Distributional Implications of Bank Branch Expansions: Evidence from India

Kanika Mahajan S. K. Ritadhi*

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Abstract

How does financial development affect firm performance? We exploit a nationwide branch expansion policy in India targeted towards private banks to examine this question. The policy classified districts as "underbanked" if their ex-ante bank branch density was less than the national average. Extending a regression discontinuity design based on the change in districts' underbanked status at the national average threshold, we find large increases in capital expenditures and credit growth by manufacturing establishments in underbanked districts. The increase in capital spending is driven by small and young establishments, who are also most likely to be credit constrained. An examination of mechanisms points to the improved ability of private banks to effectively screen borrowers and lend to small establishments with limited collateral, but high ex-ante returns to capital. Our findings show that financial deepening can aid in the relaxation of credit constraints in developing economies with imperfect capital and credit markets.

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. 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 the physical proximity of financial institutions can affect firm performance by alleviating barriers to credit access through better information acquisition and improved screening and monitoring. The question is particularly relevant for informationally-opaque

*Ashoka University & Ashoka University. Corresponding email: sk.ritadhi@ashoka.edu.in. We thank Isha Aggarwal, Aaditya Dar, Mausumi Das, Sabyasachi Das, Anirban Kar, Dileep Mookherjee, Tridip Ray, Prachi Singh, Naveen Thomas, and Shekhar Tomar for several useful comments and suggestions.

small firms for whom credit-constraints are most likely to bind. Our paper brings empirical evidence in this matter by examining a nationwide bank expansion policy in India.

The empirical challenge in causally identifying the relationship between local financial infrastructure and firm performance is the endogenous selection of locations by financial institutions. We overcome this by exploiting an 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 classified select districts[2] as "underbanked" and encouraged banks to open branches in these underbanked districts. The classification of a district as underbanked was determined by comparing districts' bank branch density to the national average bank branch density. Specifically, a district was assigned to underbanked status if its bank branch density in 2005 was less than the prevailing national average. As described in Chowdhury and Ritadhi (2022), the policy was targeted towards private banks and while no explicit targets were provided, the implicit quid-pro-quo was that annual branch expansion plans of private banks would be favourably received by the RBI, subject to their compliance with the BAP.[3]

We exploit the above rule in the spirit of a regression discontinuity (RD) design to estimate the causal impact of bank branch expansion on firm outcomes. While the policy was unveiled in the latter half of 2005, the sorting of districts to underbanked status was undertaken using bank branch data from March 2005, and population data from 2001, limiting banks' ability 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.

We identify the impact of the BAP on firm outcomes obtained 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

[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.
[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).

establishments' exposure to the BAP being determined by their 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 implication of the bank branch expansion across small and micro-enterprises in the manufacturing sector.

Our empirical strategy exploits the inherent time-variation in the establishment-level panel to extend the RD design and consider 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 around the discontinuity threshold.

We begin our empirical analysis by verifying the findings of Young (2017), Khanna and Mukherjee (2021) and Chowdhury and Ritadhi (2022), and confirming that the policy intervention affected private bank operations in underbanked districts. Using a cross-sectional RD framework, we document 73 percent higher growth in cumulative private bank branch openings between 2006 and 2010 in underbanked districts, amounting to 7 additional private bank branches being opened (relative to 9 private bank branch openings in non-underbanked districts). Critically, we identify no discontinuous change in government-owned (private) bank branch openings in underbanked districts in the post-BAP (pre-BAP) period. This allays concerns that the expansion in private bank branches in underbanked districts was an upshot of a secular growth in financial infrastructure in these regions, or pre-existing trends in private bank branch expansion.

The cross-sectional results are robust to the differences-in-discontinuity specification using an eleven-year district-level panel of bank branch openings. We use district fixed effects to control for time-invariant unobservable district characteristics which might be correlated with local financial infrastructure and identify a 20 percent increase in annual private bank branch openings in underbanked districts in the post-treatment period. Using an event-study specification, we confirm that additional private bank branch openings in underbanked districts occurred primarily in the years 2007, and 2009-2011. The increase in branch openings is also accompanied by an increase in private bank branch credit, especially to the farm and manufacturing sectors. There is however no evidence of a crowding out of government bank credit in response to the increased credit disbursement from private banks.

Having confirmed that the policy intervention indeed incentivized private banks to expand financial intermediation in underbanked districts, we apply the differences-in-discontinuity design to identify the impact of the BAP on manufacturing outcomes. Our primary outcome of interest is capital expenditure by manufacturing establishments. We identify a 4 percentage point increase in capital spending (equivalent to an INR 1.6 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 annual increase in manufacturing investment in underbanked districts is the largest in the year 2009, coinciding with the increase in private bank branch expansion, and private bank credit. The increase in manufacturing investment in underbanked districts is also accompanied by increased credit uptake by manufacturing establishments in these regions.

Our baseline results are robust to alternate functional forms and restricting capital expenditures to investments in plant and machinery, confirming that the increase in manufacturing investment was driven by an expansion in productive capital. 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.

We next explore the distributional implications of the BAP, and assess whether the increase in manufacturing investment in underbanked districts is driven by enterprises more likely to be credit-constrained. Existing literature shows that credit constraints are more likely to be binding 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 to be concentrated amongst establishments located in the bottom two quartiles of establishment size – specifically, establishments hiring less than 22 workers – which witnessed an increase in annual capital expenditures by 5 percentage points. Motivated by Criscuolo et al. (2019), we also examine heterogeneity across small and young establishments. Indeed, we find an 11 percentage point increase in capital spending for small and young establishments in underbanked districts. While there is a weak increase in capital spending for small and older establishments, we find little impact on investment by larger establishments. 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.

We examine three potential channels to explain the increase in manufacturing investment in underbanked districts: namely, a reduction in the cost of credit due to heightened lender competition; improved selection of borrowers by private banks; and aggregate demand. We find little evidence in favour of the first channel: borrowing costs for manufacturing establishments are unaffected by the policy intervention. Instead, we find support for the hypothesis that the quality of credit intermediation improved: the expansion in manufacturing investment was driven by establishments with higher pre-treatment productivity and returns to capital, consistent with the proposition of Fafchamps and Schündeln (2013). Finally, we draw from Mian and Sufi (2014) to examine the aggregate demand channel: specifically, we show that the increase in capital investment in underbanked districts is undertaken by establishments across both tradable and non-tradable industries. If the aggregate demand channel was the sole explanation for our findings, we would have expected the treatment effects to be driven exclusively by establishments in non-tradable industries which typically cater to local demand.

We conclude our empirical analysis by examining the impact of the policy intervention on other establishment-level, and aggregate manufacturing outcomes. We find limited evidence that the increase in capital expenditures in underbanked districts positively affected establishment output, hirings or returns to labour. We find weak evidence of a positive impact on value addition and productivity, driven by smaller establishments. Aggregating our data to the district-industry level, we confirm the increase in aggregate manufacturing investment in underbanked districts, and find noisy evidence suggestive of an increase in average revenue productivity and establishment entry.

In summary, our findings are consistent with the explanation that the BAP incentivized private banks to expand operations in underbanked districts, which in turn led to an increase in manufacturing investment by establishments for whom credit constraints were most likely to bind. This largely occurred as private banks were able to screen borrowers more effectively, leading to investments by productive firms. A plausible explanation could be that the increased physical proximity of private banks reduced information and monitoring costs, allowing credit disbursement to smaller – but productive – establishments with low collateral, who in turn undertook capital investments. This translated into an aggregate increase in capital spending at the regional level, affecting overall productivity.

1.1 Contributions

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 resulted in higher credit to small informal 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 an RD design and shows that the expansion of private bank branches in previously underserved areas increased capital investment and credit growth of registered manufacturing establishments. To this effect, our results 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), our paper exploits 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 an expansion in financial infrastructure on enterprises ubiquitious across developing economies (Hsieh and Olken, 2014).

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 micro-entrepreneurship. 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 and prior lending experience, financial institutions can assist in the capital accumulation of small and micro-enterprises with limited collateral. Our findings offer suggestive evidence consistent with improved screening of borrowers due to the greater proximity of lenders to firms, resulting in increased capital investments by small and micro establishments with high ex-ante returns to capital (Petersen and Rajan, 1994; Berger and Udell, 1995; Chen et al., 2015). Consequently, our paper adds 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) and Kochar (2011) who focused 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] Our paper confirms the findings of Young (2017) who shows an aggregate impact on state-level manufacturing investment and output, but focuses instead on the distributional aspects of the increase in manufacturing investment. The focus on small and micro-establishments also distinguishes our paper from Chakraborty et al. (2021) who exploit a later reform aimed at expanding bank branches in relatively smaller urban centres to show how increased lender competition disciplines government-owned banks.

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 three potential mechanisms explaining our results; and Section 6 reports

[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.

aggregate effects of bank branch expansion on the manufacturing sector.

2 Background and Policy Intervention

In the aftermath of a major episode of bank nationalization in 1969, the 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 the subject of study of Burgess and Pande (2005) and Kochar (2011). 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 discipline.[5]

In 2005, the RBI initiated a "liberalised branch authorisation policy", by which it attempted to simplify 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 and low competition. The existing system of case-by-case approvals for new branch openings was to be replaced by an annual approval of individual bank groups' 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

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

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

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

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 branch per capita. Using this rule, the RBI published in September 2005 a list of 386 "underbanked" districts.[6].

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 have lead private 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 falling around the threshold 0. For instance, 304 districts (211 underbanked and 93 non-underbanked) fell within a bandwidth of 20 around the threshold of 0, while shrinking the bandwidth to 15 and 10 provides samples of 231 and 156 districts, respectively. The concentration of a large set of districts around the discontinuity threshold provides statistical power to execute our empirical strategy, and also limits concerns regarding external validity.

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.

[6] 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`)

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 (NSS), 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.[7] 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.[8]

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 2009, while establishment identifiers were provided for the period between 1998 and 2014. 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 11 year establishment-level panel between 2001 and 2011, covering almost 18,000 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 closing and opening values of enterprise net fixed assets in a year, scaled by the average value of establishment fixed assets during the year. Specifically, for establishment i in year t, we define capital expenditures as:

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

where NFA is establishment fixed assets. The principle advantage with this formulation of capital spending is that the variable is bounded between -2 and 2, reducing sensitivity to outliers Berton et al.

[7] Such establishments are typically surveyed once every 3 years.
[8] These two legal statutes governs the operations of registered enterprises in India.

(2018). In addition to capital investment, we also consider other outcomes such as, credit growth, output, value-addition, workers hired, salaries paid and productivity. 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. All growth variables are defined as per equation (3).

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. Thus, the average establishment had fixed assets (machinery) equaling INR 40 (29) million, but the median establishment fixed asset was INR 3 (1) 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 can be classified as "small".[9] Over 80% of the establishments satisfied the definition of small-scale industries, making them eligible for subsidized bank credit.[10] The median establishment age was 15 years.

Focusing on our primary outcome of interest – we see that average annual capital expenditures equaled .023, equivalent to INR 0.8 million.[11] However, capital expenditures for the median establishment was -0.066 – an effective reduction in net fixed assets with depreciations exceeding capital spending. Capital expenditures are inherently lumpy, and we define the binary variable $AnyCapex_{it}$ to equal 1 if the closing value of net fixed assets exceeded the opening values, or $Capex_{it1} > Capex_{it-1}$. Attesting to the inherently lumpy nature of capital investments, we see that only a third of the establishments engaged in any positive capital spending in a given year.

