Targeting Industrial Policy - Evidence from Brazil

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Abstract

I study the impact of industrial policy in Brazil on firms and on external economies of scale (EES). Using data covering the universe of firm-level subsidized credit during the 2004–2014 period and a novel instrumental variable approach, I find that subsidized credit generated, at best, modest gains in sales, which are partially explained by increases in profits. I develop a novel method to identify the level at which EES operate and find that they are driven primarily by industry-specific rather than local components. I construct a multi-industry, multi-region model with heterogeneous firms that rationalizes these findings. The model suggests that industrial policy should target firms that contribute most to EES within industry–region pairs that exhibit the greatest potential for EES growth. A sufficient-statistics approach allows me to assess whether the implemented industrial policy deviates from the optimal allocation across firms, industries, and locations. I then examine whether a horizontal policy would have performed better.

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Os dados utilizados no presente trabalho são da Pesquisa PIA-Empresa, referente ao ano 1996-2021, realizada pelo IBGE e foram obtidos mediante ingresso autorizado à sala de acesso a dados restritos da instituição. Os resultados, análises e interpretações apresentados são de responsabilidade única do autor, não representando a visão oficial do IBGE, nem se constituindo em estatística oficial. This manuscript was prepared with the assistance of artificial intelligence tools, which were used to support language refinement and organization; all intellectual content was generated and verified by the author.

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JEL Codes: L52, O14, O25

1 Introduction

Recent interest in industrial policy has been driven by the perception that its use has been increasing (Van Reenen, 2023; Evenett et al., 2024).[1] This renewed interest is driven, at least in part, by the strong opposition to industrial policy among many economists, often summarized by the argument that "governments cannot pick winners" (Juhasz, Lane, and Rodrik, 2023). Indeed, targeting winners is challenging in practice, even when it is theoretically justified. The crux of the argument lies in whether external economies of scale (EES) arise primarily within industries or regions, and which firms contribute most to the generation of EES. Given that targeting is potentially the most central factor in the success of industrial policy, surprisingly little is known about how governments can optimally target such policies.

To assess the targeting of industrial policy and its relationship with the level of EES, I focus on the revival of large-scale industrial strategies implemented by the *Partido dos Trabalhadores* (PT, Workers' Party) governments in Brazil during 2004–2014. The Brazilian Development Bank, *Banco Nacional de Desenvolvimento Econômico e Social* (BNDES), served as the principal instrument of these industrial policies by channeling subsidized credit to finance productivity-enhancing investments.[2] I study the question whether the BNDES' industrial policy targeting was optimal through a combination of empirical analysis and theoretical modeling. To empirically examine whether firms receiving industrial policy support outperformed unsupported firms, I exploit detailed information on firms' cost structures to construct a source of exogenous variation based on physical depreciation from wear and tear. I use this variation to predict firms' propensity to self-select into BNDES' subsidized credit. Brazil provides an ideal setting for analyzing the level of EES, as its economic geography exhibits substantial variation in both within-industry and across-industry agglomeration patterns. This variation allows me to develop a structural estimation framework that decomposes EES into industry- and region-specific components.

To study counterfactual targeting strategies, I develop a multi-industry, multi-region model with heterogeneous firms that rationalizes the empirical findings and implies that industrial policy should target firms with the largest contributions to EES in industry–region pairs with the greatest scope for EES growth. To take the model to the data, I develop a sufficient-statistic approach to evaluate whether a counterfactual horizontal policy (i.e., no targeting at all) would have outperformed the observed targeting strategy.

[1] Closer inspection reveals that, although industrial policy has recently intensified, it has remained an active policy tool at least since 2009 (Juhasz, Lane, Oehlsen, et al., 2022).

[2] The annual BNDES reports published around the launch of the respective industrial strategies highlight the bank's central role in the implementation of Brazilian industrial policy (BNDES, 2005; 2009; 2012).

Understanding the Brazilian experience of industrial policy is important, as Brazil experienced "premature deindustralisation" at the time (Rodrik, 2015). Indeed, back then, Brazil had already spent four decades as a "middle-income-trapped country" (Felipe et al., 2012). Consequently, the economic conditions of many contemporary emerging economies are arguably more comparable to those of Brazil at the time of the reintroduction of large-scale industrial policy than to those of the industrial policy "miracles" of South Korea, Taiwan, or China (Felipe et al., 2012), thereby strengthening the external validity of this case study.

This paper makes three contributions to the literature on industrial policy. First, I develop a novel instrumental variable (IV) strategy to identify the impact of the expansion of the most significant subsidized loans directed at private-sector investment—namely, non-automatically approved BNDES credit,[3] which accounts for roughly half of the Bank's investment finance disbursements—on firm performance using firm-level data. To the best of my knowledge, this represents the first attempt to establish causal evidence on the impact of a firm-specific industrial policy on firm performance in the absence of exogenous geographic variation for identification. The setting is particularly relevant: 60% of industrial policies are firm-specific and 95% are implemented at the national level (Juhasz, Lane, Oehlsen, et al., 2022), making geographic variation unsuitable for identification.

Second, I extend the procedure in Reshef and Tepper (2026) to identify the industry- and location-specific components of EES in a theory-consistent manner. To that end, I estimate key parameters of an externality production function that indicate whether there are increasing, constant, or diminishing returns to scale in the production of externalities at the industry or location level. This, in turn, not only indicates whether EES are primarily industry- or location-specific, but also whether it is more important for policymakers to target industries or locations effectively for industrial policy support.

Third, I develop a multi-industry, multi-region general equilibrium framework with heterogeneous firms and markups, incorporating industry- and location-specific EES, to rationalize the reduced-form results. This framework allows me to run counterfactual exercises to understand the distributional consequences for industrial policy outcomes of the level (industry vs. location) at which EES occur. Furthermore, the model highlights a novel approach to the classic question of which firms industrial policy should target: those that contribute most to EES.

My research design exploits key features of non-automatic BNDES subsidized credit—

[3] I will often use the term *non-automatically approved BNDES credit*, *BNDES subsidized credit*, *BNDES credit* and *BNDES loans* interchangeably.

large operations approved through extensive negotiations with the BNDES rather than automated procedures for small loans—together with detailed firm-level data on repair expenditures from the *Instituto Brasileiro de Geografia e Estatística's (IBGE)* annual manufacturing survey, *Pesquisa Industrial Anual Empresa* (PIA Empresa), BNDES records covering the universe of subsidized credit operations, and insights from the maintenance management literature. This approach is necessary because identifying the effect of the expansion of non-automatically approved BNDES credit requires addressing endogeneity arising from (1) firms' investment decisions and (2) firms' self-selection into subsidized credit. To address endogeneity arising from firms' investment decisions, I exploit the fact that the deterioration and failure processes underlying firm-level depreciation are inherently stochastic, as established in the maintenance management literature (Dekker, 1996; A. Garg and Deshmukh, 2006). Accordingly, depreciation shocks provide a source of random variation rooted in the physical characteristics of a firm's production process, and deviations from the pre-treatment period mean depreciation rate can serve as an exogenous predictor of the timing of firm's investment. To distinguish firms that opt into subsidized credit from those that do not among investing firms, I exploit the fact that non-automatic approval of the respective BNDES credit is preceded by a two-step negotiation over operational, financial, risk, and legal details between the firm and BNDES, which effectively represents a substantial fixed cost. To avoid bias stemming from firms' optimization behavior requires lagging depreciation shocks by at least one period; otherwise, a simultaneity problem arises between firm output and depreciation shocks. To formalize the exclusion restriction in my instrumental variable strategy, I augment the model of Olley and Pakes (1996), introducing depreciation, subsidized credit, and the associated fixed cost to characterize which firms are likely to take up subsidized credit under assumptions that are standard in the industrial organization literature and beyond. Since depreciation shocks cannot be directly observed, I exploit a second insight from the maintenance management literature. Firms can respond to depreciation shocks either by repairing the affected capital or, if the shock is sufficiently large, by replacing it entirely.[4][5][6] Consequently, zero repair expenditure for a "one-machine" firm and low repair expenditure for "multi-machine" firms signal the anticipation of an imminent large depreciation shock. Finally, I observe the direct costs associated with the maintenance

[4]Concerning replacement decisions, firms frequently rely on a version of the "50% rule," whereby an asset is repaired only if the repair cost is less than half the cost of replacement. Variations of the "50% rule" are widely applied across industries (D.A.U., 2025).

[5]A simple way to illustrate firms' repair behavior is the familiar metaphor of a car: over its life cycle, repair costs rise with age and accumulated depreciation (Hulten and Wykoff, 1980), until a sufficiently large depreciation shock prompts replacement, at which point repair costs drop to zero. To avoid costly downtime and ensure smooth operation (i.e., the family has a functioning car), the owner replaces the car just before the depreciation shock realizes.

[6]*Preventive maintenance management*—the practice of replacing assets before failure occurs to avoid costly downtime and direct output losses—has become standard in manufacturing (Jardine and Tsang, 2013).

and repair of machines and equipment, and use these to calculate mean deviations from the pre-treatment period repair "rate" (normalized by capital) as a proxy for deviations from the depreciation rate, which simultaneously serves as my instrumental variable.

The main empirical finding from the reduced-form analysis is that firms' sales expand at best modestly after the take-up of non-automatically approved BNDES subsidized credit. This can be partially explained by suggestive evidence that take-up led to increases in profits. Nevertheless, investment increases slightly, largely for mechanical reasons, a finding that confirms results for a related set of BNDES subsidized credit in Machado et al. (2018). In contrast, there is no evidence that firms engage in production upgrading, a finding consistent with results for a related set of BNDES subsidized credit in Lage de Sousa and Ottaviano (2018), even though the capital–labor ratio also rises largely mechanically. Moreover, there is no evidence that the treatment effect is stronger for credit-constrained firms, nor that BNDES support was primarily directed toward zombie firms. Interestingly, there is suggestive evidence that employment — particularly production employment — increases, at least temporarily, following credit take-up.
The effects described here are not homogeneous across industries; sales growth appears to have been significantly stronger for firms in pulp and paper, pharmaceuticals, metallurgy, and especially in the wood products and motor vehicles industries. Overall, the empirical results corroborate previous findings, including by Criscuolo et al. (2019), suggesting that small firms are more suitable targets for industrial policy support than large firms.

I find EES in the studied context to be rather industry than location specific. Overall, returns to scale in the externality production function are estimated to be below one, suggesting that smaller but highly specialized industry–location pairs — such as clusters outside the main industrial hubs like São Paulo — may constitute the best targets for industrial policy.
Overall, from an empirical, reduced-form perspective, the expansion of non-automatically approved subsidized credit at best only partially achieved its intended objective of increasing firm productivity and enhancing competitiveness. The reduced-form results suggest that the long-run costs of the industrial policy likely outweighed its benefits. This aligns with contemporary observations by economic commentators, who identified the PBM in particular as one of the major contributors to the Brazilian crisis of 2014–2017.[7]

I then develop a general equilibrium framework to assess the aggregate impact of the expansion of non-automatically subsidized BNDES credit and to examine the macro margin of industrial policy discussed earlier. I combine elements from Atkeson and Burstein (2008) to model markups and from Gaubert (2018) to model location choices, construct-

[7]For a discussion of the reasons behind the Brazilian crisis of 2014-2017, see Barbosa (2017).

ing a general-equilibrium, multi-sector, closed-economy model with heterogeneous firms to analyze the equilibrium level of EES and the effectiveness of industrial policy. To test the model's predictions, I develop a sufficient-statistics approach.

Since Marshall (1920), the major theoretical argument in economics behind industrial policy is the existence of EES.[8] Most economic studies assume EES to happen either at the industry or the location level. On the one hand, in the tradition of Marshall (1920), Arrow (1962), and Romer (1986), economists defined such externalities as industry-specific. On the other hand, studies in the tradition of Jacobs (1969), Henderson (1974), Ahlfeldt et al. (2015), and Gaubert (2018) explain EES as location-specific. The starting point of my model builds on the observation that common approaches modeling EES as a sector-level scale elasticity—ergo inferred from trade flows, as in Bartelme, Costinot, et al. (2024)—do not permit an empirical analysis of firm heterogeneity or location-specific EES within a country, since the trade-flow data required for calibration are generally available only at an aggregate level. Therefore, I categorize EES-generating mechanisms as industry- or location-specific and, following the terminology of Greenwald and Stiglitz (1986), aggregate technological and pecuniary externalities, assuming that they are "produced" at the industry–region level by the corresponding entity—i.e., industries and locations—proxied by their respective labor force. These externalities constitute one component of productivity (alongside a firm-internal component), and EES capture the elasticity of externalities with respect to an entity's labor force.
The government—modeled as a social planner—recognizes the presence of EES at both the industry and location levels and takes the initial distribution of economic activity as given. There is a single time period, and heterogeneous firms each belong to one of multiple sectors and one of multiple regions, but do not internalize EES. This prevents firms from operating at their optimal scale, as there are complementarities in production: when firm f expands its operations, all firms in f's industry (and region) become more productive. The government can implement so-called "business-enhancing" policies, targeting all regions and industries equally. On the other hand, the government can implement industrial policy, redistributing tax revenue in the form of subsidies to specific firms in selected industries or regions to more effectively leverage EES. In doing so, the government relies on its knowledge of industry- and location-specific EES to optimally leverage these externalities, producing an equilibrium superior to the laissez-faire outcome. In response to subsidies, firms adjust output and markups depending on their market share: small firms primarily channel the subsidies into output, whereas large firms distribute the subsidies across both output and markups.

The optimal design of industrial policy must address two margins: the "macro" margin,

[8] Industrial policy can also be used to overcome coordination failures; see T. Garg (2025).

which determines the industries or regions to support, and the "micro" margin, which determines the type of firms (large versus small) to target. On the macro margin, a sufficient statistic for evaluating the quality of the government's industrial policy design is the Spearman rank correlation coefficient between the ranking of the government's industry–region priorities and the ranking of industry–region pairs by their EES magnitude. Accordingly, the government may implement industrial policy that performs better than horizontal policies (with ρ close to 1) or worse than horizontal policies (with ρ close to -1). On the micro margin, the average size of subsidized firms serves as the sufficient statistic. Governments face the following trade-off: large firms contribute disproportionately to EES (as they account for the largest share of the entity's labor force) but respond to subsidies by increasing output less and markups more due to their market power, whereas the opposite holds for small firms.

The counterfactual analysis suggests that a horizontal policy design would have outperformed the BNDES's industrial policy targeting strategy. Along the "macro" margin, the industry–location specificity of EES estimates and decreasing returns to scale in the externality production function imply that the optimal policy would target large industries in relatively small regions, that is, highly specialized clusters. Although the counterfactual analysis suggests that the BNDES targeting strategy only marginally outperformed a horizontal policy design along the "macro" margin, its performance remained substantially below the optimum. Along the "micro" margin, the optimal counterfactual policy would have targeted, within each industry–region pair, the firms with the greatest potential to create employment. Importantly, the BNDES targeting strategy appears particularly ineffective along the "micro" margin, where the counterfactual horizontal policy design would have substantially outperformed the observed allocation of support.

This paper contributes to three related literatures: an empirical literature on industry-specific industrial policies, a literature on place-based industrial policies, and a quantitative literature on industrial and trade policy in general equilibrium.

Recent work has documented the potential welfare gains from industry-specific industrial policy.[9] This has generated a rapidly growing literature (Juhasz, Lane, and Rodrik, 2023) and informed policy debates (Fernandes and Reed, 2026).

Most empirical studies exploit geographic variation for identification, although 95% of industrial policies are implemented nationally and 60% are firm-specific (Juhasz, Lane, Oehlsen, et al., 2022). Moreover, regression discontinuity designs often require data on unsuccessful applicants that are typically unavailable. Thus, I provide a new identification strategy for the majority of real-world industrial policies.

Successful case studies include the Napoleonic blockade, Chinese shipbuilding, and South

[9]For a review of arguments for industrial policy in the development context, see Harrison and Rodríguez-Clare (2010).

Korea's Heavy and Chemical Industry drive. Juhasz (2018) provide evidence for the infant-industry argument,[10] Kalouptsidi (2018) show that subsidies contributed to the rise of Chinese shipbuilding, while Lane (2022) and Choi and Levchenko (2025) document long-run gains from the Korean HCI drive. In contrast, I provide a large-scale case in which industrial policy appears unsuccessful and identify targeting based on EES as a potential mechanism explaining heterogeneous policy outcomes.

Existing proposals emphasize targeting (1) exporters (Reed, 2024), (2) small firms (Criscuolo et al., 2019), (3) upstream sectors (Liu, 2020; Lane, 2022), (4) investment-good sectors (Buera and Trachter, 2024),[11] (5) firms in large agglomerations (Gaubert, 2018), and (6) firms in EES sectors (Bartelme, Costinot, et al., 2024).[12] My framework encompasses these strategies by targeting firms, sectors, or regions with the greatest contribution to, or potential for growth in, EES.

Given that future employment growth is expected to occur primarily in services (Rodrik and Stiglitz, 2024), industrial policy increasingly extends beyond manufacturing (Rodrik and Sandhu, 2024). Consistent with Manelici and Pantea (2021), who document employment gains in the Romanian IT sector, I argue that industrial policy should account not only for sectoral employment but also for its geographic distribution because EES generate local spillovers. My findings on the limited effectiveness of BNDES capital subsidies also align with Cai and Harrison (2021), who show that capital subsidies increased capital intensity without improving productivity.

For BNDES specifically, my results partially confirm Lazzarini et al. (2015), who find that subsidized credit largely substituted for alternative financing. As in DeNegri et al. (2011) and Lage de Sousa and Ottaviano (2018), I find no productivity gains following BNDES support, contrasting with Cavalcanti and Vaz (2017), who document positive effects for small and medium-sized firms.

My work also relates to the literature on place-based industrial policies, closely connected to agglomeration economies and reviewed by Neumark and Simpson (2015). While agglomeration effects resemble the location-specific component of EES, my results suggest that EES are driven primarily by industry-specific rather than location-specific factors. Kline and Moretti (2014) find persistent manufacturing gains from the Tennessee Valley Authority, consistent with my result that industrial policy should target regions with relatively small manufacturing bases. Cingano et al. (2022) document large investment and employment effects of Italian investment subsidies, whereas I find more modest effects for Brazil, highlighting the importance of institutional design. Atalay et al. (2023) show

[10] For a theoretical formalization of the infant-industry argument, see Greenwald and Stiglitz (2006).

[11] In Buera and Trachter (2024), development multipliers depend on the adoption of modern technologies, making technology adoption central for industrial policy effectiveness.

[12] Pons-Benaiges (2017) finds that industrial policy targeting EES sectors can explain Japan's strong post-war growth.

that migration and input–output linkages limit the long-run regional effects of Turkish place-based subsidies; although my analysis abstracts from these linkages, they likely represent an important transmission mechanism.
Finally, Rosenthal and Strange (2004) and Audretsch and Feldman (1996) emphasize that location-specific externalities differ across industries and are stronger in innovative sectors. My results similarly suggest that identifying the right industries may be more important than identifying the right locations.

This paper also contributes to the quantitative literature on industrial policy in general equilibrium. Existing models typically assume that EES are either industry-specific or location-specific,[13] an assumption with important implications for optimal policy. I instead study how the coexistence of industry- and location-specific EES shapes optimal industrial policy.
In urban economics, Ahlfeldt et al. (2015) develop a spatial model with location-specific EES that has become a workhorse framework. Their productivity externalities are conceptually similar to those in my model, although I abstract from other determinants of urban density.
Gaubert (2018) provide the framework most closely related to mine by modeling location-specific EES that vary across sectors. I instead model EES as simultaneously industry- and location-specific. While she concludes that subsidies to smaller cities reduce aggregate welfare in France, my results suggest that optimal industrial policy targets smaller but highly specialized regions, potentially because her framework incorporates endogenous firm sorting whereas mine does not.
Finally, Bartelme, Costinot, et al. (2024) develop the first Ricardian trade model comparing industrial and trade policy and find larger gains from optimal trade policy, whereas Lashkaripour and Lugovskyy (2023) show that coordinated industrial policy is first best in a model with love-for-variety scale economies. My modeling approach is closest to Bartelme, Costinot, et al. (2024),[14] representing EES as a power function of employment, but differs by explicitly modeling technological externalities in the spirit of Greenwald and Stiglitz (1986).

The rest of the paper is organized as follows. Section 2 provides the historical background of non-automatically approved BNDES subsidized credit within the framework of the PT governments' industrial strategies and describes the data used in the analysis. Section 3 outlines the empirical strategy, with a particular focus on the identification challenges, and develops the instrumental variable approach. Section 4 presents the reduced-form re-

[13]Some studies in the international trade literature allow industry-specific EES to vary across countries but cannot fully capture their location-specific component.
[14]See also Breinlich et al. (2022), Kucheryavyy et al. (2023), and Bartelme, Jiang, et al. (2025).

sults and examines their robustness across alternative specifications. Section 5 discusses the empirical strategy used to estimate EES and demonstrates how they can be decomposed into industry- and location-specific components. Section 6 develops a quantitative framework to rationalize the empirical findings and to conduct counterfactual analyses. Section 7 reports the results from quantification of the model. Section 8 concludes.

2 Historical Background and Data

2.1 Industrial Policy

Industrial policy has deep roots in Brazil. After centuries of reliance on the export of a narrow range of primary commodities, Brazil underwent a significant industrialization push since the 1930s driven by import substitution and so-called developmentalist policies, lasting until the 1970s (Baer, 2014). This period was followed by a shift toward trade liberalization and market-oriented reforms (Muendler, 2004), as in many Latin American economies under the Washington Consensus (Rodrik, 2006). Subsequently, industrial policy experienced a revival under the governments of the *Partido dos Trabalhadores* (PT, Workers' Party). This revival was marked by the creation of the *Agência Brasileira de Desenvolvimento Industrial* (Brazilian Agency for Industrial Development) in 2004 and the implementation of three major industrial strategies: the *Política Industrial, Tecnológica e de Comércio Exterior* (PITCE) in 2003, the *Política de Desenvolvimento Produtivo* (PDP) in 2008, and the *Plano Brasil Maior* (PBM) in 2011 (Ferreira et al., 2016; Terra and Canêdo Pinheiro, 2016). The PBM was originally scheduled to run for three years, ending in 2014, but some of its measures were extended beyond that period.[15] Through the PITCE, PDP, and PBM, the Brazilian government sought to enhance the productivity and competitiveness of domestic firms (Terra and Canêdo Pinheiro, 2016). Accordingly, the government channeled funds to the BNDES through the Finance Ministry, which the Bank was then required to allocate in line with the objectives set out in the industrial strategies.[16]

Long-term loans in the Brazilian economy are primarily provided through BNDES funds (Lage de Sousa and Ottaviano, 2018), accounting for about 70% of long-term credit in the country (Cavalcanti and Vaz, 2017).[17] Therefore, BNDES naturally serves as the main instrument of industrial policy (Hamaguchi, 2022) when capital subsidies are the primary

[15]This applies particularly to the payroll tax exemptions, which several successive governments attempted to phase out (Caleiro, 2018). In July 2023, the Lula da Silva administration launched a new effort to do so, with the political debate still ongoing as of December 2023 (IPEA, 2023).

[16]In parallel, successive governments supported the creation of national champions by facilitating mergers among large firms, with the aim of fostering Brazilian companies capable of competing in international markets (Terra and Canêdo Pinheiro, 2016).

[17]Consequently, the BNDES is classified by Luna-Martinez and Vicente (2012) as one of the world's development-bank "megabanks."

policy tool. Figure 1 illustrates the evolution of BNDES disbursements to the private sector over time.

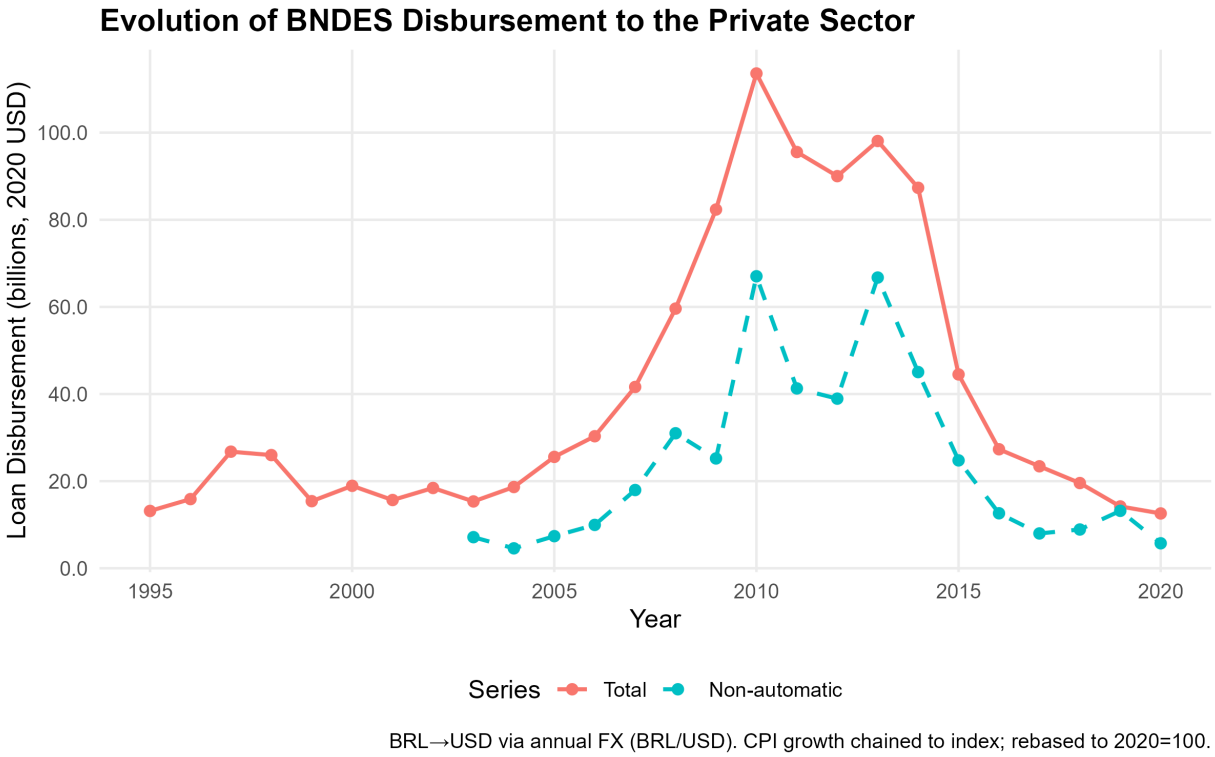


Figure 1: Annual BNDES Disbursements (1995-2020)

It shows that the sharp increase in overall BNDES lending coincided with the industrial policies implemented by the PT governments beginning in 2003/2004. As shown in Figure 1, BNDES lending peaks in 2010. However, when measured in Brazilian reais (R$), the peak occurs in 2013—mainly due to exchange rate fluctuations. The steady increase in available BNDES subsidized credit can be clearly traced, as the Brazilian government regularly enacted new laws to channel additional funding to the BNDES (see Appendix A.2.2).[18] Starting in 2014, Brazil entered a recession that lasted through 2018 and beyond, accompanied by a significant contraction in public spending (Barbosa-Filho, 2020), which may help explain the decline in BNDES lending. The peak of BNDES lending to the private sector occurred roughly between 2010 and 2014, precisely during the period in which the PBM[19] was in effect.

The BNDES, as the implementing agency, placed particular emphasis—within the manufacturing sector—on upstream, natural-resource-intensive industries.[20] Figure 2 illustrates

[18]Hence, prior to the funding cuts prompted by the recession, I am not aware of any reports of firms being denied BNDES support due to insufficient funding.

[19]For additional information on the PBM, see Appendix A.1.

[20]Although some of these sectors are primarily extractive in nature, they are included here due to their significant economic relevance to Brazil—especially the oil extraction sector, as emphasized by Cavalcanti, Da Mata, et al. (2019).

this sectoral focus.[21]

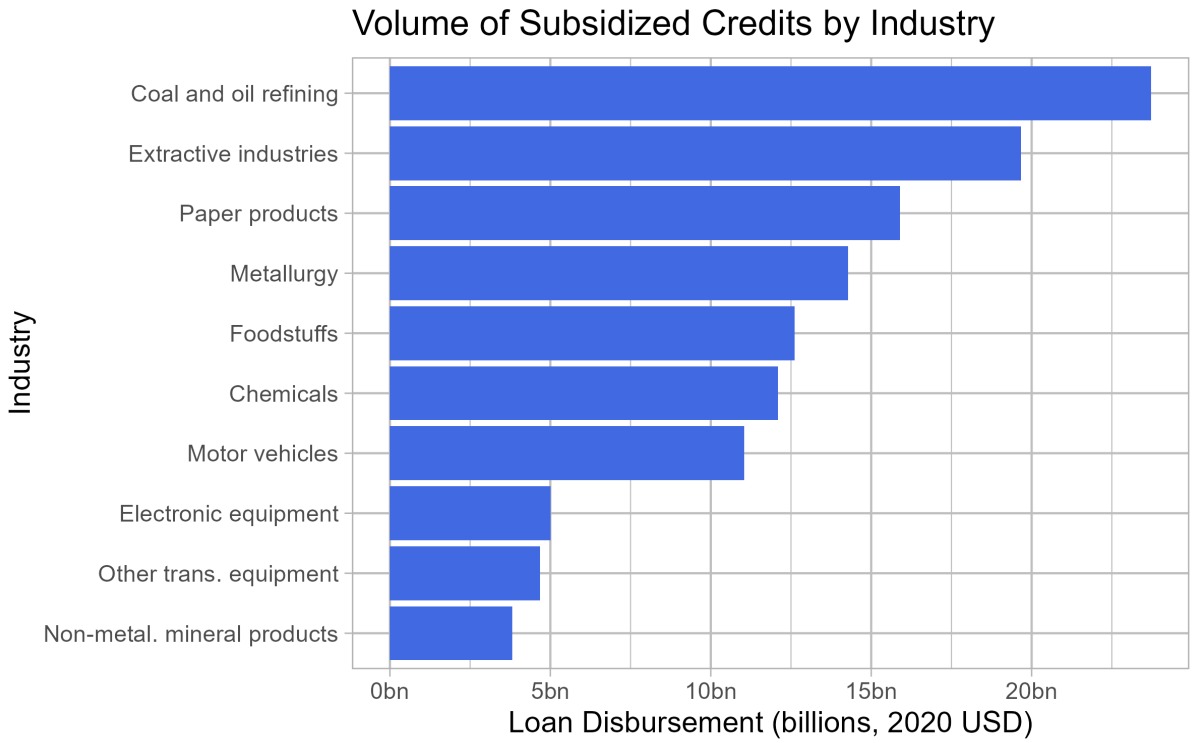


Figure 2: Volume of BNDES Disbursements by Industry (2002-2019)

Figure 3 shows that the number of firms receiving BNDES support varies substantially across industries, reflecting not only sectoral heterogeneity but also differences in the average volume of subsidized credit received per firm. BNDES support exhibits varying degrees of granularity, where granularity refers to the extent of concentration in both the volume and number of supported firms. Highly granular BNDES support indicates that a limited number of firms receive large average disbursements in a given industry (see Figure 7 in Appendix A.2).

[21]Sectoral focus is either measured by the volume, number, and per-firm amount of subsidized credit directed to each sector.

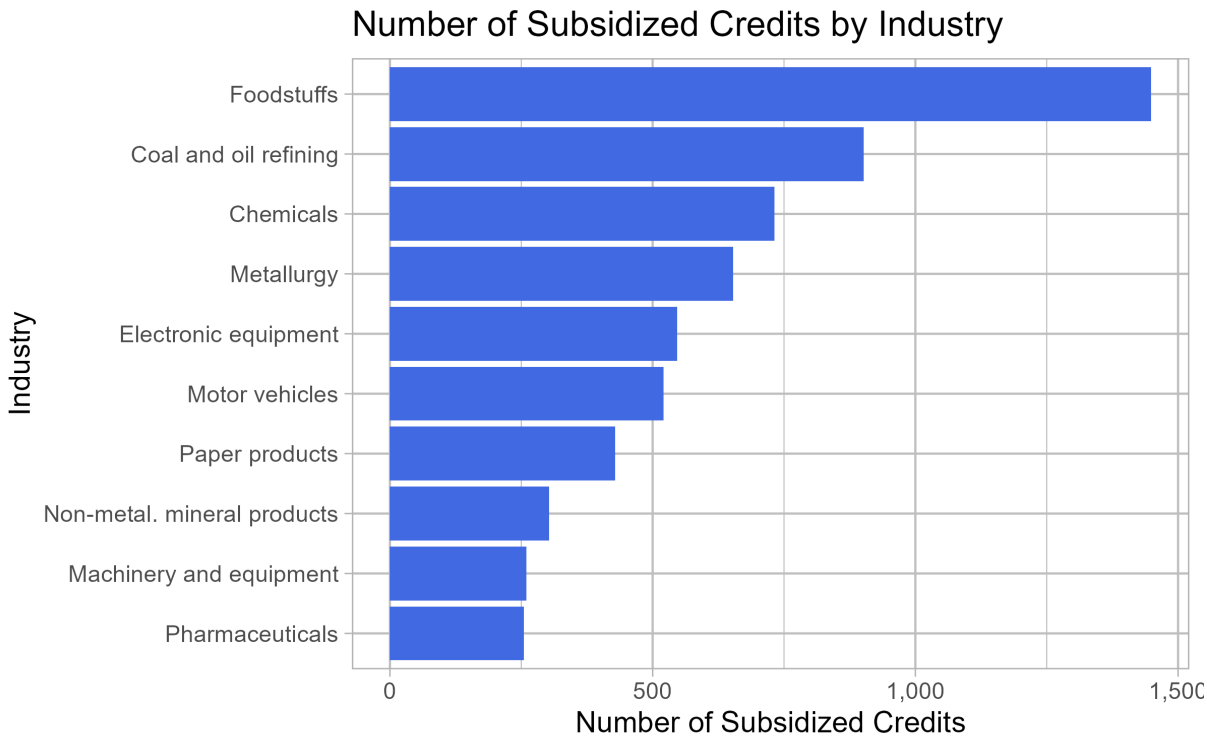


Figure 3: Number of Subsidized Credits by Industry (2002-2019)

Drawing on one of the foundational modeling frameworks for EES—namely the model by Krugman (1980), which incorporates the number of firms as a central element—it becomes evident that the impact of BNDES support on economic growth is likely to vary across industries. If EES are indeed the primary mechanism through which industrial policy generates additional welfare—and if these EES are embodied in the number of operating firms—then adopting less granular targeting strategies should amplify EES effects, thereby increasing the overall welfare gains from the policy. Even though the number of firms targeted is at best an imperfect proxy for the EES-generating potential of an IP, this insight suggests that counterfactual, less granular industrial policy designs could be more welfare-enhancing. At this stage, it is important to bear in mind that the extent to which BNDES support generates EES is likely to vary substantially across industries.

Another important dimension along which EES may vary—highlighted in the economic geography literature—is geographic location.

