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Bank Presence and Health

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This paper demonstrates that increasing bank presence in underserved areas can substantially improve households' health. I apply a regression discontinuity design to a policy of the Reserve Bank of India. Six years after the policy introduction, treatment districts have 19% more branches than control districts. Households' probability of suffering from a non-chronic disease in a given month is 36% lower. I show evidence that two understudied aspects of banking play a role: banks provide health insurance to households and credit to hospitals. In equilibrium, I observe an increase in health care demand and supply.

Keywords: Financial Development, Banks, Health, Insurance, Credit
JEL Codes: G21, O16, I15

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How to improve households' health is a critical question in developing countries. In a world where resources are scarce, the financial sector could play a key role in addressing this challenge: it distributes resources across time and space. Whether finance can move the needle on health has been primarily explored through randomized controlled trials. Surprisingly, these studies consistently find null results for savings accounts, credit, and health insurance (Karlan and Zinman (2010); Dupas and Robinson (2013b); Banerjee et al. (2015a); Haushofer et al. (2020)). In this paper, I exploit a natural experiment that introduces exogenous variation in bank presence. This allows me to examine the effect of finance on health on a large scale, over a long time frame, and in general equilibrium. In contrast to many previous studies, I identify substantial effects of finance on health.

To obtain exogenous variation in bank presence, I use a policy of the Reserve Bank of India (RBI) from 2005. This policy incentivizes banks to open branches in underbanked districts. The definition of underbanked districts allows for a regression discontinuity design: districts must have a population-to-branch ratio above the national average. This enables me to identify the causal effect by comparing districts just above and just below the national average. I combine the policy with granular datasets. Using district-level branch data, I examine whether banks react to the policy. I then investigate the impact on health using two nationally representative household-level surveys: the Indian Human Development Survey (IHDS) six years after the policy introduction and the Demographics and Health Survey (DHS) ten years after. Having established the causal effect of bank presence on health, I explore potential mechanisms. For this purpose, I supplement my data with the Economic Census, which allows me to examine the effect on the health care sector.

Initially, I demonstrate that banks indeed react to the policy. Before the policy, the number of branch licenses issued by the RBI and the number of branches are smooth around the policy's cutoff. One year after the policy's implementation, significantly more licenses are issued for treatment districts just above the cutoff than for control districts just below the cutoff. After one more year, there are significantly more branches in treatment districts. The discontinuities in the licenses and branches increase over subsequent years. Five years after the policy introduction, treatment districts have 21 percent more licenses and 19 percent more bank branches than control districts, which amounts to 1.32 additional branches (up from 6.99 in the control group) per 100k people. Pre-policy smoothness, post-policy discontinuities, and the exact correspondence of the license and branch dynamics to the policy timing provide confidence that the policy induces exogenous variation in bank presence.

Households' health substantially improves with expanded bank presence. Six years after the policy introduction, households' probability of suffering from a non-chronic illness such as fever or diarrhea in a given month is 36 percent lower in treatment districts. The decrease in morbidity rates positively affects health-related economic outcomes of households. They miss half a day of work or school less per month due to an illness and incur significantly lower medical expenses. Using the second survey, conducted ten years after the policy introduction, I replicate my finding on morbidity rates and demonstrate that health also improves along other dimensions. Households in treatment districts have higher vaccination rates and lower risks associated with pregnancies. As the probability of institutional delivery increases, the likelihood of miscarriages and stillbirths decreases.

I provide extensive evidence to reject potential threats to causal identification. First, I show that local governments do not manipulate their treatment status. By construction, manipulation of the population-to-branch ratio is unlikely. In the numerator, the total population is historical data from the 2001 Population Census. In the denominator, the total number of branches is the sum of individual decisions of all banks in a district. Additionally, banks directly report their number of branches to the RBI. Indeed, I find no evidence that more districts are located just above than just below the cutoff. Nor is there any evidence that districts just above and just below the cutoff significantly differ before the policy. To demonstrate this, I utilize data from pre-policy rounds of the IHDS, the Economic Census, and the Population Census, as well as night-light data. There is also no threat to identification due to migration, which is negligible. Finally, no policies use an identical cutoff or are significantly more likely to be implemented in treatment districts. Results are robust to different bandwidths and polynomials, and there is little evidence of discontinuities at placebo cutoffs. These tests provide confidence in the causal interpretation of my findings.

After establishing the causal effect of bank presence on health, I explore potential mechanisms: How exactly does bank presence affect health? Quantifying or causally identifying mechanisms requires isolated exogenous variation in the respective channel and is thus beyond the scope of this study. Instead, I observe outcomes in equilibrium that are suggestive of mechanisms. To guide this analysis, I pose a framework in Figure 1 with three mechanisms. First, banks might interact with local businesses. If they offer credit and stimulate business activity, this could increase households' income and boost their spending on health. Second, banks might directly interact with households. An important contribution of this study is that I show that banks not only offer savings accounts and loans: banks offer health insurance products at the local branch. This phenomenon is not unique to India; banks offer health insurance in over half of developing countries. They act as in-

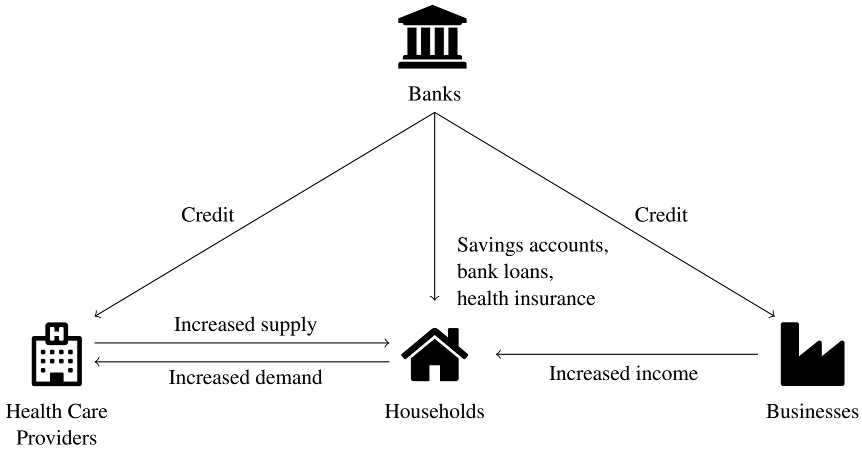


Figure 1. The Potential Role of Bank Presence in Improving Households' Health.

termediaries between insurance companies in big cities and households, potentially alleviating issues of physical distance and information asymmetries. The first and the second mechanism could improve health by stimulating health care demand. Finally, banks might directly interact with hospitals. If they relax credit constraints, this could improve health care supply. Crucially, there could be interactions between these channels. For instance, health insurance demand might increase as households observe that health care supply improves because hospitals can access credit. Any partial equilibrium study would miss these interactions.

I find empirical evidence consistent with all three mechanisms. In alignment with the first mechanism of increased incomes, I observe that six years after the policy's implementation, households in treatment districts have 8 percent higher total consumption than those in control districts. Households also increase their food consumption; they eat a quarter of a meal more per day. Consistent with the second mechanism, households are 19 percentage points more likely to have a savings account and 17 percentage points more likely to own health insurance. The coefficient on bank loans is insignificant; this financial instrument does not seem to play a role for the average household. Finally, speaking to the third mechanism of banks interacting with hospitals, I find that eight years after the policy, hospitals in treatment districts are 68 percent more likely to report loans as their primary source of finance. There are 140 percent more hospitals in a district, and households are less likely to complain about health care supply in urban areas.

This paper has two main contributions. First, I examine the effect of bank presence on health. Exploring this relationship allows me to speak to the literature on finance and health in developing countries. This literature has primarily focused on randomized controlled trials, which distribute financial instruments to households. These studies consistently find null results on health for savings accounts (Ashraf et al., 2006; Dupas and Robinson, 2013a,b; Prina, 2015; Dupas et al., 2018; Schaner, 2018), bank credit (Karlan and Zinman, 2010), microcredit (Beaman et al., 2014; Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al., 2015; Banerjee et al., 2015b; Crépon et al., 2015; Tarozzi et al., 2015; Banerjee et al., 2019), and health insurance (King et al., 2009; Levine et al., 2016; Haushofer et al., 2020; Malani et al., 2021). Two exceptions are Lin and Yi (2021), examining health insurance in China, and Gruber et al. (2014), investigating a public health care reform in Thailand. These papers use natural experiments and find impacts on health. In line with this work, I use a natural experiment that enables me to examine the effect of finance on a large scale, over a long time frame, and in general equilibrium.

Furthermore, studying the effect of bank presence on health allows me to contribute to the literature that explores the impact of bank presence or other forms of financial development on outcomes different from health. These studies investigate households' financial situations and have demonstrated positive effects on income, consumption, poverty, and financial resilience (Burgess and Pande, 2005; Burgess et al., 2005; Bruhn and Love, 2014; Jack and Suri, 2014; Suri and Jack, 2016; Bharadwaj et al., 2019; Brown et al., 2019; Célerier and Matray, 2019; Barboni et al., 2021; Breza and Kinnan, 2021). One might raise the question of whether we can simply extrapolate from these studies to improvements in health, with an income effect driving my findings. Previous evidence suggests that income alone is no silver bullet for health. Cash transfer studies that induce an increase in total consumption three times as large as in my paper find no effect on health in the short or long term (Haushofer and Shapiro, 2013, 2018; Egger et al., 2019). What are the potential reasons for this? As households become richer, they might not significantly spend more on health due to non-monetary transaction costs, behavioral biases, or a lack of information (Dupas and Miguel, 2017). Even if they spend more on health, this might not translate into health improvements if the health care supply remains scarce. A gradual increase in demand might not stimulate an increase in supply if there are high fixed costs to investments. Taken together, this suggests that we need to evaluate the impact on health individually.

The second main contribution of this paper is that it highlights two understudied functions of banks: they provide health insurance to households and credit to hospitals. Previous studies have focused on traditional banking activities such as offering business credit, savings accounts, and household loans. To the best of my knowledge, we do not have any previous evidence that banks offer health insurance. That banks offer credit to hospitals has recently been addressed by a contemporaneous paper in the U.S. (Aghamolla et al., 2021). Despite extensive documentation of credit constraints for general businesses (de Mel et al., 2008; Banerjee and Duflo, 2014), this relationship has not been explored in developing countries.

Finally, this paper connects to the literature on financial development and economic growth. Moving from cross-country studies (Goldsmith, 1969; King and Levine, 1993) to industry- and firm-level studies (Rajan and Zingales, 1996; Beck et al., 2005), this literature has now largely established that the development of the financial sector can positively influence economic growth. Empirical studies have mainly focused on the mechanism of increased access to credit for businesses (Levine, 2005). This paper contributes evidence of another potential mechanism: financial development improves citizens' health status, resulting in increased labor supply and school attendance. Both of these aspects are not only beneficial for the household itself but may also positively influence economic growth on an aggregate level.

My findings have important implications for policy and future research. Policymakers can conclude that incentivizing bank presence can benefit households' health. Since one mechanism appears to be the interaction between financial service providers and health care providers, policymakers might want to concentrate on this relationship. This is indeed what the RBI did in light of the COVID-19 crisis. The reserve bank announced a policy in May 2021 to incentivize banks to quickly deliver credit to health care providers, injecting USD 6.78 billion of liquidity. This paper also speaks to researchers, suggesting promising new areas of interest. One open question is to what extent the different mechanisms contribute to improving health. Answering this requires exogenous variation in the respective channels, for instance, credit access to health care providers only. A second line of inquiry is whether other dimensions of wellbeing, such as education, show a positive impact when evaluated in a context of a natural experiment. For instance, like health care providers, providers of educational services might benefit from credit access. Gaining an understanding of these questions could significantly advance our knowledge of the impact of finance and the scope of policymakers to improve their citizens' wellbeing.

I. Policy

I use a policy the Reserve Bank of India introduced in 2005 to incentivize banks to open new branches in underserved locations. The policy is still in effect and states that banks can increase their chance of obtaining licenses for branches in favored locations by strengthening their branch presence in underbanked districts. RBI's definition of an underbanked district is crucial for identification in this study: they are districts with a population-to-branch ratio that exceeds the national average. In 2006, the RBI published a list of underbanked districts to assist banks in identifying them. District-level ratios are not included in this document, so I reconstruct them as described in Section II. The list of underbanked districts has remained constant since its release; the RBI has not adjusted the list to account for changes in the ratio. Thus, for this study, I employ the cross-sectional variation in the district-level population-to-branch ratio in 2006. In 2010, the RBI adapted its policy to allow branch openings without licenses in eight of the 35 states or union territories that were particularly disadvantaged. I do not exploit this variation for identification, but it reflects in the dynamic patterns of my analysis on bank presence.

$$\underbrace{\frac{\text{Population}_{District}}{\text{\# Bank Branches}_{District}}}_{\text{Underbanked/Treated}} > \frac{\text{Population}_{National}}{\text{\# Bank Branches}_{National}} \tag{1}$$

Figure 2 depicts all 593 districts as of the 2001 Census. Marked in dark blue are the 375 districts that are defined as underbanked according to the reconstructed district-level ratio in 2006.

To my knowledge, this is the first paper that combines the 2005 RBI policy with household-level data. Other authors that use this policy, such as Young (2017), focus on aggregate outcomes and different questions, such as whether bank presence fosters economic activity. A similar branch licensing policy was in place between 1977 and 1990. Burgess and Pande (2005) use that policy in their seminal paper on the impact of bank presence on poverty, employing an instrumental variable strategy. The authors focus on state-level measures of poverty, and exactly how it is reduced remains in a black box. Here, I provide empirical evidence of one potential mechanism for poverty reduction: improved health. From 1990 through 2005, no comparable branch licensing policy was in place.

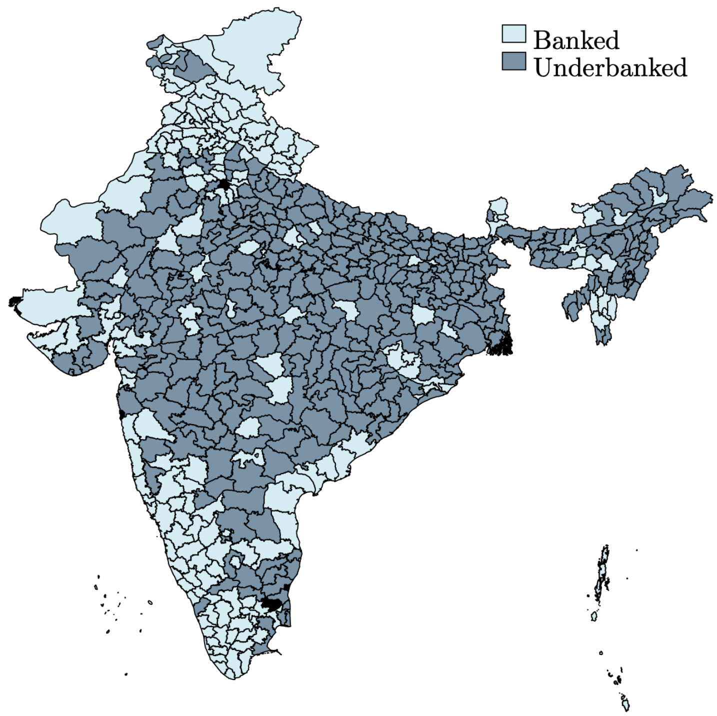


Figure 2. Banked and Underbanked Districts. District borders refer to the 2001 Census.

II. Data

Initially, I reconstruct the policy's population-to-branch ratio. To measure the population of each district, I rely on the 2001 Population Census (ORGCC, 2008). To measure the district-level number of branches in the denominator, I use an official annual publication of the RBI, the Bank Branch Statistics (RBI, 2018a). I use data from the first quarter of 2006 since the final list of underbanked districts was issued in July of that year. To measure how banks reacted to the policy, I use a second district-level branch data set: the Master Office File (RBI, 2018b). This file is dynamically updated over time to reflect changes in district borders, which means that when I trace back data to the 2001 Census borders that are used for the policy, I lose accuracy. Thus, I do not use the Master Office File to construct the ratio. The main advantage of this data is that it allows me to study the reaction of different bank types separately. One specific bank type, regional rural banks, is excluded from the policy; correspondingly, I also exclude this bank type from my variables of interest. Instead, I utilize regional rural banks to conduct placebo tests. For the years 1997 to 2016, I obtain two variables for all other bank types: the number of branch licenses and the number of branches. Using this data from 1997 to 2004, I test for pre-policy smoothness in bank licenses and branches around the policy cutoff. Data from 2005 to 2016 allows me to examine the respective discontinuities after the policy. I do not use data from after 2016, the year the final household-level survey was conducted. General summary statistics from the Master Office File are provided in Table A1.

To examine the effect of bank presence on health, I use two nationally representative household surveys. The first is the Indian Human Development Survey (IHDS). This panel survey was conducted in 2004/2005 (IHDS I), shortly before the policy, and again six years after the policy in 2011/2012 (IHDS II) (see Figure 3) (Desai and Vanneman, 2018a,b). The pre-policy round allows me to test for smoothness of household characteristics around the cutoff. The post-policy round provides the main outcome variables. Importantly, the survey not only contains information on health, but also provides a holistic picture of the households' economic situation. With this data, I can test, for instance, how many days of work or school households missed due to illness, or whether they hold various financial instruments. The first survey round was conducted in 64 percent of districts, and the second in 65 percent. Figure A1 depicts districts covered in the second survey round, distinguishing between the 218 underbanked and 166 banked districts. Both survey rounds cover all states and union territories of India except Lakshadweep, and Andaman and Nicobar Islands. The survey was not more likely to be conducted in underbanked districts (see Discussion A1). In the first survey round, 41,554 households were interviewed. In the second round, 83% of the original households plus replacement households were interviewed. This attrition does not represent a threat to identification, as I rely on comparing households in treatment and control districts in the second survey round. General summary statistics of the IHDS are described in Table A2. I also provide evidence on the external validity of my design; households in districts with a ratio in a range of $\pm 3,000$ of the policy cutoff are very similar to all households in the sample along dimensions of consumption, financial access, and health.

I complement the IHDS with a second nationally representative household-level survey, the Demographics and Health Program (DHS), conducted in 2015 and 2016, ten years after the policy (see Figure 3) (IIPS and ICF, 2017). In contrast to the IHDS, the DHS primarily focuses on health. The advantage of this survey is that it has a very large sample size, which allows me to capture the effects on low-probability events such as miscarriages. The DHS was conducted in all districts and interviewed 601,509 households. The previous round of this survey, conducted in 2005 and 2006, does not contain district-level identifiers. Consequently, I do not include that survey round in my analysis. General summary statistics for the DHS are provided in Table A3.

To examine the mechanisms driving the relationship between bank presence and health, it becomes crucial to understand the response of health care supply. For this purpose, I primarily rely on the Economic Census, from which I obtain measures of the number of hospitals, other medical service providers, and general businesses, as well as information about the major source of finance for these establishments. I focus on two census rounds; the first was conducted in 2005 and the second in 2013 (see Figure 3) (CSO and MOSPI, 2018a,b). The first Economic Census round allows me to test for smoothness around the cutoff in the respective variables pre-policy. The second round provides outcome variables. General summary statistics are provided in Table A4. To generally gain a better understanding of the health care sector, I investigate summary statistics from the Prowess database, which provides financial statements for companies of all sizes, including those conducting health services (CMIE, 2020). While providing more detailed financial information than the Economic Census, the Prowess database only contains a selective sample of health care providers for a limited number of districts. For my regression analysis, I thus concentrate on the Economic Census.

To provide further evidence on pre-policy smoothness along other dimensions, including economic activity and population characteristics, I utilize the Socioeconomic High-Resolution Rural-Urban Geographic Data Platform (SHRUG) (Henderson et al., 2011; Asher and Novosad, 2019; Asher et al., 2021). This platform combines multiple data sources on the village or town level. Economic activity is proxied by night-light data, economic employment, and road connection. Population characteristics include total population, rural and urban population, and literate population.

A final point to note is that India's district borders are very dynamic. While the 2001 Census contains 593 districts, the 2011 Census contains 640 districts (ORGCC, 2014). The RBI policy refers to the 2001 district borders. In contrast, most data sources I use are adjusted for any changes in district borders at the respective time of publication. To analyze treatment effects for districts as defined by the policy, I trace all data back to the 2001 Census borders. The main source for this is the 2011 Census.

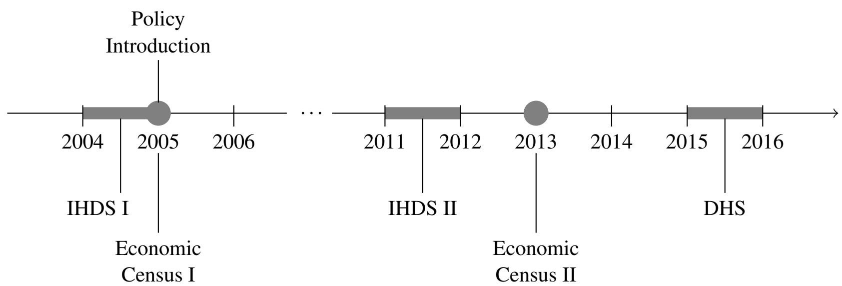


Figure 3. Timeline. The following graphic depicts a timeline of this study, with the three main data sets used (IHDS, DHS, Economic Census).

III. Identification Strategy

A. *Regression Discontinuity Design*

The design of the RBI policy allows for a regression discontinuity analysis. The district-level population-to-branch ratio is the running variable, and the national average ratio is the cutoff. Districts with a ratio above the national average are defined as underbanked or treated, while those with a ratio below the national average are defined as banked or control. Figure 4(a) depicts the histogram of the district-level ratio. The vertical line indicates the national average of the ratio: 14,780. The regression discontinuity analysis concentrates on observations within an optimal bandwidth. While this optimal bandwidth depends on the specific outcome variable (Cattaneo and Vazquez-Bare, 2017), districts included are mostly within a range of $\pm 3,000$ relative to the cutoff. This range is indicated by the blue bar on the x-axis in Figure 4(a). Figure A2 provides a map of districts in this range. As discussed below, for the identification assumption to hold, there should be no perfect manipulation around the cutoff, one implication of which is that there are approximately the same number of districts just above and just below the cutoff. At first glance, the histogram does not appear to show more districts just above the cutoff than just below. I test this formally using the McCrary (2008) density test.

