Bank Expansion, Firm Dynamics, and Structural Transformation: Evidence from India's Policy Experiment[†]

Bharat Chandar [‡]
Dian Jiao [§]
Marshall Mo [¶]

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Abstract

This paper examines the impacts of bank expansion on firm dynamics and labor allocation, exploiting a policy experiment in India designed to encourage bank expansion in "under-banked" districts. Empirical findings demonstrate significant growth in manufacturing firms in these districts due to eased credit access, resulting in increased capital accumulation, sales revenue, and employment. However, the expansion predominantly benefited incumbent firms, with minimal stimulation of firm entry or product innovation. The reform also induced notable labor reallocation towards manufacturing sectors, particularly in areas with lower agricultural productivity. To fully understand the aggregate effects of bank expansion and explore policy counterfactuals, we are developing a general equilibrium model, which will be calibrated using our micro-level estimates.

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[‡]Stanford University; chandarb@stanford.edu

[§]Columbia University; dj2531@columbia.edu

[¶]Stanford University; erxiaomo@stanford.edu

1 Introduction

Access to financial services remains markedly limited in developing countries. An estimate from the World Bank posits that approximately 1.7 billion people, primarily residing in underdeveloped countries, are devoid of basic banking facilities, such as checking accounts or standard savings products (Demirguc-Kunt et al., 2018). This situation starkly contrasts with developed economies like the United States, where a mere 4.5% of households are "unbanked" (FDIC, 2021). This deprivation from finance could impede economic development by curbing aggregate investment and employment and distorting capital allocation among firms and potential entrepreneurs (Hsieh and Klenow, 2009; Buera et al., 2011; Bazzi et al., 2021; Fonseca and Matray, 2022).

Many developing countries have implemented place-based policies to stimulate bank expansion in lagging regions, believing that these policies have the potential to ignite economic growth and alleviate poverty through better access to financial services. Previous literature, utilizing both Randomized Controlled Trials (RCTs) and natural experiments, tends to highlight substantial benefits to industrial growth and household well-being in areas that have seen an influx of banking services (Burgess and Pande, 2005; Bruhn and Love, 2014; Young, 2017; Cramer, 2021; Fonseca and Matray, 2022; Barboni et al., 2021). However, less is understood about the costs and aggregate welfare effects of these programs.[1] It is conceivable that regions under-served by banks could have an inherently low demand for credit, possibly due to low productivity levels or other market frictions.[2] Under such conditions, expanding the presence of bank branches in places with low credit demand could be counterproductive and even lead to greater misallocation of resources.

In this study, we investigate the impacts of bank expansion on firm dynamics and labor allocation, employing both empirical and theoretical approaches. Our analysis takes advantage of a nationwide policy experiment in India, which was implemented by the Reserve Bank of India in 2005. The purpose of this policy was to encourage the opening of branches in "under-banked" districts—regions where the population-to-branch ratio surpassed the national average. This arbitrary policy cutoff presents us with an ideal setting to estimate the causal effects of bank expansion using a regression discontinuity design (RDD), wherein we compare districts that barely exceed or fall below the criterion for being classified as under-

[1] One noteworthy exception is Ji et al. (2023), which integrates empirical evidence of branch openings in local markets with a spatial general equilibrium model to quantify the aggregate effects of bank expansion.

[2] Previous studies have shed light on several industrial policies that could potentially contribute to such market frictions. These policies include priority sector lending (Banerjee and Duflo, 2014), restrictions on Foreign Direct Investment (FDI) (Bau and Matray, 2023), contractual frictions (Bertrand et al., 2021), and reservations for Small-Scale Industries (Martin et al., 2017). In section 4, we outline the theoretical framework for how such market frictions can affect firm dynamics.

banked. The banking industry responded strongly to the policy, leading to an increase in the number of branches, deposits, and credit in those under-banked districts, thereby creating exogenous shocks to financial access.

Leveraging plant-level data from the Annual Survey of Industries (ASI) database from 1998 to 2013, we document a significant expansion of manufacturing firms in under-banked districts post-reform. Our findings indicate that the reform eased firms' access to credit, stimulating capital accumulation and driving growth in sales revenue and employment. However, this growth was exclusively fueled by incumbent firms that employed more resources to increase their production of existing products. Contrary to findings in prior studies (Kerr and Nanda, 2009; Bazzi et al., 2021; Fonseca and Matray, 2022), the reform failed to spur greater firm dynamics. Although treated districts saw an uptick in the exit rate for small firms (with less than 20 employees), the entry rate remained unchanged post-reform. We similarly report that the reform had limited effects on product innovation and creative destruction. In line with the lack of enhanced firm dynamics and product innovation, firms in the treated districts did not witness an improvement in their Total Factor Productivity (TFP).

These findings echo the predictions of our static framework of heterogeneous firms operating under financial constraints. We document that under-banked districts typically shoulder higher fixed overhead costs, possibly due to inefficient industrial regulations. While the bank expansion eased the borrowing constraint on the intensive margin, it did not alleviate the fixed overhead costs. Consequently, the reform primarily benefited incumbent firms at the expense of potential entrants.

Treating each Indian district as a distinct local market, we delve into the reform's equilibrium impacts by leveraging the village-level population census data.[3] We observe that bank expansion induced significant labor reallocation towards the manufacturing sector, primarily by creating more manufacturing jobs. This effect is particularly pronounced in villages with lower agricultural productivity, implying that the reform potentially mitigates labor misallocation across different sectors.

We are currently developing a general equilibrium model to interpret our results and

[3]Treating each district as a distinct local labor market is consistent with evidence of low levels of migration in our study context. Existing literature has highlighted substantial labor mobility costs in India, potentially confining labor to agriculture even in the face of low productivity. Munshi and Rosenzweig (2016) highlights that the wage gap between urban and rural areas in India is substantial, with urban wages being over 47% higher than rural wages for less educated workers engaged in menial tasks. Using detailed district-wise migration flow data from the 2001 population census, Kone et al. (2018) underscores that the 5-year inter-district migration rate in India is remarkably low, at a mere 2.8%. In stark contrast, the 5-year inter-prefecture migration rate in China stands significantly higher at 10%. Despite low levels of migration observed in practice, we are developing a spatial general equilibrium model that can accommodate both migration and trade costs.

quantify the aggregate effects of the policy. This model will be calibrated with our micro-level estimates to understand the effects on both aggregate economic growth and the misallocation of resources and talent. In addition, we will explore several policy counterfactuals, informed by our model's predictions. For instance, we will investigate the counterfactual effects of further reducing financial frictions towards zero. Additionally, we aim to understand the interplay between reduced financial frictions and other types of economic frictions.

This paper connects most closely to three main streams of literature. First, we add to a growing body of literature studying the impacts of quasi-experimental and experimental credit shocks in developing countries (Bazzi et al., 2021; Breza and Kinnan, 2021; Banerjee and Duflo, 2014; Bruhn and Love, 2014; Burgess and Pande, 2005; Fonseca and Matray, 2022; Young, 2017; Cramer, 2021; Barboni et al., 2021; Egger et al., 2022). A complementary approach exploits RCTs to study the implications of access to credit and saving products in developing countries with mixed results (see, for instance, Banerjee et al. (2015) for a review). One explanation for the modest effect of credit access in experimental studies is that they often fail to incorporate the general equilibrium effects. Using combined rich micro-data on firms and labor markets, we are able to directly study the general equilibrium impacts of the bank expansion policy. In the context of India, Young (2017) and Cramer (2021) have examined and validated the bank expansion policy that we use in the paper.[4] Our study focuses on industrial growth and structural transformation, providing new perspectives on the aggregate welfare effects of bank expansion.

Second, our paper contributes to the large literature on structural transformation, especially the impediments to the reallocation of labor from agriculture in developing countries (Gollin et al., 2014; Munshi and Rosenzweig, 2016). There is an ongoing debate about the determinants of structural transformation. Previous research has emphasized the importance of agricultural productivity as the primary "labor push" force (Gollin et al., 2002; Ngai and Pissarides, 2007; Bustos et al., 2016), as well as the importance of human capital growth (Porzio et al., 2022). On the other hand, an older strand of research has suggested the role of the manufacturing sector in the process of structural transformation—the "labor pull" hypothesis. This hypothesis posits that growth in the manufacturing sector could result in higher industrial wages and attract surplus labor from the agricultural sector, thereby driving the structural change (Lewis, 1954; Harris and Todaro, 1970; Bencivenga and Smith, 1997; Gylfason and Zoega, 2006; Alvarez-Cuadrado and Poschke, 2011). Our paper contributes to the literature by providing empirical evidence that manufacturing development acts as a sig-

[4] Cramer (2021) shows that the expansion of banks leads to better health outcomes in India. At the macro level, Young (2017) shows that treated districts had faster economic growth, proxied by nighttime light intensity.

nificant "labor pull" factor, driving the process of structural transformation and potentially reducing labor misallocation.

In addition, our paper contributes to the literature on resource misallocation. A leading explanation of cross-country economic disparity is resource misallocation; however, identifying specific policy tools to reduce misallocation and quantifying their aggregate impacts proves challenging (Hsieh and Klenow, 2009; Bartelsman et al., 2013; Restuccia and Rogerson, 2017; David and Venkateswaran, 2019; Baqaee and Farhi, 2020; Sraer and Thesmar, 2018). In a related paper, Bau and Matray (2023) shows that FDI liberalization in India can reduce misallocation across manufacturing firms. We contribute to this literature by causally examining the effects of banks on the misallocation of resources and talent, both empirically and theoretically, leveraging a rare natural experiment in India's banking system.

The rest of this paper is organized as follows. Section 2 introduces the policy and our identification strategy. Section 3 outlines the data that we use in our analysis. Section 4 presents our findings on industrial growth and provides a theoretical framework explaining the mechanism behind the results observed. In Section 5, we present our main results on structural transformation. We conclude with Section 6.

2 Policy Reform and Identification Strategy

2.1 Policy and Institutional Background

The policy reform we analyze in this study was introduced in 2005 by the Reserve Bank of India (RBI) to incentivize banks to open more branches in under-served locations.[5] As per the policy, banks are required to submit an annual branch expansion plan to the RBI, outlining proposed branch openings, closings, and shifts. Thus, by proposing to open more branches in areas that the RBI has designated as "under-banked," banks can increase their chances of obtaining licenses for their preferred locations. [6] [7]

The definition of an "under-banked" district is crucial for our identification strategy.

[5] In India, the banking sector does not permit free entry of banks or bank branches. Banks are required to apply for and acquire licenses from the RBI prior to opening any new branch. Additionally, banks must also request approval to close or relocate branches in most markets.

[6] To make their license-issuance decisions, the RBI also evaluates banks based on other factors, such as the bank's provision of "no-frills" accounts, adherence to priority sector lending obligations, and complaint resolution record. However, these requirements are applied at the bank level, not the individual branch level. For more details on the reform, refer to the 2005 issue of the RBI's *Master Circular on Branch Authorization.*

[7] Banks in India are not allowed to relocate their branches if they leave a market "unbanked". Therefore, it is not possible for banks to circumvent this policy by opening in under-banked districts and then relocating to bank-rich areas.

According to the rule adopted by the RBI, a district is considered under-banked if the average number of people per bank branch (i.e., the population-to-branch ratio) exceeds the national average for India [8] as follows:

$$\underbrace{\frac{Population_{Dist.}}{\#\ Bank\ Branches_{Dist.}}}_{\text{“}Under-banked\ District\text{”}} > \underbrace{\frac{Population_{National}}{\#\ Bank\ Branches_{National}}}_{\text{“}National\ Average\text{”}}$$

In September 2005, RBI published the list of "under-banked" districts following the above rule, which was then slightly revised in 2006. It is important to note that the RBI did not adjust the list to account for *changes in the ratio*, despite more bank branches having entered into under-banked districts. As a result, the list of "under-banked" districts remained nearly constant throughout our sample period.[9] Using the official RBI document in 2006, we define 375 out of 593 districts as "under-banked" districts, which were spatially dispersed throughout the country.

2.2 Policy Cutoff and Identification Strategy

The arbitrary policy cutoff begets a regression discontinuity (RD) design that compares under-banked districts (treatment group) and control districts with a population-to-branch ratio just above and below the national average. The identification assumption is that districts close to the cutoff are similar in the absence of bank expansion. In this subsection, we show that the policy provides exogenous variation in the presence of bank branches and validate our identification strategy. We will further explain our estimating equations along with corresponding results in Section 4 and Section 5 to avoid confusion.

To validate this design, we first show that there is no evidence of manipulation of the cutoff—so districts do not select into treatment or control groups. The left panel of Figure B.1 presents the histogram plot and non-parametric fitted lines of districts' population-branch ratios (relative to the national average). Visually, there is no sign of bunching on either side of the cutoff, suggesting limited scope of selection. We formally test it using the McCrary

[8]According to RBI's *Report of the Group to Review Branch Authorization Policy* published in 2009, the term "national average" refers to a specific statistic provided directly by the RBI. However, the precise methodology that the RBI used to compute this statistic is not explicitly disclosed. To account for this, we independently recalculated this ratio based on its definition, and we validate the accuracy of our calculations in a subsequent section.

[9]While the RBI only published the list of under-banked districts without revealing the detailed district-level population-branch ratios, we reconstruct the ratios using the 2001 population census data and bank branch data from the RBI. After 2010, certain states were made ineligible for "under-banked" status, reducing the number of "under-banked" districts, but no new district was introduced to "under-banked" status. The list of "under-banked" districts was thoroughly updated in 2014 using the 2011 population census data.

(2008) density test—the McCrary estimator is -0.065 and the p-value is 0.95. Hence, we cannot reject smoothness around the cutoff.[10]

The right panel of Figure B.1 confirms that the cutoff is indeed meaningful—there is a jump in under-banked status below and above the cutoff. The compliance with the assignment rule is not perfect. Out of 578 districts that we have bank branch data, seven districts had a status different than predicted by the population-branch ratio. Several reasons might explain the imperfect compliance. First, there may be measurement error present within the branch data.[11] Second, the RBI might have used their discretion to edit the list, potentially with the intent to "help" specific districts, or were captured by political elites. Fortunately, non-compliance does not pose a threat to our analysis, as only 1.7% of observations in our ASI establishment data belong to non-compliant districts. In our main analysis, we exclude non-compliers and use a sharp RD design, assuming perfect compliance. We also demonstrate the robustness of our results by including these observations in a Fuzzy RD design.

We further validate our RD design by showing that other covariates and pre-treatment variables are smooth around the cutoff. Using the 2001 population census data, RBI's bank branch data in 2004, and nighttime light intensity in 2004, Figure B.2a-d visually demonstrate that those district-level characteristics are continuous around the cutoff. We further test for the smoothness of firm-level outcomes in 2004 using the Annual Survey of Industries data. Figure B.2e-h show that firms in treatment and control districts are similar in sales, fixed assets, number of employees, and marginal revenue product of capital prior to policy implementation. Taken together, these results suggest that districts are properly randomized

[10] The lack of manipulation is not surprising as the banking system in India is tightly regulated. By the *1949 Banking Regulation Act*, banks should submit an detailed annual expansion proposal and cannot, without the prior approval of the RBI, open a new place of business or change the location of the existing place of business. Recall that the population-branch ratio has two components: 1) the district population in 2001, which was fixed when the expansion policy was announced; and 2) the number of bank branches in the district. To game the policy, banks need to collectively gain RBI's approval to open or close branches at least one year before the policy. It is highly unlikely, as the policy was announced in 2005 without prior notice.

[11] The total number of districts in 2001 was 593. For districts that were split from a 2001 district, we recoded them to the original district. Some districts were excluded because they were formed after 2001 by merging several existing districts, making it impossible to map them to previous districts. After a careful review of RBI documents and historical texts, we further excluded three districts due to suspected coding errors or manipulation. Ujjain, a historically wealthy city not initially included in the list of under-banked districts in 2005, was added in 2006. We suspect this addition might have been politically motivated, and hence, we excluded Ujjain. Badgam district was also removed from our sample. There was a transfer of lead bank responsibility in respect of Anantnag, Budgam, Pulwama and Srinagar districts to the Jammu & Kashmir Bank Ltd. up to March 2005. Despite the extension of the existing arrangement until March 31, 2007, these bank branches were recorded as closed in the RBI bank branch data, reversing the treatment status of Badgam. Lastly, we dropped Varanasi due to the 2002 merger of the private sector Banaras State Bank with the nationalized Bank of Baroda. This merger led to some bank branches in Varanasi being coded as closed, altering their treatment status.

around the cutoff, lending support to the causal interpretation of our RDD results.

