Banks, Misallocation and Structural Transformation: Evidence from India

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Abstract

This paper examines the role of banks in misallocation and structural transformation in India, using a banking sector policy experiment in India. Utilizing firm-level data, we first document substantial growth of manufacturing firms in districts with greater access to banks, indicating an easing of financial constraints and stimulation of capital accumulation. We then delve into the impact on labor market dynamics, revealing a significant labor reallocation from agricultural to manufacturing sectors, particularly in areas with lower agricultural productivity. These empirical findings suggest a crucial role of financial development in promoting firm growth and facilitating structural transformation. Building on these insights, our future research will develop a spatial general equilibrium model to quantify the aggregate effect of bank expansion and explore potential misallocations of capital, labor, and talent, as well as the welfare implications of improved access to finance.

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1 Introduction

" *[B]anks are considered to be very high cost and inefficiently run...[E]nabling [Indian banks] to allocate credit to the most productive users, rather than by government allocation, would make a considerable contribution to the Indian economy's growth potential...*"[1]

According to the World Bank, the GDP per capita of the United States (US) was 30 times that of India in 2021. A leading explanation for such cross-countries economic disparities is that misallocation of resources across competing firms is detrimental to aggregate productivity (Hsieh and Klenow, 2009). However, identifying policies that can reduce misallocations and quantifying their aggregate effects proves challenging.[2] Even if one could fully overcome the mismeasurement issues and quantify the effects of changing misallocation on aggregate productivity correctly, the method typically does not identify which specific sources that causally affect the misallocation from the aggregate comparisons.[3]

In this paper, we answer these questions using a nationwide policy experiment in the banking sector in India, one of the world's fastest-growing economies yet a country marked by considerable economic and financial disparities. Domestic financial intermediaries play a pivotal role in economic growth and development, particularly in emerging markets where access to finance remains a significant bottleneck for firm growth and productivity improvement (Levine, 2005). also conjectures that such a relationship comes primarily through how financial system development affects the allocation of resources in an economy.

The financial systems, especially the banking sectors, in developing countries, are considered shallow and less developed, characterized by an overall low level of credit supply and limited credit not allocated to the most credit-needy and, more importantly, credit-worthy firms (Beck, 2013). For example, the banking system in India is considered rather inefficient (Krueger et al., 2002) as state-owned and state-affiliated banks in India dominate the economy, and they often lack the incentives and resources to allocate credit to productive borrowers. Even worse, they are more likely to be captured by the political elites. The

[1] Anne Krueger, who was Deputy Managing Director of the IMF during the 2000s, wrote in *Economic Policy Reforms and the Indian Economy* (Krueger et al., 2002)

[2] In the literature, the most common method is to compare the outcomes (usually the marginal revenue products across units of production) controlling for the observable characteristics and attribute the residual dispersion to misallocation. However, it remains a challenge to distinguish misallocation from other productivity dispersion sources, such as measurement errors (Bils et al., 2021; Gollin and Udry, 2021), model misspecifications (Haltiwanger et al., 2018), and unobserved technology shocks (Gollin and Udry, 2021; David and Venkateswaran, 2019). All of those are common to create upside biases in measures of misallocation.

[3] Several recent papers have taken up this issue and made progress in disentangling the sources of capital misallocation: for example, David and Venkateswaran (2019) and Bau and Matray (2020) highlight that information friction, uncertainty, and domestic capital protectionism are the potential factors contributing to capital misallocation.

misallocation of credit could be a critical impediment to the development of manufacturing firms and the transition of resources from agricultural to manufacturing sectors.

In this paper, we seek to examine the role of banks in resource misallocation and structural transformation in India. Specifically, we take advantage of a policy initiated by the Reserve Bank of India in 2005 to incentivize commercial banks to open new branches in under-banked districts with a population-to-branch ratio that exceeds the national average (i.e., policy "cutoff"). This policy creates a unique quasi-experimental environment, enabling us to causally study our question by implementing a regression discontinuity design (RDD). Specifically, we estimate the effects of this banking system reform on the misallocation of capital across firms by comparing firms in districts just above and below the criterion of being classified as under-banked.

Firstly, our research draws on firm-level data to document the significant growth of manufacturing firms in districts that benefited from greater bank expansion. We observe that the reform facilitates firms' access to credit and stimulates their capital accumulation and growth. In addition to firm-level impacts, we further delve into the consequences on the labor market. We note that bank expansion induces substantial labor reallocation to the manufacturing sectors, particularly in places with lower agricultural productivity.

Despite these important findings, we recognize that the net effect of bank expansion on the allocation of labor and capital in the economy remains an empirical question. Therefore, in our future research, we aim to build a spatial general equilibrium model to quantify the aggregate effect of bank expansion. This will allow us to investigate the potential misallocation of capital, labor, and talent as a result of bank expansion. Further, by conducting counterfactual analyses, we intend to explore the welfare gains that could arise from improved access to finance under lower migration and trade costs.

Our study thus seeks to contribute to the ongoing discourse on the macroeconomic effects of financial development, providing valuable insights into policy discussions on banking sector reforms and economic development in emerging economies like India. As we embark on the next steps of this study, we look forward to presenting our findings at the upcoming conference in December, and believe our complete research will offer significant contributions to the body of knowledge in this field.

This paper connects most closely to two main streams of literature. First, our paper contributes to the literature on 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 closely related paper, Bau and Matray (2020) shows that FDI liberalization in India can reduce misallocation across manufacturing firms. We contribute to this literature by casually demonstrating that reform in the private banking sector in India could reduce misallocation. Furthermore, the policy we examine goes beyond the level of specific industries, enabling us to examine its consequences in the local market. We can, for instance, answer the question of to what extent misallocation inhibits structural transformation. To do so, we leverage a rare natural experiment in India's banking system with geo-coded firm-product level data, as well as SHRUG, population and economic census data.

We also add to a growing body of literature studying the aggregate effects of quasi-experimental credit shocks in developing countries (Bazzi et al., 2021; Fonseca and Matray, 2022; Breza and Kinnan, 2021; Banerjee and Duflo, 2014; Bruhn and Love, 2014; Fonseca and Matray, 2022; Burgess and Pande, 2005; Fonseca and Matray, 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, products, and labor markets, we are able to directly study the general equilibrium impacts of the bank expansion policy and provide reasonable policy implications for other developing countries.

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 discusses our results on industrial growth. 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 use in this study was introduced in 2005 by the Reserve Bank of India to incentive banks to open more branches in under-served locations.[4] As stipulated, banks must propose a set of branch openings, closings and shifts in an annual branch expansion plan to the RBI. Banks can thus increase their chance of obtaining licenses in favored locations

[4]The banking sector in India does not permit free entry of banks or branches of banks. Banks must apply for and acquire licenses from the Reserve Bank of India (RBI) before opening any new branch. Besides, banks also need to request approval for closing or shifting branches in most markets.

by proposing more branches in a set of districts that the RBI designated as "under-banked." [5] [6]

The definition of an "under-banked" district is crucial for our identification strategy. The rule adopted by the RBI was to compare a district's average number of people per branch (i.e., population-to-branch ratio) with the national average population per branch for India[7] as presented below:

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

In September 2005, RBI published the list of "under-banked" districts following the above rule. Minor revisions to the 2005 list were introduced in 2006; the list was then reissued unchanged from 2007 to 2010.[8] It is important to note that the RBI has not adjusted the list to account for *changes in the ratio* despite more bank branches entered in under-banked districts. The list of "under-banked" districts therefore remained nearly constant through our post-policy sample period. Using official RBI document in 2006, we define 375 out of 593 districts as "under-banked" districts that 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 variations in the presence of banks, and will further explain our estimating equations along with corresponding results in Section 4 and Section

[5] The RBI also evaluated banks and made their license-issuance decision on other facts, including the bank's provision of "no-frills" accounts, meeting priority sector lending obligations, and outcomes of handling complaints. However, these requirements apply at the bank-level, instead of individual bank branches. For more details of the reform, see the 2005 issue of *Master Circular on Branch Authorisation* of the RBI.

