Who Benefits from Industrial Policy? Evidence from Brazil's Development Bank

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September 11, 2026

Brazil's New Economic Matrix (NEM, 2011–2014) was the country's largest industrial policy since the military era, delivered as subsidized credit by the national development bank, BNDES. Using credit-registry, employer–employee, and firm-survey microdata, we estimate two staggered difference-in-differences designs. Against firms that never borrowed, treated firms were 0.7 to 0.9 percentage points less likely to exit, more than half of the 1.2 percent baseline exit rate. They did not grow: employment fell on impact and recovered only after four years, while the investment rate jumped 7.8 percentage points in the loan year before turning negative from the second year on. Timing mattered within the borrower pool: firms treated in 2011–2013 employed 5.8 percent more workers than the 2014 cohort. But the gain came entirely from firms with a pre-existing BNDES relationship; first-time borrowers raised investment and nothing else. Subsidized credit extended firm lifetimes and retimed capital spending more than it expanded activity.

Keywords: Industrial policy. Development banks. Subsidized credit. Firm dynamics.

We are grateful for comments and suggestions from Rodrigo Oliveira, Timothy Köhler, Bruno Barsanetti, Bernardo Fernandes, and many other FGV-EPGE colleagues for informal talks, as well as to those attending the EPGE Macro Seminar, the UNU-Wider Development Conference in Helsinki, and at the EPGE Applied Micro Workshop. All errors are ours. This research is partly funded by the Coordenação de Aperfeiçoamento de Pessoal de Nível Superior - Brasil (CAPES) - Finance Code 001. The views expressed herein are those of the authors and do not necessarily reflect the views of the Central Bank of Brazil. Corresponding author contact: vitor.santos@fgv.br (Vitor Santos).
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1. Introduction

After the 2008–09 Financial Crisis, industrial policies gained prominence in many countries, especially in developing economies. Several of these nations sought to implement such policies to overcome their role as exporters of primary goods within global production chains. Brazil is one of them. Throughout its economic history, Brazil has adopted numerous policies aimed at promoting growth and national development. Over the 20th century, the dominant strand of Brazilian economic thought was the so-called Structuralist school, with import substitution serving as the main driver of government policy.

After a period of liberal reforms in the 1990s and 2000s, in 2012 the Brazilian government announced the New Economic Matrix[1], a major economic and industrial policy program designed to intervene in the economy through fiscal and credit policies. On the credit side, the program was strongly marked by the role of the Brazilian Development Bank (BNDES).[2] Created in 1952 to finance long-term infrastructure projects, the BNDES has been used in different ways depending on the priorities of the government in power. Following a substantial cash injection from the National Treasury starting in 2010, the BNDES became a key instrument for industrial policy and implementing the NEM.

In this paper, we examine BNDES loans during the NEM period and estimate the effects of access to these loans on firm behavior. Using granular data from the System Credit Register (*SCR*) of the Brazilian Central Bank, employer-employee records from RAIS, firm-level microdata from IBGE annual surveys, and electoral data, we construct a panel from 2005 to 2019 at the firm level, encompassing a representative sample of formal firms in the Brazilian economy. We pursue two complementary empirical questions. First, among firms that borrowed from BNDES during the NEM, did those with earlier access perform better than those that accessed credit later, what we call the *within-effect*? Second, how did BNDES-treated firms perform relative to firms that never received subsidized credit, what we call the *comparative effect*?

On the *comparative effect*, using the extended two-way fixed effects (ETWFE) estimator of Wooldridge (2021) applied to the IBGE firm panel, BNDES credit extended firm survival: treated firms were 0.7 to 0.9 percentage points less likely to exit the market over 2005–2019, against a baseline exit rate of 1.2 percent, and the gain does not differ between the most and least financially constrained firms. Employment shows no significant average difference, though the dynamic evidence reveals a sequenced adjustment: a short-run decline at $t = 0$, consistent with capital-for-labor substitution as firms deploy new investment, followed by a positive labor demand response at $t = 4$. Average wages among treated firms were also 18.4 percent higher than among never-treated firms; however, the pre-treatment wage trend is not flat, so we interpret this estimate with caution and do not treat it as a central

[1] In Portuguese, *Nova Matriz Econômica* — NEM, henceforth.

[2] In Portuguese, *Banco Nacional de Desenvolvimento Econômico e Social* — BNDES, henceforth.

result.

On the *within-effect,* we implement the interaction-weighted estimator of Sun and Abraham (2021), fixing the last cohort of BNDES borrowers (2014) as the comparison group. Pre-trends hold, and we find that earlier access to BNDES credit during the NEM increased employment by approximately 5.8%, with no significant impact on wages. The effect is concentrated among small and micro firms and among firms that had prior BNDES relationships before the NEM. Firms borrowing from BNDES for the first time show no significant employment or wage response, a finding we return to below.

These results reveal a sharp duality. Within the universe of BNDES borrowers, earlier access helped. But the firms driving the within-effect gains, repeat borrowers with prior BNDES relationships, represent a minority of the treated population. The majority, first-time borrowers, show no meaningful employment or wage response, consistent with substitution of private for public credit without net expansion of real activity. Relative to firms that never borrowed from BNDES at all, the average effect on the treated is far more muted.

Although the employment effect is similar across sectors, we observe substantial heterogeneity by firm size and borrowing history. The within-effect is driven entirely by small and micro firms; large firms show no significant response, consistent with Bonomo, Brito, and Martins (2015) and Lazzarini et al. (2015). We further find that the employment gains are concentrated among firms with prior BNDES relationships: firms borrowing from the bank for the first time during the NEM show no significant employment or wage effects, suggesting that the policy's reach to new borrowers did not translate into real activity expansion.

Our paper makes three main contributions to the literature. First, we contribute to the growing literature evaluating industrial policy interventions. As highlighted by Juhász, Lane, and Pérez (2023), the number of industrial policies has been rising in recent years and becoming more *granular,* often implemented through targeted subsidies or specific programs. This trend increases the need for recent econometric methods that can credibly identify causal effects on firm behavior (Lane 2020).

In this context, we engage with a broad body of literature that aims to identify the causal impact of industrial policies on firms. Bernini and Pellegrini (2011) analyzed the effect of a law implemented between 1996 and 2004 that subsidized firms through a regional allocation system. They found that subsidized firms increased employment but experienced a decline in productivity. Dvouletý, Srhoj, and Pantea (2021) conducted a comprehensive literature review on the effects of subsidies on small and medium-sized enterprises and found that the impact on employment is almost universally positive. Overall, the literature indicates that targeted subsidized credit policies have positive employment effects (Cavalcanti and Vaz 2017; Lazzarini et al. 2015).

Our findings are consistent with this body of work: firms with access to subsidized credit increase employment. However, the effects are highly heterogeneous and are essentially driven by small and micro firms. The positive impact on employment, given the volume of credit involved, is not surprising, but its small magnitude certainly is. As is the non-significant effect on wages.

Second, our paper presents a new perspective on how industrial policy can be effectively implemented in practice. We examine the performance of a state-owned development bank operating within the framework of a major federal policy initiative. This provides an alternative lens for understanding industrial policy and highlights the risk of misinterpreting its effects. Bloom et al. (2024) documents how countries around the world used industrial incentives to support their shipbuilding industries. In Europe, for instance, even massive subsidies failed to sustain the sector's global relevance. Kim, Lee, and Shin (2021) studies the Heavy-Chemical Industry Drive in South Korea during the 1970s and argues that the success of industrial policy is inversely related to the degree of misallocation it introduces.

Our main analytical framework is the New Economic Matrix (NEM). Since the NEM was a well-known industrial policy launched by President Rousseff's administration, we assume that the overarching objectives of that policy guided all BNDES disbursements during this period. The NEM also had a cyclical dimension. At its inception, economic conditions differed markedly from those in 2014, a contrast we exploit in our identification strategy.

Finally, we contribute to the extensive literature on state-owned banks (Yeyati, Micco, and Panizza 2004; Panizza 2023; de Luna-Martinez and Vicente 2012), particularly BNDES, credit policy and its outcomes (Lazzarini et al. 2015; Bonomo, Brito, and Martins 2015; Cavalcanti and Vaz 2017; Bazzi et al. 2023). There is an ongoing debate in Brazilian academia about the role and effectiveness of BNDES in providing subsidized credit and promoting economic growth and job creation. Barboza et al. (2020) conducted a comprehensive review of the literature up to 2020, which studied the effects of BNDES policies at various levels of aggregation (e.g., firms, municipalities, and macroeconomic outcomes).

These studies often raised more questions than answers, largely due to their research designs, as most lacked a clearly defined policy context. To the best of our knowledge, we are the first to analyze the role of BNDES specifically during the New Economic Matrix. The study most closely related to ours is Bazzi et al. (2023), which analyzed the introduction of the *Cartão BNDES*, a financial instrument designed to expand access to credit for small and micro firms. Our approach, however, does not rely on the *Cartão BNDES* instrument.

The remainder of the paper is organized as follows. Section 2 outlines the institutional background of the NEM and BNDES operations. Section 3 presents the data and sample selection. Section 4 details our identification strategies and presents the firm-level results

for employment, wages, investment, and survival. Section 5 offers concluding remarks.

2. Institutional background

The New Economic Matrix - NEM

In 2012, Dilma Rousseff's government announced its new economic policy: the New Economic Matrix. This was the first major government-led economic intervention since the *II PND*[3]. In this section and throughout the paper, we focus primarily on the disbursements made by BNDES. Although nearly all public banks provided some form of credit subsidy, BNDES was largely responsible for the credit expansion during this period, accounting for approximately 4% of GDP in disbursements—equivalent to more than 20% of total private sector credit (Lazzarini et al. 2015; Orair and Gobetti 2017).

The NEM ended in 2015, when the Minister of Finance, and mentor of the New Economic Matrix, was replaced, and the government initiated a series of measures to control the public deficit and inflation. At that moment, the economy was on the verge of a severe recession. Following Rousseff's impeachment in 2016, the presidency of BNDES changed, and the bank adopted a less interventionist approach.

The BNDES policy during the NEM

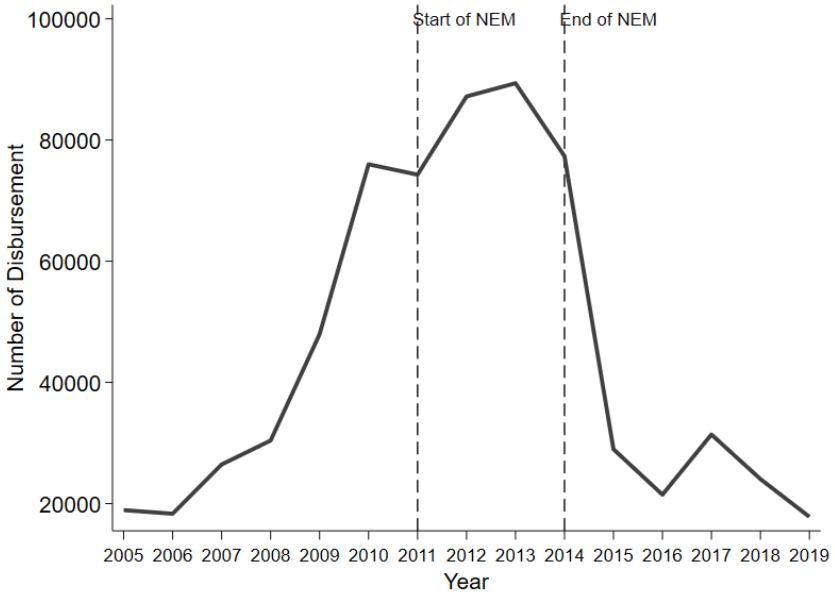
Figure 1 shows BNDES' credit policy through 2005-2019 period. It's clear that the number of disbursals made by the bank during NEM is higher compared to any other event before or after. During NEM, the BNDES made an average of 84,050 disbursements per year. For comparison, during the Financial Crisis (2008-2009), the BNDES made an average of 39,191 disbursements per year. This difference highlights how the Federal Government has utilized the bank to implement its policies through subsidized credit. After the policy ended in 2014, the number of disbursements fell to 2010 levels and has been on a downward path since then.

We report in Table 1 the evolution of average disbursements per year, by firm size and sector. Considering all firms jointly, the average disbursement declined from R$ 540 thousand in 2010 to R$ 214 thousand in 2015. After 2015, the bank experienced reductions across all dimensions: the number of credit operations and the average credit per firm. In Panel B, medium and small firms followed a similar trajectory in terms of average annual disbursements. While large firms received, on average, R$ 9.33 billion in 2009[4], the average annual disbursement granted by BNDES to large firms over the entire period was

[3]The Second National Development Program, in Portuguese Segundo Programa Nacional de Desenvolvimento — II PND, implemented during the Brazilian Military Regime under the Geisel administration (Colistete 2011).

[4]Petrobrás, the main state-owned oil company, received a credit of R$ 25 billion in 2009 (Banco Nacional de Desenvolvimento Econômico e Social BNDES).

FIGURE 1. BNDES disbursements policy over time



Notes: Authors elaboration. Source: BNDES.