While the ASI does not record the source of credit, it does inform us of outstanding loans for establishments. Based on closing and opening values of outstanding establishment loans, we apply the formulation of equation (5) and compute the average annual loan growth to be 4 percent or INR 1.2

[9] 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.

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

[11] Average capital expenditures equaled INR 41.416.

million.[12] The median establishment however saw no increase in 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.[13] 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 15%, while the average cost of credit was 25%.[14]

3.1.1 Basic Statistical Returns

We use publicly available data from the Basic Statistical Returns (BSR), hosted by the RBI, to assess the impact of the BAP on bank branches, deposits and credits. The BSR annually aggregates this 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 2010.

Aggregate trends point to an increase in private bank branch openings after the adoption of the BAP. For instance, in 2005, the median private bank branch density across the full sample of 556 districts was 0.66 branches per million population, and almost 45 percent of districts had no private bank branch. By 2010, the median private bank branch density had increased to 2.85 bank branches (per million persons), and the fraction of districts without a single private bank branch had fallen below 20%.

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 a causal estimation of the branch expansion policy

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

[13] 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.

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

on manufacturing investment. As data on both enterprise and banking outcomes are available over multiple time periods, we exploit the time-variation in our datasets to extend the RD design and apply a differences-in-discontinuity design. The differences-in-discontinuity design is alike a standard differences-in-difference design, with the inherent discontinuity in districts' classification as underbanked being the source of variation in assigning districts to treatment and control status. Akin to the RD design, this ensures ex-ante comparability of treatment and control groups in the neighbourhood of the discontinuity threshold in the pre-treatment period. The differences-in-discontinuity design therefore 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:

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

where Y is the outcome of interest for establishment i, located in district d, and observed in year t. α denotes establishment fixed effects, partialling out time-invariant establishment-level factors affecting the outcome of interest, while δ 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.[15] $\mathbf{X}$ includes a quadratic in establishment age, dummies for establishment ownership categories, and district covariates.[16] We opt for two-way clustering of standard errors – by district and industry (4-digit) for inference. Regressions are weighted with establishment-specific weights provided by the ASI.[17]

[15] 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.

[16] 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.

[17] The weights equal the inverse of the sampling probability. For establishments surveyed every year, the assigned

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 situated 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).[18] 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:

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

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 $\beta_j = 0 \ \forall \ j \in \{-5, ..., -2\}$

3.3 Pre-Treatment Covariate Balance

Prior to discussing our empirical 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 A1 and A2 undertake covariate balance checks using pre-treatment district covariates based

weight is 1.

[18] 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.

on 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 for the pre-treatment covariates at the discontinuity threshold. Appendix Figures A3 and A4 replicate this exercise for pre-treatment establishment-level manufacturing outcomes of interest.[19] Akin 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 A2-A5 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.[20] Across all pre-treatment characteristics, we only find a statistically significant impact at the 10% level for the fraction of Muslims in a district. Collectively, Appendix Figures A1-A4 and Tables A2-A5 confirm that within a narrow window of the discontinuity threshold, 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 the impact of the policy intervention on bank branch openings and credit disbursement. Next, we identify how the policy intervention affected capital investment by registered manufacturing enterprises. We subsequently discuss mechanisms explaining the results, and finally explore aggregate effects.

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

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

4.1 Private Bank Branches and Credit in Underbanked Districts

4.1.1 Bank Branch Openings

To assess the BAP's impact on local financial infrastructure, we begin by comparing cumulative bank branch openings between 2006 and 2010 across underbanked and non-underbanked districts located within a narrow bandwidth around the discontinuity threshold. Figure 3 undertakes a graphical comparison using the methodology of Calonico et al. (2020). The top left panel 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 signicant at the 1% level and suggests a near 60% increase in private bank branch openings in underbanked districts over this period. The top right panel includes pre-treatment district covariates while the one on the bottom considers a quadratic fit. The inclusion of district covariates does not affect the discontinuity estimate (top right panel, 3), or its statistical precision. The quadratic fit reduces the discontinuity estimate, but the coefficient remains statistically significant at the 1% level. Overall, we find that private bank branch openings increased by 48-73 percent. Two placebo tests confirm that this increase in private bank branch openings cannot be explained by either a) prior trends in private bank banch openings; or b) an overall expansion of financial infrastructure in underbanked districts: Appendix Figure A5 confirms that cumulative private bank branch openings between 2001 and 2005, and cumulative government-owned bank branch openings between 2006 and 2010 were comparable across underbanked and non-underbanked districts.

The average non-underbanked district witnessed 8.6 private bank branch openings between 2006 and 2010. Based on the discontinuity estimates, this implies that the BAP resulted between 4-6 additional private bank branch openings in the average underbanked district. The treatment effect is economically significant when considering that underbanked districts saw only 2 private bank branch openings between 2001 and 2005. Compared to the "social banking" era, a back of the envelope calculation suggests that the BAP's impact was between a third and a fourth of the impact of the state-driven push to expand banking infrastructure in rural unbanked locations.[21]

We next use the differences-in-discontinuity specification described in equation (4) to identify the

[21] 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 are most likely to reflect an upper bound.

treatment's impact on new bank branch openings, exploiting the inherent discontinuity in districts' assignment to underbanked status. This specification allows us to exploit the panel characteristics of our data and flexibly control for time-invariant trends in local financial infrastructure. In particular, as banks were accorded significant flexibility 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 Table 2 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 20 percent 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 to account for the large number of districts with no new branch openings in a year.[22] Lastly, columns (5)-(8) identify a null effect on government-owned bank branch openings – the coefficients are negative, and statistically non-significant. This again assuages concerns that the increase in private bank branches in underbanked districts can be explained by an overall convergence effect, leading to a secular increase in financial infrastructure 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 Figure 5 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 higher. 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. Consistent with the policy's

[22] 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 (**?**).

focus on private banks, we find no differential effect in the post-treatment period on government bank branch openings in underbanked districts (right-hand panel).

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 A6 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 upon focusing upon a single set of districts located within 15 bank branches per capita of the discontinuity threshold.

4.1.2 Bank Credit Disbursement

The above findings show that the BAP had a positive impact on private bank branch openings in underbanked districts. We next identify whether the policy also affected credit disbursement, or whether the new branches served primarily as deposit collection centres. We use our differences-in-discontinuity specification and identify the BAP's impact on credit disbursement across both the extensive and the intensive margins. In Appendix Table A6, the outcome of interest in the odd-numbered columns is the (logged) amount of outstanding credit and in the even-numbered columns, the (logged) number of credit accounts. The sample is restricted to districts within a bandwidth of 15 around the discontinuity threshold.

Panel A shows the results for credit disbursed by private bank branches. Columns (1) and (2) identify a positive but statistically insignificant (p-values .236 and .156 respectively) coefficient estimate corresponding to aggregate credit disbursement. The remaining columns consider sectoral allocations: while all the point estimates are positive, they are statistically significant for farm credit (extensive and intensive margins), manufacturing credit (intensive margin), and services and personal loans (intensive margin). The treatment effect for manufacturing credit is large, but that's also reflective of the low volume of private bank credit in these districts, resulting in large percentage changes. Nonetheless, relative to the pre-treatment mean manufacturing credit from private banks in non-underbanked districts, the coefficient estimate in column (5) of Appendix Table A6 points to a INR 700 million increase in manufacturing credit from these banks.[23] Consistent with the absence of government bank

[23] In perspective, the aggregate outstanding loans for registered manufacturing establishments in non-underbanked districts in the pre-treatment period was INR 185 million.

branch openings in underbanked districts, Panel B of Appendix Table A6 fails to identify any impact on government bank credit across any of the sectors, ruling out the possibility of a crowd out of government bank credit due to credit expansions from private banks.

Appendix Figures A7-A10 show event-study plots corresponding to the differences-in-discontinuity results shown in Table A6. Along both the intensive and extensive margins, we find no evidence of any differential trends in private bank credit prior to the policy intervention. In succeeding years, there is an upward trend in credit disbursement, especially for farm and manufacturing credit. Consistent with the timing of private bank branch openings in underbanked districts, there is a sizeable and significant increase in outstanding farm and manufacturing loans since 2008, and continuing till the end of our sample period in 2011. In contrast, the event study plots for government-owned banks exhibit no consistent trend in credit disbursement; neither is there any differential trend prior to the policy intervention (Appendix Figures A9 and A10).

In summary, this section affirms that the BAP generated a sharp increase in private bank branches in districts classified as underbanked, relative to observationally equivalent non-underbanked districts. This increase in private bank branches cannot be attributed to either a secular expansion in financial infrastructure during this period, or pre-existing trends in private bank expansion in these districts. The expansion in private bank branches was also accompanied by a corresponding increase in private bank credit to the farm and manufacturing sectors. We now identify whether this policy-induced expansion in financial intermediation by private banks was accompanied by higher manufacturing investment in underbanked districts.

4.2 Bank Branch Expansion and Manufacturing Investment

Having established that the policy intervention incentivized private banks to expand operations in underbanked districts, we now use the differences-in-discontinuity design to identify the treatment's impact on manufacturing investment. Column (1) of Table 3 includes establishment and 2-digit industry year fixed effects, along with establishment-specific covariates. The industry-year fixed effects limit our comparison to establishments operating in the same broad industry and year, with the identifying variation arising from districts' underbanked status. Specifically, these fixed effects absorb industry-specific time-varying demand and productivity shocks common to all firms operating in the broad industry category. The identified treatment effect is positive and statistically significant, indicating that the average manufacturing establishment in underbanked districts saw a 3 percentage point increase in capital expenditures in the post-treatment period, relative to manufacturing establishments in observationally equivalent non-underbanked districts. Column (2) shows our preferred specification where we also include district covariates, causing a slight increase in the coefficient of interest. Column (3) replaces the quadratic in establishment age with age fixed effects, while column (4) replaces 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 coefficients in both instances remain highly comparable to those obtained with our preferred specification in column (2).

Focusing on our preferred specification , the coefficient implies a 4.2 percentage point or INR 1.6 million increase in capital spending in underbanked districts, relative to observationally equivalent establishments in non-underbanked districts.[24] The coefficient estimate is economically large when considering that the pre-treatment median establishment fixed capital in non-underbanked districts was INR 2.4 million.

Appendix Table A7 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 fixed assets. Section A1 acknowledged the lumpiness of capital investments and noted that only a third of the establishments undertake 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 fixed assets was in excess of the opening value, and zero otherwise. We find that the treatment increased establishments' likelihood of engaging in any positive capital spending by 4 percentage points. With 57 manufacturing establishments operating in the pre-treatment period in the average non-underbanked district, this implies capital investments by 2 additional manufacturing establishments in the post-treatment period. Finally, columns (3)-(5) show that our results are very similar if we restrict capital expenditures to only investments in plant and machinery (excluding investments in land, buildings and transportation). This affirms that the increase in capital spending by manufacturing establishments in underbanked districts was driven by investments in productive capital.