[6] 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.

[7] According to RBI's *Report of the group to review branch authorization policy* in 2009, the "national average" is a statistic provided directly by RBI. We do not know exactly how RBI calculated such statistics. Hence, We re-calculate this ratio according to its definition, and we verify that our calculation is accurate in a later section.

[8] 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.

5 to avoid confusion.

To validate this design, we first show that there is no evidence of manipulation around the cutoff--so districts do not select into treatment or control groups. The left panel of Figure A.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. We formally test it using the McCrary (2008) density test--the MaCrary estimator is -0.065 and the p-value is 0.95. Hence, we cannot reject smoothness around the cutoff.[9]

The right panel of Figure A.1 confirms that the cutoff is indeed meaningful--there is a jump in under-banked status below and above the cutoff. The compliance to the assignment rule is not perfect -- out of 578 districts that we have bank branch data, there are seven districts with a status different than predicted by the population-branch ratio. There are several reasons why the compliance is not perfect. First, the branch data might have measurement errors.[10] Second, the RBI might have used their discretion to edit the list because they decided to "help" specific districts or were captured by the political elites. Fortunately, non-compliance does not pose a threat to our analysis as there are only 1.7% observations in our ASI data that belong to non-complied districts. We drop non-compliers in our main analysis and use a sharp RD design (assuming perfect compliance) and show our results are robust to keeping those 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

[9]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 annual basis detailed proposal and they 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.

[10]There are 593 districts in 2001 in total. For districts that were split from a 2001 district, we re-code them to the original district. Some districts are dropped as they were formed after 2001 by merging several existing districts and thus cannot be mapped to districts. After a careful review of RBI documents and historical texts, we drop another 3 districts suspected of coding errors or manipulation. Ujjain was a historically rich city and not on the original list of under-banked districts in 2005; however, it was added to the list in 2006. We drop Ujjain because we suspect it was added to the list due to political reasons. We drop Badgam district 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. The matter has been reviewed and it has been decided to continue the existing arrangement till 31st March 2007. However, these bank branches are coded as being closed in the RBI bank branch data, which flips the treatment status of Badgam. Finally, Varansi is also dropped due to the 2002 merger of the private sector Banaras State Bank with the nationalized bank, Bank of Baroda. The merger rendered some bank branches in Varansi coded as being closed and change their treatment status.

branch data in 2004, and nighttime light intensity in 2004, Figure A.2a-d present a series of plots showing that those district-level characteristics are continuous around the cutoff. We further test for the smoothness of firm-level outcomes before the policy in 2004 using the Annual Survey of Industries data. Figure A.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. Taken together, these results suggest that districts are properly randomized around the cutoff, lending support to the causal interpretation of our RDD results.

The reform is also highly effective by introducing powerful incentives for banks to open in districts that were considered unprofitable and thus under-banked, by leveraging licenses in high-profit areas.[11] 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 A.3 and A.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 an large increase in deposit and credit after the policy.[12]

3 Data

Our primary source of data is the establishment-level data from the Annual Survey of Industries (ASI) from 1998 to 2013.[13] 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 a cross-sectional version of ASI, which contains district identifiers until 2009, based on time-invariant factors and open/close variables.[14] The ASI includes comprehensive plant-level information on revenues, labor costs,

[11] 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.

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

[13] 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.

[14] 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.

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 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),[15] which integrates data from multiple rounds of population 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 2005. 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 2016, 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.[16]

4 Did Bank Expansion Lead to Industrial Growth?

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

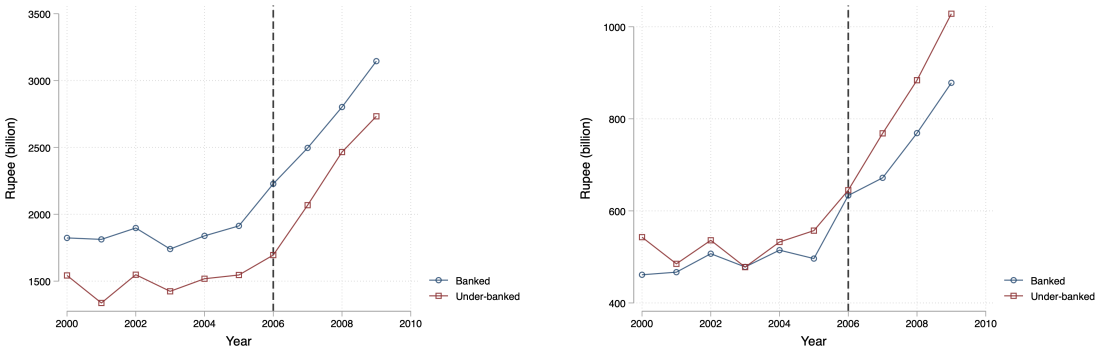
Before detailing our estimation equations and presenting statistical results, we first provide a series of time-series plots using the raw ASI data.

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

[15] Asher and Novosad (2020) provides details of the data construction, accessible at https://www.devdatalab.org/shrug.

[16] It is important to note that the district borders in India are very volatile. There were 593 districts in 2001 and 640 districts in 2011.

capital is constructed as the weighted sum of firm-level fixed assets[17] 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.



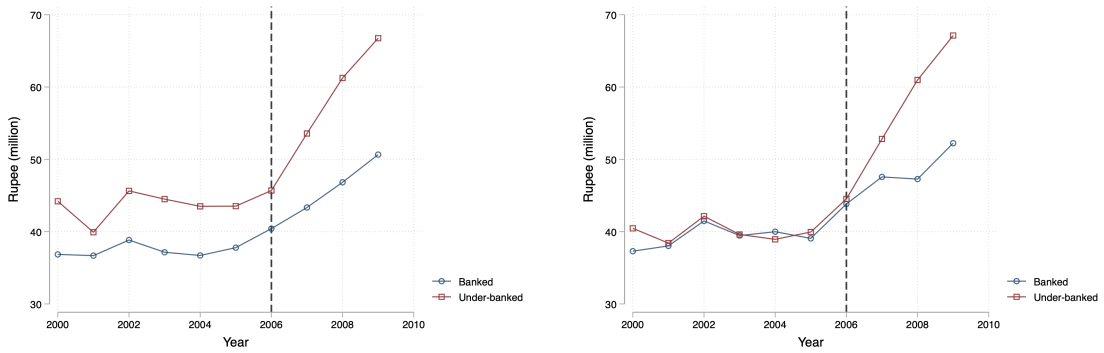
(a) All distircts (b) Districts around the policy cutoff

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

The left panel reveals that under-banked districts had approximately 25% less total capital than banked districts in 2000. This gap remained stable despite the rapid industrial growth observed from 2000 to 2005. Following the policy implementation, under-banked districts began to catch up as new bank branches opened and more credits were 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, which suggests that the assignment of the treatment status 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.

