Banking on Education: How Credit Access Promotes Human Capital Development *

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Abstract

This paper presents new evidence on the impact of bank branch expansion and credit access on human capital outcomes for children. Using a regression discontinuity design, we study a branch authorization policy by the Reserve Bank of India that encouraged banks to open branches in underbanked districts, where the population-to-branch ratio exceeded the national average. Bank presence, bank lending, and household borrowing increased. We find significant improvements in test scores: children in underbanked districts scored 0.16–0.22 SD higher on reading and math. We document three mechanisms. First, we find evidence for a demand-side channel where parents spent more on their children's education and children spent more time on homework. Second, we document supply-side impacts in improvements in the quantity and quality of schools and teachers. Third, we find support for a labor market channel, with shifts away from agricultural employment and towards employment in manufacturing, while self-employed individuals expanded their businesses.

Keywords: Financial inclusion, human capital accumulation, regression discontinuity.

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1 Introduction

Globally, differences in human capital account for 20 to 50 percent of cross-country differences in income (Angrist et al., 2021). While many interventions have been proposed to solve entrenched development problems, including low levels of human capital, in recent years, much hope has been placed on the transformative power of financial access (Karlan and Morduch, 2010). However, little is known about the causal relationship between financial inclusion – specifically, credit for education – and human capital in developing country settings. On the one hand, financial access may allow credit-constrained families to invest more in their children's education (Prina, 2015). It may also allow credit-constrained businesses to expand (Banerjee et al., 2019), thereby further increasing resources for households that may be invested in children's education. On the other hand, the prevalence of child labor in developing countries may lead to more children working in agriculture or business, and reduce schooling (Bau et al. 2020, Hossain 2023). Financial access may also lead to sizeable general equilibrium effects (Breza and Kinnan, 2021) owing to supply-side responses by schools that may interact with demand-side responses from households. Overall, the net effect of financial access on human capital in developing country contexts is unclear.

This paper presents new causal evidence on the impact of bank branch expansion and credit access on human capital outcomes in India, where the largest share of the world's unbanked population (approximately 230 million adults) resides (Demirgüç-Kunt et al., 2022). As Burgess and Pande (2005) argue, credible evidence on whether banking access can improve economic and human capital outcomes remains limited given the nonrandom nature of these programs. Banks typically favor opening branches in richer areas, while state-led bank branch expansion programs tend to target poorer areas, making identification of the causal impact of branch expansion on outcomes challenging.

To overcome these identification challenges, we utilize a new branch authorization policy introduced by the RBI in September 2005 that encouraged banks to open branches in underbanked areas. While similar in spirit to the work of Burgess and Pande (2005), who use an earlier RBI bank branch expansion policy from 1977-1990 for identification, we focus on a newer policy reform introduced in 2005. This differs from the earlier 1977 reform, which mandated that to obtain a license for a branch opening in a location with one or more branches (a banked location), a bank must open branches in four eligible unbanked locations (known as the 1:4 licensing policy). In contrast, in 2005, the RBI defined underbanked districts as districts where the Average Population Per Branch Office (APPBO) was greater than the national average, and encouraged banks to open branches in underbanked areas. This presents a natural experiment that lends itself to a fuzzy regression discontinuity (RD) empirical design (Khanna and Mukherjee 2023, Kulkarni et al. 2023, Cramer 2022, Young 2021). Our empirical design also helps overcome several criticisms of the work of Burgess and Pande (2005) by excluding other licensing rule amendments (Buliskeria and Baxa, 2022) and focusing on district-level population-based rules (Kochar, 2011) (see also Panagariya 2006). We show that the 2005 RBI policy led to an increase in bank presence in treated districts from 2007-2011. By 2012, treated districts had an estimated 36 more bank branches relative to control districts.

As a first step, we study the impacts of bank presence on credit from the perspectives of banks and households. From the banks' perspective, underbanked (treated) districts had an estimated 62,173 more accounts, Rs. 4.72 billion larger credit limits, and Rs. 3.17 billion more in loan amounts outstanding. Focusing on credit for education, underbanked districts had an estimated 259 more accounts, Rs. 44.2 million higher credit limits, and Rs. 29.3 million (138%) more in loan amounts outstanding. The increase in bank credit for education is also supported by household survey data. Using household survey data, we show that households in treated districts were 1.5 percentage points more likely to have a bank loan for education, have 0.02 more education loans, and borrowed Rs. 1,100 more in education loans relative to households in non-underbanked districts.

Our main results document significant improvements in test scores, an important measure of human capital. Children aged 11-16 in underbanked districts scored 0.16 SD and 0.2 SD higher on reading and math, respectively. They were 4.9 percentage points more likely to read a paragraph or story and 9.6 percentage points more likely to perform subtraction or division. Children aged 6 - 10 in treated districts scored 0.22 SD higher on reading, and were 10 percentage points more likely to read words, a paragraph, or a story.

Financial access can improve human capital outcomes through a number of mechanisms, and we provide evidence in favor of three important channels. First, we show that the increase in credit, and specifically, loans for education, led to demand-side impacts in the form of increases in education inputs for children. We study two key education inputs relating to money and time – expenditures on education and children's time spent on homework. In our setting, substantial education inputs are typically required by parents. The most recent Annual Status of Education Report found that 30.5% of children in rural areas were taking some form of paid tuition classes outside of school (ASER, 2023). We estimate that households in treated districts spent an estimated Rs. 1,860 (89%) more on total education expenses. In particular, they spent Rs. 1,050 more on fees, Rs. 270 more on books, and Rs. 235 more on tuition fees. Children in treated districts also spent an estimated 8.77 more hours on homework per week.

Second, we show supply-side impacts in the form of improvements in the quantity and quality of schools and teachers. For context, there are substantial inefficiencies in the public delivery of education services in India. Kremer et al. (2005) show, using a nationally-representative dataset of primary schools in India, that 25% of teachers were absent on any given day, and that less than half of them were engaged in any teaching activity. We estimate that treated districts saw 0.145 (47%) more private schools per 1,000 people. Quality within private schools also improved: schools in treated districts were 17 percentage points more likely to have a boundary wall, have four more classrooms, and approximately 60 fewer students per functional toilet. We also document important improvements in the quality of teachers hired in treated schools. Treated schools had 28 percentage points more teachers with at least a graduate level of education.

Third, bank presence could also lead to increases in business loans that could provide households with more resources to invest in children's education (a labor market channel). We show that households in underbanked districts were 1.2 percentage points more likely to have loans for non-agricultural business purposes. They had 0.02 more non-agricultural business loans and borrowed Rs. 1,275 more for non-agricultural business purposes. These results are driven by borrowing for current expenditures rather than capital expenditures. Furthermore, individuals in underbanked districts were 14.9 percentage points less likely to be engaged in agriculture as their primary occupation. The decrease in agricultural employment is mirrored by an increase in manufacturing employment of similar magnitude. Individuals in treated districts were 10.4 and 9.5 percentage points more likely to be engaged in craft and related trades, as well as plant and machine operation and assembly, respectively.

We also observe important changes in the nature of self-employment. Individuals in treated districts were 5.1 percentage points less likely to be self-employed on their own account, i.e. without hiring any employees. Conversely, they were 1.8 percentage points more likely to be self-employed as an owner that employed at least one other worker. These results are in line with the results on increases in credit for non-agricultural current expenditures, which include salaries and wages.

While our RD design is well-validated in the literature (Khanna and Mukherjee 2023, Kulkarni et al. 2023, Cramer 2022, Young 2021), we additionally assess the validity of the RD design in four ways. First, we assess continuity in the density of the assignment variable at the cutoff and highlight that we do not observe any manipulation around the cutoff (Lee, 2008). We formally test this using the McCrary (2008) density test to study bunching around the cutoff of the running variable. Second, we test for pre-policy jumps in a set of outcome variables and covariates. We test for discontinuities in pre-policy household-level borrowing and savings, as well as district-level population, rates of literacy, and poverty and show that we do not find evidence of discontinuities for these variables around the cutoff. This is further confirmed by a permutation test that tests the continuity of the distribution of the covariates at the cutoff (Canay and Kamat, 2017). Third, we conduct a falsification check and assess discontinuities in our outcome variables of interest around placebo cutoffs. We do not find statistically significant results from placebo cutoffs that are smaller or larger than the true cutoff value. Fourth, we conduct a series of robustness checks to assess the sensitivity of our estimates along four key dimensions: (i) the size of the bandwidth, (ii) the type of bandwidth selector used, (iii) the kernel used, and (iv) the order of polynomial used. Overall, our estimates do not change significantly across these robustness checks.

Our paper makes three important contributions to the literature. First, we make a novel contribution to recent work that studies the impacts of financial access on human capital by focusing on the role of credit for education. Several studies in developed country or historical settings have studied human capital, financial access, and household debt (Brown et al. 2019, Chakrabarti et al. 2022, Célerier and Matray 2019, Stein and Yannelis 2020, Sun and Yannelis 2016). For example, Stein and Yannelis (2020) exploit the staggered rollout of bank branches of the Freedman's Savings Bank, which gave financial access to former slaves in the U.S. after the Civil War, and show that families with accounts were more likely to have children in school. However, bank access to freed former slaves in the Reconstruction-era U.S. (1865-1877) provides a setting distinct from modern day developing countries. Furthermore, while Stein and Yannelis (2020) do not observe account balances or credit access, we study credit and deposits from the perspectives of banks and households. Our results on increases in credit, rather than deposits, highlight an important mechanism through which bank expansion affects human capital outcomes.

Studies that focus on financial access and human capital in modern day developing country contexts are limited. As noted earlier, it is unclear whether financial access will lead to positive impacts on human capital in such settings. Chiapa et al. (2016) and Prina (2015) study the effects of access to savings accounts in Nepal and show positive impacts on education spending and schooling for girls. In contrast, our paper focuses on bank branch expansions, the impacts of which are driven primarily by increases in credit, rather than deposit, accounts (see Section 4.3). Such access to formal credit can be important in developing countries – for the median Indian household, shifting from non-institutional debt to institutional debt can lead to gains equivalent to 2-4% of annual income (Ramadorai and Committee, 2017).

Second, we contribute to broader work that studies the impacts of financial access on a range of economic outcomes in developing countries that may serve as potential mechanisms for the effects on human capital. Demirgüç-Kunt et al. (2018) review the recent literature on financial inclusion and note that financial access can help people smooth their consumption and manage financial risks. Studies from India, Mexico, and South Africa show that financial access led to improvements in labor market activity, entrepreneurship, income, and economic self-sufficiency (Banerjee et al. 2019, Bruhn and Love 2014, Karlan and Zinman 2009). However, Dupas et al. (2018) show that expanding access to basic bank accounts in Uganda, Malawi, and Chile did not lead to discernible effects on savings or any downstream outcomes. More recently, Fonseca and Matray (2022) show that higher financial development fosters firm creation and firm expansion in Brazil, while Breza and Kinnan (2021) also show that district-level reductions in credit supply in India were associated with significant reductions in business investment. In line with this recent work, we document shifts away from agricultural employment and towards employment in manufacturing, while self-employed individuals expanded their businesses.

Third, we make important contributions to prior work that assesses the effects of financial access in India. The landmark studies by Burgess and Pande (2005) and Burgess et al. (2005) that used an earlier RBI bank branch expansion policy from 1977-1990 to show that financial access significantly reduced rural poverty. Fulford (2013) uses the increased credit access resulting from the same policy and finds that rural areas in which branches per capita increased saw increased consumption and reduced poverty initially but lower consumption and higher poverty later. More recently, Agarwal et al. (2017) study the Pradhan Mantri Jan Dhan Yojana, a financial inclusion scheme launched by the Indian government in 2014 that led to 255 million new bank account openings. They show that the program increased borrowing and spending for health related reasons. Somville and Vandewalle (2023) study the impacts of randomized access to bank accounts through an RCT in Chhattisgarh, India, and find that their intervention improves consumption smoothing by alleviating savings constraints.

Several studies also use the branch authorization policy of the RBI that was announced in 2005 for identification. Cramer (2022) studies the impact of bank presence on health and shows positive impacts, including lower rates of illness, morbidity, and pregnancy-associated risks, and higher rates of vaccination. Kulkarni et al. (2023) and Young (2021) study the impact of the banking expansion on manufacturing establishments and economic growth and document positive impacts on local GDP growth. Khanna and Mukherjee (2023) utilize the policy to identify the impacts of demonetization's economic severity at the bank-expansion cutoff. Our paper differs from these studies by focusing on human capital, and in particular, credit for education and the demand- and supply-side responses relating to education inputs.

2 Context & Policy Reform

The Reserve Bank of India (RBI) has considerable control over the opening of new bank branches in India. Under Section 23 of the Banking Regulation Act (1949), banks cannot, without the prior approval of the RBI, open a new bank branch, or change location of an existing branch, unless within the same city, town, or village. The Act aimed to extend credit facilities to rural areas and develop a banking habit among individuals in rural areas (Reserve Bank of India, 2009). Over time, several policy reforms have been introduced to further this goal, including the branch expansion policy introduced in 1977 (later discontinued in 1990) and studied by Burgess and Pande (2005).[1]

We exploit a new branch authorization policy introduced by the RBI in September 2005 that encouraged banks to open branches in underbanked areas. Specifically, the policy noted that the "the RBI will, while considering applications for opening branches give weightage to the nature and scope of banking facilities provided by banks to common persons, particularly in *underbanked* areas (districts)." Policy documents detailed the list of underbanked districts and this list was forwarded to banks.[2] In our analysis, we exploit the RBI definition of underbanked districts: these are districts where the Average Population Per Branch Office (APPBO) is more than the national average.[3] To implement the policy in practice, the RBI replaced the existing system of granting authorizations for opening individual branches from time to time with a system of giving aggregated approvals, on an annual basis. Banks had to submit an Annual Branch Expansion Plan (ABEP) to the RBI, clearly specifying how many new branches the bank proposed to open in underbanked and non-underbanked districts. The policy had bite through the ABEP – as noted by Young (2021), the reform effectively created a quota-like system that required banks to expand in underbanked districts in order to receive licenses for entry in rich

[1]As Panagariya (2006) notes, there have been several earlier RBI branch licensing reforms, including a reform that required Indian commercial banks to observe a ratio of 2:1 between banked and unbanked areas beginning in July 1962.

[2]Please refer to the Master Circular on Branch Authorisation (Reserve Bank of India, 2005) and Report of the Group to review the extant Branch Authorisation Policy (Reserve Bank of India, 2009) for more details.

[3]District population totals were based on the 2001 Census. The initial list of underbanked districts released by the RBI in September 2005 was superseded by a revised list in July 2006. We use the July 2006 list for our analysis.

markets.

The policy had a large effect on the number of authorizations for new bank branches. Relative to the rate of 62 authorizations granted to banks for every 100 authorizations sought in 2005 (prior to introduction of the revised policy), the rate increased to 68% in 2006, 87% in 2007, and 91% in 2008, respectively, in the three years after the introduction of the revised policy (Reserve Bank of India, 2009). In July 2008, the RBI revised the policy such that the proposals submitted by banks for opening of branches in underbanked districts would be considered, provided that the location of the proposed branch is not: (a) within the municipal limits of state capitals, metropolitan centers, or district headquarters and (b) within 100 kilometers from the four major metropolitan centers (Mumbai, New Delhi, Kolkata, and Chennai) and 50 kilometers from a state capital.[4] We further explore expansion in the number of new bank branches and total bank branches over time in Section 4.2.

In the next section, we describe the datasets that we use and present descriptive statistics for our key variables.

3 Data

The values of the Average Population Per Branch Office (APPBO) were not disclosed by the RBI at the district- or national-level. Our first step is to construct the population-to-branches ratio used to classify districts as underbanked. For the numerator of the ratio, we use district-level population from the 2001 Population Census. For the denominator of the ratio, we obtain district totals of bank branches from the Bank Branch Statistics available from the RBI.[5] The Bank Branch Statistics also provide a quarterly time series of new bank branch openings, which we use to assess whether the policy was effective in spurring new branch openings in underbanked districts.

[4]Exceptions to this revision were granted for the state of Jammu & Kashmir and the seven northeastern states: Arunachal Pradesh, Assam, Manipur, Meghalaya, Mizoram, Nagaland, and Tripura. Please refer to the Master Circular on Branch Authorisation (Reserve Bank of India, 2008) for more details.

