Do Firms Need Cheaper Credit to Grow? *

Daniel Grimaldi †

Jose Renato H. Ornelas ‡

Abstract

This paper explores a unique event that abruptly and unexpectedly increased the subsidy levels associated with a traditional earmarked credit line in Brazil. Using a local difference-in-differences approach, we find strikingly different results depending on firms' size. For mid-large firms, despite an increase in subsidy intake of almost 90%, there were no relevant effects on employment or debt, suggesting they mostly used new loans to replace older (more expensive) debt. For smaller firms, we observed a similar increase in the dosage of subsidies, but we also saw an increase in earmarked debt (roughly 75%) and employment (around 6% in the number of employees and 10% in the payroll). However, all labor-related effects were short-lived and vanished after two years. A cost-effectiveness analysis for a two-year window shows that monthly credit subsidies were higher than the increase in payroll at the affected firm.

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†Schar School of Policy and Government - George Mason University. Corresponding author: dgrimald@gmu.edu

‡Banco Central do Brasil and FGV-EPPG

1 Introduction

Stiglitz and Weiss (1981) posit the credit sector is particularly susceptible to the challenges of asymmetric information. They argue the responses of banks to this inherent information imbalance systematically give rise to equilibrium conditions characterized by credit constraints. In extreme cases, this process of adverse selection may even precipitate a complete market shutdown (Akerlof (1970)). The theory posits credit constraints could exert significant impacts on the real economy, imperiling entrepreneurs' ability to advance investment projects that would otherwise yield societal benefits. Empirical evidence appears to support this proposition, as financially constrained firms have lower levels of investment (Choi et al. (2018), Baker et al. (2003)), sales (McKenzie (2017)), exports (Minetti and Zhu (2011), Manova et al. (2015)), profitability (McKenzie (2017)), employment (McKenzie (2017), Fonseca and Van Doornik (2022)), and productivity (Choi et al. (2018)).

To alleviate the adverse impacts of credit constraints, both developed and developing nations have embraced targeted credit initiatives. These policies typically employ a blend of subsidized interest rates and second-tier operations designed to enhance credit access. Within this framework, the public sector, often lacking the expertise to manage extensive credit technologies, allocates subsidized funds through commercial banks. These banks then assume the responsibility for disbursing loans to qualifying firms. Empirical evidence indicates the effectiveness of such policies in stimulating the economic activities of their beneficiaries, as highlighted in studies by Banerjee and Duflo (2014), Eslava et al. (2011), Eslava et al. (2012), Bach (2014), and Brown and Earle (2017).

Although there is ample evidence of the efficacy of credit policies, there is a notable lack of guidance on their quantitative parameters. For example, questions persist about the optimal extent of subsidies provided to firms. This paper seeks to fill this gap by estimating the causal impact of an escalation in the intensity of loan subsidies, specifically disentangling the effects on access to credit (extensive margin) from the effects on the size of subsidies (intensive margin). This paper explores a unique event that abruptly and unexpectedly heightened subsidy levels associated with the Brazilian Federal Government's Finame program. In mid-2009, the Brazilian government introduced the PSI [1], representing a substantial reduction in the final interest rates available for Finame's customers. Notably, some borrowers fortuitously secured lower interest rates through PSI, while others remained entangled in the older program with higher interest rates. Importantly, given the absence of other significant changes in Finame's distribution channels to its final beneficiaries, this scenario allows us to meticulously isolate the price effect from the access mechanism. While previous studies estimate the combined effects of access to credit and subsidy magnitude, our approach uniquely isolates and scrutinizes the impact of subsidy magnitude itself.

We employ a difference-in-differences methodology to analyze the impact of the new program (PSI) by comparing its effects on borrowers from the new program (PSI) with those of the old

[1] PSI is a Portuguese acronym for *Programa de Sustentação do Investimento*, which roughly translates into Program for Supporting Investments.

program. Our findings reveal marked disparities between mid-large and small-micro enterprises. Despite the infusion of subsidies, mid-large firms exhibited no discernible effects on employment or debt, suggesting they merely substituted old debt with cheaper alternatives. In contrast, micro-small firms showed an increase in earmarked debt and employment metrics, with a notable 6% increase in employment and approximately a 10% increase in payroll. However, these effects are short-lived and vanish after two years.

In addition, we conduct a cost-effectiveness analysis over a two-year period after the PSI. We contrast the monthly credit subsidies with the rise in the firm's monthly payroll. The findings indicate the monthly credit subsidies exceeded the increase in the affected firm's monthly payroll by BRL 393 for micro and small firms and by BRL 165,685 for mid-large firms.

To interpret these outcomes, we build a theoretical model incorporating credit-constrained entrepreneurs, capable of opting for either productive or financial investments. We introduce a government-subsidized loan into the model. Theoretical results suggest unconstrained firms tend to substitute pricier loans with subsidized ones, without real effects. Conversely, credit-constrained firms seize the opportunity to augment their access to affordable credit, leveraging it to expand operations, and thereby fostering overall economic activity.

According to Moll (2014) and Banerjee and Moll (2010), if productivity shocks are persistent, then these negative impacts might only occur in the short run because financial frictions would only slow the economy's convergence towards its steady state. Even in the presence of credit constraints, the more productive firms could still fund their investments through their profits. So, as long as productivity differences are stable, the misallocation associated with initial conditions will eventually disappear.

Thus, the negative effects of credit constraints might have long or short-term effects, but there is a strong consensus in the literature that they exist. Allegedly to mitigate them, both developed and developing countries have adopted earmarked credit policies – The United States, France, India, Brazil, Mexico, and Colombia are some examples. Despite small differences in terms of their implementation, these policies typically rely on a combination of subsidized interest rates and second-tier operations to increase credit access. Under this structure, the public sector, which typically lacks the skills to operate wide-range credit technologies, provides subsidized funds to commercial banks, which are the ones responsible for channeling loans to eligible firms.

Overall evidence suggests these policies are effective in boosting the economic activity of its beneficiaries. A key reference in this literature is Banerjee and Duflo (2014), who used changes in eligibility criteria for earmarked credit lines in India as an exogenous shock to study their impacts. Following a government policy, Indian banks were forced to lend at least 40% of their net credit to priority groups, which included small-scale industries – defined as companies whose total tangible assets were inferior to a given threshold. The threshold defining the size of the prioritized firms was changed twice between 1998 and 2000. The first change included a broad set of relatively larger businesses in the eligible population, thus, expanding access to targeted loans, while the second change did the opposite.[2] They showed firms newly included in the eligible group expanded their total credit intake from the banking sector and used it to expand their production levels, with subsequent positive impacts on total sales and profits. The reverse occurred when firms were excluded from the priority list after 2000.

Eslava et al. (2011) and Eslava et al. (2012) also studied the impacts of earmarked credit policies. They used second-tier loans operated by Bancoldex, a state-owned development bank in Colombia, and compared the economic performance of beneficiary firms with similar non-beneficiaries. In both studies the comparison group was defined by matching firms' previous credit histories and other observable features. Though with different magnitudes, their overall results are similar to those found by Banerjee and Duflo (2014). Firms that accessed these earmarked loans increased their average debt levels, and used it to expand total output, employment levels, investment and productivity.

Moving to the developed world, Bach (2014) studied similar targeted credit policies in France. The author's identification strategy explored a set of policy decisions that unexpectedly changed the availability of subsidized funds for productive sectors, with wholesale trade working as the treated industry and retail trade as control. The overall results pointed out a positive impact on small firms, with beneficiaries experiencing an acceleration of economic activity (measured by costs, sales, and earnings) as a consequence of an increase in total firms' financial debt.

More recently, Brown and Earle (2017) have investigated the impacts on small and medium firms of earmarked loans operated in the United States by the Small Business Administration (SBA). They used two different identification strategies: a traditional Difference-in-Differences with matching in observables (Heckman et al., 1998), and a shift-share design (Adão et al., 2019) based on the geographical dispersion of commercial banks that had closer ties with SBA. Again, results indicated a positive impact on firm growth, measured by employment level. In addition, they found the effects were stronger for smaller and younger companies. Since these features are typically associated with higher levels of financial constraint, these results reinforce the narrative that earmarked loans' effectiveness is, at least in part, due to their capacity to expand credit access. This narrative is also consistent with Zia (2008), who found that in Pakistan only privately owned companies (in opposition to larger publicly listed companies) had their export levels affected by targeted credit lines.

Though with important differences in terms of economic context and identification strategies, all these previous studies share one common feature: they evaluated the impacts of earmarked credit policies by comparing beneficiaries and non-beneficiaries. By doing this, they were mixing two different mechanisms: (i) the expansion to external funds availability or extensive margin (that we will define here as the access mechanism), and (ii) the benefits of receiving subsidized interest rates or intensive margin (defined here as the price mechanism). Disentangling these two different

[2]Until 1998, the threshold was equivalent to 6.5 million rupees. This limit was initially elevated to 30 million and reduced in 2000 to 10 million.

causal paths is particularly relevant because the rationale for targeted credit is typically associated with scarce credit access, not necessarily with its price. The large and unexpected change in the interest rate, but not in the access allows us to exactly disentangle these two mechanisms – access and price – so that our paper is able to measure the price mechanism separately.

Reduced (subsidized) interest rates could not necessarily cause banks to change their eligibility criteria for loan decisions. In the worst-case scenario, it could lead them to channel more funds for firms that are good clients (low risk), but whose investment and growth decisions are not limited by the lack of financial resources. From a theoretical perspective, this means the price mechanism, *per se*, could be ineffective in alleviating the original market failure that earmarked credit policies were (supposedly) designed to tackle. From a policy perspective, this means subsidized loans could be operating mostly as a rent-seeking endeavor, with low-risk and financially unconstrained companies capturing treasury funds without generating any social benefits.

So far, only one study has tackled this question. Similarly to Banerjee and Duflo (2014), Cavalcanti and Vaz (2017) also used changes in eligibility rules as an identification strategy to investigate the impacts of earmarked loans in Brazil. However, this time the threshold defined access to marginally better (or worse) financial conditions associated with FINAME, a traditional second-tier earmarked credit line operated by the Brazilian Development Bank (BNDES). In this case, the group of firms that benefited from a cut on their earmarked loans' final interest rates were able to increase their investment rates, with positive impacts on labor and total factor productivity. The magnitudes were also impressive: a 1.5 p.p decrease in the *all-in* interest rate led to roughly a 10% increase in the productivity growth rates six years after. Still, according to the authors, this positive impact could only be observed when the change in the price of the credit was perceived as permanent.

However, results from Cavalcanti and Vaz (2017) were obtained by comparing only small firms in very specific size ranges. Thus, their external validity is very limited. This can explain why Bonomo et al. (2015) and Lazzarini et al. (2015) found no impact on investments when they investigated the effectiveness of similar earmarked credit loans operated by BNDES, but used mostly by larger and publicly traded companies. Unlike Cavalcanti and Vaz (2017), in our empirical setup, the magnitude of the changes in interest rates evaluated is much more expressive, and it affected firms from a wide range of sizes. Due to that broad external validity, our research can also help reconcile the conflicting results obtained by Cavalcanti and Vaz (2017), on the one hand, and Bonomo et al. (2015) and Lazzarini et al. (2015), on the other.

The remainder of this article is divided into five sections (beyond this introduction). The next section develops a partial equilibrium model with heterogeneous firms and credit constraints. Results from this partial equilibrium analysis suggest that: (i) for credit-constrained firms, a reduction in final interest rates could help to increase productive investments by equalizing their marginal financial costs to those observed by their non-constrained peers; and (ii) once the subsidized interest rates become relatively lower, in comparison to the risk-adjusted return of an alternative financial investment available in the economy, financially unconstrained companies should have incentives to seek these earmarked loans as a primary source for their productive investments while (re)directing alternative funds for alternative financial investments – a process that we will define here as funds arbitrage. In this setting, the model predicts companies engaging in funds arbitrage would increase their subsidy intake without increasing their economic activity.

In light of this theoretical model, Section 3 explains in detail the Brazilian context, how Finame and PSI were operated by BNDES, and why the creation of PSI represents an ideal experiment to test the prediction of the theoretical model. It shows how, at the dawn of PSI, very similar firms, all beneficiaries of earmarked loans, were almost randomly allocated to substantially different interest rates simply because they applied for credit a few days apart. Section 4 unfolds the empirical framework applied in this research, including description of the data sets and identification strategy. Section 5 describes the results and robustness checks. As usual, the last section consolidates key lessons learned.

2 A theoretical model for credit-constrained entrepreneurs

We will start with a simple economy, formed by J entrepreneurs seeking to maximize profits. Each entrepreneur j has an initial endowment (E_j) and can use it to make two different types of investments: one productive (L_j) and one financial (I_j). The return on productive investment has diminishing returns to scale and depends positively on an entrepreneur-specific element (A_j). Alternatively, the financial return (r) is strictly positive, demand-inelastic, and homogeneously available to all individuals; we can think of I_j as a Treasury bond that can be freely bought by all entrepreneurs. Furthermore, with probabilities $1 - \sigma_j^L$ and $1 - \sigma^I$, the investments might fail, and then entrepreneurs lose the total invested amount.

Within this set, equation 1 represents how each entrepreneur j could combine the two investment options to generate profits. I_J and L_J are substitute goods, and the last one has diminishing returns to scale ($\alpha < 1$). Thus, the marginal return on the financial investment (F_i) works as an opportunity cost for the productive investment, providing a lower bound for the marginal productivity of L_j (F_l). Furthermore, assuming the risk-adjusted return on financial investment is strictly positive ($\sigma^I r > 0$), entrepreneurs will invest the total of their endowment. Therefore, any allocation of investments defined by entrepreneurs will have to respect two conditions: (i) $F_l \geq F_i$ and (ii) $L_j + I_j = E_j$.

$$F(L_j, I_j) = \sigma_j^L A_j L_j^\alpha + \sigma^I r I_j \tag{1}$$

Equation 2 brings the values of L_j and I_j that maximize wealth for a given level of treasury rate r (L_{jr}^* and I_{jr}^*). It can be seen that only a share of the entrepreneurs will reach the investment level that equalizes the marginal productivity of L_j to its marginal cost ($\sigma^I r$). Entrepreneurs whose initial endowment is low relative to their idiosyncratic productivity factor (A_j), will invest less than they desire. More importantly, these unimplemented investments would generate wealth and improve overall welfare in the economy because their social return would overcome their social cost. This group of entrepreneurs will be here defined as credit-constrained. They have two common features: access to profitable investment opportunities and a scarcity of capital to implement them.

$$(L^*_{jr}, I^*_{jr}) = \begin{cases} ([\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}, \quad E_j - [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} \leq E_j \\ (E_j, \quad 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} > E_j \end{cases} \tag{2}$$

Without restrictions given by the initial endowment, the productive investment of each individual would be as high as it is necessary to equalize F_l to its marginal opportunity cost $\sigma^I r$. Therefore, equation 2 also provides a theoretical measure for the stringency of credit constraints imposed on each entrepreneur – $\rho(L^*_{jr})$. Defined as the distance from the feasible to the desirable investment, this measure is presented formally in Equation 3. It is worth noting that $\rho_j \geq 0 \quad \forall j$. This fact is an outcome of the availability of an alternative investment option. Even when $E_j > L^*_{jr}$, entrepreneurs will not overinvest in L_j. Instead, they will channel their capital surplus to I_j because it provides a better return.

$$\rho(L^*_{jr}) = [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} - L^*_{jr} \tag{3}$$

Introducing a banking sector

Now we will increase the complexity of this economy by introducing a banking sector. The banking sector is made up of several small banks that maximize profits within a perfect competition market.[3] They all use the same lending technology, which allows them to correctly assess the initial endowment (E_j) and the risks associated with the productive investments of their clients (σ^L_j). However, the existence of an informational failure impairs their ability to assess the productivity factor that is entrepreneur-specific (A_j). Therefore, transactions between banks and entrepreneurs will be characterized by asymmetric information.

This banking sector raises funds from entrepreneurs who are willing to apply their capital on financial investments, paying the equivalent to $\sigma^I r$ to them. Then they lend resources to other entrepreneurs, charging an interest rate of r_b. Within each loan, banks' earnings ($r_b P_j$) must compensate for funding ($\sigma^I r P_j$) and intermediation costs (θP_j). In this setting, supply and demand for credit will equalize with $r_b = \sigma^I r + \theta$.

Now, entrepreneurs whose initial endowment was insufficient to implement profitable investments can borrow from banks, even though they pay more for this external source – which is consistent

[3] Deviations from this perfectly competitive banking sector would only increase problems associated with credit constraints (Joaquim et al., 2020).

with the Pecking Order Theory in corporate finance (Myers and Majluf, 1984; Shyam-Sunder and C. Myers, 1999).

As in Fonseca and Van Doornik (2022), if a client refuses to pay the debt, banks might enforce payment up to the limit of the client's remaining assets. Thus, entrepreneurs whose assets are inferior to their total debt can maximize wealth by defaulting. As a reaction, banks will automatically limit the amount they lend ($\bar{P}_j$) to guarantee loan payments ($r_b P_j$) are never higher than the expected assets of the client ($E[E_j]$).

Equations 4 and 5 represent, respectively, the optimal levels of productive and financial investments (L^*_{jb} and I^*_{jb}), and the loan limit imposed on each entrepreneur by the banks in this new setting. By comparing equations 4 and 2, one can see productive investments are at least as high as they were in the initial setting - that is $L^*_{jb} \geq L^*_{jr} \quad \forall j$. Also, since intermediation costs are strictly positive, the interest rates paid by originally constrained entrepreneurs will necessarily be higher than the opportunity cost ($r_b > \sigma^I r$).

$$(L^*_{jb}, I^*_{jb}) = \begin{cases} (L^*_{jr}, I^*_{jr}) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} \leq E_j \\ ([\frac{\alpha\sigma^L_j A_j}{r_b}]^{(\frac{1}{1-\alpha})}, 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} > E_j \quad \wedge \quad [\frac{\alpha\sigma^L_j A_j}{r_b}]^{(\frac{1}{1-\alpha})} \leq E_j + \bar{P}_j \\ (E_j + \bar{P}_j, 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{r_b}]^{(\frac{1}{1-\alpha})} > E_j + \bar{P}_j \end{cases} \tag{4}$$

$$\begin{aligned} \bar{P}_j &= \frac{\sigma^L_j (L^*_{jb} + \sigma^I I^*_{jb}) + (1 - \sigma^L_j)\sigma^I I^*_{jb}}{r_b} \\ &= \frac{\sigma^L_j L^*_{jb} + \sigma^I I^*_{jb}}{r_b} \end{aligned} \tag{5}$$

Additionally, the comparison between Equations (4) and (2) allows two main conclusions to be drawn. First, entrepreneurs without credit constraints do not change their behavior - $L^*_{jb} = L^*_{jr}$ and $I^*_{jb} = I^*_{jr}$. Second, the only entrepreneurs affected by the existence of this banking system were all credit-constrained, and they responded by expanding productive investments. Finally, the substitution of L^*_{jr} by L^*_{jb} in equation (3) shows that $\rho(L^*_{jr}) \leq \rho(L^*_{jb}) \quad \forall j$. Therefore, the introduction of this banking system contributes to improving social welfare by alleviating credit constraints. However, the fact that banks limit the total amount they lend according to entrepreneurs' initial endowments means this market failure will not be eliminated.