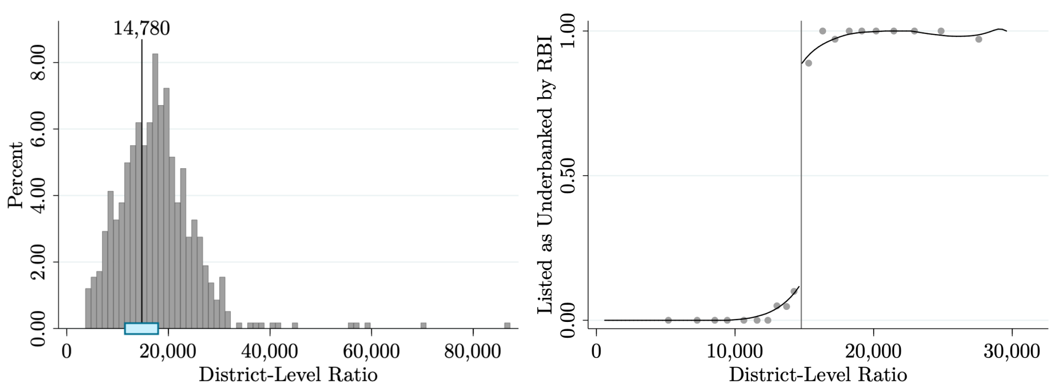
While I do not perfectly predict which districts are listed as underbanked by the RBI, there are only a few districts, 10 out of 593, that have a different status than predicted.[1] Figure 4(b) shows that when a district's ratio crosses the national average, there is a large jump in the probability that it is listed as underbanked. Consequently, I implement a fuzzy regression discontinuity design with a strong first stage. I use the following specification for household-level regressions. Regressions on more aggregated levels, such as the district level, exactly mirror the household-level regressions but with higher-level indices.

$$\begin{aligned}\text{Underbanked}_{d,s} = \alpha_0 + \alpha_1 \text{Above}_{d,s} + \alpha_2 \text{DistRatio}_{d,s} \\ + \alpha_3 \text{DistRatio}_{d,s} \text{Above}_{d,s} + \lambda X_{d,s} + \mu_s + \upsilon_{d,s}\end{aligned} \tag{2}$$

$$\begin{aligned}\text{y}_{h,d,s} = \beta_0 + \beta_1 \text{Underbanked}_{d,s} + \beta_2 \text{DistRatio}_{d,s} \\ + \beta_3 \text{DistRatio}_{d,s} \text{Above}_{d,s} + \gamma X_{d,s} + \eta_s + \varepsilon_{h,d,s}\end{aligned} \tag{3}$$

Here h denotes household, d denotes district, and s denotes state. $\text{Underbanked}_{d,s}$ is an indicator equal to one if the district is listed as underbanked. $\text{DistRatio}_{d,s}$ is the district-level ratio. $\text{Above}_{d,s}$ is an indicator equal to one if the district-level ratio is larger than its national average. I control for the ratio's components in $X_{d,s}$ and include state-level fixed effects. I cluster standard errors at the level of treatment, the

[1] There are two potential reasons why I do not perfectly predict which districts are listed as underbanked. First, despite conversations with the RBI, I do not know which exact data sources they used to construct the ratio. Second, the RBI might have used discretion, deciding to include a district in the list despite having a ratio below the cutoff or vice versa. Both reasons are no threat to identification but give rise to the fuzzy RDD.



(a) Histogram

(b) First stage

Figure 4. Histogram and First Stage. The vertical line in both graphs indicates the national average of the population-to-branch ratio (14,780).

district level. To choose the optimal bandwidth, I follow an MSE-optimal procedure (Calonico et al., 2014). I demonstrate robustness to other bandwidths. Following Gelman and Imbens (2019), I apply linear functions within the optimal bandwidth. I test for robustness to higher-order polynomials. The primary coefficient of interest is β_1. If the identification assumption is satisfied, the estimator can be interpreted as the local average treatment effect (LATE) of receiving the underbanked status for a district with a ratio equal to the cutoff.

B. Identification Assumption

The identification assumption of this setting is continuity of all characteristics other than being underbanked at the cutoff. This assumption is violated if agents precisely manipulate the ratio of their district. Consider the following to understand how systematic differences could be introduced by manipulation. Assume local governments hear about the policy and want to benefit from more banks in their area. Also, assume that they can manipulate the population-to-branch ratio, moving from just below the cutoff to just above it. If these districts have a particularly rich or healthy population, I would confuse their characteristics with a treatment effect of the policy.

Manipulation of the population-to-branch ratio is unlikely due to its construction. First, the numerator contains population data from the 2001 Census. To manipulate this historical data, local governments would have to have anticipated the detailed policy rule years before its implementation. Second, the denominator is the sum of individual decisions of all banks in the district. The total number of bank branches in the first quarter of 2006 is not determined by a specific bank or bank type alone, making manipulation unlikely. Also, banks directly report their number of branches to the RBI, leaving no room for an intermediary party to manipulate. I also test empirically for manipulation.

The first implication of manipulation refers to the density of the forcing variable. If local governments indeed manipulate their population-to-branch ratio, there should be more districts just above the cutoff than just below. On a first glance, there is no evidence of this in Figure 4(a). To formally test for smoothness around the cutoff, I use the McCrary (2008) density test, depicted in Figure A3. I obtain an estimator of -0.1998 with a p-value of 0.8416, suggesting that I should not reject smoothness around the cutoff. The second implication of manipulation is that districts just above the cutoff should already be different from districts just below the cutoff before the policy. Assume, for example, that local governments that can manipulate their ratio have a richer or healthier population. In this case, I would observe discontinuities in pre-policy consumption and health measures.

To test for smoothness before the policy, I utilize the RBI Master Office File (2004), the IHDS I (2004/2005), and the Economic Census (2005). Results are depicted in Table 1. Columns 1 and 2 show the mean for all treated and control observations. Columns 3 and 4 depict means only for observations within the optimal bandwidth. Column 5 reports the fuzzy RDD coefficients, referring to β_1 as defined above. As expected, all coefficients are statistically insignificant. Treatment districts do not have significantly more branch licenses or actual branches before the policy introduction. Importantly, households in treatment districts are not significantly healthier than those in control districts before the policy. I observe smoothness in the number of days with non-chronic illnesses in the past month, days missed of work or school due to an illness, and medical expenses. Treatment and control households also have smooth consumption and financial access before the policy, including access to health insurance. Finally, districts do not have significantly more hospitals that report an institutional loan as their major source of finance or more hospitals generally. Correspondingly, I observe graphical smoothness in Figure 5. Additionally, I use the SHRUG data to also show that village- and town-level general economic activity and population characteristics are smooth (Table A5). Taken together, these tests suggest that there was no manipulation.

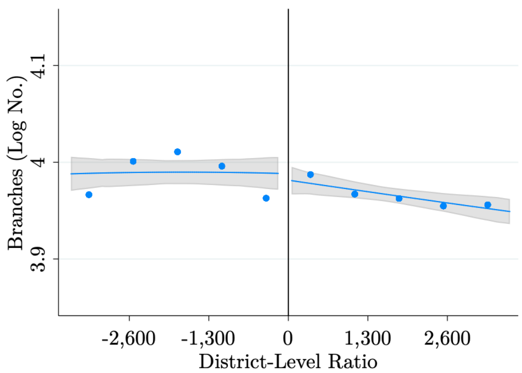
A second potential threat to identification is migration. If households migrate to treatment districts due to increased bank presence and these households are richer or healthier, I would confuse their characteristics with a treatment effect of the policy. I have detailed data on migration that allows me to test for this threat. Less than 0.5 percent of households report that they moved to their current location from another district in the five years prior to the 2011/2012 IHDS II. The coefficient on this migration pattern is insignificant when formally testing for it as described in the regression framework (Table A6).

Finally, I demonstrate that other policies do not pose a threat to identification. The concern is that I may mistake discontinuities around the cutoff for the effect of the 2005 RBI policy when they actually stem from other policies. To my knowledge, no other policy uses the same cutoff rule described in this paper. For other nationwide policies to coincidentally threaten identification, they would need to be significantly more likely to be implemented in this study's treatment districts (Moscoe et al., 2015). Otherwise, their impact would be smooth around the cutoff. While many policies define certain priority districts, these are unlikely to be identical or highly correlated to treatment districts in this setting. The reason is that priority districts are often defined according to the target of the policy, for instance, certain health indicators. In Discussion A2, I describe other nationally implemented policies, including those issued by the Ministry of Health and Family Welfare and the Ministry of Women and Childhood Development, and other policies not directly related to health such as the National Rural Employment Guarantee Act (NGREA), a labor guarantee program. For each policy, I collect a list of priority districts and map them to the 2001 Census borders. I then create an indicator that is one if a district is defined as a priority district under a certain policy and zero otherwise. Using this indicator variable as an outcome, I test whether the policy was significantly more likely to be implemented in treatment districts (Table A7). All coefficients are statistically insignificant. I provide further evidence on the distribution of priority districts in Table A8. Within a bandwidth of $\pm 4,000$, priority districts depict a low share of overall districts, ranging from 19 to 28 percent. Correlation coefficients between an indicator for priority district and an indicator for being above the cutoff within the bandwidth range from -0.07 to 0.25. This evidence suggests that other policies do not threaten causal identification.

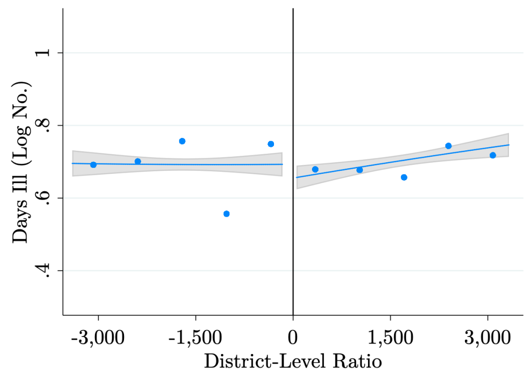
Table 1: Smooth Pre-Policy Covariates

	All observations		Within bandwidth		RDD
	Treated	Not treated	Treated	Not treated	Coefficient
	(1)	(2)	(3)	(4)	(5)
Banks					
Branch licenses 2004 (log no.)	3.59	4.19	3.86	4.17	0.02
	(0.97)	(1.14)	(0.89)	(1.07)	(0.02)
Branches 2004 (log no.)	3.58	4.19	3.82	4.17	0.01
	(0.98)	(1.14)	(0.91)	(1.07)	(0.02)
Health					
Days ill (yes/no)	0.53	0.40	0.48	0.41	-0.06
	(0.50)	(0.49)	(0.50)	(0.49)	(0.06)
Days ill (log no.)	0.86	0.61	0.75	0.64	-0.11
	(0.97)	(0.89)	(0.94)	(0.90)	(0.13)
Days missed (yes/no)	0.41	0.30	0.33	0.34	-0.11
	(0.49)	(0.46)	(0.47)	(0.48)	(0.08)
Days missed (log no.)	0.58	0.42	0.45	0.48	-0.19
	(0.84)	(0.74)	(0.77)	(0.78)	(0.14)
Medical expenses (yes/no)	0.51	0.39	0.46	0.40	-0.08
	(0.50)	(0.49)	(0.50)	(0.49)	(0.06)
Medical expenses (log Rs)	1.68	1.25	1.57	1.32	-0.14
	(2.26)	(2.11)	(2.22)	(2.15)	(0.27)
Consumption					
Total consumption (log Rs)	6.38	6.57	6.42	6.51	-0.01
	(0.42)	(0.42)	(0.43)	(0.42)	(0.05)
Food consumption (log Rs)	5.81	5.95	5.84	5.90	-0.03
	(0.32)	(0.32)	(0.33)	(0.32)	(0.03)
Financial Access					
Any loan (yes/no)	0.50	0.42	0.50	0.45	0.00
	(0.50)	(0.49)	(0.50)	(0.50)	(0.10)
Largest loan from bank (yes/no)	0.11	0.12	0.12	0.12	0.00
	(0.31)	(0.32)	(0.33)	(0.32)	(0.03)
Largest loan amt (log Rs)	3.87	2.38	3.65	3.03	0.12
	(4.46)	(4.08)	(4.47)	(4.35)	(0.86)
Health insurance (yes/no)	0.02	0.04	0.02	0.02	0.01
	(0.14)	(0.18)	(0.15)	(0.15)	(0.01)
Health Care Supply					
Institutional loan (share)	0.020	0.033	0.021	0.033	0.001
	(0.030)	(0.035)	(0.024)	(0.035)	(0.012)
Hospitals (log no.)	5.38	5.42	5.01	5.26	-0.15
	(1.16)	(1.33)	(1.26)	(1.35)	(0.16)

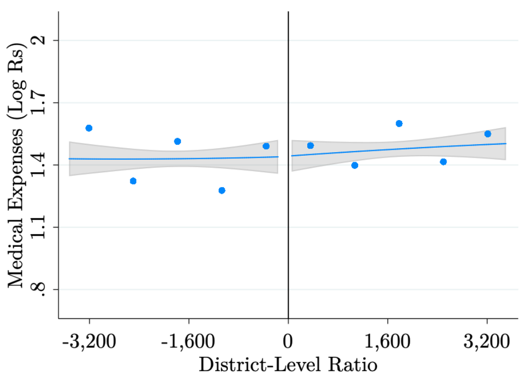
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data RBI Master Office File, IHDS I (2005/2006), and Economic Census (2005). District and household level. For district variables that are measured in numbers, I transform them into log form and winsorize at the 1st and 99th percentile. For household variables measured in days or currency, I transform them into log form and trim at the 10th and 90th percentile. Variables depicted here are later used in post-policy regressions, explained in more detail in respective tables.



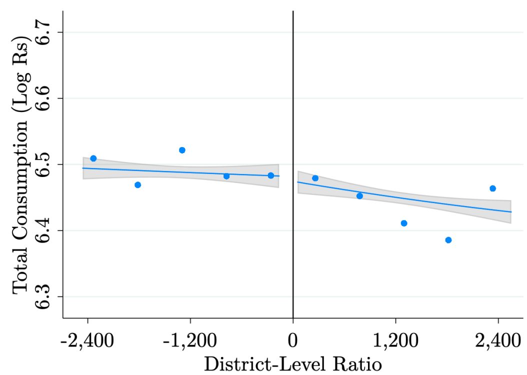
(a) Pre-Policy (2004/2005)



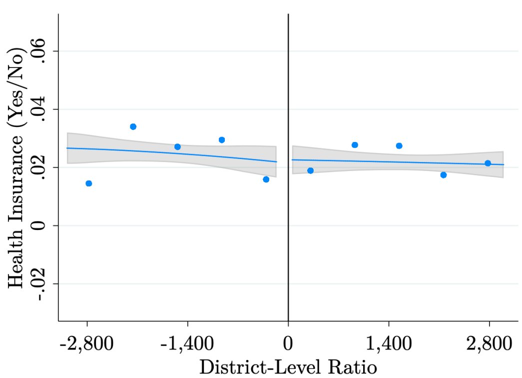
(b) Pre-Policy (2004/2005)



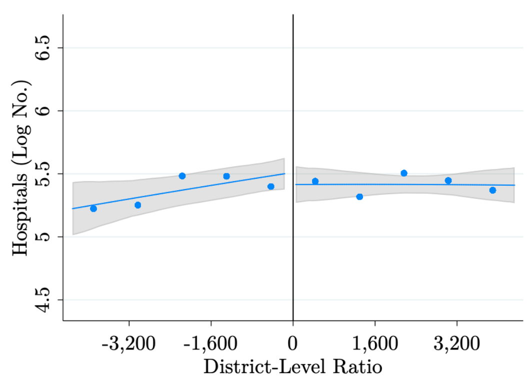
(c) Pre-Policy (2004/2005)



(d) Pre-Policy (2004/2005)



(e) Pre-Policy (2004/2005)



(f) Pre-Policy (2004/2005)

Figure 5. Smooth Pre-Policy Covariates. These graphs show binned means to the left and right of the cutoff, within the optimal bandwidth. They also show local linear polynomials to the left and right of the cutoff, with 90 percent confidence intervals in gray. The cutoff is normalized to zero.

IV. Banks Open Branches

In the first step of the analysis, I provide evidence that banks indeed reacted to the policy. I examine two outcomes: number of branch licenses and number of branches. Since I observe years between 1997 and 2016, I test both for smoothness pre-policy and for discontinuities post-policy. In Table 2, I examine the number of branch licenses in 2004, one year before the policy, and in 2010, five years after the policy.[2] As expected, coefficients in the year before the policy are statistically insignificant. Treatment districts have neither more branch licenses nor more branches than control districts. Post-policy, as expected, I observe statistically significant discontinuities in both branch licenses and branches. In 2010, treatment districts have on average 21 percent more branch licenses and 19 percent more branches than control districts. The latter corresponds to an increase to 8.31 branches per 100,000 people, compared to the control mean of 6.99 branches.

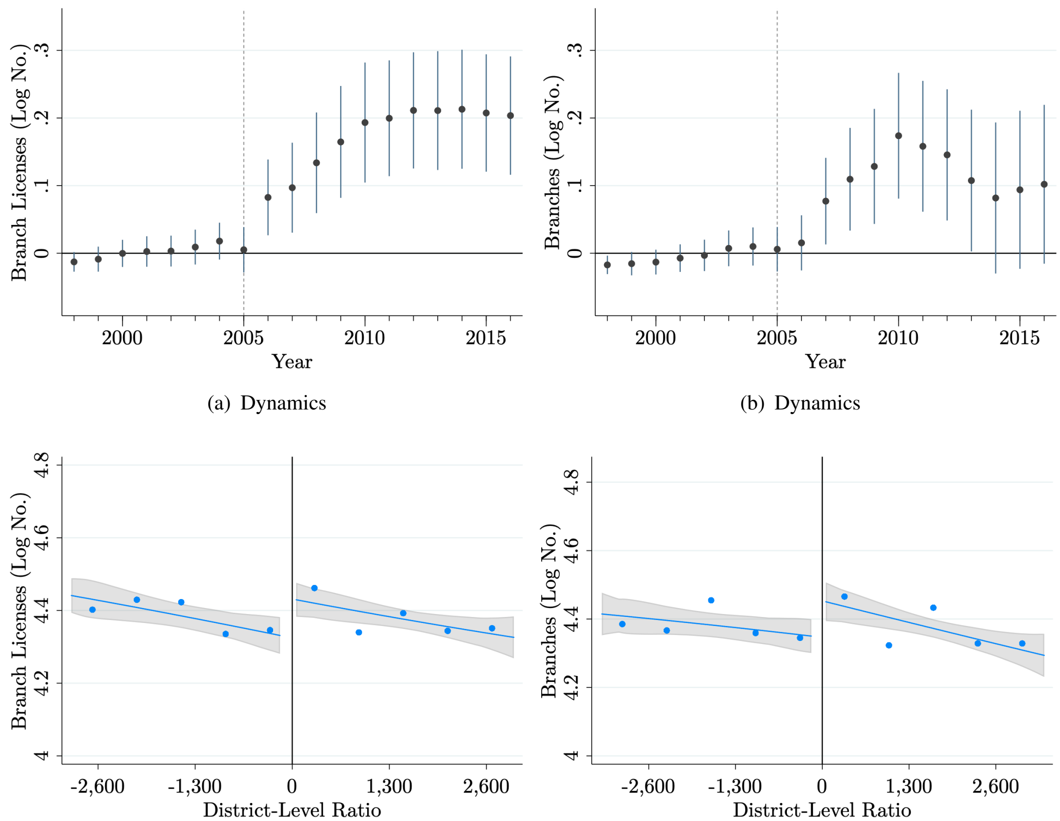
Turning to the graphical analysis, I observe discontinuities in branch licenses (Figure 6(c)) and in branches (Figure 6(d)) five years after the policy. Notably, the dynamic response of banks corresponds exactly to the policy timing. Figure 6(a) depicts the dynamics for branch licenses and Figure 6(b) for branches.

Table 2: Banks Open Branches

	Pre-Policy (2004)		Post-Policy (2010)	
	Branch Licenses (log no.) (1)	Branches (log no.) (2)	Branch Licenses (log no.) (3)	Branches (log no.) (4)
Treated	0.02	0.01	0.19***	0.17***
	(0.02)	(0.02)	(0.05)	(0.06)
Control Mean	4.17	4.17	4.55	4.54
Mean Change (%)	1.81	1.01	21.32	18.98
First Stage	0.81	0.80	0.80	0.80
Bandwidth	3,490	3,621	2,972	3,329
Observations in BW	223	230	196	213
Total Observations	561	562	561	561
Baseline Control	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data RBI Master Office File. District level. All variables are transformed into log form and winsorized at the 1st and 99th percentile. The variable from 1997 is included as a baseline control.

[2] Tables that describe treatment effects contain the following information: The first line provides the main coefficient of interest, β_1. This is followed by the control mean within the optimal bandwidth and the mean change. Next, the reader can find the first stage coefficient, α_1. Following that are the optimal bandwidth and the number of observations within the optimal bandwidth. The next line, observations, describes the total size of the sample before conditioning on the bandwidth. Finally, the last line indicates whether any baseline controls are included in the regression.



(a) Dynamics (b) Dynamics

(c) Post-Policy (2010) (d) Post-Policy (2010)

Figure 6. RBI Issues Licenses and Banks Open Branches. Figure 6(a) and 6(b) depict the dynamic effects of branch licenses and branches. Figures 6(c) and 6(d) depict the discontinuities in branch licenses and branches five years after the policy was introduced. Respective regressions are described in Columns 3 and 4 of Table 2.

As expected, there is smoothness around the cutoffs before the policy, and coefficients become statistically significant after the policy. The reaction in branch licenses issued is immediate: the coefficient on branch licenses becomes statistically significant in 2006, the year the final list of underbanked districts is published. As expected, the reaction in branches is slightly lagged by one year: the coefficient becomes statistically significant in 2007. There is another pattern that can be explained by the policy. In 2010, as discussed in Section I, the RBI allowed banks to open branches without licenses in eight states. The observed pattern in the dynamics—a stagnation in the coefficient on licenses issued and a decrease in the coefficient on number of branches—corresponds exactly to what one expects to see if banks increasingly open branches in districts to the left of the cutoff (remaining in the control group) in states where licenses are not required. While the change in the policy attenuates the difference in branches between treatment and control districts after 2010, treatment districts have historically been exposed to more branches. Aggregated district-level credit and deposit amounts are discussed in Figure A4, and branch profitability is examined in Discussion A3. One can conclude that banks indeed reacted to the policy.

