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Bank presence and health

Kim Fe Cramer[1,*]

[1]LSE Finance Department, London, United Kingdom

*Corresponding author: Finance Department, LSE, Houghton Street, WC2A 2AE London, UK.
Email: k.f.cramer@lse.ac.uk

Abstract

This article examines whether more bank presence in underserved areas can improve households' health. Leveraging a 2005 Reserve Bank of India policy and a regression discontinuity design, I demonstrate that 5 years post-policy, treatment districts have twenty-seven more bank branches than control districts. This expansion increases household employment and access to savings accounts, enhancing health investments. On the healthcare supply side, hospitals utilize more credit and expand services. Six years after the policy, households in treatment districts are nineteen percentage points less likely to suffer from non-chronic illnesses in a given month. Chronic diseases remain unaffected.

Keywords: financial development; banks; health.
JEL classifications: G21, O16, I15.

What are the general equilibrium effects of banks on households? Previous research has focused on labor markets. Banks extend credit to firms, thereby fostering economic activity and employment (Bruhn and Love 2014; Fonseca and Matray 2022). In this article, I demonstrate that beyond that, bank presence can contribute toward tackling hard-to-crack development challenges, studying the third UN Sustainable Development Goal of improving households' health. In addition to stimulating employment that allows households to invest more in health, banks may improve health through three distinct activities. First, banks might offer savings accounts to households. Second, they may provide personal bank loans to households. Both savings accounts and bank loans could allow households to invest more in health when necessary. Third, banks could extend credit to healthcare providers, thereby stimulating healthcare supply, a crucial determinant of health status. Despite these strong motivations, we lack causal evidence on the impact of bank presence on households' health.

To obtain exogenous variation in bank presence, I use a Reserve Bank of India (RBI) policy from 2005. The policy incentivizes banks to set up new branches in underbanked districts. These districts have a population-to-branch ratio that exceeds the national average. In a regression discontinuity design, I compare districts with a ratio just above the national average (treated) and those just below (control). To examine whether the policy introduced economically meaningful bank entry, I use annual district-level data from the RBI on the number of branches, total credit amount, and total deposit accounts. To learn how bank presence could affect health, I rely on additional datasets. I use the Economic Census, conducted 8 years after the policy introduction, to measure employment effects. To study households' financial access and health investments, I utilize the nationally representative Indian Human Development Survey (IHDS), conducted 6 years post-policy. To measure

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healthcare supply, I also rely on the Economic Census. Finally, I investigate health outcomes using the IHDS as well as the Demographic and Health Survey (DHS), the latter conducted 10 years after the policy implementation.

Initially, I demonstrate that the policy increased bank presence in treatment districts. Smooth before the policy introduction, treatment districts have significantly more branches 2 years later. Matching the dynamics of the policy, these discontinuities continue to grow. Five years post-policy, treatment districts have 19 percent or 27 more branches, compared to 142 branches in control districts. This effect is economically meaningful. I find that treatment districts have 161,977 more deposit accounts and US$148 million more in credit. Moreover, private banks were the driving force behind the expansion. While 17 percent of branches are private in the control, 53 percent of the new branches are private. Entry of private banks could have further boosted competition in the banking sector. Overall, this suggests that the policy introduced exogenous and economically meaningful bank entry.

Next, I test mechanisms through which banks may improve health. First, I show that, as previous studies suggest, households benefit from increased employment. Second, I investigate households' financial access. I find that households gain more access to savings accounts. In contrast, the average household does not gain access to a personal bank loan. In alignment with the literature that discusses the problems of information asymmetries in personal loan markets in developing countries (Karlan and Zinman 2009), this suggests that formal medical debt can be ruled out as a channel. Both employment and savings accounts may allow households to invest more in health. Indeed, I find evidence that households increase their healthcare demand and spend more on low-fixed-cost items such as food or hygiene products. In contrast, households do not invest more in high-fixed-cost items such as fridges or toilet facilities, which is in alignment with a lack of credit take-up. Next, I examine healthcare supply. I find evidence that hospitals take up more credit and expand the healthcare supply in equilibrium. Further heterogeneity analysis suggests that employment and savings accounts are more important than the healthcare supply channel in improving health.

Finally, I turn toward measures of health status. Six years after the policy's implementation, households' probability of suffering from a non-chronic illness such as fever or diarrhea in a given month is nineteen percentage points lower, compared to a control mean of 52 percent. This effect size is in the middle of the range of other successful health interventions in developing countries (see Waddington et al. (2009) for a meta-analysis). The reduction in non-chronic diseases positively affects labor supply and school attendance. I do not find effects for chronic diseases such as diabetes. The second survey, conducted a decade after the policy introduction, allows me to replicate my findings on non-chronic illnesses. Thus, evidence from two different surveys demonstrates that bank presence can play a vital role in improving health.

I provide extensive evidence to reject potential threats to causal identification. First, I establish that local governments do not manipulate their treatment status. By construction, manipulation of the population-to-branch ratio is unlikely. The numerator relies on 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 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 under varying bandwidths and polynomials, and there is little evidence of discontinuities at placebo cutoffs. In summary, this evidence strengthens the confidence in the causal interpretation of my findings.

The contribution of this study is to examine the impact of bank presence on health. It primarily informs the literature on the general equilibrium effects of bank presence on households. This literature includes studies in developing countries (Burgess and Pande 2005; Bruhn and Love 2014; Barboni, Field, and Pande 2022; Fonseca and Matray 2022) and developed countries (Brown, Cookson, and Heimer 2019; Célerier and Matray 2019). While prior work has established that bank presence can stimulate employment and household income, the relationship to health has not been explored. One might raise the question of whether we can simply extrapolate that health must improve. We might be inclined to do so if there were a strong relationship between income and health in developing countries. Empirically, substantial cash transfers to households do not show positive health effects (Haushofer and Shapiro 2013; Egger et al. 2019). Theoretically, three key factors could explain the discrepancy in results between bank presence and cash transfers. First, as banks stimulate employment, households may be more inclined to invest in health following increased labor income than after temporary cash transfers (Friedman 1957). Second, bank presence may increase credit to hospitals, improving the supply of healthcare. Under cash transfers, even if households spend more on health, the healthcare supply might not sufficiently expand to improve health if there are high fixed costs to investments and credit constraints. Finally, bank presence may alleviate transaction costs that reduce health-enhancing spending, such as travel time to the nearest healthcare provider, as healthcare supply increases (Dupas and Miguel 2017).

This article closely connects to a second literature that studies the impact of other forms of financial access in developing countries (Kanz 2016; Agarwal et al. 2017; Giné and Kanz 2018; Bachas et al. 2021; Breza and Kinnan 2021; Doornik et al. 2024; Garber et al. 2021; Aydin 2022; Fonseca and Van Doornik 2022; Ghosh and Vats 2022; Limodio 2022; De Roux and Limodio 2023; Dubey and Purnanandam 2023; Fiorin, Hall, and Kanz 2023; Higgins 2024). Whether these other forms of financial access can affect health has been primarily explored through randomized controlled trials that offer financial products to households. These studies frequently find null results for savings accounts (Dupas and Robinson 2013a; Prina 2015; Dupas et al. 2018), bank credit (Karlan and Zinman 2010), and microcredit (Beaman et al. 2014; Angelucci, Karlan, and Zinman 2015; Attanasio et al. 2015; Augsburg et al. 2015; Banerjee et al. 2015; Crépon et al. 2015; Tarozzi, Desai, and Johnson 2015; Banerjee et al. 2019). There are two important distinctions between my work and these randomized controlled trials. First, examining bank presence allows me to capture the effects of finance that are mediated through the labor and healthcare market. Second, a large treated sample and long-term effects up to 10 years after the policy introduction allow me to capture general equilibrium effects.

Finally, this article speaks to a growing literature that connects finance and health in developed countries. This literature explores, for instance, the relationship between households' financial decisions and health, such as the mental health effects of credit (Andersen et al. 2022). It also investigates the relationship between hospitals' finances and health, for example, the effect of a financial crisis (Adelino, Lewellen, and McCartney 2022), cash flow shocks (Adelino, Lewellen, and Sundaram 2015), credit shocks (Aghamolla et al. 2024), bankruptcies (Antill et al. 2023), and private equity ownership (Liu 2022; Gupta et al. 2024) on the quality of hospital services. These studies demonstrate an increasing interest in the understudied relationship between finance and health.

Regarding the use of the 2005 RBI policy, this article builds on Young (2017), who developed the regression discontinuity design in this context. Like this study, the author examines the policy's impact on RBI branches and credit data. Young (2017) then explores the impact on economic activity, proxied by measures of night-light, crop yields, and manufacturing balance sheet data.[1] This is the first article that investigates household-level implications of the policy. Since the distribution of my article in 2021, subsequent research

[1] The manufacturing analysis uses state-level variation in a difference-in-difference framework due to the lack of district-level identifiers and does not examine employment.

has employed the policy, partly following the approach of combining it with household-level data and validating my results, thus strengthening the robustness of the design (Chakraborty et al. 2022; Gupta and Sedai 2023, 2024; Jakaria 2023; Jiao and Mo 2023; Kulkarni, Mahajan, and Ritadhi 2023).[2] This article is the only one that examines the effect of the policy on health.

This article's findings carry implications for both policy and future research. Policymakers worldwide have been implementing branch-opening initiatives in underserved regions. While we recognize their positive impact on businesses and labor markets, we now learn they also have a role in improving households' well-being beyond their financial situation. This article also encourages further exploration into the effects of bank presence on dimensions such as education. Gaining insights into these inquiries can substantially advance our understanding of the impact of bank presence and the potential for policymakers to enhance their citizens' well-being.

1. Policy

I use a policy the RBI 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. Districts are defined as underbanked if their population-to-branch ratio 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 2. 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 thirty-five states or union territories that were particularly disadvantaged. I do not exploit this variation for identification, but it appears in the dynamic patterns of banks' responses to the policy. Figure 1 depicts all 593 districts as of the 2001 Census. Marked in a darker shade are the 375 districts defined as underbanked according to the reconstructed district-level ratio in 2006.

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

Similar to the 2005 policy, another branch licensing policy was in place between 1977 and 1990. Burgess and Pande (2005) use the 1977–1990 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. From 1990 through 2005, no comparable branch licensing policy was in place.

2. 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 annual publication of the RBI, the Bank Branch Statistics (RBI 2018a). I focus on data from the first quarter of 2006

[2] After distributing my article during the 2021/2022 job market, authors such as Gupta and Sedai, Jakaria, and Jiao and Mo contacted me and asked to share the branch data, which I had obtained from the RBI website and was no longer publicly available. This data is an essential input of these studies.