Introducing a subsidized earmarked credit policy

To analyze the potential impact of public policies, we will introduce a government. This government wants to maximize productive investment, which is not reaching socially optimal levels due to credit constraints (which is the market failure being targeted). To do so, it uses a subsidized credit. Since the government has no lending technology, it operates through second-tier loans that reduce the interest rates paid by entrepreneurs to r_g without lowering the bank's remuneration (r_b).

Additionally, we will assume the government has an inelastic source of funds outside of this economy. Therefore, at least in the short-term, the public sector can pay for this subsidized earmarked credit policy. Although very simplistic, this assumption allows us to focus on the short-term impacts of this class of policies.[4]

The major consequence of this simplification is that the results suggested by this theoretical model represent, in terms of welfare increase, an upper limit for credit policies. Necessarily, achievable policies would be less efficient due to either distortion generated by taxation or difficulties in channeling subsidies to the target population.

Therefore, in this best-case scenario, subsidized credit policies would simply represent a reduction in the final interest rates paid by entrepreneurs. In practical terms, we could define $(r_g - r_b)\ P_j$ as the amount of subsidies associated with an earmarked loan. To understand how such a policy would impact their investment decisions, it is interesting to analyze two substantially distinct scenarios: when $r_g \geq \sigma^I r$, and when $r_g < \sigma^I r$.

In the first case, reductions in r_g are comparable to improving the efficiency of the bank's lending technology by reducing intermediation cost θ. As the interest rate reduces, two mechanisms help to induce productive investments of credit-constrained companies. First, the reduction of the credit cost increases the optimal level of productive investments for those who are credit-constrained[5]. Second, the reduction in total interest payments expands the total amount entrepreneurs can borrow to execute these investments[6].

Equation 6 brings the optimal investment levels when r_g converges to $\sigma^I r$ - the extreme case within this first context. Credit constraints are reduced even further $(\rho(L^*_{jb}) \leq \rho(L^*_{jg})\ \forall j)$, although not eliminated due to the existence of a credit limit associated with the initial endowment. Again, entrepreneurs who were initially unconstrained by credit do not show any behavioral change.

$$(L^*_{jg}, I^*_{jg}) = \begin{cases} ([\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}, \quad E_j - [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} \leq E_j \\ ([\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}, \quad 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} \in]E_j, E_j + \bar{P}_j] \\ (E_j + \bar{P}_j, 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} > E_j + \bar{P}_j \end{cases} \tag{6}$$

The second case is substantially different because when the subsidized credit rate falls below $\sigma^I r$, the marginal cost of capital drops below the opportunity investment cost. All entrepreneurs will be willing to borrow in this case, even those that were credit-unconstrained. This group lacks

[4] Alternatively, the government resources could be endogenized in this economy by making the government tax a subset of entrepreneurs (those that are not credit-constrained). However, it would introduce complexities associated with distributive and efficiency issues. Those are relevant topics for a full policy evaluation. It is heroic to assume the public sector would have the capacity to perfectly target taxation towards unconstrained entrepreneurs and to channel those resources towards constrained ones with no cost. However, a broad discussion of the capacities of the government is beyond the scope of this research.

[5] $[\frac{\alpha\sigma^L_j A_j}{r_g}]^{(\frac{1}{1-\alpha})} > [\frac{\alpha\sigma^L_j A_j}{r_b}]^{(\frac{1}{1-\alpha})} \quad \forall j$

[6] For all credit-constrained entrepreneurs it is valid that $\frac{\sigma^L_j L^*_{jb} + \sigma^I I^*_{jb}}{r_g} > \frac{\sigma^L_j L^*_{jb} + \sigma^I I^*_{jb}}{r_b}$.

investment opportunities with a return greater than $\sigma^I r$. However, since r_g is even lower, they can maximize profits by channeling loans to financial investments. This way, previously unconstrained entrepreneurs can take advantage of the subsidized interest rates without overinvesting - which would lead to lower overall profits. Equation 7 shows how optimal investment levels respond to subsidized rates inferior to $\sigma^I r$.

$$(L^*_{jg}, I^*_{jg}) = \begin{cases} ([\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}, \quad E_j + \bar{P}_j - [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})}) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} \leq E_j + \bar{P}_j \\ (E_j + \bar{P}_j, \quad 0) & ; \quad [\frac{\alpha\sigma^L_j A_j}{\sigma^I r}]^{(\frac{1}{1-\alpha})} > E_j + \bar{P}_j \end{cases} \tag{7}$$

By reducing funding rates up to r, the policy helps to equalize the marginal investment cost among entrepreneurs. From this point on, this mechanism ceases to operate because all firms already have the same opportunity cost – now defined by the financial investment. By comparing equations (7) and (2), it becomes clear that, through funds arbitrage, credit-unconstrained companies will be able to use subsidies to expand their financial investments without increasing their economic activity (defined here by their productive investments).

It is worth noting what happens if, once $r_g < \sigma^I r$, subsidized interest rates are further reduced. Firms that were originally unconstrained will tend to increase their debt intake, because, as defined by Equation (5), $\bar{P}_j$ will continue to increase as r_g falls. However, in such a context, the debt intake will tend to be proportionally stronger among credit-constrained companies, because for them there are two mechanisms affecting $\bar{P}_j$. Not only it increases directly because of r_g, but also because their productive investment L^*_{jg} will increase.

Overall, as long as there is a nonempty set of unconstrained firms accessing subsidized loans, the marginal effectiveness of the earmarked credit policy starts to decrease. It happens because for this group of beneficiaries, the policy becomes an instrument for income transference, without any relevant impact in terms of economic growth and social welfare. In contrast, for firms that remain credit-constrained the policy is still effective because the second mechanism will keep functioning and will allow them to expand their credit intake and use it to expand economic activity.

3 BNDES, PSI, and the earmarked credit in Brazil

The Brazilian Development Bank (BNDES) is one of the largest development banks in the world (Lazzarini et al., 2015). It was created in 1952 by the Brazilian federal government. During that time, Brazil was a predominantly agrarian economy, highly dependent on international prices of coffee, sugar, and other commodities. The creation of BNDES was recommended by the Joint Brazil-United States Economic Development Commission (Paiva and Pessoa, 2012). This binational government group had previously identified a set of key infrastructure projects that should be executed to allow an industrialization process to take place in the Brazilian economy.

Therefore, the first goal of BNDES was to structure and fund this previously defined set of infrastructure projects. At that time, long-term credit markets were practically non-existent. After the exhaustion of this initial portfolio, BNDES not only continued to play a role in selecting and financing infrastructure projects for the federal government, but also continuously expanded its areas. Beyond infrastructure, at the end of the 2000s BNDES was also funding general investments, exports, innovation, and working capital for companies in almost every economic activity (Além and Madeira, 2015). Table 1 states the importance of BNDES for the Brazilian corporate credit market at the end of the 2000s.

Table 1. Corporate credit portfolio in the Brazilian economy (2007-2010)

	Total	Free-market		Earmarked		BNDES		
Period	BRL	BRL	% of total	BRL	% of total	BRL	% of total	% of earmarked
2007-Q1	244.6	75.32	30.79	169.3	69.21	118.3	48.34	69.84
2007-Q2	257.1	83.66	32.54	173.4	67.46	119.2	46.35	68.71
2007-Q3	280.7	97.28	34.66	183.4	65.34	126.0	44.89	68.70
2007-Q4	312.7	113.43	36.27	199.3	63.73	138.1	44.16	69.30
2008-Q1	338.6	132.44	39.11	206.2	60.89	145.2	42.88	70.41
2008-Q2	368.8	150.28	40.75	218.5	59.25	151.3	41.02	69.23
2008-Q3	410.8	175.20	42.64	235.6	57.36	164.4	40.02	69.78
2008-Q4	437.3	174.77	39.97	262.5	60.03	188.7	43.16	71.90
2009-Q1	435.1	164.78	37.87	270.3	62.13	195.5	44.93	72.31
2009-Q2	432.0	154.26	35.71	277.7	64.29	200.9	46.52	72.35
2009-Q3	431.9	110.72	25.64	321.2	74.36	238.6	55.24	74.29
2009-Q4	444.3	94.58	21.29	349.7	78.71	263.6	59.33	75.38
2010-Q1	449.4	94.90	21.12	354.5	78.88	268.8	59.80	75.81
2010-Q2	467.9	85.02	18.17	382.9	81.83	293.4	62.70	76.62
2010-Q3	481.7	69.25	14.38	412.4	85.62	316.1	65.62	76.63
2010-Q4	502.8	68.69	13.66	434.1	86.34	333.4	66.31	76.80

Source: The Central Bank of Brazil (BCB-DSTAT). BRL represents BRL Billions in current values.

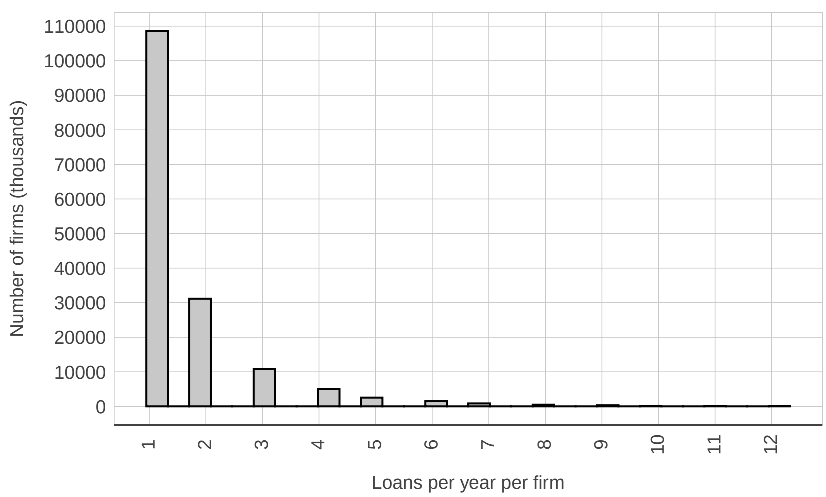
From 2007 to 2010, the importance of BNDES for the total corporate credit portfolio oscillated from a minimum of 40.02% (in 2008-Q1) to a maximum of 66.31% (2010-Q4). During this entire period, BNDES alone represented roughly two-thirds of all corporate earmarked loans in the Brazilian economy. It is also possible to note a rapid expansion of BNDES portfolio from 2009-Q2 to 2009-Q4, indicating an acceleration in BNDES' new loans. This movement coincides with the dawn of PSI, introduced by the Federal Government as a reaction to the international financial crisis. The program was formally announced to all BNDES's financial intermediaries (FIs) on July 10th 2009 by BNDES's Resolution n^{o} 1793/2009. Motivated by the international financial crisis, the Brazilian government conceived this program to avoid a liquidity crisis in the corporate credit markets that could jeopardize the firm's investments.

In practical terms, PSI was fundamentally an increase in the dosage of subsidies embarked on an already traditional earmarked credit line called Finame. Having been operating since the 1960s, Finame is one of BNDES' most traditional financial products. It operates through a second-tier loan mechanism, similar to the one described in our theoretical model. Under Finame, BNDES provides subsidized funds for FIs and sets general rules for them to lend using these funds. These rules are a set of eligibility criteria that define the type of firm and investment that can be financed.

In terms of firm eligibility, Finame is flexible, since it accepts firms of all sizes and industries as long as they hold formal status and comply with their tax obligations. In terms of investment eligibility, Finame is more stringent because it can only be used to fund the acquisition of new machinery, including buses and trucks, which are accepted as transportation machinery. This limitation means Finame is not a high-frequency credit line that can be used for working capital necessities.

As Figure 1 shows, the vast majority of companies do not use Finame loans more than once or twice a year. This scenario is consistent with a financial product that is mostly associated with the expansion or renovation of firms' production capacity, a kind of investment decision that typically requires long-term considerations.

Figure 1. Frequency of Finame loans per firm-year (2007-2010)



Source: BNDES; Authors' elaboration.

The step-by-step for a typical Finame loan would start with a firm selecting machinery to renovate or expand its production activities.[7] After selecting the equipment, this firm would reach out to a commercial bank's branch and request the loan. FIs will be responsible for evaluating compliance with the eligibility rules and for pricing the credit risk associated with each loan. BNDES does not interfere with the risk-analysis, but in case of default, FIs are held responsible for the amount they loan. If the FI agrees to fund the firm, it will submit the loan request for BNDES approval. After authorization, BNDES will transfer funds to the FI, which will request the delivery of the machinery. Once the new equipment is delivered, the FI will simultaneously pay the supplier

[7]BNDES keeps a list of all eligible machinery in its website.

and sign the loan with the investing firm.

This design allows BNDES to use commercial banks' capillarity and lending technologies, increasing its capacity to reach a broad spectrum of firms. This can be seen in Table 2, which shows that, at the end of the 2000s, Finame responded to a significant share of BNDES loans and engaged a relevant set of firms and financial intermediaries. The number of active FIs on Finame oscillates from a low of 44 to a maximum of 60 per quarter. To give a better sense of that number, there was a total of 130 commercial banks operating in the Brazilian economy at the end of 2011.[8] In terms of firms, Finame represents 80% to 90% of all BNDES borrowers each quarter. It is also relevant to note that loan amounts range from a minimum of a few thousand to tens of millions of reais[9], corroborating the idea that Finame is used by a broad spectrum of firm sizes.

Table 2. Overview of Finame's loans (2007-2010)

		Borrowers		Loans (Total)		Distribution of loan's amounts (BRL)				
Period	FIs	Firms	% of BNDES	BRL	% of BNDES	Min	1st decile	Mean	9th decile	Max
2007-Q1	51	6237	90.14	3641	28.81	0.0021	0.0459	0.5838	0.5850	10.00
2007-Q2	55	8079	85.97	4523	33.26	0.0020	0.0450	0.5599	0.6120	10.00
2007-Q3	54	8464	90.13	5606	28.98	0.0010	0.0455	0.6624	0.6244	25.13
2007-Q4	55	8322	85.06	5396	21.41	0.0018	0.0468	0.6484	0.5850	14.32
2008-Q1	55	7577	89.86	4552	31.54	0.0025	0.0522	0.6008	0.6120	10.00
2008-Q2	52	10001	94.18	6269	32.92	0.0016	0.0530	0.6268	0.6012	15.90
2008-Q3	59	11394	95.60	7573	46.44	0.0022	0.0575	0.6647	0.5611	24.80
2008-Q4	50	10802	94.80	5730	28.94	0.0013	0.0510	0.5305	0.5003	40.21
2009-Q1	47	8692	89.98	4252	24.54	0.0015	0.0480	0.4892	0.4966	30.00
2009-Q2	44	9456	83.93	4229	15.18	0.0006	0.0376	0.4472	0.4556	12.66
2009-Q3	50	12204	82.06	5094	11.80	0.0009	0.0230	0.4174	0.3680	10.00
2009-Q4	58	20409	83.22	12591	26.58	0.0008	0.0349	0.6169	0.5360	100.00
2010-Q1	53	22205	97.59	12064	61.45	0.0007	0.0311	0.5433	0.4960	62.13
2010-Q2	58	25264	92.59	16057	37.31	0.0005	0.0360	0.6356	0.5520	156.40
2010-Q3	59	23929	90.81	11273	45.33	0.0008	0.0332	0.4711	0.4503	51.06
2010-Q4	60	24357	92.08	12573	44.28	0.0004	0.0320	0.5162	0.4650	119.13

Source: BNDES. BRL represents BRL Millions in current values.

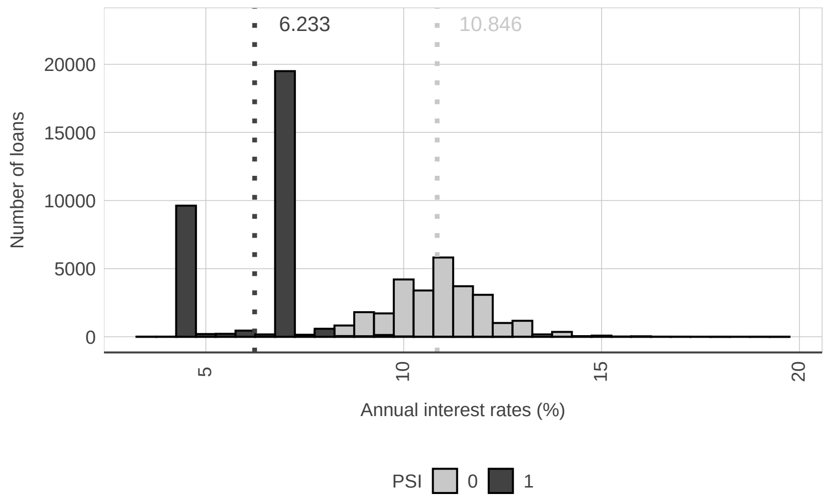
It is also worth highlighting how some statistics in Table 2 change dramatically between 2009-Q2 and 2009-Q4. Compared with the levels observed before, the total amount of new loans and investing firms remained unusually high from the end of 2009 onward. This movement seems to be a consequence of PSI. Figure 2 shows the distribution of annual interest rates for Finame and PSI loans in the year 2009. Before PSI, Finame interest rates were the sum of 3 different components: (i) BNDES' funding cost, (ii) BNDES' intermediation spread, and (iii) FI's intermediation spread. While the first two components were roughly fixed, the last one was negotiated directly between FIs and borrowers. With the dawn of PSI, the Federal Government established fixed all-in interest rates for all loans - either 7% per year, for transportation equipment, or 4.5%, for all other machinery. As we can see in Figure 2, interest rates in PSI (on average equal to 6.23%) were substantially

[8]ESTBAN (Central Bank of Brazil).

[9]At the end of 2010 1.00 USD was roughly equivalent to BRL 1.70.

lower than those observed in regular Finame (10.85%, on average). This strong reduction was only possible because the Brazilian Treasury used resources from the federal budget to directly compensate BNDES and FIs for the difference between PSI and regular Finame rates.

Figure 2. Distribution of Finame interest rates during 2009



Source: BNDES; Authors' elaboration.
Note: The dotted lines represent the mean values of each category (either PSI or Regular Finame).

Using the Brazilian short-term treasury rate, denominated SELIC, as a benchmark provides a clear sense of the magnitude of the financial cost reduction associated with Finame. SELIC rate was equal to 11.25% per year at the beginning of 2009. Looking at the distribution of interest rates in 2, it is fair to say Finame loans were already operating slightly below the opportunity capital cost for the Brazilian economy.[10] Once PSI kicks in, subsidized interest rates are reduced even further. In that case, it is fair to portray PSI as the last case described in our theoretical model.