[5]The Bank Branch Statistics data can be accessed at https://dbie.rbi.org.in/DBIE/dbie.rbi?site=publications#!17

We examine the effect of banking access on several categories of outcomes. To evaluate the impact on bank lending and deposits, we use the RBI's Basic Statistical Return (BSR) data. The BSR 1 and BSR 2 contain comprehensive data on the credit and deposits, respectively, of Scheduled Commercial Banks (SCBs). Using this data, we construct district-level totals of credit accounts, credit limit, amount of credit outstanding, deposit accounts, and deposits.

To assess how households respond to the increase in financial access, we use the survey on Debt and Investment from the 70th Round of the National Sample Survey (NSS). The survey was conducted January to December of 2013 on a nationally-representative sample covering all of India. It collects information from surveyed households on their assets, liabilities, capital expenditures, real estate holdings, and businesses. For each household, we can observe the value of their bank deposits (if any), the number of loans they have outstanding, the number of bank loans they have outstanding, and the original amount borrowed on their outstanding loans. The survey also classifies loans by loan purpose. Thus, we also look at the same borrowing measures only for loans taken out for education or business purposes. In later analysis, we also use data from the survey on Debt and Investment from the 59th Round of the NSS, conducted in 2003, for pre-policy placebo tests.

We use test score data from the Annual Status of Education Report (ASER) to analyze educational outcomes. ASER provides annual estimates of schooling status and basic learning levels of children in India. The survey assesses basic reading and arithmetic levels of children in their homes, as opposed to schools, thereby mitigating potential biases caused by selective enrollment in schools. The survey sample contains more than 600,000 children each year and is representative of children in rural districts.

Turning to mechanisms that could explain impacts on test scores, we construct data to explore how financial access affects education inputs. We obtain measures of household education inputs from the 2012 wave of the India Human Development Survey (IHDS). Specifically, we use information on education expenditures and time use for children. Expenditures are divided into categories based on purpose, such as school fees, books, and tuition. Time use comprises time spent in school, doing homework, or with a tutor. Households also report the number of days the child has been absent from school in the past 30 days. On the supply side of education inputs, we use data on primary and secondary schools across India from the Unified District Information System for Education (UDISE) available from the National Data and Analytics Platform (NDAP). The data is a 2018 snapshot of schools and their characteristics. We use the data to construct district-level totals of public and private schools in 2018. We also construct and examine various measures of school and teacher quality, including teacher type and educational qualifications, enrollment, classrooms, and toilets.

We also explore labor market outcomes as potential mechanisms using NSS Rounds 66 (2010) and 68 (2012). The nationally-representative survey collects data on the usual principal activity of individual household members. For each individual, we determine their unemployment status, self-employment status, and occupation types based on their principal activity. We also use NSS Round 68 to identify cases of child labor based on the principal activity of children aged 5-17. We follow Bau et al. (2020) and identify instances of child labor as those where the child's primary activity was any form of wage/salary labor, work with or without pay at a home enterprise, or domestic chores.

Finally, we use 2005 district-level literacy and poverty rates as controls throughout our analysis. These rates are constructed using the NSS Round 61 (2005) socioeconomic survey. The nationally-representative survey contains questions on literacy and monthly household consumption. We use state-level urban- and rural-specific poverty thresholds published by the RBI to determine whether a household is in poverty and then aggregate households within the same district to calculate district poverty rates.

Table A1 and Table 1 present the sources and summary statistics, respectively, for our key variables of interest. Panel A of Table 1 presents statistics at the district level, while Panel B presents them at the household or individual level. Panel C presents statistics at the school level. From the perspective of banks, districts had, on average, 59,910 loan accounts and Rs. 7.4 billion in outstanding loans, based on BSR data from 2010 - 2015. On average, in 2018 districts had 1.6 schools per 1,000 people, based on UDISE data. From the perspective of households, 59% of household had a deposit account, and 8% of households had a bank loan and borrowed Rs. 4,710 from banks, on average. Households spent Rs. 1,900 annually on education expenses, most of which was spent on school fees, books, and tuition. Children spent, on average, 32 hours in school per week, and 8.5 hours on homework. They were absent, on average, for 4 days in the past 30 days and 7% were engaged in child labor. On a scale of 0 - 4, students aged 6-10 scored 1.8 and 2.0 on math and reading, respectively, based on data from ASER (2011 - 2012). Older students aged 11-16 scored 3.1 and 3.4 on math and reading, respectively. In terms of schools in 2018, 88% of schools had a boundary wall or one under construction. On average, schools enrolled 456 students with 43 students per classroom available for instruction. Toilet availability was similar for boys and girls, both with 117 students per toilet. The average school had 10 teachers, and 83% of teachers at the average school had at least a graduate education (university degree), while 43% had a Bachelor or Master of Education.

4 Empirical Strategy

4.1 Regression Discontinuity Design

The RBI policy introduced in 2005 is a natural experiment that lends itself to a fuzzy regression discontinuity empirical design. Districts with a ratio of population to bank branches exceeding the national average were designated by the RBI as underbanked (treated), while districts with a ratio below the national average were defined as non-underbanked (control). Thus, the policy induced a cutoff at the value of the national average, with the ratio of population to bank branches serving as the running variable. While the value of the national Average Population Per Branch Office (APPBO) ratio used by the RBI was not disclosed in policy documents, our calculations yield a national average of 14,828.

Since households, banks, and districts had no control over the assignment variable, every district near the cutoff had approximately the same probability of having a ratio of population to bank branches that was just above or just below the cutoff – similar to a coin-flip experiment (Lee and Lemieux, 2010). Thus, while there have been policies to improve financial access and education in India over the years (examples include the Pradhan Mantri Jan Dhan Yojana and Pradhan Mantri Mudra Yojana schemes of 2014-2015 that opened bank accounts and extended credit to individuals, as well as the Right to Education Act of 2010), the RD design only compares the outcomes of banks and households just above and below the cutoff, thereby differencing out the impacts of other policies. In this sense, the RD works as a local randomized experiment (Lee and Lemieux, 2010). To the best of our knowledge, no other policy was implemented using the same rule and national Average Population Per Branch Office (APPBO) ratio.

Figure 1a presents a district map of underbanked and non-underbanked districts as per our classification of districts using the computed cutoff value. We observe significant spatial variation in the location of underbanked districts across the country. However, a potential concern with RD empirical designs is the use of observations only within the optimal bandwidth around the cutoff. To address this concern, we present a district map of underbanked and non-underbanked districts within the largest bandwidth used across our outcome variables (4,000 on either side of the cutoff) in Figure 1b. We observe significant spatial variation in the estimation sample across the country, thereby strengthening the external validity of our results.

Figure 2 presents a plot of the probability of being assigned an underbanked status against the running variable. We see a sharp jump in the probability of being underbanked when crossing the national average ratio; however, this jump in probability is not equal to one. This could, in part, be due to differences in the national average ratio used by the RBI and us. Furthermore, we do not have access to the specific district ratios used by the RBI to classify the districts. Given the imperfect prediction, we implement a fuzzy RD empirical strategy.

As Hahn et al. (2001) note, a key identifying assumption for the RD is that of continuity, i.e. "all other factors" determining the outcome must be evolving "smoothly" with respect to the running variable. We conduct two sets of analyses to assess the validity of the RD design. First, we assess continuity in the density of the assignment variable at the cutoff (Lee, 2008). Figure 3a plots a histogram of the ratio of population to bank branches for each district. The national average ratio is depicted by the vertical line at 14,828. Visually, we do not observe any manipulation around the cutoff. This is not surprising given that the two components of the assignment variable, number of bank branches in the district and district population, cannot be changed easily; changes to the number of operating branches is subject to approval from the RBI and the district population used to calculate the ratio was likely already determined before the policy was announced. In addition, any changes in the number of operating branches would also result in a change to the cutoff value (i.e., the national average). We also formally test this using the McCrary (2008) density test to study bunching around the cutoff of the running variable.[6] Figure 3b presents the plot assessing manipulation. This figure shows that the distribution of the ratio of population to bank branches is smooth around the threshold. Overall, we fail to reject the null hypothesis of continuity (the p-value corresponding to this test is 0.512).

Second, we assess whether covariates that may affect our outcomes of interest change discontinuously at the cutoff by testing for jumps in our set of covariates. We visually test for discontinuities in pre-policy district-level population (2001 Census), literacy rates, and poverty rates in Figures A1a, A1b, and A1c, respectively. We do not find evidence of discontinuities for these variables around the cutoff of the running variable. Additionally, we implement a permutation test that tests the continuity of the distribution of the covariates at the cutoff, as described in Canay and Kamat (2017). Table A2 reports the p-values for continuity of each of the three covariates individually, as well as the joint test for the continuity of the three-dimensional vector of covariates. The p-values indicate that we are not able to reject the null hypothesis of continuity of the conditional distributions of the covariates at the cutoff. This is not surprising, given that the RBI algorithm assigned underbanked status based on the national average ratio of population to bank branches. We present additional placebo tests and robustness checks that assess the internal validity of our RD empirical design in Section 7. These tests confirm that our RD design is consistent with the literature that establishes the validity of this identification strategy (Khanna and Mukherjee 2023, Kulkarni et al. 2023, Cramer 2022, Young 2021).

Given the validity of the fuzzy RD empirical strategy, we estimate the following reduced form equation for household h in district d of state s in year t:

$$Y_{hdst} = \beta_0 + \beta_1 AboveCutoff_d + f(DistrictRatio_d - Cutoff) + \beta_2 X_d + \gamma_{st} + \varepsilon_{hdst} \quad (1)$$

where Y_{hdst} denotes the outcome variable of interest and $AboveCutoff_d$ is an indicator equal to 1 if district d's ratio of population to branches exceeds the national average ratio of

[6]This is implemented using the "rddensity" program in Stata. The program implements manipulation testing procedures using the local polynomial density estimators proposed in Cattaneo et al. (2020), and implements graphical procedures with valid confidence bands using the results in Cattaneo et al. (2023).

14,828 (i.e., the cutoff). $f(DistrictRatio_d - Cutoff)$ is a polynomial in the difference between district d's ratio and the national average ratio. Following Gelman and Imbens (2019), we use linear functions within the optimal bandwidth. X_d is a set of district-level controls, including population and its square, as well as pre-policy literacy and poverty rates (2005). γ_{st} denotes state by year fixed effects, thereby allowing for a comparison across households in different districts within the same state and year. β_1 is the coefficient of interest that estimates the size of the discontinuity. In our fuzzy RD design, this represents the Local Average Treatment Effect (LATE) for the set of compliers.

We report fuzzy RD estimates implemented using the "rdrobust" Stata program by Calonico et al. (2017) using the defaults of a triangular kernel and an MSE-optimal bandwidth selector. The triangular kernel ensures that greater weight is placed on observations closer to the cut-off, where observations most closely approximate the benchmark of random assignment to treatment. Standard errors are clustered by district.

4.2 Bank Branch Expansion

As a first step, we assess whether the RBI policy led to an increase in bank presence in treated districts. Figure 4 presents treatment impacts on the number of new bank branches and the total number of bank branches from 2007 - 2015 using Bank Branch Statistics data from the RBI. Each estimate presents results from an RDD estimation for the given year, as discussed in Section 4.1.

As noted by Young (2021), profit-maximizing incentives should drive bank branch expansion in response to the policy. Thus, we expect private banks to respond more to the policy than public banks, which have other non profit-maximizing motives. Figure 4a shows that treated districts saw a greater number of new private bank branches from 2007 - 2010. Since RBI authorizations for branch expansions were valid for a duration of one year from communication of approval (Reserve Bank of India, 2005), our results are consistent with banks seeking approvals in 2006 and opening more bank branches starting in 2007. Each year, underbanked (treated) districts had an estimated four more new bank branches relative to non-underbanked (control) districts. Figure 4b confirms the prediction that public bank expansion should have little, if any, response to the policy, as we do not observe statistically significant differences in the number of new public bank branches for treated districts.

The net effects are reflected in the total number of bank branches. Figure 4c shows statistically significant increases in the total number of bank branches in treated districts from 2009 - 2012. These results are consistent with an appropriate lag for the new branches built to be reflected in the branch totals for each district. By 2012, treated districts had an estimated 36 more bank branches relative to an average of 261 branches in control districts. The lack of an increase in the number of bank branches beyond 2012 may be, in part, due to the revisions later introduced by the RBI that disallowed the authorization of bank branches in specific areas of underbanked districts (see Section 2).

Overall, the estimates and dynamics show that the policy had a clear effect on banking presence in treated districts.

4.3 Credit & Deposits

Next, we study the impact of bank presence on credit and deposits from the perspectives of banks and households. Section 4.3.1 presents district-level results on bank credit and deposits, while section 4.3.2 presents household-level results on bank borrowing and savings. In each of these sections, we study total credit as well as credit for education purposes.

4.3.1 District-level Bank Credit & Deposits

The Basic Statistical Returns of Scheduled Commercial Banks (BSR) system of the RBI allows us to study the number of accounts, credit limit, and amount of loans outstanding at the district level. Table 2 presents results using BSR data and the RDD estimation discussed in Section 4.1. We study the number of accounts, credit limit, and amount of loans outstanding in columns (1), (2), and (3), respectively. Underbanked districts had an estimated 62,173 more accounts ($p < 0.05$), Rs. 4.72 billion larger credit limit ($p < 0.1$), and Rs. 3.17 billion larger loan amounts outstanding ($p < 0.1$). The policy generated large increases in overall lending relative to control districts: for example, underbanked districts had an outstanding loan value that was 87% larger in comparison with control districts.

These discontinuities are also seen visually in Figure A2. The RD plots use the Average Population Per Branch Office (APPBO) as the running variable, centered at the national average (i.e., the cutoff). Districts with a ratio above the national average are underbanked and hence treated. Figures A2a, A2c, and A2e present RD plots for the number of accounts, credit limit, and amount outstanding on loans, respectively. Visually, we observe discontinuities for the three outcomes.

The rich BSR data allows us to focus on the number of accounts, credit limit, and loan amount outstanding specific to personal loans for education. Columns (4), (5), and (6) of Table 2 present the results for the three variables, respectively. Focusing on personal loans for education, underbanked districts had an estimated 259 more accounts ($p = 0.129$), Rs. 44.2 million higher credit limit ($p < 0.05$), and Rs. 29.3 million (138%) greater loan amounts outstanding ($p < 0.1$). We also present RD plots for these three outcomes in Figures A2b, A2d, and A2f, respectively. We observe visual discontinuities for the credit limit and loan amount outstanding specific to personal loans for education.

The BSR data also allows us to study savings from the perspective of banks. We study the number of deposit accounts and value of deposits in columns (7) and (8) of Table 2, respectively. Underbanked districts have an estimated 185,000 (115%) more savings accounts ($p = 0.126$) and Rs. 4.12 (101%) billion greater deposits ($p = 0.145$). However, we interpret these estimates as weak evidence of increases in deposits given the marginal statistical significance of these results.

4.3.2 Household-level Bank Borrowing & Savings

The estimated increase in bank credit at the district level is also supported by household-level survey data. Using Round 70 of the National Sample Survey (2013), we study the extensive and intensive margins of household borrowing. Specifically, we assess whether households had any bank loan, the number of bank loans, and the amount borrowed for bank loans. These results are presented in columns (1), (2), and (3) of Table 3, respectively. Households in treated (underbanked) districts were 10.5 percentage points more likely to have a bank loan ($p < 0.05$), have 0.17 more loans ($p < 0.05$), and borrowed Rs. 6,860 more in loans relative to households in control (non-underbanked) districts ($p < 0.1$). The corresponding RD plots also confirm these discontinuities visually in Figures 5a, 5c, and 5e.

Importantly, the NSS data allows us to assess whether households had any bank loan, the number of bank loans, and the amount outstanding for bank loans specific to education only. Columns (4), (5), and (6) of Table 3 present the results for these variables, respectively. Households in treated (underbanked) districts were 1.5 percentage points more likely to have a bank loan for education ($p < 0.01$), have 0.02 more education loans ($p < 0.05$), and borrowed Rs. 1,100 more in education loans relative to households in control (non-underbanked) districts ($p < 0.05$). The corresponding RD plots also confirm these discontinuities visually in Figures 5b, 5d, and 5f.

The NSS data also allows us to study savings from the perspectives of households. We assess the extensive and intensive margins of bank savings in columns (7) and (8) of Table 3, respectively. Households in underbanked districts were 10.5 percentage points more likely to have bank deposits and had Rs. 11,500 more in bank deposits relative to households in control districts. These results, however, are not statistically significant at conventional levels.

