how far they are from being a counterfactual to each other, given a set of controls. Observations which have a distance to its counterfactual larger than a certain threshold δ are excluded in the sub-sample (King & Nielsen, 2016).

Since the decision unit in implementing RSBY is the district, we would like to conduct matching using district level covariates to mitigate selection bias. Once we perform matching we conduct the parallel trends trends test once more in the sub-sample because our objective is to diminish the coefficient β_5 on the pre-treatment regression. This would suggest that treatment and control group are closer to a randomized treatment in the matched sub-sample than in the original data set.

Propensity score matching (PSM) is the predominant matching approach in the impact evaluation literature. The method usually consists of running a logit regression of Y on X where Y is the outcome and X are the relevant controls, then comparing the probabilities assigned to each combination of controls. However recent criticisms (King & Nielsen, 2016) have shed light on the limitations of this approach. PSM has actually been shown to increase imbalance and statistical bias when the distance threshold is too strict. Researchers that want to get closer to an exact match, may prefer to use different matching techniques.

Coarsened exact matching (CEM) is an alternative strategy which means observations are matched on exact equivalence of their covariates which are predominantly categorical variables. For example, if we included a control which is a dummy for being below the poverty line, CEM will match households with the same values. By setting a group of relevant covariates, CEM is able to establish a number of strata based on covariate values with control group observations and treatment group observations. All strata without at least one observation from each group are dropped in the sub-sample. (King & Nielsen, 2015)

Given that our chosen matching covariates are partly of discrete nature and we have a large enough sample size to apply exact matching, we decided to continue our analysis using coarsened exact matching.

4.4.1 Matching strategy

Coarsened exact matching is flexible with respect to the variables included, how many subcategories are created for each variable and the matching algorithm chosen. Before we began any type of testing we decided on covariates we found to be relevant based on the current literature, which are reported in Table 6 below. After our selection we did not alter the list to minimize model dependence in our results.

4.4.2 Variable selection

A good covariate should affect both the probability of being treated as well as confound our dependent variables. A variable that only affects the probability of treatment will mainly decrease the precision of our estimates and decrease our sample size, thus not contribute to better estimates. Covariates that are only correlated with outcome variables may also be included, as these variables actually increase the precision of the estimated exposure effect without increasing bias (King & Nielsen, 2015).

Choosing relevant variables is thus mainly a task involving current knowledge on the selection of treatment, rather than an econometric exercise. As accounted for in the previous section, we cannot precisely identify a consistent logic (e.g. focusing on lower performing districts) or specific criteria (e.g. share of sick population or share of BPL population) for selection into treatment with the available data, although it is seemingly non-random. General criteria are having enough insurance companies, health providers and updated BPL lists. It is also likely that districts were targeted on socio-economic factors such as fraction of BPL households, as the Indian health policy has an overall strategy focused on low performing states (Nandan, 2010).

Preferred covariates are thus socio-economic variables and proxies for health care and insurance infrastructure. Our data set unfortunately does not contain relevant infrastructure variables and we are unaware of publicly available district level data on health care or banking infrastructure. Our main focus is thus on socio-economic variables.

The relevant socio-economic factors are:

Table 6: Relevant Covariates

Nr	Variable
1	Fraction of district households below poverty line (BPL)
2	Fraction of district households belonging to backward castes
3	Fraction of district households residing in urban areas
4	Fraction of households with any literate family member
5	Regional dummies (North, East, West, South and North-East)

1 BPL households, as districts with large fractions of BPL households are both likely to be targeted by the policy and are correlated with both their health status (Wagstaff, 2002) and out-of-pocket (OOP) expenditure (Shahrawat et al., 2011).

2 Households belonging to backward castes (BC), as they are marginalized frequently in India. BC is a good proxy for socio-economic status that has implications for health-status as well (Nayar, 2007).

3 Residing in urban areas is likely to affect the probability of treatment, as the infrastructure for insurance and healthcare is considerably better. Take the example of Madhya Pradesh, were there is a 20 times higher provenance of hospital beds per 100,000 population in urban areas (Costa et al., 2007). Maternal mortality is also much higher in rural areas (Deogaonkar, 2004).

4 Household literacy is relevant, as health-seeking behavior is more likely for literate households (Ahmed et al., 2000). It is also a viable proxy for socio-economic status in general.

5 India is a diverse country; there are regional clusters with very different cultures and socio-economic composition (Joshi et al., 2003). One way to account for this diversity is creating regional dummies, which has been done in similar research by Johnson & Krishnaswamy (2012) and Karan et al. (2015).

As the fraction of BPL households and urban households are likely to be the most relevant criterias for selecting implementation districts, we divide these variables into six strata with equal size. Backward caste and literacy are likely to be less relevant, which is why we only divide these variables into 3 categories.

Carrying out the CEM resulted in 103 unmatched districts, leaving us with 101 control and 104 treated districts. Looking at Table A1, we can evaluate the differences between treated and untreated districts. Although there are still significant differences due to our limited sample size, p-values do indicate more similar characteristics for relevant variables. In the next section we present and discuss results for each of the steps in our methodology.

5 Results

In this section we present our results for the parallel trends test, matching and our DiDiD estimates. We run the parallel trends on the full sample and on a matched sub-sample, then we compare coefficients to argue that matching mitigated possible selection bias in our sample. Finally we move on to conduct our DiDiD regression with and without matching.

5.1 Parallel trends test

In this section we evaluate if our underlying assumption of parallel trends holds using the framework outlined by Reggio & Mora (2012) and Duflo (2000).

We performed a DiDiD on the two pre-treatment periods in order to check if the coefficient on treatment is sizeable and is statistically significant even in the absence of treatment. It is important to note that we only can perform this parallel trends test for part of our sample - the observations that were re-interviewed in IHDS survey round I in 2005, 13,900 out of 41,554 households and this sub-sample only consists of rural households. Our parallel trends tests is thus limited, but testing this sub-sample still provides valuable insight to evaluate whether our identification strategy holds. All further analysis in this section is performed on the panel data set of the 13,900 matched households from the 1995 survey.

Regression Table 7 below shows results for each of our outcome variables, where the coefficient of interest is the triple interaction term $Y2005 * BPL * treat$. Overall, we do not find any statistically significant effect of our fake treatment analysis on any of the relevant outcome variables, suggesting that possible bias is limited. For a BPL household residing in a treated district, the change in morbidity expenditures between 1995 and 2005 that is related to our fake treatment was INR -344.2 (USD 5), though statistically insignificant. The regressions also suggests that our fake treatment results in a 3.67 percentage point increase in households incurring catastrophic expenditures as well as a a 2.49 percentage point decrease in hospitalization rate which are also statistically insignificant. Regarding hospital days for hospitalized households there's a 6% decrease due to our fake treatment, but it is not statistically significant given our large standard deviation.

