Distributional Implications of Bank Branch Expansions: Evidence from India

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Abstract

Does financial deepening affect capital investment by credit constrained firms? We examine this question by exploiting a nationwide branch expansion policy in India that incentivized banks to open branches in "underbanked" districts i.e., where the ex-ante bank branch density was less than the national district level average. Extending a regression discontinuity design, we find large increases in both capital expenditures and credit growth undertaken by manufacturing establishments in underbanked districts following the policy intervention. The increase in capital spending is driven by small and young establishments, which are also the most likely to be credit constrained. Two key channels explain our findings: increased physical proximity of lenders to borrowers and the comparative advantage of select banks in lending to small manufacturing units. Our results show that financial deepening can aid in the relaxation of credit constraints in developing economies with imperfect capital and credit markets.

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1 Introduction

Financial frictions contribute towards productivity differences in firms across developing and developed economies (Hsieh and Klenow, 2009; Bloom and Mahajan, 2010). A combination of information asymmetries, inadequate collateral and high ex-post monitoring costs can result in the exclusion of firms from formal credit markets in developing economies, leading to low capital accumulation and productivity. This is particularly true for smaller firms, despite existing evidence documenting that these firms have high returns to capital – well in excess of prevailing deposit rates in formal financial institutions (De Mel et al., 2008; Banerjee and Duflo, 2010, 2014). This raises the question of whether increased physical proximity of financial institutions can affect capital investment by alleviating barriers to credit access through improved screening, information acquisition and monitoring. The question is particularly relevant for informationally-opaque small firms, likely to face binding credit-constraints. Our paper empirically examines this question, exploiting a nationwide bank expansion policy in India.

The empirical challenge in causally establishing the relationship between local financial infrastructure and firm performance is the endogenous selection of locations by financial institutions. We overcome this by exploiting a unique policy experiment undertaken by India's central bank – the Reserve Bank of India (RBI)[1] – to expand bank branches in regions with under-developed financial infrastructure. We study the Branch Authorisation Policy (BAP) of 2005 which replaced the existing system of case-by-case approvals of new bank branch openings with a comprehensive review of banks' annual expansion plans. Under this policy, the RBI noted that banks' overall expansion plans would be favourably received conditional on lenders' financial intermediation in select districts[2] classified as "underbanked". The selection of underbanked districts was undertaken by comparing districts' bank branch density to the national average bank branch density. Specifically, a district was deemed underbanked if its bank branch density in 2005 was less than the prevailing national average. While no specific targets were provided, banks were encouraged to expand operations – primarily branch openings, rural credit, and credit to small and micro-enterprises – in underbanked districts, with the approval of bank groups' overall expansion plans being contingent on the extent of their financial intermediation in these areas.

We exploit the above rule using a regression discontinuity (RD) design to estimate the causal

[1] In addition to currency management and monetary policy, the RBI also serves as the banking regulator in India.
[2] Districts form the third tier of administration in India, below states.

impact of bank branch expansion on firm outcomes. While the policy was announced in the latter half of 2005, the assignment of districts to underbanked status was undertaken using bank branch data from March 2005, and population data from 2001, limiting the ability of banks or local governments to influence treatment assignment. Formally, we rule out the selective sorting of districts into “treatment” and “control” status, and also demonstrate the balance of pre-treatment district covariates across underbanked and non-underbanked districts within a narrow window of the discontinuity threshold.

Existing work by Young (2017), Khanna and Mukherjee (2021) and Cramer (2022) shows that the policy indeed led to an expansion of bank branches in these districts. We confirm their findings and show that underbanked districts received between 5 and 8 additional bank branches between 2006 and 2011, relative to observationally equivalent non-underbanked districts. Using administrative data, we also identify an increase in farm and manufacturing credit in underbanked districts in response to the policy intervention. The increase in financial intermediation – both in terms of bank branches and credit disbursement – was entirely driven by private banks, which also increased their staffing of officers typically responsible for credit disbursement. This is not unexpected when considering that over 60% of underbanked districts (and 45% of districts overall) saw no operation by private banks in 2005. In contrast, government-owned banks – possibly due to the large push during the “social banking era” in the 1980s – had existing operations across all districts, and did not respond to the policy.[3]

Having confirmed that the policy intervention increased both branches and manufacturing credit from private banks, we identify the impact of the BAP on manufacturing investment using data from the Annual Survey of Industries (ASI) – a large nationally representative survey of registered manufacturing establishments in India. The ASI provides annual data at the establishment level on fixed assets, raw materials, output, workers hired, and salaries paid. The ASI also provides district and establishment identifiers between 1998 and 2012, allowing for the construction of an 11 year establishment-level panel (2001-2011), with exposure to the BAP being determined by establishments' location in a district classified as underbanked. Importantly, while the dataset by definition is restricted to registered establishments, the median establishment size in the year 2000 was 15 employees, and two-thirds of establishments hired less than 20 employees, allowing us to identify the distributional

[3] Government-owned banks dominate the banking landscape in India, accounting for over 60% of the credit disbursed and the majority of bank branches. These banks had also led the initial expansion in branch banking to areas lacking financial infrastructure in the period between 1969 and 1990, after the nationalization of the 14 largest banks (Burgess and Pande, 2005).

implication of the bank branch expansion across small and micro-enterprises in the manufacturing sector.

Our empirical strategy exploits the time-variation in the establishment-level panel to extend the RD design using a differences-in-discontinuity approach, which compares establishment outcomes across underbanked ("treated") and non-underbanked ("control") districts, before and after the policy intervention. The inherent discontinuity in districts' assignment to underbanked status ensures the comparability of treatment and control units prior to the policy intervention. We verify this by showing that manufacturing outcomes were statistically indistinguishable in the pre-treatment period across these two sets of districts within a narrow window around the discontinuity threshold.

Our primary outcome of interest is capital expenditure by manufacturing establishments, defined as investments in plant and machinery. We identify a 6 percentage point increase in capital spending (equivalent to INR 1.8 million) for establishments in underbanked districts, relative to observationally equivalent manufacturing establishments in non-underbanked districts. Our preferred specification uses establishment and industry-year fixed effects, along with establishment and district-level covariates. The use of industry-year fixed effects restricts the comparison of manufacturing investment to establishments in the same broad industry category and year, with the identifying variation stemming from differences in districts' underbanked status. An event-study specification documents the absence of any differential pre-treatment trends in capital spending in underbanked districts, but a sharp uptick following the policy intervention. The increase in manufacturing investment in underbanked districts is also accompanied by higher credit growth. While we cannot distinguish across sources of credit, the findings are consistent with the aggregate increase in manufacturing credit by private banks in underbanked districts.

Our baseline results are robust to alternate functional forms and definitions of the outcome variable. To ensure the ex-ante comparability of treatment and control units, we restrict our primary sample to establishments in districts within a bandwidth of 15 (bank branches per million population) around the discontinuity threshold but demonstrate robustness to a number of alternate bandwidths. This affirms that the increase in manufacturing investment is not driven by enterprises operating out of a select subset of districts. We also show robustness to the exclusion of any individual state or industry, allaying concerns that the identified treatment effect is emanating from confounding state or industry-specific policies, the timing of which is correlated with the BAP. Using a placebo test, we confirm a null effect upon restricting the sample to 2005: the period prior to the implementation of the BAP.

We next explore the distributional implications of financial deepening and assess whether the increase in manufacturing investment in underbanked districts is driven by enterprises likely to be credit-constrained. A large literature has shown credit constraints are likely to bind for smaller firms (Beck et al., 2008; Clark et al., 2004; Galindo and Micco, 2007). Consistent with this, we find the increase in capital spending and credit growth to be concentrated amongst establishments hiring under 25 workers. Motivated by Criscuolo et al. (2019), we also examine heterogeneity across small and young establishments and find the increase in capital investment and credit growth in underbanked districts to be driven by young establishments hiring less than 16 workers (small). The results are similar if pre-treatment establishment fixed assets is used to determine establishment size: notably, the positive treatment effects are concentrated amongst establishments satisfying the administrative classification of small-scale industries. Consistent with the inference of Farre-Mensa and Ljungqvist (2016) that being unlisted is a necessary condition for a firm to be financially constrained, we also find the positive impact on capital investments and credit growth to be generated by establishments which are not publicly listed.

We examine five potential channels to explain the increase in manufacturing investment in underbanked districts: namely, the physical proximity of lenders to borrowers; comparative advantage of creditors in lending to small borrowers; reductions in the cost of credit due to lender competition; enhanced borrower screening leading to improvements in borrower quality; and aggregate demand. In the absence of information on the precise location of establishments in the ASI, we first scrape data on the physical addresses of bank branches to establish higher dispersion of private bank branches away from district headquarters in underbanked districts. Specifically, private banks in underbanked districts were located 7 kilometres farther from district headquarters, relative to a pre-treatment mean of 19 kilometres in control districts. Next, we confirm using an alternate financial database for large Indian firms that the average distance between relatively small manufacturing firms and district headquarters was 18 kilometres, and one fourth of the firms were located at distances in excess of 25 kilometres. This points to a non-trivial mass of small manufacturing firms located away from district headquarters, who were possibly closer to private banks in the post-treatment period owing to the expansion of private bank branches in underbanked districts. If the physical proximity of lenders and borrowers reduces the costs of information acquisition and monitoring, this can serve as a plausible channel in explaining the positive response of manufacturing investment to financial deepening.

Next, we examine whether lenders' comparative advantage in transacting with small borrowers can explain the increase in manufacturing investment by small and young establishments. Exploiting proprietary data on banks' lending portfolios, we identify the increase in capital investment in underbanked districts only when the district witnessed entry by a private bank which transacted with a relatively high (above median) share of small manufacturing borrowers in the pre-treatment period. Given the inherent flexibility accorded by the BAP to banks to select underbanked districts in which to expand operations, these results need to be interpreted with caution, but nonetheless point to the role of lenders' comparative advantage in transacting with small borrowers in explaining the increase in capital investment.

We find modest evidence suggesting that financial deepening by private banks led to an improvement in borrower quality in underbanked districts: the increase in capital investment is driven by establishments which had relatively high marginal returns to capital, and faced low interest costs in the pre-treatment period. There is also no increase in the share of non-performing loans on private banks' balance sheet in these districts, ruling out that the expansion in private bank credit came at the cost of borrower quality. We, however, find no evidence suggesting a reduction in the cost of credit in underbanked districts due to increased lender competition. Finally, we draw from Mian and Sufi (2014) and show that the increase in capital investment in underbanked districts is undertaken by establishments in both tradable and non-tradable industries. If increased aggregate demand in response to financial deepening was the sole explanation for our findings, we would have expected the treatment effects to be driven exclusively by establishments in non-tradable industries catering to local demand.

We conclude our empirical analysis by examining the aggregate impacts of the policy intervention at the district level. We confirm the increase in aggregate manufacturing investment and credit growth in underbanked districts. We find strong evidence showing an increase in the number of establishments operating in underbanked districts, suggesting that the expansion of financial infrastructure resulted in firm entry. Consistent with higher firm entry, we find evidence pointing to an aggregate increase in manufacturing workers in these districts, although the point estimates are imprecise. We however find no significant impact of the policy intervention on aggregate output and productivity, though the effects are positive.

Collectively, our empirical findings show that private banks responded to the BAP by expanding operations in underbanked districts in the form of increased bank branches, and higher disbursement of manufacturing and farm credit. Consistent with the increase in manufacturing credit, we identify an increase in capital investment and credit growth for manufacturing establishments in underbanked districts, driven by small, young and unlisted establishments. The policy resulted in a wider dispersion of private banks around district headquarters, which likely led to increased physical proximity between lenders and borrowers. The increase in capital investment is also driven by districts witnessing entry by private banks which specialized in small manufacturing loans, pointing to the role played by lenders' comparative advantage. At an aggregate level, we also identify higher manufacturing investment and firm entry in underbanked districts.

Our paper contributes to the large literature studying the economic impacts of financial deepening, often using episodes of branch deregulation as natural experiments for causal identification. An early pioneer in this field, Jayaratne and Strahan (1996), showed that branch deregulation in the U.S. improved the quality of credit intermediation. Beck et al. (2010) studied the same intervention and found a lowering of inequality through higher demand for unskilled labour. D'Onofrio and Murro (2019) and Minetti et al. (2021) exploited historical restrictions imposed on bank expansion in Italy and found similar results. In Mexico, Bruhn and Love (2014) showed that the unexpected opening of Banco Azteca branches (a bank specializing in lending to low and middle income groups) resulted in higher credit to small businesses, while Fafchamps and Schündeln (2013) found increased local bank availability in Morocco to facilitate faster growth for small and medium sized firms in high growth sectors. Studies have also reported increases in firm entry due to improved financial access (Black and Strahan, 2002; Bruhn and Love, 2014).

In relation to these papers, our paper extends a RD design and shows that the expansion of private bank branches in previously under-served areas increased capital investment and credit growth of registered manufacturing establishments. We differ from Beck et al. (2010), Rice and Strahan (2010) and D'Onofrio and Murro (2019), who find no direct impact of financial deepening on firm capital, but a decline in inequality through general equilibrium channels. While our broad findings are consistent with those of Fafchamps and Schündeln (2013), we exploit the quasi-exogenous entry of new bank branches to hitherto underbanked areas, as opposed to exploiting the existing stock of financial infrastructure. Moreover, the prevalence of micro and small establishments in our data permits us to precisely identify the impact of financial deepening on enterprises ubiquitious across developing economies (Hsieh and Olken, 2014). Importantly, while much of the literature on credit in developing economies has focused on government-owned banks and microfinance institutions, we show that private banks too can be incentivized to engage in financial intermediation in under-served regions, which in turn can positively affect small firms' capital.

By documenting the distributional implications of the bank branch expansion, our paper also relates to the large body of research studying how access to finance affects small and micro-enterprises. While field experiments have estimated high returns to capital for micro-enterprises, information frictions, monitoring costs, and the absence of collateral limit lenders' willingness to extend credit to micro-entrepreneurs. Our paper shows that conditional on formalization, the expansion of financial infrastructure can aid in the alleviation of credit constraints for small enterprises with limited collateral. Additionally, our mechanisms offer evidence consistent with the explanation that the physical proximity of lenders to borrowers, and lenders' comparative advantage in lending to small manufacturing borrowers are the key channels through which financial deepening affects manufacturing investment in under-developed regions (Petersen and Rajan, 1994; Berger and Udell, 1995; Chen et al., 2015). Consequently, we add to the literature showing that local financial intermediation aids in the collection of firm-level information (Agarwal and Hauswald, 2007).

In the Indian context, our paper contributes to the existing body of work estimating the economic impacts of financial deepening. Unlike Burgess and Pande (2005), Kochar (2011) and Gupta and Dehejia (2021), who focus on the massive state-directed push by government-owned banks, our paper identifies the impact of expansions in private bank operations, which are perceived to have superior corporate governance, and greater alignment with market forces. To this effect, we extend the work of Young (2017) who showed that the BAP-induced increase in private bank branches positively affected farm credit and nightlights-based measures of economic activity.[4] We confirm the findings of Young (2017) who shows an aggregate impact on state-level manufacturing capital and output, but focus instead on the distributional aspects of the increase in manufacturing investment, and also precisely identify the mechanisms through which financial deepening affects manufacturing investment. The focus on small and micro-establishments also distinguishes our work from Chakraborty et al. (2021) which exploits a later reform aimed at expanding bank branches in relatively smaller urban centres to show how increased lender competition disciplines government-owned banks.

[4] Khanna and Mukherjee (2021) also exploits the same policy intervention to show how bank branches served as a coping mechanism when districts faced an aggregate negative shock to cash supply. Cramer (2022) uses the same policy intervention to show the impact of financial deepening on health outcomes.

The remainder of the paper is organized as follows: Section 2 outlines the Branch Authorisation Policy; Section 3 formally details our empirical strategy and data sources; Section 4 presents our key findings; Section 5 explores potential mechanisms explaining our results; and Section 6 reports aggregate effects of financial deepening.

2 Background and Policy Intervention

After bank nationalization in 1969, the Indian federal government, in conjunction with the central bank embarked on an aggressive policy of branch expansion between 1977 and 1991, led by government-owned banks. The impact of branch expansion during this period of "social banking" has been examined by Burgess and Pande (2005), Kochar (2011) and Gupta and Dehejia (2021). With the onset of economic liberalization in 1991, the central bank formally abandoned the rule-based branching policy in 1993 and allowed commercial banks to open branches as determined by market forces.[5]

In 2005, the RBI initiated a "liberalised branch authorisation policy" which simplified the branch authorisation process, but also accorded greater weightage to branches opened in hitherto "underbanked" areas (RBI, 2005). Unlike the social banking era, no explicit rules were framed, but the RBI incentivised banks to open new branches in areas with fewer existing branches. The existing system of case-by-case approvals for new branch openings was replaced by the annual approval of individual banks' branch expansion plans. The implicit nudge to banks was that those opening new branches in underbanked areas would receive favourable treatment from the banking regulator with regard to their overall expansion plans. Specifically, the RBI noted that while evaluating proposals for bank expansion, weightage would be accorded to "the nature and scope of banking facilities provided by banks to common persons, particularly in underbanked areas (districts), actual credit flow to the priority sector, pricing of products and overall efforts for promoting financial inclusion." (RBI, 2007) Furthermore, the RBI noted that it would assess "compliance with not only the letter of the regulations but also whether the bank's activities are in compliance with the spirit and underlying principles of the regulations."[6] (RBI, 2007)

To classify regions as "underbanked", the RBI followed a simple rule based on districts' bank

[5] Under social banking, banks were required to open 4 additional branches in "underbanked" area, for every branch opened in a "banked" area.

[6] For instance, annual branch expansion plans of banks now had to be accompanied by a statement depicting the distribution of operational bank branches in underbanked districts, as well as semi-urban and rural centres (RBI, 2007).

branch density in 2005. For each district, the RBI computed persons per branch using the district's population from the 2001 Census, and the number of commercial bank branches in operation on March 31, 2005. This was compared to the "national" persons per branch ratio across all districts, and districts were classified as "underbanked" if their persons per branch ratio exceeded this national persons per branch ratio. For the ease of exposition, we invert RBI's persons per branch ratio to define for each district d an indicator variable *Underbanked* as:

$$Underbanked_d = \mathbb{1}(BranchPC_d < \overline{BranchPC}) \tag{1}$$

where $BranchPC$ is the number of bank branches in the district, scaled by the district population in millions and $\overline{BranchPC}$ is the national average bank branches per capita. Using this rule, the RBI published in September 2005 a list of 386 "underbanked" districts.[7].

As data prior to 2005 was used to determine districts' underbanked status, districts could not plausibly select into "underbanked" status. Nor is there any anecdotal evidence of prior intimation of the policy, which could lead banks to open branches prior to the treatment intervention in underbanked districts. Empirically, Figure 1 confirms using the McCrary test (McCrary, 2008) the absence of any selective sorting of districts into treatment and control status around the national average threshold. This allows us to use the national average branch density – $\overline{BranchPC}$ – as an arbitrary threshold in a RD design to causally identify the impact of bank branch expansion on manufacturing outcomes.

With $\overline{BranchPC}$ serving as the discontinuity threshold for a district's underbanked status, the running variable of interest – $Runvar_d$ – is defined as:

$$Runvar_d = BranchPC_d - \overline{BranchPC} \tag{2}$$

Thus, districts are underbanked if $Runvar_d < 0$ or the district's bank branch density in 2005 fell below the national average. Figure 2 shows the distribution of $Runvar_d$, with a significant mass of districts around the threshold 0. For instance, 304 districts (211 underbanked and 93 non-underbanked) fall within a bandwidth of 20 around the threshold of 0. Reducing the bandwidth to 15 and 10 results in

[7] While the rule for classifying districts as underbanked was followed for the vast majority of districts, the RBI amended this rule for a total of 9 districts in 2006. Thus, 6 districts were classified as underbanked, even though their branch density exceeded the national average, while 3 districts were not classified as underbanked, even though their branch density fell below the national average. For addition details, see RBI's master circular on branch authorisation, issued on August 3, 2005 (available at `https://rbi.org.in/Scripts/BS_CircularIndexDisplay.aspx?Id=2408`)

231 and 156 districts around the threshold lying within the bandwidth, respectively. The concentration of a large set of districts around the discontinuity threshold provides statistical power to execute our empirical strategy, and also limits external validity concerns.

While the policy applied to all banks, we expect private banks to be disproportionately affected. At the beginning of 2005, 45% of Indian districts lacked a private bank branch, while 60% of districts classified as underbanked had no private bank branch. On the contrary, government-owned banks had branches across all districts, including those classified as underbanked. This made the revised policy binding on private banks, which were also mandated by the policy to have at least 25% of their branches in semi-urban or rural centres. Aggregate trends too pointed to an increase in private bank branch openings after the adoption of the BAP: thus, the median private bank branch density increased from 0.66 branches to 2.85 branches (per million population) between 2005 and 2010. The fraction of districts without a private bank branch also fell below 20% in 2010.

Young (2017) was the first to rigorously study this intervention and documented an increase in private bank branches in underbanked districts, with no corresponding effects for government bank branches. The expansion in private bank branches was accompanied by an increase in private bank credit to these areas. While our paper focuses on manufacturing investment, Appendix A extensively documents how the BAP impacted banking outcomes. We confirm the findings of Young (2017) and show that upon restricting the sample to a narrow window around the threshold, the policy resulted in the opening of 5-9 additional private bank branches in underbanked districts between 2006 and 2011 (relative to 12 branches in observationally equivalent non-underbanked districts). There was no corresponding increase in government-owned bank branches in underbanked districts during this period, or private bank branches between 2001 and 2005. This rules out that the expansion of private banks in these regions was driven by prior trends in private bank operations, or a secular contemporaneous increase in financial infrastructure. The expansion in private bank branches was accompanied by higher credit disbursement, particularly to the farm and manufacturing sectors. Reassuringly, there is no differential trends in private bank credit disbursement across underbanked and non-underbanked districts prior to the policy intervention. Credit from government-owned banks remained comparable across underbanked and non-underbanked districts around the discontinuity threshold in the aftermath of the BAP.