Overall, the district- and household-level data show strong evidence of increases in credit – specifically, credit for education – and weak evidence of increases in deposits, as a result of the increase in bank presence. These results are consistent with prior work that has highlighted the severity of credit constraints for individuals in developing countries (Banerjee and Duflo 2010, Banerjee and Duflo 2007). They are also consistent with theories linking distance, competition, and lending in other contexts (Kärnä et al. 2021, Beck et al. 2007, Degryse and Ongena 2005).

5 Main Results on Human Capital Outcomes

What are the impacts of increases in bank presence and the resulting improvements in credit for education purposes on test scores, an important measure of human capital? We use the Annual Status of Education Report (ASER) data from 2011 - 2012 to study these impacts. We separately study children of younger (6 - 10 years) and older (11 - 16 years) age groups, given recent work that has highlighted the growing differences in educational achievement of young children by age in India (Muralidharan et al. 2019, Singh 2019).

We present our main results on test scores in Table 4. Panels A and B present results for older and younger cohorts, respectively. For each age group, we assess their reading and math scores (in SD units), as well as their ability to display skills appropriate for their age or better. For example, when assessing children aged 11-16, we study their ability to read a paragraph or story, as well as their ability to perform subtraction or division. For children aged 6-10, we study their ability to read words, a paragraph, or story, as well as their ability to recognize two-digit numbers, perform subtraction, or division.

Panel A highlights significant treatment effects on test scores for children aged 11-16. Children in underbanked (treated) districts scored 0.16 SD and 0.2 SD higher on reading and math, respectively ($p < 0.1$). They were 4.9 percentage points more likely to read a paragraph or story ($p < 0.1$) and 9.6 percentage points more likely to perform subtraction or division ($p < 0.05$). Panel B highlights statistically significant impacts on test scores for children aged 6 - 10. Children in treated districts scored 0.22 SD higher on reading ($p < 0.1$), and were 10 percentage points more likely to read words, a paragraph or story ($p < 0.1$). However, the results on math for children in this age group are not statistically significant. The visual differences for reading and math scores for children of younger (6 - 10 years) and older (11 - 16 years) age groups are shown in Figure 6. In general, we observe visual discontinuities for these variables.

The estimated impacts are sizeable, considering the magnitudes from randomized evaluations of education interventions in India and other developing countries. For example, Muralidharan et al. (2019) estimate 0.37 SD impacts on math and 0.23 SD impacts on language scores in response to a personalized technology-aided, after-school instruction program in India. Banerjee et al. (2007) estimate 0.28 SD impacts on average test scores from a remedial education program that hired young women to teach students in India. In a review of 35 studies in low- and middle-income countries, Baird et al. (2013) find that conditional and unconditional cash transfer programs have at best a small effect on learning outcomes. In comparison, our estimated impacts on test scores, ranging from 0.16 - 0.22 SD, highlight that bank presence can have impacts of a similar magnitude to intensive education programs in developing countries.

6 Mechanisms

In this section, we study three potential mechanisms underlying our results on human capital outcomes. First, the increase in credit for education purposes may lead to increases in education inputs for children. We study this in Section 6.1 and refer to this channel as a demand-side channel, since it operates through parents and households. The estimated demand-side impacts are the aggregate effects in equilibrium, given any potential crowding-out (Das et al., 2013) or crowding-in (Attanasio et al., 2020) of household's education inputs in response to potential supply-side impacts. Second, the increase in bank presence may lead to increases in the quantity and quality of schools and teachers. We study this in Section 6.2 and refer to this as a supply-side channel, since it operates through schools and teachers (similarly, these are the aggregate effects in equilibrium, given household responses). Third, increased bank presence could also lead to increases in business loans that could provide households with more resources to invest in children's education, and we refer to this as a labor market channel. We first present results on non-agricultural business loans in Section 6.3, followed by results on occupational shifts away from agriculture and towards self-employment in Section 6.4.

We stress that there may be other channels through which the RBI policy affected education, and we do not seek to provide an exhaustive list of potential channels. For example, the literature highlights impacts of the RBI policy on health (Cramer, 2022). Better child health and nutrition in early ages has been linked with higher enrollment, lower absenteeism, increases in time in school, and higher test scores in developing countries (Glewwe and Miguel 2007, Alderman et al. 2001, Behrman 1996). We do not focus on such channels in this paper, given prior work establishing these mechanisms. Instead, we aim to understand whether there is any evidence in favor of the three mechanisms proposed.

6.1 Demand-side Mechanisms: Education Inputs

First, we assess whether the increase in credit (specifically, credit for education purposes) led to demand-side impacts in the form of increases in education inputs for children.

In Table 5, we study two key education inputs relating to money and time – expenditures on education and children's time spent on homework. Column (1) presents impacts on total educational expenditures, while columns (2) - (5) present impacts on specific education categories relating to school fees, books, bus, and tuition in the past one year, respectively. Households in treated districts spent an estimated Rs. 1,860 (89%) more on total education expenses relative to households in control districts ($p < 0.05$). In particular, they spent Rs. 1,050 more on school fees ($p < 0.05$), Rs. 270 more on books ($p < 0.1$) and Rs. 235 more on tuition fees ($p < 0.1$). The discontinuity in total education expenses is also shown visually in Figure A3a.

Columns (6) - (10) of Table 5 present results for time use by children. While children in treated districts spent more time in school and with a tutor, these differences are not statistically significant. Importantly, however, we observe large and statistically significant increases in their time spent on homework. Column (7) highlights that children in treated districts spent an estimated 8.77 more hours on homework per week relative to children in the control group ($p < 0.01$). We also observe a clear visual discontinuity for the time spent on homework in Figure A3b. This result is significant, given earlier work highlighting the link between time spent on homework and academic achievement (Cooper et al., 2006). Children in treated districts were not more or less likely to be absent from school or engaged in child labor. We do not find evidence in support of the hypothesis that financial access may lead to more children working in agriculture or business, and reduce schooling (Bau et al. 2020, Hossain 2023).

Overall, our results are in line with literature that stresses the relationship between demand-side inputs to education and education outcomes (Leibowitz, 1977).

6.2 Supply-side Mechanisms: Quantity & Quality of Schools & Teachers

Next, we study whether the increase in bank presence led to supply-side impacts in the form of increases in the quantity and quality of schools and teachers in treated districts. We use data on schools from the Unified District Information System for Education (UDISE, 2018) in India to study these supply-side impacts.

6.2.1 Schools

We study the impact of the policy on the number of schools per capita in Table 6. While we do not observe increases in the overall number of schools, we estimate that treated districts had 0.145 (47%) more private schools per 1,000 people relative to control districts ($p < 0.1$). We also observe a visual discontinuity in the number of private schools per capita in Figure A4a. Thus, overall, the increase in bank presence led to supply-side impacts in the form of increases in the number of private schools per capita. These supply-side impacts on private schools are important, given prior work that has highlighted the higher value-added of private schools relative to public schools in India (Muralidharan and Sundararaman 2015, Singh 2015). Given the lack of results for public schools, we focus on private schools for the remainder of the analysis on supply-side impacts.

In addition to increases in the number of private schools per capita, we study whether the policy led to improvements in quality within schools. Table 7 presents impacts of the RBI policy on physical infrastructure within schools, an important metric of school quality. We start by studying boundary walls in column (1). A lack of boundary walls has been shown to invite thieves, stray animals, and even garbage on school premises (The Tribune, 2019). Thus, boundary walls are important to provide a safe environment for students and teachers conducive to learning. We estimate that treated schools were 17 percentage points more likely to have a boundary wall ($p < 0.1$).

Next, we study classrooms, the core physical infrastructure of schools. India's Right to Education Act (2009) stipulates that schools should not have more than 40 students per classroom (The Times of India, 2019). In columns (2) and (3) of Table 7, we study impacts on the number of classrooms and the number of students per classroom, respectively. We find that treated schools had an estimated four more classrooms ($p = 0.114$). However, there were no changes to the number of students per classroom (see Figure A4b).

Toilets are also seen as part of the critical physical infrastructure of schools. In particular, due to privacy and safety concerns, access to girls' toilets is strongly linked with the enrollment of girls in schools in India (Adukia, 2017). Despite this focus on toilets, the ratio of students to toilets remains high in India. While the National School Sanitation Manual recommends one toilet for every 80 students (The Hindu, 2012), we see that schools in non-underbanked districts had more than 110 students per functional toilet (Table 7, columns (4) - (7)). We document that the RBI policy led to improvements in the ratio of students to functional toilets: treated schools had 60 fewer boys per functional boys' toilet ($p < 0.1$) and 63 fewer girls per functional girls' toilet ($p < 0.05$). These visual differences are also shown in Figures A4c and A4d.

6.2.2 Teachers

In this section, we study the impacts of the policy on the quantity and quality of teachers within private schools. Under the Right to Education Act 2009, the stipulated pupil-teacher ratio for primary classes and upper primary classes is 30:1 and 35:1, respectively (Press Information Bureau, Government of India, 2017). Columns (1) and (2) of Table 8 study the two components of this ratio, enrollment and the number of teachers, respectively. While schools in treated districts saw 96 more students enrolled and four more teachers hired, these differences are not statistically significant at conventional levels. Consequently, we do not find statistically significant impacts on the ratio of students to teachers.

Muralidharan and Sundararaman (2015) highlight that private schools in India typically hire teachers who are less educated, and much less likely to have professional teaching credentials. Thus, we study teachers' highest educational qualification in columns (3) and (4), and their highest professional qualification in columns (5) - (7) of Table 8. We show that treated districts had 27 percentage points fewer teachers with a below graduate level (university degree) of education ($p < 0.01$) and 28 percentage points more teachers with at least a graduate level of education ($p < 0.01$). Furthermore, treated schools had 36 percentage points fewer teachers with a diploma as their highest qualification ($p < 0.05$), 8 percentage points fewer teachers with a Bachelor of Elementary Education ($p < 0.05$), and 20 percentage points more teachers with Bachelor or Master of Education degrees ($p = 0.11$). The visual differences on teacher quality are also highlighted in Figures A4e and A4f.

Overall, our results on test scores and supply-side improvements in school and teacher quality align with work demonstrating higher reading and math test scores for students in private schools relative to public schools in India (Singh 2014, Tabarrok 2013). More broadly, the results are in line with work by Eble and Escueta (2022), who highlight important complementarities between demand- and supply-side education inputs for learning outcomes in developing countries.

6.3 Non-Agricultural Business Loans

Section 4.3 highlighted an increase in overall bank lending and, in particular, bank loans for education in treated districts. In this section, we study the impacts of the RBI policy on non-agricultural business lending by banks using household-level survey data.

Table 9 presents our results on business loans from banks. Columns (1) - (3) present results for agricultural loans, while columns (4) - (6) present results for non-agricultural business loans. The likelihood of having a business loan, the number of business loans, and amount borrowed for business purposes are presented in Panels A, B, and C, respectively. The rich NSS data also allows us to distinguish between borrowing for capital versus current expenditures.

We observe consistently lower borrowing for agricultural purposes in treated districts in columns (1) - (3) of Panels A, B, and C. However, these results are not statistically significant at conventional levels. On the other hand, we observe statistically significant impacts for non-agricultural business loans. Panel A highlights that households in underbanked (treated) districts were 0.5 and 0.6 percentage points more likely to have loans for non-agricultural capital and current expenditures, respectively ($p < 0.05$). Overall, these households were 1.2 percentage points more likely to have loans for non-agricultural business purposes ($p < 0.01$).

Panels B and C highlight that households in treated districts had 0.02 more non-agricultural business loans ($p < 0.01$) and borrowed Rs. 1,275 more for non-agricultural business purposes relative to households in control districts ($p = 0.104$). These results are driven by borrowing for current expenditures rather than capital expenditures. Households in treated districts had 0.01 more non-agricultural business loans for current expenditures ($p < 0.05$) and borrowed Rs. 592 more for non-agricultural current expenditures ($p < 0.1$).

The discontinuities corresponding to the likelihood of having a business loan, the number of business loans, and the amount borrowed for business purposes are shown visually through RD plots in Figure A5. Visually, we observe discontinuities for all three variables. These results are in line with findings by Banerjee and Duflo (2014), who document that firms in India are severely credit constrained.

Taken together, the results highlight a shift away from agricultural loans and towards non-agricultural business loans, particularly for current expenditures.

6.4 Labor Market Impacts: Occupational Shifts & Self-Employment

Did the shifts away from agricultural loans and towards non-agricultural business loans lead to shifts away from agriculture and towards non-agricultural business and self-employment? In this section, we study labor market impacts of the RBI policy in the form of occupational shifts, followed by impacts on employment and self-employment.

Table 10 presents the results on labor market occupational shifts. Column (6) shows that individuals in underbanked (treated) districts were 14.9 percentage points less likely to be engaged in agriculture as their primary occupation ($p < 0.1$). This represents a significant decline relative to a control mean of 26.1%. The decrease in agricultural employment is mirrored by an increase in manufacturing employment of similar magnitude. Columns (7) and (8) highlight that individuals in treated districts were 10.4 and 9.5 percentage points more likely to be engaged in craft and related trades, as well as plant and machine operation and assembly, respectively ($p < 0.05$).

Table 11 presents results on employment and self-employment. We do not find evidence that individuals in treated districts were less likely to be unemployed. However, we observe important changes in the nature of self-employment. Column (2) shows that individuals in treated districts were 5.1 percentage points less likely to be self-employed on their own account, i.e. without hiring any employees ($p < 0.1$). Conversely, column (3) highlights that individuals in treated districts were 1.8 percentage points more likely to be self-employed as an owner that employed at least one other worker ($p < 0.05$). These results are in line with the results on increases in credit for non-agricultural current expenditures, which include salaries and wages.

Overall, the results show that increased bank presence led to important labor market impacts in the form of shifts from agricultural employment to employment in manufacturing. Self-employed individuals also saw a shift from own account businesses to an expansion of their business with at least one employee hired. These are important mechanisms that likely increased resources for households in treated districts, leading to impacts on test scores.

7 Placebo Tests & Robustness Checks

As noted earlier, our RD design is well-validated in the literature (Khanna and Mukherjee 2023, Kulkarni et al. 2023, Cramer 2022, Young 2021). We additionally assess the validity of the RD design in several ways. To further the analysis in Section 4 that utilized density tests and placebo outcomes, we present, in this section, additional placebo tests and robustness checks to strengthen the internal validity of our estimates.

First, we conduct a falsification check and assess discontinuities in our outcome variables of interest around placebo cutoffs. We use two placebo cutoffs: one that is smaller than the true cutoff (90% of its value) and another that is larger (110% of its value). These results are shown in columns (2) and (3) of Table 12, respectively. Overall, we do not find statistically significant results from these placebo cutoffs for any of the 25 outcome variables considered. This falsification check supports our empirical strategy and highlights that the impacts we estimate are due to the specific cutoff from the RBI branch authorization policy (i.e. the National Average Population Per Branch Office, or National APPBO).

Second, we run placebo tests on pre-policy outcome data and show that our outcomes are smooth around the cutoff. We present results on borrowing for household expenses, borrowing for business, and savings in Tables A3 and A4. Using data from 2002-2003, we show that pre-policy measures of borrowing and savings on the extensive and intensive margins are not statistically distinguishable around the cutoff. Furthermore, we see no differences for borrowing for household expenses (columns 4-6 of Table A3) and agricultural and non-agricultural business loans (Table A4). This provides evidence against the potential concern that our estimates may reflect pre-policy differences across areas.

Third, we run placebo tests using regional rural banks, which were not subject to the new branch authorization policy. Figure A6a shows that, with the exception of new bank branches in 2006, there were no statistically significant differences in new regional rural bank branch openings in treated districts. In addition, Figure A6b illustrates that treated and control districts did not display significant differences in the total number of regional rural banks.

Fourth, we conduct a series of robustness checks to assess the sensitivity of our estimates along four key dimensions: (i) the size of the bandwidth, (ii) the type of bandwidth selector, (iii) the kernel, and (iv) the order of polynomial. These results are shown in Table 13, where results from our baseline specification are shown in column (1).

Columns (2) and (3) use bandwidths that are 25% smaller or larger relative to the bandwidth used in our baseline regression for the corresponding dependent variable. Intuitively, choosing a very small bandwidth around the cutoff will tend to reduce the misspecification error in the approximation. A very small bandwidth, however, reduces the sample, leading to estimators with larger variance. Our baseline specification uses the widely-used Mean Squared Error (MSE) bandwidth selector that optimally trades off the bias and variance of the RD point estimator. Overall, our results are largely robust to the size of the bandwidth used. In column (4), we use a Coverage Error Probability (CER) bandwidth selector that minimizes the coverage probability. We show that our results are also largely robust to the bandwidth selector used.