Standard robustness and placebo tests on bank outcomes are discussed in Section VII, but one placebo test that emerges from the design of the policy is discussed here. One type of bank, regional rural banks, is excluded from the policy. Consequently, one does not expect to observe positive coefficients for this bank type. I test for discontinuities in branch licenses and branches of regional rural banks in 2010 (Table A9), and, as expected, coefficients in the placebo test are insignificant.

V. Health Improves

In the second step of the analysis, I investigate the effects of bank presence on three pillars of health—morbidity rates, vaccination rates, and pregnancy risks—and demonstrate that the health status of households improves in treatment districts. For this, I rely on two complementary household surveys. The IHDS II (2011/2012) allows me to measure morbidity rates and paints a holistic picture of the economic situation of the households. For instance, I observe how many days of work or education households missed in the past month due to an illness. The DHS (2015/2016) has a large sample size, allowing me to capture effects on low-probability events such as miscarriages. Furthermore, using a second data set enables me to replicate findings from the first, providing further confidence in my results.

A. *Lower Morbidity Rate*

Initially, I investigate the effect of bank presence on morbidity rates, which are measured six years after the policy in the IHDS II. I concentrate on non-chronic illnesses such as fever, cough, or diarrhea. I set aside chronic diseases, such as cancer, which are less likely to be responsive to the policy. In a module on health, households are asked whether any household member was ill in the past month and how many days members were ill in total. Results are depicted in Columns 1 and 2 of Table 3. I observe that households in treatment districts have a 36 percent lower probability of any member being affected by a non-chronic illness. Roughly speaking, every second household in control districts is affected by an illness in a given month, while in treatment districts, only every third household is affected. In alignment, the total number of days that household members are ill is 25 percent lower in treatment districts. Results remain similar in magnitude and significance when controlling for baseline measures (see Table A10). Pre-policy smoothness for this variable is depicted in Figure 5(b). The post-policy discontinuity is shown in

Figure 7(a). To provide a further piece of evidence of the validity of my findings, I compare my effect sizes on morbidity rates to the literature and show that they are in line with effects of other successful health interventions (Discussion A4).

Illnesses can have important economic consequences for households. First, if someone in the household falls ill, that person is unlikely to be able to go to work or school, resulting in income loss or missed learning experiences for the household. Second, the household has to pay for medical expenses. The survey data allows me to capture the effect on these health-related economic outcomes. Based on the significant improvement in health status, I expect treated households to miss less work or school and spend less on medical expenditures related to non-chronic illnesses. Indeed, this is what I observe in Table 3. Columns 3 and 4 refer to the question of whether (or how many days) members missed work or school due to an illness. Households in treatment districts are 71 percent less likely to give an affirmative answer than those in control districts. On the internal margin, the number of days missed is 35 percent lower in treatment districts, corresponding to a decrease to 1.05 days compared to a control mean of 1.62 days. Regarding medical expenses, households are asked to report whether (or how much) they spent on treatment of non-chronic illnesses in the past month. The effect on medical expenses is depicted in Columns 5 and 6. I find that the probability that households spend on treatment is 34 percent lower in treatment districts. This is a significant improvement compared to the control mean, which shows that more than half of the households spend on treatment. It is also in alignment with the coefficient size in Column 1 on probability of illness. In control households, the amount spent on medical expenses is 121 rupees on average, which means that treatment households save 71 rupees or around $1.32 per month. These effects are not calculated conditional on having an illness. Pre-policy smoothness of medical expenses is depicted in Figure 5(c), and post-policy discontinuity in Figure 7(b). I conclude that the improvement in health status is accompanied by a decrease in economic costs borne by households.

All variables considered in this analysis are smooth on baseline as demonstrated in Table 1 and Figure 5(b) and 5(c). The full regressions with pre-policy variables are depicted in Table A11. While coefficients pre-policy are insignificant, they point in the same direction as my treatment effect. I thus repeat my analysis, controlling for baseline measures of the respective variable of interest. Table A10 shows that coefficients post-policy remain statistically significant when including baseline controls. Finally, since chronic illnesses such as cancer, diabetes, or heart disease are less likely to be affected in probability, they depict a natural placebo outcome. I repeat the analysis for these kinds of diseases in Table A12. As expected, coefficients are insignificant.

Table 3: Lower Morbidity Rate

	Days ill		Days missed		Medical expenses	
	(yes/no) (1)	(log no.) (2)	(yes/no) (3)	(log no.) (4)	(yes/no) (5)	(log Rs) (6)
Treated	-0.19**	-0.29**	-0.30***	-0.44***	-0.18**	-0.88**
	(0.09)	(0.12)	(0.10)	(0.13)	(0.08)	(0.35)
Control Mean	0.53	0.82	0.41	0.58	0.52	2.12
Mean Change (%)	-35.74	-25.21	-71.46	-35.40	-33.61	-58.56
First Stage	0.65	0.70	0.66	0.68	0.66	0.69
Bandwidth	2,204	2,658	2,331	2,513	2,373	2,948
Observations in BW	11,749	12,968	12,730	12,421	12,862	14,576
Total Observations	35,103	32,280	36,805	33,346	36,805	32,983
Baseline Control	No	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. All variables measured in currency Rs are in log form and trimmed at the 10th and 90th percentile. All variables refer to non-chronic illnesses such as fever. Days missed measures the number of days that the household was not able to do usual activities and had to miss work or school. All questions refer to the past 30 days. Results remain similar in magnitude and significance when controlling for baseline measures (see Table A10).

B. Higher Vaccination Rate

Increasing child vaccination rates is crucial for reducing child morbidity and mortality. Usually, children are vaccinated against tuberculosis, diphtheria, whooping cough, tetanus, polio, and measles. Due to the policy, the fraction of households with children that have received any vaccination is 8 percent higher in treatment districts. This corresponds to an increase to 93 percent of households, compared to a control mean of 86 percent (Table 4, Column 1). The discontinuity is graphically depicted in 7(c). The DHS measures vaccination rates for all children below the age of five approximately ten years after the policy, in 2015/2016. Measuring the effect on vaccination provides further confidence in my results, since these outcomes are less likely to be affected by self-reporting biases. Over half of the affirmative answers on vaccination status were obtained from an official vaccination card. In addition to examining the effect on vaccination rates, I utilize the data to replicate my findings on morbidity rates. Again, I find a negative effect on the probability of non-chronic illnesses such as fever, diarrhea, or cough. The DHS collects this data only for children below five, not for other household members. Column 2 of Table 4 indicates that the fraction of households with a child that fell ill in the past two weeks is 23 percent lower in treatment districts, corresponding to a decrease to 21 percent in treatment districts compared to a control mean of 27 percent. The discontinuity is observable in 7(d). Another proxy for health status is the number of visits to health care providers. This is an imperfect proxy since it could also reflect households shying away from visits. I do find that households in treatment districts

Table 4: Higher Vaccination Rate

	Vaccinated child (yes/no) (1)	Sick child (yes/no) (2)	Health care visit (any) (yes/no) (3)	Health care visit (child sick) (yes/no) (4)
Treated	0.07*	-0.06*	-0.08**	-0.02*
	(0.04)	(0.03)	(0.03)	(0.01)
Control Mean	0.86	0.27	0.29	0.11
Mean Change (%)	8.34	-23.12	-26.84	-22.99
First Stage	0.72	0.70	0.72	0.73
Bandwidth	2,898	3,539	3,287	3,383
Observations in BW	26,117	66,658	166,756	187,208
Total Observations	86,079	171,471	431,148	471,985
Baseline Control	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data DHS (2015/2016). Household level. Data on health status is coded as missing for households without children below five. Data on health care visits is obtained from the women's questionnaire; households without an eligible woman are coded as missing.

are 26 percent less likely to have gone to a health care provider for any reason in the past three months. In particular, households are 23 percent less likely to have gone to a provider to treat a sick child. This allows for the conclusion that children not only have a higher probability of being vaccinated, they generally benefit from better health.

C. Safer Pregnancies

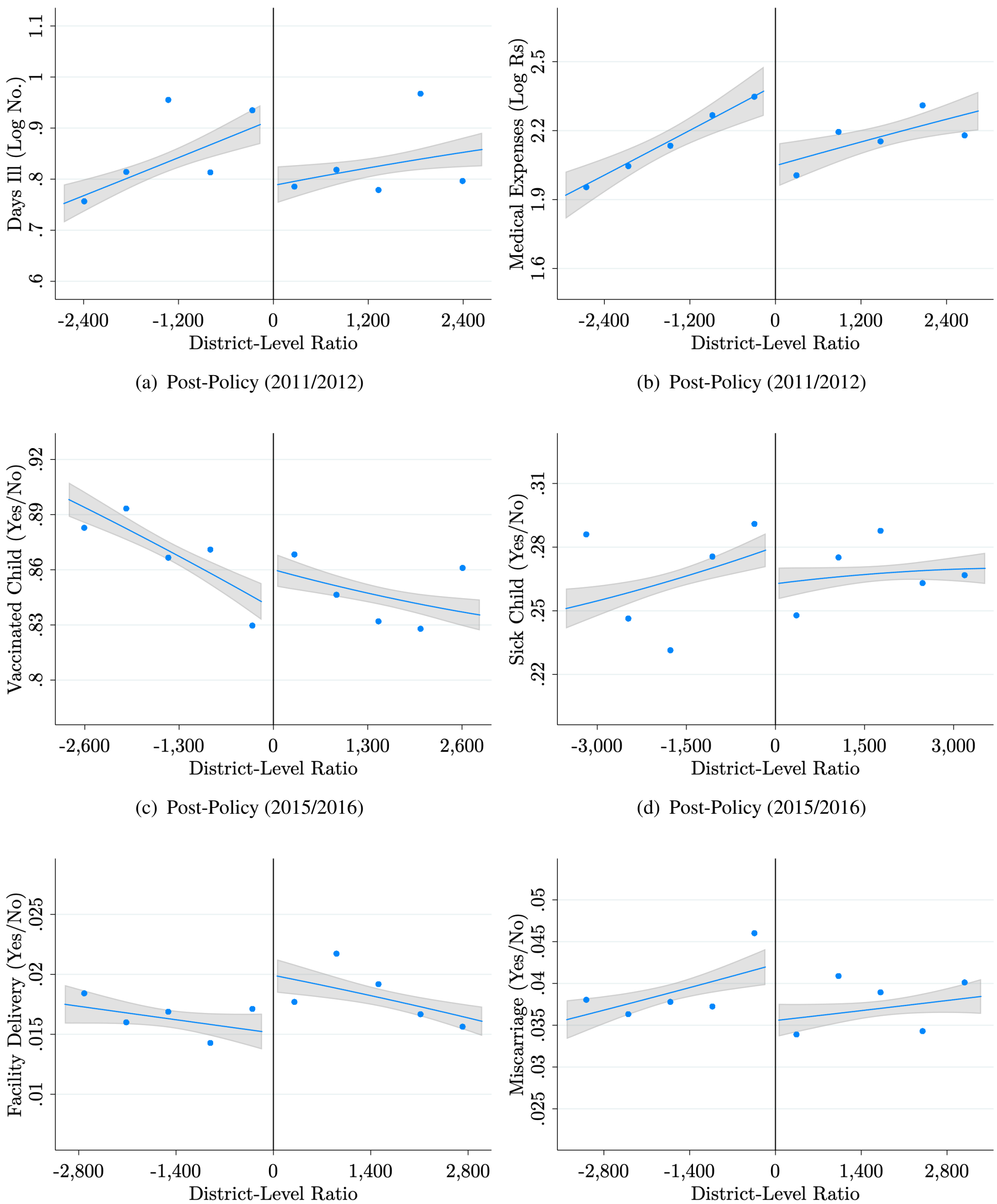
Another important aspect of health is maternal wellbeing. There are still significant risks associated with pregnancies in developing countries. In this section, I demonstrate that the policy played an important role in making progress on this dimension. Table 5 depicts the effect of the policy on respective outcomes, measured in the DHS (2015/2016). Initially, I observe that households in treatment districts are 34 percent more likely to have a woman who reported to have delivered in a health care facility (Column 1). The discontinuity is depicted in Figure 7(e). The low control mean arises because the question only refers to pregnancies in the past three months and is coded zero if there was no pregnancy. In alignment with this observation and potentially resulting from it, I find that the fraction of households with a woman who ever experienced a miscarriage or stillbirth is significantly lower in treatment districts (Columns 2 and 3). The fraction of households with a woman who experienced a miscarriage is 26 percent lower, corresponding to a decrease to 2.80 percent compared to a control mean of 3.78 percent. The discontinuity is graphically observable in Figure 7(f). The fraction of stillbirths decreases from a control mean of 0.45 percent to 0.24 percent, a reduction of 46 percent. While

Table 5: Safer Pregnancies

	Health care facility delivery (yes/no) (2)	Miscarriage (yes/no) (2)	Stillbirth (yes/no) (3)	Health care visit (woman sick) (yes/no) (4)
Treated	0.005***	-0.010*	-0.002*	-0.051*
	(0.002)	(0.006)	(0.001)	(0.027)
Control Mean	0.016	0.038	0.004	0.170
Change (%)	33.52	-26.30	-45.92	-29.84
First Stage	0.72	0.73	0.73	0.72
Bandwidth	3,023	3,430	3,386	3,277
Observations in BW	172,892	188,571	187,208	182,318
Total Observations	471,985	471,985	471,985	471,985
Baseline Control	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data DHS (2015/2016). Household level. Data on health status and health care visits is coded as missing for households without an eligible woman.

miscarriages and stillbirths are low-probability events, they can have large consequences for women's physical and mental health. Consequently, any progress on this dimension is of high importance. Finally, I provide indirect evidence that effects on general morbidity rates are replicable for women in the 2015/2016 survey. While data on health status related to non-chronic illnesses is only collected for children, I do observe whether women went to a health care facility in the past three months for their own treatment. The fraction of households with women who went for a visit is 30 percent lower in treatment than control districts. Summarizing this evidence, women's health appears to improve due to the policy.



(a) Post-Policy (2011/2012)

(b) Post-Policy (2011/2012)

(c) Post-Policy (2015/2016)

(d) Post-Policy (2015/2016)

(e) Post-Policy (2015/2016)

(f) Post-Policy (2015/2016)

Figure 7. Health Improves. These graphs show binned means to the left and right of the cutoff, within the optimal bandwidth. They also show local linear polynomials to the left and right of the cutoff, with 90 percent confidence intervals in gray.

VI. Mechanisms

How exactly does bank presence affect health? To shed light on this question, I lay out a framework of potential mechanisms and then discuss which ones have bearing in the data. Imagine a village or small city, in a developing country such as India, experiencing the opening of its first bank branch (Figure 1). Initially, the bank provides credit to local businesses, stimulating business activity. Households benefit from higher income and invest more in health. Additionally, the bank provides access to financial instruments for households. Importantly, this includes not only savings accounts and bank loans but also health insurance. In the context of many developing countries, banks act as intermediaries between households and health insurance providers in far-away cities, offering health insurance at local branches. Finally, the bank provides credit for health care providers, allowing them to improve health care supply. This small narrative lays out potential channels for how bank presence could positively impact health.[3] There could also be interactions between mechanisms. For instance, it might be especially profitable for health care providers to take up credit if they want to respond to increased health care demand. Additionally, households might be able to afford health insurance offered by banks now that they benefit from higher income (Rampini and Viswanathan, 2018). Any partial equilibrium study would miss these interactions. The main contribution of this section to the literature is that it highlights two aspects of banking previously understudied: banks provide health insurance to households and credit to hospitals.

There is an important trade-off in this study. In contrast to RCTs, the natural experiment allows me to realistically capture effects that emerge due to interactions between financial access by different types of agents, and due to a higher diffusion over a longer time frame. However, this means that I cannot exactly quantify the extent to which each mechanism contributed to improving health, which would require isolated exogenous variation in the respective mechanisms. For instance, I would need exogenous variation in credit access for health care providers only. While providing isolated exogenous variation is out of this study's scope, it provides a promising avenue for future research. In this study, I observe outcomes in equilibrium that are indicative of mechanisms. For example, I demonstrate that health care providers take up credit and increase supply. While this is indicative of this specific mechanism, it is also in alignment with a response to increased aggregate demand. I review empirical evidence on potential mechanisms in the following sections.

[3] An additional possibility is that local governments increase their spending on health or health-related items such as hygiene, either because they directly gain access to loans or benefit from higher tax revenue. While I only have state-level data on government expenditure, I do not find evidence that local governments play a role (Table A13).

A. Banks Stimulate Business Activity and Increase Households' Income

A first important determinant of poor health in developing countries is that households have limited resources to invest in health. For instance, they may be unable to afford a nutritious diet or pay medical bills. Previous literature has established that financial development in general and increased bank presence in particular can stimulate business activity and increase households' income (Bruhn and Love, 2014), which should in turn reflect in higher consumption.[4] Indeed, I observe that six years after the policy, households in treatment districts have 8 percent higher total consumption than those in control districts.[5] This finding is depicted in Column 1 of Table 6 and Figure 9(a). Next, I examine whether households increase their investments in health. Three important determinants of health are food intake, sanitary conditions, and health care demand. Indeed, I observe that households spend 6 percent more on food, resulting in a quarter of a meal more per day (Table 6, Columns 2 and 3). This alone could have had a positive impact on health. I find null effects on hygiene-related expenses, including the amount spent on soap, insecticides, or toilet articles (Table 6, Column 4).

Did households increase their health care demand? Six years after the policy introduction, households report significantly lower outpatient expenses; inpatient expenses remain stable (Table 6, Columns 5 and 6). This aligns with the finding of lower medical spending on non-chronic illnesses in Table 3. Crucially, however, one cannot conclude from these results that there is a decrease in health care demand for three reasons. First, I do not measure historical medical expenses, but only a snapshot at the time of the survey, six years after the policy. By then, households may have already improved their health status, which would reflect in lower medical expenses. Second, prices could have decreased in equilibrium, but these are not observed in this study. Finally, increased access to health insurance could have decreased households' out-of-pocket share and thereby lowered medical expenses. Summarizing, it is likely that an income channel is at play; households have higher total consumption and spend more on food. While the effect on medical expenses is negative, it is a bad proxy for health care demand. Other findings, such as increased vaccination rates or increased probability of deliveries at health care facilities, point towards increased health care demand.

[4] I do not directly measure income, as this measure is often unreliable in survey data (Deaton and Zaidi, 2002).

[5] The increase in consumption is an equilibrium result, reflecting not only increased income through stimulated business activity but also, e.g., access to financial instruments for households or more work due to fewer illnesses.

Table 6: Households Have Higher Total and Food Consumption

	Total consumption (log Rs) (1)	Food consumption (log Rs) (2)	Meals per day (no.) (3)	Hygiene expenses (log Rs) (4)	Outpatient expenses (log Rs) (5)	Inpatient expenses (log Rs) (6)
Treated	0.07**	0.06*	0.24**	0.06	-0.45*	-0.14
	(0.04)	(0.03)	(0.10)	(0.06)	(0.23)	(0.30)
Control Mean	7.48	6.71	2.75	4.02	2.73	1.33
Change (%)	7.68	5.73	8.64	5.82	-36.06	-13.46
First Stage	0.75	0.71	0.68	0.66	0.70	0.56
Bandwidth	4,120	2,755	3,004	2,246	3,793	1,902
Observations in BW	14,903	11,415	16,611	9,896	17,418	8,537
Total Observations	21,410	21,345	34,773	23,010	29,182	27,312
Baseline Control	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. All variables in rupees are per capita per month and trimmed at the 10th and 90th percentile. Hygiene expenses refer to amount spent on soap, insecticide, toilet articles, etc.

B. Households Gain Access to Financial Instruments

A second important determinant of poor health in developing countries is that households have limited ability to move available resources across time and states. For example, they might skip a necessary doctor's visit if they have not built up enough emergency savings, cannot take an emergency loan, or do not have health insurance. As bank presence increases, households might gain access to savings accounts, bank loans, or health insurance. As a first step, I utilize questions from the IHDS II to investigate whether households in treatment districts are more likely to take up these financial instruments due to the policy. Households are asked whether, in the past five years, they used any savings account, had any bank loan, or had any health insurance. Findings are reported in Table 7. Six years after the policy was introduced, households in treatment districts are 36 percent more likely to have used a savings account. In control areas, 51 percent of households have savings accounts. The discontinuity is depicted in Figure 9(b). Considering all households in the sample, I find a positive but insignificant effect on bank loans. This is in line with other studies on credit impact that find low take-up (Banerjee et al., 2015a). Importantly, households also gain access to health insurance; based on the low control mean of six percent, I indeed observe a large increase in this dimension (273%). The respective discontinuity is depicted in Figure 9(c). Take-up is balanced pre-policy as indicated in Table A14. Not all outcome variables are available pre-policy, in which case similar dimensions of financial access are shown to be smooth.[6]

[6]Strengthening the idea that financial access for households played a role, Agarwal et al. (2017) provide correlational evidence that it is associated with increased health expenses and decreased death rates.

Table 7: Households Gain Access to Savings Accounts and Health Insurance

	Savings account (yes/no) (1)	Bank loan (yes/no) (2)	Health insurance (yes/no) (3)
Treated	0.19*	0.04	0.17**
	(0.10)	(0.05)	(0.07)
Control Mean	0.51	0.23	0.06
Change (%)	36.48	19.70	272.69
First Stage	0.69	0.66	0.56
Bandwidth	3,023	2,370	1,704
Observations in BW	16,674	12,856	8,482
Total Observations	36,786	36,785	34,181
Baseline Control	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. Questions ask whether the household used or owned the respective instrument in the past five years.

To examine whether households that took up financial instruments experienced a stronger effect on health, I utilize the pre-policy survey to predict the take-up probability. Consistent with financial instruments having played an important role, households in the upper half of these probability distributions show stronger positive effects on health for all three instruments (Table A15). Coefficients for households in the lower half of the distribution—those less likely to take up the products—are smaller, and they are significant for savings accounts and bank loans but insignificant for health insurance. This evidence suggests that savings accounts and health insurance could have played a key role. Note that financial devices are not randomly distributed in these tests. This means that the predicted take-up probability correlates with unobservables potentially driving the treatment strength.