In line with higher credit disbursement from private banks in underbanked districts, we document a discontinuous jump in the share of "officers" (as a share of bank employees) in private banks in these districts. Officers are typically tasked with overall branch management and are also responsible for lending decisions. Collectively, the results indicate that following the BAP, private banks opened new branches in underbanked districts, and also engaged in higher financial intermediation in these districts. We also find evidence pointing to an increase in "priority sector" lending – namely farm credit and credit to small, micro and village enterprises – in these districts, indicating that private banks complying with the BAP used operations in underbanked districts to better meet their priority sector targets.[8]

3 Data and Empirical Strategy

This section describes the primary datasets used in the paper and the empirical strategy to causally identify the impact of bank branch expansion on manufacturing investment.

3.1 Manufacturing Enterprise Data

We use data from the Annual Survey of Industries (ASI) to identify the impact of bank branch expansion on manufacturing investment. The ASI is a nationally representative survey undertaken every year by the National Sample Survey Organisation (NSSO), covering registered manufacturing enterprises in India. The unit of observation is the manufacturing establishment (and not the firm). The ASI has two components: a census component whereby establishments employing over 100 workers are surveyed every year, and a survey component, under which the ASI uses for each year a stratified random sample for establishments hiring less than 100 workers.[9] The ASI by design excludes enterprises not registered under either the Factories Act 1948 or the Companies Act 1956, making it a dataset pertaining exclusively to formal enterprises.[10]

The ASI provides rich data on enterprise fixed capital, plant and machinery, raw materials, output, workers hired and wages paid. Additional information on loans and interest payments are also provided, although there is no information on the source of credit. The ASI included district identifiers between 1998 and 2007, while establishment identifiers were provided for the period between 1998 and 2014.

[8] Every bank in India is mandated to lend 40% of its overall credit to the priority sector. This rule is binding at the bank-level, but not at the district-level.

[9] Such establishments are typically surveyed once every 3 years.

[10] These two legal statutes governs the operations of registered enterprises in India.

The district identifiers allow us to determine whether an enterprise was located in an underbanked district. We use the procedure outlined in Martin et al. (2017) to construct our primary sample: a decade long establishment-level panel between 2001 and 2011, covering over 10,000 manufacturing establishments. As the BAP was initiated in 2005, this provides us with 4 years of data prior to the intervention, and 6 years post-intervention.

Our primary outcome of interest is capital expenditures, defined as the difference between the closing and opening values of an enterprise's plant and machinery in a year, scaled by the average value of the establishment's plant and machinery during the year. Specifically, for establishment i in year t, we define capital expenditures as:

$$Capex_{it} = \frac{Plant_{i,t} - Plant_{i,t-1}}{0.5 \times Plant_{i,t-1} + 0.5 \times Plant_{i,t}} \tag{3}$$

where $Plant$ is the establishment's value of plant and machinery, net of depreciation. The principal advantage with the formulation of capital spending in (3) is that the variable is bounded between -2 and 2, reducing sensitivity to outliers (Berton et al., 2018). We opt to focus on establishment plant and machinery as our measure of manufacturing capital as it captures the productive capital of a firm. We however confirm robustness of our baseline results to using establishments' aggregate fixed assets (inclusive of land and buildings, in addition to plant and machinery) as our measure of capital. In addition to capital investment, we also consider other outcomes such as, credit growth, output, workers hired, and productivity. All growth variables are defined as per equation (3). All nominal (INR) values are deflated to 2011 values using a wholesale price index deflator for manufacturing commodities and top-coded at the 1% level to limit the influence of outliers.

Table 1 presents summary statistics from the ASI data for our primary sample: namely establishments situated in districts located within a narrow window around the discontinuity threshold (Section 3.2 describes this in detail). Similar to most firm-level data, Table 1 documents a large right tail for a number of variables of interest. The average establishment had machinery (fixed assets), net of depreciation, equaling INR 29 (40) million, but the median establishment machinery (fixed asset) was INR 1 (3) million. Similarly, while the mean establishment size (workers hired) was 90, the median establishment size was 20. Based on administrative definitions, two-thirds of the establishments qualified as “micro” enterprises, while another quarter qualified as “small”.[11] Over 80% of the establishments satisfied the definition of small-scale industries, making them eligible for subsidized bank credit.[12] The median establishment age was 15 years.

Our primary outcome of interest – average annual capital expenditures during this period equaled -.002, – a net reduction in the value of plant machinery during the year. Its 75th percentile value was 0.03. This implies that the median establishment did not engage in capital spending during the year to offset the depreciation in the value of plant and machinery. Capital expenditures are inherently lumpy, and we define the binary variable $AnyCapex_{it}$ to equal 1 if the closing value of plant and machinery exceeded the opening values, or $Capex_{i,t} > Capex_{i,t-1}$. Attesting to the inherently lumpy nature of capital investments, we see that only a fourth (third) of the establishments engaged in any positive investment in plant and machinery (fixed assets), net of depreciation, in a given year.

While the ASI does not record the source of credit, it collects data on outstanding loans for establishments. Based on closing and opening values of outstanding establishment loans, we compute the average annual loan growth to be 4 percent or INR 1.2 million.[13] The median establishment however saw no loan growth while along the extensive margin, 38% of establishments had closing values of outstanding loans in excess of opening values, reflecting a net increase in outstanding credit. Despite being registered establishments, almost a fourth of the establishments had no outstanding credit during the year.[14] Entry into credit markets during the year was also limited – less than 3% of establishments reported having no outstanding credit at the beginning of the accounting period, but a positive loan balance at the end of the accouting year. Based on the reported interest expense during the year, we compute the cost of credit for the median establishment to be 14%, while the average cost of credit was 24%.[15]

[11] We use administrative definitions for classifying establishments as micro, small, medium and large enterprises. In 2005, establishments with plant and machinery worth less than INR 2.5 million were classified as micro-enterprises; between INR 2.5 and 5 million as small enterprises; between INR 5 and 10 as medium enterprises; and exceeding INR 10 million as large enterprises. We use pre-treatment maximum values of establishment plant and machinery to classify enterprises into these 4 categories.

[12] Small-scale enterprises are those whose investment in plant and machinery do not exceed INR 10 million.

[13] Average annual outstanding loans in this period equaled INR 27.9 million.

[14] We classify an establishment to have no outstanding credit if it reports no outstanding loans for both the opening and closing values in a year.

[15] We use the ASI data on annual interest expenses and scale it by opening value of outstanding loans to impute the rate of interest.

3.1.1 Basic Statistical Returns

We use data from the Basic Statistical Returns (BSR), hosted by the RBI, to assess the impact of the BAP on bank branches, deposits, loans, interest rates, non-performing assets, and staffing decisions. The publicly available version of the BSR annually aggregates the deposit and credit information from commercial bank branches at the level of district. The data is disaggregated by bank ownership and sectoral allocation of credit, allowing us to compare branch openings and credit disbursement across underbanked and non-underbanked districts, and also by bank group. To gauge new branch openings, we use publicly available information on commercial bank branch opening dates between 2001 and 2011. We also aggregate proprietary data at the branch-level on interest rates, employment, and non-performing loans to the level of district. Results using this data are discussed in Appendix A.

3.2 Empirical Strategy

The use of an arbitrary threshold – national average bank branch density – to classify districts as "underbanked" lends itself to the RD framework for identifying the causal impact of the branch expansion policy on manufacturing investment. We exploit the inherent time-variation in ASI's enterprise-level data to extend the RD design and apply a differences-in-discontinuity specification. The differences-in-discontinuity is akin to a standard differences-in-difference specification, with the inherent discontinuity in districts' classification as underbanked being the source of variation in assigning districts to treatment and control status. Conditional on the smoothness of pre-treatment covariates around the discontinuity threshold – critical to the RD design – the differences-in-discontinuity design ensures the ex-ante comparability of treatment and control groups in the neighbourhood of the discontinuity threshold. The strategy compares enterprise outcomes before and after the policy intervention, for enterprises operating in districts located within a narrow window around the national average bank branch threshold. Our primary estimating equation can be expressed as:

$$Y_{idt} = \alpha_i + \delta_t + \beta Underbanked_d \times Post_t + f(Runvar_d) + \gamma \mathbf{X}_{idt} + \epsilon_{idt} \qquad (4)$$

where Y is the outcome of interest for establishment i, located in district d, and observed in year t. α_i denotes establishment fixed effects, partialling out time-invariant establishment-level factors affecting the outcome of interest, while δ_t denotes year fixed effects. Our preferred specification uses industry-year (2-digit) fixed effects and compares establishment outcomes in the same broad industry category and year. $Underbanked_d$ is a dummy equaling 1 if establishment i is located in a district classified as "underbanked". The coefficient of interest is β, comparing establishment outcomes across underbanked and non-underbanked districts in the post-treatment period. Similar to the RD design, we include a linear polynomial in the running variable ($Runvar$), interacted with the post-treatment and underbanked indicators.[16] $\mathbf{X}$ includes a quadratic in establishment age, dummies for establishment ownership categories, and district covariates.[17] Standard errors are clustered by district, the level at which our treatment varies. Regressions are weighted with establishment-specific weights provided by the ASI.[18]

The quasi-random assignment of districts to underbanked status provides local variation in establishments' exposure to the treatment. We ensure the pre-treatment comparability of the treatment and control groups by restricting our sample to establishments in districts located within a bandwidth of 15 (bank branches per capita) around the discontinuity threshold. This bandwidth is selected using the optimal bandwidth calculation suggested by Calonico et al. (2020).[19] We also exhibit robustness of our coefficients to a range of bandwidths between 10 and 20 bank branches per capita.

A causal interpretation of β is subject to the standard assumption in a differences-in-difference specification: namely enterprise outcomes across underbanked and non-underbanked districts would have evolved comparably in the absence of the policy intervention. While the counterfactual is fundamentally untestable, we use an event-study framework to test whether outcomes of interest exhibited parallel trends across underbanked and non-underbanked districts prior to the policy intervention in 2005. Specifically, we estimate:

[16] Namely, we include $Runvar_d \times Post_t$ and $Runvar_d \times Underbanked_d \times Post_t$ in all our specifications. The presence of establishment fixed effects cause the main effects of $Runvar_d$ and its interaction with the underbanked indicator to be omitted from the specification.

[17] The covariates considered are population density; labour force participation and unemployment rate; fraction of self-employed, salaried and causal workers; fraction of workers employed in farm, manufacturing, trade, construction and services sectors; fraction of adults with secondary or higher education; fraction of rural population; gender ratio; fraction of Muslim population; logged per capita household consumption. As the policy intervention could have affected aggregate district outcomes through general equilibrium effects, we use district covariates observed in 2004, and interact them with a post-treatment indicator.

[18] The weights equal the inverse of the sampling probability. For establishments surveyed every year, the assigned weight is 1.

[19] In the absence of a prescribed method for computing the optimal bandwidth in differences-in-discontinuity designs, we use the optimal bandwidth computed by methodology of Calonico et al. (2020) when identifying the impact of the policy intervention on cumulative private bank branch openings in 2010. As this forms the "first stage" of policy intervention of interest, we opt to use this bandwidth for all our main specifications. For the sake of comparison, the optimal bandwidth used by Young (2017) to study the same policy intervention is 13, while Khanna and Mukherjee (2021) uses an optimal bandwidth of 20.

$$Y_{idt} = \alpha_i + \delta_t + \sum_{k=-5}^{5} \beta_k Underbanked_d \times \mathbb{1}(Year_{2006+k}) + f(Runvar_d) + \gamma \mathbf{X}_{idt} + \epsilon_{idt} \quad (5)$$

Specification (5) identifies a separate treatment effect corresponding to each year in the sample. The coefficients are benchmarked to the year 2005 – the year in which the BAP was announced. If establishment outcomes were comparable across underbanked and non-underbanked districts prior to the BAP, we would expect $\beta_j = 0 \; \forall \; k \in \{-5, ..., -2\}$

3.3 Pre-Treatment Covariate Balance

Before discussing our main findings, we empirically confirm that underbanked and non-underbanked districts were "balanced" along pre-treatment observable characteristics. This would substantiate the validity of the RD design, and attest to the comparability of treatment and control units. Appendix Figures B1 and B2 undertake covariate balance checks using pre-treatment district covariates based on the data collected by the NSS in 2004-05. These include demographic factors such as population, urbanization and education, as well as employment characteristics and household consumption. The running variable is plotted along the horizontal axis and each point depicts the mean of the district characteristic across 20 equally spaced bins of the running variable. The sample is restricted to districts within a bandwidth of 15 around the discontinuity threshold. Visually, there is no evidence of any discontinuity across the 18 covariates. The discontinuity estimates and the accompanying standard errors at the bottom of the individual figures also fail to detect any statistically significant jumps at the discontinuity threshold. Appendix Figures B3 and B4 replicate this exercise for pre-treatment establishment-level manufacturing outcomes of interest.[20] Similar to the aggregate district-level covariates, we find little evidence of divergence in manufacturing outcomes across underbanked and non-underbanked districts prior to the treatment.

Appendix Tables B1-B4 confirm these results using linear regressions. Specifically, we regress the observable characteristic of interest on the underbanked indicator, conditional on a linear polynomial in the running variable and state fixed effects.[21] Across all pre-treatment characteristics, we only find

[20] For manufacturing establishments, we collapse the pre-treatment data by computing within-establishment averages between 2000 and 2004.

[21] For the establishment-level regressions, we also include 2-digit industry fixed effects.

a statistically significant impact at the 10% level for the fraction of Muslims in a district. Collectively, Appendix Figures B1-B4 and Tables B1-B4 confirm that within a narrow window of the discontinuity threshold and prior to the policy intervention, a) underbanked districts were observationally equivalent to non-underbanked districts; and b) manufacturing outcomes were also statistically indistinguishable across these districts.

4 Results

We now present our key findings. We first show how the policy intervention affected capital spending by manufacturing establishments. We next explore the distributional aspects of the impact by examining which enterprises show the largest increase. Subsequently, we discuss mechanisms explaining the results, and explore aggregate effects.

4.1 Bank Branch Expansion and Manufacturing Investment

We use the differences-in-discontinuity design to identify the treatment's impact on manufacturing investment. We limit our focus to investments in plant and machinery, with the outcome of interest being defined as per equation (3). The sample is restricted to establishments located in districts within a bandwidth of 15 bank branches per capita around the discontinuity threshold. All specifications are weighted using establishment-specific weights and standard errors are clustered by district.

Column (1) of Table 2 shows a parsimonious specification including only establishment and year fixed effects, and identifies a near 5 percentage point increase in manufacturing investment, with the coefficient being statistically significant at the 5% level. Column (2) replaces the year fixed effects with 2-digit industry-year fixed effects with little impact on the point estimate. The industry-year fixed effects absorb industry-specific time-varying demand and productivity shocks common to all firms operating within the broad industry category and year. This effectively limits our comparison to establishments operating in the same broad industry and year, with the identifying variation arising from the districts' underbanked status. Column (3) includes establishment-specific covariates, while column (4) adds district covariates.[22] Column (5) includes age fixed effects, as opposed to a quadratic

[22] We consider the following establishment specific covariates: a dummy for whether the establishment is located in a rural area; a quadratic in establishment age; a dummy for whether the establishment uses computers for accounting; and 5 dummies for pre-treatment establishment size, interacted with a post-treatment indicator.

in establishment age, while column (6) replaces the 2-digit industry year fixed effects with 3-digit industry-year fixed effects, limiting our comparison to an even smaller set of establishments in each year. The inclusion of district covariates increases the coefficient slightly in magnitude, and also improves its precision to the 1% level.

Column (4) of Table 2 reports our preferred specification, with establishment and 2-digit industry-year fixed effects, and establishment and district covariates. The coefficient is large in magnitude, indicating that the average manufacturing establishment in underbanked districts saw a 6 percentage point increase in capital expenditures in the post-treatment period, when compared to manufacturing establishments in observationally equivalent non-underbanked districts. Relative to the pre-treatment mean of the dependent variable in non-underbanked districts, the coefficient implies an additional INR 1.8 million investment in plant and machinery for manufacturing establishments in underbanked districts in the post-treatment period.[23] This is economically large when considering that the median value of establishment plant and machinery in the pre-treatment period was INR 0.9 million in non-underbanked districts.

Appendix Table B5 considers alternate functional forms and outcomes of interest. Column (1) shows that our results are robust to measuring capital expenditures as the logged difference between closing and opening values of net plant and machinery. Section 3.1 acknowledged the lumpiness of capital spending and noted that only about a fourth of the establishments undertook any positive capital spending in a given year. To this effect, the outcome of interest in column (2) is an indicator variable equal to 1 if the closing value of establishment net plant and machinery was in excess of the opening value, and 0 otherwise. We find that the treatment increased establishments' likelihood of engaging in any positive capital spending by 5 percentage points – a 20% increase when considering that only 23 percent of establishments in non-underbanked districts undertook any positive spending in the pre-treatment period. With 57 manufacturing establishments operating in the pre-treatment period in the average non-underbanked district, this implies that approximately 3 additional establishments engaged in positive spending on plant and machinery in the post-treatment period in underbanked districts. Finally, columns (3)-(5) show that the results are comparable if we use net fixed assets to

[23] In the pre-treatment period, the mean establishment value of plant and machinery (average of net opening and closing values) equaled INR 29.8 million for establishments in non-underbanked districts, located within a bandwidth of 15 around the discontinuity threshold. A 6 percentage point increase thus amounts to INR 1.82 million higher spending on plant and machinery.

define establishment capital.

The results discussed till now causally identify a significant increase in capital spending by registered manufacturing establishments in underbanked districts while Appendix A.2 documented an increase in manufacturing credit from private banks in these regions. Appendix Table B6 directly identifies the policy intervention's impact on manufacturing credit at the establishment level. The outcome of interest in column (1) is credit growth, defined as in equation (3), while column (2) measures credit growth as the logged difference in closing and opening values of outstanding establishment loans. We identify a positive and statistically significant coefficient for both outcomes. Column (1) points to a 13 percentage point increase in credit growth for manufacturing establishments in underbanked districts – equivalent to INR 3.7 million. While the absence of data on the source of loans (see Section 3.1) precludes us from precisely attributing the increase in credit growth to private banks, the results are consistent with the overall increase in manufacturing investment in underbanked districts, as well as the increased disbursement of manufacturing credit by private banks in these districts (See Appendix A.2 for details). Columns (3)-(5) indicates that the increase in credit is primarily along the extensive margin, with little impact of the treatment on the likelihood of initiating a new loan during the year or entry into credit markets. Finally, column (6) finds no impact of the policy intervention on the cost of credit.

Section 3.2 noted that a causal interpretation of the differences-in-discontinuity estimate was subject to the identification assumption that manufacturing outcomes across underbanked and non-underbanked districts would have evolved comparably in the absence of the policy intervention. We assess the validity of this assumption using the event-study design in specification (5). To ensure consistency with our preferred baseline specification, we include 2-digit industry-year fixed effects, as well as establishment and district-level controls, in addition to establishments fixed effects. Standard errors remain clustered by district and the sample is restricted to establishments located in districts within a bandwidth of 15 around the discontinuity threshold.

Figure 3 depicts the event-study plots, with the coefficients benchmarked to the year 2005 – the year in which the BAP was unveiled – which forms the reference year. We plot coefficients for three outcomes of interest: capital expenditures, a dummy indicating positive capital spending during the year, and credit growth. For all three outcomes, we find no evidence of differential trends prior to 2005 across enterprises located in underbanked and non-underbanked districts. This supports the identifying assumption that manufacturing outcomes in underbanked and non-underbanked districts exhibited parallel trends prior to the treatment intervention. In the aftermath of the policy intervention, we identify a visible (albeit noisy) increase in the coefficient estimates for all three outcomes of interest. For instance, for capital expenditures, the coefficient estimates are positive and statistically significant at the 5% level for the years 2007 and 2009, while they are statistically significant at the 10% level for all years except 2010 (p-value .118). While we are unable to distinguish across sources of credit, the bottom panel of Figure 3 identifies a positive and significant increase in credit growth during the years 2009 and 2011, coinciding with the large increase in the disbursement of manufacturing credit from private banks. Collectively, the event-study plots confirm that manufacturing investment in underbanked districts increased after the treatment intervention, and was unaccompanied by any significant differential pre-treatment trends.

4.1.1 Robustness Checks

We subject our baseline results to a number of robustness checks. Columns (1) and (2) of Table 3 show that our results are unchanged if we do not weight the specifications with the establishment-specific weights (column 1); or two-way cluster by district and industry (column 2). Column (3) excludes the 9 districts for which the RBI did not precisely follow the assignment rule laid out in equation (1) and the results are unaffected by this sample restriction, although the coefficient is significant only at the 10% level.

All specifications discussed till now restricted the primary sample till 2011 as the RBI introduced a new branching policy in that year, encouraging banks to open branches in relatively small urban centres. Column (4) relaxes this restriction and uses data till 2014. We find that the treatment had a long-term impact on capital investments in underbanked districts, with the coefficient being comparable to the medium term effects identified in Table 2. Column (5) undertakes a placebo test by restricting the sample to 2005 – the year of introduction of the BAP – and defines the post-treatment period as starting from 2002.[24] The coefficient obtained using this pseudo-treatment is attenuated towards 0, and not statistically significant, assuaging concerns that the identified treatment effect can be attributed to an overall positive trend in manufacturing investment, coinciding with the timing of the policy intervention.

[24] This provides us with 4 years of pre-treatment data, and 3 years of post-treatment data.