The reform was highly effective by introducing powerful incentives for banks to open in districts previously considered unprofitable and thus under-banked, by leveraging licenses in high-profit areas.[12] Cramer (2021) and Young (2017) have also examined this policy and showed that commercial, especially private sector banks, strongly respond to the policy, as we confirm in Figure B.3 and B.4. On average, treatment districts received 21% more branch licenses and 19% more branches than control districts by 2010, which corresponds to an increase to 8.31 branches per 100,000 people, compared to the control mean of 6.99 branches (Cramer, 2021). Furthermore, treatment districts also saw a large increase in deposit and credit after the policy.[13]

3 Data

Our primary source of data is the establishment-level data from the Annual Survey of Industries (ASI) from 1998 to 2013.[14] The ASI provides a representative sample of all registered manufacturing establishments in India, with large establishments covered yearly and smaller establishments surveyed on a sampling basis. The publicly available ASI includes unique plant identifiers that are consistent across the years starting from 1998. However, it lacks district information, which is critical to our analysis. To address this, following Martin et al. (2017), we match the panel version of ASI with an older cross-sectional version, which contains district identifiers until 2009, based on time-invariant factors and open/close variables.[15] The ASI includes comprehensive plant-level information on revenues, labor costs, stock of fixed assets, and materials, among others, which are essential for constructing our key firm outcome variables. We perform substantial data cleaning and deflate all nominal outcome variables to constant 2004-2005 Rupee following Allcott et al. (2016).

Our labor market data come from the Socioeconomic High-resolution Rural-Urban Geographic Dataset (SHRUG),[16] which integrates data from multiple rounds of the population

[12] This kind of "bundling" policy requires high demand for branches in "rich" areas, which is ensured by India's fast economic growth beginning in 2003 and continuing through the decade.

[13] The increase in deposit and credit is especially pronounced for private sector banks, as depicted in Figure B.3. One concern is that private banks might simply "steal" market share from existing public sector banks, which could lead to minimal aggregate effects in local markets. However, our results in Figure B.4 suggest that although the stealing effect might be possible, its effects tend to be small and insignificant.

[14] The reporting period for the ASI is the Indian fiscal year, which begins on April 1 and ends on March 31. Throughout the paper, when we refer to a survey year, we use the calendar year in which the fiscal year commences. All financial amounts are expressed in 2004 Rupees.

[15] Since the ASI does not provide district identifiers after 2009, we use the panel structure of our data to infer the district information for a subset of firms (approximately 2/3) that appear in the data before 2010.

[16] Asher and Novosad (2020) provides details of the data construction, accessible at

and economic census. In particular, we use the 2001 and 2011 village-level Population Census data from the Primary Census Abstract and Village Directory tables. This data provides information on village infrastructure, demographics, employment, occupation, and population, which are used to construct variables related to labor supply in agricultural and manufacturing & service sectors. It also provides a basis for connecting all other datasets at the village level. We complement this with crop suitability data from the FAO Global Agro-Ecological Zones (GAEZ). This dataset assesses crop suitability and production potential based on plant characteristics, climate, and soil quality, aggregated to the village level by SHRUG.

Data pertaining to the banking sector and the implementation of the policy is obtained from the Reserve Bank of India. The list of under-banked districts is digitized from the report of RBI in 2006. While the exact district-level population-branch ratios are not included in the report, we are able to reconstruct them using the 2001 population census data and bank branch data from the Master Office File published by the RBI. We also obtain district-by-bank-group level credit and deposit data from 2003 to 2016 from the RBI. Since the list of under-banked districts is based on the 2001 population census districts (and remains unchanged until 2014, when the list was updated according to the 2011 population census), we build a crosswalk to map all data to the 2001 population census districts.[17]

4 Did Bank Expansion Lead to Industrial Growth?

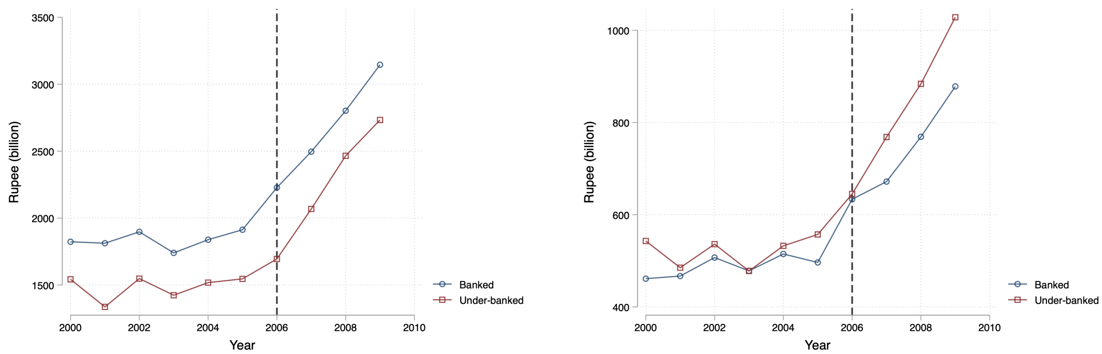
In this section, we conduct a comprehensive examination of the effects of bank expansion on manufacturing firms. Starting with a visual exploration of time-series plots using raw data from the ASI, we identify key trends and variations in total capital and firm size across under-banked and control districts. Guided by these initial findings and the existing literature, we then develop a simple theoretical framework of heterogeneous firms under financial constraints, which provides a conceptual roadmap for our empirical investigation. Subsequently, we detail our identification strategy, employing a Difference-in-Discontinuity design that leverages the policy cutoff and its implementation timeline to isolate the causal effects of bank expansion. Finally, we present our empirical findings at the firm and district levels, providing insights into the micro and macro impacts of bank expansion on firm dynamics, labor allocation, and industrial growth.

https://www.devdatalab.org/shrug.

[17]The district borders in India are very volatile. There were 593 districts in 2001 and 640 districts in 2011.

4.1 Firms in Under-banked and Banked Districts: Time Trends

Total Capital. Figure 1 displays the time trends of total capital in all under-banked and banked districts (left panel), and focusing exclusively on districts around the policy cutoff (right panel). Total capital is constructed as the weighted sum of firm-level fixed assets[18] in the treatment and control groups, using the survey sampling weights. Capital is deflated to constant 2004-2005 Rupee values using the Gross Capital Formation data from the RBI.



(a) All distircts (b) Districts around the policy cutoff

Figure 1: Trends of Total Fixed Assets in Treatment and Control Districts

The left panel reveals that under-banked districts had approximately 25% less capital than banked districts. This gap remained stable despite rapid industrial growth from 2000 to 2005. Under-banked districts began to catch up following the policy implementation as new bank branches entered and more credit was issued. This catch-up is more evident when focusing on districts around the policy cutoff, as displayed in the right panel. Under-banked and banked districts had remarkably similar levels of capital prior to the policy, suggesting that the treatment status assignment was as good as random around the policy cutoff. This further validates our RD design. However, divergence began as under-banked districts received more bank branches.

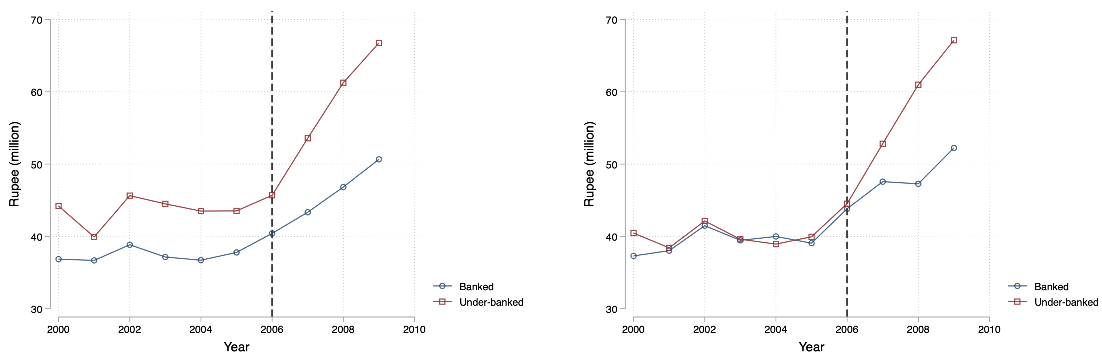
Average Firm Size. These patterns interestingly invert when examining the capital of the average firm in Figure 2. The left panel suggests that before the policy, firms in under-banked districts were approximately 20% *larger* than firms in banked districts in terms of fixed assets.

There are two potential explanations. Firstly, the lack of access to finance might *cause* high costs of operation, such as a fixed cost of using credit, as posited by Ji et al. (2023).

[18] Fixed assets include tangible assets such as plants, land, and machinery owned by firms, but exclude mining rights and other intangible assets.

Consequently, only the more productive and talented entrepreneurs would bear the costs and enter the market.[19] In this case, we would expect that opening more bank branches in under-banked districts could lower the operating costs, promoting the entry of smaller (and possibly less productive) firms (Bazzi et al., 2021), thereby reducing the average firm size.

Alternatively, the larger average firm size and the scarcity of bank branches could both be the *result* of inefficient industrial policies or other structural barriers. Suppose firms in under-banked districts are less profitable due to other inefficient policies or barriers, then only a few talented entrepreneurs enter and stay in the market. The aggregate demand for credit is suppressed if firms choose to operate on a smaller scale or not enter the market in the first place. This muted credit demand could then discourage the entry of banks into these districts, rendering them "under-banked". In this scenario, simply opening more bank branches might not improve the situation by promoting the entry of potential entrepreneurs.



(a) All districts (b) Districts around the policy cutoff

Figure 2: Trends of Firm Average Fixed Assets in Treatment and Control Districts

Proceeding to the post-2006 time-series, both panels indicate that firms in under-banked districts grew even larger following the policy, aligning more with the second hypothesis. This outcome yields two critical insights. Firstly, despite firms in under-banked districts being positively sorted and consequently larger than their counterparts in banked districts, they remain too small due to financial constraints. Secondly, bank expansion seems to alleviate these constraints more substantially along the intensive margin than the extensive one. As a result, the reform appears to have primarily driven the growth of incumbent firms rather than fostering the entry of potential entrants.

[19]The intuition is similar in spirit to the heterogeneous firm model in Melitz (2003) that a fixed export cost will induce only the more productive firms to enter the export market.

4.2 Bank Expansion and Firm Dynamics: A Framework

Setup. Now, we show a simple framework to illustrate the forces behind the expansion of banks and firm dynamics. In this framework, we consider an economy composed of M districts, each characterized by varying capital-market conditions that result in different financial constraints. Each district m is populated by a continuum of residents L_m, and wage rates w_m clear the local labor market. For now we assume that each district is a closed economy.

Households in each district are endowed with personal wealth c and an entrepreneurial idea, represented by the productivity parameter z. Households have a choice to be entrepreneurs or workers. If they choose to be workers, they supply one unit of labor inelastically and earn the local labor wage w_m. If a household with a productivity z chooses to be an entrepreneur, the production function is given by

$$y = zk^{\alpha}l^{\beta} \tag{1}$$

where k is the capital, and l is labor employed by the entrepreneur with an idea z. $\alpha + \beta < 1$ such that $\alpha, \beta \in (0, 1)$, ensuring diminishing returns to scale. Assume all firms pay the same national interest rate, r. For simplicity, we assume that firms produce a homogeneous final consumption product, which is used as the numeraire.[20] We can solve the optimal k and l using the first order conditions.

$$k = \left(\frac{1}{z}\left(\frac{\alpha w_m}{\beta r}\right)^{\beta}\frac{r}{\alpha}\right)^{\frac{1}{\alpha+\beta-1}} \tag{2}$$

$$l = \left(\frac{1}{z}\left(\frac{\beta r}{\alpha w_m}\right)^{\alpha}\frac{w_m}{\beta}\right)^{\frac{1}{\alpha+\beta-1}} \tag{3}$$

Firm Entry Condition. We introduce an overhead cost f_m that firms must pay to produce, which could represent regulatory burdens or a fixed cost of borrowing. f_m in district m could be greater because of inefficient industrial regulations or the scarcity of bank branches. The net profit is thus $\pi(z) = y - rk - w_m l - f_m$. A household with an idea z becomes an entrepreneur if $\pi(z) \geq w_m$. Note that we can write $\pi(z)$ in terms of wage bills such that $\pi(z) = \frac{(1-\alpha-\beta)w_m l}{\beta} - f_m$. Substituting Equation 3 into the firm entry condition gives the lower

[20] All the results hold if we assume a CES demand function and firms produce differentiated products as in Bazzi et al. (2021).

bound of productivity $\underline{z}$ as below.

$$\underline{z} = \left(\frac{r}{\alpha}\right)^{\alpha} \left(\frac{w_m}{\beta}\right)^{\beta} \left(\frac{w_m + f_m}{1-\alpha-\beta}\right)^{1-\alpha-\beta} \tag{4}$$

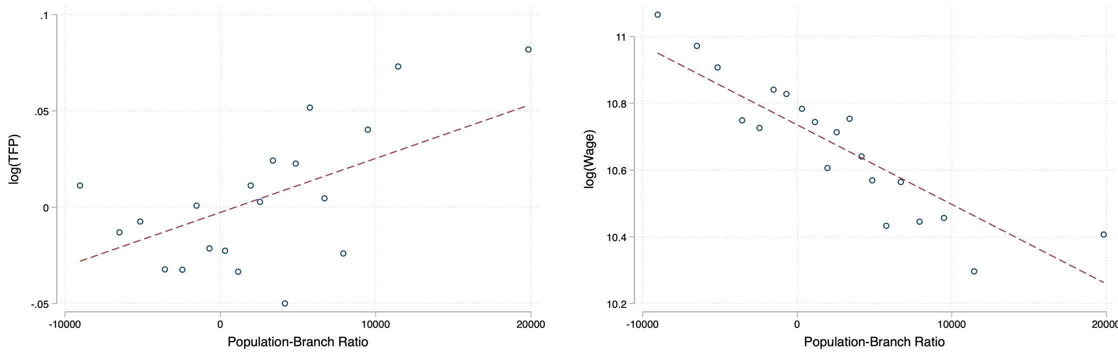
In this model, households with a productivity level of $z \geq \underline{z}$ opt for entrepreneurship over wage labor. This threshold decreases when firms are subject to smaller overhead costs, i.e., when f_m is low. With lower overhead costs, households become more willing to undertake entrepreneurship, even at lower productivity levels, which in turn reduces the average productivity across all incumbent firms.

At the same time, there is a general equilibrium effect at work. The entry of additional entrepreneurs elevates the overall demand for labor at the district level, which in turn pushes up local wages and reduces the optimal firm size across all productivity levels. Consequently, districts with a lower f_m tend to have a smaller firm size. This is due to two factors: the influx of smaller firms and the increase in equilibrium wages.

Figure 3 illustrates the pre-reform correlations between district-level Total Factor Productivity (TFP), wages, and the population-to-branch ratio.[21] Notably, districts with higher population-to-branch ratios (indicating fewer banks) tend to exhibit higher TFP and lower wages. Figure B.6 shows that these districts also have fewer entrants and a smaller number of plants. These patterns align with our interpretation that such districts are characterized by a higher overhead cost, or f_m.[22]

[21] District-level TFP is measured by the average Solow residuals across all firms in the district, using the sampling weights. Year fixed effects are projected out. In our analysis, we effectively assume that all district-level TFP heterogeneity originates from variations in the productivity threshold $\underline{z}$, rather than district-specific productivity parameters or differing talent distributions across districts. One testable prediction of our model is that these variations in $\underline{z}$ influence district TFP by altering the lower tail of the firm productivity distribution, while the upper tail remains constant across districts, assuming a Pareto distribution of firm productivity. To test this prediction, we categorize districts based on their aggregate TFP into High-TFP and Low-TFP groups (those above and below the median aggregate TFP, respectively). For each group, we depict the cumulative distribution of firm-specific TFP, conditional on their TFP surpassing the 90th percentile in the overall distribution. Figure B.5 demonstrates that the two distributions (truncated at the 90th percentile) are strikingly similar, confirming our model's implication that heterogeneity in district TFP is driven by variations of $\underline{z}$.