In order to establish a treatment and control group with more similar characteristics, we proceed to establish a matched sub-sample as has been done in the RSBY impact evaluation literature (Karan et al., 2015 and Johnson & Krishnaswamy, 2012). We re-run the parallel trends test on the sub-sample and compare the new results with the results on the original sample. We present our matching results in the next section.

Table 7: DiDiD on two pre treatment periods, w/o CEM, only HHs surveyed in both periods

VARIABLES	(1) Total morbidity expenditure	(2) % households with catastrophic expenditure	(3) Hospitalization rate	(4) Log of hospital days
Y2005*BPL*Treat	-344.2	0.0367	-0.0249	-0.0676
	(265.3)	(0.0390)	(0.0329)	(0.331)
Treat	-91.85	0.0216	0.0323	-0.0880
	(148.2)	(0.0289)	(0.0312)	(0.183)
Treat*BPL	-148.5	-0.0262	0.0317	0.245
	(122.8)	(0.0299)	(0.0252)	(0.183)
Year 2005	488.9**	-0.113***	-0.0734***	-0.894***
	(216.0)	(0.0258)	(0.0259)	(0.223)
Y2005*Treat	-396.1	-0.0485	-0.0536	0.0295
	(243.0)	(0.0360)	(0.0392)	(0.298)
BPL HH	76.42	-0.0635***	0.00419	-0.171
	(71.58)	(0.0203)	(0.0145)	(0.136)
Y2005*BPL	-561.0**	0.0572**	-0.0187	-0.251
	(217.7)	(0.0250)	(0.0197)	(0.258)
Backward Caste HH	-45.32	-0.00730	-0.0161	0.0387
	(113.6)	(0.0120)	(0.0109)	(0.123)
Any literate in HH	-103.3	-0.00903	-0.0171	-0.0364
	(80.65)	(0.0128)	(0.0116)	(0.0754)
Mean age in HH	10.17***	0.00319***	0.00238***	0.00770**
	(2.507)	(0.000367)	(0.000380)	(0.00383)
HH yearly income	0.000915	-8.56e-07***	-6.41e-08	2.54e-06*
	(0.00208)	(1.32e-07)	(1.49e-07)	(1.48e-06)
# HH members	88.44***	0.0103***	0.0101***	-0.135***
	(12.89)	(0.00178)	(0.00179)	(0.0156)
Years of education	56.30***	0.00432***	0.00515***	-0.0344**
	(11.70)	(0.00157)	(0.00154)	(0.0140)
Hinduist HH	-14.21	-0.0168	-0.0119	0.0458
	(147.7)	(0.0207)	(0.0218)	(0.124)
Muslim HH	217.9	0.00710	0.00222	-0.230
	(200.5)	(0.0265)	(0.0321)	(0.178)
Constant	-117.0	0.137***	0.0503	2.326***
	(283.6)	(0.0369)	(0.0329)	(0.284)
Observations	9,117	9,117	9,117	1,130
R-squared	0.028	0.043	0.072	0.403
C. E. Matching	NO	NO	NO	NO
Analysis	DiDiD	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State	State

5.2 Parallel trends test with matching

Carrying out the coarsened exact matching resulted in 65 unmatched districts and 3,228 unmatched households, leaving us with 111 districts and 5,889 households to perform our parallel trends test on. Although many districts are dropped, the remaining sample size is in line with previous studies (Johnson & Krishnaswamy, 2012 and Karan et al., 2015). As we reduce the sample size a lot, the change in coefficients is of greatest interest, as standard deviations will naturally increase in size.

Regression Table 8 below shows results for each of our outcome variables. Again, we note no statistically significant results for the coefficients of $Y2005 * BPL * treat$ in the different regressions. The estimated effect of the fake treatment on total morbidity expenditure is now only INR 41.83 (USD 0.50), a fraction of its size without matching and close to zero. The supposed effect on catastrophic expenditure is diminished somewhat but still accounts for a 3.1 percentage point increase in households with catastrophic expenditure. The estimated effect on hospitalization rate is now only 0.4 percentage points, about a fifth of its size in the regression without matching. The coefficient for log of hospital days is the only one that has increased in size, it is however likely that matching does worsen the estimates for this variable, as its sample size becomes very limited.

As we found no statistically significant results in our regressions with and without matching, we can now claim that our parallel trends assumption is likely to hold for the 10 years preceding RSBY (1995–2005). We still have to assume that the trends between our treatment and control groups have not changed in the years since RSBY program was implemented (2005–12), but this can not be more rigorously tested than what has been done in this section. Given the success of our testing it is likely that we can make causal claims in our impact analysis. We now move on to present the results of the actual impact analysis with our triple differences specification and compare them with results on the matched sub-sample.

Table 8: DiDiD on two pre treatment periods, with CEM, only HHs surveyed in both periods

VARIABLES	(1) Total morbidity expenditure	(2) % households with catastrophic expenditure	(3) Hospitalization rate	(4) Log of hospital days
Y2005*BPL*Treat	41.83	0.0310	0.00422	0.167
	(351.8)	(0.0488)	(0.0402)	(0.440)
Treat	-69.45	0.0251	0.0194	-0.122
	(200.5)	(0.0352)	(0.0430)	(0.305)
Treat*BPL	-137.9	-0.0487	0.0111	0.240
	(154.4)	(0.0347)	(0.0286)	(0.236)
Year 2005	418.2	-0.120***	-0.0618**	-0.866***
	(280.0)	(0.0324)	(0.0253)	(0.252)
Y2005*Treat	-167.2	-0.0424	-0.0284	0.0820
	(310.9)	(0.0455)	(0.0490)	(0.394)
BPL HH	57.20	-0.0670***	0.0135	-0.221
	(101.4)	(0.0224)	(0.0155)	(0.153)
Y2005*BPL	-353.8	0.0843***	-0.0355	-0.371
	(282.6)	(0.0299)	(0.0216)	(0.345)
Backward Caste HH	-88.00	0.000381	-0.0144	0.104
	(156.5)	(0.0130)	(0.0132)	(0.154)
Any literate in HH	-143.8	-0.0139	-0.0160	-0.0838
	(120.1)	(0.0178)	(0.0149)	(0.0908)
Mean age in HH	8.350**	0.00309***	0.00215***	0.00385
	(3.572)	(0.000454)	(0.000504)	(0.00517)
HH yearly income	0.00335	-1.01e-06***	-6.49e-08	4.21e-06***
	(0.00343)	(1.45e-07)	(1.96e-07)	(1.03e-06)
# HH members	83.81***	0.0129***	0.0110***	-0.145***
	(15.14)	(0.00228)	(0.00230)	(0.0200)
Years of education	47.27***	0.00163	0.00354*	-0.0298*
	(15.22)	(0.00204)	(0.00189)	(0.0166)
Hinduist HH	80.42	-0.0436	0.00997	0.172
	(242.7)	(0.0293)	(0.0262)	(0.159)
Muslim HH	147.7	-0.0299	0.0308	-0.214
	(311.3)	(0.0354)	(0.0402)	(0.212)
Constant	-31.40	0.173***	0.0265	2.350***
	(417.8)	(0.0476)	(0.0411)	(0.378)
Observations	5,889	5,889	5,889	697
R-squared	0.025	0.045	0.060	0.405
C. E. Matching	YES	YES	YES	YES
Analysis	DiDiD	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State	State