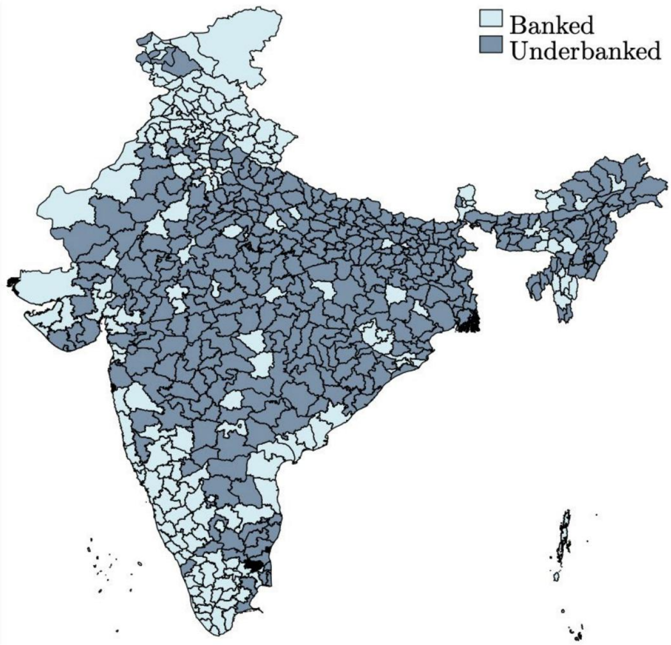

Figure 1. Underbanked and banked districts. Map of India depicting districts based on 2001 Census borders. Three hundred seventy-five of 593 districts are underbanked, colored in a darker shade. Banked districts are colored in a lighter shade.

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 dataset: 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 outcomes 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. In 2016, the final household-level survey was conducted. I supplement this with district-level RBI data on total credit and number of deposit accounts (RBI 2018c). General summary statistics are provided in Supplementary Appendix Table B1.

To test mechanisms through which banks may improve health, I use additional datasets. First, I utilize the Economic Census to learn about the employment effect and healthcare supply. The Economic Census covers all informal and formal establishments in India, except those engaged in activities of farming, plantation, public administration, and defense. I focus on two census rounds; the first was conducted in 2005 and the second in 2013 (see fig. 2) (CSO and MOSPI 2018a, 2018b). 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. Note that for the employment analysis, I rely on a cleaned version of the Economic Census provided by the Socioeconomic High-Resolution

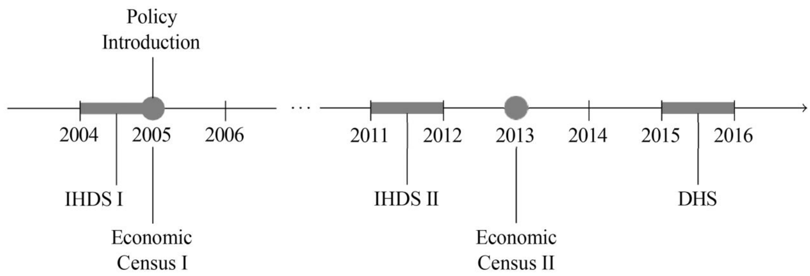

Figure 2. Timeline. The figure depicts the timeline from 2004 to 2016, marking the 2005 RBI policy and key data sources (IHDS I in 2004/2005, IHDS II in 2011/2012, DHS in 2015/2016, and Economic Censuses in 2005 and 2013).

Rural-Urban Geographic Data Platform (SHRUG) (Henderson, Storeygard, and Weil 2011; Asher and Novosad 2019; Asher et al. 2021). This excludes, for instance, industries that were not consistently measured in the Economic Census. For the healthcare supply data, I rely on the raw Economic Census published in the National Data Archive since the SHRUG does not allow a disaggregation by three-digit National Industry Classification (NIC), which I utilize to define healthcare providers (code 861, following the NIC-2008 classification). Summary statistics are provided in Supplementary Appendix Table B4.

To further investigate the effect on labor demand, I examine the National Sample Survey (NSS), Round 66, which was conducted in 2009/2010 by the Ministry of Statistics and Programme Implementation of the Indian government (NSS 2011b). This data covers a substantial sample of 266 thousand individuals across 611 districts. It allows me to measure daily wages. To test for pre-policy smoothness, I utilize the NSS round from 2004/2005 (NSS 2011a).

To get a more detailed picture of the healthcare industry, I additionally investigate the Prowess database, which provides financial statements for companies of all sizes, including those conducting health services (CMIE 2020). The Prowess sample includes all companies traded on the National Stock Exchange and the Bombay Stock Exchange, as well as a selected sample of unlisted public limited companies and private limited companies. While providing more detailed financial information than the Economic Census, due to the limited number of districts represented in Prowess, I concentrate on the Economic Census for the regression analysis.

To learn about households—their financial access, health investments, and health status—I use the IHDS. This panel survey was conducted in 2004/2005 (IHDS I), shortly before the policy, and again 6 years after the policy in 2011/2012 (IHDS II) (see fig. 2) (Desai and Vanneman 2018a, 2018b). The pre-policy round allows me to test for the smoothness of household characteristics around the cutoff. 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 the Andaman and Nicobar Islands. In the first survey round, 41,554 households were interviewed. In the second round, 83 percent of the original households plus replacement households were interviewed. This attrition does not threaten 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 Supplementary Appendix Table B2. In Supplementary Appendix Table B2, I also provide evidence of 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.

I complement the IHDS with a second nationally representative household-level survey specifically on health, the DHS, conducted in 2015 and 2016, 10 years after the policy (see fig. 2) (IIPS and ICF 2017). The survey was conducted in all districts, and 601,509 households were interviewed. 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 Supplementary Appendix Table B3.

To provide further evidence on pre-policy smoothness along other dimensions, I investigate additional variables on economic activity and population characteristics from the SHRUG. Economic activity is proxied by night-light data, economic employment, and road connections. Population characteristics include total 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.

3. Identification strategy

3.1 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 below the national average are defined as banked or control. Figure 3a 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 ±3,000 relative to the cutoff. 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.

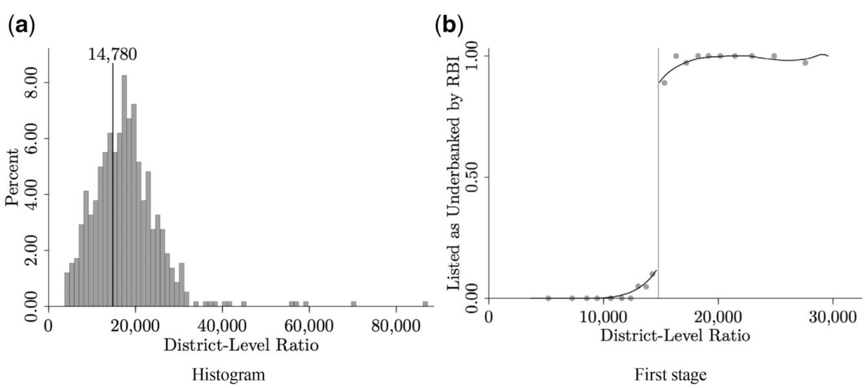

Figure 3. Histogram and first stage. Figure 3a depicts the histogram of district-level population-to-branch ratios. Figure 3b depicts the first stage, with the probability of being listed as underbanked by the RBI on the y-axis and the district-level population-to-branch ratio on the x-axis. The vertical line in both figures indicates the national average of the population-to-branch ratio (14,780).

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. 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 do not threaten identification but give rise to the fuzzy regression discontinuity design. Figure 3b 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.

$$\begin{aligned}\text{Underbanked}_{d,s} &= \alpha_0 + \alpha_1\text{Above}_{d,s} + \alpha_2\text{DistRatio}_{d,s} \\ &\quad + \alpha_3\text{DistRatio}_{d,s}\text{Above}_{d,s} + \lambda X_{d,s} + \mu_s + \upsilon_{d,s} \qquad (2)\\ y_{h,d,s} &= \beta_0 + \beta_1\text{Underbanked}_{d,s} + \beta_2\text{DistRatio}_{d,s} \\ &\quad + \beta_3\text{DistRatio}_{d,s}\text{Above}_{d,s} + \gamma X_{d,s} + \eta_s + \varepsilon_{h,d,s} \qquad (3)\end{aligned}$$

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 district level. To choose the optimal bandwidth, I follow an MSE-optimal procedure (Calonico, Cattaneo, and Titiunik 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 of receiving the underbanked status for a district with a ratio equal to the cutoff.

3.2 Identification assumption

The identification assumption of this setting is the 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 that local governments learn 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 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 the 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. At first glance, there is no evidence of this in figure 3a. 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 differ from those just below the cutoff before the policy. Assume, for example, that local governments that can manipulate their ratio have a healthier population. In this case, I would observe discontinuities in pre-policy health measures.

To test for smoothness before the policy, I utilize the RBI Master Office File (2004), the Economic Census (2005), the NSS (2004/2005), and the IHDS I (2004/2005). 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 regression discontinuity design 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. I demonstrate smoothness for employment, households' financial access, and healthcare supply. Employment is smooth before the policy, households are not more likely to own financial products in treatment districts, and healthcare providers are not more likely to be financed mainly by a loan or have more presence in treatment districts. Households in treatment districts are not significantly healthier than those in control districts before the policy, neither for non-chronic nor for chronic illnesses. For non-chronic illnesses, I observe smoothness in the incidence of disease in the past 30 days, total days household members were ill, as well as days of work or school they missed due to an illness. For chronic illnesses, I observe smoothness in the incidence of disease and the days of work or school they missed due to an illness. Correspondingly, I observe graphical smoothness in figure 4. Additionally, I use the SHRUG data to show that village- and town-level general economic activity and population characteristics are smooth (Appendix Table A1). 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 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 5 years before the IHDS II (2011/2012). The coefficient on this migration pattern is insignificant when formally testing for it as described in the regression framework (Appendix Table A2).

Third, I demonstrate that other policies do not threaten identification. The concern is that I may mistake discontinuities around the cutoff for the effect of the 2005 RBI policy when they stem from other policies. To my knowledge, no other policy uses the same cutoff rule described in this article. 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, Bor, and Bärnighausen 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, such as certain health indicators. In Supplementary Appendix Discussion B1, I describe other nationally implemented policies, including those issued by the Ministry of Health and Family Welfare, the Ministry of Women and Childhood Development, the Ministry of Labour and Employment, and other policies not directly related to health, such as the National Rural Employment Guarantee Act, 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 specific 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 (Appendix Table A3). All coefficients are statistically insignificant. I provide further evidence on the distribution of priority

Table 1. Smooth pre-policy covariates.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data RBI Master Office File (1998-2016), Economic Census (2005), National Sample Survey (NSS, 2004/2005), and IHDS I (2004/2005). District, household, and individual levels. Count and amount variables are transformed to log and winsorized at the 1st and 99th percentile. Variables depicted here are later used in post-policy regressions, explained in more detail in respective tables. Column 5 shows the regression discontinuity design (RDD) coefficients. $^{*}P<.1$, $^{**}P<.05$, $^{***}P<.01$.