The results discussed till now causally identify a significant increase in capital spending for

[24] In the pre-treatment period, the mean establishment fixed assets in non-underbanked districts (average of net opening and closing values) equaled INR 37.5 million. A 4.2 percentage point increase amounts to 1.6 million.

registered manufacturing establishments in underbanked districts while Section 4.1.2 documented an increase in manufacturing credit from from private banks in these regions. Appendix Table A8 directly identifies the policy intervention's impact on manufacturing credit at the level of establishments. 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 11 percentage point increase in credit growth for manufacturing establishments in underbanked districts – equivalent to INR 3 million – and a 38 percent growth in new credit. 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 capital spending by manufacturing establishments in underbanked districts, as well as the increased disbursement of manufacturing credit by private banks in these districts. 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 the credit market. Finally, column (6) identifies a null effect 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 again 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 are two-way clustered by district and industry, and the sample is restricted to establishments located in districts within a bandwidth of 15 around the discontinuity threshold.

Figure 5 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, investment in plant and machinery, 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. For instance, for capital expenditures, the coefficient estimates are positive and statistically significant at the 5% level in the years 2007 and 2009. The latter year in particular coincides with the period in which private bank branches and private bank credit witnessed the largest expansions in underbanked districts. While we are unable to determine the source of credit, the bottom panel of Figure 5 identifies a positive and significant increase in credit growth during the years 2009 and 2010, 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.

4.2.1 Robustness Checks

We subject our baseline results to a number of robustness checks. Columns (1) and (2) of Table 4 show that our results are unchanged if we do not weight the specifications with the establishment-specific weights (column 1); or cluster only at the level of district (column 2) – the level at which the treatment varies. 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.

All specifications discussed till now restricted the primary sample till 2011 as the RBI introduced a new branching policy in that year, encouraging both private and government-owned banks to open branches in relative small urban centres. Column (4) relaxes this restriction and uses data till 2014 to show 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 3. 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.[25] 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 capital expenditures, coinciding with the initiation of the policy intervention.

Our primary sample covers establishments operating in districts located within a bandwidth of 15 (branches per million persons) around the discontinuity threshold. Figure 6 shows that our results are not sensitive to alternate bandwidth choices. Specifically, we re-estimate our baseline specification

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

for bandwidths between 10 and 20 and plot the coefficients in Figure 6. For all the 20 bandwidths explored, we identify a positive and significant coefficient on capital expenditures.[26] Expectedly, the precision of the point estimates reduce at smaller bandwidths due to a reduction in sample size, but all the coefficients are significant at the 10% level (the noisiest estimate has a p-value of .078) and range between 0.03 and 0.05 in magnitude, affirming that our results are not dependent on any specific bandwidth.

Figure 7 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 7, the coefficients are not sensitive to the exclusion of any single state or industry – all the coefficient estimates remain positive, centred around 0.04, and statistically significant at the 10% level or better. This reassures us that the positive treatment effect on manufacturing investment is not driven by some confounding state or industry-specific place-based policy, the timing of which also coincides with the policy intervention of interest.

4.3 Distributional Impacts of Bank Branch Expansion

Section 4.1 documented an increase in both private bank branches and manufacturing credit from private banks in underbanked districts following the policy intervention, while Section 4.2 reported an increase in both manufacturing investment and credit growth for enterprises operating in these districts. 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. Thus, if information asymmetries and monitoring costs are a decreasing function of distance, then the proximity of financial infrastructure to manufacturing units can reduce barriers to credit access, allowing firms which where hitherto credit-constrained to undertake capital investments. Alternatively, if private banks are more efficient financial intermediaries, we would expect them to be more effective in screening creditworthy borrowers. As credit constraints are more likely to bind for smaller establishments, we begin by exploring treatment heterogeneity by establishment size.

To avoid the contamination of establishment size by the policy intervention of interest, we compute the average size of each establishment using the mean number of workers hired between 2001 and

[26] The first coefficient in Figure 6 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.

2004 and classify establishments as "large" or "small" based on the median establishment size in the pre-treatment period.[27] The specification of interest is:

$$Y_{idt} = \alpha_i + \delta_{jt} + \beta_1 Underbanked_d \times Post_t + \beta_2 Underbanked_d \times Large_i \times Post_t + f(Runvar_d) + \gamma \mathbf{X}_{idt} + \epsilon_{idt} \quad (6)$$

The double-difference coefficient corresponding to β_1 now compares capital investments across underbanked and non-underbanked districts for smaller establishments. The triple difference coefficient (β_2) identifies the differential effect on capital spending in underbanked districts for larger establishments. We consider treatment heterogeneity across both capital investment and credit growth.

Columns (1) and (5) of Table 5 explore treatment heterogeneity across "large" establishments. In both instances, the treatment effects are driven by establishments hiring less than 22 workers (median size). The double-difference coefficient in both instances is positive and statistically significant at the 1% level, while the triple difference coefficient is negative, albeit imprecisely estimated in column (1). The sum of β_1 and β_2 is not statistically distinguishable from 0, implying that the increase in capital expenditures in response to the policy intervention is concentrated amongst relatively smaller establishments. Columns (2) and (6) extend specification (6) using arbitrary size cutoffs to test for non-linearities in the treatment effect across the establishment-size distribution, with the omitted category, corresponding to β_1, being establishments hiring under 11 workers. The coefficient corresponding to β_1 continues to be significant at the 1% level, indicating that capital expenditures (credit growth) increased for these establishments in underbanked districts by an additional 5 (24) percentage points. While none of the triple interaction terms are statistically significant, they are negative, and the sum of the coefficients ($\beta_1 + \beta_2$) is only significant for establishments hiring between 12 and 22 workers. For the remainder of the establishment size distribution, the aggregate treatment effect in underbanked districts is not statistically distinguishable from 0.

Columns (1)-(2) and (5)-(6) show the increase in capital spending in underbanked districts was restricted to relatively small establishments, for whom credit constraints are also more likely to bind. However, while firm size is widely used as an indicator of credit-constraints, firm size is also endogenous

[27] 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.

to the firm. Thus, 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 young firms require time to scale up and are initially small due to operational and logistical constraints (including limited credit availability). Criscuolo et al. (2019) in particular showed that firms which are both small in size *and* young are most likely to be credit-constrained.

In columns (3) and (7) of Table 5, 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.[28] Consistent with Criscuolo et al. (2019), columns (3) and (7) confirm that the positive treatment effects are driven by establishments which are both small and young, indicating yet again that establishments most likely to face binding credit constraints responded most to the policy intervention.

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. Their empirical analysis instead recommends using firms' listing status to determine financial constraints.[29] In this regard, we exploit information 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 are driven entirely by establishments which are *not* publicly listed.

Columns (1)-(3) of Appendix Table A8 show that our results are very similar if pre-treatment establishment fixed assets are used to determine establishment size. 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 the findings of Banerjee and Duflo (2014), who documented

[28] 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.

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

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 by INR 0.27 million.

Column (4) of Appendix Table A8 consider 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 the locational proximity of banks to borrowers in underbanked regions reduces the need for pledgeable collateral, we would expect the treatment effects to be concentrated amongst establishments with relatively low collateral. Column (4) offer partial support to this hypothesis: while we fail to identify any heterogeneity across establishments with high (above-median) ex-ante collateral values, the uninteracted coefficient corresponding to establishments with low collateral is positive and statistically significant, signifying that the availability of large collateral was not a necessary condition for undertaking capital investment in these districts.

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 make individual proprietorships and family-owned enterprises our benchmark category and explore treatment heterogeneity across establishments classified as partnerships, private limited companies, government-owned/aided enterprises and public limited (listed) companies. 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 partnerships. Finally, column (6) shows little evidence of heterogeneity across urban and rural enterprises, although the triple interaction coefficient is negative.

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 an expansion in local financial infrastructure aided the alleviation of credit-constraints and allowed enterprises most likely to face binding credit-constraints to undertake capital investments.

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 are most likely to bind. We turn now to potential factors explaining this increase in manufacturing investment in regions which saw a sizeable increase in financial infrastructure. We consider three key channels: reductions in the cost of credit, improvements in borrower quality, and aggregate demand.

5.1 Reduced Cost of Borrowing

The entry of private banks can possibly increase competition amongst financial institutions, which in turn can affect the cost of credit. If increased competition amongst lenders reduces interest rates, this would manifest as a reduction in firms' marginal cost, and can lead to higher capital investment, or investment in capital or labour inputs. If previously banks charged higher rates of interest to small informationally opaque firms or new firms with limited networks, it is possible that increased lender competition can increase small firms' access to credit markets through lower costs of borrowing. Appendix A8 however shows no evidence of a decline in lending rates. If anything, there's weak evidence suggesting that interest rates increased for smaller establishments, and declined slightly for larger establishments. Thus, the increase in manufacturing investment and credit growth in underbanked districts cannot be attributed to lower costs of borrowing in the face of heightened lender competition as a result of the policy intervention.

5.2 Improvements in Borrower Quality

Jayaratne and Strahan (1996) in their early work on branch deregulation show that the entry of new bank branches does not affect the overall volume of credit disbursement, but instead improves the quality of credit intermediation.[30] 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 a propensity to political capture Banerjee et al. (2004). If better corporate governance and superior technology of private banks facilitate the screening

[30] Jayaratne and Strahan (1996) uses bank-level data on non-performing loans to establish this channel. Unfortunately, data on non-performing loans at the level of districts is not publicly available, precluding us from directly testing this channel.

of creditworthy borrowers, this could explain the increase in manufacturing investment by smaller informationally opaque enterprises in underbanked districts.

In the absence of establishment-level data on credit received from private banks, we examine whether the increase in manufacturing investment was concentrated amongst productive establishments. This would be consistent with private banks being more effective at screening high quality borrowers. It would also be consistent with the overall findings of Table 3 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 four pre-treatment measures of firm productivity: namely revenue productivity, marginal product of capital, output per worker, and value-addition as a fraction of establishment assets. Additionally, we also consider pre-treatment interest rates and whether the firm used imported inputs in any year prior to 2005. Interest rates are often used as a proxy for credit risk, with higher rates of interest signaling riskier firms; firms using imported inputs arguably have access to better technology and are likely to have higher productivity.

The results in Table 6 suggest that the increase in manufacturing investment in underbanked districts was indeed driven by establishments of ex-ante higher quality. With the exception of value-addition (column (4)), the triple interaction coefficients across columns (1)-(3) is positive, albeit significant only for borrowers with high pre-treatment returns to capital (column (2)).[31] Importantly, the double-difference coefficient ($Underbanked \times Post$) is not statistically significant across either columns (1)-(3), suggesting that manufacturing investment was comparable across establishments in underbanked and non-underbanked districts in the post-treatment period for establishments with relatively low productivity, low marginal returns to capital and low output per worker. The sum of the coefficients is significantly different from 0 across columns (1)-(3), confirming that manufacturing investment increased in underbanked districts only for establishments which had relatively high productivity and high returns to capital.

Consistent with the above results, we identify a negative coefficient (albeit not statistically significant) corresponding to the interaction term identifying treatment heterogeneity across establishments with high pre-treatment borrowing costs. The double-difference coefficient is positive and significant at the 1% level, suggesting that the increase in manufacturing investment is driven by establishments

[31] The coefficient is weakly significant at the 10% level (p-value).

which had relatively low costs of borrowing in the pre-treatment period. Unexpectedly, the treatment has no impact on manufacturing investment by importers located in underbanked districts. This is possibly because importers are more likely to be larger establishments, and the increase in capital investment in driven entirely by smaller establishments.

Collectively, columns (1)-(5) of Table 6 offer evidence indicating that the increase in manufacturing investment was driven by establishments with high ex-ante productivity. This supports the explanation that private banks were able to effectively screen productive establishments, without having to rely on collateral values (Appendix Table A8). When considering that the increase in capital spending was undertaken by establishments which faced lower rates of interest, but had high marginal returns to capital, it again points to the presence of credit constraints, which were relaxed in the aftermath of private banks' entry to these regions.