5.3 Impact analysis

We proceed to perform the main DiDiD analysis on our four outcome variables in order to evaluate the impact of RSBY on the matched sub-sample. The results without matching are available in Tables A3, A4 and A5 in the appendix. As discussed before, we will conduct the regressions on rural and urban households separately, as well as all households with a dummy control variable for urban households. The reason for this separation is that there are substantial differences in access to health care and health insurance between urban and rural households which may lead to differential effects from treatment (De Costa & Diwan, 2007, Parambath & Sekher, 2013 and Selvaraj & Karan 2012). One must note, however, that since about 70% of Indian households reside in the rural area and the rural population being poor to a larger extent, the regressions for urban households are restricted in sample size with large uncertainty in the regressions as a result.

5.3.1 Total morbidity expenditure

We begin by evaluating the effect of RSBY on total morbidity expenditures, which is captured by the coefficient for $Y2012 * BPL * treat$. Regressions on the matched sample can be seen in Table 9. The average morbidity expenditures in 2012 was INR 4,300 (USD 65), with this in mind we can note quite a sizable coefficient for the rural population. The suggested impact of RSBY, a decrease of INR 1,391, amounts to 30% of the average morbidity expenditure for this sub-sample, although statistically insignificant given the large standard deviation. As the rural population constitutes the majority of the sample, the coefficient is similar when regressing on the full sample, although being somewhat smaller. This is due to the negligible impact on the urban households, an increase of about INR 300 (USD 4.5), which is very close to zero, especially considering the standard errors. The results are similar in the non-matched sample, seen in Table A3 in the appendix. The coefficient for the rural sample is somewhat smaller, but the impact on the urban sample actually shows a larger increase than in the non-matched sample. This is still well within the standard error of the coefficient. Overall, the regression results indicate a decrease in total morbidity expenditure for Indian households, mostly due to its effect on rural households, while the impact on urban households appears to be limited. We can see significant economic effects of RSBY on total morbidity expenditure, they are however not statistically significant.

5.3.2 Catastrophic morbidity expenditure

In regression Table 10 we can evaluate the effect of RSBY on the fraction of households with catastrophic morbidity expenditure. We can clearly see differing trends between rural and urban households, where rural households are largely unaffected having a coefficient size of less than 1 percentage point increase, which is not statistically significant, while urban households have a decrease of 7.8 percentage points in the likelihood of incurring catastrophic morbidity expenditures. Although large, the result is not statistically significant, likely due to the limited sample size on urban households. Looking at the regression with the entire sample size with a urban dummy variable instead, there is no visible effect of RSBY. The results are quite robust between regressions with and without matching, in Table A4 in the appendix, there are very small effects on the rural population as well as the entire population as a whole in the regression without matching. For the urban population, there is a considerable decrease of 3 percentage points of households incurring catastrophic expenditure. The difference in size between matched and non-matched urban sample could indicate problems with the small sample size in the matched regression. RSBY however appears to have a negative effect.

5.3.3 Hospitalization rate

The effect on hospitalization rate has a similar trend as catastrophic morbidity expenditure; mainly urban households seem to be affected. Table 11 shows a marginal decrease in hospitalization rate of 0.6 percentage points for the rural households. The urban households have a 5.4 percentage points decrease in hospitalization rate. Looking at the overall population, the suggested impact of RSBY seems to reduce hospitalization rate by 1.5 percentage points. All coefficients are however not statistically significant. Looking at regressions without matching in Table A5, we find similar, but smaller coefficient sizes. The effect sizes on rural and overall population are just fractions of a percentage point while the effect on urban households is -1.3 percentage points.

5.3.4 Hospitalized days

In order to check if there was any impact of RSBY on the intensive margin of hospitalization, we run a regression conditional on anyone in the household being hospitalized at least once in the past year. Our outcome variable in this case is log(hospitalized days) as we need to address the skewed distribution of the variable. As the sample size is limited for this regression, we mainly look at the table without matching.

In Table 12 we note a large difference between rural and urban households when examining log of days hospitalized. There's a 6% increase in hospital days for rural households, which is not statistically significant considering the standard error of 22%. For the urban households there's however a large increase in hospital days of 81%, statistically significant on the 10% level. This may seem like an abnormal impact of RSBY, but when examining the data closer we note that the bulk part of this coefficent comes from households with less than 5 days hospitalized. In regression (3) in Table 12 we re-examine the urban sample, now only looking at households where one of the members stayed in the hospital 5 or more days. The coefficient is now only a quarter of its original size and half of its standard error. Thus, the effect of RSBY on urban households seems to adress patients spending few days at the hospital.

Hence, we have found no statistically significant evidence that RSBY impacted total household OOP expenditure on morbidity, likelihood of incurring catastrophic expenditure or hospitalization rates for Indian households. There is however suggestive evidence that RSBY reduced total OOP expenditure on morbidity with about 30% of the total morbidity expenditure for rural households. In the case of urban households there is negligible effect on OOP expenditure, but there is suggestive evidence on decreases in catastrophic expenditures and hospitalization. Given the small urban sample, and the discrepancy in coefficient size between the matched and non-matched data, determining the actual magnitude of the impact is hard. Regarding the impact on days spent hospitalized for those being hospitalized, we a large impact on urban households, significant at the 10% level, especially for household members staying less than 5 days in total. In the next section we move on to a discussion on possible reasons for our results.