	All observations		Within bandwidth		RDD
	Treated (1)	Not treated (2)	Treated (3)	Not treated (4)	Coefficient (5)
Banks					
Branch licenses (log no.)	4.04	4.74	3.76	4.14	0.02
	(0.80)	(0.88)	(0.85)	(1.09)	(0.02)
Branches (log no.)	4.02	4.74	3.87	4.16	0.01
	(0.81)	(0.88)	(0.93)	(1.13)	(0.02)
Employment					
Employment (log no.)	11.63	11.79	11.31	11.53	0.02
	(0.87)	(1.01)	(1.02)	(1.20)	(0.11)
Daily wage (log Rs)	4.06	4.37	4.09	4.19	0.03
	(1.16)	(1.07)	(1.10)	(1.04)	(0.06)
Households' financial access					
Any loan (yes/no)	0.48	0.36	0.47	0.41	0.02
	(0.50)	(0.48)	(0.50)	(0.49)	(0.09)
Largest loan amount (log Rs)	4.43	3.56	4.42	3.88	0.39
	(4.70)	(4.83)	(4.78)	(4.85)	(0.78)
Largest loan from bank (yes/no)	0.11	0.13	0.12	0.12	–0.02
	(0.31)	(0.33)	(0.33)	(0.33)	(0.02)
Healthcare supply					
Institutional loan (share)	0.02	0.04	0.02	0.04	0.00
	(0.03)	(0.04)	(0.02)	(0.03)	(0.01)
Healthcare providers (log no.)	5.57	5.83	5.40	5.36	–0.15
	(0.97)	(1.14)	(1.17)	(1.37)	(0.16)
Health					
Non-chronic: any illness (yes/no)	0.52	0.39	0.47	0.40	–0.07
	(0.50)	(0.49)	(0.50)	(0.49)	(0.06)
Non-chronic: days ill (log no.)	1.13	0.79	0.95	0.83	–0.21
	(1.19)	(1.09)	(1.12)	(1.10)	(0.17)
Non-chronic: days missed (log no.)	0.78	0.55	0.59	0.63	–0.28
	(1.05)	(0.92)	(0.95)	(0.96)	(0.19)
Chronic: any illness (yes/no)	0.26	0.28	0.25	0.26	0.03
	(0.44)	(0.45)	(0.44)	(0.44)	(0.05)
Chronic: days missed (log no.)	0.68	0.70	0.62	0.70	–0.07
	(1.53)	(1.55)	(1.48)	(1.57)	(0.19)

districts in Appendix Table A4. Correlation coefficients between an indicator for priority district and an indicator for being underbanked within the bandwidth range from –0.08 to 0.22. This evidence suggests that other policies with priority districts do not threaten causal identification. Additionally, other major policies such as India's road construction program Pradhan Mantri Gram Sadak Yojana (PMGSY) (Asher and Novosad 2020) or the mobile infrastructure program Shared Mobile Infrastructure Scheme (SMIS) (Gupta, Ponticelli, and Tesei 2023) are based on village population cutoffs, not district population cutoffs, and thus do not pose a threat to identification. In summary, tests of the identification assumption strengthen the causal interpretation of my findings.

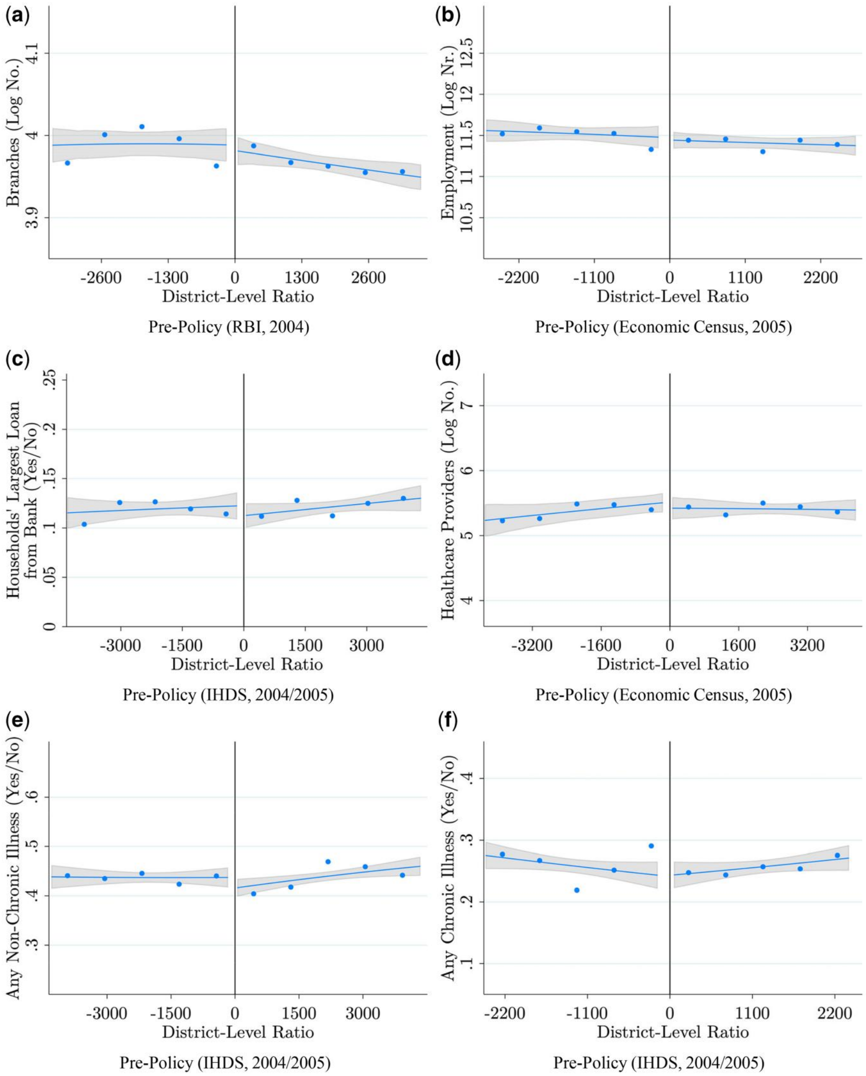

Figure 4. Smooth pre-policy covariates. These figures depict binned means to the left and right of the cutoff within the optimal bandwidth. They also depict local linear polynomials to the left and right of the cutoff, with 95% confidence intervals in gray. The cutoff is normalized to zero.

Finally, one might be concerned about some remaining types of bank selection; those do not pose a threat to identification. Indeed, banks might select into certain districts among underbanked districts, or into certain locations within a district. Both types of selection do not threaten identification since the identification strategy relies on the variation across districts, banked versus underbanked.

4. Bank branches

In the first step of the analysis, I provide evidence that the policy resulted in meaningful bank entry in treatment districts. I examine two outcomes: the number of branch licenses

Table 2. Banks open branches.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. 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. *$P<.1$, **$P<.05$, ***$P<.01$.

	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
First stage	0.81	0.80	0.80	0.80
Bandwidth	3,490	3,621	2,972	3,329
Obs. in BW	223	230	196	213
Observations	561	562	561	561
Baseline control	Yes	Yes	Yes	Yes

and the number of branches. Since I observe years between 1997 and 2016, I test both for smoothness pre-policy and discontinuities post-policy. In Table 2, I examine the number of branch licenses and branches in 2004, 1 year before the policy, and in 2010, 5 years after the policy.[3] 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, I observe statistically significant discontinuities in both branch licenses and branches. In 2010, treatment districts have 21 percent more branch licenses and 19 percent more branches than control districts (fig. 5a and b).[4] The latter corresponds to an increase of 27 branches, compared to 142 branches in control districts. Private banks were the driving force behind this expansion, potentially boosting competition in the banking sector (Appendix Table A5). Private branches increase by 60 percent, while public branches increase by 12 percent relative to the control mean. In other words, while 17 percent of branches in the control are private, 53 percent of the new branches are private. The branch entry is economically meaningful. Utilizing administrative data on deposit accounts and credit amounts from the RBI, I find that treatment districts have 11 percent more deposit accounts (161,977 new accounts) and 15 percent more credit (US$148 million) (Appendix Table A6). Credit by private banks increases by 54 percent, contributing US$77 million to the total credit gained. While private banks provide 15 percent of total credit in the control, they provide 52 percent of the new credit. To summarize, the policy introduced exogenous and economically meaningful bank entry.

Providing further support of design, the dynamics of the branch opening follow the policy timing (fig. 5c and d). 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 when the final list of underbanked districts is published. As expected, the branch reaction is slightly lagged by 1 year: the coefficient becomes statistically significant in 2007. There is another pattern that the policy can explain. In 2010, as discussed in Section 1, the RBI allowed banks to open branches without licenses in eight states. The

[3] 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 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 the regression includes baseline controls.

[4] I convert log points into percentages by applying $e^{coef}-1$.

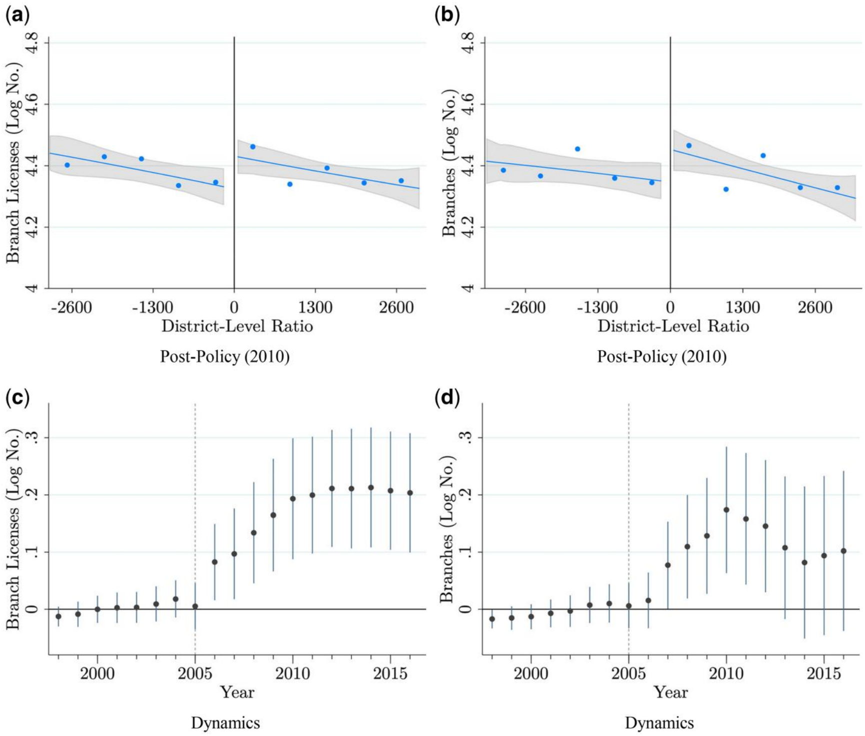

Figure 5. RBI issues licenses and banks open branches. (a) and (b) depict the discontinuities in branch licenses and branches five years after the policy was introduced. Respective regressions are described in columns 3 and 4 of Table 2. Figure (c) and (d) depict the dynamic effects of branch licenses and branches.

observed pattern in the dynamics—a stagnation in the coefficient on licenses issued and a decrease in the coefficient on the 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. One can conclude that the branch opening dynamics follow the RBI policy.