5.3 Aggregate Demand

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

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 use the approach of Mian and Sufi (2014) and use the geographic dispersion of industries to classify them as tradable and non-tradable.[32] The intuition is that industries with a large degree of geographic dispersion are more likely to be non-tradable.

Column (6) of Table 6 fails to identify treatment heterogeneity across establishments in industries with relatively high geographic dispersion (tradable): the point estimate is positive but the confidence

[32] 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.

intervals are sufficiently wide to rule out a null effect. The double-interaction term is positive and significant at the 10% level (p-value .057), although smaller in magnitude, implying that capital spending in underbanked districts increased for establishments operating in non-tradable industries. The sum of the coefficients is however 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 aggregate treatment effect for establishments operating in industries with low geographic dispersion indicates that the aggregate demand channel cannot be the sole explanation for our findings.

6 Aggregate Effects of Bank Branch Expansion

We conclude our study by examining the aggregate effects of bank branch expansions. We first study how aggregate establishment level outcomes varied across underbanked and non-underbanked districts, and then consider outcomes aggregated at the district-industry level.

6.1 Other Manufacturing Outcomes

We begin by examining whether the increase in capital investments also affected establishment output, hirings, wage payments and productivity. As the increase in manufacturing investment in underbanked districts was concentrated amongst smaller enterprises, we explore treatment heterogeneity across large (size above median) establishments. The results are shown in Appendix Table A9. Panel A shows the average treatment effects, while Panel B examines heterogeneity by establishment size. There is little evidence that the treatment affected establishment output or hiring. Contrary to expectations, the coefficient on establishment output is in fact negative, albeit not statistically significant. Columns (3) and (4) shows that the policy intervention did not affect workers' hiring, although there is a negative impact on daily wages, driven primarily by larger establishments (column (4), Panel B). Finally, columns (5) and (6) offer suggestive evidence of increased productivity and profitability in underbanked districts: column (5) identifies a positive and significant effect on value-addition (as a share of assets), driven by smaller establishments (Panel B). Column (6) points to a positive impact of the policy intervention on establishment productivity, driven again by smaller establishments, although the standard errors in this case are large enough to not rule out a null effect (Panel B). In summary, Appendix Table A9 indicates that the expansion of financial infrastructure in underbanked districts had limited impact on establishments' aggregate production and hiring, although there is some weak evidence of an increase in establishment productivity.

6.2 Aggregate Effects

We conclude by identifying the aggregate effects of an expansion in financial infrastructure at the level of the district. As discussed in Section 3.1, our primary sample only includes establishments which were observed at least once both before the policy intervention, and after the policy intervention. This limits our ability to identify whether the expansion in financial infrastructure in underbanked districts affected establishments' entry or exit. For instance, if the entry of private banks also facilitated the entry of new establishments, which had higher capital investments and (or) productivity, our existing results would be an under estimate of the true impact of an increase in financial infrastructure. Similarly, if increased competition amongst lenders lead to a reallocation of credit only to the most productive firms, it can contribute to the exit of inefficient firms and raise aggregate productivity.

To this effect, we collapse the establishment-level data at the district-industry (2-digit) level, and explore the aggregate impact of an expansion in financial infrastructure on the manufacturing sector. We continue to use our difference-in-discontinuity design, but replace the establishment fixed effects with district fixed effects. We are able to retain the 2-digit industry-year fixed effects, ensuring that our identifying variation arises from variations in districts' underbanked status, while conditioning on aggregate time-varying industry-level shocks. The sample remains restricted to districts within a bandwidth of 15 from the discontinuity threshold and standard errors are clustered by district.

Table 7 shows these results. Columns (1)-(3) replicate the establishment-level results on capital investments and credit growth at the aggregate district-industry level. Reassuringly, we find the results to be directionally consistent with those obtained at the level of establishments. Thus, capital expenditures increased by 5 percentage points (significant at the 10% level) in industries located in underbanked districts, while credit growth too increased by 5 percentage points, although the coefficient is not precisely estimated. There is no impact on output or workers in columns (4) and (5), although the coefficient corresponding to output is positive and relatively large in magnitude.

Column (6) suggests a 6 percent increase in average industry-level productivity although the coefficient again is not precisely estimated (p-value .105) . Taken together, columns (7) and (8) suggest an increase in establishment entry, but offer no evidence of establishment closure. Thus, we identify a positive (but noisy) coefficient associated with the aggregate number of establishments in operation, which increases by 9 percent in underbanked districts. Relative to the pre-treatment mean number of establishments in each industry-district cell in non-underbanked districts, this suggests 2 additional establishments operating in the post-treatment period in underbanked districts. Column (8) shows that the policy intervention left unaffected the fraction of establishments which were reported to be closed.

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 by manufacturing 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, benefit more from increased access to bank branches.

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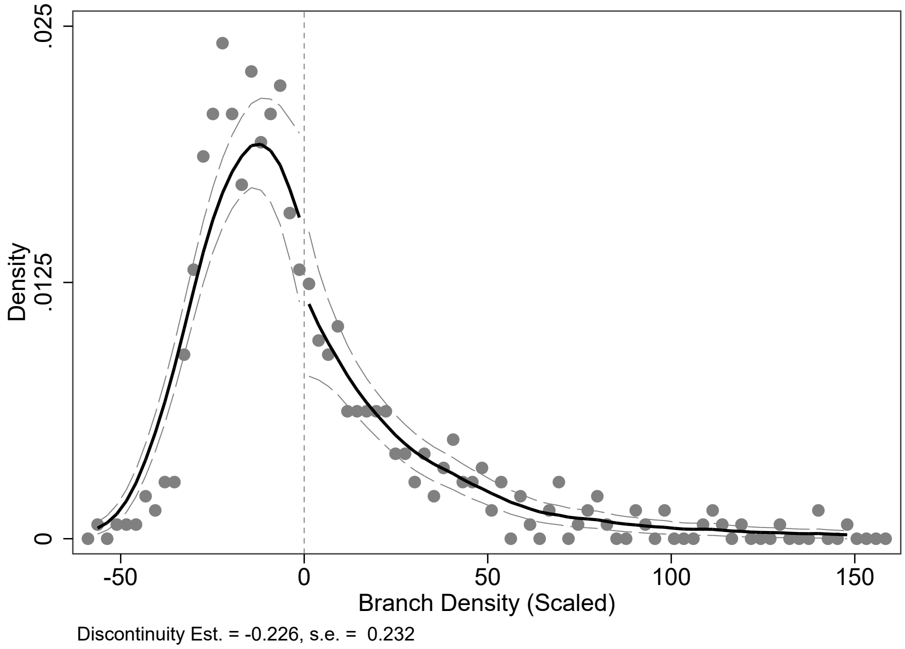
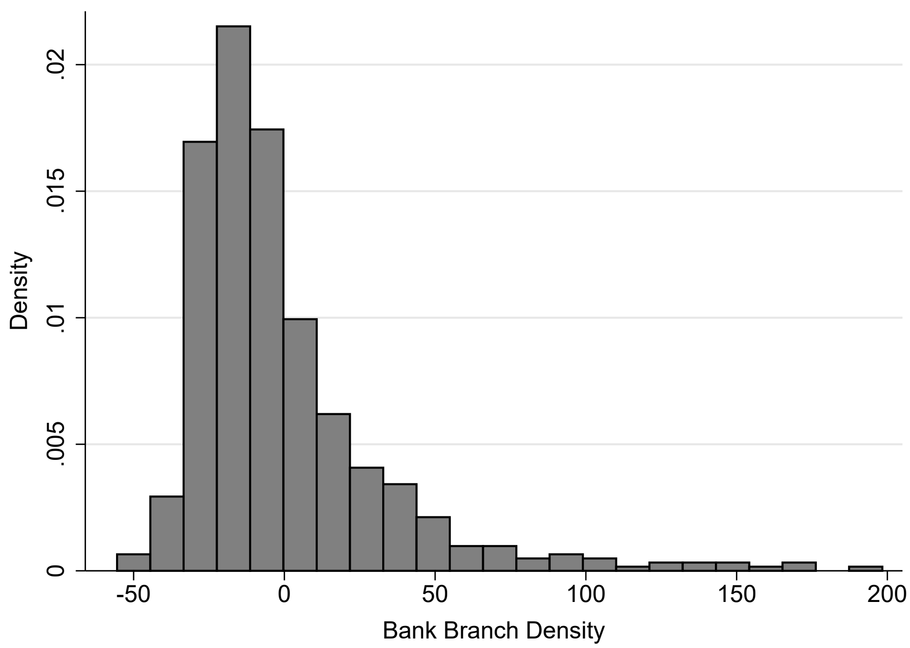
Figure 1: Selection of Districts Into Underbanked Status: McCrary Test



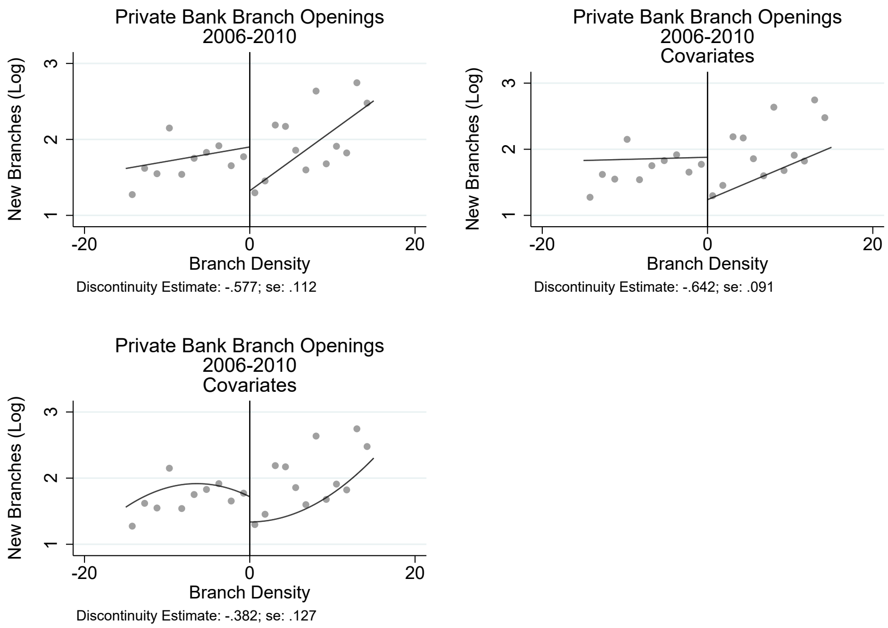
Notes: The above figure tests for bunching of the running variable around the threshold of 0 using the McCrary test. The solid line shows the local polynomial estimate, while the dashed lines show the 95% confidence intervals. The figure is replicated from Chowdhury and Ritadhi (2022).