[22] An alternate method models f_m as a fixed entry cost, a price that entrants must pay *prior to* the productivity draw, following the tradition of Hopenhayn (1992). An implication of this model is that higher entry costs deter entry, resulting in fewer entrants willing to pay the sunk cost to receive productivity draws. Consequently, wages drop due to decreased demand for labor, and the productivity threshold reduces. However, according to this model, districts with higher entry costs should have lower average TFP, which contradicts the evidence depicted in Figure 3.



(a) log(TFP)

(b) log(Wage)

Figure 3: District-Average TFP, Wage, and Population-Branch Ratio

Does Limited Bank Access *Cause* High Overhead Costs? While a high f_m can lead to larger firm size in under-banked districts, it is uncertain whether this is driven by the scarcity of banks. Suppose the overhead cost f_m primarily consists of a fixed cost of using credit, and firms in under-banked districts must pay a higher f_m due to limited access to banking services.[23] This credit entry cost may capture, for example, both transportation and information costs, and we make a reduced-form assumption that it is decreasing in the number of branches in a district as in Ji et al. (2023). In this case, we would expect that by opening more branches in under-banked districts, the bank expansion policy should, in theory, bring down the average firm size in treated districts by lowering f_m and allowing smaller (and less productive) firms to enter. However, this is inconsistent with our time-series evidence reported in Figure 2, demonstrating that the average capital actually *increased* following the reform.

Therefore, the correlations highlighted in Figure 3 could be spurious and not indicative of any real causal relationship. Districts with higher f_m, potentially attributable to other inefficient policies, would have a higher productivity threshold $\underline{z}$, resulting in a suppressed demand for *both* labor and credit. This subdued credit demand could, in turn, deter bank entry, leading to a higher population-to-branch ratio. This line of reasoning prompts the question as to why the overhead cost, f_m, might be higher in under-banked districts, given that the scarcity of banks alone cannot explain it. Previous research has shed light on various industrial policies that could potentially contribute to inefficiently high overhead costs and entry barriers. These include policies such as priority sector lending (Banerjee and Duflo,

[23]We do not model the borrowing decision here, implicitly assuming that all firms borrow and pay the cost. This could be the case if the optimal level of capital k for all existing firms is large relative to their initial wealth endowment c. It is consistent with the data that more than 85% of ASI firms in our sample reported having positive outstanding loans even before the reform was implemented.

2014), restrictions on Foreign Direct Investment (FDI) (Bau and Matray, 2023), contractual frictions (Bertrand et al., 2021), and reservations for Small-Scale Industries (Martin et al., 2017).

Borrowing Constraint. To account for the post-reform increase in the average firm size, we explore an alternative approach to model credit frictions. This approach posits a borrowing constraint that limits the maximum amount of credit firms can borrow, drawing from the frameworks presented by Evans and Jovanovic (1989) and Bazzi et al. (2021). An entrepreneur is a net borrower if the optimal level of capital exceeds her initial wealth endowment c, and she can borrow at most $(\lambda_m - 1)c$, with $\lambda_m > 1$. This borrowing constraint thus places an upper bound on the capital stock such that $0 \leq k \leq \lambda_m c$.

The borrowing constraint is binding if the unconstrained optimal capital in Equation 2 is greater than $\lambda_m c$, and the firm uses capital at the upper bound. A firm is financially constrained if

$$z \geq (\lambda_m c)^{1-\alpha-\beta} \left(\frac{\alpha w_m}{\beta r}\right)^{\beta} \frac{r}{\alpha} \equiv \underline{z_{uc}} \tag{5}$$

In the (z, c) space, potential entrepreneurs are constrained if they are relatively talented but do not have enough wealth to borrow to the optimal level. Financially constrained firms, conditional on choosing entrepreneurship, use the following optimal capital and labor.

$$k_c = \lambda_m c \tag{6}$$

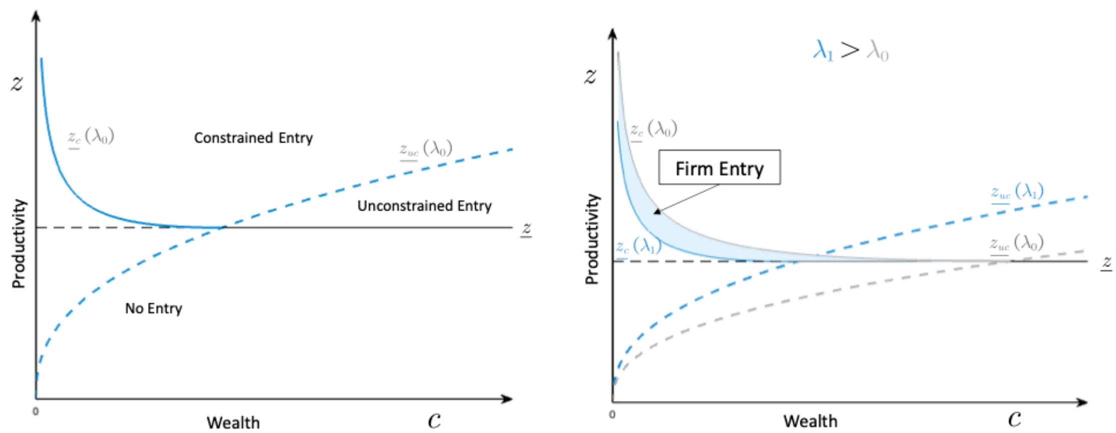
$$l_c = \left(\frac{1}{z}\left(\frac{1}{\lambda_m c}\right)^{\alpha} \frac{w_m}{\beta}\right)^{\frac{1}{\beta-1}} \tag{7}$$

We can readily observe that k_c and l_c increase with c, implying that constrained firms are smaller than their unconstrained peers with the same level of productivity. In addition, constrained firms have a lower capital-labor ratio. Our firm entry condition (4) still applies to unconstrained firms, but constrained ones have a higher productivity threshold. They enter if constrained profit $\pi(z,c) = \frac{(1-\beta)w_m l_c}{\beta} - r\lambda_m c - f_m \geq w_m$. We can similarly solve the lower bound for constrained productivity $\underline{z_c}$.

$$\underline{z_c} = \left(\frac{1}{\lambda_m c}\right)^{\alpha} \left(\frac{w_m}{\beta}\right)^{\beta} \left(\frac{w_m + f_m + r\lambda_m c}{1-\beta}\right)^{1-\beta} \tag{8}$$

Similar to the unconstrained threshold, $\underline{z_c}$ is smaller if f_m is lower. Appendix A.1 shows that $\underline{z_c}$ is decreasing in c if and only if the unconstrained optimal capital k is strictly greater than the borrowing constraint $\lambda_m c$; in other words, when firms are strictly financially constrained. Intuitively, a constrained entrepreneur with productivity z is more likely to enter if he or she has a higher level of wealth c. The constrained threshold is always higher than the unconstrained threshold, but decreases with c until it reaches the level for unconstrained entrepreneurs.

The left panel of Figure 4 plots firms' entry conditions and the financial constraint condition in the (z, c) space. The horizontal line represents the unconstrained threshold $\underline{z}$, which is independent of wealth and λ_m. The dotted blue curve maps the financial constraint condition $\underline{z_{uc}}$ from Equation 5; households above this curve are financially constrained. The solid blue curve shows the constrained threshold $\underline{z_c}$, which decreases in c. All three curves meet at $(\underline{z}, \underline{c})$. Households below either $\underline{z_c}$ or $\underline{z}$ opt for employment over entrepreneurship. Those with a (z, c) between $\underline{z}$ and $\underline{z_{uc}}$ enter without financial constraints, while those above $\underline{z_{uc}}$ and $\underline{z_c}$ enter under financial constraints.



(a) Initial λ_0 (b) Higher λ_1

Figure 4: Selection into Entrepreneurship under λ_0 and λ_1

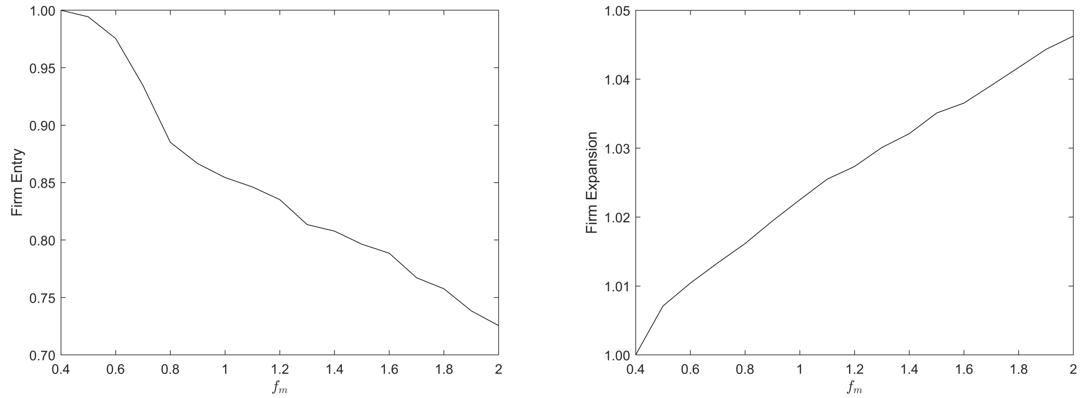
Firm Dynamics under Borrowing Constraint. We now discuss how relaxing the borrowing constraint (by increasing λ_m) affects firm dynamics. In the partial equilibrium, financially constrained firms strongly respond to the increase in λ_m by increasing their capital stock and labor. The size of incumbent firms would therefore increase.

In terms of entry and exit, while changes in λ_m do not directly affect the unconstrained threshold productivity $\underline{z}$, a higher λ_m can still lead to more unconstrained entry by raising the financial constraint threshold $\underline{z_{uc}}$ in Equation (5). Intuitively, there is initially a set of financially constrained entrepreneurs ($z > \underline{z_{uc}}$) that find it unprofitable to enter, if $z < \underline{z_c}$. When λ increases, they might choose to enter if they are no longer financially constrained and have a $z > \underline{z}$.

For constrained entrepreneurs, relaxing the borrowing constraint directly increases their entry, as shown by Appendix A.1 that $\underline{z_c}$ is decreasing in λ_m. The right panel of Figure 4 illustrates the impact of easing the borrowing constraint on firm entry in partial equilibrium (assuming local wages remain unchanged). When the borrowing constraint moves from λ_0 to λ_1, both the financial constraint line $\underline{z_{uc}}$ and the constrained threshold $\underline{z_c}$ shift leftward, shown by the blue curves. The blue-shaded region indicates the mass of entrants. Note that the average productivity and size of new entrants, both constrained and unconstrained ones, may not necessarily be lower than those of incumbents. This is because the entrants are on the margins of the borrowing constraint and the constrained entry threshold, which are higher than the unconstrained threshold. The average productivity and size of entrants are determined by the joint distribution of productivity and initial wealth endowment (z, c).

Interaction between f_m and λ_m. In this framework, the overhead cost, f_m, interacts with the borrowing constraint λ_m in a way that impacts entrepreneurial entry. In districts with a high f_m, both the constrained ($\underline{z}$) and unconstrained productivity thresholds ($\underline{z_c}$) would be higher, resulting in a smaller mass of potential entrepreneurs on the margin of entry. Consequently, a relaxation of the borrowing constraint (with an increase in λ_m) would trigger fewer entries in districts with a higher f_m when the entry thresholds are in the tail of the productivity distribution. The simulations reported in the left panel of Figure 5 lend support to this observation. These simulations depict how the mass of entries, emerging from halving the borrowing constraint, varies with different levels of overhead costs (with the baseline mass of entry normalized to 1 when $f_m = 0.4$). It is particularly noteworthy that as the overhead cost increases from 0.4 to 2, there is a 25% decline in the rate of firm entry, underscoring the sensitivity of new business formation to the overhead costs in the market. The intuition is straightforward: with a high overhead cost, even a less stringent borrowing constraint does not significantly improve the prospects for potential entrepreneurs.

In contrast, the right panel of Figure 5 illustrates that incumbent firms in districts with a higher overhead cost tend to increase their capital more when the borrowing constraint is relaxed to the same degree. Yet, firm expansion is less affected by overhead costs—there is a mere 5% larger expansion in capital in districts with higher overhead costs compared to those with lower overhead costs, with the latter's value normalized to 1. This highlights an important interaction between the financial constraints faced by potential entrepreneurs and the overhead costs inherent to the business environment of a district.



(a) Firm Entry

(b) Firm Expansion

Figure 5: Effect of Easing Borrowing Constraint on Firm Entry and Expansion against f_m

Note. The figure illustrates the relationship between district-level overhead costs f_m, ranging from 0.4 to 2, the mass of entrants (left panel), and the change in firm capital size (right panel), induced by a relaxation of the borrowing constraint, λ_m, increasing from 1.5 to 3. For each given f_m, we sample 10^5 (z, c) from a bi-variate Pareto distribution ($a = 1.5, \theta_1 = 0.01, \theta_2 = 0.01$), and calculate the mass of entrants and change in firm capital size. We repeat this procedure 1000 times and use the average values.

General Equilibrium Effects. Beyond the direct effects on firm expansion and entry, relaxing the borrowing constraint also has several important general equilibrium effects on local labor markets. Both the expansion of constrained incumbent firms and new entrants raise labor demand and bid up local wage w_m, as determined by the labor market clearing condition:

$$\begin{aligned} &\int_c \int_{\max\{\underline{z_c}, \underline{z_{uc}}\}}^{\infty} l_c(w_m, \lambda_m) dF(z,c) + \int_{\underline{c}}^{\infty} \int_{\underline{z}}^{\underline{z_{uc}}} l(w_m) dF(z,c) \\ &= \int_c \int_z 1\{\max\{\pi(w_m), \pi_c(\lambda_m, w_m)\} < w_m\} dF(z,c) \end{aligned} \tag{9}$$

$\underline{c}$ is the level of wealth such that the financial constraint condition (5) intersects with the unconstrained threshold $\underline{z}$ at $(\underline{z}, \underline{c})$.[24] The first term represents the total labor demand from constrained firms, which increases with λ_m due to higher l_c and lower $\underline{z_c}$. The second term is the total labor demand from unconstrained firms, which also increases because a higher λ_m allows for more unconstrained entry, as discussed above.[25] The right-hand side represents

[24]It is straightforward to confirm that $(\underline{z}, \underline{c})$ is also the intersection of the financial constraint condition (5) with the constrained (8) threshold.

[25]Theoretically, a very large increase in λ_m could lead to a decrease in the total employment within constrained firms if many constrained firms transition to an unconstrained status. However, the total employment across both constrained and unconstrained firms would still increase with λ_m.

the mass of households who choose to be workers over entrepreneurs. As λ_m increases, w_m must increase to maintain equality, which could partially offset the expansion of constrained firms.

A higher w_m raises both constrained ($\underline{z_c}$) and unconstrained ($\underline{z}$) productivity thresholds following Equations (8) and (4), thus elevating the exit rate. Intuitively, marginal entrepreneurs are more likely to exit due to lower profits and a higher local wage. It is important to note that λ_m has no direct effect on the unconstrained threshold $\underline{z}$, so unconstrained firms have an unambiguously higher exit rate following an increase in λ_m, especially for those on the margin (firms that are smaller and less productive). The *net* effect on the constrained threshold $\underline{z_c}$ is ambiguous and depends on the relative strength of the GE effects.

Extensions. In this simple framework, we do not consider rural-to-urban or cross-district migration, instead assuming an exogenous urban labor supply. We hope to relax these assumptions in a future version of the model by endogenizing sectoral labor supply and cross-district migration. In the subsequent section we will show empirical results that suggest that the natural experiment had an important and sizeable effect on rural-to-urban migration. We can also extend our framework by incorporating endogenous innovation, in the spirit of Klette and Kortum (2004). We hope to allow for firms in the model to produce multiple products, in line with empirical results we will show in this section on the effect of bank expansion on product innovation.