Standard robustness and placebo tests on bank outcomes are discussed in Section 8, but one placebo test that emerges from the design of the policy is outlined 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 (Appendix Table A7), and the coefficients in the placebo test are insignificant.

One question the reader may remain curious about is whether these new branches are profitable for the banks. Answering this question requires data on branch profits. 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. This article does not target a full policy evaluation but instead uses the policy to obtain exogenous variation in bank presence. However, it is possible to make one specific statement on profitability: 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 profitable for banks.

5. Mechanisms

5.1 Hypotheses and context

Many households across the world struggle to come up with financial resources to invest in health. In 2017, 800 million people spent more than 10 percent of their household budget on healthcare, and almost 100 million people were pushed into extreme poverty each year because of out-of-pocket medical expenses (WHO 2017). Beyond medical expenses, households need to invest in other items that are critical inputs for health. These include daily low fixed-cost items such as food or hygiene expenses, as well as high fixed-cost items such as fridges or toilet facilities. Two potential ways how households could come up with financial resources to invest in health include securing employment as well as relying on personal financial services such as savings accounts or credit products.[5]

New bank branches have the potential to both stimulate employment and provide households access to financial products. Research has demonstrated that increased bank presence can stimulate employment in other contexts (Bruhn and Love 2014). Many small and medium-sized enterprises—a backbone of the Indian economy—are credit-constrained (de Mel, McKenzie, and Woodruff 2008; Bruhn and Love 2014; Banerjee and Duflo 2014). Additionally, banks might provide households with savings or credit products. Increased access to savings accounts could improve health primarily through an increase in precautionary savings, which can then be utilized to pay for healthcare in times of need. Dupas and Robinson (2013b) explore how exactly savings products help households save, particularly in terms of health expenditures. They highlight the importance of mental accounting: Setting money aside to a designated place increases resistance to unplanned expenditures such as transfers to friends and relatives or temptation goods. In contrast, the study finds that other behavioral biases and reduced risk of theft play a minor role. While Dupas and Robinson (2013b) examine informal savings products, when considering bank accounts, deposit rates could also increase the money available for health. While this explanation should not be ruled out, it likely plays a minor role. During the period of this study, regular bank accounts that allow unlimited withdrawals did not pay interest, and accounts that restrict the number or timing of withdrawals were fixed at 3.5 percent per annum between 2003 and 2011, meaning banks were not competing on these rates (RBI 2016).

Besides savings accounts, personal loans could allow households to invest in health. However, existing research finds little evidence of positive impacts on health investments (Karlan and Zinman 2010; Banerjee, Karlan, and Zinman 2015). Personal loans in developing countries face two key challenges. From the supply side, there are large information asymmetries, which could limit households' access to loans despite increased branch presence (Karlan and Zinman 2009). Even for those with enough collateral to bridge information frictions, demand-side challenges remain. Take-up of personal loans has been low in many studies, and demand has been shown to be sensitive to product features such as interest rates and maturity (Karlan and Zinman 2008). Additionally, households in developing countries often report an aversion to holding debt (Karlan, Morduch, and Mullainathan 2010).

While how much households can invest in health is a critical determinant of health status, the other side of the coin is healthcare supply. In India, like many developing economies, public and private healthcare provision exists. While public healthcare provision is often free or low-cost, studies have demonstrated that the quantity and quality of healthcare services are often insufficient. Banerjee, Deaton, and Duflo (2004) show that in rural areas of the state of Rajasthan, in weekly surveys of public health facilities, 45 percent of medical personnel are absent. Less than 3 percent of visits result in a medical test. While households often prefer the more expensive private healthcare, these services are also not

5 In developing countries, health insurance markets are often under-serving the local population due to large information asymmetries. For instance, it is very hard for an insurance provider to verify whether a household went to a hospital.

Table 3. Employment increases.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data Economic Census (2013). District level. All variables are transformed into log form and winsorized at the 1st and 99th percentile. *$P < .1$, **$P < .05$, ***$P < .01$.

	Total employment		
	Total (log nr.) (1)	Manufacturing (log nr.) (2)	Services (log nr.) (3)
Treated	0.12*	0.09	0.11*
	(0.07)	(0.10)	(0.06)
Control mean	11.83	10.35	11.50
First stage	0.80	0.78	0.80
Bandwidth	3,548	2,626	3,796
Obs. in BW	225	169	239
Observations	555	555	556
Baseline control	Yes	Yes	Yes

without their problems, such as unregulated and untrained personnel (Banerjee, Deaton, and Duflo 2004).[6] Against this background, improving healthcare provision could be critical. If bank presence allows (private) healthcare providers to relax their credit constraints, this could result in more and better healthcare provision.

The following analysis on intermediary outcomes allows me to rule in and out certain mechanisms, providing suggestive evidence. There is a small caveat. Intermediary outcomes like employment are determined in equilibrium. For instance, employment might increase because firms get access to credit; it might also be partially increasing because households are getting healthier. After ruling in and out certain mechanisms, Section 7 provides heterogeneity tests on the main health results to provide further evidence on the importance of respective mechanisms.

5.2 Results

5.2.1 Employment

First, I investigate whether banks provide credit access to firms, stimulating employment. As outlined in Section 4, total credit in the economy increases by 15 percent, or around US$148 million in 2010. To test whether this translates into an employment effect, I utilize the Economic Census pre-policy (2005) and post-policy (2013). I find that total employment increases in the economy by 12 percent (Table 3). This effect size is consistent with other branch expansion policies focused on labor market outcomes (Bruhn and Love 2014). The result is primarily driven by increased employment in the service sector. Employment is smooth pre-policy, as outlined in Appendix Table A8. Appendix Figure A4 shows graphical evidence of the mechanisms. This suggests that increased employment could allow households to invest more in health.[7]

To further investigate the hypothesis of a labor demand channel, I supplement my analysis of the Economic Census with the National Sample Survey, which provides information on daily wages. If there is an increase in labor demand, we should expect increased quantities (employment) and prices (daily wages). The main outcome is the average daily wage, which is calculated as wages and salaries obtained during the past week divided by the number of days worked in the past week for employed individuals. Table 4 describes the

[6] Across public and private healthcare provision, households are critical of the sector. In the DHS 2015/2016 survey, 36 percent saw the long distance to the closest health facility as a big problem. Fifty-two percent reported that personnel absenteeism is a big issue, and 53 percent had large problems with drug availability.

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

Table 4. Daily wages.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data National Sample Survey (NSS, 2009/2010). Individual level. Variable in log and winsorized at the 1st and 99th percentile. *$P<.1$, **$P<.05$, ***$P<.01$.

	Daily wage (log Rs) (1)
Treated	0.15**
	(0.07)
Control mean	4.88
First stage	0.90
Bandwidth	3,389
Obs. in BW	25,158
Observations	53,286
Baseline control	No

Table 5. More savings accounts but not bank loans for households.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS II (2011/2012). Household level. Households are asked whether they had any savings account or bank loan in the past 5 years. *$P<.1$, **$P<.05$, ***$P<.01$.

	Savings account (yes/no) (1)	Bank loan (yes/no) (2)
Treated	0.19**	0.04
	(0.10)	(0.05)
Control mean	0.51	0.23
First stage	0.69	0.66
Bandwidth	3,023	2,370
Obs. in BW	16,911	13,093
Observations	38,348	38,351
Baseline control	No	No

results. Consistent with a labor demand channel and employment results, I find that daily wages increase by 16 percent.

5.2.2 Savings accounts and personal credit

To explore the financial access of households directly, I utilize the IHDS (2011/2012). Households are asked whether they had any savings account or bank loan in the past 5 years. I find that households are significantly more likely to own a savings account. Households in treatment districts are nineteen percentage points more likely to own a savings account compared to a control mean of 51 percent (Table 5). In contrast, the average household in my sample is not more likely to have a bank loan. Take-up of financial instruments by households is balanced pre-policy (Appendix Table A9). Not all outcome variables are available pre-policy, in which case, similar dimensions of financial access are shown to be smooth. Thus, these results provide suggestive evidence that savings accounts to households played a role in improving health, while households' formal medical debt is unlikely to play a role. While Dupas and Robinson (2013b) demonstrate the causal effect of savings products on health savings, it remains possible that increased use of savings accounts is a byproduct of a positive income shock rather than a direct driver of improved health outcomes.

5.2.3 Households' health investments

Both employment and savings accounts could have translated into changes in households' health investments. Households might improve their health by spending more on low-fixed-cost items such as food or hygiene, high-fixed-cost items such as toilet facilities or fridges, or by increasing their healthcare demand. Consistent with a gradual increase in the availability of resources through employment and savings accounts, I find that households in treatment districts consume more meals and spend more on hygiene expenses (Appendix Table A10). Hygiene expenses include soap, insecticides, and toilet articles. Both higher food consumption by strengthening the body and higher hygiene expenses by reducing infection risks can positively impact non-chronic diseases. In alignment with a lack of credit take-up, I do not find evidence that households invest in high-fixed-cost items such as toilet facilities or fridges (Appendix Table A10).