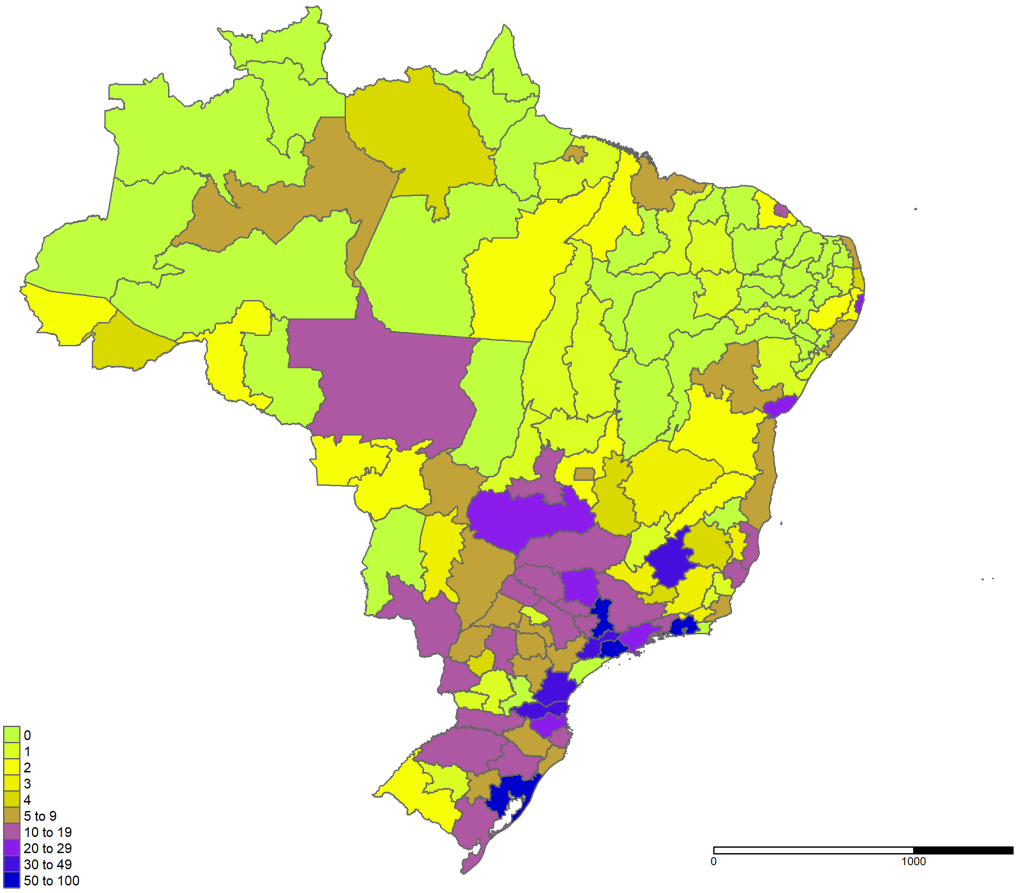


Figure 4: BNDES Disbursements by Mesoregion (2002-2019)

Figure 4 presents the geographic distribution of BNDES disbursements across mesoregions, based on the number of firms that received BNDES subsidized credit. BNDES subsidized credit is highly unequally distributed across Brazil. The main recipients of BNDES support are firms located in the metropolitan areas surrounding (1) São Paulo, (2) Rio de Janeiro, (3) Porto Alegre, and (4) Campinas.

BNDES lending reached its peak during the Dilma Rousseff administration's implementation of the PBM (2011-2014), at a time when Brazil's trade with the rest of the world slowed due to economic contractions in the United States, the European Union, and China (Barbosa-Filho, 2020).[22]

In addition to the challenging international economic environment, Brazil entered a major economic crisis beginning in 2014. This economic downturn escalated into a political crisis that culminated in the impeachment of Dilma Rousseff in 2016. Under her successor,

[22]The United States experienced a deceleration of economic growth, due in part to a standoff between the Obama administration and the Republicans that culminated in the 2013 United States Debt-Ceiling Crisis (Barbosa-Filho, 2020). The European Union was fully absorbed by the European Sovereign Debt Crisis in 2012 (Lapavitsas, 2012). China's 12^{th} Five-Year Plan emphasized the "rebalancing" of its economy toward greater domestic consumption rather than investment (Roach, 2022, pp.196–197).

President Michel Temer, the Brazilian economy began to stabilize and recover gradually (Barbosa-Filho, 2020). The subsequent administration under Jair Bolsonaro maintained the strategy adopted by Temer while further reducing BNDES funding drastically.
During the observation period, Brazil—like other Latin American countries—experienced premature deindustrialization, a process underway since at least the 1990s (Rodrik, 2015). Formally, premature deindustrialization denotes the contraction of the manufacturing sector in low- and middle-income economies prior to their attaining high-income status, which accounts for the characterization as *premature*.

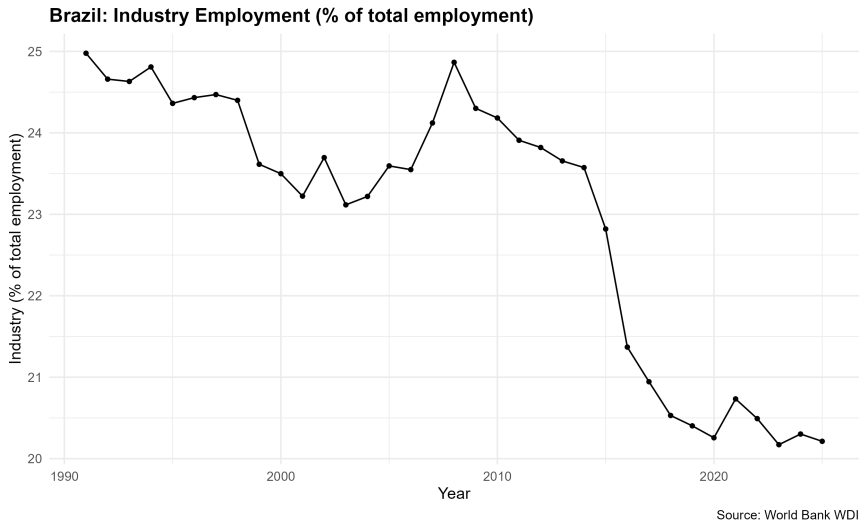


Figure 5: Trend of Brazilian Industry Employment over time (1991-2025)

Figure 5 illustrates the decline of Brazilian manufacturing, measured as a share of industry employment in total employment.[23] In the context of premature deindustrialization, even a stabilization of manufacturing's role in the overall economy may be regarded as a success of industrial policy. As such, Figure 5 depicts an unfavorable outcome of Brazilian industrial policy efforts.

2.2 BNDES

BNDES is the principal instrument of the Brazilian government for financing long-term private-sector investment, with subsidized interest rates. All firms in Brazil, including foreign-owned ones, are eligible for BNDES support. It operates through various credit lines, offering a range of financial products. Operations can be approved either automatically (through software) or manually (by direct review). Both automatic and non-automatic operations may consist of subsidized credit granted directly to clients—referred

[23]The World Bank defines industry employment as employment in manufacturing, mining, construction, and public utilities. Data disaggregated to manufacturing employment alone were not available for a sufficiently long period from the World Bank or the ILO.

to in BNDES terminology as "direct operations." Alternatively, credit may be provided indirectly through commercial banks, which are termed "indirect operations."
Manual operations involve large-scale transactions exceeding a specified threshold, which in 2012 was set at R$10 million for direct operations and R$20 million for indirect operations (BNDES, 2013, p.20). This study focuses on non-automatic operations for two reasons. First, although BNDES credit lines intended to support investment are precisely defined by the institution, in practice—i.e., in the data—some of these credit lines target very similar purposes, often becoming conflated and therefore difficult to distinguish. Appendix A.2.1 provides a more detailed description of non-automatic BNDES operations and the associated credit lines. Second, the identification strategy will later exploit a decisive feature of non-automatic operations—namely, the substantial bureaucratic burden in the credit concession process, arising from the involvement of a larger number of internal BNDES actors (BNDES, 2013, p.21). Over the 2003–2019 period, BNDES disbursed subsidized credit through non-automatic operations totaling USD 435.4 billion (in 2020 prices). These non-automatic operations represented 48.4% of the total subsidized credit volume directed to private-sector operations by BNDES.

Obtaining BNDES subsidized credit approved non-automatically entails a considerably higher bureaucratic burden than accessing credit approved automatically (BNDES, 2013, p.21).[24] This process involves multiple bureaucratic stages: First, firms approach BNDES (or a commercial bank administering BNDES subsidized credit) for a *Consulta prévia*, a preliminary consultation in which they provide basic information about their projects. Second, during the *análise* phase, the firm submits a detailed project proposal and negotiates directly with the BNDES. In addition to the bureaucratic burden imposed in the first step, the second step adds yet another layer through its lengthy procedures and negotiations. Appendix A.2.3 provides a detailed description of the administrative process for obtaining non-automatically approved BNDES credit, the substantial bureaucratic fixed costs it entails, and a visual overview of the process reproduced from the BNDES 2012 annual report (BNDES, 2013, p.22).
Most importantly, firms must allocate personnel capable of negotiating with the operational, financial, and legal departments of BNDES. To conclude, since these are highly specialized functions, access to non-automatically approved BNDES subsidized credit entails a significant fixed cost, which may induce especially large firms with large projects to apply for such credit as shown in Table 1.

[24] A natural question is why firms do not switch to automatically approved credit. The answer is that BNDES rules require loans above a certain threshold to be processed through the non-automatic approval channel.

Table 1: Average Investment and Sales of Treated and Untreated Firms

	(1) Non-Automatic BNDES	(2) Automatic BNDES	(3) Without BNDES
Investment (Mio. 2020 USD)	50.31	0.695	0.652
Sales (Mio. 2020 USD)	446.88	5.13	18.5
Observations	7,754	183,957	184,116

2002-2019. Column (3) comprises all firms in the PIA-Empresa sample with 30 or more workers that "survived" until 2019.

2.3 Data

My dataset combines firm-level administrative data, firm-level subsidy data and region- and sector-level variables. The dataset is annual and covers the period 1996-2021.[25] The IBGE defines 137 *mesorregiões* (mesoregions) in Brazil and 27 manufacturing sectors.[26] Data construction is described in further detail in Appendix A.3. Following Lage de Sousa and Ottaviano (2018), I restrict the analysis to consistently sampled manufacturing firms with 30 or more employees. Of all private-sector firms that received non-automatically approved BNDES subsidized credit, 25% appear in the complete PIA Empresa sample. This is because PIA Empresa surveys only *manufacturing firms*, whereas firms outside the manufacturing sector may nonetheless undertake *manufacturing projects* that are eligible for BNDES support. As such, treated firms constitute 1.8% of observations among consistently sampled firms in PIA Empresa, yet account for approximately 43% of total PIA Empresa sales.

Firm-level survey - PIA Empresa is meant to be representative of Brazilian manufacturing. Therefore, it covers firms that employ at least 5 people. Firms that employ 30 or more people are consistently part of the sample. The variables include sales; assets, including write-offs and improvements; various categories of investment; different types of employment distinguished by function; various categories of employment remuneration; materials and energy inputs; inventories; cost categories such as repair and maintenance, third-party services, water expenditure, and transport costs; as well as establishment locations and the number of local sites. Further information on the dataset's characteristics, including the number of observations, is provided in Appendix A.

To ensure consistency between firm-level and establishment-level regressions, and to facilitate comparability with previous studies, I restrict the sample to census-sampled firms (571,674 observations), as establishment-level information is available only for firms with 30 or more employees. Moreover, in several specifications I restrict the sample to firms

[25] To avoid the confounding effects of the Covid-19 pandemic, I exclude the years 2020 and 2021 from the empirical analysis.

[26] For data protection reasons, I extract information on firms only if at least three firms are present in the respective mesoregion. This regulation is imposed by IBGE to protect firm-level confidentiality. This restriction applies only to statistics and regressions at the industry–mesoregion–year level.

that remain in the sample throughout the period to ensure better comparability with the control group, given that treated firms are on average much larger. Among the census-wise sampled firms, there are 317,969 observations of firms that "survived" until 2019. Additionally, I exclude the top 0.5% of observations in material repair and maintenance expenditures, as these observations exert extreme leverage on the 2SLS second-stage estimates and yield economically implausible magnitudes. The exclusion is not driven by weak-instrument concerns: the first stage remains strong, and the Kleibergen–Paap weak-instrument F-statistic is, if anything, larger when these observations are included. Rather, the restriction is intended to ensure that the estimated treatment effect is not driven by a very small number of high-leverage observations in the upper tail of the distribution.

Subsidized credit registry - Federal Law nº 12.527/2011 (the "Access to Information Law") obliges all public entities to publish information of public interest (except that deemed legally confidential).[27] In the spirit of compliance with the law and to promote transparency, the BNDES publishes information on each of its "direct and non-direct" operations.[28] These information sheets constitute my main data source for subsidized credit. These sheets contain information about the credit recipient, the credit line, contract date, duration, project description, amount, and more. The resulting dataset covers the universe of subsidized credit allocated by BNDES to firms between 2002 and 2023, encompassing the "hot" industrial policy period under the PT governments, including the PITCE (2004–2008), the PDP (2008–2011), and notably the PBM (2011–2014) periods. The subsidized credit data are merged with firm-level survey data based on the *Cadastro Nacional da Pessoa Jurídica* (CNPJ) number of each firm—that is, the National Registry of Legal Entities in which every company is automatically enrolled upon incorporation.

Descriptive statistics - Table 2 presents descriptive statistics for subsidized credit loans recorded in the BNDES credit registry between 2002 and 2019. Between 2002 and 2019, there are 1,462 firm-year observations in which a manufacturing firm received at least one non-automatically approved subsidized loan. The average size of a non-automatically approved subsidized loan for a manufacturing firm was R$108 million,[29] with an average repayment period of about five years[30] and an average interest rate of 4.1%. All subsidized loans were denominated in Brazilian reais (R$). The average Brazilian CPI inflation over

[27] See Federal Law nº 12.527/2011.

[28] For more information, see BNDES Data about Financial Operations.

[29] This corresponds to USD 20.9 million in 2020, USD 32.4 million in 2015, USD 61.4 million in 2010, USD 44.4 million in 2005, and USD 37 million in 2002. These conversions use the BRL–USD exchange rate recorded on January 1 of each respective year by the Economic Research Division of the Federal Reserve Bank of St. Louis (FRED).

[30] Although the BNDES primarily specializes in long-term financing, as noted earlier, the subsidized credit lines analyzed in this study are more appropriately characterized as medium-term financing instruments given the average repayment period.

the period was approximately 5.5% (World-Bank, 2025),[31] implying a real interest rate on R$-denominated loans of around –1.4%. Between 2002 and 2019, total non-automatically approved subsidized credit provided to manufacturing firms amounted to approximately R$158 billion,[32] equivalent to 13.3% of investment in machinery recorded in PIA Empresa. This implies that non-automatically approved subsidized credit, within the framework of the PT governments' industrial strategies, constituted a large-scale industrial policy at the national level.

Table 3 reports descriptive statistics of the main firm balance sheet variables, disaggregated by treatment status. Column (1) reports average sales, employment, and investment for treated firms. By contrast, Column (2) presents the same variables for the non-treated group. Subsidized credit is sizable, amounting on average to approximately 10% of total annual sales.

Table 2: Descriptive Statistics of BNDES Subsidized Credits

	(1) Loan Size (Mio. 2020 USD)	(2) Repayment Period (Years)	(3) Interest Rate (%)
Mean	20.9	4.8	4.1
Std.	123.1	2	2.1

This table reports descriptive statistics of non-automatically approved BNDES subsidized credit for manufacturing firms between 2002 and 2019 at the firm-year level. The dataset includes 1,462 such subsidized credit "bundles."

Table 3: Descriptive Statistics of Firm Balance Sheets

	(1) Treated Firms	(2) Non-Treated Firms
Investment (Mio. 2020 USD)	50.31	0.68
	(862.5)	(5.4)
Sales (Mio. 2020 USD)	446.88	20.3
	(3,156)	(78.7)
Employment	1,694	166
	(3,804)	(543)
Observations	7,754	310,215

Standard deviations are in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year level.

Purely descriptive event-study estimates, using the estimator of De Chaisemartin et al. (2025), indicate that firms selecting into subsidized BNDES credit are already on an upward trajectory. Figure 6 suggests that firms do not randomly select into BNDES subsidized credit, but instead exhibit self-selection. In other words, there are strong trends in certain firm characteristics prior to taking up BNDES subsidized credit; that is, there are pre-trends. This constitutes an endogeneity concern that the econometric analysis must address.

[31] Additional information on the evolution of inflation during the observation period is provided in Appendix A.1.

[32] This corresponds to USD 30.6 billion in 2020, USD 47.4 billion in 2015, USD 89.8 billion in 2010, USD 64.9 billion in 2005, and USD 54.1 billion in 2002.

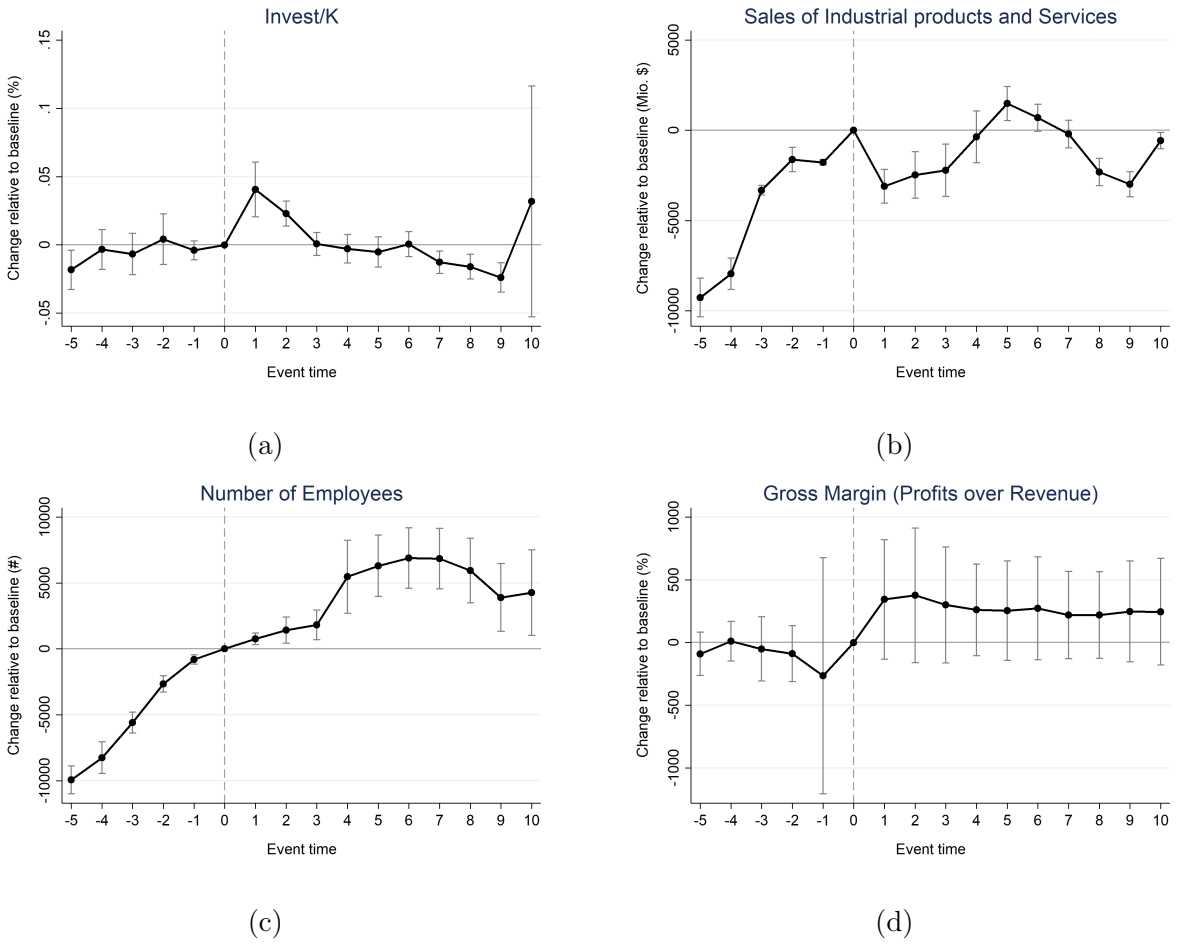


Figure 6: Event study estimates for investment, sales, labor, and gross margin.

Figure 6 presents standard event-study estimates with absorbing treatment for four key firm characteristics: (a) the investment-to-capital ratio, (b) sales of industrial products and services, (c) the number of employees, (d) and the gross margin (profits over revenue). First, as shown in Figure 6a, investment spikes following the take-up of subsidized credit, consistent with the BNDES disbursement procedure, under which funds are released according to an implementation plan. Second, Figure 6b shows that firms selecting into subsidized credit experience strong pre-treatment sales growth, consistent with the BNDES targeting firms with a higher probability of repayment. Following treatment, however, their sales growth appears to stagnate. Third, as revealed in Figure 6c, employment increases as well but continues to grow, albeit more slowly. Fourth, Figure 6d shows that the gross margin falls slightly before treatment and rises modestly afterward, although the estimated effect is not statistically different from zero.

3 Empirical Strategy for Firm-Level Analysis

Figure 6 shows that BNDES support is associated with little to no additional sales growth for supported firms. In this section, I develop the identification strategy and present details of the main results, including a heterogeneity analysis.

3.1 Identification Strategy

The main empirical strategy employs a lag-augmented local projection design, following Montiel Olea and Plagborg-Møller (2021), combined with an instrumental variables approach in the spirit of Stock and Watson (2018) and Jordà and Taylor (2025).[33][34] The instrument is constructed from lagged depreciation, proxied by the deviation of repair and maintenance costs from the pre-treatment period industry-specific mean, and its validity is assessed using a matched sample. The design leverages the institutional setting, particularly the bureaucratic effort required to obtain BNDES support, and the fact that the vast majority of firms are never treated. To improve the comparability between the treatment and control groups, I use propensity score matching to construct a comparable control group.

The instrumental variable and the institutional setting jointly address two distinct selection margins: First, among all firms, only a subset chooses to invest; the instrumental variable accounts for this selection. Second, among the firms that invest, only a subset selects into subsidized credit—namely those able to bear the bureaucratic fixed costs associated with BNDES support and to incur these sunk costs. Moreover, omitted variables related to firms' optimization behavior—such as productivity and other inputs—also affect the capital stock and must be accounted for.
This set of analyses relies on the firm-level sample of firms that remain continuously in the sample ("stayers"), which are, on average, larger than the typical firm, as Section 2 shows that treated firms are larger than their control group peers. Section 4.5 discusses which comparison groups are most appropriate in the context of this study.

[33] In this context, an event study design would be a natural methodological choice. However, although recent advances in the difference-in-differences/event study literature—specifically the estimators proposed by De Chaisemartin et al. (2025)—permit difference-in-differences estimation with instruments, event study frameworks that incorporate instrumental variables to address endogeneity are, to date, unavailable. Consequently, an alternative empirical design is required to analyze the dynamic effects in this context.

[34] Lag augmentation in local projections simplifies inference, as it accounts for serial correlation and thereby allows for the use of standard textbook formulas for standard errors (Montiel Olea and Plagborg-Møller, 2021). Moreover, lag augmentation substantially reduces estimation bias (Piger and Stockwell, 2025).

I estimate the following structural equation:[35]

$$\Delta y_{fir,t+h} = \delta_{it+h} + \kappa_{rt+h} + \beta_h \Delta BNDES_{firt} + \sum_{t=0}^{\tau=-3} \Delta y_{fir,t} \eta_{h,t} + e_{fir,t+h} \tag{1}$$

The data take the form of a standard panel dataset with periods indexed by $t \in T$, and the structural equation is estimated separately for each outcome horizon $h \in \{1, \ldots, 6\}$. $\Delta y_{fir,t+h}$ denotes the log difference in sales between periods t and $t+h$, that is, cumulative sales growth h periods after the baseline period t. $\Delta BNDES_{firt}$ is a dummy variable that indicates, for a given year t, whether a firm switches into treatment (i.e., is granted BNDES subsidized credit), implying that treatment is absorbing. Δy_{firt} denotes a vector containing the τ lagged values of the log difference of the outcome variable, with each log difference measured between periods t and $t-1$. The corresponding coefficient vector, η_{ht}, is horizon-specific and therefore varies with the outcome horizon h. In the baseline specification, three lags ($\tau = 3$) are included. I investigate alternative lag lengths, e.g., $\tau = 1$, $\tau = 2$, $\tau = 4$. The industry fixed effects δ_{it+h} and the mesoregion fixed effects κ_{rt+h} capture the respective trends at the industry and mesoregion levels. The parameter of interest is β_h; its estimated value captures the differential impact of BNDES support on firm output h years after take-up. Under the assumptions that the take-up of BNDES support is uncorrelated with the error term and that no reverse causality exists, the estimation of (1) enables one to assess not only whether BNDES support affects firm output, but also when its impact materializes. As discussed earlier, these assumptions are unlikely to hold, as firms strategically select into investment and BNDES support based on a number of confounding factors.

To address these identification concerns, the instrument exploits temporal, within-firm variation in lagged depreciation shocks to predict investment decisions and, ultimately, the take-up of BNDES subsidized credit. Combined with the bureaucratic effort required to obtain BNDES support—which I model below as a fixed cost—only firms able to absorb this fixed cost ultimately take up BNDES support. There are several underlying hypotheses that are key for this approach. First, I assume that firms purchase different machines at different points in time from different producers. Second, I build on findings from the maintenance management literature, as summarized in Dekker (1996) and A. Garg and Deshmukh (2006), which point out that the deterioration and failure processes are inherently stochastic. Depreciation shocks around the (industry-specific) mean serve

[35] Whereas the original local projection framework of Jordà (2005) specifies the system of equations in levels, the subsequent literature has increasingly adopted specifications in long differences (Jordà and Taylor, 2025, p.7). At least since Marriott and Pope (1954), it has been well established that autoregressive models are subject to small-sample downward bias. Herbst and Johannsen (2024) show that this issue also arises in the context of local projections. However, estimation bias can be substantially mitigated by specifying the system in long differences and including lagged differences as controls (Piger and Stockwell, 2025).

as a source of random variation which—under the assumptions on machine purchases and the presence of bureaucratic fixed costs associated with BNDES support—can be used to predict take-up of BNDES support.

3.1.1 Measuring Depreciation Shocks

Information on when and for what reason firms record a depreciation shock is generally not publicly available and therefore not observable. However, maintenance and repair activities are closely related to depreciation (Albonico et al., 2014).[36]

A first important insight from maintenance management and engineering is that private-sector firms commonly apply a version of the "50% rule" when deciding whether to repair or replace an asset (D.A.U., 2025). The *50% rule* states that if repair costs exceed approximately 50% of replacement costs, replacement is preferable (D.A.U., 2025).[37]

A second important insight from maintenance management and engineering is that *preventive maintenance management* has become standard practice in manufacturing (Jardine and Tsang, 2013). Indeed, firms that allow their assets to depreciate completely and break down manage those assets far from optimally. Accordingly, accelerated depreciation—i.e., faster wear and tear—should lead firms to anticipate the replacement of an asset out of concern that a major depreciation shock could temporarily halt production.

The reasoning behind the *50% rule* and *preventive maintenance management* can be illustrated with a capital good familiar to most people: a car. Over its life cycle, repair costs for a car rise, as depreciation increases with age (Hulten and Wykoff, 1980). This process continues until the owner anticipates that the next depreciation shock will be too large. At that point, the owner invests in a new car to ensure that operations continue smoothly (i.e., the family has a working car). In the "car metaphor" illustration, the major depreciation shock leading to replacement investment at time t is signaled by one period of zero repair expenditure in period $t-1$. Introducing BNDES subsidized credit into the "car metaphor," the owner chooses to take up BNDES support at time t (signaled in the data by zero repair expenditure in $t-1$) if the expected benefits from replacing the car are sufficiently large to justify the fixed costs associated with obtaining BNDES financing.

The argumentation regarding the measurement of depreciation shocks is consistent with the economic literature on capital depreciation. More recently, drawing on survey evidence from Uganda showing that machine repair costs can be economically significant in a developing economy context, Graff (2026) states that demand in the repair market is

[36] McGrattan and Schmitz (1999) document that maintenance and repair expenditures in Canada (1956–1993) averaged 6.3% of annual GDP. Hence, it is reasonable to assume that depreciation $> 0 \; \forall f, t$ in the Brazilian case.

[37] The 50% threshold may vary across firms. However, this does not undermine the argument that firms do not endogenously choose between replacement and repair, but rather follow a threshold rule determined by physical and technical considerations.

unpredictable, highlighting the stochastic nature of depreciation and its direct link to repair expenditures. In earlier work, Feldstein and Foot (1971) and Eisner (1972) show that the assumption that replacement investment is a constant share of the capital stock—as in Jorgenson (1963)—is unsatisfactory and far from economic reality. Moreover, the economic theory of optimal replacement investment developed by Feldstein and Rothschild (1974) posits that firms do not allow their machines to break down, in order to maintain an optimal level of capital services. This underscores that preventive maintenance management has a strong grounding in economic theory and that, in principle, depreciation shocks should not have direct effects on output, but only operate indirectly through their impact on the timing of (replacement) investment. In turn, Feldstein and Rothschild (1974) note that the timing of replacement investment is highly sensitive to the cost of financing, which is precisely what BNDES subsidized credit tackles.

In the firm-level survey data, firms report the direct costs associated with the maintenance and repair of machinery and equipment. Bringing the car metaphor to the data, for a hypothetical firm owning only one capital good, zero repair expenditure would signal an anticipated major depreciation shock and, subsequently, replacement investment. Since virtually all firms in the firm-level survey own more than one capital good, one would expect a negative correlation between repair expenditures and investment.
In other words, firms with zero material repair expenditure (in the case of an indicator variable), firms with a low repair-to-capital expenditure ratio (in the case of a "repair rate" variable), or firms with negative deviations from the mean repair rate (industry-specific to ensure more stable means) (in the case of a "repair rate mean deviation" variable) are more likely to have experienced depreciation shocks in a given year. In line with this argument, Table 4 reports significant negative correlations between investment and the various measures of repair expenditure, providing empirical support for their use as proxies for depreciation shocks.

Table 4: Depreciation Shocks and Investment

Material MRO Cost	Investment at t (in Mio. R$)		
	(1)	(2)	(3)
Rate (0/1) at t	-1,780,0***	-6,920***	-7,330***
	(3,690)	(1,060)	(1,730)
R-squared	0.3	0.72	0.82
Obs	406,333	406,333	404,879
Mean Deviation from Rate at t	-6,470,0***	-9,060***	-3,260***
	(1,610,0)	(1,140)	(1,030)
R-squared	0.06	0.69	0.8
Obs	400,990	400,990	399,628
Rate at t	-4,060,0***	-9,060***	-3,220***
	(1,060,0)	(1,140)	(1,020)
R-squared	0.05	0.69	0.8
Obs	400,990	400,990	399,628
Firm FE	No	No	Yes
Ind FE	No	Yes	No
Year FE	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
The results reported in this table are based on the following specification: $I_{firt} = \alpha_{(f),(i),(t)} + \beta mro_cost_{firt} + \epsilon_{firt}$.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All columns report results from the same regression specification, differing only in the included fixed effects. Column (1) includes only year fixed effects, Column (2) adds industry fixed effects to the year fixed effects, and Column (3) presents the most restrictive specification, including both firm and year fixed effects. As expected, adding fixed effects to account for unobserved shocks affecting firm performance and potentially confounding the relationship reduces the magnitude of the estimated effect, while a clear and statistically significant negative correlation remains. To align with the maintenance management literature emphasizing the stochastic nature of depreciation and subsequently repair schedules, I construct the instrument as the deviation of a firm's actual repair rate from its industry-specific mean over the full observation period:[38]

$$depr_shock_{firt-2} = \frac{repr_{firt-2}}{K_{firt-2}} - \frac{1}{T} \sum_{t \in T_{pre-treat}} \frac{repr_{firt}}{K_{firt}} \; \forall t \in T \tag{2}$$

Here, $depr_shock_{firt-2}$ denotes the proxy for lagged depreciation shocks, $repr_{firt-2}$ denotes the level of repair and maintenance expenditures, and K_{firt} denotes the capital

[38]Since firms are observed only a few times in the pre-treatment period, mean deviations are calculated relative to the industry-specific mean, which is by construction more stable due to the law of large numbers, thereby mitigating bias from firm-level fluctuations caused by outliers.

stock. As illustrated by the "car metaphor," lagging depreciation shocks is necessary for correctly predicting the timing of BNDES subsidized credit take-up using repair expenditure data. Given that manufacturing projects often span several years, including a planning phase of 1–2 years (Brandsaas et al., 2025), and projects in a subsample of the BNDES credit lines analyzed in this paper average five years from contract signing to the completion of installments (Lage de Sousa and Ottaviano, 2018),[39] lagging depreciation shocks appears even more justified, given potentially long negotiations prior to the take-up of subsidized credit. As a result, the lag length is set to two periods. Robustness checks using lag lengths of one and three periods for the first-stage estimation described in Subsection 4.2 are reported in Appendix B.2 and yield quantitatively and qualitatively similar results.

The first-stage equation is, thus:

$$\Delta BNDES_{firt} = \lambda \; depr_shock_{firt-2} + \varphi_i + \vartheta_r + \zeta_{firt} \tag{3}$$

This design compares firms that followed similar pre-BNDES support trajectories but differed in the magnitude of depreciation shocks they experienced. Each product has distinct physical characteristics that generate different stochastic depreciation processes for the associated capital stock, thereby providing exogenous variation in depreciation shocks across time within firms that can be exploited for identification. This variation is arguably exogenous to unobserved factors influencing firm optimization behavior, as firms cannot influence physical laws. The exclusion restriction requires that, conditional on controls, these depreciation shocks affect firms only through their investment demand. Therefore, I explicate *preventive maintenance management* and why it is highly likely that depreciation shocks do not directly affect the output of reasonably well-functioning firms, but instead only alter their maintenance and replacement schedules. To alleviate concerns about omitted firm-level variables, I adapt the framework of Olley and Pakes (1996) to characterize the conditions—standard in the industrial organization literature—under which the exclusion restriction holds. Consequently, subsection 3.3 introduces depreciation into a framework similar to that of Olley and Pakes (1996) and Pavcnik (2002).

3.2 Local Projection Instrumental Variable Approach Conditions

The econometric assumptions underlying the local projection instrumental variable (LP-IV) approach for estimating dynamic causal effects differ in one key respect from those in "static" IV approaches commonly applied in microeconomic settings (Stock and Wat-

[39]Lage de Sousa and Ottaviano (2018) examine the BNDES FINEM and BNDES Automático credit lines, which are intended to finance large projects aimed at expanding production capacity, such as new plants or production lines. Therefore, five years can be regarded as an upper bound on project duration for the projects analyzed in this paper, which are on average smaller in scale.

son, 2018, p.924). Local projections estimate impulse response functions to a random treatment. The econometric term for this random treatment, here $depr_shock_{firt-2}$, is a "structural shock": a primitive, unanticipated economic disturbance that is inherently unforecastable and uncorrelated with other shocks. Impulse response functions trace the causal effects of such treatments on variables of interest over time, that is, dynamic causal effects. Define the vector of other shocks—that is, shocks to economic variables other than BNDES support—as $\epsilon_{2:N,firt}$, where N denotes the number of economic variables in the system and $2:N$ indexes all variables except the first, which corresponds to the treatment variable, BNDES support. If $BNDES_{firt}$ were uncorrelated with other shocks and any measurement error, the dynamic causal effect β_h can be expressed as:

$$\beta_h = \mathbb{E}(y_{fir,t+h}|BNDES_{firt} = 1, \epsilon_{2:N,firt}, \epsilon_{firs}, s \neq t) - \mathbb{E}(y_{fir,t+h}|BNDES_{firt} = 0, \epsilon_{2:N,firt}, \epsilon_{firs}, s \neq t) \tag{4}$$

where $s \neq t$ indexes all time periods other than t, and ϵ_{firs} denotes the corresponding shocks.[40] Under randomization, conditioning on other shocks is superfluous; nevertheless, this representation emphasizes that the causal effect can be interpreted as the partial derivative $\partial y_{t+h}/\partial BNDES_{firt}$, conditional on all other shocks remaining constant.