In India, the largest ten banks provide access to health insurance.[7] Banks act as intermediaries between health insurance providers in far-away cities and households to mitigate two main challenges. Due to their branch network, they bridge the distance between health insurance providers and potential customers. Additionally, as they have already verified information such as households' identity, they mitigate information asymmetries between the two parties. Figure 8 shows that this phenomenon is not unique to the Indian context. In 52 percent of developing countries, the largest bank offers health insurance. In another 21 percent of developing countries, the largest bank offers at least life or accident insurance. In developed countries, bank and insurance markets are largely separated, but in developing

[7]This includes public as well as private banks: HDFC (private), SBI (public), ICICI (private), AXIS Bank (private), Kotak Mahindra Bank (private), IndusInd Bank (private), Yes Bank (private), Punjab National Bank (public), Bank of Baroda (public), Bank of India (public).

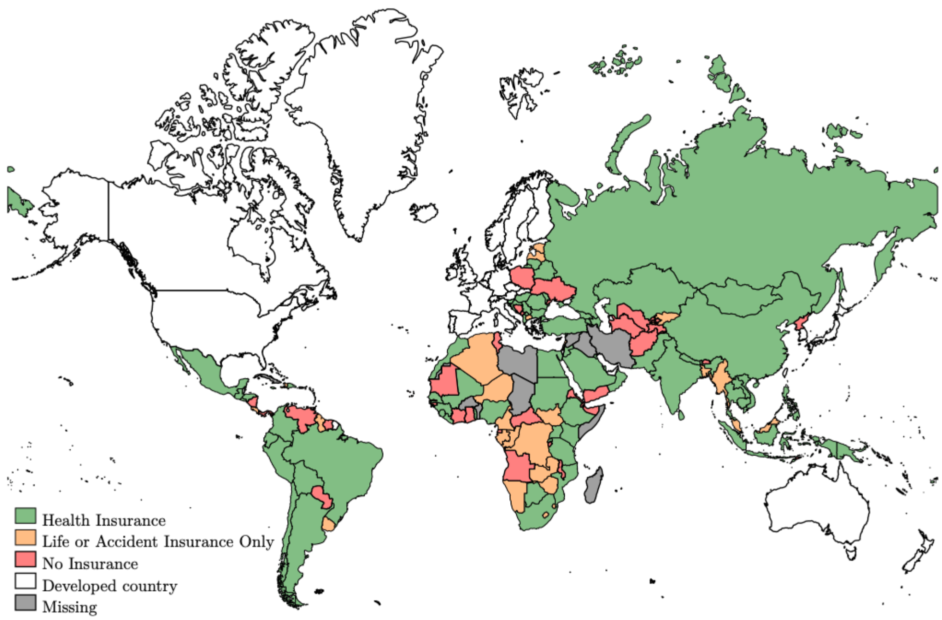


Figure 8. Banks Offer Health Insurance. This figure depicts for all developing countries whether the largest bank offers a health insurance product (green). Additionally, if the largest bank does not offer a health insurance product, I examine whether it offers other insurance products, such as life or accident insurance (orange).

countries, it is clear that frictions such as distance and information asymmetries mean that banks often act as intermediaries and offer insurance.

C. Health Care Providers Gain Access to Credit

A third determinant of poor health in developing countries is that households often have limited access to good health care services (Banerjee et al., 2004). I thus investigate whether increased bank presence allowed health care providers to gain access to credit and whether health care supply increased in equilibrium. Given the context of the health care system in India, making progress on access to or quality of health care might have significant consequences for health status. Many households are highly unsatisfied with the sector. Thirty-six percent of households in the DHS (2015/2016) state that distance to the closest health facility is a big problem. Fifty-two percent report that personnel absenteeism is a big issue, and 53 percent have large problems with drug availability at health care facilities. If bank presence allows health care providers to relax their credit constraints, this could allow investing into new health care facilities, providing monetary incentives for medical personnel to decrease absenteeism rates, or purchasing drugs on stock. For bank presence to trigger an increase in supply, two conditions need to be satisfied: health

Table 8: Hospitals Increasingly Use Institutional Loans

	Major source of finance				
	Institutional loan (share) (1)	Self-finance (share) (2)	Government sources (share) (3)	Donations (share) (4)	Non-institutional loan (share) (5)
Treated	0.010**	-0.058	0.053	-0.010	0.002
	(0.004)	(0.048)	(0.047)	(0.016)	(0.003)
Control Mean	0.014	0.495	0.332	0.085	0.002
Change (%)	67.77	-11.67	16.06	-11.42	87.01
First Stage	0.79	0.80	0.80	0.80	0.81
Bandwidth	2,435	4,393	3,928	4,018	5,044
Observations in BW	163	272	245	248	303
Total Observations	538	539	539	539	541
Baseline Control	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data Economic Census (2013). District level. I report the share of hospitals in a given district that state their major source of finance is coming from a certain source.

care providers generally rely on bank loans and are credit constrained. I first discuss my regression results and then provide evidence that these two conditions hold.

Using the Economic Census (2013), I examine whether health care providers are more likely to cite institutional loans as their major source of finance in treatment than in control districts. Running a district-level regression, I find that eight years after the policy, the share of establishments with hospital activities that cite an institutional loan is significantly higher in treatment districts. The finding is depicted in Column 1 of Table 8, and the respective discontinuity is shown in Figure 9(d). The coefficient on establishments that conduct medical or dental activities is insignificant. Other forms of financing, such as self-finance, are not increasingly more likely to be reported after the policy. This can cautiously be interpreted against evidence of other drivers of supply growth, including an aggregate demand effect or that hospital owners generally become richer due to increased business activity and thus finance hospitals from their own pockets.

If health care providers are indeed credit constrained, I expect the increased take-up of institutional loans to translate into an improvement in health care supply. Note that I observe health care supply in the equilibrium and do not isolate the effect of increased credit access to health care providers on supply. I find that eight years after the policy was introduced, treatment districts have significantly more hospitals. As described in Column 3 of Table 9, treatment districts have 140 percent more hospitals than control districts, corresponding to 74 hospitals per 100,000 people versus the control mean of 31 hospitals per 100,000 people.[8] While the con-

[8]These results connect to recent literature on finance and health care in the U.S. that demonstrates the importance of financial constraints for hospital investment and health (Adelino et al., 2015; Aghamolla et al., 2021).

Table 9: Hospitals Open

	Pre-policy (2005)		Post-policy (2013)	
	Hospitals (log no.) (1)	Other medical service providers (log no.) (2)	Hospitals (log no.) (3)	Other medical service providers (log no.) (4)
Treated	-0.15	0.26	0.88***	0.10
	(0.16)	(0.31)	(0.33)	(0.35)
Control Mean	5.42	5.22	5.96	5.28
Change (%)	-13.96	29.96	140.07	10.55
First Stage	0.80	0.80	0.80	0.81
Bandwidth	4,328	3,176	3,127	3,417
Observations in BW	268	203	201	213
Total Observations	539	538	538	538
Baseline Control	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data Economic Census (2005 and 2013). District level. All variables are transformed into log form and winsorized at the 1st and 99th percentile.

trol mean appears large, these hospitals are relatively small; they have on average only seven employees listed in the census. The respective discontinuity is depicted in Figure 9(e). The coefficient for establishments that conduct medical or dental activities is insignificant. These facilities are even smaller, with an average of two employees per establishment. The presence of both hospitals and other medical service providers is balanced on baseline as observable in Columns 1 and 2.

I further complement this information from the Economic Census with household survey evidence from the DHS (2015/2016). I utilize questions that ask whether the household has big problems with health care facilities in terms of access (distance, transport) and quality (personnel absenteeism, lack of female health care personnel, lack of drugs at the facility). Here, I focus on the urban sample of the population; coefficients are mostly insignificant for the rural sample. As becomes evident studying the control means in Table 10, a high share of households are unsatisfied with the health care system. Ten years after the policy, households in urban areas of treatment districts are significantly less likely to report big problems with health care providers. Probabilities for access being an issue are 58 and 65 percent lower in treatment than in control districts. In terms of quality concerns, probabilities are 32 to 54 percent lower in treatment districts (see Figure 9(f) for the discontinuity of personnel absence). These findings supplement the evidence from the Economic Census, further suggesting that health care supply improved as a result of the policy.

Finally, in alignment with the credit access narrative, I observe a stronger reaction for private hospitals on loan take-up and supply increase. Private hospitals account for approximately three-quarters of all hospitals in the country. Findings

Table 10: Households Report Less Problems

	Big problem with health care providers				
	Access		Quality		
	Distance to facility (yes/no) (1)	Taking transport to facility (yes/no) (2)	No personnel at facility (yes/no) (3)	No female personnel at facility (yes/no) (4)	No drugs at facility (yes/no) (5)
Treated	-0.12***	-0.11***	-0.14**	-0.20**	-0.15**
	(0.04)	(0.04)	(0.06)	(0.08)	(0.07)
Control Mean	0.20	0.17	0.44	0.37	0.45
Change (%)	-57.66	-65.35	-32.39	-54.27	-32.35
First Stage	0.60	0.57	0.62	0.62	0.59
Bandwidth	2,053	1,922	2,216	2,258	2,015
Observations in BW	34,937	34,395	41,751	42,131	34,829
Total Observations	128,525	128,525	129,568	129,568	128,525
Baseline Control	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data DHS (2015/2016). Urban sample. Household level.

are indicated in Table 11. Eight years after the policy, treatment districts have an 88 percent higher share of private hospitals that cite an institutional loan as their main source of finance than control districts have. No government hospital cites an institutional loan as its main source of finance. However, from the Prowess data, I know that bank loans are still highly relevant for government hospitals, even though they are unlikely to be the major source of finance. Any effect on government hospitals is hidden due to the lack of more detailed balance sheet data in the census. Examining the response in supply, I find that treatment districts have 130 percent more private hospitals than control districts, but only 81 percent more government hospitals after the policy. Corresponding to the data of the Economic Census, I find that households are significantly more likely to state that they generally go to private providers for treatment. Households partly substitute away from government providers. Evidence for this is depicted in Table A16 (DHS (2015/2016)). Summarizing, I observe a stronger credit take-up and supply-side reaction for private providers, the group that is more likely to benefit from access to bank loans. Finally, qualitative interviews with Indian bank employees support the hypothesis that banking services after 2005 allowed the health care sector to grow.

After having investigated these results, I discuss the two conditions that need to be satisfied for this mechanism to be at play. First, health care providers generally need to rely on bank loans. Second, they need to be credit constrained; otherwise, they would substitute loans and might not improve health care supply. To examine whether the first condition holds true, I use two data sets: the Prowess database and the Economic Census. The Prowess database provides detailed financial infor-

Table 11: Stronger Reaction for Private Hospitals

	Private		Government	
	Institutional loan (share) (1)	Hospitals (log no.) (2)	Institutional loan (share) (3)	Hospitals (log no.) (4)
Treated	0.020**	0.84**	-	0.64*
	(0.009)	(0.36)	(-)	(0.33)
Control Mean	0.025	5.27	0.000	4.41
Change (%)	87.52	16.02	-	14.63
First Stage	0.79	0.81	-	0.81
Bandwidth	2,357	3,382	-	3,633
Observations in BW	156	211	-	226
Total Observations	528	538	-	539
Baseline Control	No	No	-	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data Economic Census (2013). District level. Outcomes in Columns 1 and 3 are shares of private or government hospitals in a given district that state their major source of finance is a certain source, e.g., institutional loans. Variables in Columns 2 and 4 are transformed into log form and winsorized at the 1st and 99th percentile.

mation about a sample of relatively large health care providers from 1988 to 2017. These observations are limited to only 89 districts; thus, I use this data only for descriptive purposes and not in the regression analysis. Initially, I examine statistics for the 385 companies in the Prowess data that conduct hospital activities, averaging over the years they are present in the database. These companies have a broad asset range of USD 2,000 to 410 million, with a mean asset size of 15 million. Of these companies, 65 percent have a bank loan. For those with a bank loan, the mean size of the loan is USD 5.09 million, corresponding to 33 percent of their total assets. As expected, private companies rely more heavily on bank loans; 72 percent report a bank loan. Government companies, however, also frequently report bank financing; 58 percent have a bank loan. Bank loans as a financial instrument are used by companies across the size range (see Figure A5). Additionally, a high dependency on bank loans is not unique to companies that conduct hospital activities. A similar picture emerges for the 22 companies in the data that offer medical or dental activities. These have a narrower asset range of USD 115,000 to 107 million and a mean asset size of 10 million. Of these companies, 77 percent have a bank loan. For those that report a bank loan, the mean size of the loan is USD 1.88 million, corresponding to 27 percent of their assets. Examining the Prowess data suggests that relatively large companies heavily rely on bank financing.

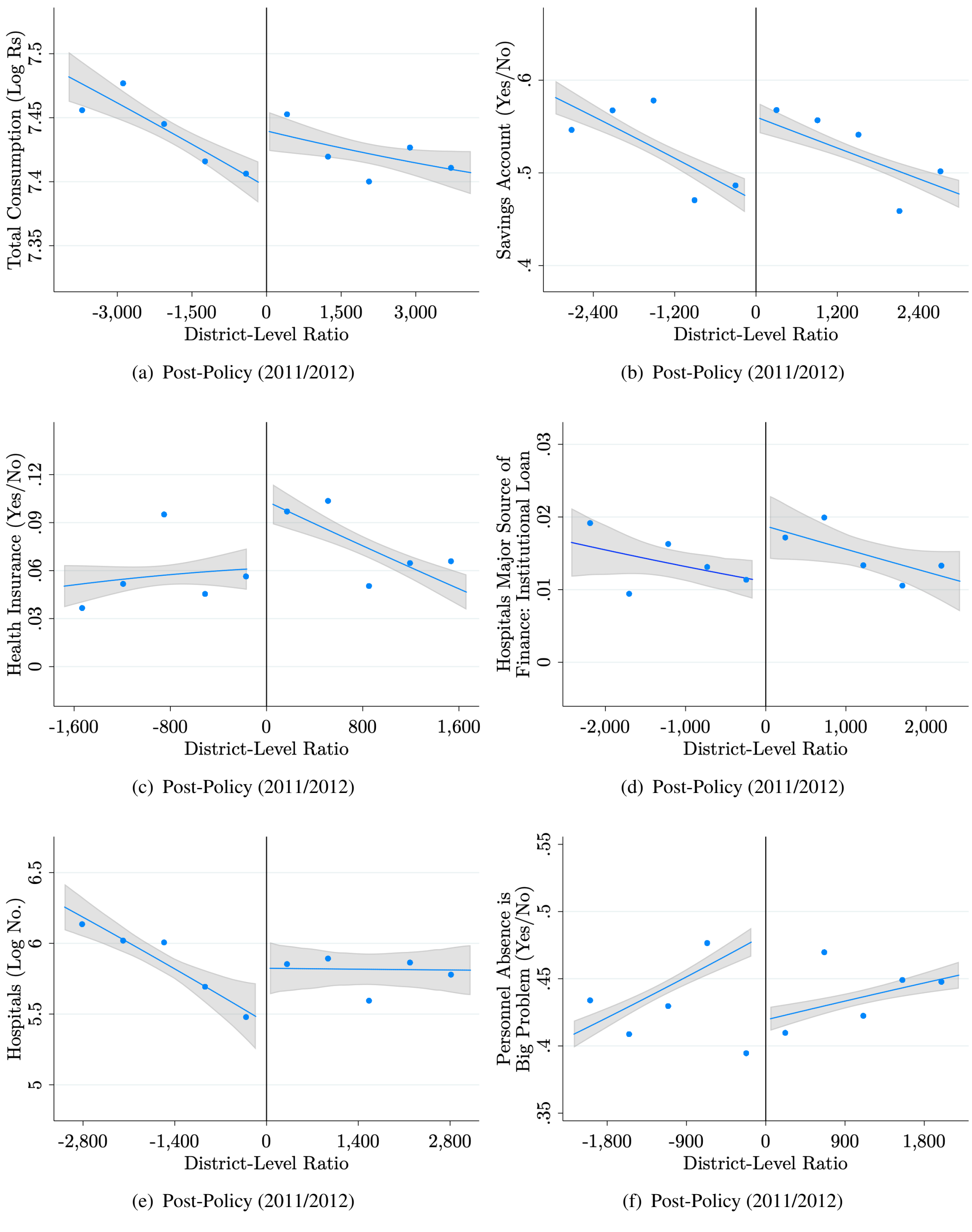
To examine whether smaller health care providers also rely on bank loans, I turn to the Economic Census, which only collects data on the major source of finance. It does not contain additional balance sheet data. Institutional loans are rarely the major source of finance for health care providers: only 1.59 percent of establishments with hospital activities and 2.00 percent of establishments with medical or dental activities list institutional loans as their major source of finance.[9] Instead, commonly cited major sources of finance for establishments with hospital activities (medical or dental activities) are self-finance with 44 percent (72 percent) and government sources with 39 percent (12 percent). That few health care providers cite institutional loans as their major source of finance does not imply that they do not rely on bank loans. Health care providers are only slightly less likely to cite an institutional loan as their major source of finance than all businesses (2.11 percent). This provides cautious evidence that they rely on bank loans.

Finally, the question arises whether the second condition is satisfied: that health care providers are credit constrained. If they were not, they would either not take up the extra bank loans or would only substitute more expensive credit. While there is no evidence available for health care providers specifically, academic research has established that, generally, medium-sized companies (Banerjee and Duflo, 2014) and small businesses (de Mel et al., 2008) in developing countries are credit constrained. I conclude that the conditions are met for credit access to health care providers to play a role.

D. Summary of Mechanisms

Summarizing, I find evidence of all the mechanisms depicted in Figure 1. Zooming into the second mechanism, bank loans do not seem to play a role for the average household. Since I observe outcomes in equilibrium, I do not identify which mechanisms are driving the results. It is however important to note that all partial equilibrium studies of households' income, savings accounts, bank loans, and health insurance do not find impacts on health. This suggests that either one of the mechanisms required a large-scale, long-term setting to play out or that an interaction between the mechanisms was crucial in general equilibrium.

[9] Note that institutional loans are likely to refer to bank loans here since other major loan distributors such as money lenders are listed under a category of non-institutional loans.



(a) Post-Policy (2011/2012)

(b) Post-Policy (2011/2012)

(c) Post-Policy (2011/2012)

(d) Post-Policy (2011/2012)

(e) Post-Policy (2011/2012)

(f) Post-Policy (2011/2012)

Figure 9. Mechanisms. These graphs show binned means to the left and right of the cutoff, within the optimal bandwidth. They also show local linear polynomials to the left and right of the cutoff, with 90 percent confidence intervals in gray.

VII. Robustness and Placebo Tests

To demonstrate the robustness of my results, I initially test whether coefficients remain statistically significant for different bandwidth choices. In a first approach, I examine bandwidth multipliers in the range of 0.50 to 2.00, in steps of 0.25. For instance, if the MSE-optimal bandwidth (Calonico et al., 2014) is 2,000, I examine bandwidths from 1,000 to 4,000. Results for main outcomes are described in Table A17 and for mechanism outcomes in Table A18. Figures A6 and A7 provide summaries. For main outcomes, considering the optimal bandwidth with multipliers of 0.75 and 1.25, 75 to 88 percent of coefficients remain statistically significant. Examining bandwidth multipliers of 0.50 and 1.50, 63 percent remain statistically significant. For mechanism outcomes, applying the bandwidth multipliers of 0.75 and 1.25, 81 percent of coefficients remain statistically significant. Considering the optimal bandwidth with multipliers of 0.50 and 1.50, 56 percent remain significant. This suggests that results are robust to different bandwidth multipliers.

In a second approach, I examine different bandwidth selectors. The default is an MSE-optimal bandwidth selector by Calonico et al. (2014) that chooses identical bandwidths to the left and to the right of the cutoff. In Tables A19, A20, A21, and A22, I also consider an MSE-optimal selector that separately chooses bandwidths to the left and to the right of the cutoff. Additionally, I examine another selector, suggested by Calonico et al. (2020), that optimizes the coverage error rate (CER). Again, I examine the selector with identical and different bandwidths to the left and right of the cutoff. Figure A8 summarizes the results. For main outcomes, 69 to 88 percent of results remain statistically significant; for mechanism outcomes, 88 to 94 percent remain statistically significant. This suggests that results are robust to different bandwidth selectors.

Results are also robust taking into account possible bias corrections due to the MSE-optimal bandwidth selector, discussed by Calonico et al. (2014) and Cattaneo and Vazquez-Bare (2017). Results are depicted in Tables A19, A20, A21, and A22, and summarized in Figure A12. All of the coefficients in both main and mechanism outcomes remain statistically significant, suggesting that findings are highly robust to these adjustments.

I next examine robustness with respect to polynomial degrees. Gelman and Imbens (2019) argue that researchers should apply either linear or quadratic approximations. Additionally, I examine robustness with respect to polynomials of degree three. Findings are described in Table A23 and A24 and summarized in Figure A9. For polynomials of degree two, 94 percent of main outcomes and 69 percent of mechanism outcomes remain statistically significant. For polynomials of degree three, not recommended by the current literature, I find that 56 percent of main outcomes and 19 percent of mechanism outcomes remain significant. Summarizing, results are highly robust to alternative polynomials suggested by the econometric literature.

Another classical regression discontinuity robustness test is to examine smoothness around placebo cutoffs. I examine three placebo cutoffs on each side of the normalized true cutoff (zero): $\pm$ 1,000, 2,000, and 3,000. This choice of placebo cutoffs ensures that there are enough observations around the placebo cutoff to conduct an analysis. Evidence is provided in Table A25 and A26 and summarized in Figure A10. I find very little evidence of discontinuities for placebo cutoffs to the left of the true cutoff; on average, only 2 percent of results are statistically significant for main outcomes and 6 percent for mechanism outcomes. To the right of the cutoff, there is stronger evidence of discontinuities, with 23 percent of results being statistically significant for main outcomes and 8 percent for mechanism outcomes. However, this is still at most only a quarter of results being significant compared to the true cutoff.