Summary. Our simple framework offers insights on firm dynamics in the presence of borrowing constraints and overhead costs. The model shows that while we need a high overhead cost to rationalize the larger firm size and a smaller number of firms in under-banked districts, the overhead cost cannot be primarily driven by the scarcity of bank branches, which can hardly explain why firm size grew further following the reform. Assuming a borrowing constraint dependent on the number of bank branches, the model predicts that relaxing financial constraints would lead to the expansion of constrained firms and the exit of smaller unconstrained firms as the unconstrained threshold $\underline{z}$ increases. The *net* effect on firm entry is ambiguous and depends on the relative strength of the direct effect and the GE effects through local wages. However, in the presence of migration constraints and a relatively inelastic labor supply, the GE effect might outweigh the direct effect and potentially mitigate entry. Therefore, following the reform, the average firm size in under-banked districts could become even larger, if the effects on entry are more muted and the exit rate increases. We will test these predictions in the next subsection.

4.3 Estimating Equation: Difference-in-Discontinuity

In this subsection, we test several key predictions of the model by estimating the treatment effects of bank expansion on average firm size, entry, and exit. In addition, we will also examine the aggregate effects at the district level. To capture the dynamic effects as depicted in Figure 1 and 2, we employ the Difference-in-Discontinuity design, following Grembi et al. (2016). Intuitively, this approach first compares firms in treatment versus control districts with a population-per-branch ratio close to the cutoff, as in the standard RD design. Then we compare this discontinuity around the cutoff before and after the policy. Importantly, we control for district fixed-effects in our baseline regression and only use with-district variation in the timing of the policy.

To estimate the treatment effect of the bank expansion policy on firms, we have the following equation:

$$
\begin{aligned}
y_{idt} =& \beta_1 UnderBank_d + \beta_2 Ratio_d + \beta_3 UnderBank_d * Ratio_d \\
& + Post_{2006} * (\delta_1 UnderBank_d + \delta_2 Ratio_d + \delta_3 UnderBank_d * Ratio_d) \\
& + \sigma_d + \sigma_t + X_{idt} + \varepsilon_{itd} \\
s.t. & - h < Ratio_d < h
\end{aligned}
$$

where y_{idt} includes outcome variables of firm i in district d in year t. The first three terms on the right-hand side comprise the standard RD design with the running variable $Ratio_d$—the population-branch-ratio of district d. By fitting a first-order polynomial of the running variable on both sides of the cutoff, β_1 captures the effect of being in treated districts on the outcome variable y_{idt}. The next three terms in the second row are interacted with a post-policy dummy variable, allowing for variations in the discontinuity before and after the policy. δ_1 thus captures the treatment effect of the bank expansion policy on firms.

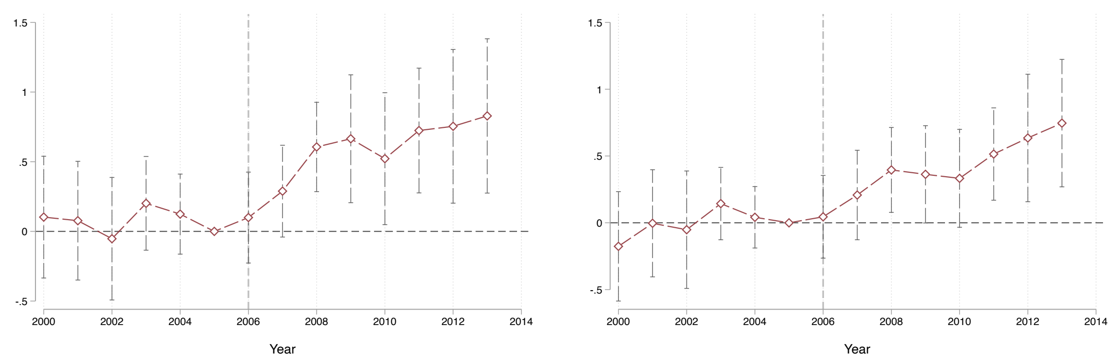
The covariate X_{idt} includes firm ownership fixed effects, an urban dummy variable, and district characteristics (including district population and the number of bank branches), interacting with a linear time trend. σ_t and σ_d are year and district fixed effects, respectively. Notably, the main RD effects in the first row are absorbed by the district fixed effects. Standard errors are clustered at the district level to account for potential correlation within districts. The parameter h is the MSE-optimal bandwidth, and only firms within this bandwidth are included in the sample. We conduct several robustness checks to confirm the consistency of our results across different bandwidths.

A similar empirical equation is used to estimate the treatment effect on district-level aggregate capital, sales, and manufacturing employment. These outcome variables are constructed as the weighted sum of firm-level counterparts, using the sampling weights.

The assumption of our identification strategy is that firms in districts just below and just above the national average follow a (local) parallel trend, akin to the standard Difference-in-Difference design. Note that this assumption is considerably weaker than the assumptions in the standard RD approach, which requires that all covariates should exhibit smoothness around the cutoff—a feature we have validated for several key covariates in Figure B.2. Our identification strategy merely requires that any discontinuities around the cutoff, resulting from any cause, remain unchanged in the absence of the policy—an assertion we will validate in the following event study plot.

4.4 Effects of Bank Expansion on Firms

Event study graphs. Figure 1 and 2 visually illustrate that the gaps in firms' fixed assets between under-banked and banked districts remain remarkably stable throughout the pre-treatment period. Furthermore, they become indistinguishable from one another when we focus on districts close to the policy cutoff. We can formally test the assumption of (local) parallel trend in event study graphs, by substituting the post-policy dummy with year dummies in our estimating equation.



(a) log(Fixed Assets)

(b) log(Sales Revenue)

Figure 6: Event Study Graphs for the Treatment Effects on Capital and Sales

Figure 6 displays the event study graphs for capital and sales revenue. These graphs report the yearly treatment effects of being situated in (near cutoff) treated districts relative to the controls, using the same controls and bandwidth as our baseline equation. The nonexistence of a discernible effect prior to the reform provides visual evidence of the "parallel trends" assumption, thereby validating our identification strategy.[26] Figure B.7 reports sim-

[26]A potential concern could be the migration of plants from control to treated districts, in anticipation

ilar patterns and corroborates the absence of pre-trends for wage bills and total employment.

Following the bank expansion, firms in treated districts grow larger by using more capital and labor, and generating more revenues. These effects are both economically significant and unfold progressively over time, in line with the idea that changes in the allocation of resources are typically slow-moving. Meanwhile, it is worth noting that the adjustments in labor appear to lag even further, likely due to other constraints in the labor markets, as documented by Bertrand et al. (2021).

Baseline estimates. Table 1 presents the estimated treatment effects of the bank expansion policy on firm-level sales revenue, fixed assets, and wage bills using our baseline estimating equation. For the average firm, capital increases by 37% (column 2), indicating that the policy exerts large positive effects on capital investments.[27] The higher capital investment does not crowd out labor, as wage bills and employment increase by approximately the same amount (columns 3-4), suggesting strong complementarity between capital and labor. Post-reform, firms in treated districts expand their sales revenue by approximately 30%.

Table 1: Treatment Effect of Bank Expansion on Firms

	(1)	(2)	(3)	(4)
Dependent Variable	Revenues	Capital	Wage Bill	Employment
Treated * Post	0.291**	0.371**	0.357***	0.306***
	(0.142)	(0.166)	(0.133)	(0.113)
Observations	135,673	135,673	135,673	135,673
R-squared	0.173	0.212	0.199	0.147
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes

Notes: All outcome variables are in logs. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

of improved financial access. However, such a movement is implausible given India's stringent industrial regulation scheme. Evidence from our plant panel data from 1998 to 2013 indicates that less than 5% of all plants have ever relocated their districts. Note that this figure could be inflated due to possible measurement or coding errors in the district information. Our baseline analysis uses a plant's modal district as the time-invariant district. Our results remain robust when plants that have relocated are excluded.

[27] As shown in B.3, the increase in fixed assets is mostly driven by investments in buildings, plants, and machinery, affirming that firms use newly acquired credit to improve production capacity.

Robustness. Table B.1 shows that the estimates are robust under the most parsimonious specification, which includes only district and year fixed effects. As we demonstrate in Table B.2, our results are robust to the inclusion of 2-digit industry-by-year fixed effects. By comparing firms in the same 2-digit industry in the same year, this specification accounts for any unobserved, time-varying, sector-level shocks, such as aggregate trade shocks and changes in the priority sector lending policies at the 2–digit industry level.[28] In addition, to account for the possibility that some Indian states are more exposed to the reform and may have adjusted their state-level banking regulations or been affected by other concurrent shocks, we flexibly control for any state-level time-varying unobserved shocks. Table B.2 shows that our results are robust to the inclusion of state-by-year fixed effects. Moreover, Figure B.8 and B.9 demonstrate that our results hold robustness to dropping individual states and 2-digit industries one by another. Figure B.10 shows that using different bandwidths does not change our estimates qualitatively.

Outstanding loans. One natural question is to what extent the growth of firms documented above is attributable to bank expansion alleviating credit market frictions. An alternative hypothesis is that bank expansion might stimulate local economic activity and consumption, allowing firms to generate higher sales revenue, which they can reinvest. While we ideally want to examine the treatment effect on loans that firms obtained directly from banks, the ASI data only provides information on total outstanding loans without specifying their sources. Nevertheless, we present the effect on total outstanding loans in Table B.4 (column 1-3) and the event study graph in Figure B.7 (panel (c)). Following the reform, outstanding loans experienced a 41% increase (using our baseline equation in column 1) for the average firm in treated districts.[29] The substantial increase in total outstanding loans suggests a real expansion in firm-level credit rather than a reorganization of existing liabilities—using bank loans to replace other higher-interest debts. Notably, this point estimate aligns with the estimate for fixed assets, implying that the alleviation of borrowing constraints can account for the entirety of the observed firm growth.

[28] All banks (public and private) are required to lend at least 40% of their net credit to the "priority sector", which includes agriculture, agricultural processing, transport industry, and small scale industry (SSI). If banks fail to satisfy the priority sector target, they are required to lend money to specific government agencies at very low interest rates (Banerjee and Duflo, 2014). The definition of the priority sector has expanded over time. There are 53 distinct 2-digit industries in our ASI data.

[29] In our sample, more than 85% of firms reported having positive outstanding loans even before the reform. Considering that most firms had already accessed some form of loan, we did not observe a significant effect of bank expansion on the binary variable representing loan usage, as shown in Table B.4 (column 4-6). However, it's crucial to highlight that this finding does not rule out the possibility that bank expansion could reduce the fixed costs of accessing credit. If credit usage is a prerequisite for market entry, we would expect all existing firms to borrow. In this scenario, a reduction in borrowing costs would not change the proportion of firms using loans; instead, it could lower entry barriers and alter the composition of the firm pool.

Product Scope. Another dimension to consider is that as households become richer, they may diversify their consumption portfolio by purchasing more varieties of products (Li, 2021). This increase in the demand for variety could encourage firms to expand their production by introducing new products. This discussion ties into a broader debate in growth theory (Garcia-Macia et al., 2019): do firms achieve growth through the creation of new products, the process of creative destruction, or by improving their existing products? If relaxing financial constraints could lead to the improvement of existing products or the introduction of new products, the bank expansion reform could yield substantial dynamic gains by stimulating innovation.

We can directly test the effects on product composition by leveraging a unique feature of the ASI, which reports both total product sales and total quantity sold at the firm-product level, as mandated by the 1956 Companies Act.[30] With this information, we compute the establishment-level total number of products, a price index constructed as the weighted average of product prices, and indicators of product addition and deletion using the firm-product panel.

Table 2: Treatment Effect of Bank Expansion on Product Portfolio

	(1)	(2)	(3)	(4)
Dependent Variable	log(Product)	log(Price)	Add Product	Del. Product
Treated*Post	0.029	-0.048	-0.004	-0.003
	(0.035)	(0.188)	(0.023)	(0.018)
Observations	123,972	123,871	86,286	86,286
R-squared	0.093	0.104	0.025	0.021
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes

Notes: log(Product) is the log number of products, and log(Price) is the firm-level average product price, weighted by product sales revenue. Add Product is a dummy variable equal to 1 if the firm has more products than the previous year. Del. Product equals 1 if the firm has fewer products than the previous year. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2 shows that the reform has a negligible and statistically insignificant impact on the number of products (column 1), contradicting the demand for variety hypothesis. Column 2 reveals that bank expansion exerts a slightly negative, yet not significant, effect on the

[30] The Act requires Indian firms to disclose product-level information on capacities, production, and sales in their annual reports. The product is defined based on the 5-digit product codes and comprises 11,880 distinct products.

price index, hinting at limited improvement in product quality. The muted effect on price also alleviates another concern that our results might be driven by an increase in demand in treated districts, which should push up output prices. Furthermore, columns 3-4 indicate that firms in treated districts are not more likely to add or delete products post-reform compared to their counterparts in control districts. Thus, the observed firm expansion is unlikely to be driven by firms creating new products or stealing from others. Instead, the finding suggests firms primarily grow by increasing the production of existing products, hinting at limited dynamic gains.

Firm Entry and Exit. While we have demonstrated that the average firm in treated districts grew larger following the reform, it remains unclear whether it is driven by the growth of firms or changes in the *composition* of firms. Our framework predicts that bank expansion would unambiguously lead to a higher exit rate among small firms, owing to the elevation of the unconstrained productivity threshold, $\underline{z}$. However, the net effect on entry remains ambiguous, contingent upon the balance between the direct effect caused by a lower constrained threshold, $\underline{z_c}$, and the general equilibrium effect attributed to increased local wages. The average firm size could potentially shrink post-reform if the entry rate surges significantly, even amidst the expansion of constrained firms.

We construct firm entry and exit indicators using our panel data, following the methodology of Harrison et al. (2015).[31] Both entry and exit rates in our sample are at 6.7%,[32] indicating that the economy is in a steady state. Table 3 presents the treatment effects on firm entry in Panel A and exit in Panel B. The first column reveals that the reform has positive but statistically insignificant effects on both entry and exit rates. This suggests that our observed firm expansion results are primarily driven by the growth of incumbent firms, rather than changes in the composition of firms.

However, a closer examination reveals substantial variation across firm sizes. Column 2 presents the heterogeneous treatment effects, in which our "Treated*Post" term is interacted with a "Big" dummy variable, assigned as 1 if a firm's average employment surpasses the national average.[33] The main "Treated*Post" term suggests an increase in the exit rate of

[31] An entry is defined as a firm appearing in the data for the first time within three years of the initial production year. An exit is when a firm is officially declared "closed" in the ASI and remains so.

[32] The figures are consistent with the exit rate imputed from plant age cohorts by Hsieh and Klenow (2014), also using the ASI data.

[33] We use the average establishment size over the years as a proxy for firm-level productivity. Given that firms usually enter the market at a smaller size, and considering that some firms may already be on a downward trajectory before their exit, we opt not to utilize the establishment size at the points of entry and exit. This "Big" dummy is interacted with all single and cross-terms in our baseline Difference-in-Difference specification.

Table 3: Treatment Effect of Bank Expansion on Firm Entry and Exit

Panel A: Entry

	(1) Full Sample	(2) Full Sample	(3) $L \in (0, 20)$	(4) $L \in [20, 100)$	(5) $L > 100$
Treated*Post	0.009	0.001	0.002	0.024	-0.001
	(0.010)	(0.016)	(0.018)	(0.017)	(0.010)
Treated*Post*Big		0.009			
		(0.021)			
Observations	166,094	166,094	51,623	52,553	61,917
R-squared	0.047	0.054	0.062	0.054	0.032
Mean of dependent variable	0.0674	0.0674	0.0918	0.0867	0.0306

Panel B: Exit

	(1) Full Sample	(2) Full Sample	(3) $L \in (0, 20)$	(4) $L \in [20, 100)$	(5) $L > 100$
Treated*Post	0.013	0.047**	0.069**	0.033	0.003
	(0.012)	(0.022)	(0.027)	(0.025)	(0.008)
Treated*Post*Big		-0.038*			
		(0.020)			
Observations	166,094	166,094	51,623	52,553	61,917
R-squared	0.027	0.045	0.053	0.040	0.021
Mean of dependent variable	0.0674	0.0674	0.101	0.0768	0.0314

Notes: Entry equals 1 in the first year an establishment appears in the data within three years of the initial production year. Exit equals 1 if an establishment is officially declared "closed" in the ASI and remains closed thereafter. All regressions include district and year fixed effects, firm controls, and district trends. Big is a dummy variable equal to 1 if the establishment's average employment number is greater than the national average. We interact this Big dummy with all the single and cross-term in our baseline Diff-in-Desc specification. Firm controls include firm ownership fixed effects and a dummy variable of being in urban areas. District trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

smaller firms by 4.7 percentage points (approximately 70% of the sample mean). This aligns with our theoretical framework, as the reform raises the exit rate among smaller firms due to the elevation of the unconstrained productivity threshold. Interestingly, this main effect is almost entirely offset by the interaction term, indicating that larger firms' exit rates are unaffected by the reform. Regarding firm entry, the reform also exerts a slightly larger, albeit still insignificant, effect on the entry of larger firms, consistent with our model. To further validate our findings, we present additional results in columns 3-5 using the baseline Difference-in-Discontinuity specification, disaggregated by firm sizes. These results confirm that the observed higher exit rate is mainly driven by very small firms (with fewer than 20 employees), while the effects on larger firms are small and insignificant.