The conventional relevance condition (sometimes referred to as a "strong first stage") can, in this context, be expressed as:

$$\mathbb{E}(BNDES_{firt}, depr_shock_{firt-2}) \neq 0 \tag{5}$$

and does conceptually not differ from the standard case (Stock and Watson, 2018). This condition is, of course, testable and appears to be satisfied, as indicated by the results and the corresponding Kleibergen–Paap weak-IV F-statistic reported in Table 6 and Appendix B.2.

An instrumental variable must also satisfy a contemporaneous exogeneity condition (sometimes referred to as "exclusion restriction"). In other words, the instrument must be uncorrelated with other shocks $\epsilon_{1:n,t}$ at time t:

$$\mathbb{E}(depr_shock_{firt-2}, \epsilon_{1:n,firt}) = 0 \tag{6}$$

As in the standard case, this condition cannot be directly tested, as not all shocks are observed. Section 3.3 discusses potential violations of the exclusion restriction and outlines the assumptions under which it holds.

[40] s denotes all time periods except t, encompassing both periods before the shock occurs and periods between the realization of the shock at t and the outcome horizon h.

In contrast to the standard microeconometric case, LP-IV approaches require an additional key assumption—the lead-lag exogeneity assumption—as the framework explicitly accounts for dynamic effects. The key idea underlying this condition is that the outcome y_{t+h} generally depends on the full history of shocks. Therefore, for $depr_shock_{firt-2}$ to identify the effect of $BNDES_{firt}$ in isolation, it must be uncorrelated with all shocks at both leads and lags.

$$\mathbb{E}(\epsilon_{firt+j}, depr_shock_{firt-2}) = 0 \text{ for} j \neq 0 \tag{7}$$

The requirement that $depr_shock_{firt-2}$ be uncorrelated with future $BNDES_{firt}$ is generally not restrictive: when $depr_shock_{firt-2}$ contains only depreciation shocks realized until date $t-2$, it follows from the definition of depreciation shocks as unanticipated accelerations of the wear and tear process in production. For past shocks, the requirement that $depr_shock_{firt-2}$ be uncorrelated with past $\epsilon_{1:n,firt}$ is more restrictive and constitutes a strong assumption. This implies that $depr_shock_{firt-2}$ must be unpredictable given past realizations of $depr_shock_{firt-2}$ and $\epsilon_{1:n,firt}$.[41] Fortunately, the lag exogeneity condition in (7) is testable, as it implies that $depr_shock_{firt-2}$ should not be predictable using lagged values of y_{firt}, conditional on the included control variables. Table 18 in Appendix B.3.1 shows that this condition generally holds (except for the $t-2$ lag in the specification that use only year fixed effects which is the less preferred main specifications). To improve estimator efficiency, Stock and Watson (2018, p.926) suggest including control variables that capture the dynamics of y_{firt}, in particular its lagged values. This paper already incorporates such controls within the framework of Montiel Olea and Plagborg-Møller (2021).

3.3 Exclusion Restriction

A major concern regarding the exclusion restriction is that a depreciation shock might directly reduce output and subsequently lower output growth. However, there exist different strategies in maintenance management (Jardine and Tsang, 2013). The complete breakdown of a machine occurs only when a firm follows a "run-to-failure" strategy for an asset, which is optimal only for assets that are *non-critical* to the production process, since their failure causes no downtime in the production line (Tsang, 2002). In other words, this approach is common for *non-critical* assets, such as small auxiliary machines. To avoid costly downtime and optimize asset utilization, preventive maintenance strategies are standard in manufacturing (Jardine and Tsang, 2013). Their main aim is to avoid costly disruptions to the manufacturing process (Basri et al., 2017). As such, "normal" major depreciation shocks (i.e., shocks not stemming from natural catastrophes) should

[41] Stock and Watson (2018) note that, strictly speaking, the requirement is that $depr_shock_{firt-2}$ is not linearly predictable given past realizations of $depr_shock_{firt-2}$ and $\epsilon_{1:n,firt}$.

advance the decision to replace rather than repair a machine, in line with the 50% rule, but should not lead to production losses in cases where maintenance management is properly implemented. This assumption appears reasonable, as well-functioning firms should manage maintenance properly, given that it can account, for example, for 12%–23% of total factory operating costs in the UK (Cross, 1988) and up to 30% of total staff in refineries (Dekker, 1996).

To clarify the conditions under which the exclusion restriction is not compromised by confounding variables, I follow Pavcnik (2002) and derive a version of the dynamic framework of plant-level profit maximization—developed by Ericson and Pakes (1995) and Olley and Pakes (1996)—adapted to the context studied here.
I assume that each firm consists of a single establishment to align with my data. In the data, approximately 94% of the observed firms operate as single-establishment units. A plant's objective is to maximize the expected value of its current and future profits. Plants within an industry face the same input prices and market structure. However, they differ in their efficiency levels and are subject to plant-specific uncertainty regarding future market conditions and investment opportunities. In each period, a plant first decides whether to exit the market or continue production. A plant chooses to stay in operation if the expected present value of its future net cash flows exceeds the value of liquidation. Conditional on remaining in the market, the plant subsequently chooses its input levels. As a result, the plant's optimal exit and input decisions depend on its observable characteristics, such as its capital stock and investment levels. Plant-specific uncertainty about future market conditions influences these choices and leads firms to follow divergent trajectories. The model structure is compatible with imperfectly competitive market environments. In each period, firms take into account the market structure and the behavior of other firms when making decisions about exit and investment. To account for strategic interactions across firms, both profits and the resulting equilibrium exit and investment policies are explicitly time-indexed.
In a given industry i and a given region r, the capital stock at the beginning of the next period $t+1$ of firm f is described by the law of motion:

$$k_{firt+1} = (1 - \delta_{firt})k_{firt} + i_{firt}. \tag{8}$$

where δ_{firt} is the depreciation rate and i_{firt} is investment. Note that the depreciation rate δ_{firt} is firm-specific and varies over time. Specifically, I assume that δ_{firt} consists of a component anticipated by the firm and an unanticipated, stochastic component. For simplicity, I assume that this process follows a Markov structure:

$$\delta_{firt} = h(\delta_{firt-1}) + \eta_{firt}, \tag{9}$$

with $h(\cdot)$ denoting a function of depreciation in the previous period. Here, $h(\delta_{firt-1})$ represents the anticipated component, and η_{firt} denotes the unanticipated, stochastic depreciation shock. On the one hand, with regard to the anticipated component, I consider a setting in which firms plan their operations based on a depreciation rate informed by past experience and observe the actual depreciation rate only at the end of each period, once the depreciation shock has realized. On the other hand, the assumption of an unanticipated, stochastic depreciation shock incorporates key insights from the maintenance management literature, which emphasizes the inherently stochastic nature of deterioration and failure processes. In other words, "[...] the state of affairs in a maintenance organization is often dominated by unplanned events" (A. Garg and Deshmukh, 2006).

Productivity is known to the firm and evolves according to an exogenous Markov process. Following Pavcnik (2002), I assume that this Markov process is first-order to ensure the model remains tractable. Thus, the plant's state variables in the current period depend on their values in the previous period. Equilibrium profits and firm value are functions of the underlying market structure and input prices. Market conditions affect a firm f's profits and are common to all firms within a given time period and industry. The Bellman equation for an incumbent firm can then be written as:

$$V_{firt}(\omega_{firt}, k_{firt}) = max\Bigg\{\Phi, \sup_{i_{firt}\geq 0} \pi_{firt}(\omega_{firt}, k_{firt}) - c(i_{firt}) + \beta\, \mathbb{E}_{firt}\Big[V_{firt+1}(\omega_{firt+1}, k_{firt+1})|J_{firt}\Big]\Bigg\}, \tag{10}$$

where $\pi_{firt}(\cdot)$ are current period profits as a function of the vector of state variables, $c(i_{firt})$ is the firm's overall cost of current investment i_{firt}, β is the discount factor, and J_{firt} represents information available at time t.

According to Ericson and Pakes (1995), solving this dynamic control problem results in a Markov Perfect Equilibrium strategy governing the plant's exit and investment choices. The maximization operator in equation (10) reflects the firm's choice between two alternatives: exiting the market and receiving the plant's liquidation value Φ, or continuing operations and earning the expected discounted value of future profits. Let χ_{firt} be an indicator variable that equals one if the firm continues operating and zero if it exits. The exit rule can then be expressed as:

$$\chi_{firt} = \begin{cases} 1, & \text{if } \omega_{firt} \geq \underline{\omega_{firt}}(k_{firt}) \\ 0, & \text{otherwise} \end{cases} \tag{11}$$

Conditional on remaining active, the plant chooses its investment level based on its beliefs about future productivity and profitability. The optimal investment decision i_{firt} thus depends on the current capital stock, the productivity level, depreciation rate and the

cost of investment in absence of BNDES intervention:

$$i_{firt} = i(\omega_{firt}, \delta_{firt}, k_{firt}, c_t). \tag{12}$$

where c_t denotes the per-unit cost of investment in period t. The functions $\omega(\cdot)$ and $i(\cdot)$ are endogenously determined within the Markov Perfect Nash Equilibrium and depend on the full set of parameters governing equilibrium behavior. Equation (12) extends the investment demand functions in Olley and Pakes (1996) and Pavcnik (2002) by incorporating both investment costs and the depreciation rate. This extension builds on two key lines of literature. First, parts of the Real Business Cycle literature incorporate stochastic depreciation as a factor influencing investment behavior, as illustrated, for example, by Burnside and Eichenbaum (1996). Second, at least since the seminal work of Rajan and Zingales (1998), economists have emphasized a close link between investment demand and the cost of investment. c_t captures both financial costs and potential financial constraints.[42]

Equilibrium - I assume that a firm's investment cost is linear in the amount of investment undertaken—that is, it equals the unit cost of investment multiplied by the number of units invested. Consequently, in equilibrium, the total investment cost faced by a firm in a given period is:

$$c(i_{firt}) = c_t \times i_{firt} = c_t \times i(\omega_{firt}, \delta_{firt}, k_{firt}, c_t), \tag{13}$$

where c_t is the cost of one unit of investment. Firms are more likely to increase investment when the cost of investment is low. I now introduce the assumption that subsidized investment financing is made available through BNDES. However, the availability of subsidized credit is subject to a fixed cost that reflects institutional and bureaucratic factors, as discussed in Section 2. Accordingly, the introduction of subsidized investment modifies equation (13) as follows:

$$c_t^s \times i_{firt}^s = (1-s)c_t \times i(\omega_{firt}, \delta_{firt}, k_{firt}, (1-s)c_t) + f_t \tag{14}$$

where c_t^s and i_{firt}^s denote the unit cost and the amount of subsidized investment, respectively. The variable s represents the subsidized credit, expressed as the fraction of the unit cost that is saved. Which firms will self-select into subsidized investment? Profit-maximizing firms will choose subsidized investment if it is less costly than conventional

[42]Nevertheless, I address this concern explicitly later by testing for credit constraints, following Lage de Sousa and Ottaviano (2018).

financing. This condition is satisfied for all firms for which equation (15) holds:

$$c_t^s \times i_{firt}^s < c_t \times i_{firt} \tag{15}$$

Solving equation (15) for s allows one to conduct comparative statics on (16) (see Appendix B.1 for details) and identify the firms most likely to select into subsidized credit:

$$s > 1 - \frac{i(\omega_{firt}, \delta_{firt}, k_{firt}, c_t) + f_t + 1}{c_t \times i(\omega_{firt}, \delta_{firt}, k_{firt}, (1-s)c_t)}. \tag{16}$$

Before conducting comparative statics on how the decision to take up subsidized credit varies with the firm characteristics on the right-hand side of the equation, it is necessary to impose a few assumptions on the functional form of the investment demand $i(\cdot)$. First, investment demand $i(\cdot)$ is assumed to be a continuous and monotonic function covering maintenance and repair costs. Second, $i(\cdot)$ increases with productivity ($\frac{\partial i}{\partial \omega_{firt}} > 0$), as more productive firms derive greater marginal benefits from additional capital. Third, $i(\cdot)$ decreases with the capital stock ($\frac{\partial i}{\partial k_{firt}} < 0$), consistent with the standard assumption of diminishing marginal returns to capital. Fourth, $i(\cdot)$ is increasing in the depreciation rate ($\frac{\partial i}{\partial \delta_{firt}} > 0$), since higher depreciation necessitates greater investment to maintain the capital stock. Fifth, investment demand declines with the cost of investment ($\frac{\partial i}{\partial c_t} < 0$), reflecting the sensitivity of firms to investment prices. This is consistent with the condition that profits increase with capital ($\frac{\partial \pi_{firt}}{\partial k_{firt}} > 0$).

Hence, for any given level of investment, one can characterize the firms most likely to self-select into subsidized credit. Totally differentiating equation (16) yields comparative statics (see Appendix B.1 for details) showing that the condition in equation (16) is most likely to hold for firms with high productivity (ω_{firt}), which are consequently larger in size, experience larger depreciation shocks (δ_{firt}), and have a relatively low capital stock (k_{firt}). In other words, the firms that are disproportionally more likely to take up subsidized credit are large (high ω_{firt}) and face substantial capital renewal needs (high δ_{firt}), as discussed in Section 2.

To predict subsidized credit take-up, I require an instrumental variable that shifts one of the right-hand side variables in equation (16) without being correlated with firm outcomes in period t. To that end, I return to the lagged variation in depreciation, as expressed in equation (9). Importantly, δ_{firt} varies randomly within firms over time. I use the randomness in η_{firt-1}, embedded in δ_{firt-1}, as a source of exogenous variation to predict the take-up of subsidized credit.

Validity of Design - Identification relies on two key assumptions, in addition to the dynamic framework building on Olley and Pakes (1996) and Pavcnik (2002), and the econometric assumptions underlying the local projection instrumental variable (LP-IV)

approach, as developed by Jordà, Schularick, et al. (2015), Stock and Watson (2018), and Jordà and Taylor (2025). First, among firms that invest, only firms that are able to absorb the fixed cost select into BNDES-subsidized credit. Second, firms purchase different types of machines from different producers at different points in time. Whether these assumptions hold is difficult to verify, as the fixed cost is not directly observable. Moreover, I do not observe which firm purchased which machine, when, and from which producer. Subsection 3.4 presents evidence in support of the empirical design. Robustness exercises are conducted in subsection 4.6 to test whether the results are driven by omitted factors—such as political economy influences, technological innovations, migration, or oil discoveries—or by outlying observations.

3.4 Validity of the Empirical Design

This subsection examines several potential threats to the causal interpretation of the results. First, I discuss the bureaucratic fixed costs introduced earlier in the analysis. Second, I examine the assumption related to the timing and nature of firms' investment decisions. Third, I assess the potential bias that could arise if eligibility for BNDES support were based on differing pre-trends across firms. Finally, I explore whether firms' strategic manipulation of their sector or activity choice might confound the results.

Bureaucratic Fixed Costs — The bureaucratic process required to obtain BNDES support was previously described in Section 2.2. To manage these bureaucratic fixed costs, firms require administrative capacity in the form of personnel dedicated to navigating regulatory procedures. Accordingly, one would expect a positive correlation between the take-up of BNDES-subsidized credit and the number of administrative workers within a firm. The data provide information on the number of employees not directly involved in production.[43] I use this variable as a proxy for a firm's administrative workforce[44] and estimate the following regression under various sets of fixed effects:

$$BNDES_{firt} = \alpha + \beta L_{\text{not-prod.}} + \varphi_{(f),(i),(r),(t)} + \epsilon_{firt}, \tag{17}$$

where $\varphi_{(f),(i),(r),(t)}$ denotes different sets of fixed effects that control, among other things, for time-invariant differences (including firm size) as well as time-varying factors common across firms, such as changes in labor regulation and classification. The results, presented in Table 13 in Appendix A.4, indicate a statistically significant positive association be-

[43] These employees of the firm might carry out one of the following tasks: administrative tasks, security tasks, cleaning tasks, accounting, controlling, commercial tasks, transport, construction, gardening etc. (IBGE, 2004).

[44] Even when firms outsource BNDES application tasks to third-party consultants, a substantial internal administrative capacity is still required to supply necessary information and respond to consultants' requests.

tween BNDES take-up and both the number of non-production employees within a firm and the wage bill share of non-production employees.

Firms' Machine Purchases — A potential concern regarding the assumption that firms purchase different machines from different producers at different points in time is that some of these dimensions—such as timing, machine type, or producer—may not actually vary sufficiently. If this is the case, the resulting variation in depreciation may not be random, thereby weakening the identification strategy. Regarding the number of producers accredited by BNDES, there were 11,409 accredited firms in 2025 (BNDES Accredited Firm List, 14/07/2025). Similarly, the number of products eligible for BNDES support in 2025 was 15,970 (BNDES Eligible Product List, 14/07/2025). These figures suggest substantial heterogeneity in both machines and suppliers. Similar patterns of heterogeneity in machines and suppliers have been documented in other settings by previous studies. In the U.S. manufacturing sector (1972–1997), Bernard et al. (2010) report that multiproduct firms produced, on average, 3.5 product varieties. These firms accounted for 87% of total manufacturing output. In India (1989–2003), Goldberg et al. (2010) find that multiproduct firms produced an average of 3 varieties, generating 80% of manufacturing output. Therefore, it is likely that each BNDES-eligible product category in Brazil is served by multiple firms. Given that investment volumes vary substantially across both time and firms in the data, it is unlikely that firms purchase identical machines at exactly the same point in time. Nevertheless, this possibility cannot be entirely ruled out, as it is not directly observed in the data.

Eligibility — To identify the causal effect of BNDES support, it is important to consider the potential confounding role of eligibility for subsidized credit. If the Brazilian government or BNDES designed eligibility criteria while anticipating differential trends across firms or sub-industries, this would violate parallel trends assumptions underlying alternative difference-in-differences specification. In such a case, firms receiving BNDES support might have followed systematically different trajectories than those receiving little or no support. However, since this paper adopts an instrumental variable approach one does not need to impose a parallel trend assumption. Eligibility design based on firms' depreciation patterns is highly implausible, as such patterns are unobservable and only indirectly captured through the method developed here. To mitigate potential bias from differential trends in observables, I implement propensity score-based methods similar in context and methodology to Lage de Sousa and Ottaviano (2018). Details of the implementation are provided in Subsection 4.5.

Firms' Manipulation of Sectoral Choice — Another potential concern is that firms may strategically select their activity/sector in response to the announcement of BNDES

support schemes. If this were the case, the estimated effect of BNDES support would conflate the true treatment effect with a selection effect, thereby biasing the results. This concern is largely mitigated by the fact that the instrumental variable exploits physical variation. Moreover, even if one were concerned that sectoral selection could affect the identifying variation, available evidence indicates that Brazilian firms exhibit a high degree of sectoral immobility.

Using the universe of formal Brazilian firms between 2008 and 2017, Lobel (2024) document that sector switching is relatively rare. Lobel (2024) attributes this immobility to the significant bureaucratic costs associated with switching sectors in Brazil. First, firms must revise their operating agreement, which includes providing documentation to prove activity in the new industry. Second, they are required to obtain new operational licenses from municipal, state, and federal tax authorities. In addition, clearance must be obtained from local tax authorities and civil registry offices. Failure to comply with any part of this process can result in legal sanctions and financial penalties (Lobel, 2024).

4 Empirical Results

This section presents the main results of the empirical analysis. I begin by investigating whether BNDES support generated additional investment. I proceed by examining the first-stage estimates of the instrumental variable strategy. I then turn to the second stage and assess the impact of BNDES support on firm outcomes, with a particular focus on output proxied by cumulative sales revenue growth. Next, I introduce a procedure—based on propensity-score matching and an argument by Rosenbaum (2012)—to refine the construction of comparison groups and conduct a sensitivity analysis. I then examine whether the effects of BNDES support exhibit meaningful heterogeneity along several dimensions identified by previous research as important for industrial policy design. Subsequently, I evaluate the robustness of the results through a series of empirical checks. Finally, I take stock of the findings and discuss their broader implications, laying the groundwork for the theoretical framework needed to interpret the general equilibrium effects.

4.1 Investment

As Criscuolo et al. (2019) demonstrate, increases in firms' economic activity in response to industrial policy support are far from automatic. To assess whether BNDES credit induced investment, I analyze the correlation between firms' investment levels and their receipt of subsidized credit. A positive, statistically significant correlation would provide evidence of investment crowd-in, whereas a statistically insignificant relationship would suggest that crowd-out, i.e., the substitution of financing sources, is the more likely explanation.

Table 5: BNDES Support and Investment-to-Capital Ratio

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
Sub. Credit (0/1)	.16**	.075	.042	.076	.12	.08
	(.071)	(.084)	(.088)	(.099)	(.13)	(.13)
Obs.	86,166	76,283	67,679	59,755	52,402	45,781
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 reports estimates from lag-augmented local projections in long differences of firm-level investment-to-capital ratios on an indicator for BNDES subsidized credit receipt, including firm fixed effects and fixed effects capturing industry and mesoregion trends. Because supported investment projects may span multiple years, changes in investment-to-capital ratios are examined from $t+1$ to $t+6$. The OLS results show a statistically significant positive association between BNDES subsidized credit take-up and investment at $t+1$, consistent with investment crowd-in. This finding, particularly the initial investment spike, is consistent with the hypothesis that BNDES subsidized credit led to additional investment. Nevertheless, it remains possible that firms used BNDES subsidized credit to substitute for more expensive financing of already planned investments, which could explain the relatively small additional output growth.

4.2 First Stage Analysis: Implications for Subsidized Credit Take-Up

I estimate specification (3), where $BNDES_{firt}$ is a binary indicator for take-up (0/1) that remains equal to one after initial take-up; that is, treatment is assumed to be absorbing. The instrumental variable, $depr_shock_{firt-2}$, captures lagged depreciation shocks. Firm fixed effects, although differenced out in the estimation, control for unobserved, time-invariant firm characteristics such as size. Given that the estimation is conducted in long differences, fixed effects capture unobserved heterogeneity in trends across industries, regions and firms.

Table 6: First Stage Analysis

Dep. Variable	Sub. Credit (0/1)					
Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-2}$	-.29***	-.31***	-.31***	-.38***	-.34***	-.38***
	(.053)	(.057)	(.062)	(.056)	(.063)	(.076)
R-squared (1^{st} Stage)	.0031	.0044	.0041	.0047	.0041	.0053
Weak F-test (2^{nd} Stage)	29.41	30.73	25.29	46.98	29.74	24.46
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,200	142,900	125,044	108,143	92,969	78,931
# (Control) Lags	3	3	3	3	3	3
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6 presents the first-stage regression results. All Columns report estimates where the dependent variable is an indicator for whether a firm received BNDES support. The only aspect that varies across columns is the sample, as local projections are estimated at different time horizons, which mechanically reduces the sample size as the horizon length increases. Therefore, differences in magnitudes are driven not by economic forces per se but by the declining number of treated observations and the resulting changes in the sample. The preferred specification is the one with the largest sample, as it contains the most information—specifically, Column (1). Column (1) suggests that a one point increase in the mean deviation of the industry-specific material repair rate at $t-2$ is associated with a 0.29% point reduction in the probability that a firm takes up non-automatically approved subsidized BNDES credit at t.

The results are consistent with the hypothesized mechanism. Firms that do not spend on repairs and are in need of investment are more likely to access, and make greater use of, subsidized credit from BNDES, as indicated by $\hat{\lambda} < 0$. Appendix B.2 presents robustness checks that use alternative time lags of depreciation shocks. The alternative specifications produce results that are consistent with those reported in Table 6.[45] I next report the second-stage results.

[45]This paper focuses on the extensive margin of subsidized BNDES credit—whether a firm takes up subsidized credit—rather than the intensive margin—the amount received—because the latter is difficult to measure, requiring information on loan amounts, repayment periods, alternative credit availability, and interest rate differentials, among other factors. The analysis of the intensive margin is therefore left for future research.

4.3 Second Stage Analysis: Implications for Sales Growth

This section evaluates the impact of BNDES support on firm output by estimating specification (1). Table 7 reports the estimated effects of receiving BNDES support on output, measured in terms of sales revenue.

Table 7: Impact of BNDES Support on Sales

Dep. Variable	% Changes in Sales w.r.t Base Year					
Sample	All Firms					
Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	.024	.033	.038	.078*	.14***	.16***
	(.028)	(.033)	(.033)	(.042)	(.043)	(.043)
2SLS	.012	.19	.13	.069	.069	.042
	(.25)	(.22)	(.25)	(.23)	(.27)	(.33)
Weak F-test (2^{nd} Stage)	29.41	30.73	25.29	46.98	29.74	24.46
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,200	142,900	125,044	108,143	92,969	78,931
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7 reports the estimated effects of BNDES support on cumulative firm sales growth relative to the year prior to treatment $(t-1)$.[46] Results for the baseline specification using alternative lag lengths (e.g., $\tau = 1$, $\tau = 2$, $\tau = 4$) are reported in Appendix B.3.2 and yield similar conclusions.[47] Columns (1)–(6) report the estimated coefficients from either OLS or 2SLS specifications of the structural equation at horizons $t+1$ to $t+6$. For the IV specifications, I include the Kleibergen-Paap F-statistic, which provides a robust test for strong instruments, alongside the 10% critical values (CV 10%). The results differ qualitatively between OLS and 2SLS, and this divergence increases with the horizon of the local projections.

OLS estimates suggest that treatment effects take time to materialize, with a statistically

[46] Treatment is assumed to be absorbing after the first take-up, implying that treatment effects are invariant to the timing of disbursements within the disbursement schedule and that subsequent subsidized loans may overlap with the initial treatment, potentially confounding its estimated effects.

[47] On the one hand, the OLS results broadly point to a similar pattern as in Table 7, with additional significant cumulative sales effects emerging from $t+4/t+5$ onward and increasing over time. On the other hand, the 2SLS results are generally larger in magnitude than the OLS results, but indicate that cumulative sales growth largely stagnates (only in the specification with four lags does it increase from $t+4$ to $t+5$ to $t+6$) if anything, as it never reaches statistical significance This holds despite a strong first stage: the Kleibergen–Paap F-statistic exceeds the conventional threshold in all specifications (with the exception of $t+6$ in the specification with four lags in Column (6) in Table 21).

significant positive association between BNDES support and firm output emerging only from $t + 4$ onward, which warrants further contextualization.

Although fixed effects account for observable, time-invariant firm characteristics and trends in their environment (at the industry and mesoregion levels), *unobserved* factors may still bias the OLS estimates. OLS estimates may be upward biased due to time-varying firm characteristics, particularly credit constraints. Firms facing credit constraints due to poor prior performance, are on a declining trajectory, resulting in a relatively low initial level of sales from which subsequent cumulative sales growth may appear mechanically large. To address these concerns, the 2SLS estimates are reported in the corresponding row below.

The 2SLS estimates indicate that the take-up of subsidized credit is associated with statistically insignificant increases in firms' cumulative sales growth. Importantly, from $t + 4$ onward, these estimates are smaller in magnitude than the corresponding OLS estimates. The previously described channels may account for the smaller 2SLS estimates when comparing the coefficients in the OLS row with the coefficients in the 2SLS row in Columns (4)–(6). Table 31 in Appendix B.5.1 reports the results of the main specification in equation 1 by industry, revealing heterogeneity in treatment effects across industries. In conclusion, the IV estimates suggest that cumulative sales growth six years after BNDES take-up is at most one quarter of the corresponding OLS estimate and is statistically indistinguishable from zero.

To better interpret the difference in magnitudes, the IV estimates can be viewed as identifying a Local Average Treatment Effect (LATE). If firms that are more likely to respond to the intention-to-treat (depreciation shocks) benefit less from BNDES support, then the Local Average Treatment Effect (LATE) will be smaller than the Average Treatment Effect (ATE). The next section examines the mechanisms underlying the causal evidence, which points to at most lower additional sales growth relative to the OLS estimates.

4.4 Mechanisms

This section examines potential mechanisms underlying the finding that subsidized credit is associated with, at most, modest output gains. First, I examine whether subsidized credit alleviated firms' credit constraints. Second, I review whether there is evidence that BNDES subsidized credit was allocated to zombie firms. Third, I analyze whether access to subsidized credit altered firms' input mix, encouraging a shift toward more capital-intensive and potentially more productive production processes. Relatedly, I explore whether BNDES credit spurred production upgrading.

4.4.1 Credit Constraints

The question of whether firms in developing economies face credit constraints—and whether public policy interventions can effectively alleviate them—remains a central issue in the literature (Banerjee and Duflo, 2014). This section investigates whether, in the present context, BNDES succeeded in relaxing firms' credit constraints.[48] I slightly modify the approach of Lage de Sousa and Ottaviano (2018) by defining credit constraints as an indicator equal to one when the ratio of a firm's investment-to-capital ratio to its cash-flow-to-capital ratio is positive. This approach builds on the following observation: in case firms are credit constrained, investment needs to be financed by own liquidity. As argued by Fazzari et al. (1988), credit-constrained firms should exhibit a positive correlation between investment and cash flow. Hence, other things equal, these firms would be expected to show a comparatively higher investment-to-cash flow ratio. As emphasized by Lage de Sousa and Ottaviano (2018), measuring credit constraints with the investment-to-cash-flow sensitivity has been subject to criticism—most prominently by Kaplan and Zingales (1997)—yet alternative methods (e.g., H. Almeida et al. (2004)) require data on firms' cash holdings, which PIA Empresa does not provide.

Table 22 in Appendix B.4 reports results testing whether the effect of subsidized credit take-up is more pronounced among credit-constrained firms. To assess whether subsidized credit has a differential effect on credit-constrained firms, an interaction term between the subsidized credit indicator and the credit-constraint measure is included in the specification. Because this specification includes two potentially endogenous regressors, two instruments are employed: (i) the depreciation shock measure $depr_shock_{firt-2}$, as in the baseline model, and (ii) the interaction of this measure with the credit-constraint variable. The absence of a statistically significant, stronger effect for credit constrained firms in case of take-up of subsidized credit offers a first explanation why the cumulative sales growth gains associated with BNDES subsidized credit take up are positive but not significant. If beneficiary firms were not credit constrained and could have financed their projects elsewhere, BNDES credit likely financed investment that was not optimal. The result stands in slight contrast to the results reported by Lage de Sousa and Ottaviano (2018) who for their examined subsidized credit lines found that treated firms were more likely to be credit constrained in the first place. Several factors may account for this, including differences in the observation period and the restriction to the subsample of non-automatically approved BNDES subsidized credit.

While credit constraints represent a key market failure targeted by industrial policy—illustrated

[48] This analysis adds to the extensive literature on credit constraints in Brazil, from Terra (2003), Aldrighi and Bisinha (2010), and Ambrozio et al. (2017) to the recent study by Lage de Sousa and Ottaviano (2018).

by the South Korean case in Choi and Levchenko (2025)—recent research highlights that keeping credit-constrained firms afloat without exit may lead to the emergence of so-called zombie firms. The next section examines whether the potential zombie status of BNDES-supported firms could explain the modest gains in sales growth associated with the take-up of BNDES subsidized credit.

4.4.2 Zombie Firms

Another explanation behind at best modest sales growth after subsidized credit take-up is that all receiving firms might have been in pretty bad shape already and barely surviving, that is they were "zombies".[49] The seminal paper on zombie lending by Caballero et al. (2008) defines zombie firms as those receiving subsidized (i.e., below-market-rate) credit, which is not a suitable definition for the context of this study. A more appropriate definition for this study is that of Albuquerque and Iyer (2024), who classify firms as zombies if, for at least two consecutive years, they have (i) an interest coverage ratio below one, (ii) above-median leverage within their industry, and (iii) negative real sales growth. However, measures (i) and (ii) are not available in PIA Empresa. I therefore proxy for zombie status using the share of financial costs in total firm costs, while acknowledging the limitations of this approach.[50]

Table 23 in Appendix B.4 does not provide evidence that gains in cumulative sales growth differ systematically with firms' financial cost shares. In this exercise, I instrument BNDES take-up and its interaction with firms' financial cost shares using the instrument and its corresponding interaction term. This specification pushes the IV approach toward its limits in terms of predictive power, as reflected in the declining Kleibergen–Paap weak-instrument F-statistic. Overall, although zombification is difficult to identify with the available data, the results do not suggest that BNDES subsidized credit was primarily allocated to zombie firms, as these firms would be expected to exhibit stronger sales growth (i.e., a statistically significant positive interaction term) given their tight credit constraints.

4.4.3 Production Upgrading

Verhoogen (2023) identifies four channels of production upgrading: learning (process innovation), quality upgrading,[51] technology adoption, and product innovation. The mea-

[49] Although depreciation shocks are independent of zombie status, the latter could still explain the results if all treated firms were zombie firms, as suggested by the definition of Caballero et al. (2008).

[50] The financial cost share may also reflect other confounding factors, such as credit rationing in financial markets.

[51] For a detailed study on quality upgrading by Brazilian exporters, refer to Flach (2016).

surement of these mechanisms is challenging. Most studies proxy upgrading by examining changes in total factor productivity (TFP), yet, as Verhoogen (2023) notes, standard TFP estimation procedures rely on assumptions that often fail in developing-country contexts. Two indicators are particularly salient: First, as in much of the literature, I examine whether different measures of productivity respond to BNDES subsidized credit take-up. Second, because production upgrading implies changes to the production process, it should lead to changes in input composition. As Acemoglu and Restrepo (2022) note, technological change in industrialized economies—especially the United States—has been skill-biased and accompanied by automation and the replacement of low-skilled labor with machines, robots, and software. In other words, if production upgrading occurs, a firm's capital-labor ratio or skill intensity should increase significantly, and I examine this as well.

Overall, the results do not provide evidence of production upgrading among firms that received BNDES credit. Table 24 in Appendix B.4 exhibits that the capital–labor ratio increases persistently following the take-up of subsidized credit, which is consistent with firms undertaking production upgrading. However, Table 25 in Appendix B.4 shows that total factor productivity[52] did not change in response to subsidized credit, which is perhaps the clearest indication that production upgrading is unlikely to have occurred. Moreover, Table 26 in Appendix B.4 shows no increase in labor productivity, measured as sales per worker, and if anything a decline.

Apart from the capital–labor ratio as a preliminary indicator of production upgrading, the various productivity measures reinforce the conclusion that BNDES subsidized credit was not used for production upgrading. A plausible explanation is that production upgrading depends not only on capital investment but also on the availability of other inputs, such as skilled labor and high-quality intermediate inputs.[53]

4.4.4 Employment

Employment is another key outcome frequently emphasized by policymakers as a central objective of industrial policy (Criscuolo et al., 2019).[54] To examine whether employment increased in response to the take-up of BNDES-subsidized credit, Table 27 reports the

[52] I employ the productivity estimation procedure developed by Grieco et al. (2016) and estimate the production function at the two-digit industry level.

[53] As documented by Cox (2025), Brazil's higher education capacity remained below demand for an extended period following the 1996 education reform, likely contributing to shortages of skilled labor needed for production upgrading. Alternatively, limited access to high-quality imported intermediate inputs may also have constrained production upgrading, as suggested by Bas and Paunov (2021) for Ecuador and given Brazil's relatively low trade openness (Squalli and Wilson, 2011).

[54] Skilled labor is a key complementary input for production upgrading (Fieler et al., 2018; Verhoogen, 2023). The increase in the capital–labor ratio following BNDES support may therefore reflect changes in firms' labor demand, especially as modern automation requires new skills and can either increase or reduce employment depending on the production process (Acemoglu and Restrepo, 2018; 2022).

results from regressions of equation 1 using total employment as the outcome variable. The OLS estimates suggests that the magnitude and significance of the positive effect initially increase over time, but fade after approximately six periods. These results are primarily driven by the creation of employment among production (blue-collar) workers, as shown in Table 28 in Appendix B.4. For white-collar workers, Table 29 in Appendix B.4 indicates that the effect is not precisely estimated.

4.4.5 Profits

As firm activity shows little evidence of significant expansion after BNDES support, the central question becomes how the funds were used. Since BNDES conditions loan disbursements on demonstrated progress in project execution, confirming that investments took place is unnecessary. Accordingly, BNDES support could have eased financing constraints, effectively releasing resources that firms could allocate elsewhere. One possibility is that the support translated into higher profits.