Interestingly, these patterns reverse when considering firms' average capital 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. This indicates that while under-banked districts have less capital in aggregate, the burden is not evenly distributed among all firms, hinting at constraints on the intensive margin. Otherwise, the average firm size would be smaller in under-banked districts. However, if lack of access to banks results in a fixed cost of borrowing--a constraint on the extensive margin--only the most talented and productive entrepreneurs would bear the cost and enter the market. Consequently, firms in under-banked districts would be larger, signaling a misallocation of entrepreneurial talents.

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



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

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

Then the natural question is whether and to what extent the expansion of bank branches, induced by the 2006 policy, can alleviate the intensive and extensive margin credit fictions and reduce the misallocation of talent and capital. Both panels show that firms in under-banked districts grew even larger after the policy. There are two key takeaways from this result. First, firms in under-banked districts are too small, despite already being positively sorted and thus larger than their peers in banked districts. Second, bank expansion relaxes borrowing constraints more on the intensive margin, with incumbent firms, rather than potential entrants, benefiting more and growing larger. If bank expansion worked more on the extensive margin and reduced the fixed cost of borrowing, we would observe many entrepreneurs (including some less talented) entering the market and lowering the average capital of firms in under-banked districts. To quantify the aggregate effects of the bank expansion policy, our model includes both the intensive and the extensive margin of credit frictions, and allows them to depend on the number of bank branches in a district. The above results will help us pin down their elasticities relative to the number of bank branches.

4.2 Estimating equation: Difference-in-Discontinuity

To capture the dynamic effects on firms as depicted in Figure 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 variations in the timing of the policy.

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

following equation:

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

where y_{di} includes outcome variables of firm i in district d. The first three terms on the right hand side comprise a standard RD design with the running variable $Ratio_{id}$--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 outcome variable y_{idt}. Then the three terms in the second row interact with a post-policy dummy variable to allow the discontinuity between treated and control districts to vary due to the policy. δ_1 thus captures the treatment effect of the bank expansion policy on firms. 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 effects, $\beta_1 - \beta_3$, are absorbed by the district fixed effects. Standard errors are clustered at the district level. h is the MSE-optimal bandwidth, and only firms within this bandwidth are included in the sample. We show that our results hold robust to different bandwidths.

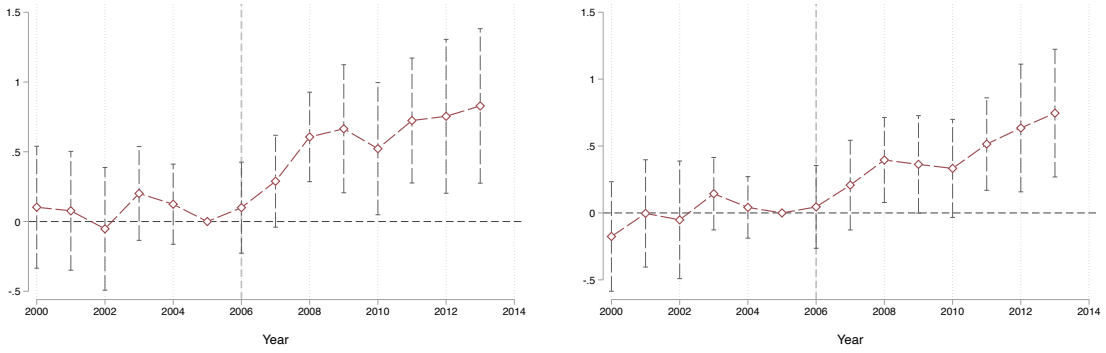
A similar equation is used to estimate the treatment effect on district-level aggregate capital, sales, and manufacturing employment. These values are constructed as the weighted sum of firm-level counterparts, using the sampling weights.

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 its counterpart 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 A.2. Our identification strategy merely requires that any discontinuities around the cutoff, resulting from any cause, remain unchanged before and after the policy - an assertion we will validate in the following event study plot.

4.3 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 3: Event Study Graphs for the Treatment Effects on Capital and Sales

Figure 3 presents the event study graphs for the treatment effects on capital and sales revenue. The graph estimates the yearly effect of belonging to a (near cutoff) treated district pre and post reform, using the same controls and bandwidth as our baseline equation. The absence of an observable effect before the reform implies the absence of a pre-trend, thus validating our identification strategy.[18] Figure A.5 reports similar 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. Although these effects are economically significant, they unfold progressively over time, in line with the idea that changes in the allocation of resources are typically slow-moving. 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

[18] A potential concern could be the migration of plants from control to treated districts post-reform, in anticipation of improved financial access. However, given India's stringent industrial regulation scheme, such movement is implausible. 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 subject to inflation due to possible measurement or coding errors in the district information. In our baseline analysis, we use a plant's mode district as the time-invariant district. Our results remain robust when plants that have relocated are excluded.

that the policy exerts large positive effects on firm investments.[19] 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 also expand their sales revenue by approximately 30%.

Table 1: Treatment Effect of Bank Expansion on Firms

	(1)	(2)	(3)	(4)
Dependent Variable	Revenues	Capital	Wages	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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Robustness. Table A.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 A.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.[20] 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 with the reform, we flexibly control for state-level time-varying unobserved shocks. Table A.2 shows that our results are robust to the inclusion of state-by-year fixed effects. Moreover, Figure A.6 and A.7 demonstrate that our results hold robust to dropping individual states

[19] As shown in A.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.

[20] 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.

and 2-digit industries one by another. Figure A.8 shows that using different bandwidths does not change our estimates qualitatively.

Outstanding loans. One natural question is to what extent the growth of firms we documented is attributable to bank expansion alleviating fiction in the credit market. 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, our data source, the ASI, only provides information on total outstanding loans without specifying their sources. Nevertheless, we present the effect on total outstanding loans in Figure A.5 (panel (c)) and Table A.4 (column 1-3). Following the reform, outstanding loans experienced a 41% increase for the average firm in treated districts.[21] The substantial increase in total outstanding loans suggests that firms do not merely increase their borrowing from banks to replace other higher-interest debts. This trend implies a real expansion in firm-level credit rather than a reorganization of existing liabilities. It is noteworthy that 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.

TFP. We conclude our firm-level analysis by noting that while the reform changed the allocation of resources across districts, it had minimal effect on firm productivity. We measure firm-level productivity (TFPR) by OLS residuals and by estimating revenue production function following Levinsohn and Petrin (2003). Table A.5 shows that the reform has precisely estimated null effects on both measures.

4.4 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

[21] In our sample, more than 85% of firms reported having positive outstanding loans even before the reform was implemented. 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 A.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.

weights. To ensure that we are comparing the same set of treatment and control districts, we use the same bandwidth as in our plant-level analysis with the same estimating equation.[22]

To further validate our empirical strategy at the district level, Figure A.9 presents the event study plots for 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 2: Treatment Effect of Bank Expansion on District Aggregate

	(1)	(2)	(3)	(4)
Dependent Variable	Revenues	Capital	Wages	Employment
Treated * Post	0.390**	0.490***	0.394***	0.347***
	(0.176)	(0.131)	(0.125)	(0.098)
Observations	1,477	1,477	1,477	1,477
R-squared	0.972	0.955	0.983	0.980
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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2 reports our Diff-in-Disc results at the district level. The coefficients suggest that (narrowly) under-banked districts experienced sizeable industrial growth. Following the reform, treated districts generated a 39% increase in manufacturing output, which was made possible by the large increase in fixed assets (49%) and manufacturing labor--a 39% increase in wage bills (column 3) and a 35% increase in employment (column 4). It is important to note that our district-level aggregate effects are generally larger in magnitude than those at the firm level reported in Table 1, suggesting that bank expansion reduces the fixed cost of using capital, thereby allowing less talented entrepreneurs to enter the market.