TFP. We conclude our firm-level analysis by noting that the reform had minimal effect on firm productivity. We measure firm-level productivity (TFP) using two methods: (1) calculating Solow residuals, and (2) estimating the revenue production function, following the approach outlined by Levinsohn and Petrin (2003). Table B.5 presents the effects on TFP using both measures. Interestingly, our results indicate that the reform has precisely estimated null effects on both measures of firm productivity. This finding aligns with our static model that abstracts from innovation and previous empirical results, indicating that incumbent firms grow by employing more resources to increase the production of existing products under the same technology. In comparison, changes in firm composition and innovation (including both product and process innovation) play a relatively insignificant role.

4.5 District-level Aggregated Effect

Our analyses above have shown that the average firm expands due to bank expansion. In this subsection, we present district-level results to explore the aggregate effect of the reform on local markets, echoing our graphic evidence reported in Figure 1. Our district-level variables are constructed by aggregating the establishment-level variables, inflated by their sampling weights. We use the same bandwidth as in our plant-level analysis with the same estimating equation to ensure that we are comparing the same set of treatment and control districts.[34]

To further validate our empirical strategy, Figure B.11 presents the event study plots for district-level aggregate revenues, capital, wages, and employment. Consistent with the time-series plots of total capital in Figure 1, treated districts did not experience faster industrial growth prior to the policy implementation, providing visual evidence that pre-trends cannot bias our estimates.

Table 4 presents our Difference-in-Discontinuity results at the district level. Importantly, these results align closely with the firm-level estimates reported in Table 1. The coeffi-

[34]We replace our firm-level controls, including firm ownership fixed effects and an urban dummy variable, with the district share of private firms and the share in urban areas. Some districts have very few firms in some years and generate a significant amount of noise; hence, we drop district-years with less than 20 firms. Dropping these observations does not change our estimates. In addition, we control for the number of plants in a district-year to account for the noise, which, again, does not change our estimates.

Table 4: Treatment Effect of Bank Expansion on District Aggregate

Dependent Variable	(1) Revenue	(2) Capital	(3) Wage Bill	(4) Employment
Treated * Post	0.385**	0.387***	0.340*	0.308**
	(0.183)	(0.136)	(0.172)	(0.126)
Observations	1,838	1,838	1,838	1,838
R-squared	0.960	0.935	0.972	0.969
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes

Notes: All outcome variables are constructed as the weighted sum of firm-level variables using the sampling weights and then transformed in logs. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Additional controls include the share of private firms, the share in urban areas, and the number of firms in a district-year. Regressions are weighted by district-level capital size during the pre-reform period. District-year observations with less than 10 plants are Dropped. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

cients suggest a significant industrial growth in districts that were narrowly under-banked prior to the reform. Post-reform, these treated districts saw a 39% surge in manufacturing output, facilitated by a substantial increase in fixed assets (39%) and manufacturing labor—demonstrated by a 34% rise in wage bills (column 3) and a 31% growth in employment (column 4).

The remarkable consistency between the district-level aggregate results and the firm-level findings suggests that changes in firm composition have a negligible impact on the observed trends. This is further supported by the insignificant effects on firm entry and exit documented in Table 3.

5 Effects on Structural Transformation

So far, we've shown that manufacturing firms experienced significant expansion in treated districts following the reform. In this section, we further explore the implications of this growth on local labor markets, particularly focusing on the extent to which banking sector reforms shifted labor from agricultural to manufacturing sectors. On one hand, the growth of manufacturing firms, acting as a pull-side factor, creates new industrial job opportunities and elevates wages in the manufacturing sector. As a consequence, the reform might "trickledown" to poor households and draw workers away from agriculture.[35] On the other hand, improved access to finance could also stimulate agricultural investments, potentially increasing the marginal productivity of agricultural labor.[36] Hence, the net effect on inter-sectoral labor reallocation is *ex-ante* unclear.

Furthermore, existing literature has highlighted substantial inter-sectoral labor mobility costs in India, potentially confining labor to agriculture even in the face of low productivity. Munshi and Rosenzweig (2016) highlights that the wage gap between urban and rural areas in India is substantial, with urban wages being over 47% higher than rural wages even for unskilled workers. This wage gap remains constant over time and is significantly larger than in other developing countries, such as China and Indonesia. Surprisingly, despite the potential for higher wages in urban areas, rural workers in India do not capitalize on these arbitrage opportunities, as evidenced by the country's low migration rate.[37] Several factors may impede labor mobility and lead to the misallocation of labor in India, including limited access to transportation infrastructure such as roads (Asher and Novosad, 2020) and the informal insurance networks deeply rooted in the traditional caste system (Munshi and Rosenzweig, 2016). Consequently, it remains an empirical question whether industrial growth can effectively release workers trapped in the agricultural sector, particularly those with lower agricultural productivity.

In this section, we illustrate that bank expansion, by fueling industrial growth and creating more non-agricultural job opportunities in treated districts, can alleviate labor misallocation. Farmers in villages with lower agricultural suitability strongly respond to the reform and transition away from agriculture. To quantify this effect, we utilize the village-level population census data from 2001 and 2011,[38] matched with crop suitability data from the

[35] In a similar vein, Barboni et al. (2021) investigates the effects of bank expansion also in the context of India, leveraging experimental evidence. Their study reveals that while bank expansion facilitated greater access to loans for impoverished rural households, the recipients primarily used these loans for consumption purposes rather than for investment. Despite this consumption-focused utilization of loans, these households experienced an increase in rural income. This outcome is potentially attributed to the "trickling-down" effect: the heightened economic activity engendered by the expansion of banking services indirectly catalyzes an increase in local wages.

[36] Agricultural investments and new technology can potentially lower the marginal productivity of labor, as exemplified by the case of Brazil's GE soybean seeds (Bustos et al., 2016). However, Madhok et al. (2022) demonstrates that, in India, most agricultural investments serve to augment labor rather than replace it.

[37] Drawing on detailed district-wise migration flow data from the 2001 population census, Kone et al. (2018) underscores that the 5-year inter-district migration rate in India is remarkably low, at a mere 2.8%. In stark contrast, the 5-year inter-prefecture migration rate in China stands significantly higher at 10%. In light of these facts, spatial sorting of workers according to skills a la Young (2014) is insufficient to explain the substantial rural-urban wage gap in India.

[38] Villages in India are typically small, with approximately 500,000 in total, each averaging around 1,000 inhabitants. Over 70% of all main workers are engaged in the agricultural sector.

FAO Global Agro-Ecological Zones (GAEZ)[39] as facilitated by Shrug (Asher and Novosad, 2020). Villages are sorted into quartiles based on their crop suitability *within* a sub-district, where they likely encounter similar industrial labor demands.

We then evaluate the treatment effect of bank expansion on the number of farmers in each quartile separately. More specifically, we estimate the following empirical equation using the local linear RD approach with MSE-optimal bandwidth, following the methodology proposed by Calonico et al. (2014).

$$Y_{ids} = \beta_1 UnderBank_{ds} + \beta_2 Ratio_{ds} + \beta_3 UnderBank_{ds} * Ratio_{ds} + X_{ids} + \sigma_s + \varepsilon_{ids}$$

$$s.t. - h < Ratio_{id} < h$$

Here i denotes village, d denotes district, and s denotes state. Y_{ids} includes our village-level outcome variables in 2011. $Ratio_{ds}$ is district d's population-branch ratio (relative to national average) and $UnderBank_{ds} = 1$ if district d that has a $Ratio_{ds} > 0$. We control for the ratio's components, village's land area, and population in 2001 in X_{ids}, and state fixed effect in σ_s. We also control for the baseline measures (using the 2001 population census) of the respective variable of interest as recommended by Lee and Lemieux (2010). h is the estimated MSE-optimal bandwidth. Standard errors are clustered at the district level.

Figure 7 illustrates the treatment effect on the log number of farmers by cereal crop suitability quartiles. The number of farmers decreases by roughly 40% in villages with the lowest crop suitability, as demonstrated on the left side of the plot. As we move to villages with higher crop suitability, the treatment effect gradually diminishes in magnitude and becomes statistically indistinguishable from zero. These findings align with the hypothesis that bank expansion reduces labor misallocation by encouraging labor mobility away from the agricultural sector in villages with lower agricultural productivity.

The calculation of crop suitability requires an assumption about input usage.[40] While the farming system in India typically aligns more closely with the low-input case (represented by blue lines), our findings remain robust under the assumptions of high-input usage (indicated

[39]The GAEZ crop suitability data has been extensively applied in the literature, as reviewed by Donaldson and Storeygard (2016), among others. GAEZ utilizes agronomic models to predict potential crop yields based on location characteristics (including topography and soil type) under various levels of input usage.

[40]Under the low-input/traditional management scenario, farming is predominantly subsistence-based, with practices involving the use of traditional cultivars, labor-intensive techniques, minimal nutrient and pest control, and limited conservation measures. Conversely, the high-input/advanced management scenario assumes improved high-yielding varieties, full mechanization with low labor intensity, and optimal applications of nutrients and chemical pest, disease, and weed control.

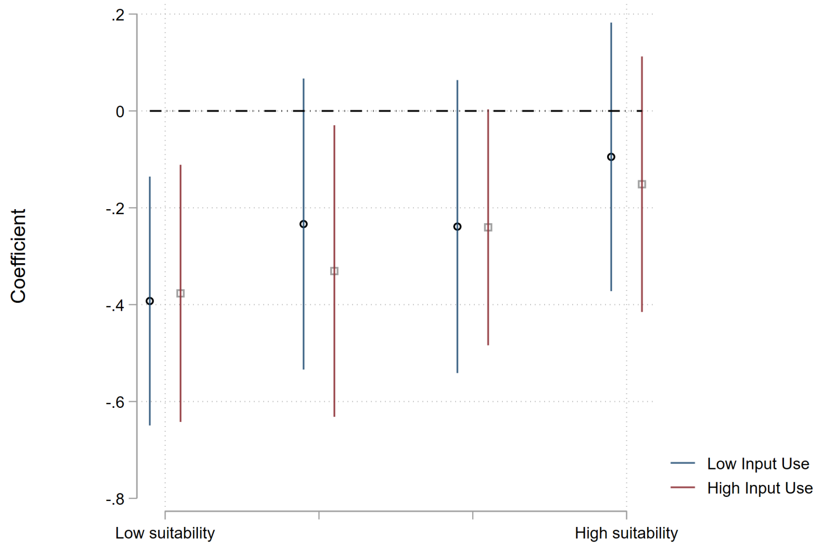
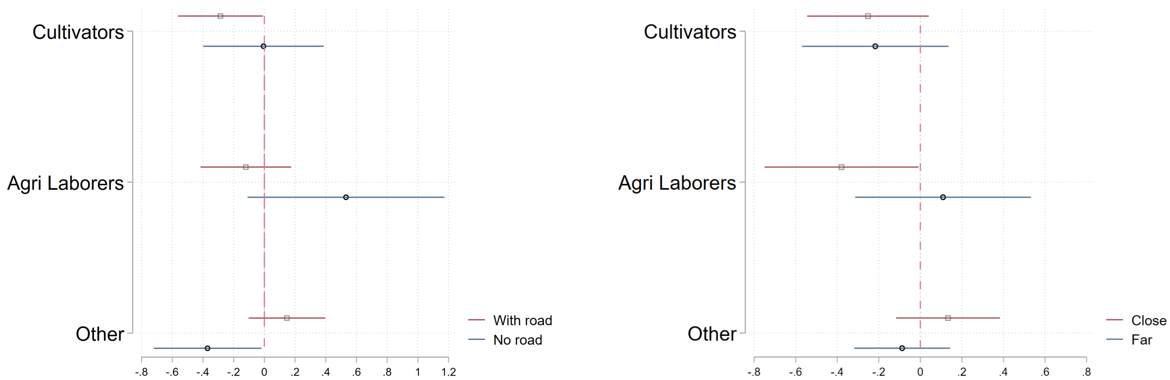


Figure 7: Treatment Effect of Bank Expansion on log(Farmer) by Crop Suitability

by red lines). Here, as reported in Table B.6, we document the effects on the logarithm of the number of farmers, controlling for the village population in 2001. Table B.12 confirms that our estimates are quantitatively similar when using the number of farmers as an alternative outcome variable. As a placebo test, we use data from the 2001 population census in Table B.9, revealing that the reform had no discernible effects on cultivators prior to the policy implementation for any given quartile. This indicates that the effects we found in the post-reform period are not merely a result of pre-existing trends or other factors unrelated to the bank expansion.

Labor demand or labor supply? The results presented in Figure 7 lend support to our main hypothesis that industrial growth creates more job opportunities in non-agricultural sectors and thus facilitates structural transformation. However, it is important to consider an alternative hypothesis that bank expansion may also increase industrial labor supply. Assuming non-homothetic preferences, such as a subsistence consumption requirement for agricultural goods, workers in places with lower agricultural productivity may self-select into agriculture because they are poorer (Lagakos and Waugh, 2013). Bank expansion would have a stronger impact in less productive places by relaxing this subsistence requirement and increasing industrial labor supply.

To provide additional evidence supporting our hypothesis regarding the role of labor demand, we examine the heterogeneous effects of the reform. If the labor demand hypothesis is valid, we would expect to see a stronger effect of the reform in villages that can better



(a) Rural Road

(b) Distance to Population Center

Figure 8: Heterogeneity in the Effect of Bank Expansion

leverage the industrial expansion. These could include villages near urban centers (Madhok et al., 2022), where there is higher demand for labor due to industrial growth. Additionally, villages connected by roads may also experience a stronger impact, as improved infrastructure facilitates the movement of goods and labor between rural and urban areas (Asher and Novosad, 2020).

On the other hand, if the labor supply channel is the main driver of the results, we would anticipate a more pronounced response to the reform in more isolated and remote villages. In these areas, the subsistence requirement may have a greater impact on labor allocation decisions, and the relaxation of this requirement through bank expansion could result in a greater shift in labor from agriculture to non-agricultural sectors.

Figure 8 presents the impact of bank expansion on employment in villages with and without road connections (left panel), as well as those situated close to or far from population centers with populations exceeding 10,000 (right panel). It becomes evident that villages connected by roads and those closer to population centers exhibit a stronger response to the bank expansion reform. In these locales, we observe a more pronounced decrease in the number of cultivators and agricultural laborers, coupled with an increase in 'other workers' (including workers in both manufacturing and service sectors). These patterns suggest that subsistence requirements and the labor supply channel alone are insufficient to explain the structural transformation documented in our results.

The regression results are reported in Table B.7 and B.8. Table B.13 and Table B.14 confirm that our estimates are similar using levels instead of logs as outcome variables. Using data from the 2001 population census as a placebo test, Table B.10 and Table B.11 show that the estimates tend to be much smaller and less statistically significant before the policy implementation, thus further support our identification strategy.