Column (5) of Table 13 presents results with a uniform kernel, as opposed to the triangular kernel used in our baseline specification that places more weight on observations closer to the cutoff. With the exception of our results on test scores, our estimates are largely robust to the choice of kernel used. Lastly, we present results estimated with a quadratic polynomial in column (6), as opposed to the linear polynomial used in our baseline specification. Similarly, with the exception of our results on test scores, our estimates are largely robust to the order of polynomial used.

Overall, the placebo tests and robustness checks presented in this section, taken together with the density tests and assessment of placebo outcomes in Section 4, highlight that our estimates can be interpreted as robust, unbiased, and causal estimates of the RBI policy that we study.

8 Conclusion

This paper provides new evidence on the impact of bank branch expansion and the resulting improvements in credit for education purposes on human capital outcomes in a developing country setting. We utilize a new branch authorization policy introduced by the RBI in 2005 that encouraged banks to open branches in underbanked districts, where the Average Population Per Branch Office (APPBO) was more than the national average. This natural experiment lends itself to a regression discontinuity empirical design. The policy led to an increase in bank presence in treated districts and a significant increase in bank lending and household borrowing for education. Households in underbanked districts were significantly more likely to have a bank loan and borrowed more in loan value.

Our main results highlight improvements in test scores, an important measure of human capital. Children in underbanked districts scored significantly higher on reading and math tests. We provide evidence in favor of three mechanisms that can explain the improvements in test scores. On the demand side, households in treated districts spent more on their children's education and children spent more time on homework. On the supply side, we document improvements in the quantity and quality of schools and teachers. We also show labor market impacts induced by increases in non-agricultural business loans. We document shifts away from agricultural employment and towards employment in manufacturing, while self-employed individuals expanded their businesses.

Our work has important implications for financial inclusion and education policies worldwide. For example, improvements in financial access could help households and schools leverage India's ambitious new National Education Policy 2020 to boost human capital outcomes in the country (Government of India, 2020). Strengthening financial inclusion could also help countries recover from the substantial learning losses that arose early in the Covid-19 pandemic and persisted over time (Betthäuser et al., 2023). We leave the study of these topics for future research.

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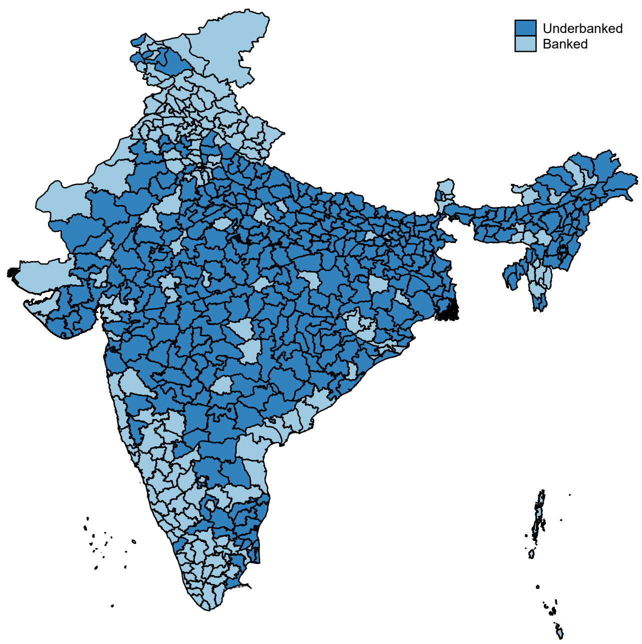
Figures and Tables

Figure 1: Maps of Underbanked Districts

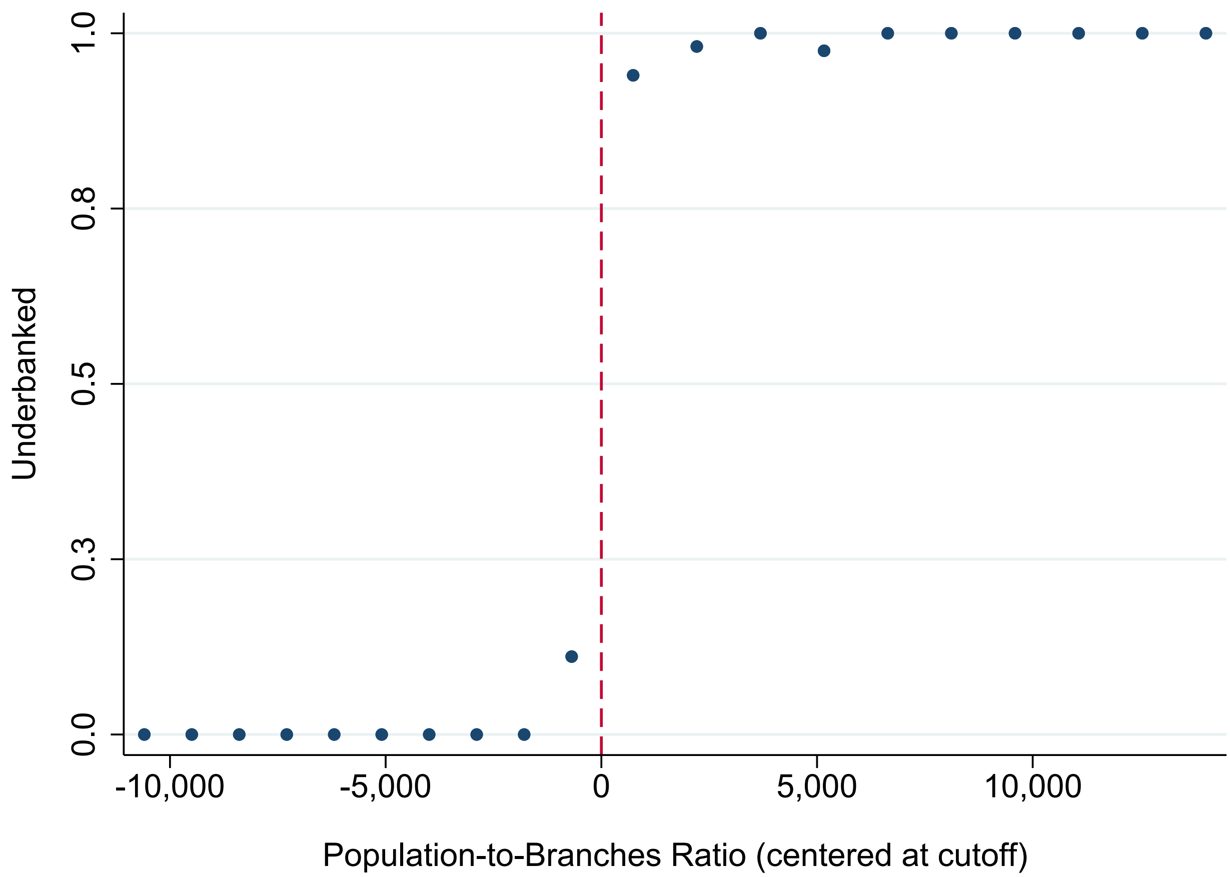
(a) All Districts

(b) Districts Within Bandwidth



Notes: These figures show district maps of India based on the 2001 Census boundaries. Districts with an Average Population Per Branch Office (APPBO) greater than the national average are classified as "Underbanked", while districts with an APPBO lower than the national average are classified as "Banked". The classification is based on the authors' computation of APPBO values. Figure 1a presents a map of underbanked and banked districts across India, while Figure 1b presents a similar map for districts within the largest bandwidth used across our outcome variables (4,000 on either side of the cutoff).

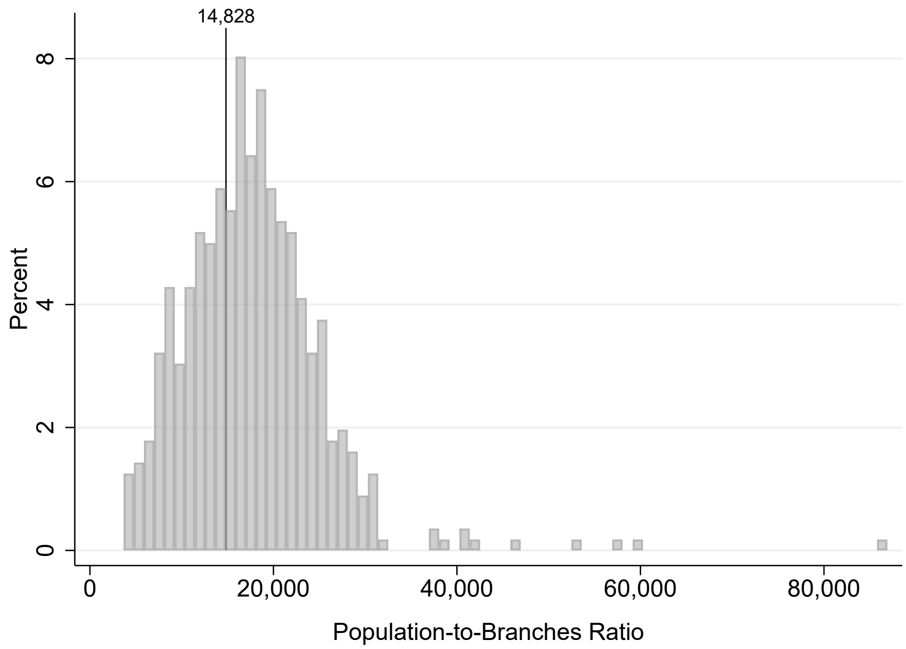
Figure 2: First Stage Plot of RBI Underbanked Status Against APPBO



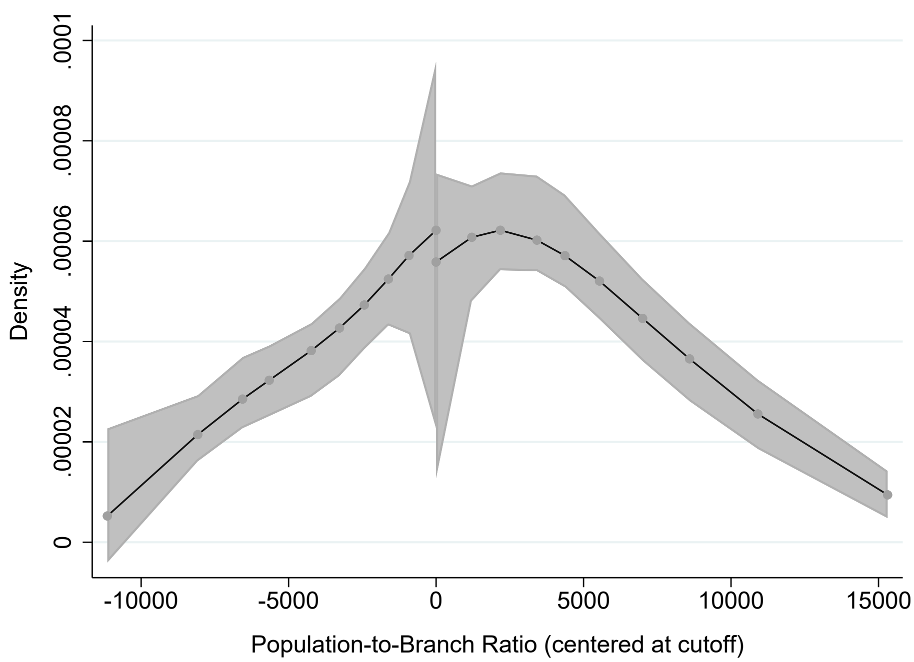
Notes: This figure presents a plot of "Underbanked" status (RBI classification) against the running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO). The running variable has been centered at the cutoff value of 14,828 and the cutoff is indicated by the dashed vertical line. 10 bins were used on either side of the cutoff and each dot plots the proportion of underbanked districts within the corresponding bin.

Figure 3: Assessing Continuity of the Running Variable (APPBO)

(a) Histogram



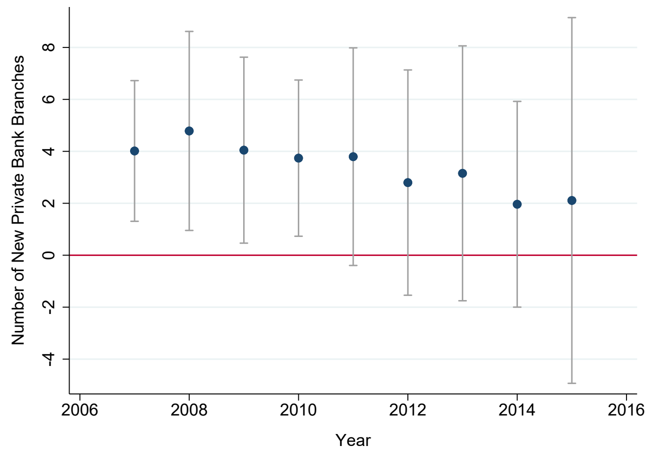
(b) Manipulation Test



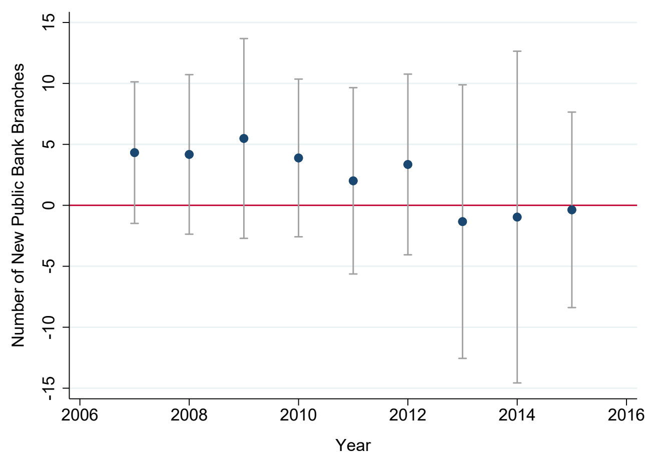
Notes: These figures assess continuity of the running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO). Figure 3a plots a histogram of the population-to-branches ratio. The vertical line at 14,828 indicates the national average of this ratio (i.e., the cutoff value). Figure 3b presents a density plot of the running variable to assess manipulation around the cutoff value (Cattaneo et al., 2023; McCrary, 2008). 95% confidence intervals are shown.

Figure 4: Post-Policy Bank Branch Expansion

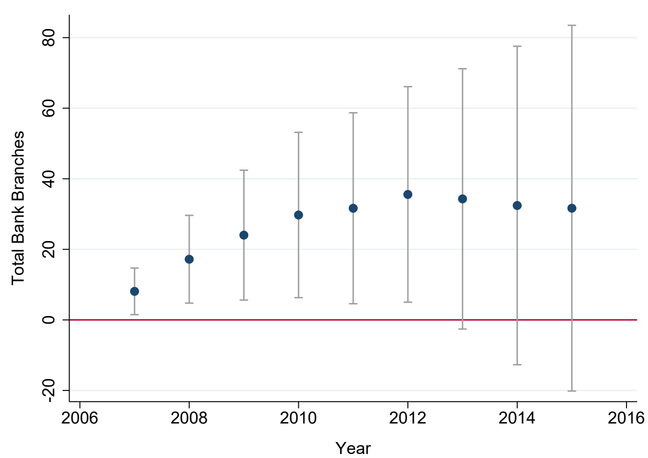
(a) New District Private Bank Branches



(b) New District Public Bank Branches



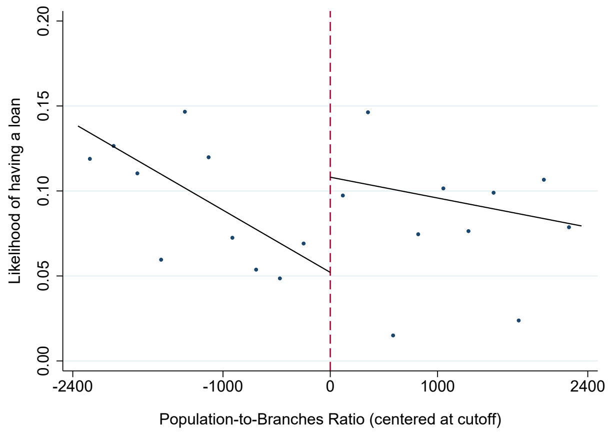
(c) Total District Bank Branches



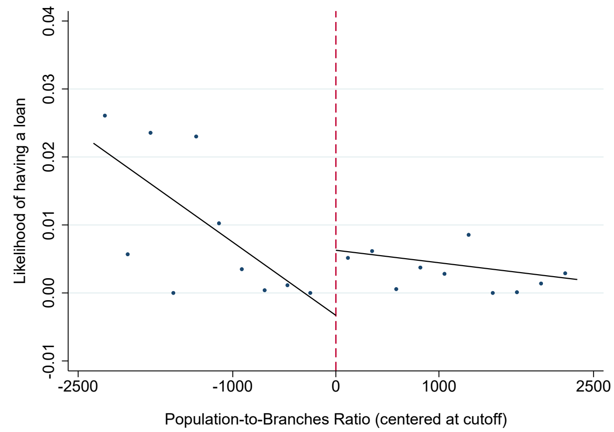
Notes: These figures plot estimates of β_1 from the following equation: $Y_{dst} = \beta_0 + \beta_1 AboveCutoff_d + f(DistrictRatio_d - Cutoff) + \beta_2 Y_{ds,2006} + \varepsilon_{dst}$. The regressions are estimated using the Bank Branch Statistics data from the RBI at the district-level, separately by year (2007 - 2015). Figures 4a and 4b plot the number of new private and public bank branches, respectively, while Figure 4c plots the total number of bank branches. 95% confidence intervals are shown.