Studying healthcare demand is complex. One might be tempted to take healthcare expenditure or visits for diseases as proxies for demand. In the context of this study, they are not suitable proxies. To understand why, note that these variables are measured 6 to 10 years after the policy introduction. It is possible that households spent more and visited more in the years after the policy introduction, are healthier at the point of the survey, and require fewer healthcare services, respectively. A negative effect on spending and visits then does not reflect a decrease in healthcare demand but an increase in health status. Thus, I explore an alternative proxy for healthcare demand. I examine healthcare utilization of services that should not decrease as households get healthier. In particular, I consider vaccinations and pregnancy care. Both are higher in treatment districts, providing suggestive evidence that households increase their healthcare demand (Appendix Table A11). To summarize, employment and savings accounts likely translated into higher spending on low-fixed-cost items and healthcare demand but not on high-fixed-cost items.

5.2.4 Healthcare supply

To investigate the effect of bank presence on healthcare supply, I utilize data from the Economic Census. The main question on finance in this data is the major source of financing. One caveat is that this question masks the true prevalence of bank financing since major sources of finance are often self-finance (44 percent) or government sources (39 percent). The fact that few healthcare providers cite institutional loans as their major source of finance does not imply that they do not rely on bank loans. Healthcare providers are only slightly less likely to cite an institutional loan as their major source of finance than all businesses (2.11 percent).

With this caveat, I investigate the Economic Census to learn how healthcare activity responds to bank presence (Table 6). I find that treatment districts have a one percentage point or 65 percent increase in the share of healthcare providers primarily financed by a loan. As outlined, this likely masks an overall larger effect in absolute terms, as the survey only asks about the biggest loan and not any loan. In equilibrium, I observe an increase in the number of healthcare providers. These are not large healthcare providers; they have, on average, only seven employees. Appendix Table A12 shows the smoothness of financial access and healthcare supply before the policy. Consistent with the increase in healthcare supply post-policy, I find in the DHS that households are more likely to shift to private providers (Appendix Table A13). To summarize, intermediary outcomes suggest that bank presence affects health through established activities (employment, savings accounts) and understudied aspects (credit to healthcare providers) but not through personal bank loans.

Additionally, I utilize the Prowess dataset to provide insights into the liability structure of healthcare providers. This data provides detailed financial information about a sample of relatively large healthcare providers. I focus on the financial year 2013/2014, corresponding to the year of the Economic Census. The reader should note that the number of distinct hospitals and districts is limited. Thus, I use this data only for descriptive purposes

Table 6. Healthcare supply increases.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data Economic Census (2013). District level. Variables in log and winsorized at the 1st and 99th percentile. *$P < .1$, **$P < .05$, ***$P < .01$.

	Healthcare providers	
	Institutional loan (share) (1)	Number (log nr.) (2)
Treated	0.01**	0.89***
	(0.00)	(0.33)
Control mean	0.01	5.96
First stage	0.79	0.80
Bandwidth	2,435	3,127
Obs. in BW	164	204
Observations	556	556
Baseline control	No	No

and not for regression analysis. Supplementary Appendix Table B5 provides the liability structure for healthcare providers and other non-financial firms. The average healthcare provider has a size of US$30.87 million in terms of total assets or total liabilities, approximately a third of the size of other firms in the sample. These healthcare providers rely on borrowing, in particular from banks. The average bank loan liability is US$2.67 million (short-term plus long-term liabilities), corresponding to 9 percent of total liabilities. This is approximately two-thirds of that of other companies (15 percent). In terms of the probability of having a bank loan, 70 percent of healthcare providers obtain one. While Prowess describes relatively large health-care providers, bank loans as a financial instrument are used by healthcare providers across the size range within the data (see Supplementary Appendix fig. B1). Prowess only contains one public hospital, but these usually rely on government sources for funding and not on private capital. Supplementary Appendix Discussion B2 provides an overview of other resources on healthcare financing in developing countries.

5.2.5 Governments

Additionally, I test another hypothesis: the increase in business activity post-policy could have increased local tax revenue and, thus, government spending on health. However, empirically, I do not find any effects on government spending on health-related categories (Appendix Table A14). This is consistent with local governments' difficulties in collecting taxes in this context. Similar to the test that other government policies, like the insurance scheme Rashtriya Swasthya Bima Yojana (RSBY), do not threaten identification, this suggests that the government did not play a role beyond incentivizing banks to enter.

6. Health status

Can the increase in employment, savings accounts, and healthcare supply move the needle on households' health? To answer this question, I next turn to indicators of households' health status. Consistent with other health economics studies, I investigate two primary outcomes: non-chronic and chronic illnesses. Non-chronic illnesses are frequent in many developing countries; they include illnesses such as fever, diarrhea, and cough. In the IHDS II, 55 percent of households experience a non-chronic disease in a given month. Conditional on the illness, households face 10 days of illness, summed over the members. They spend 621 rupees (6 percent of total monthly consumption) and lose 6 days of work

Table 7. Fewer non-chronic illnesses.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS II (2011/2012). Household level. All variables measured in days are in log and winsorized at the 1st and 99th percentile. Non-chronic illnesses include fever, diarrhea, and cough in the past 30 days. Chronic illnesses include, for instance, heart disease and cancer, ever diagnosed (column 4) or days unable to work in the past 12 months (column 5). *$P < .1$, **$P < .05$, ***$P < .01$.

	Non-chronic illness			Chronic illness	
	Any (yes/no) (1)	Days ill (log no.) (2)	Days missed (log no.) (3)	Any (yes/no) (4)	Days missed (log no.) (5)
Treated	–0.19**	–0.43**	–0.61***	–0.00	–0.31
	(0.08)	(0.19)	(0.20)	(0.05)	(0.20)
Control mean	0.52	1.08	0.79	0.39	1.04
First stage	0.65	0.66	0.67	0.65	0.63
Bandwidth	2,204	2,312	2,440	2,189	2,087
Obs. in BW	11,986	12,927	13,595	11,953	10,518
Observations	36,673	38,375	38,485	36,673	36,673
Baseline control	No	No	No	No	No

or school. Thus, non-chronic illnesses are frequent and consequential. To test for changes in health status, I rely on two nationally representative household surveys. The IHDS II (2011/2012) allows me to measure non-chronic and chronic diseases 6 years after the policy introduction. The DHS (2015/2016) allows me to replicate the results in the long term, 10 years later.

The IHDS II (2011/2012) provides information on whether any household member was ill in the past 30 days with a non-chronic disease, which refers to fever, diarrhea, or cough (Table 7, column 1). Additionally, I observe the number of days household members were ill (column 2) or could follow usual activities such as work or school (column 3), aggregated over members. I find improvements for non-chronic illnesses. Households in treatment districts are nineteen percentage points less likely to have a member who suffered from a non-chronic disease in the past month. Comparing this to the control mean corresponds to a decrease of 36 percent. The discontinuity is depicted in figure 6a. As households gain, on average, two healthy days, they also increase their labor supply and school attendance. Thus, this article provides evidence of both an increase in labor demand and labor supply. An interaction may exist between the two: higher labor demand raises income levels, enabling greater investments in health, which in turn improves health outcomes and further enhances labor supply.

While I observe a positive impact on non-chronic illnesses, I do not find an improvement in chronic illnesses like heart disease or cancer (Table 7, columns 4 and 5). This could have multiple reasons. First, unlike non-chronic diseases, the prevalence of chronic diseases is likely much less responsive to household investments in food and sanitation. Additionally, even with an increase in healthcare demand or supply, healthcare providers might not be equipped to deal with these diseases as they lack expertise or expensive equipment.

How does the effect size on non-chronic illnesses compare to other health interventions? Appendix Table A15 provides an overview of meta-studies and other benchmark papers, showing that the effect size is in the middle of the range of other successful health interventions in developing countries. The health economics literature contextualizes these effect sizes. For many non-chronic diseases, there exist highly effective and relatively cheap treatments, for example, oral rehydration solutions for diarrhea (Banerjee and Duflo 2011; Dupas and Miguel 2017). Additionally, improving the health of some households could

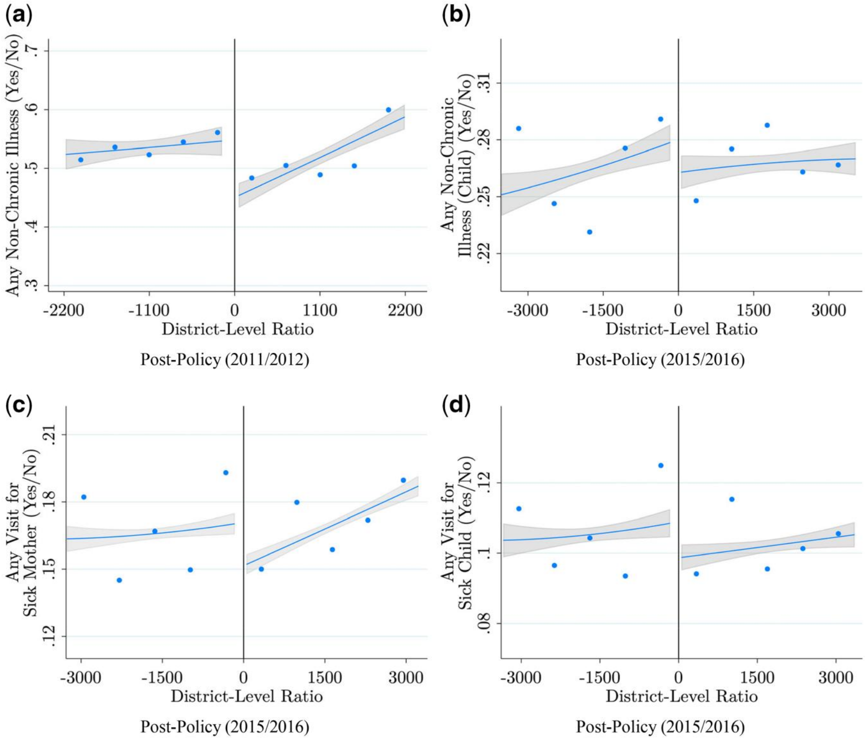

Figure 6. Health improves. These figures depict binned means to the left and right of the cutoff within the optimal bandwidth. They also depict local linear polynomials to the left and right of the cutoff, with 95% confidence intervals in gray. The cutoff is normalized to zero.

have spillover effects, reducing infection risks of others (Kremer and Glennerster 2011). Thus, the effect sizes on non-chronic diseases are comparable to the literature and sensible given the context. To provide further confidence in the effect, I show that outcomes in Table 7 are smooth on baseline (see fig. 4e, Table 1 and Appendix Table A16) and robust to controlling for baseline measures (see Appendix Table A17). Appendix Table A18 discusses robustness to different transformations, including level and inverse hyperbolic sine. Further robustness is discussed in Section 8.