[22]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 coefficients. In addition, we control for the number of plants in a district-year.

5 Effects on Structural Transformation

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 labor markets, particularly focusing on the extent to which banking sector reforms shifted labor from agricultural to manufacturing sectors. On the one hand, the proliferation of manufacturing firms—-acting as a pull factor—-creates new industrial job opportunities and elevates wages in the manufacturing sector, potentially drawing workers away from agriculture. Conversely, improved access to finance could also stimulate agricultural investments, increasing the marginal productivity of agricultural labor.[23] 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) asserts that the urban wage is more than 47 percent higher than the rural wage for less educated workers engaged in menial tasks; this gap remains constant over time and is significantly larger than in other developing countries, such as China and Indonesia. Despite this, rural workers do not capitalize on these arbitrage opportunities, as evidenced by India's unusually low migration rate.[24] Several elements, such as limited access to roads (Asher and Novosad, 2020) and the informal insurance networks rooted in the traditional caste system (Munshi and Rosenzweig, 2016), may impede labor mobility and lead to the misallocation of labor. It remains an empirical question whether industrial growth can release workers entrapped in agriculture, especially those who exhibit 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 operationalize, we utilize the village-level population census data from 2001 and 2011,[25] paired with crop suitability data from the FAO Global

[23] 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.

[24] 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.

[25] 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.

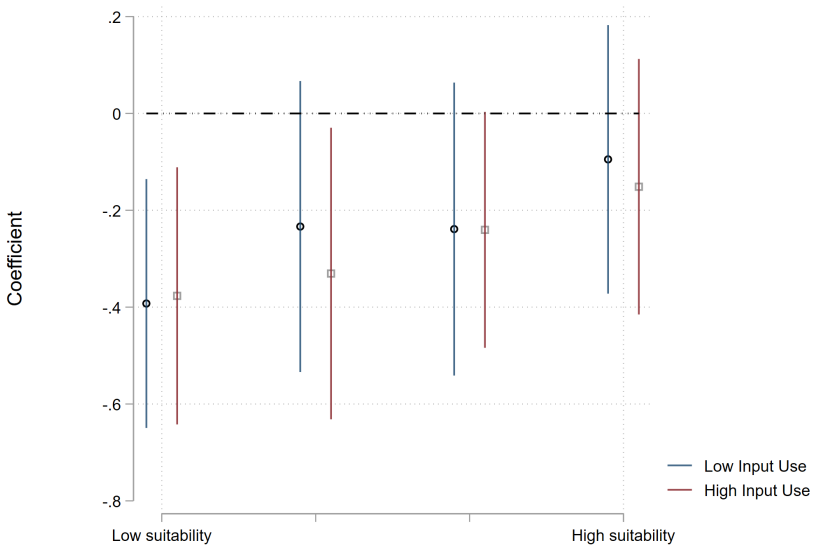


Figure 4: Treatment Effect of Bank Expansion on log(Farmer) by Crop Suitability

Agro-Ecological Zones (GAEZ)[26] 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 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.

[26]The GAEA 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.

Figure 4 illustrates the treatment effect on the logarithm of the 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 at the left end of the plot. As we transition to villages with higher crop suitability, the effects gradually diminish in magnitude and become not statistically distinguishable 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 where agricultural productivity is lower.

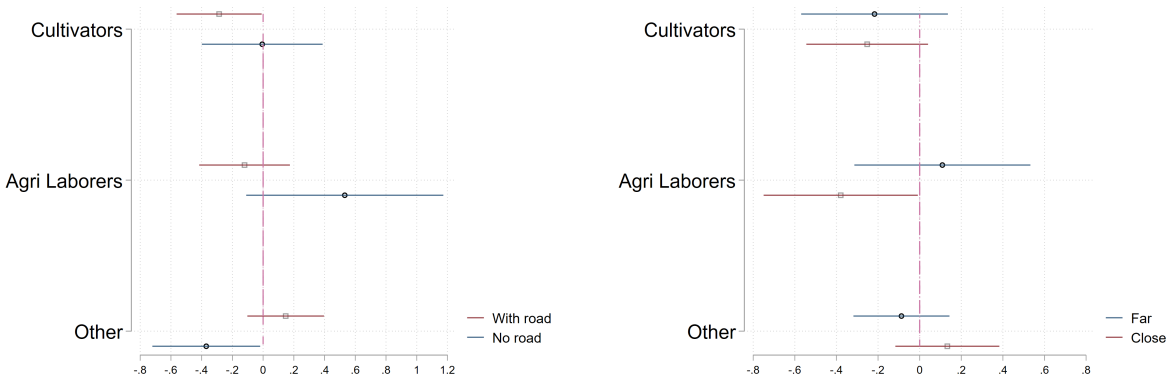
The calculation of crop suitability requires an assumption about input usage.[27] While the farming system in India typically aligns more closely with the low-input case (represented by blue lines), our findings remain robust under the assumptions of high-input usage (indicated by red lines). Here, we document the effects on the logarithm of the number of farmers, controlling for the village population in 2001. Our results are also robust when using the village-level proportion of farmers among all workers as an alternative outcome variable.

Labor demand or labor supply? While the results reported in Figure 4 lend support to our main hypothesis that industrial growth creates more job opportunities in non-agricultural sectors and thus facilitates structural transformation, it is also consistent with an alternative hypothesis that bank expansion increases industrial labor supply. Assuming non-homothetic preference, such as a subsistence consumption requirement in the agricultural good, workers in places with lower agricultural productivity self-select in agriculture because they are poorer (Lagakos and Waugh, 2013). Bank expansion thus has a stronger impact in less productive places by relaxing the subsistence requirement and increasing the industrial labor supply.

To further corroborate our hypothesis that the documented results are driven by an expansion in industrial labor demand, we examine the reform's heterogeneous impacts. Under the labor demand hypothesis, we would anticipate a stronger effect in places that can better leverage the industrial expansion—for instance, villages in close proximity to urban centers (Madhok et al., 2022) or those connected by roads (Asher and Novosad, 2020). Conversely, if the labor supply channel primarily drives our results, we would expect to see a more pronounced response to the reform in more isolated and remote villages, where the subsistence requirement has more bite.

Figure 5 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

[27] 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.



(a) Rural Road (b) Distance to Population Center

Figure 5: Heterogeneity in the Effect of Bank Expansion

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 the 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.

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, our research has demonstrated the significant and multifaceted impacts of bank expansion on both firm growth and structural transformation within the labor market. We have found that bank expansion, driven by the reform in the banking sector in 2006, has

directly facilitated firm growth by alleviating frictions in the credit market. Such changes have precipitated an increase in both the total outstanding loans and fixed assets of firms in treated districts, indicating a tangible improvement in credit access and suggesting a subsequent stimulation of local economic activity.

This expansion, in turn, has had profound implications for the labor market. As bank expansion stimulates industrial growth, it creates more non-agricultural job opportunities. This effect acts as a 'pull' factor, potentially luring labor away from agricultural sectors and towards manufacturing. The reform reduced labor misallocation by releasing workers entrapped in agriculture, especially in places with lower agricultural productivity.