Finally, I test whether results are robust to adjustments for multiple hypothesis testing and spatial correlation of standard errors (Table A27 and A28), summarized in Figure A11. To address concerns regarding multiple hypothesis testing, I adjust for the false discovery rate, following Anderson (2008). The false discovery rate is the expected proportion of rejections that are Type I errors (false rejections). To adjust for spatial correlation of standard errors, I adjust for Conley standard errors (Conley, 1999) in district-level regressions. Since the most granular location data available for households is their district, I do not adjust household-level regressions for spatial correlation. I find that results are highly robust to multiple hypothesis testing and spatial correction; 100 percent of coefficients for main and mechanism outcomes remain statistically significant.

VIII. Conclusion

I utilize a natural experiment to study the relationship between finance and health in a large-scale, long-term general equilibrium setting. In contrast to partial equilibrium studies, I find that finance can substantially improve the health of households. Six years after a Reserve Bank of India policy incentivized banks to enter underserved areas, households in treatment districts are a third less likely to suffer from a non-chronic illness in a month. This positively affects health-related economic outcomes. They miss fewer days of work or school due to illness and have lower medical expenses. Ten years after the policy introduction, I observe persistently lower morbidity rates, higher vaccination rates, and lower risks associated with pregnancies. Exploring mechanisms of the relationship between bank presence and health, I highlight two aspects of banking previously understudied: banks provide health insurance to households and credit to hospitals.

This paper has important implications for policy and future research. Policymakers can conclude that it can be beneficial for the health of their citizens to incentivize banks to enter underserved locations. They might also focus on the interaction of banks with local providers of services that policymakers want to foster. Indeed, the RBI announced a new policy in May 2021 to incentivize banks to quickly deliver credit to health care providers in light of the COVID crisis, announcing plans to inject USD 6.78 billion of liquidity. This paper also speaks to researchers, suggesting promising new areas of interest. One open question is to what extent the different mechanisms contribute to improving health, requiring exogenous variation in, for instance, credit access to health care providers only. A second line of inquiry is asking whether other dimensions of wellbeing, such as education, can be positively impacted by bank presence. Like health care providers, education service providers are likely to be credit constrained. Gaining an understanding of these questions could significantly advance our knowledge of the impact of finance and the scope of policymakers to improve their citizens' wellbeing.

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Appendix (for online publication)

Tables

Table A1: Branch Summary Statistics

	All districts				[-3,000,+3,000]			
	1997	2004	2010	2016	1997	2004	2010	2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Branch licenses (no.)	65	72	103	115	73	80	117	132
	(67)	(78)	(120)	(140)	(59)	(67)	(102)	(120)
Branches (no.)	65	71	103	172	73	79	116	198
	(68)	(76)	(116)	(185)	(59)	(66)	(100)	(166)
Observations	581	581	581	581	199	199	199	199

Standard deviations in parentheses. Data RBI. District level. All variables are winsorized at the 1st and 99th percentile. Regional rural banks are excluded. Between 2004, one year before the policy, and 2016, the final year of the last survey, I generally observe a large branch growth of 142 percent in the average district. Districts with a population-to-branch ratio in the range of $\pm 3,000$ of the policy cutoff have a slightly higher number of branches on average.

Table A2: Households Summary Statistics (IHDS)

	IHDS I 2004/2005		IHDS II 2011/2012	
	All districts (1)	[-3,000,+3,000] (2)	All districts (3)	[-3,000,+3,000] (4)
Consumption				
Total consumption (Rs)	705	699	1,841	1,828
	(315)	(309)	(823)	(804)
Food consumption (Rs)	372	370	848	844
	(121)	(119)	(281)	(278)
Financial Access				
Savings account (yes/no)			0.57	0.53
			(0.49)	(0.50)
Any loan (yes/no)	0.46	0.48	0.60	0.62
	(0.50)	(0.50)	(0.49)	(0.49)
Any bank loan (yes/no)			0.22	0.23
			(0.41)	(0.42)
Largest loan from bank (yes/no)	0.11	0.12	0.18	0.17
	(0.32)	(0.32)	(0.38)	(0.38)
Largest loan amt (Rs)	4,482	4,862	15,448	17,134
	(8,698)	(9,048)	(25,365)	(26,498)
Health insurance (yes/no)	0.03	0.02	0.11	0.08
	(0.16)	(0.15)	(0.31)	(0.28)
Health				
Days ill (yes/no)	0.47	0.45	0.55	0.52
	(0.50)	(0.50)	(0.50)	(0.50)
Days ill (no.)	2.78	2.54	3.23	2.97
	(4.08)	(3.89)	(4.19)	(4.00)
Days missed (yes/no)	0.36	0.34	0.40	0.39
	(0.48)	(0.47)	(0.49)	(0.49)
Days missed (no.)	1.46	1.36	1.64	1.60
	(2.62)	(2.51)	(2.76)	(2.73)
Treatment spending (yes/no)	0.45	0.43	0.53	0.51
	(0.50)	(0.49)	(0.50)	(0.50)
Treatment spending (Rs)	43	41	126	121
	(82)	(80)	(204)	(202)
Observations	39,584	16,184	41,703	16,965

Standard deviations in parentheses. Data IHDS I (2004/2005) and IHDS II (2011/2012). Household level. Variables in Rs or days are trimmed at the 10th and 90th percentile. No entry if not available in IHDS I. Amounts in Indian rupees are not inflation adjusted; inflation was 70 percent between 2004 and 2011. Generally speaking, I observe a positive trend in consumption measures and financial access, while health status remained stable over the period between 2004/2005 and 2011/2012. Assume that general consumption measures are positively correlated with being more sensitive or informed about illnesses. This would explain that health status does not improve, e.g., the number of days ill would have an upward bias due to self-reporting. Notice that this bias would go in the other direction than the effects I detect; in treatment districts, consumption measures show increases, but health measures show decreases. Self-reporting effects thus make it potentially less likely for me to find an effect on health. I complement evidence on health status with measures that are not self-reported. Additionally, I observe that households in districts within the range of -3,000 to +3,000 of the normalized ratio are remarkably similar to households in all districts, strengthening external validity of my design.

Table A3: Households Summary Statistics (DHS)

	DHS 2015/2016	
	All districts (1)	[-3,000,+3,000] (2)
Morbidity		
Sick child (yes/no)	0.27	0.26
	(0.45)	(0.44)
Health Care Visits		
Any reason (yes/no)	0.28	0.28
	(0.45)	(0.45)
Children's treatment (yes/no)	0.11	0.10
	(0.31)	(0.30)
Women's treatment (yes/no)	0.16	0.16
	(0.37)	(0.37)
Facility delivery (yes/no)	0.02	0.02
	(0.13)	(0.13)
Generally go to: public provider (yes/no)	0.53	0.54
	(0.50)	(0.50)
Generally go to: private provider (yes/no)	0.44	0.44
	(0.50)	(0.50)
Generally go to: drug shop etc. (yes/no)	0.00	0.00
	(0.05)	(0.05)
Vaccinations		
Vaccinated child (yes/no)	0.85	0.86
	(0.36)	(0.35)
Pregnancies		
Experienced miscarriage (yes/no)	0.04	0.04
	(0.20)	(0.19)
Experienced stillbirth (yes/no)	0.01	0.00
	(0.08)	(0.07)
Health Care Supply		
Big problem: distance to provider (yes/no)	0.36	0.34
	(0.48)	(0.47)
Big problem: transport to provider (yes/no)	0.34	0.32
	(0.47)	(0.47)
Big problem: no personnel (yes/no)	0.52	0.51
	(0.50)	(0.50)
Big problem: no female personnel (yes/no)	0.43	0.42
	(0.50)	(0.49)
Big problem: no drugs (yes/no)	0.53	0.52
	(0.50)	(0.50)
Observations	487,109	172,149

Standard deviations in parentheses. Data DHS (2015/2016). Household level.

Table A4: Economic Census District-Level Summary Statistics

	All districts		[-3,000,+3,000]	
	2005	2013	2005	2013
	(1)	(2)	(3)	(4)
Hospitals				
Hospitals (no.)	314	464	418	549
	(366)	(471)	(396)	(483)
Major source bank financing (yes/no)	0.02	0.02	0.03	0.01
	(0.03)	(0.02)	(0.03)	(0.02)
Other medical service providers				
Other medical service providers (no.)	448	546	494	556
	(658)	(829)	(628)	(772)
Major source bank financing (yes/no)	0.03	0.02	0.03	0.01
	(0.05)	(0.06)	(0.03)	(0.02)
All businesses				
All businesses (no.)	70,259	98,882	87,510	119,033
	(73,894)	(104,648)	(75,932)	(105,646)
Major source bank financing (yes/no)	0.03	0.02	0.03	0.02
	(0.03)	(0.02)	(0.02)	(0.01)
Observations	576	576	198	198

Standard deviations in parentheses. Data Economic Census. Household level. All variables in numbers are winsorized at the 1st and 99th percentile. Districts in the range of $\pm 3,000$ of the policy cutoff ratio have a slightly higher number of hospitals, other medical service providers, and all businesses.

Table A5: Economic Activity and Population Characteristics Are Smooth Pre-Policy

	1990	1991	...	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Nightlights															
Total light (log)				-0.07	-0.03	0.03	0.06	-0.03	0.16	0.02	0.05	-0.16	-0.00	-0.13	-0.06
				(0.25)	(0.27)	(0.28)	(0.28)	(0.28)	(0.27)	(0.28)	(0.31)	(0.27)	(0.30)	(0.29)	(0.29)
Economic Census															
Empl. (log no.)	-0.16							-0.04							0.07
	(0.25)							(0.15)							(0.13)
Empl. manuf. (log no.)	-0.05							-0.04							0.02
	(0.19)							(0.14)							(0.16)
Empl. services (log no.)	-0.16							0.03							0.06
	(0.24)							(0.11)							(0.13)
Population Census															
Pop. (log no.)		0.01									-0.00				
		(0.11)									(0.10)				
Pop. rural (log no.)		0.01									0.00				
		(0.10)									(0.10)				
Pop. urban (log no.)		-0.11									-0.06				
		(0.08)									(0.08)				
Pop. literate (log no.)		-0.05									-0.07				
		(0.14)									(0.11)				
Tar road (yes/no)		-0.08									0.04				
		(0.07)									(0.06)				

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data SHRUG. District level. Combining different data sets, including night-light data, Economic Census data, and Population Census data. The unit of observation is town or village. I test whether units in treatment districts have, e.g., higher night light than units in control districts prior to the policy. The variables are defined as follows. Total light is the sum of the luminosity values of all pixels in a unit, obtained from the DMSP-OLS annual measures of nighttime luminosity. Employment measures the total employment, followed by a split by manufacturing and services. Population measures the total population, followed by a split in rural and urban. The last two variables from the Population Census measure the total literate population and whether there is a tar road.

Table A6: Negligible Migration

	Migrated 5 years ago from other district (yes/no) (1)	Migrated anytime in past 90 years from other district (yes/no) (2)	Migrated 5 years ago from anywhere (yes/no) (3)
Treated	0.01	0.05	0.01
	(0.00)	(0.04)	(0.01)
Control Mean	0.00	0.11	0.01
Change (%)	284.06	46.22	90.26
First Stage	0.54	0.66	0.61
Bandwidth	1,633	2,363	1,982
Observations in BW	8,104	12,862	9,783
Total Observations	34,415	36,805	34,832
Baseline Control	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. Data IHDS II (2011/2012). Household level.

Table A7: Other Policies Do Not Confound Results (1/2)

	Priority districts				
	NHM (yes/no) (1)	ICDS (1st wave) (yes/no) (2)	ISSNIP (yes/no) (3)	NREGA (1st wave) (yes/no) (4)	NREGA (2nd wave) (yes/no) (5)
Treated	0.21	-0.14	-0.23	-0.25	-0.02
	(0.20)	(0.19)	(0.19)	(0.23)	(0.25)
Control Mean	0.18	0.25	0.15	0.16	0.24
Change (%)	118.66	-57.84	-152.46	-151.04	-8.59
First Stage	0.70	0.77	0.78	0.70	0.67
Bandwidth	2,671	4,160	4,595	2,706	2,290
Observations in BW	176	260	290	181	151
Total Observations	581	581	581	581	581
Baseline Control	No	No	No	No	No

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. Data Ministry of Health and Family Welfare, Ministry of Women and Child Development, Ministry of Rural Development. District level. Regressions do not include state-level fixed effects.

Table A8: Other Policies Do Not Confound Results (2/2)

	Priority districts				
	NHM	ICDS (1st wave)	ISSNIP	NREGA (1st wave)	NREGA (2nd wave)
	(1)	(2)	(3)	(4)	(5)
All sample					
Total priority districts (no.)	169	180	156	196	125
Total priority districts (%)	29	31	27	34	22
Priority districts above cutoff (no.)	135	142	136	170	85
Priority districts above cutoff (%)	36	38	36	45	23
Priority districts below cutoff (no.)	34	38	20	26	40
Priority districts below cutoff (%)	17	19	10	13	20
Corr priority district and 1[Above]	0.20	0.20	0.28	0.33	0.04
Within bandwidth [-4,000,+4,000]					
Total priority districts (no.)	58	68	57	71	47
Total priority districts (%)	23	27	23	28	19
Priority districts above cutoff (no.)	37	42	39	54	23
Priority districts above cutoff (%)	26	29	27	38	16
Priority districts below cutoff (no.)	21	26	18	17	24
Priority districts below cutoff (%)	19	24	16	15	22
Corr priority district and 1[Above]	0.08	0.06	0.13	0.25	-0.07

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data Ministry of Health and Family Welfare, Ministry of Women and Child Development, Ministry of Rural Development. District level. Percent refers to the number of total districts within a given category; e.g., for priority districts above cutoff (%) within bandwidth, they constitute 26 percent of all districts above the cutoff within bandwidth.

Table A9: Placebo Test: Regional Rural Banks Do Not React to the Policy

	Post-Policy (2010)	
	Branch Licenses (log no.)	Branches (log no.)
	(1)	(2)
Treated	-0.54	-0.08
	(0.48)	(0.48)
Control Mean	1.51	1.09
Change (%)	-41.94	-7.63
First Stage	0.80	0.80
Bandwidth	2,812	2,959
Observations in BW	187	195
Total Observations	561	561
Baseline Control	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data RBI. District level. All variables are winsorized at the 1st and 99th percentile. Only regional rural banks are analyzed.

Table A10: Lower Morbidity Rate Holds With Baseline Control

	Morbidity		Economic consequences			
	Days ill		Days missed		Medical expenses	
	(yes/no)	(log no.)	(yes/no)	(log no.)	(yes/no)	(log Rs)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.18**	-0.25**	-0.25***	-0.39***	-0.15**	-0.61**
	(0.07)	(0.12)	(0.08)	(0.12)	(0.06)	(0.31)
Control Mean	0.53	0.81	0.40	0.57	0.51	2.05
Change (%)	-34.11	-22.37	-63.27	-32.40	-30.30	-45.60
First Stage	0.67	0.70	0.68	0.70	0.68	0.70
Bandwidth	2,600	2,894	3,237	3,087	3,139	3,867
Observations in BW	13,870	14,559	17,374	15,469	17,113	17,435
Total Observations	35,294	27,603	35,371	29,387	35,294	28,610
Baseline Control	Yes	Yes	Yes	Yes	Yes	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. All variables measured in currency Rs are in log form and trimmed at the 10th and 90th percentile. All variables refer to non-chronic illnesses such as fever. Days missed measures the number of days that the household was not able to do usual activities and had to miss work or school. All questions refer to the past 30 days.

Table A11: Morbidity Rates Are Smooth Pre-Policy

	Days ill		Days missed		Medical expenses	
	(yes/no)	(log no.)	(yes/no)	(log no.)	(yes/no)	(log Rs)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.06	-0.11	-0.11	-0.19	-0.08	-0.14
	(0.06)	(0.13)	(0.08)	(0.14)	(0.06)	(0.27)
Control Mean	0.42	0.64	0.34	0.48	0.41	1.32
Change (%)	-14.77	-10.49	-31.90	-17.68	-18.73	-13.03
First Stage	0.71	0.69	0.67	0.67	0.72	0.69
Bandwidth	4,432	3,418	2,797	2,524	4,580	3,566
Observations in BW	20,799	15,574	14,730	12,122	21,585	16,019
Total Observations	35,480	31,375	35,294	32,442	35,480	31,812
Baseline Control	No	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS I (2004/2005). Household level. All variables measured in currency Rs are in log form and trimmed at the 10% and 90% level. All illnesses refer to fever, diarrhea, or cough. Days missed measures the number of days that the household was not able to do usual activities and had to miss work or school. All questions refer to the past 30 days.

Table A12: Placebo Test: No Effect on Chronic Illnesses

	Morbidity	Economic consequences			
	Days ill	Days missed		Medical expenses	
	(yes/no)	(yes/no)	(log no.)	(yes/no)	(log Rs)
	(1)	(2)	(3)	(4)	(5)
Treated	-0.00	-0.05	-0.02	0.00	-0.20
	(0.05)	(0.05)	(0.15)	(0.05)	(0.37)
Control Mean	0.39	0.30	0.59	0.37	1.67
Change (%)	-0.96	-15.55	-1.57	0.02	-17.98
First Stage	0.65	0.62	0.60	0.64	0.58
Bandwidth	2,189	2,038	1,934	2,107	1,920
Observations in BW	11,716	9,962	8,697	10,981	8,700
Total Observations	35,103	34,883	31,426	35,103	31,621
Baseline Control	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. All variables measured in currency Rs are in log form and trimmed at the 10% and 90% level. All illnesses refer to a variety of chronic diseases including cancer, diabetes, or heart disease. Days missed measures the number of days that the household was not able to do usual activities and had to miss work or school. All questions refer to the past 365 days.

Table A13: No Effect on State Expenditure

	Medical and Public Health (log lakh Rs)	Water supply and Sanitation (log lakh Rs)	Nutrition (log lakh Rs)
	(1)	(2)	(3)
Treated	-0.04	0.06	0.01
	(0.13)	(0.14)	(0.24)
Control Mean	11.92	10.45	10.57
Mean Change (%)	-3.66	6.36	1.30
First Stage	0.86	0.82	0.82
Bandwidth	5,102	5,744	6,892
Observations in BW	264	266	250
Total Observations	444	459	371
Baseline Control	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data RBI (2010). Variable in lakh Rs and transformed to log plus trimmed at the 10th and 90th percentile.

Table A14: Financial Access is Smooth Pre-Policy

	Any loan (yes/no) (1)	Largest loan amount (log Rs) (2)	Largest loan from bank (yes/no) (3)	Health insurance (yes/no) (4)
Treated	0.00	0.30	-0.00	0.01
	(0.10)	(0.83)	(0.03)	(0.01)
Control Mean	0.41	3.11	0.12	0.02
Change (%)	0.18	35.62	-3.14	55.55
First Stage	0.68	0.69	0.70	0.68
Bandwidth	3,345	3,404	4,115	3,086
Observations in BW	17,045	15,459	19,417	16,057
Total Observations	35,369	32,271	35,420	35,204
Baseline Control	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS I (2004/2005). Variable in Rs is transformed to log and trimmed at the 10th and 90th percentile.

Table A15: Larger Effects with High Probability to Take Up Financial Instruments

	Savings account		Bank loan		Health insurance	
	High	Low	High	Low	High	Low
	Days ill (yes/no) (1)	Days ill (yes/no) (2)	Days ill (yes/no) (3)	Days ill (yes/no) (4)	Days ill (yes/no) (5)	Days ill (yes/no) (6)
Treated	-0.29**	-0.10*	-0.24**	-0.12**	-0.33***	-0.07
	(0.12)	(0.06)	(0.11)	(0.06)	(0.12)	(0.08)
Control Mean	0.53	0.53	0.53	0.53	0.53	0.56
Change (%)	-55.10	-19.27	-45.52	-23.61	-62.55	-13.31
First Stage	0.57	0.75	0.59	0.73	0.55	0.82
Bandwidth	2,222	2,953	2,226	2,916	2,336	1,718
Observations in BW	7,656	5,976	7,608	5,934	7,838	3,506
Total Observations	23,061	13,739	23,249	13,555	22,687	13,731
Baseline Control	No	No	No	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data IHDS II (2011/2012). Household level. I run three predictions, one for each of taking up a savings account (Columns 1 and 2), having a bank loan (Columns 3 and 4), and having health insurance (Columns 5 and 6). All predictions are run with state fixed effects within a bandwidth of -3,000 to +3,000. Baseline characteristics from the IHDS I are whether their largest loan was from a bank, whether they are urban, their assets, and their per capita consumption. The odd columns use the sample of households in the upper half of the respective distribution. The even columns use the sample of households in the lower half of the respective distribution. The outcome is the number of days ill in the IHDS I.

Table A16: Shift Towards Private Providers

	Generally go for treatment to		
	Government provider (yes/no) (1)	Private provider (yes/no) (2)	Shop or stay home (yes/no) (3)
Treated	-0.06**	0.10***	-0.00
	(0.03)	(0.03)	(0.00)
Control Mean	0.52	0.45	0.00
Mean Change (%)	-12.42	21.19	-2.43
First Stage	0.73	0.71	0.69
Bandwidth	2,898	2,648	2,262
Observations in BW	202,459	184,429	156,853
Total Observations	577,928	577,928	566,715
Baseline Control	No	No	No

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Data DHS (2015/2016). Household level.