To replicate my findings from the IHDS and obtain long-term effects, I utilize the DHS (2015/2016) in Table 8. Data on non-chronic diseases are only collected for children below five. I find that households are six percentage points less likely to have a child with fever, diarrhea, or cough in the past two weeks. Since in the control group, around every fourth household has an ill child, this corresponds to a mean change of 23 percent. I use the proxy of healthcare visits to understand diseases for other family members. Visits are a function of health status, demand, and supply; thus, they do not perfectly reflect the incidence of illnesses. With this caveat, results are consistent with households getting healthier. They are two percentage points less likely to go to a healthcare provider for treatment of a sick child and five percentage points less likely for treatment of a sick mother. Discontinuities are depicted in figure 6. Note that visits are measured 10 years after the policy. Healthcare visits likely increased in the first years after the policy, but as households get healthier, they require these services less. With positive effects in the DHS, two different surveys indicate that bank presence improves non-chronic diseases.

Table 8. Results hold in second survey.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data DHS (2015/2016). Household level. Column 1 shows whether a child had a non-chronic disease (fever, diarrhea, cough) in the past two weeks. Columns 2 and 3 indicate healthcare visits for any illness in the past 3 months. Data on non-chronic diseases are only collected for children below 5 years. Data is missing for households without children below 5 years in column 1 and without eligible women in columns 2 and 3. *$P < .1$, **$P < .05$, ***$P < .01$.

	Non-chronic illness	Any illness	
	Sick child (yes/no) (1)	Visit for sick child (yes/no) (2)	Visit for sick mother (yes/no) (3)
Treated	–0.06*	–0.02*	–0.05*
	(0.03)	(0.01)	(0.03)
Control mean	0.27	0.11	0.17
First stage	0.70	0.73	0.72
Bandwidth	3,539	3,383	3,277
Obs. in BW	66,658	187,208	182,318
Observations	171,471	471,985	471,985
Baseline control	No	No	No

The reader may ask whether the results are biased by differential reporting on health status in treatment and control. First, the bias would go in the opposite direction. If banks positively affect households' awareness about diseases or the likelihood of being diagnosed, this would create an upward bias in the estimate, reducing the chance of detecting a decrease in reported diseases. Second, I can study outcomes that are not self-reported, such as vaccinations reported on a vaccination card. As expected, I find positive effects for these outcomes (Appendix Table A19). Thus, self-reporting biases are unlikely to play a role.

7. Heterogeneity

I next turn toward a heterogeneity analysis. This investigation allows me to provide further suggestive evidence of the relative importance of mechanisms.[8] The key assumption is that ex-ante levels of, for instance, employment should be predictive of how strong the respective employment mechanism plays out. Then, if employment is an important mechanism, we expect the health status results to differ based on whether ex-ante employment is below or above the median.[9] Table 9 describes the results. Employment exhibits the largest differential, followed by savings and then healthcare supply. In terms of relative magnitudes, the differential for employment is 1.1 times larger than that for savings, and the differential for savings is 2.8 times larger than that for healthcare supply. This carefully suggests that employment and savings are important determinants of the effect, meaning a demand-side story is more relevant than a supply-side story.[10]

[8] Note that exactly quantifying the relative contribution of the channels requires isolated exogenous variation in each of the mechanisms, which is very hard to obtain and out of the scope of this study.

[9] The direction of the effect—whether ex-ante higher exposure increases or decreases the effect—is not obvious. For instance, we might expect a stronger effect of employment in areas below the median (because employment that gets created is more important) or above the median (because more employment gets created if the region has some baseline employment). Thus, I focus on the difference in coefficients in the subsamples.

[10] A second method to gauge the relative importance of the mechanism is benchmarking to other studies. Unfortunately, there is a lack of studies that vary employment, savings accounts, or healthcare supply. Concerning employment, one might refer to cash transfers (Haushofer and Shapiro 2013; Egger et al. 2019). These studies find null effects on health but have large differences to regular employment and are relatively short-term in smaller samples. Literature has shown that providing a place to keep money safe can substantially increase health savings (Dupas and Robinson 2013b).

Table 9. Heterogeneity tests.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Household level. The sample is split by median based on ex-ante employment, savings accounts, and hospital presence. *$P < .1$, **$P < .05$, ***$P < .01$.

	Non-chronic illness (yes/no)					
	Employment below median (1)	Employment above median (2)	Savings below median (3)	Savings above median (4)	Hospital below median (5)	Hospital above median (6)
Treated	–0.06	–0.21***	–0.16	–0.30***	–0.22**	–0.17***
	(0.07)	(0.05)	(0.15)	(0.06)	(0.09)	(0.05)
Control mean	0.50	0.51	0.51	0.54	0.51	0.47
First stage	0.75	0.80	0.46	0.84	0.77	0.78
Bandwidth	4,901	2,753	2,030	2,382	4,658	2,908
Obs. in BW	8,555	9,708	5,470	5,853	10,002	9,053
Observations	15,915	19,816	16,677	17,714	17,812	18,208
Baseline control	No	No	No	No	No	No

8. Robustness and placebo tests

To demonstrate the robustness of my results, I initially test whether coefficients remain statistically significant for different bandwidth choices. I examine bandwidth multipliers in the range of 0.50 to 2.00, in steps of 0.25. For instance, if the mean square error (MSE)-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014) is 2,000, I examine bandwidths from 1,000 to 4,000. Results are described in Supplementary Appendix Table B6 as well as Supplementary Appendix figures B2 and B3. Considering the optimal bandwidth with multipliers of 0.75 and 1.25, 73 percent remain statistically significant. Examining bandwidth multipliers of 0.50 and 1.50, 59 percent remain statistically 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, Cattaneo, and Titiunik (2014) that chooses identical bandwidths to the left and to the right of the cutoff. In Supplementary Appendix Tables B7 and B8, I also consider an MSE-optimal selector that separately chooses bandwidths to the left and to the right of the cutoff. Additionally, I examine a selector by Calonico, Cattaneo, and Farrell (2020) that optimizes the coverage error rate. I again consider the selector with identical and different bandwidths to the left and right of the cutoff. Supplementary Appendix figure B4 summarizes the results. I find that 72 to 82 percent of results remain statistically significant. This suggests that results are robust to different bandwidth selectors.

Results are also robust considering possible bias corrections due to the MSE-optimal bandwidth selector, discussed by Calonico, Cattaneo, and Titiunik (2014) and Cattaneo and Vazquez-Bare (2017). This is depicted in Supplementary Appendix Tables B7 and B8, and summarized in Supplementary Appendix figure B5. All of the coefficients 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 linear or quadratic approximations. Additionally, I examine the robustness of polynomials of degree three. Findings are described in Supplementary Appendix Table B9 and summarized in Supplementary Appendix figure B6. For polynomials of degree two, 82 percent of outcomes remain statistically significant. For polynomials of degree three, I find that 55 percent of outcomes remain significant. In summary, results are robust to alternative polynomials.

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): ± 1,000, 2,000, and 3,000. This choice of placebo cutoffs ensures enough observations around the placebo cutoff to conduct an analysis. Evidence is provided in Supplementary Appendix Table B10 and summarized in Supplementary Appendix figure B7. On average, 12 percent of outcomes are statistically significant. Thus, I find little evidence of discontinuities at placebo cutoffs.

Finally, I test whether results are robust to adjustments for multiple hypothesis testing and spatial correlation of standard errors in Supplementary Appendix Table B11, summarized in Supplementary Appendix figure B8. 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. Results are robust to multiple hypothesis testing and spatial correction. In summary, the standard robustness and placebo tests support the validity of the findings.

9. Conclusion

What are the general equilibrium effects of banks? While previous work has focused on labor market effects, this study demonstrates that banks can contribute toward tackling hard-to-crack development challenges, focusing on the third UN Sustainable Development Goal of improving health. This article utilizes a 2005 RBI policy to obtain exogenous variation in bank presence, applying a regression discontinuity design. After establishing that the policy introduced exogenous and economically meaningful bank entry, I show evidence that households benefit from higher employment and savings accounts, increasing healthcare demand and spending on low-fixed-cost items such as food or hygiene. Personal bank loans do not play a role; in alignment with this, I do not find an effect on high-fixed-cost items such as fridges or toilet facilities. Additionally, healthcare supply expands, even though a heterogeneity analysis suggests that this channel plays a smaller role. To investigate the effect on health status, I examine two nationally representative household-level surveys 6 and 10 years after the policy introduction. Both surveys confirm that banks can move the needle on non-chronic diseases. There is no effect on chronic illnesses. The study encourages further exploration into the impact of finance on various dimensions of well-being, including education. Gaining insights into these inquiries can substantially advance our understanding of the impact of banks on households.

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Supplementary material

Supplementary material is available at *Review of Finance* online.

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Conflict of Interest

There is no conflict of interest to declare.

Data availability statement

Please refer to the data availability statement in the supplementary materials.

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Table A1. Economic activity and population characteristics are smooth pre-policy.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data SHRUG. District level. Combining different datasets, 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 Defense Meteorological Satellite Program Operational Line-Scan System annual measures of nighttime luminosity. Employment measures the total employment, followed by a split by manufacturing and services. The Population Census measures total population, total literate population, and whether there is a tar road. $*P<.1$, $**P<.05$, $***P<.01$.

	1990	1991	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Nightlights														
Total light (log)			−0.48	−0.25	−0.13	−0.13	−0.20	−0.18	−0.19	−0.25	−0.26	−0.20	−0.16	−0.11
			(0.35)	(0.22)	(0.23)	(0.21)	(0.22)	(0.21)	(0.20)	(0.21)	(0.20)	(0.21)	(0.19)	(0.21)
Economic Census														
Empl. (log no.)	−0.06						−0.03							0.01
	(0.52)						(0.17)							(0.13)
Empl. manuf. (log no.)	−0.08						−0.33							−0.11
	(0.46)						(0.22)							(0.21)
Empl. services (log no.)	0.09						0.03							0.05
	(0.51)						(0.19)							(0.11)
Population Census														
Pop. (log no.)		0.05								0.02				
		(0.09)								(0.07)				
Pop. literate (log no.)		−0.01								−0.02				
		(0.14)								(0.09)				
Tar road (yes/no)		−64.59								32.16				
		(61.27)								(105.73)				
Observations	574	574	574	574	574	574	574	574	574	574	574	574	574	574

Table A2. Negligible migration.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS II (2011/2012). Household level. *$P < .1$, **$P < .05$, ***$P < .01$.