Intriguingly, we also note a decline in 'other workers' and a non-significant increase in agricultural laborers in villages without road connections or those situated further from population centers. This pattern suggests a spatial reallocation of agricultural activities to places with lesser access to industrial job opportunities, echoing the results reported in Madhok et al. (2022). This reallocation could be driven by two channels. First, bank expansion could directly impact agricultural investments and activities, and this effect may be more evident in remote and isolated areas. Second, there could be a general equilibrium effect of bank expansion. If farmers in neighboring villages transition to non-agricultural sectors, local food prices could increase, rendering farming more profitable. We plan to assess the relative strength of these two channels in our quantitative exercise.

6 Concluding Remarks

In conclusion, this paper provides a comprehensive analysis of the impacts of bank expansion on industrial growth and labor allocation based on a nationwide policy experiment in India. Our findings reveal that the expansion of banks in under-banked districts alleviated firms' borrowing constraints, stimulating their growth in sales revenue, employment, and capital accumulation. However, contrary to initial expectations, the expansion did not translate into increased firm dynamics or higher entry rates. Moreover, we find that this policy had minimal effects on product innovation and total factor productivity. This policy primarily benefited incumbent firms and left potential entrants largely unaffected, hinting at limited dynamic gains from bank expansion.

On the labor front, we find that bank expansion created more high-paying jobs and led to a significant labor reallocation towards the manufacturing sector, particularly in regions with lower agricultural productivity. This labor movement underscores the role of manufacturing development as a significant "labor pull" factor, shaping the process of structural transformation.

Moving forward, we plan to delve deeper into these findings by constructing a spatial general equilibrium model. This model will help us interpret our empirical results and quantify the aggregate effects of bank expansion. With this model, we intend to examine the consequences of the reform on aggregate economic growth, resource allocation, and labor mobility. We also aim to calibrate this model with the micro-estimates derived from our current analysis. Additionally, we aim to explore various policy counterfactuals, such as the potential efficiency gains from relaxing migration constraints compared to merely increasing the bank presence in underdeveloped regions.

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A Theoretical Appendix

A.1 $\underline{z_c}$ decreases with c and λ_m.

$$
\begin{aligned}
\frac{\partial \underline{z_c}}{\partial c} &= \frac{-\alpha}{c}\left[\left(\frac{1}{\lambda_m c}\right)^{\alpha}\left(\frac{w_m}{\beta}\right)\right]\left(\frac{\beta(w_m+f_m+r\lambda_m c)}{w_m(1-\beta)}\right)^{1-\beta} \\
&+ \frac{\beta r\lambda_m}{w_m}\left[\left(\frac{1}{\lambda_m c}\right)^{\alpha}\left(\frac{w_m}{\beta}\right)\right]\left(\frac{\beta(w_m+f_m+r\lambda_m c)}{w_m(1-\beta)}\right)^{-\beta} \\
&= \frac{-\alpha}{c}(zl_c)^{\beta-1}l_c^{1-\beta} + \frac{\beta r\lambda_m}{w_m}(zl_c)^{\beta-1}l_c^{-\beta} \\
&= z^{\beta-1}\frac{\beta r\lambda_m c - w_m l_c\alpha}{w_m c}
\end{aligned}
$$

Note that $\underline{z_c}$ is solved in Equation 8 when the constrained firm entry condition $\frac{(1-\beta)w_m l_c}{\beta} - r\lambda_m c - f_m \geq w_m$ holds in equality; the second equality then follows from substituting the constrained entry condition and Equation 7 into the equation.

$\frac{\partial \underline{z_c}}{\partial c}$ is thus less than 0 if and only if $\beta r\lambda_m c < w_m l_c\alpha$. Substituting l_c in Equation 7 into the inequality shows that it holds if and only if the unconstrained optimal capital k in Equation 2 is strictly greater than borrowing constraint $\lambda_m c$. We can similarly show $\frac{\partial \underline{z_c}}{\partial \lambda_m} < 0$ if and only if $k > \lambda_m c$.

B Empirical Appendix

B.1 Empirical Appendix

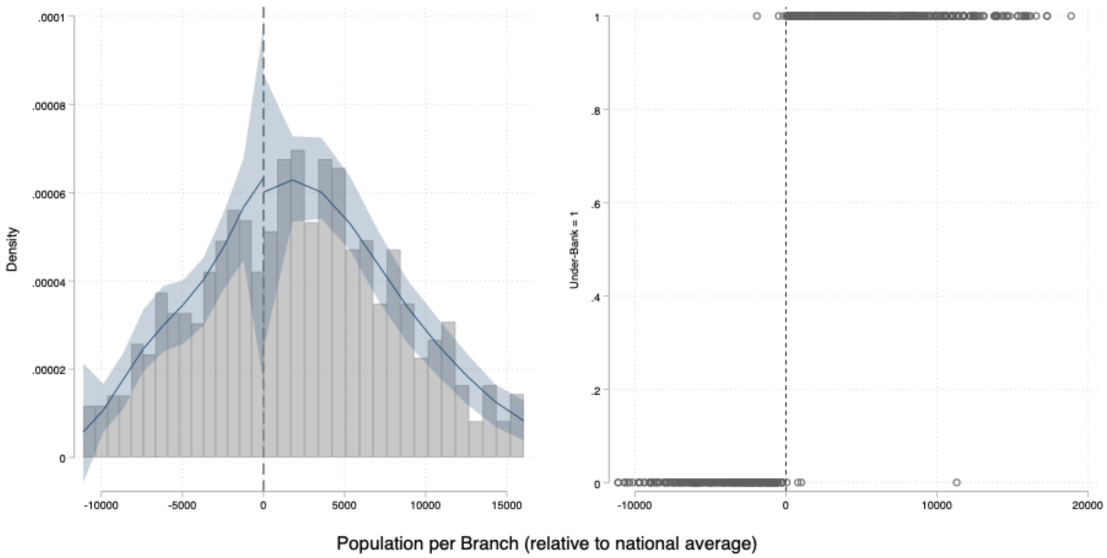
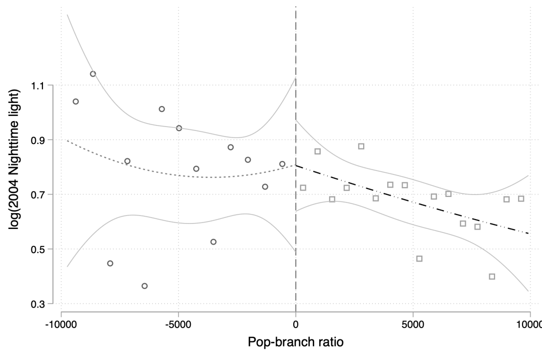
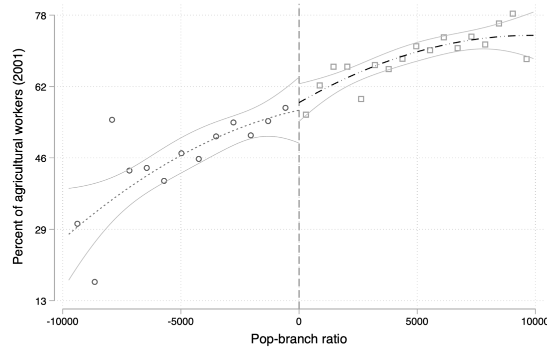


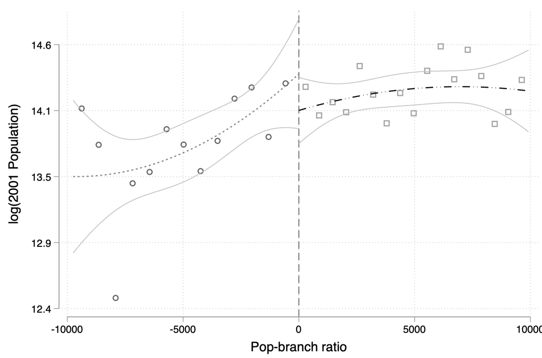
Figure B.1: MaCrary manipulation test and the first stage.



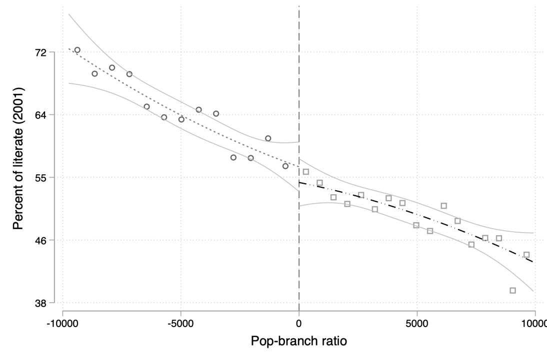
(a) log(Light intensity) in 2004



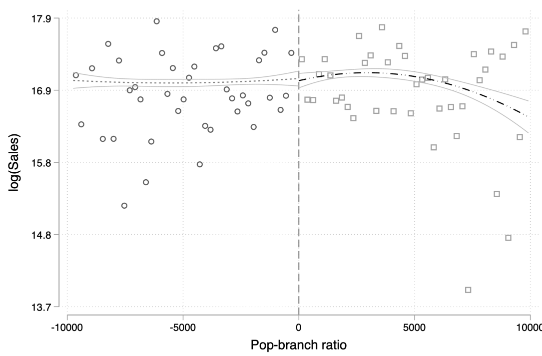
(b) Percent of agricultural workers



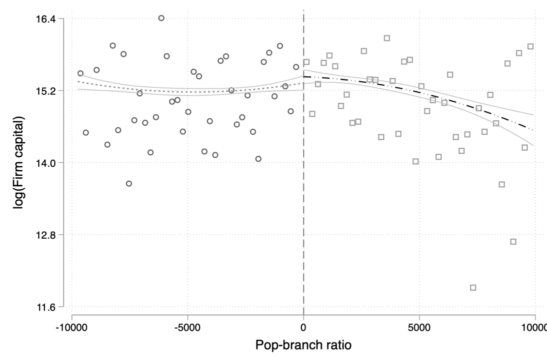
(c) log(Population)



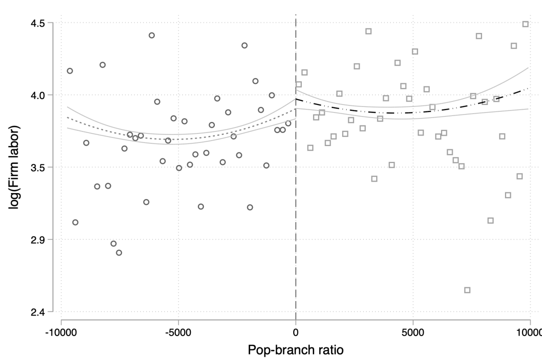
(d) Percent literate



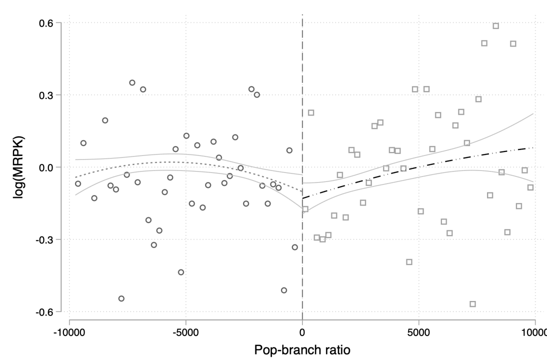
(e) log(Firm sales)



(f) log(Firm fixed asset)

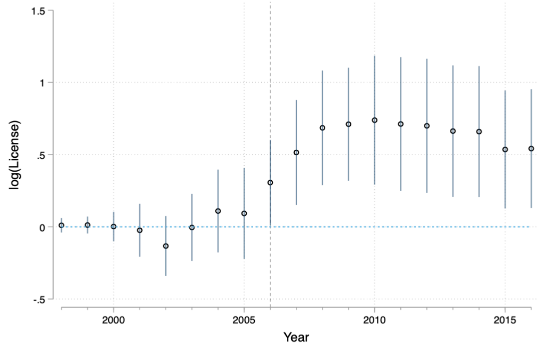


(g) log(Firm employee)

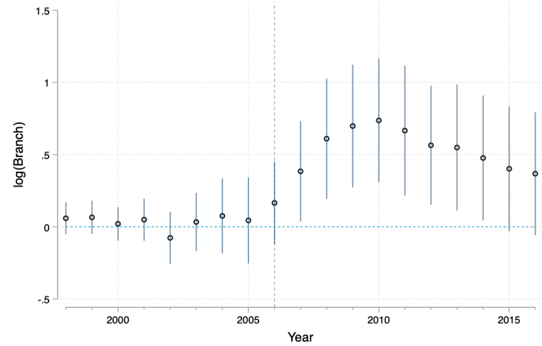


(h) log(Firm MRPK)

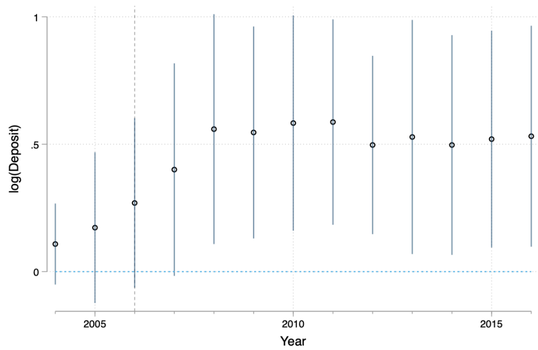
Figure B.2: Smoothness of pre-policy covariates



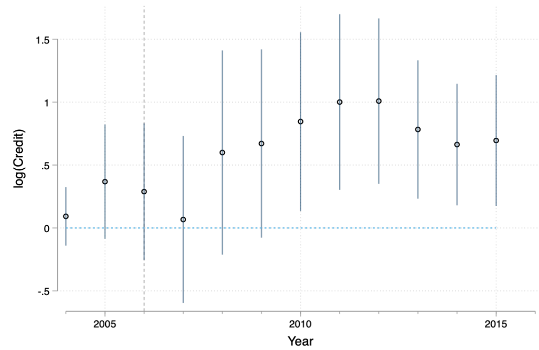
(a) log(Bank licenses)



(b) log(Branches)

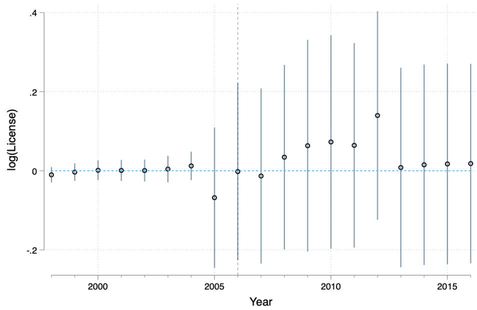


(c) log(Deposit)

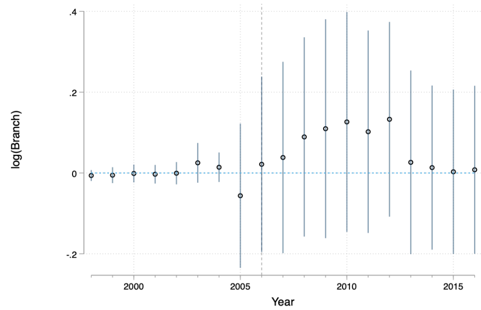


(d) log(Credit)

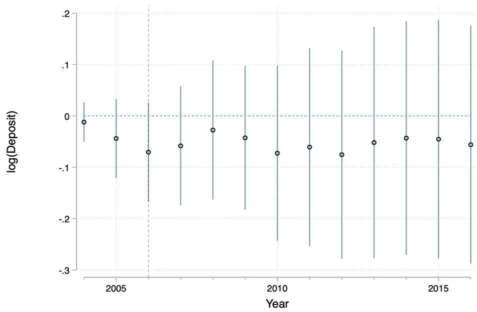
Figure B.3: Efffects on Private Sector Banks



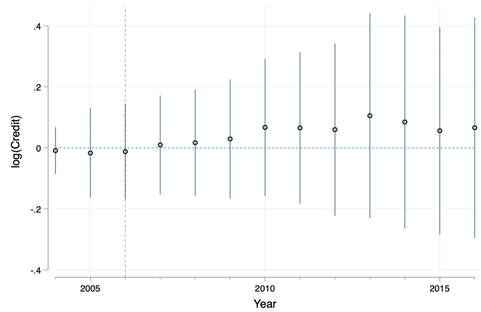
(a) log(Public Bank licenses)