R$ 5 billion. The main takeaway from Table 1 is the policy's emphasis on micro firms as a key target. Despite receiving smaller individual loans, the number of credit operations for micro firms was considerably higher.

A key characteristic of the BNDES disbursement policy from 1994 to 2017 — and thus during the NEM — was the use of a highly subsidized interest rate on most of its long-term loans, known as the *TJLP* (Taxa de Juros de Longo Prazo, or *Long-Term Interest Rate*). This government-set rate, administratively determined and typically below market levels, was designed to stimulate investment by reducing borrowing costs. The TJLP played a central role in Brazil's development financing strategy throughout this period.

Figure 2 illustrates this pattern. The interest rate charged by BNDES was consistently below the *Selic*. The *Selic rate* is Brazil's *benchmark interest rate*, set by the Central Bank. It represents the average rate at which banks lend to each other overnight using government bonds as collateral and serves as the *floor for interest rates in the economy*, Between 2011 and 2014, in particular, the TJLP appears detached from economic fundamentals, as it decreased during this period in contrast to the *Selic*, which experienced a steep increase. This persistent spread between the TJLP and the Selic fueled debate over the implicit subsidy provided by the National Treasury to BNDES, as the government issued bonds at market rates and transferred the funds to BNDES, which then lent at subsidized rates.

Given its nature as a development bank, the BNDES had a heterogeneous policy across sectors and firms size. Between 2011 and 2014, around 328 thousand disbursements were

Table 1. Average BNDES's outlays between 2005-2017 in million reais (*R*$)

	Year							
	2008	2009	2010	2011	2012	2013	2014	2015
Panel A - All firms								
All	0.82	0.74	0.52	0.30	0.27	0.28	0.27	0.21
Panel B - By firm size								
Large	5.58	9.33	5.27	3.58	4.91	4.31	4.36	4.39
Medium	0.71	0.50	0.55	0.47	0.39	0.38	0.32	0.23
Small	0.24	0.15	0.20	0.17	0.14	0.15	0.13	0.09
Micro	0.13	0.09	0.10	0.07	0.05	0.06	0.06	0.04
Panel C - By sector								
Agriculture	0.21	0.23	0.24	0.21	0.20	0.23	0.23	0.19
Business and services	0.74	0.55	0.37	0.26	0.24	0.24	0.24	0.19
Manufacturing	1.73	1.91	1.29	0.47	0.40	0.42	0.36	0.31
Extractive	5.02	4.49	0.99	1.99	0.73	1.33	1.09	1.05

Approximated values. The credit operations values are deflated to 2010 values. The firm size category is defined by BNDES and suffer changes over time (see the classification here: BNDES classification) and is based on the gross operating revenue of the firm. The sector categories are the *CNAE 1* classification and is the most aggregated level of sectoral analysis.

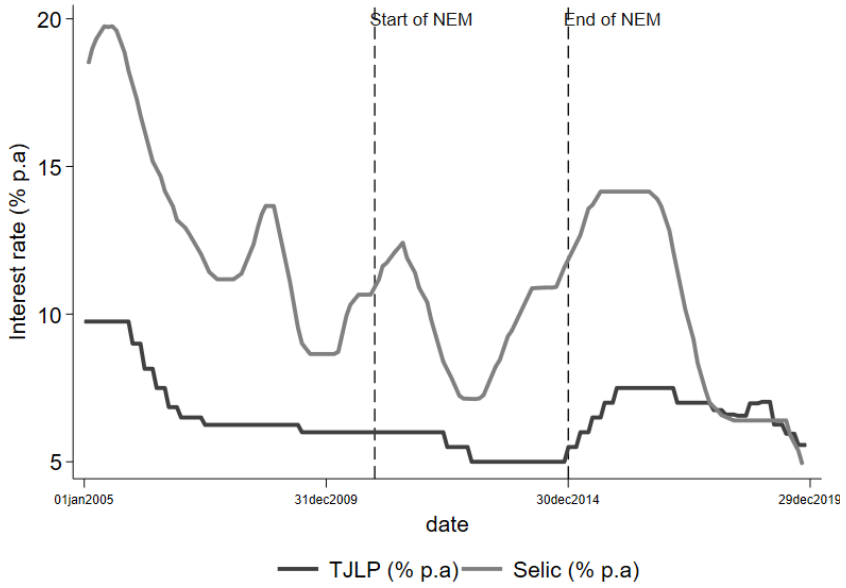
made, and almost all of them to the private sector. Figure 3 shows how BNDES behaved over the period. We can notice two distinct patterns for the share of loans in reais and the share of total disbursements made by the bank. While large firms concentrated over 40% of the value of all available loans (*R*$), over 60% of all credit operations were allocated to micro firms. Hence, large firms received proportionally fewer credit operations, but each operation was, on average, significantly larger than those extended to firms of other sizes.

Event design

We now describe how a firm can get a loan at the BNDES. The disbursement can be made *directly* or *indirectly* by the BNDES. Direct disbursement means that the BNDES itself transfers the money to the firm, while indirect disbursement implies that a third-party (a commercial bank) plays an intermediary role between the BNDES and the firm. In the latter the third-party is responsible to conduce the credit analysis and also assumes any future loss in the operation and BNDES only provides the credit line. Also, there are *automatic* and *non-automatic* credit operations. If a firm requests a credit below a *certain* threshold, the loan is automatically given to the firm after the credit analysis.[5]

[5]Publicly, the BNDES does not say what the real threshold is and which guidelines the bank follows to characterize a credit operation as automatic or non-automatic. Using anonymized public data Figueiredo et al. (2025) noticed that there is a limbo between *R*$ 10 million and *R*$ 150 million in which any request below *R*$ 10 million are always automatic and requests above *R*$ 150 million pass through BNDES credit analysis. Credit operations inside this interval can be both automatic or not.

FIGURE 2. Interest rates evolution



Notes: Selic is the basic interest rate of the economy charged by the Brazilian Central Bank. TJLP is the interest rate applied by the BNDES in their credit operations.

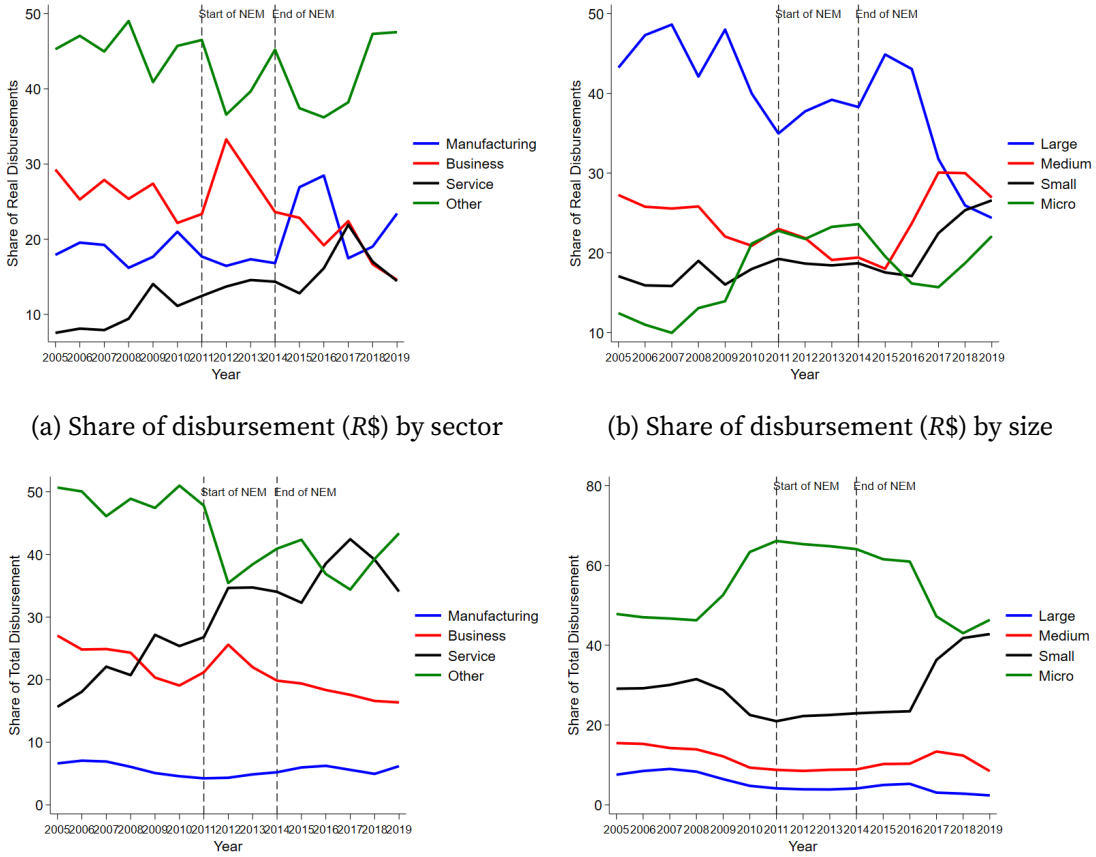
We aim to quantify the effect of the BNDES outlays policy *due to* the NEM program. The main question is: Did a major subsidized credit policy implemented by a State-owned bank impact the firm's behavior? To understand the effects and the mechanisms through which the subsidized credit policy affected firms' behavior, we rely on firms' employment and wages. The employment can tell us how effective the policy was in generating jobs in those firms. In turn, wages can inform us about the task valuation at the firm level. We set 2011 as the starting year of the NEM, and the final year is 2014. The event encompasses all disbursements made by the BNDES from 2011 to 2014.

3. Data

We utilize several sources to construct our panel and perform our analysis, drawing on information from 2005 to 2019.

RAIS - Annual Social Information Report. To obtain information about workers' income and labor, we use the *Relação Anual de Informações Sociais* (Annual Social Information Report - RAIS), an administrative employer-employee dataset covering all formal employment contracts in Brazil, with rich information on employee wages, education levels, and economic sectors of employment. We collapse worker's information at firm-level to construct a panel of firms. Each firm is identified by its national *id* (CNPJ), which allows us to match this dataset with several others.

FIGURE 3. BNDES policy over time



(a) Share of disbursement (*R*$) by sector

(b) Share of disbursement (*R*$) by size

(c) Share of disbursement (#) by sector

(d) Share of disbursement (#) by size

Notes: Authors elaboration. Source: BNDES.

BNDES – Annual Disbursement. We use data on all disbursements made by BNDES between 2005 and 2019. The dataset contains firm-level information on credit operations, including the financial instrument used, the amount borrowed, the interest rate applied to the contract, the number of installments, and firm characteristics such as sector, size, and legal status. Unlike Bazzi et al. (2023), our study does not include operations under the *Cartão BNDES*. The data were obtained through the *Lei de Acesso à Informação*[6], and only include traditional financial instruments—characterized by direct transfers from BNDES or third parties to the firm's bank account. From this dataset we construct our treatment variable, assigning firms to be treated if received any credit between 2011-2014.

SCR – Sistema de Informações de Crédito. We use confidential data from the *Sistema de Informações de Crédito* (SCR), administered by the Brazilian Central Bank (BCB). The

[6]The *Lei de Acesso à Informação* (Information Access Act) is a legal instrument in Brazil that grants access to public information, including data that may contain sensitive details such as firm names or registration numbers. Protocol number: 52021.003217/2024-11.

SCR is a registry system for credit operations and their respective guarantees contracted by legal entities and individuals across all financial institutions operating in Brazil. From this dataset we extract contract-level information on loan operations between firms and financial institutions. A key feature of the SCR is its near-universal coverage: all operations with total client responsibility equal to or above R$200 are recorded, which allows us to construct, for each firm, the full picture of its borrowing across all financial institutions simultaneously. We use this dataset to construct firm-level measures financial insertion.

PIA, PAS, PAC - Annual Economic Surveys. We use firm-level microdata from three annual surveys conducted by the Brazilian Institute of Geography and Statistics (IBGE): the *Pesquisa Industrial Anual* (PIA), the *Pesquisa Anual de Serviços* (PAS), and the *Pesquisa Anual de Comércio* (PAC). Together, these surveys cover the universe of formally constituted Brazilian firms whose primary revenue source is, respectively, industrial production, non-financial services, and commercial activity. All three surveys adopt a stratified sampling design anchored to the *Cadastro Central de Empresas* (CEMPRE), and have followed the CNAE 2.0 classification since the 2008 reference year. Each survey stratifies firms into two groups. The *estrato certo* comprises all firms with 30 or more employees, which are required to complete the survey every year and together form a census of larger firms within each sector. The *estrato amostrado* comprises firms with at least five but fewer than 30 employees, selected annually through probability sampling within strata defined by sector and size class. IBGE assigns each firm in the *estrato amostrado* an expansion weight reflecting the number of firms in the population it represents within its stratum.

Auxiliary datasets. We combine secondary data sources to obtain additional variables required in the empirical specification. We use the electoral data from *Tribunal Superior Eleitoral* (Superior Electoral Court - TSE), to obtain data of firms' campaign donations[7]. County level information of partisanship between the county and the federal government built by Figueiredo et al. (2025).[8] Additionally, we use the trade status of the firm (exporter/importer/both) and the shareholder composition of each firm provided by the Ministry of Finance. Unfortunately, due to the sensitive nature of the data, it is not possible to merge the BCB and IBGE databases.