	Migrated five years ago from other district (yes/no) (1)	Migrated anytime in the past ninety years from other district (yes/no) (2)	Migrated five 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
First stage	0.54	0.66	0.61
Bandwidth	1,633	2,363	1,982
Obs. in BW	8,302	13,099	9,981
Observations	35,985	38,375	36,397
Baseline control	No	No	No

Table A3. Other policies do not confound results (1/2).
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. National Health Mission (NHM), Integrated Child Development Services (ICDS), National Rural Employment Guarantee Act (NREGA), Rashtriya Swasthya Bima Yojana (RSBY). Data Ministry of Health and Family Welfare, Ministry of Women and Child Development, Ministry of Rural Development, and Ministry of Labour and Employment. District level. Regressions do not include state-level fixed effects. *$P < .1$, **$P < .05$, ***$P < .01$.

	NHM (yes/no) (1)	ICDS (1st wave) (yes/no) (2)	NREGA (1st wave) (yes/no) (3)	NREGA (2nd wave) (yes/no) (4)	RSBY (yes/no) (5)
Treated	0.22	–0.16	–0.23	–0.03	–0.04
	(0.24)	(0.15)	(0.20)	(0.25)	(0.25)
Control mean	0.17	0.24	0.16	0.23	0.55
First stage	0.69	0.76	0.71	0.67	0.71
Bandwidth	2,557	3,995	2,876	2,333	2,806
Observations in BW	171	253	191	156	187
Observations	581	581	581	581	581
Baseline control	No	No	No	No	No

Table A4. Other policies do not confound results (2/2).
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data Ministry of Health and Family Welfare, Ministry of Women and Child Development, Ministry of Rural Development, and Ministry of Labour and Employment. District level. Percent refers to the number of total districts within a given category; for example, for priority districts above cutoff (percent) within bandwidth, they constitute 26 percent of all districts above the cutoff within bandwidth. *$P < .1$, **$P < .05$, ***$P < .01$.

	NHM (1)	ICDS (1st wave) (2)	NREGA (1st wave) (3)	NREGA (2nd wave) (4)	RSBY (5)
All districts					
Total priority districts (no.)	169	180	196	125	355
Total priority districts (%)	29	31	34	22	61
Priority districts above cutoff (no.)	135	142	170	85	217
Priority districts above cutoff (%)	36	38	45	23	58
Priority districts below cutoff (no.)	34	38	26	40	138

(continued)

Table A4. (continued)

	NHM (1)	ICDS (1st wave) (2)	NREGA (1st wave) (3)	NREGA (2nd wave) (4)	RSBY (5)
Priority districts below cutoff (%)	17	19	13	20	67
Corr priority district and 1 [above]	0.20	0.20	0.33	0.04	–0.09
Within BW (–3,000; 3000) districts					
Total priority districts (no.)	44	55	53	41	102
Total priority districts (%)	29	31	34	22	61
Priority districts above cutoff (no.)	26	33	39	20	53
Priority districts above cutoff (%)	23	30	35	18	48
Priority districts below cutoff (no.)	18	22	14	21	49
Priority districts below cutoff (%)	20	25	16	24	56
Corr priority district and 1 [above]	0.04	0.05	0.22	–0.07	–0.08

Table A5. Private banks react stronger.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data RBI. District level. All variables are winsorized at the 1st and 99th percentile. *$P < .1$, **$P < .05$, ***$P < .01$.

	Post-policy (2010)	
	Private branches (log no.) (1)	Public branches (log no.) (2)
Treated	0.47***	0.12**
	(0.17)	(0.05)
Control mean	2.77	4.30
First stage	0.80	0.80
Bandwidth	2,963	3,115
Obs. in BW	195	205
Observations	561	561
Baseline control	Yes	Yes

Table A6. Branch entry is economically meaningful.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data RBI. District level. All variables are winsorized at the 1st and 99th percentile. The total credit amount in column 2 does not include regional rural banks, which were excluded from the policy. *$P < .1$, **$P < .05$, ***$P < .01$.

	Deposit accounts (log no.) (1)	Credit amount (log no.) (2)	Private bank credit amount (log no.) (3)
Treated	0.10*	0.14*	0.43***
	(0.06)	(0.07)	(0.15)
Control mean	13.81	3.14	1.37
First stage	0.79	0.79	0.79
Bandwidth	2,542	2,348	2,248
Obs. in BW	170	157	147
Observations	561	553	553
Baseline control	Yes	Yes	Yes

Table A7. Placebo test: regional rural banks do not react to the policy.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data RBI. District level. All variables are winsorized at the 1st and 99th percentile. Only regional rural banks are analyzed. *$P < .1$, **$P < .05$, ***$P < .01$.

	Post-policy (2010)	
	Branch licenses (log no.) (1)	Branches (log no.) (2)
Treated	–0.54	–0.08
	(0.48)	(0.48)
Control mean	1.51	1.09
First stage	0.80	0.80
Bandwidth	2,812	2,959
Obs. in BW	187	195
Observations	561	561
Baseline control	Yes	Yes

Table A8. Employment is smooth pre-policy.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data Economic Census (2005). District level. Variables in log and winsorized at the 1st and 99th percentile. *$P < .1$, **$P < .05$, ***$P < .01$.

	Total employment		
	Total (log nr.) (1)	Manufacturing (log nr.) (2)	Services (log nr.) (3)
Treated	0.01	–0.12	0.04
	(0.12)	(0.20)	(0.11)
Control mean	11.57	10.09	11.23
First stage	0.78	0.77	0.79
Bandwidth	2,713	2,635	2,985
Obs. in BW	180	171	195
Observations	555	555	555
Baseline control	No	No	No

Table A9. Households' financial access is smooth pre-policy.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS I (2004/2005). Household level. Variable in Rs is transformed to log and winsorized at the 1st and 99th percentile. Note that other variables of financial access reported post-policy are not available in the first survey round. *$P < .1$, **$P < .05$, ***$P < .01$.

	Any loan (yes/no) (1)	Largest loan amount (log Rs) (2)	Largest loan from bank (yes/no) (3)
Treated	0.02	0.39	–0.02
	(0.09)	(0.78)	(0.02)
Control mean	0.40	3.92	0.12
First stage	0.69	0.70	0.71
Bandwidth	3,821	3,862	4,325
Obs. in BW	16,183	16,395	18,090
Observations	31,911	31,913	31,912
Baseline control	No	No	No

Table A10. Households spend more on food and hygiene.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS (2011/2012). Household level. Hygiene expenses in the past month in log rupees and winsorized at the 1st and 99th percentile. The toilet variable is a rank where 1 is no toilet, 2 is the traditional pit latrine, 3 is a semi-flush latrine, and 4 is a flush toilet. $^{*}P<.1$, $^{**}P<.05$, $^{***}P<.01$.

	Low fixed cost		High fixed cost	
	Hygiene expenses (log Rs) (1)	Meals per day (no.) (2)	Fridge (yes/no) (3)	Toilet (rank) (4)
Treated	0.17**	0.24**	–0.01	–0.08
	(0.08)	(0.12)	(0.07)	(0.14)
Control mean	4.03	2.74	0.21	2.15
First stage	0.65	0.65	0.58	0.70
Bandwidth	2,193	2,266	1,837	3,426
Obs. in BW	11,974	12,458	9,563	18,088
Observations	36,640	38,045	36,432	38,397
Baseline control	No	No	No	No

Table A11. Suggestive evidence of higher healthcare demand.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data DHS (2015/2016). Household level. These indicators for healthcare utilization are indirect measures of healthcare demand that—unlikely medical expenditure or visits for diseases—are not likely to decrease with improved health status. $^{*}P<.1$. $^{**}P<.05$. $^{***}P<.01$.

	Vaccinated child (yes/no) (1)	Birth in healthcare facility (yes/no) (2)
Treated	0.072*	0.005***
	(0.040)	(0.002)
Control mean	0.86	0.02
First stage	0.72	0.72
Bandwidth	2,898	3,023
Obs. in BW	26,117	172,892
Observations	86,079	471,985
Baseline control	No	No

Table A12. Healthcare activity is smooth pre-policy.
Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data Economic Census (2005). District level. Variables in log and winsorized at the 1st and 99th percentile. $^{*}P<.1$, $^{**}P<.05$, $^{***}P<.01$.

	Healthcare providers	
	Institutional loan (share) (1)	Number (log nr.) (2)
Treated	0.00	–0.15
	(0.01)	(0.16)
Control mean	0.03	5.42
First stage	0.79	0.80
Bandwidth	2,638	4,328
Obs. in BW	173	273
Observations	556	557
Baseline control	No	No

Table A13. Shift toward private providers.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data DHS (2015/2016). Household level. *$P<.1$, **$P<.05$, ***$P<.01$.

	Government provider (yes/no) (1)	Generally go for treatment to 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
First stage	0.73	0.71	0.69
Bandwidth	2,898	2,648	2,262
Obs. in BW	202,459	184,429	156,853
Observations	577,928	577,928	566,715
Baseline control	No	No	No

Table A14. No effect on state expenditure.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data RBI (2010). Variable in lakh (= hundred thousand) Rs and transformed to log plus winsorized at the 1st and 99th percentile. *$P<.1$, **$P<.05$, ***$P<.01$.

	Medical and public health (log lakh Rs) (1)	Water supply and sanitation (log lakh Rs) (2)	Nutrition (log lakh Rs) (3)
Treated	–0.14	0.06	–0.43
	(0.25)	(0.30)	(0.60)
Control mean	11.95	10.58	10.75
First stage	0.71	0.74	0.76
Bandwidth	2,769	3,397	3,377
Obs. in BW	182	213	194
Observations	570	570	470
Baseline control	No	No	No

Table A15. Health studies.

Study	Context	Treatment	Duration	Outcome	Effect size
Waddington et al. (2009) (IIIE)	Meta-analysis	Water, sanitation, and hygiene interventions	5 months –2 years	Child diarrhea	31–42 percent decrease in past weeks
Gertler (2004) (AER)	Mexico	Conditional cash program	2 years	Child non-chronic	27 percent decrease diseases past month
Kremer and Glennerster (2011) (QJE)	Kenya	Spring water protection	3 years	Child diarrhea	25 percent decrease past week
Luby et al. (2005) (Lancet)	Pakistan	Hand washing promotion	1 year	Child diarrhea	53 percent decrease
Sazawal and Black (2003) (Lancet infectious diseases)	Meta-analysis	Pneumonia case management	various	Child mortality	24 percent decrease
Björkman and Svensson (2009) (QJE)	Uganda	Community-based monitoring	1 year	Child mortality	33 percent decrease
Björkman-Nykvist et al. (2014) (working paper)	Uganda	Community workers health	3 years	Child mortality	27 percent decrease
Kidane and Morrow (2000) (Lancet)	Ethiopia	Education for mothers to detect and treat malaria	1 year	Child mortality	40 percent decrease
Bang et al. (1999) (Lancet)	India	Home-based neonatal care	2 years	Child mortality	46 percent decrease

Table A16. Smooth health status pre-policy.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS I (2004/2005). Household level. All variables measured in days are in log and winsorized at the 1st and 99th percentile. Non-chronic illnesses include fever, diarrhea, and cough in the past 30 days. Chronic illnesses include, for instance, heart disease and cancer, ever diagnosed (column 4) or days unable to work in the past 12 months (column 5). $*P<.1$, $**P<.05$, $***P<.01$.