As expected, this situation ultimately led to a boost in demand for Finame funds. However, while interest rates changed abruptly, demand reacted at a much slower pace. Figure 3 makes this point clear. It compares before and after daily averages for Finame loans, using as the center point July 29^{th} 2009 - which is the day the first loan with PSI financial conditions was processed by BNDES.[11] Panels (a) to (c) show a fairly smooth transition from Finame to PSI improved financial conditions. Daily values for the number of loans, borrowers, and total amounts show economic

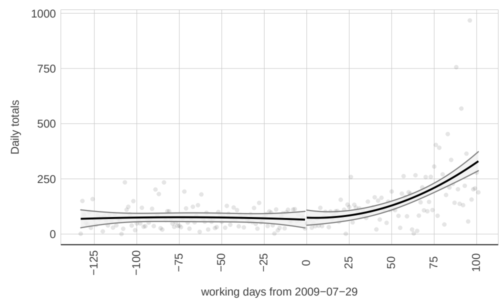
[10]It is particularly true for larger companies, whose risk spreads are typically lower and, thus, have access to lower all-in interest rates.

[11]In 2009, the full analysis cycle for a loan approval would typically take some days to be processed. Thus, even though PSI financial conditions were announced to FIs in July $10th$, the first PSI loan was only processed 19 days later.

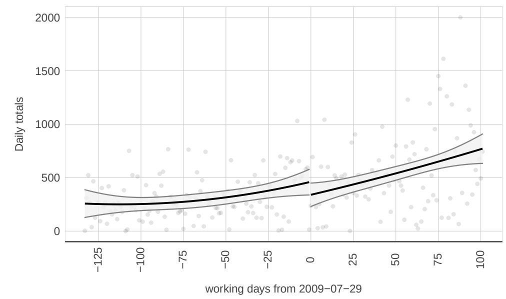
agents were not anticipating this sudden change, as there was no unusual decrease before the dawn of PSI.

Figure 3. Finame volumes in 2009 (before and after PSI comparison)

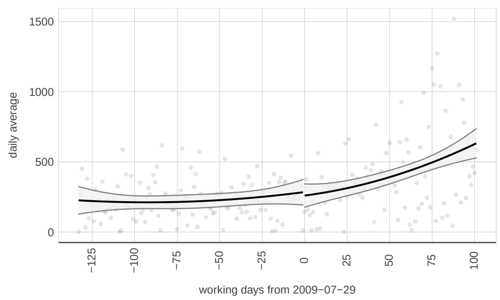
(a) Loan amount (BRL millions)



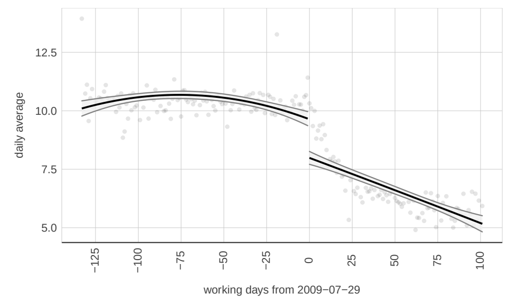
(b) Number of loans



(c) Number of unique firms



(d) Interest rates (% per year)

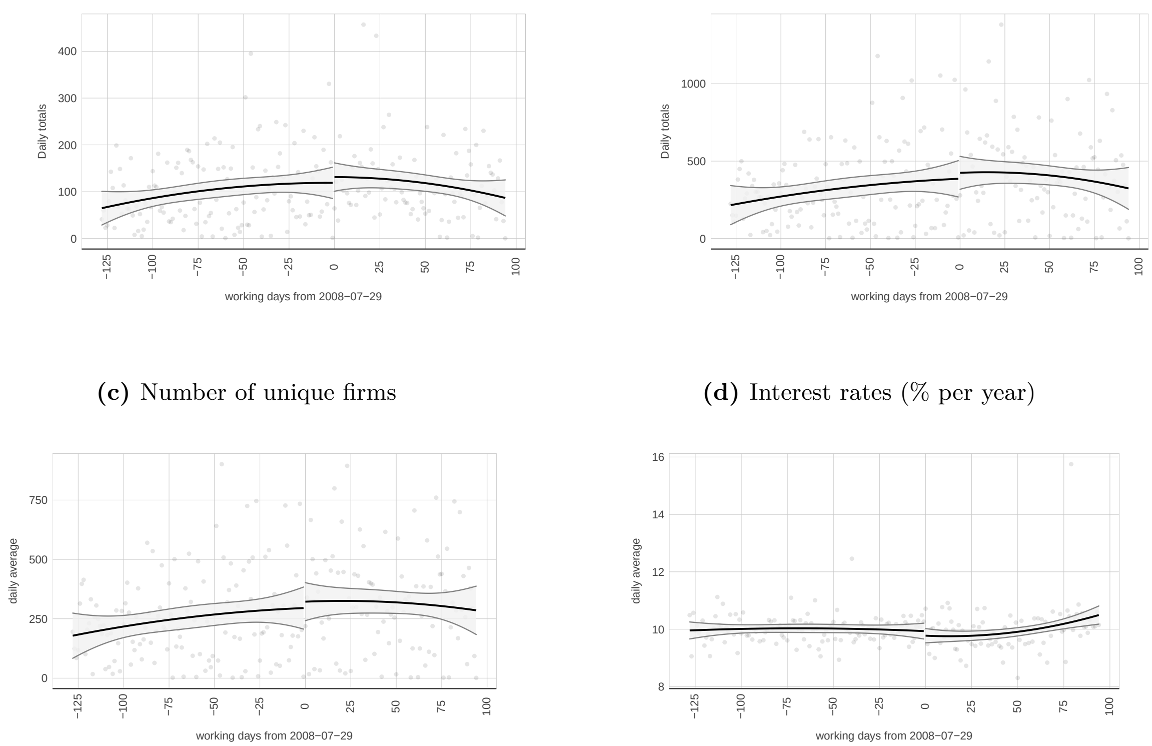


Source: BNDES; Authors' elaboration.
Note: Each dot is a daily observation and the black line represents predicted values for a regression of y on a quadratic time-trend and a dummy equal to one after the cutoff (defined at the dawn of PSI), while the light gray areas are 95% confidence intervals for this prediction.

It is also clear that, by the end of 2009 (roughly 100 working days after the center point), Finame was operating at higher than usual levels. However, for several days after the cutoff date, loan flows remained fairly stable. This shows it took some time for FIs and firms to respond to the new financial conditions. This dynamic is explained by two main factors. First, it took some time for news about the improved interest rates to reach commercial bank's branches and their potential customers. Second, Finame loans are, as previously mentioned, attached to long-term investment decisions and require a relatively long analysis flow. Thus, firms that had recently replaced or expanded their machinery would hardly seek new loans, despite the improved conditions. Similarly, firms that were not yet considering implementing this kind of investment would need some time to plan and execute the machinery acquisition before being able to apply for a Finame loan.

On the other side, Panel (d) shows a clear discontinuity in predicted daily average interest rates, with loan costs plummeting right after the dawn of PSI. Serving as a placebo comparison, Figure 4 shows the same series as observed exactly one year before. As expected, there are no signs of sudden changes, reinforcing the argument that the exogenous introduction of PSI explains the behavior observed for interest rates in 2009.

Figure 4. Finame volumes in 2008 (placebo comparison)



(a) Loan amount (BRL millions)
(b) Number of loans
(c) Number of unique firms
(d) Interest rates (% per year)

Source: BNDES; Authors' elaboration.
Note: Each dot is a daily observation and the black line represents predicted values for a regression of y on a quadratic time-trend and a dummy equal to one after the cutoff (defined at exactly 1 year before the dawn of PSI), while the light gray areas are 95% confidence intervals for this prediction.

This situation creates an ideal experiment to isolate the aforementioned price and access mechanisms associated with earmarked credit lines. Firms that used Finame general conditions right before the approval of PSI were similar (in terms of observable and non-observable characteristics) to those that used PSI right after. Roughly around the same time, both groups were capable of having their loan's claims approved by FIs, and they were willing to acquire new machinery for their business - a decision typically associated with companies' growth cycles. Despite that, part of them was randomly exposed to substantially lower financial costs, simply because they applied for loans a couple of weeks later. In the following section, we will dive deeper into this identification strategy.

4 The empirical framework

4.1 The identification strategy

As mentioned above, access to Finame in the Brazilian economy is not random. Firms self-select themselves because they need to apply for those loans and they need to be willing to expand or renovate their machinery. In that sense, companies that do not seek Finame loans might be in a different phase of their growth cycle. In addition, borrowers are selected by commercial banks during the credit risk analysis. Thus, firms that do not receive Finame loans might have a worse financial performance or may be engaged in high-risk economic activities.

Because of that, it is reasonable to assume that, both in observable and unobservable features, firms that hold Finame loans are fundamentally different from those that do not. Therefore, typical causal inference techniques based on controlling for observable factors (such as those used by Bonomo et al. (2015) and Lazzarini et al. (2015) to study the impact of BNDES) would not solve this selection bias.

Because of that, the exercise proposed here will focus on comparing two specific groups: *(i)* those who received Finame loans under PSI conditions; and *(ii)* those that used Finame, but under its regular conditions. From a theoretical perspective, this comparison has the advantage of isolating the access (extensive margin) from the price mechanism (intensive margin). From an empirical perspective, the benefit comes from comparing a more homogeneous set of firms. The key underlying hypothesis is that around the dawn of PSI, in 2009, the separation between these two groups was random, with companies getting better financial conditions simply because they requested credit a few weeks later.

Assuming that, we can estimate a local Difference-in-Differences (DiD), considering a time interval around this sudden change in financial conditions. Equation 8 formalizes the identification strategy, where i defines firms; t represents a relative-time, counted as months from the earmarked loan approval date; α_i is firm fixed effect; α_t and α_m are dummies controlling for, respectively, relative and chronological-time fixed effects; PSI is equal to 1 only after the earmarked loan approval date (i.e, $t \geq 0$) and for borrowers that received PSI conditions; and $y_{i,t}$ represents firms outcome measures[12].

$$y_{i,t} = \alpha_i + \alpha_m + \alpha_t + \delta \; PSI_{i,t} + \varepsilon_{i,t} \tag{8}$$

In addition to the advantage of increasing comparability, the fact that all firms are treated,

[12] Our estimations did not use *log* transformations because some of our key variables (such as subsidy or debt level) might assume zero values.

either by Finame or by PSI, has another empirical advantage. In such a context, there are no bad comparisons, as defined by Baker et al. (2022), to bias the estimator. These bad comparisons arise from the fact that, with multiple periods, the TWFE estimative for δ becomes a weighted average of multiple canonical 2x2 comparisons (Goodman-Bacon, 2021). When this happens, early-treated units might serve as controls for late-treated units, which is problematic in the presence of long-term heterogeneous treatment effects. In the setting proposed here, all firms transit from untreated to treated at the same relative time ($t = 0$). Although variance on specific chronological-time effects could influence the outcome for a given re-centered time, this is controlled by α_m. Thus, this design makes it possible to re-center companies' trajectories around the earmarked loan date approval and estimate δ through a re-centered Fixed-Effects (FE) - we will call this estimator $\hat{\delta}^{did}$.

Alternatively, following the work of Arkhangelsky et al. (2021), our identification strategy can rely on the Synthetic Difference-in-differences method (SDiD). In our setting, SDiD can be implemented by estimating Equation (8) through a weighted FE, where the weight of each observation is equivalent to the product $\hat{\omega_i}^{sdid}\ \hat{\lambda_t}^{sdid}$ - we will call this estimator $\hat{\delta}^{sdid}$. We can define $\hat{\omega_i}^{sdid}$ as a unit weight that arises by minimizing the average difference, considering only pre-treatment outcome values, between treated and control units. $\hat{\lambda_t}^{sdid}$ is a time weight that minimizes, for control units, the average difference between pre-treatment and post-treatment outcome values.[13]

Intuitively, one can think about SDiD as an estimator that puts more weight on control observations that are more similar to the treated ones - whether at the unit or at the time level. As shown by Arkhangelsky et al. (2021), SDiD is robust and more precise than DiD when systematic outcome heterogeneity between units or periods is high. In our context, and as long as the proposed identification strategy holds, the SDiD approach can be understood as a way to increase the power of our test.

However, there is one drawback associated with the use of SDiD: the calculation of $\hat{\omega_i}^{sdid}$ and $\hat{\lambda_t}^{sdid}$ requires a balanced panel. As we shall see later, this is not the case for all outcome variables. The SDiD can still be implemented by selecting only perfectly balanced firms, but it triggers the question of whether estimation differences are driven by the change in methods or by sample selection. To tackle this reasonable question, we will also implement a traditional DiD estimation that uses only perfectly balanced units - we will call that estimator $\hat{\delta}^{did-b}$. With this, we will be able to disentangle the effect of sample selection (defined by the difference between $\hat{\delta}^{did}$ and $\hat{\delta}^{did-b}$) and the effect of improved comparability of SDiD (defined by the difference between $\hat{\delta}^{did-b}$ and $\hat{\delta}^{sdid}$) for our coefficients.

Despite the method applied, the proposed estimation of δ will indicate whether higher subsidies provided by PSI had any differential impact on firms' economic trajectory, considering those treated by traditional Finame as controls. The lack of a positive and statistically significant coefficient would suggest that PSI's improved conditions increased the fiscal burden of the earmarked policy

[13] The formal definition of the weights and the optimization problem implementation can be consulted on Arkhangelsky et al. (2021). Here, the optimization was implemented using the *synthdid* R-package, also developed by Arkhangelsky et al. (2021).

without improving its main-street impacts.

Guided by our theoretical model, the expected transmission chain from the subsidized interest rates towards firms dynamics should go as follows: ***(i)* all firms benefited by PSI conditions should increase their subsidy intake ($(r_g - r_b)\bar{P}_j$), due to the reduction on the earmarked interest rate; *(ii)* productive activity (L^*_{jg}) would increase only among previously credit-constrained firms due to the price mechanism; and *(iii)* debt levels ($\bar{P}_j$) should increase proportionally more among previously credit-constrained companies, because their productive investments (L^*_{jg}) would also expand**.

The main threat to this identification strategy comes from a possible self-selection behavior arising as firms react to PSI. Firms that were already moving to acquire new machinery and, due to their investment timing, ended up with PSI conditions will be similar to those that received traditional Finame conditions right before the announcement of PSI. This group will be here denominated as early-comers. However, two groups of firms would likely bias our results: the ones that, before PSI, decide to postpone or cancel an investment hoping to access better financial conditions in the future; and the ones that, after PSI, choose to acquire the equipment they would not do otherwise. These two groups will be denominated, respectively, defying and latecomers, and their presence would necessarily reflect in unusual demand flows for Finame loans.

If firms were postponing their credit request while waiting for the new financial conditions, demand would drop immediately before PSI. As we have seen in Figure 3, this was not the case, which indicates firms were not anticipating the sudden change. Also, BNDES Resolution nº 1793/2009 explicitly forbade loans requested up to that date to claim PSI new interest rates. Data suggests defying firms is only a theoretical worry.

As we have also seen in the previous section, Finame is not a high-frequency credit line that can be quickly accessed. On top of that, information about Finame new conditions took some time to circulate in the economy. Those two facts make it reasonable to assume that, for some time after PSI announcement, loans would still reflect the investment decisions taken despite the new loan conditions. However, as Figure 3 shows, demand eventually responded, and an unusual loan volume is clear at the end of the year. This result suggests that, after some time, our key hypothesis is no longer reasonable because latecomers were present. Thus, the key question for our identification strategy is: for how much time after the PSI announcement were firms not reacting directly to it (or when can we draw the separation between early-comers and latecomers)?

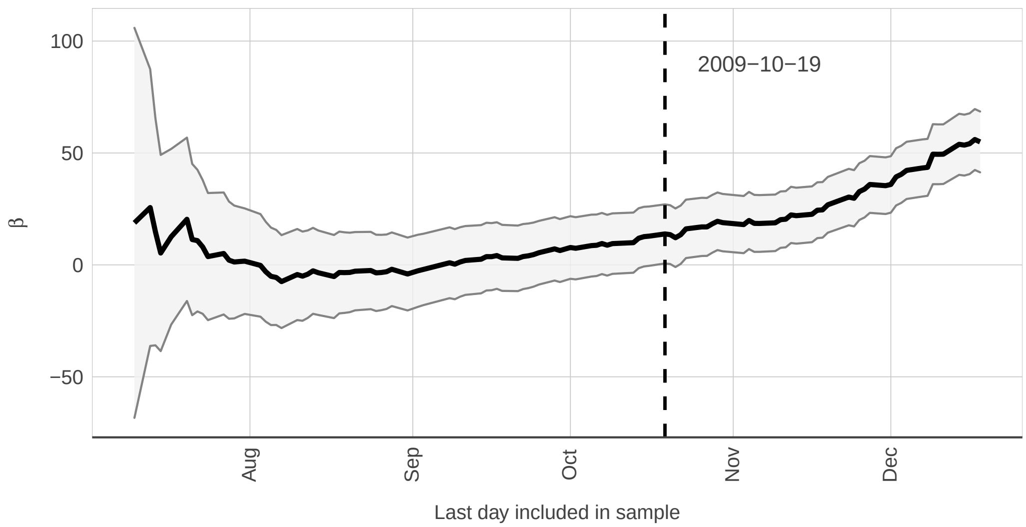
To delve deeper into this question, we estimated the simple model described by Equation (9), where: *Loans* stands for daily Finame loans approved by BNDES; $alpha_{year}$ is a year fixed-effect; α_{day} is a daily fixed-effect (days are relative and counted from July 10th of its reference year); and *Post* is a dummy equal to 1 for all observations after July 10th of 2009 (the period marked by PSI existence). Daily dummies will capture regular seasonal movements within the year - for instance, demand for agricultural machinery might be affected by the harvest calendar. The yearly dummies will capture macroeconomic shocks and contexts that could differ from year to year and affect credit demand.

$$Loans_i = \alpha_{year} + \alpha_{day} + \beta \; Post + \epsilon_i \tag{9}$$

Equation 9 was estimated for different sequential samples. The entire period from 2002 until the dawn of PSI is used as a benchmark, but days after July 10th (the day that marked the PSI announcement) are added one at a time. The intuition is that while $\hat{\beta}$ remains statistically null, Finame loans are still within the expected range, considering typical seasonal movements in the year and within the year. In contrast, a positive and significant $\hat{\beta}$ indicates a demand higher than expected and likely caused by a response to specific PSI interest rates.

Results of this exercise can be observed in Figure 5. $\hat{\beta}$ oscillates between positive and negative values up until the end of August, roughly 6 weeks after the dawn of PSI. Starting in September, $\hat{\beta}$ initiates a sustained upward movement. October 19^{th} is the first day that $\hat{\beta}$ becomes statistically positive - 95% confidence intervals are marked by the light gray areas. In practice, a clear acceleration in earmarked loans becomes noticeable only then. This result suggests that,from the last third of October onward, the share of firms already reacting to PSI interest rates was relevant. Then, in mid-November, $\hat{\beta}$ initiates a strong ascending movement for each additional day included in the sample.

Figure 5. Finame loans after PSI



Source: BNDES; Authors' elaboration.
Note: The black line represents $\hat{\beta}$ for each estimation of Equation (9), with days after the dawn of PSI beeing added to the sample one at a time. The light gray areas are 95% confidence intervals for this prediction.