Figure 5: Household Borrowing

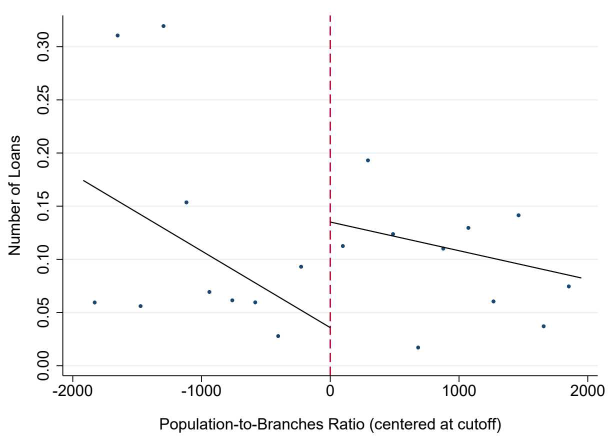
(a) Has a Bank Loan



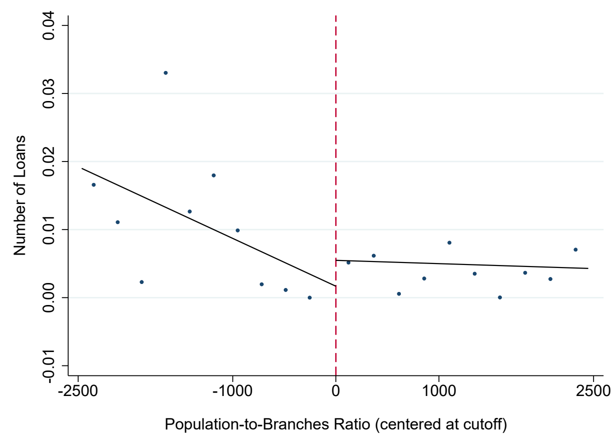
(b) Has a Bank Loan for Education



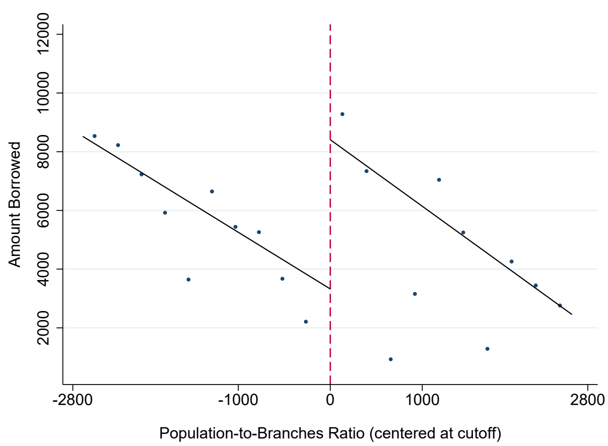
(c) Number of Bank Loans



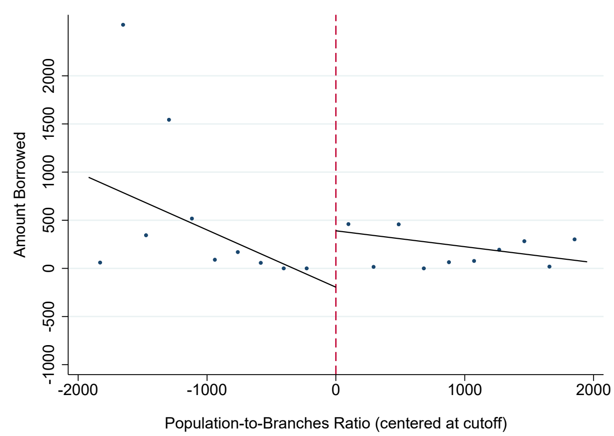
(d) Number of Bank Loans for Education



(e) Amount Borrowed (Bank Loans)



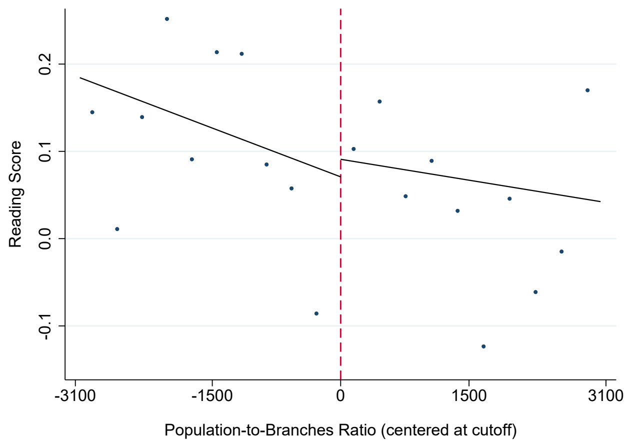
(f) Amount Borrowed (Bank Loans for Education)



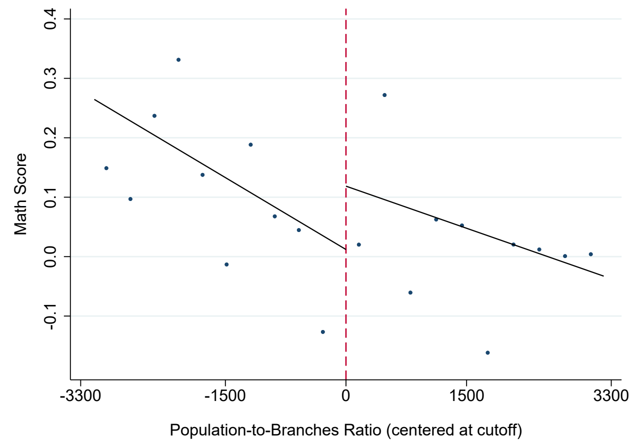
Notes: Each figure presents a binned scatter plot and linear fit for household borrowing variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. Household-level data from NSS Round 70 (2013) is used. NSS survey weights are used.

Figure 6: Main Results on Test Scores

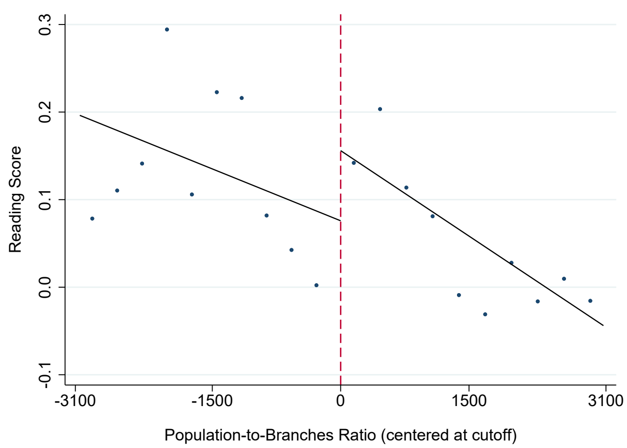
(a) Reading Score (Ages 11-16)



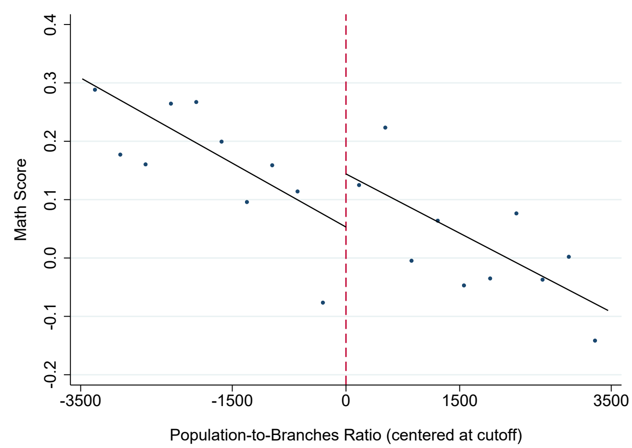
(b) Math Score (Ages 11-16)



(c) Reading Score (Ages 6-10)



(d) Math Score (Ages 6-10)



Notes: Each figure presents a binned scatter plot and linear fit for test score variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. Child-level data from ASER (2011-2012) is used. ASER survey weights are used.

Table 1: Summary Statistics

Panel A: District-Level Variables	Mean	SD	Obs.
Underbanked	0.64	0.48	560
Poverty Rate	0.34	0.17	555
Literacy Rate	0.60	0.16	555
Number of Loan Accounts (Thousands)	59.91	506.38	3,360
Credit Limit (Rs Crore)	1,198.44	8,432.31	3,360
Amount Outstanding (Rs Crore)	744.94	4,855.05	3,360
Number of Deposit Accounts (Thousands)	200.88	956.56	3,360
Deposits (Rs Crore)	948.81	7,057.66	3,360
Number of Schools (per 1,000 people)	1.62	0.95	560
Panel B: Household-Level Variables	Mean	SD	Obs.
Has a Loan	0.08	0.27	107,179
Number of Loans	0.09	0.35	107,179
Amount Borrowed (Rs)	4,710.93	46,404.41	107,179
Has Deposits	0.59	0.49	107,178
Value of Deposits (Rs)	13,747.42	108,851.89	107,179
Education Expenditures: Total (Rs)	1,896.79	4,707.15	41,108
Education Expenditures: School Fees (Rs)	902.47	3,913.60	42,191
Education Expenditures: Books (Rs)	545.35	789.42	42,469
Education Expenditures: Bus (Rs)	178.70	619.58	41,897
Education Expenditures: Tuition (Rs)	279.82	1,060.06	41,678
Weekly Hours in School	32.10	9.18	40,859
Weekly Hours on HW	8.47	6.43	40,530
Weekly Hours in Tutoring	2.53	5.31	38,308
Days Absent in Last 30 Days	4.21	5.25	41,975
Child Labor (%)	0.07	0.25	112,524
Reading Test Score (Ages 6-10)	1.98	1.40	422,989
Reading Test Score (Ages 11-16)	3.40	1.08	439,066
Math Test Score (Ages 6-10)	1.79	1.19	420,976
Math Test Score (Ages 11-16)	3.06	1.10	437,999
Panel C: School-Level Variables	Mean	SD	Obs.
Has Boundary Wall	0.88	0.33	309,559
Students per Classroom	43.05	55.33	305,291
Boys per Toilet	117.08	151.79	283,143
Girls per Toilet	116.58	153.07	284,575
Enrollment	456.06	699.83	309,215
Number of Teachers	10.18	12.24	309,559
Teachers with at Least Graduate Education (%)	82.57	27.35	304,742
Teachers with Bachelor or Master of Education (%)	42.55	36.56	304,742

Notes: Panel A presents summary statistics for district-level variables. "Underbanked" uses the RBI classification of districts into underbanked status. Poverty and literacy rates are obtained from NSS Round 61 (2005). Loan and deposit data are obtained from the BSR (2010 - 2015) and are at the district-year level. The number of schools per capita is obtained from NDAP UDISE (2018). Panel B presents summary statistics for household- and individual-level variables. Loan and deposit data are obtained from NSS Round 70 (2013). Education expenses and time use are obtained from IHDS II (2011-2012). Child labor is obtained from NSS Round 68 (2011-2012). Test scores are obtained from ASER (2011-2012). Panel C presents summary statistics for school-level variables from NDAP UDISE (2018).

Table 2: Bank Credit & Deposits

	All Lending			Lending for Education			Deposits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Accounts (Thousands)	Credit Limit (Rs Crore)	Amount Outstanding (Rs Crore)	Number of Accounts	Credit Limit (Rs Crore)	Amount Outstanding (Rs Crore)	Number of Accounts (Thousands)	Deposits (Rs Crore)
Underbanked	62.2**	472*	317*	259	4.42**	2.93*	185	412
	(30)	(261)	(187)	(171)	(2.24)	(1.53)	(121)	(282)
State x Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Control Districts	38.7	506	363	313	2.98	2.13	161	407
First Stage	0.616	0.618	0.623	0.646	0.623	0.623	0.630	0.616
Bandwidth	1,666	1,732	1,828	2,232	1,830	1,839	1,967	1,665
Observations	3,330	3,330	3,330	3,330	3,330	3,330	3,330	3,330
Effective Obs.	576	588	642	816	642	642	684	576

Notes: Regressions are estimated at the district level using data on private sector banks from the BSR (2010-2015). "Lending for education" refers to personal loans taken out for education. All regressions include state by year fixed effects and control for district-level population and its square, literacy rates, and poverty rates. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Household Borrowing & Savings

	All Borrowing			Borrowing for Education			Deposits	
	(1) Has a Loan	(2) Number of Loans	(3) Amount Borrowed	(4) Has a Loan	(5) Number of Loans	(6) Amount Borrowed	(7) Has Deposits	(8) Value of Deposits
Underbanked	.105**	.172**	6,858*	.0152***	.0154**	1,097**	.105	11,501
	(.0419)	(.0683)	(4,005)	(.00583)	(.00621)	(433)	(.13)	(8,832)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.0979	.112	6,138	.0102	.0112	436	.602	9,632
First Stage	0.747	0.710	0.774	0.748	0.754	0.711	0.760	0.701
Bandwidth	2,392	1,973	2,745	2,407	2,476	1,993	2,551	1,863
Observations	106,835	106,835	106,835	106,835	106,835	106,835	106,834	106,835
Effective Obs.	28,677	21,453	33,714	28,677	30,214	21,453	31,522	20,740

Notes: Regressions are estimated at the household level using NSS Round 70 (2013). Loans in columns (1) - (6) refer to loans outstanding between June 30, 2012 and the survey date, with the exception of long-term loans that may have been taken out before the policy. Deposits in columns (7) - (8) refer to deposits as of June 30, 2012. NSS borrowing and deposit information pertains to commercial banks including regional rural banks. All regressions include state fixed effects, a rural indicator, and control for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Main Results on Test Scores

Panel A: Ages 11-16	Reading		Math	
	(1)	(2)	(3)	(4)
	Score	Read Paragraph	Score	Subtraction or Division
Underbanked	.157*	.0488*	.199*	.0964**
	(.0866)	(.0283)	(.108)	(.0467)
State x Year FE	✓	✓	✓	✓
Mean of Control Districts	.129	.878	.14	.784
First Stage	0.767	0.772	0.776	0.761
Bandwidth	3,061	3,145	3,221	2,946
Observations	435,615	435,615	434,567	434,567
Effective Obs.	143,280	148,320	149,166	134,554
Panel B: Ages 6-10	Reading		Math	
	(1)	(2)	(3)	(4)
	Score	Read Words	Score	Two-Digit Numbers
Underbanked	.219*	.1*	.152	.0716
	(.12)	(.0572)	(.133)	(.0676)
State x Year FE	✓	✓	✓	✓
Mean of Control Districts	.138	.654	.181	.68
First Stage	0.758	0.763	0.776	0.779
Bandwidth	3,079	3,173	3,480	3,533
Observations	419,916	419,916	417,926	417,926
Effective Obs.	131,625	134,801	144,822	146,621

Notes: Regressions are estimated at the child level using ASER data from 2011 - 2012. Panel A presents regressions for children aged 11 - 16 while Panel B presents regressions for children aged 6-10. Reading and math scores in columns (1) and (3) are in SD units. "Read paragraph" in column (2) of Panel A is an indicator equal to one if the child was able to read a paragraph or story. "Subtraction or division" in column (4) of Panel A is an indicator equal to one if the child was able to perform subtraction or division. "Read words" in column (2) of Panel B is an indicator equal to one if the child was able to read words, a paragraph, or story. "Two-digit numbers" in column (4) of Panel B is an indicator equal to one if the child was able to recognize two-digit numbers, perform subtraction, or division. All regressions include state by year fixed effects, controls for age and gender, as well as controls for district-level population and its square, literacy rates, and poverty rates. ASER survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Demand-side Mechanisms: Education Inputs