Our primary sample covers establishments operating in districts located within a bandwidth of 15 (branches per million persons) around the discontinuity threshold. Figure 4 shows that our results are not sensitive to alternate bandwidth choices. Specifically, we re-estimate our baseline specification for bandwidths between 10 and 20 and plot the coefficients in Figure 4. For all the 20 bandwidths explored, the coefficient estimate is between .03 and .06, and only 3 out of 20 coefficients being statistically non-significant at the 10% level (the largest p-value being .13).[25] This affirms that our results are not dependent on any specific bandwidth. Importantly, the most conservative bandwidth of 10 identifies a 5 percentage point increase in manufacturing investment in underbanked districts, with the coefficient being significant at the 5% level.

Figure 5 shows that our baseline findings are not driven by any single state or industry. We establish this by re-estimating our baseline specification after dropping one state/industry at a time. As seen from both panels of Figure 5, the coefficients are not sensitive to the exclusion of any single state or industry – all the coefficient estimates remain positive, centred around 0.06, and statistically significant at the 10% level or better. This reassures us that the positive treatment effect on manufacturing investment was not driven by some confounding state or industry-specific place-based policy, the timing of which also coincided with the policy intervention of interest.

4.2 Distributional Impacts of Bank Branch Expansion

Our baseline results showed that districts witnessing an expansion in financial infrastructure saw higher capital investment and credit growth for manufacturing establishments. We now examine the distributional implications of the policy intervention to gauge whether the expansion of financial infrastructure in underbanked districts aided in the alleviation of credit constraints. This would happen if physical distance from financial institutions increases the costs of information acquisition and monitoring, hindering credit access for informationally opaque firms. Additionally, entrant private banks could be more effective in screening creditworthy borrowers. As small and young firms have limited public information and networks with financial institutions, credit constraints are most likely to bind for such firms. We thereby begin by exploring treatment heterogeneity by establishment size.

To avoid any contamination of establishment size by the policy intervention of interest, we compute

[25] The first coefficient in Figure 4 is estimated using a bandwidth of 10. Subsequent specifications are re-estimated by incrementally increasing the bandwidth by 0.5. The last specification uses a bandwidth of 20.

the size of each establishment using the average number of workers hired between 2001 and 2004 and classify establishments as "large" or "small" based on the median establishment size in the pre-treatment period (16 workers).[26] We then estimate the below specification:

$$Y_{ijdt} = \alpha_i + \delta_{jt} + \pi_1 Underbanked_d \times Post_t + \pi_2 Underbanked_d \times Large_i \times Post_t + f(Runvar_d) + \gamma \mathbf{X}_{ijdt} + \epsilon_{ijdt} \quad (6)$$

The unit of analyses is outcome Y for establishment i in industry j in time t. The double-difference coefficient corresponding to π_1 now compares capital investments across underbanked and non-underbanked districts for smaller establishments. The triple difference coefficient (π_2) identifies the differential effect on capital spending in underbanked districts for larger establishments.

Table 4 estimates treatment heterogeneity for both capital investment and credit growth. Columns (1) and (5) explore treatment heterogeneity across "large" establishments. In both instances, we identify a positive and significant coefficient on the double-difference π_1 term. The triple interaction (π_2) coefficient is negative, albeit statistically significant only for credit growth. We cannot reject the null of $\pi_1 + \pi_2 = 0$ for credit growth, indicating that larger manufacturing establishments saw no increase in credit growth, although they did exhibit higher capital spending.

Columns (2) and (6) further explores heterogeneity by establishment size across different cutoffs. The base category is establishments hiring less than 10 workers, and we subsequently use the thresholds of 25, 50 and 100 workers and identify differential effects within each set of establishments. Column (2) identifies a positive 6 percentage point but noisy (p-value .15) coefficient of the treatment effect on capital spending for the smallest set of establishments (less than 10 workers). The triple interaction coefficients corresponding to establishments hiring between 10 and 25, and 25 and 50 workers are positive, albeit imprecisely estimated. The triple interaction terms for the two largest group of establishments – 50 to 100 workers, and in excess of 100 workers are negative. The sum of the double and triple interaction terms significantly differ from 0 for establishments hiring between 10 and 25 workers (9.3 percentage point, p-value: .05), and 25 and 50 workers (11.1 percentage point, p-value: .06). Column (6) identifies a positive and significant impact on credit growth for the smallest

[26] Specifically, we use the pre-treatment median establishment size for establishments located in non-underbanked districts and within a bandwidth of 15 around the discontinuity threshold.

establishments, hiring less than 10 workers. The triple interaction coefficients are again negative, statistically significant only for the largest set of establishments. Overall, the coefficient estimates in columns (2) and (6) suggest that relatively smaller establishments, hiring less than 50 workers, drive the increase in manufacturing investment and credit growth in underbanked districts in response to the BAP, which facilitated financial deepening in these areas.

Columns (1)-(2) and (5)-(6) show that the increase in credit growth and capital spending was driven by relatively smaller establishments for whom credit constraints were also more likely to bind. However, while firm size is widely used as an indicator of credit-constraints, firm size is also endogenous to the firm: firms can choose to remain small either because it is optimal, or in response to market distortions. This is particularly relevant in the Indian context as firms qualify for subsidized credit only if they satisfy a certain size threshold. To this effect, we consider heterogeneity across the combination of establishment size and age. The intuition is that younger firms require time to scale up and are initially small due to operational and logistical constraints (including limited credit availability). Criscuolo et al. (2019) for instance showed that credit constraints were most likely to bind for firms which were both young, *and* small in size.

In columns (3) and (7) of Table 4, we split our sample of establishments into 4 mutually exclusive groups: small and young (omitted category); small and old; large and young; and large and old. We use the pre-treatment median establishment size to distinguish establishments as small or large. Establishments are classified as young if their operations started after 1992.[27] Consistent with Criscuolo et al. (2019), columns (3) and (7) show that the positive treatment effects are driven by establishments which are both small *and* young, with the triple interaction coefficients for both columns being negative.[28] Capital expenditures and credit growth for small and young establishments increased by 13 and 32 percentage points respectively in underbanked districts in the post-treatment period.

Farre-Mensa and Ljungqvist (2016) caution that firm size and age might be inaccurate measures of financial constraints and capture life-cycle effects of firms. They recommend using instead firms' listing status as the signal for being financially constrained.[29] In this regard, we exploit information

[27] We use this year as the cutoff as a major overhaul of the Indian economy was undertaken in 1991, encouraging private competition. Using this cutoff implies that establishments classified as young were at most 13 years old at the time of the policy intervention.

[28] When comparing the sum of the double and triple interaction coefficients, we identify a positive impact for capital spending (credit growth) for large and young (small and old) establishments.

[29] In Farre-Mensa and Ljungqvist (2016), being publicly unlisted is a necessary condition for being financially constrained, but not a sufficient condition.

in the ASI on enterprise organization and create the binary variable *Listed* if the establishment is classified as a public limited company. Consistent with credit constrained establishments increasing their capital investments in areas witnessing an expansion in financial infrastructure, columns (4) and (8) show that the positive treatment effects on capital investment and credit growth are driven entirely by establishments which are *not* publicly listed.

Columns (1)-(3) of Appendix Table B7 show that our results are very similar if pre-treatment establishment fixed assets are used to determine establishment size, instead of workers hired. In particular, columns (2) and (3) use administrative definitions based on the value of establishment plant and machinery to show that the treatment effects are concentrated amongst small establishments, and establishments qualifying as small-scale industries. This is in line with Banerjee and Duflo (2014), who document that a relaxation in the administrative cutoff for small-scale industries resulted in the alleviation of credit constraints. The point estimates in column (3) show that such establishments in underbanked districts increased their capital investments in plant and machinery by an additional INR 0.13 million. As seen from Appendix Table B8, the results are comparable when the outcome of interest is credit growth.

Column (4) of Appendix Table B7 considers heterogeneity by establishments' tangible assets. Tangible assets refer to the value of land and building owned by the establishment, often used as collateral to secure credit. If locational proximity of banks to borrowers in underbanked regions reduces the cost of information acquisition, we would expect the treatment effects to be concentrated amongst establishments with relatively low collateral values. Column (4) offers partial support for this hypothesis: while the triple interaction coefficient identifying treatment heterogeneity across establishments with high (above-median) ex-ante collateral values is not significantly different from 0, the uninteracted coefficient corresponding to establishments with low collateral is positive and statistically significant at the 5% level. This signifies that the availability of large collateral was not a necessary condition for undertaking capital investment by firms in these districts. Credit growth in underbanked districts too was driven by establishments with relatively low collateral values.

The ASI precludes the linking of establishments to parent firms but does provide broad ownership categories. We use these to determine the type of enterprises which responded most to the treatment. We use individual proprietorships and family-owned enterprises as our benchmark category and explore treatment heterogeneity across establishments classified as partnerships, private limited companies, government-owned/aided enterprises and public limited (listed) companies in column (5). Consistent with an increase in manufacturing investment amongst small enterprises in underbanked districts, we find the treatment effects to be driven by individual and family owned enterprises, as well as private limited companies. Finally, column (6) shows little evidence of heterogeneity across urban and rural enterprises.

Collectively, the findings discussed in this section show that the increase in manufacturing investments in underbanked districts was driven by smaller establishments, particularly, small and young establishments, and establishments which were not publicly listed. With the extensive literature studying the finance-growth nexus uncovering that these establishments are also most likely to face credit-constraints, the results support the explanation that the expansion in local financial infrastructure aided the alleviation of credit-constraints and allowed enterprises most likely to face binding credit-constraints to undertake investments in productive capital.

5 Mechanisms

The previous section showed that the increase in manufacturing investment in underbanked districts was undertaken primarily by small and young establishments for whom credit constraints were most likely to bind. We now consider five channels which can explain this increase in manufacturing investment in response to a regional expansion in financial infrastructure by firms that were most likely to be credit constrained: namely, increased physical proximity between lenders and borrowers; specialization of private banks in lending to small manufacturing borrowers; increased competition leading to a lower cost of credit; improved screening of borrowers by private banks; and aggregate demand.

5.1 Distance to Manufacturing Establishments

A key argument in favour of branch expansion is that screening and monitoring costs are an increasing function of distance. Physical proximity also allows lenders to collect "soft information" on borrowers, which is not easily substitutable (Nguyen, 2019). Consequently, the expansion of bank branches can reduce the physical distance between lenders and borrowers, easing creditors' costs of information collection and ex-post monitoring. Lower costs of financial intermediation through increased proximity to manufacturing establishments thereby can explain the increase in capital investment and credit.

The empirical challenge in establishing this channel is that while bank branch addresses are publicly listed, the ASI only provides location identifiers at the level of district, precluding the mapping of each enterprise to its nearest private bank branch. We overcome this in the following manner: first, we identify the impact of the policy intervention on private banks' average distance from a district's headquarters. If the average distance between district headquarters and private bank branches are significantly *larger* in underbanked districts, it would point to a wider dispersion of bank branches across the district. Next, we use the Prowess database maintained by the Centre for Monitoring the Indian Economy to confirm that a non-trivial mass of small manufacturing firms are sufficiently dispersed from district headquarters, implying that the distance between private bank branches and firms is likely to reduce in the event private banks are dispersed away from district headquarters.

We use publicly available information on bank branch locations to obtain the pincode of each bank branch.[30] Using the geo-coordinates of the pincodes, we compute the Euclidean distance between pincodes and district headquarters to gauge the spatial distribution of private bank branches in underbanked districts. If the new private banks that entered were spread across the district, we would expect the average distance to district headquarters to be higher in underbanked districts, relative to non-underbanked districts.

We first compare the average distance between bank branches and district headquarters across underbanked and non-underbanked districts in 2011 using a traditional RD design. Figure 6 identifies a large increase in the average distance of private bank branches to district headquarters, and the fraction of private bank branches located at a distance in excess of 20 kilometres. These relationships are unaffected across the discontinuity threshold for government-owned bank branches. Table 5 uses the differences-in-discontinuity design to show results consistent with the RD estimates: the average distance from private bank branches to district headquarters increased by 4.5-7 km in the aftermath of the policy intervention, and the average private bank branch also had a 23-30 percentage point higher likelihood of being located at a distance in excess of 20 km from district headquarters.[31] Relative to the pre-treatment mean in non-underbanked districts, the average private bank branch in underbanked districts was now located at a distance of 23-26 kilometres from district headquarters.

[30] The BSR only provides the physical addresses of branches currently in operation. Using this data, we are able to assign pincodes to over two-thirds of the branches covered in our sample till 2010 (74,435 out of 108,897 branches).

[31] In Table 5, we use two distinct samples for our estimation: in columns (1) and (3), the sample covers all districts. In columns (2) and (4), we restrict the sample to districts which had at least 1 private bank branch between 2002 and 2010. While the coefficient estimates in columns (1) and (3) are statistically insignificant, they are very similar in size to those estimated in columns (2) and (4), which are significant at the 10% level.

While the above results show that the policy intervention caused a higher dispersion of private bank branches within underbanked districts, this would affect lending outcomes through the physical proximity channel only if manufacturing establishments – especially smaller manufacturing establishments – were also dispersed away from district headquarters. In the absence of granular location identifiers in the ASI data, we rely on the Prowess database for suggestive evidence on firm locations. The Prowess is a financial database maintained by the Centre for Indian Economy which compiles information for firms with publicly available balance sheets. In addition to balance-sheet information, the Prowess provides location identifiers of firm headquarters in the form of pincodes. We use this information to construct the average distance of firms from district headquarters. As the Prowess over-samples large firms, we drop firms in metropolitan districts,[32] and show the kernel density distribution of firm locations in Figure 7 for relatively small and large manufacturing firms, operating between 2001 and 2007.[33]. As the Prowess has no information on the number of workers hired, we use a firm's average sales over this period as a measure of firm size, and report separately the average distance to district headquarters across large and small firms.[34]

Figure 7 shows that irrespective of firm size, the average distance of manufacturing firms to district headquarters is 18 kilometres, while the median distance is 9 kilometres. Over a fourth of the firms lie at distances in excess of 24 kilometres from district headquarters, and would be most likely to gain from the dispersion in private bank branches in response to the policy intervention. Combined with the increase in the average distance between private bank branches and district headquarters in underbanked districts, the results lend support to the hypothesis that the entry of private banks in underbanked districts indeed reduced the distance between lenders and select manufacturing firms. While we are unable to distinguish whether this reduced screening or ex-post monitoring costs, the increased proximity of lending institutions to manufacturing establishments does serve as a plausible explanation behind the increase in credit growth and capital investments in underbanked districts.

[32] We drop the districts corresponding to the metropolitan cities of Ahmedabad, Bangalore, Chennai, Hyderabad, Kolkata, Mumbai, Pune and districts located within the state of Delhi.

[33] It is not necessary for a firm's production to be located at the headquarters. However, if loans are taken on the firm's books, the location of the firm's registered headquarters is likely to form the local market for the bank, as opposed to its site of production

[34] The sample is restricted to manufacturing firms' balance sheets between the years 2001 and 2007. For each firm, the within-firm average sales are computed over this period, and subsequently, the median value of the respective distributions are used to classify firms as large and small.

5.2 Bank Specialization

We next explore if the specific attributes of entrant private banks can explain the increase in manufacturing investment in underbanked districts. We examine three hypotheses in this regard: first, if the district saw entry by a small private bank; second, if the district saw entry by a private bank specializing in lending to small borrowers; and third, if the district saw entry by a private bank specializing in lending to small manufacturing borrowers. Each of the above hypothesis essentially tests for comparative advantage in lender operations. Small banks might have informational advantages in lending to small borrowers. Given that the increase in capital spending in underbanked districts is driven by small and micro-enterprises, the entry of small banks could have increased lending to such firms. Similarly, if select private banks specialized in the issuance of small and micro credit, or had a comparative advantage in lending to small manufacturing units, the entry of such banks could have facilitated credit disbursement to relatively small manufacturing establishments.

We use lending data from the BSR between 2000 and 2005 to classify banks into the categories described above. We focus exclusively on private banks as only these banks responded to the policy intervention (see Appendix A). "Small" banks are those whose aggregate loan portfolio was less than the median loan portfolio during this period. Banks specializing in small loans are those where the average loan size was less than the median loan size across all banks.[35] Banks specializing in lending to small manufacturing enterprises are those which had a relatively high (above median) share of small borrowers within their manufacturing portfolio. Prior to examining heterogeneity across these characteristics, it is worth noting that due to the flexibility accorded in the BAP, the decision of banks to enter into select underbanked districts remained endogenous. Consequently, results from tests of heterogeneity using these above characteristics should be interpreted with caution.

Column (1) of Table 6 offers weak evidence on the first hypothesis. While the triple interaction term corresponding to the entry of small private banks is positive, we cannot reject the null of no differential effect. The sum of the coefficients is positive and significant at the 5% level (p-value .013), suggestive of a 6 percentage point increase in manufacturing investment in underbanked districts witnessing entry by a small private bank. We find no evidence supporting the second hypothesis [column (2)]: the triple interaction term corresponding to entry by private banks specializing in small

[35] The average loan size is computed as total outstanding loans, divided by total loan accounts.

loans is negative, large in magnitude, and statistically significant

Finally, the triple interaction coefficient in column (3) is positive and statistically significant, indicating that the increase in capital spending was undertaken by establishments located in districts which saw entry by banks with a relatively high share of small manufacturing loans. If this signals banks' comparative advantage in lending to small manufacturing units, it would be consistent with the hypothesis that capital investment increased in districts witnessing entry by private banks which specialized in lending to small manufacturing establishments. Moreover, the double-difference coefficient in column (4) is attenuated towards 0 and statistically insignificant, implying that the manufacturing investment in underbanked and non-underbanked districts remained comparable in the post-treatment period if the underbanked district did not witness entry by a private bank specializing in lending to small manufacturing borrowers.

Given the degree of flexibility accorded to banks in terms of selecting underbanked districts, a number of district-specific factors could be correlated with a small bank's decision to locate in a select district in response to the policy. The results in Table 6, hence, should be interpreted with caution. Nonetheless, columns (1) and (3) of Table 6 offer suggestive evidence that the operational specialization of entrant private banks in small manufacturing loans is a plausible explanation for the increase in credit growth and capital investments in underbanked districts among the small sized establishments. We however remain unable to distinguish whether this is due to improved screening or enhanced monitoring of borrowers by entrant private banks.

5.3 Cost of Credit

Private bank entry would be expected to increase competition amongst financial institutions in underbanked districts. Higher competition amongst lenders can facilitate financial intermediation, both through incumbents' incentive to preserve their market share, and a reduction in the cost of credit (Carlson et al., 2022). Thus, if higher private bank entry resulted in a reduction in the cost of credit through heightened competition, it can increase capital investments by reducing the marginal cost of capital. If the cost of credit was higher for smaller informationally opaque firms, it is possible that these firms gained disproportionately from a reduction in the costs of borrowing due to increased lender competition.

We test this channel using the proprietary BSR data to identify whether average lending rates declined in underbanked districts in the post-treatment period. Specifically, we compare lending rates charged by private and government banks across underbanked and non-underbanked districts.[36] We undertake this comparison for the year 2011 using a sharp RD specification as a number of underbanked districts had no private banks prior to 2005, resulting in the lending rate being undefined in such districts.

The last panel in the top row and the first panel in the middle row of Appendix Figure A10 graphically illustrate this, with the discontinuity estimate noted below. Across both private and government banks, we find no evidence of a reduction in lending rates across underbanked and non-underbanked districts. The discontinuity estimates are small and statistically insignificant. Thus, increased entry by private banks did not result in a reduction in rates of interest charged by creditors. A possible reason for this could be that private banks competed with government banks in these districts, which were charging a relatively low rate of interest prior to the policy intervention.

We also examine this channel using reported annual interest payments by registered enterprises in the ASI, which we use to impute lending costs for manufacturing establishments. Unfortunately, the ASI does not distinguish between credit from bank and non-bank sources, and only provides information on aggregate interest payments and lending by enterprises. Consistent with the district-level results, column (6) of Appendix Table B6 and Appendix Table B9 shows no evidence of a decline in lending rates due to the BAP. If anything, there's weak evidence suggesting that interest rates increased for smaller establishments. Thus, the increase in manufacturing investment and credit growth in underbanked districts due to bank branch expansion cannot be attributed to lower costs of borrowing in the face of heightened lender competition.

5.4 Improvements in Borrower Quality

Jayaratne and Strahan (1996) in their pioneering work on branch deregulation showed that the entry of new bank branches did not affect the overall volume of credit allocation, but instead improved the quality of credit intermediation. This is applicable in the current context, particularly in light of existing evidence documenting poor corporate governance in India's government-owned banks, leading to credit rationing, inefficient credit allocation, and increased risk of political capture (Banerjee et al.,

[36] The administrative BSR data provides information on the weighted average lending rate charged by each bank branch. The average lending rate in the district is computed as the mean across all branches, weighted by the volume of outstanding loans in each branch.

2004). Since the credit expansion in the aftermath of the BAP was driven by private banks, we would expect increased credit allocation towards better borrowers if enhanced corporate governance and superior technology of private banks improved the screening of creditworthy borrowers.

Similar to Jayaratne and Strahan (1996), we first proxy borrower quality using the share of non-performing loans in a district, and undertake a cross-sectional comparison across underbanked and non-underbanked districts. As delinquency is often a function of time, we compare the share of non-performing loans in March 2016 – a decade after the policy intervention. The bottom row of Appendix Figure A10 finds no difference in the share of non-performing loans across underbanked and non-underbanked districts at the discontinuity threshold. Thus, while we cannot infer that financial intermediation by private banks improved borrower quality in underbanked districts, it also rules out that the expansion in private bank credit in these regions primarily comprised of risky lending.

Next, we use ASI's establishment-level data to identify whether the increase in manufacturing investment was concentrated amongst productive establishments. While we cannot distinguish across sources of credit, an increase in capital expenditures by enterprises with relatively high ex-ante quality would be consistent with the explanation that private banks are more effective at allocating credit to high quality borrowers. It would also be consistent with the overall findings of Table 2, as we would expect an expansion of financial infrastructure to affect manufacturing investment only if firms had existing projects with net positive returns.