For our empirical framework, firms that accessed Finame loans in late October will most likely be latecomers. As such, they were not accessing PSI financial conditions by chance and therefore are likely to be a poor comparison group for estimating PSI impacts. However, latecomers can still be used to evaluate the quality of our empirical approach. We expect early-comers to be similar, considering observable and unobservable features, to firms that accessed Finame right before the dawn of PSI. However, if it is true these latecomers are self-selecting themselves into the program, they are likely substantially different.

Thus, saturating Equation (8) with them would have two consequences. First, naturally, $\hat{\delta}$ would be distorted by the self-selection bias. Second, the key identification hypothesis for DiD models (i.e. previous parallel trends) would be violated because fundamental differences between treated (PSI) and control (Finame) groups would affect not only their outcome after treatment (defined by access to earmarked credit loans), but also their previous growth trajectory.

4.2 Data and descriptive statistics

The first important piece of information to implement this identification strategy comes from BNDES, which granted us access to a loan-level data set comprising all Finame loans from 2002 onward.[14] It gathered information on the identity of all borrowers and FIs (both tagged with their respective tax identification numbers), and on the characteristics of the loan (approval date, interest rates, term, and loan value). Additionally, these data also included a firm-size categorical variable for all borrowers, which we use to explore heterogeneous effects of PSI - more on that in Section 5.3. This variable takes four possible values (micro, small, medium and large), based on firms' yearly gross sales.[15] The information used to categorize firms comes from their last available balance sheet and is validated by FIs when they perform their credit risk analysis, that is, right before they decide whether they accept a loan claim or not.

BNDES data were used to assign firms to treated and control groups and to define the time of treatment. Initially, all firms with a Finame loan approved in 2009 were taken into account. Those exposed to PSI interest rates were deemed as PSI-treated, while those granted regular financial conditions served as their controls. The month of the first earmarked credit approval in 2009 defined the time of treatment for all firms. Companies that had loans with both PSI and regular financial conditions represented a challenge to our identification strategy, as their behavior can be a sign of self-selection. They were considered non-compliers and were excluded from the analysis to safeguard the internal validity of our estimations.

In addition, with detailed information regarding financial conditions, we used BNDES data to calculate two key variables for our exercise: current subsidies and accumulated subsidies. Both are measured in BRL per month and capture the amount of subsidies granted by the government through Finame loans. To properly explain those variables, it is worth understanding that every loan l is characterized by its principal (P_l), an interest rate paid by the borrower (r_{gl}, following the notation of our theoretical model), by a repayment term (t_{rl}), and by a grace period (t_{gl}).

For Finame loans, payments are monthly and, as usual, they are formed by two components: amortization and debt service. There is no amortization during the grace period, and after that, they are uniform and depend only on the loan amount and the total number of payments. This dynamic allows us to calculate the outstanding debt at any time t (D_{lt}) through Equation 10, and $\mathbb{1}$ is an indicator function equal to 1 when $t > t_{grace\ l}$ and 0 otherwise.

$$D_{lt} = LV_l - \sum_{k=0}^{t} \frac{P_l}{t_{tl} - t_{gl}}\ k\ \mathbb{1} \tag{10}$$

For any given outstanding debt, there is also a monthly debt service payment equivalent to $r_{gl}\ D_{lt}$. Beyond r_{gl}, our theoretical model had two other referential interest rates. The first is the

[14] Access to this data set was possible due to a formal request submitted by the authors and based on the Brazilian Transparency Law, which is equivalent to a Freedom of Information Act valid for all public organizations answering to the federal government.

[15] The classification scheme is defined by BNDES and, in 2009, the respective thresholds used to separate the categories were: up to BRL 1.2 million (Micro), between BRL 1.2 and BRL 10.5 million (Small), from BRL 10.5 million until BRL 60 million (Medium), and above BRL 60 million (Large). As a reference, at the time, 1 USD was roughly equivalent to 1.7 BRL.

counterfactual debt service that would be paid by the borrower for the same loan in the free-market banking sector ($r_{bl}\ D_{lt}$). The difference $(r_{gl} - r_{bl})\ D_{lt}$ can be defined as the total subsidy associated with the loan l at time t, but it cannot be calculated since we never observe r_{bl}. The second is the opportunity investment cost r, which is associated with a (adjusted) risk-free treasury bond that can be bought by all entrepreneurs in the economy. For the Brazilian case, as explained in Section 3, SELIC serves this purpose. Thus, $(r_{gl} - r)\ D_{lt}$ works as a measure of explicit subsidy, since it works as a stimulus for all companies (constrained or not) to get into an earmarked loan.

BNDES data provided detailed financial information, allowing us to calculate r_{gl} for any Finame loan approved between 2002 to 2012. For traditional Finame loans, r_g depends on an earmarked interest rate called TJLP, which is not fixed at its approval date. So, the final interest rate for these contracts varies during the repayment period - so we will have r_{glt}. However, since we were calculating the subsidies roughly 10 years after the period of analysis, we were able to use the actual TJLP. For PSI loans, as explained in Section 3, the final interest rates were fixed by the government. Thus, for those contracts, the debt service was perfectly known at the approval date - thus, for PSI loans, $r_{glt} = r_{gl}\ \forall\ t$. Finally, SELIC also varies with t, because the short-term treasury bond interest rate reacts to market conditions - thus we also have r_t. Since we are looking back in the past, SELIC is also a completely known interest rate.

In such a context, we define the current subsidies appropriated by a firm i at time t applying equation 11, where L_i represents all earmarked loans taken by a firm i. In practical terms, this measure assumes the effects of the subsidy vanish once the associated debt is paid. Thus, it is an interesting measure if we believe subsidies' impacts are short-lived.

$$CS_{it} = \sum_{l \in L_i} (r_{glt} - r_t)\ D_{lt} \tag{11}$$

On the other extreme, one can think the effects of an earmarked subsidy never vanish. In that scenario, all past subsidies received by a firm would simply accumulate over time. Thus, the treatment dose for this case (AS_{it}) can be calculated by Equation 12. For this paper, we had access to all Finame loans since 2002. Thus, both CS_{it} and AS_{it} take into account loans taken by our sample of firms from January 2002 until December 2011.

$$AS_{it} = \sum_{k \leq t} \sum_{l \in L_i} (r_{glk} - r_k)\ D_{lk} \tag{12}$$

The second important source was the Annual Social Information Report (RAIS), which is a rich employer-employee data set administered by the Brazilian Ministry of Economy. RAIS has been extensively explored for economic research in labor and development; see, for instance, Ulyssea (2018) and Dix-Carneiro and Kovak (2019). All formal businesses in Brazil are required to report

RAIS once a year as long as they remain active, and it gathers detailed information both on firms (such as tax identification, geographic location and main economic sector) and their labor force.

Workers are linked through time to their respective employers, and for each link RAIS also registers contractual wages, worked hours, time of employment, type of occupation, and educational level. Thus, we use RAIS to extract monthly firm-level series for the total number of employees, as well as their labor hours and payroll. Those measures will serve as proxies for the productive investment defined in our theoretical model (L_j). Reporting RAIS is mandatory, but there are entrants or dying companies, as well as non-compliers. Therefore, for all estimations using RAIS information, we can expect the balanced sample to differ from the unbalanced sample - which might cause $\hat{\delta}^{did}$ to diverge from $\hat{\delta}^{did-b}$.

The last source used for this research is the Credit Information Registry (SCR), which is administered by the Central Bank of Brazil (BCB). Brazilian financial regulations require all loans above a certain threshold to be reported on SCR. Even though this threshold has historically changed, at the time of this study it remained stable and equal to BRL 5,000 - which was roughly equivalent to USD 2,900. Although very small loans (typically associated with short-term working capital needs) might be outside of SCR coverage, the vast majority of corporate credit activity (and especially those associated with expansion and renovation) will be captured by it.[16] Due to this almost complete coverage of the credit market in Brazil, SCR has been widely used for banking and finance research – Bonomo et al. (2015), Fonseca and Van Doornik (2022) and Ornelas et al. (2022) are examples.

For this research, SCR provided the data on total debt, which represents the total amount borrowed by entrepreneurs in our model ($\bar{P}_j$).[17] The total debt was later divided into earmarked debt (the amount contracted by firms when using earmarked credit lines), and free-market debt (the amount borrowed using regular commercial credit lines) to explicitly evaluate possible crowding-out effects from one market to the other. SCR uses information sent monthly to the BCB by all formal lenders, and there are no missing observations, since the absence of a registry for a firm can be interpreted as the lack of any corporate debts with the formal lending sector. Thus, for regressions based on SCR information samples will be naturally balanced.

Both RAIS and SCR traced back information from January-2007 until December-2011, guaranteeing at least 24 months before and after the earmarked approval date for all firms in our sample. All information was merged using corporate tax-identification numbers that were available in the three sources. Finally, the earmarked credit approval date was then used to create 4 different samples, with treated and controls being defined according to expanding time intervals centered around July, 2009 – the month of the dawn of PSI. The most restricted case uses only firms that accessed earmarked loans between June and August. For this period, our previous exploratory analysis

[16] In fact, Finame data serve as support to this claim. In 2009, less than 0.5% of all loans granted in Finame were below this BRL 5,000 threshold.

[17] Considering the focus of our investigation, real state loans and personal credit lines (such as credit card accounts) were excluded from the analysis.

has shown no sign of self-selection, and thus we hope to have high inter-group comparability and previous parallel trends. The time intervals are then expanded to 5 months, 7 months, and finally for the entire year of 2009. This last sample will serve as a validation test for our identification strategy, because it will most likely be saturated with latecomers that should bias the estimation and violate the ex-ante comparability between groups.

Table 3. Basic descriptive statistics (all samples)

	Sample			
	3 months	5 months	7 months	All 2009
Firms				
Total	6473.00	11733.00	18409.00	34144.00
Share PSI	0.24	0.41	0.50	0.54
Share SMEs	0.96	0.96	0.96	0.95
Monthly employees				
Mean	55.90	60.57	60.16	69.18
Std. Error	311.15	336.38	327.79	400.45
Monthly payroll				
Mean	71.82	78.55	84.08	102.40
Std. Error	496.16	545.39	763.91	1101.63
Monthly working hours				
Mean	2529.11	2771.04	2847.99	3315.08
Std. Error	13952.57	15076.15	18698.51	22116.30
Total Earmarked loans				
Mean	389.06	412.16	412.64	517.37
Std. Error	798.35	791.72	734.52	1107.35
Monthly current subsidies				
Mean	1.38	1.47	1.38	1.65
Std. Error	11.18	10.64	10.07	13.33
Monthly accumulated subsidies				
Mean	26.48	29.45	28.24	30.26
Std. Error	213.86	212.02	205.08	226.39
Monthly total debt				
Mean	253.22	289.73	346.91	376.70
Std. Error	2333.68	3121.59	5660.39	5933.23

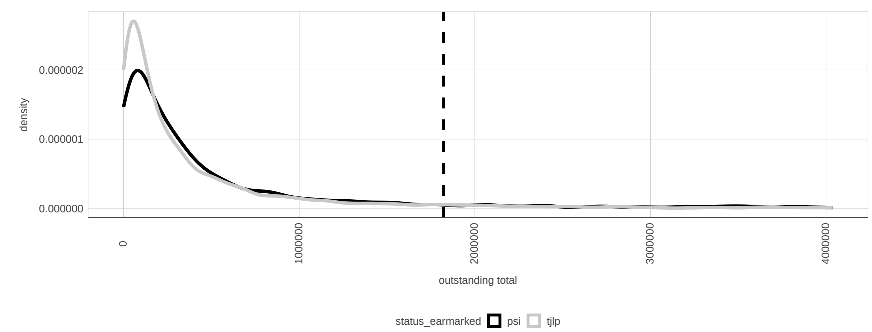
Source: Author's elaboration. Statistics consider the last 12 months before the earmarked loan. Monetary values are reported in BRL thousands.

Table 3 shows basic descriptive statistics for these four samples, considering the average values for the 12 months before the approval of the earmarked loan. First, it is interesting to note that the total number of firms increased almost six times from the three-month sample to the sample encompassing the entire year of 2009. Again, it is associated with the strong demand increase observed for Finame loans at the end of the year, especially in November and December.

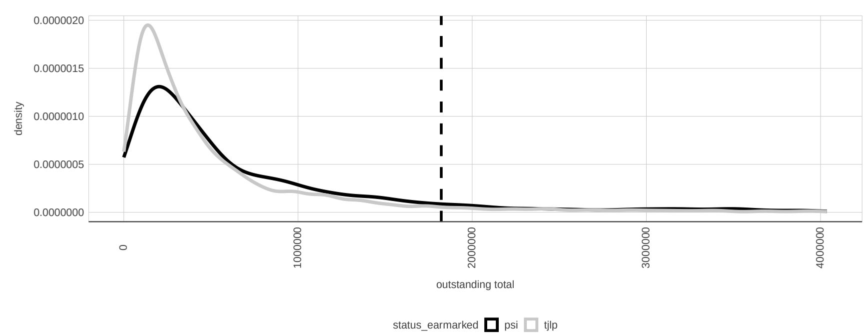
However, the sample is not only increasing, but it is also changing its composition. As expected, the share of loans with PSI conditions increases as we expand the time window. Moreover, the average size of a beneficiary firm seems to increase systematically as we expand the time intervals around the dawn of PSI. Comparing the less restricted (all 2009) with the most restricted (3-months) sample, we can see, for instance, that the average number of employees hired is roughly 24% larger. This behavior is not restricted to labor-related measures. The same happens with the amount of previous earmarked loans (a 33% increase), current subsidies (24%), accumulated subsidies (14%), and debt levels (16%).[18] This is testimony that the latecomers tended to be larger firms. This dynamic is compatible with a self-selection story, with larger firms taking advantage of PSI's improved interest rates to anticipate investments they would not do otherwise.

Figure 6. Density distribution for total debt according to treatment status

(a) Before earmarked loan



(b) After earmarked loan



Source: Authors' elaboration.
Note: The before(after) takes into account 12 months pre(post) the earmarked loan approval date. Dashed-black vertical lines represent mean values considering both groups (PSI and regular Finame) and both periods (before and after the earmarked loan).

Using only our preferred sample (three months), figures 6 and 7 show, for each treatment group, the density distribution for total debt and employees considering the twelve months before and after the Finame approval date.[19] Debt totals are extremely skewed, with a high density of firms holding fundamentally no debt before. After the earmarked loan, total debt levels increase for both

[18]The accumulated subsidies represent the sum of all subsidies received through Finame earmarked loans since 2002. In comparison, current subsidies consider only the amount of subsidies associated with current earmarked debts. More about those measures can be consulted in Section 5.5.

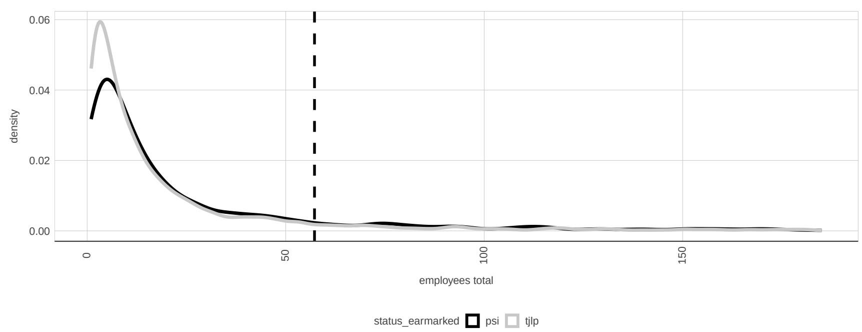
[19]The general appearance of the distributions are not that different for the other 3 samples.

groups, but those exposed to PSI show a slightly heavier tail. This suggests a larger debt expansion occurred among them.

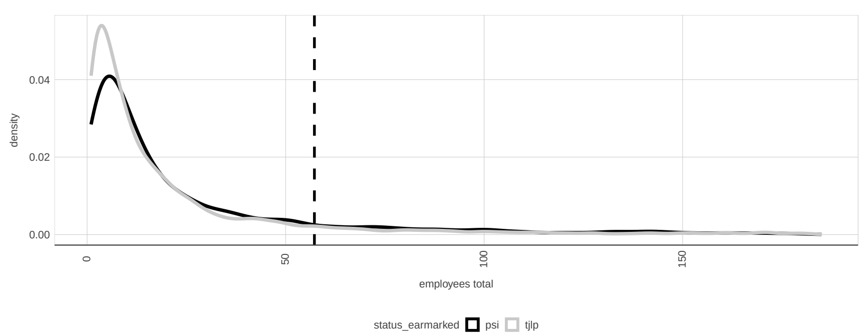
For both groups, the employment distributions are also skewed and concentrated in lower levels, reflecting the presence of small and micro firms (SMEs) among Finame borrowers. As observed for the debt levels, changes are mild. Both employment distributions seem to move similarly and, as a consequence, there is no clear sign of a larger (or smaller) labor contraction for any of the groups.

Figure 7. Density distribution for total employees according to treatment status

(a) Before earmarked loan



(b) After earmarked loan



Source: Authors' elaboration.
Note: The before(after) takes into account the first month before (pre) and the 12th month after (post) the earmarked loan approval date. The dashed black vertical lines represent the mean values considering both groups (PSI and regular Finame) and both periods (before and after the earmarked loan).

Overall, before and after distributions are fairly similar between groups, reinforcing the comparability argument that supports our identification strategy. Though existent, smaller ex ante differences between firms exposed to PSI or traditional-Finame conditions should be controlled for by firm fixed effects. Also, possible economic context effects will be exogenous to all companies and thus should be captured by the chronological-time dummies included in Equation (8).

4.3 Evaluating previous parallel trends

To evaluate the hypothesis of previous parallel trends, the proposed local DiD strategy was also estimated in its dynamic specification, where the treatment dummy interacts with dummies marking a firm's re-centered time period. This specification is defined by Equation 13, where α_t are relative-time dummies; and $PSI_{i,h}$ is equal to 1 if $h = t$ and firms are part of PSI beneficiaries. As usual, the last period prior to treatment ($t = -1$) is omitted for normalization purposes.

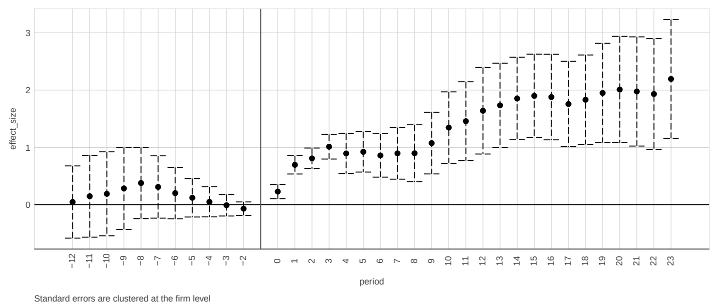
$$y_{i,t} = \alpha_i + \alpha_t + \alpha_m + \sum_{h=-12}^{h=-2} \delta_h \ \ PSI_{i,h} + \sum_{h=0}^{h=23} \delta_h \ \ PSI_{i,h} + \varepsilon_{i,t} \tag{13}$$

For selected variables, Figures 8 and 9 allow us to visually inspect previous parallel trends. They plot δ_h^{did} as estimated by Equation (10) for, respectively, the most restricted and the least restricted samples - graphs for all outcomes, estimators, and subsamples can be consulted in Appendix A. Considering only firms financed in the three-month window around the dawn of PSI, there are no statistically significant differences between treated and control groups in any of the twelve months before the earmarked loan. When we expand the sample to include all firms financed in 2009, past trajectories for earmarked debt and subsidies violate the assumption of previous parallel trends. This supports our initial suspicion that the latecomers were different from typical Finame clients.