Figure 2: Distribution of Running Variable



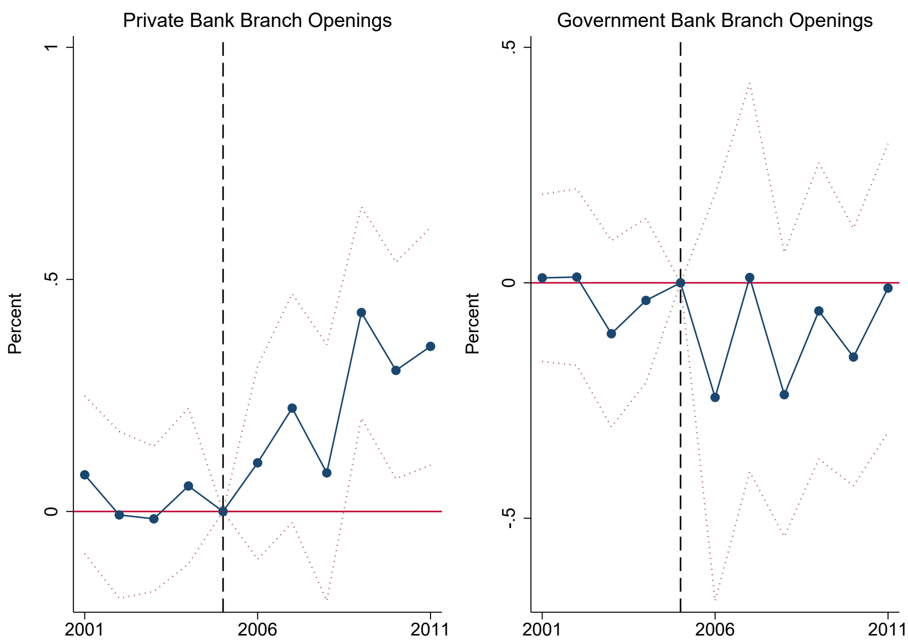
Notes: This figure shows the distribution of the running variable where the running variable is defined at the district level as $Runvar_d = BranchDensity_d - \mu_{BranchDensity}$. *BranchDensity* refers to the bank branch density in district d in 2005, while $\mu_{BranchDensity}$ is the national average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

Figure 3: Private Bank Branch Openings in Underbanked Districts



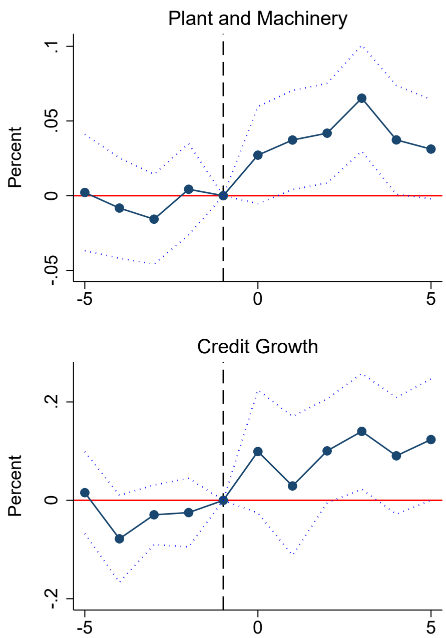
Notes: The above figure shows the impact of the policy intervention on district-level cumulative private bank branch openings between 2006 and 2010. The top-left panel shows the treatment effect only condition for state-region fixed effects. The remaining figures also condition for pre-treatment district 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). Standard errors are clustered by state-region. The figure is replicated from Chowdhury and Ritadhi (2022).

Figure 4: Bank Branch Openings in Underbanked Districts: Event-Study Plots



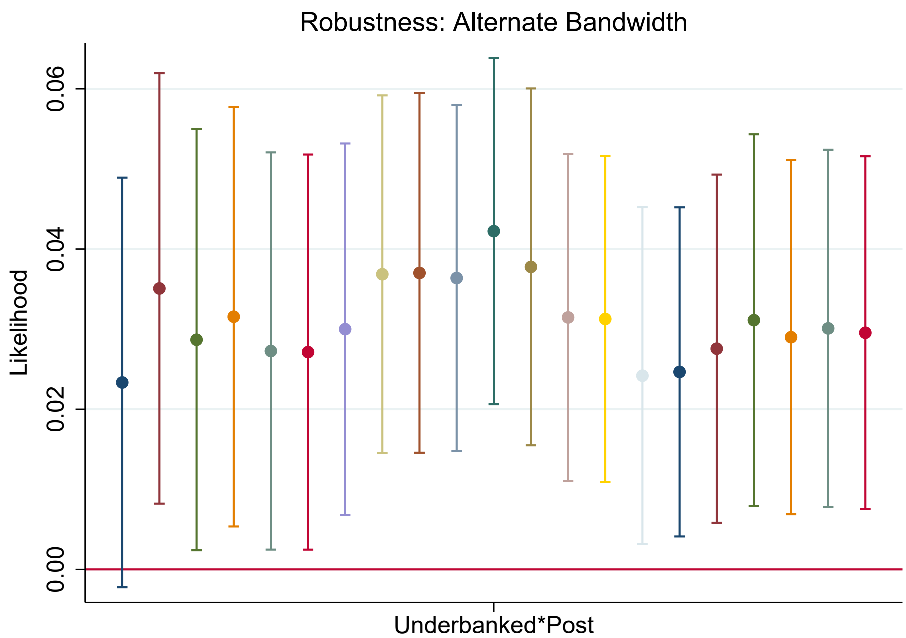
Notes: The above figures shows event-study plots estimating the impact of a district's underbanked status on annual bank branch openings. The unit of observation is the district. The outcome of interest in the left panel is the (logged) number of private bank branch openings; in the right panel, the (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 5: Capital Expenditure in Underbanked Districts: Event-Study Plots



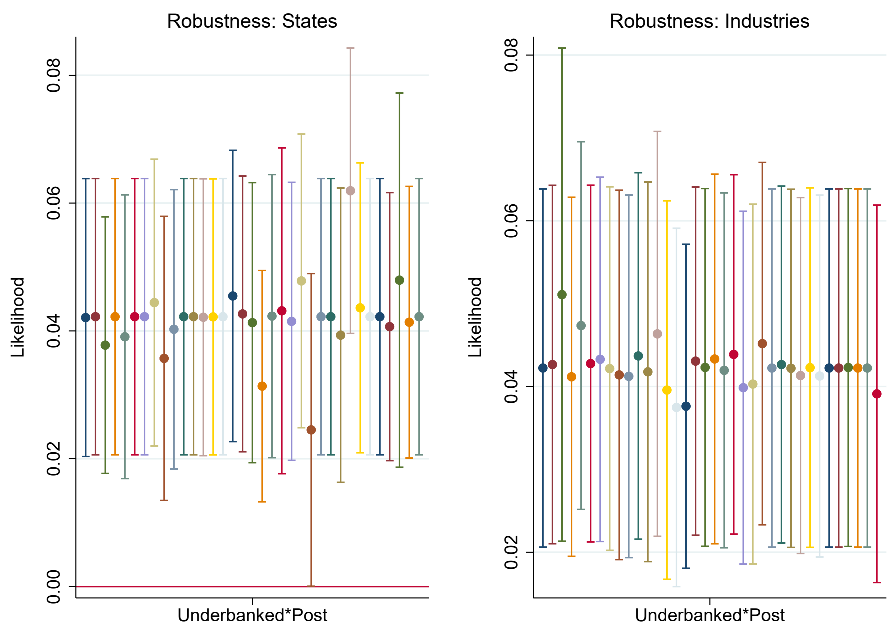
Notes: The above figure presents event-study plots estimating capital expenditures, plant machinery, and credit in underbanked districts. The unit of observation is the manufacturing enterprise. The outcome of interest in the top-right panel is capital expenditures (logged); in the top-left panel, capital expenditures excluding spending on land, buildings and transport (logged); in the bottom-right panel, credit growth (logged). 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 two-way clustered by district and industry.

Figure 6: 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 enterprise and the outcome of interest is capital expenditures (logged). 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 two-way clustered by district and industry.

Figure 7: 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 enterprise and the outcome of interest is capital expenditures (logged). 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 two-way clustered by district and industry.

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 constant 2005 millions of rupees. Growth variables are defined as in equation (3). Micro, small, small-scale, medium and large enterprises are defined according to administrative definitions.

Table 2: 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	.194**	.216**	.252**	.280***	-.113	-.098	-.149	-.133
	(.097)	(.085)	(.122)	(.107)	(.159)	(.122)	(.193)	(.149)
Observations	2280	2280	2280	2280	2280	2280	2280	2280
R^2	.55	.57	.55	.57	.55	.57	.57	.59
Control Mean	1.25	1.25	1.25	1.25	1.81	1.81	1.81	1.81
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 3: Manufacturing Investment in Underbanked Districts

	(1)	(2)	(3)	(4)
			Capital Expenditures	
Underbanked × Post	.029**	.042***	.041***	.038***
	(.014)	(.013)	(.013)	(.014)
Observations	70285	70285	70280	70264
R^2	.42	.42	.42	.42
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry-Year FE	Y	Y	Y	Y
Age FE	N	N	Y	N
Firm Controls	Y	Y	Y	Y
District Controls	N	N	Y	Y
Control Mean	.005	.005	.005	.005

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). All specifications include establishment, 2-digit industry-year fixed effects, a linear polynomial in the running variable, and establishment specific covariates. Columns (2)-(4) includes district covariates. Column (3) includes age fixed effects while column (4) replaces 2-digit industry fixed effects with 3-digit industry 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, two-way clustered by district and industry. Significant levels: *10%, **5%, and ***1%

Table 4: Manufacturing Investment in Underbanked Districts

	(1)	(2)	(3)	(4)	(5)
	Capital Expenditures				
	Unweighted	District Cluster	Exclude Districts	Long Term Effects	Placebo
Underbanked × Post	.035**	.042**	.037**	.040***	
	(.014)	(.016)	(.018)	(.012)	
Underbanked × Post 2001					.009
					(.024)
Observations	70285	70285	67487	91801	41888
R^2	.28	.42	.42	.37	.47
Control Mean	.015	.005	.005	.005	.001

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). 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, two-way clustered by district and industry in all columns except column (2). Significant levels: *10%, **5%, and ***1%

Table 5: 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	.059***	.053*	.114***	.061***	.196***	.236**	.218**	.127***
	(.020)	(.028)	(.035)	(.014)	(.066)	(.114)	(.100)	(.045)
Underbanked × Est. *Size* > Median × Post	-.034				-.141			
	(.029)				(.090)			
Underbanked × 11 > Est. *Size* ≤ 22 × Post		.015				-.076		
		(.051)				(.157)		
Underbanked × 22 > Est. *Size* ≤ 66 × Post		-.002				-.243		
		(.050)				(.151)		
Underbanked × Est. *Size* > 66 × Post		-.053				-.117		
		(.038)				(.124)		
Underbanked × Large, Young × Post			-.089**				-.186	
			(.044)				(.147)	
Underbanked × Large, Old × Post			-.090**				-.138	
			(.044)				(.102)	
Underbanked × Small, Old × Post			-.107*				-.043	
			(.061)				(.155)	
Underbanked × Listed × Post				-.136***				-.141
				(.032)				(.115)
Observations	70285	70285	70285	70285	52477	52477	52477	52477
R^2	.42	.42	.42	.42	.36	.36	.36	.36
Control Mean	.014	.014	.014	.014	.014			

Notes: This table identifies the treatment heterogeneity on manufacturing investment and credit growth across establishment size and age. The unit of observation is the manufacturing establishment. The outcome of interest in columns (1)-(4) is capital expenditures; in columns (5)-(8), 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. *Est. Size* refers to the pre-treatment average number of employees employed by the establishment. The establishment size cutoffs of 11, 22 and 66 corresponds to the 25th, 50th and 75th percentile of the pre-treatment distribution of establishment size. *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, twoway clustered by district and industry. Significant levels: *10%, **5%, and ***1%