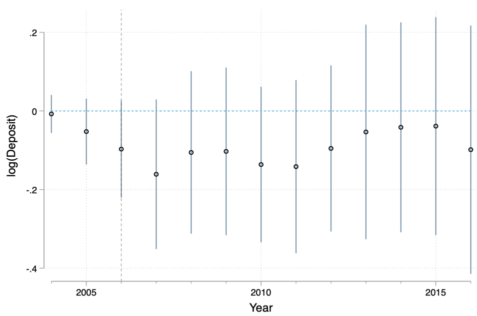
(b) log(Public Bank Branches)



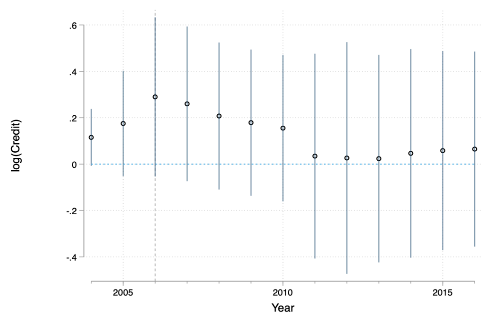
(c) log(Nationalized Bank Deposit)



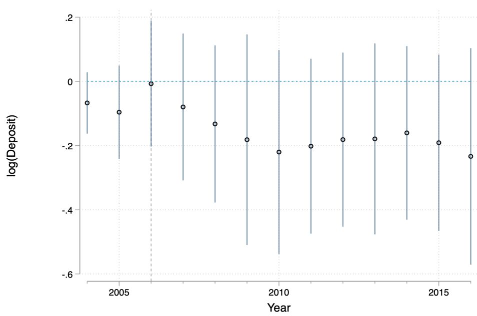
(d) log(Nationalized Bank Credit)



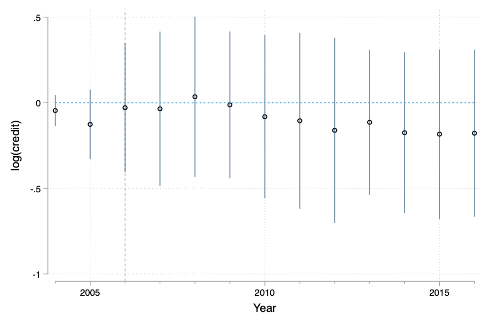
(e) log(SBI Deposit)



(f) log(SBI Credit)



(g) log(RRB Deposit)



(h) log(RRB Credit)

Figure B.4: Effects on Public Sector Banks

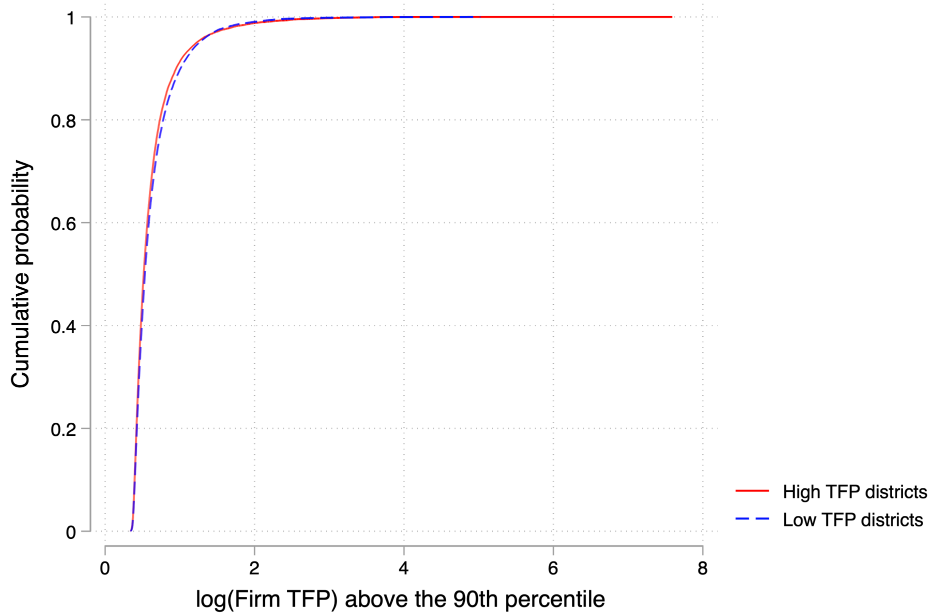
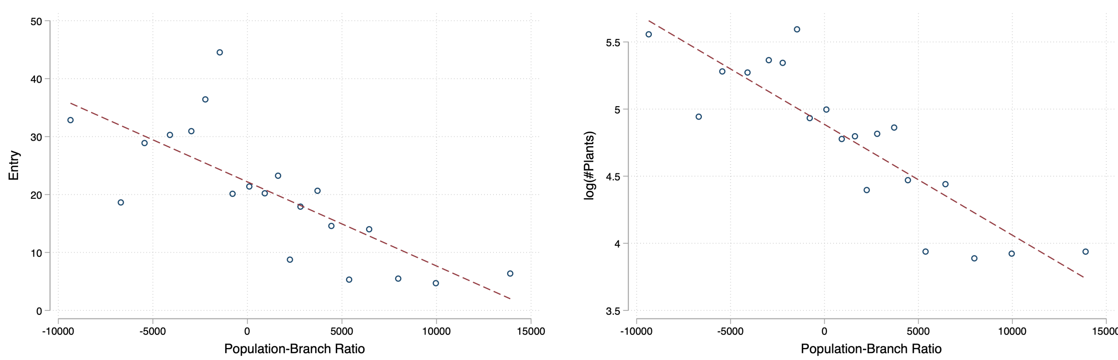
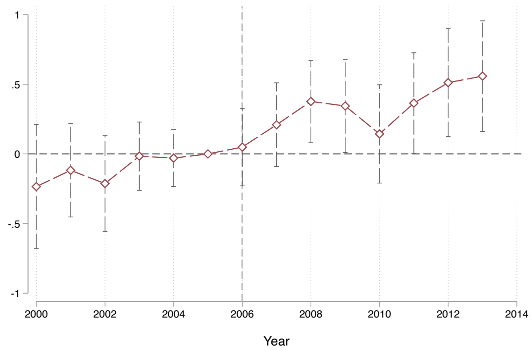


Figure B.5: Truncated Distribution of log(TFP) for district with Low and High Aggregate TFP

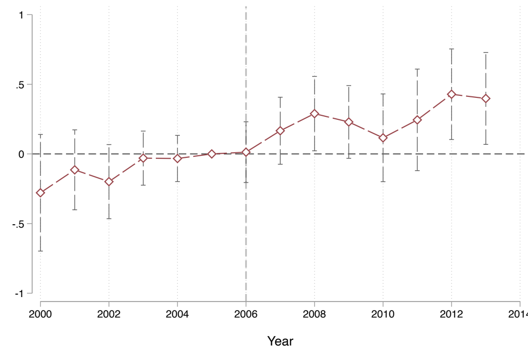


(a) Entry (b) log(Num. of Plants)

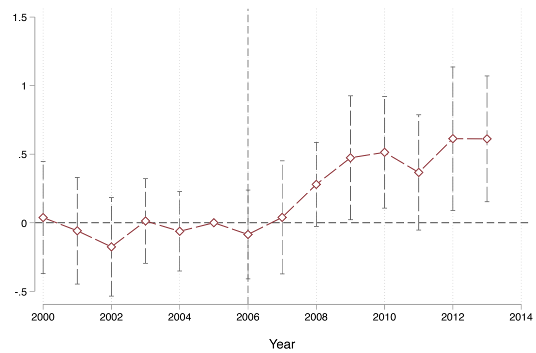
Figure B.6: District-level Firm Entry, Number of Plants, and Population-Branch Ratio



(a) log(Wage Bill)

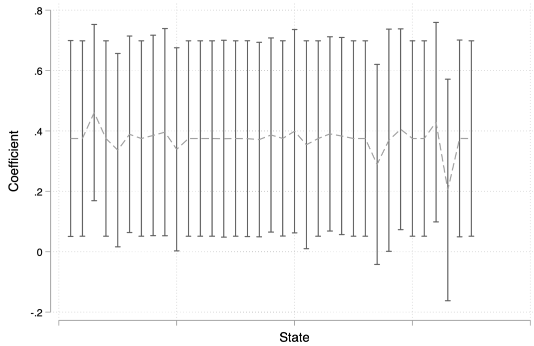


(b) log(Employment)

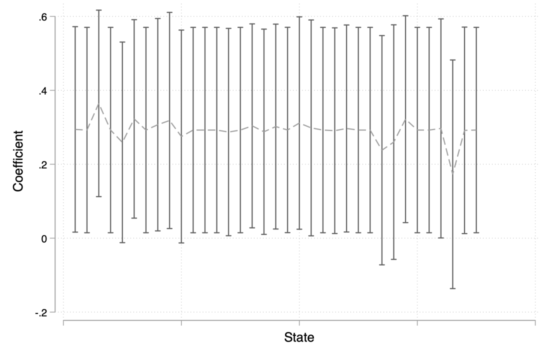


(c) log(Outstanding Loan)

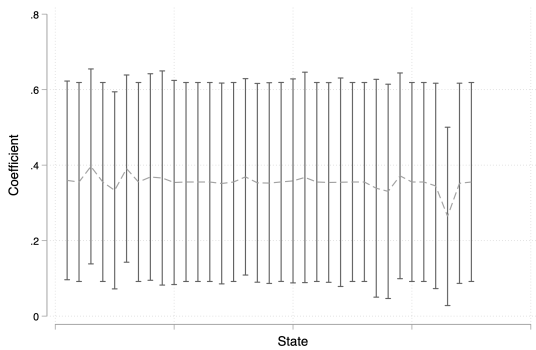
Figure B.7: Event Study Graphs for the Treatment Effects on Wage Bill, Employment and Outstanding Loan



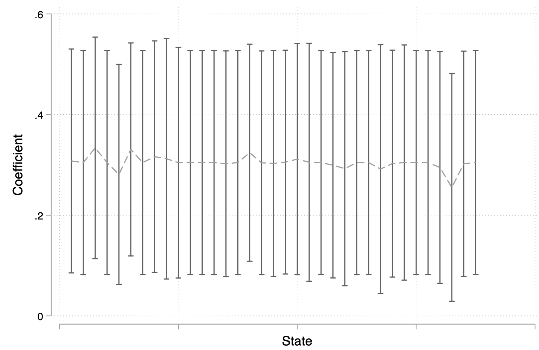
(a) log(Fixed Assets)



(b) log(Sales Revenue)

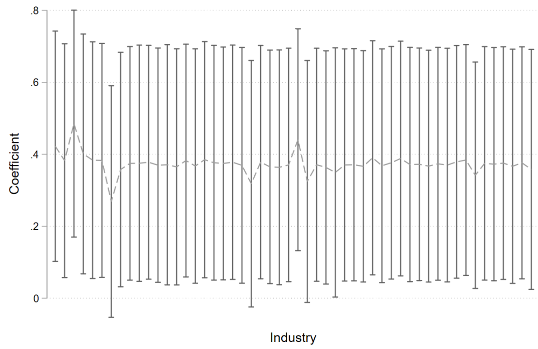


(c) log(Wage Bills)

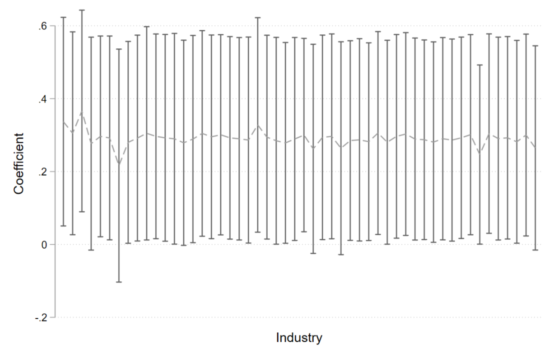


(d) log(Employment)

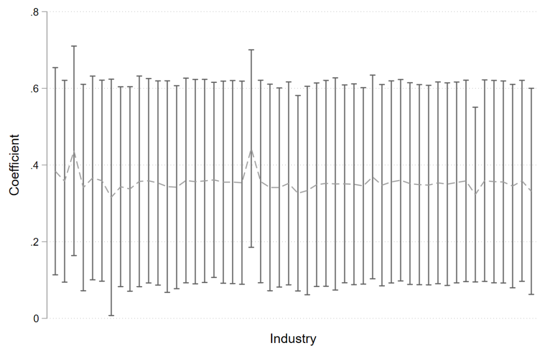
Figure B.8: Robustness: Drop Individual States



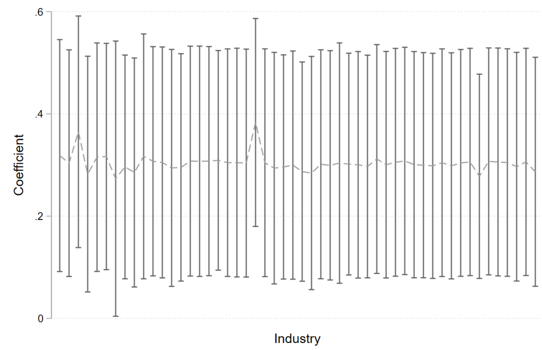
(a) log(Fixed Assets)



(b) log(Sales Revenue)

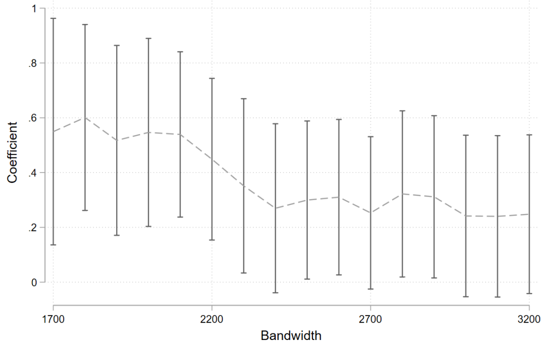


(c) log(Wage Bills)

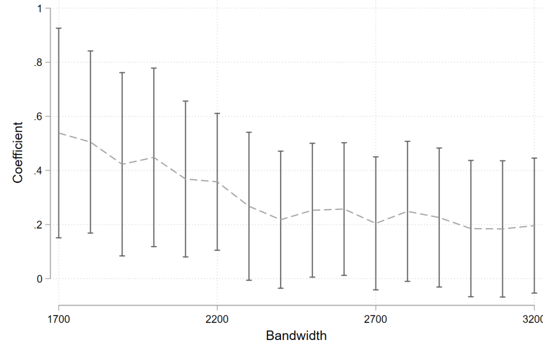


(d) log(Employment)

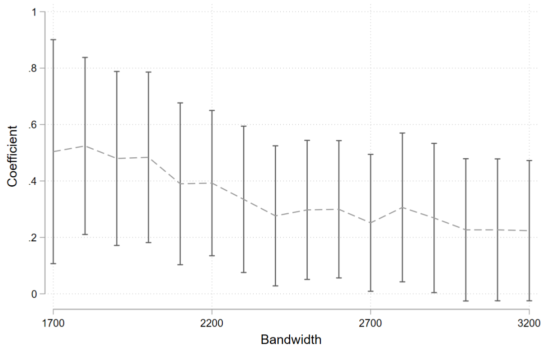
Figure B.9: Robustness: Drop Individual 2-digit Industries



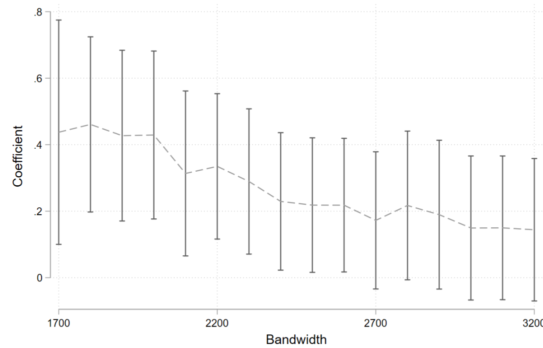
(a) log(Fixed Assets)



(b) log(Sales Revenue)

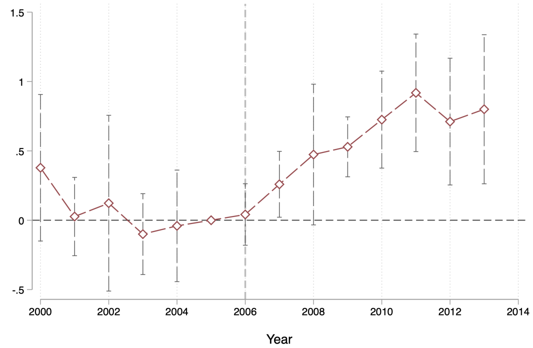


(c) log(Wage Bills)