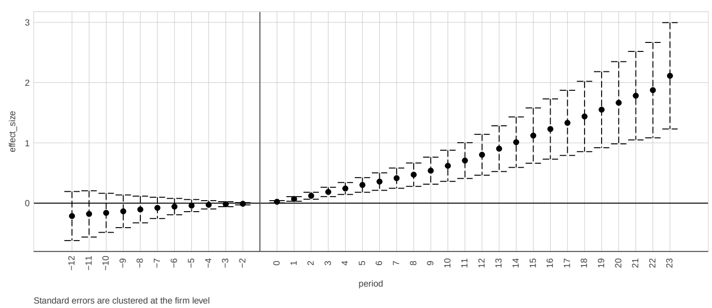
Table 4 provides a more comprehensive analysis of the hypothesis of previous parallel trends. For each outcome and each subsample, it reports the p-value for a Wald test evaluating the null hypothesis that δ_h is jointly equal to zero $\forall \ h < 0$. The effect of restricting the sample to the 3-months period around the dawn of PSI becomes crystal clear. For this more restricted sample, we do not reject the null hypothesis that all δ_h before the earmarked loan are jointly equal to zero. Starting with the five-month sample, we can notice relevant imbalances for the amount of subsidies (both in current and in accumulated terms) accessed by treated and control firms. From the 7-month period onward, we can also notice imbalances in terms of payroll and working hours. Important to say, the results are fundamentally the same whether we use the unbalanced sample ($\delta_h^{\hat{did}}$) or the balanced sample ($\delta_h^{\hat{did}-b}$).

Figure 8. $\hat{\delta_h^{did}}$ according to Equation (10) for the 3-months sample

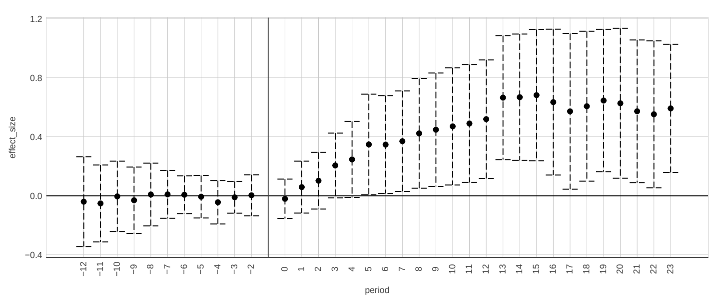
(a) Current subsidies



(b) Accumulated subsidies



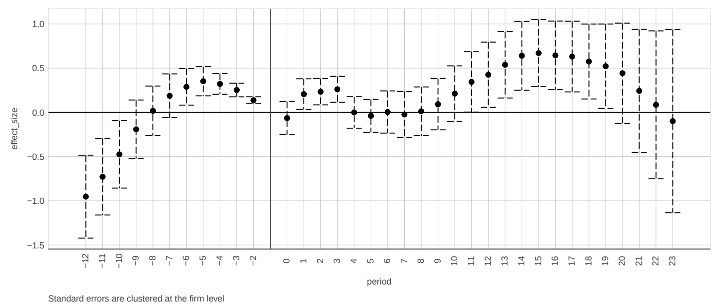
(c) Earmarked debt



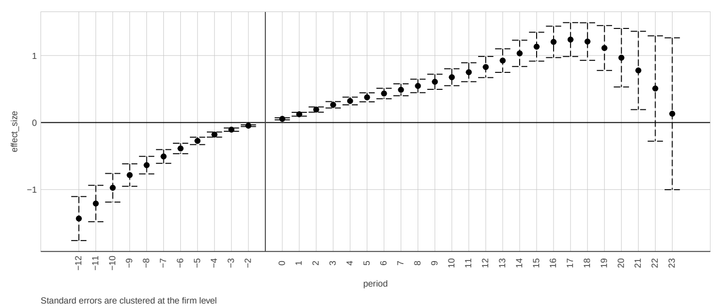
Source: Authors' elaboration.

Figure 9. $\hat{\delta_h^{did}}$ according to Equation (10) for the 2009 sample

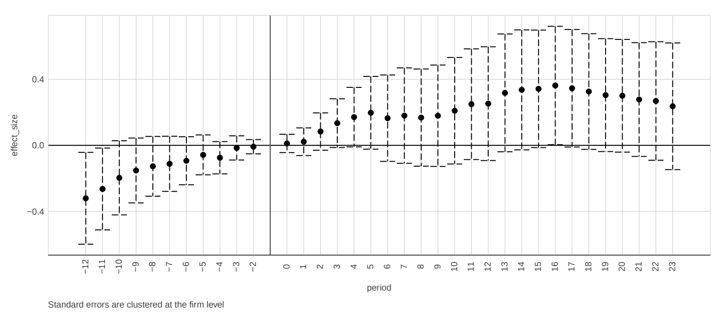
(a) Current subsidies



(b) Accumulated subsidies



(c) Earmarked debt



Source: Authors' elaboration.

Considering a 5% significance, imbalances arise starting with the five-months sample. It indicates we should rejected the null hypothesis of previous parallel trends for all samples with time ranges above three months, because in those cases past differences between treated and control groups trajectories would invalidate the causal identification supporting our local DiD. Imbalances arise early for the level of debt and subsidies. Since those variables are the key transmission mechanisms linking the dawn of PSI to the firm's future performance, results obtained for other variables should also be guided by our most restricted sample.

Table 4. Pre-exposure joint significance p-value ($\delta_h = 0 \; \forall \; h < 0$)

	3 months		5 months		7 months		All 2009	
Variable	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$
Current subsidies	0.730	0.730	0.043	0.043	0.199	0.199	0.000	0.000
Accumulated subsidies	0.862	0.862	0.000	0.000	0.000	0.000	0.000	0.000
Employees	0.829	0.907	0.383	0.640	0.182	0.382	0.185	0.203
Payroll	1.000	1.000	0.955	0.986	0.099	0.163	0.050	0.068
Working Hours	1.000	1.000	0.273	0.474	0.001	0.002	0.963	0.905
Earmarked Debt	1.000	1.000	1.000	1.000	0.997	0.998	0.019	0.082
Free-market debt	0.860	0.869	0.329	0.351	0.642	0.590	0.025	0.024

Source: Author's elaboration.

5 Results

We will use only our preferred three-month sample to delve deeper into the results. Equation (8) was estimated to investigate the impact of PSI on a firm's performance. Also, to further investigate possible time-heterogeneous impacts, estimations took into consideration three different ex-post periods: up to 12, 18, or 24 months after the earmarked approval date. Following the debate brought up by Moll (2014), we are particularly interested in evaluating whether the effects on labor-associated variables (when they happen) are sustained in the long term.

Tables 5, 6, and 7 summarise our main results for, respectively, $\delta^{\hat{did}}$, $\delta^{\hat{did}-b}$, and $\delta^{s\hat{did}}$. First, it should be noted the results are robust across different estimators, with, as expected, $\delta^{s\hat{did}}$ showing systematically smaller standard errors. Results support the claim that firms exposed to PSI financial conditions increased their subsidy intake and their earmarked debt level.

Using results from $\delta^{s\hat{did}}$ (Table 7) the monthly subsidy levels are roughly 51% higher among treated firms twelve months after the approval date, and this effect continues to increase until it reaches a 91% differential when we consider an ex post time window equal to 24 months. This result is not a complete surprise, since Finame loans have long terms (roughly 60 months). It seems natural that different financial conditions would have long-term impacts.

However, it could be the case that our control firms would try to compensate for their originally worst financial conditions by taking relatively more PSI loans in the future; it is worth remembering that PSI financial conditions were available until 2015 and our investigated time window reaches until the end of 2011. If that were the case, we would see a differential in terms of current subsidy levels vanishing. At least for up to two years after our treatment, it did not happen.

The levels of earmarked debt also increase (around 30%) and this difference persists up to two years after the loan. The average earmarked debt is approximately BRL 300K higher for a firm treated by PSI. This is consistent with the previous result in terms of subsidy intake and with the idea that Finame is not a high-frequency credit line. It seems firms defined their optimal earmarked

Table 5. Results for $\hat{\delta^{did}}$, estimated according to Equation (8)

Periods after loan	Outcome	Obs	Coef.		Std. Error	Effect size (%)
12 months	Current subsidies	155352	1.09	***	0.39	78.93%
	Accumulated subsidies	155352	10.96	***	4.04	41.39%
	Employees	136238	0.80		2.26	1.43%
	Payroll	136238	1.49		6.92	2.07%
	Working hours	136238	-34.32		134.23	-1.36%
	Earmarked debt	136504	228.35	*	126.85	28.15%
	Free-market debt	136504	60.83		116.39	7.22%
18 months	Current subsidies	194190	1.48	***	0.43	107.15%
	Accumulated subsidies	194190	17.37	***	5.13	65.58%
	Employees	171351	1.63		3.10	2.92%
	Payroll	171351	3.99		8.91	5.56%
	Working hours	171351	-5.41		182.45	-0.21%
	Earmarked debt	174980	334.46	**	146.01	41.23%
	Free-market debt	174980	206.85		180.07	24.55%
24 months	Current subsidies	233028	1.74	***	0.47	125.39%
	Accumulated subsidies	233028	24.88	***	6.50	93.94%
	Employees	206009	2.21		3.48	3.95%
	Payroll	206009	10.96		10.86	15.26%
	Working hours	206009	126.63		217.07	5.01%
	Earmarked debt	213361	372.12	**	162.05	45.87%
	Free-market debt	213361	284.87		220.75	33.81%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by average outcome value for the untreated units at the last month before the earmarked loan approval date.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

debt levels considering financial conditions at the time of the loan, and once they do, effects remain in the long run.

The estimated results for free-market debt indicate there is no sign of crowding-in or crowding-out effects. So, it seems earmarked loans were not used to replace other banking sources, even though they could still be replacing companies' internal funding. This result is consistent with the idea of funding specialization in Brazil, with free-market loans that basically serve as working capital. In addition, it shows how firms responded to the financial conditions associated with each funding source.

Finally, there is no noticeable impact when it comes to a firm's demand for labor. The number of employees and monthly payroll are systematically positive, but without statistical significance. The effect sizes are only a glimpse of the magnitudes observed for subsidies and debt levels. Contrasting these results with our theoretical model suggests PSI was mostly used by unconstrained companies, which took advantage of the subsidized financial conditions to increase their (earmarked) debt intake but did not use it to expand their economic activity. Thus, the general history appears to corroborate the findings of Bonomo et al. (2015) and Lazzarini et al. (2015).

Table 6. Results for $\delta^{\widehat{did}-b}$, estimated according to Equation (8)

Periods after loan	Outcome	Obs	Coef.		Std. Error	Effect size (%)
12 months	Current subsidies	155,352	1.09	***	0.39	71.51%
	Accumulated subsidies	155,352	10.96	***	4.04	81.36%
	Employees	120,960	1.31		2.44	2.36%
	Payroll	120,960	1.57		7.51	2.51%
	Working hours	120,960	-21.05		145.43	-0.86%
	Earmarked debt	105,504	252.91	*	147.32	33.72%
	Free-market debt	105504	61.79		136.22	5.25%
18 months	Current subsidies	194,190	1.48	***	0.43	103.03%
	Accumulated subsidies	194,190	17.37	***	5.13	130.77%
	Employees	148,950	2.10		3.44	3.80%
	Payroll	148,950	4.06		9.82	6.70%
	Working hours	148,950	2.04		201.84	0.08%
	Earmarked debt	131,550	367.34	**	168.98	49.93%
	Free-market debt	131550	227.51		208.90	19.24%
24 months	Current subsidies	233,028	1.74	***	0.47	125.75%
	Accumulated subsidies	233,028	24.88	***	6.50	193.41%
	Employees	174,888	2.36		3.93	4.26%
	Payroll	174,888	10.81		12.11	18.23%
	Working hours	174,888	129.36		242.70	5.32%
	Earmarked debt	157,356	404.95	**	187.43	56.51%
	Free-market debt	157,356	312.77		255.49	26.19%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by average outcome value for the untreated units at the last month before the earmarked loan approval date.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 7. Results for $\delta^{\hat{sdid}}$, estimated according to Equation (8)

Periods after loan	Outcome	Obs	Coef.		Std. Error	Effect size (%)
	Current subsidies	90580	1.21	***	0.20	51.41%
	Accumulated subsidies	84149	8.31	***	1.77	83.75%
	Employees	80640	0.72		2.27	1.28%
12 months	Payroll	75600	0.35		5.62	0.70%
	Working hours	70560	-32.16		104.77	-1.56%
	Earmarked debt	61544	238.87	*	132.85	29.09%
	Free-market debt	61544	31.47		114.61	2.68%
	Current subsidies	129380	1.59	***	0.27	74.27%
	Accumulated subsidies	122987	14.02	***	2.95	145.31%
	Employees	104265	1.58		2.90	2.71%
18 months	Payroll	104265	2.84		7.70	5.70%
	Working hours	104265	-8.93		145.64	-0.43%
	Earmarked debt	92085	346.18	**	151.52	42.86%
	Free-market debt	87700	183.14		183.44	16.62%
	Current subsidies	168220	1.84	***	0.32	91.00%
	Accumulated subsidies	161825	20.88	***	4.32	230.91%
	Employees	131166	1.79		2.89	3.08%
24 months	Payroll	131166	9.75		9.77	18.92%
	Working hours	131166	119.22		188.02	5.58%
	Earmarked debt	118017	381.24	**	165.27	50.39%
	Free-market debt	113646	267.48		229.06	24.69%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by he the average outcome value for the untreated units in the last month before the earmarked loan approval date. For the SDiD, 'Obs' reflects the number of observations with non-zero weights after optmization.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

5.1 Placebo exercises

To further test the robustness of our results, two placebo exercises were implemented. The first replicated the entire exercise around a placebo treatment date. That is, we built a new sample of firms, using the same three-month range, but now centered around July 10^{th} of 2008. This date was selected because it is exposed to the same seasonal effects that could affect the demand for Finame loans at the dawn of PSI. In this estimation, those whose earmarked loan was approved within the time range but before (after) the placebo date, were defined as controls (treated). By doing this, we can assess whether results from previous sections are caused by some specific seasonal dynamic associated with Finame loans that were not properly controlled for with our chronological time dummies (α_m).

The second placebo exercise replicated our main strategy, but the treatment date is lagged by 12 months. In practical terms, this alternative specification uses the same group of treated and control firms, as defined by their true exposure to PSI financial conditions in 2009, but their trajectories are re-centered exactly one year before their original earmarked approval date.[20] By doing this, we can investigate if our key results were driven not by the price mechanism of PSI, but by a long-term differential in terms of treatment and control firms' trajectories.

Both of our placebo exercises were estimated twelve months before and after the false treatment date, and we expect to observe previous parallel trends and no statistically significant impact. Table 8, which is a replica of Table 4, confirms the first of our expected results. None of the estimations associated with the placebo exercises shows any sign of imbalances before the proposed fake treatment dates - all p-values are way above the 5% confidence level. This result indicates our identification strategy seems to hold for different time periods and that firms applying for Finame loans around the same date are comparable.

Table 8. Pre-exposure joint significance ($\delta_h = 0 \; \forall \; h < 0$)

	Placebo 1 (2008 sample)		Placebo 2 (original sample)	
Variable	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$	$\hat{\delta}_h^{did}$	$\hat{\delta}_h^{did-b}$
Current subsidies	1.000	1.000	0.731	0.731
Accumulated subsidies	0.982	0.982	0.982	0.982
Employees	0.876	0.996	0.656	0.501
Payroll	0.912	0.891	0.943	0.908
Working Hours	0.998	0.998	0.445	0.361
Earmarked debt	0.867	0.873	0.719	0.903
Free-market debt	0.856	0.847	0.945	0.965

Source: Author's elaboration.

Table 9 summarizes results considering our SDiD estimator, while results for $\delta^{\hat{did}}$ and $\delta^{\hat{did-b}}$ can be observed in Appendix C. As expected, we see no relevant impact in terms of debt or subsidies level. Although we see a statistically significant coefficient associated with total working hours for

[20] Formally, while our identification model sets $t = 0$ at the earmarked loan approval date, this first placebo model sets $t = 0$ twelve months earlier.

our first placebo exercise, this is not supported by payroll or total employees. Also, as can be seen in Appendix C, the results for $\delta^{\hat{did}}$ and $\delta^{\hat{did-b}}$ suggest no statistically significant effects on working hours. Thus, when it comes to labor-related outcomes, we also see no consistent impact. In general, this set of results reinforces the claim that the rise of PSI was the key transmission mechanism behind the impacts observed in our main set of estimations.

Table 9. Results for $\delta^{\hat{sdid}}$, considering placebo exercises

Placebo Exercise	Outcome	Obs	Coef.		Std. Error	Effect size (%)
Sample 2008	Current subsidies	107565	-0.28		0.30	-16.68%
	Aggregagted subsidies	93223	-2.11		2.20	-14.45%
	Employees	91040	-0.51		1.71	-0.61%
	Payroll	73970	-3.97		5.49	-5.55%
	Working hours	79660	-337.42	**	170.76	-9.97%
	Earmarked debt	75615	-31.10		60.51	-2.97%
	Free-market debt	70574	-17.34		124.66	-1.23%
Sample 2009	Current subsidies	103552	0.14		0.11	17.22%
	Aggregagted subsidies	84149	1.05		4.19	300.60%
	Employees	73392	0.53		1.69	1.06%
	Payroll	68805	0.53		3.19	0.81%
	Working hours	64176	-4.37		83.33	-0.17%
	Earmarked debt	46358	16.66		75.27	1.67%
	Free-market debt	49924	-173.58		144.78	-15.99%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by the average outcome value for the untreated units in the last month before the earmarked loan approval date. For the SDiD, 'Obs' reflects the number of observations with non-zero weights after optmization.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

5.2 Heterogeneous impacts of PSI: SMEs vs Large companies

As mentioned in the Introduction, the works of Bonomo et al. (2015) and Lazzarini et al. (2015), on one side, and Cavalcanti and Vaz (2017), on the other side, have important differences in terms of research design. The last one studied the impact of Finame loans for micro and small firms (those with an annual gross revenue between BRL 6 million and BRL 10.5 million), because their identification strategy did not allow them to extrapolate results for larger companies. On the other hand, the first two works concentrated the bulk of their analysis on larger firms, basically because their samples were restricted to publicly traded companies.