We use three pre-treatment measures of firm quality: namely marginal product of capital, value-added per worker, and pre-treatment interest rates. Interest rates are often used as a proxy for credit risk, with higher rates of interest signaling riskier firms. The results in Appendix Table B10 offer some support for the explanation that the increase in manufacturing investment in underbanked districts was driven by establishments of ex-ante higher quality. We identify a positive coefficient on the triple interaction term corresponding to establishments with higher marginal product of capital [column (1)], and a negative coefficient for establishments facing a higher ex-ante rate of interest [column (3)]. The double-difference coefficient for the latter is positive and significant at the 1% level, signifying that the increase in manufacturing investment in underbanked districts is driven by establishments which had relatively low costs of borrowing in the pre-treatment period. These results are consistent with the absence of any increase in non-performing loans for private banks in underbanked districts, suggesting that the interest rates faced by the establishments in this context do signal some information on credit risk. There is however no evidence of treatment heterogeneity across establishments with relatively high pre-treatment value-addition per worker [column (2)], although the sum of the coefficients are positive and significant at the 5% level, suggesting that capital investments did increase for these establishments in underbanked districts.

Collectively, columns (1)-(3) of Appendix Table B10 provide suggestive evidence for better screening of creditworthy borrowers by the private banks entering underbanked districts, without having to rely on collateral values (Appendix Table B7). When considering that the increase in capital spending was undertaken by establishments which faced lower rates of interest, but had higher marginal returns to capital, it again points to the presence of credit constraints, which were relaxed as private banks' entered these districts.

5.5 Aggregate Demand

The final channel considered is aggregate demand. This is particularly relevant when considering the findings of Young (2017), which shows that the same policy intervention increased farm productivity and night-lights measured economic activity. Thus, financial deepening could have boosted regional economic activity, resulting in higher local demand through general equilibrium effects, which in turn affected manufacturing investment by smaller firms.

To assess whether our results are explained by the aggregate demand channel, we explore treatment heterogeneity across tradable and non-tradable industries. If the increase in manufacturing investment is solely an upshot of higher local demand, we would expect the treatment effects to be driven by establishments operating in non-tradable industries. As the ASI lacks data on exports and imports, we rely on Mian and Sufi (2014) and use the geographic dispersion of industries to classify industries as tradable and non-tradable.[37] The intuition is that industries with wide geographic dispersion are more likely to be non-tradable.

Column (4) of Appendix Table B10 fails to identify treatment heterogeneity across establishments in industries with relatively low geographic dispersion (tradables): the point estimate is positive but the confidence intervals are sufficiently wide to rule out a significant effect. The double-interaction term

[37] We use data from the Economic Census of 2005 for this exercise. The Economic Census provides the total number of workers hired by every business establishment, irrespective of their registration status, allowing us to obtain aggregate estimates of employment at the industry-district level. We first compute the number of manufacturing workers employed in each 4-digit industry as a fraction of total manufacturing employment in the district. We next sum the square of these shares within industries to create an industry-level measure of geographic dispersion.

is positive and significant at the 10% level (p-value .057), although smaller in magnitude, implying that capital spending in underbanked districts did increase for establishments operating in non-tradable industries. The sum of the coefficients however is statistically significant at the 1% level, implying an increase in manufacturing investment amongst establishments operating in tradable industries too. Thus, while we cannot rule out the aggregate demand channel, the positive and statistically significant treatment effect for establishments operating in industries with low geographic dispersion indicates that the aggregate demand channel cannot be the only explanation for our findings.

6 Aggregate Effects of Bank Branch Expansion

Finally, we identify the aggregate effects of an expansion in financial infrastructure. Importantly, this allows us to identify the impact of bank branch expansions on firm entry, closures, and aggregate employment. As discussed in Section 3.1, our primary sample only includes establishments which were observed at least once before, and after the policy intervention. This limits our ability to identify the impact of the bank branch expansion on firm entry or exit using the establishment-level data. For instance, if the entry of private banks facilitated the entry of new establishments which increased their capital spending, our existing results would under-estimate the true impact of an increase in financial infrastructure on manufacturing investment.

We examine this by collapsing the establishment-level data at the district-industry (3-digit) level and use the following specification:

$$Y_{jdt} = \alpha_d + \delta_{jt} + \beta Underbanked_d \times Post_t + f(Runvar_d) + \gamma \mathbf{X}_{dt} + \epsilon_{jdt} \tag{7}$$

The unit of observation in equation (7) is the district-industry (3 digit-level) in district d in industry j in time t. We continue to use our difference-in-discontinuity design. The establishment fixed effects are replaced with district fixed effects, absorbing district-specific time-invariant factors affecting the outcomes of interest. We include industry-year fixed effects (3-digit industry), limiting our comparison to outcomes within similar industry groups in the same year, with the identifying variation arising from changes in districts' underbanked status. The sample is restricted to districts within a bandwidth of 15 from the discontinuity threshold and standard errors are clustered by district.

Columns (1)-(3) of Table 7 replicate the results for our three main outcomes of interest – capital expenditures, fraction of establishments undertaking any positive capital spending, and credit growth. Capital expenditures is limited to plant and machinery The aggregate district-level findings are consistent with the establishment-level results: there is a 5.4 percentage point increase in capital investments, and a 3.3 percentage point increase in the fraction of establishments engaging in some positive capital spending. This equals an aggregate INR 18 million increase in capital investments for establishments in underbanked districts in the post-treatment period.[38] Credit growth too increased by 10 percentage points, and the coefficient is significant at the 5% level, signifying an increase in outstanding loans in underbanked districts, equivalent to INR 33 million.

Columns (4)-(6) consider total output, employment and revenue productivity. While the point estimates are positive, the confidence intervals are too wide to rule out a null effect. For employment, the coefficient suggests a 12 percent increase in workers hired, although the coefficient is only significant at the 15% level (p-value: .141). Relative to the pre-treatment mean in non-underbanked districts though, this amounts to an additional 120 workers employed in the average industry in underbanked districts. Finally, while column (8) does not identify any impact on the fraction of establishments being closed, column (7) identifies a positive and significant effect on the total number of establishments in operation. In the absence of establishment closures, an increase in total establishments points to higher entry in underbanked districts in response to the policy intervention. Again, compared to the dependent variable mean in non-underbanked districts, the coefficient signifies 2 additional establishments operating in underbanked districts in the post-treatment period. Overall, Table 7 shows that the policy intervention resulted in higher aggregate manufacturing investment and credit growth, accompanied by increased entry of manufacturing establishments in underbanked districts. The results also suggest higher manufacturing employment in response to an expansion in financial infrastructure and manufacturing credit in these districts.

7 Conclusion

Using firm level panel data from registered manufacturing sector enterprises in India and exploiting a bank branch expansion policy in 2005 that led to an increase private bank branches and credit in underbanked districts, we find an increase in capital investment and credit growth by manufacturing

[38] The average value of plant and machinery within a 3-digit industry in INR 328 million. A 5.4 percentage point increase equals .054*328.96621 = 17.76 million.

firms located in these districts. Smaller and young firms, which are more likely to be credit constrained lead this increase. We provide suggestive evidence in support of better screening by private banks and rule out the channel of increased aggregate demand as the only factor behind the observed results. These results show that a reduction in physical proximity to banking institutions can reduce frictions in the credit market for the credit constraint firms by lowering information and monitoring costs. Thus, increased access to banking has distributional consequences with smaller firms, having higher returns to capital, benefiting more from increased access to bank branches.

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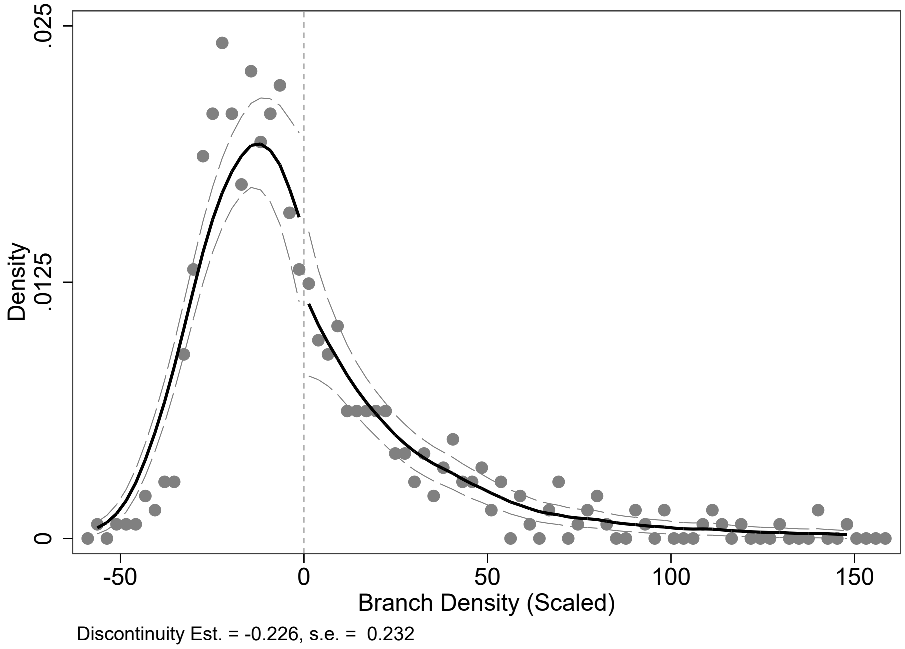
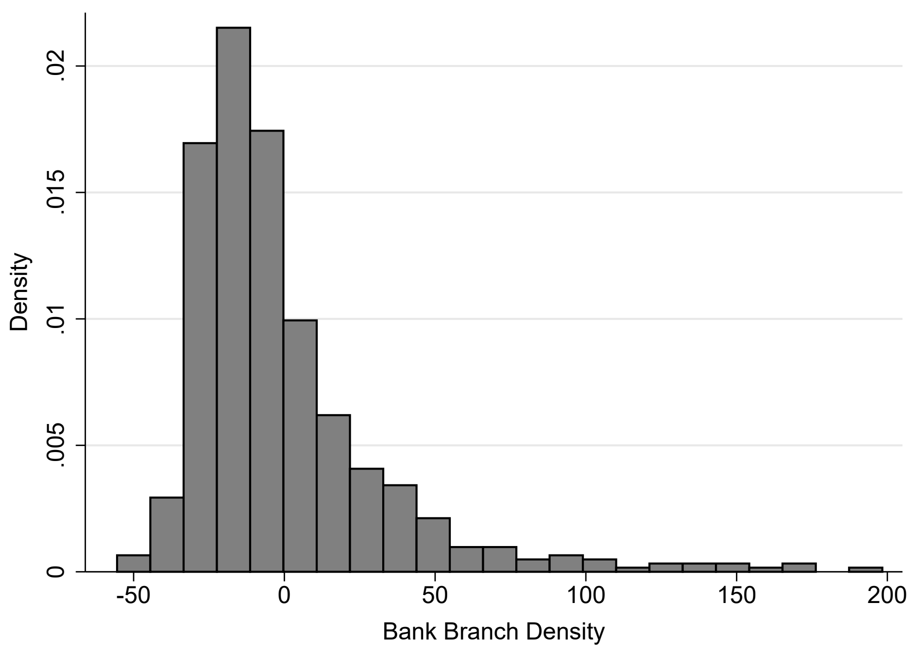
Figure 1: Selection of Districts Into Underbanked Status: McCrary Test



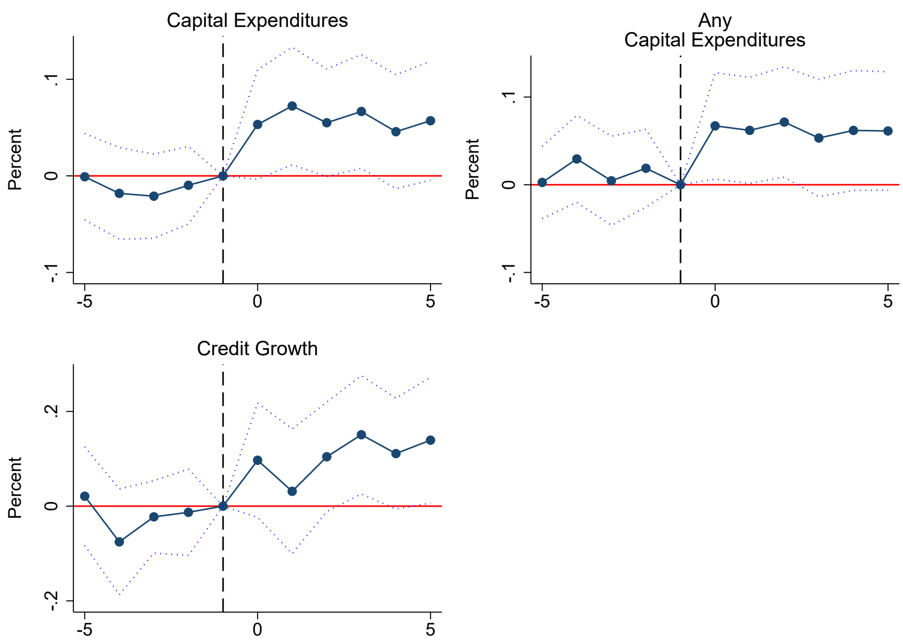
Notes: The above figure tests for bunching of the running variable around the threshold of 0 using the McCrary test. The solid line shows the local polynomial estimate, while the dashed lines show the 95% confidence intervals. The figure is replicated from Chowdhury and Ritadhi (2022).

Figure 2: Distribution of Running Variable



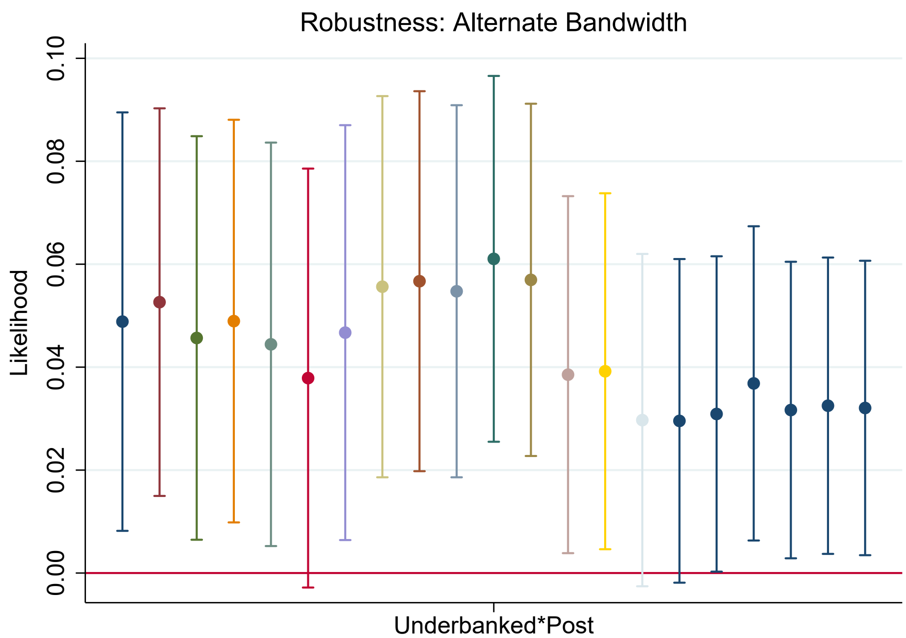
Notes: This figure shows the distribution of the running variable where the running variable is defined at the district level as $Runvar_d = BranchPC_d - \overline{BranchPC}$. $BranchPC$ refers to the bank branch density in district d in 2005, while $\overline{BranchPC}$ is the national average bank branch density in 2005. Districts are classified as "underbanked" if $Runvar_d < 0$ – located to the left of the threshold 0.

Figure 3: Capital Expenditure in Underbanked Districts: Event-Study Plots



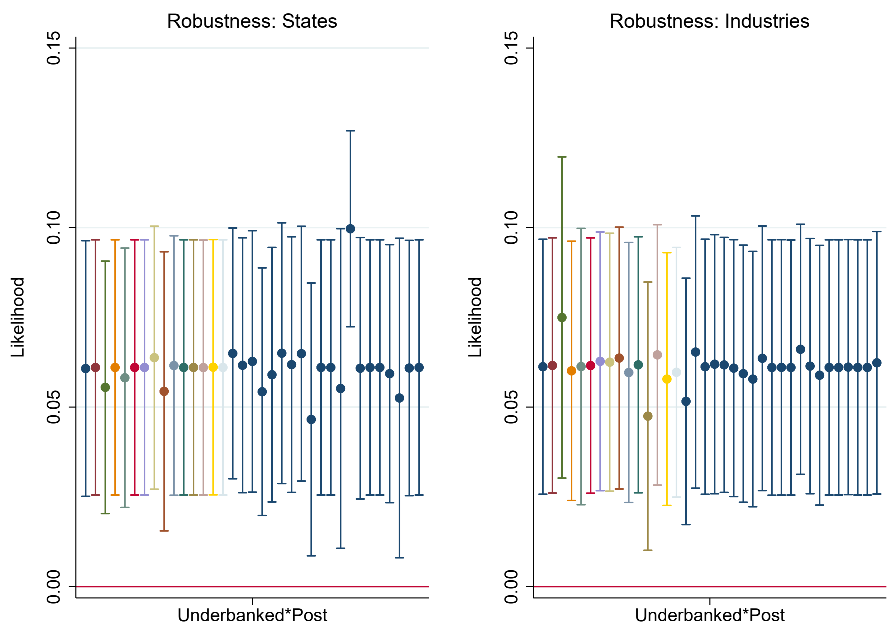
Notes: The above figure presents event-study plots estimating capital expenditures, any capital expenditures, and credit growth in underbanked districts. The unit of observation is the manufacturing establishment. The outcome of interest in the top-left panel is capital expenditures; in the top-right panel, any positive capital spending; in the bottom-right panel, credit growth. Capital expenditures refer to investment in plant and machinery. The solid line represents the average annual treatment effects, and the dashed lines denote the 95% confidence intervals. The treatment effects are benchmarked to the year 2005 (dashed vertical line) – the year in which the treatment is initiated. All specifications include district and industry-year fixed effects, a linear polynomial in the running variable, establishment and district covariates. All specifications are weighted using establishment-specific weights. The sample in each instance is restricted to districts within a bandwidth of 15 around the discontinuity threshold. Standard errors are clustered by district.

Figure 4: Manufacturing Investment in Underbanked Districts: Robustness to Alternate Bandwidths



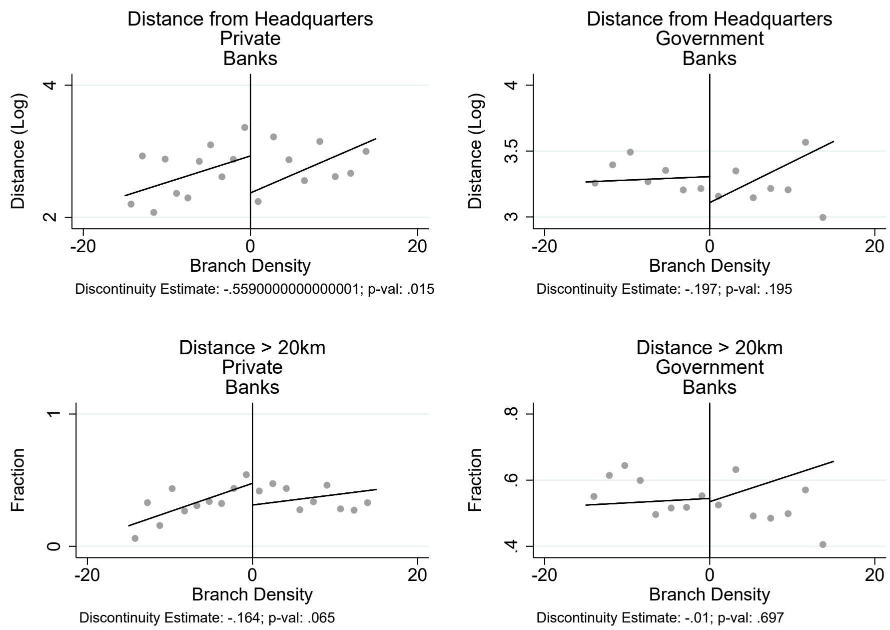
Notes: The above figure shows the robustness of the baseline results to alternate bandwidths. The unit of observation is the manufacturing establishment and the outcome of interest is capital expenditures (investment in plant and machinery). All specifications include district and industry-year fixed effects, a linear polynomial in the running variable interacted with the district underbanked indicator and a post-treatment indicator, establishment covariates, and pre-treatment district covariates interacted with a post-treatment indicator. All specifications are weighted using establishment-specific weights. The sample in the first specification estimated is restricted to districts within a bandwidth of 10 around the discontinuity threshold; subsequent specification estimates increase the bandwidth by 0.5, till the final specification, which equals a bandwidth of 20 around the discontinuity threshold. The vertical lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure 5: Manufacturing Investment in Underbanked Districts: Robustness to Dropping Individual States and Industries



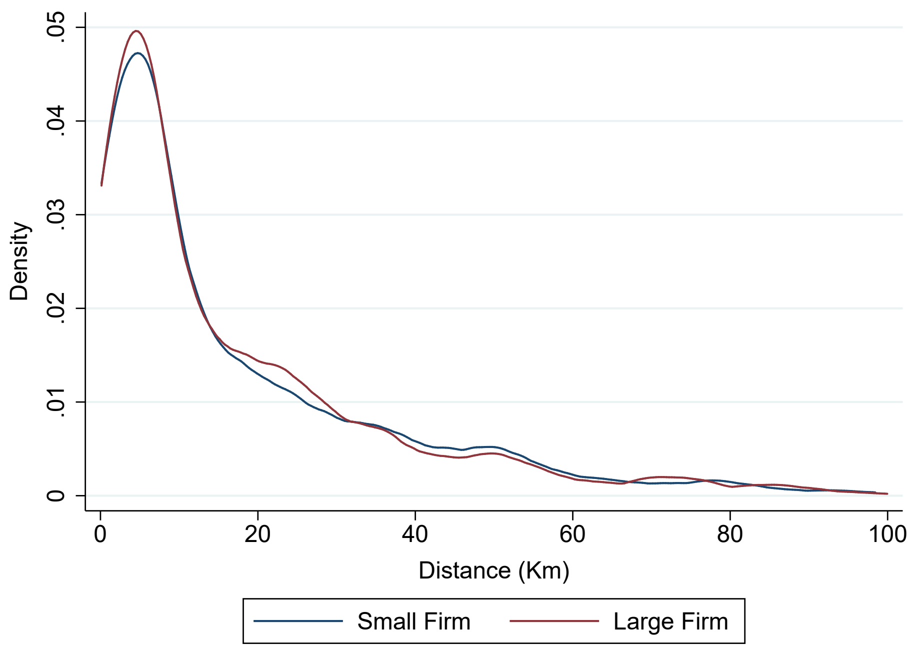
Notes: The above figure shows the robustness of the baseline results to the dropping of individual states and industries. The unit of observation is the manufacturing establishment and the outcome of interest is capital expenditures (investment in plant and machinery). All specifications include district and industry-year fixed effects, a linear polynomial in the running variable, establishment covariates, and district covariates. All specifications are weighted using establishment-specific weights. Specifications are estimated by dropping one state (two-digit industry) at a time. The vertical lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure 6: Bank Branch Distance to District Headquarters



Notes: The above figure shows the average distance between district headquarters and bank branches in a district in 2011. The outcome of interest in the top-panel is the (logged) average Euclidean distance of bank branches to district headquarters; in the bottom panel, the fraction of bank branches located in excess of 20 kilometres from district headquarters. The right-hand figures refer to government owned bank branches; the left hand figures, private bank branches. All specifications include state-region fixed effects and district covariates. Regression discontinuity estimates and robust standard errors, clustered by district, and computed using the method proposed by Calonico et al. (2020) are shown at the bottom of each panel.