	Expenditures					Time Use				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Fees	Books	Bus	Tuition	School	HW	Tutor	Days Absent	Child Labor
Underbanked	1,862**	1,052**	270*	99.1	235*	1.53	8.77***	.32	.214	.0102
	(755)	(514)	(158)	(108)	(134)	(3.69)	(3.39)	(1.2)	(2.37)	(.0249)
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE						✓	✓	✓	✓	✓
Mean of Control Districts	2,081	994	589	242	268	32.1	7.99	2.2	3.94	.0666
First Stage	0.540	0.550	0.573	0.564	0.550	0.644	0.548	0.557	0.559	0.737
Bandwidth	1,543	1,600	1,931	1,848	1,678	2,823	1,690	2,272	1,952	2,783
Observations	40,997	42,080	42,358	41,786	41,567	40,606	40,277	38,058	41,710	111,876
Effective Obs.	8,027	8,334	9,479	9,082	8,376	14,249	8,282	10,372	9,506	32,961

Notes: Regressions in columns (1) - (9) are estimated at the child level using IHDS II (2011-2012). The sample in these columns is restricted to children aged 18 and below. The regression in column (10) is estimated at the child level using NSS Round 68 (2011-2012), with the sample restricted to children aged 5-17. Education expenditures in columns (1) - (5) refer to expenses in the past one year from the survey date. Time use variables in columns (6) - (8) refer to the mean number of hours spent per week over the past one month. The dependent variable in column (9) refers to the number of days absent in the past 30 days. The dependent variable in column (10) is an indicator variable equal to 1 if the child's primary activity was any form of wage/salary labor, work with or without pay at a home enterprise, or domestic chores. All regressions include state fixed effects, a rural indicator, controls for age and gender, as well as controls for district-level population and its square, literacy rates, and poverty rates. Columns (6) - (10) additionally include month fixed effects to account for potential seasonality in the time use data. IHDS survey weights are used for regressions in columns (1) - (9) and NSS survey weights are used for the regression in column (10). All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Supply-side Mechanisms: School Quantity

	(1) Total	(2) Private	(3) Public
Underbanked	-.0125	.145*	-.15
	(.312)	(.0827)	(.271)
State FE	✓	✓	✓
Mean of Control Districts	1.52	.311	1.22
First Stage	0.706	0.725	0.705
Bandwidth	3,160	3,612	3,141
Observations	555	555	555
Effective Obs.	194	217	194

Notes: Regressions are estimated at the district level using NDAP UDISE (2018) data. Dependent variables in columns (1) - (3) denote number of schools per 1,000 people. All regressions include state by year fixed effects, as well as controls for district-level population and its square, literacy rates, and poverty rates. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Supply-side Mechanisms: School Quality

		Classrooms		Functional Boys' Toilets		Functional Girls' Toilets	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Has Boundary Wall	Number	Students per Classroom	Number	Boys per Toilet	Number	Girls per Toilet
Underbanked	0.17*	3.97	-19.32	1.58	-60.09*	2.04	-63.28**
	(0.10)	(2.51)	(17.33)	(1.02)	(31.62)	(1.30)	(31.71)
State FE	✓	✓	✓	✓	✓	✓	✓
Mean of Control Districts	0.88	10.67	49.45	3.42	115.63	3.64	113.73
First Stage	0.722	0.721	0.720	0.723	0.721	0.725	0.714
Bandwidth	1,844	1,854	1,744	1,821	1,730	1,782	1,749
Observations	308,907	308,907	304,641	308,907	282,516	308,907	283,968
Effective Obs.	64,168	64,201	59,393	63,391	54,965	62,070	55,589

Notes: Regressions are estimated at the school level using NDAP UDISE (2018) data. The dependent variable in column (1) is a dummy variable equal to 1 if the school has a boundary wall, and 0 otherwise. The dependent variables in columns (2), (4), and (6) denote the number of classrooms, functional boys' toilets, and functional girls' toilets, respectively. The dependent variables in columns (3), (5), and (7) denote ratios of the number of students per classroom, number of boys per toilet, and number of girls per toilet, respectively. All regressions include state by year fixed effects, as well as controls for district-level population and its square, literacy rates, and poverty rates. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Supply-side Mechanisms: Teacher Quantity & Quality

	(1) Enrollment	(2) Number of Teachers	Highest Education (3) % Below Graduate	Highest Education (4) % At Least Graduate	Highest Professional Qualification (5) % Diploma	Highest Professional Qualification (6) % Bachelor of Elementary Education	Highest Professional Qualification (7) % Bachelor or Master of Education
Underbanked	95.69	3.94	-26.85***	28.35***	-35.73**	-8.00**	19.64
	(116.31)	(3.04)	(9.96)	(10.21)	(17.53)	(3.97)	(12.30)
State FE	✓	✓	✓	✓	✓	✓	✓
Mean of Control Districts	511.78	10.21	18.70	81.81	17.74	5.65	42.66
First Stage	0.724	0.723	0.709	0.713	0.709	0.709	0.707
Bandwidth	1,808	1,828	1,874	1,785	1,881	1,862	1,936
Observations	308,563	308,907	304,091	304,091	304,091	304,091	304,091
Effective Obs.	63,391	64,168	63,666	60,721	63,666	62,456	65,064

Notes: Regressions are estimated at the school level using NDAP UDISE (2018) data. Dependent variables in columns (1) and (2) denote the number of students enrolled and the number of teachers, respectively. The dependent variables in columns (3) - (7) classify teachers by educational qualifications and are expressed in percentage points. All regressions include state by year fixed effects, as well as controls for district-level population and its square, literacy rates, and poverty rates. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Labor Market Mechanisms: Business Loans

	Agricultural			Non-Agricultural		
	(1) Capital Expenditures	(2) Current Expenditures	(3) Total	(4) Capital Expenditures	(5) Current Expenditures	(6) Total
Panel A: Has a Loan						
Underbanked	-.00616	-.00734	-.0109	.00514**	.00643**	.0121***
	(.00545)	(.0189)	(.0194)	(.00251)	(.00257)	(.00406)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.00841	.0297	.0375	.00406	.00294	.00693
First Stage	0.826	0.814	0.822	0.743	0.724	0.733
Bandwidth	4,048	3,622	3,907	2,336	2,133	2,224
Observations	106,835	106,835	106,835	106,835	106,835	106,835
Effective Obs.	48,070	44,157	46,368	28,201	25,258	26,649
Panel B: Number of Loans						
Underbanked	-.00556	-.00339	-.0074	.00132	.00888**	.0167***
	(.00553)	(.0212)	(.0219)	(.00246)	(.00413)	(.00598)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.00903	.0327	.0443	.00458	.00267	.00797
First Stage	0.825	0.806	0.813	0.792	0.686	0.715
Bandwidth	4,005	3,413	3,617	3,064	1,675	2,046
Observations	106,835	106,835	106,835	106,835	106,835	106,835
Effective Obs.	47,314	41,229	44,157	37,909	18,548	23,580
Panel C: Amount Borrowed						
Underbanked	-320	-1260	-1582	950	592*	1275
	(278)	(2091)	(2135)	(765)	(332)	(783)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	454	1,846	2,213	291	211	564
First Stage	0.779	0.794	0.783	0.729	0.786	0.775
Bandwidth	2,824	3,093	2,901	2,183	2,949	2,765
Observations	106,835	106,835	106,835	106,835	106,835	106,835
Effective Obs.	34,901	38,133	35,841	26,537	35,953	34,217

Notes: Regressions are estimated at the household level using NSS Round 70 (2013). Loans refer to loans outstanding between June 30, 2012 and the survey date, with the exception of long-term loans that may have been taken out before the policy. NSS borrowing information pertains to commercial banks, including regional rural banks. All regressions include state fixed effects, a rural indicator, and control for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Labor Market Mechanisms: Occupational Shifts

	NCO Division								
	1	2	3	4	5	6	7	8	9
Underbanked	-.0126	.00436	-.00637	.00119	-.00255	-.149*	.104**	.0952**	-.0158
	(.0316)	(.0176)	(.0135)	(.00968)	(.0207)	(.0785)	(.0484)	(.0458)	(.0586)
State x Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean of control districts	0.068	0.031	0.032	0.023	0.077	0.261	0.122	0.047	0.335
First Stage	0.719	0.755	0.705	0.693	0.779	0.722	0.695	0.668	0.707
Bandwidth	2,141	2,675	1,924	1,708	3,111	2,183	1,734	1,384	1,964
Observations	297,778	297,778	297,778	297,778	297,778	297,778	297,778	297,778	297,778
Effective Obs.	73,777	93,695	61,405	54,660	111,897	75,583	55,548	46,527	62,360

Notes: Regressions are estimated at the adult level using NSS Rounds 66 (2010) and 68 (2012). The sample is restricted to adults aged 18 and above. The dependent variable in column i is an indicator variable equal to 1 if the respondent reported their primary occupation to be in National Classification of Occupations (NCO) Division i. The NCO (2004) one-digit division codes are as follows: 1 - Legislators, Senior Officials, and Managers, 2 - Professionals, 3 - Associate Professionals, 4 - Clerks, 5 - Service Workers and Shop & Market Sales Workers, 6 - Skilled Agricultural and Fishery Workers, 7 - Craft and Related Trades Workers, 8 - Plant and Machine Operators and Assemblers, 9 - Elementary Occupations. All regressions include state by year fixed effects, a rural indicator, controls for age and gender, as well as controls for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Labor Market Mechanisms: Unemployment & Self-employment

	Unemployed	Self-Employed	
	(1)	(2)	(3)
	Yes/No	Own Account	Owner
Underbanked	.01	-.051*	.0182**
	(.0177)	(.0281)	(.00794)
State x Year FE	✓	✓	✓
Mean of control districts	0.018	0.182	0.007
First Stage	0.678	0.727	0.697
Bandwidth	1,573	2,267	1,929
Observations	309,205	547,121	547,121
Effective Obs.	53,720	139,947	108,449

Notes: Regressions are estimated at the adult level using NSS Rounds 66 (2010) and 68 (2012). The sample is restricted to adults aged 18 and above. All dependent variables are indicator variables. "Unemployed" in column (1) is an indicator equal to one if the individual did not work but was seeking and/or available for work, and zero otherwise. Self-employed on own account in column (2) is an indicator equal to one if the individual was self-employed on their own without any employees, and zero otherwise. Self-employed as owner in column (3) is an indicator equal to one if the individual was self-employed and employed at least one worker, and zero otherwise. All regressions include state by year fixed effects, a rural indicator, controls for age and gender, as well as controls for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Placebo Tests

		Cutoff	
	(1) Baseline	(2) .9x	(3) 1.1x
Total Lending			
Number of Loan Accounts (Thousands)	62.2**	-1975	43.8
	(30)	(12334)	(56.5)
Credit Limit (Rs Crore)	472*	-4333	388
	(261)	(12620)	(1011)
Amount Outstanding (Rs Crore)	317*	-1403	267
	(187)	(2440)	(677)
Lending for Education			
Number of Loan Accounts for Education	259	-1186	762
	(171)	(1651)	(1108)
Credit Limit for Education Loans (Rs Crore)	4.42**	-13.3	6.32
	(2.24)	(14.6)	(6.99)
Amount Outstanding for Education Loans (Rs Crore)	2.93*	-10.1	4.48
	(1.53)	(10.7)	(4.89)
Household Borrowing from Banks			
Has a Loan	.105**	-.106	.291
	(.0419)	(.438)	(.243)
Number of Loans	.172**	.00621	.399
	(.0683)	(.376)	(.313)
Amount Borrowed	6858*	22.8	13654
	(4005)	(20836)	(8744)
Has a Loan for Education	.0152***	.0278	.02
	(.00583)	(.0578)	(.0168)
Number of Loans for Education	.0154**	.0192	.0165
	(.00621)	(.0667)	(.0191)
Amount Borrowed for Education	1097**	1151	-123
	(433)	(2750)	(864)
Has a Loan for Non-Agricultural Business	.0121***	-.0226	.0229
	(.00406)	(.0359)	(.0169)
Number of Loans for Non-Agricultural Business	.0167***	-.0269	.0214
	(.00598)	(.0564)	(.0168)
Amount Borrowed for Non-Agricultural Business	1275	-1068	-1001
	(783)	(3564)	(3077)
Supply-Side Education Inputs			
Number of Private Schools (per 1,000 people)	.145*	.726	2.66
	(.0827)	(.611)	(7.05)
Number of Students per Classroom	-19.3	-673	60.3
	(17.3)	(667)	(134)
% Teachers with at Least a Graduate Degree	28.3***	256	11.9
	(10.2)	(1470)	(99.5)
% Teachers with Bachelor or Master of Education	19.6	108	-82.2
	(12.3)	(253)	(78)
Number of Boys per Toilet	-60.1*	-1242	198
	(31.6)	(1087)	(605)
Number of Girls per Toilet	-63.3**	-1119	83.1
	(31.7)	(1011)	(403)
Test Scores			
Reading Score (Ages 11-16)	.157*	-.0604	-.00181
	(.0866)	(.453)	(.569)
Math Score (Ages 11-16)	.199*	.26	-37.3
	(.108)	(.551)	(91.7)
Reading Score (Ages 6-10)	.219*	.225	-2.87
	(.12)	(.423)	(4.3)
Math Score (Ages 6-10)	.152	.739	-4.77
	(.133)	(1.11)	(10.4)

Notes: Column (1) uses 14,828 for the value of the cutoff, while column (2) uses 13,345 and column (3) uses 16,311. Lending regressions are estimated at the district level using data on private sector banks from the BSR (2010-2015). Household borrowing regressions are estimated at the household level using NSS Round 70 (2013). Supply-side education input regressions are estimated at the school level using NDAP UDISE (2018) data. Test score regressions are estimated at the child level using ASER data from 2011 - 2012. All regressions include state by year fixed effects and control for district-level population and its square, literacy rates, and poverty rates. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Robustness Checks

		Bandwidth			Kernel	Polynomial Order
	(1) Baseline	(2) .75x	(3) 1.25x	(4) CER	(5) Uniform	(6) 2nd-order
Total Lending						
Number of Loan Accounts (Thousands)	62.2**	60.1*	49.9**	60.6*	70.3**	69.9*
	(30)	(35.4)	(24.4)	(36.4)	(33)	(38.9)
Credit Limit (Rs Crore)	472*	555*	349	591*	347	436
	(261)	(302)	(247)	(316)	(350)	(314)
Amount Outstanding (Rs Crore)	317*	382*	217	397*	269	300
	(187)	(221)	(171)	(226)	(227)	(232)
Lending for Education						
Number of Loan Accounts for Education	259	469*	196	477*	131	294
	(171)	(241)	(168)	(243)	(193)	(220)
Credit Limit for Education Loans (Rs Crore)	4.42**	5.26*	2.9	5.24*	4.42*	2.45
	(2.24)	(2.81)	(1.88)	(2.82)	(2.47)	(2.68)
Amount Outstanding for Education Loans (Rs Crore)	2.93*	3.54*	1.89	3.52*	2.16	1.56
	(1.53)	(1.92)	(1.3)	(1.92)	(1.57)	(1.87)
Household Borrowing from Banks						
Has a Loan	.105**	.135**	.0741**	.137**	.129**	.0695*
	(.0419)	(.0591)	(.036)	(.0608)	(.0596)	(.0368)
Number of Loans	.172**	.185**	.147***	.184**	.194**	.127**
	(.0683)	(.085)	(.0516)	(.0855)	(.0816)	(.0502)
Amount Borrowed	6858*	8625*	6001*	8721*	6995	8754**
	(4005)	(5100)	(3393)	(5159)	(4280)	(4261)
Has a Loan for Education	.0152***	.0203***	.00861*	.02**	.0195**	.0238**
	(.00583)	(.00771)	(.00454)	(.0078)	(.00877)	(.00958)
Number of Loans for Education	.0154**	.0226**	.00816	.0229**	.0306**	.0266**
	(.00621)	(.00881)	(.00514)	(.00895)	(.0147)	(.0111)
Amount Borrowed for Education	1097**	1223**	758**	1229**	1623**	1146**
	(433)	(513)	(321)	(513)	(813)	(473)
Has a Loan for Non-Agricultural Business	.0121***	.016**	.00935***	.0161***	.0132***	.00944**
	(.00406)	(.00623)	(.00355)	(.00623)	(.00447)	(.00387)
Number of Loans for Non-Agricultural Business	.0167***	.0193**	.0134***	.0193**	.0189**	.0121**
	(.00598)	(.00774)	(.00472)	(.00767)	(.0088)	(.00543)
Amount Borrowed for Non-Agricultural Business	1275	1620	1201*	1694*	1323	1391*
	(783)	(994)	(663)	(1009)	(866)	(794)
Education Expenditures and Time Use						
Expenditures on Education	1862**	1532**	1472**	1525**	797	1271*
	(755)	(681)	(573)	(669)	(624)	(670)
Children's Time Spent on Homework	8.77***	7.36**	7.44***	7.29**	9.39***	6.51**
	(3.39)	(3.29)	(2.76)	(3.23)	(3.46)	(2.81)
Supply-Side Education Inputs						
Number of Private Schools (per 1,000 people)	.145*	.131	.125*	.129	.137*	.1
	(.0827)	(.0978)	(.0723)	(.0989)	(.0785)	(.076)
Number of Students per Classroom	-19.3	-17.9	-10.8	-17.4	-19.6	-4.04
	(17.3)	(18.8)	(16.2)	(18.7)	(17.7)	(20.6)
% Teachers with at Least a Graduate Degree	28.3***	33.4***	22.4**	33.8***	22.4**	27.3**
	(10.2)	(11.2)	(9.03)	(11.2)	(9.82)	(10.9)
% Teachers with Bachelor or Master of Education	19.6	27.7*	13	29**	1.34	14.3
	(12.3)	(14.5)	(10.4)	(14.7)	(9.56)	(12.2)
Number of Boys per Toilet	-60.1*	-72.5**	-32.5	-74.5**	7.67	-25.8
	(31.6)	(36.3)	(27.9)	(36.8)	(23.3)	(33.9)
Number of Girls per Toilet	-63.3**	-76.5**	-34.9	-78.5**	-.408	-30.2
	(31.7)	(36.5)	(27.8)	(37)	(20.2)	(33.5)
Test Scores						
Reading Score (Ages 11-16)	.157*	.29***	.103	.3***	.0122	.12
	(.0866)	(.104)	(.0771)	(.107)	(.0791)	(.0885)
Math Score (Ages 11-16)	.199*	.293**	.152	.297**	.151	.178
	(.108)	(.126)	(.0962)	(.127)	(.112)	(.11)
Reading Score (Ages 6-10)	.219*	.348**	.161	.356**	.136	.17
	(.12)	(.146)	(.105)	(.148)	(.114)	(.121)
Math Score (Ages 6-10)	.152	.255	.102	.264	.0818	.129
	(.133)	(.159)	(.115)	(.161)	(.123)	(.136)