Unlike previous studies, our sample covers a broad spectrum of firms and our empirical strategy can be used to measure differential impacts for small and micro firms (SMEs) and mid-large companies. To do so, we reestimated Equations (8) and (9) splitting firms into two groups. The first encompasses firms that reported, for the year before the loan request, a maximum annual gross revenue of BRL 10.5 million. This group is closer to the one evaluated by Cavalcanti and Vaz (2017). The second group is formed by medium and large firms whose annual maximum gross revenue was above that threshold. This last group is closer to the beneficiaries investigated by Bonomo et al. (2015) and Lazzarini et al. (2015).

This group separation will also allow us to better understand the transmission mechanism. There is a vast economic literature showing smaller firms are disproportionately affected by credit constraints (Beck and Demirguc-Kunt, 2006). Thus, if credit constraints are the key causal channel linking PSI reduced interest rates to firm performance, we could expect to see a clear impact on smaller firms' demand for labor and debt. On the other hand, since larger firms typically have alternative funding sources, we could expect a weaker PSI effect in terms of their economic activity. However, they should still take advantage of PSI financial conditions to increase their subsidy intake by marginally increasing debt or by replacing older (more expensive) debt.

Table 10. Results for $\hat{\delta^{sdid}}$, considering only SMEs

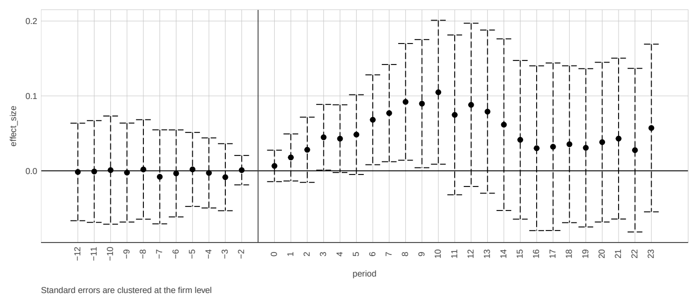
Periods after loan	Outcome	Obs	Coef.		Std. Error	Effect size (%)
12 months	Current subsidies	77644	0.47	***	0.06	62.13%
	Accumulated subsidies	72098	3.11	***	0.46	49.35%
	Employees	58464	1.05	**	0.41	6.98%
	Payroll	58464	1.25	***	0.41	11.08%
	Working hours	66816	44.02	**	18.00	6.87%
	Earmarked debt	49952	63.97	***	12.82	37.69%
	Free-market debt	57088	-7.31		9.46	-3.39%
18 months	Current subsidies	110920	0.55	***	0.08	76.79%
	Accumulated subsidies	105374	4.76	***	0.73	75.54%
	Employees	82160	1.03	*	0.53	6.85%
	Payroll	78052	1.15	**	0.53	10.18%
	Working hours	82160	34.54		23.79	5.35%
	Earmarked debt	78298	89.44	***	14.98	56.87%
	Free-market debt	74739	-13.70		12.46	-6.64%
24 months	Current subsidies	144196	0.57	***	0.09	80.98%
	Accumulated subsidies	138650	6.60	***	1.07	105.57%
	Employees	104286	0.85		0.59	5.78%
	Payroll	104286	1.03		0.65	9.48%
	Working hours	108297	24.07		27.39	3.77%
	Earmarked debt	99288	107.74	***	17.29	75.10%
	Free-market debt	95742	-21.06		14.85	-10.47%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by the average outcome value for the untreated units in the last month before the earmarked loan approval date. For the SDiD, 'Obs' reflects the number of observations with non-zero weights after optmization.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

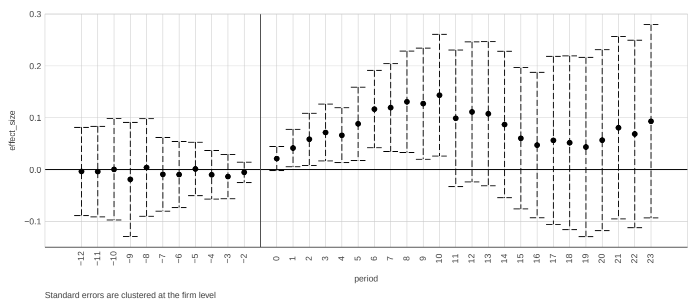
Table 10 summarizes the results of the canonical model for SMEs. The impacts on subsidies and debt intake are positive, statistically significant, and persistent in the long term. The magnitudes of the coefficients are slightly higher than what we have seen for the overall sample. The current level of subsidies is approximately 70% higher and, 24 months later, the earmarked debt increases by 75%. This outcome is consistent with our theoretical prediction that the price mechanism triggered by PSI would have stronger impacts among previously credit-constrained companies. However, the major change appears when we analyze results for labor-related outcomes.

Figure 10. $\hat{\delta_h^{sdid}}$ on SMEs, according to Equation (9) for the 3-months sample

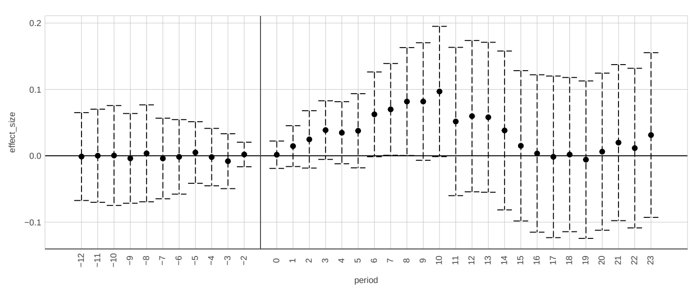
(a) Employees



(b) Payroll



(c) Working hours



Note: To allow previous trends, $\hat{\delta_h}^{sdid}$ shown here uses only $\hat{\omega_i}^{sdid}$.
Source: Authors' elaboration.

Up to twelve months after treatment, smaller firms showed an increase in the number of employees (6.98%), working hours (6.87%) and payroll (11.08%). This outcome indicates SMEs treated by PSI conditions mostly used better financial conditions to increase debt intake and expand their economic activity levels faster than their counterfactuals, as we would expect from credit-constrained companies. Also, this dynamic corroborates results found previously by Cavalcanti and Vaz (2017), and shows the intensive margin can help SMEs grow faster.

However, this impact seems to disappear in the long run, and differences between PSI and traditional Finame companies are statistically insignificant after 24 months. This short-lived dynamics can be seen in Figure 10. The differential between PSI and Finame groups starts to increase just after the earmarked loan approval date, and it reaches its peak roughly 10 months after that. Then, $\hat{\delta_h}^{sdid}$ coefficients start to decrease, and approximately 18 months after the loan we cannot see any relevant impact in terms of labor-related outcomes.

Moll (2014) seems to offer a reasonable explanation for that temporary effect: the price mechanism accelerates convergence towards optimal investment level, but it does not change it. After all, our control firms are also able to fund their investment, even if they need to use more of their profits. Of course, it might be critical when the time of convergence is way longer, and the literature brings little evidence on that matter. Our exercise suggests the strong reduction in earmarked interest rates was only able to accelerate convergence by roughly one year. Thus, the effectiveness of the price mechanism in alleviating credit constraints seems to be modest, at least when they are operating near or below the investment opportunity cost of an economy.

Table 11. Results for $\delta^{\hat{sdid}}$, considering only medium and large companies

Periods after loan	Outcome	Obs	Coef.		Std. Error	Effect size (%)
12 months	Current subsidies	12978	4.78	***	1.07	47.35%
	Accumulated subsidies	12051	21.81	***	7.71	140.50%
	Employees	13824	-4.34		10.87	-1.89%
	Payroll	12960	-18.72		27.63	-8.37%
	Working hours	12960	-712.17		544.00	-8.37%
	Earmarked debt	11592	819.77		625.75	23.82%
	Free-market debt	11592	-162.40		523.15	-3.40%
18 months	Current subsidies	18540	6.36	***	1.42	69.66%
	Accumulated subsidies	17613	41.62	***	13.93	225.96%
	Employees	17997	0.23		14.18	0.10%
	Payroll	17997	-10.26		37.98	-4.53%
	Working hours	17997	-689.66		772.61	-8.01%
	Earmarked debt	17346	1203.02	*	718.84	35.21%
	Free-market debt	16520	534.12		840.65	12.10%
24 months	Current subsidies	24102	7.57	***	1.70	89.21%
	Accumulated subsidies	23175	66.22	***	21.32	341.83%
	Employees	22869	1.47		13.96	0.61%
	Payroll	22869	19.40		48.19	8.15%
	Working hours	22869	-39.33		968.79	-0.43%
	Earmarked debt	22275	1248.58		787.88	38.78%
	Free-market debt	21450	920.81		1049.38	21.37%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by the average outcome value for the untreated units in the last month before the earmarked loan approval date. For the SDiD, 'Obs' reflects the number of observations with non-zero weights after optmization.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

For medium and large companies, results can be seen in Table 11. There is a noticeable impact on subsidy intake, but no relevant impact in terms of debt or labor demand from PSI-benefited firms. For earmarked debt, it is worth noting that coefficients are systematically positive, but their magnitudes are roughly 60% of what we have estimated for SMEs. It indicates that on average, medium and large firms were not using earmarked loans to increase their debt levels, but they might be using it to replace older and more expensive credit lines.

Results in terms of labor demand are very different from what we have observed for SMEs. Coefficients oscillate between negative and positive values, with no consistent pattern. Coupled with the debt outcome, it seems to indicate the price mechanism did not affect medium and large companies' economic activity. As our theoretical model predicts, they have taken advantage of the improved financial conditions - as the results for subsidy intake show - but they have not changed their growth trajectory.

5.3 A cost-effectiveness analysis of PSI

On one side, the effects on labor outcomes seem to be short-lived among SMEs and nonexistent among larger companies. On the other hand, the increase in subsidies-intake is widespread and can be observed up to two years after the loan approval. Together, these elements raise an important question for a typical policymaker: Was PSI cost-effective?

The dynamic coefficients previously estimated can be used to delve deeper into this question. From a policy perspective, the increase in total payroll, because of the increased economic activity among SMEs beneficiaries, appears to be the main positive externality associated with PSI. The key cost was the fiscal cost associated with the subsidies that were transferred to PSI borrowers.

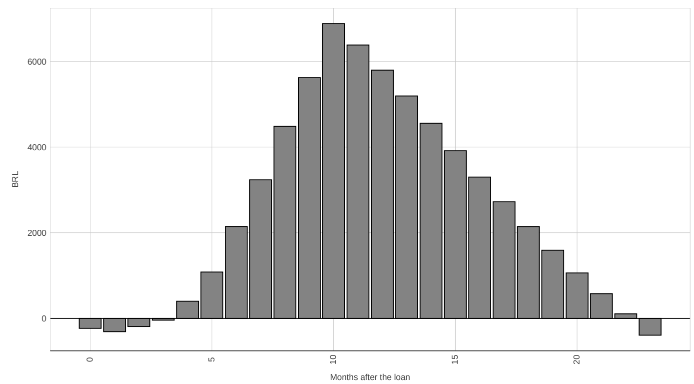
The coefficients reported in figures 19 and 22, for SMEs, and 26 and 29, for larger companies, can be used to evaluate the net benefit of PSI for each month after loan approval. To do so, we subtract the monthly amount spent by the Government on subsidies from the increase in wages induced by the policy. In both cases, non-statistically significant coefficients were treated as null for the purpose of the cost-effectiveness analysis.

Since the results are heterogeneous, the analysis was performed for each category of firm size and then aggregated using the share of SMEs and larger companies. This cost-effectiveness analysis of PSI was performed using the three-month identification window and 24 months after the events. Results can be observed in Figure 11.

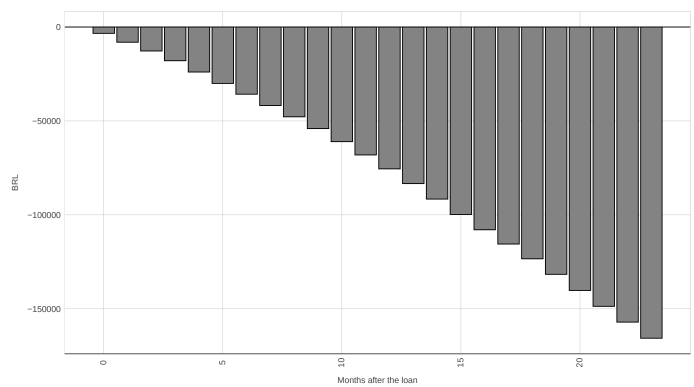
Among SMEs (Figure 11(a)), the average net benefit increases during the first ten months, following the behavior observed for the payroll coefficients. It peaks at BRL 6,883 exactly ten months after the loan approval and then decreases continuously. At the end of our studied period (24 months), even for SMEs, the estimated average net benefit is negative in BRL 393. This means the monthly subsidies were on average 393 BRL higher than the wage increases.

Figure 11. Cost-effectiveness analysis of PSI

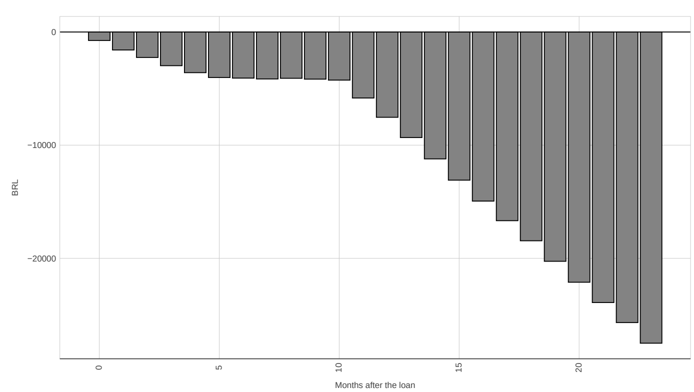
(a) SMEs



(b) Larger



(c) All



Note: The analysis uses dynamic coefficients estimated for payroll and current subsidies to measure, respectively, the benefits and costs associated with PSI. Coefficients that were not statistically significant were considered equal to zero. All net benefits were deflated to the July 2009 prices using the Brazilian consumer price index (IPCA).
Source: Authors' elaboration.

Since there is no statistically significant impact on the payroll of larger companies, the average net benefit for this group is negative and decreasing throughout the period, reaching minus BRL 165,685 two years after the loan (Figure 11(b)). Thus, the average monthly net benefit of a PSI loan was estimated to be negative by BRL 27,502 (Figure 11 (c)).

With the average cost-effectiveness estimated for PSI loans, Table 12 calculates the net benefits of the program considering only loans from 2009.[21] As a whole, the cost of PSI was equivalent to roughly BRL 511 million - which represents 6.78% of the total loan volume of 2009. Most of the cost comes from loans with larger companies. They represent only 16,40% of all beneficiaries, but the cost associated with them is equivalent to BRL 505 million (or roughly 98% of all PSI's costs).

Table 12. Total net benefit estimated for PSI loans in 2009

	Beneficiaries		Loans		Net benefit		
Firm category	Number	Share	Value	Share	Average	Total	Ratio
SMEs	15,537	83.60	3,587,310	47.61	-0.39	-6,115	-0.17
Larger	3,048	16.40	3,947,216	52.39	-165.69	-505,009	-12.79
All	18,585	100.00	7,534,526	100.00	-27.50	-511,124	-6.78

Note:
All monetary values are reported in BRL thousands. The total net benefit is equivalent to the the average net benefit multiplied by the number of firms that accessed PSI. The ratio is equivalent to the total net benefit divided by the total loan volume.

Evidently, this cost-effectiveness analysis does not take into account the totality of benefits associated with PSI, since it does not capture general equilibrium impacts from the increased economic activity. For example, it is possible to argue tax revenues would increase as a response to increased labor demand. Still, from a policy perspective, these partial equilibrium results are troublesome as they indicate a strong negative impact of PSI on society's gains.

6 Concluding remarks

The empirical setup of this paper allows us to disentangle the effects of credit subsidy intensity from those of credit access. This is accomplished by analyzing an unexpectedly sharp reduction in the interest rates from a subsidized credit program without any relevant change in their distribution channels. This allowed us to answer one relevant policy question: does the effectiveness of earmarked credit policies depend on their subsidies?

We found evidence the subsidy level matters, but not for all companies. For larger ones, more subsidies did not lead to an increase in economic activity - measured by overall debt levels and demand for labor. For this set of firms, the subsidized interest rates induced funds arbitrage but had no relevant result for their growth trajectory - as previously found by Lazzarini et al. (2015) and Bonomo et al. (2015).

[21] It is worth mentioning PSI remained in place until the end of 2015. However, financial conditions were later altered several times, so it seems unreasonable to extrapolate results estimated here for the entirety of PSI.

For small and micro firms, the increase in the intensity of subsidies promoted an expansion of debt intake and labor demand - a result consistent with the findings of Cavalcanti and Vaz (2017). Although effects on debt levels were sustained for at least two years after the loan approval, impacts on firms' demand for labor were short-lived. It started to reduce after roughly one year of the loan, and around two years after we could not observe any statistically significant effects.

The strong heterogeneity between SMEs and larger firms indicates the effectiveness of earmarked policies is highly dependent on their capacity to target credit-constrained firms. Still, even when they do reach the targeted population, the effect is to shorten the convergence speed to the optimal activity level without huge changes in the firm's long-run trajectory.

With short-lived benefits and long-term sustained costs, policymakers should be extremely cautious about using the price mechanism to boost the effectiveness of earmarked credit policies. Our cost-effectiveness analysis suggests PSI loans during 2009 represented a net loss of income equal to BRL 511 million - which is equivalent to 6,78% of the total loan amount. Despite being only 16,4% of the total number of beneficiaries, larger companies account for 98% of this total loss. However, even when the loans reached SMEs, their net benefits were still negative.

These findings reinforce the importance of policymakers focusing on earmarked credit policies that can effectively change the way funds are directed by the financial system. Access seems to be more important than price. Simply increasing the subsidies associated with *status quo* banking distribution channels seems to be, in the best-case scenario, a policy with low cost-effectiveness. In the worst-case scenario, which seems to be the case when larger companies (typically credit-unconstrained) capture the subsidies, the policy might lead to a relevant welfare loss for society.

To avoid the worst-case scenario, policymakers need to carefully consider firms' characteristics when increasing the subsidies attached to earmarked credit policies. Targeting credit-constrained companies is crucial for the policy to have any relevant effects. Since this is not a trivial task, focusing on policies such as innovative public guarantee schemes or new credit risk analysis technologies might be a more effective approach.