The sample is restricted to firms in the manufacturing, business, and services sectors. Different from most of the studies analyzed by Barboza et al. (2020), we do not restrict the size of firms in the sample, but we keep only private firms with automatic operations. As Figure 3 shows, there is a large heterogeneity in the BNDES policy. Moreover, the empirical approach in Section 4 enables us to investigate heterogeneity by firm size or sector. Restricting the sample to be only private firms, we avoid including in the study

[7] Until 2014, firms were allowed to donate for political campaigns. After 2014, the Federal Supreme Court implemented a new juridical understanding restricting campaign donations to the public electoral fund and individual contributions

[8] We thank the authors for kindly sharing their dataset with us.

the disbursement made by the BNDES to governmental autarkies or state-owned firms. For instance, if a city council wanted to improve its urban sanitation or mobility, it would be possible to take a loan through some BNDES credit lines in an automatic operations. Moreover, state-owned firms, such as *Petrobrás*, were likely to have more access to credit given their political lobby on BNDES (Barboza et al. 2020). By restricting the sample to private firms and automated operations, we capture the policy side of the BNDES disbursements.

4. Firm-level effects of BNDES disbursements

This section examines the firm-level effects of BNDES credit disbursements during the New Economic Matrix through two complementary identification strategies that answer distinct questions. Section 4.1 estimates the *comparative effect*: how did BNDES-treated firms perform relative to firms that never received subsidized credit during the NEM? We employ the extended two-way fixed effects (ETWFE) approach of Wooldridge (2021), applied to the IBGE firm panel with never-treated firms as the control group. Also, we conduct several exercises to understand how the subsidized credit where allocated and how the allocation could affect other firms aspect beyond investment and employment. Section 4.2 estimates the *within-effect*: among firms that received BNDES subsidized loans, did those that gained access earlier in the NEM period (2011) perform better than those treated later (2014)? We answer this question using the interaction-weighted estimator of Sun and Abraham (2021), which compares treated cohorts against the last-treated group and recovers cohort-specific average treatment effects on the treated.

4.1. BNDES loans comparative-effect

Borrow, firms characteristic, and sample

Our fist analysis in this paper is to understand how firms which received any disbursement from IBGE over the NEM performed when we compare them against firms that never accessed the subsidized credit. The question is not only about timing: it is about whether receiving subsidized credit under the NEM was itself beneficial relative to the counterfactual of no BNDES loan at all. This section addresses it directly.

Constructing a valid control group is the first challenge. Unlike studies that exploit firm-level application records to select comparable untreated units (Bernini and Pellegrini 2011; Bernini, Cerqua, and Pellegrini 2017), we do not have access to this information: BNDES does not publish application outcomes, and credit decisions by third-party intermediaries are subject to bank secrecy rules. We rely on two samples. The first draws on the IBGE firm-level surveys (PIA/PAS/PAC), which provide the treated and never-treated firms from surveys and from where we conduct firm-level analysis of investment and labor. The

second is a random ten percent sample of firms present in RAIS in 2010 that did not receive any BNDES disbursement during the NEM period, used to understand how different are BNDES firms from other firms in the economy and credit-market insertion.

Before turning to dynamic treatment effects, we characterize the selection process that determined which firms received BNDES credit. We estimate a linear probability model for the treatment indicator at baseline period (2010) using firm, sector, and political characteristics as predictors. We use RAIS rather than the IBGE surveys for this exercise for three reasons. First, RAIS covers the universe of formal firms in Brazil, providing substantially greater statistical power than the stratified IBGE samples. Second, because the specification is cross-sectional we can merge the full SCR credit registry at that date to construct two additional firm-level predictors unavailable in IBGE data: measures of banking market insertion, namely the stock of bank debt and previous banking relationships held by each firm. The exercise is descriptive rather than causal.

The New Economic Matrix was a federal policy, but its credit allocation varied across sectors and geographies: not all sectors received equal BNDES attention (Figure 3), and disbursements could be allocated in particular states and municipalities following other reasons than economics. We argue that firms operating within the same industry and state therefore faced a likely common competitive environment, similar exposure to the policy, and comparable baseline trajectories, making the industry-state cell a natural unit within which to assess treatment assignment. We incorporate this dimension by constructing rank-normalized measures of firm characteristics within each industry-state cell and including industry-state fixed effects throughout. The treatment assignment specification is:

$$\mathbb{I}\{\text{BNDES Credit}\}_{i,2010} = c + \mathbf{X}\beta + \alpha_{sm} + \varepsilon_{i,2010} \tag{1}$$

whereby c is the constant, α_{sm} is the industry-state fixed-effect, ε the idiosyncratic error term, and $\mathbf{X}$, is a list of all possible controls at firm, sector, and geographical level. Besides the traditional variables associated with firms characteristics, we also include political connections between the city and the Federal Government (Figueiredo et al. 2025), industry-state dispersion of the subsidies captured by a Herfindahl index,[9] and banking information. We construct within–industry-state rank-based measures of firm characteristics. For each variable, firms are ranked within their sector and municipality, and the resulting rank is normalized to lie between zero and one. We additionally include squared transformations of these rank variables to allow for flexible nonlinear effects (Bernini and Pellegrini 2011). Rank-based normalization ensures that firm characteristics

[9] As proposed by Aghion et al. (2015), given by: $\text{Dispersion Index}_{isc} \equiv H_{-i,sc(i),t} = 1 - \sum_{i \in \mathcal{F}_{sc}} \left(\frac{\text{Subsidy}_{ijc}}{\sum_{k \in \mathcal{F}_{sc}} \text{Subsidy}_{ksc}} \right)^2$, where $\mathcal{F}_{sc}$ denotes the set of firms operating in sector s and state c.

are measured relative to local competitors, reducing sensitivity to outliers and differences in scale across industries and states

Table 2 reports the results. The specification explains approximately 26% of the variation in treatment assignment, and the estimated coefficients exhibit the expected signs. The probability of receiving BNDES credit declines with firm size within an industry-state cell: the linear rank term is −0.18, partly offset by a positive quadratic term, so that moving from the smallest to the largest firm reduces the probability by around 7 percentage points. The banking and financial variables add a distinct dimension to the selection picture: firms with higher financial constraint, measured by the standardized FCI, are significantly more likely to receive BNDES credit, with one standard deviation of the index raising the probability by 1.2 percentage points. The stock of bank debt enters non-monotonically: the probability rises over the lower ranks of debt and falls steeply at the top (−0.56 on the squared term), so that the most indebted firms are the least likely to be treated. Additionally, simply having access to private bank credit reduces the probability of BNDES treatment by 7.9 percentage points. Together, these patterns suggest that BNDES under the NEM reached financially constrained firms that were underserved by private financial markets. Political connections also matter: donating in the 2008 elections raises the probability of receiving BNDES credit by 6.0 percentage points, and donating in the 2010 elections raises it by 7.7 percentage points; being located in a city whose ruling party belongs to the federal government's coalition raises it by 51.8%. Among trade-exposed firms, importers are 5.6% more likely to be treated, while exporter status is not significant.

Table B2 reports the same specification as Table 2, changing only the local political alignment variables for party and coalition in the periods 2011–2012 and 2013–2014. The results hold. This result reinforces the electoral component of BNDES actions. These coalition dummies are particularly informative because Brazil has a political system with more than thirty political parties, such that the same party can be in government or in opposition depending on the state and the year.[10] All other variables retain their interpretation.

We report on figure 4 the distribution of financial constraint by treatment status at the pre-policy period. The group of firms that did not received BDNES credit under the NEM is concentrated at lower FCI values, while BNDES-treated firms exhibit a rightward shift and a heavier right tail, indicating that the BNDES subsidized credit through the NEM reached firms facing tighter financial constraints. This pattern is consistent with the positive and significant coefficient on the FCI in table 2. Table 2 and figure 4 shows that the firms which had any subsidized credit between 2011-2014 were considerable different from firms that never received, even explaining 40% of the treatment status correlation.

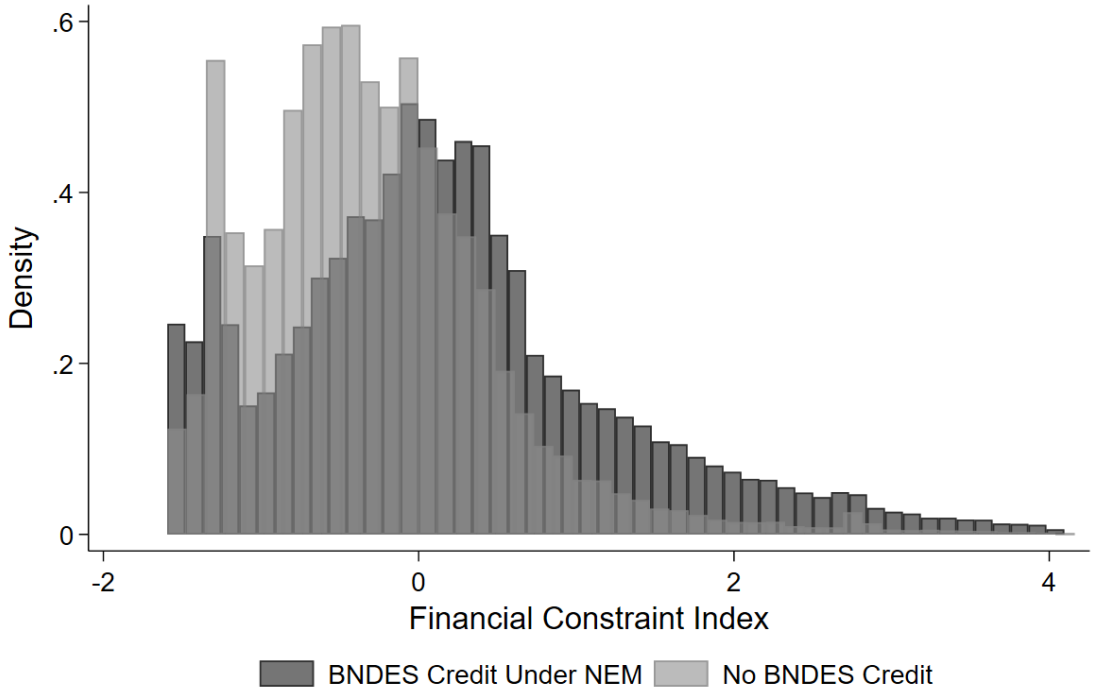
[10] Table B3 reports the correlations among the coalition dummies, illustrating substantial variation across years. Municipal elections took place in 2008 and 2012, while national and state elections occurred in 2010 and 2014.

Table 2. OLS Estimates on BNDES Treatment Assignment

	Sample			
Outcome: $\mathbb{I}\{\text{BNDES Credit}\}$	Full	Manufacturing	Business	Service
Panel A: Firm characteristics				
r Number of workers	-0.1797***	-0.1605***	-0.6460***	-0.0876*
r^2 Number of workers	0.1060***	-0.2516***	0.3646***	-0.0177
r Firm age	-0.0343**	0.0206	-0.0537	0.0037
r^2 Firm age	0.0174	-0.0224	0.0190	-0.0059
r Average education	0.1212***	0.1394***	0.1697***	0.1266***
r^2 Average education	-0.0669***	-0.1432***	-0.0226	-0.0545**
Subsidy Dispersion Index	0.6703***	0.2494***	0.1516	0.6712***
$\mathbb{I}\{\text{Simples}\}$	0.0035	0.0246***	-0.0052	0.0236***
Panel B: Sector characteristics				
$\mathbb{I}\{\text{Manufacturing}\} \times r$ Number of workers	-0.2064***			
$\mathbb{I}\{\text{Business}\} \times r$ Number of workers	-0.0552***			
Panel C: Firm size				
$\mathbb{I}\{\text{Medium firm}\}$	-0.0442***	-0.0477***	-0.0881***	-0.0366***
$\mathbb{I}\{\text{Small firm}\}$	0.1924***	-0.0048	0.1151***	0.0725***
$\mathbb{I}\{\text{Micro firm}\}$	0.0670***	-0.0044	0.0288**	0.0496***
$\mathbb{I}\{\text{Medium}\} \times r$ Number of workers	-0.0022	-0.0529*	0.0252	-0.0825***
$\mathbb{I}\{\text{Small}\} \times r$ Number of workers	-0.2446***	0.2284***	-0.3252***	-0.1616***
$\mathbb{I}\{\text{Micro}\} \times r$ Number of workers	0.0165	0.2322***	0.1849***	-0.0177
Panel D: Trade exposure				
$\mathbb{I}\{\text{Exported}\}$	-0.0016	0.0012	-0.0618***	0.0574
$\mathbb{I}\{\text{Imported}\}$	0.0559***	0.0577***	-0.0480	0.1035***
Panel E: Political connections				
$\mathbb{I}\{\text{Political Donation in 2008}\}$	0.0598***	0.0171*	0.0973***	0.0642***
$\mathbb{I}\{\text{Political Donation in 2010}\}$	0.0771***	0.0094	0.0720***	0.0638***
Credit Share $\times \mathbb{I}\{\text{Party Alignment}\}$	0.0313	-0.4887**	-4.9124*	0.2382**
Credit Share $\times \mathbb{I}\{\text{Coalition Alignment}\}$	0.5183***	0.6536***	6.3189**	0.0263
Panel F: Banking				
Financial Constraint Index	0.0118***	0.0192***	0.0310***	0.0171***
r Stock of Total Debt	0.1003**	0.1116**	-0.0986	-0.0397
r^2 Stock of Total Debt	-0.5596***	-0.3768***	-0.4749***	-0.5493***
$\mathbb{I}\{\text{Private Credit}\}$	-0.0793***	-0.0601***	-0.1091***	-0.0970***
Observations	140,283	34,499	60,023	45,483
R^2	0.26	0.23	0.18	0.49