While we have made significant strides in understanding the relationship between bank expansion and economic activity, our work is far from complete. Moving forward, we aim to construct a spatial general equilibrium model to quantify the aggregate welfare effect of bank expansion, as well as its implications for the misallocation of capital, labor, and talent. In doing so, we hope to shed more light on the complex interplay between credit access, labor migration, and economic development. Furthermore, we plan to conduct several counterfactual analyses to explore the welfare gains of financial inclusion that could be achieved if migration and trade costs were lowered. This next phase of our research will provide more comprehensive insights into the far-reaching implications of banking reforms and their role in shaping economic landscapes.

As we submit this draft, we are cognizant that our work is an ongoing endeavor. Currently, we are in the process of developing our spatial general equilibrium model and have yet to complete this part of our research. However, we wish to assure the conference committee that by the time of the event in December, we anticipate having a complete draft that includes our most recent findings and the results derived from the aforementioned model. We are confident that this more comprehensive analysis will provide a richer understanding of the economic effects of bank expansion, as well as a robust platform for discussion and feedback at the conference. We look forward to presenting our full findings and engaging in a meaningful dialogue with fellow researchers and attendees.

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A Appendix

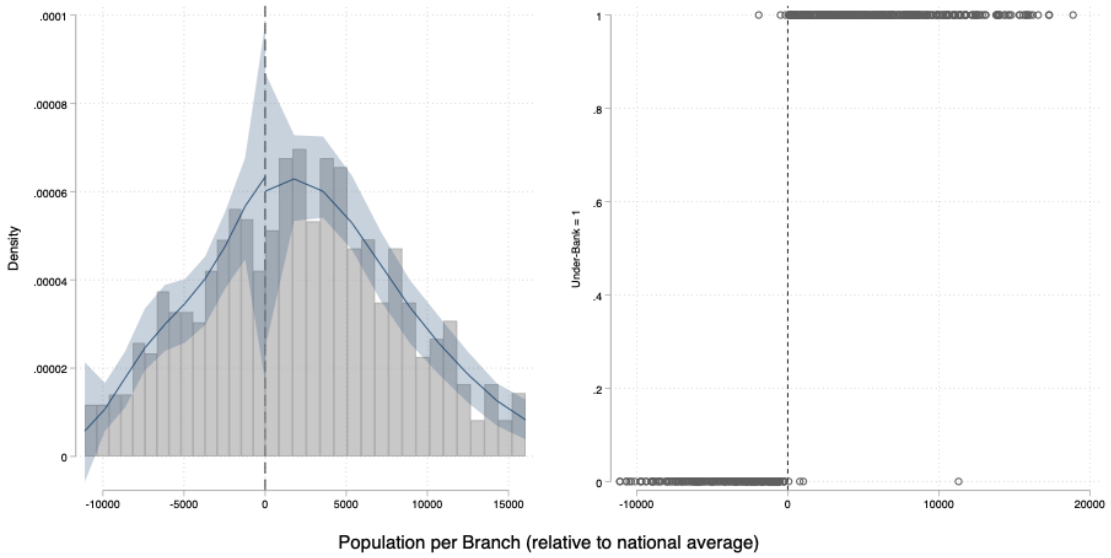
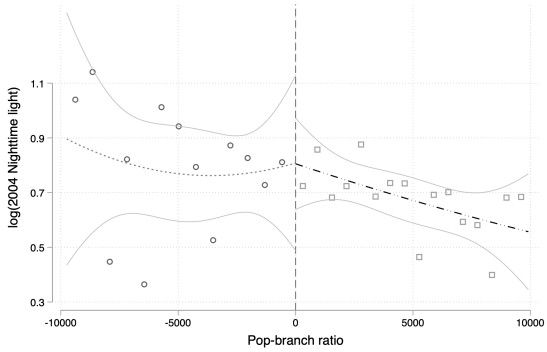
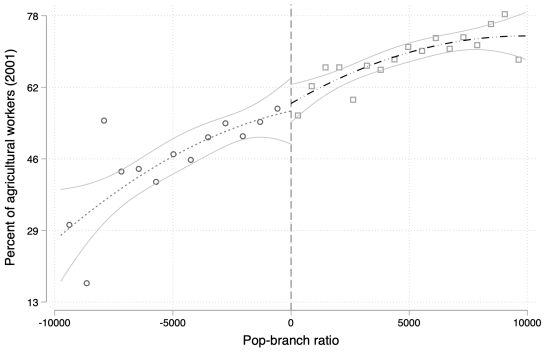


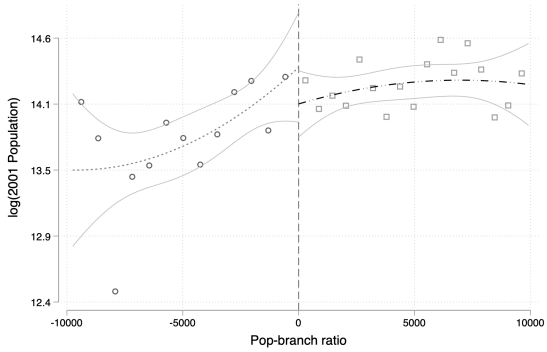
Figure A.1: MaCrary manipulation test and the first stage.



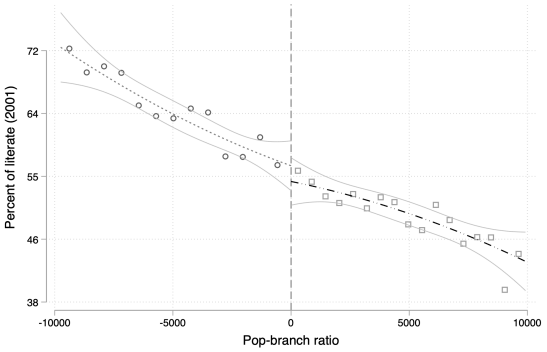
(a) log(Light intensity) in 2004



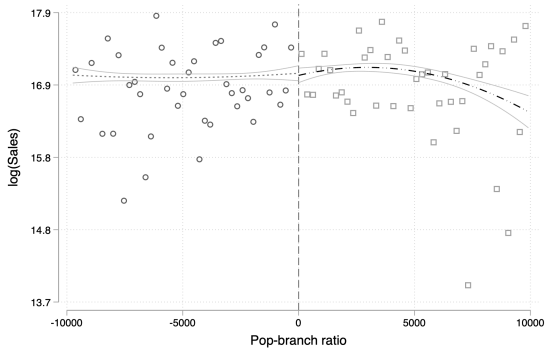
(b) Percent of agricultural workers



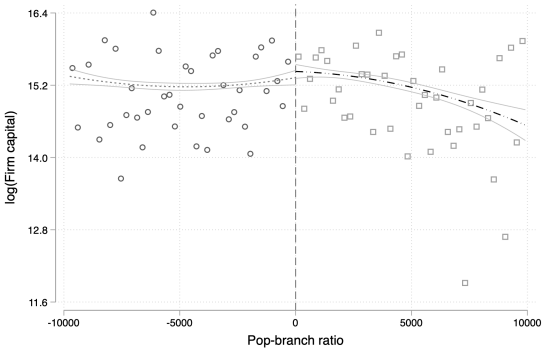
(c) log(Population)