Table 30 in Appendix B.4 displays regression results examining the impact of BNDES-subsidized credit on firm profits. Taken together, the results in Table 30 point to increases in profits following subsidized credit take-up; however, these effects are no longer statistically significant six years after treatment. The estimation is carried out in levels as profits can turn negative and make meaningful long-differencing impossible.[55] The increase in profits, together with the simultaneous increase in employment and the capital–labor ratio despite at most modest sales gains reported earlier, is consistent with the "quiet life hypothesis,"[56] first formulated by Hicks (1935), which states that managers do not always maximize only profits when competitive pressure is weak. Indeed, the "quiet-life hypothesis" can be rationalized by managerial theories of the firm. For example, in the seminal model of Williamson (1963), managers maximize employment, prestige, and discretionary profits subject to reported profits exceeding the minimum profits required by shareholders plus taxes.[57]

4.5 Sensitivity Analysis

To assess whether BNDES support enables Brazilian manufacturers to undertake additional productive investments that would not have occurred otherwise, it is instructive

[55] The specification does not include firm fixed effects, which will be incorporated in future versions of this study.

[56] When competitive pressure is weak, managers may prefer a "quiet life" by avoiding difficult decisions, innovation, or aggressive expansion rather than maximizing firm performance. Hicks (1935, p.8) famously stated, "The best of all monopoly profits is a quiet life."

[57] Williamson (1963) shows that the first-order conditions of this optimization problem imply that firms continue to employ some workers even where the marginal product of labor is below its marginal cost. This implies a degree of substitutability between profits and employment in his model.

to examine the characteristics of potential treatment and control groups. According to Rosenbaum (2012), researchers often work with datasets that do not represent the full "natural" population. As a result, there is no compelling reason to estimate the treatment effect for all observed units in the dataset. This paper aims to estimate the treatment effects of non-automatically approved BNDES support for manufacturing projects in Brazil. However, the PIA dataset does not include all firms that received BNDES support, nor does it cover the entire universe of Brazilian manufacturing firms. Additionally, it omits firms engaged in manufacturing activities whose primary classification lies in the service sector. As a result, the empirical analysis must be restricted to a subsample of the broader population of Brazilian firms engaged in manufacturing activities. Nonetheless, it is not immediately clear which subsample is most appropriate for estimating treatment effects, and this choice requires careful consideration.

I define treatment and control groups following the methodology of Lage de Sousa and Ottaviano (2018), who examine the FINEM and Automático subsidized credit lines of BNDES, with slight modifications to tailor their framework to the context of this study—particularly to accommodate the longer time span covered in the analysis and to enhance the robustness of the results. Ideally, the goal is to answer the counterfactual question: what would have happened to the granted firms' output had they not received BNDES support—making them, comparable to the non-granted, firms in the control group in this regard. Answering this question first requires identifying the group of treated firms for which sufficient information is available to support a robust analysis. Next, it is essential to define a "valid" counterfactual group that provides an appropriate benchmark for comparison. Verifying whether firms have implemented their projects is, by contrast, redundant. As previously discussed, BNDES disburses funds in installments, and—except for the first payment—subsequent disbursements are conditional on the firm's compliance with the agreed implementation plan.

A detailed account of the refinement of the comparison groups can be found in Appendix B.6. Appendix B.6.1 discusses how this study departs from the taxonomy of Lage de Sousa and Ottaviano (2018) in refining the treatment group. Appendix B.6.2 examines alternative definitions of control groups and how treated and control groups differ prior to treatment. Appendix B.6.4 explains the choice of variables used to predict treatment take-up in the propensity score matching, how many firms are ultimately matched, and how similar these firms are in terms of observable characteristics thereafter. The matched firms exhibit similar trends in profits and productivity; however, while they show comparable levels of productivity and credit constraints, they do not display similar levels in proxies for firm size, which remains a caveat. Finally, the results in Appendix B.6.5 can be cautiously interpreted as reinforcing the earlier main findings. On the one hand, the propensity score matching procedure yields a sample that is small. Together with the high-dimensional

fixed effects, this may explain why the Kleibergen–Paap weak-IV F-statistic falls slightly below its conventional threshold for outcome horizons $t+2$ through $t+5$, and substantially below it for horizons $t+1$ and $t+6$. For outcome horizons $t+2$ through $t+5$, although the 2SLS coefficients are implausibly large—likely reflecting the amplification of weak-IV bias in small samples described by Staiger and Stock (1997)[58] —their magnitudes do not increase further, consistent with the interpretation of the main results in Table 7. On the other hand, the OLS estimates suggest sales growth gains that are only slightly above zero and diminish further over time. All in all, the refinement of comparison groups supports the hypothesis that, following the expansion of non-automatically subsidized credit, firms increased their output only modestly on average, at best.

4.6 Robustness

During the period of BNDES subsidized credit expansion, other factors also contributed to Brazil's economic development and firm growth. The robustness of the empirical results presented above may be further challenged by two additional threats not yet addressed: outliers and omitted variable bias. To address these robustness concerns, this section identifies the dimensions along which such issues may arise and proposes corresponding robustness checks.

4.6.1 Omitted Variables

In contrast, unobserved productivity shocks—such as shifts in the political economy, agricultural innovations, or oil discoveries—may negatively affect aggregate manufacturing productivity and activity. This could cause the estimation strategy to understate the true impact of BNDES support.

Political Economy - A natural concern for identifying the effects of industrial policy is regulatory capture. It is indeed plausible that firms contribute to political campaigns in exchange for inclusion in industrial policy programs. In such a case, industrial policy is compromised. To address such concerns, Lobel (2024) investigates whether firm contributions to political campaigns are correlated with eligibility for labor subsidies implemented during the 2010s. He finds no evidence of such a relationship and rejects the hypothesis that political contributions influenced subsidy allocation. From this perspective, the fact that labor subsidies are not associated with corruption alleviates concerns that BNDES capital subsidies are subject to corruption.
Carvalho (2012) shows that, between 1995 and 2006, firms eligible for BNDES funding expanded employment in politically attractive regions prior to elections. This, in turn,

[58]Staiger and Stock (1997) show that under weak-instrument asymptotics, the first-stage coefficients shrink at the rate $\sqrt{n}$ (where n denotes the sample size).

led to more favorable lending conditions. Note that the depreciation shocks used as exogenous variation in the identification strategy should, a priori, not be correlated with political connections, as they reflect random variation driven by physical wear and tear. Carvalho (2012) focuses on federal and state-level elections. In the absence of data on firm-level political contributions, the best available approach is to account for concerns about influence-buying during elections by including a set of fixed effects for the corresponding elections, under the hypothesis that all affected firms are similarly inclined to engage in rent-seeking. Consequently, year fixed effects capture federal election years, while interactions between year and state fixed effects account for state-level elections. The current set of fixed effects in equation (1) captures only linear trends at the industry and mesoregion levels and therefore does not control for year-specific shocks at these levels. Appendix B.6.6 reports the results of a specification that additionally includes interactions between year and industry fixed effects, as well as between year and mesoregion fixed effects (a smaller unit than states), thereby capturing omitted factors at these levels. The results show that the effect of BNDES support remains largely unchanged. Effect magnitudes are 1–5 percentage points higher, and the only notable difference from the baseline results is that the 2SLS estimate at $t+6$ exceeds the OLS coefficient but is not statistically significant. Nevertheless, the magnitude of the coefficients suggests that subsidized firms do not outperform the average Brazilian firm, as the 2SLS estimates are not statistically significant despite adequate instrument strength. To sum up, political factors should not be driving the estimated effects.

Innovation - Innovation is another factor that could confound the estimated effect of BNDES support on firm expansion, as it may simultaneously influence both firm growth and access to subsidized credit. On the one hand, technological innovations increase firm productivity and, subsequently, output. On the other hand, sectors characterized by high rates of technological innovation are particularly attractive targets for industrial policy. Therefore, technological innovations can lead to an upward bias of the estimate of the impact of BNDES support on firm growth.
Lower-income economies are further from the technological frontier (Caselli and Coleman, 2006). Using data from the World Bank's Investment Climate Survey of Brazilian manufacturing firms from 2000–2002, Goedhuys and Veugelers (2012) investigate whether firms pursue product innovations through internal development ("technology make") or external acquisition ("technology buy") strategies. They find that successful process and product innovations in Brazilian firms occur predominantly through "technology buy" strategies.[59] Therefore, it is reasonable to assume that technological innovations affect all firms within a given industry uniformly. Consequently, since the regressions are estimated at the firm

[59]More precisely, through the purchase of machinery and equipment. For more details, see Goedhuys and Veugelers (2012).

level, interactions between year and industry fixed effects should absorb the influence of technological innovations on the estimated effect of BNDES support on firm sales. Appendix B.6.6 presents the results of this regression and demonstrates that the estimated effect of BNDES support remains largely unchanged.

Agricultural innovations - In theory, income-maximizing workers relocate to sectors offering the highest wages. As long as this process is random, it does not pose a threat to the identification strategies employed in this paper. However, agricultural innovation is a potential channel that, if unaccounted for, could introduce a downward bias in the estimation of the BNDES support's impact on firm productivity. This is because agricultural innovations—such as the genetically engineered soybeans studied by Bustos et al. (2020)—may increase agricultural productivity, leading to higher wages in the agricultural sector. In turn, manufacturing workers may be drawn into agriculture, a mechanism that could dampen the gains from subsidized credit. In states such as São Paulo and Minas Gerais, some mesoregions simultaneously exhibit strong manufacturing and agricultural sectors. Therefore, including mesoregion–year fixed effects that capture agricultural innovations would help account for this potential confounding factor. The results of these regressions, reported in Appendix B.6.6, indicate that the effects of BNDES subsidized credit on sales remain largely unchanged once geographic–year-specific shocks are controlled for.

Migration - Significant migration occurred from the Northeastern states to the Southeastern states (IBGE, 2025). At the same time, the Southeastern states concentrated the majority of Brazil's manufacturing hubs. Different segments of firms may be affected by this phenomenon in distinct ways. The included mesoregion-trends fixed effects should account for the potential confounding influence of migration. As reported in Appendix B.6.6 and discussed earlier in this section, the results from these regressions show that the estimated effects of BNDES subsidized credit on firm sales remain largely unchanged.

Oil Discovery - Brazil possesses the largest recoverable ultra-deepwater oil reserves in the world and is Latin America's leading oil producer (ITA, 2023). Consequently, oil discoveries during the observation period may introduce a simultaneity problem in estimating the impact of BNDES support, as both may influence firm outcomes contemporaneously. Therefore, mesoregion–year fixed effects should mitigate the potential confounding effects of oil discoveries. As shown in Appendix B.6.6, the estimated effects of BNDES subsidized credit on sales are not materially affected.

Omitted Policies - It is plausible that Brazilian firms were simultaneously affected by other economic policies over the observation period, which may confound the estimated

effects. Over the past two decades, industrial policy in Brazil has primarily been implemented by governments led by the *Partido dos Trabalhadores* (PT, Workers' Party), rather than by administrations from other political parties. The first government of Luiz Inácio Lula da Silva launched the *Política Industrial, Tecnológica e de Comércio Exterior* (PITCE) in 2004 (M. Almeida, 2011). In 2008, PITCE was replaced by the broader *Política de Desenvolvimento Produtivo* (PDP) (M. Almeida, 2011). Subsequently, the administration of Dilma Rousseff introduced the *Plano Brasil Maior* in 2011, which remained in effect until 2014. Another feature of the PT governments' economic agendas was the *Programa de Aceleração do Crescimento* (PAC). PAC was meant to accelerate economic growth via strategic infrastructure investments in various dimensions such as transport, energy, housing and others. PAC investments were not directed at individual firms and, as such, are unlikely to be systematically related to lagged depreciation shocks—or vice versa.

Within a production function framework, the government can intervene by targeting either the output side or the input side of a firm. On the input side, there are three main channels through which the government could theoretically influence firm performance: capital subsidies, labor subsidies, and intermediate input subsidies.
Concerning capital subsidies, previous studies[60] have noted that BNDES is the largest financial institution in Brazil providing loans for long-term projects. Consequently, I assume that BNDES loans constitute the main policy instrument affecting firms' productivity.[61] Moreover, BNDES played a central role in the implementation of the PITCE and PDP industrial policies under the Lula da Silva administrations (M. Almeida, 2011), as well as in the *Plano Brasil Maior* introduced by the Dilma Rousseff government. In this paper, I relax that assumption by considering BNDES loans as the primary policy tool for subsidizing firm capital, rather than the sole policy instrument affecting productivity. Given that non-automatic loans are significantly larger than the average BNDES subsidized loan, this assumption appears reasonable.
For labor subsidies to confound the estimated effect of BNDES support on firm sales, they would need to respond to lagged depreciation shocks—and vice versa. This seems unlikely, given that depreciation is a capital-related phenomenon, whereas labor subsidies are typically designed to influence employment outcomes rather than respond to capital stock variations. A similar argument applies to intermediate input subsidies. Moreover, in the context of the PBM, intermediate input subsidies primarily target specific product categories rather than individual firms. As a result, their effects would be at least partially absorbed by industry fixed effects.

[60] See, for example, Cavalcanti and Vaz (2017) and Lage de Sousa and Ottaviano (2018).
[61] Lage de Sousa and Ottaviano (2018), for example, follows the same approach.

4.6.2 Outliers

One might suspect that individual observations that differ substantially from the rest are driving the results. In particular, observations on large firms, which account for the bulk of manufacturing activity and the vast majority of treated firms, may exert a disproportionate influence on the results. To address these concerns, Appendix B.6.7 presents results from regressions based on the main specification (1), excluding the top 0.01%, 0.1%, and 0.5% of firm observations by size.[62]

The first stage remains comfortably strong in 89% of cases; only for the horizons $t+5$ and $t+6$ in the sample excluding the largest 0.01% of observations does the Kleibergen–Paap weak-instrument F-statistic fall below the conventional threshold. Excluding the largest 0.01% of firms, the results remain largely unchanged relative to the main specification. However, once the top 0.1% and 0.5% of firms ranked by sales are excluded, the coefficients turn strongly negative but are imprecisely estimated, even though the Kleibergen–Paap weak-instrument F-statistic remains above conventional thresholds.[63] A straightforward explanation for this finding is that the number of treated firms is drastically reduced, and the high dimensionality of the fixed effects may restrict comparisons to a small set of firm observations, rendering the estimates particularly sensitive to outliers. Nevertheless, it can be concluded that the baseline results are primarily driven by large firms.

Overall, the previously presented robustness checks support and refine the main baseline results. More precisely, the estimated LATE effect primarily pertains to the largest firms in the sample of stayers.

5 Empirical Strategy for External Economies of Scale

Building on conventional productivity estimation procedures in the industrial organization literature, Reshef and Tepper (2026) develop a framework to infer EES from firm-level data. This section extends the framework by developing a decomposition that distinguishes between an industry-specific component (in line with Marshallian externalities emphasized in the international trade literature) and a location-specific component (in line with Jacobs externalities emphasized in the urban economics literature).

[62] In future versions of the paper, I will report these regressions after excluding all observations for the identified outlier firms.

[63] This stands in stark contrast to the results of the sensitivity analysis based on the PSM sample.

5.1 Estimation Procedure

The starting point in Reshef and Tepper (2026) is the assumption that productivity follows a Markov process, as is standard in the production function estimation literature. Accordingly, the Markov process governing log productivity can be written as follows:

$$\omega_{firt} = h(\omega_{firt-1}) + \eta_{firt} \tag{18}$$

where $h(\cdot)$ is a function capturing the persistence of firm-level productivity, and η_{firt} is an unanticipated productivity shock at time $t-1$ that affects firm-level productivity. Recall that researchers typically estimate production functions at the industry level due to insufficient observations at the firm level. I follow this approach and conduct the estimation at the industry level using the procedure of Grieco et al. (2016) (hereafter GLZ).

In anticipation of the model developed in Section 6, I define log productivity $\omega_{firt} = \log(A_{firt}E_{irt})$ as comprising two components: an internal component, $\log(A_{firt}) = a_{firt}$, capturing factors under the control of the firm's management, and an externality component, $\log(E_{irt}) = e_{irt}$, which lies beyond managerial control. Introducing industry–mesoregion–year fixed effects in the estimation of (18) would allow one to isolate the e_{irt} term within these fixed effects. Due to data constraints, it is not possible to estimate an e_{irt} term, as this would require estimating production functions at the industry–mesoregion level. Instead, I estimate an e_{it} term by including industry–year fixed effects in equation (18):

$$\omega_{firt} = e_{it} + h(a_{firt-1}) + \eta_{firt}. \tag{19}$$

The industry–year fixed effects capture all external factors that affect firms' productivity, are not internalized by firm management, and operate uniformly within an industry. In other words, the e_{it} terms capture productivity-enhancing (or -reducing), "technological" (in the terminology of Greenwald and Stiglitz (1986)) externalities. Accordingly, I estimate equation (19) using the productivity estimates recovered from the procedure of Grieco et al. (2016).

In a next step, I recover the industry–time fixed-effects e_{it} and pool them across industries. I observe a total of 22 industries over 18 years. Including the year 2001 to avoid losing one year of observations due to the Markov process yields 396 observations of industry–time fixed effects. Following, I adapt the ideas of Mundlak (1978) and regress these industry–time fixed effects on my variables of interest. In the following subsection, I explain the derivation of the structural equation that allows to study the level of EES.

5.1.1 The Level of External Economies of Scale

As in Kucheryavyy et al. (2023) and Bartelme, Costinot, et al. (2024), I assume EES at a certain entity-level to be a function of the entity size measured as the amount of entity-labor. Hence, to capture industry-specific EES, I include industry-employment in the function. Similarly, to detect location-specific EES, I add mesoregion-employment in the function which can then be written as:

$$E_{it} = F(L_{it}, L_{rt}). \tag{20}$$

Before bringing equation (20) via a structural estimation to the data, a few transformations need to be undertaken to address some theoretical and econometric issues.
First, equation (20) suffers from double-counting: the employment of industry i within region r shows up in both employment terms L_{it} and in L_{rt}. Therefore, one needs to subtract industry i's employment in region r (L_{irt}) from either L_{it} or L_{rt}. For reasons that will become clear later on, I subtract L_{irt} from L_{rt} and write:

$$E_{it} = F(L_{it}, L_{-irt}). \tag{21}$$

where $L_{-irt} = L_{rt} - L_{irt}$.
Second, there is a dimensionality issue as the e_{it} do not take into account the geography of economic activity. For this reason, one needs to weight the L_{-irt} terms depending on the exposure of industry i to region r and sum over the regions $r \in R$.[64] Consequently, I weight the L_{-irt} by the employment share of industry i in region r:

$$E_{it} = F(L_{it}, \sum_{r \in R} s_{irt} L_{-irt}). \tag{22}$$

where $s_{irt} = \frac{L_{-irt}}{L_{rt}}$.[65]
Third, one needs to assume functional forms. To make progress in the study of the level of EES, I assume a Cobb-Douglas functional form for the "externality production function":

$$E_{it} = L_{it}^{\alpha} (\sum_{r \in R} s_{irt} L_{-irt})^{\beta}. \tag{23}$$

[64] This sum captures both the amount of regional employment (L_{-irt}) to which industry i is exposed and the intensity of that exposure, as measured by its employment shares (s_{irt}). In other words, it indicates whether an industry is concentrated in large regions in terms of employment and therefore more exposed to regional externalities.

[65] A natural alternative would be to define the share as $s_{irt} = \frac{L_{-irt}}{L_{it}}$, capturing the share of employment in industry i located in region r. In this case, summing over regions $r \in R$ would yield a different, and perhaps more direct, measure of industry i's exposure to regional externalities. However, after log-linearization, this definition leads to perfect multicollinearity in the subsequent regression. To see the source of the problem, note that log-linearizing (23) yields $\alpha \log(L_{it}) - \beta \log(L_{it}) + \beta \log\big(\sum_{r \in R} L_{irt} L_{-irt}\big)$.

Cobb-Douglas functional forms in the context of EES are not new to the literature. In their Armington model with external economies of scale, Kucheryavyy et al. (2023) assume labor productivity to depend on industry labor to the power of a parameter that governs the strength of EES in a given industry. Similarly, here, the parameters α and β govern the strength of EES at their respective level ($\alpha \rightarrow$ industry-level & $\beta \rightarrow$ region-level).

Fourth, the estimated e_{it} terms are expressed in logs. Log-linearizing the RHS of the equation (23) yields:

$$e_{it} = \alpha \; log(L_{it}) + \beta \; log(\sum_{r \in R} s_{irt} L_{-irt}). \tag{24}$$

Fifth, it will be more instructive to regress changes of externalities e_{it} on changes of industry and regional employment. Consequently, equation (23) can be rewritten in terms of changes in e_{it} on the left-hand side (LHS) and changes in the corresponding terms on the right-hand side (RHS), yielding the following regression:

$$\Delta e_{it} = \alpha \Delta log(L_{it}) + \beta \Delta log(\sum_{r \in R} s_{irt} L_{-irt}) + u_{it}. \tag{25}$$

where u_{it} denotes the error term. Table 8 reports the results of a regression similar to (25) but estimated in levels. Future versions of this paper will present results corresponding exactly to (25).

Table 8: EES Level Analysis

Dep. Variable	(Log) Externalities (GLZ)	
Sample	Stayers	
Estimated in	Levels	
	(1)	(2)
(Log) Industry labor	.87**	.55
	(.38)	(1.8)
(Log) Mesoregion labor	-.86**	-.59
	(.38)	(1.8)
Obs.	456	456
R-squared	.021	.016
Industry FE	No	Yes
Year FE	No	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the industry-year-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

On the one hand, the number of employees within an industry is positively associated with the externality proxy based on the GLZ productivity estimator. On the other hand, the number of employees within a mesoregion is negatively associated with the same externality proxy. In other words, for a given firm, an increase in industry-level employment is

associated with higher productivity, as the productivity-enhancing externalities common to firms within that industry intensify. In terms of magnitudes, a one-percentage-point increase in an industry's workforce is associated with a 0.55 percentage point increase in the level of externalities within that industry, on average. In turn, under the linear production technology described in Section 6, this implies that firms in that industry become approximately 0.55% more productive due to industry-specific EES. This average effect can be explained by a range of mechanisms, from within-industry specialization (e.g., the car industry with its highly specialized supply chains) to productivity spillovers (e.g., Henry Ford's introduction of the production line, which primarily benefited the car industry).

What mechanisms could explain the negative association between the externality proxy and mesoregion-level labor? To address this question, it is important to note that the externality proxy reflects the net effect of both productivity-enhancing and productivity-reducing externalities. As well established in the urban economics literature, both agglomeration forces and congestion forces can be important. In case of a negative association between mesoregion labor and the level of externalities, it must then be that congestion forces are dominating agglomeration forces at the mesoregion level in the studied sample.

The results presented in Table 8 do not constitute causal evidence. OLS estimates in Section 5 may be affected by endogeneity, as unobserved shocks may simultaneously affect the different labor aggregates. For example, input–output linkages—such as inter-industry and inter-mesoregional connections—may simultaneously affect both labor aggregates and the externality proxy. Moreover, as argued by Helm (2020), the thick labor market effects postulated by Marshall (1890) are a key driver of EES and may simultaneously influence α and β. For these and other reasons, establishing a causal estimate of industry specific EES and mesoregion specific EES proves challenging. Common identification strategies employed in the literature such as Helm (2020), Bartelme, Costinot, et al. (2024), and Mion and Silva (2024) use trade-induced demand-side shocks that yield plausibly exogenous variation across industries. However, identifying demand-side shocks that can be cleanly separated into purely industry-specific or purely mesoregion-specific components (excluding a given industry) is nearly impossible, due to the joint exposure of industries to mesoregions and vice versa. To the best of my knowledge, no study has achieved this, and shift–share IV approaches are not applicable in this context by construction. Consequently, the approach applied here, while not without caveats, appears to be the best feasible option at present.

5.1.2 Are EES industry, region or industry-region specific?

When studying the level of EES, it is possible that they are specific to each industry–mesoregion pair. However, industry–mesoregion labor cannot be introduced separately into (25) to identify an industry–mesoregion-specific EES component, as the dependent variable e_{it} varies only at the industry–year level. Nevertheless, the most feasible approach is to mechanically decompose the fitted effect α of changes in total industry log labor $\Delta log(L_{it})$ into the portion attributable to labor in a given region r and the portion attributable to labor elsewhere. This can be done with a few auxiliary algebraic transformation of the $\Delta log(L_{it})$ term that are detailed in Appendix B.7. This allows one to rewrite the structural equation (25) as:

$$\Delta e_{it} = \alpha\left(\frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{it-1}} \times \Delta log(L_{irt})\right) + \beta \Delta log(\sum_{r \in R} s_{irt} L_{-irt}). \quad (26)$$

Equation (26) shows that $\Delta \log(L_{it})$ can be decomposed. On the one hand, it consists of log changes in industry employment excluding a given region, $\Delta \log(L_{i-rt})$, weighted by its share in total industry employment at $t-1$, L_{i-rt-1}/L_{it-1}. On the other hand, it consists of log changes in industry employment within a given region, $\Delta \log(L_{irt})$, weighted by its share in total industry employment at $t-1$, L_{irt-1}/L_{it-1}.

5.2 Taking Stock

The empirical analysis provides novel evidence on the effectiveness of firm-specific industrial policy delivered through subsidized credit, as well as on the level of EES. Suggestive evidence indicates that investment increases following the take-up of non-automatically approved BNDES support. BNDES support generates, at best, modest increases in firm sales growth, with the effects driven primarily by large firms. The absence of evidence supporting the operation of primary mechanisms potentially driving firm expansion—such as technological upgrading or the alleviation of firms' credit constraints—reinforces these findings. Additionally, the findings indicate that the impact of BNDES support is heterogeneous across industries, with OLS and 2SLS estimates suggesting significantly larger sales growth for firms in a subset of industries. Although BNDES credit take-up generates only modest increases in sales, there is evidence that it raises employment—particularly in production-related occupations—and profits, consistent with the predictions of managerial theories of the firm, such as Williamson (1963).

These estimates identify the relative firm-level effects of BNDES support but cannot assess its aggregate effects or whether industrial policy was optimally targeted. Answering these questions requires accounting for general equilibrium mechanisms, including EES and changes in market structure. For example, the output expansion of a single firm can

generate industry- or mesoregion-wide EES. At the same time, the expansion of larger and more productive firms may alter their comparative advantage, potentially crowding out smaller competitors and reducing their market share. Moreover, the at-best modest output expansion, combined with a potential increase in firm profits following BNDES support, raises the question of whether firms increased markups after gaining access to subsidized credit, thereby highlighting the role of underlying general equilibrium dynamics.

To assess these broader consequences, I develop a multi-industry, multi-region model in which each region corresponds to a mesoregion, and each industry corresponds to a 2-digit industry in the data. The model highlights the novel margins revealed by the empirical analysis—namely, an at-best modest increase in firm output (particularly among large firms) without evidence of production upgrading, an increase in (production) employment, a partially significant rise in profits, and an implied increase in markups—while providing a general equilibrium framework for counterfactual analysis. Since BNDES support is targeted at domestic firms, and given that Brazil is among the most closed economies within middle- and high-income countries (OECD, 2023), the model abstracts from explicitly incorporating an open-economy setting.

The empirical findings inform the structural analysis in two key ways. First, they motivate several of the model's core mechanisms and assumptions. Second, they provide well-identified empirical moments that are used in the calibration of the quantitative model to carry out the counterfactual analysis. Because each industry and region in the model correspond to a 2-digit industry and a mesoregion, respectively, the model can be used to assess the optimality of alternative industrial policy targeting strategies. The empirical strategy for taking the model to the data is partly inspired by Choi and Levchenko (2025). Finally, the quantitative model enables a counterfactual analysis in which the targeting strategy of industrial policy is altered in isolation, while all other fundamentals are held constant. This exercise quantifies the extent to which the expansion of BNDES subsidized credit between 2003 and 2014 was optimally targeted.

6 Quantitative Framework

My main empirical finding is that subsidized credit extended during the expansion of non-automatically approved BNDES loans, implemented as part of the *PT governments'* industrial strategies, at best modestly increased cumulative sales growth. Additionally, there is suggestive evidence that it led to higher employment and, to some extent, increased profits. Given that BNDES support was received mainly by large firms, I interpret this as evidence that subsidized credit primarily enabled these firms to increase their

markups as part of their response to receiving subsidized credit, while the observed rise in employment may have—albeit modestly—generated EES. I then develop a theoretical framework that captures these empirical patterns and use it to assess whether BNDES targeting at the macro margin could have been improved in counterfactual scenarios. The main mechanism in the model are EES (Bartelme, Costinot, et al., 2024):[66] firms do not internalize that expanding their current labor force increases the scale of their sector or location, thereby raising productivity for themselves and their peers. Consequently, short-run reallocations of subsidies toward "EES-generating firms" may lead to overall welfare gains in the long run.[67] Firms also exhibit heterogeneous markups depending on their market share. Thus, firms may respond to subsidies by expanding output to varying degrees, as they may simultaneously increase their markups. These features are consistent with both the formal econometric evidence and the narrative historical record. In such an environment, industrial policy plays a meaningful role. Government subsidies can reallocate resources toward "EES-generating firms"—those that respond to subsidies by substantially increasing employment while raising markups only modestly—within industry–region pairs characterized by high EES growth potential. In turn, this may generate welfare gains from EES that would otherwise be unattainable in a purely market-driven allocation. Nevertheless, industrial policy interventions are costly, as they require the government to raise tax revenue. Hence, as with all major policy initiatives, the central question is whether the benefits exceed the costs. I discipline the model by deriving the estimating equations used in the empirical analysis, allowing the key parameters of the model to be recovered from the econometric estimates.

6.1 Model

This section develops a static general equilibrium model combining elements from Atkeson and Burstein (2008), Gaubert (2018), and Bartelme, Costinot, et al. (2019).

Set-Up - The basic set-up builds on Gaubert (2018). A closed economy consists of locations called regions $r \in R$ in which production takes place. The fixed land supply of regions acts as a natural congestion force. This imaginary economy comprises a variety of industries $i \in I$, each populated by firms that are heterogeneous in productivity. These

[66] For additional papers emphasizing EES as the primary mechanism, see Gaubert (2018) and Bartelme, Jiang, et al. (2025).

[67] Brazilian firms' competitiveness is emphasized in the narrative accounts of the goals stated by the Brazilian governments behind its industrial strategies (Terra and Canêdo Pinheiro, 2016). Moreover, this interpretation is consistent with historical narrative evidence suggesting that, at the time, Brazilian firms had lost competitiveness in international markets (Barbosa-Filho, 2020). At least since Krugman (1980), it has been well established that competitiveness is intrinsically linked to the exploitation of economies of scale. Since the factors underlying internal economies of scale are largely under managerial control, industrial policy in the Brazilian context studied here primarily targets the realization of EES, as these represent the externalities underlying the market failures such policies aim to address.

firms $f \in F$ carry out production using local "equipped labor" (Alvarez and Lucas, 2007)[68] as only input.

There is one time period. Within the time period, the order of events is the following: First, the government raises tax revenue to finance industrial policy subsidies. Second, the government takes the initial allocation of economic activity as given and allocates industrial policy support in the form of subsidies to maximize the generation and exploitation of EES. Third, upon learning the amount of subsidy allocated to them, firms choose output and input levels accordingly. The rationale to conduct industrial policy for a benevolent government relies on otherwise unrealized EES that yield otherwise unattainable welfare gains. Consequently, increased productivity leads to increased output. Finally, more effective exploitation of EES leads to higher output and, consequently, higher welfare relative to the laissez-faire equilibrium.

Externalities - As a preliminary step toward operationalizing the alternative conceptualization of EES in Bartelme, Costinot, et al. (2019), I assume the presence of externalities, following Greenwald and Stiglitz (1986), which I restrict to be technological and productivity-enhancing in nature.[69] I extend the framework by allowing these externalities to be generated at the industry–region level by the corresponding industry and regional labor. In doing so, I depart from the earlier trade and urban economics literature by assuming that EES consist of both a location-specific and an industry-specific component. In line with most of the existing literature, I assume that EES depend on labor rather than on output.[70] For the functional form, I adopt a power function specification,[71] which I combine in a Cobb–Douglas structure to account for the two EES-generating inputs—industrial and regional labor:

$$E_{ir} = E(L_i, L_r) = L_i^{\alpha} L_r^{\beta} \tag{27}$$

where α denotes the industry-specific component of EES, and β denotes the location-specific component. For simplicity, I assume that EES affect all firms homogeneously; that is, within a given industry-region pair, each firm benefits equally from EES in proportional

[68] See also Choi and Levchenko (2025) in the context of industrial policy.

[69] As noted by Greenwald and Stiglitz (1986), externalities can be distinguished into technological externalities—those that have a direct impact on others' profits or utility—and pecuniary externalities, which affect others indirectly through prices. In an undistorted economy, pecuniary externalities do not reduce efficiency, as distributional effects net out: gains—for example, to firms whose prices rise—are exactly offset by losses to those who must pay higher prices in case prices reflect correctly opportunity costs. However, in distorted economies—characterized by imperfect information and incomplete markets, as is the case for all real-world economies—pecuniary externalities can lead to efficiency losses. Nevertheless, I distinguish between technological and pecuniary externalities.

[70] For other studies modelling EES as a function of labor rather than output, see Gaubert (2018), Kucheryavyy et al. (2023), Bartelme, Costinot, et al. (2024), and Bartelme, Jiang, et al. (2025).

[71] For other papers assuming a power functional form for EES, see Kucheryavyy et al. (2023), Bartelme, Costinot, et al. (2024), and Bartelme, Jiang, et al. (2025).

terms. Firms determine their location by resolving the trade-off between location-specific externalities and local input prices. Because firms face heterogeneous externalities and input prices, they make heterogeneous location choices. Firms take the level of externalities in a given location as exogenous. However, they neglect their own contribution to these externalities. As a result, firms do not fully internalize EES, leaving room for welfare-improving policy interventions.
Note that the chosen specification also captures EES mechanisms that are simultaneously industry- and location-specific. This is because the industry–location-specific labor force, L_{ir}, is simultaneously part of both L_r and L_i. Hence, increases in L_{ir} have a more than proportional impact on the generation of EES affecting firms within a given industry–location pair. Consequently, the specification adopted here allows for a more general formulation of EES than that in Gaubert (2018), where only location-specific EES exist and their impact varies across sectors according to a sector-specific parameter indexing industries from low- to high-EES sectors.

Workers - The worker problem follows Atkeson and Burstein (2008) but replaces leisure with housing H_r (inspired by Gaubert (2018)). Within each region r, L_r identical workers each supply one unit of equipped labor inelastically. Hence, the corresponding utility maximization problem can be written as that solved by a representative agent in a given region r. The worker problem is given by:

$$\max_{c,h_r} \quad \left(\frac{c}{\vartheta}\right)^{\vartheta}\left(\frac{h_r}{1-\vartheta}\right)^{1-\vartheta} \quad \text{s.t.} \quad P^C c + P_r^H h_r = w_r(L_r) \tag{28}$$

where ϑ denotes the share of income spent on final-good consumption, c the quantity of the final good per worker, and h_r housing per worker in a given region r. The budget constraint states that a worker's expenditure on the final good, $P^C c$, and on housing, $P_r^H h_r$, must equal its labor income, w_r, which—following Gaubert (2018)—is also a function of the regional labor force L_r. As in Gaubert (2018), workers are freely mobile; hence, in equilibrium, utility is equalized across all inhabited locations at a common level $\bar{U}$.