Table 9: Impact analysis, DiDiD with C. E. Matching

Variable	Total morbidity expenditure		
Population	Rural	Urban	All
Y2012*BPL*Treat	-1,391	294.7	-1,199
	(1,192)	(4,114)	(1,292)
Treat	384.7	-2,308**	97.12
	(500.2)	(928.3)	(447.5)
Treat*BPL	83.11	2,308*	278.4
	(515.7)	(1,192)	(492.7)
Year 2012	1,336**	2,616	1,447**
	(541.1)	(3,539)	(707.9)
Y2012*Treat	771.6	-2,039	454.6
	(1,021)	(3,815)	(1,091)
BPL HH	-708.9	-1,775	-828.0*
	(441.5)	(1,320)	(452.7)
Y2012*BPL	144.2	-216.1	82.43
	(637.7)	(3,794)	(878.6)
Backward Caste HH	-139.7	-408.8	-204.2
	(257.7)	(669.4)	(262.3)
Any literate in HH	373.1	-2,993*	-21.94
	(338.7)	(1,769)	(370.8)
Urban HH			355.3
			(512.7)
Mean age in HH	64.99***	125.7**	75.28***
	(21.10)	(49.93)	(20.29)
HH yearly income	0.00360	0.0123	0.00563
	(0.00242)	(0.0109)	(0.00380)
# HH members	254.5***	325.9**	258.5***
	(64.26)	(132.6)	(58.65)
Years of education (HH head)	49.13	302.2**	87.52*
	(49.57)	(122.9)	(46.74)
Hinduist HH	-1,355	-381.1	-1,279*
	(859.5)	(1,351)	(741.0)
Muslim HH	-1,092	-851.7	-1,194
	(865.4)	(1,327)	(756.3)
Constant	-1,851	-2,115	-1,987
	(1,501)	(2,959)	(1,369)
Observations	13,423	2,773	16,196
R-squared	0.043	0.028	0.032
C. E. Matching	YES	YES	YES
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State

Table 10: Impact analysis, DiDiD with C. E. Matching

Variable	% households with catastrophic expenditure		
Population	Rural	Urban	All
Y2012*BPL*Treat	0.00947	-0.0780	-0.000810
	(0.0503)	(0.0574)	(0.0460)
Treat	0.0142	-0.0605*	0.00329
	(0.0239)	(0.0356)	(0.0225)
Treat*BPL	-0.00572	0.107***	0.00648
	(0.0275)	(0.0388)	(0.0261)
Year 2012	-0.0102	-0.115***	-0.0244
	(0.0264)	(0.0398)	(0.0251)
Y2012*Treat	-0.0449	0.0143	-0.0365
	(0.0402)	(0.0497)	(0.0372)
BPL HH	-0.0150	-0.0843***	-0.0248
	(0.0180)	(0.0259)	(0.0176)
Y2012*BPL	-0.00470	0.0883**	0.00671
	(0.0357)	(0.0413)	(0.0331)
Backward Caste HH	0.0110	0.0150	0.0138*
	(0.00898)	(0.0122)	(0.00802)
Any literate in HH	0.0133	-0.0145	0.00814
	(0.0161)	(0.0270)	(0.0146)
Urban HH			-0.00325
			(0.00894)
Mean age in HH	0.00194***	0.00295***	0.00199***
	(0.000346)	(0.000672)	(0.000318)
HH yearly income	1.57e-08	2.29e-08	8.14e-09
	(6.86e-08)	(8.05e-08)	(5.48e-08)
# HH members	0.0168***	0.0151***	0.0165***
	(0.00232)	(0.00365)	(0.00213)
Years of education (HH head)	-0.000424	0.00429**	0.000418
	(0.00171)	(0.00182)	(0.00150)
Hinduist HH	-0.0578	0.00161	-0.0545
	(0.0386)	(0.0394)	(0.0363)
Muslim HH	-0.0323	-0.00591	-0.0345
	(0.0388)	(0.0426)	(0.0363)
Constant	-0.0347	-0.0661	-0.0306
	(0.0439)	(0.0621)	(0.0431)
Observations	13,423	2,773	16,196
R-squared	0.054	0.073	0.053
C. E. Matching	YES	YES	YES
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State

Table 11: Impact analysis, DiDiD with C. E. Matching

Variable	Hospitalization rate		
Population	Rural	Urban	All
Y2012*BPL*Treat	-0.00641	-0.0543	-0.0143
	(0.0249)	(0.0492)	(0.0224)
Treat	0.0160	-0.0263	0.00988
	(0.0122)	(0.0266)	(0.0116)
Treat*BPL	0.00846	0.0287	0.0125
	(0.0143)	(0.0299)	(0.0126)
Year 2012	0.0406***	-0.0367	0.0326**
	(0.0148)	(0.0307)	(0.0137)
Y2012*Treat	0.00198	0.0581	0.00781
	(0.0212)	(0.0423)	(0.0196)
BPL HH	-0.00987	-0.0273	-0.0136
	(0.0101)	(0.0195)	(0.00870)
Y2012*BPL	-0.0111	0.0727**	0.00133
	(0.0168)	(0.0328)	(0.0148)
Backward Caste HH	0.00828	-0.00465	0.00649
	(0.00639)	(0.0119)	(0.00565)
Any literate in HH	0.00244	-0.0267	-0.000625
	(0.00742)	(0.0213)	(0.00710)
Urban HH			0.00855
			(0.00934)
Mean age in HH	0.00234***	0.00199***	0.00227***
	(0.000279)	(0.000618)	(0.000264)
HH yearly income	-2.48e-08	5.17e-08	-3.60e-10
	(3.36e-08)	(8.14e-08)	(3.47e-08)
# HH members	0.00949***	0.00798***	0.00932***
	(0.00132)	(0.00283)	(0.00120)
Years of education (HH head)	0.00223***	0.00136	0.00202***
	(0.000783)	(0.00166)	(0.000706)
Hinduist HH	-0.00845	0.0201	-0.00571
	(0.0138)	(0.0293)	(0.0127)
Muslim HH	0.000702	0.0295	0.00532
	(0.0170)	(0.0359)	(0.0152)
Constant	-0.0583***	-0.00549	-0.0519***
	(0.0186)	(0.0450)	(0.0181)
Observations	13,423	2,773	16,196
R-squared	0.035	0.039	0.033
C. E. Matching	YES	YES	YES
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State

