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The Impact of Public Credit Programs on Brazilian Firms

Abstract[1]

Joao Alberto DeNegri,[2] Alessandro Maffioli,[3] Cesar M. Rodriguez,[4] Gonzalo Vázquez[5]

This paper analyzes the effectiveness of public credit lines in promoting the performances of Brazilian firms. We focus on the impact of the credit lines managed by BNDES and FINEP in fostering growth measured in terms of employment, labor productivity and export. For this purpose, we use a unique panel data set developed by the *Instituto de Pesquisa Econômica Aplicada* (IPEA), which includes information on both firm-level performances and access to public credit lines. This particular data setting allows us to use quasi-experimental techniques to control for selection bias when estimating the impact of the public credit lines. The core of our estimation strategy is based on a difference-in-differences technique, which we complement with matching methods for robustness check. Our results consistently show that access to public credit lines has a significant and robust positive impact on employment growth and exports, while we do not find evidence of a significant effect on our measure of productivity. Interestingly enough, our findings show that impact on exports is driven by the increase in export volumes among exporting firms, while no significant effect on the probability of becoming an exporter is detected.

Keywords: Public Credit, Impact Evaluation, SMEs, Difference in Difference, Panel Data, Brazil, BNDES, FINEP.

JEL Classification: C23, H43, L25, O12, O54

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1. Introduction

Public credit plays an important role in supporting the Brazilian productive sector. Data show that the presence of the public sector in the banking sector is high. The largest state owned development bank –the Banco Nacional do Desenvolvimento (BNDES)– accounted for 11 percent of all outstanding credit in 2006. Considering that the state also owns two of the three largest commercial banks in Brazil, the percentage of outstanding credit accounted for state-owned banks increases to around 44 percent. Although the importance of the public sector in the Brazilian financial system has been broadly debated, not much has been said on the effectiveness of these policy instruments in improving the conditions of final beneficiaries of these resources.

This paper aims at shedding some light on the effectiveness of public credit programs in promoting the performances of the productive sector in Brazil. In particular, we focus on the impact of the credit lines managed by BNDES and FINEP in fostering growth measured in terms of employment, labor productivity and export. For this purpose, we use a unique panel data set developed by the Instituto de Pesquisa Econômica Aplicada (IPEA), which includes information on both firm-level performance and access to public credit lines. This particular data setting allows us to use quasi-experimental techniques to control for selection bias when estimating the impact of the public credit lines. The core of our estimation strategy is based on a difference-in-difference technique, which we complement with matching methods for robustness check.

Our results consistently show that access to public credit lines has a significant and robust positive impact on employment growth and exports, while we do not find evidence of a significant effect on our measure of productivity. Interestingly enough, our findings show that impact on exports is driven by the increase in export volumes among exporting firms, while no significant effect on the probability of becoming an exporter is detected.

The scope of this paper is mainly empirical and its contribution to the existing literature should be considered in this context. This means that we do not develop any formal model aimed at assessing the theoretical linkages between access to credit and the firm-level performances. However, we complement our empirical analysis with a brief discussion of these linkages in light of the existing literature. To put our paper into context, we also review the most recent impact evaluations of public programs with objectives and means similar to the ones of the credit lines we analyze.

The paper is structured as follows: after this introduction, section one provides a brief review on the justification of public credit program aimed at fostering firm performances and on the evidence that have been produced on the effectiveness of such programs. Section two discusses more in detail the main characteristics of public credit programs in Brazil, with particular emphasis on the credit lines managed by BNDES and FINEP. Section three describes the data we are using for our analysis, including a review of the main basic statistics of interest. Section four discusses our identification strategy, focusing on the approach we adopted to control for selection biases. Section five presents the results of our estimations. Finally, section six concludes and provides some policy recommendations.