Table A17: Robustness to Different Bandwidth Multipliers: Main Results

	Bandwidth Multiplier						
	x0.50	**x0.75**	**x1.00**	**x1.25**	**x1.50**	**x1.75**	**x2.00**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Banks (Table 2)							
Branch licenses 2010 (log no.)	0.17**	0.21***	0.19***	0.17***	0.15***	0.14***	0.14***
	(0.08)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Branches 2010 (log no.)	0.15*	0.19***	0.17***	0.14**	0.13**	0.12**	0.14***
	(0.08)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)
Households' health (Table 3)							
Days ill (yes/no)	-0.48	-0.26	-0.19**	-0.18***	-0.16**	-0.15**	-0.12**
	(0.35)	(0.17)	(0.09)	(0.07)	(0.07)	(0.06)	(0.06)
Days ill (log no.)	-0.55	-0.32*	-0.29**	-0.25**	-0.20*	-0.16	-0.13
	(0.41)	(0.17)	(0.12)	(0.12)	(0.11)	(0.11)	(0.10)
Days missed (yes/no)	-0.69	-0.39**	-0.30***	-0.28***	-0.25***	-0.22***	-0.19***
	(0.42)	(0.18)	(0.10)	(0.08)	(0.08)	(0.07)	(0.06)
Days missed (log no.)	-0.80*	-0.52***	-0.44***	-0.39***	-0.34***	-0.28***	-0.24***
	(0.44)	(0.20)	(0.13)	(0.12)	(0.11)	(0.10)	(0.09)
Medical expenses (yes/no)	-0.39	-0.21*	-0.18**	-0.17**	-0.15**	-0.13**	-0.11*
	(0.28)	(0.13)	(0.08)	(0.07)	(0.06)	(0.06)	(0.06)
Medical expenses (log Rs)	-1.65	-1.03**	-0.88**	-0.72**	-0.58*	-0.46*	-0.38
	(1.03)	(0.45)	(0.35)	(0.32)	(0.29)	(0.27)	(0.25)
Households' health (Table 4)							
Vaccinated child (yes/no)	0.13***	0.11**	0.07*	0.05	0.04	0.02	0.02
	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Sick child (yes/no)	-0.12***	-0.10***	-0.06*	-0.04	-0.03	-0.02	-0.02
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
HC visit (any reason) (yes/no)	-0.17***	-0.11***	-0.08**	-0.05*	-0.03	-0.02	-0.01
	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
HC visit (child's treatment) (yes/no)	-0.07***	-0.04**	-0.02*	-0.02	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Households' health (Table 5)							
Health care facility delivery (yes/no)	-0.001	0.003	0.005***	0.005***	0.005***	0.005***	0.004**
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Experienced miscarriage (yes/no)	-0.020*	-0.013*	-0.010*	-0.010*	-0.009**	-0.008**	-0.007**
	(0.010)	(0.007)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)
Experienced stillbirth (yes/no)	-0.006**	-0.003**	-0.002*	-0.002*	-0.001	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
HC visit (woman's treatment) (yes/no)	-0.109**	-0.074**	-0.051*	-0.036	-0.020	-0.007	-0.004
	(0.048)	(0.033)	(0.027)	(0.024)	(0.022)	(0.020)	(0.018)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. For details of the regression, refer to the respective main table. Summarized in Figure A6.

Table A18: Robustness to Different Bandwidth Multipliers: Mechanisms Results

	Bandwidth Multiplier						
	x0.50	**x0.75**	**x1.00**	**x1.25**	**x1.50**	**x1.75**	**x2.00**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Households' consumption (Table 6)							
Total consumption (log Rs)	0.10**	0.08**	0.07**	0.05	0.04	0.03	0.03
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Food consumption (log Rs)	-0.00	0.04	0.06*	0.06*	0.06*	0.05	0.04
	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Meals per day (no.)	0.42*	0.25**	0.24**	0.25***	0.26***	0.24***	0.23***
	(0.23)	(0.12)	(0.10)	(0.09)	(0.09)	(0.08)	(0.07)
Outpatient expenses (log Rs)	-0.43	-0.58**	-0.45*	-0.35*	-0.25	-0.21	-0.16
	(0.40)	(0.26)	(0.23)	(0.21)	(0.19)	(0.16)	(0.14)
Households' financial access (Table 7)							
Savings account (yes/no)	0.21	0.24*	0.19*	0.17**	0.12*	0.08	0.06
	(0.22)	(0.13)	(0.10)	(0.08)	(0.07)	(0.07)	(0.06)
Health insurance (yes/no)	0.25	0.25**	0.17**	0.12**	0.08*	0.05	0.04
	(0.18)	(0.12)	(0.07)	(0.05)	(0.04)	(0.04)	(0.03)
Health care supply (Table 8, 9, and 11)							
Institutional loan (share)	0.008	0.008	0.010**	0.010**	0.011**	0.011***	0.011***
	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Institutional loan (share) - private	0.002	0.015	0.020**	0.022***	0.020**	0.019**	0.019**
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
Hospitals (log no.)	1.74**	1.27***	0.88***	0.62**	0.42*	0.29	0.21
	(0.77)	(0.45)	(0.33)	(0.27)	(0.24)	(0.22)	(0.20)
Hospitals (log no.) - private	2.01**	1.22**	0.84**	0.57*	0.34	0.23	0.15
	(0.87)	(0.47)	(0.36)	(0.31)	(0.27)	(0.25)	(0.22)
Hospitals (log no.) - government	1.38*	0.88**	0.64*	0.50*	0.41*	0.33	0.25
	(0.72)	(0.43)	(0.33)	(0.28)	(0.24)	(0.21)	(0.19)
Survey on problems (Table 10)							
Distance to facility (yes/no)	-0.33**	-0.21***	-0.12***	-0.04	-0.01	0.00	0.00
	(0.14)	(0.07)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Taking transport to facility (yes/no)	-0.37**	-0.19***	-0.11***	-0.04	-0.00	0.01	0.02
	(0.18)	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
No personnel at facility (yes/no)	-0.20*	-0.23***	-0.14**	-0.11*	-0.08	-0.06	-0.05
	(0.12)	(0.08)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
No female personnel at facility (yes/no)	-0.34*	-0.31***	-0.20**	-0.15**	-0.11**	-0.09*	-0.08*
	(0.18)	(0.11)	(0.08)	(0.06)	(0.06)	(0.05)	(0.05)
No drugs at facility (yes/no)	-0.22	-0.19**	-0.15**	-0.10*	-0.06	-0.04	-0.03
	(0.14)	(0.08)	(0.07)	(0.06)	(0.05)	(0.05)	(0.05)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. For details of the regression, refer to the respective main table. Summarized in Figure A6.

Table A19: Robustness to Different Bandwidth Selectors: Main Results (1/2)

	MSE-optimal		CER-optimal	
	Common	**Two-sided**	**Common**	**Two-sided**
	(1)	(2)	(3)	(4)
Banks (Table 2)				
Branch licenses 2010 (log no.)	0.19***	0.20***	0.23***	0.18***
	(0.05)	(0.06)	(0.07)	(0.06)
	0.24***	0.27***	0.26***	0.22***
	(0.05)	(0.06)	(0.07)	(0.06)
	0.24***	0.27***	0.26***	0.22***
	(0.06)	(0.07)	(0.07)	(0.07)
Branches 2010 (log no.)	0.17***	0.17***	0.20***	0.17**
	(0.06)	(0.06)	(0.07)	(0.07)
	0.21***	0.24***	0.22***	0.20***
	(0.06)	(0.06)	(0.07)	(0.07)
	0.21***	0.24***	0.22***	0.20***
	(0.07)	(0.07)	(0.07)	(0.07)
Households' health (Table 3)				
Days ill (yes/no)	-0.19**	-0.13*	-0.26	-0.17
	(0.09)	(0.08)	(0.17)	(0.15)
	-0.21**	-0.16**	-0.28*	-0.19
	(0.09)	(0.08)	(0.17)	(0.15)
	-0.21**	-0.16	-0.28	-0.19
	(0.11)	(0.10)	(0.19)	(0.17)
Days ill (log no.)	-0.29**	-0.24*	-0.32*	-0.41
	(0.12)	(0.13)	(0.17)	(0.26)
	-0.36***	-0.29**	-0.35**	-0.45*
	(0.12)	(0.13)	(0.17)	(0.26)
	-0.36**	-0.29*	-0.35*	-0.45
	(0.15)	(0.17)	(0.19)	(0.29)
Days missed (yes/no)	-0.30***	-0.25***	-0.40**	-0.38**
	(0.10)	(0.09)	(0.19)	(0.17)
	-0.35***	-0.32***	-0.44**	-0.43**
	(0.10)	(0.09)	(0.19)	(0.17)
	-0.35***	-0.32***	-0.44**	-0.43**
	(0.12)	(0.11)	(0.21)	(0.20)
Days missed (log no.)	-0.44***	-0.42***	-0.52***	-0.57**
	(0.13)	(0.13)	(0.20)	(0.25)
	-0.54***	-0.51***	-0.59***	-0.64**
	(0.13)	(0.13)	(0.20)	(0.25)
	-0.54***	-0.51***	-0.59***	-0.64**
	(0.16)	(0.17)	(0.22)	(0.28)
Medical expenses (yes/no)	-0.18**	-0.14*	-0.21*	-0.19
	(0.08)	(0.07)	(0.13)	(0.14)
	-0.20***	-0.16**	-0.23*	-0.21
	(0.08)	(0.07)	(0.13)	(0.14)
	-0.20**	-0.16*	-0.23	-0.21
	(0.09)	(0.09)	(0.14)	(0.15)
Medical expenses (log Rs)	-0.88**	-0.63*	-1.04**	-0.94*
	(0.35)	(0.34)	(0.46)	(0.53)
	-1.10***	-0.83**	-1.19***	-1.10**
	(0.35)	(0.34)	(0.46)	(0.53)
	-1.10***	-0.83**	-1.19**	-1.10*
	(0.41)	(0.42)	(0.50)	(0.60)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The first and second columns are MSE-optimal bandwidths, initially identical and then different to the left and right of the cutoff. The third and fourth columns indicate CER (coverage error rate)-optimal bandwidths, first identical and then different to the left and right of the cutoff (Calonico et al., 2020). In each parcel, I first report the conventional RD estimator with conventional variance estimator. Below that is the bias-corrected RD estimator with the conventional variance estimator, followed by the bias-corrected RD estimator with robust variance estimator (Calonico et al., 2014). For details of the regression, refer to the respective main table. Summarized in Figures A8 and A12.

Table A20: Robustness to Different Bandwidth Selectors: Main Results (2/2)

	MSE-optimal		CER-optimal	
	Common	**Two-sided**	**Common**	**Two-sided**
	(1)	(2)	(3)	(4)
Households' health (Table 4)				
Vaccinated child (yes/no)	0.07*	0.06	0.11**	0.07
	(0.04)	(0.04)	(0.05)	(0.05)
	0.10**	0.07**	0.13**	0.08*
	(0.04)	(0.04)	(0.05)	(0.05)
	0.10**	0.07*	0.13**	0.08
	(0.05)	(0.04)	(0.06)	(0.05)
Sick child (yes/no)	-0.06*	-0.04	-0.11***	-0.08*
	(0.03)	(0.03)	(0.04)	(0.04)
	-0.08**	-0.06*	-0.12***	-0.09**
	(0.03)	(0.03)	(0.04)	(0.04)
	-0.08*	-0.06	-0.12***	-0.09*
	(0.04)	(0.04)	(0.04)	(0.05)
HC visit (any reason) (yes/no)	-0.08**	-0.04	-0.12***	-0.09**
	(0.03)	(0.03)	(0.04)	(0.03)
	-0.11***	-0.07**	-0.14***	-0.11***
	(0.03)	(0.03)	(0.04)	(0.03)
	-0.11***	-0.07*	-0.14***	-0.11***
	(0.04)	(0.04)	(0.05)	(0.04)
HC visit (child's treatment) (yes/no)	-0.02*	-0.03*	-0.04**	-0.04**
	(0.01)	(0.01)	(0.02)	(0.02)
	-0.04**	-0.04***	-0.05***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.02)
	-0.04*	-0.04**	-0.05**	-0.05***
	(0.02)	(0.02)	(0.02)	(0.02)
Households' health (Table 5)				
Health care facility delivery (yes/no)	0.005***	0.006***	0.003	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
	0.006***	0.007***	0.003*	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
	0.006**	0.007***	0.003	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
Experienced miscarriage (yes/no)	-0.010*	-0.009*	-0.013*	-0.011*
	(0.006)	(0.005)	(0.007)	(0.006)
	-0.012**	-0.011**	-0.015**	-0.013*
	(0.006)	(0.005)	(0.007)	(0.006)
	-0.012*	-0.011*	-0.015*	-0.013*
	(0.007)	(0.007)	(0.008)	(0.007)
Experienced stillbirth (yes/no)	-0.002*	-0.001	-0.004**	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
	-0.003**	-0.001	-0.004***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
	-0.003*	-0.001	-0.004**	-0.003*
	(0.001)	(0.001)	(0.002)	(0.002)
HC visit (woman's treatment) (yes/no)	-0.051*	-0.027	-0.076**	-0.060**
	(0.027)	(0.024)	(0.034)	(0.029)
	-0.077***	-0.051**	-0.094***	-0.078***
	(0.027)	(0.024)	(0.034)	(0.029)
	-0.077**	-0.051*	-0.094**	-0.078**
	(0.033)	(0.030)	(0.039)	(0.034)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The first and second columns are MSE-optimal bandwidths, initially identical and then different to the left and right of the cutoff. The third and fourth columns indicate CER (coverage error rate)-optimal bandwidths, first identical and then different to the left and right of the cutoff (Calonico et al., 2020). In each parcel, I first report the conventional RD estimator with conventional variance estimator. Below that is the bias-corrected RD estimator with the conventional variance estimator, followed by the bias-corrected RD estimator with robust variance estimator (Calonico et al., 2014). For details of the regression, refer to the respective main table. Summarized in Figures A8 and A12.

Table A21: Robustness to Different Bandwidth Selectors: Mechanism Results (1/2)

	MSE-optimal		CER-optimal	
	Common	**Two-sided**	**Common**	**Two-sided**
	(1)	(2)	(3)	(4)
Households' consumption (Table 6)				
Total consumption (log Rs)	0.07**	0.07**	0.08**	0.10**
	(0.04)	(0.03)	(0.04)	(0.04)
	0.10***	0.10***	0.10***	0.12***
	(0.04)	(0.03)	(0.04)	(0.04)
	0.10**	0.10**	0.10**	0.12***
	(0.04)	(0.04)	(0.04)	(0.04)
Food consumption (log Rs)	0.06*	0.07*	0.03	0.07
	(0.03)	(0.04)	(0.04)	(0.05)
	0.08**	0.09**	0.05	0.08
	(0.03)	(0.04)	(0.04)	(0.05)
	0.08**	0.09*	0.05	0.08
	(0.04)	(0.05)	(0.05)	(0.06)
Meals per day (no.)	0.24**	0.37**	0.25**	0.58**
	(0.10)	(0.16)	(0.13)	(0.27)
	0.29***	0.48***	0.28**	0.66**
	(0.10)	(0.16)	(0.13)	(0.27)
	0.29**	0.48**	0.28**	0.66*
	(0.12)	(0.19)	(0.14)	(0.35)
Outpatient expenses (log Rs)	-0.45**	-0.43*	-0.59**	-0.56*
	(0.23)	(0.23)	(0.26)	(0.31)
	-0.57**	-0.53**	-0.69***	-0.65**
	(0.23)	(0.23)	(0.26)	(0.31)
	-0.57**	-0.53*	-0.69**	-0.65*
	(0.27)	(0.27)	(0.30)	(0.34)
Households' financial access (Table 7)				
Savings account (yes/no)	0.19*	0.22*	0.24*	0.26
	(0.10)	(0.11)	(0.13)	(0.18)
	0.27***	0.30***	0.30**	0.32*
	(0.10)	(0.11)	(0.13)	(0.18)
	0.27**	0.30**	0.30**	0.32
	(0.12)	(0.14)	(0.14)	(0.20)
Health insurance (yes/no)	0.17**	0.08	0.25**	0.16**
	(0.07)	(0.05)	(0.12)	(0.07)
	0.20***	0.09*	0.29**	0.18***
	(0.07)	(0.05)	(0.12)	(0.07)
	0.20**	0.09	0.29**	0.18**
	(0.08)	(0.06)	(0.13)	(0.08)
Health care supply (Table 8, 9, and 11)				
Institutional loan (share)	0.010**	0.007*	0.008*	0.007*
	(0.004)	(0.003)	(0.005)	(0.004)
	0.010**	0.008**	0.008	0.007*
	(0.004)	(0.003)	(0.005)	(0.004)
	0.010*	0.008*	0.008	0.007
	(0.005)	(0.004)	(0.006)	(0.005)
Institutional loan (share) - private	0.020**	0.014	0.015*	0.019**
	(0.009)	(0.009)	(0.009)	(0.010)
	0.020**	0.017*	0.014	0.020**
	(0.009)	(0.009)	(0.009)	(0.010)
	0.020*	0.017	0.014	0.020
	(0.012)	(0.011)	(0.011)	(0.012)

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses. The first and second columns are MSE-optimal bandwidths, initially identical and then different to the left and right of the cutoff. The third and fourth columns indicate CER (coverage error rate)-optimal bandwidths, first identical and then different to the left and right of the cutoff (Calonico et al., 2020). In each parcel, I first report the conventional RD estimator with conventional variance estimator. Below that is the bias-corrected RD estimator with the conventional variance estimator, followed by the bias-corrected RD estimator with robust variance estimator (Calonico et al., 2014). For details of the regression, refer to the respective main table. Summarized in Figures A8 and A12.

Table A22: Robustness to Different Bandwidth Selectors: Mechanism Results (2/2)

	MSE-optimal		CER-optimal	
	Common (1)	**Two-sided** (2)	**Common** (3)	**Two-sided** (4)
Health care supply (Table 8, 9, and 11)				
Hospitals (log no.)	0.88***	0.78**	1.32***	1.16**
	(0.33)	(0.33)	(0.46)	(0.49)
	1.14***	1.03***	1.51***	1.34***
	(0.33)	(0.33)	(0.46)	(0.49)
	1.14***	1.03**	1.51***	1.34**
	(0.40)	(0.40)	(0.51)	(0.54)
Hospitals (log no.) - private	0.84**	0.81**	1.28***	1.12**
	(0.36)	(0.36)	(0.49)	(0.52)
	1.11***	1.13***	1.47***	1.33**
	(0.36)	(0.36)	(0.49)	(0.52)
	1.11**	1.13**	1.47***	1.33**
	(0.43)	(0.44)	(0.55)	(0.57)
Hospitals (log no.) - government	0.64*	0.61**	0.91**	0.78*
	(0.33)	(0.30)	(0.45)	(0.42)
	0.78**	0.68**	1.01**	0.84**
	(0.33)	(0.30)	(0.45)	(0.42)
	0.78*	0.68*	1.01**	0.84*
	(0.40)	(0.37)	(0.50)	(0.47)
Survey on problems (Table 10)				
Distance to facility (yes/no)	-0.12***	-0.09**	-0.22***	-0.13***
	(0.04)	(0.04)	(0.07)	(0.05)
	-0.15***	-0.12***	-0.22***	-0.15***
	(0.04)	(0.04)	(0.07)	(0.05)
	-0.15***	-0.12**	-0.22**	-0.15***
	(0.05)	(0.05)	(0.10)	(0.06)
Taking transport to facility (yes/no)	-0.11***	-0.08**	-0.20***	-0.10**
	(0.04)	(0.04)	(0.06)	(0.05)
	-0.15***	-0.12***	-0.20***	-0.13**
	(0.04)	(0.04)	(0.06)	(0.05)
	-0.15***	-0.12***	-0.20**	-0.13**
	(0.05)	(0.05)	(0.09)	(0.06)
No personnel at facility (yes/no)	-0.14**	-0.14**	-0.22***	-0.17**
	(0.06)	(0.06)	(0.08)	(0.07)
	-0.17***	-0.18***	-0.22***	-0.19***
	(0.06)	(0.06)	(0.08)	(0.07)
	-0.17**	-0.18**	-0.22**	-0.19**
	(0.07)	(0.07)	(0.10)	(0.08)
No female personnel at facility (yes/no)	-0.20**	-0.17***	-0.31***	-0.24***
	(0.08)	(0.06)	(0.11)	(0.07)
	-0.24***	-0.21***	-0.32***	-0.27***
	(0.08)	(0.06)	(0.11)	(0.07)
	-0.24***	-0.21***	-0.32**	-0.27***
	(0.09)	(0.07)	(0.14)	(0.09)
No drugs at facility (yes/no)	-0.15**	-0.17***	-0.20**	-0.15**
	(0.07)	(0.06)	(0.08)	(0.07)
	-0.18***	-0.23***	-0.19**	-0.18**
	(0.07)	(0.06)	(0.08)	(0.07)
	-0.18**	-0.23***	-0.19*	-0.18**
	(0.08)	(0.08)	(0.11)	(0.08)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The first and second columns are MSE-optimal bandwidths, initially identical and then different to the left and right of the cutoff. The third and fourth columns indicate CER (coverage error rate)-optimal bandwidths, first identical and then different to the left and right of the cutoff (Calonico et al., 2020). In each parcel, I first report the conventional RD estimator with conventional variance estimator. Below that is the bias-corrected RD estimator with conventional variance estimator, followed by the bias-corrected RD estimator with the robust variance estimator (Calonico et al., 2014). For details of the regression, refer to the respective main table. Summarized in Figures A8 and A12.

Table A23: Robustness to Different Polynomial Degrees: Main Results

	Polynomial Degree		
	One	**Two**	**Three**
	(1)	(2)	(3)
Banks (Table 2)			
Branch licenses 2010 (log no.)	0.19***	0.33***	0.46***
	(0.05)	(0.09)	(0.14)
Branches 2010 (log no.)	0.17***	0.31***	0.44***
	(0.06)	(0.09)	(0.14)
Households' health (Table 3)			
Days ill (yes/no)	-0.19**	-0.22*	-0.23
	(0.09)	(0.13)	(0.17)
Days ill (log no.)	-0.29**	-0.35*	-0.41
	(0.12)	(0.19)	(0.26)
Days missed (yes/no)	-0.30***	-0.39**	-0.45*
	(0.10)	(0.17)	(0.24)
Days missed (log no.)	-0.44***	-0.56**	-0.65**
	(0.13)	(0.22)	(0.31)
Medical expenses (yes/no)	-0.18**	-0.21*	-0.23
	(0.08)	(0.12)	(0.16)
Medical expenses (log Rs)	-0.88**	-1.02**	-1.28
	(0.35)	(0.50)	(0.83)
Households' health (Table 4)			
Vaccinated child (yes/no)	0.07*	0.16**	0.21**
	(0.04)	(0.08)	(0.10)
Sick child (yes/no)	-0.06*	-0.08	-0.23**
	(0.03)	(0.05)	(0.11)
Health care visit (any reason) (yes/no)	-0.08**	-0.13**	-0.22*
	(0.03)	(0.05)	(0.11)
Health care visit (child's treatment) (yes/no)	-0.02*	-0.06**	-0.13**
	(0.01)	(0.03)	(0.05)
Households' health (Table 5)			
Health care facility delivery (yes/no)	0.005***	0.006**	0.003
	(0.002)	(0.003)	(0.005)
Experienced miscarriage (yes/no)	-0.010*	-0.017*	-0.029
	(0.006)	(0.010)	(0.018)
Experienced stillbirth (yes/no)	-0.002*	-0.003*	-0.006
	(0.001)	(0.002)	(0.004)
HC visit (woman's treatment) (yes/no)	-0.051*	-0.106**	-0.175*
	(0.027)	(0.046)	(0.098)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. For details of the regression, refer to the respective main table. Summarized in Figure A9.