Table 6: Manufacturing Investment in Underbanked Districts: Heterogeneity by Borrower Quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Capital Expenditures			
Underbanked × Post	.018	.016	.028	.045**	.075***	.048***	.030*
	(.022)	(.015)	(.020)	(.021)	(.024)	(.014)	(.018)
Underbanked × High TFP × Post	.048						
	(.035)						
Underbanked × High MRPK × Post		.053*					
		(.032)					
Underbanked × High Output × Post			.027				
			(.033)				
Underbanked × High GVA × Post				-.015			
				(.024)			
Underbanked × High Interest × Post					-.038		
					(.041)		
Underbanked × Importer × Post						-.050*	
						(.026)	
Underbanked × Tradable × Post							.027
							(.026)
Observations	58837	69236	70285	70285	58232	70285	67650
R^2	.41	.42	.42	.42	.42	.42	.42
Control Mean	.014	.014	.014	.014	.014	.014	.014

Notes: This table identifies the treatment heterogeneity on manufacturing investment across borrower quality. The unit of observation is the manufacturing establishment. The outcome of interest is capital expenditures. 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. *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, two-way clustered by district and industry. Significant levels: *10%, **5%, and ***1%

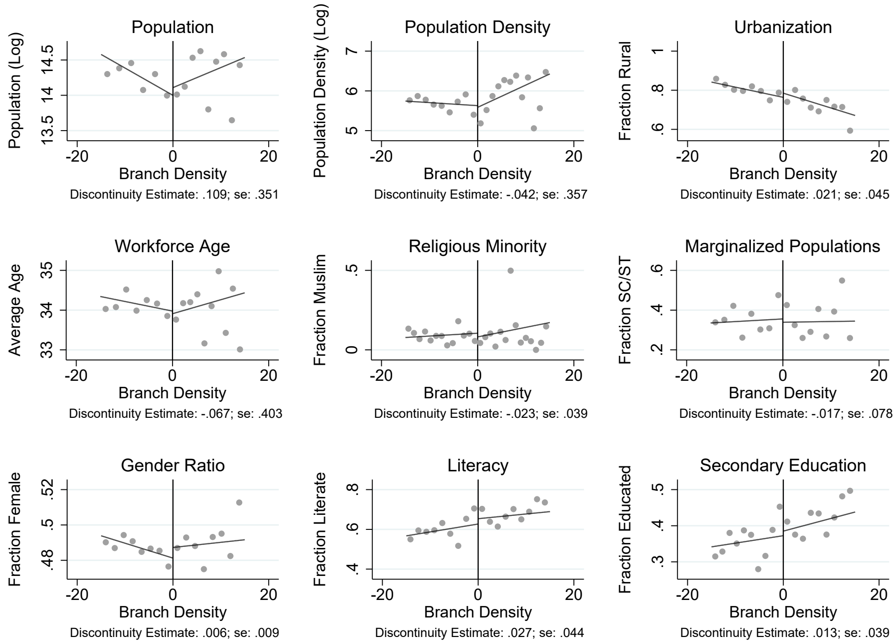
Table 7: Manufacturing Outcomes in Underbanked Districts: Aggregate Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Capital Expenditures	Any Capital Expenditures	Credit Growth	Output (Log)	Workers (Log)	Revenue TFP	Establishments (Log)	Pr(Closed = 1)
Underbanked*Post	.052*	.034	.051	.074	.034	.066	.092	-.019
	(.027)	(.023)	(.045)	(.124)	(.093)	(.042)	(.063)	(.028)
Observations	13530	14367	12112	13477	13493	11979	14367	14367
R^2	.07	.11	.05	.43	.41	.11	.43	.20

Notes: This table identifies the aggregate treatment effect on manufacturing investment. The unit of observation is the district-industry (2-digit). The outcome of interest in column (1) is capital expenditures, defined as in (3); column (2), probability of any positive capital expenditures; column (3), credit growth; column (4), aggregate output (log); column (5), total employees (log); column (6), average revenue TFP; column (7), number of establishments in operation; column (8), fraction of establishments which are closed. All specifications include 2-digit industry-year fixed effects, 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 are in parentheses, two-way clustered by district and industry. Significant levels: *10%, **5%, and ***1%

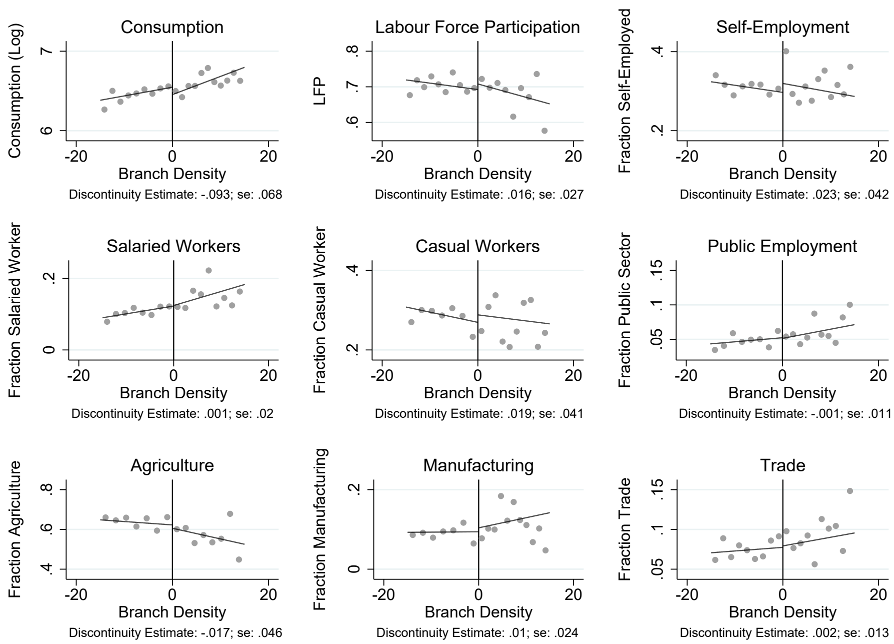
A Appendix: Additional Figures and Tables

Figure A1: Pre-Treatment Covariate Balance: District Demographics



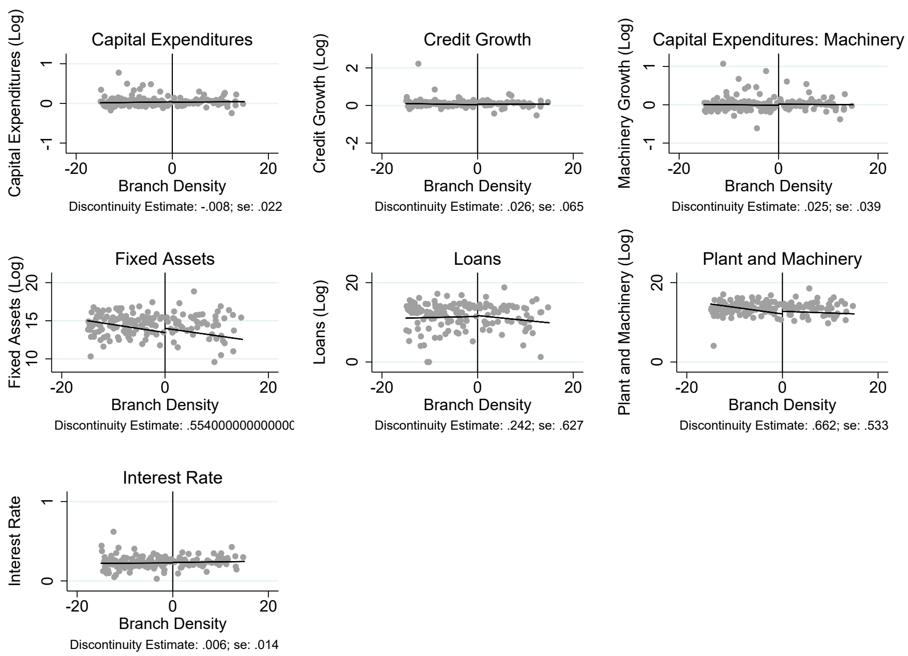
Notes: The above figure shows the pre-treatment covariate balance across district-specific demographic covariates. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as “underbanked”. The sample is restricted to a bandwidth of 15 around the threshold. 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 A2: Pre-Treatment Covariate Balance: District Demographics



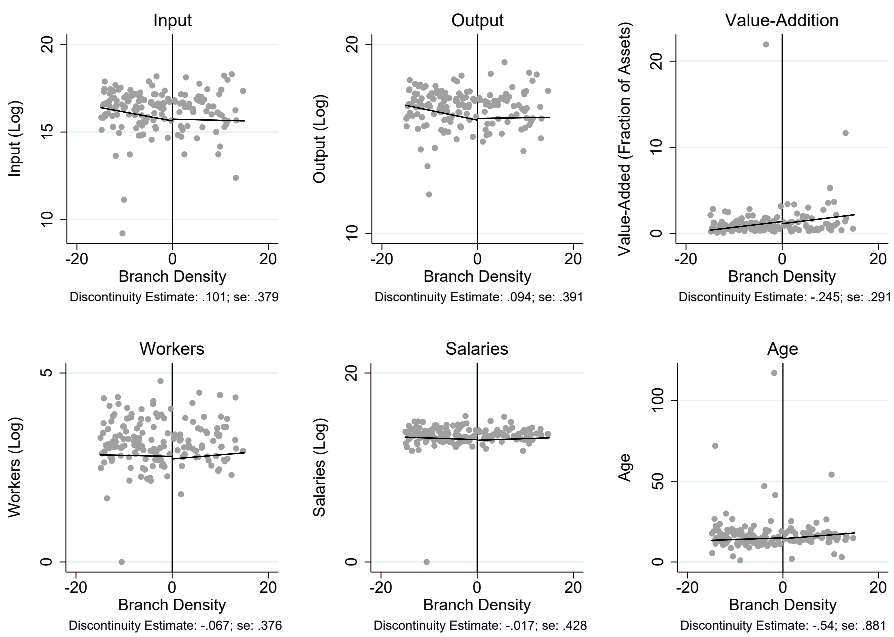
Notes: The above figure shows the pre-treatment covariate balance across district-specific demographic covariates. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". Consumption refers to households monthly per capita consumption. 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 A3: Pre-Treatment Covariate Balance: Manufacturing Enterprise Characteristics