Figure 7: Manufacturing Firms' Distance to District Headquarters



Notes: The above figure shows the average distance between manufacturing firms and district headquarters between 2001 and 2007, using the Prowess database. Firm sales is used to classify firms as "large" (above median) and "small" (below median). The kernel density plots are based on a sample of 3,000 manufacturing firms.

Table 1: Summary Statistics: Manufacturing Establishments

	N	Mean	SD	P25	P50	P75
Capital Expenditures	70285	0.023	0.328	-0.129	-0.066	0.060
Any Capital Expenditure	70285	0.318	0.466	0.000	0.000	1.000
Capital Expenditures – Machinery	69071	-0.002	0.398	-0.162	-0.105	0.030
Any Capital Expenditure – Machinery	70285	0.269	0.443	0.000	0.000	1.000
Loan Growth	54066	0.042	0.753	-0.207	0.000	0.266
Any Loan Growth	69891	0.383	0.486	0.000	0.000	1.000
New Loan	70285	0.025	0.157	0.000	0.000	0.000
No Loan	69891	0.226	0.418	0.000	0.000	0.000
Interest Rate	54046	0.244	0.280	0.071	0.144	0.282
Fixed Assets (INR)	70285	40.335	124.906	0.799	3.241	15.985
Plant and Machinery (INR)	70285	28.679	102.425	0.195	1.204	8.082
Raw Materials (INR)	57602	15.970	45.519	0.618	2.459	8.999
Land and Buildings (INR)	70285	11.709	34.696	0.203	1.120	5.353
Assets (INR)	69890	127.688	383.564	4.284	14.663	60.823
Loans (INR)	69891	27.442	92.475	0.096	2.076	10.479
Hired Workers	70285	89.372	481.087	8.000	20.000	63.000
Contract Workers	70240	26.975	354.670	0.000	0.000	5.000
Supervisors	70240	10.029	81.256	1.000	2.000	6.000
Salaries – Hired Workers (INR)	70285	4.693	11.952	0.298	0.829	2.921
Salaries – Contract Workers (INR)	70240	0.940	3.069	0.000	0.000	0.174
Salaries – Supervisor (INR)	70240	2.442	7.487	0.057	0.235	1.119
Output (INR)	70285	182.752	480.885	5.877	23.301	109.496
Value-Addition (INR)	70285	35.866	106.813	1.216	3.850	16.743
Value-Addition (Share of Assets)	70285	0.820	2.612	0.186	0.322	0.570
Rural	70285	0.438	0.496	0.000	0.000	1.000
Computer Use	70285	0.592	0.492	0.000	1.000	1.000
Age	70285	17.841	14.709	8.000	14.000	23.000
Young Establishment	70285	0.449	0.497	0.000	0.000	1.000
Micro-Enterprise	70050	0.649	0.477	0.000	1.000	1.000
Small Enterprise	70050	0.272	0.445	0.000	0.000	1.000
Medium Enterprise	70050	0.029	0.168	0.000	0.000	0.000
Large Enterprise	70050	0.050	0.218	0.000	0.000	0.000
Small-Scale Industries	70050	0.808	0.394	1.000	1.000	1.000

Notes: This table shows the summary statistics for registered manufacturing establishments. The sample is restricted to . establishments situated in districts located within a bandwidth of 15 around the discontinuity threshold. Rupee values are in real using 2005 prices (in millions INR). Growth variables are defined as in equation (3). Micro, small, small-scale, medium and large enterprises are defined according to administrative definitions.

Table 2: Manufacturing Investment in Underbanked Districts

	(1)	(2)	(3)	(4)	(5)	(6)
			Capital Expenditures			
Underbanked × Post	.046**	.043**	.046**	.061***	.062***	.061***
	(.019)	(.020)	(.020)	(.022)	(.022)	(.022)
Observations	71542	71542	71542	71542	71536	71522
R^2	.37	.38	.38	.38	.38	.39
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Industry-Year FE	N	Y	Y	Y	Y	Y
Firm Controls	N	N	Y	Y	Y	Y
District Controls	N	N	N	Y	Y	Y
Age FE	N	N	N	N	Y	N
Control Mean	-.031	-.031	-.031	-.031	-.031	-.031

Notes: This table identifies the treatment effect on manufacturing investment. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures, defined as in (3). Capital expenditures is restricted to capital expenditures in plant and machinery. All specifications include establishment fixed effects, and a linear polynomial in the running variable. Column (1) includes establishment and year fixed effects. Columns (2)-(5) replace year fixed effects with 2-digit industry-year fixed effects, while column (6) considers 3-digit industry year fixed effects. Column (3) includes establishment specific covariates while column (4) additionally includes district covariates. Column (5) replaces the quadratic in establishment age with age fixed effects. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 3: Manufacturing Investment in Underbanked Districts: Robustness and Placebo Checks

	(1)	(2)	(3)	(4)	(5)
			Capital Expenditures		
	Unweighted	District Cluster	Exclude Districts	Long Term Effects	Placebo
Underbanked × Post	.037**	.061**	.045*	.061***	
	(.016)	(.025)	(.027)	(.021)	
Underbanked × Post 2001					-.003
					(.030)
Observations	71542	71484	68648	85633	38813
R^2	.26	.38	.38	.35	.47
Control Mean	-.019	-.031	-.038	-.017	-.023

Notes: This table shows robustness of the treatment effect on manufacturing investment to alternate specifications and placebo tests. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures, defined as in (3). Capital expenditures is restricted to those in plant and machinery. All specifications include establishment, 2-digit industry-year fixed effects, a linear polynomial in the running variable, establishment and district covariates. Column (1) excludes establishment weights; column (2) clusters the standard errors by district only; column (3) excludes the 9 districts for which the underbanked rule was violated; column (4) extends the sample till the year 2014; column (5) restricts the sample to the years between 1998 and 2005 and considers the period after 2001 to comprise of the post-treatment period. All specifications except column (1) include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district, with the exception of column (2), where they are twoway custered by district and industry. Significant levels: *10%, **5%, and ***1%

Table 4: Manufacturing Investment and Credit Growth in Underbanked Districts: Heterogeneity by Establishment Size and Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Capital Expenditures				Credit Growth			
Underbanked × Post	.068**	.058	.131***	.077***	.242***	.261***	.320***	.143***
	(.033)	(.040)	(.047)	(.023)	(.071)	(.079)	(.094)	(.047)
Underbanked × Est. *Size* > Median × Post	-.012				-.187**			
	(.035)				(.083)			
Underbanked × 10 > Est. *Size* ≤ 25 × Post		.035				-.154		
		(.061)				(.111)		
Underbanked × 25 > Est. *Size* ≤ 50 × Post		.053				-.224		
		(.062)				(.161)		
Underbanked × 50 > Est. *Size* ≤ 100 × Post		-.072				-.132		
		(.064)				(.134)		
Underbanked × Est. *Size* > 100 × Post		-.028				-.183**		
		(.050)				(.078)		
Underbanked × Large, Young × Post			-.027				-.276**	
			(.055)				(.129)	
Underbanked × Large, Old × Post			-.105**				-.254**	
			(.052)				(.098)	
Underbanked × Small, Old × Post			-.114**				-.129	
			(.050)				(.117)	
Underbanked × Listed × Post				-.136***				-.121
				(.037)				(.098)
Observations	71542	71542	71542	71542	53666	53666	53666	53666
R^2	.38	.38	.38	.38	.34	.34	.34	.34
Control Mean	-.031	-.031	-.031	-.031	.043	.043	.043	.043

Notes: This table identifies heterogeneity in the impact of financial infrastructure on manufacturing investment across credit constrained establishments. The unit of observation is the manufacturing establishment. The outcome of interest in columns (1)-(4) is manufacturing investment; in columns (5)-(8), credit growth. Growth variables are defined as in equation (3); manufacturing investment is restricted to investments in plant and machinery (defined as in equation 3). All specifications include establishment and industry-year fixed effects, a linear polynomial in the running variable, and establishment and district covariates. *Median* refers to the median establishment size (*Est.Size*); *Est.Size* refers to establishment size in the pre-treatment period, defined as the number of workers hired. *Large* and *Small* refer to establishments with above and below median sizes. *Young* refers to establishments which started operations after 1992. *Listed* refers to establishments which are publicly listed. All specifications are weighted using establishment-specific weights. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 5: Average Bank Branch Distance to District Headquarters

	(1)	(2)	(3)	(4)	(5)	(6)
	Private Banks				Government Banks	
	Distance (Log)	Distance > 20	Distance (Log)	Distance > 20	Distance (Log)	Distance > 20
Underbanked × Post	.245	.359*	.067	.104*	-.011	-.010
	(.195)	(.202)	(.060)	(.060)	(.013)	(.008)
Observations	1398	891	1398	891	2040	2040
R^2	.86	.88	.89	.92	.99	.99
Control Mean	18.73	19.38	.29	.30	27.42	.53

Notes: This table shows the impact of private bank branch expansion on the distance between bank branches and distance headquarters. The unit of observation is the district. The outcome of interest in columns (1), (3), and (5) is the logged average distance between district headquarters and bank branches; in columns (2), (4) and (6), the fraction of bank branches in the district located at distances exceeding 20 kilometres. Columns (1)-(4) restrict the sample to private bank branches; columns (5) and (6), government bank branches. Columns (2) and (4) restrict the sample to districts with at least 1 private bank branch for all years between 2003 and 2010. All specifications include district and year fixed effects, in addition to district-specific covariates. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 6: Manufacturing Investment in Underbanked Districts: Heterogeneity by Private Bank Characteristics

	(1)	(2)	(3)
		Capital Expenditures	
Underbanked × Post	.043	.146***	.022
	(.035)	(.044)	(.028)
Underbanked × Small Bank × Post	.020		
	(.045)		
Underbanked × Small Loans × Post		-.103**	
		(.051)	
Underbanked × Small Mfg. × Post			.118***
			(.039)
Observations	71542	71542	71542
R^2	.38	.38	.38
Control Mean	-.03	-.03	-.03

Notes: This table identifies treatment heterogeneity on manufacturing investment across bank characteristics. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures, defined as in equation (3). Capital expenditures include spending on plant and machinery. All specifications include establishment, 2-digit industry-year fixed effects, a linear polynomial in the running variable, establishment and district specific covariates. $SmallBank$ is a dummy equaling 1 if the district saw entry by a private bank whose total loan portfolio was relatively small (less than median); $SmallLoan$ is a dummy equaling 1 if the district saw entry by a private bank whose average loan size is relatively small (less than median); $SmallMfg$ is a dummy equaling 1 if the district saw entry by a private bank which had a relatively high (above median) share of small manufacturing borrowers; All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table 7: Aggregate Effects of Bank Branch Expansion

	(1) Capital Expenditure	(2) Any Capital Expenditure	(3) Credit Growth	(4) Output (Log)	(5) Workers (Log)	(6) Revenue TFP	(7) Total Establishments (Log)	(8) Fraction Closed
Underbanked×Post	.054**	.033	.103**	.018	.121	.032	.140**	-.032
	(.024)	(.023)	(.045)	(.087)	(.082)	(.040)	(.057)	(.026)
Observations	17962	18152	15908	18015	17898	16128	19388	19388
R^2	.07	.12	.06	.36	.35	.10	.33	.19
Control Mean	.03	.28	.07	1586.71	.97	-.03	14.59	.07

Notes: This table estimates the impact of financial infrastructure on aggregate district-level outcomes. The unit of observation is the district. The outcomes of interest in columns (1) and (3) are constructed using equation (3). The outcomes of interest in columns (2) and (8) are fractions. The remaining outcomes of interest are log transformed. Capital expenditures refer to expenditures in plant and machinery. All specifications include district and 3-digit industry-year fixed effects, in addition to district-specific controls. Standard errors are in parentheses, clustered by district.
Significant levels: *10%, **5%, and ***1%

A Branch Authorisation Policy and Banking Outcomes

We document the impact of the Branch Authorisation Policy (2005) on major banking outcomes such as bank branch openings, deposits, credit, cost of credit and non-performing assets. We also consider outcomes such as staffing of bank branches in hitherto under-served regions.

A.1 Bank Branch Openings

We begin by assessing whether the BAP incentivized banks to open branches in underbanked districts – the primary goal of the policy. The BAP made the approval of banks' annual expansion plans conditional on their level of financial intermediation in underbanked districts. At the time of policy announcement, over 45% of districts (60% of districts classified as underbanked) in India had no private bank branch, impying limited operations in these districts. Government-owned banks, owing to the push during the social banking era, however, had commercial branches across all districts. Consequently, while the policy did not specifically target any bank group, we would expect private banks to be more responsive to the policy, owing to their limited operations in these regions prior to 2005. The empirical evidence in the following sections supports this assertion.

We begin by comparing cumulative private bank branch openings between 2006 and 2011 across underbanked and non-underbanked districts. To exploit the RD design inherent in the policy, we restrict our sample to districts located within a narrow bandwidth around the discontinuity threshold, using the methodology of Calonico et al. (2020). The top left panel of Appendix Figure A1 conditions only on state-region fixed effects and identifies a discontinuous drop in (logged) cumulative private bank branch openings at the threshold 0, where a district switches from being underbanked to non-underbanked. The discontinuity estimate (noted below each figure) is statistically significant at the 5% level and suggests a near 70% increase in private bank branch openings in underbanked districts over this period. The reported p-values correspond to robust standard errors computed as per Calonico et al. (2020), and clustered by state-region. The top right panel includes pre-treatment district covariates while the figure in the bottom panel considers a quadratic fit. The inclusion of district covariates does not affect the discontinuity estimate (top right panel, A1), or its statistical precision. The quadratic fit reduces the discontinuity estimate and the coefficient is now significant only at the 15% level (p-value .15). Overall, we find that private bank branch openings increased by 38-74 percent in underbanked districts between 2006 and 2011. The findings echo those of Young (2017) and Khanna and Mukherjee (2021), who also exploit this policy in their respective papers. While Cramer (2022) does not distinguish across bank ownership, she too finds an increase in overall bank licenses issued and bank branch openings in underbanked districts.

Two placebo tests suggest that this increase in private bank branch openings is not driven by either a) prior trends in private bank banch openings; or b) an overall expansion of financial infrastructure in underbanked districts. The top row of Appendix Figure A2 shows that cumulative private bank branch openings between 2001 and 2005 were comparable across underbanked and non-underbanked districts. The bottom row of Appendix Figure A2 compares cumulative government-owned bank branch openings between 2006 and 2011. We find no evidence of any discontinuity at the threshold when conditioning solely on state-region fixed effects (bottom-left panel). The coefficient estimate increases upon the inclusion of district covariates as is weakly significant (p-value of .074) (bottom-middle panel). In subsequent specifications, we identify no significant increase in government bank branches in underbanked districts, assuaging concerns that the sharp increase in private bank branches in these regions stems from a secular increase in financial infrastructure.

The average non-underbanked district witnessed approximately 12 private bank branch openings between 2006 and 2011. Based on the discontinuity estimates, this implies that the BAP resulted in the opening of 5-9 additional private bank branches in the average underbanked district. The treatment effect is economically significant when considering that underbanked districts on average saw only 2 private bank branch openings between 2001 and 2005. Compared to the "social banking" era, a conservative back of the envelope calculation suggests that the BAP's impact was approximately a third of the impact of the state-driven push to expand banking infrastructure in rural unbanked locations.[1]

We next use the differences-in-discontinuity specification described in equation (4) to identify the BAP's impact on new bank branch openings. We again exploit the inherent discontinuity in districts' assignment to underbanked status to construct "treatment" (underbanked) and "control' (non-underbanked)' groups around the discontinuity threshold. The differences-in-discontinuity specification allows us to exploit the panel characteristics of the bank branch data and flexibly control for time-invariant trends in local financial infrastructure. In particular, as banks were accorded significant leeway to select locations within the set of underbanked districts, a district-level panel permits the use of district fixed effects to control for time-invariant unobserved district characteristics which can influence the location of financial infrastructure.

Column (1) of Appendix Table A1 includes only district and year fixed effects, and a linear polynomial in the running variable, and identifies a positive treatment effect on logged private bank branch openings. The coefficient is stable to the inclusion of district covariates and suggests a 22 percent [column (2)] increase in private bank branch openings in the average underbanked district in the post-treatment period. Columns (3)-(4) show the robustness of the point estimates to an inverse hyperbolic sine transformation of the dependent variable, accounting for the large number of districts with no new branch openings in a year.[2] The results if anything become stronger with using the inverse hyperbolic sine transformation. Lastly, columns (5)-(8) identify a null effect on government bank branch openings – the coefficients are small, attenuated towards 0, and statistically non-significant. This is consistent with our expectation that private banks would be most likely to respond to the policy incentive outlined by the RBI, owing to their limited prior presence in underbanked districts, as opposed to government-owned banks. It also assuages concerns that the increase in private bank branches in underbanked districts can be explained by an overall convergence effect, leading to aggregate financial

[1] Burgess and Pande (2005) notes that around 30,000 bank branches were opened over a 20 year period between 1969 and 1990. This equates to approximately 84 new branches opened per district over a 2 decade period. The number needs to be interpreted with caution as a number of these districts were subsequently divided into smaller districts during the 1990s, so the aggregate average effects of the social banking program most likely reflect an upper bound.

[2] Our main specification uses a log-transformation of the dependent variable and adds 1 to the dependent variable in such cases. The inverse hyperbolic sine is equivalent to the natural log, with the added advantage of being defined at 0 (Burbidge et al., 1988).

deepening in under-developed regions.

A causal interpretation of the treatment effect from specification (4) is subject to the assumption that private bank branch openings in underbanked and non-underbanked districts would have evolved comparably in the absence of the policy intervention. We assess the validity of this assumption using the event-study specification outlined in equation (5). The annual treatment effects are plotted in Appendix Figure A3 with the dashed lines denoting 95% confidence intervals. There is no evidence of any differential pre-treatment trends in bank branch openings for either private (left panel) or government-owned (right panel) banks. In 2007 – a year after the initiation of the BAP – we see evidence of an increase in private bank branch openings in underbanked districts, although the coefficient is only significant at the 10% level (p-value .076). The point estimate increases further in the final three years of our sample (2009-11), and is statistically significant at the 5% level or better. In the absence of any pre-treatment differential trends in underbanked districts and the strong positive impact in the post-treatment period, we can attribute the increase in private bank branch openings to the BAP. We again find no differential effect in the post-treatment period on government bank branch openings in underbanked districts: while the coefficient estimates are positive, the wide confidence intervals not preclude us from ruling out a null effect.

All the above results restrict the sample to districts located within a bandwidth of 15 bank branches per million around the discontinuity threshold to ensure comparability across treatment and control units. Appendix Figure A4 confirms that our results are unchanged for a host of bandwidths in the range of 10-20 bank branches per capita, alleviating concerns that the results are contingent on focusing upon a single set of districts located within 15 bank branches per capita of the discontinuity threshold. While moving to narrower bandwidths increases the noisiness of the coefficient estimates, they remain statistically significant at the 10% level (with the exception of 1 estimate). The estimated coefficients also remain qualitatively similar in size. The absence of any sharp increase in the coefficient estimates upon restricting the sample to smaller bandwidths also implies that private banks were not strategically selecting underbanked districts which were located adjacent to the discontinuity threshold. The coefficient estimates for government banks (right-hand panel) continues to remain attenuated towards 0 and statistically non-significant, irrespective of the bandwidth considered.