2. Discussion on Potential Impacts of Public Credit Programs

The fact that informational asymmetries generate credit constraints appears to be a consensus in the literature at least since Stiglitz and Weiss (1981). In turn, the fact that financial constraints may hinder firm performance has also been well-studied. For instance, poor access to financial markets may negatively affect firm growth, especially among small firms (Beck, Demirgüç-Kunt and Maksimovic, 2005). Rendón (2000) shows that capital market imperfections may restrict the creation of permanent jobs, and remarks the importance of removing financial constraints to promote job creation, particularly in economies with a high proportion of small firms. Moreover, lack of access to credit may prevent firms from exporting, since this practice involves entry costs related acquiring of information about foreign markets, customizing products to fit local tastes and setting up distribution networks (Minetti and Zhu, 2010). Bellone et al (2010) claim that, in this context, public intervention can help efficient but financially constrained firms to overcome these fixed entry costs and expand their activities abroad.

Thus, in presence of financial constraints, public financing may be an effective alternative to boost firm performance. In fact, several empirical studies show that public credit is successful in relaxing financial constraints. For instance, Aivazian, Masundar and Santor (2003) find that the World Bank's Small and Medium Industries program in Sri Lanka led to a relaxation of credit constraints and higher levels of investment for firms that received the subsidies. This effect is, however, rather limited, not least because of the relatively small amount of resources committed to this purpose.

Another finding of their analysis is that the public guarantee lowered the SMEs' borrowing cost to a substantial extent. Banerjee and Duflo (2004) exploit the exogenous variation generated by a policy change in India to test whether firms are credit constrained based on their reaction to changes in directed lending programs. According to the authors, while both constrained and unconstrained firms may be willing to absorb all the directed credit that they can get, constrained firms will use it to expand production, while unconstrained firms will primarily use it as a substitute for other borrowing. Their findings reveal that credit is used to finance more production, which implies an increment in the rate of growth of sales and profits; this provides evidence both on the existence of credit constraints and on the possibility of mitigate them through public credit. Finally, Bach (2009) tests if the French loan program CODEVI succeeds in improving access to credit for small French firms. The results show that access to the financing subsidy substantially increased debt financing on the firm side. However, this did not lead to significant substitution between subsidized and unsubsidized financing channels, which can be taken as evidence of financial constraints.

As to the impact of credit programs on firm performance, to our knowledge, none of the extant studies rely on experimental designs to evaluate this type of programs. Instead, the literature focused on non-experimental techniques aimed at eliminating or at least mitigating selection biases that are pervasive in this context since participation depends both on administrative eligibility criteria and individual decisions of the firms. The most popular approach, and the one used by all the evaluations described here, consists on applying difference-in-differences methods to panel databases combined with propensity score matching techniques to ensure the similarity between participants and non-participants.

Hall and Maffioli (2008) offer a review of empirical evaluations in Latin America. According to the authors, studies reveal generally positive effects of credit programs on intermediate outcomes like R&D expenditures, worker training and the introduction of new processes and quality control practices, especially in developing countries (López Acevedo and Tan, 2010). However, the evidence on the impact on longer-term performance outcomes like sales growth, exports, employment, labor productivity and TFP is mixed. For instance, Chudnovski et al (2005) analyze the FONTAR in Argentina, a program aiming at improving R&D and technology development through matching grants. They find positive effects of 57 to 79% on innovation investment, but no significant impacts on labor productivity or new product sales. Similarly, for the case of ADTEN, a subsidy program for R&D and technological development in Brazil, De Negri et al (2006) find increased R&D expenditures by 50 to 90% but no impact on sales, employment and labor productivity. Benavente, Crespi and Maffioli (2007) study the Chilean FONTEC, designed to promote technology transfer and development and R&D support. The authors estimate a 40% increase on sales growth and 3% increase on export intensity, although they find no impact on labor productivity in Chile.