Table 12: Impact analysis, DiDiD without C. E. Matching

Variable	Log of days hospitalized			
	(1)	(2)	(3)	(4)
Population	Rural All hospitalized	Urban All hospitalized	Urban Hospitalized ⩾ 5 days	All All hospitalized
Y2012*BPL*Treat	0.0631	0.814*	0.203	0.129
	(0.217)	(0.473)	(0.452)	(0.192)
Treat	-0.0545	0.711**	0.275	0.0283
	(0.163)	(0.299)	(0.298)	(0.150)
Treat*BPL	0.0496	-0.385	0.0720	0.0277
	(0.172)	(0.335)	(0.326)	(0.150)
Year 2012	-0.175	0.286	0.121	-0.134
	(0.136)	(0.322)	(0.299)	(0.130)
Y2012*Treat	-0.0410	-0.939**	-0.412	-0.141
	(0.175)	(0.424)	(0.412)	(0.165)
BPL HH	-0.117	0.00576	-0.0117	-0.172
	(0.129)	(0.241)	(0.233)	(0.114)
Y2012*BPL	0.0588	-0.426	-0.149	0.0307
	(0.163)	(0.346)	(0.354)	(0.145)
Backward Caste HH	-0.0419	0.0139	-0.00974	-0.0325
	(0.0562)	(0.102)	(0.0968)	(0.0495)
Any literate in HH	0.00826	-0.00503	-0.101	0.0167
	(0.111)	(0.206)	(0.213)	(0.0955)
Urban HH				0.0201
				(0.0572)
Mean age in HH	-0.000611	0.00988**	0.00159	0.0105***
	(0.00230)	(0.00422)	(0.00373)	(0.00217)
HH yearly income	1.53e-07	5.23e-08	-1.60e-07	8.23e-08
	(1.93e-07)	(3.23e-07)	(2.82e-07)	(1.58e-07)
# HH members	0.000328	0.0225	-0.00852	-0.154***
	(0.0113)	(0.0244)	(0.0219)	(0.0133)
Years of education (HH head)	-0.00142	0.00325	-0.000552	-0.00814
	(0.00900)	(0.0122)	(0.0125)	(0.00726)
Hinduist HH	-0.00963	0.0119	0.00466	-0.0127
	(0.112)	(0.235)	(0.113)	(0.103)
Muslim HH	0.0259	0.0380	-0.0839	0.0143
	(0.161)	(0.259)	(0.149)	(0.128)
Constant	2.200***	1.403***	2.613***	1.148***
	(0.200)	(0.429)	(0.416)	(0.193)
Observations	1,831	546	368	2,377
R-squared	0.049	0.100	0.071	0.230
C. E. Matching	YES	YES	YES	YES
Analysis	DiDiD	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State	State

6 Discussion

Having carried out the the regressions, we note the importance of looking at urban and rural households separately, as a simple dummy control variable on residing in an urban area does not account for all the differing effects between these two types of households. The effect of RSBY, captured by the coefficient of the triple interaction term $Y2012 * BPL * Treat$, is very different between urban and rural residents for all the different dependent variables. The main difference we find between rural and urban households is that rural households seem to utilize the program to decrease their overall morbidity expenditures, while urban households rather focus on utilizing the healthcare facilities an extra day, perhaps incurring supplemental medical expenditures that RSBY does not cover. There is suggestive evidence for this behavior as rural households presented a large decrease in OOP morbidity expenditures, about 30% of the average expenditure in our sample, while urban households presented no change in their OOP morbidity expenditures. The impact of RSBY on urban households, however, is that they spend more days at the healthcare facility if already hospitalized. This effect is statistically significant on the 10% level. There is only suggestive evidence that RSBY had an impact on morbidity expenditures for rural households and we find no statistically significant impacts on either catastrophic morbidity expenditures or hospitalization rates. There are a couple of possible reasons why this is the case.

Firstly, only a fraction of BPL households are enrolled in RSBY, resulting in a very diluted impact when evaluating the program within an “intention to treat” framework. Moreover, technical difficulties encountered by hospitals and insurance companies when implementing the scheme mean that a fraction of empanelled hospitals are not actually accessible to people insured by RSBY, further diluting its impact (Dasgupta et al., 2013).

Secondly, although hospitalization and surgical procedures might be the main cause of catastrophic expenditure as a single event, RSBY fails to target the main reason why people become impoverished. The majority of OOP health expenditures is actually on medicines, with a considerably higher share for outpatient (82%) relative to inpatient consultations (42%) (Shahrawat & Rao , 2012).

Furthermore, specialized treatments for cancer or cardiac conditions are unaffordable under the INR 30,000 covered by RSBY. This raises concern as they were the cause for 60% of the claims for insurance in the state of Karnataka under their state-run scheme Vajpayee Arogyasri (Sood et al., 2014).

Finally, although we utilize data from 2012, four years after the first district implemented RSBY, the scheme is still in its infancy in many of the districts in the sample. Only 224 out of India’s 624 districts had implemented RSBY before 2010 and therefore have at least 2 years of experience with the scheme. It may be the case that further research can be conducted on an even longer time frame to pick up the effects of RSBY.

Nevertheless, international experience and research has shown that health insurance schemes have the potential to improve health equity and protect the poor from financial risk. We argue that the lack of significance in our results is not a matter of failure of RSBY, but rather of possibilities of improvement. We move on to discuss policy implications in the next section.

6.1 Policy implications

In this section we discuss key improvement points of RSBY and more generally key issues in the successful implementation of health insurance schemes in low and middle income countries.

Firstly, we note that RSBY has very different effects on urban and rural households. This is important to keep in mind as a policy maker since the same goal for RSBY will likely not be met for both rural and urban households. One must thus tailor the scheme for urban and rural households separately depending on desired policy outcome. In the case of rural households, it seems that RSBY has had an impact in total morbidity expenditures, but no significant impact on utilization of hospitals. Policy makers should aim to make better quality health facilities more available for the poor in rural areas, or emphasize the coverage of transportation costs in the RSBY scheme so they can also benefit from an increase in hospitalization. In the case of urban households we find evidence that healthcare utilization has increased in terms of days spent hospitalized but no signficant effect was found on total morbidity expenditure in this group. It may be the case that complementary fees not covered by RSBY but correlated with increased hospitalization (e.g. expenditure on medicine) are offsetting the benefits of RSBY for urban households.

Expanding coverage is a key point of improvement for RSBY. Limited coverage is a recurring reason why researchers argue that no significant effects for RSBY are found in evaluation studies. Inpatient treatment has been found to be more costly and more likely to end up in catastrophic expenditure in comparison to outpatient costs in a single event. However, households spend a more substantial share of their income on outpatient costs long term (Shahrawat & Rao 2012). 60% of total OOP expenditure on health both in rural and urban areas is on medicines (CBHI of the Government of India, 2015). Coverage also needs to be expanded to tertiary care because diseases such as cancer or cardiac conditions are unaffordable under the INR 30,000 covered by RSBY.

The government can expand coverage by including households above poverty line in the scheme and introducing cross-subsidies. Households above the poverty line are likely to be less vulnerable to sickness than those below the poverty line, which decreases the average risk in the insured pool. Compulsory contributions could be introduced rather than voluntary enrollment because this will hinder the richer and healthier to opt out of the health insurance scheme and help the government expand its sources of financing. Cross-subsidies can help the government finance the scheme, for instance Ghana's national health insurance scheme introduced cross-subsidies between the richest and the poorest to help balance the financial burden (Escobar et al.,2010) .