A.2 Bank Branch Expansion and Financial Intermediation

The previous section showed that the BAP had a positive impact on bank branch openings in underbanked districts, driven by an expansion in private bank branches. In addition to the opening of new bank branches, the BAP had also encouraged banks to engage in greater financial intermediation in underserved regions, especially in terms of rural credit and credit to the "priority sector" – namely farm credit, and credit to small and micro-enterprises. We thereby examine whether the BAP affected banking outcomes in underbanked districts such as aggregate deposits and credit disbursement. Given that government-owned banks were operating in underbanked districts prior to the initiation of the BAP, we would expect changes in financial intermediation to be driven primarily by private banks in order to comply with the policy. We use the annual district-level BSR data to identify the impact of the BAP on bank deposits and credits using the differences-in-discontinuities specification. We also show corresponding event-study plots to verify that underbanked and non-underbanked districts within a narrow window around the threshold exhibited comparable trends prior to the policy intervention. We also separately present our results by bank group.

Appendix Table A2 identifies an overall increase in the number of deposit accounts (significant at the 10% level), but the volume of deposits remains unaffected by the policy [columns (1) and (2)]. Disaggregating deposits by private and government banks, we positive and imprecise coefficients for private banks [columns (3) and (4)], and a null effect for government banks [columns (5) and (6)]. Event-study plots corresponding to these regressions are shown in Appendix Figure A5 and present a consistent story, with the exception that private bank deposits (top row) in the final two years of the sample show a modest increase, both in terms of accounts and amounts. This is seen with regard to deposit accounts in government-owned banks as well. This could be possible if the stock of savings accumulate over time, leading to muted effects over the short and medium-term, but increasing over the long-term.

We next use our proprietary disaggregated BSR data to identify the impact of the branch expansion policy on district-level credit disbursement. Appendix Table A3 looks at aggregate credit disbursement, and credit allocated to the farm and manufacturing sectors. Panel A identifies a significant increase in credit disbursement from private banks for both the farm and manufacturing sectors [columns (3)-(6)]. While the BAP increased farm credit from private banks across both the intensive and extensive margins, the increase in private bank manufacturing credit is limited to the intensive margin – the extensive margin coefficient is positive, but not statistically significant. The coefficients are expectedly large as private banks had no operations in a number of underbanked districts prior to 2006. Panels B and C show little impact on government bank credit, or aggregate credit. The overwhelming dominance of government owned banks, unaffected by the intervention, possibly explains the null effect of the policy intervention on aggregate credit in underbanked districts.

Appendix Table A4 considers other credit heads. In addition to credit issued for trading, services, and personal finance, we also examine priority sector lending and credit for small and micro-enterprises. Priority sector lending involves lending to the farm sector, non-farm village enterprises, as well as small and micro establishments, and every bank is mandated to allocate 40 percent of its aggregate portfolio towards the priority sector. While the point estimates are not precisely estimated, Appendix Table A4 identifies a positive and significant impact on priority sector lending along the extensive margin, and the coefficient corresponding to credit disbursement along the intensive margin for both the priority sector, and small and micro-enterprises is relatively large in magnitude, albeit imprecisely estimated (p-values .175 and .205). The increased credit allocation to the priority sector is consistent with the explanation that private banks used the compliance with the BAP to meet their priority sector targets.[3] Similar to Appendix Table A3, there is no impact of the BAP on financial intermediation by government banks.

Appendix Figures A6-A9 show the event-study plots corresponding to the regressions in Appendix Tables A3 and A4. For none of the plots do we identify any differential trends across underbanked

[3] There are no binding district-level targets for priority sector lending.

and non-underbanked districts prior to 2005. The absence of any pre-treatment trends for both bank credit and deposits confirm that banking outcomes were not trending differentially across treatment and control units, providing credence to our empirical strategy. Along the extensive margin (Appendix Figure A7), we see almost an immediate increase in private bank farm credit, priority sector lending, and credit to small borrowers. As the increase in bank branches occurred predominantly after 2008, this suggests that existing private bank branches also increased lending to these sectors in the aftermath of the policy. This would be consistent with the directives of the BAP, which nudged banks to expand financial intermediation in underserved regions, in addition to opening new branches in these areas.

Along the intensive margin (Appendix Figure A6) in the last three years, we identify a positive and significant impact on overall credit, farm credit, manufacturing, services, and priority sector loans from private banks. The evidence suggest that following the policy intervention, existing private banks increased credit disbursement in underbanked districts, which subsequently expanded once new private bank branches were established. Overall, these results document that the BAP incentivized private banks to expand branches in underbanked districts, and the branch expansion also led to increased financial intermediation.

As seen from Appendix Figures A8 and A9, there is no impact on credit issued by government banks along either the intensive or extensive margin. Neither is there evidence that these banks offered higher credit to underbanked regions prior to 2005, and private banks were catching up to government banks in these areas. TThe null results with respect to credit disbursement by government banks offer two explanations: they first rule out a shifting of credit disbursement from government to private banks; second, they rule out an aggregate increase in credit demand in underbanked districts. If there was an aggregate outward shift in credit demand, we would have expected credit from government banks to also have differentially increased in these districts.

A.3 Bank Branch Expansion and Other Banking Outcomes

We finish our assessment of the BAP by investigating its impact on select banking ratios such as credit-deposit ratios, interest rates, priority sector lending, and non-performing loans. We also use the proprietary BSR data to identify the BAP's impact on bank staffing. As outcomes in this section are ratios, we use a sharp RD specification and restrict the sample to the year 2011. This ensures that our outcome variables are defined across the majority of districts in the sample and the cross-sectional RD framework compares outcomes across underbanked and non-underbanked districts within a narrow bandwidth around the discontinuity threshold. The results are shown graphically in Appendix Figure A10. The discontinuity estimate, computed using the methodology of Calonico et al. (2020) is noted at the bottom of each figure; p-values from robust standard errors, clustered by state-region are also reported. All specifications include state-region fixed effects and district covariates.

The first two figures in the top row of Appendix Figure A10 indicates an increase in the credit-deposit ratio of private banks in underbanked districts, although the coefficient is not statistically significant. Using proprietary data from the BSR on branch-level weighted lending rates,[4] we also

[4] The proprietary BSR data provides a weighted average lending rate for each branch. We aggregate this to the

compare whether the entry of private banks reduced the cost of credit. While the large literature on bank branch expansions suggests that increased competition can lower the cost of credit, we see no evidence of this in our cross-sectional RD plots. A possible explanation could be that the primary competitor of private banks in these districts were incumbent government banks, which already offered credit at sufficiently low rates of interest. The absence of disaggregated data on the cost of credit across sectors precludes us from identifying whether higher entry by private banks led to a reduction in the pricing of manufacturing loans.

Consistent with the evidence found in Section A.2, we identify a statistically significant 10 percentage point increase in the fraction of loans issued to the priority sector by private banks in underbanked districts. The coefficient corresponding to government banks is close to 0, ruling out again a secular increase in priority sector lending across all bank groups. The results again indicate that private banks took advantage of the BAP to better target their priority sector credit allocation through local branches set up in underbanked districts. The final two figures of Appendix Figure A10 compare non-performing loans in underbanked districts across bank groups. As non-performance has a time component, we use data from 2016 to estimate the fraction of non-performing loans in each district. The key question of interest is whether the expansion in private bank credit comes at the cost of borrower quality. If private banks lacked the necessary expertise to screen borrowers and undertake due dilligence in these under-served areas and only responded to the policy, we would expect to see an increase in the share of non-performing loans in private banks operating in underbanked districts. On the contrary, if private banks have improved screening mechanisms and can better target credit to high quality borrowers, we would expect a decline in the share of non-performing loans in underbanked districts. The bottom row of Appendix Figure A10 is unable to reject the null of no difference in the share of non-performing loans at the threshold for private banks. While the point estimate is negative, the confidence intervals are too wide to rule out a null effect. Consequently, we are unable to rule out a decline in borrower quality as a result of increased financial intermediation by private banks in underbanked districts.

The final test undertaken is to compare staffing levels for private and government banks across underbanked and non-underbanked districts. The BSR data classifies bank employees in each branch into two categories: officers and clerks. Typically, lending decisions and overall branch management are undertaken by the former, while the latter handle teller operations, maintain cash balances and so on. If private banks were complying with the BAP by increasing the level of financial intermediation, we would expect these branches to be staffed with more officers, responsible for lending activity. Resultantly, we compare the number of officers and employees across underbanked and non-underbanked districts for private and government owned banks.

Appendix Figure A10 shows a large and significant increase in the number of officers and employees in private banks in underbanked districts. There is also weak evidence of an increase in the number of officers and employees in government banks. The final two figures of Appendix Figure A10 shows a

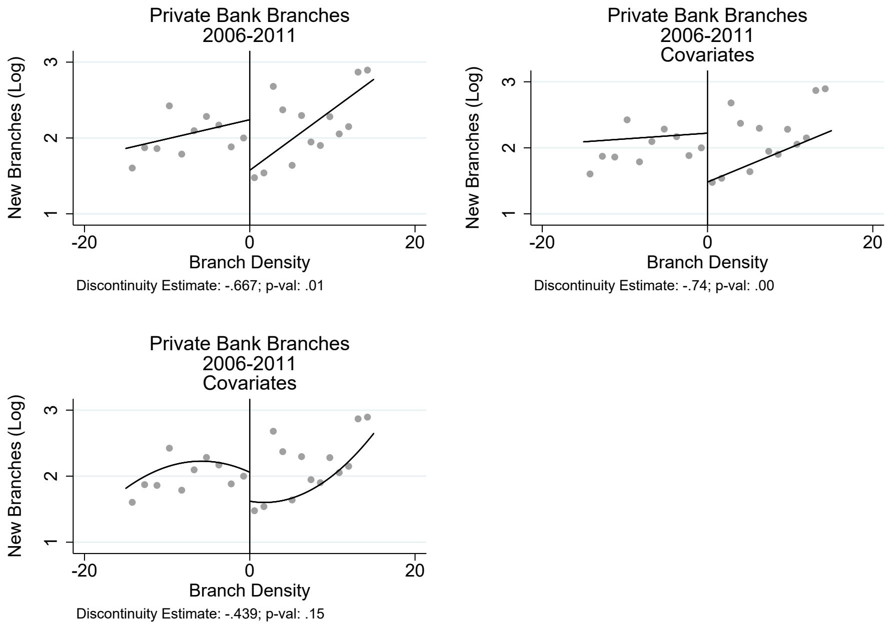
level of district by computing the average lending rate across all branches in the district, weighted by the volume of outstanding loans in each branch.

significant increase in the fraction of officers in private banks in a district. In contrast, there is a decline in the share of government bank officers in underbanked districts. The latter result in particular rules out that overall management needs in underbanked districts warrant a higher staffing of bank officers.

In summary, the results in this section confirm that the BAP incentivized private banks to open branches in underbanked districts. The increase in bank branches was substantial, and led to higher manufacturing and farm credit. The event-study plots suggests that existing private bank branches in underbanked districts expanded their lending activities, and this was further augmented by the entry of new private banks in these regions. The observed increase in lending activities is further bolstered by the fact that private banks in underbanked districts had a higher share of officers, who are typically responsible for lending decisions. There is also evidence of a higher share of lending to the priority sector in these districts. This suggests that private banks were using banking operations in these districts to meet their priority sector targets. Finally, higher financial intermediation by private banks in underbanked districts did not come at the cost of borrower quality, or give rise to risky lending practices, as seen from the absence of any significant increase in the fraction of non-performing loans.

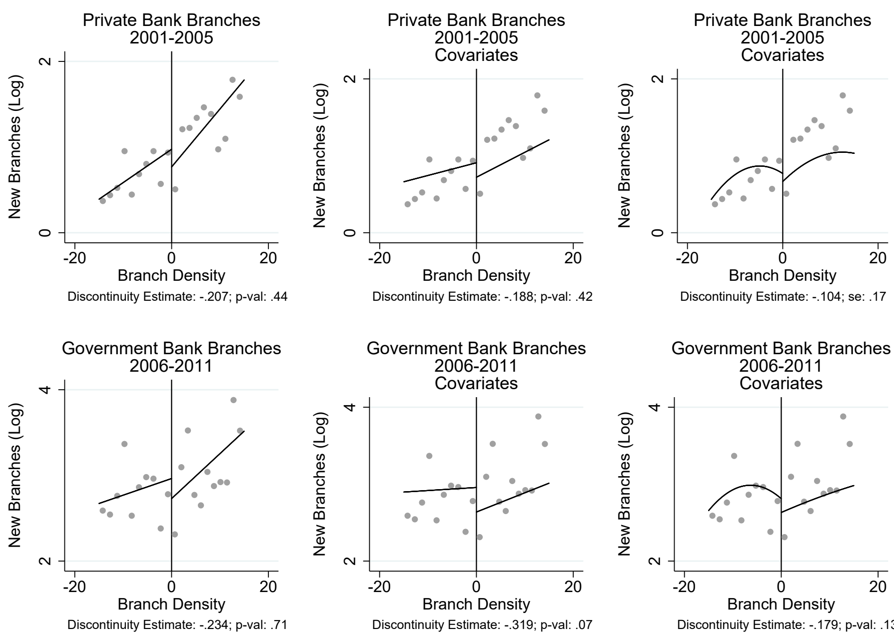
A.4 Figures

Figure A1: Private Bank Branch Openings in Underbanked Districts



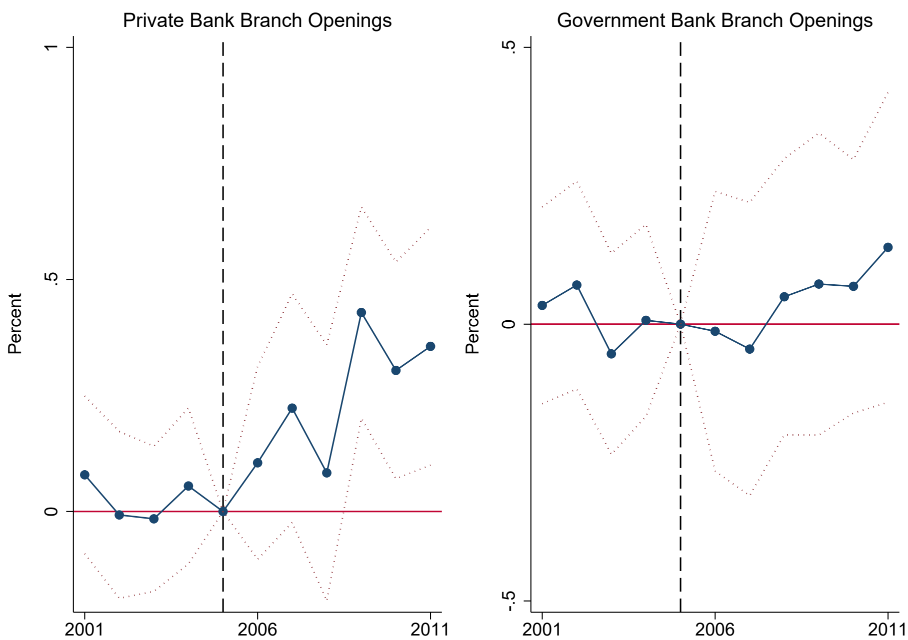
Notes: The above figure shows the impact of the policy intervention on district-level cumulative private bank branch openings between 2006 and 2010. The top-left panel shows the treatment effect, conditioning only on state-region fixed effects. The remaining figures also control for pre-treatment district covariates. The bottom figure considers a quadratic fit. The sample in each figure is restricted to districts within a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors are computed as outlined in Calonico et al. (2020). P-values correspond to robust standard errors, clustered by state-region. The figure is replicated from Chowdhury and Ritadhi (2022).

Figure A2: Branch Expansion Policy and Bank Branch Openings: Placebo Tests



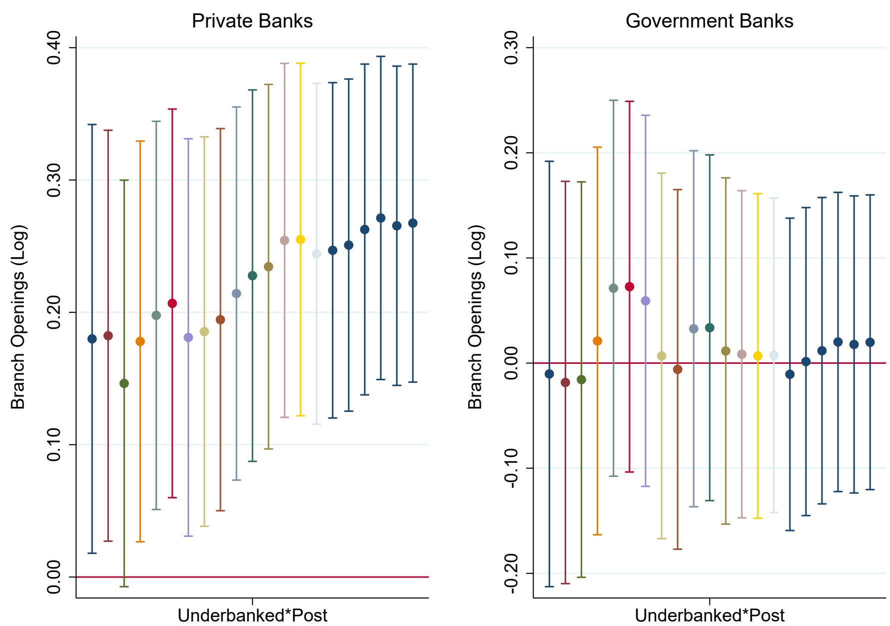
Notes: The above figure shows the impact of the policy intervention on district-level cumulative bank branch openings for a) government-owned bank branches between 2006 and 2010 (top row) and b) private bank branches between 2001-2005 (bottom row). The left panel shows the treatment effect only conditioning on state-region fixed effects. The remaining figures also control for pre-treatment district covariates. The right panels consider a quadratic polynomial fit. The sample in each figure is restricted to districts within a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors are computed as outlined in Calonico et al. (2020). P-values correspond to robust standard errors, clustered by state-region. The figure is replicated from Chowdhury and Ritadhi (2022).

Figure A3: Bank Branch Openings in Underbanked Districts: Event-Study Plots



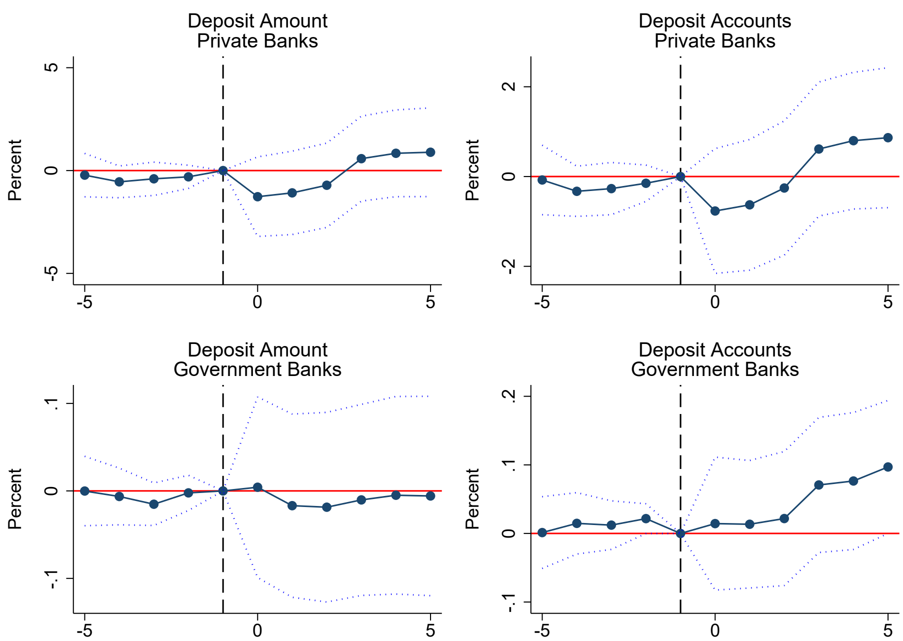
Notes: The above figures shows event-study plots estimating the impact of a district's underbanked status on annual bank branch openings. The unit of observation is the district. The outcome of interest in the left panel is the (logged) number of private bank branch openings; in the right panel, the (logged) number of government-owned bank branch openings. The solid line represents the average annual treatment effects, and the dashed lines denote the 95% confidence intervals. The treatment effects are benchmarked to the year 2005 (dashed vertical line) – the year in which the treatment is initiated. The sample is restricted to districts within a bandwidth of 15 around the discontinuity threshold. Both specifications include district and year fixed effects, a linear polynomial in the running variable, and district covariates. Standard errors are clustered by district.