Building on this results, López Acevedo and Tan (2010) provide an evaluation of SME credit programs in Mexico (Nafinsa, Bancomext, CONACyT, STPS and other programs from the Ministry of Economy), Chile (SENCE, CORFO, PROCHILE, FONDEF), Colombia (FOMIPYME) and Peru (BONOPYME, PROMPYME, CITE). The authors find positive gains in sales, labor productivity and employment in Chile, and higher value added, sales, export and employment in Mexico. In Colombia, the results suggest positive effects on exports, investment in R&D and TFP. Finally, in Peru the findings show significant positive effects in sales and profits. Confirming the findings of Hall and Maffioli, López Acevedo and Tan note that some of the estimated impacts do not materialize until after several years. Thus, they claim that the lack of impact of previous studies may be due to the short time dimension of the available databases, and remark the importance not only of controlling for potential selection biases but also to account for time lags to correctly estimate the effects of credit programs.

3. Public Credit Programs in Brazil

One important aspect of Latin American financial markets is the likelihood that firms are credit constrained and rely too heavily on their own sources to finance investment (Galindo and Schiantarelli, 2003, IDB, 2005). For instance, using data from The World Bank, approximately 25% of firms consider that they are credit constrained in Colombia. In Brazil, Bond, Soderbon and Wu (2007) estimate that about 40% of firms are credit constrained using the same data from 2000-2003.

This has negative implications for aggregate investment levels. Various factors contribute in generating credit constraints for MSMEs; from the demand side: their size, lack of collateral, and their technical deficiencies to manage and/or implement sustainable investment projects. From the supply side: limited medium- and long-term sources of funding in the domestic market and lack of transparency and information to conduct proper credit risk assessments, leading to reduced banks' appetite to serve this particular market segment.

Under this scenario, institutions such as BNDES in Brazil or Bancóldex in Colombia, with their access to domestic and foreign sources of medium- and long-term funding would most certainly be easing credit constraints, improving investment levels and generating a more efficient allocation.

The main objective of public credit programs is to support increased competitiveness and job creation in (MSMEs) by channeling medium- and long-term financing for investments. The Bank resources are added to those of development agencies or banks, commingled without distinction, and disbursed through programs under their indirect operations system. IFIs must comply with all Central Bank regulations and are responsible for evaluating the risk associated with sub-borrowers and the decision to grant financing. Program funds will be used to finance fixed investments or permanent working capital associated with the execution of investment projects by qualifying MSMEs.

In Brazil, while BNDES is not the only source of public credit, it is by and large the one with the biggest outlays for machinery and equipment acquisition: it accounts for 20% of all credit demand in the economy and 5% of GDP. Many public banks, such as regional development banks act only as financial intermediaries to BNDES, basically. The other two large public banks, Banco do Brasil and Caixa, provide mainly agriculture credit and housing credit, respectively, as well as acting as financial intermediaries to BNDES. Furthermore, the Financiadora de Estudos e Projectos (FINEP) is the Brazilian innovation agency and provides public financing for research and development projects for the entire Science, Technology and Innovation system.

4. Data Description

For the purpose of this study, we rely on a unique dataset based on the combination of existing administrative and statistical information[6]. Our final database is an unbalanced panel containing annual firm level information from 1997 to 2007. The main source of information are two administrative datasets: the Relação Anual de Informações Sociais (RAIS), which is an administrative file maintained by the Brazilian Ministry of Employment and Labor (Ministério do Trabalho e Emprego, MTE), and the Foreign Trade Dataset from the Secretariat of Foreign Trade (SECEX) of the Ministry of Development, Industry and Foreign Trade (MDIC). RAIS has a universal coverage: all registered tax-paying establishments must send every year to the Ministry information about every single worker who had been employed by the establishment anytime during the reference year. The RAIS information provides a matched employer-employee longitudinal data set, similar to those available in developed countries. The data from SECEX provide information on the value of export of all Brazilian exporters for the same period covered by RAIS. The two datasets were matched through a unique firm identifier number (Cadastro Nacoinal de Pessoa Juridica, CNPJ).