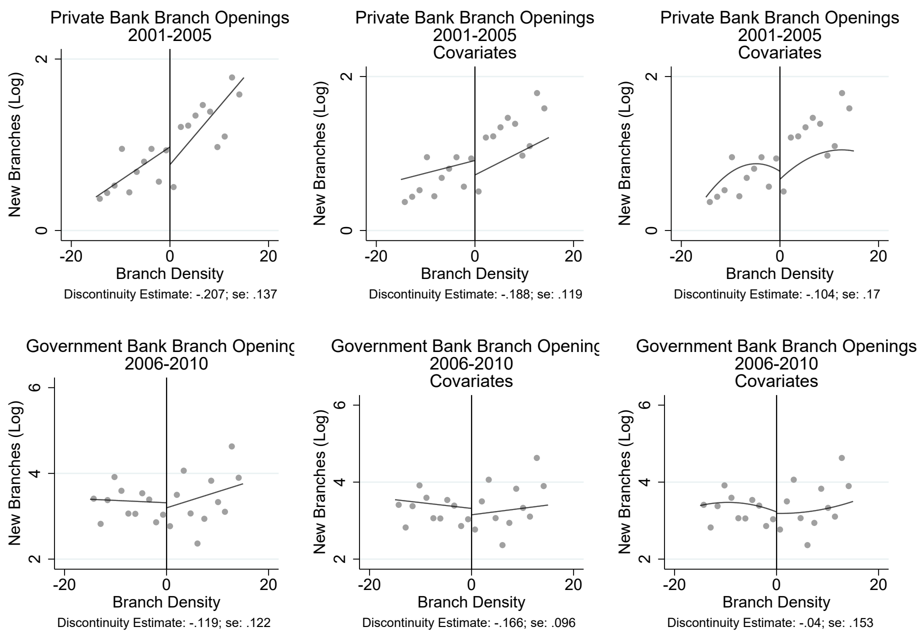
Notes: The above figure shows the pre-treatment balance across manufacturing enterprise characteristics. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". The sample is restricted to a bandwidth of 15 around the threshold. The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

Figure A4: Pre-Treatment Covariate Balance: Manufacturing Enterprise Labour Characteristics



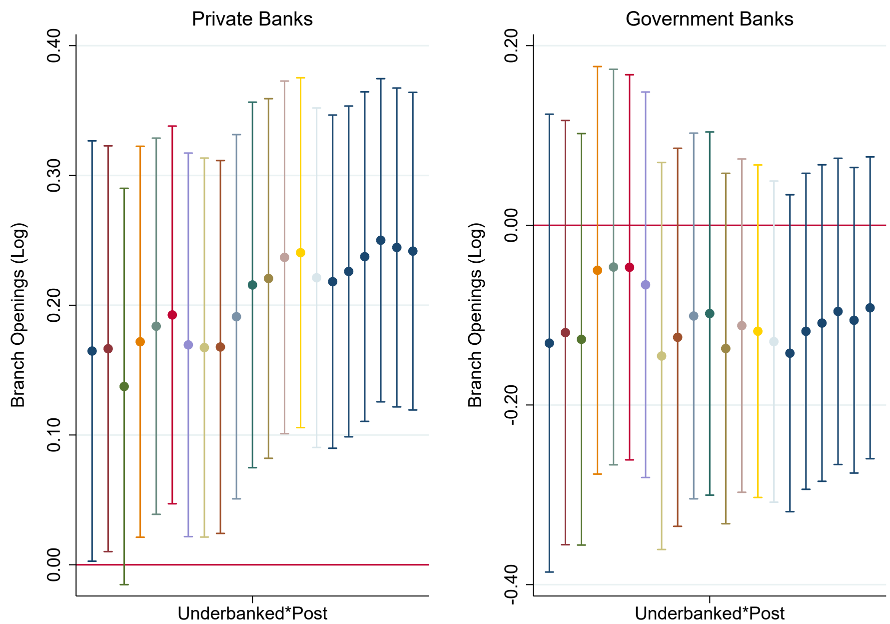
Notes: The above figure shows the pre-treatment balance across manufacturing enterprise characteristics. The vertical line denotes the national average threshold. Districts to the left of the cutoff are classified as "underbanked". Consumption refers to households monthly per capita consumption. The 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).

Figure A5: Branch Expansion Policy and Bank Branch Openings: Placebo Tests



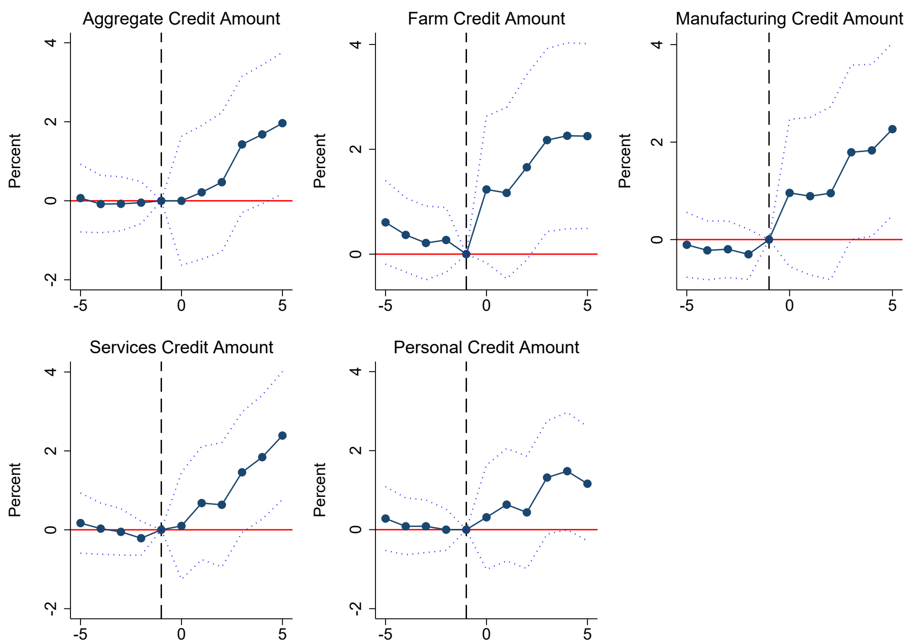
Notes: The above figure shows the impact of the policy intervention on district-level cumulative bank branch openings for a) government-owned bank branches between 2006 and 2010 (top row) and b) private bank branches between 2001-2005 (bottom row). The left panel shows the treatment effect only conditioning for state-region fixed effects. The remaining figures also condition 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). Standard errors are clustered by state-region.

Figure A6: 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 A7: Private Bank Credit in Underbanked Districts: Intensive Margin

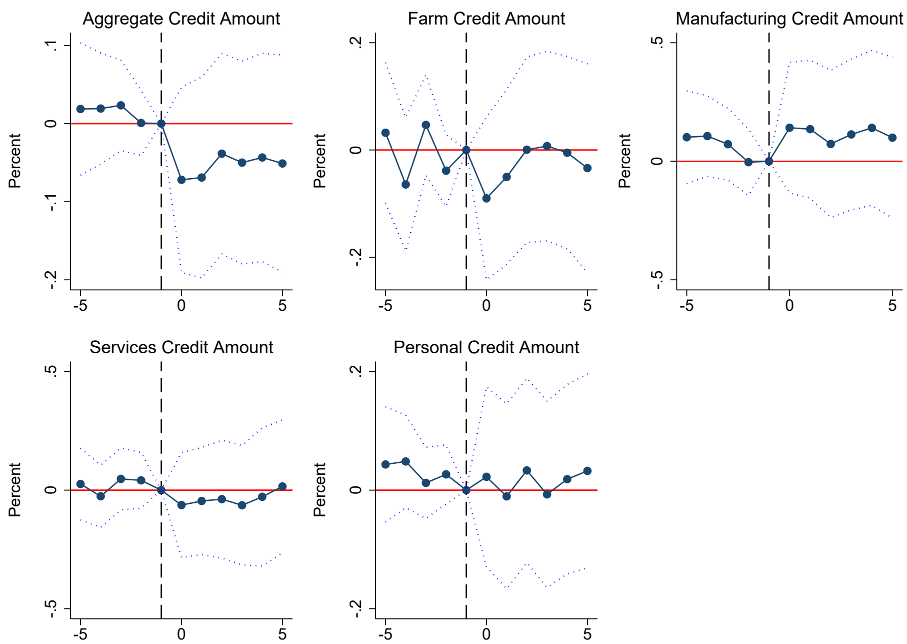


Notes: The above figure presents event-study plots to show the average annual treatment effects on private bank credit, along the extensive margin. The unit of observation is the district. The outcome of interest in each instance is logged outstanding credit in the above-mentioned categories. All specifications include district and year fixed effects, along with a linear polynomial in the running variable, and district covariates. The vertical line corresponds to the year 2005 – the year in which the BAP was implemented. The solid line shows the coefficient estimates; the dashed lines plot 95% confidence intervals. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold and standard errors are clustered by district.

Figure A8: Private Bank Credit in Underbanked Districts: Extensive Margin

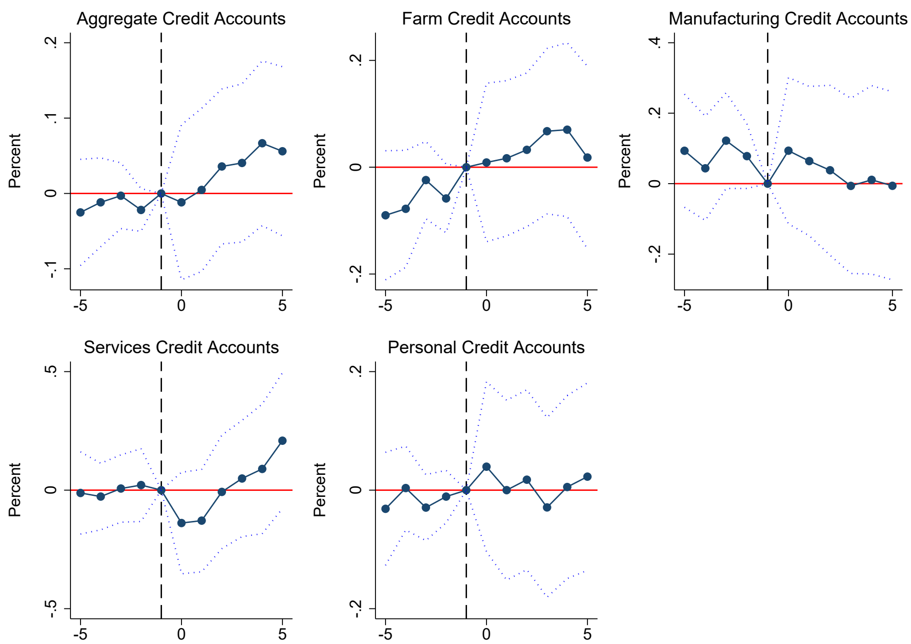
Notes: The above figure presents event-study plots to show the average annual treatment effects on private bank credit, along the intensive margin. The unit of observation is the district. The outcome of interest in each instance is logged credit accounts in the above-mentioned categories. All specifications include district and year fixed effects, along with a linear polynomial in the running variable, and district covariates. The vertical line corresponds to the year 2005 – the year in which the BAP was implemented. The solid line shows the coefficient estimates; the dashed lines plot 95% confidence intervals. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold and standard errors are clustered by district.

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



Notes: The above figure presents event-study plots to show the average annual treatment effects on government bank credit, along the extensive margin. The unit of observation is the district. The outcome of interest in each instance is logged outstanding credit in the above-mentioned categories. All specifications include district and year fixed effects, along with a linear polynomial in the running variable, and district covariates. The vertical line corresponds to the year 2005 – the year in which the BAP was implemented. The solid line shows the coefficient estimates; the dashed lines plot 95% confidence intervals. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold and standard errors are clustered by district.

Figure A10: Government Bank Credit in Underbanked Districts: Extensive Margin



Notes: The above figure presents event-study plots to show the average annual treatment effects on government bank credit, along the intensive margin. The unit of observation is the district. The outcome of interest in each instance is logged credit accounts in the above-mentioned categories. All specifications include district and year fixed effects, along with a linear polynomial in the running variable, and district covariates. The vertical line corresponds to the year 2005 – the year in which the BAP was implemented. The solid line shows the coefficient estimates; the dashed lines plot 95% confidence intervals. The sample in each figure is restricted to districts within a bandwidth of 15 around the discontinuity threshold and standard errors are clustered by district.