Figure A4: Private Bank Branch Openings in Underbanked Districts: Robustness to Alternate Bandwidths



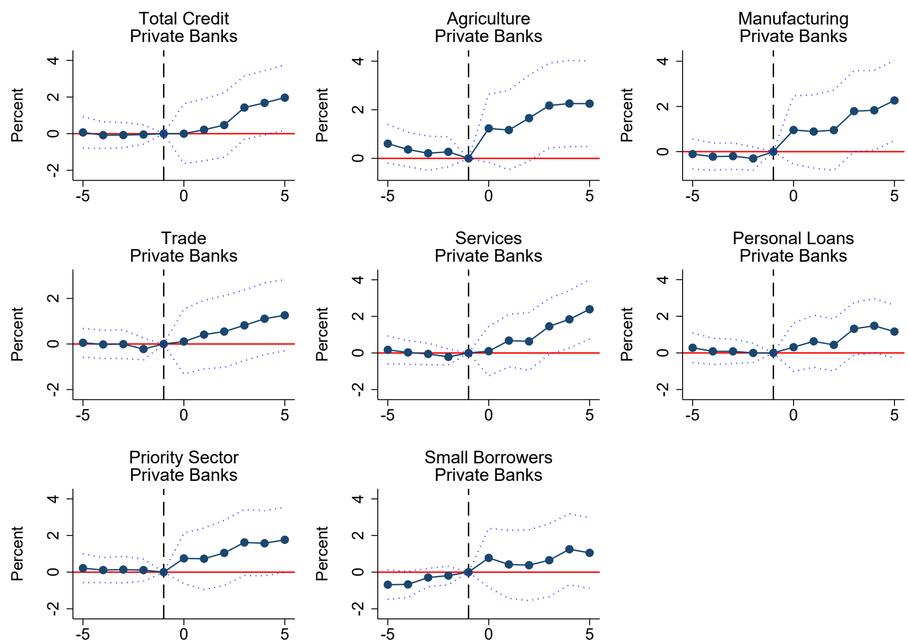
Notes: The above figure shows the robustness of private bank branch openings in underbanked districts to alternate bandwidths. The unit of observation is the district and the outcome of interest is new bank branch openings (logged). The left-hand panel shows private bank branch openings; the right-hand panel, government bank branch openings. All specifications include district and year fixed effects, a linear polynomial in the running variable, and district covariates. The sample in the first specification estimated is restricted to districts within a bandwidth of 10 around the discontinuity threshold; subsequent specification estimates increase the bandwidth by 0.5, till the final specification, which equals a bandwidth of 20 around the discontinuity threshold. The vertical lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A5: Bank Deposits in Underbanked Districts: by Bank Group



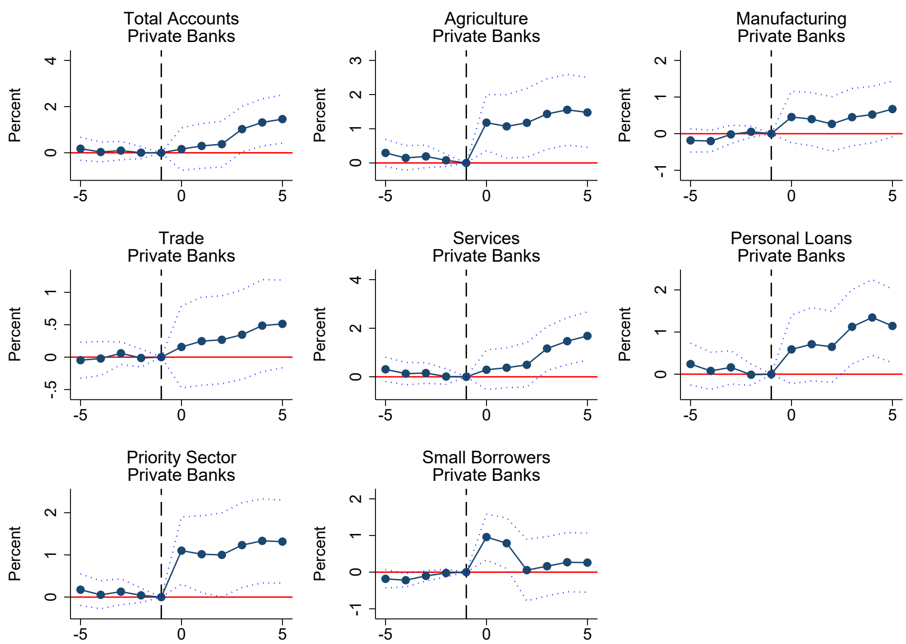
Notes: The above figure presents event-study plots comparing deposits across underbanked and non-underbanked districts. The unit of observation is the district. The top row shows event-study plots for private banks; the bottom row shows the same for government banks. We consider both deposit amounts (right-hand panel), and accounts (left-hand panel). The outcome variable in each instance is logged. All specifications include district and year fixed effects, in addition to time-varying district covariates. The sample is restricted to districts located within a bandwidth of 15 around the threshold. The solid line represents the point estimates while the broken lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A6: Private Bank Credit in Underbanked Districts: Intensive Margin



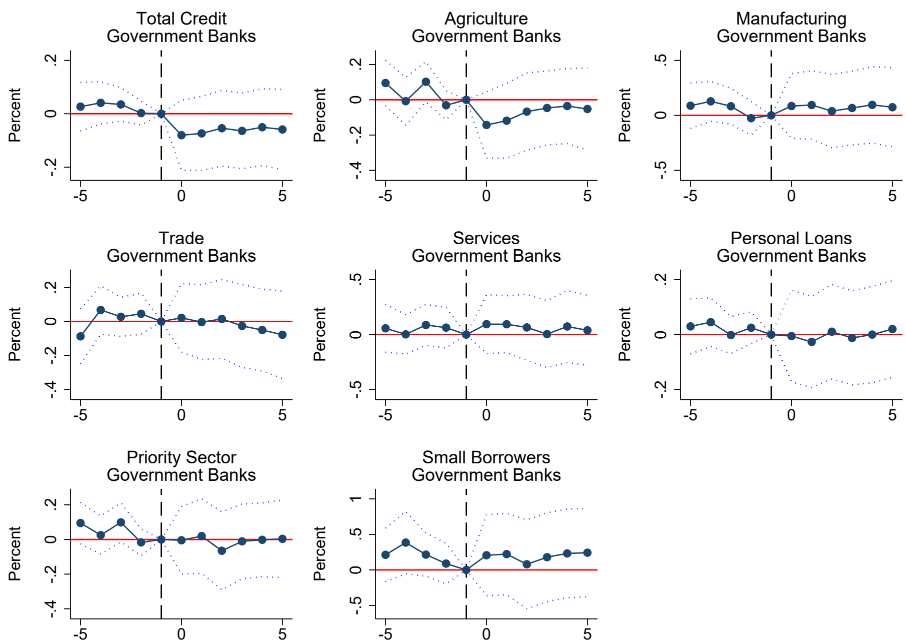
Notes: The above figure presents event-study plots comparing sectoral credit allocation across underbanked and non-underbanked districts for private banks. Credit allocation is along the intensive margin (loan amounts). The unit of observation is the district. The outcome variable in each instance is logged. The first figure in the top row is for total credit amount. All specifications include district and year fixed effects, in addition to time-varying district covariates. The sample is restricted to districts located within a bandwidth of 15 around the threshold. The solid line represents the point estimates while the broken lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A7: Private Bank Credit in Underbanked Districts: Extensive Margin



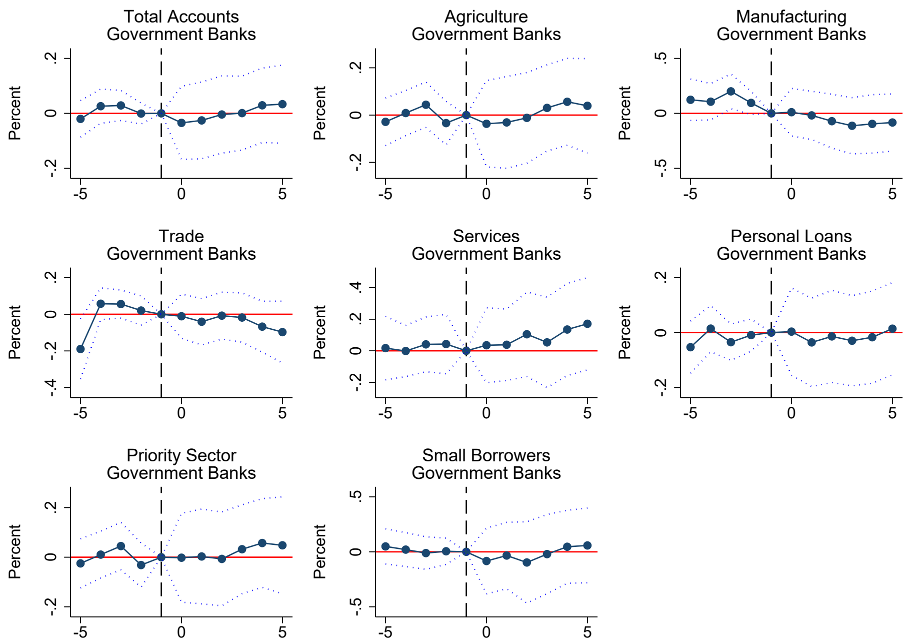
Notes: The above figure presents event-study plots comparing sectoral credit allocation across underbanked and non-underbanked districts for private banks. Credit allocation is along the extensive margin (loan accounts). The unit of observation is the district. The outcome variable in each instance is logged. The first figure in the top row is for total credit amount. All specifications include district and year fixed effects, in addition to time-varying district covariates. The sample is restricted to districts located within a bandwidth of 15 around the threshold. The solid line represents the point estimates while the broken lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A8: Government Bank Credit in Underbanked Districts: Intensive Margin



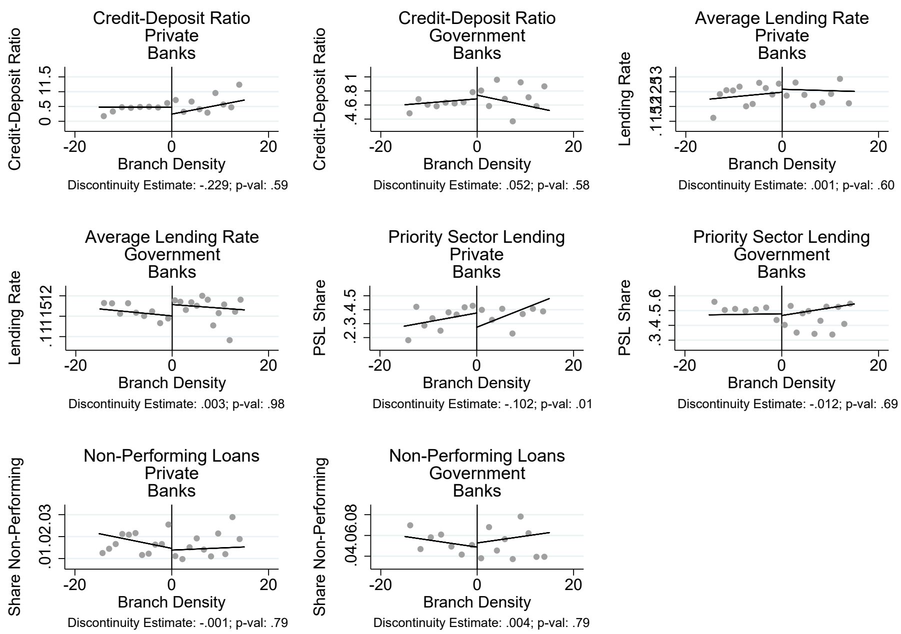
Notes: The above figure presents event-study plots comparing sectoral credit allocation across underbanked and non-underbanked districts for private banks. Credit allocation is along the intensive margin (loan amounts). The unit of observation is the district. The outcome variable in each instance is logged. The first figure in the top row is for total credit amount. All specifications include district and year fixed effects, in addition to time-varying district covariates. The sample is restricted to districts located within a bandwidth of 15 around the threshold. The solid line represents the point estimates while the broken lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A9: Government Bank Credit in Underbanked Districts: Extensive Margin



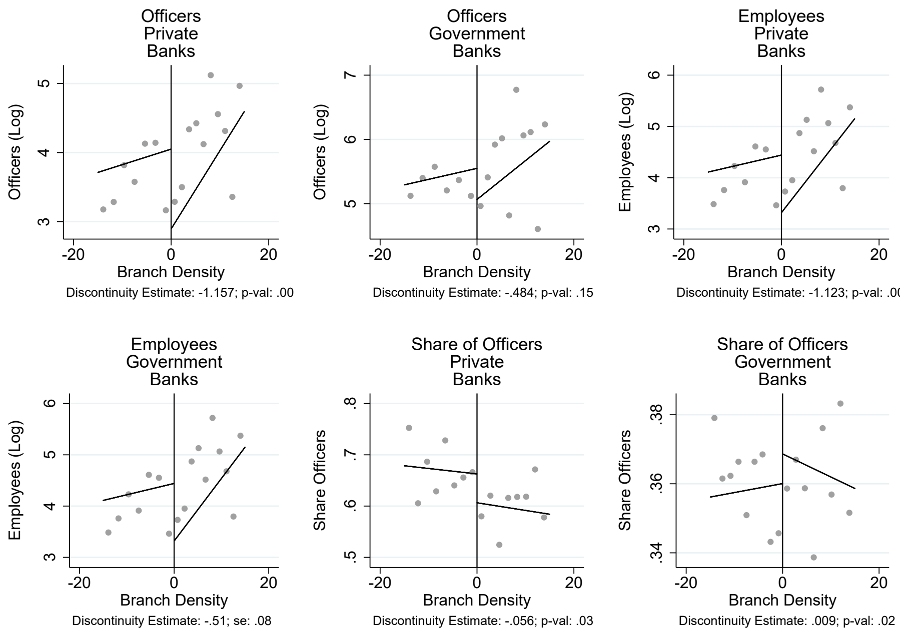
Notes: The above figure presents event-study plots comparing sectoral credit allocation across underbanked and non-underbanked districts for government banks. Credit allocation is along the extensive margin (loan accounts). The unit of observation is the district. The outcome variable in each instance is logged. The first figure in the top row is for total credit amount. All specifications include district and year fixed effects, in addition to time-varying district covariates. The sample is restricted to districts located within a bandwidth of 15 around the threshold. The solid line represents the point estimates while the broken lines denote the 95% confidence intervals. Standard errors are clustered by district.

Figure A10: Bank Branch Expansion and Other Outcomes of Interest



Notes: The above figure presents cross-sectional RDD plots comparing private and government bank outcomes across underbanked and non-underbanked districts. Non-performing loans are measured in 2016. The remaining outcomes are measured in 2010. All regressions contain state-region fixed effects and district covariates. Non-performing loans and priority sector loans are scaled by total loans in the district. Interest rates are computed at the branch-level, and aggregated to the level of district using branch-level loans as weights. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold. The discontinuity estimates and standard errors are computed as outlined in Calonico et al. (2020). P-values correspond to robust standard errors, clustered by state-region.

Figure A10: Bank Branch Expansion and Loan Officers



Notes: The above figure presents cross-sectional RDD plots comparing bank staffing patterns across underbanked and non-underbanked districts for private and government banks. Employees refer to all employees employed by the bank. Share of officers is officers scaled by total employees. All regressions contain state-region fixed effects and district covariates. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold, The discontinuity estimates and standard errors are computed as outlined in Calonico et al. (2020). P-values correspond to robust standard errors, clustered by state-region.

A.5 Tables

Table A1: New Bank Branch Openings in Underbanked Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Private Banks				Government Banks			
	New Branches (Log)		New Branches Inverse Hyperbolic Sine		New Branches (Log)		New Branches Inverse Hyperbolic Sine	
Underbanked × Post	.216**	.228***	.278**	.293***	.035	.034	.033	.030
	(.098)	(.085)	(.123)	(.105)	(.138)	(.100)	(.171)	(.124)
Observations	2508	2508	2508	2508	2508	2508	2508	2508
R^2	.57	.58	.56	.58	.71	.74	.72	.74
Control Mean	1.25	1.25	1.25	1.25	1.58	1.58	1.58	1.58
Covariates	N	Y	N	Y	N	Y	N	Y

Notes: This table estimates the treatment effect on annual new bank branch openings in underbanked districts. The unit of observation is the district. Columns (1)-(4) estimate the treatment effect on private bank branch openings; columns (5)-(8) consider government bank branch openings. The outcome of interest in columns (1)-(2) and (5)-(6) is logged new bank branch openings; in columns (3)-(4) and (7)-(8), the inverse hyperbolic sine transformation of new bank branch openings. The odd-numbered columns include district and year fixed effects, and a linear polynomial in the running variable; the even-numbered columns include district covariates. The sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A2: Bank Branch Expansion and District Deposits: by Bank Group

	(1)	(2)	(3)	(4)	(5)	(6)
	All Banks		Private Banks		Government Banks	
	Accounts (Log)	Amount (Log)	Accounts (Log)	Amount (Log)	Accounts (Log)	Amount (Log)
Underbanked × Post	.080*	.031	.269	.168	.039	-.004
	(.043)	(.050)	(.796)	(1.084)	(.046)	(.054)
Observations	2505	2505	2505	2505	2505	2505
R^2	.99	.99	.85	.83	.99	.99
Control Mean	1625.03	49801.24	223.16	9848.57	1401.87	39952.67

Notes: This table presents the impact of a district's underbanked status on bank deposits using a differences-in-discontinuity design. The unit of observation is the district. *Underbanked* is a dummy equaling 1 if the district's per capita bank branch density in 2005 was less than the national average bank branch density. Post is a dummy equaling 1 for years between 2006 and 2010. The sample is restricted to districts within a bandwidth of 15 on either side of the national average threshold. The dependent variables are logged. Odd numbered columns show outcomes along the extensive margin (number of deposit accounts); even numbered columns show outcomes along the intensive margin (amount of deposits). All specificiations include a linear polynomial in the running variable and its interaction with a district's underbanked status and the post indicator. All specifications include district and year fixed effects, along with pre-treatment district covariates, interacted with a post indicator. Standard errors are clustered by district. Significant levels: *10%, **5%, and ***1%.

Table A3: Credit Disbursement in Underbanked Districts: By Major Sectors and Bank Groups

Panel A: Private Banks

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		Farm		Manufacturing	
	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)
Underbanked × Post	.987	.709	1.499*	1.172**	1.614*	.531
	(.920)	(.529)	(.794)	(.492)	(.856)	(.363)
Observations	2505	2505	2505	2505	2505	2505
R^2	.86	.89	.86	.90	.85	.89
Control Mean	5930.69	35.19	299.38	2.34	2157.63	.75

Panel B: Government Banks

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		Farm		Manufacturing	
	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)
Underbanked × Post	-.085	-.007	-.109	.010	.021	-.167
	(.073)	(.073)	(.102)	(.104)	(.168)	(.113)
Observations	2505	2505	2505	2505	2505	2505
R^2	.99	.99	.98	.98	.96	.92
Control Mean	22356.45	94.44	2610.18	31.93	8608.02	6.38

Panel C: All Banks

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		Farm		Manufacturing	
	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)
Underbanked × Post	-.013	.036	-.046	.027	.130	-.148
	(.072)	(.075)	(.100)	(.106)	(.163)	(.114)
Observations	2505	2505	2505	2505	2505	2505
R^2	.99	.99	.98	.98	.96	.92
Control Mean	28287.15	129.64	2909.56	56.41	10765.65	11.87

Notes: This table estimates the treatment effect on bank credit disbursement in underbanked districts. The unit of observation is the district. Panel A considers private bank credit; panel B credit from government banks; Panel C, aggregate bank credit across both government and private banks. All outcome variables are logged. The outcome variable in odd-numbered columns is bank credit along the intensive margin (log outstanding credit amount); in even-numbered columns, bank credit along the extensive margin (logged credit accounts). Columns (1) and (2) consider aggregate bank credit; columns (3) and (4) farm credit; and columns (5) and (6) manufacturing credit. All specifications include district and year fixed effects, in addition to a linear polynomial in the running variable and district covariates. The sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table A4: Credit Disbursement in Underbanked Districts: By Sectors and Bank Groups

Panel A: Private Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Trade		Services		Personal Finance		Priority Sector		Micro and Small Credit	
	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)
Underbanked × Post	.747	.341	1.196	.791*	.800	.834*	1.133	1.086**	1.123	.522
	(.788)	(.337)	(.782)	(.455)	(.736)	(.450)	(.832)	(.480)	(.883)	(.361)
Observations	2505	2505	2505	2505	2505	2505	2505	2505	2505	2505
R^2	.86	.92	.85	.86	.88	.90	.85	.90	.81	.85
Control Mean	645.96	.89	1350.57	3.42	1477.15	27.80	504.80	2.39	205.42	.06

Panel B: Government Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Trade		Services		Personal Finance		Priority Sector		Micro and Small Credit	
	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)
Underbanked × Post	-.031	-.029	.020	.070	-.022	.004	-.051	.022	.013	-.034
	(.118)	(.063)	(.133)	(.109)	(.090)	(.087)	(.108)	(.103)	(.321)	(.162)
Observations	2505	2505	2505	2505	2505	2505	2505	2505	2505	2505
R^2	.96	.96	.94	.94	.98	.98	.98	.98	.91	.95
Control Mean	2989.45	7.58	3764.53	14.36	4384.28	34.20	4060.98	32.47	1450.80	.54

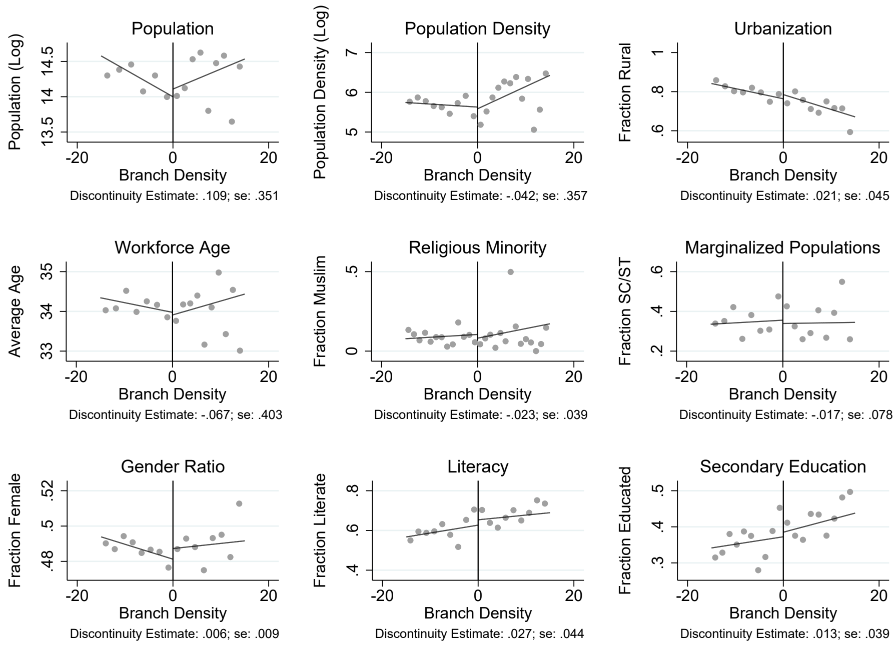
Panel C: All Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Trade		Services		Personal Finance		Priority Sector		Micro and Small Credit	
	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)
Underbanked × Post	.012	-.003	.136	.168	.023	.057	.008	.042	.023	.020
	(.119)	(.061)	(.138)	(.118)	(.089)	(.090)	(.106)	(.104)	(.298)	(.164)
Observations	2505	2505	2505	2505	2505	2505	2505	2505	2505	2505
R^2	.96	.96	.94	.94	.98	.97	.98	.98	.91	.95
Control Mean	3635.40	14.03	5115.10	29.33	5861.43	100.39	4565.78	57.01	1656.22	.60