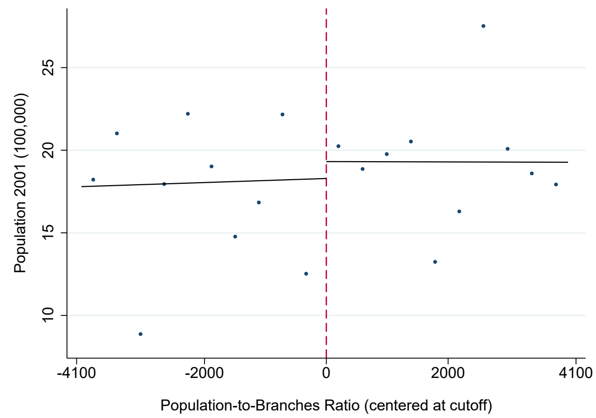
Notes: Column (1) presents the baseline specification that uses an MSE-optimal bandwidth selector, triangular kernel, and first order polynomial. Columns (2) and (3) use 75% and 125% of the bandwidth for the corresponding variable, respectively. Column (4) uses a CER-optimal bandwidth selector. Column (5) uses a uniform kernel while column (6) uses a second-order polynomial. Lending regressions are estimated at the district level using data on private sector banks from the BSR (2010-2015). Household borrowing regressions are estimated at the household level using NSS Round 70 (2013). Supply-side education input regressions are estimated at the school level using NDAP UDISE (2018) data. Test score regressions are estimated at the child level using ASER data from 2011 - 2012. All regressions include state by year fixed effects and control for district-level population and its square, literacy rates, and poverty rates. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

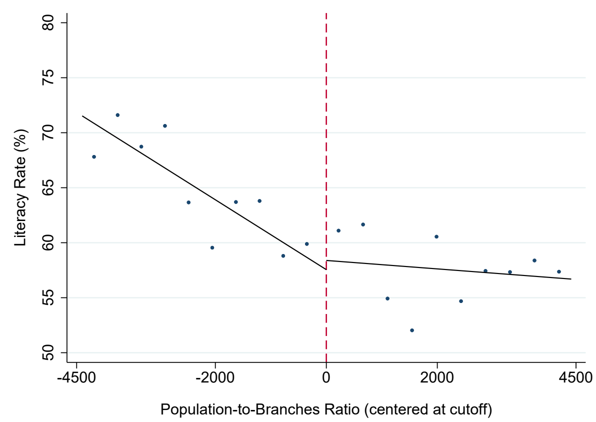
For Online Publication

Figure A1: Assessing Continuity of Covariates (Pre-Policy)

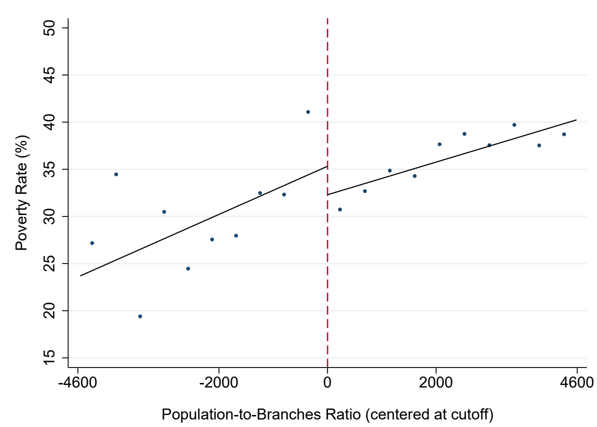
(a) Population (Census, 2001)



(b) Literacy Rate (NSS Round 61, 2004-2005)



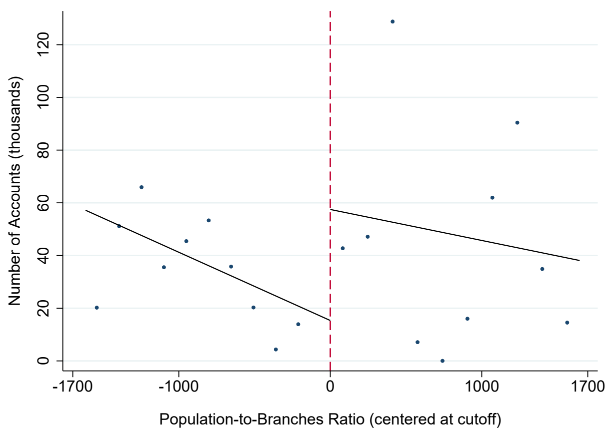
(c) Poverty Rate (NSS Round 61, 2004-2005)



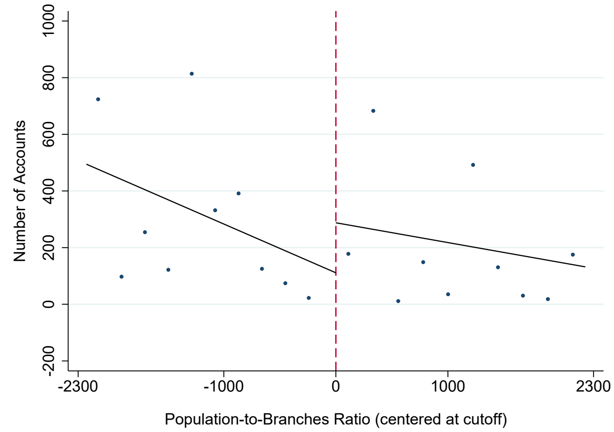
Notes: Each figure presents a binned scatter plot and linear fit for the pre-policy covariates. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. Figure A1a uses data from the Census (2001) while Figures A1b and A1c use data from NSS Round 61 (2004-2005).

Figure A2: Bank Credit

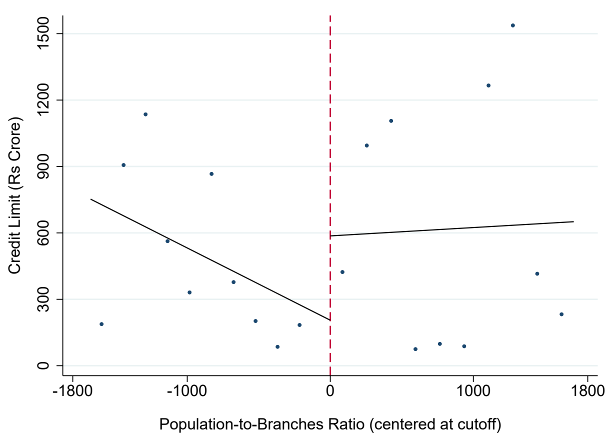
(a) Number of Accounts



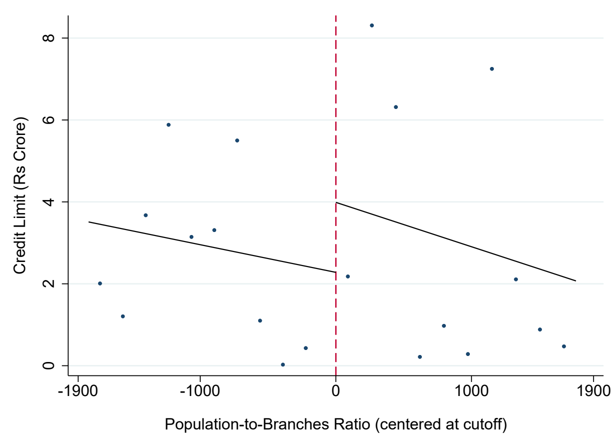
(b) Number of Accounts (Education Loans)



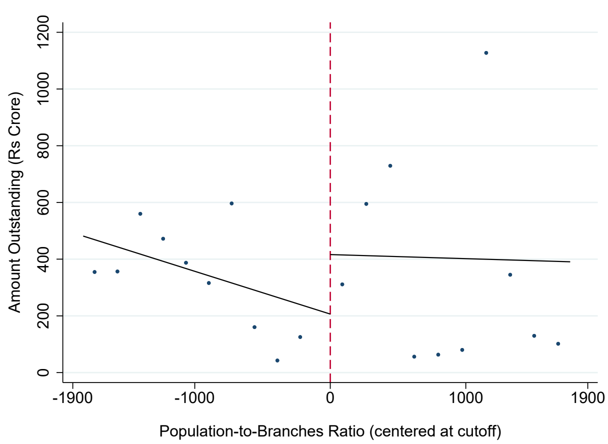
(c) Credit Limit



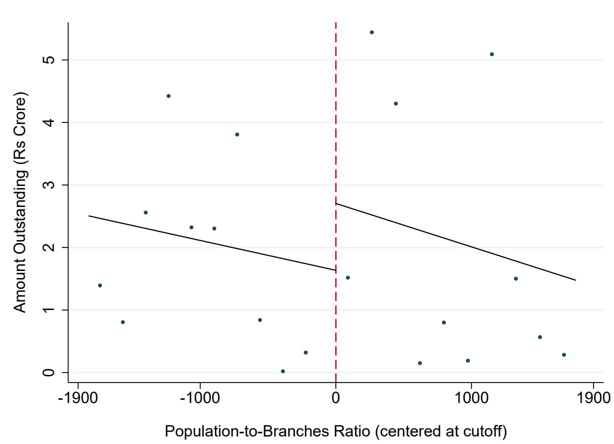
(d) Credit Limit (Education Loans)



(e) Amount Outstanding



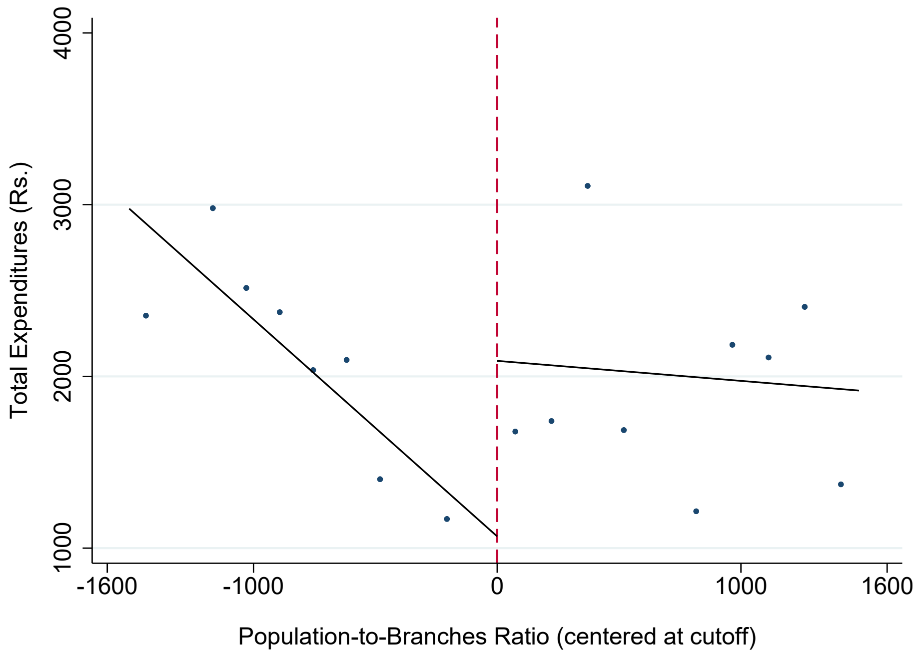
(f) Amount Outstanding (Education Loans)



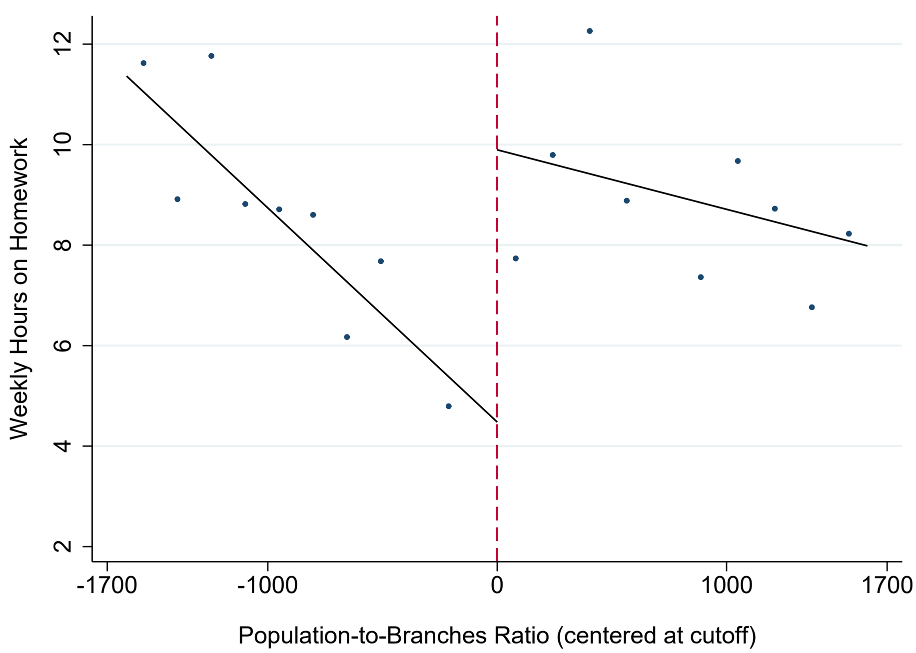
Notes: Each figure presents a binned scatter plot and linear fit for bank credit variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. District-level data on private sector banks from the BSR (2010-2015) is used.