7 Appendix A: Dynamic models for different estimators (all firms)

Figure 12. Current subsidies

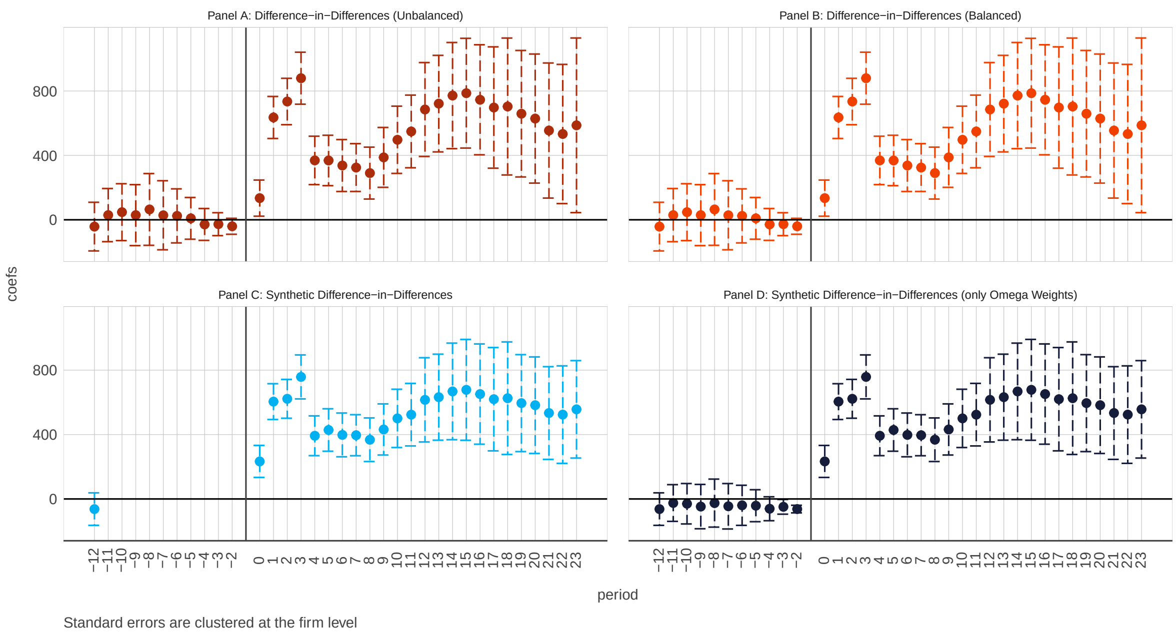


Figure 13. Accumulated subsidies

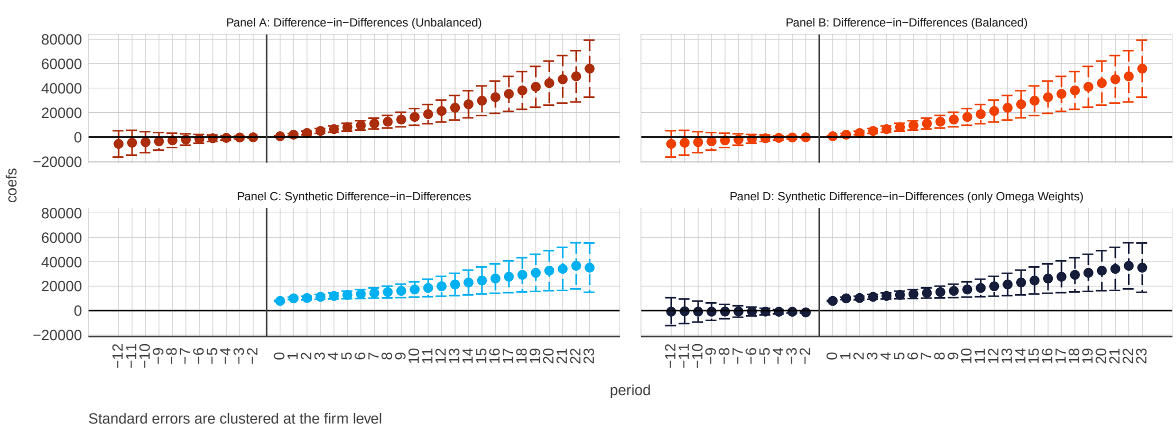
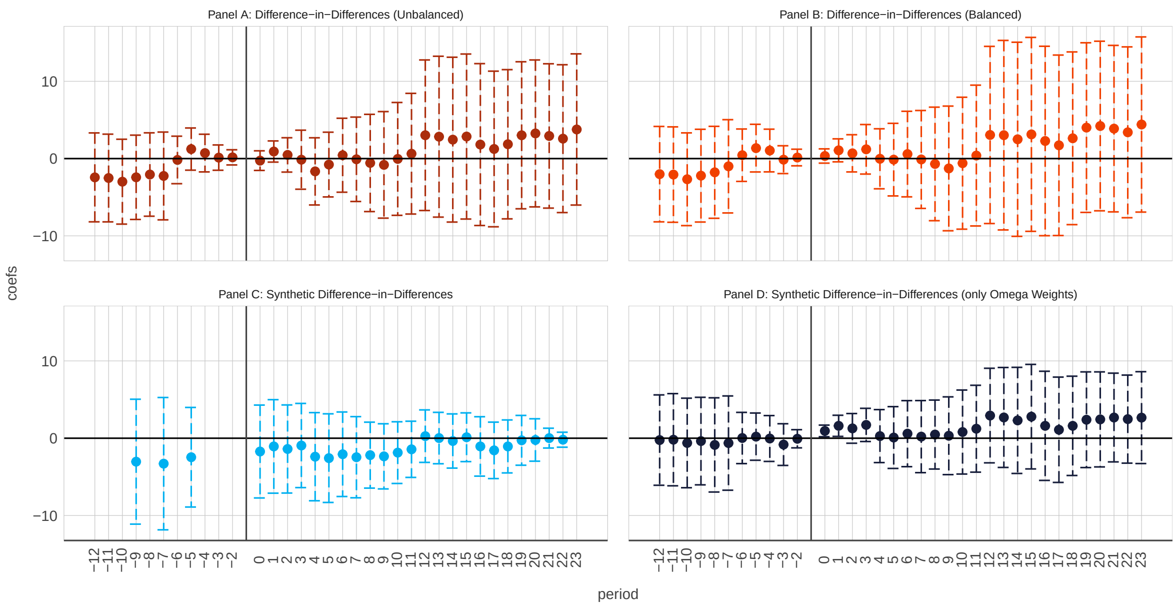
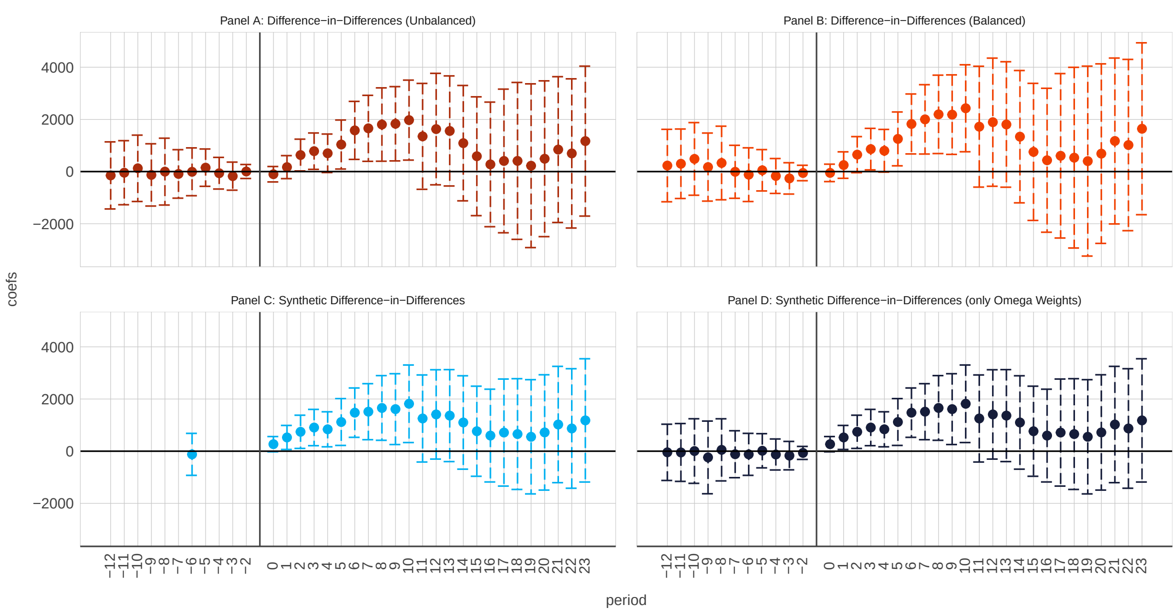


Figure 14. Employees



Standard errors are clustered at the firm level

Figure 15. Payroll



Standard errors are clustered at the firm level

Figure 16. Working hours

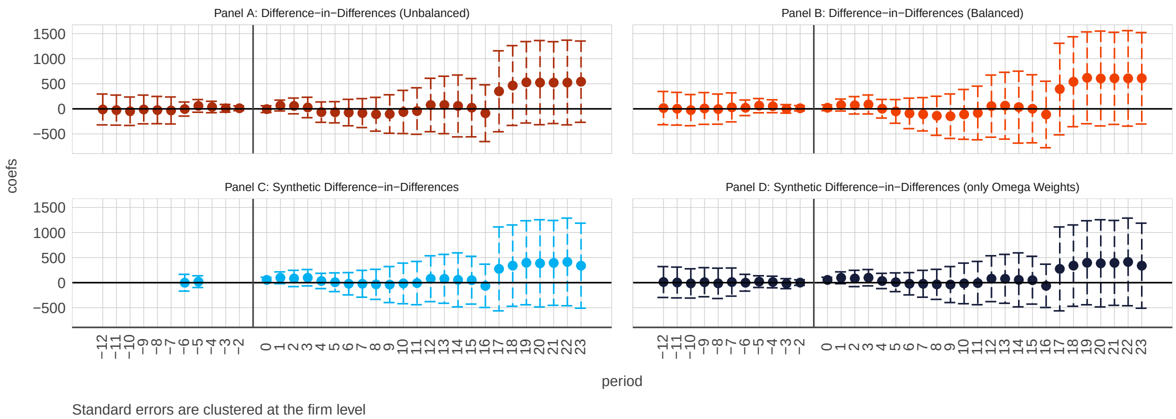


Figure 17. Earmarked debt

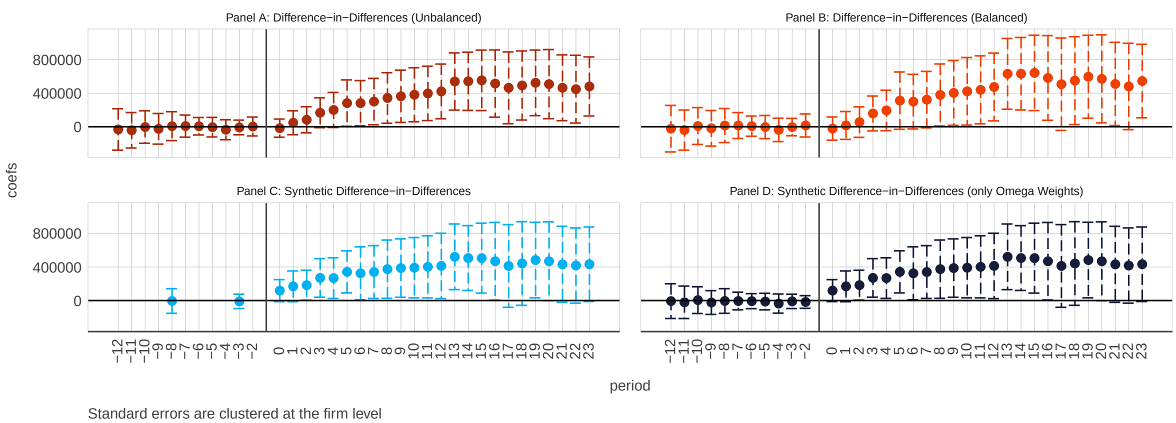
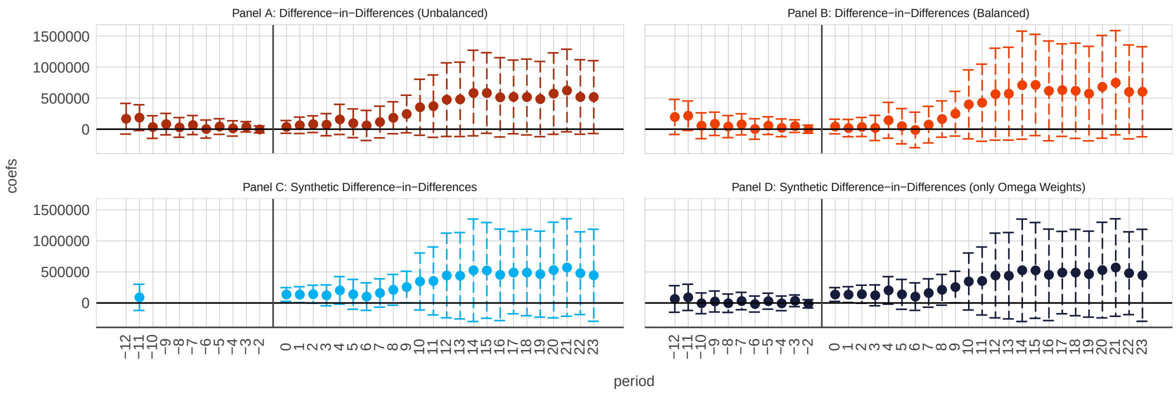


Figure 18. Free-market debt



Standard errors are clustered at the firm level

8 Appendix B: Placebo estimations

Table 13. Results for $\hat{\delta^{did}}$, considering placebo exercises

Placebo Exercise	Outcome	Obs	Coef.	Std. Error	Effect size (%)
Sample 2008	Current subsidies	172104	-0.28	0.42	-19.43%
	Aggregagted subsidies	172104	-4.16	4.43	-23.08%
	Employees	152704	-1.66	2.93	-1.93%
	Payroll	152704	-19.19	20.32	-15.30%
	Working hours	152704	-551.64	584.51	-12.51%
	Earmarked debt	153991	-37.60	95.81	-4.73%
	Free-market debt	153991	87.65	247.96	8.19%
Sample 2009	Current subsidies	155352	0.12	0.17	12.26%
	Aggregagted subsidies	155352	5.46	6.53	51.85%
	Employees	127773	0.49	1.98	0.89%
	Payroll	127773	-0.11	4.00	-0.18%
	Working hours	127773	-47.04	103.56	-1.92%
	Earmarked debt	114276	76.82	130.37	11.63%
	Free-market debt	114276	-43.17	142.19	-6.65%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by the average outcome value for the untreated units at the last month before the earmarked loan approval date.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 14. Results for $\hat{\delta^{did-b}}$, considering placebo exercises

Placebo Exercise	Outcome	Obs	Coef.	Std. Error	Effect size (%)
Sample 2008	Current subsidies	172104	-0.00	0.00	-16.42%
	Aggregagted subsidies	172104	-0.00	0.00	-39.79%
	Employees	136560	-0.73	3.14	-0.87%
	Payroll	136560	-0.02	0.02	-17.23%
	Working hours	136560	-552.56	634.05	-14.14%
Sample 2009	Current subsidies	155352	0.00	0.00	16.88%
	Aggregagted subsidies	155352	0.01	0.01	157.45%
	Employees	110088	0.16	2.19	0.30%
	Payroll	110088	-0.00	0.00	-1.30%
	Working hours	110088	-67.93	114.94	-2.71%

Note:
Robust standard errors are clustered at the firm level. Effect size is equal to the estimated coefficient divided by average outcome value for the untreated units at the last month before the earmarked loan approval date. For the SDiD, 'Obs' reflects the number of observations with non-zero weights after optmization.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