Notes: This table estimates the treatment effect on bank credit disbursement in underbanked districts. The unit of observation is the district. Panel A considers private bank credit; panel B credit from government banks; Panel C, aggregate bank credit across both government and private banks. All outcome variables are logged. The outcome variable in odd-numbered columns is bank credit along the intensive margin (log outstanding credit amount); in even-numbered columns, bank credit along the extensive margin (logged credit accounts). Columns (1) and (2) consider credit for trading activities; columns (3) and (4), credit for services; columns (5) and (6), personal loans; columns (7) and (8), loans to priority sector; and columns (9) and (10), loans to micro and small enterprises. All specifications include district and year fixed effects, in addition to a linear polynomial in the running variable and district covariates. The sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

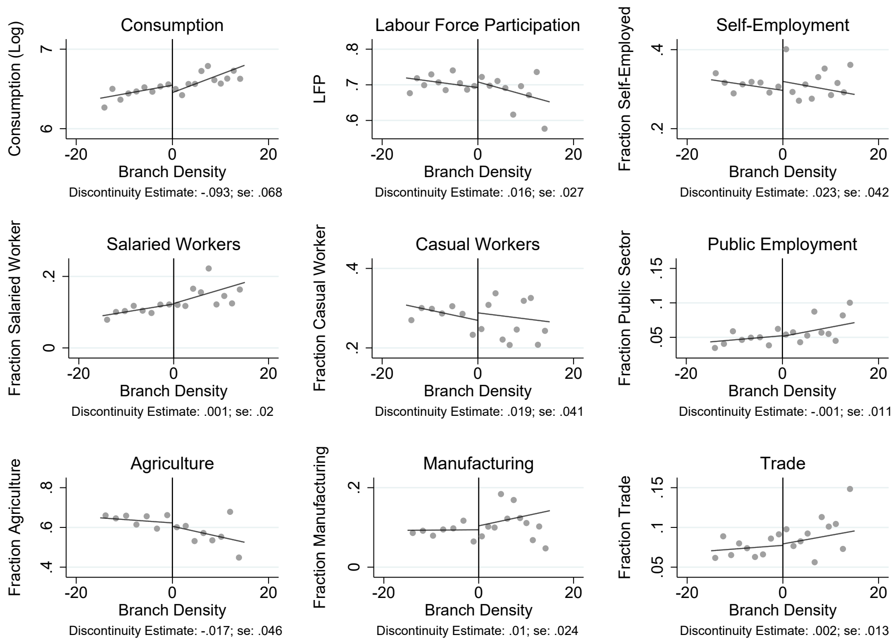
B Appendix: Additional Figures and Tables

Figure B1: Pre-Treatment Covariate Balance: District Demographics



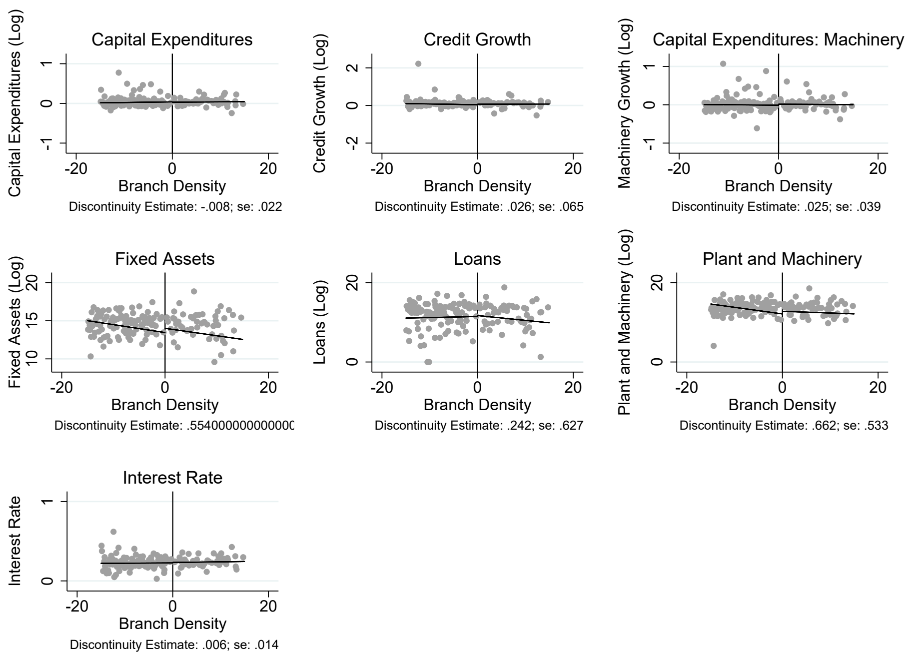
Notes: The above figure shows the pre-treatment covariate balance across district-specific demographic covariates. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". The sample is restricted to a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors corresponding to each figure are computed as outlined in Calonico et al. (2020). The figure is replicated from Chowdhury and Ritadhi (2022).

Figure B2: Pre-Treatment Covariate Balance: District Demographics



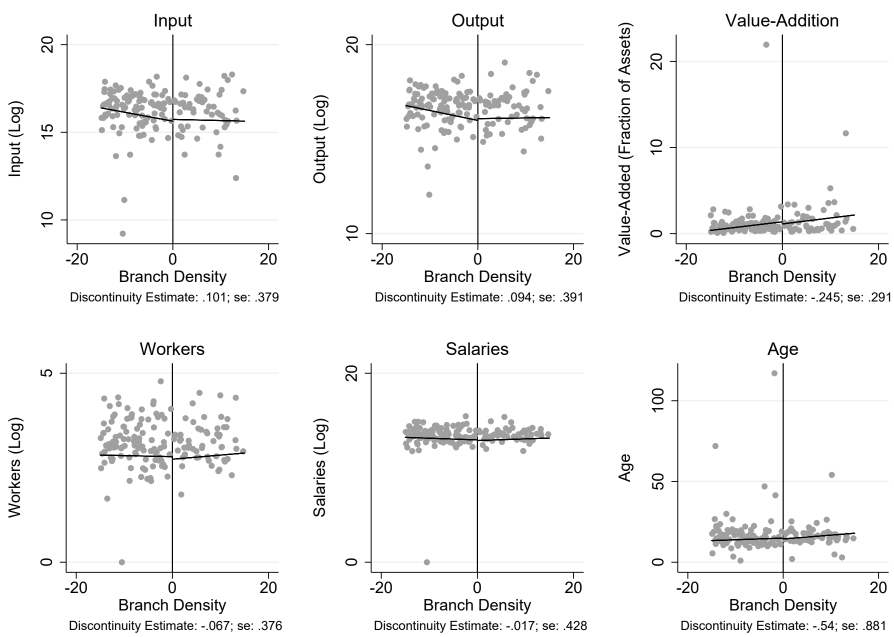
Notes: The above figure shows the pre-treatment covariate balance across district-specific demographic covariates. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". Consumption refers to households monthly per capita consumption. The sample is restricted to a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors corresponding to each figure are computed as outlined in Calonico et al. (2020).

Figure B3: Pre-Treatment Covariate Balance: Manufacturing Enterprise Characteristics



Notes: The above figure shows the pre-treatment balance across manufacturing enterprise characteristics. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". The sample is restricted to a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors corresponding to each figure are computed as outlined in Calonico et al. (2020).

Figure B4: Pre-Treatment Covariate Balance: Manufacturing Enterprise Labour Characteristics



Notes: The above figure shows the pre-treatment balance across manufacturing enterprise characteristics. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". Consumption refers to households monthly per capita consumption. The sample is restricted to a bandwidth of 15 around the threshold. The discontinuity estimates and standard errors corresponding to each figure are computed as outlined in Calonico et al. (2020).

Table B1: Covariate Balance Across Pre-Treatment District Demographic Covariates

	(1) Population (Log)	(2) Population Denisty (Log)	(3) Fraction Rural	(4) Workforce Age	(5) Fraction Muslim	(6) Fraction Marginalized	(7) Fraction Female	(8) Fraction Literate	(9) Fraction Educated
Underbanked	.339	.089	-.024	.389	-.046*	-.007	.002	-.041	-.006
	(.242)	(.171)	(.035)	(.428)	(.025)	(.045)	(.008)	(.027)	(.031)
Observations	228	228	228	228	228	228	228	228	228
R^2	.83	.86	.66	.58	.91	.82	.52	.84	.78
Control Mean	2.106	543.41	.74	34.11	.11	.35	.49	.67	.41

Notes: This table shows the pre-treatment covariate balance across district-level demographic covariates. The unit of observation is the district. *Underbanked* is a dummy equaling 1 if the district's per capita bank branch density in 2005 was less than the national average bank branch density. The sample is restricted to districts within a bandwidth of 15 on either side of the national average threshold. Workforce age is the average age of workers in the district; marginalized castes refer to the fraction of *Dalits* and *Adivasis* in the district; educated refers to the fraction of adults with secondary or higher education. All specificiations include a linear polynomial in the running variable and its interaction with a district's underbanked status. All specifications include state-region fixed effects and standard errors are clustered by state-region. Significant levels: *10%, **5%, and ***1%.

Table B2: Covariate Balance Across Pre-Treatment District Economic Covariates

	(1) LFP	(2) Fraction Self-Employed	(3) Fraction Salaried Workers	(4) Fraction Casual Workers	(5) Fraction Farm Activities	(6) Fraction Manufacturing Activities	(7) Fraction Trade Activities	(8) Fraction Public Employment	(9) Per Capita Consumption (Log)
Underbanked	.018	.007	-.026	.004	.006	.012	.004	-.007	.013
	(.027)	(.021)	(.020)	(.033)	(.042)	(.022)	(.013)	(.009)	(.063)
Observations	228	228	228	228	228	228	228	228	228
R^2	.62	.82	.60	.74	.66	.55	.55	.67	.77
Control Mean	.69	.31	.15	.28	.57	.11	.09	.06	752.72

Notes: This table shows the pre-treatment covariate balance across district-level economic covariates. The unit of observation is the district. *Underbanked* is a dummy equaling 1 if the district's per capita bank branch density in 2005 was less than the national average bank branch density. The sample is restricted to districts within a bandwidth of 15 on either side of the national average threshold. All specificiations include a linear polynomial in the running variable and its interaction with a district's underbanked status. Per capita consumption is the district's average household monthly per capita consumption. All specifications include state-region fixed effects and standard errors are clustered by state-region. Significant levels: *10%, **5%, and ***1%.

Table B3: Pre-Treatment Balance of Manufacturing Characteristics Across Underbanked Districts

	(1) Capital Expenditures (Log)	(2) Any Capital Expenditures	(3) Plant Machinery Investment (Log)	(4) Credit Growth (Log)	(5) Interest Rate
Underbanked	.004	-.039	-.025	-.014	-.001
	(.022)	(.027)	(.026)	(.029)	(.021)
Observations	21110	22079	20620	16044	17077
R^2	.01	.05	.01	.00	.02

Notes: This table shows balance across pre-treatment manufacturing characteristics. The unit of observation is the manufacturing establishment. All specifications include state and 2-digit industry year fixed effects, along with a linear polynomial in the running variable. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B4: Pre-Treatment Balance of Manufacturing Characteristics Across Underbanked Districts

	(1) Fixed Assets (Log)	(2) Plant Machinery (Log)	(3) Loans (Log)	(4) Input (Log)	(5) Workers (Log)	(6) Salaries (Log)	(7) Output (Log)	(8) Value-Addition	(9) Age
Underbanked	-.595	-.398	-.818	-.242	.051	-.019	-.149	.444	.022
	(.447)	(.510)	(.712)	(.253)	(.131)	(.174)	(.208)	(.444)	(1.004)
Observations	22079	21462	21962	22079	22079	22079	22079	22079	21321
R^2	.15	.15	.16	.16	.08	.09	.14	.06	.08

Notes: This table shows balance across pre-treatment manufacturing characteristics. The unit of observation is the manufacturing establishment. All specifications include state and 2-digit industry year fixed effects, along with a linear polynomial in the running variable. Value-addition is defined as establishment value-addition, scaled by establishment assets. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B5: Manufacturing Investment in Underbanked Districts: Alternate Outcome Variables

	(1)	(2)	(3)	(4)	(5)
	Capital Expenditures: Plant and Machinery		Capital Expenditures		
	Log Difference	Pr(Any Capex = 1)	Capital Expenditure	Log Difference	Pr(Any Capex = 1)
Underbanked × Post	.073***	.050**	.031*	.035**	.042*
	(.026)	(.024)	(.017)	(.017)	(.024)
Observations	71542	71542	71542	71542	71542
R^2	.37	.44	.38	.38	.45
Control Mean	-.003	.233	.003	.013	.268

Notes: This table shows the robustness of the baseline specification to alternate functional forms and outcome variables. The unit of observation is the manufacturing establishment. All specifications include establishment and 2-digit industry year fixed effects, along with a linear polynomial in the running variable, as well as establishment and district covariates. Columns (1) and (2) restricts capital expenditures to capital expenditures in plant and machinery; capital expenditures in columns (3)-(5) is net fixed assets. Columns (1) and (4) measure capital expenditures as the logged difference in closing and opening values of establishment plant and machinery (fixed assets); the outcome in columns (2) and (5) is a dummy equaling 1 if the establishment undertook any positive capital spending during the year. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B6: Credit Growth for Manufacturing Establishments in Underbanked Districts

	(1) Credit Growth	(2) Credit Growth (Log)	(3) Any Credit Growth	(4) New Loan	(5) No Loan	(6) Interest Rate
Underbanked × Post	.128***	.417***	-.014	.012	.010	.012
	(.043)	(.140)	(.027)	(.008)	(.018)	(.017)
Observations	53666	71138	71138	71542	71138	53645
R^2	.34	.30	.44	.35	.71	.58
Control Mean	.043	.146	.381	.025	.224	.241

Notes: This table identifies the treatment effect on credit growth. The unit of observation is the manufacturing establishment. All specifications include establishment, 2-digit industry-year fixed effects, a linear polynomial in the running variable, district and establishment covariates. The outcome of interest in column (1) is credit growth, defined as in equation (3); in column (2), logged difference in closing and opening values of outstanding loans; column (3), a dummy equaling 1 if the closing value of loans exceeded the opening value; column (4), a dummy equaling 1 if the establishment had no outstanding loans through the year; in column (5), a dummy equaling 1 if the establishment had no outstanding credit at the beginning of the accounting period, but positive outstanding loans at the year-end; in column (6), the imputed interest rate. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B7: Manufacturing Investment in Underbanked Districts: Heterogeneity by Establishment and Industry Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Capital Expenditures					
Underbanked × Post	.083**	.068**	.077***	.085**	.065*	.068**
	(.035)	(.031)	(.027)	(.037)	(.039)	(.028)
Underbanked × High Capital × Post	-.035					
	(.041)					
Underbanked × *Small* × Post		-.017				
		(.041)				
Underbanked × *Medium* × Post		-.155**				
		(.060)				
Underbanked × *Large* × Post		-.092*				
		(.055)				
Underbanked × Non-SSI × Post			-.105**			
			(.047)			
Underbanked × High Collateral × Post				-.037		
				(.048)		
Underbanked × Partnership × Post					-.008	
					(.047)	
Underbanked × Private Ent. × Post					.032	
					(.048)	
Underbanked × Govt. × Post					.029	
					(.102)	
Underbanked × Listed × Post					-.096**	
					(.048)	
Underbanked × Rural × Post						-.008
						(.036)
Observations	71542	71280	71280	71280	71542	71542
R^2	.38	.38	.38	.38	.38	.38
Control Mean	-.03	-.03	-.03	-.03	-.03	-.03

Notes: This table identifies the treatment heterogeneity on manufacturing investment across establishment fixed assets, tangibility, ownership, and enterprise location. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures. Capital expenditures refers to capital spending on plant and machinery, defined as in equation (3). All specifications include establishment and 2-digit industry year fixed effects, along with a linear polynomial in the running variable, as well as establishment and district covariates. *High Capital* refers to establishments whose fixed assets exceed the median pre-treatment fixed assets. Administrative definitions are used to classify establishments as *Small*, *Medium*, *Large* and *SSI* (Small Scale Industries), based on their pre-treatment establishment fixed capital. *High Collateral* refers to establishments whose value of land and buildings exceed the pre-treatment median. *Rural* refers to establishments operating in a rural location. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B8: Credit Growth in Underbanked Districts: Heterogeneity by Establishment and Industry Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Credit Growth			
Underbanked × Post	.207**	.166***	.173***	.165**	.207**	.143*
	(.084)	(.059)	(.046)	(.077)	(.084)	(.075)
Underbanked × High Capital × Post	-.110					
	(.098)					
Underbanked × *Small* × Post		-.049				
		(.079)				
Underbanked × *Medium* × Post		-.227				
		(.165)				
Underbanked × *Large* × Post		-.135*				
		(.076)				
Underbanked × Non-SSI × Post			-.182***			
			(.068)			
Underbanked × High Collateral × Post				-.056		
				(.092)		
Underbanked × Partnership × Post					-.059	
					(.112)	
Underbanked × Private Ent. × Post					-.092	
					(.117)	
Underbanked × Govt. × Post					-.191	
					(.161)	
Underbanked × Listed × Post					-.222**	
					(.107)	
Underbanked × Rural × Post						-.019
						(.098)
Observations	53666	53507	53507	53507	53666	53666
R^2	.34	.34	.34	.34	.34	.34
Control Mean	.04	.04	.04	.04	.04	.04

Notes: This table identifies the treatment heterogeneity on credit growth across establishment fixed assets, tangibility, ownership, and enterprise location. The unit of observation is the manufacturing establishment. The outcome of interest is credit growth. All specifications include establishment and 2-digit industry year fixed effects, along with a linear polynomial in the running variable, as well as establishment and district covariates. *High Capital* refers to establishments whose fixed assets exceed the median pre-treatment fixed assets. Administrative definitions are used to classify establishments as *Small*, *Medium*, *Large* and *SSI* (Small Scale Industries), based on their pre-treatment establishment fixed capital. *High Collateral* refers to establishments whose value of land and buildings exceed the pre-treatment median. *Rural* refers to establishments operating in a rural location. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B9: Cost of Credit for Manufacturing Establishments in Underbanked Districts: Heterogeneity by Establishment Size and Age

	(1)	(2)	(3)	(4)
			Interest Rate	
Underbanked × Post	.049*	.079***	.046	.005
	(.027)	(.029)	(.034)	(.020)
Underbanked × Est. *Size* > Median × Post	-.061*			
	(.032)			
Underbanked × 10 > Est. *Size* ≤ 25 × Post		-.104***		
		(.037)		
Underbanked × 25 > Est. *Size* ≤ 50 × Post		-.120***		
		(.044)		
Underbanked × 50 > Est. *Size* ≤ 100 × Post		-.081*		
		(.047)		
Underbanked × Est. *Size* > 100 × Post		-.064		
		(.044)		
Underbanked × Large, Young × Post			-.018	
			(.045)	
Underbanked × Large, Old × Post			-.085**	
			(.040)	
Underbanked × Small, Old × Post			.005	
			(.034)	
Underbanked × Listed × Post				.056
				(.044)
Observations	53645	53645	53645	53645
R^2	.58	.58	.58	.58
Control Mean	.24	.24	.24	.24
Control Mean	.244	.244	.244	.244

Notes: This table identifies the treatment heterogeneity on imputed interest rates for manufacturing enterprises. The unit of observation is the manufacturing establishment. The outcome of interest is the imputed interest rate. All specifications include establishment and 2-digit industry year fixed effects, along with a linear polynomial in the running variable, as well as establishment and district covariates. *Est. Size* refers to the pre-treatment average number of employees employed by the establishment. *Young* refers to establishments which started operation after 1992. Listed establishments are those which are publicly listed. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%

Table B10: Manufacturing Investment in Underbanked Districts: Heterogeneity by Borrower Quality

	(1)	(2)	(3)	(4)
			Capital Expenditures	
Underbanked × Post	.026	.055	.120***	.049*
	(.031)	(.037)	(.032)	(.027)
Underbanked × High MRPK × Post	.062*			
	(.035)			
Underbanked × High GVA × Post		.003		
		(.048)		
Underbanked × High Interest × Post			-.069*	
			(.041)	
Underbanked × Tradable × Post				.035
				(.037)
Observations	69918	71294	59425	68826
R^2	.38	.38	.38	.38
Control Mean	-.03	-.03	-.03	-.03

Notes: This table identifies the treatment heterogeneity on manufacturing investment across borrower and industry characteristics. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures (spending on plant and machinery) defined as in equation (3). All specifications include establishment and 2-digit industry year fixed effects, along with a linear polynomial in the running variable, as well as establishment and district covariates. *High MRPK* refers to establishments with relatively high (above median) marginal product of capital; *High GVA* refers to establishments with relatively high (above median) value-addition per worker; *High Interest* refers to establishments with relatively high (above median) interest rates; *Tradable* refers to establishments operating in industries with relatively low geographic dispersion. All specifications include establishment-specific weights and the sample is restricted to districts located within a bandwidth of 15 around the discontinuity threshold. Standard errors are in parentheses, clustered by district. Significant levels: *10%, **5%, and ***1%