Table A24: **Robustness to Different Polynomial Degrees: Mechanism Results**

	Polynomial Degree		
	One	**Two**	**Three**
	(1)	(2)	(3)
Households' consumption (Table 6)			
Total consumption (log Rs)	0.07**	0.14**	0.19***
	(0.04)	(0.05)	(0.07)
Food consumption (log Rs)	0.06*	0.11***	0.14***
	(0.03)	(0.04)	(0.05)
Meals per day (no.)	0.24**	0.30	0.38
	(0.10)	(0.19)	(0.24)
Outpatient expenses (log Rs)	-0.45*	-0.71	-0.63
	(0.23)	(0.60)	(0.73)
Households' financial access (Table 7)			
Savings account (yes/no)	0.22**	0.34*	0.36
	(0.09)	(0.17)	(0.22)
Health insurance (yes/no)	0.15**	0.13	0.20
	(0.07)	(0.08)	(0.14)
Health care supply (Table 8, 9, and 11)			
Institutional loan (share)	0.010**	0.012*	0.015
	(0.004)	(0.006)	(0.011)
Institutional loan (share) - private	0.020**	0.027*	0.033
	(0.009)	(0.014)	(0.023)
Hospitals (log no.)	0.88***	1.22**	1.74
	(0.33)	(0.56)	(1.25)
Hospitals (log no.) - private	0.84**	1.44**	1.92
	(0.36)	(0.69)	(1.54)
Hospitals (log no.) - government	0.64*	0.99	1.49
	(0.33)	(0.62)	(1.35)
Survey on problems (Table 10)			
Distance to facility (yes/no)	-0.12***	-0.10	-0.28
	(0.04)	(0.07)	(0.17)
Taking transport to facility (yes/no)	-0.11***	-0.11**	-0.24
	(0.04)	(0.05)	(0.16)
No personnel at facility (yes/no)	-0.14**	-0.23**	-0.28
	(0.06)	(0.10)	(0.22)
No female personnel at facility (yes/no)	-0.20**	-0.30**	-0.45*
	(0.08)	(0.12)	(0.23)
No drugs at facility (yes/no)	-0.15**	-0.22**	-0.31
	(0.07)	(0.11)	(0.23)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. For details of the regression, refer to the respective main table. Summarized in Figure A9.

Table A25: Placebo Cutoffs: Main Results

	Placebo cutoff						
	-3,000	**-2,000**	**-1,000**	**0**	**1,000**	**2,000**	**3,000**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Banks (Table 2)							
Branch licenses (log no.)	0.92	0.01	0.22	0.00	0.78	0.06	0.04
Branches (log no.)	0.87	0.52	0.40	0.00	0.50	0.14	0.04
Households' health (Table 3)							
Days ill (yes/no)	0.70	0.98	0.33	0.03	0.26	0.03	0.55
Days ill (log no.)	0.84	0.95	0.33	0.02	0.18	0.04	0.62
Days missed (yes/no)	0.99	0.19	0.58	0.00	0.33	0.06	0.51
Days missed (log no.)	0.93	0.44	0.46	0.00	0.32	0.06	0.58
Medical expenses (yes/no)	0.90	0.91	0.80	0.02	0.24	0.02	0.62
Medical expenses (log Rs)	0.68	0.53	0.93	0.01	0.27	0.11	0.28
Households' health (Table 4)							
Vaccinated child (yes/no)	0.50	0.55	0.65	0.07	0.27	0.07	0.49
Sick child (yes/no)	0.21	0.64	0.96	0.06	0.15	0.70	0.37
HC visit (any reason) (yes/no)	0.57	0.78	0.79	0.02	0.29	0.87	0.56
HC visit (child's treatment)	0.44	0.84	0.56	0.10	0.35	0.99	0.44
Households' health (Table 5)							
Health care facility delivery (yes/no)	0.37	0.38	0.99	0.01	0.86	0.84	0.51
Experienced miscarriage (yes/no)	0.59	0.19	0.67	0.09	0.27	0.47	0.45
Experienced stillbirth (yes/no)	0.38	0.92	0.83	0.09	0.00	0.34	0.09
HC visit (woman's treatment) (yes/no)	0.46	0.34	0.84	0.06	0.51	0.65	0.87

P-values of respective regressions with different (placebo) cutoffs shown. For details of the regressions, refer to the respective main table. Summarized in Figure A10.

Table A26: Placebo Cutoffs: Mechanism Results

	Placebo cutoff						
	-3,000	**-2,000**	**-1,000**	**0**	**1,000**	**2,000**	**3,000**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Households' consumption (Table 6)							
Total consumption (log Rs)	0.51	0.91	0.16	0.04	0.93	0.64	0.25
Food consumption (log Rs)	0.82	0.52	0.38	0.10	0.27	0.67	0.76
Meals per day (no.)	0.73	0.53	0.26	0.02	0.98	0.91	0.48
Outpatient expenses (log Rs)	0.59	0.25	0.27	0.05	0.71	0.17	0.16
Households' financial access (Table 7)							
Savings account (yes/no)	0.57	0.84	0.24	0.01	0.46	0.24	0.77
Health insurance (yes/no)	0.56	0.06	0.53	0.03	0.91	0.04	0.74
Health care supply (Table 8, 9, and 11)							
Institutional loan (share)	0.77	0.90	0.74	0.02	0.23	0.90	-
Institutional loan (share) - private	0.69	0.65	0.25	0.03	0.05	0.93	0.85
Hospitals (log no.)	0.59	0.97	0.01	0.01	0.17	0.91	0.87
Hospitals (log no.) - private	0.50	0.85	0.03	0.02	0.30	0.53	0.62
Hospitals (log no.) - government	0.72	0.14	0.36	0.05	0.41	0.21	0.19
Survey on problems (Table 10)							
Distance to facility (yes/no)	0.86	0.66	0.71	0.01	0.29	0.70	0.00
Taking transport to facility (yes/no)	0.86	0.38	0.61	0.00	0.79	0.35	0.09
No personnel at facility (yes/no)	-	0.33	0.34	0.02	0.57	0.92	-
No female personnel at facility (yes/no)	0.82	0.77	0.25	0.01	0.42	0.95	-
No drugs at facility (yes/no)	0.75	0.38	0.50	0.03	0.42	0.45	-

P-values of respective regressions with different (placebo) cutoffs shown. For details of the regressions, refer to the respective main table. Summarized in Figure A10.

Table A27: Standard Error Adjustments: Main Results

	Adjustment			
	None	Multiple hypothesis testing	Spatial correlation (500km)	Spatial correlation (100km)
	(1)	(2)	(3)	(4)
Banks (Table 2)				
Branch licenses 2010 (log no.)	0.00	0.01	0.00	0.00
Branches (log no.)	0.00	0.02	0.00	0.00
Households' health (Table 3)				
Days ill (yes/no)	0.03	0.04	-	-
Days ill (log no.)	0.02	0.03	-	-
Days missed (yes/no)	0.00	0.02	-	-
Days missed (log no.)	0.00	0.02	-	-
Medical expenses (yes/no)	0.02	0.03	-	-
Medical expenses (log Rs)	0.01	0.03	-	-
Households' health (Table 4)				
Vaccinated child (yes/no)	0.07	0.04	-	-
Sick child (yes/no)	0.06	0.04	-	-
HC visit (any reason) (yes/no)	0.02	0.03	-	-
HC visit (child's treatment)	0.10	0.05	-	-
Households' health (Table 5)				
Health care facility delivery (yes/no)	0.01	0.03	-	-
Experienced miscarriage (yes/no)	0.09	0.05	-	-
Experienced stillbirth (yes/no)	0.09	0.05	-	-
HC visit (woman's treatment) (yes/no)	0.06	0.04	-	-

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Column 2 shows adjustments to multiple hypothesis testing (false discovery rate), Columns 3 and 4 to spatial correlation (Conley standard errors, 100km, and 500km). For details of the regression, refer to the respective main table. Summarized in Figure A11.

Table A28: Standard Error Adjustments: Mechanism Results

	Adjustment			
	None	**Multiple hypothesis testing**	**Spatial correlation (500km)**	**Spatial correlation (100km)**
	(1)	(2)	(3)	(4)
Households' consumption (Table 6)				
Total consumption (log Rs)	0.04	0.04	-	-
Food consumption (log Rs)	0.10	0.05	-	-
Meals per day (no.)	0.02	0.03	-	-
Outpatient expenses (log Rs)	0.05	0.04	-	-
Households' financial access (Table 7)				
Savings account (yes/no)	0.05	0.04	-	-
Health insurance (yes/no)	0.02	0.03	-	-
Health care supply (Table 8, 9, and 11)				
Institutional loan (share)	0.02	0.03	0.08	0.01
Institutional loan (share) - private	0.03	0.04	0.09	0.02
Hospitals (log no.)	0.01	0.03	0.04	0.03
Hospitals (log no.) - private	0.02	0.03	0.10	0.04
Hospitals (log no.) - government	0.05	0.04	0.00	0.04
Survey on problems (Table 10)				
Distance to facility (yes/no)	0.01	0.03	-	-
Taking transport to facility (yes/no)	0.00	0.02	-	-
No personnel at facility (yes/no)	0.02	0.03	-	-
No female personnel at facility (yes/no)	0.01	0.03	-	-
No drugs at facility (yes/no)	0.03	0.04	-	-

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Column 2 shows adjustments to multiple hypothesis testing (false discovery rate), Columns 3 and 4 to spatial correlation (Conley standard errors, 100km, and 500km). For details of the regression, refer to the respective main table. Summarized in Figure A11.

Figures

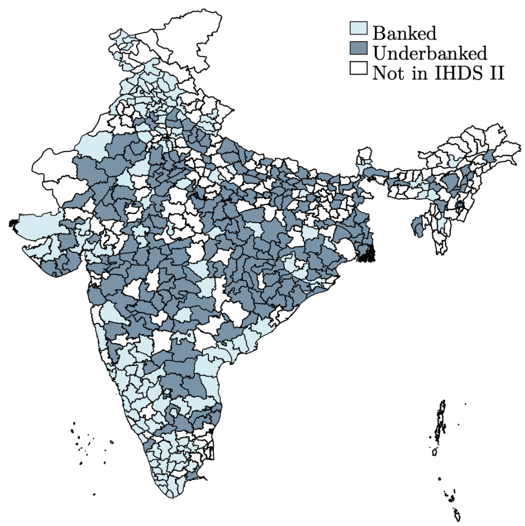


Figure A1. Districts Interviewed. In IHDS II, interviews were conducted in 65 percent of all districts.

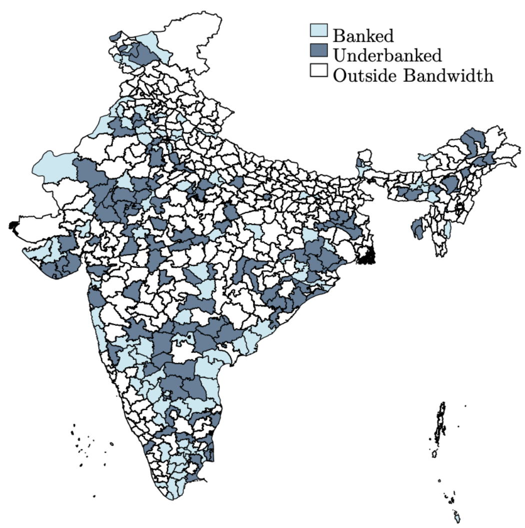


Figure A2. Districts With a Population-to-Branch Ratio Within Typical Bandwidth. There are 111 districts underbanked and 88 districts banked within the typical bandwidth of $\pm$ 3,000.

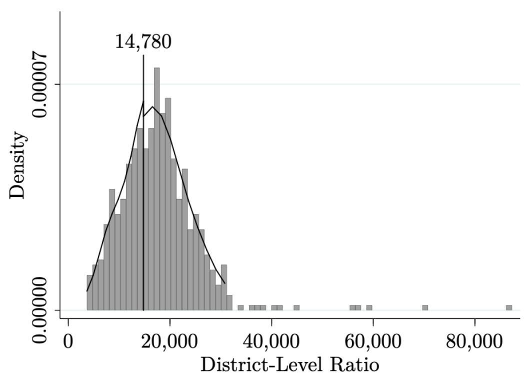
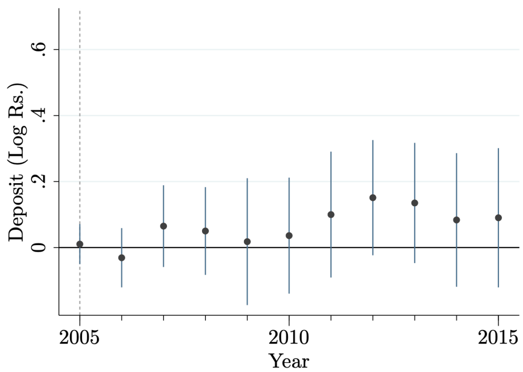
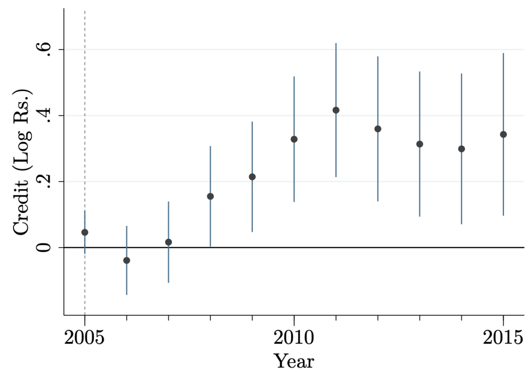


Figure A3. McCrary (2008) Density Test. There is no evidence of manipulation around the cutoff. The McCrary estimator is -0.1998 with a p-value of 0.8416; I do not reject smoothness around the cutoff.



(a) Bank Deposit (Dynamics)



(b) Bank Credit (Dynamics)

Figure A4. Dynamics of Aggregated Deposit and Credit for Private Banks. Data RBI. The dynamic figures depict coefficients on aggregated deposit and credit amounts reported. Credit and deposit are measured in billion rupees. The focus is on private banks, which experienced a particularly large growth post 2005. Coefficients for all banks are insignificant.

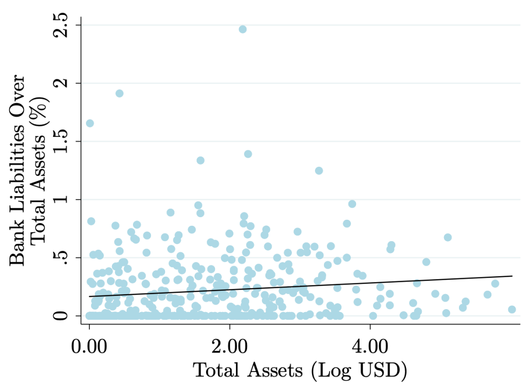
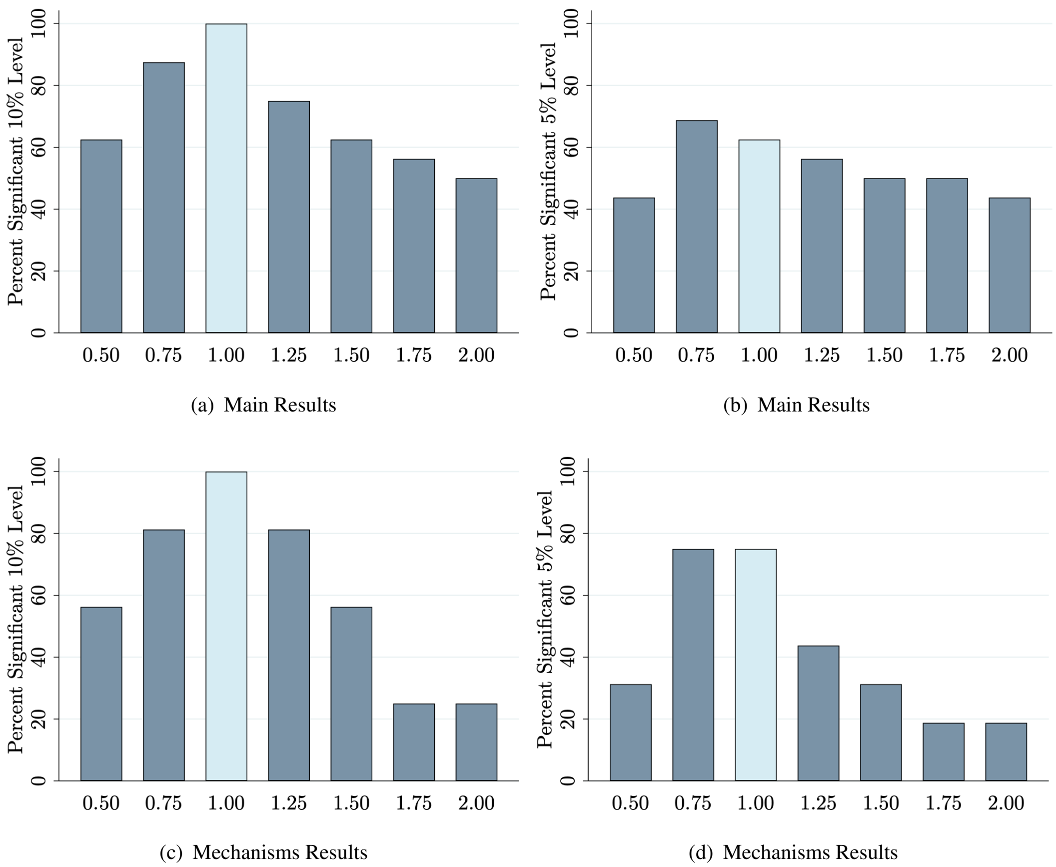


Figure A5. Relationship Between Bank Liabilities and Total Assets. As expected, there is a positive relationship between the share of bank liabilities over total assets and the size of the company proxied by total assets. However, there are many companies of lower asset size that have a relatively high share of bank liabilities over total assets.



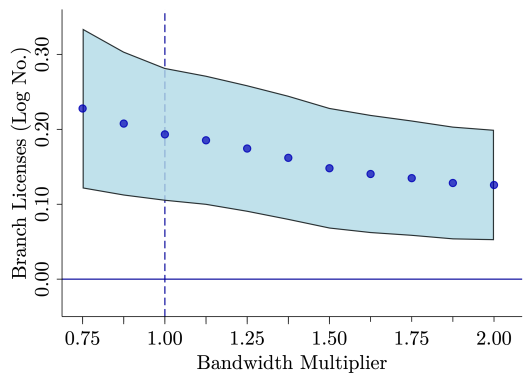
(a) Main Results

(b) Main Results

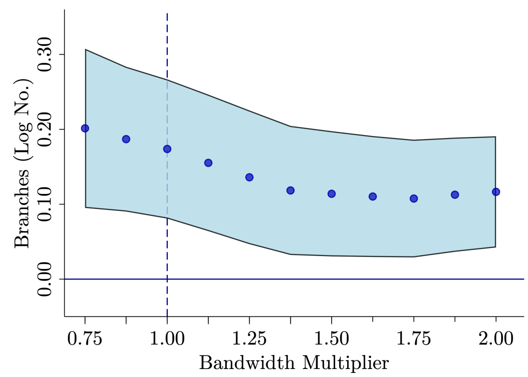
(c) Mechanisms Results

(d) Mechanisms Results

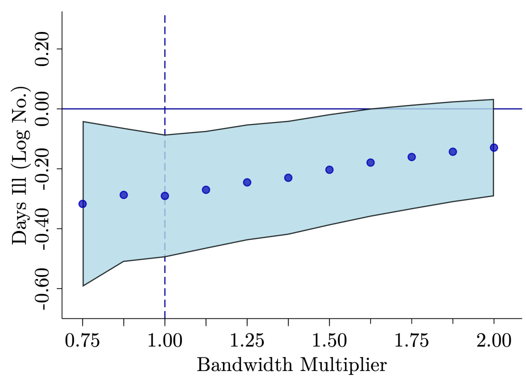
Figure A6. Percent of Results Significant Under Different Bandwidth Multipliers. Light blue indicates the main specification (optimal bandwidth), dark blue indicates alternative specifications (optimal bandwidth multiplied by factor, e.g., 1.25 times optimal bandwidth). Refers to Table A17 and A18.