Note: This table reports results from OLS estimates of the probability of receiving BNDES support. Variables prefixed by r denote ranking-based firm-level characteristics, and r^2 denotes their square. *Simples* indicates whether a firm belongs to the simplified tax system. *Manufacturing* and *Service* are dummies for the firm's sector. *Imported* and *Exported* indicate whether a firm ever imported or exported in 2010 or earlier. *Political Donation* indicates whether a firm made any donation to a party, candidate, or committee in the 2008 or 2010 elections. *Credit Share* is the percentage of credit received by each firm within its sector–city cluster. *Party Alignment* indicates whether the party in power in city c is the same as the party of the federal government. *Coalition Alignment* indicates whether the party in power in city c belongs to the same coalition as the federal government's party. *Financial Constraint Index* follows Hadlock and Pierce (2010) (see appendix C), constructed from SCR-declared revenue and CNPJ firm age, and standardized to mean zero and unit variance. *Stock of Total Debt* is the rank-normalized stock of private bank debt in 2010. *Private Credit* indicates whether the firm held any private bank credit prior to 2011. All specifications include industry-state fixed effects. Standard errors are clustered at the industry-state level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 4. Financial Constraint and BNDES Subsidized Credit



Notes: This figure shows the distribution of the Financial Constraint Index (FCI) in 2010 for firms that received BNDES credit under the New Economic Matrix (dark gray) and firms that did not receive any BNDES credit during the same period (light gray). The FCI follows Hadlock and Pierce (2010) (see appendix C) and is constructed from SCR-declared revenue and firm age; it is standardized to mean zero and unit variance, so that higher values indicate greater financial constraint.

Firm exit

We ask whether BNDES credit lowered the probability of exit relative to firms that never borrowed, and whether any survival gain outlasted the policy or only delayed closures until credit dried up. We estimate a linear probability model on the pooled sample of treated firms and the ten percent random sample of never-treated RAIS firms, over 2010-2019:

$$\mathbb{I}\{\text{Exit}\}_{i,t} = c + \delta \cdot \text{Treatment} + \mathbf{X}\beta + \gamma_t + \varepsilon_{i,t}, \tag{2}$$

where c is a constant, γ_t denotes year fixed effects, $\varepsilon_{i,t}$ is an idiosyncratic error term, $\mathbf{X}$ is a vector of firm- and sector-level controls, and $t \in [2010, 2019]$. We model *treated* in two ways. First, as a single absorbing treatment indicator that ($\mathbb{I}\{\text{treated}\}$) that assumes value equal one after the first credit from BNDES within the New Economic Matrix. Second, as a interact variable ($\mathbb{I}\{\text{Was Treated}\} \times \mathbb{I}\{\text{Post NEM}\}$) between a indicator if a firm was treated or not within the NEM and a dummy for period after the policy (2015 forwards).

Table 3. Firm Exit and BNDES Treatment Status

Outcome: $\mathbb{I}\{\text{Exit}\}$	**Benchmark**		**FCI Heterogeneity**	
	(I)	(II)	(III) *Top 25%*	(IV) *Bottom 25%*
$\mathbb{I}\{\text{Treated}\}$	-0.0068^{***}			
	(0.0003)			
$\mathbb{I}\{\text{Was Treated}\} \times \mathbb{I}\{\text{Post NEM}\}$		-0.0090^{***}	-0.0075^{***}	-0.0078^{***}
		(0.0004)	(0.0016)	(0.0005)
Mean of $\mathbb{I}\{\text{Exit}\}$	0.0122	0.0122	0.0122	0.0122
Observations	1,296,024	1,296,024	1,296,024	1,296,024

Note: The table reports OLS estimates of the probability of firm exit using firm-year data from 2010 to 2019. $\mathbb{I}\{\text{Treatment}\}$ is an absorbing indicator equal to one from the year of first BNDES loan onward. $\mathbb{I}\{\text{Was Treated}\}$ equals one for firms that ever received a BNDES loan during the NEM. $\mathbb{I}\{\text{Post NEM}\}$ equals one for years 2015 and beyond. Columns (III) and (IV) interact the treatment with indicators for the top and bottom quartiles of the Financial Constraint Index (FCI), respectively; higher FCI values indicate greater financial constraints. Controls include the baseline hazard, the subsidy dispersion index, the standardized FCI, log employment, an indicator for private bank credit, and the log stocks of commercial-bank and BNDES debt. Fixed effects are year and industry-state. Clustered standard errors at industry-state level in parentheses. Significance levels: $^{*}\,p < 0.1$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table 3 reports the change in the probability of firm market exit. Column (I) shows that receiving any subsidized credit from BNDES during the NEM reduced the probability of exit by 0.7 percentage points over 2005–2019, more than half of the 1.2 percent baseline exit rate, controlling for the financial constraint index, firm size, private credit access, the stocks of commercial-bank and BNDES debt, the subsidy dispersion index, industry-state, and the baseline hazard. Column (II) refines the timing: the interaction $\mathbb{I}\{\text{Was Treated}\} \times \mathbb{I}\{\text{Post NEM}\}$ indicates that treated firms were 0.9 percentage points less likely to exit during 2015–2019, the period immediately following the policy reversal described in Section 2, when macroeconomic conditions deteriorated sharply.

Columns (III) and (IV) examine heterogeneity by financial constraint. Among the most constrained firms (top quartile of the FCI), BNDES credit reduced post-NEM exit probability by 0.75 percentage points; among the least constrained firms (bottom quartile), by 0.78 percentage points. The two estimates are statistically indistinguishable. The evidence therefore does not indicate that BNDES credit extended the operating lifetime primarily of firms that faced the tightest access to alternative financing; the survival gain is spread across the distribution of financial constraint.

Dynamic Effect of BNDES subsidized credit on investment and labor

Identification and empirical specification. The outcomes in this section draw on the IBGE firm-level surveys. The treated group comprises BNDES NEM borrowers observed in at least one of these surveys; the control group comprises firms in the same surveys that never received any BNDES credit during the NEM period. As detailed in Section 3, each survey stratifies firms into the *estrato certo* (firms with 30 or more workers, observed every year) and the *estrato amostrado* (firms with at least five but fewer than 30 workers, selected by probability sampling). Because *estrato amostrado* firms enter the panel only in the years they are sampled, the dataset is unbalanced by design. All specifications use IBGE expansion weights to recover population-representative estimates within each stratum.

As noted in Section 3, treatment is heterogeneous. Figure B1 reports the number of firms within each cohort. Consequently, traditional approaches may yield biased estimates, as parameters from a given period may be contaminated by effects from other periods, potentially invalidating both the pre-trends test and the estimation of dynamic effects (Callaway and Sant'Anna 2021; Sun and Abraham 2021; Wooldridge 2021).

We estimate the dynamic treatment effects using the extended two-way fixed-effects (ETWFE) estimator of Wooldridge (2021). The ETWFE augments the standard TWFE regression with a full set of cohort-by-post-period interaction cells, which restores consistency of the average treatment effect in the presence of treatment-effect heterogeneity across cohorts and time. With a never-treated comparison group and no covariates, the ETWFE is numerically equivalent to the Callaway and Sant'Anna (2021) estimator under an unconditional parallel trends assumption. Formally,

$$y_{it} = \sum_{e \in \mathcal{G}} \sum_{t' \geq e} \delta_{e,t'} \cdot \mathbb{1}\{G_i = e\} + \alpha_{sm} + \gamma_t + \varepsilon_{it}, \tag{3}$$

where α_{sm} and γ_t are industry-state and year fixed effects, $\mathcal{G} = \{2011, 2012, 2013, 2014\}$ is the set of NEM treatment cohorts, G_i denotes the year of firm i's first NEM loan, and $\delta_{e,t'}$ is the cohort–calendar-time average treatment effect on the treated. The identifying assumption is unconditional parallel trends: absent the NEM, firms that received the BNDES credit and those that did not received the credit would have followed the same outcome paths. The ATET is recovered as a cohort-size-weighted average of the $\hat{\delta}_{e,t'}$ estimates, and standard errors are clustered at the industry-state level.

Results and discussion. We examine the effects on average wages, employment, investment rate, and the extensive margin of investment. Table 4 reports the aggregate ATET for each outcome, and Figure 5 plots the corresponding dynamic effects. Pre-trends are flat and statistically insignificant for the investment rate and the investment dummy, supporting the identifying assumption of the ETWFE estimator for these two outcomes.

Employment shows a significant negative point estimate five years before treatment that does not recur in the remaining leads ($t = -4$ through $t = -1$), which are flat and insignificant. Average wages are the exception: the pre-treatment coefficients decline monotonically and significantly from $t = -5$ to $t = -2$ before converging to the normalized zero at $t = -1$, a pattern inconsistent with parallel trends. We report the wage ATET for completeness but do not treat it as a central result of the comparative-effect analysis.

Table 4. Average Treatment Effect of BNDES on Firm Outcomes

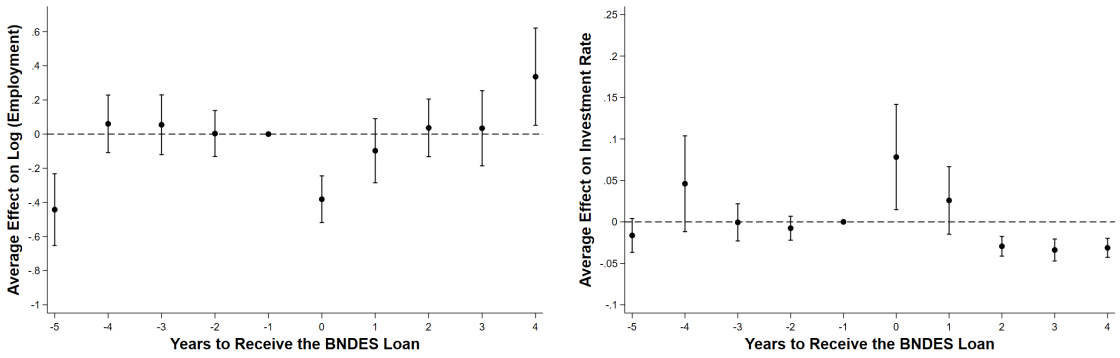
	log(Average Wage)	log(Employment)	Investment Rate	$\mathbb{I}\{\text{Investment} > 0\}$
Panel A: *All firms (baseline)*				
ATET	0.1841***	0.0174	0.0041	0.0197
	(0.0383)	(0.1015)	(0.0102)	(0.0236)

Note: The table reports the Average Treatment Effect on the Treated estimated via the Extended Two-Way Fixed Effects (ETWFE) estimator (Wooldridge 2021). The sample is an unbalanced repeated cross-section of firms drawn from the IBGE annual industry surveys (PIA, PAS, and PAC). Firms with 30 or more workers are observed continuously (*estrato certo*); firms with fewer than 30 workers enter the sample probabilistically in each survey round (*estrato amostrado*). All specifications include year fixed effects. The never-treated group comprises firms that received no BNDES credit during the NEM period. Column *Average Wage* uses the log of average firm wage as the outcome. Column *Employment* uses the log of total firm employment. Column *Investment Rate* is the ratio of gross investment to lagged capital stock. Column $\mathbb{I}\{\text{Investment} > 0\}$ is an indicator for positive gross investment. Standard errors clustered at the industry-state level are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The investment margin tells a different story. The investment rate jumps at $t = 0$ ($\hat{\delta} = 0.078$, significant at the 5% level), consistent with firms deploying subsidized credit for immediate capital expenditure, but the effect reverses from $t = 2$ onward, turning negative and significant. The probability of positive gross investment ($\mathbb{I}\{\text{Investment} > 0\}$) shows no significant effect at any horizon. The pattern on the intensive margin is characteristic of lumpy, one-time investment: firms bring forward a discrete capital purchase but do not sustain investment beyond the initial disbursement; the aggregate ATET on both investment outcomes is near zero, for the rate because the early gain is offset by the later reversal, and for the dummy because it is never significantly different from zero.