Regions - All regions are identical ex ante, and their characteristics are modeled following Gaubert (2018). Each region is endowed with a fixed stock of land, normalized to one. Distance between regions is irrelevant, as goods produced in the economy are either freely traded across regions within the country or, in the case of housing, non-tradable. Land is used to produce housing, which is perfectly divisible and consumed by workers. Atomistic landowners produce housing H_r^S by combining their land endowment γ with local labor L_r^H. In doing so, they use the following Cobb-Douglas housing production function:

$$H_r^S = \gamma^b \left(\frac{L_r^H}{1-b}\right)^{1-b} \tag{29}$$

Landowners are price takers in both the housing and labor markets, taking P_r^H and $w_r(L_r)$ as given.

The amount of housing consumed per worker in a given region can be derived by combining the landowners' profit-maximization problem with the housing expenditure share in the worker's overall budget and its aggregation, given $w_r(L_r)$ and P_r^H (for details, see Appendix C.1):

$$h(L_r) = (1-b)^b L_r^{-b} \tag{30}$$

As is clear from equation (30), housing per worker decreases with a region's population, as regions are land-constrained. This congestion force offsets the agglomeration-inducing effects of positive production externalities in regions, thereby preventing complete agglomeration of economic activity into a single region.

Workers are freely mobile. Consequently, their utility must be equalized at the level $\bar{U}$ across all inhabited locations in equilibrium. To offset the decline in utility resulting from the reduction in housing per worker as a region's population increases, wages must rise with region size. To determine a region's wage $w_r(L_r)$, it suffices to solve the worker's utility maximization problem, combined with the expression for housing per worker (30) and the requirement that utility be equalized across inhabited regions at the level $\bar{U}$ in equilibrium (for details, see Appendix C.1):

$$w_r(L_r) = \bar{w}((1-\vartheta)L_r)^{b\frac{1-\vartheta}{\vartheta}} \tag{31}$$

where $\bar{w} = \bar{U}^{\frac{1}{\eta}} P^C$ is a constant to be determined in general equilibrium as in Gaubert (2018).

Sectors - The sectoral structure follows a simplified version of Atkeson and Burstein (2008), adapted to a closed-economy setting. Output in a given industry i is a CES aggregate of that industry's firms' $f \in F_i$ outputs:

$$Q_i = \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}} \right]^{\frac{\zeta}{\zeta-1}} \tag{32}$$

where Q_i denotes the total output of industry i, Q_{fir} the output of firm f, and ζ the elasticity of substitution across firms within a sector. Analogously, the price of industry i's output is aggregated as follows:

$$P_i = \left[\sum_{f_i}^{F_i} P_{fir}^{1-\sigma} \right]^{\frac{1}{1-\sigma}}, \tag{33}$$

where P_{fir} represents firm f's price. Within each of the industries $i \in I$, each firm produces a distinct good. The number of firms is small and exogenously determined, so

there is no endogenous entry. Firms compete oligopolistically in quantities, engaging in a Cournot-type game. Each firm determines its output Q_{fir} taking as given the output levels of other firms, the regional wage w_r, the final consumption price P^C, and the aggregate quantity C. However, firms recognize that industry-level prices P_i and quantities Q_i are endogenous to their own output decisions.

The final good consumed by workers is produced by a perfectly competitive firm that prices at marginal cost. Therefore, as in Atkeson and Burstein (2008), the consumption composite C is obtained by aggregating the outputs of a continuum of sectors:

$$C = \left(\int_0^1 Q_i^{\frac{\eta-1}{\eta}} di \right)^{\frac{\eta}{\eta-1}} \tag{34}$$

where η is the elasticity of substitution across sectors. Goods are imperfect substitutes ($\zeta < \infty$), with varieties being more substitutable within a sector than across sectors ($1 < \eta < \zeta$). The price of the final good is simply equal to a standard theoretical price index:

$$P^C = \left(\int_0^1 P_i^{1-\eta} di \right)^{\frac{1}{1-\eta}} \tag{35}$$

In what follows, I derive the standard optimal demand functions faced by firms and industries in this setting. To obtain optimal firm-level demand, one maximizes industry-level expenditure—defined as the total expenditure on the outputs of firms within a given industry[72] —subject to the industry-level aggregator of firms' outputs in (32):

$$Q_{fir} = \left(\frac{P_{fir}}{P_i} \right)^{-\zeta} Q_i \tag{36}$$

Similarly, at the sector level, optimal demand is obtained by maximizing the profits of the final-good producer subject to total expenditure on the different industry-level composites:

$$Q_i = \left(\frac{P_i}{P^C} \right)^{-\eta} C \tag{37}$$

Firms - Production is subject to EES. Bartelme, Costinot, et al. (2024) restrict EES to operate through physical productivity, conceptualizing them as the constant elasticity of productivity with respect to sectoral scale—a measure they term "scale elasticity." To apply the alternative estimation approach for EES proposed by Bartelme, Costinot, et al. (2019), which can be used in within-country settings where firm-level data are available but trade data are not, it is necessary to slightly deviate from their definition of firms'

[72]Recall that firms recognize that Q_i is endogenous to their own output decisions; hence, formulating the firm's problem that yields optimal firm-level demand is straightforward.

production processes with respect to EES. Since firms control their own productivity, an external component is required to formally define EES and to clearly articulate the rationale for industrial policy intervention—namely, the productivity-enhancing externality that EES represent. Therefore, I define productivity as consisting of an internal component A_{fir}—factors under the control of the firm's management—and an externality component E_{ir}, as introduced earlier, which is beyond managerial control and constitutes the main rationale for policy intervention in the model. Consequently, EES are defined here as the constant elasticity of the externality component E_{ir} in productivity with respect to the relevant entity's scale—either industry-specific (α in Equation 27) or region-specific (β in Equation 27). Correspondingly, firms in each industry-region pair produce with a linear technology according to:

$$Q_{fir} = A_{fir} E_{ir} L_{fir} \tag{38}$$

where firms differ exogenously in their internal productivity component A_{fir}. It should be noted that firms do not internalize E_{ir} and thus treat it as constant.
As in the closed-economy version of Atkeson and Burstein (2008), but with the modification that firms receive a subsidy on their input cost (wages), which they take as given, each firm solves the following profit-maximization problem:

$$\underset{P_{fir}Q_{fir}}{max} \quad P_{fir}Q_{fir} - Q_{fir}\frac{(1-\tau)w_r}{A_{fir}E_{ir}} \tag{39}$$

subject to the inverse demand functions derived from (37) and (36):

$$\frac{P_{fir}}{P^C} = \left(\frac{Q_{fir}}{Q_i}\right)^{-\frac{1}{\zeta}} \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}}. \tag{40}$$

For a detailed derivation of equation (40), see Appendix C.3. It is also useful to consider the following accounting identity for firm f's market share m_{fir} in industry i:

$$m_{fir} \equiv \frac{P_{fir}Q_{fir}}{\sum_f^F P_{fir}Q_{fir}} = \frac{Q_{fir}^{\frac{\zeta-1}{\zeta}}}{\sum_f^F Q_{fir}^{\frac{\zeta-1}{\zeta}}} \tag{41}$$

Appendix C.3 shows how this result can be derived starting from equation (40). Solving the problem in equation (39) subject to constraint (40), while taking into account equation (41), yields the optimal price for firm f (for detailed derivation, see Appendix C.3):

$$P_{fir} = \frac{\epsilon(m_{fir})}{\epsilon(m_{fir})-1}\frac{(1-\tau)w_r}{A_{fir}E_{ir}} \quad \text{where } \epsilon(m_{fir}) \equiv \left[\frac{1}{\rho}(1-m_{fir}) + \frac{1}{\eta}m_{fir}\right]^{-1} \tag{42}$$

Equilibrium - An oligopolistically competitive equilibrium is a set of prices $\{P_{fir}\}_{f\in\mathcal{F}}$, $\{P_i\}_{i\in\mathcal{I}}$, P^C, $\{P_r^H\}_{r\in\mathcal{R}}$, $\{w_r\}_{r\in\mathcal{R}}$ and quantities $\{Q_{fi}\}_{f\in\mathcal{F}}$, $\{Q_i\}_{i\in\mathcal{I}}$, C, $\{H_r^S\}_{r\in\mathcal{R}}$, $\{L_{fir}\}_{f\in\mathcal{F}}$ such that:

1. workers maximize utility,
2. utility is equalized across all inhabited regions,
3. firms maximize profits,
4. landowners maximize profits given $w_r(L_r)$, P_r^H and P^C,
5. markets, that is:
   (a) final goods,
   (b) industry-level composite goods,
   (c) firm-level goods,
   (d) regional labor markets
   (e) and regional housing markets clear
6. and the government budget is balanced.

Solving the government's problem yields the vector of subsidies τ_{fir}. I detail in Appendix C.5 how to solve for the different equilibrium conditions and how to construct the unique equilibrium.

6.2 Optimal Policy

I now turn to the analysis of optimal policy. By optimal, I refer to the vector of subsidies τ_{fir} that maximizes the utility of the workers in all regions, taking the initial distribution of economic activity as given. To characterize the structure of the optimal policy, the analysis proceeds in two steps. First, I study an "ignorant" government that implements a horizontal policy, distributing the same subsidy to all firms while ignoring EES. Second, I demonstrate how a "technocratic" government can achieve welfare outcomes that are better or worse than under a horizontal policy when implementing industrial policy by targeting specific firms across regions and industries.

Government Problem - Therefore, the government maximizes global welfare by solving the following problem:

$$\max_{\tau_{fir}} \quad \left(\frac{C}{\vartheta}\right)^{\vartheta}\left(\frac{\sum_r^R H_r}{1-\vartheta}\right)^{1-\vartheta} \tag{43}$$

subject to a budget constraint in which government revenue is raised by fully taxing landowner profits, as in urban economics models such as Henderson (1974):[73]

$$\underbrace{\sum_{r}^{R} b(1-\vartheta)L_r w_r(L_r)}_{\text{Landowner Profits}} = \underbrace{\sum_{f}^{F} \tau_{fir} w_r(L_r) L_{fir}}_{\text{Subsidies}} \tag{44}$$

There are several dimensions along which the government must make appropriate choices to achieve optimal industrial policy targeting. The first two relate to what I previously termed the macro margin of industrial policy and are closely linked to EES. Therefore, the government needs to consider which industries have the greatest potential for increases in externalities. Similarly, it must ensure that subsidies target firms in these industries that are located in mesoregions with the greatest potential for increases in externalities. In other words, the government must account for returns to scale in the externality production function.

The implications of returns to scale in externality production for optimal industrial policy targeting are straightforward in the case where $\alpha, \beta > 0$. In the presence of decreasing returns to scale in externality production, industrial policy is best implemented by targeting industry–mesoregion pairs with a relatively small labor force. If returns to scale in externality production are constant, targeting at the macro margin becomes irrelevant, and horizontal and industrial policies perform equally well along this dimension. In case that returns to scale in externality production are increasing, it is optimal for the government to target industry–mesoregion pairs with a large labor force when designing industrial policy at the macro margin. If either α or β is negative—reflecting that congestion forces at the industry or mesoregion level outweigh EES—the government must adopt a more refined targeting approach than in the cases described above. For example, if α is negative, the optimal industrial policy at the macro margin is to target firms in small industries and—depending on whether β is above or below one—in large or small mesoregions. Conversely, if β is negative, it is optimal to target firms in small mesoregions and—depending on whether α is above or below one—in large or small industries.

The remaining two dimensions relevant for industrial policy targeting pertain to the micro margin of industrial policy. On the one hand, the government must decide how many firms—and in particular which firms—to subsidize. On the other hand, it must determine how much subsidy to allocate to each selected firm. This gives rise to a classic "quantity–quality" trade-off at the micro margin of industrial policy. Two considerations are central to resolving this trade-off. First, recall that the government in the model subsidizes firms' marginal cost in hope of firms' expansion leading to more EES. Since the

[73] See also Gaubert (2018).

level of externalities E_{ir} in a given industry–mesoregion pair depends on the size of the labor force, large firms can be expected— from an "EES perspective"— to generate the greatest absolute increase in employment for a given marginal cost subsidy. Second, bear in mind that firms translate a subsidy partly into output expansion and partly into higher markups depending on their market share. Consequently, from a "markup perspective", smaller firms are more attractive targets for industrial policy support. Taken together, these considerations imply an optimal firm size at which the trade-off is best resolved.

In this framework, industrial policy potentially benefits households and increases their welfare via two main channels that might be subject to attenuating general equilibrium effects. First, an optimal leveraging of externalities—and the resulting increase in EES—will enhance productivity. In turn, higher productivity reduces marginal costs and, ultimately, prices. Second, the subsidy lowers the cost of labor, enabling firms to meet consumer demand that they previously could not satisfy due to production costs. Consequently, labor demand increases, benefiting workers through higher wages.

The Structure of Optimal Industrial Policy - The government takes the initial economic allocation as given. In contrast to individual firms, the government internalizes that, through EES, the labor force of one firm indirectly increases the productivity of other firms within the same industry–region pair. Accordingly, the government can implement subsidies by covering a share of the wages paid by firms. Using these instruments, the government can achieve an outcome for the overall economy that is superior to the equilibrium under a horizontal policy. This is because the planner can reallocate resources, via subsidies, toward firms that contribute most to the positive externalities underlying EES. This indirect maximization of aggregate productivity leads to higher output and consumption relative to a horizontal policy equilibrium. In the following, I compare the equilibrium outcomes of the decentralized economy with those of the centralized economy and derive the conditions on the vector of firm-level subsidies τ_{fir} under which the two equilibria coincide.

First, consider the factor that prevents the decentralized economy from reaching its optimum in the absence of industrial policy: the firm's hiring decision. The firm's hiring decision is obtained by taking the first-order condition with respect to L_{fir} from the firm's profit maximization problem in equation (39), after substituting Q_{fir} with its expression in equation (38). On the one hand, in the decentralized economy, where firms do not internalize their contribution to E_{ir}, and abstracting from subsidies, a firm hires labor up to the point at which it faces the following condition:

$$w_r^{dc} = P_{fir} A_{fir} E_{ir} \tag{45}$$

On the other hand, in the centralized economy, where the planner internalizes the contribution of firms to E_{ir}, a firm would hire labor up to the point such that:

$$w_r^c = P_{fir} A_{fir} E_{ir} \left(1 + \frac{\alpha}{L_i} + \frac{\beta}{L_r}\right) \tag{46}$$

Therefore, the optimal subsidy for any given firm equalizes its social and private returns by incentivizing it to hire the optimal amount of labor. Therefore, it simply reflects the externality term and can be interpreted as a classic Pigouvian subsidy in the spirit of Pigou (1920):

$$\tau_{fir}^* = \frac{\alpha}{L_i} + \frac{\beta}{L_r} \tag{47}$$

Notice that, for a given firm f, τ_{fir}^* is indirectly decreasing in firm size, as the terms L_i and L_r increase with the firm's labor force L_{fir}.

6.3 Counterfactuals

I assess the extent to which BNDES industrial policy targeting was optimal. Accordingly, the main counterfactual exercise computes the welfare change in a scenario in which BNDES targets industrial policy optimally in light of EES. In the model, this corresponds to targeting firms that generate the highest employment bang-for-buck when subsidized, as explained below, in the smallest industry–region pairs, given decreasing returns to scale in the externality production function.

To compute the counterfactual gains from optimal industrial policy, I develop a sufficient-statistics approach in the spirit of Arkolakis et al. (2012). That is, to compute the gains from optimal industrial policy in the model, it suffices to compute the Spearman rank correlation coefficient ρ between the government's ranking of industry–region priorities and the ranking of industry–region pairs by their EES potential, as well as the firm employment growth potential $\frac{\partial L_{fir}}{\partial \tau_{fir}}$. The Spearman rank correlation coefficient ρ is calculated as follows. First, for any industry–region pair, let $\hat{E}_{ir}$ denote the predicted level of externalities in that pair, obtained using the estimated values of α and β. Second, compute the partial derivative $\frac{\partial \hat{E}_{ir}}{\partial L_{fir}}$, which indicates how much the level of externalities would increase when an additional worker is added to a given firm in that industry–region pair. Third, let the matrix $R_{\hat{E}_{ir}}$ denote the ranking of industry–region pairs according to their predicted externality "growth potential," $\frac{\partial \hat{E}_{ir}}{\partial L_{fir}}$. Fourth, let the matrix $R_{IP_{ir}}$ represent the ranking of industry–region pairs based on the actual amount of BNDES subsidized credit they received over the treatment period. Fifth, compute Spearman's rank correlation

coefficient $\rho^{macro}_{opt.\ IP}$ to assess the similarity between the two rankings:

$$\rho^{macro}_{opt.\ IP} = \frac{Cov(R^{macro}_{IP}, R_{\hat{E}_{ir}})}{\sigma_{R^{macro}_{IP}} \sigma_{\hat{E}_{ir}}} \tag{48}$$

Sixth, the interpretation of whether industrial policy is optimal depends crucially on whether there are increasing ($\alpha + \beta > 1$) or decreasing ($\alpha + \beta < 1$) returns to scale in the externality production function $E_{ir}(L_i, L_r)$ in (20). On the one hand, in the case of increasing returns to scale in externality production ($\alpha + \beta > 1$), optimal industrial policy is characterized by $\rho^{macro}_{opt.\ IP} = 1$. On the other hand, in the empirically relevant case of decreasing returns to scale in externality production, $\rho^{macro}_{opt.\ IP} = -1$ indicates maximal industrial policy efficiency.

Which firms the government should optimally target in a given industry–region pair—can be determined as follows. Intuitively, the government should target firms that generate the largest increase in employment in response to a subsidy, that is, firms with the highest value of $\frac{\partial L_{fir}}{\partial \tau_{fir}}$. Accordingly, begin with the equilibrium price charged by a given firm in (42), which can be extended to express L_{fir} as a function of the subsidy, among other factors. Next, take the partial derivative with respect to τ_{fir} (recalling that the firm treats τ_{fir} as exogenous), which yields the following expression (a detailed derivation is provided in Appendix C.6):

$$\frac{\partial L_{fir}}{\partial \tau_{fir}} = \frac{P_{fir} Q_{fir}}{\mu_{fir} w_r} (1 - \tau_{fir})^{-2} \tag{49}$$

where μ_{fir} denotes the markup, $\mu_{fir} = \frac{\epsilon(m_{fir})}{\epsilon(m_{fir})-1}$. Equation (49) clearly highlights the trade-off the government faces when selecting firms for industrial policy support in a given industry–region pair. On the one hand, $\frac{\partial L_{fir}}{\partial \tau_{fir}}$ is increasing in Q_{fir}, implying that larger firms generate greater absolute increases in employment when subsidized. On the other hand, $\frac{\partial L_{fir}}{\partial \tau_{fir}}$ is decreasing in μ_{fir}, implying that firms with higher markups convert a subsidy into smaller increases in employment. The values of $\frac{\partial L_{fir}}{\partial \tau_{fir}}$ can be computed, allowing firms within a given industry–region pair to be ranked accordingly. Denote this ranking by the vector $R_{\partial L_{fir}/\partial \tau_{fir}}$. Accordingly, firms within a given industry–region pair can also be ranked based on the actual amount of BNDES subsidized credit they received, denoted by the matrix R^{micro}_{IP}. Finally, one can compute Spearman's rank correlation coefficient $\rho^{\text{micro}}_{\text{opt. IP}}$ to determine the optimality of the actual industrial policy:

$$\rho^{micro}_{opt.\ IP} = \frac{Cov(R^{micro}_{IP}, R_{\partial L_{fir}/\partial \tau_{fir}})}{\sigma_{R^{micro}_{IP}} \sigma_{\partial L_{fir}/\partial \tau_{fir}}} \tag{50}$$

In contrast to $\rho^{\text{macro}}_{\text{opt. IP}}$, $\rho^{\text{micro}}_{\text{opt. IP}}$ has a unique interpretation: optimal industrial policy is characterized by $\rho^{\text{micro}}_{\text{opt. IP}} = 1$. In other words, $\rho^{\text{micro}}_{\text{opt. IP}} = 1$ ensures that employment gains

from subsidies—and thus the generation of EES—are maximized.

7 Taking the Model to the Data

To implement the counterfactual, I require, for the macro margin, the following inputs: the industry-specific component of EES, α, and the location-specific component, β, the industry workforce $\{L_i\}_{i\in I}$, the mesoregion workforce $\{L_r\}_{r\in R}$, and the total amount of BNDES subsidized credit allocated to each industry–mesoregion pair. For the micro margin, I require: firm-level revenue, $P_{fir}Q_{fir}$, firm-level market shares, m_{fir}, the elasticity of substitution across industries, η, the elasticity of substitution across firms within industries, ζ, and the amount of BNDES subsidized credit received by each firm. Table 9 summarizes the calibration.

Table 9: Calibration

Parameter	Value	Description	Moment	Source
Elasticities				
α	.55	Elast. of externalities w.r.t ind. labor	OLS estimates	Data
β	-.59	Elast. of externalities w.r.t mesoreg. labor	OLS estimates	Data
η	1.01	Elast. of subst. btw. sectors		Atkeson and Burstein (2008)
ζ	3	Elast. of subst. varieties		Broda and Weinstein (2006)
Labor Pools				
$\{L_i\}_{i\in I}$		Industry labor		Data
$\{L_r\}_{r\in R}$		Mesoregion labor		Data
Firm Characteristics				
$\{P_{fir}Q_{fir}\}_{f\in F}$		Revenue		Data
$\{m_{fi}\}_{f\in F}$		Market shares		Data
$\{\tau_{fir}\}$		BNDES subsidized credit		Data

Notes **-** The table summarizes the calibrates values used for the quantitative analysis.

The model is static and therefore contains only one time period. Hence, its empirical counterpart must be mapped carefully to the panel structure of the data. I interpret the model as describing a cross-sectional equilibrium of the economy. Since the model has no dynamics, each year in the panel can be treated as a separate steady-state cross section. Accordingly, firm-year observations can be interpreted as repeated observations from the model economy, potentially after controlling for aggregate year-specific shocks.

EES level - The mapping of the econometric estimates for α and β from Section 5 into the model is straightforward. Accordingly, I calibrate them to the coefficients from the fixed-effects specification reported in column (2) of Table 8. In doing so, I acknowledge the main caveat of this procedure: equation (25) is estimated at the industry level rather than at the industry–mesoregion level due to data constraints, which requires weighting the L_{-irt} terms by s_{irt}.

Subsidies - Computing the counterpart in the data for the subsidies in the model proceeds as follows. In the model, the subsidy takes the form of a share of the wages paid by firms—i.e., their marginal cost, as labor is the only input in production—whereas in the data, the subsidy takes the form of subsidized credit. Note that the approach developed here requires only the ranking of firms and does not necessitate mapping the exact amount of subsidized credit observed in the data to the model. Accordingly, I rank subsidized firms based solely on the amount of subsidized credit they received in a given year.

Calibration of the remaining parameters and data inputs - Firm-level market shares m_{fir} are calculated as described below. Sales revenue is directly observed in the main dataset. Firms without information on sales are excluded from the analysis. After aggregating observed firm-level sales within each industry for a given year, I compute total sales per firm and derive each firm's share in these industry totals. To compute industry and mesoregion labor, I follow a similar procedure, aggregating employment within each observed year.

7.1 Welfare Results

I compute the extent to which the BNDES industrial policy targeting deviates from an optimal targeting strategy in a counterfactual scenario ($\rho^{\text{macro}}_{\text{opt. IP}} = -1$ & $\rho^{\text{micro}}_{\text{opt. IP}} = 1$). Accordingly, I first compute the values of $\rho^{\text{macro}}_{\text{opt. IP}}$ and $\rho^{\text{micro}}_{\text{opt. IP}}$ in the factual scenario, thereby assessing how well BNDES support was targeted. I then compare these values to the counterfactual scenario of optimal industrial policy. Table 10 reports the results averaged across all years.

Table 10: Counterfactual Analysis

Targeting Strategy	Optimal IP (1)	BNDES (2)	Horizontal (3)
Macro Margin			
$\rho^{\text{macro}}_{\text{opt. IP}}$	-1	-0.07	0.005
Micro Margin			
$\rho^{\text{micro}}_{\text{opt. IP}}$	1	-0.71	0.005

Notes - The table reports the degree to which industrial policy targeting is optimal under different targeting approaches.

Under the industrial policy targeting implemented by BNDES, overall targeting—considering both the micro and macro margins—was suboptimal.

Regarding the macro margin, in the factual scenario—that is, under the actual targeting implemented by BNDES—the chosen industry–region pairs were not optimal, as $\rho^{\text{macro}}_{\text{opt. IP}}$ is close to zero, which is far from -1. In other words, the generation of EES under BNDES

targeting falls short of its potential and is more consistent with a setting characterized by increasing returns to scale in externality production. That is, BNDES would have performed better by directing support toward industry–region pairs characterized by a large industrial workforce within a relatively small regional workforce.

Similarly, at the micro margin, the targeting implemented by BNDES yields a negative $\rho^{\text{micro}}_{\text{opt. IP}}$, which is far from the optimal value of 1. Indeed, as discussed in Section 2, firms receiving BNDES support are substantially larger than those in the control group, and, as shown in Section 4, they experience increases in profits following take-up. This suggests that these firms may have translated the subsidy more into higher markups rather than additional employment. In sum, BNDES should have targeted, on average, smaller firms than those it supported, thereby generating greater employment creation and subsequently EES.

The industrial policy design implemented by BNDES has quantitatively sizable long-run impacts, consistent with the empirical finding that subsidies generated, at best, modest additional sales growth, while also entailing fiscal costs for the Brazilian government—abstracted from here for simplicity. The suboptimal design of BNDES industrial policy may have prevented the Brazilian government from realizing the anticipated gains in economic activity and tax revenue intended to finance these interventions. It may also have contributed to the subsequent economic crisis in Brazil. This interpretation is consistent with contemporaneous accounts, such as Barbosa-Filho (2020), which link pre-crisis expansionary fiscal policies—including the government's industrial strategies—to the ensuing fiscal contraction during the crisis.

The welfare analysis above is based on the actual subsidies received by each firm, reflecting BNDES' observed targeting. While the counterfactual in which BNDES targets its support optimally is well-defined by construction, it is also informative to consider a scenario without any targeting. Such a strategy, which denies BNDES and the government the ability to target industrial policy effectively, is commonly referred to as a horizontal policy. To assess this scenario, I conduct an alternative counterfactual in which subsidized credit is distributed evenly across all firms. This exercise effectively approximates a scenario with no industrial policy. I hold the level of EES constant and abstract from fiscal costs, as in the baseline, to ensure comparability of the results. A horizontal policy can be welfare-maximizing at the macro margin only in a specific case, namely when $\alpha = \beta = 0.5$. Put another way, it can be welfare-maximizing when the level of EES does not vary across industry–region pairs ($\alpha = \beta$) and returns to scale in externality production are constant ($\alpha = \beta = 0.5$). Consequently, a horizontal policy can perform better or worse than industrial policy, depending on how optimally the latter is targeted. Relative to the observed BNDES allocation, a horizontal policy may have yielded higher welfare, as shown in column (3) of Table 10, particularly along the micro margin of industrial

policy.

7.2 Alternative Exercises

Having presented the main results of the counterfactual analysis, I outline several alternative implementations to be explored in future work.

EES Level - The baseline model assumes that EES consist of an industry-specific and a location-specific component. In doing so, this assumption conflicts with two well-established traditions in the literature, which typically assume either industry-specific, location-specific, or industry–location-specific EES (with the location defined at the country level), but not all three simultaneously. On the one hand, following the tradition of Marshallian externalities (Marshall, 1920),[74] which has been widely adopted, particularly in the international trade literature, it is assumed that EES are industry-specific. On the other hand, following the tradition of Jacobs externalities (Jacobs, 1969),[75] EES are assumed to be location-specific—an idea that has found widespread resonance in the urban economics literature. To assess how this assumption affects the results, future versions of this work will include specifications in which I impose $\beta = 0$ and $\alpha = 0$, respectively; that is, externalities are assumed to be generated solely by the industry-specific or mesoregion-specific labor pool.

Fiscal costs - The baseline model assumes that the government fully taxes away landowners' profits to finance subsidies. However, the government may only have access to distortionary tax instruments. In that case, to fix one market failure, the government would have to aggravate another. To capture this possibility, an extension of the model in which the government can tax households and borrow on financial markets would be required; however, this lies outside the scope of the current study.

8 Conclusion

This paper provides new evidence on the role that targeting plays in the success of industrial policy and on how the level of EES interacts with this process. Combining empirical and structural methods, I show that optimal industrial policy targeting depends on both the micro margin—which firms to target—and the macro margin—which industry–region pairs to target. In doing so, the analysis addresses a central question in industrial policy: which firms should the government target, and in which industries and locations?

[74] See also Arrow (1962) and Romer (1986).
[75] See also Henderson (1974) and Ahlfeldt et al. (2015).

On the empirical front, focusing on the micro margin of industrial policy targeting, I leverage the revival of large-scale industrial strategies implemented by the PT governments in Brazil between 2004 and 2014. A central component of these strategies was industrial policy in the form of subsidized credit provided by the Brazilian Development Bank, BNDES, which served as the principal instrument by channeling credit to finance productivity-enhancing investments. Exploiting variation in the physical depreciation of machinery and equipment, I construct an instrument for BNDES subsidized credit take-up that generates plausibly exogenous exposure to the expansion of such credit intended to finance major manufacturing investment projects. The results reveal that BNDES subsidized credit led to little additional investment, at most modest additional sales growth, increases in profits, and increases in employment among treated firms. These findings suggest a particular mechanism: the expansion of BNDES subsidized credit not only enhances firms' output at most modestly, but also allows firms to substitute away from high-cost financing, thereby increasing their profitability, likely in the form of higher markups given lower costs.

Focusing on the macro margin, I exploit Brazil's economic geography, in particular variation in within-industry spatial concentration and across-industry agglomeration. Therefore, I further develop the procedure in Reshef and Tepper (2026) to identify the industry- and location-specific components of EES in a theory-consistent manner. I find that EES are predominantly industry-specific in my sample and estimate decreasing returns to scale in the production of externalities. These results highlight that targeting industrial policy at the macro margin requires not only selecting the right industries but also the right locations within which these industries are present.

To assess the optimality of the industrial policy design chosen by the BNDES—including general equilibrium forces and spillovers—I build a multi-industry, multi-region model with firms that have heterogeneous markups and can translate subsidies into higher output and higher markups. The model features two components of productivity: an internal component and an externalities component produced at the industry–region level by the corresponding industry and regional labor pools, leading firms to not internalize the productivity-enhancing effects of their employment on within-industry and within-region peers. Adopting a sufficient-statistics approach, I show that the industrial policy design of the BNDES was suboptimal at both margins, the micro and the macro. On the one hand, at the micro margin, firms that were too large were targeted and did not generate maximum employment, as they translated subsidies not only into employment but also into higher markups. On the other hand, at the macro margin, the targeting approach chosen by the BNDES did not correspond to a setting with decreasing returns to scale in externality production; that is, it would have been optimal to target smaller industry–region pairs.

These patterns point to important complementarities: not only is it important to target the right industries in the right regions, but this must go hand in hand with targeting the appropriate firms. Moreover, industrial policymakers must consider which mechanism underlying industrial policy—commonly summarized by EES—they seek to leverage and carefully assess the level at which EES arise.

All in all, these results provide a more complete picture of how industrial policy should be optimally targeted. They show that optimal industrial policy targeting consists not only of choosing the right industries, but also of targeting the appropriate firms within those industries and in the industry–region pairs where positive productivity-enhancing externalities are abundant. While much of the existing literature has focused on either the appropriate firms (e.g., small or exporting firms) or sectors (e.g., upstream industries), this paper highlights the distinct role of externalities and EES in guiding industrial policy targeting.

Several avenues for future research remain. Exploring the different mechanisms that can generate EES, assessing whether these mechanisms are primarily location- or industry-specific, and studying the joint design of industrial policy across the different margins uncovered here, as well as others such as the number of firms to target, can yield valuable insights for both policy and theory. More broadly, this research contributes to a growing literature showing that industrial policy can yield positive welfare effects when appropriately implemented.

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A Data

Number of Observations - In total, my dataset comprises 764,711 firm-year observations for 2002–2019 (941,255 for 1996–2019), of which 571,674 (700,325 for 1996–2019) firm-year observations originate from firms in the "census sample"—that is, firms with more than 29 employees. The remaining 193,037 observations (240,930 for 1996–2019) come from randomly sampled firms with between 5 and 29 employees. The sampling procedure yielded observations on approximately 29,000 firms for the reference year 1996. The number of firms observed increased steadily, peaking at approximately 51,000 in 2012. After that, the sample size decreased to 42,000 observations in 2020.[76]

All in all, the sample covers approximately 95% of Brazilian economic activity stemming from manufacturing firms (IBGE, 2004). Because IBGE receives information on firms' number of employees for a given year only at the end of the following year, there is a two- to three-year time lag before firms enter the consistently sampled portion of PIA-Empresa. Firms in the employment range 5-29 are randomly sampled. In doing so, the IBGE stratifies the randomly sampled part of its sample by federal unities (26 states plus a federal district) and industry-level.[77] Therefore, the six most industrialised federal unities[78] are stratified at the three-digit level of the Classificação Nacional de Atividades Econômicas (CNAE).[79] The other 21 federal unities are stratified at the two-digit level.

[76] For a detailed account of the sample size, see Table 11 in Appendix A.

[77] The stratification is based on the information where firms are registered.

[78] Minas Gerais (MG), Rio de Janeiro (RJ), São Paulo (SP), Paraná (PR), Santa Catarina (SC) and Rio Grande do Sul (RS)

[79] The CNAE knows 112 industries at the 3-digit level.

Table 11: Number of Firm Observations by Year

Year	Number of Observations in PIA Empresa (Rounded to the Nearest Thousand)
1997	29,000
1998	30,000
1999	31,000
2000	32,000
2001	34,000
2002	35,000
2003	36,000
2004	37,000
2005	39,000
2006	41,000
2007	44,000
2008	44,000
2009	46,000
2010	47,000
2011	50,000
2012	51,000
2013	50,000
2014	50,000
2015	47,000
2016	44,000
2017	44,000
2018	43,000
2019	42,000

These numbers comprise all firms sampled in PIA-Empresa $>$ 4 employees.

A.1 The Brazilian Context

Plano Brasil Maior -

The government of Dilma Rousseff launched the *Plano Brasil Maior* (PBM) through *Decreto n° 7.540* on August 2^{nd}, 2011, in the aftermath of the Great Recession. This decree announced the launch of the PBM and established the administrative structures responsible for its implementation.[80] The primary objective of the PBM was to restore the competitiveness of Brazil's manufacturing sector, both domestically and in international markets.[81]

Inflation - The cumulative CPI inflation rate over 2002–2019 was 198% (125% for 2005–2019) while the Brazilian economy's cumulative growth rate was 51% (37% for 2005–2019). Annual cumulative CPI inflation rates since 2005 are reported in Table 12.

[80]For more information, refer to Decreto n° 7.540, 02/08/2011.

[81]Paragraph 5 of the government's *Exposição de Motivos* for Provisional Measure n° 540/2011 explicitly states this goal (available at: https://www.planalto.gov.br/ccivil_03/_ato2011-2014/2011/Mpv/540impressao.htm).