Policy makers might have chosen to restrict coverage because a smaller set of procedures is easier to monitor. Hospitals and doctors may be tempted to overperform procedures or prescribe unnecessary medicines to maximize their kickbacks. A possible solution to mitigate these problems is to make payments contingent on disease conditions rather than procedures (Dasgupta et al., 2013). Then hospital and doctors may have an incentive to follow best treatment practices rather than overperform advanced surgeries. The RSBY scheme is actually being pilot-tested for outpatient coverage (Nandi et al. ,2015) and (Karan et al., 2015) and we look forward to see results from this experience.

Another key point of improvement is monitoring and incentive structures as the lack of rigorous monitoring and data collection is hindering quality control. Currently there are no quality standards within RSBY and empanelled hospitals have several problems with handling the smart card technology, poor quality of the information stored on the smart cards and settling claims (Dasgupta et al., 2013).

One possible solution is to harness the smart card technology to collect time trend data on enrolment rates, rates of attrition and re-enrolment, patient satisfaction, uptake and benefit utilization. This could provide policy makers and researchers with data directly collected from the scheme rather than having to rely on sporadic reports and surveys for data analysis. Moreover, it would allow the Indian Government to reward hospitals providing better quality services by paying a higher package rate by the insurer.

6.2 Suggestions for further research

The current literature predominantly focuses on the effects of health insurance on financial protection rather than health outcomes and analysis based on surveys and observational data. They also often neglect that insurance policies can have very different effects for urban and rural residents, which is apparent in our research.

Given that previous studies by both Johnson & Krishnaswamy (2012) and Karan et al. (2015) have neglected to study differences between the rural and urban sub-sample, an analysis of the impact of RSBY on inpatient and outpatient morbidity expenditures separately is an interesting topic for future research. Given that we see increased healthcare usage and no decrease in healthcare expenditure for the urban sub-sample, it is especially interesting to evaluate if urban households incur additional outpatient expenditures due to the RSBY scheme.

Measuring effects of health insurance on variables such as population height, average mortality and morbidity presents a challenge because these variables respond in the long term to changes in health policy. With longer term data there are new confounders that must be addressed such as the introduction of other policies and attrition problems. Nevertheless the question of whether RSBY has positive effects on health outcomes deserves further research.

Randomized controlled trials (RCTs) may provide a way forward for RSBY research. There have been randomized controlled trials addressing the effect of insurance on health seeking behavior and health outcomes such as the RAND experiment in the United States (Newhouse & RAND Corporation, 1993) and the Seguro Popular in Mexico (King et al., 2009), but they are rare because of their high costs. University of Chicago's Anup Malani and his team are the first to have gathered resources and government support to run randomized trials on RSBY and we look forward to their results.

Expanding the evaluation of RSBY to take into account spillover effects of health insurance is another interesting derivation of our research. If the objective of RSBY is to reach people that would otherwise not have access to health care or even awareness, impacts on their behavior could have spillover effects on their families and neighbors. Understanding these effects could help RSBY policy makers to reach more isolated communities and tribes adverse to the idea of alopathic medicine.

Finally, authors have raised concerns about inadequate budget allocation to RSBY over the years (Nandi et al., 2015). Even if districts improve the implementation of the scheme over the years, budgetary constraints may be stopping them from expanding. An analysis of the financing strategy of RSBY, data on claims settled may provide insight into financial bottlenecks of the scheme.

7 Conclusion

RSBY is one of the largest schemes in the world, covering over 40 million households in India. The strength of the program is its use of technology with smart cards and cashless transactions, proactive outreach to BPL population and use of public-private partnerships. Unfortunately, its shortcomings related to lack of quality control and governance mechanisms, insufficient budget allocation and low enrollment ratios seem to be hindering its impact from scaling.

Our impact analysis has found suggestive evidence that RSBY reduced total morbidity out-of-pocket expenditure for rural households, however these results were not statistically significant. We have found no statistically significant evidence of the impact of RSBY on likelihood of catastrophic expenditure and hospitalization rates. The OOP expenditure for urban households has not been impacted significantly by RSBY, but they seem to be spending an incremental day at a healthcare facility if hospitalized. This suggests that rural households utilizes RSBY in order to decrease their healthcare spending, while urban households rather increase their healthcare usage which in turn might incur supplementary OOP expenditures such as medicines that RSBY does not cover. This might be the reason for the lack of change in OOP expenditures for the urban population, a question that further research can answer.

We have identified three key points of improvement: tailoring the scheme for urban and rural households separately depending on desired policy outcome, as the program has differing effects on the two groups. If one wants to decrease morbidity expenditures, RSBY mainly works for rural households, while increasing healthcare utilization only works for urban households. One should also consider to expand the coverage to medicines as well as improving the monitoring and quality control mechanisms of the scheme. Our policy recommendations are directed towards to RSBY but can be easily extrapolated to health insurance schemes in other low and middle income countries.

Finally, interesting extensions on our research are those related to questions on financing of the scheme and randomized controlled trials to assess impact.

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Appendix

Table A1: Comparing treated and untreated districts, 2005

Variable	C E Matched sample Untreated Mean	 Treated Mean	Pearson chi2 p-value
Any literate in HH (%)	80.0	80.9	0.091
Any HH member disabled in past year (%)	19.3	17.7	0.002
BPL HH (%)	22.6	24.3	0.003
# HH members	5.36	5.36	0.9797
Hinduist HH (%)	77.4	81.2	0.000
Years of education (HH head)	7.74	7.80	0.000
Urban HH (%)	35.1	32.5	0.000
			T-test, p-value
HH morbidity expenditures (INR)	1735	1700	0.7745
HH monthly consumption per capita (INR)	1020	1001	0.1653
HH total yearly income (INR)	59041	56822	0.0474
Mean age in HH	29.02	28.81	0.1721