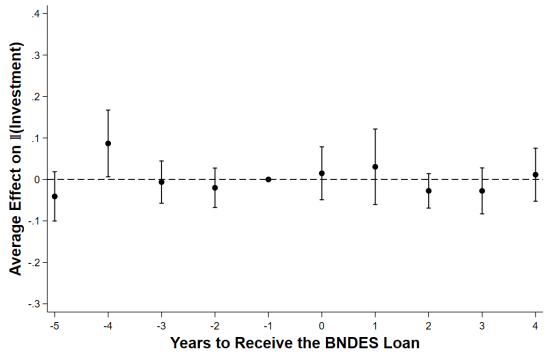
Employment dynamics suggests a short-run capital-for-labor substitution. The $t = 0$ effect is negative (−0.381, significant at the 1% level), suggesting that firms initially shed workers as they reorganize production around the new capital. Effects are statistically indistinguishable from zero at $t = 2$ and $t = 3$, but turn positive and significant at $t = 4$ (0.336). This delay is consistent with a mechanism in which new capital raises labor productivity and expands labor demand only after it has been fully incorporated into production. The aggregate ATET on employment is close to zero because these opposing forces cancel in the pooled estimate. Taken together, the dynamic evidence points to a sequenced adjustment: credit arrival triggers investment ($t = 0$), which temporarily

FIGURE 5. Dynamic Effects of BNDES Credit, Comparative Effect (ETWFE)

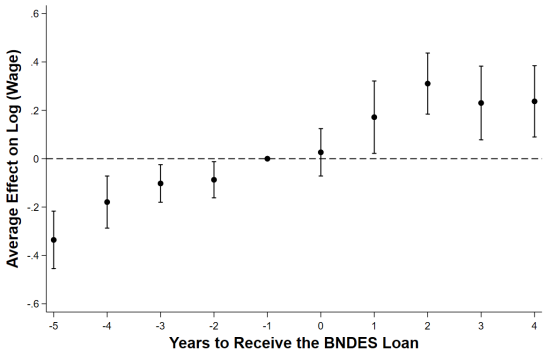


(a) Log Employment

(b) Investment Rate



(c) $\mathbb{I}\{\text{Investment} > 0\}$



(d) Log Average Wage

Notes: Cohort–calendar-time ATET estimates from the ETWFE specification (equation 3), with $t = -1$ normalized to zero. Vertical lines indicate 95% confidence intervals. The sample is the unbalanced IBGE panel (PIA/PAS/PAC); the control group is firms that never received BNDES credit during the NEM. All specifications include year fixed effects and IBGE expansion weights. Panel (d) exhibits a significant pre-treatment trend; see discussion in text.

displaces labor, before generating employment gains at longer horizons.

Average wages increased by 18.4 percent among BNDES-treated firms relative to never-treated firms, the only statistically significant aggregate effect in Table 4. Given the pre-trend documented above, however, we read this magnitude with caution: it may partly reflect differential wage growth between treated and never-treated firms that predates the NEM rather than a treatment effect of BNDES credit.

4.2. BNDES loans within-effect

Identification strategy and empirical specification

The question now is how crucial it was to gain access to BNDES loans early in the NEM period (in 2011) compared to obtaining access at a later stage. This question raises a deeper

issue. As discussed in Section 2, the macroeconomic environment in the initial year of the policy—which also marked the beginning of President Rousseff's administration—differed significantly from the economic conditions in 2014, even though the policy remained in effect.

Moreover, as highlighted by Orair and Gobetti (2017), Brazil experienced slower economic growth between 2011 and 2014 compared to the preceding five-year period, along with a sharp decline in activity, despite the implementation of the NEM's stimulus policies. Since BNDES disbursements were a central component of this broader government strategy, we examine whether the effects of the NEM exhibited a cyclical dimension—specifically, whether firms that accessed subsidized loans prior to 2013 performed better than those that received them in 2014.

Let $\tau \in 0, 1$ denote the policy indicator, such that $\tau = 1$ for any firm that received a positive amount of credit through the NEM policy from BNDES, and $\tau = 0$ otherwise. In this analysis, we compare only firms treated under the NEM with those in the last-treated group. We estimate the dynamic policy effect of BNDES credit following Sun and Abraham (2021):

$$y_{it} = \alpha_i + \gamma_t + \sum_{e \in C} \sum_{l}^{e+2} \delta_{e,l} \mathbb{1}\{E_i = e\} \tau_{i,t}^{l} + \epsilon_{it} \tag{4}$$

where α_i and γ_t are firm and time fixed effects, $C = \{2011, 2012, 2013\}$ is the set of treated cohorts, and l indexes event-time relative to treatment onset. The $\delta_{e,l}$ coefficients are the Cohort-Specific Average Treatment Effects on the Treated (CATT). We then aggregate the CATTs into the overall ATET using the interaction-weighted (IW) estimator,[11] which weights each cohort-period estimate by the share of units belonging to that cohort. Standard errors are clustered at the firm-size level.

One concern when treatment is heterogeneous is the possibility of anticipatory effects. Did firms behave strategically and alter their outcomes in anticipation of treatment? Figures B3a and B3b show the evolution of wages and employment across cohorts, and these results support the view that no anticipatory effects were present within borrowers. We also examine the value of BNDES loans to investigate whether earlier cohorts allocated a larger share of the loan to labor expenses than later ones. Figure A1 shows that the 2014 cohort had a higher wage bill as a share of total loan value. For example, the 2011 cohort had an average wage-bill-to-loan ratio of approximately 8%, while the 2014 cohort averaged 14%. All cohorts experienced a notable decline in this ratio in 2014.

[11] See Appendix C in Sun and Abraham (2021).

Results and discussion

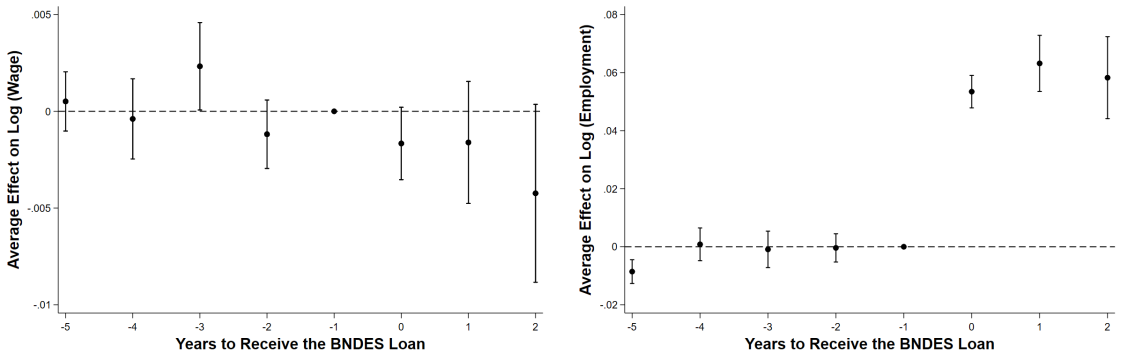
In this section we present and discuss the effects of BNDES credit operations during the NEM on firms' outcomes. Panel A of table 5 presents the average effect of BNDES credit operations on treated firms, comparing those that received subsidized loans between 2011 and 2013 to those that benefited only in 2014. Figure 6 displays the corresponding dynamic effects. Among firms that received BNDES subsidized credit during the NEM period, those treated earlier (2011–2013) hired more workers. On average, firms that received loans earlier increased employment by 5.83%. As shown in Figure 6b, pre-trends are stable, and the effect grows slightly over time. In contrast, we observe a small negative effect on wages, with treated firms paying, on average, 0.25% less to their workers. The dynamic effects in Figure 6a reveal a downward trajectory, although the confidence intervals are relatively wide.

Table 5. Earlier Access to the Subsidized Loan on Firms Labor Outcomes

	$log(Wage)$	$log(Employment)$
Panel A: Overall Effect		
$ATET$	-0.00250*	0.0583***
	(0.0015)	(0.0047)
Panel B: Effects by Firm Size		
$ATET_{Large}$	-0.0100	0.0095
	(0084)	(0.0275)
$ATET_{Medium}$	-0.0072*	0.0237*
	(0.0043)	(0.0141)
$ATET_{Small}$	0.0012	0.0648***
	(0.0027)	(0.0088)
$ATET_{Micro}$	0.0002	0.0638***
	(0.0020)	(0.0060)
Panel C: Effects by Firm Sector		
$ATET_{Manufacturing}$	-0.0013	0.0565***
	(0.0028)	(0.0088)
$ATET_{Business}$	0.0041*	0.0535***
	(0.0023)	(0.0066)
$ATET_{Service}$	-0.0080***	0.0619***
	(0.0030)	(0.0096)

Note: This table reports the results of the dynamic specification from equation 4. Panel A are the baseline estimates. Panel B breakdown the sample by firm size; Panel C breakdown the sample by firm sector. Column 1 show the estimated for average wage and column 2 for the extensive margin of labor demand. Standard errors clustered at firm-level are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. ATET is recovered by the linear combination of pos-treatment coefficients. All specifications include firm and year fixed-effects.

FIGURE 6. Dynamic Effects of BNDES Credit on Earlier Access to the Subsidized Loan



(a) Log Average Wage

(b) Log Employment

Notes: Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the firm level. Both specifications include firm and year fixed-effects.

We therefore break down our estimation by firm size and sector classification as defined by BNDES.[12] Panels B and C of table 5 show the effects by firm size and sector classification, respectively. Regarding size, only small and micro firms exhibit significant effects on employment, while no consistent effects are observed for wages. When analyzing the heterogeneous effects by broad sector we observe positive and significant effect of employment across all sectors, whereas wages varies. On manufacturing firms there is no significant effects. Firms on business sector shows a positive effect on wages, around 0.4% at 10% of significance. However, firms on service sector shows a higher, negative, and significant effect on wages. In average, firms on service that received the BNDES disbursement earlier paid 0.8% less to their workers. We report the dynamic effects for size and sector in figures B4 and B5, respectively.

We extend our within-effect analysis to firm investment behavior using the IBGE datasets. Unlike a conventional unbalanced panel, where attrition may introduce selection concerns, the missingness here is governed by a known probability sampling scheme and corrected by design. All specifications using IBGE outcomes are estimated with IBGE expansion weights, so the results are representative of the firm population within each stratum rather than merely of the observed sample.

Table 6 decomposes the investment response into five margins.[13] The first two columns capture the extensive margin: Column (1) estimates the effect on the probability that a firm

[12] Since firms may borrow from BNDES multiple times and may evolve over time, it is possible for a firm to have more than one size classification or be associated with more than one broad sector (e.g., multi-plant firms with different activities at each plant). To address this, we fix the BNDES firm size and broad sector classification to those observed at the time of the first loan.

[13] The investment rate $IR_{it} = I_{it}/K_{it} \geq 0$, where K is the total assets of the firm. A small fraction of observations (0.6% of the sample) present investment rates exceeding unity. We winsorise IK_{it} at the 99th percentile.

Table 6. Earlier Access to the Subsidized Loan on Firms Investment Behavior

	Extensive Margin		Intensive Margin		
	$\mathbb{I}\{\text{Investment}\}$		Investment Rate	IR × $\mathbb{I}\{\text{Spike or Non-spike}\}$	
	$\mathbb{I}\{\text{IR} > 0\}$	$\mathbb{I}\{\text{IR} \geq 0.2\}$	*IR*	$IR \times \mathbb{I}\{\text{Inv.} \geq 0.2\}$	$IR \times \mathbb{I}\{\text{Inv.} < 0.2\}$
	(1)	(2)	(3)	(4)	(5)
Panel A: All Firms					
ATET	0.1450*	0.1260*	0.1180**	0.1200**	-0.0001
	(0.0780)	(0.0722)	(0.0574)	(0.0573)	(0.0020)
Observations					
Panel A: Med.-Large (≥ 30 workers)					
ATET	-0.0032	0.0074	0.0061	0.0031	0.0030**
	(0.0167)	(0.0105)	(0.0041)	(0.0042)	(0.0013)
Observations					

Note: The table reports average treatment effects on the treated (ATET). Panel A uses an unbalanced panel of all firms drawn from the *Pesquisa Industrial Anual* (PIA), the *Pesquisa Anual de Serviços* (PAS), and the *Pesquisa Anual de Comércio* (PAC). Panel B restricts the sample to medium and large firms observed continuously over the period 2005-2014, forming a strongly balanced panel. The dependent variables are: $\mathbb{I}\{IR > 0\}$, a dummy equal to one if the firm reports positive investment; $\mathbb{I}\{IR \geq 0.2\}$, a dummy equal to one if the investment rate exceeds the spike threshold of 0.2; *IR*, the investment rate defined as investment divided by the capital stock, winsorised at the 99th percentile; $IR \times \mathbb{I}\{\text{Inv.} \geq 0.2\}$, the spike investment rate, set to *IR* when $IR \geq 0.2$ and zero otherwise; and $IR \times \mathbb{I}\{\text{Inv.} < 0.2\}$, the non-spike investment rate, set to *IR* when $IR < 0.2$ and zero otherwise. All specifications absorb firm and year fixed effects. Standard errors clustered at the firm level are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

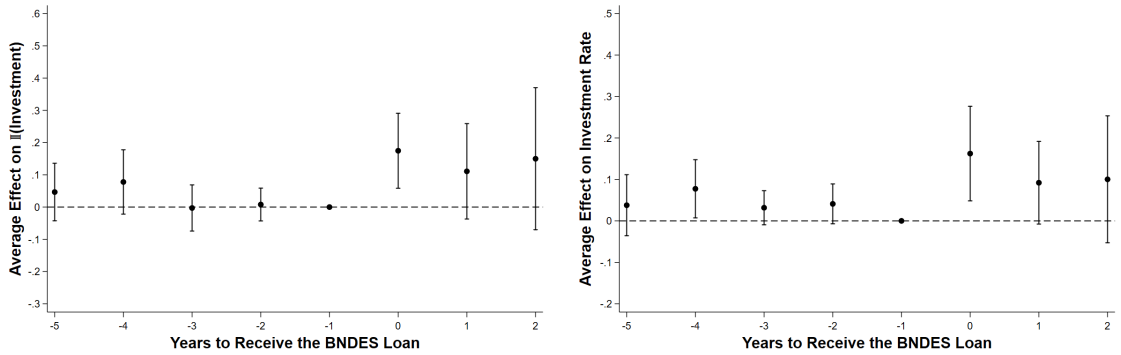
undertakes any positive investment, while Column (2) estimates the effect on the probability of a large investment episode, defined as an investment rate of at least 20% of the capital stock. Column (3) captures the overall intensive margin through the unconditional investment rate. Columns (4) and (5) further decompose the intensive margin into spike and non-spike components: Column (4) reports the spike investment rate, equal to the investment rate when $IR \geq 0.2$, isolating the contribution of lumpy investment episodes; Column (5) reports the non-spike investment rate, equal to the investment rate when $IR < 0.2$, capturing the effect on routine capital expenditure. Together, these five margins allow us to distinguish whether BNDES credit activated dormant investment projects, triggered large-scale capital accumulation episodes, or merely raised the intensity of ongoing routine investment.