Table 12: Descriptive Statistics of Inflation & Growth Rates

Year	Cumulative CPI Inflation Rate	Yearly CPI Inflation Rate	Cumulative GDP Growth Rate	Yearly GDP Growth Rate
2005	6.9%	6.9%	3.2%	3.2%
2006	11.4%	4.2%	7.3%	4%
2007	15.4%	3.6%	13.9%	6.1%
2008	22%	5.7%	19.7%	5.1%
2009	28%	4.9%	18.5%	-0.1%
2010	34.4%	5%	27.4%	7.5%
2011	43.2%	6.6%	32.5%	4%
2012	50.9%	5.4%	35%	1.9%
2013	60.3%	6.2%	39%	3%
2014	70.4%	6.3%	39.7%	0.5%
2015	85.8%	9%	34.8%	-3.5%
2016	101.9%	8.7%	30.4%	-3.3%
2017	109%	3.5%	32.1%	1.3%
2018	116.5%	3.6%	34.5%	1.8%
2019	124.5%	3.7%	36.1	1.2%
Average	58.7%	5.5%	26.9%	2.2%

A.2 BNDES

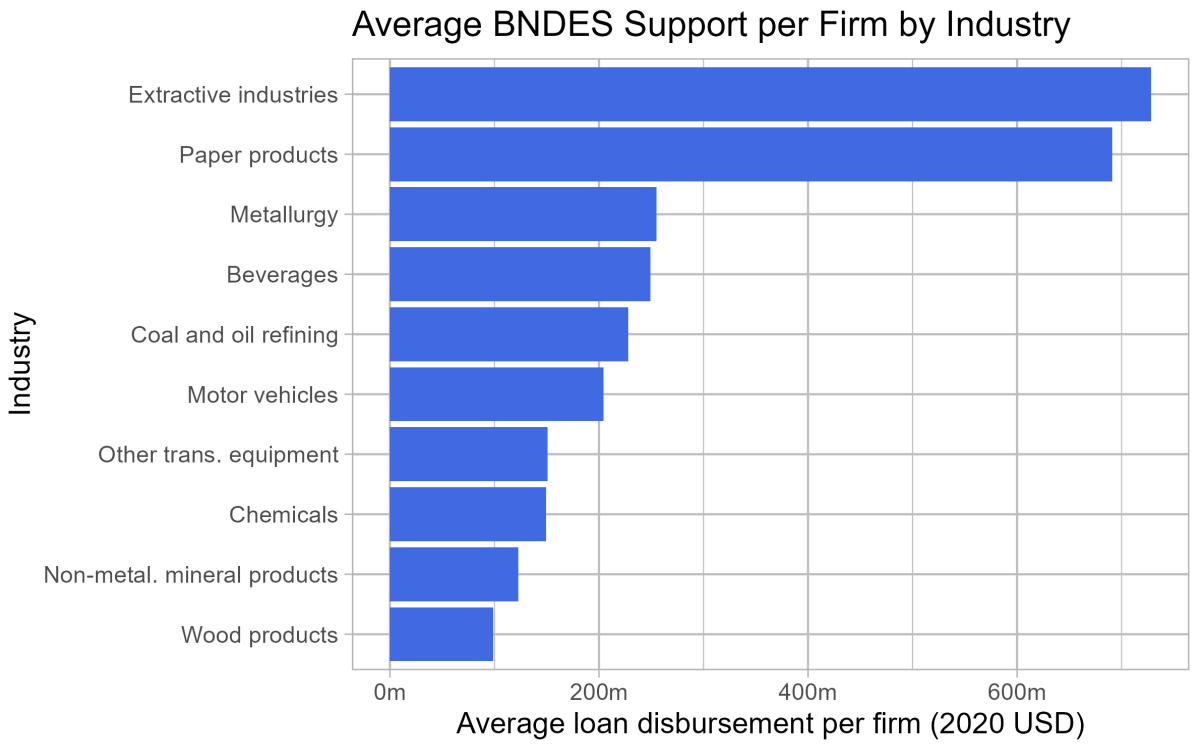


Figure 7: Average BNDES Support per Firm by Industry (2002-2019)

A.2.1 Structure of BNDES Operations

BNDES has four main credit lines that directly affect investment (De Menezes Barbosa et al., 2020): *BNDES FINAME*, designed for the acquisition of machinery and equipment; *BNDES FINEM*, intended to finance large investment projects (currently above R$20 million) that upgrade production and expand production capacity; *BNDES Au-*

tomático, which serves the same purpose as *BNDES FINEM* but covers smaller projects (currently below R$20 million); and the *Cartão BNDES*, aimed at financing investments by small and medium-sized enterprises. Together, these credit lines account for 90.5% of non-automatic operations, with *BNDES FINAME* representing 52.1%, *BNDES FINEM* 28.2%, and *BNDES Automático* 10.2%. By contrast, *Cartão BNDES* is not reported due to the small scale of its projects.[82]

A.2.2 Government Funding For BNDES

Complementary Law n°19 of 1974 established that the BNDES has a continuous funding stream from the social contributions PIS (private sector) and PASEP (public sector) fund. In addition, Law n°8.019 stipulates that BNDES receives at least 28% of the contributions to the Fund for Workers' Assistance (FAT). Together, these sources accounted for 59.4% of total liabilities in 2004, for example (BNDES, 2005). Additionally, the Brazilian government continuously approved new funding, transferred to BNDES through the Ministry of Finance in forms of grants and subsidized credits.

2008 - The government adopted Provisional Measure n°439, later approved as Law n°11.805, which granted BNDES a credit line of R$15 billion.

2009 - The government issued Provisional Measure n°465, later converted into Law n°12.096, which renegotiated debts owed by BNDES to the Treasury to provide greater financial flexibility. Additionally, it allocated R$44 billion (USD 22 billion) in new funding to BNDES.

2011 - The government issued Provisional Measure n°526, later converted into Law n°12.453. This increased the budget of the BNDES by R$164 billion (USD 98 billion) in new funding.
Additionally, it also approved Provisional Measure n°541, later converted into Law n°12.545. Thereby, the budget was increased by one billion R$ (USD 0.6 billion).

2012 - The government issued Provisional Measure n°564, later converted into Law n°12.712. It added R$18 billion (USD 9.2 billion) to the BNDES budget and extended a credit line of R$100 billion (USD 51 billion) to BNDES.

2013 - The government issued Provisional Measure n°594 in 2012, later converted into Law n°12.814 in 2013. These acts increased the BNDES budget by R$85 billion (USD 39.4 billion).

[82]Introduced in 2003, *Cartão BNDES* has accounted for an average of 2.9% of annual BNDES disbursements.

Additionally, it enacted Provisional Measure n°619, later converted into Law n° 12.873 which provided an additional R$10 billion (USD 4.6 billion).
Moreover, it issued Provisional Measure n°633, later converted into Law n°13.000. Provisional Measure n°633 added another R$50 billion (USD 23.2 billion) to the BNDES budget.

2014 - When Provisional Measure n°633 was converted into Law n°13.000 in 2014, the government added another R$40 billion (USD 17 billion) to the BNDES budget in addition to what Provisional Measure n°633 had envisaged.

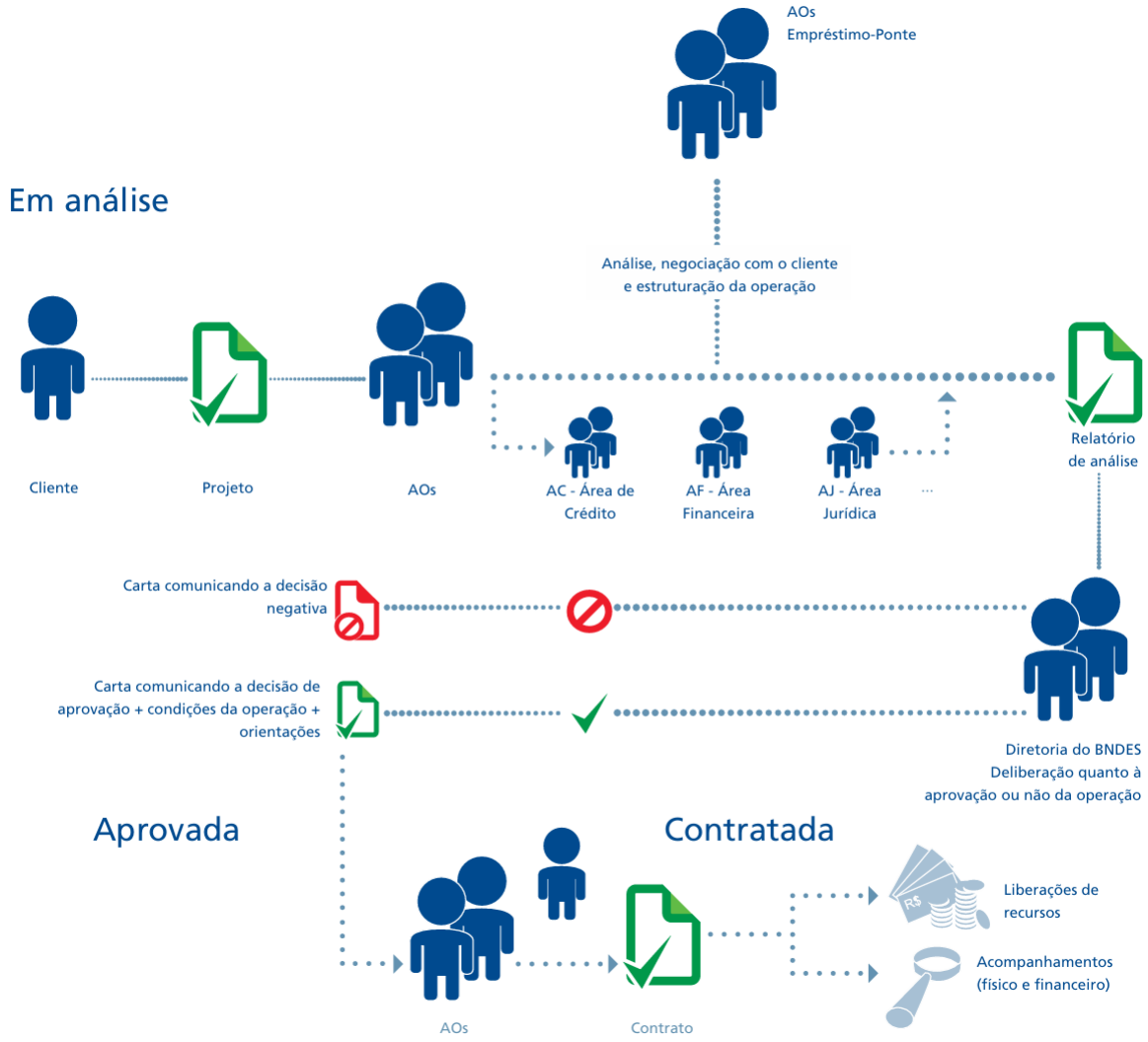
2015 - The government issued in late 2014 Provisional Measure n°663, later converted into Law n°13.132 in 2015. This measure added R$50 billion (USD 14.9 billion) to the BNDES budget.

A.2.3 BNDES Non-Automatic Approval Process

The first phase of the negotiation process, the *Consulta prévia*, already constitutes a complex and bureaucratic procedure involving various stakeholders within the IBGE with whom the applying firm must interact. Within BNDES, the credit risk unit, together with the relevant operational units depending on the project, evaluates the proposal and submits their recommendations to the evaluation committee. Subsequently, the evaluation committee prepares a decision card indicating whether BNDES is willing to support the project, along with guidance and comments for the next steps. The *Consulta prévia*, with the involvement of multiple internal actors, already constitutes a substantial bureaucratic burden for firms, which must follow the process closely and comply with the corresponding information requests. However, the bureaucratic process does not end with the approval of the project plan. In the event of approval, firms enter a second phase—the *análise* phase—during which the project undergoes in-depth evaluation and negotiation (BNDES, 2013, p.22).



The second phase, *análise*, further increases the bureaucratic burden, as firms must interact with multiple departments within the BNDES to negotiate details related to risk, credit conditions, financial arrangements, and operational aspects. The detailed project plan is used to assess its economic viability, the type of collateral required to guarantee the loan, balance sheet data, and other financial information, among other factors. Most importantly, the firm engages in direct negotiations with BNDES and the relevant units, including operational, financial, and legal departments (BNDES, 2013).



If all units approve the final agreement, it is submitted to BNDES's board of directors for final approval, after which the loan contract between BNDES and the firm can be signed. After the contract is signed, the operational unit of BNDES begins disbursing the funds in accordance with the implementation plan and periodically monitors the progress of the project. Therefore, verifying whether firms have implemented their projects is ultimately redundant, given that BNDES disburses funds in installments[83] —and, except for the first installment, each subsequent disbursement[84] is conditional on the firm's successful adherence to the agreed implementation plan.[85]

[83] According to Lage de Sousa and Ottaviano (2018), amortization generally begins only once all installments have been disbursed by BNDES.

[84] Lage de Sousa and Ottaviano (2018) note that, for their observation period 1996–2005, projects financed through *BNDES FINEM* and *BNDES Automático* took on average five years from contract signing to completion.

[85] As reported by Lage de Sousa and Ottaviano (2018), BNDES always participates in a project up to an upper limit that varies over time but is generally around 80%.

A.3 Data Construction

Identifying Treated Firms - To identify treated firms—that is, firms receiving BNDES support in the form of non-automatically approved loans—I proceed as follows: First, I use firm-year–level aggregated BNDES data to identify firms that received non-automatically approved loans between 2002 and 2019. During this period, an average of 322 new firms per year received at least one of the BNDES subsidized credit lines under consideration, with a peak of 462 firms in 2013.[86] However, not all of these firms can be included in the analysis, as some—particularly smaller firms—are not covered in the PIA dataset. This is because the IBGE consistently samples manufacturing firms with 30 or more employees for the PIA survey, whereas smaller firms with 5–29 employees are sampled randomly.

Subsidized Credit - Information on subsidized credit allocated by the BNDES was obtained from the BNDES subsidized credit registry. Access to the subsidized credit registry containing the full firm identifiers, rather than only the first five digits available in the online version, was granted following a request filed under Federal Law nº 12.527/2011, which governs access to information. At the time of writing, the data were available from 2002 through the end of 2024. The key variables include the total amount borrowed, the interest rate, and the repayment period for each financial contract. I collapse the data to firm–year observations by credit line, summing the total amount borrowed and computing the average interest rate and the average repayment period.

Firm Balance Sheets - Firm balance sheet data are drawn from IBGE's PIA Empresa survey and were largely available from 1996 to 2021, with some exceptions. Due to the COVID-19 pandemic and its far-reaching and highly heterogeneous impact on the Brazilian economy, I restrict the analysis to data up to 2019. Given that subsidized credit data are only available from 2002 onward, I restrict the sample to start in 2002 and use information from 1996 to 2002 solely for the construction of certain versions of the instrument. In 2007, IBGE updated its industrial classification system from *CNAE 1* to *CNAE 2*, in line with the transition from version 3 to version 3.1 of the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC) (IBGE, 2004). Accordingly, *CNAE 1* classifications for firm–year observations prior to 2007 were mapped to *CNAE 2* using the correspondence table provided by IBGE in Appendix 2 of IBGE (2004).

The IBGE conducts the PIA annually. Law n°5.534 of the 14^{th} of November 1968 about "compulsory statistical reporting and other measures" obliges all natural and judicial persons in Brazil to respond correctly to surveys from the IBGE on pain of a penalty of

[86] More precisely, BNDES non-automatically approved 51,259 subsidized loans (6,532 when aggregated at the firm-year level) for 3,822 distinct firms during 2002–2020.

20 minimum salaries in case of misreporting or no-reporting.[87] Hence, one can expect firms to not have incentives to misreport information.

The study uses a combination of variables directly collected by IBGE through the PIA-Empresa questionnaire and variables computed by IBGE based on the reported information.

Sales revenue for a given year is computed as the sum of revenue from the sale of products and industrial services plus the change in product inventories between the previous year and the year in question.

Labor expenditure is measured as the total wage bill, including salaries, withdrawals, and other forms of remuneration, as of December 31 of each year. Total employment, employment in production, and employment not linked to production are measured as the number of employees in each respective category as of December 31 of the corresponding year.

Intermediate input expenditure is defined as the sum of: (i) expenditure on material purchases in the year in question; (ii) changes in the value of inventories of raw materials, materials, and components between the previous year and the year in question; (iii) energy expenditure, defined as the sum of expenditures on electricity and fuel; and (iv) starting in 2007, expenditure on water, reflecting the water-intensive nature of industries such as beverages, pulp and paper, and textiles.

Capital is measured as the sum of current and non-current assets, which are available from 2000 onward; this aligns well with the estimation sample, which begins in 2002. IBGE distinguishes five types of investment: (i) acquisition of land; (ii) acquisition of buildings; (iii) acquisition of machinery and equipment; (iv) acquisition of transport equipment; and (v) other acquisitions, including computers, furniture, and similar items. Given the focus of the policy under study, this paper considers only (iii) investment in machinery and equipment and (v) investment in computers and other categories.

Exporter status is allocated to a firm once the share of its sales going abroad is positive. For the application of the productivity estimator proposed by Barrows et al. (2023), information on the destination of sales—distinguishing between (i) the domestic market, (ii) the Mercosur and (iii) the rest of the world—is used.

Net profits are computed as the difference between total revenue and total costs in a given year. Cashflow is calculated as the difference between net profits and depreciation as in Lage de Sousa and Ottaviano (2018). The gross margin ratio is calculated as the ratio of sales revenue over maintenance cost and all other costs not directly linked to production.

Subsidized credit data are merged with firm balance sheet data using the firm identifier

[87] For further information see Law n°5.534 from 14.11.1968 about Statistical Reporting. Penalties for manipulation of data by bureaucrats are one-montly salary. Paying the penalty does not exonerate someone from reporting the misreported information correctly afterwards.

from the Brazilian National Registry of Legal Entities (Portuguese: Cadastro Nacional de Pessoas Jurídicas, "CNPJ") and the year. The CNPJ is a nationwide registry of corporations, partnerships, foundations, investment funds, and other legal entities maintained by the Brazilian Federal Revenue Service (Receita Federal do Brasil, "RFB").

Inflation and Exchange Rate Data - Information on inflation and exchange rates used to deflate monetary variables and to convert values into 2020 USD for comparability with the literature is drawn from two sources. First, data on inflation in Brazil over the observation period are taken from the World Bank's World Development Indicators (WDI) database and are used to deflate all monetary variables into 2020 Brazilian real (R$) values. Second, data on U.S. inflation and the exchange rate of the Brazilian real relative to the U.S. dollar are taken from the Federal Reserve Economic Data (FRED) database of the Federal Reserve Bank of St. Louis.

A.4 Validity of the Empirical Design

Table 13: BNDES Support Take-Up and Non-Production-Workers

Sample	Stayers					
Dep. Variable	Sub. Credit (0/1)					
Specification	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Employees not in Production	.000032***	.000047***	.000058***			
	(3.2e-06)	(1.6e-06)	(.000021)			
Wage Bill Share of Labor not in Prod.				.15***	.06***	.037*
				(.028)	(.022)	(.019)
R-squared	.25	.37	.85	.05	.31	.84
Obs.	317,969	317,967	315,042	317,969	317,967	315,042
Firm FE	No	No	Yes	No	No	Yes
Ind FE	No	Yes	No	No	Yes	No
Mesoreg FE	No	Yes	No	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. Data: Brazil (2002–2019). All observations are at the firm-year level. All regressions use firm sales as analytical weights. Results pertain to subsidized credit coded as an absorbing treatment. Results for punctual treatment are available upon request and display a qualitatively similar pattern. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Estimation Results

B.1 Exclusion Restriction

As most variables of interest with respect to which one seeks to perform comparative statics in equation (16) are endogenous, it is necessary to take total differentials using the implicit function theorem. Therefore, rewrite equation 16 as an equality, as it defines the

cutoff above which firms are likely to select into subsidized credit.

$$s = 1 - \frac{i(\omega, \delta, k, c) + f_t + 1}{c\, i(\omega, \delta, k, (1-s)c)}. \tag{51}$$

For simplicity, define:

$$i_0 \equiv i(\omega, \delta, k, c), \qquad i_1 \equiv i(\omega, \delta, k, (1-s)c), \tag{52}$$

Next, write the implicit function as

$$F(s, \omega, \delta, k, c, f_t) \equiv s - 1 + \frac{i_0 + f_t + 1}{c\, i_1} = 0. \tag{53}$$

The total differential is

$$dF = F_s\, ds + F_\omega\, d\omega + F_\delta\, d\delta + F_k\, dk + F_c\, dc + F_f\, df_t = 0. \tag{54}$$

Next, isolate the term ds.

$$ds = -\frac{F_\omega\, d\omega + F_\delta\, d\delta + F_k\, dk + F_c\, dc + F_f\, df_t}{F_s}. \tag{55}$$

For $x \in \{\omega, \delta, k\}$, the partial derivatives take the following form:

$$F_x = \frac{1}{c} \frac{i_{0x} i_1 - (i_0 + f_t + 1) i_{1x}}{i_1^2}. \tag{56}$$

The derivative of s can also be expressed as:

$$F_s = 1 + \frac{i_0 + f_t + 1}{i_1^2}\, i_{1c'}, \tag{57}$$

where I define c' as:

$$c' = (1-s)c, \qquad i_{1c'} \equiv \frac{\partial i(\omega, \delta, k, c')}{\partial c'}. \tag{58}$$

It is important to note that i_1 depends on s through $(1-s)c$, so $F_s \neq 1$. The object of interest is:

$$\frac{ds}{dx} = -\frac{F_x}{F_s} = \frac{\frac{1}{c} \frac{i_{0x} i_1 - (i_0 + f_t + 1) i_{1x}}{i_1^2}}{1 + \frac{i_0 + f_t + 1}{i_1^2}\, i_{1c'}}. \tag{59}$$

Or explicitly,

$$\frac{ds}{dx} = -\frac{\frac{1}{c} \frac{i_x(\omega,\delta,k,c) i(\omega,\delta,k,(1-s)c) - (i(\omega,\delta,k,c) + f_t + 1) i_x(\omega,\delta,k,(1-s)c)}{i(\omega,\delta,k,(1-s)c)^2}}{1 + \frac{i(\omega,\delta,k,c) + f_t + 1}{i(\omega,\delta,k,(1-s)c)^2}\, i_{c'}(\omega, \delta, k, (1-s)c)}. \tag{60}$$

The expression on the left-hand side in equation (60) is negative for $x \in \{\omega, \delta\}$ and positive for $x = k$ given the function $i(\cdot)$, implying that increases in $x \in \{\omega, \delta\}$ make it more likely that equation (16) holds, if:

$$i_x(\omega, \delta, k, c)i(\omega, \delta, k, (1-s)c) < (i(\omega, \delta, k, c) + f_t + 1)i_x(\omega, \delta, k, (1-s)c) \tag{61}$$

In other words, $ds/dx < 0$ implies that (16) is more easily satisfied for firms with larger $x \in \{\omega, \delta\}$, meaning that these firms can profitably select into treatment even when the subsidy s is relatively small. Whether (61) and, subsequently, (16) hold depends, ceteris paribus, primarily on the magnitude of the subsidy and the fixed costs.

B.2 First Stage Analysis

B.2.1 Alternative Timing of Depreciation Shocks

Mean deviations from the depreciation rate at $t-1$

Table 14: First Stage Analysis: Depr. Shock at $t-1$

Dep. Variable	Sub. Credit (0/1)					
Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-1}$	-.31***	-.36***	-.36***	-.35***	-.39***	-.42***
	(.051)	(.059)	(.063)	(.069)	(.059)	(.07)
R-squared (1^{st} Stage)	.0033	.0048	.0045	.0043	.0046	.0057
Weak F-test (2^{nd} Stage)	37.44	37.34	32.05	25.30	43.28	36.20
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,127	142,854	125,014	108,127	92,952	78,916
# (Control) Lags	3	3	3	3	3	3
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Mean deviations from the depreciation rate at $t-3$

Table 15: First Stage Analysis: Depr. Shock at $t-3$

Dep. Variable			Sub. Credit (0/1)			
Sample			Stayers			
Estimated in			Long-Differences			
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-3}$	-.28***	-.31***	-.33***	-.35***	-.32***	-.34***
	(.057)	(.064)	(.064)	(.069)	(.072)	(.08)
R-squared (1^{st} Stage)	.0033	.0045	.0044	.0044	.004	.0051
Weak F-test (2^{nd} Stage)	24.76	24.01	26.85	25.01	20.32	18.27
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,254	142,938	125,069	108,171	92,988	78,946
# (Control) Lags	3	3	3	3	3	3
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

B.2.2 Alternative Measures of Depreciation Shocks

Indicator depreciation at $t-2$

Table 16: First Stage Analysis: Depr. Indicator

Dep. Variable			Sub. Credit (0/1)			
Sample			Stayers			
Estimated in			Long-Differences			
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-2}$	-.037***	-.043***	-.044***	-.048***	-.041***	-.044***
	(.0058)	(.0061)	(.0066)	(.0067)	(.0075)	(.0087)
R-squared (1^{st} Stage)	.0053	.0073	.0074	.0082	.007	.0086
Weak F-test (2^{nd} Stage)	41.12	48.86	45.58	51.76	29.24	25.74
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,784	143,327	125,368	108,397	93,163	79,082
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Depreciation rate at $t-2$

Table 17: First Stage Analysis: Depr. Rate

Dep. Variable	Sub. Credit (0/1)					
Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-2}$	-.28***	-.31***	-.31***	-.38***	-.33***	-.37***
	(.052)	(.056)	(.061)	(.055)	(.063)	(.076)
R-squared (1^{st} Stage)	.0031	.0044	.0041	.0046	.004	.0051
Weak F-test (2^{nd} Stage)	29.05	30.31	24.81	46.09	28.28	23.44
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,784	143,327	125,368	108,397	93,163	79,082
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3 Second Stage Analysis

B.3.1 Lead-Lag Exogeneity Condition

Table 18: Testing the LP-IV Lag-Exogeneity Condition

Sample	Stayers			
Dep. Variable	Depr. Shock (Mean Dev. Repair Rate) $t-2$			
Specification	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
% Δ Sales (t-1)	.001	.00028	.00025	.00061
	(.0018)	(.0017)	(.0018)	(.0014)
% Δ Sales (t-2)	.0021**	.00076	.00097	.00078
	(.00094)	(.00071)	(.00065)	(.00083)
% Δ Sales (t-3)	.0012	.00014	.00035	.000072
	(.00082)	(.00062)	(.00061)	(.00063)
R-squared	.007	.059	.093	.34
Obs.	185,272	185,271	185,189	183,662
Firm FE	No	No	No	Yes
Ind × Year FE	No	No	Yes	No
Mesoreg × Year FE	No	No	Yes	No
Ind FE	No	Yes	No	No
Mesoreg FE	No	Yes	No	No
Year FE	Yes	Yes	No	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3.2 Alternative lag lengths

Table 19: Impact of BNDES Support on Sales (1 lag)

Dep. Variable	% Changes in Sales w.r.t Base Year					
Sample	All Firms					
Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	.01	.01	.022	.065*	.1***	.12***
	(.027)	(.029)	(.029)	(.034)	(.038)	(.037)
2SLS	.12	.29	.24	.23	.27	.18
	(.17)	(.19)	(.2)	(.19)	(.23)	(.29)
Weak F-test (2^{nd} Stage)	46.03	43.84	37.11	59.68	33.17	26.56
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	210,910	188,290	167,477	148,116	129,787	112,210
# (Control) Lags	1	1	1	1	1	1
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Impact of BNDES Support on Sales (2 lags)

Dep. Variable	% Changes in Sales w.r.t Base Year					
Sample	All Firms					
Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	.035	.032	.038	.083**	.12***	.15***
	(.023)	(.028)	(.03)	(.035)	(.038)	(.037)
2SLS	.12	.18	.15	.15	.12	.15
	(.2)	(.18)	(.2)	(.19)	(.23)	(.28)
Weak F-test (2^{nd} Stage)	36.21	35.16	30.04	49.81	26.36	21.78
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	185,363	164,500	145,210	127,237	110,017	94,598
# (Control) Lags	2	2	2	2	2	2
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Impact of BNDES Support on Sales (4 lags)

Dep. Variable	% Changes in Sales w.r.t Base Year					
Sample	All Firms					
Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
OLS	.014	.021	.014	.052	.12***	.13***
	(.027)	(.033)	(.032)	(.039)	(.039)	(.036)
2SLS	-.018	.092	.059	.19	.32	.41
	(.3)	(.26)	(.3)	(.26)	(.32)	(.39)
Weak F-test (2^{nd} Stage)	22.95	23.21	18.60	38.06	20.90	14.75
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	141,067	123,226	106,476	91,531	77,683	64,921
# (Control) Lags	4	4	4	4	4	4
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.4 Mechanisms

Credit Constraints -

Table 22: BNDES Support and Credit Constraints

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	.098	.38	.34	.31	.28	.84
	(.33)	(.27)	(.34)	(.35)	(.42)	(.63)
Credit Constrained (0/1)	.038**	.071***	.073***	.083***	.077***	.13***
	(.018)	(.017)	(.021)	(.023)	(.03)	(.04)
Sub. Credit (0/1) × Credit Constrained (0/1)	-.29	-.82*	-.8	-.79	-.56	-1.5*
	(.47)	(.43)	(.51)	(.53)	(.68)	(.88)
Weak F-test (2^{nd} Stage)	11	10.52	11.64	9.418	10.80	6.581
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	.041	.067*	.11***	.12**	.16***	.18***
	(.027)	(.037)	(.039)	(.049)	(.057)	(.056)
Credit Constrained (0/1)	.027***	.036***	.038***	.045***	.054***	.058***
	(.0056)	(.0078)	(.01)	(.012)	(.014)	(.017)
Sub. Credit (0/1) × Credit Constrained (0/1)	.0015	-.025	-.019	.0028	-.065	-.094
	(.039)	(.046)	(.053)	(.065)	(.076)	(.073)
Obs.	162,114	142,815	124,961	108,061	92,896	78,863
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A reports the 2SLS estimates, whereas Panel B reports the OLS estimates. The Kleibergen–Paap weak-instrument F-statistic indicates a weak first stage, rendering the 2SLS estimates uninformative. In contrast, the OLS results align with the baseline findings, with cumulative sales growth increasing over time and becoming statistically significant at similar magnitudes. The interaction term suggests that the differential effects for credit-constrained firms, relative to unconstrained firms, are not statistically distinguishable from zero. The positive and significant association between credit constraints and cumulative sales growth should be interpreted cautiously, as it may partly reflect measurement error in the credit-constraint indicator, a concern well-documented in the finance literature (H. Almeida et al., 2004; Kaplan and Zingales, 1997).

Zombie firms -

Table 23: BNDES Support and Changes of the Financial Cost Share

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	-.022	.29	.24	.19	.17	.27
	(.27)	(.25)	(.3)	(.28)	(.33)	(.39)
Fin. Cost Share	-.0017	-.0091*	-.0075	-.0048	-.0012	-.0037
	(.0058)	(.0055)	(.0057)	(.0071)	(.0077)	(.0095)
Sub. Credit (0/1) × Fin. Cost Share	-.13	-.023	-.063	-.064	-.053	-.025
	(.1)	(.076)	(.081)	(.079)	(.076)	(.1)
Weak F-test (2^{nd} Stage)	12.35	11.20	8.885	16.68	10.62	9.811
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	.026	.032	.037	.078*	.14***	.15***
	(.027)	(.031)	(.036)	(.043)	(.044)	(.044)
Fin. Cost Share	-.011***	-.01***	-.012***	-.01**	-.008	-.012**
	(.0032)	(.0036)	(.004)	(.0043)	(.0049)	(.0058)
Sub. Credit (0/1) × Fin. Cost Share	-.004	-.027	-.036*	-.024	.0052	.046**
	(.011)	(.021)	(.022)	(.017)	(.018)	(.023)
Obs.	135,518	119,518	104,702	90,707	78,145	66,546
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Capital-Labor Ratio - An increase in the capital–labor ratio in response to the take-up of BNDES-subsidized credit does not necessarily occur if a firm is merely expanding its production capacity. Therefore, a positive and significant correlation between changes in the capital–labor ratio and the take-up of BNDES-subsidized credit can be interpreted as a first indication of production upgrading.

Table 24: BNDES Support and the Capital-Labor Ratio

Sample	Stayers					
Estimated in	Levels (capital (in R$) per worker)					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS						
Sub. Credit (0/1)	611,480***	705,384***	661,500**	819,979***	817,486***	889,622***
	(205211)	(252049)	(256838)	(294532)	(190150)	(168340)
Obs.	185,280	164,416	145,128	127,158	109,939	94,530
# (Control) Lags	3	3	3	3	3	3
Firm FE	No	No	No	No	No	No
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 24 reports regression results in which the dependent variable, the capital–labor ratio, is measured as capital (in R$) per worker. From left to right, the table presents results for different time spans after treatment take-up, ranging from $t+1$ to $t+6$.
An important caveat of the results presented in Table 24 is that the estimation is conducted in levels and does not include firm fixed effects, which will be incorporated in future versions. As a necessary condition for production upgrading, the capital–labor ratio increases significantly after BNDES subsidized credit take-up, with effects that strengthen over time. If beneficiary firms merely replace old machines without adopting more modern, capital-intensive production processes, BNDES credit likely does not generate additional activity beyond the laissez-faire equilibrium, as the externalities associated with production upgrading are not internalized. The result speaks to the findings of De Souza (2023), who show that Brazilian firms, in the context of a large R&D subsidy program, tended to create local versions of foreign products rather than advance the technological frontier.

Productivity - To obtain total-factor productivity (TFP) measures, production function parameters are typically estimated at the two-digit industry level to ensure sufficient statistical power, as is done here. When examining the impact of BNDES-subsidized credit on TFP, one must be aware of aggregation issues that arise in industry-level regressions. Since BNDES support is provided at the firm level, industry-level averages or medians of BNDES support mask important firm-level heterogeneity. This is particularly relevant because non-automatically approved BNDES support is taken up mainly by large firms.

Table 25: BNDES Support and Productivity

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	-.002	-.0015	-.0035**	-.0014	.0016	-.0033*
	(.0025)	(.002)	(.0014)	(.00098)	(.0014)	(.0018)
Weak F-test (2^{nd} Stage)	26.96	27.26	22.57	41.58	26.32	24.14
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	.00033	-.00018	-.00023***	-.000095	.000095	.00018
	(.0003)	(.00013)	(.000082)	(.00011)	(.000069)	(.00012)
Obs.	157,999	139,189	121,791	105,332	90,544	76,868
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A of Table 25 presents the 2SLS results. The Kleibergen–Paap weak-instrument F-statistic appears reasonably strong; however, these estimates may not be causal, as the framework underlying the exclusion restriction in Section 3.3 is not directly applicable in this context. Table 25 paints, if anything, a picture in which BNDES support appears to reduce the TFP of treated firms.

Labor Productivity - In the following, I analyze the impact of BNDES-subsidized credit on a simple single-factor, revenue-based labor productivity measure that does not adjust for changes in product specifications or quality: sales revenue per worker.

Table 26: BNDES Support and Labor Productivity

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	-.85**	-.94*	-.93*	-.96**	-1.5**	-1.6***
	(.41)	(.54)	(.56)	(.4)	(.65)	(.56)
Weak F-test (2^{nd} Stage)	25.81	26.73	20.35	39.75	24.25	20.33
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	-.23***	-.16*	-.12	-.11	.13**	-.062
	(.071)	(.092)	(.1)	(.11)	(.062)	(.15)
Obs.	147,250	130,132	114,149	98,867	84,992	72,030
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26 presents the results of regressions for the cumulative growth rates. Columns (1)–(6) analyze labor productivity, measured relative to sales, and test whether the marginal product of labor increased over time following BNDES support take-up. This is an important outcome in light of the skill-biased, automation-driven technological change that generally characterizes the 21^{st} century (Acemoglu and Restrepo, 2022). On the one hand, the decline in labor productivity following the take-up of BNDES-subsidized credit is not surprising, given that the measure is based on sales and that sales growth after BNDES take-up is, at best, modest. On the other hand, this may help explain why BNDES-subsidized credit is not associated with substantial output growth, as the decline in labor productivity suggests that production upgrading is unlikely to have occurred.