Table A2: Total number of districts covered under the RSBY by Year

Participating States	All districts[1]	Until July 2010[2]	Until July 2012[2]	Until May 2016[2]
Andhra Pradesh	23	0	0	0
Assam	26	3	5	23
Bihar	38	13	30	38
Chattisgarh	16	16	18	27
Delhi	7	1	1	0
Goa	2	2	2	2
Gujarat	26	10	26	26
Haryana	21	20	20	21
Himachal Pradesh	12	2	12	12
Jammu & Kashmir	14	0	0	0
Jharkhand	24	7	21	24
Karnataka	30	0	30	30
Kerala	14	14	14	14
Madhya Pradesh	48	0	0	0
Maharashtra	34	27	23	-
Orissa	30	6	9	30
Punjab	22	19	17	22
Rajasthan	32	4	0	33
Tamilnadu	30	2	2	0
Uttar Pradesh	75	63	71	75
Uttarakhand	15	2	13	13
West Bengal	19	4	12	20
Other NE states	**54**	**8**	**38**	**52**
Arunachal Pradesh	-	0	9	0
Manipur	-	0	1	6
Meghalaya	-	1	2	11
Mizoram	-	0	8	8
Nagaland	-	3	7	0
Tripura	-	3	2	8
UTs	**12**	**1**	**1**	**1**
Andaman & Nicobar	-	0	0	
Chandigarh	-	1	1	0
Lakshadweep	-	0	0	
Pondicherry	-	0	0	0
All districts	624	224	365	463

[1] From Karan et al., (2015)

[2] Indian Ministry of Health, (2016)