Figure A3: Demand-side Mechanisms: Education Inputs

(a) Expenditures on Education

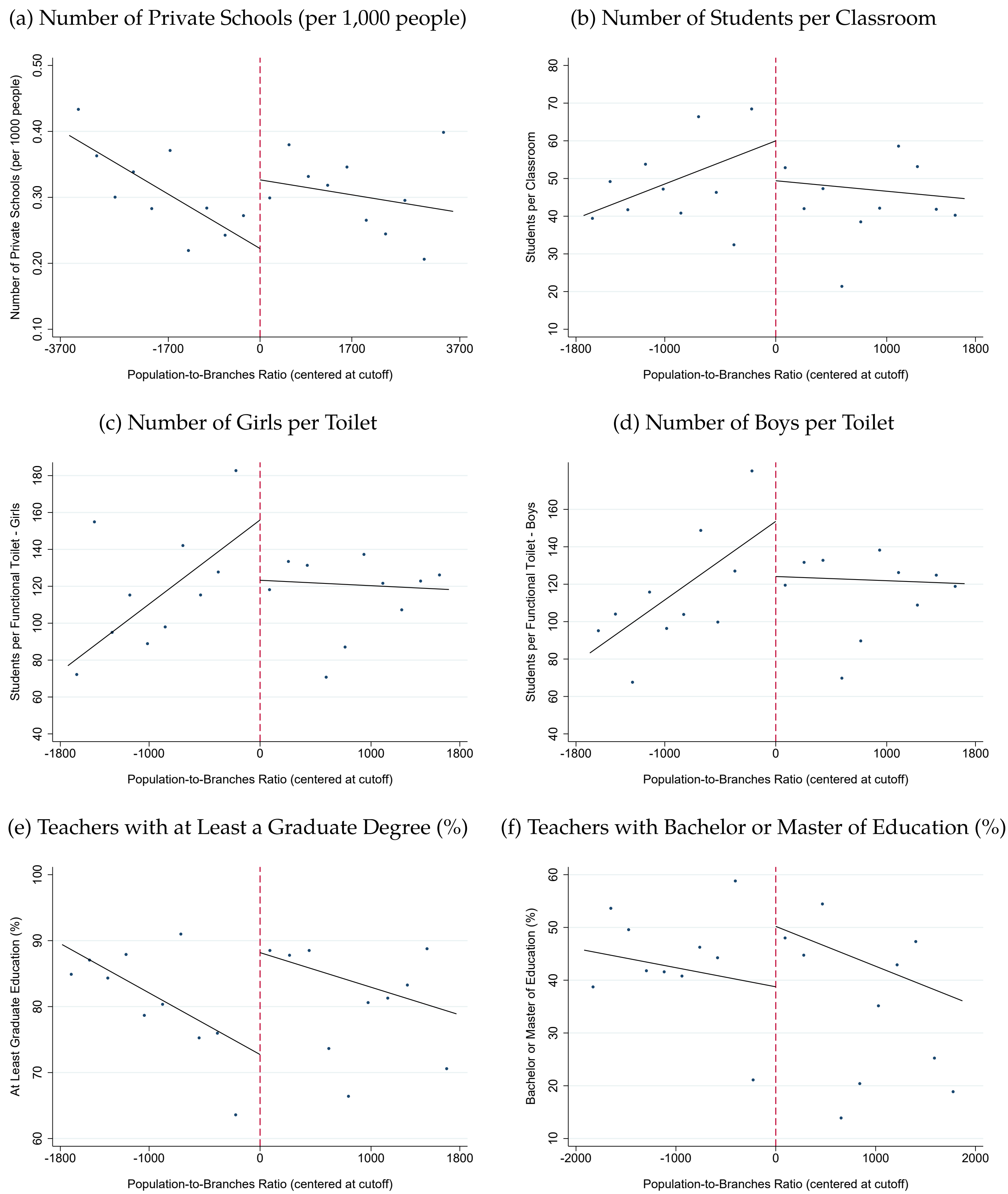


(b) Children's Time Spent on Homework



Notes: Each figure presents a binned scatter plot and linear fit for education input variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. Child-level data from IHDS II (2011-2012) is used. IHDS survey weights are used.

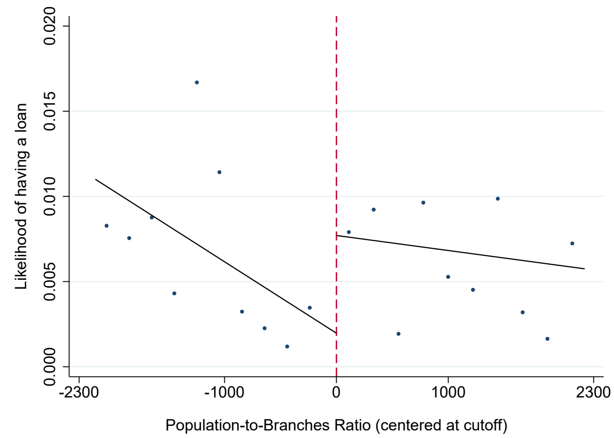
Figure A4: Supply-side Mechanisms: School Quantity & Quality



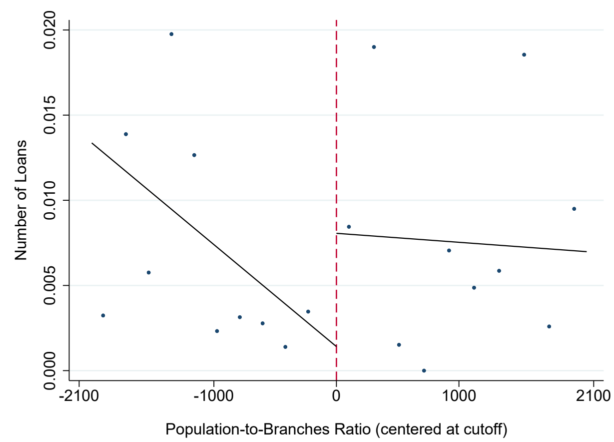
Notes: Each figure presents a binned scatter plot and linear fit for school variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. School-level data from NDAP UDISE (2018) is used.

Figure A5: Labor Market Mechanisms: Business Loans

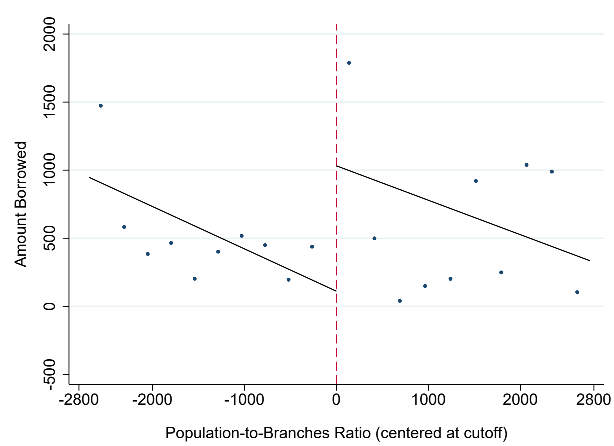
(a) Has a Bank Loan for Business



(b) Number of Bank Loans for Business



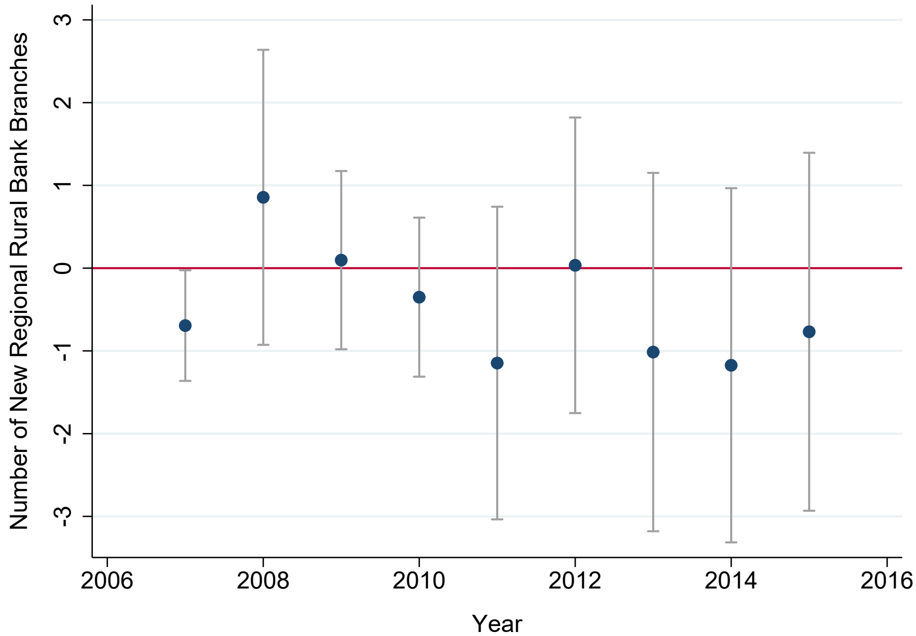
(c) Amount Borrowed (Bank Loans for Business)



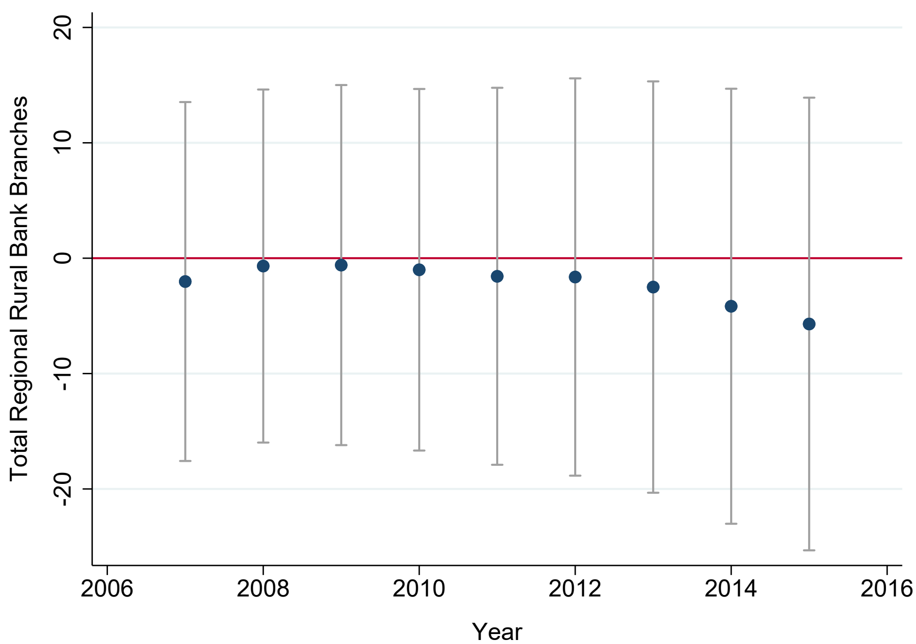
Notes: Each figure presents a binned scatter plot and linear fit for business loan variables. 10 equally-spaced bins were used on either side of the cutoff and each dot plots the mean value within the corresponding bin. The running variable, the population-to-branches ratio (Average Population Per Branch Office, or APPBO) is centered at the cutoff value of 14,828. This cutoff is indicated by the dashed vertical line. Household-level data from NSS Round 70 (2013) is used. NSS survey weights are used.

Figure A6: Placebo Tests Using Regional Rural Banks

(a) New District Regional Rural Bank Branches



(b) Total District Regional Rural Bank Branches



Notes: These figures plot estimates of β_1 from the following equation: $Y_{hdst} = \beta_0 + \beta_1 AboveCutoff_d + f(DistrictRatio_d - Cutoff) + \beta_2 Y_{hds,2006} + \varepsilon_{hdst}$. The regressions are estimated using the Bank Branch Statistics data from the RBI at the district-level, separately by year (2007 - 2015). Figure A6a plots the number of new bank branches, while Figure A6b plots the total number of bank branches. 95% confidence intervals are shown.

Table A1: Summary of Key Datasets Used

Dataset	**Years Covered**	**Geography Covered**	**Key Variables**
RBI Bank Branch Statistics	2006-2015	Rural + Urban	Bank branches
RBI Basic Statistical Return	2010-2015	Rural + Urban	Bank lending, deposits
National Sample Survey (NSS) Debt and Investment Round 70	2013	Rural + Urban	Household borrowing, deposits
National Sample Survey (NSS) Employment Rounds 61, 66, 68	2004-2012 (with gaps)	Rural + Urban	Employment, poverty, literacy
India Human Development Survey (IHDS) Round 2	2011-2012	Rural + Urban	Time use, education expenditures
Unified District Information System for Education (UDISE)	2018	Rural + Urban	School teachers, enrollment, school facilities
Annual Status of Education Report (ASER)	2011-2012	Rural only	Test scores
Census of India	2001, 2011	Rural + Urban	Population

Table A2: Permutation Tests for Continuity of the Distribution of Covariates

Variable	p-value
Population (Census, 2001)	0.91
Literacy Rate	0.48
Poverty Rate	0.28
Joint Test - Max Statistic	0.61
Joint Test - CvM Statistic	0.56

Notes: This table presents p-values for permutation tests that test for the continuity of the distribution of the covariates at the cutoff, as described in Canay and Kamat (2017). We present p-values for tests of the continuity of each of the three covariates individually, as well as the joint test for the continuity of the three-dimensional vector of covariates. For the joint test, we separately present p-values corresponding to the Cramér-von Mises (CvM) test statistic and the max-type test statistic introduced in Canay and Kamat (2017). These tests were implemented using the $rdperm$ Stata package.

Table A3: Placebo Test: Household Borrowing & Savings (Pre-Policy)

	All Borrowing			Borrowing for Household Expenses			Deposits	
	(1) Has a Loan	(2) Number of Loans	(3) Amount Borrowed	(4) Has a Loan	(5) Number of Loans	(6) Amount Borrowed	(7) Has Deposits	(8) Value of Deposits
Underbanked	-.0243	-.0215	-1,086	.0157	.0201	134	.104	2,240
	(.0282)	(.0353)	(1,067)	(.0126)	(.0146)	(314)	(.0782)	(1,418)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.0616	.0682	2,130	.0191	.02	442	.152	2,262
First Stage	0.741	0.742	0.766	0.696	0.696	0.705	0.720	0.693
Bandwidth	2,359	2,370	2,720	1,654	1,667	1,837	2,090	1,591
Observations	139,303	139,303	139,303	139,303	139,303	139,303	139,303	139,303
Effective Obs.	36,300	36,300	43,345	23,620	23,620	26,134	30,778	23,329

Notes: Regressions are estimated at the household level using NSS Round 59 (2003). Loans in columns (1) - (6) refer to loans outstanding between June 30, 2002 and the survey date, with the exception of long-term loans that may have been taken out before the policy. Deposits in columns (7) - (8) refer to deposits as of June 30, 2002. NSS borrowing and deposit information pertains to commercial banks including regional rural banks. All regressions include state fixed effects, a rural indicator, and control for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Placebo Test: Business Loans (Pre-Policy)

	Agricultural			Non-Agricultural		
	(1) Capital Expenditures	(2) Current Expenditures	(3) Total	(4) Capital Expenditures	(5) Current Expenditures	(6) Total
Panel A: Has a Loan						
Underbanked	-.00995	-.00999	-.0164	.00116	-.00269	-.00138
	(.00812)	(.0175)	(.0219)	(.00392)	(.002)	(.00507)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.013	.0154	.0288	.00698	.00294	.0102
First Stage	0.777	0.759	0.765	0.710	0.725	0.712
Bandwidth	2,932	2,618	2,713	1,917	2,145	1,968
Observations	139,303	139,303	139,303	139,303	139,303	139,303
Effective Obs.	46,742	40,826	43,345	26,666	32,835	27,352
Panel B: Number of Loans						
Underbanked	-.0136	-.00936	-.0229	.00077	-.00278	-.00224
	(.0127)	(.0198)	(.0291)	(.00385)	(.00232)	(.00507)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	.015	.0161	.0312	.00719	.00323	.0102
First Stage	0.757	0.758	0.758	0.711	0.730	0.716
Bandwidth	2,586	2,597	2,596	1,953	2,211	2,032
Observations	139,303	139,303	139,303	139,303	139,303	139,303
Effective Obs.	40,407	40,546	40,546	27,352	33,479	29,521
Panel C: Amount Borrowed						
Underbanked	-605	93.6	-505	-884	-159	-1072
	(428)	(391)	(743)	(751)	(132)	(752)
State FE	✓	✓	✓	✓	✓	✓
Mean of Control Districts	474	274	749	463	107	577
First Stage	0.758	0.756	0.758	0.743	0.762	0.746
Bandwidth	2,599	2,570	2,593	2,387	2,666	2,429
Observations	139,303	139,303	139,303	139,303	139,303	139,303
Effective Obs.	40,546	40,407	40,546	36,300	42,351	36,818

Notes: Regressions are estimated at the household level using NSS Round 59 (2003). Loans refer to loans outstanding between June 30, 2002 and the survey date, with the exception of long-term loans that may have been taken out before the policy. NSS borrowing information pertains to commercial banks, including regional rural banks. All regressions include state fixed effects, a rural indicator, and control for district-level population and its square, literacy rates, and poverty rates. NSS survey weights are used. All regressions use a triangular kernel and an MSE-optimal bandwidth selector. Standard errors are clustered by district and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.