Employment -

Table 27: Impact of BNDES Support on Total Employment over Time

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Sub. Credit (0/1)	.053***	.068***	.06**	.061*	.07*	.062
	(.018)	(.022)	(.025)	(.033)	(.039)	(.045)
Obs.	162,200	142,900	125,044	108,143	92,969	78,931
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 28: Impact of BNDES Support on Blue-Collar Employment over Time

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Sub. Credit (0/1)	.1***	.12***	.098***	.1**	.078*	.069
	(.024)	(.031)	(.038)	(.045)	(.046)	(.052)
Obs.	161,251	142,075	124,335	107,541	92,482	78,540
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 29: Impact of BNDES Support on White-Collar Employment over Time

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Sub. Credit (0/1)	-.018	-.032	-.011	-.0081	.026	.016
	(.025)	(.027)	(.037)	(.041)	(.048)	(.058)
Obs.	116,215	102,406	89,906	78,250	67,811	58,142
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Profits - Table 30 reports coefficients in millions of 2020 R$. To express the results in 2020 USD, the coefficients can be divided by approximately 5 (the exchange rate on January 1, 2020 was 5.1587 R$ per USD).

Table 30: Impact of BNDES Support on Profits over Time

Sample	Stayers					
Estimated in	Levels					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	1,676***	1,353***	1,468***	853***	816**	-71
	(337)	(351)	(374)	(299)	(380)	(357)
Weak F-test (2^{nd} Stage)	67.32	53.45	42.72	112.5	96.61	78.70
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	438***	219***	332***	282***	268***	36
	(83)	(67)	(59)	(70)	(77)	(79)
Obs.	185,280	164,416	145,128	127,158	109,939	94,530
# (Control) Lags	3	3	3	3	3	3
Firm FE	No	No	No	No	No	No
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.5 Heterogeneity

B.5.1 Industries

The impact of BNDES support on firm output is likely to be heterogeneous across industries. Production functions may differ across industries. Bartelme, Costinot, et al. (2024) demonstrate that the impact of industrial policy varies across industries, as different industries exhibit different scale elasticities.

Table 31: Impact of BNDES Support by Industry

Sample	Specification	Respective Industry (only Stayers)					
Dep. Variable		% Changes in Sales w.r.t. Base Year (Long-Diff)					
Industry	Estimator	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
Foodstuffs	OLS	.043	.074	.13**	.11	.075	.13
	2SLS	.41	.48	.89	-.18	.17	-2
	Weak F-stat	1.739	1.769	1.095	9.092	0.426	0.242
	Observations	21,742	18,936	16,400	14,026	11,925	10,048
Beverages	OLS	.019	-.016	.015	.073	-.066	.44
	2SLS	-6.2	-4.8	-1.8	-2	-3.3	-5.2
	Weak F-stat	0.439	0.469	0.676	0.834	0.848	1.398
	Observations	2,382	2,093	1,829	1,576	1,361	1,156
Textiles	OLS	.067	-.031	-.036	.034	.061	-.028
	2SLS	1.6	6.3	16	-36	-40	29
	Weak F-stat	0.0552	0.0920	0.00252	0.00215	0.00229	0.0114
	Observations	7,323	6,509	5,732	4,992	4,324	3,700
Clothing and Accessories	OLS	-.076**	-.13***	-.096**	-.16***	-.11	-.27***
	2SLS	2	6	8.4	8.6	16	4.2
	Weak F-stat	0.262	0.421	0.216	0.250	0.345	0.148
	Observations	12,041	10,323	8,769	7,360	6,154	5,081
Leather and Footwear	OLS	-.092	-.12	-.082	.12	.048	.00088
	2SLS	-125	-105	-288	-255	-97	-112
	Weak F-stat	0.0118	0.0292	0.0105	0.0127	0.164	0.167
	Observations	5,606	4,818	4,084	3,429	2,907	2,446
Wood Products	OLS	.19***	.66***	.84***	.93***	1.1***	1.2***
	2SLS	-1.1	1.4***	1.7***	1.4***	2***	1.8***
	Weak F-stat	2.248	2.525	3.752	7.485	3.545	7.330
	Observations	3,833	3,396	2,984	2,619	2,274	1,965
Pulp, Paper, and Paper Products	OLS	.07**	.14**	-.097	.26***	.31***	.16

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Table 31 – continued from previous page

Sample	Specification	Respective Industry (only Stayers)					
Dep. Variable		% Changes in Sales w.r.t. Base Year (Long-Diff)					
Industry	Estimator	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	2SLS	-.69	-1	-.97	-1.8	-1.2	-1.2
	Weak F-stat	3.642	4.104	3.733	2.854	2.679	2.618
	Observations	4,629	4,084	3,598	3,139	2,740	2,340
Coke, Refining and Alcohol	OLS	.25	.39***	.25	.034	-.046	-.17
	2SLS	-3.2	-2.4	-1	-.67	-.8	-7.3
	Weak F-stat	5.754	5.600	4.689	2.057	1.167	0.312
	Observations	877	753	640	539	451	372
Chemicals	OLS	-.064	-.03	-.01	-.003	-.04	-.17**
	2SLS	-.69	1.5	1	1.8	1.4	3.8
	Weak F-stat	3.321	4.431	4.920	3.777	3.413	4.162
	Observations	9,305	8,224	7,229	6,269	5,389	4,578
Pharmaceuticals	OLS	.11**	.22***	.29***	.34***	.43***	.54***
	2SLS	.87	.26	.39	1	2	.57
	Weak F-stat	1.924	1.888	1.629	1.442	1.276	1.001
	Observations	1,800	1,633	1,474	1,321	1,173	1,027
Rubber and Plastics	OLS	-.13**	-.28**	-.022	-.026	.13	-.13
	2SLS	.26	1.1	.69	-4.7	-11	-11
	Weak F-stat	3.677	4.558	4.409	4.052	3.509	2.775
	Observations	12,491	11,027	9,663	8,366	7,187	6,096
Non-Metallic Mineral Products	OLS	-.017	.018	.072	.11	.16	.25**
	2SLS	.17	3	6.2	5.2	5.7	4.7
	Weak F-stat	1.790	2.184	0.982	1.518	1.287	1.197
	Observations	9,926	8,784	7,736	6,703	5,740	4,851
Metal Products	OLS	.18	.3**	.28	.18	.11	-.034
	2SLS	.029	23	50	-15	-18	-3.9
	Weak F-stat	0.168	0.0412	0.0252	0.0143	0.0625	1.114
	Observations	3,528	3,115	2,721	2,350	2,013	1,698
Metallurgy	OLS	-.05	.082	.037	.23***	.24***	.28***
	2SLS	-.34	.71	-3.2	18	72	72
	Weak F-stat	1.850	1.642	0.121	0.0843	0.00346	0.00465
	Observations	11,335	10,020	8,751	7,547	6,446	5,429
IT, Electronic and Optical Equipment	OLS	.11	.14	.21	.43	.22	.17

Continued on next page

Table 31 – continued from previous page

Sample	Specification	Respective Industry (only Stayers)					
Dep. Variable		% Changes in Sales w.r.t. Base Year (Long-Diff)					
Industry	Estimator	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	2SLS	1.3	-20	-21	-455	-60	-33
	Weak F-stat	0.217	0.218	0.227	0.0004	0.0402	0.0940
	Observations	2,482	2,167	1,861	1,565	1,304	1,062
Electronic Equipment	OLS	.052	.057	-.28	-.36	-.32	.06
	2SLS	-88	-78	-75	-80	-46	-45
	Weak F-stat	0.424	0.406	1.141	1.048	2.083	2.060
	Observations	4,147	3,661	3,192	2,750	2,343	1,962
Machinery	OLS	-.036	-.084	-.15	.044	.049	
	2SLS	45	18	16	37	21	
	Weak F-stat	2.929	2.803	2.646	1.889	0.830	
	Observations	9,299	8,033	6,797	5,607	4,486	
Motor Vehicles	OLS	.065	.053	.094	.2***	.13**	.13*
	2SLS	.59***	.76***	.81***	.89***	1.2***	1.4***
	Weak F-stat	17.30	16.15	13.97	19.08	16.27	18.72
	Observations	6,022	5,398	4,803	4,243	3,732	3,235
Other Transport Equipment	OLS	-.48***	-.26	-.11	.37**	.78***	.82**
	2SLS	2.6	2	-3.3	-2.1	.68	4
	Weak F-stat	1.732	0.564	0.687	1.186	1.256	1.296
	Observations	907	795	690	589	495	415
Other Manufacturing	OLS	.14**	-.0085	-.034	.022	.089	-.015
	2SLS	-9.9	-24	-23	-36	-20	-23
	Weak F-stat	0.240	0.405	0.412	0.437	0.376	0.739
	Observations	3,903	3,363	2,839	2,342	1,912	1,534

Data: Brazil (2002–2019). All observations are at the firm-year-level.

All regressions are estimated using firm sales as analytical weights.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 31 reports the results for the main specification in equation 1 by industry, estimated using OLS and 2SLS, respectively. For the vast majority of industries, the Kleibergen–Paap weak-instrument F-statistic indicates that the 2SLS estimates are affected by weak instruments, likely reflecting the combination of a substantially reduced sample size (i.e., using only observations from a single industry) and the inclusion of high-dimensional fixed effects. Nevertheless, there is substantial heterogeneity across industries, and both OLS and 2SLS coefficients are statistically significant in several cases. In some specifications, the Kleibergen–Paap weak-instrument F-statistic is also sufficiently large, indicating ad-

equate instrument strength.

According to the OLS estimates, on the one hand, firms in the wood products, pulp and paper, pharmaceuticals, metallurgy, and motor vehicles industries are associated with significant increases in output for the majority of time horizons following BNDES take-up, often with coefficients exceeding those in the main specification. On the other hand, the clothing and accessories industry stands out as one in which the take-up of subsidized BNDES credit is associated with a subsequent decline in sales growth. According to the 2SLS estimates, firms in the wood products and motor vehicles industries are associated with significant accelerations in cumulative sales growth. However, only for firms in the motor vehicles industry does the Kleibergen–Paap weak-instrument F-statistic exceed the conventional threshold in the majority of time horizons. In contrast, no industry appears to exhibit a subsequent decline in sales growth following BNDES take-up.

B.6 Robustness

B.6.1 Treatment Groups

Refining the treatment group may improve the robustness of the analysis, as any meaningful comparison must account for potential differences beyond treatment status that could bias the results. In what follows, I examine various factors that could potentially affect the composition and size of the treatment group.

Certain firms might have benefited simultaneously from additional government programs or interventions beyond the BNDES subsidized credit. To address this issue, regressions including industry fixed effects should, in general, be robust to the large majority of other government programs. Conceptualizing the firm within a production function framework, this statement can be justified as follows. Capital subsidies in Brazil are primarily firm-specific but are administered through BNDES, which is controlled for in the following results. Labor subsidies, such as those studied in Lobel (2024), which were the quantitatively most important labor subsidies during the observation period, had eligibility defined at the product and industry level. Industry fixed effects should also account for this, at least approximately. Intermediate input subsidies—most frequently implemented as exemptions from the IPI tax (tax on industrialized products)—occurred primarily at the product level during the observation period as well. The only type of policy that is highly firm-specific consists of R&D subsidies, most notably studied in De Souza (2023), for which I unfortunately lack data access. Concerning the intensive margin of additional government programs, I follow the assumption of Lage de Sousa and Ottaviano (2018) that BNDES support constitutes the primary policy instrument influencing firms' productive investment and, consequently, output. This assumption is justified by the fact that, as noted by Lage de Sousa and Ottaviano (2018), the BNDES is the largest financial institution providing loans for long-term investment projects in Brazil.

There may be a delay before the effects of BNDES loans become observable, as outcomes often do not materialize immediately after funds are disbursed—or even before project implementation is complete. Since some investment projects span at least five years, it is necessary to consider a period beyond this horizon to evaluate their impacts both during and after implementation. Given that the BNDES loan records span the period from 2002 to 2023, this extended timeframe allows for a comprehensive long-term evaluation of all granted loans. I continue to focus on the sample of surviving firms, among which 517 received BNDES support at least once. I do not further refine the treated groups by distinguishing firms based on the number of times they received BNDES support, as Lage de Sousa and Ottaviano (2018) do, since this paper focuses on the extensive margin and leaves the intensive margin for future research.

B.6.2 Control Groups

As previously discussed, the goal is to assess what would have happened to the treated firms had they not received BNDES support. This raises the key question: how can one construct a credible counterfactual for these groups of treated firms? In the absence of a natural experiment or randomized control trial, identifying such a counterfactual is inherently challenging and requires careful methodological considerations. Following Lage de Sousa and Ottaviano (2018), I explore several alternative strategies to construct control groups that most closely resemble the treated groups in terms of their pre-treatment characteristics. Clearly, for the specific purpose of this investigation using propensity score matching, depreciation shocks and credit constraints should be excluded from the set of pre-treatment characteristics used to define control groups. This exclusion is justified by the fact that eligibility for BNDES funding does not require firms to demonstrate the existence of prior depreciation shocks or credit constraints. This approach allows for an ex post comparison of treated and non-treated firms that were ex ante similar across several key dimensions—except for their experienced depreciation shocks.

B.6.3 "Naive" Control Group

The first, so-called “naive” control group (Group A), includes all Brazilian manufacturing firms with more than 29 employees that did not receive any BNDES support throughout the observation period. The instrumental variable strategy addresses the non-random selection of firms into BNDES support. Nonetheless, systematic differences due to residual heterogeneity may persist within the “naive” groups of treated and untreated firms, highlighting the need for alternative group definitions to test the robustness of the results. For a general overview, Table 32 summarizes key pre-treatment characteristics of treated and untreated firms prior to receiving BNDES support.

Table 32: Average of Treated and Untreated Firms Before Matching

Groups Variables	Non-Treated Firms All Stayers	Treated Firms
$depr_shock_{firt}$	0.0068	-0,0039
(Mean Deviations)		
Labor Productivity	98,816	197,415
(Sales per Worker in R$)		
Labor Productivity Growth	-146.8	5.51
Number of Employees	172	1979
Labor not in Prod.	38	600
Investment/Capital	0.055	0.043
Cash Flow/Capital	-33.1	-0.5
Exporter Status	0,28	0,74
OECD Classification		
Medium-High Tech	0.25	0.37
Medium-Low Tech	0.63	0.61
Number of Firm Observations	312,483	5486

Treated firms appear to experience stronger depreciation shocks, as negative mean deviations indicate shocks that could not be repaired and instead required investment to renew depreciated capital. This finding aligns with the instrumental variable strategy, which assumes that firms experiencing greater past depreciation shocks are more likely to take up BNDES investment—provided they can absorb the associated fixed cost. Likewise, consistent with the IV strategy, treated firms tend to employ a larger number of non-production (administrative) workers, which better equips them to manage the bureaucratic fixed costs associated with accessing BNDES support.

Next, treated firms seem to face more binding credit constraints than their non-treated counterparts. This is evidenced by a lower cash flow-to-capital ratio among treated firms. Interestingly, treated firms also exhibit a lower investment-to-capital ratio than non-treated firms. On the one hand, this pattern is consistent with tighter credit constraints among treated firms—i.e., they have less cash available to finance their investments. On the other hand, the low investment-to-capital ratio could also reflect a form of Ashenfelter dip, whereby firms invest little prior to selecting into treatment, after which they undertake substantial investment. Furthermore, it could also reflect sectoral differences. In particular, treated firms tend to operate more frequently in higher-risk industries, as categorized by the OECD technological classification.

Turning to productivity and size indicators, treated firms are, on average, larger and exhibit higher productivity than non-treated firms. This pattern holds for labor productivity (measured as sales per worker) and for its growth rate, which is also higher among treated firms.

B.6.4 Balancing Treated and Non-Treated Groups

As discussed earlier, differences between treated and non-treated groups reflect the presence of selection bias. To verify the robustness of the baseline results to selection bias arising from observables, this section minimizes the differences between treated and non-treated groups in terms of the observable characteristics presented in Table 32. Following Lage de Sousa and Ottaviano (2018), I employ a propensity score matching (PSM) procedure that combines caliper matching with one-to-one matching. Under one-to-one matching, each treated firm is paired with the control firm whose propensity score is closest in value (Caliendo and Kopeinig, 2005). When using pure caliper matching, a tolerance level is imposed on the maximum allowable propensity score distance, thereby avoiding poor matches that may arise under nearest-neighbor matching when the closest neighbors are still far apart (Caliendo and Kopeinig, 2005). Combining both approaches mitigates the weaknesses of each: pure one-to-one matching may produce poor matches, while pure caliper matching reduces the number of matches and thus increases the variance of estimates. Following Lage de Sousa and Ottaviano (2018), I implement matching without replacement but set a stricter caliper of 0.001.

The choice of variables on which the matching strategy is based must take several considerations into account. First, the most appropriate comparison to avoid selection bias in this context, is between treated and non-treated firms that are both eligible for non-automatically approved BNDES-subsidized credit. Second, other observable characteristics may influence firms' cumulative sales growth. Indeed, omitting relevant variables can be a significant source of bias (Heckman et al., 1997). Third, selection bias arising from unobservables can also be problematic. Lage de Sousa and Ottaviano (2018) mitigate this issue in line with common practice in the PSM literature, by increasing the number of covariates included in the propensity score estimation (Caliendo and Kopeinig, 2005). To enhance the comparability between the treatment and control groups, both selection based on eligibility (point 1) and selection based on observables (point 2) are important considerations for choosing variables in the matching procedure. Selection based on unobservables (point 3), however, is addressed by the instrumental variable strategy developed in this paper and therefore does not need to be considered when choosing covariates for the propensity score estimation.

Regarding eligibility, the included covariates unsurprisingly relate to how banks address their basic principal–agent problem with a borrower, namely, assessing the borrower's ability to repay the loan. One key indicator of a firm's ability to repay is the availability of collateral. Since firms typically collateralize tangible assets, I proxy collateral availability using the firm's capital stock. A second set of indicators reflects the availability of

cash flow, which I measure using: net profits; the ratio of financial costs minus financial revenue to total revenue (a measure of "solvency"); and proxies for firm size. To proxy for firm size, I use: the number of employees and intermediate input expenditures. I deviate from Lage de Sousa and Ottaviano (2018) by including net profits instead of profits over total sales, since sales revenue is not included as it serves as my outcome variable. However, I also include labor productivity (one of the outcomes in Lage de Sousa and Ottaviano (2018)) as additional proxy for a firm's ability to generate cash flow. According to Lage de Sousa and Ottaviano (2018), other indicators considered by BNDES include: firms' market share, multinational status, and geographic location, specifically whether firms are situated in Brazil's most developed ("rich") regions. These correspond to the South and Southeast, which together account for nearly 85% of Brazilian manufacturing production (Lage de Sousa and Ottaviano, 2018). Multinational status is not observed. Nevertheless, to capture firms' integration into the global economy, I include an export indicator variable. Whether a firm is located in the South or Southeast is represented by an indicator variable. Finally, a two-digit sectoral dummy is included to capture the sectoral structure of BNDES operations. In addition, I include an indicator variable for whether a three-digit sector is classified as high-tech. To account for pre-treatment time trends, the growth rates of labor productivity (revenue in Lage de Sousa and Ottaviano (2018)), profits (calculated as in Davis et al. (1996)), and employment are also included.

To identify which of these indicators are most strongly associated with BNDES-subsidized credit take-up, I estimate a probit model and, for robustness, also report the results of a logit model. Table 33 presents the results of these estimations.

Table 33: Propensity Score Estimation

Estimator	Probit	Logit
Dep. Variable	BNDES Dummy	
Total Assets	1.8e-11***	3.5e-11
	(5.4e-12)	(2.6e-11)
Net Profits	-3.7e-10***	-7.4e-10*
	(3.9e-11)	(4.1e-10)
(DHS) Profit Growth	.0055	.016
	(.0052)	(.014)
Solvency	-.000038	-.000024
	(.00016)	(.00028)
Number of Employees (Levels)	.0002***	.00054***
	(.000019)	(.000045)
Number of Employees (Growth)	-.11***	-.19***
	(.023)	(.052)
Intermediate Inputs	3.8e-10***	6.6e-10***
	(3.5e-11)	(7.0e-11)
Exporter Status	.63***	1.5***
	(.016)	(.041)
Market Share	.022**	.022
	(.011)	(.026)
Labor Productivity (Levels)	1.6e-08	3.1e-08
	(1.1e-08)	(2.3e-08)
Labor Productivity (Growth)	-2.3e-07	-4.4e-07
	(1.7e-07)	(2.8e-07)
Rich Region	.012	.047
	(.021)	(.05)
Observations	244,401	244,401
Sector Dummies (2-digit)	YES	YES
OECD High-Tech Dummies (3-digit)	YES	YES
Pseudo-R-squared	0.238	0.231
Hosmer and Lemeshow Statistic (p-value)	0	0
Concordance rate (cutoff=0.5)	97.95	97.96

Column (1) reports the results of the probit estimation, while Column (2) presents the results of the logit estimation. Qualitatively, both regressions yield analogous results. The percentage of concordance and the Hosmer–Lemeshow statistic suggest that the model provides a reasonable fit. Several estimated coefficients are significantly different from zero and display the expected sign. Accordingly, firms with more employees, higher intermediate input expenditures, export status, a larger market share (i.e., larger firms), and a location in the South or Southeast are more likely to receive non-automatically approved subsidized credit. The negative and statistically significant association with the growth rate of employment suggests an Ashenfelter-type dip: it is reasonable that firms do not expand their workforce before obtaining subsidized credit to finance investment, but rather increase hiring once the investment is underway or completed. The statistically significant negative associations with net profits also appear reasonable. Firms with lower net profits may face tighter financial constraints, making subsidized credit particularly at-

tractive.

Next, treated and non-treated firms can be paired based on their similar ex-ante probability of receiving non-automatically approved BNDES subsidized credit, as estimated through PSM. Table 34 reports the number of firms matched. At least 85% of treated firms are successfully matched to non-treated firms with similar characteristics.

Table 34: Number of Matched Firms

Firm Type	Number of Firms
Treated Matched	444
Treated Not Matched	73
Treated	517
Percentage Matched	85.9%

After matching the treated group with its corresponding control group, the next step is to analyze the similarity of the two groups in terms of observable characteristics.[88] To assess whether differences in observable characteristics between the groups are statistically significant, I calculate the mean for each group and perform T-tests, reporting the corresponding p-values. Table 35 reports the group means in levels for the treated group. The treated group displayed here, whose firms may receive treatment multiple times during the observation period, constitutes the refined treated group with the largest number of observations and is therefore likely the most relevant for estimating the average overall effect. The treated group is compared with its corresponding matched control group, and the table also reports T-values and p-values testing whether the differences are statistically significant.

[88]I now proceed with PSM matching based on the probit model, as the results from the logit model in Table 33 are similar in both statistical significance and coefficient signs.

Table 35: Comparing Firms after Matching (Treated Group 1)

Groups	Non-Treated (Stayers)		Treated (Stayers)		Testing Matched Firms	
	Not-Matched	Matched	Matched	Not-Matched	T-Value	P-Value
Sales (in Mio. R$)	52	331	1,442	1,313	-10.26	0
Total Assets (in Mio. R$)	59	404	2,708	2,160	-6.48	0
Number of Employees	152	587	1603	2483	-25	0
Solvency (in %)	23	6	8	11	-2.35	0.02
Net Profits (in Mio. R$)	2.5	22	100	20	-3.35	0.0008
(DHS) Profit Growth (in %)	-0.004	0.017	0.03	0.05	-0.86	0.39
Employment Growth (in %)	1.9	2.9	4.8	3.1	-2.94	0.003
Sales Growth (in %)	3.7	3.9	5.7	6.1	-3.19	0.001
Exporter Status (in %)	26	71	74	68	-4.18	0
Labor Productivity Growth (in %)	-159	163	16	58	0.97	0.33
Cash Flow/K	-34.46	-0.06	0.61	-0.27	-0.88	0.38
Invest/K	0.06	0.05	0.05	0.04	0.04	0.97
Labor not in Prod.	32	178	485	627	-20.7	0
Labor Productivity (in Mio. R$ per Worker)	0.09	0.2	0.2	0.2	0.65	0.52
High & Medium-High Tech (in %)	24	42	41	29	1.38	0.17
Low & Medium-Low Tech	64	58	59	51	-1	0.32
Gross Margin (in %)	320	20	5	5	1.21	0.23
Capital Productivity (in R$ per R$ of Total Assets)	1139	3	2.6	1.4	0.48	0.63
Number of firms	18,933	546	444	73		

All in all, treated and non-treated firms exhibit a higher level in similarity in Table 35 than they did in Table 32. Importantly, at the 5% significance level, the trends in group means of profits and labor productivity do not exhibit any statistically significant differences. Group means in levels exhibit some statistically significant differences, although certain indirect measures of firm size, such as labor and capital productivity, no longer show any significant differences. Most notably, this pattern holds not only for eligibility-related variables selected through the probit model, but also for additional variables (gross margin) excluded from the model due to their irrelevance for eligibility. Since my goal is to measure the impact of non-automatically approved BNDES loans on the cumulative sales growth of treated firms, it is crucial that matched firms display similar trends. This also applies to the ratio of cash flow to capital. In this regard, although treated firms still invest more and appear larger than non-treated firms, their capacity to finance investments has become more similar after PSM.

B.6.5 Baseline Analysis using a Matched Sample

Finally, the baseline analysis can be conducted on the matched sample to assess the robustness of the main results. Rather than replicating the specification used by Lage de Sousa and Ottaviano (2018) with a different outcome (my outcome being cumulative sales growth, unlike productivity for them), I re-estimate the 2SLS specification from section 3 for several reasons. Unlike Lage de Sousa and Ottaviano (2018), I do not observe disaggregated data on domestic and imported capital, and therefore cannot include these as control variables. However, this limitation does not compromise the identification strategy, as the instrumental variable approach addresses potential confounding related to variation in capital composition. Importantly, my instrumental variable approach does

not require the parallel-trend assumption inherent to common difference-in-differences methods, such as that used by Lage de Sousa and Ottaviano (2018), an assumption likely stronger than those underlying the identification strategy employed here. Estimating equation (1) with different time lags in the outcome variable allows me to assess not only whether BNDES support affects firm performance, but also when this impact materializes. Table 36 reports the estimation results for the matched sample. The control group is Group D (the PSM-matched firms, 'Paired') for Table 36.

Table 36: Impact of BNDES Support on Sales over Time in a Matched Sample

Sample			Stayers			
Estimated in			Long-Differences			
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	1.3**	1.4**	1.5***	1.5**	1.3*	2.5**
	(.63)	(.59)	(.5)	(.61)	(.71)	(1.1)
Weak F-test (2^{nd} Stage)	3.350	13.20	14.30	14.50	15.41	8.881
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	.003	.049	.00043	-.01	.012	-.26*
	(.06)	(.074)	(.087)	(.096)	(.13)	(.15)
Obs.	2,576	2,327	2,098	1,880	1,667	1,466
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

All reported regression results are estimated in long differences with industry and mesoregion fixed effects that capture their respective linear trends, consistent with the preferred specification in the baseline results. Panel A presents the 2SLS estimates, while Panel B reports the OLS estimates. Regarding the parameter of interest, the evidence remains partly inconclusive. When restricting the sample to matched firms, the instrument comes close to failing to predict BNDES-subsidized credit take-up with sufficient statistical confidence, as indicated by the Kleibergen–Paap weak-IV F-statistic, which falls slightly below the conventional threshold. The 2SLS results suggest a positive association between BNDES-subsidized credit and cumulative sales growth that is statistically significant and larger than the OLS coefficients—an outcome that is difficult to interpret, likely due to instrument weakness arising from the smaller sample size combined with the high-dimensional fixed effects. The OLS results indicate that the effect of BNDES support is, at best, slightly above zero—similar to earlier findings—but declines over longer time horizons.

Overall, refining the treatment and control groups via propensity score matching results in

a sample in which the instrument becomes close to weak, likely due to the smaller sample size and the high-dimensional fixed effects in the structural equation. The OLS coefficients in this exercise suggest as well that BNDES-subsidized loans do not substantially enhance the relative performance of treated firms in the years following treatment. Since the more refined comparison between treated Group 1 and control Group D (paired) shows no strong differential effect of the treatment—neither in OLS nor in 2SLS with a comfortably strong instrument—I conclude that there is no strong evidence in my sample that BNDES support has a transformative impact on firms' cumulative sales growth.

B.6.6 Omitted Variables

Table 37: First Stage Analysis

Dep. Variable	Sub. Credit (0/1)					
Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
$depr_shock_{firt-2}$	-.25***	-.29***	-.29***	-.34***	-.3***	-.31***
	(.038)	(.042)	(.045)	(.043)	(.05)	(.061)
R-squared (1^{st} Stage)	.0019	.0025	.0024	.0029	.0028	.003
Weak F-test (2^{nd} Stage)	43.71	46.41	41.13	63.98	35.39	25.62
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Obs.	162,114	142,815	124,961	108,061	92,896	78,863
# (Control) Lags	3	3	3	3	3	3
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 38: Impact of BNDES Support on Sales

Sample	Stayers					
Estimated in	Long-Differences					
Outcome Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Sub. Credit (0/1)	.038	.17	.14	.085	.13	.21
	(.27)	(.22)	(.26)	(.25)	(.3)	(.37)
Weak F-test (2^{nd} Stage)	43.71	46.41	41.13	63.98	35.39	25.62
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: OLS						
Sub. Credit (0/1)	.041*	.059**	.11***	.12***	.14***	.15***
	(.021)	(.028)	(.031)	(.037)	(.043)	(.041)
Obs.	162,114	142,815	124,961	108,061	92,896	78,863
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoreg × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.6.7 Outliers

Without Largest 0.01% Firms -

Table 39: Impact of BNDES Support on Sales

Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second Stage						
Sub. Credit (0/1)	.091	.33	.21	.097	.094	-.0077
	(.39)	(.34)	(.44)	(.41)	(.59)	(.68)
Weak F-test (2^{nd} Stage)	20.58	21.46	16.51	30.51	11.26	10.13
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: First Stage						
$depr_shock_{firt-2}$	-.18***	-.21***	-.2***	-.25***	-.2***	-.22***
	(.04)	(.045)	(.048)	(.045)	(.058)	(.07)
R-squared (1^{st} Stage)	.0014	.0022	.0017	.0024	.0022	.0026
Obs.	162,177	142,880	125,026	108,127	92,956	78,922
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Without Largest 0.1% Firms -

Table 40: Impact of BNDES Support on Sales

Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second Stage						
Sub. Credit (0/1)	-.21	.081	-.36	-.54	-.72	-1.1
	(.58)	(.53)	(.66)	(.66)	(.77)	(1.1)
Weak F-test (2^{nd} Stage)	38.35	33.33	27.16	28.39	20.87	16.92
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: First Stage						
$depr_shock_{firt-2}$	-.13***	-.13***	-.13***	-.15***	-.14***	-.14***
	(.02)	(.022)	(.025)	(.028)	(.031)	(.033)
R-squared (1^{st} Stage)	.00099	.0011	.00089	.0011	.0012	.0013
Obs.	161,979	142,704	124,875	108,001	92,850	78,835
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Without Largest 0.5% Firms -

Table 41: Impact of BNDES Support on Sales

Sample	Stayers					
Estimated in	Long-Differences					
2^{nd} Stage Timing	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second Stage						
Sub. Credit (0/1)	.23	-.23	-.49	-.94	-1.3	-1.3
	(.79)	(.94)	(1.1)	(1.1)	(1.4)	(1.6)
Weak F-test (2^{nd} Stage)	29.84	31.14	28.46	27.32	17.24	17.19
(CV 10%)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Panel B: First Stage						
$depr_shock_{firt-2}$	-.071***	-.074***	-.077***	-.081***	-.072***	-.077***
	(.013)	(.013)	(.014)	(.015)	(.017)	(.019)
R-squared (1^{st} Stage)	.00047	.00055	.00038	.00047	.00039	.00047
Obs.	161,116	141,935	124,191	107,401	92,337	78,400
# (Control) Lags	3	3	3	3	3	3
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry Trends FE	Yes	Yes	Yes	Yes	Yes	Yes
Mesoregion Trends FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses.
Data: Brazil (2002-2019). All observations are at the firm-year-level.
All regressions are estimated using firm sales as analytical weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.7 Decomposition of EES Level

The following section undertakes a few auxiliary algebraic transformations of the $\Delta log(L_{it})$ term. To start, notice that log changes can be approximated with %-changes when changes are not too large. In the case of changes in industry-employment, this seems reasonable:

$$\Delta log(L_{it}) = \frac{L_{it} - L_{it-1}}{L_{it-1}}. \tag{62}$$

Using the distributive law, I first separate the nominator and then extend the first term with $-L_{irt}$ & $+L_{irt}$:

$$\Delta log(L_{it}) = \frac{L_{it} - L_{irt} + L_{irt}}{L_{it-1}} - \frac{L_{it-1}}{L_{it-1}}. \tag{63}$$

Parts of the first term on the RHS can be summed up as $L_{it} - L_{irt} = L_{i-rt}$, that is employment in industry i net of region r. Moreover, the second term can be summed up as: $\frac{L_{it-1}}{L_{it-1}} = 1$:

$$\Delta log(L_{it}) = \frac{L_{i-rt} + L_{irt}}{L_{it-1}} - 1. \tag{64}$$

Still focusing on the first term on the RHS, one can extend it with the lagged terms $-L_{i-rt-1}$ & $+L_{i-rt-1}$:

$$\Delta log(L_{it}) = \frac{L_{i-rt} - L_{i-rt-1} + L_{i-rt-1} + L_{irt}}{L_{it-1}} - 1. \tag{65}$$

Continuing with rewriting the first term, I split up the sum and I multiply parts of it by $\frac{L_{i-rt-1}}{L_{i-rt-1}}$:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{i-rt-1}} \times \frac{L_{i-rt} - L_{i-rt-1}}{L_{it-1}} + \frac{L_{i-rt-1} + L_{irt}}{L_{it-1}} - 1. \tag{66}$$

This expression can be rearranged as:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \frac{L_{i-rt} - L_{i-rt-1}}{L_{i-rt-1}} + \frac{L_{i-rt-1} + L_{irt}}{L_{it-1}} - 1, \tag{67}$$

and expressed as:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{i-rt-1} + L_{irt}}{L_{it-1}} - 1, \tag{68}$$

Having obtained the log-changes expression of L_{i-rt}, that is employment in industry i net of region r, I turn to the second term. Following the same algebraic strategy as for the first term, I augment it by $-L_{irt}$ & $+L_{irt}$:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt} - L_{irt-1} + L_{irt-1} + L_{i-rt-1}}{L_{it-1}} - 1, \tag{69}$$

I split up the term in question and multiply parts of it by $\frac{L_{irt-1}}{L_{irt-1}}$:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{irt-1}} \times \frac{L_{irt} - L_{irt-1}}{L_{it-1}} + \frac{L_{irt-1} + L_{i-rt-1}}{L_{it-1}} - 1, \tag{70}$$

such that I can rearrange the product as $\frac{L_{irt-1}}{L_{it-1}} \times \frac{L_{irt} - L_{irt-1}}{L_{irt-1}}$ and write it as:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{it-1}} \times \Delta log(L_{irt}) + \frac{L_{irt-1} + L_{i-rt-1}}{L_{it-1}} - 1, \tag{71}$$

The third term can be simplified too. Noticing that the sum of employment of industry i in region r at time $t-1$ (L_{irt-1}) and employment of industry i net of region r at time $t-1$ (L_{i-rt-1}) equals simply the employment of industry i at time $t-1$ (L_{it-1}), one can write:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{it-1}} \times \Delta log(L_{irt}) + \frac{L_{it-1}}{L_{it-1}} - 1, \tag{72}$$

and:

$$\Delta log(L_{it}) = \frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{it-1}} \times \Delta log(L_{irt}). \tag{73}$$

Finally, one can replace $\Delta log(L_{it})$ in (25) with the expression in (73) to obtain (26):

$$\Delta e_{it} = \alpha \left(\frac{L_{i-rt-1}}{L_{it-1}} \times \Delta log(L_{i-rt}) + \frac{L_{irt-1}}{L_{it-1}} \times \Delta log(L_{irt}) \right) + \beta \Delta log(\sum_{r \in R} s_{irt} L_{-irt}).$$

C Theory and Quantification

C.1 Housing Market

In the following, I derive the conditions characterizing the housing market in a given region r, keeping the subscript r for consistency with the rest of the paper.