Table A1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	N
Full sample					
Rural Enterprise	0.55	0.5	0	1	310688
Own-Account Enterprise	0.86	0.35	0	1	310688
Female Owner	0.18	0.38	0	1	310688
ST Owner	0.04	0.2	0	1	310688
SC Owner	0.13	0.34	0	1	310688
Keeps accounts	0.1	0.3	0	1	310688
Any registration	0.29	0.45	0	1	310688
Govt. Assistance	0.02	0.14	0	1	310688
Marketing Arrangement	0.06	0.24	0	1	310688
Age	10.55	8.5	0	42	310661
Entrant	0.17	0.37	0	1	310688
Manufacturing	0.33	0.47	0	1	310688
Trade	0.36	0.48	0	1	310688
Productive Capital	17985.42	56053.9	0	588093	310688
Workers	1.85	3.2	0	1259	310688
Wage Per Worker	295.41	960.68	0	121980.85	310629
Operating Expenses	21843.04	64281.27	0	636050	310685
Revenues	30198.23	78347.69	0	800000	310685
Value-Addition	7767.16	14200.54	0	151220	310685
Any Credit	0.09	0.29	0	1	310688
Bank Credit	0.03	0.18	0	1	310688
Informal Credit	0.04	0.2	0	1	310688
Any Problem	0.29	0.45	0	1	310688
Problem: Credit	0.08	0.27	0	1	310688
Enterprise Expanding	0.32	0.47	0	1	310688
Enterprise Stagnant	0.51	0.5	0	1	310688
Restricted sample					
Rural Enterprise	0.55	0.5	0	1	133462
Own-Account Enterprise	0.86	0.35	0	1	133462
Female Owner	0.18	0.39	0	1	133462
ST Owner	0.05	0.22	0	1	133462
SC Owner	0.11	0.31	0	1	133462
Keeps accounts	0.1	0.3	0	1	133462
Any registration	0.29	0.45	0	1	133462
Govt. Assistance	0.03	0.17	0	1	133462
Marketing Arrangement	0.05	0.22	0	1	133462
Age	10.48	8.56	0	42	133447
Entrant	0.18	0.38	0	1	133462
Manufacturing	0.34	0.47	0	1	133462
Trade	0.35	0.48	0	1	133462
Productive Capital	18065.81	55475.16	0	588093	133462
Workers	1.93	2.7	0	571	133462
Wage per worker	290.99	937.17	0	72271	133437
Operating Expenses	21016.52	62978.56	0	636050	133459
Revenues	29395	77050.8	0	800000	133459
Value-Addition	7757.37	14036.35	0	151220	133459
Any Credit	0.11	0.32	0	1	133462
Any Bank Credit	0.03	0.18	0	1	133462
Any Informal Credit	0.05	0.23	0	1	133462
Any Problem	0.25	0.43	0	1	133462
Problem: Credit	0.06	0.23	0	1	133462
Enterprise Expanding	0.34	0.47	0	1	133462
Enterprise Stagnant	0.48	0.5	0	1	133462

Notes: This table presents the summary statistics from the electoral and household data. The unit of observation is the household. The top panel presents the summaries for the electoral variables; the middle panel presents the summaries for low caste households; the bottom panel presents the summaries for non-low caste households. The fraction of close elections won by low caste parties is conditional on there being a close election in the district. Landlessness is calculated only for rural households.

Table A2: 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 A3: 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 A4: 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 A5: Pre-Treatment Balance of Manufacturing Characteristics Across Underbanked Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Fixed Assets (Log)	Plant Machinery (Log)	Loans (Log)	Input (Log)	Workers (Log)	Salaries (Log)	Output (Log)	Value-Addition	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 A6: Bank Credit Disbursement in Underbanked Districts

Panel A: Private Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total		Farm		Manufacturing		Services		Personal	
	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)	Amt. (Log)	Act. (Log)
Underbanked × Post	1.073	.729	1.414*	1.070**	1.656**	.507	1.216	.789*	.835	.781*
	(.903)	(.512)	(.763)	(.474)	(.835)	(.359)	(.749)	(.429)	(.740)	(.453)
Observations	2277	2277	2277	2277	2277	2277	2277	2277	2277	2277
R^2	.87	.90	.86	.91	.86	.89	.87	.87	.89	.90
Control Mean	5816.53	34.54	293.58	2.30	2114.96	.73	1324.77	3.35	1449.33	27.27

Panel B: Government Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total		Farm		Manufacturing		Services		Personal	
	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)
Underbanked × Post	-.071	.041	-.029	.088	.057	-.029	-.036	-.019	-.017	.018
	(.063)	(.049)	(.074)	(.074)	(.157)	(.099)	(.117)	(.098)	(.080)	(.078)
Observations	2277	2277	2277	2277	2277	2277	2277	2277	2277	2277
R^2	.99	.99	.98	.98	.96	.93	.95	.94	.98	.97
Control Mean	22520.97	109.52	2860.83	40.31	8467.09	7.02	3766.30	16.30	4431.54	36.34

Panel C: All Banks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total		Farm		Manufacturing		Services		Personal	
	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)	Amount (Log)	Account (Log)
Underbanked × Post	-.006	.076	.025	.096	.158	.004	.041	.047	.032	.072
	(.061)	(.052)	(.071)	(.074)	(.152)	(.105)	(.120)	(.103)	(.080)	(.083)
Observations	2277	2277	2277	2277	2277	2277	2277	2277	2277	2277
R^2	.99	.99	.98	.98	.96	.93	.95	.94	.98	.97
Control Mean	28337.49	144.05	3154.41	70.12	10582.05	12.92	5091.07	32.44	5880.88	103.09

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; columns (5) and (6) manufacturing credit; columns (7) and (8) credit to service sectors; columns (9) and (10) consumer 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 A7: Manufacturing Investment in Underbanked Districts: Alternate Outcome Variables

	(1)	(2)	(3)	(4)	(5)
	Capital Expenditures		Capital Expenditures: Plant and Machinery		
	Log Difference	Pr(Any Capex = 1)	Capital Expenditure	Log Difference	Pr(Any Capex = 1)
Underbanked × Post	.049***	.042**	.054***	.070***	.047**
	(.015)	(.021)	(.020)	(.025)	(.021)
Observations	70285	70285	68867	70285	70285
R^2	.42	.47	.41	.40	.47
Control Mean	.014	.265	-.034	-.004	.223

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) measure capital expenditures in terms of net fixed assets; columns (3)-(5) restrict capital expenditures to investments in plant and machinery. Columns (1) and (4) measure capital expenditures as the logged difference in closing and opening values of establishment fixed assets (plant and machinery); 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, two-way clustered by district and industry. Significant levels: *10%, **5%, and ***1%

Table A8: Credit Growth for Manufacturing Establishments in Underbanked Districts

	(1)	(2)	(3)	(4)	(5)	(6)
	Credit Growth	Credit Growth (Log)	Any Credit Growth	No Loan	New Loan	Interest Rate
Underbanked × Post	.110**	.381**	-.019	.009	.016	.010
	(.042)	(.149)	(.020)	(.010)	(.018)	(.022)
Observations	52477	69837	69837	70285	69837	52455
R^2	.36	.33	.47	.38	.72	.60
Control Mean	.005	.172	.385	.227	.243	

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 (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 are in parentheses, two-way clustered by district and industry. Significant levels: *10%, **5%, and ***1%

Table A8: Manufacturing Investment in Underbanked Districts: Heterogeneity by Establishment and Industry Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Capital Expenditures			
Underbanked × Post	.050*	.052***	.056***	.056**	.049*	.060***
	(.027)	(.019)	(.016)	(.024)	(.028)	(.016)
Underbanked × High Capital × Post	-.007					
	(.034)					
Underbanked × *Small* × Post		-.029				
		(.027)				
Underbanked × *Medium* × Post		-.115**				
		(.048)				
Underbanked × *Large* × Post		-.050				
		(.039)				
Underbanked × Non-SSI × Post			-.084**			
			(.035)			
Underbanked × High Collateral × Post				-.020		
				(.029)		
Underbanked × Partnership × Post					-.000	
					(.033)	
Underbanked × Private Ent. × Post					.022	
					(.044)	
Underbanked × Govt. × Post					-.004	
					(.065)	
Underbanked × Listed × Post					-.094**	
					(.042)	
Underbanked × Rural × Post						-.031
						(.028)
Observations	70285	70050	70050	70050	70285	70285
R^2	.42	.42	.42	.42	.42	.42
Control Mean	.005	.005	.005	.005	.005	.005

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. 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, two-way clustered by industry and district. Significant levels: *10%, **5%, and ***1%

Table A8: Cost of Credit for Manufacturing Establishments in Underbanked Districts: Heterogeneity by Establishment Size and Age

	(1)	(2)	(3)	(4)
			Interest Rate	
Underbanked × Post	.050	.085*	.048	.005
	(.035)	(.050)	(.048)	(.024)
Underbanked × Est. *Size* > Median × Post	-.067*			
	(.040)			
Underbanked × 11 > Est. *Size* ≤ 22 × Post		-.065		
		(.048)		
Underbanked × 22 > Est. *Size* ≤ 66 × Post		-.120*		
		(.064)		
Underbanked × Est. *Size* > 66 × Post		-.086*		
		(.047)		
Underbanked × Large, Young × Post			-.044	
			(.067)	
Underbanked × Large, Old × Post			-.078*	
			(.046)	
Underbanked × Small, Old × Post			.001	
			(.039)	
Underbanked × Listed × Post				.030
				(.040)
Observations	52455	52455	52455	52455
R^2	.60	.60	.60	.60
Control Mean	.242	.242	.242	.242

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. The establishment size cutoffs of 11, 22 and 66 corresponds to the 25th, 50th and 75th percentile of the pre-treatment distribution of establishment size. *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, twoway clustered by district and industry. Significant levels: *10%, **5%, and ***1%

Table A9: Other Outcomes in Underbanked Districts

	Panel A: ITT					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output (Log)	Output Per Worker (Log)	Workers (Log)	Daily Wage (Log)	Value-Addition	Revenue TFP
Underbanked × Post Underbanked*Post	-.129	-.051	-.018	-.045*	.313***	.040
	(.122)	(.057)	(.045)	(.023)	(.079)	(.054)
Observations	70285	69655	70285	70034	70285	56206
R^2	.85	.86	.92	.87	.75	.74
Control Mean	131.034	2.418	190.328	97.958	.989	-.001
	Panel B: Treatment Heterogeneity					
	(1)	(2)	(3)	(4)	(5)	(6)
	Output (Log)	Output Per Worker (Log)	Workers (Log)	Daily Wage (Log)	Value-Addition	Revenue TFP
Underbanked × Post	-.111	-.004	.001	.010	.524***	.057
	(.169)	(.087)	(.050)	(.034)	(.192)	(.074)
Underbanked × Large Est. × Post	-.027	-.082	-.032	-.100**	-.364	-.037
	(.144)	(.090)	(.064)	(.043)	(.276)	(.067)
Observations	70285	69655	70285	70034	70285	56206
R^2	.85	.86	.92	.87	.75	.74
Control Mean	131.034	2.418	190.328	97.958	.989	-.001

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; columns (5) and (6) manufacturing credit; columns (7) and (8) credit to service sectors; columns (9) and (10) consumer 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%