Panel A, which includes all firms, shows that BNDES credit raises the probability of any positive investment by 14.5 percentage points and the probability of a large investment episode (spike, $IR \geq 0.2$) by 12.6 percentage points, both significant at the 10 percent level. The overall investment rate rises by 11.8 percentage points, entirely driven by spike episodes, which increase by 12.0 percentage points; the routine component is negligible and indistinguishable from zero. Among medium and large firms (Panel B), neither

extensive margin nor the spike component is statistically significant; the only detectable effect is a small positive response in routine investment of 0.30 percentage points ($p < 0.05$). Taken together, the two panels support a financial-constraint relaxation hypothesis: BNDES credit activated discrete, lumpy investment projects for firms that were previously capital-constrained, while larger, unconstrained firms adjusted only at the margin of ongoing routine expenditure. The positive correlation between asset deepening and financial constraint (figure 8) reinforces this interpretation.

Figure 7 plots the corresponding dynamic effects. Pre-trends are flat and statistically insignificant for both margins. At $t = 0$, both the probability of positive investment and the investment rate rise sharply and significantly, consistent with the ATET reported in Table 6; the effect fades within one to two years, consistent with a discrete capital-adjustment episode rather than a sustained increase in investment activity.

FIGURE 7. Dynamic Effects of BNDES Credit on Investment, Within-Effect



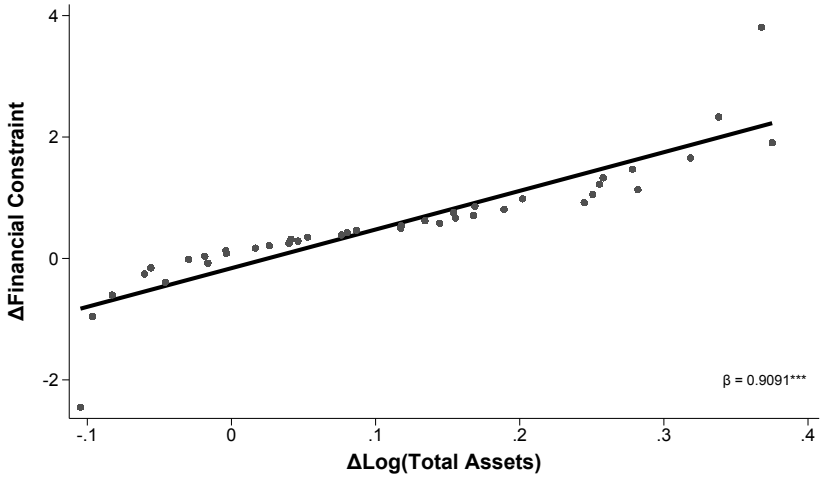
(a) $\mathbb{I}\{\text{Investment} > 0\}$ (b) Investment Rate

Notes: Dynamic estimates from the unbalanced IBGE panel (PIA/PAS/PAC), comparing firms treated earlier in the NEM (2011–2013) to the last-treated (2014) cohort, with $t = -1$ normalized to zero. Vertical lines indicate 95% confidence intervals. All specifications include firm and year fixed effects and IBGE expansion weights.

A natural question is how BNDES credit operates on firm outcomes. One possibility is that the credit itself drives adjustment — firms borrow, invest, and expand employment. A second possibility is that the effects are concentrated among firms with prior BNDES relationships, which may already have established investment plans, accumulated credit history with the bank, or benefited from earlier subsidized lending during the 2008–2010 financial crisis. Disentangling these channels requires comparing firms for whom the NEM represented a first exposure to BNDES financing against those for whom it represented a continuation of an existing relationship.

As shown in figure B2 around half of the firms received a BNDES loan for the first time during the NEM period, while one-fourth had already borrowed between 2008 and 2010. Table 7 reports results under four alternative sample restrictions designed to isolate first-

FIGURE 8. Correlation Between Firms Expansion and Financial Constraint



Note: Each dot represents the mean of a bin of approximately equal size (40 bins total), constructed by grouping firms according to their demeaned log total assets and demeaned Financial Constraint Index (FCI). The FCI follows Hadlock and Pierce (2010) (see appendix C) constructed from survey-declared revenue and CNPJ firm age, and standardized to mean zero and unit variance.; higher values indicate greater financial constraints. The solid line is an OLS fit estimated on the underlying firm-level data.

time borrowers. The *2010 Cohort* column excludes firms that also received a loan in 2010. The *Financial Crisis* column drops firms that borrowed during the NEM and also in 2008 or 2009, following Bonomo, Brito, and Martins (2015), who document that BNDES served as a counter-cyclical instrument during that episode. The *BNDES Credit Boom* column extends this restriction to 2010, where figure 1 reveals an additional spike in disbursements. Finally, the *Loan Before NEM* column drops all firms that received any BNDES loan prior to the NEM (2005–2010). Summary statistics for each restriction are reported in table B1 and illustrated in figure B6.

Table 7 confirms the baseline finding that BNDES disbursements raised employment while exerting modest downward pressure on wages. Among firms that borrowed during the NEM and also had prior BNDES relationships — whether through the Financial Crisis window, the broader 2008–2010 credit boom, or any disbursement before the NEM — employment rose between 3.74% and 5.36%, while average wages fell by 0.28% to 0.46%. The investment picture for this group is asymmetric across margins: the extensive margin ($\mathbb{1}$Investment > 0) is statistically indistinguishable from zero in all three restrictions, but the spike probability ($\mathbb{1}$Investment ≥ 0.2) is positive and significant throughout, rising by 1.31 to 1.57 percentage points. Firms with prior BNDES relationships were therefore already participating in investment markets; what credit enabled was an escalation into larger, lumpy investment episodes rather than activation of investment per se. The pattern is sharply different for firms receiving BNDES credit for the first time. The *Loan Before NEM* column shows no significant effect on employment or wages, yet both investment margins

Table 7. First Borrow and Previous Relationship as Mechanisms

	Sample Restriction			
	2010 Cohort	*Financial Crisis*	*BNDES Credit Boom*	*Loan Before NEM*
log{*Wage*}				
ATET	-0.0028*	-0.0037**	-0.0046**	0.0000
	(0.0016)	(0.0016)	(0.0017)	(0.0020)
log{*Employment*}				
ATET	0.0536***	0.0457***	0.0374***	0.0003
	(0.0049)	(0.0050)	(0.0053)	(0.0060)
$\mathbb{I}\{\text{Investment} > 0\}$				
ATET	-0.0033	-0.0020	0.0015	0.0153*
	(0.0069)	(0.0070)	(0.0076)	(0.0085)
$\mathbb{I}\{\text{Investment} \geq 0.2\}$				
ATET	0.0131***	0.0137***	0.0157***	0.0167**
	(0.0050)	(0.0051)	(0.0055)	(0.0061)

Note: Wage and Employment are estimated through a strongly balanced panel. Investment variables uses an unbalanced panel of firms drawn from the *Pesquisa Industrial Anual* (PIA), the *Pesquisa Anual de Serviços* (PAS), and the *Pesquisa Anual de Comércio* (PAC). ATET is recovered by the linear combination of post-treatment coefficients. All specifications include firm and year fixed-effects. Column *2010 Cohort* drops all firms that took out a BNDES loan in 2010. Column *Financial Crisis* drops all firms that took out a BNDES loan in 2008 or 2009. Column *BNDES Credit Boom* drops all firms that took out a BNDES loan between 2008 and 2010. Column *Loan Before NEM* drops all firms that took out a BNDES loan before 2011. Standard errors clustered at firm-level are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

are now positive and significant: credit raised the probability of any positive investment by 1.53 percentage points and the probability of an investment spike by 1.67 percentage points. For these firms, the capital constraint was binding at the extensive margin, and BNDES credit released it.

This asymmetry is consistent with two distinct mechanisms operating at different margins. Firms with established BNDES relationships were already investing; prior credit had relieved the capital constraint at the extensive margin before the NEM began. NEM disbursements allowed these firms to scale employment at the existing capital stock — depressing the average wage per worker — and to escalate into larger investment episodes, but not to initiate new investment activity. First-time borrowers, by contrast, faced a capital constraint that was still binding at the extensive margin: credit moved them from no investment to positive investment, and often directly into spike territory. Yet because capital installation precedes employment expansion, the employment effect did not materialize within the estimation window.

5. Final Remarks

This paper studies the firm-level effects of Brazil's New Economic Matrix (NEM), the most ambitious industrial policy undertaken in the country since the military era. Between 2011 and 2014, the federal government injected unprecedented resources into BNDES, the national development bank, generating approximately 328,000 disbursements, more than double the pace of the 2008-09 financial crisis period, and accounting for over 20 percent of total private sector credit. We ask whether this intervention improved firm outcomes, distinguishing between the timing of credit access within the pool of borrowers and the counterfactual of never having received BNDES credit at all.

We bring two complementary empirical strategies to bear on this question. First, we exploit staggered treatment timing within BNDES borrowers, comparing firms that accessed credit earlier in the NEM to those that accessed it later, using the interaction-weighted estimator of Sun and Abraham (2021). Second, we compare BNDES-treated firms to a matched sample of firms that never received BNDES credit during the NEM, using the extended two-way fixed effects estimator of Wooldridge (2021) alongside linear probability models for firm survival.

The results reveal a sharp duality between the two designs. Within the universe of BNDES borrowers, earlier access to credit during the NEM raised employment by 5.8 percent relative to later-treated cohorts, with effects concentrated among small and micro firms and among firms that had prior BNDES relationships. These firms appear to have made productive use of subsidized credit: the dynamic evidence from IBGE surveys shows wage gains that persist through $t = 4$, front-loaded investment that tapers off, and delayed employment gains consistent with a capital-deepening adjustment. BNDES credit also extended firm survival: treated firms were 1.1 to 1.4 percentage points less likely to exit the market, with the largest survival gains among the most financially constrained firms.

Yet when we turn to the comparative effect — how BNDES borrowers fared against firms that never received subsidized credit — the picture is more nuanced. Using the ETWFE estimator on the IBGE panel, employment shows no significant average difference: treated firms neither expanded nor contracted their workforce relative to the never-treated counterfactual in the aggregate. The dynamic evidence clarifies this average: employment was negative at $t = 0$, suggesting short-run capital-for-labor substitution, turned insignificant through $t = 3$, and became positive only at $t = 4$, suggesting that labor demand gains materialized only after the investment cycle was complete. Average wages among treated firms were also 18.4 percent higher than among never-treated firms, but the pre-treatment wage trend is not flat, and we do not treat this estimate as a central finding of the comparative-effect analysis.

This duality is not a contradiction: it reflects selection and heterogeneous adjustment. First-time BNDES borrowers, who had no prior relationship with the bank, show no signif-

icant within-effect in table 6. One consistent interpretation is that these firms substituted public for private credit without expanding real activity, yielding no immediate employment response. Repeat borrowers, already embedded in the BNDES ecosystem, did expand — but they represent the minority of the treated population, and their gains take time to appear in the labor market. Relative to firms that never received BNDES credit, the clearest gain is survival, not wages or headcount: treated firms remained in operation longer, and their employment response, when it eventually appeared, took four years to materialize. The same policy therefore tells two different stories depending on the counterfactual: a clear win within the pool of borrowers, and a far more modest, delayed gain relative to firms that never borrowed at all.