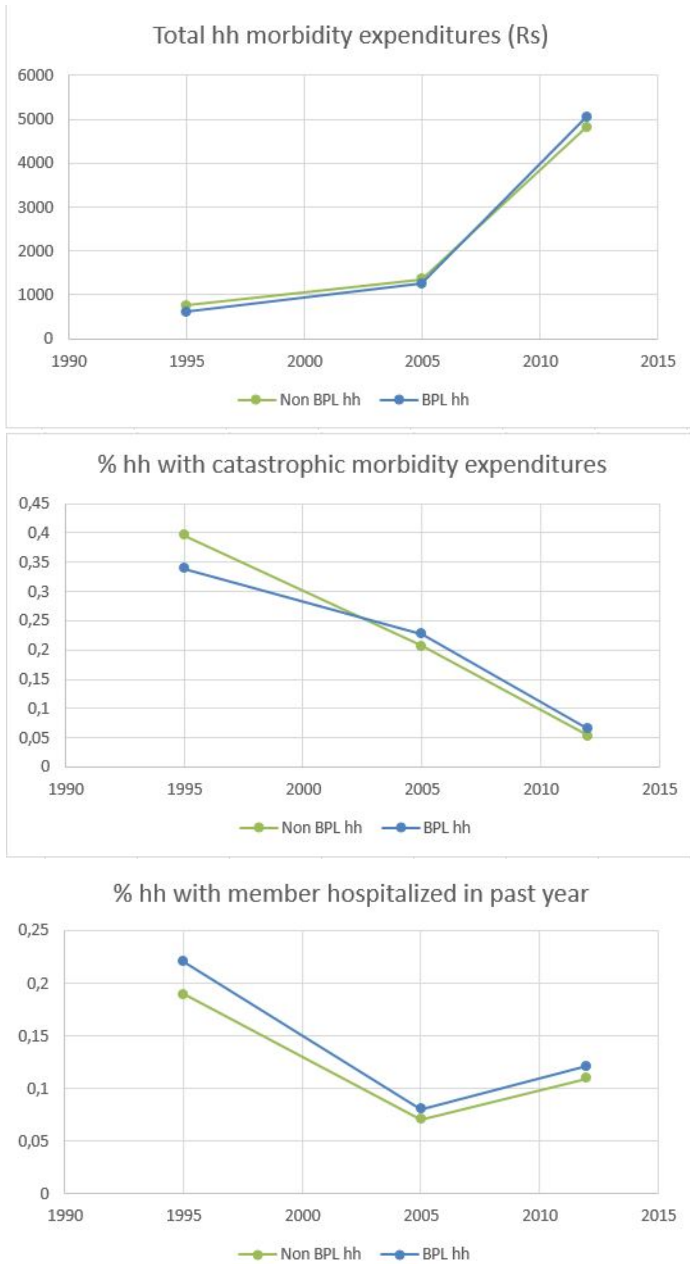

Figure 3: Comparing BPL and APL households in treated districts

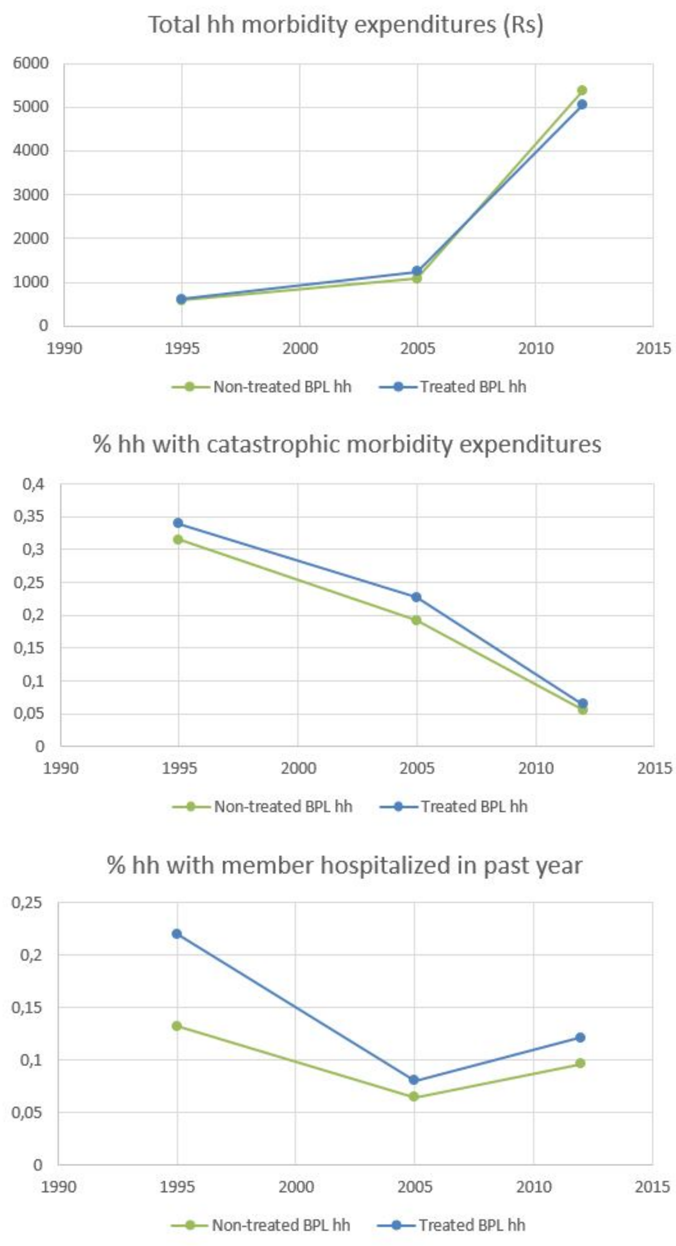

Figure 4: Comparing treated and non-treated BPL households

Table A3: Impact analysis, DiDiD without C. E. Matching

Variable	Total morbidity expenditure		
Population	Rural	Urban	All
Y2012*BPL*Treat	-1,119	1,321	-679.8
	(843.7)	(1,663)	(813.5)
Treat	-116.0	-996.0*	-210.0
	(396.1)	(596.0)	(363.0)
Treat*BPL	329.6	624.9	317.0
	(361.7)	(656.8)	(326.4)
Year 2012	1,528***	2,242	1,683***
	(407.4)	(1,379)	(441.3)
Y2012*Treat	1,121	-609.7	847.3
	(804.9)	(1,500)	(768.2)
BPL HH	-678.2**	-798.4	-680.6**
	(301.1)	(547.8)	(273.2)
Y2012*BPL	198.0	-638.8	-6.664
	(469.7)	(1,467)	(499.4)
Backward Caste HH	-342.5*	-234.1	-376.8**
	(201.3)	(337.1)	(189.2)
Any literate in HH	201.1	-123.6	122.7
	(253.3)	(493.9)	(230.2)
Urban HH			159.7
			(260.4)
Mean age in HH	63.54***	77.06***	65.68***
	(16.08)	(14.19)	(14.17)
HH yearly income	0.00163	0.00432	0.00187
	(0.00158)	(0.00534)	(0.00176)
# HH members	349.7***	317.8***	342.8***
	(74.96)	(73.89)	(66.07)
Years of education (HH head)	75.49**	133.8**	90.18***
	(36.53)	(57.91)	(32.39)
Hinduist HH	-933.1	696.0	-698.2
	(587.2)	(600.9)	(514.0)
Muslim HH	-456.5	766.8	-369.3
	(634.7)	(716.4)	(548.2)
Constant	-1,496	-3,139***	-1,695**
	(939.4)	(1,087)	(827.4)
Observations	21,041	5,130	26,171
R-squared	0.040	0.041	0.038
C. E. Matching	NO	NO	NO
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State

Table A4: Impact analysis, DiDiD without C. E. Matching

Variable	% households with catastrophic expenditure		
Population	Rural	Urban	All
Y2012*BPL*Treat	-0.00529	-0.0302	-0.00776
	(0.0363)	(0.0590)	(0.0327)
Treat	0.00182	-0.0484	-0.00407
	(0.0215)	(0.0386)	(0.0197)
Treat*BPL	0.00551	0.0652	0.0119
	(0.0223)	(0.0428)	(0.0205)
Year 2012	-0.00590	-0.0690	-0.0133
	(0.0199)	(0.0474)	(0.0185)
Y2012*Treat	0.00153	0.00225	0.00132
	(0.0308)	(0.0541)	(0.0282)
BPL HH	-0.0184	-0.0501	-0.0222*
	(0.0139)	(0.0354)	(0.0128)
Y2012*BPL	0.0105	0.0560	0.0147
	(0.0252)	(0.0489)	(0.0229)
Backward Caste HH	-0.00195	0.00826	-0.000226
	(0.00779)	(0.0118)	(0.00690)
Any literate in HH	0.0104	-0.0172	0.00731
	(0.0121)	(0.0210)	(0.0107)
Urban HH			-0.00933
			(0.00752)
Mean age in HH	0.00205***	0.00307***	0.00215***
	(0.000303)	(0.000519)	(0.000274)
HH yearly income	-1.26e-08	-1.92e-07**	-4.71e-08
	(5.35e-08)	(8.46e-08)	(4.44e-08)
# HH members	0.0179***	0.0180***	0.0178***
	(0.00195)	(0.00307)	(0.00176)
Years of education (HH head)	0.00123	0.00464***	0.00179
	(0.00128)	(0.00159)	(0.00110)
Hinduist HH	-0.0350	0.0196	-0.0291
	(0.0252)	(0.0237)	(0.0224)
Muslim HH	-0.00266	0.0189	-0.00405
	(0.0275)	(0.0250)	(0.0239)
Constant	0.00956	-0.0355	0.00701
	(0.0277)	(0.0503)	(0.0256)
Observations	21,041	5,130	26,171
R-squared	0.039	0.044	0.038
C. E. Matching	NO	NO	NO
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State

Table A5: Impact analysis, DiDiD without C. E. Matching

Variable	Hospitalization rate		
Population	Rural	Urban	All
Y2012*BPL*Treat	0.000111	-0.0134	0.00258
	(0.0185)	(0.0378)	(0.0169)
Treat	0.0130	-0.0186	0.00795
	(0.00977)	(0.0235)	(0.00956)
Treat*BPL	0.00693	0.0229	0.00710
	(0.0108)	(0.0247)	(0.00956)
Year 2012	0.0315***	-0.0221	0.0256**
	(0.0114)	(0.0269)	(0.0109)
Y2012*Treat	0.00233	0.0634*	0.00892
	(0.0162)	(0.0347)	(0.0151)
BPL HH	-0.00991	-0.0228	-0.0113*
	(0.00741)	(0.0163)	(0.00655)
Y2012*BPL	-0.00337	0.0398	0.00181
	(0.0123)	(0.0284)	(0.0112)
Backward Caste HH	-0.00139	-0.00460	-0.00186
	(0.00514)	(0.00941)	(0.00454)
Any literate in HH	0.00480	0.0148	0.00809
	(0.00604)	(0.0162)	(0.00567)
Urban HH			0.00794
			(0.00635)
Mean age in HH	0.00215***	0.00249***	0.00220***
	(0.000204)	(0.000456)	(0.000193)
HH yearly income	1.42e-08	3.43e-08	2.02e-08
	(4.12e-08)	(6.81e-08)	(3.52e-08)
# HH members	0.0108***	0.00942***	0.0105***
	(0.00109)	(0.00211)	(0.000973)
Years of education (HH head)	0.00206***	-0.000190	0.00155***
	(0.000610)	(0.00124)	(0.000541)
Hinduist HH	-0.0118	0.0277	-0.00687
	(0.0105)	(0.0197)	(0.00951)
Muslim HH	-0.00576	0.0323	0.000882
	(0.0132)	(0.0251)	(0.0119)
Constant	-0.0539***	-0.0559*	-0.0548***
	(0.0139)	(0.0333)	(0.0135)
Observations	21,041	5,130	26,171
R-squared	0.032	0.037	0.031
C. E. Matching	NO	NO	NO
Analysis	DiDiD	DiDiD	DiDiD
Fixed effects	State	State	State