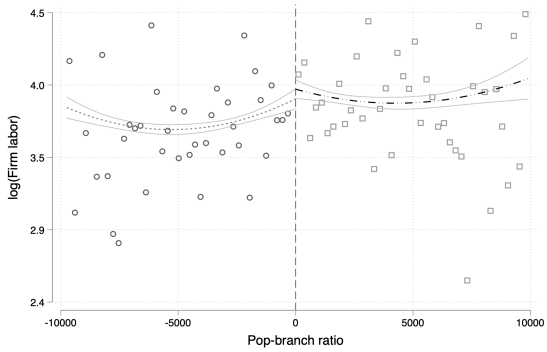
(d) Percent literate



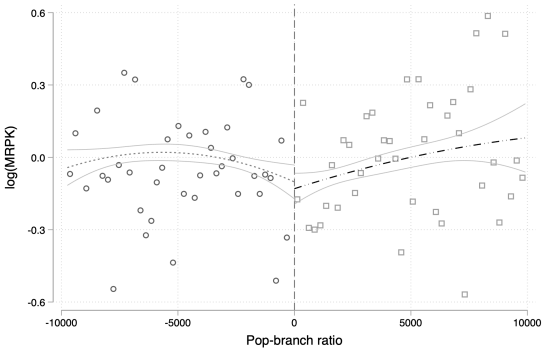
(e) log(Firm sales)



(f) log(Firm fixed asset)

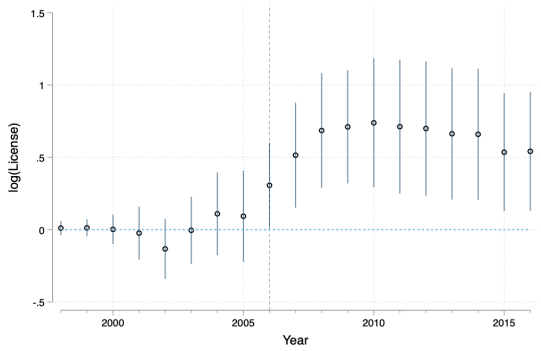


(g) log(Firm employee)

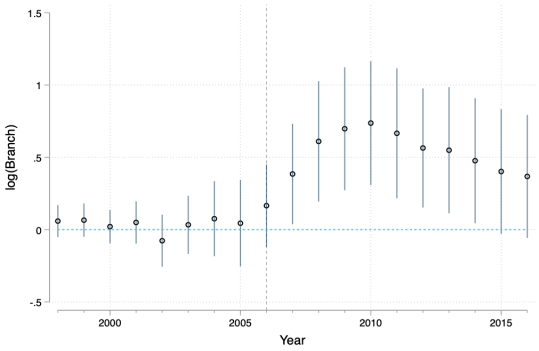


(h) log(Firm MRPK)

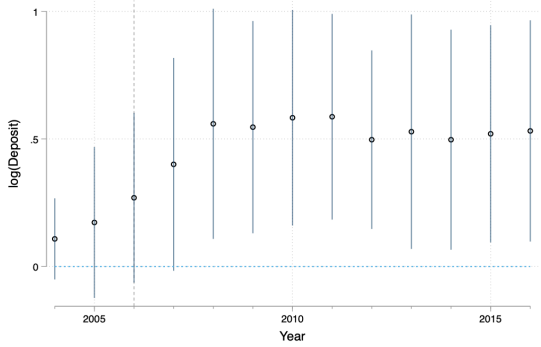
Figure A.2: Smoothness of pre-policy covariates



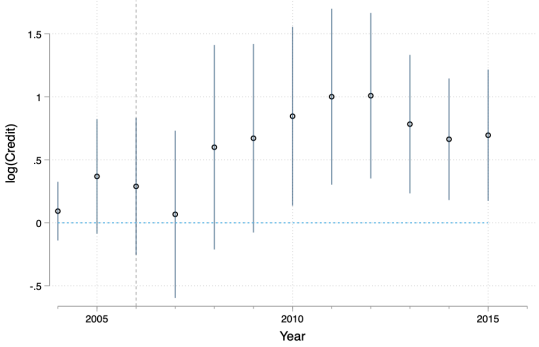
(a) log(Bank licenses)



(b) log(Branches)

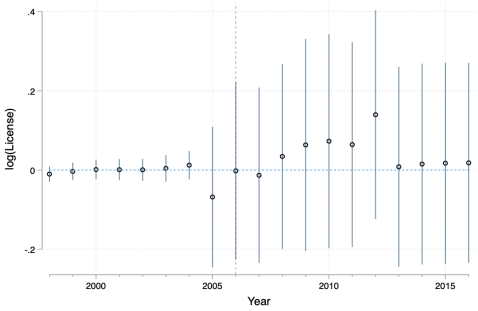


(c) log(Deposit)

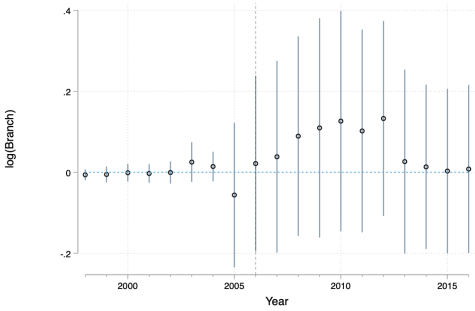


(d) log(Credit)

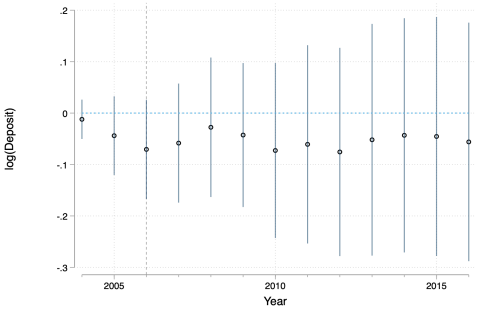
Figure A.3: Efffects on Private Sector Banks



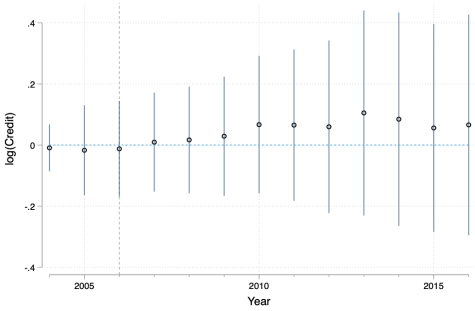
(a) log(Public Bank licenses)



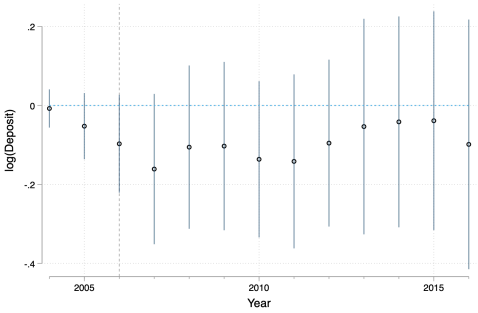
(b) log(Public Bank Branches)



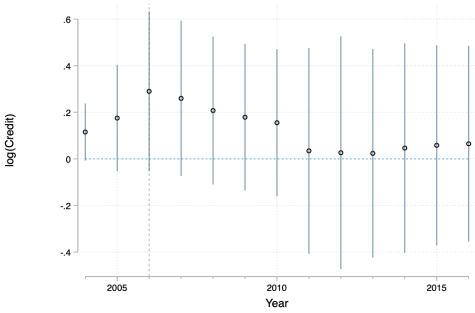
(c) log(Nationalized Bank Deposit)