Taken together, our findings offer three contributions to the literature. For the industrial policy evaluation literature, we provide the most granular causal evidence on NEM's firm-level effects, using identification strategies that account for staggered treatment timing and selection into treatment. For the state-owned bank literature, we show that ignoring policy context — specifically the shift from crisis-era credit support to peacetime industrial policy — can produce misleading conclusions about development bank effectiveness. And for the Brazilian political economy literature, we document that the NEM's credit expansion was partly shaped by electoral incentives, which helps explain the gap between the policy's stated objectives and its measured outcomes. Industrial policy can improve firm outcomes when it reaches firms with genuine financing constraints and productive projects. The NEM reached some of those firms — but not enough of them, and not efficiently enough, to justify its scale.

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Who Benefits from Industrial Policy? Evidence from Brazil's Development Bank

Online Appendices

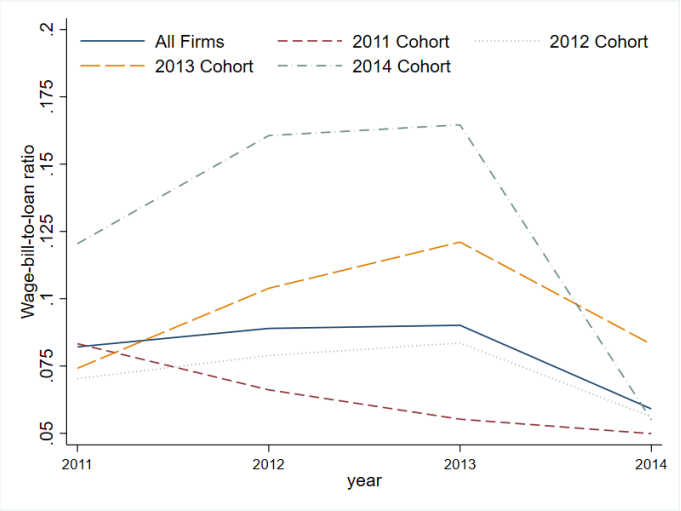
Gustavo Araujo, Pedro Ferreira, Vitor Santos

September 11, 2026

A. Summary Statistics 1
B. Empirics 2
C. Financial Constraint Index at Firm-Level 10

Appendix A. Summary Statistics

FIGURE A1. Wage-bill-BNDES-loan ratio by Cohort



Appendix B. Empirics

Table B1. OLS Estimates on BNDES Treatment Assignment

	(1)	(2)
Panel A: Firm characteristics		
r Number of workers	-0.1774***	-0.1770***
r^2 Number of workers	0.1041***	0.1035***
r Firm age	-0.0345**	-0.0336**
r^2 Firm age	0.0177	0.0166
r Average education	0.1210***	0.1215***
r^2 Average education	-0.0669***	-0.0675***
Subsidy Dispersion Index	0.1170**	0.1678***
$\mathbb{I}\{\text{Simples}\}$	0.0034	0.0034
Panel B: Sector characteristics		
$\mathbb{I}\{\text{Manufacturing}\} \times r$ Number of workers	-0.2047***	-0.2048***
$\mathbb{I}\{\text{Business}\} \times r$ Number of workers	-0.0562***	-0.0563***
Panel C: Firm size dummies		
$\mathbb{I}\{\text{Medium firm}\}$	-0.0439***	-0.0439***
$\mathbb{I}\{\text{Small firm}\}$	0.1895***	0.1893***
$\mathbb{I}\{\text{Micro firm}\}$	0.0670***	0.0669***
$\mathbb{I}\{\text{Medium}\} \times r$ Number of workers	-0.0024	-0.0023
$\mathbb{I}\{\text{Small}\} \times r$ Number of workers	-0.2385***	-0.2389***
$\mathbb{I}\{\text{Micro}\} \times r$ Number of workers	0.0169	0.0178
Panel D: Trade exposure		
$\mathbb{I}\{\text{Exported}\}$	-0.0019	-0.0026
$\mathbb{I}\{\text{Imported}\}$	0.0545***	0.0547***
Panel E: Political connections		
$\mathbb{I}\{\text{Political Donation in 2008}\}$	0.0596***	0.0598***
$\mathbb{I}\{\text{Political Donation in 2010}\}$	0.0742***	0.0746***
Credit Share $\times \mathbb{I}\{\text{Party Alignment}\}$	0.0157	0.0279
Credit Share $\times \mathbb{I}\{\text{Coalition Alignment}\}$	0.9804***	0.9492***
Panel F: Banking		
Financial Constraint Index	0.0117***	0.0117***
r Stock of Total Debt	0.1032**	0.1033**
r^2 Stock of Total Debt	-0.5613***	-0.5621***
$\mathbb{I}\{\text{Private Credit}\}$	-0.0790***	-0.0791***
Observations	140,283	140,330
R^2	0.26	0.26
Political Alignment	2011-2012	2013-2014

Note: This table reports results from OLS estimates of the probability of receiving BNDES support. Variables prefixed by r denote ranking-based firm-level characteristics, and r^2 denotes their square. *Simples* indicates whether a firm belongs to the simplified tax system. *Manufacturing* and *Service* are dummies for the firm's sector. *Imported* and *Exported* indicate whether a firm ever imported or exported in 2010 or earlier. *Political Donation* indicates whether a firm made any donation to a party, candidate, or committee in the 2008 or 2010 elections. *Credit Share* is the percentage of credit received by each firm within its sector–city cluster. *Party Alignment* indicates whether the party in power in city c is the same as the party of the federal government. *Coalition Alignment* indicates whether the party in power in city c belongs to the same coalition as the federal government's party. *Financial Constraint Index* follows Hadlock and Pierce (2010) (see appendix C), constructed from SCR-declared revenue and CNPJ firm age, and standardized to mean zero and unit variance. *Stock of Total Debt* is the rank-normalized stock of private bank debt in 2010. *Private Credit* indicates whether the firm held any private bank credit prior to 2011. All specifications include industry–state fixed effects. Standard errors are clustered at the industry–state level. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2. Correlation Coalition Dummies

	$\mathbb{I}\{\textit{Colation}\ 2009-2010\}$	$\mathbb{I}\{\textit{Colation}\ 2011-2012\}$	$\mathbb{I}\{\textit{Colation}\ 2013-2014\}$
$\mathbb{I}\{\textit{Colation}\ 2009-2010\}$	1.0000		
$\mathbb{I}\{\textit{Colation}\ 2011-2012\}$	0.4067	1.0000	
$\mathbb{I}\{\textit{Colation}\ 2013-2014\}$	0.0750	0.1519	1.0000

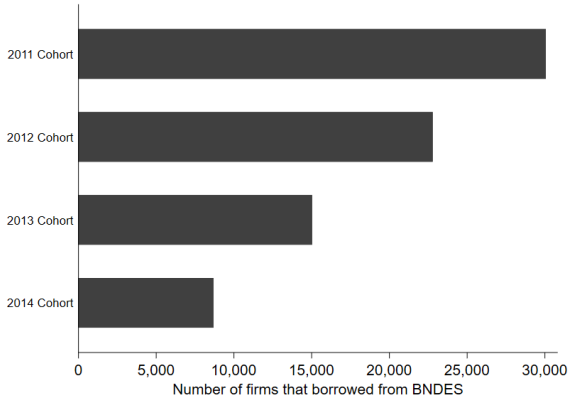
Note: The table reports pairwise Pearson correlation coefficients between coalition dummies at city level from Figueiredo et al. (2025).

Table B3. Firm Exit and BNDES Credit Intensity

	Benchmark			**FCI Heterogeneity**					
				Top 25%			*Bottom 25%*		
Outcome: $\mathbb{I}\{\text{Exit}\}$	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)
asinh(Cumulative NEM Credit)	-0.00052^{***}			n.r.			n.r.		
	(0.00003)								
asinh(Outstanding NEM Credit)		n.r.			n.r.			n.r.	
asinh(First NEM Loan) × $\mathbb{I}\{\text{Post NEM}\}$			-0.00069^{***}			n.r.			n.r.
			(0.00003)						
Mean of $\mathbb{I}\{\text{Exit}\}$	0.0122	0.0122	0.0122	0.0122	0.0122	0.0122	0.0122	0.0122	0.0122
R^2	0.0081	0.0081	0.0082	0.0078	0.0078	0.0079	0.0079	0.0079	0.0079
Observations	1,296,024	1,296,024	1,296,020	1,296,024	1,296,024	1,296,020	1,296,024	1,296,024	1,296,020

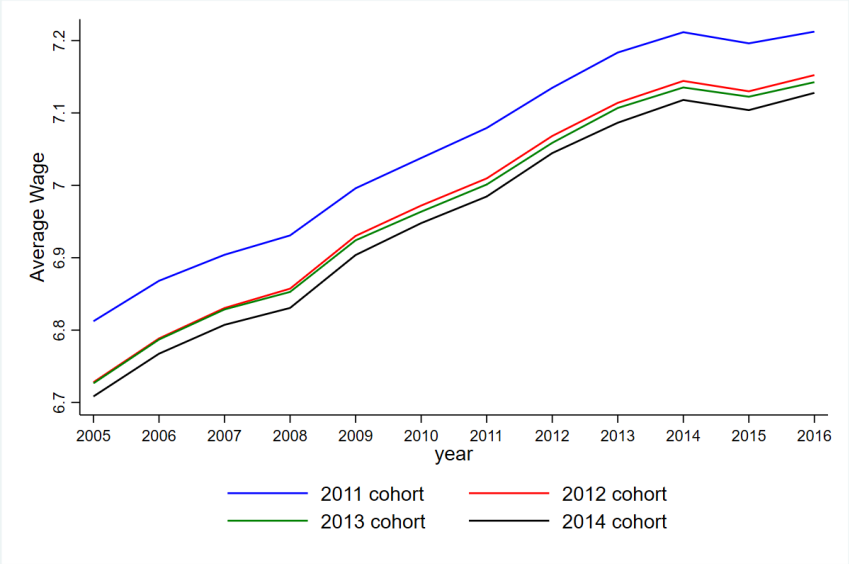
Note: The table reports OLS estimates of the probability of firm exit using firm-year data from 2005 to 2019, replacing the binary treatment indicators of the main text with continuous measures of BNDES credit intensity. *Cumulative NEM Credit* is the running sum of BNDES disbursements received during the NEM (2011–2014), in constant reais. *Outstanding NEM Credit* approximates the outstanding balance of NEM credit through a perpetual-inventory rule with a 20 percent annual amortization rate. *First NEM Loan* is the size of the firm's first BNDES disbursement during the NEM, fixed at treatment entry. All three measures are set to zero for firms that never received a BNDES loan during the NEM and enter as inverse hyperbolic sines. $\mathbb{I}\{\text{Post NEM}\}$ equals one for years 2015 and beyond. Columns (IV)–(IX) interact the dose measure with indicators for the top and bottom quartiles of the Financial Constraint Index (FCI), respectively; higher FCI values indicate greater financial constraints. Controls include the baseline hazard, the subsidy dispersion index, the standardized FCI, log employment, an indicator for private bank credit, and the log stocks of commercial-bank and BNDES debt. Fixed effects are year and industry-state. Clustered standard errors at industry-state level in parentheses. Significance levels: $^{*}\,p<0.1$, $^{**}\,p<0.05$, $^{***}\,p<0.01$.

FIGURE B1. Treatment Heterogeneity

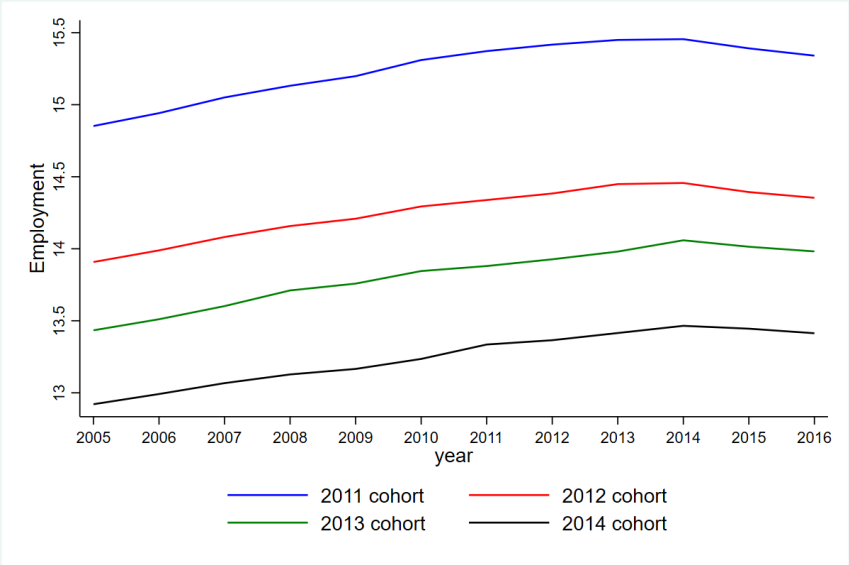


Notes: This figure shows the number of firms that took a BNDES loan during the NEM by cohort. The number of firms is calculated based on the final sample used to estimate equation 1. Strong balanced panel on time and covariates.