9 Appendix C: Heterogeneous impacts

9.1 SMEs

Figure 19. Current subsidies

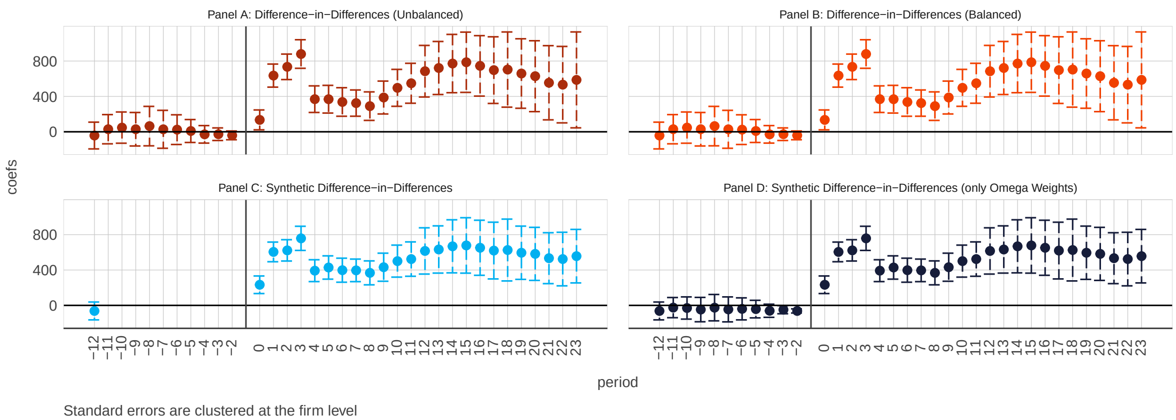


Figure 20. Accumulated subsidies

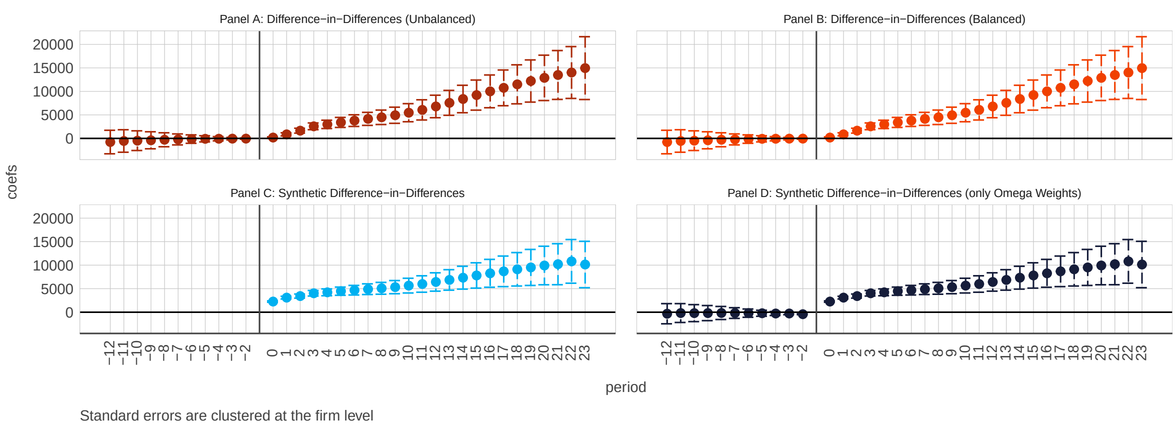


Figure 21. Employees

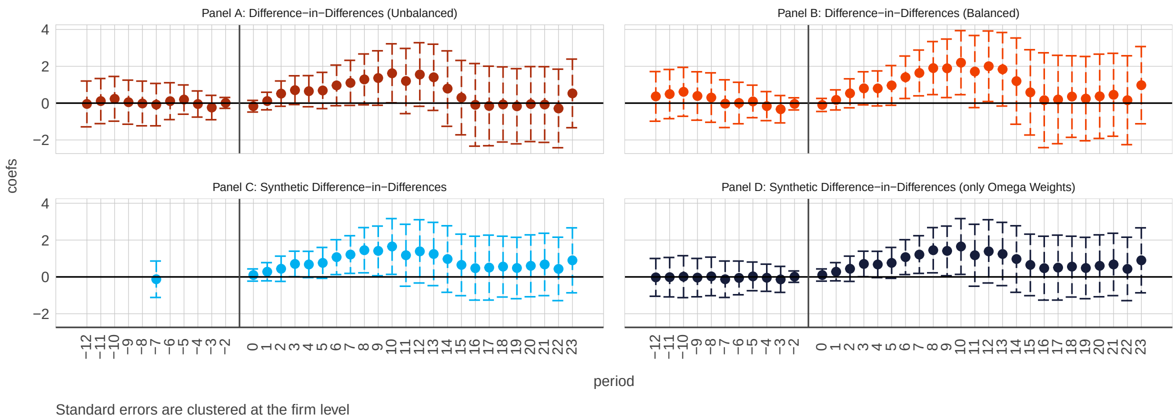


Figure 22. Payroll

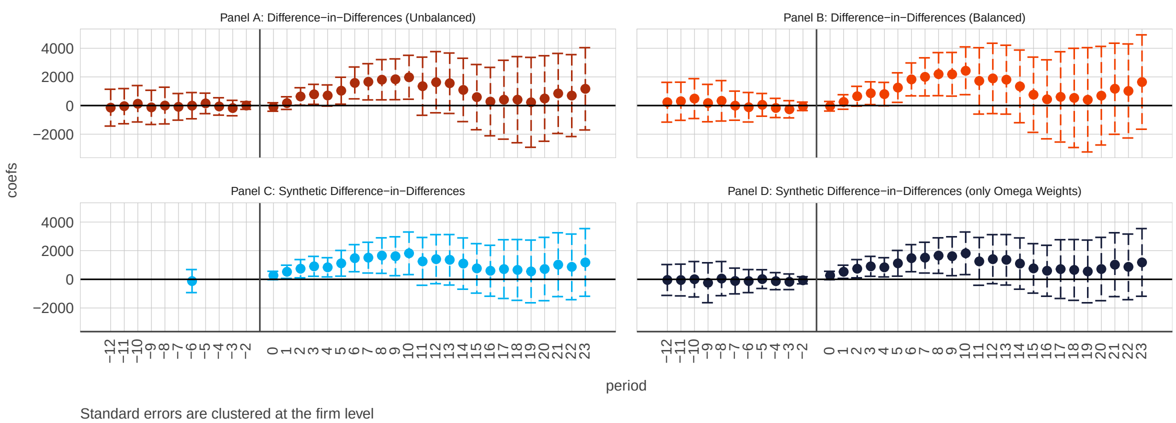


Figure 23. Working hours

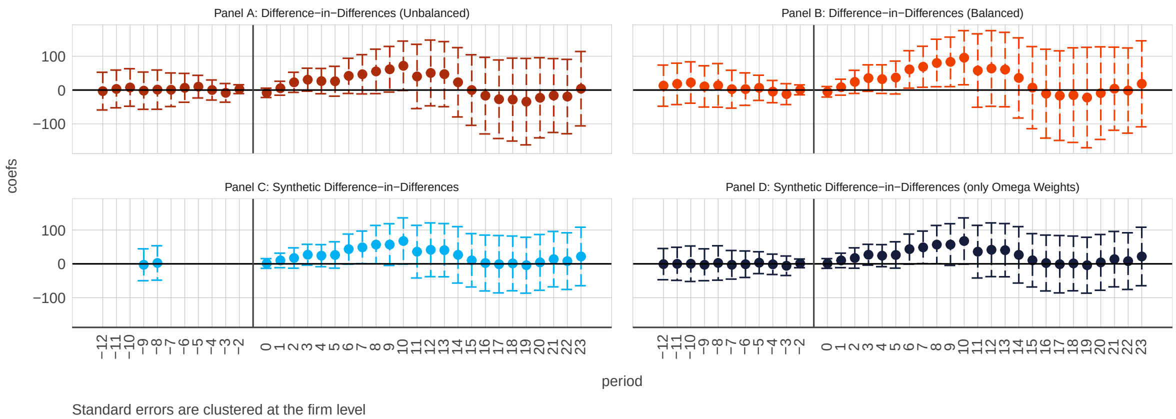


Figure 24. Earmarked debt

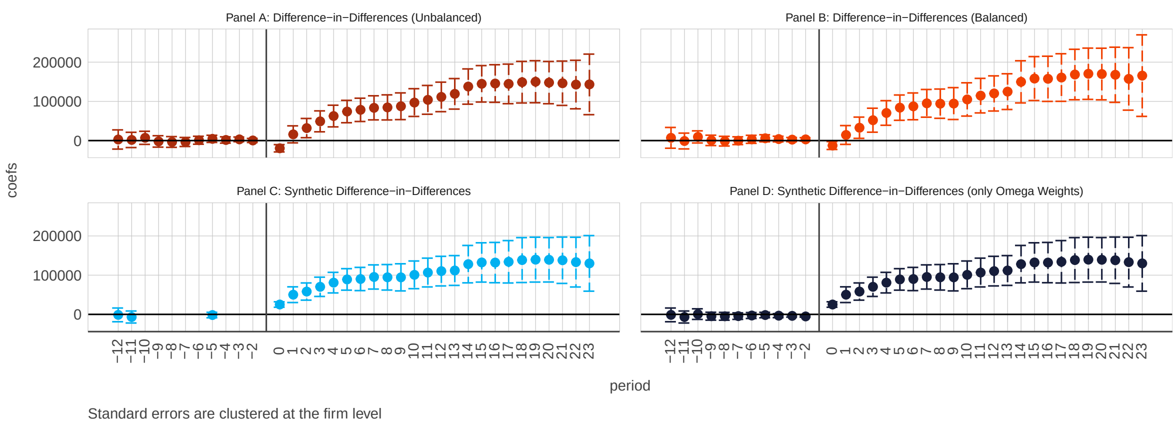
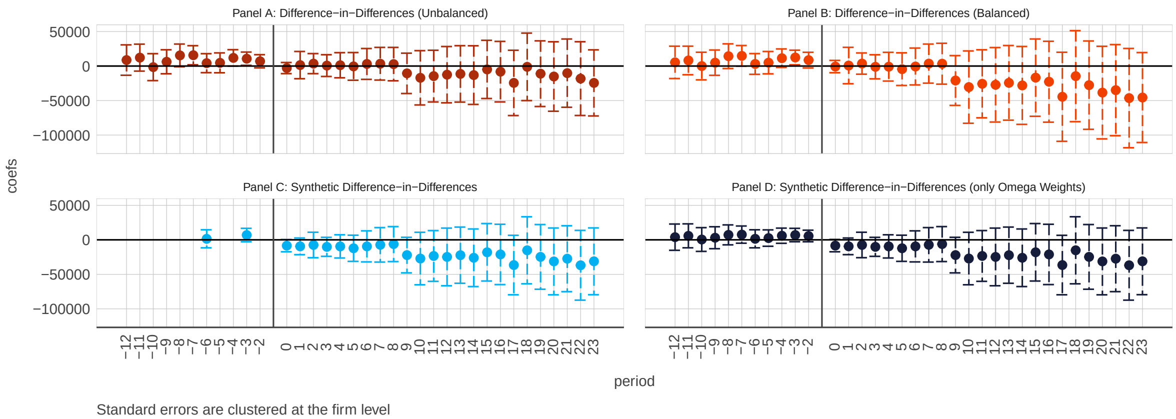


Figure 25. Free-market debt



9.2 Medium and Large

Figure 26. Current subsidies

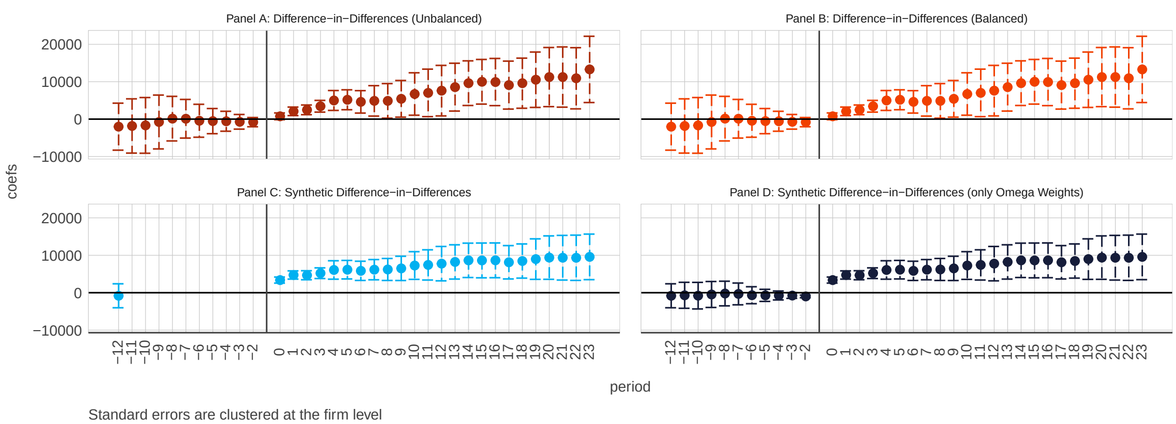


Figure 27. Accumulated subsidies

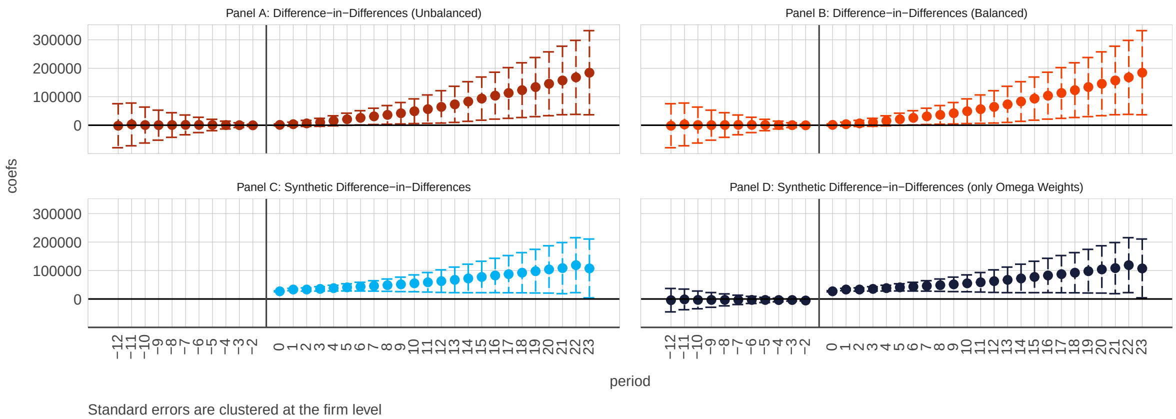


Figure 28. Employees

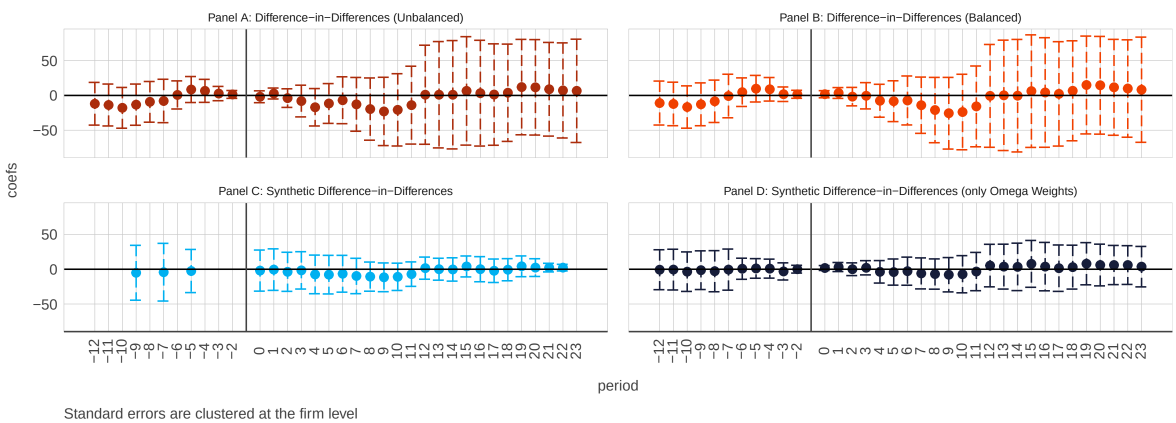
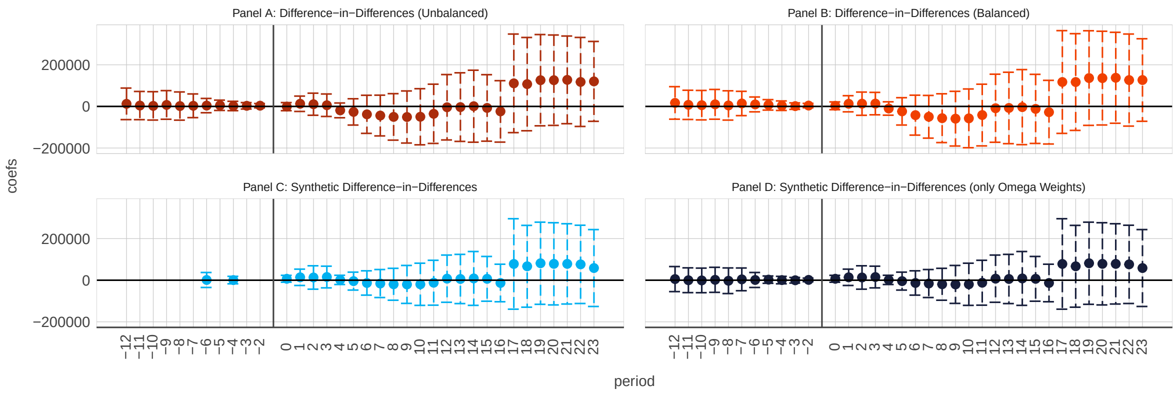
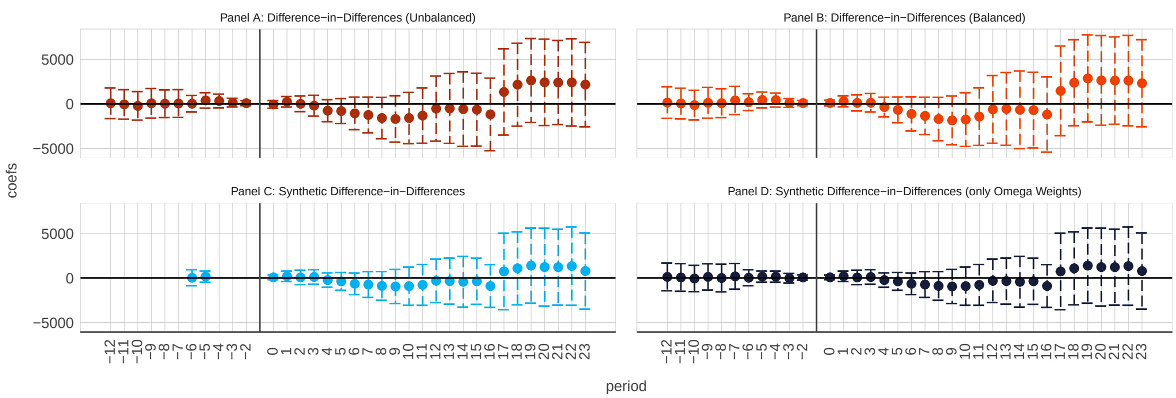


Figure 29. Payroll



Standard errors are clustered at the firm level

Figure 30. Working hours



Standard errors are clustered at the firm level

Figure 31. Earmarked debt

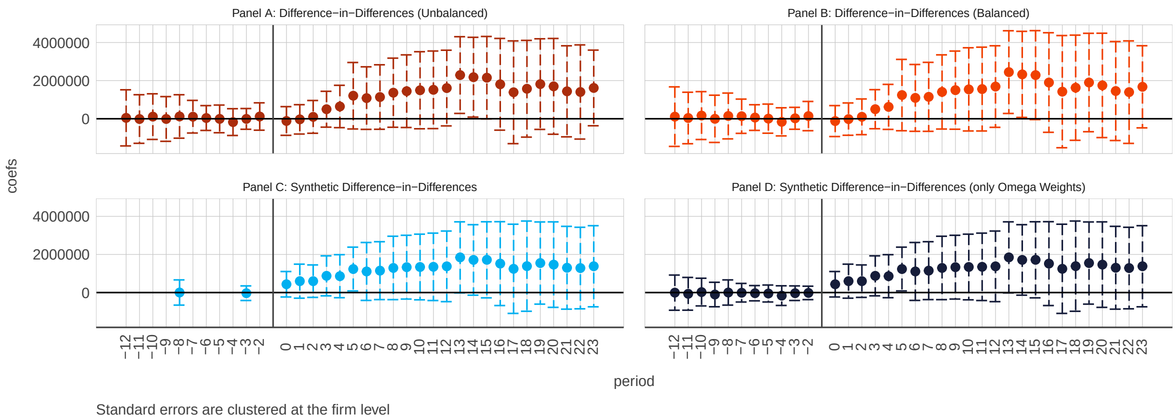
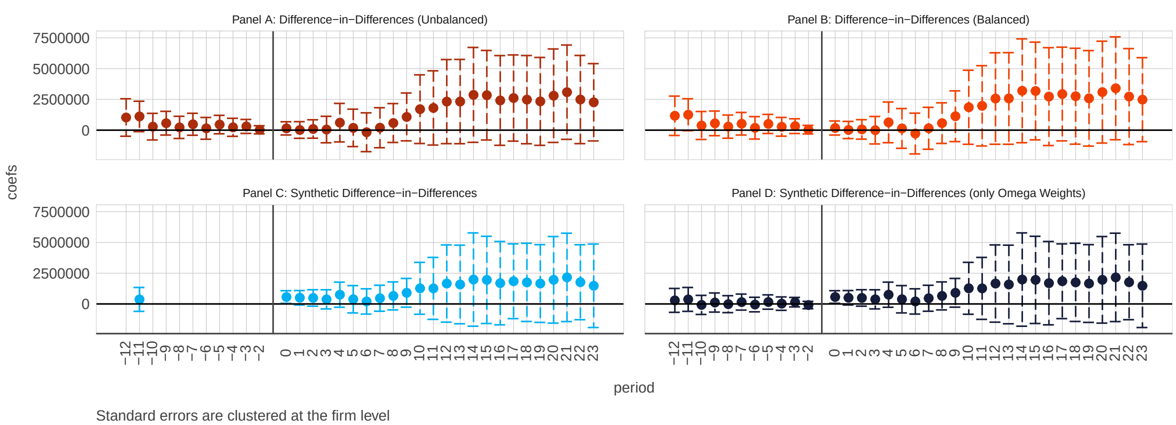


Figure 32. Free-market debt



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