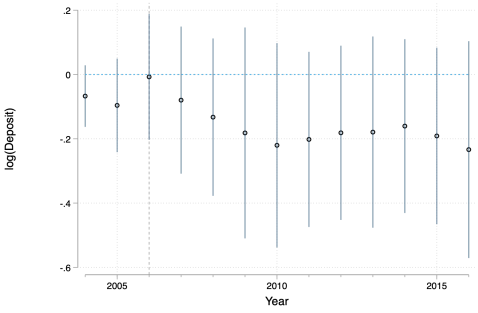
(d) log(Nationalized Bank Credit)



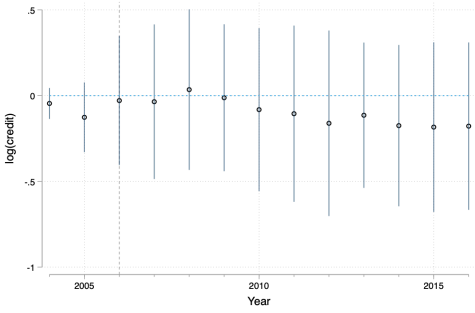
(e) log(SBI Deposit)



(f) log(SBI Credit)

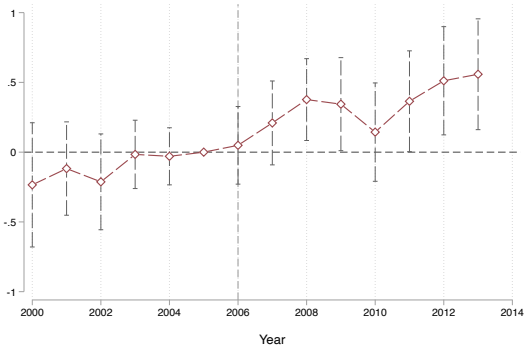


(g) log(RRB Deposit)

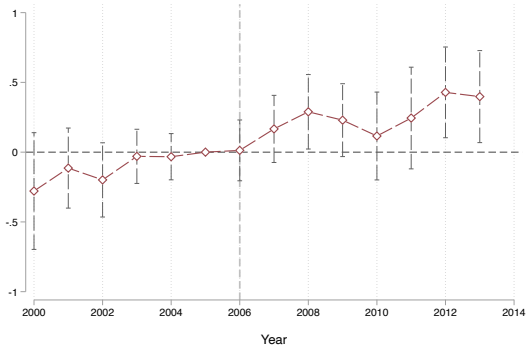


(h) log(RRB Credit)

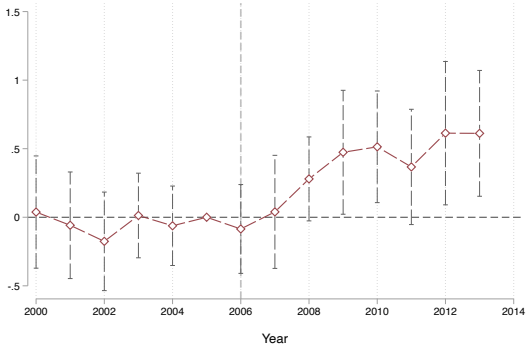
Figure A.4: Effects on Public Sector Banks



(a) log(Wage Bill)

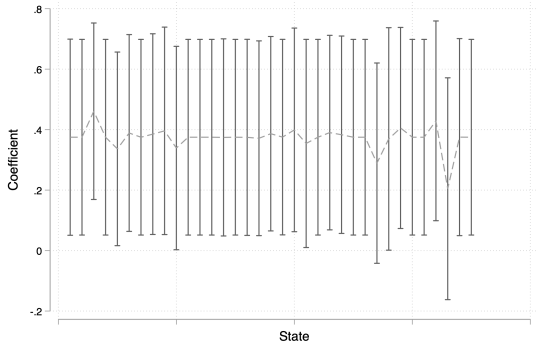


(b) log(Employment)

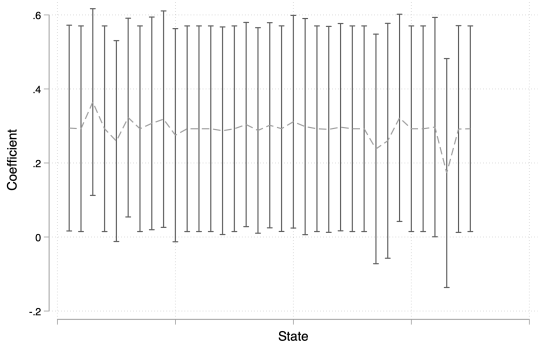


(c) log(Outstanding Loan)

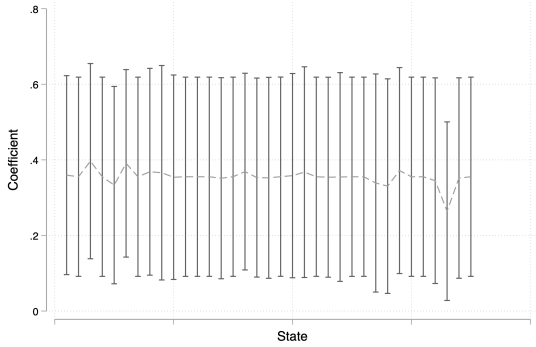
Figure A.5: Event Study Graphs for the Treatment Effects on Wage Bill, Employment and Outstanding Loan



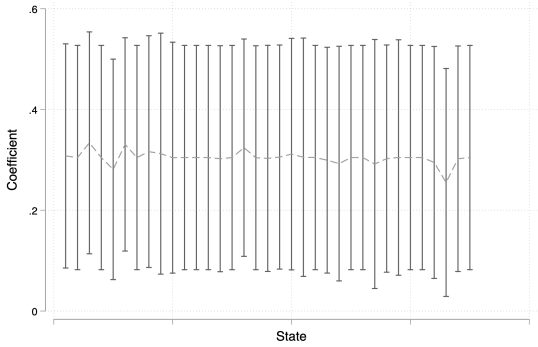
(a) log(Fixed Assets)



(b) log(Sales Revenue)

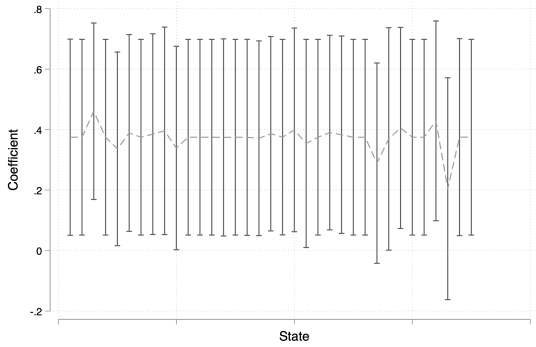


(c) log(Wage Bills)

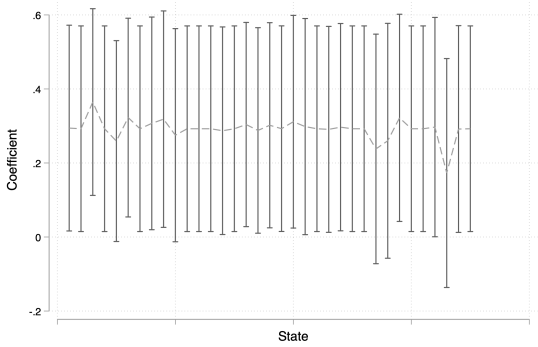


(d) log(Employment)