Housing per Worker - First, note that the first-order conditions of the household's utility maximization problem with Cobb–Douglas preferences imply that a constant share—here $1 - \vartheta$—is spent on housing ($h_r^S(L_r^H) = (1 - \vartheta) w_r(L_r)$). This expression can be solved for housing per worker as:

$$h_r^S(L_r^H) = \frac{(1 - \vartheta) w_r(L_r)}{P_r^H} \tag{74}$$

Second, as there are L_r identical workers in the city, total housing consumption is

$$H(L_r) = L_r \; h(L_r). \tag{75}$$

Third, multiplying the individual housing demand condition (74) by L_r yields aggregate housing expenditure:

$$P_r^H \; H(L_r) = (1 - \eta) \; w(L_r) \; L. \tag{76}$$

Fourth, housing is produced competitively by atomistic landowners using land and labor. The Cobb-Douglas form of the housing production function implies that labor's revenue share is $(1 - b)$. Hence, total labor payments in housing satisfy

$$w(L_r) \, L_r^H(L_r) = (1 - b) \, P_r^H \, H(L_r), \tag{77}$$

Fifth, substituting aggregate housing expenditure in (77) with (76) gives:

$$w(L_r) \, L_r^H(L_r) = (1 - b)(1 - \eta) \, w(L_r) \, L. \tag{78}$$

Sixth, cancelling out the wages allows to write:

$$L_r^H(L_r) = (1 - b)(1 - \eta) \, L. \tag{79}$$

Seventh, each city has a fixed land endowment normalized to one, with atomistic landowners each owning a land share γ. Aggregating housing production in equation (29) across landowners yields total housing production:

$$H_r(L_r) = \left(\frac{L_r^H(L_r)}{1-b} \right)^{1-b}. \tag{80}$$

Eighth, one can substitute $L_r^H(L_r)$ with the expression in (79):

$$H(L_r) = \left(\frac{(1-b)(1-\eta)L}{1-b} \right)^{1-b} = \big((1-\eta)L\big)^{1-b}. \tag{81}$$

Ninth, one can finally divide total housing by the regional population L_r to obtain housing per worker:

$$h(L_r) = \frac{H(L_r)}{L} = \frac{\big((1-\eta)L\big)^{1-b}}{L} = (1-\eta)^{1-b}\, L^{-b}. \tag{82}$$

Wage Compensation for Congestion Costs - First, recall the worker's problem where workers in a given region choose consumption C and housing H to maximize utility:

$$\begin{aligned} U &= \left(\frac{c}{\vartheta} \right)^{\vartheta} \left(\frac{h}{1-\vartheta} \right)^{1-\vartheta}, \\ s.t. \quad & P^C c + P_r^H h = w_r(L_r) \end{aligned} \tag{83}$$

Second, taking the respective first-order conditions with respect to c and h, dividing one by the other, and substituting in the budget constraint yields the well-known result under Cobb–Douglas preferences that optimal demands for the respective goods are constant shares of income, in this case:

$$c = \vartheta \frac{w_r(L_r)}{P^C}, \qquad h_r(L_r) = (1-\vartheta)\frac{w(L_r)}{P_r^H}. \tag{84}$$

Third, one can substitute back the optimal demands obtained in (84) into the utility function which yields the indirect utility:

$$U(L_r) = \left(\frac{w_r(L_r)}{P} \right)^{\vartheta} \left(\frac{w_r(L_r)}{P_r^H} \right)^{1-\vartheta} = \frac{w_r(L_r)}{P^{\vartheta}[P_r^H]^{1-\vartheta}}. \tag{85}$$

Fourth, solving the optimal demand for housing in (84) for P_r^H and replacing $H_r(L_r)$ with

its equilibrium value in (30) gives:

$$P_r^H = (1-\vartheta)\frac{w_r(L_r)}{(1-\vartheta)^{1-b}L^{-b}} = w_r(L_r)(1-\vartheta)^b L_r^b. \tag{86}$$

Fifth, indirect utility needs to be equalized across regions since workers are freely mobile:

$$\bar{U} = \frac{w_r(L_r)}{P^{\vartheta}[P_r^H]^{1-\vartheta}} \tag{87}$$

Sixth, one can solve (87) for $w_r(L_r)$:

$$w_r(L_r) = \bar{U}\, P^{\vartheta}[P_r^H]^{1-\vartheta}. \tag{88}$$

Seventh, one can plug in the expression for P_r^H obtained in (86)

$$w_r(L_r) = \bar{U}\, P^{\vartheta}\Big(w_r(L_r)(1-\vartheta)^b L^b\Big)^{1-\vartheta}. \tag{89}$$

Eighth, one can rearrange the expression in (89) via dissolving the expression in the bracket

$$w_r(L_r) = \bar{U}\, P^{\vartheta} w_r(L_r)^{1-\vartheta}(1-\vartheta)^{b(1-\vartheta)} L^{b(1-\vartheta)}. \tag{90}$$

Ninth, one can divide both sides of (90) by $w_r(L_r)^{1-\vartheta}$,

$$w_r(L_r)^{\vartheta} = \bar{U}\, P^{\vartheta}(1-\vartheta)^{b(1-\vartheta)} L^{b(1-\vartheta)}. \tag{91}$$

Tenth, take equation (91) to the power of $1/\vartheta$,

$$w_r(L_r) = \bar{U}^{1/\vartheta} P (1-\vartheta)^{\frac{b(1-\vartheta)}{\vartheta}} L^{\frac{b(1-\vartheta)}{\vartheta}}. \tag{92}$$

Eleventh, as in Gaubert (2018), define $\bar{w} \equiv \bar{U}^{1/\vartheta} P$ and plug it into equation (92) to obtain the equilibrium wage schedule that compensates workers' for the congestion costs:

$$w_r(L_r) = \bar{w}\,((1-\vartheta)L)^{\frac{b(1-\vartheta)}{\vartheta}},$$

Landowner Profits - First, recall the housing production function under which landowners combine land γ and local labor ℓ to produce housing h_S (29). Accordingly, one can write the landowners' profit-maximization problem, taking the local housing price P_r^H and wage $w(L_r)$ as given.

$$\max_{\ell \geq 0} \;\; \pi(\gamma, L_r) = P_r^H\, h_S(\ell) - w(L_r)\,\ell = P_r^H\, \gamma^b \ell^{1-b} - w(L_r)\,\ell. \tag{93}$$

Second, the first-order condition with respect to ℓ characterizes the wage rate up to which

landowners are willing to hire an additional unit of labor:

$$\frac{\partial \pi(\gamma; L_r)}{\partial \ell} = P_r^H \, \gamma^b (1-b) \ell^{-b} - w(L_r) = 0 \tag{94}$$

$$\iff \quad w(L_r) = (1-b) \, P_r^H \, \gamma^b \ell^{-b}. \tag{95}$$

Third, solving for ℓ yields the landowner's labor demand:

$$\ell(L_r) = \left(\frac{(1-b) \, P_r^H}{w(L_r)} \right)^{1/b} \gamma. \tag{96}$$

Importantly, labor hired is proportional to owned land γ. Multiplying equation (95) by ℓ and substituting the housing production function (29) in implies that labor costs constitute a constant share of revenue, equal to $(1-b)$:

$$w(L_r) \, \ell = (1-b) \, P_r^H \, \gamma^b \ell^{1-b} = (1-b) \, P_r^H \, h_S(\gamma, \ell). \tag{97}$$

Fifth, for notational convenience, define housing revenue for a landowner as $R_H(L_r) \equiv P_r^H \, h_S(\ell)$. Then the identity is

$$w(L_r) \, \ell = (1-b) \, R_H(L_r). \tag{98}$$

Sixth, using equation (98) and the definition of housing revenue for a landowner, it follows that landowner profits are a constant share of revenue:

$$\pi(\gamma, L_r) = R_H(L_r) - w(L_r) \, \ell \tag{99}$$

$$= R_H(L_r) - (1-b) \, R_H(L_r) = b \, R_H(L_r) \tag{100}$$

$$= b \, P_r^H \, h_S(\ell). \tag{101}$$

Seventh, due to workers' Cobb–Douglas preferences with housing expenditure share $(1-\vartheta)$ in equation (83), each worker spends a fraction $(1-\vartheta)$ of wage income on housing, i.e. $P_r^H \, h(L_r) = (1-\vartheta) \, w(L_r)$. Consequently, aggregate housing expenditure—and thus housing revenue—is given by:

$$P_r^H \, H(L_r) = (1-\vartheta) \, w(L_r) \, L_r. \tag{102}$$

Eighth, since land is normalized to one in a given region ($\int \gamma \, dG(\gamma) = 1$), aggregate landowner profits are obtained by integrating individual landowner profits in equation (101)

and substituting aggregate housing revenue from equation (102):

$$\begin{aligned}\pi_H(L_r) &\equiv \int \pi(\gamma, L_r)\, dG(\gamma) \\ &= b \int P_r^H\, h_S(\gamma, \ell)\, dG(\gamma) \\ &= b\, P_r^H\, H(L_r) \\ &= b\,(1-\vartheta)\, w(L_r)\, L_r.\end{aligned} \tag{103}$$

Labor in Housing Sector - First, recall the individual landowner's labor demand in equation (96), derived from the landowner's profit-maximization problem (93) via the first-order condition with respect to housing-sector labor (95). Given that total land in a region is normalized to one, $\int \gamma\, dG(\gamma) = 1$, one can integrate equation (96) across landowners to obtain total labor demand in the housing sector in a given region:

$$\ell_H(L_r) = \left(\frac{(1-b)\, P_r^H}{w(L_r)}\right)^{1/b}. \tag{104}$$

Second, combining equation (98) with the definition of a landowner's housing revenue, $R_H(L_r) \equiv P_r^H\, h_S(\ell)$, and aggregating over landowners' housing supply, $H(L_r) \equiv \sum h_S(\ell)$, aggregate labor payments in the housing sector are given by

$$w(L_r)\, \ell_H(L_r) = (1-b)\, P_r^H\, H(L_r). \tag{105}$$

Third, substituting aggregate housing revenue from equation (102) into equation (105) and subsequently dividing both sides by $w_r(L_r)$ shows that labor employed in the housing sector is a constant share of the overall regional labor force:

$$\begin{aligned} w(L_r)\, \ell_H(L_r) &= (1-b)\,(1-\eta)\, w(L_r)\, L_r \\ \implies \quad \ell_H(L_r) &= (1-b)(1-\eta)\, L_r. \end{aligned} \tag{106}$$

C.2 Sectors

Optimal Demands - The firm's optimal demand Q_{fir} is obtained by solving the following revenue maximization problem of a given firm f, where Q_i in the value function is replaced

by $\left[\sum_i^I Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}}$ and the associated first-order condition is solved for Q_{fir}:

$$\begin{aligned} &\underset{Q_{fir}}{max} \quad P_i Q_i \\ &s.t. \sum_f^{F_i} P_{fir} Q_{fir} \\ &Q_i = \left[\sum_i^I Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}} \end{aligned} \tag{107}$$

At the sector level, optimal demand Q_i is obtained by solving the profit maximization problem of the competitive firm producing the final good for a given industry i, where C in the value function is replaced by $\left(\int_0^1 Q_i^{\frac{\eta-1}{\eta}} di\right)^{\frac{\eta}{\eta-1}}$, and the associated first-order condition for industry i is solved for Q_i:

$$\begin{aligned} &\underset{Q_i}{max} \quad P^C C \\ &s.t. \sum_{i \in I} P_i Q_i \\ &C = \left(\int_0^1 Q_i^{\frac{\eta-1}{\eta}} di\right)^{\frac{\eta}{\eta-1}} \end{aligned} \tag{108}$$

C.3 Firms

Firms Inverse Demand Constraints - The inverse demand constraint faced by firms in the profit-maximization problem (39) can be derived in two steps. First, the optimal sector- and firm-level demand functions in (37) and (36) are solved for P_i and P_{fir} respectively:

$$\begin{aligned} P_i &= \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}} P^C \\ P_{fir} &= \left(\frac{Q_{fir}}{Q_i}\right)^{-\frac{1}{\zeta}} P_i \end{aligned} \tag{109}$$

Second, substitute the expression for P_i obtained in Step 1 into the inverse firm-level demand function P_{fir} and solve for $\frac{P_{fir}}{P^C}$. Together, these steps yield equation (40).

Market Share Accounting Identity - The convenient accounting identity for market share as a function of quantities given in equation (41) can be derived in four steps. First,

recall the constraint (40) in the firm's profit maximization problem:

$$\frac{P_{fir}}{P^C} = \left(\frac{Q_{fir}}{Q_i}\right)^{-\frac{1}{\zeta}} \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}}. \tag{110}$$

Second, multiply the constraint (40) by P^C and then by Q_{fir}:

$$P_{fir}Q_{fir} = P^C Q_{fir}^{\frac{\zeta-1}{\zeta}} Q_i^{\frac{1}{\zeta}} \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}}. \tag{111}$$

Third, sum the relationship in (111) over the firms F_i within an industry i such that:

$$\sum_f^F P_{fir}Q_{fir} = P^C Q_i^{\frac{1}{\zeta}} \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}} \sum_f^F Q_{fir}^{\frac{\zeta-1}{\zeta}}. \tag{112}$$

Fourth, divide (111) by (112) to obtain (41):

$$m_f \equiv \frac{P_{fir}Q_{fir}}{\sum_f^F P_{fir}Q_{fir}} = \frac{Q_{fir}^{\frac{\zeta-1}{\zeta}}}{\sum_f^F Q_{fir}^{\frac{\zeta-1}{\zeta}}}. \tag{113}$$

Another convenient accounting identity is the market share as a function of prices. First, recall the inverse demand constraint faced by firms (109) and solve it for Q_{fir}:

$$Q_{fir} = Q_i P_i^{\zeta} P_{fir}^{-\zeta} \tag{114}$$

Second, multiply (114) by P_{fir} to derive nominal expenditure on firm f's variety:

$$P_{fir}Q_{fir} = Q_i P_i^{\zeta} P_{fir}^{1-\zeta} \tag{115}$$

Third, sum over all firms within an industry i in (115) to obtain total industry expenditure:

$$\sum_f^{F_i} P_{fir}Q_{fir} = \sum_f^{F_i} Q_i P_i^{\zeta} P_{fir}^{1-\zeta} = Q_i P_i^{\zeta} \sum_f^{F_i} P_{fir}^{1-\zeta} \tag{116}$$

Fourth, recall the market share definition:

$$m_f = \frac{P_{fir}Q_{fir}}{\sum_f^{F_i} P_{fir}Q_{fir}} \tag{117}$$

into which one substitutes (115) for expenditure on firm f's variety in the numerator and

(116) for total industry expenditure in the denominator:

$$m_f = \frac{Q_i P_i^{\zeta} P_{fir}^{1-\zeta}}{Q_i P_i^{\zeta} \sum_f^{F_i} P_{fir}^{1-\zeta}} \tag{118}$$

Fifth and finally, notice that the term $Q_i P_i^{\zeta}$ cancels out and one obtains the definition of the market share in terms of prices:

$$m_f = \frac{P_{fir}^{1-\zeta}}{\sum_f^{F_i} P_{fir}^{1-\zeta}} \tag{119}$$

Optimal Price - Deriving the optimal price faced by a firm requires combining information from the firm's profit-maximization problem (39), its inverse demand constraint (40), and the market share accounting identity (113). First, the inverse demand constraint (40) is solved for P_{fir}, yielding:

$$P_{fir} = P^C \left(\frac{Q_{fir}}{Q_i}\right)^{-\frac{1}{\zeta}} \left(\frac{Q_i}{C}\right)^{-\frac{1}{\eta}}. \tag{120}$$

Second, using equation (120) to substitute for P_{fir} in the firm's profit-maximization problem (39) yields:

$$P^C C^{\frac{1}{\eta}} Q_{fir}^{1-\frac{1}{\zeta}} {Q_i}^{\frac{1}{\zeta}-\frac{1}{\eta}} - Q_{fir} MC_f \tag{121}$$

where MC_f denotes the marginal cost of firm f. Third, one can take the first order conditions with respect to Q_{fir} using the product and the chain rule to recover:

$$\frac{\partial \Pi}{\partial Q_{fir}} = 0 \Rightarrow P^C C^{\frac{1}{\eta}} \left[\frac{\zeta - 1}{\zeta} Q_{fir}^{-\frac{1}{\zeta}} {Q_i}^{\frac{1}{\zeta}-\frac{1}{\eta}} + Q_{fir}^{1-\frac{1}{\zeta}} \frac{\partial Q_i}{\partial Q_{fir}} \left(\frac{1}{\zeta} - \frac{1}{\eta}\right) Q_i^{\frac{1}{\zeta}-\frac{1}{\eta}-1}\right] = MC_f. \tag{122}$$

Fourth, recall that $Q_i = \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}}$ from equation (32). It follows that the partial derivative $\frac{\partial Q_i}{\partial Q_{fir}}$ for a given firm f is given by:

$$\begin{aligned} \frac{\partial Q_i}{\partial Q_{fir}} &= \frac{\zeta - 1}{\zeta} Q_{fir}^{-\frac{1}{\zeta}} \frac{\zeta}{\zeta - 1} \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{-\frac{1}{\zeta-1}} \\ &= Q_{fir}^{-\frac{1}{\zeta}} \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{-\frac{1}{\zeta-1}} \\ &= Q_{fir}^{-\frac{1}{\zeta}} Q_i^{-\zeta}. \end{aligned} \tag{123}$$

Fifth, multiplying the right-hand side of equation (123) by $\frac{Q_{fir}}{Q_{fir}}$ allows the expression to

be rearranged as:

$$\frac{\partial Q_i}{\partial Q_{fir}} = Q_{fir}^{\frac{\zeta-1}{\zeta}} Q_i^{-\zeta} Q_{fir}^{-1}. \tag{124}$$

Sixth, note that the term $\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}$ can be written as the product of $\left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}}$ and $\left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{-\frac{1}{\zeta-1}}$, by standard exponent rules:

$$\begin{aligned}
\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}} &= \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}} \left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{-\frac{1}{\zeta-1}} \\
&= \frac{\left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{\zeta}{\zeta-1}}}{\left[\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}\right]^{\frac{1}{\zeta-1}}} \\
&= \frac{Q_i}{Q_i^{\zeta}}
\end{aligned} \tag{125}$$

Seventh, using equation (125), it is straightforward to show that $Q_i^{-\zeta} = \frac{Q_i}{\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}}$. Substituting this expression into equation (124) yields a formulation that implicitly incorporates market share:

$$\frac{\partial Q_i}{\partial Q_{fir}} = Q_i \left(\frac{Q_{fir}^{\frac{\zeta-1}{\zeta}}}{\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}} \right) Q_{fir}^{-1} \tag{126}$$

Eighth, using (126) to replace $\frac{\partial Q_i}{\partial Q_{fir}}$ and (120) to replace $P^C C^{\frac{1}{\eta}} Q_{fir}^{-\frac{1}{\zeta}} {Q_i}^{\frac{1}{\zeta}-\frac{1}{\eta}}$, one can simplify (122):

$$\frac{\partial \Pi}{\partial Q_{fir}} = 0 \Rightarrow \frac{\zeta-1}{\zeta} P_{fir} + \left(\frac{Q_{fir}^{\frac{\zeta-1}{\zeta}}}{\sum_f^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}}} \right) \left(\frac{1}{\zeta} - \frac{1}{\eta} \right) P_{fir} = MC_f. \tag{127}$$

Ninth, one can substitute the implicit expression of the market share with (113) such that:

$$\frac{\zeta-1}{\zeta} P_{fir} + \left(\frac{1}{\zeta} - \frac{1}{\eta} \right) m_f P_{fir} = MC_f. \tag{128}$$

Tenth, one can solve the previous equation (128) for P_{fir}:

$$P_{fir} = \left[\frac{\zeta-1}{\zeta} + \left(\frac{1}{\zeta} - \frac{1}{\eta} \right) m_f \right]^{-1} MC_f. \tag{129}$$

Eleventh, the expression in brackets can be simplified by multiplying the numerator and

denominator by ζ and η, respectively:

$$P_{fir} = \left[\frac{\eta(\zeta - 1) + m_f(\eta - \zeta)}{\zeta\eta}\right]^{-1} MC_f. \tag{130}$$

Twelfth, the expression in brackets can be further simplified by rearranging terms:

$$P_{fir} = \left[\frac{\eta\zeta - ([1 - m_f]\eta + \zeta m_f)}{\zeta\eta}\right]^{-1} MC_f. \tag{131}$$

Thirteenth, recall that the demand elasticity ϵ is a function of the firm's market share m_f:

$$\epsilon(m_f) = \left[\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta}m_f\right]^{-1} = \left[\frac{(1 - m_f)\eta + \zeta m_f}{\zeta\eta}\right]^{-1} \tag{132}$$

Fourteenth, the expression in brackets in equation (131) can be extended by multiplying both the numerator and denominator by $([1 - m_f]\eta + \zeta m_f)$:

$$P_{fir} = \left[\frac{\eta\zeta([1 - m_f]\eta + \zeta m_f)}{\zeta\eta([1 - m_f]\eta + \zeta m_f)} - \frac{([1 - m_f]\eta + \zeta m_f)^2}{\zeta\eta([1 - m_f]\eta + \zeta m_f)}\right]^{-1} MC_f. \tag{133}$$

Fifteenth, several terms in the expression in brackets cancel out, and substituting the inverse of the expression in (132) for $\epsilon(m_f)$ yields, finally:

$$\begin{aligned} P_{fir} &= \left[1 - \frac{([1 - m_f]\eta + \zeta m_f)}{\zeta\eta}\right]^{-1} MC_f \\ &= \left[1 - \left(\frac{\zeta\eta}{([1 - m_f]\eta + \zeta m_f)}\right)^{-1}\right]^{-1} MC_f \\ &= \left[1 - \frac{1}{\epsilon(m_f)}\right]^{-1} MC_f \\ &= \left[\frac{\epsilon(m_f) - 1}{\epsilon(m_f)}\right]^{-1} MC_f \\ P_{fir} &= \frac{\epsilon(m_f)}{\epsilon(m_f) - 1} MC_f. \end{aligned} \tag{134}$$

Effective Demand Elasticity - Deriving the effective demand elasticity faced by a firm relies on the intermediate condition in the derivation of the optimal price, given in equation (129), which combines information from the firm's profit-maximization problem (39), the inverse demand constraint (40), and the market share accounting identity (113). First, recall equation (129), which displays a relationship between the price, the substitution

elasticities ζ and η, and marginal costs:

$$P_{fir} = \left[\frac{\zeta - 1}{\zeta} + \left(\frac{1}{\zeta} - \frac{1}{\eta}\right) m_f\right]^{-1} MC_f. \tag{135}$$

Second, divide equation (135) by MC_f and expand the bracketed terms:

$$\frac{P_{fir}}{MC_f} = \left[1 - \frac{1}{\zeta} + \frac{1}{\zeta} m_f - \frac{1}{\eta} m_f\right]^{-1}. \tag{136}$$

Third, factor out $\frac{1}{\zeta}$ to simplify the right-hand side:

$$\frac{P_{fir}}{MC_f} = \left[1 + \frac{1}{\zeta}(-1 + m_f) - \frac{1}{\eta} m_f\right]^{-1}. \tag{137}$$

Fourth, collect all terms on the right-hand side in (137) into a single bracket and factor out the minus sign for notational convenience and ease of interpretation, in line with Atkeson and Burstein (2008):

$$\frac{P_{fir}}{MC_f} = \left[1 - \left(\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta} m_f\right)\right]^{-1}. \tag{138}$$

Fifth, recall that the expression on the left-hand side in (138) corresponds to the markup, defined as $\mu \equiv \frac{P_{fir}}{MC_f}$:

$$\mu_f = \left[1 - \left(\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta} m_f\right)\right]^{-1}. \tag{139}$$

Sixth, to facilitate comparison with the literature—particularly on monopolistic competition—define the markup as a function of the effective demand elasticity $\mu \equiv \frac{\epsilon}{\epsilon - 1}$ and substitute it for the left-hand side of equation (139):

$$\frac{\epsilon}{\epsilon - 1} = \left[1 - \left(\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta} m_f\right)\right]^{-1}. \tag{140}$$

Seventh, take the inverse of equation (140):

$$\frac{\epsilon - 1}{\epsilon} = 1 - \left[\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta} m_f\right]. \tag{141}$$

Eighth, using the identity $\frac{\epsilon - 1}{\epsilon} = 1 - \frac{1}{\epsilon}$ allows the constant terms on both sides of equation (141) to cancel:

$$-\frac{1}{\epsilon} = -\left[\frac{1}{\zeta}(1 - m_f) + \frac{1}{\eta} m_f\right]. \tag{142}$$

Ninth, multiplying both sides by -1 and taking both sides to the power -1 yields the

final expression, with ϵ expressed as a function of m_f:

$$\epsilon(m_f) = \left[\frac{1}{\zeta}(1-m_f) + \frac{1}{\eta}m_f\right]^{-1}. = \left[\frac{(1-m_f)\eta + \zeta m_f}{\zeta\eta}\right]^{-1}. \tag{143}$$

The effective demand elasticity $\epsilon(m_f)$ represents a harmonic mean of the within-industry elasticity ζ and the across-industry elasticity η, with weights given by the firm's market share. As $m_f \to 0$, $\epsilon(m_f) \to \zeta$, corresponding to a small firm that does not internalize its impact on the industry price index. As $m_f \to 1$, $\epsilon(m_f) \to \eta$, reflecting a dominant firm whose pricing affects aggregate industry demand.

C.4 Optimal Policy

C.5 Equilibrium

C.5.1 Market Clearing

Final, industry-level composite, firm-level good market clears:

$$\begin{aligned} C &= \left(\int_0^1 Q_i^{\frac{\eta-1}{\eta}} di\right) \\ \left(\frac{P_i}{P^c}\right)^{-\eta} C &= \left[\sum_{f_i}^{F_i} Q_{fir}^{\frac{\zeta-1}{\zeta}} df\right]^{\frac{\zeta}{\zeta-1}} \qquad \forall i \in I. \\ \left(\frac{P_{fir}}{P_i}\right)^{-\zeta} Q_i &= A_f E_{ir} L_f \qquad \forall f \in F. \end{aligned} \tag{144}$$

Labor markets clear in each region:

$$L_r^h + L_r^{ind} = L_r \quad \forall r \in R. \tag{145}$$

Housing market clears in each region:

$$P^H(L_r)h(L_r) = (1-\vartheta)w_r(L_r) \tag{146}$$

C.5.2 Constructing the Equilibrium

The equilibrium is constructed in various consecutive steps. Each step builds on the preceding ones and on the equilibrium values of the respective variables pinned down in those steps. In short, the procedure described below allows one to construct a system of equations in which all equilibrium prices and quantities can be expressed as functions of L_r and exogenous fundamentals b and ϑ, the equilibrium constant $\bar{w}$, and elasticities ρ and η. As such, taking $\bar{w}$ as the numeraire and for an arbitrarily chosen geographic distribution

of economic activity $\{L_r\}_{r \in R}$, equilibrium values can be computed and compared to their empirical counterparts to assess the optimality of the policy in question.

First, by region r, solve for the quantity of housing within a region H_r^S as a function of regional labor L_r as in equation (81) in Appendix C.1

$$H(L_r) = \big((1-\eta)L_r\big)^{1-b}. \tag{147}$$

Second, by region r, solve for the regional wage w_r as a function of regional labor L_r as in equation (31):

$$w_r(L_r) = \bar{w}\left((1-\vartheta)L_r\right)^{\frac{b(1-\vartheta)}{\vartheta}}. \tag{148}$$

Third, by region $r \in R$, solve for the price of housing P_r^H as in equation (86) in Appendix C.1:

$$P_r^H = w_r(L_r)(1-\vartheta)^b L_r^b. \tag{149}$$

Fourth, for each industry–region pair, solve for the optimal level of externalities E_{ir}, defined as the level that maximizes productivity-enhancing externalities across the economy. Therefore, one needs to solve the following externalities maximization problem:

$$\begin{aligned} &\underset{L_i, L_r}{max} \quad L_i^{\alpha} L_r^{\beta} \\ &s.t. \sum_{L_i \in \bar{L}} L_i = \sum_{L_r \in \bar{L}} L_r = \bar{L} \end{aligned} \tag{150}$$

Next, derive the first-order conditions with respect to L_i (1) and L_r (2), and divide (1) by (2) to characterize the optimal choice of L_i as a function of L_r, α, and β. Under the Cobb–Douglas functional form, this relationship is well known:

$$L_i = \frac{\alpha}{\beta} L_r \tag{151}$$

Fifth, for each firm $f \in F$, characterize the optimal subsidy τ_{fir} by comparing marginal product conditions in a hypothetical decentralized economy and a centralized economy, and choosing τ_{fir} such that the two allocations coincide, following Bartelme, Costinot, et al. (2019). Accordingly, firms do not internalize that their labor choice L_f contributes to industry-level labor L_i and regional labor L_r, which leads them to perceive their marginal product as too low and, consequently, to hire too little labor. To derive the marginal product conditions, substitute the production function for Q_{fir} in the firm's profit maximization problem and take the first-order condition with respect to firm labor L_f. To induce firms to internalize the externality and hire more labor, the optimal subsidy in the decentralized economy must replicate the externality term present in the centralized

economy's marginal product condition:

$$\tau_{fir} = \frac{\alpha}{L_i} + \frac{\beta}{L_r} \tag{152}$$

Sixth, by firm $f \in F$, jointly determine the price the firm charges as in equation (42) and the market share. Note that the market share m_f can equivalently be written as a function of prices as in (119). Consequently, solving for the price P_{fir} boils down to finding a fixed point of the simultaneous system of equations (42) and (119): Importantly, following Atkeson and Burstein (2008), firms treat τ_{fir}, w_r, E_{ir}, and competitors' prices $P_{f' \neq f}$ as given, while taking their own productivity A_f as known:

$$\begin{aligned} P_{fir} &= \frac{\epsilon(m_f)}{\epsilon(m_f) - 1} \frac{(1-\tau) w_r}{A_f E_{ir}} \quad \text{where } \epsilon(m_f) \equiv \left[\frac{1}{\rho}(1 - m_f) + \frac{1}{\eta} m_f \right]^{-1} \\ m_f &= \frac{P_{fir} Q_{fir}}{\sum_f^F P_{fir} Q_{fir}} = \frac{P_{fir}^{1-\zeta}}{\sum_{f=1}^{F_i} P_{fir}^{1-\zeta}} \end{aligned} \tag{153}$$

Seventh, for each industry $i \in I$, the price P_i of the industry-level composite good is given by equation (33):

$$P_i = \left[\sum_{f_i}^{F_i} P_{fir}^{1-\sigma} \right]^{\frac{1}{1-\sigma}}. \tag{154}$$

Eighth, the price of the final good P^C is given by equation (35):

$$P^C = \left(\int_0^1 P_i^{1-\eta} di \right)^{\frac{1}{1-\eta}} \tag{155}$$

Ninth, the quantity of the final good C as a function of L_r is obtained from the market-clearing condition that aggregate demand in the economy equals total spending on the final good by workers across regions, where the spending share follows from the first-order conditions of the workers' utility maximization problem, by summing over all regions:

$$P^C C = \sum_r^R \vartheta w_r \ (L_r) \quad \Rightarrow C = \frac{\vartheta}{P^C} w_r(L_r) \ L_r \tag{156}$$

Tenth, for each industry $i \in I$, the quantity of the composite industrial good Q_i is obtained by solving equation (109) in Appendix C.3 for Q_i:

$$Q_i = \left(\frac{P_i}{P^C} \right)^{-\eta} C \tag{157}$$

Eleventh, by firm $f \in F$, solve for the quantity the firm Q_{fir} produces based on equation

(109) in Appendix C.3:

$$Q_{fir} = \left(\frac{P_{fir}}{P_i}\right)^{\zeta} Q_i \tag{158}$$

Twelfth, for each firm $f \in F$, solve for the firm's labor demand. Since labor is the sole input in production, total variable cost equals the wage bill $w_r(L_r)\, L_f$. Equivalently, it can be expressed as marginal cost MC_f times output:

$$w_r(L_r) \times L_f = MC_f \times Q_{fir} \tag{159}$$

Next, operating profits are:

$$\pi_f = (P_{fir} - MC_f)Q_{fir} \tag{160}$$

where I substitute the pricing rule $P_{fir} \equiv \mu\, MC_f$, i.e., the firm-level price is a variable markup μ_f over marginal cost MC_f:

$$\begin{aligned} \pi_f &= (\mu_f MC_f - MC_f)Q_{fir} \\ &= (\mu_f - 1)MC_f Q_{fir} \end{aligned} \tag{161}$$

Subsequently, recall that the markup satisfies $\mu_f = \frac{\epsilon(m_f)}{\epsilon(m_f)-1}$, and substitute this expression into (161):

$$\begin{aligned} \pi_f &= (\frac{\epsilon(m_f)}{\epsilon(m_f) - 1} - 1)MC_f Q_{fir} \\ &= \frac{1}{\epsilon(m_f) - 1} MC_f Q_{fir} \end{aligned} \tag{162}$$

This relationship implies that variable costs can be expressed as a function of firm profits.

$$MC_f Q_{fir} = \Big(\epsilon(m_f) - 1\Big)\pi_f \tag{163}$$

Consequently, combining (159) with (163) yields:

$$w_r(L_r) \times L_f = \Big(\epsilon(m_f) - 1\Big)\pi_f \tag{164}$$

Finally, solve (164) for L_f and substitute the profit expression $\pi_f = P_{fir}Q_{fir} - w_r(L_r)L_f$ to characterize the equilibrium labor demand at the firm level:

$$\begin{aligned} L_f &= \Big(\epsilon(m_f) - 1\Big)\frac{\pi_f}{w_r(L_r)} \\ &= \frac{\epsilon(m_f) - 1}{\epsilon(m_f)} \frac{P_{fir}Q_{fir}}{w_r(L_r)} \end{aligned} \tag{165}$$

C.6 Counterfactual

The expression in (49) can be derived in several steps. First, recall the expression for the optimal price, which consists of the markup, $\mu_{fir} = \frac{\epsilon(m_{fir})}{\epsilon(m_{fir})-1}$, and marginal cost:

$$P_{fir} = \mu_{fir} \frac{(1-\tau_{fir})w_r}{A_{fir}E_{ir}} \tag{166}$$

Multiplying (166) by $A_{fir}E_{ir}$ and extending the left-hand side by $\frac{L_{fir}}{L_{fir}}$ yields:

$$P_{fir}A_{fir}E_{ir}\frac{L_{fir}}{L_{fir}} = \mu_{fir}(1-\tau_{fir})w_r \tag{167}$$

Recall the production function $Q_{fir} = A_{fir}E_{ir}L_{fir}$ and substitute for Q_{fir} accordingly, yielding:

$$\frac{P_{fir}Q_{fir}}{L_{fir}} = \mu_{fir}(1-\tau_{fir})w_r \tag{168}$$

and solve for L_{fir}:

$$L_{fir} = \frac{P_{fir}Q_{fir}}{\mu_{fir}w_r(1-\tau_{fir})} \tag{169}$$

Taking the partial derivative with respect to τ_{fir} in (169) using the chain rule yields the expression in (49):

$$L_{fir} = \frac{P_{fir}Q_{fir}}{\mu_{fir}w_r}(1-\tau_{fir})^{-2} \tag{170}$$