The novelty of the RAIS data is the possibility to match the employer-employee structure with detailed information available on workers' occupation, wages and schooling. So, the main use of RAIS will be to provide the labor inputs variables. In addition, the SECEX data provides reliable information regarding the value of total exports of firms. The coverage of the combined database includes all firms that declare hiring workers in Brazil since 1996. For instance, in 2001, this represents more than 76 millions of workers declared in more than 230 thousand firms from a range of manufacturing types. The panel data information allows classifying firms by activity, size, age of the firm and region of activity.

Finally, to capture the beneficiaries of public credit in Brazilian firms, we benefitted from a novel database of public credit use collected by the Institute for Applied Economic Research (Instituto de Pesquisa Econômica Aplicada, IPEA). This database has the foremost advantage of being able to cross-reference the information using the CNPJ of each firm with other databases at the firm level in Brazil. This information was available in an annual frequency from 1997 to 2007.

There are two main advantages of using a database with the characteristics described above. First, the large number of observations (firms) makes statistically feasible to find firms that did not participate in the program with similar characteristics to the ones that actually did participate (counterfactual). Second, the panel data structure allows controlling for non-observable effects that determine program participation and firm performance. Nevertheless, the main disadvantage is that RAIS database does not have information regarding total sales, and hence, it is not possible to construct total factor productivity

[6] The details and definitions of the variables used appear in the Appendix I.

(TFP) measures. Still, it can be argued that total salary expenditure and total exports have a close relationship with firms' TFP. Formally, from basic production theory, real wages are a measure of labor productivity. Under this hypothesis, evaluating the impact of the program in terms of average real wages would be an approximation of the impact in terms of labor productivity. Nevertheless, there are also arguments that challenge this view. For instance, the existence of collective wage agreements, special benefits for years worked in the firm or efficiency wages. To deal with a more precise measure of real wages we construct a synthetic measure of average standardized wages that represents an approximation of labor productivity at the firm level. Annex I describes the construction of this variable.

Given the nature of the data and the fact that public credit programs have been in place since before 1997 we needed to make a decision regarding which year should be considered as the starting point for our analysis. In other words, these programs have been in place for years before the first year of the sample we have -1997-, and are still active throughout the entire sample.

In order to evaluate the effectiveness of such intervention we need to consider an alternative starting point for those programs. This decision is far from trivial and inevitably involves some discretionarily, but such simplification, if something, should go in the direction of underestimating the long-run effect of the use of public credit. Assuming this caveat and its consequences we therefore decided to consider 2001 as the alternative starting point of the use of public credit in Brazil mainly based on a statistical argument. Thus, all the firms that enter the program before or after 2001 are excluded from the analysis. The decision is based on the fact that the year 2001 divides the sample evenly such that it maximizes the statistical power of the analysis by placing an equal number of years before and after the chosen starting year. Needless to say, we understand our results as a first and therefore preliminary analysis of the impact of such program.

4.1 Baseline Characteristics

Table 1 shows that in 2001, public credit use comprises almost 17 thousand firms of which 23% were exporters. Almost a third of the beneficiary firms are producers of food and plastic, mainly concentrated in the south and southeast region. The vast majority -80%- of such firms are micro and small sized.