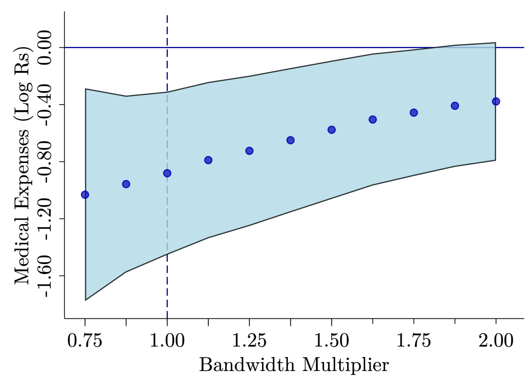
(a) Main Results



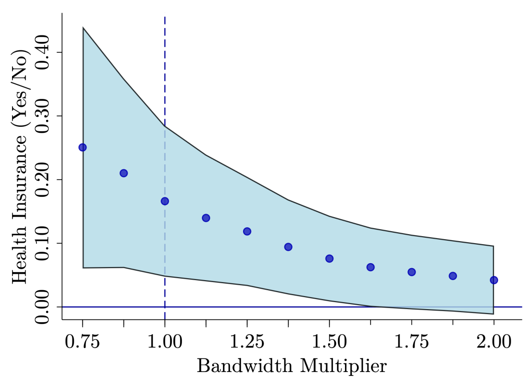
(b) Main Results



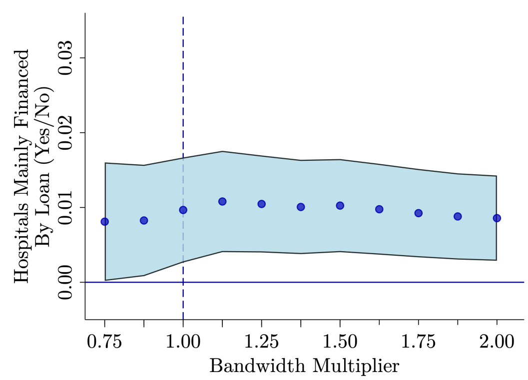
(c) Main Results



(d) Main Results

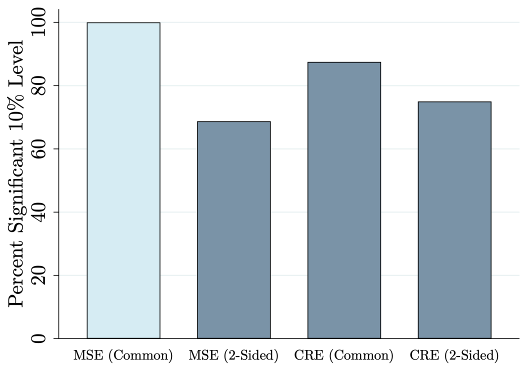


(e) Mechanisms Results

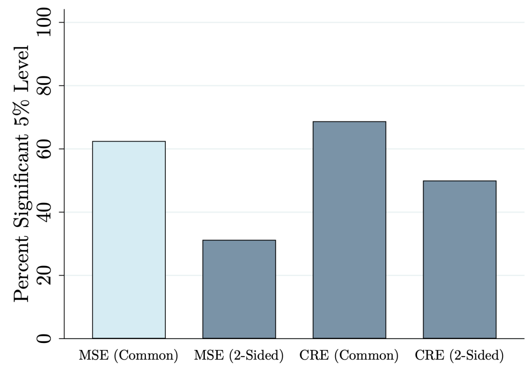


(f) Mechanisms Results

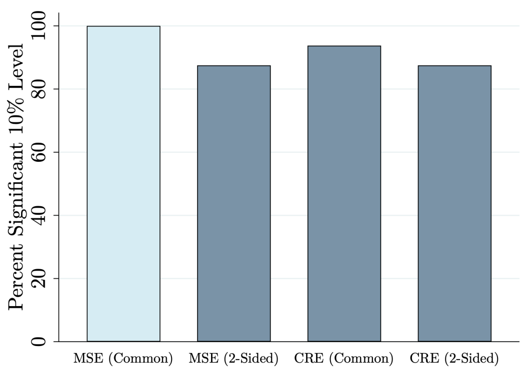
Figure A7. Robustness Under Different Bandwidth Multipliers. 90 percent confidence intervals. Refers to Table A17 and A18.



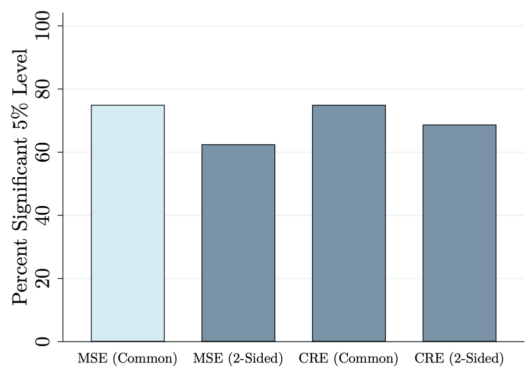
(a) Main Results



(b) Main Results



(c) Mechanisms Results



(d) Mechanisms Results

Figure A8. Percent of Results Significant Under Different Bandwidth Selectors. Light blue indicates the main bandwidth (MSE-optimal with common bandwidth to the left and to the right of the cutoff), dark blue indicates alternative bandwidths. The second columns indicate MSE-optimal bandwidths different to the left and to the right of the cutoff. This is followed by coverage error rate (CER)-optimal bandwidths, first common bandwidth and then different to the left and right of the cutoff (Calonico et al., 2020). Refers to Table A19, A20, A21, and A22.

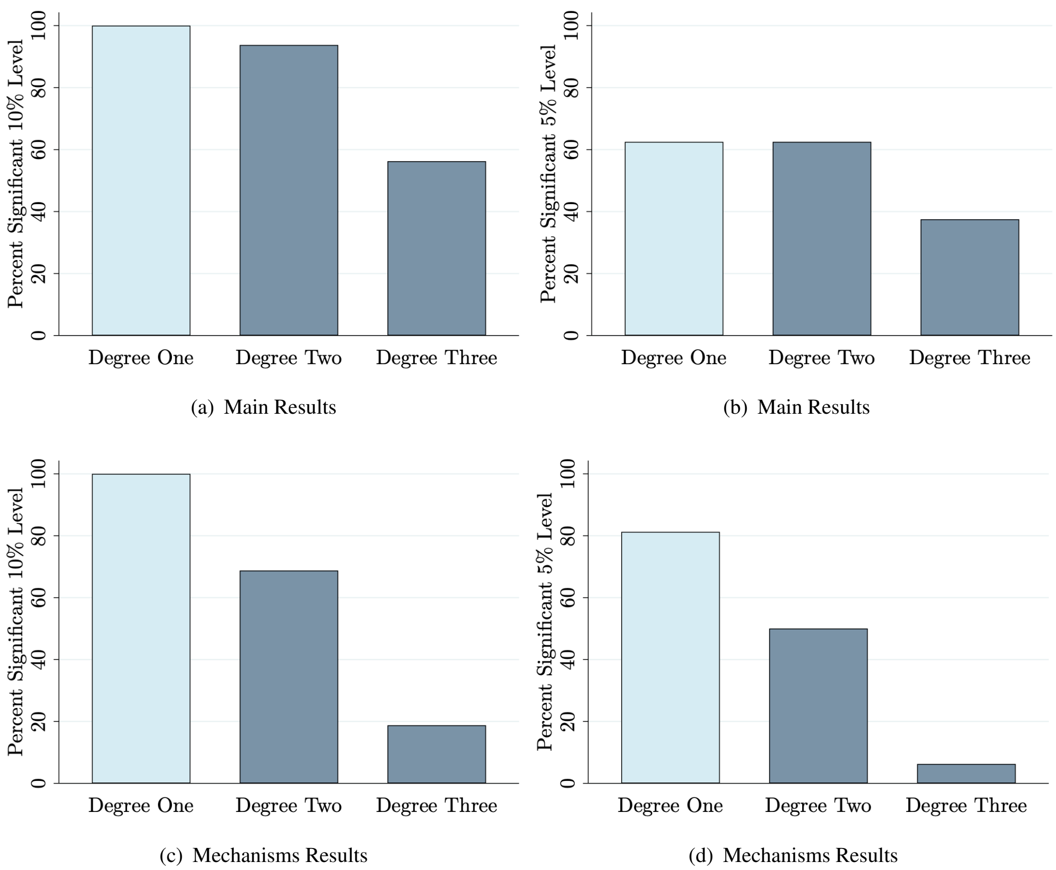
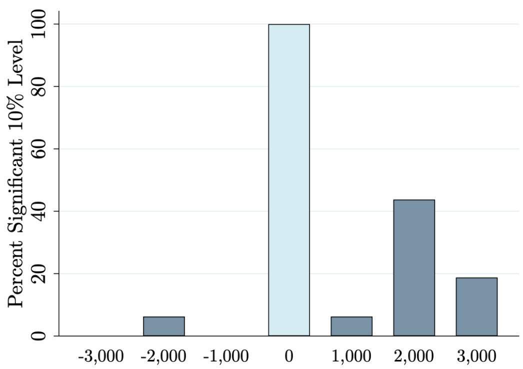
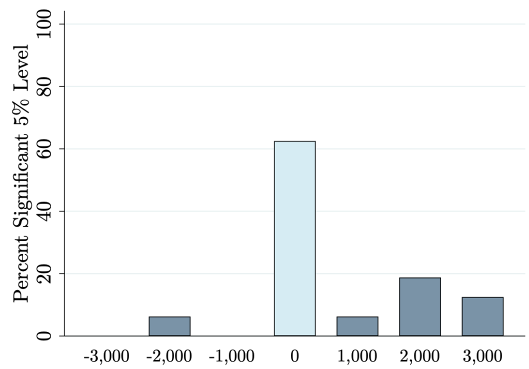


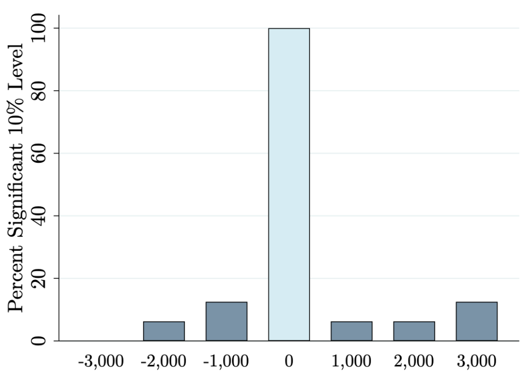
Figure A9. Percent of Results Significant Under Different Polynomial Degrees. Light blue indicates the main specification (degree one), dark blue indicates alternative specifications (degree two and three). Refers to Table A23 and A24.



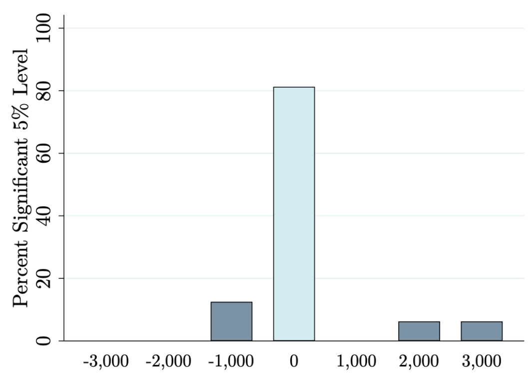
(a) Main Results



(b) Main Results

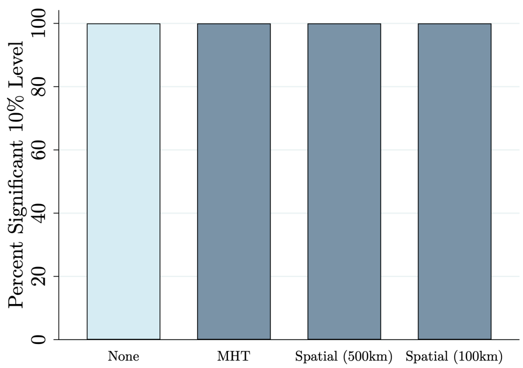


(c) Mechanisms Results

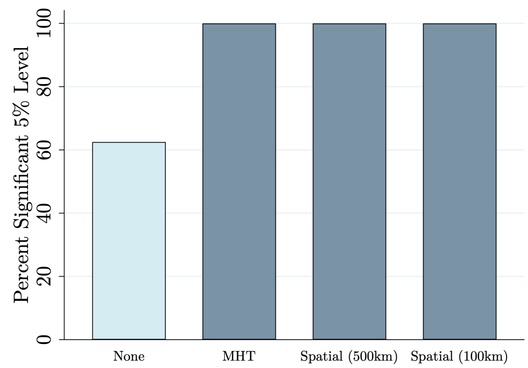


(d) Mechanisms Results

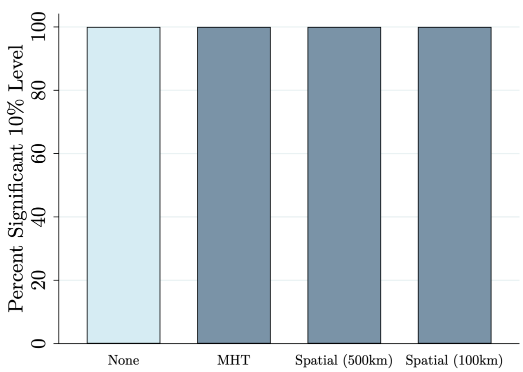
Figure A10. Percent of Results Significant Under True Cutoff (Zero) and Placebo Cutoffs. Light blue indicates the true cutoff (zero), dark blue indicates alternative cutoffs to the left and to the right of the true cutoff. Refers to Table A25 and A26.



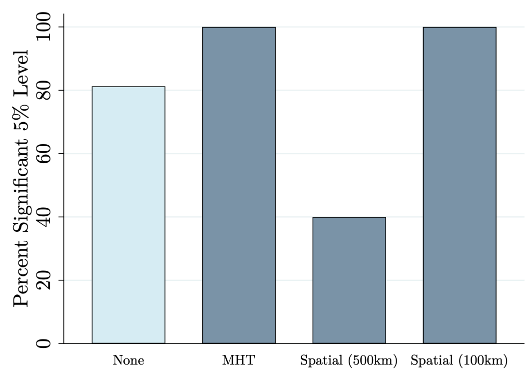
(a) Main Results



(b) Main Results

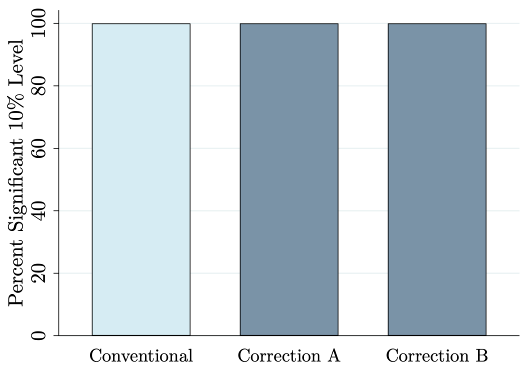


(c) Mechanisms Results

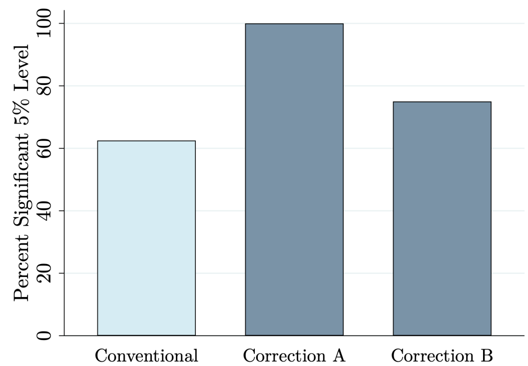


(d) Mechanisms Results

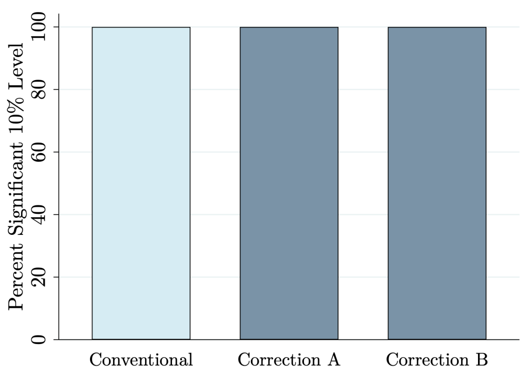
Figure A11. Percent of Results Significant Under Default (No Adjustment) and Adjustments (Multiple Hypothesis Testing and Spatial Correlation). Light blue indicates the default (no adjustment), dark blue indicates standard error adjustments. Column 2 shows adjustments to multiple hypothesis testing (false discovery rate), Columns 3 and 4 to spatial correlation (Conley standard errors, 100km, and 500km). Refers to Table A27 and A28.



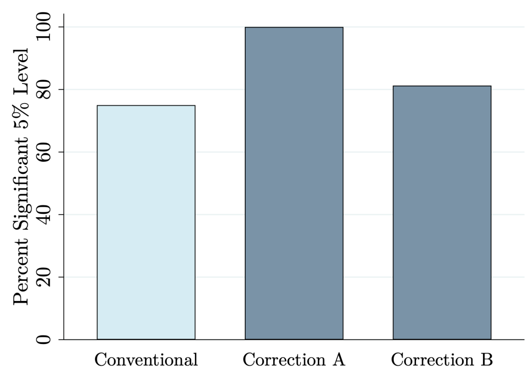
(a) Main Results



(b) Main Results



(c) Mechanisms Results



(d) Mechanisms Results

Figure A12. Percent of Results Significant Under Conventional Specifications and Corrections. Light blue indicates the conventional RD estimator with conventional variance estimator. Correction A is the bias-corrected RD estimator with the conventional variance estimator. Correction B is the bias-corrected RD estimator with the robust variance estimator (Calonico et al., 2014). Refers to Table A19, A20, A21, and A22.

Discussions

Discussion A1. One potential threat is that the IHDS may have been significantly more likely to be conducted in treatment districts. To determine if this is the case, I specify an indicator variable that is 1 if the survey was conducted in a given district and 0 otherwise. I run the main regression specification (Equations 2 and 3) without state-level fixed effects. The resulting coefficient is statistically insignificant (coefficient: 0.07, standard error: 0.20); thus, the survey is not significantly more likely to have been conducted in treatment districts than in control districts. There is no need to conduct this exercise for the DHS or the Economic Census data, since data for all districts is collected.

Discussion A2. The Ministry of Health and Family Welfare is a government agency that implements policies related to health. In 2005, the ministry initiated the National Rural Health Mission (NRHM). In 2013, the NRHM was joined by the National Urban Health Mission (NUHM), and both approaches were combined under one umbrella, the National Health Mission (NHM). Through these programs, both of which comprise multiple initiatives, the Ministry of Health and Family Welfare focuses on improving health outcomes, especially by targeting supply of health care services. For example, the NRHM includes a safe motherhood intervention scheme that provides cash assistance to promote institutional delivery. Many of these initiatives focus on certain priority states; as there is no variation on the district level, they do not pose a threat to identification. However, in 2013 the ministry published a list of 184 priority districts, which multiple initiatives used as guidance to allocate resources. Priority districts were those that were, within a state, in the bottom quarter of the distribution of a composite health index. For districts with left-wing extremism or a high share of tribal population, those falling in the bottom half of the distribution within a state were included. Because it was implemented in 2013, this definition of priority districts is unlikely to drive the IHDS II findings but could potentially impact health outcomes in the DHS. I do not find any evidence that this is the case. The regression coefficient is insignificant. Additionally, the difference in percent of priority districts in treatment and control districts within the bandwidth is seven percentage points. The correlation coefficient within the bandwidth is low at 0.08.

Another ministry that introduced health-related policies is the Ministry of Women and Child Development. Two policies in particular are worth considering in this context: the Integrated Child Development Services (ICDS) program and the ICDS Systems Strengthening and Nutrition Improvement Project (ISSNIP). The ICDS was introduced in 1975 and has, among other goals, the objective to reduce mortality, morbidity, and malnutrition. Services under this program include for instance immunization and supplementary nutrition. In 2012/2013, a restructured and strengthened ICDS program was rolled out in 200 priority districts. In 2013/2014, a second wave of rollout followed in another 200 districts. Priority districts were defined based on nutritional status of children and anemia level among pregnant women. Unfortunately, only the list of the 200 districts in the first wave is available. In 2012, around the same time that the strengthened ICDS was rolled out, the ministry implemented the ISSNIP. This policy had the objective to shift the focus of the ICDS scheme to younger children. It focuses on 162 priority districts, also defined on the basis of undernutrition measures. Both policies have negative and insignificant coefficients, meaning that they were not significantly more likely to be implemented in treatment districts. Additionally, the difference in percent of priority districts in treatment and control districts within the bandwidth is 5 percentage points for ICDS wave one and 11 percentage points for ISSNIP. Correlation coefficients within the bandwidth are low at 0.06 and 0.13 respectively.

Another often discussed nationwide policy is the National Rural Employment Guarantee Act (NREGA) from 2005. It is an employment scheme that guarantees a minimum amount of wage employment for unskilled labor. NREGA was rolled out in three waves. The first was conducted in 2006/2007, followed by one in 2007/2008, and a final wave in 2008/2009. The phase in which each district was covered was based on an index consisting of parameters such as poverty, education, and health. Both the first and the second wave of NREGA have coefficients that are negative and statistically insignificant. The difference in percent of priority districts in treatment and control districts within the bandwidth is 23 for the first wave and -6 for the second wave. Correlation coefficients within the bandwidth are 0.25 and -0.07 respectively.

Discussion A3. One question that arises is whether it is profitable for banks to open branches in underbanked districts. Answering this question requires data on branch profits on at least the district level. Unfortunately, neither the RBI nor any other institution provides this data. Without data on branch profitability, it is not possible to estimate the costs of the policy, which are potentially carried by the financial sector. However, it is possible to make one specific statement: As banks indeed react to the policy, the combination of opening a branch in an underbanked district and obtaining a license for another location appears to be profitable for banks.

Discussion A4. To provide further evidence of the validity of my findings, I compare my effect sizes to those of other successful health interventions. One study that examines very similar outcomes in terms of morbidity rates is Gertler (2004). The author evaluates a large conditional cash-transfer program, Progresa, initiated in Mexico in 1977. Eligible families received a cash transfer of about 25 percent of household income every two months. The eligibility conditions were designed to improve health status of families. The payout was received if, for instance, children got immunized and mothers visited nutrition monitoring clinics. In 1998, the government enrolled entire villages, but randomly chose which ones to enter initially and which to enter two years later. Using this experimental design, the author compares households in treatment villages to those in control villages, two years after Progresa was rolled out. Very similar to the morbidity measure for children in DHS, the author uses as an outcome variable a question regarding whether the child had any non-chronic illness in the past four weeks. The study finds that after two years of program exposure, children age zero to three at baseline (or two to five at time of the survey) experience a decrease in probability of being ill by 39 percent. In contrast, I find that children who lived in a treatment district for ten years have a 27 percent lower probability of non-chronic illness. Here, I directly run child-level regressions to make the results as comparable as possible, but some differences remain. For example, the estimate of the conditional cash transfer refers to the past four weeks, while the DHS refers to the past two weeks. However, one can make the cautious statement that ten years of bank exposure result in an effect on the probability of illness among children that is approximately 70 percent as large as two years of exposure to a program that includes large cash transfers of around 25 percent of household income and that directly incentivizes families to engage in behavior designed to improve health.

Other successful health interventions provide similarly large effect sizes. Examining the effect of improved water quality, Kremer et al. (2011) find that children's probability of diarrhea decreases by 25 percent. Even stronger effects are observed in successful supply-side interventions. Evaluating the impact of trained informal health care providers, Björkman-Nykvist et al. (2014) find a decrease in child mortality of 25 percent. An intervention that allows beneficiaries to monitor health care providers even reduced child mortality by 33 percent (Björkman and Svensson, 2009). As outlined by Banerjee and Duflo (2011) and Dupas and Miguel (2017), there are highly effective and relatively cheap treatments for many diseases that affect the poor. For example, oral rehydration solutions are a highly effective way to treat dehydration caused by diarrhea, a major cause of child mortality. Considering this context, the large effect sizes I observe in this study appear reasonable.