FIGURE B2. Antecipatory Behavior Check



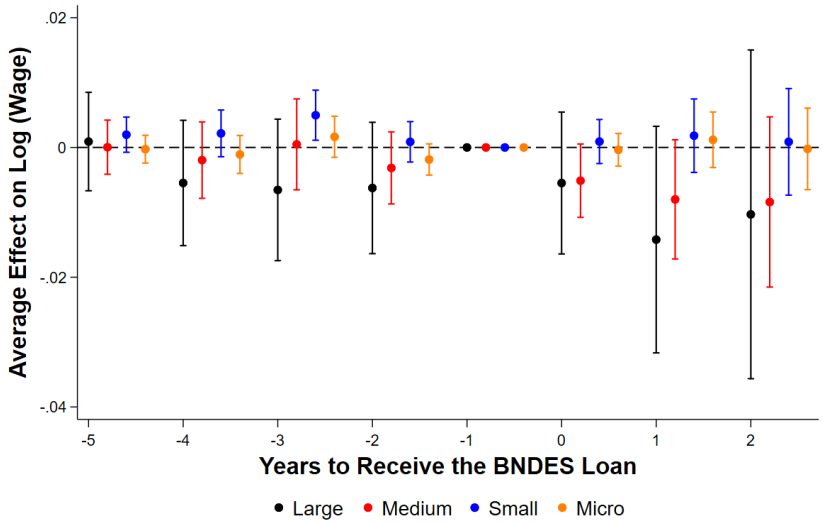
(a) Log Average Wage



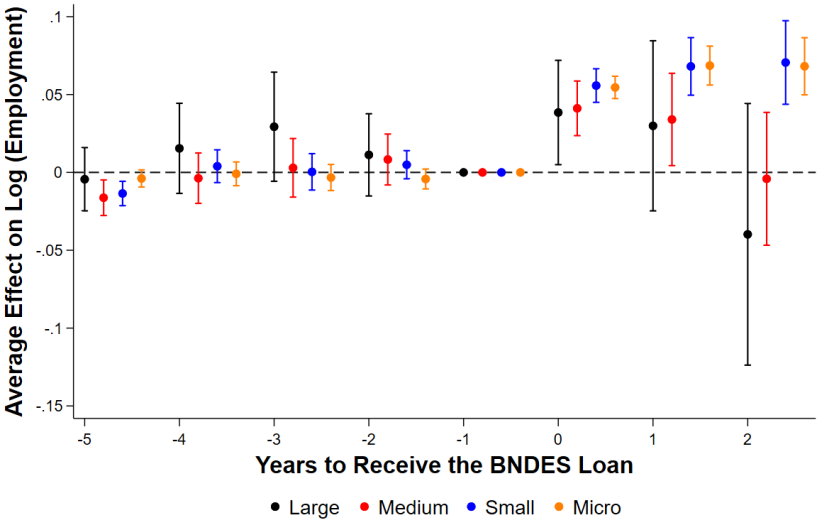
(b) Log Employment

Notes: All variables are in logarithms.

FIGURE B3. Hetogeneity Dynamic Effects by Firm Size (BNDES Classification) - *Within-Effects*



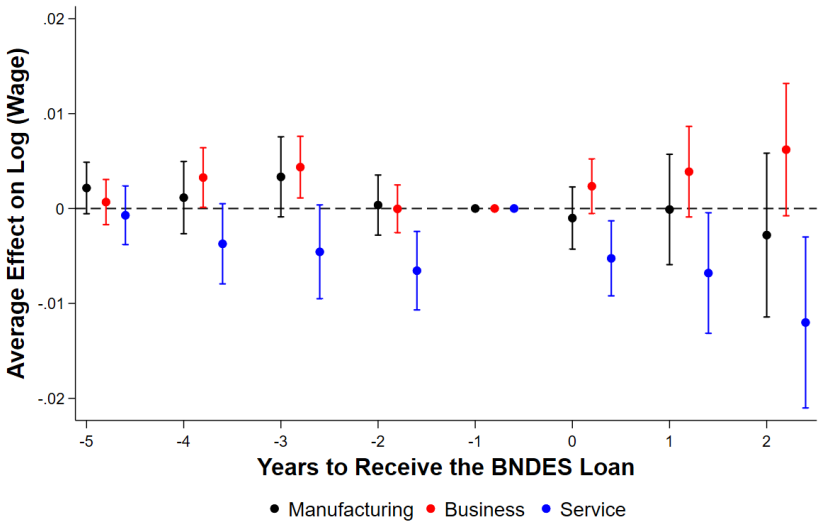
(a) Log Average Wage



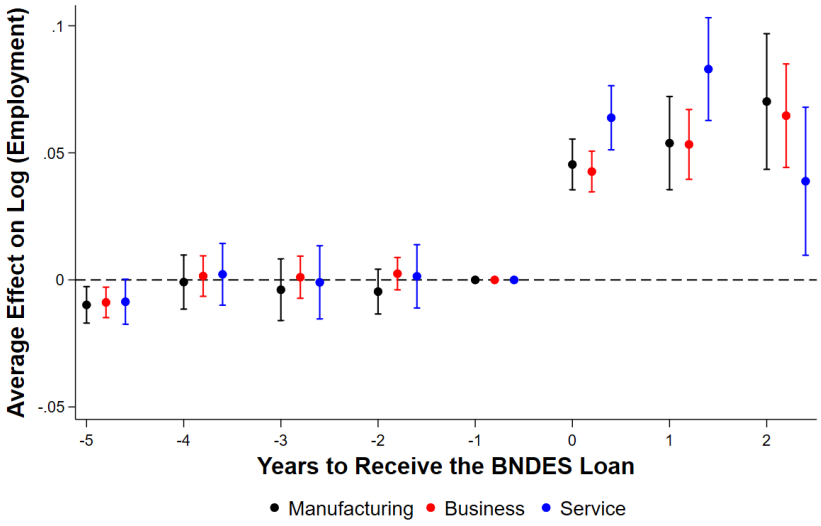
(b) Log Employment

Notes: Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the firm level. Both specifications include firm and year fixed-effects.

FIGURE B4. Hetogeneity Dynamic Effects by Firm Sector - *Within-Effect*



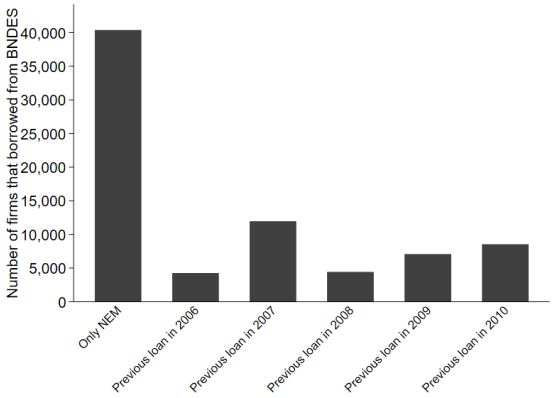
(a) Log Average Wage



(b) Log Employment

Notes: Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the firm level. Both specifications include firm and year fixed-effects.

FIGURE B5. Previous loan distribution given BNDES loan during



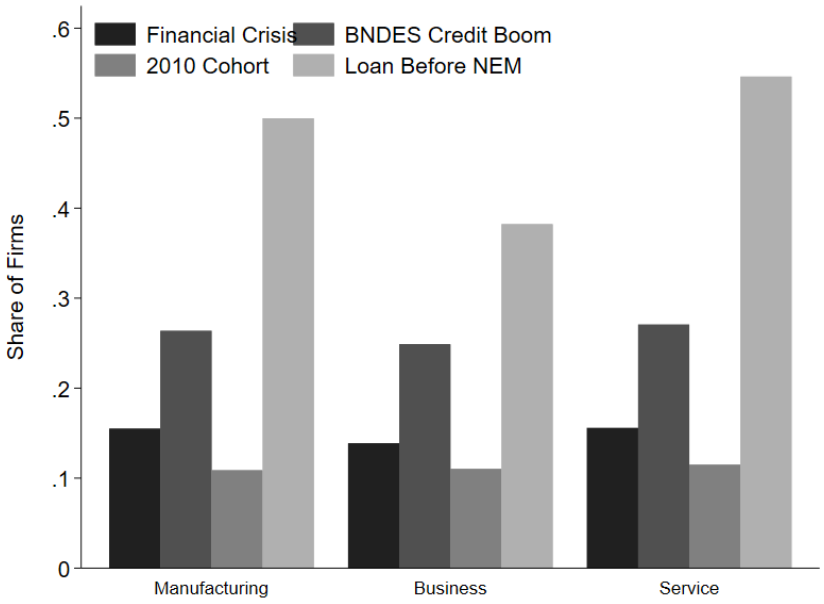
Notes: This figure shows the number of firms that get a loan from BNDES during NEM but also received disbursement previously. The number of firms is calculated based on the final sample used to estimate equation 1. Strong balanced panel on time and covariates.

Table B4. Previous loan Statistics

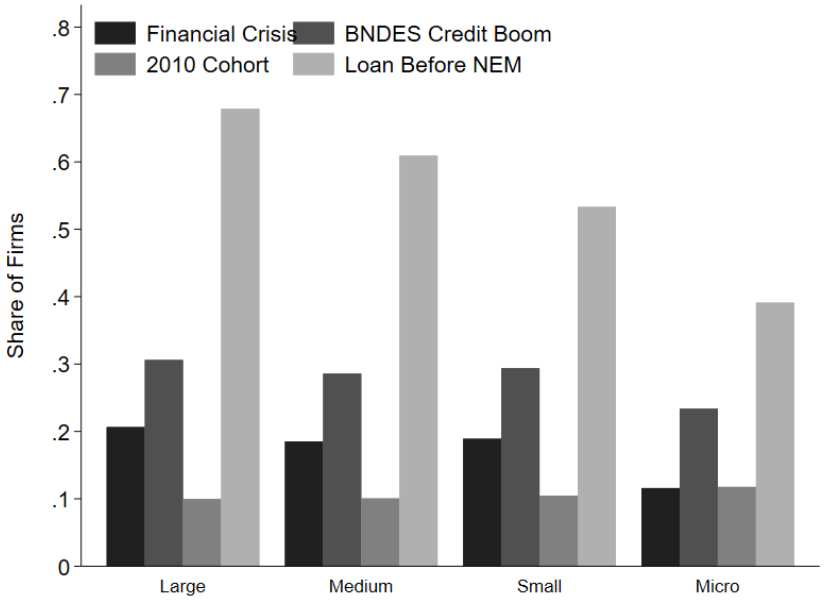
	Number of Observations	*% Sample*	*Number of Firms*
Full Sample	918,696	100%	76,558
Sample Restriction			
2010 Cohort	102,300	11.13%	8,525
Financial Crisis	137,604	14.98%	11,467
BNDES Credit Boom	239,904	26.11%	19,992
Loan Before NEM	434,220	47.26%	36,185

Note: The statistics in the table are calculated based on the sample regression given a strongly balanced panel at firm level for firms with information from 2005 to 2014. *2010 Cohort* are the firms that took out a BNDES loan in 2010. *Financial Crisis* are the firms that took out a BNDES loan in 2008 or 2009. *BNDES Credit Boom* are the firms that took out a BNDES loan between 2008 and 2010. *Loan Before NEM* are the firms that took out a BNDES loan before 2011.

FIGURE B6. Distribution of sample loss given the sample selection and firm characteristic



(a) Sample loss by sample selection and sector



(b) Sample loss by sample selection and firm size

Appendix C. Financial Constraint Index at Firm-Level

The finance literature on financial constraints and on how to measure it is quite extensive. There are few attempts to provide a better measure of financial constraint *at* firm level. As highlighted by Hadlock and Pierce (2010), firm size and age are good predictors of the level of financial constraint faced by a firm. They suggests a more direct way to estimate the effect by plugging these firms characteristics into the equation of *SA Index*. There are alternatives approaches, such as Whited and Wu (2006) or Lamont, Polk, and Saá-Requejo (2001), for which Hadlock and Pierce (2010) already show the straight of the *SA Index* over last two approaches.

We follow Hadlock and Pierce (2010) and built a slightly modified version of the *SA Index* by changing our size proxy. Instead proxy size with firm's book asset we use firms total sales. Even with the Dang, Li, and Yang (2018) argument that firms size proxies may be sensitive to the environment that they are being used, we judge in our present case that raw total sales appears as a good alternative proxy for firm size, even because is one of the BNDES criteria for classification. We calculate the financial constraint of firm i at time t as

$$FC_{i,t} = -0.737 * size_{it} + 0.043 * size_{it}^2 - 0.040 * age_{it} \tag{C1}$$

where size is the total sales and age is measured by that date that the firm opened their first baking account.

Many firms supported by BNDES lack information on book assets because they are micro, small, and medium-sized enterprises (MSMEs), which are not required to report balance-sheet information since they are not publicly traded. The available datasets covering a substantial share of Brazilian firms cannot be linked to one another. As highlighted in Section 3, it is not possible to merge the IBGE dataset with the BACEN data. However, the BACEN data can be merged with RAIS, making the *FC* measure calculated using SCR the preferred approach, given its higher match rate with our labor data. Since the IBGE datasets also include firms' annual gross total sales, we also calculate *FC* for our sample using PIA, PAS, and PAC.

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