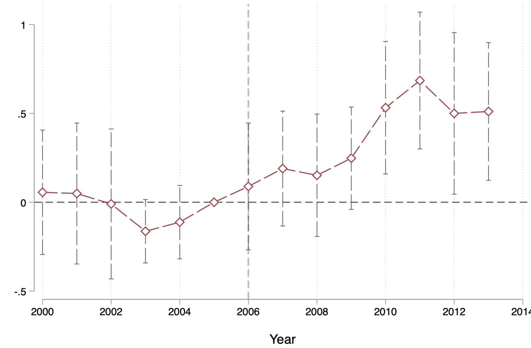


(d) log(Employment)

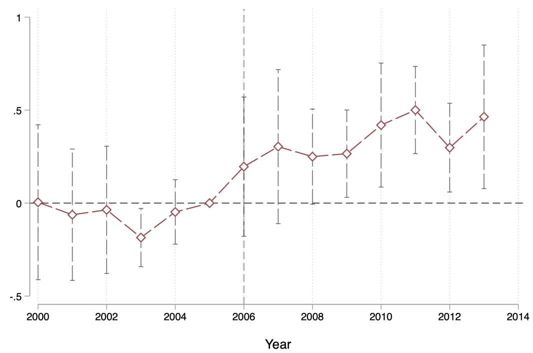
Figure B.10: Robustness: Change Bandwidth



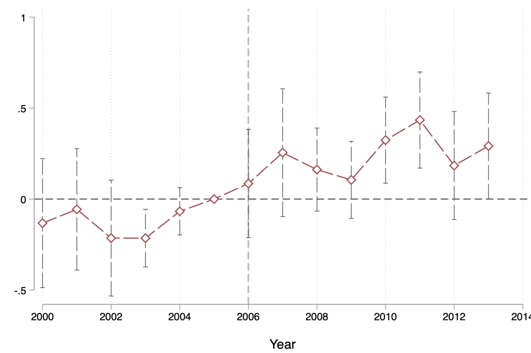
(a) log(Fixed Assets)



(b) log(Sales Revenue)



(c) log(Wage Bills)



(d) log(Employment)

Figure B.11: District Aggregate Outcomes: Event Study Plots

Table B.1: Treatment Effect of Bank Expansion on Firms: Parsimonious Specification

Dependent Variable	(1) Revenues	(2) Capital	(3) Wages	(4) Employment
Treated * Post	0.322**	0.407**	0.395**	0.334**
	(0.159)	(0.180)	(0.157)	(0.130)
Observations	135,797	135,797	135,797	135,797
R-squared	0.146	0.174	0.161	0.110
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: All outcome variables are in logs. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.2: Treatment Effect of Bank Expansion on Firms: High-Dimensional Fixed Effects

Dependent Variable	(1) Revenues	(2) Capital	(3) Wage Bills	(4) Employment
Treated * Post	0.270***	0.300***	0.317***	0.253***
	(0.094)	(0.099)	(0.096)	(0.081)
Observations	134,929	134,929	134,929	134,929
R-squared	0.262	0.283	0.262	0.227
District FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
State*Year FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes

Notes: All outcome variables are in logs. State*Year FE are Indian states interacted with year fixed effects. Industry*Year FE are 2-digit industry interacted with year fixed effects. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: Treatment Effect of Bank Expansion on Capital

	(1)	(2)	(3)	(4)
Dependent Variable	Land	Building	Plant/Machine	Computer
Treated * Post	0.285**	0.481***	0.648**	0.412
	(0.143)	(0.173)	(0.309)	(0.254)
Observations	100,387	121,476	133,200	88,237
R-squared	0.173	0.192	0.198	0.120
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
District Trends	Yes	Yes	Yes	Yes

Notes: All outcome variables are in logs. All capital variables are deflated to constant 2004-2005 Rupee using the Gross Capital Formation data from the RBI. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: Treatment Effect of Bank Expansion on Outstanding Loan

Dependent Variable	Outstanding Loan			**1**(Loan > 0)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	0.415**	0.465**	0.244**	0.016	0.012	0.003
	(0.171)	(0.184)	(0.119)	(0.022)	(0.021)	(0.020)
Observations	109,832	109,914	109,241	135,673	135,797	134,929
R-squared	0.129	0.093	0.195	0.068	0.064	0.105
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	No	No	Yes	No	No	Yes
State*Year FE	No	No	Yes	No	No	Yes
Firm Controls	Yes	No	Yes	Yes	No	Yes
District Trends	Yes	No	Yes	Yes	No	Yes

Notes: Outstanding Loan variables are in logs. **1**(Loan > 0) is a dummy variable equal to 1 if the firm reports a positive outstanding loan in a year. State*Year FE are Indian states interacted with year fixed effects. Industry*Year FE are 2-digit industry interacted with year fixed effects. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.5: Treatment Effect of Bank Expansion on Firm Productivity

	TFP					
	OLS			LP		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	0.023	0.024	0.009	0.035	0.037	0.022
	(0.022)	(0.022)	(0.016)	(0.026)	(0.026)	(0.017)
Observations	135,509	135,633	134,770	135,509	135,633	134,770
R-squared	0.029	0.027	0.105	0.037	0.034	0.098
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
Industry*Year FE	No	No	Yes	No	No	Yes
State*Year FE	No	No	Yes	No	No	Yes
Firm Controls	Yes	No	Yes	Yes	No	Yes
District Trends	Yes	No	Yes	Yes	No	Yes

Notes: All outcome variables are in logs. TFP is measured by the OLS residuals (columns 1-3) and estimating revenue production functions (columns 4-6) following Levinsohn and Petrin (2003). State*Year FE are Indian states interacted with year fixed effects. Industry*Year FE are 2-digit industry interacted with year fixed effects. Firm Controls include firm ownership fixed effects and a dummy variable of being in urban areas. District Trends include district population in 2001 and the number of bank branches in 1997, interacted with a linear time trend. Standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.6: Treatment Effect of Bank Expansion on Farmers by Crop Suitability Quartiles

	Panel A: Crop Suitability, High Input Use			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Conventional	-0.377***	-0.331**	-0.240*	-0.151
	(0.135)	(0.154)	(0.124)	(0.135)
Bias-corrected	-0.428***	-0.387**	-0.282**	-0.185
	(0.135)	(0.154)	(0.124)	(0.135)
Robust	-0.428***	-0.387**	-0.282**	-0.185
	(0.155)	(0.172)	(0.143)	(0.151)
Observations	179,465	117,043	107,733	74,178
State FE	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes

	Panel B: Crop Suitability, Low Input Use			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Conventional	-0.392***	-0.234	-0.239	-0.095
	(0.131)	(0.153)	(0.154)	(0.141)
Bias-corrected	-0.444***	-0.282*	-0.277*	-0.135
	(0.131)	(0.153)	(0.154)	(0.141)
Robust	-0.444***	-0.282	-0.277	-0.135
	(0.152)	(0.174)	(0.172)	(0.160)
Observations	162,389	119,715	114,646	82,845
State FE	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. Standard errors clustered at the district level in parentheses. Crop suitability refers to cereal crop potential production measure (low/high input usage, log) from the FAO Global Agro-Ecological Zones (GAEZ) at the village level. Villages are then sorted into quartiles based on their crop suitability within a sub-district.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.7: Treatment Effect of Bank Expansion on Employment by Road

Panel A: With Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.286**	-0.121	0.147
	(0.141)	(0.151)	(0.127)
Bias-corrected	-0.331**	-0.151	0.180
	(0.141)	(0.151)	(0.127)
Robust	-0.331**	-0.151	0.180
	(0.163)	(0.175)	(0.155)
Observations	314,962	286,293	313,112
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Panel B: Without Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.006	0.531	-0.370**
	(0.201)	(0.327)	(0.179)
Bias-corrected	-0.032	0.556*	-0.385**
	(0.201)	(0.327)	(0.179)
Robust	-0.032	0.556	-0.385*
	(0.257)	(0.438)	(0.225)
Observations	165,804	127,494	156,479
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. tandard errors clustered at the district level in parentheses. Road refers to black topped (pucca) roads in the villages.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.8: Treatment Effect of Bank Expansion on Employment by Distance to Population Center

	Panel A: Close to Population Center		
	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.252*	-0.379**	0.133
	(0.149)	(0.189)	(0.128)
Bias-corrected	-0.305**	-0.425**	0.170
	(0.149)	(0.189)	(0.128)
Robust	-0.305*	-0.425*	0.170
	(0.173)	(0.218)	(0.152)
Observations	246,666	221,712	243,872
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes
	Panel B: Far from Population Center		
	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.217	0.109	-0.088
	(0.180)	(0.216)	(0.118)
Bias-corrected	-0.255	0.125	-0.096
	(0.180)	(0.216)	(0.118)
Robust	-0.255	0.125	-0.096
	(0.211)	(0.255)	(0.141)
Observations	234,483	192,392	226,086
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. Standard errors clustered at the district level in parentheses. Distance refers to the minimum distance to the nearest municipality with a population above 10k in 2011. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.9: Treatment Effect of Bank Expansion on Farmers by Crop Suitability Quartiles in 2001

Panel A: Crop Suitability, High Input Use

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
Conventional	-0.057	-0.125	0.009	0.033
	(0.218)	(0.226)	(0.216)	(0.218)
Bias-corrected	-0.047	-0.158	0.030	0.070
	(0.218)	(0.226)	(0.216)	(0.218)
Robust	-0.047	-0.158	0.030	0.070
	(0.260)	(0.263)	(0.251)	(0.254)
Observations	193,400	125,903	115,745	79,052
State FE	Yes	Yes	Yes	Yes

Panel B: Crop Suitability, Low Input Use

	(1) Quartile 1	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4
Conventional	-0.126	0.060	0.013	-0.070
	(0.221)	(0.225)	(0.219)	(0.244)
Bias-corrected	-0.129	0.040	0.023	-0.061
	(0.221)	(0.225)	(0.219)	(0.244)
Robust	-0.129	0.040	0.023	-0.061
	(0.260)	(0.261)	(0.261)	(0.293)
Observations	174,998	128,866	123,187	88,554
State FE	Yes	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. Standard errors clustered at the district level in parentheses. Crop suitability refers to cereal crop potential production measure (low/high input usage, log) from the FAO Global Agro-Ecological Zones (GAEZ) at the village level. Villages are then sorted into quartiles based on their crop suitability within a sub-district.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.10: Treatment Effect of Bank Expansion on Employment by Road in 2001

Panel A: With Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.023	0.221	0.062
	(0.262)	(0.261)	(0.322)
Bias-corrected	-0.046	0.275	0.076
	(0.262)	(0.261)	(0.322)
Robust	-0.046	0.275	0.076
	(0.297)	(0.313)	(0.374)
Observations	282,238	260,027	278,966
State FE	Yes	Yes	Yes

Panel B: No Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.162	0.124	-0.362
	(0.244)	(0.196)	(0.235)
Bias-corrected	-0.184	0.139	-0.410*
	(0.244)	(0.196)	(0.235)
Robust	-0.184	0.139	-0.410
	(0.274)	(0.237)	(0.262)
Observations	230,867	192,298	213,160
State FE	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. Standard errors clustered at the district level in parentheses. Road refers to black topped (pucca) roads in the villages.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.11: Treatment Effect of Bank Expansion on Employment by Distance to Population Center in 2001

Panel A: Close to Population Center

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-0.024	0.157	0.072
	(0.295)	(0.249)	(0.336)
Bias-corrected	-0.032	0.250	0.137
	(0.295)	(0.249)	(0.336)
Robust	-0.032	0.250	0.137
	(0.342)	(0.293)	(0.389)
Observations	260,447	239,604	254,787
State FE	Yes	Yes	Yes

Panel B: Far from Population Center

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	0.025	0.371*	0.059
	(0.197)	(0.217)	(0.244)
Bias-corrected	0.035	0.414*	0.070
	(0.197)	(0.217)	(0.244)
Robust	0.035	0.414	0.070
	(0.228)	(0.255)	(0.291)
Observations	257,148	215,378	241,956
State FE	Yes	Yes	Yes

Notes: The dependent variables are expressed in logarithmic form. Standard errors clustered at the district level in parentheses. Distance refers to the minimum distance to the nearest municipality with a population above 10k in 2001. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.12: Treatment Effect of Bank Expansion on Num. of Farmers by Crop Suitability Quartiles

	Panel A: Crop Suitability, High Input Use			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Conventional	-37.730*	-48.448*	-15.395	0.134
	(22.141)	(25.772)	(25.825)	(25.600)
Bias-corrected	-42.863*	-57.594**	-20.345	-0.344
	(22.141)	(25.772)	(25.825)	(25.600)
Robust	-42.863*	-57.594**	-20.345	-0.344
	(24.392)	(28.747)	(27.959)	(29.500)
Observations	187,549	121,840	111,851	76,669
Mean of dependent variable	150.1	155.6	161.8	176.5
State FE	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes

	Panel B: Crop Suitability, Low Input Use			
	(1)	(2)	(3)	(4)
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Conventional	-47.861*	-23.161	-19.212	7.961
	(25.690)	(25.789)	(23.696)	(27.756)
Bias-corrected	-54.596**	-30.087	-22.573	7.782
	(25.690)	(25.789)	(23.696)	(27.756)
Robust	-54.596*	-30.087	-22.573	7.782
	(28.503)	(28.105)	(26.952)	(32.252)
Observations	169,471	124,408	119,285	85,948
Mean of dependent variable	157.9	154.3	156.3	165.8
State FE	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the district level in parentheses. Crop suitability refers to cereal crop potential production measure (low/high input usage, log) from the FAO Global Agro-Ecological Zones (GAEZ) at the village level. Villages are then sorted into quartiles based on their crop suitability within a sub-district.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.13: Treatment Effect of Bank Expansion on Num. of Employment by Road

Panel A: With Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	-31.440	-60.968	30.564***
	(28.910)	(39.333)	(10.242)
Bias-corrected	-40.391	-62.958	35.753***
	(28.910)	(39.333)	(10.242)
Robust	-40.391	-62.958	35.753***
	(32.044)	(49.980)	(10.240)
Observations	325,515	301,575	317,864
Mean of dependent variable	187.4	170.6	122.3
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Panel B: Without Road

	(1) Cultivators	(2) Agri. Laborers	(3) Other Workers
Conventional	11.176	15.579	18.338
	(35.284)	(30.371)	(11.886)
Bias-corrected	15.535	13.958	17.392
	(35.284)	(30.371)	(11.886)
Robust	15.535	13.958	17.392
	(43.961)	(35.546)	(15.579)
Observations	174,778	142,253	162,925
Mean of dependent variable	104.8	69.01	50.32
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Notes: Standard errors clustered at the district level in parentheses. Road refers to black topped (pucca) roads in the villages.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.14: Treatment Effect of Bank Expansion on Num. of Employment by Distance to Population Center

	Panel A: Close to Population Center		
	(1)	(2)	(3)
	Cultivators	Agri. Laborers	Other Workers
Conventional	-39.229	-56.577	39.262**
	(28.147)	(42.011)	(15.702)
Bias-corrected	-48.330*	-55.803	45.032***
	(28.147)	(42.011)	(15.702)
Robust	-48.330	-55.803	45.032***
	(30.939)	(54.827)	(15.347)
Observations	253,282	233,245	247,606
Mean of dependent variable	165.4	160.8	127.8
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

	Panel B: Far from Population Center		
	(1)	(2)	(3)
	Cultivators	Agri. Laborers	Other Workers
Conventional	-1.057	-0.623	16.472*
	(21.191)	(27.381)	(9.845)
Bias-corrected	-3.183	-6.640	17.857*
	(21.191)	(27.381)	(9.845)
Robust	-3.183	-6.640	17.857
	(25.184)	(32.173)	(11.325)
Observations	247,420	210,931	233,565
Mean of dependent variable	150.4	107.8	65.51
State FE	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes

Notes: Standard errors clustered at the district level in parentheses. Distance refers to the minimum distance to the nearest municipality with a population above 10k in 2011. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$