	Non-chronic illness			Chronic illness	
	Any (yes/no) (1)	Days ill (log no.) (2)	Days missed (log no.) (3)	Any (yes/no) (4)	Days missed (log no.) (5)
Treated	–0.07	–0.21	–0.28	0.03	–0.07
	(0.06)	(0.17)	(0.19)	(0.05)	(0.19)
Control mean	0.40	0.83	0.63	0.26	0.70
First stage	0.71	0.69	0.66	0.65	0.66
Bandwidth	4,363	3,812	2,625	2,482	2,659
Obs. in BW	18,207	16,061	11,553	11,296	12,006
Observations	31,913	31,913	31,794	31,794	31,794
Baseline control	No	No	No	No	No

Table A17. Results hold with baseline control.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS II (2004/2005). Household level. All variables measured in days are in log and winsorized at the 1st and 99th percentile. Non-chronic illnesses include fever, diarrhea, and cough in the past 30 days. Chronic illnesses include, for instance, heart disease and cancer, ever diagnosed (column 4) or days unable to work in the past 12 months (column 5). $*P<.1$, $**P<.05$, $***P<.01$.

	Non-chronic illness			Chronic illness	
	Any (yes/no) (1)	Days ill (log no.) (2)	Days missed (log no.) (3)	Any (yes/no) (4)	Days missed (log no.) (5)
Treated	–0.20**	–0.44**	–0.57***	–0.01	–0.28
	(0.08)	(0.20)	(0.17)	(0.06)	(0.18)
Control mean	0.53	1.10	0.77	0.39	1.04
First stage	0.63	0.64	0.67	0.60	0.63
Bandwidth	2,327	2,376	2,922	2,045	2,256
Obs. in BW	12,967	13,099	16,453	10,160	12,544
Observations	31,710	31,710	31,794	30,179	31,710
Baseline control	Yes	Yes	Yes	Yes	Yes

Table A18. Results robust to different transformations.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data IHDS II (2011/2012). Household level. Since the number of days that household members are ill is zero for 45 percent of households, I show robustness for different transformations. The winsorization is at the 1st and 99th percentile. IHS is the inverse hyperbolic sine (IHS) transformation (IHS(x) $= \ln(x + \sqrt{(x^2+1)})$). Results are robust to different transformations. $^{*}P<.1$, $^{**}P<.05$, $^{***}P<.01$.

	Non-chronic illness (days ill)					
	log(1+x) winsorized (1)	log(1+x) not winsorized (2)	IHS winsorized (3)	IHS not winsorized (4)	Level winsorized (5)	Level not winsorized (6)
Treated	–0.43**	–0.43**	–0.53**	–0.53**	–2.68*	–2.80*
	(0.19)	(0.19)	(0.23)	(0.24)	(1.42)	(1.56)
Control mean	1.08	1.09	1.35	1.36	4.88	5.00
First stage	0.66	0.66	0.66	0.66	0.64	0.63
Bandwidth	2,312	2,303	2,314	2,306	2,139	2,079
Obs. in BW	12,927	12,834	12,927	12,927	11,222	10,518
Observations	38,375	38,375	38,375	38,375	36,673	36,673
Baseline control	No	No	No	No	No	No

Table A19. No evidence of bias due to self-reporting.

Standard errors are in parentheses and clustered on the district level. Bandwidth is abbreviated by BW. Data DHS (2015/2016). Household level. $^{*}P<.1$, $^{**}P<.05$, $^{***}P<.01$.

	Vaccinated child (yes/no) (1)
Treated	0.07*
	(0.04)
Control mean	0.86
First stage	0.72
Bandwidth	2,898
Obs. in BW	26,117
Observations	86,079
Baseline control	No

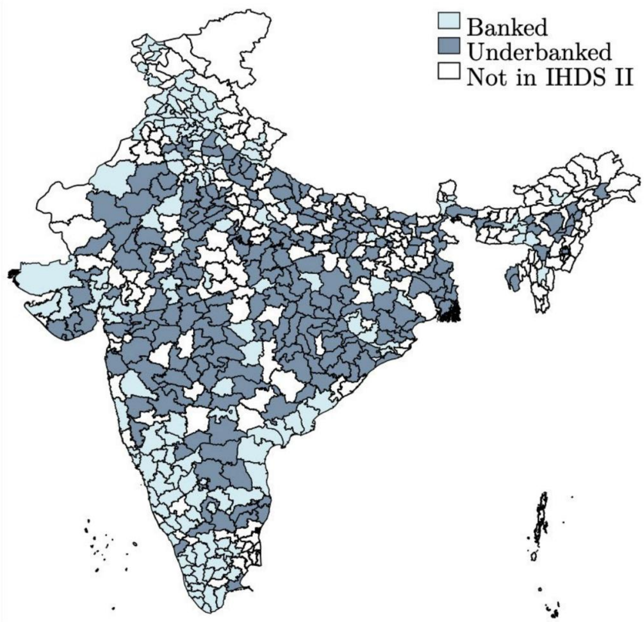

Figure A1. Districts surveyed. Map of India highlighting districts surveyed in IHDS II. Around 65 percent of all districts were covered.

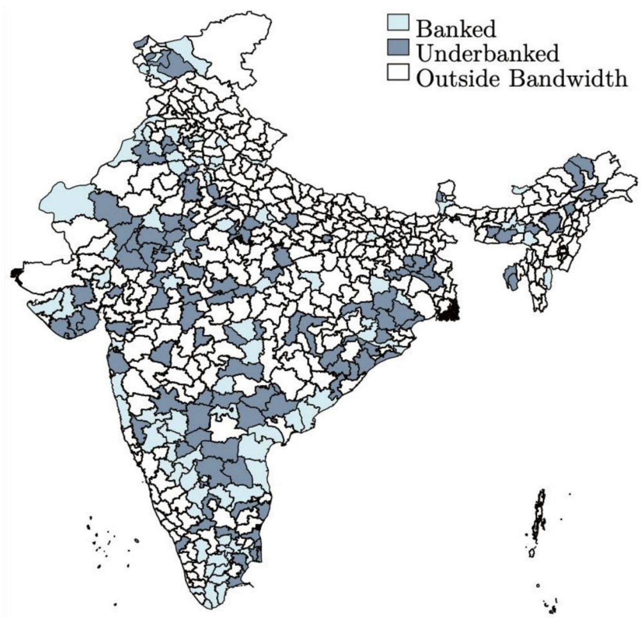

Figure A2. Districts with a population-to-branch ratio within the typical bandwidth. Map highlighting 111 districts underbanked (dark shade) and eighty-eight districts banked (light shade) within the typical bandwidth of ± 3,000.

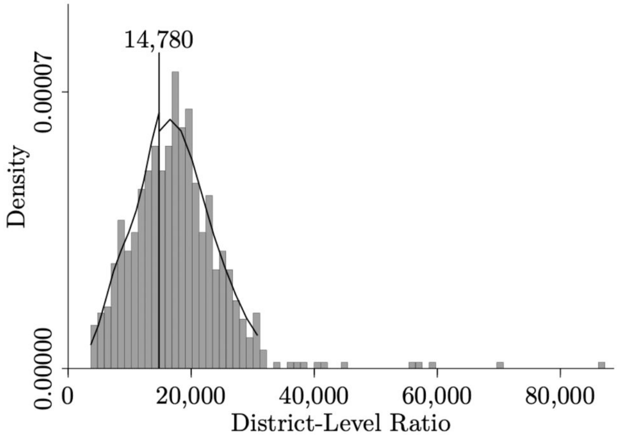

Figure A3. McCrary (2008) density test. This figure depicts the density of the population-to-branch ratio. The McCrary estimator is –0.1998 with a *P*-value of 0.8416; I do not reject smoothness around the cutoff.

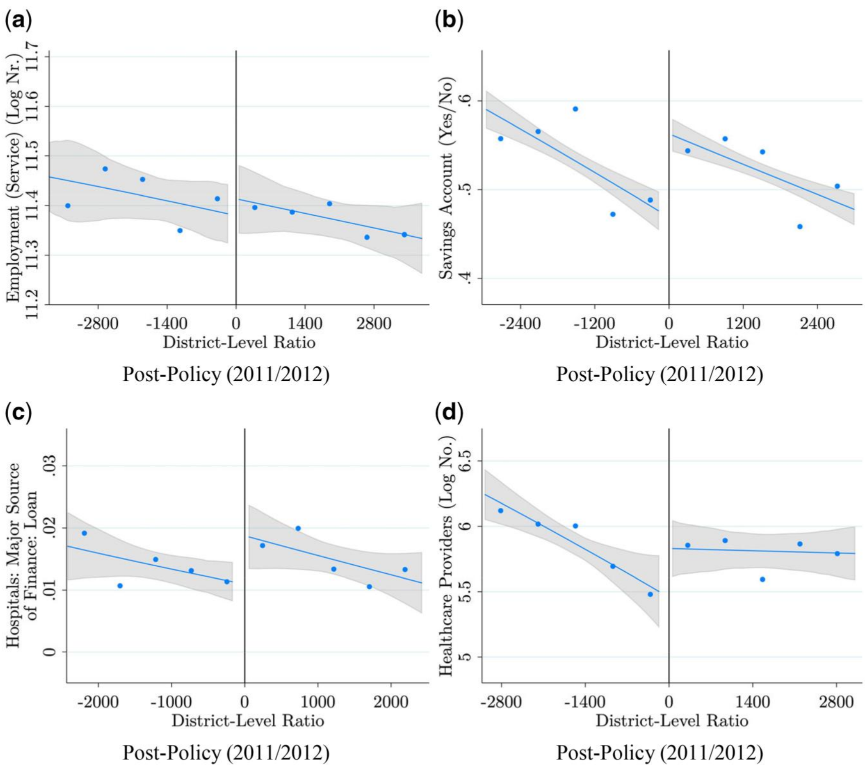

Figure A4. Mechanisms. These figures depict binned means to the left and right of the cutoff within the optimal bandwidth. They also depict local linear polynomials to the left and right of the cutoff, with 95% confidence intervals in gray. The cutoff is normalized to zero.

Appendix A

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Article