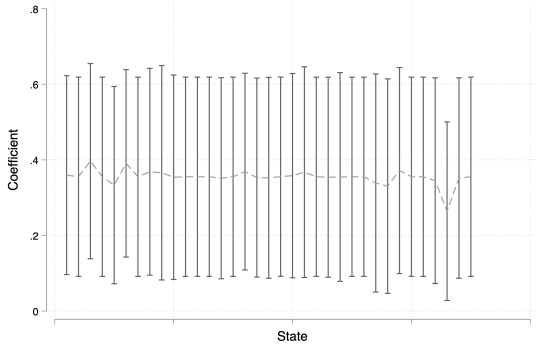
Figure A.6: Robustness: Drop Individual States



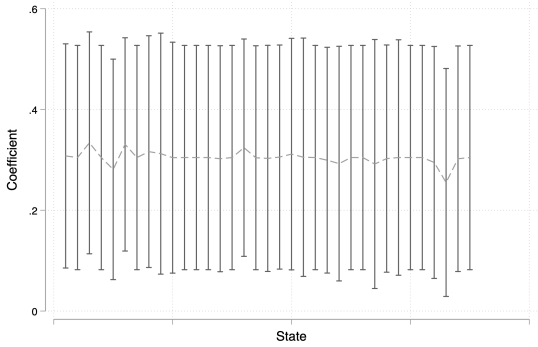
(a) log(Fixed Assets)



(b) log(Sales Revenue)

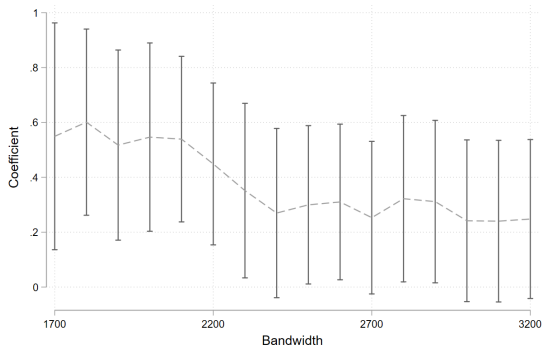


(c) log(Wage Bills)

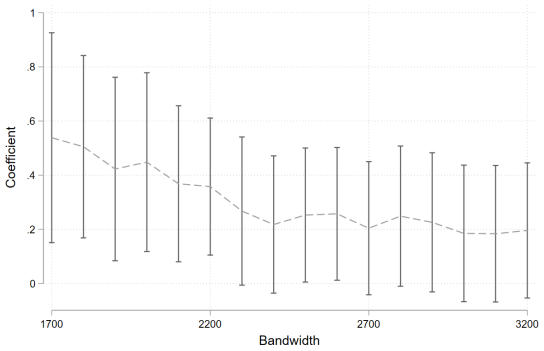


(d) log(Employment)

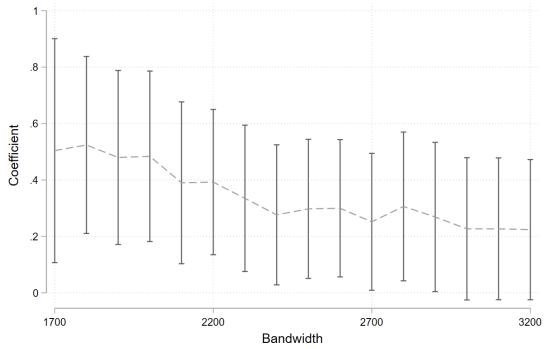
Figure A.7: Robustness: Drop Individual States



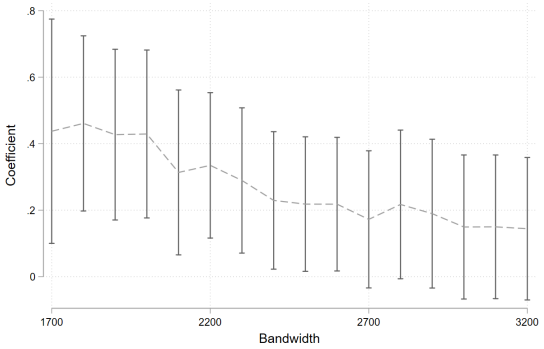
(a) log(Fixed Assets)



(b) log(Sales Revenue)

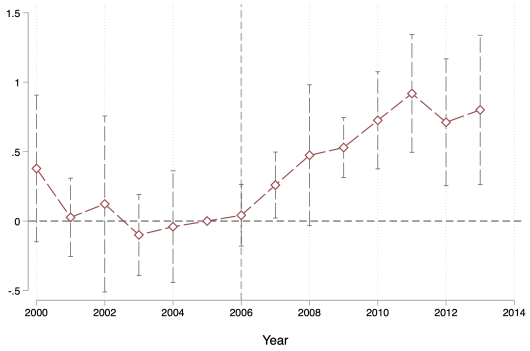


(c) log(Wage Bills)

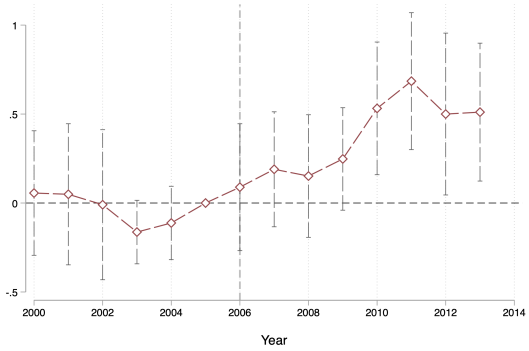


(d) log(Employment)

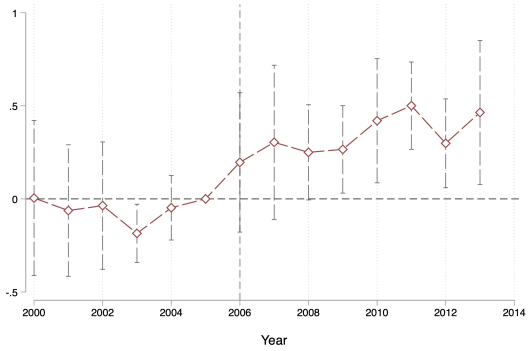
Figure A.8: Robustness: Change Bandwidth



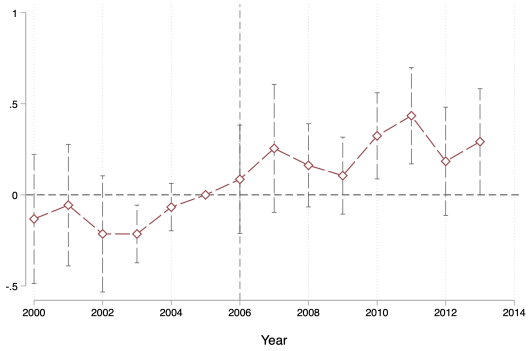
(a) log(Fixed Assets)



(b) log(Sales Revenue)



(c) log(Wage Bills)



(d) log(Employment)

Figure A.9: District Aggregate Outcomes: Event Study Plots

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

	(1)	(2)	(3)	(4)
Dependent Variable	Revenues	Capital	Wages	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 A.2: Treatment Effect of Bank Expansion on Firms: High-Dimensional Fixed Effects

	(1)	(2)	(3)	(4)
Dependent Variable	Revenues	Capital	Wages	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			
Districts Trend		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 A.3: Treatment Effect of Bank Expansion on Capital

Dependent Variable	(1) Land	(2) Building	(3) Plant/Machine	(4) 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 A.4: Treatment Effect of Bank Expansion on Outstanding Loan

Dependent Variable	Outstanding Loan			$\mathbf{1}$(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. $\mathbf{1}$(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 A.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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$