Table 1. Main Characteristics of Public Credit Recipients in 2001

	Treated		Controls	
	Number	**Distribution**	**Number**	**Distribution**
Firms	16,700	100%	215,183	100%
Exporters	3,786	23%	6,963	3%
Non-exporters	12,914	77%	208,220	97%
Sectors:				
Coal extraction	9	0%	80	0%
Oil and Natural Gas extraction	3	0%	141	0%
Metallic mineral extraction	26	0%	246	0%
Non-metallic mineral extraction	613	4%	4,396	2%
Foods and beverages	2,826	17%	31,725	15%
Tobacco	10	0%	131	0%
Textile	693	4%	8,440	4%
Clothing and accessories	600	4%	33,971	16%
Leather	412	2%	9,830	5%
Wood products	1,030	6%	14,080	7%
Paper products	372	2%	2,300	1%
Edition and printing	611	4%	14,391	7%
Petroleum refining	86	1%	129	0%
Chemical products	798	5%	6,430	3%
Rubber and plastic	1,727	10%	7,665	4%
Manufacture of non-metallic minerals	1,309	8%	16,696	8%
Basic metals	510	3%	4,173	2%
Manufacture of metal products	1,584	9%	20,874	10%
Machinery and equipment	1,205	7%	8,378	4%
Computer equipment	31	0%	420	0%
Electric machinery and equipment	301	2%	2,927	1%
Electronics	99	1%	1,294	1%
Medical equipment and precision instruments	138	1%	1,591	1%
Fabrication and assembly of automotive vehicles	456	3%	3,075	1%
Manufacture of transport equipment	48	0%	952	0%
Furniture	1,156	7%	20,028	9%
Recycling	47	0%	820	0%

Regions:				
North	385	2%	5,645	3%
Northeast	3,425	21%	52,297	24%
Southeast	6,593	39%	82,592	38%
South	5,667	34%	62,000	29%
West	630	4%	12,649	6%
Size (employment):				
Micro (<5)	2,151	13%	121,013	56%
Small (5-100)	11,148	67%	90,427	42%
Medium (100-500)	2,627	16%	3,293	2%
Large (>500)	774	5%	450	0%
Multinational:				
0	16,190	97%	213,409	99%
1	510	3%	1,774	1%

In Table 2 we present summary statistics for the outcomes and covariate variables before the beginning of the program in 2001, for beneficiaries and non-beneficiaries of public credit. It can be seen that beneficiaries have systematic greater magnitudes in all variables (employment, total wage expenditure, total exports, total imports, total age of the firm, average credit size and average standardized wage) and their difference with non-beneficiaries is strongly significant.[7]

The information presented here is consistent with the previous table and is giving evidence suggesting that firms that enter the program are larger in size, they spend more in wages, they export and import more, they are older, they take more public credit and they have a higher average standardized wage than the rest. In fact, this could be reflecting the presence of unobserved factors affecting the participation decision. The identification strategy, to be explained below, will take into consideration these issues to find appropriate control firms and avoid biases generated by these unobserved factors.

[7] Appendix I present a description of the variables used and its construction.

Table 2. Descriptive Statistics

	Treated		Controls		t-test	
	Average	**St. Dev.**	**Average**	**St. Dev.**	**T**	**p-value**
Employment	135	662	14	76	83.0	0.00
Salary Expenditure (Ths R$)	2,749,683	28,400,000	188,421	2,448,082	40.9	0.00
Exports (US$)	1,948,057	32,800,000	66,405	6,558,752	26.0	0.00
Imports (US$)	1,453,841	49,700,000	76,486	4,716,537	12.6	0.00
Age of the firm	14.95	1.86	1.44	7.59	2113.2	0.00
Public Credit (Ths R$)	940	18,455	0	0	23.1	0.00
Profits per worker (Ths R$)	0.04	0.51	-0.08	0.68	92.1	0.00

Furthermore, when inspecting the trends of the main outcomes (exports, employment, and profits per worker) before the starting of the program –between 1997 and 2000-, it can be seen that there is a different behavior between treated and non-treated firms. Figures 1 to 3 show pre-treatment trends behavior for exports, employment and average standardized wage. Although at first sight the pre-treatment performance may look alike between treated and non-treated firms, when performing a test of equality of trends the null hypothesis of equality is rejected.[8] This divergent performance could be due, among other factors, to the fact that the non beneficiaries are a very heterogeneous group of firms and may not constitute an accurate comparison group for treated firms. To analyze the impact of the program in such setup will require finding an appropriate *counterfactual* to the treated firms. This will be the first task of the identification strategy.

[8] Tables available upon request to Alessandro Maffioli (alessandrom@iadb.org).

Figure 1. Exports in Logs (Before Matching)

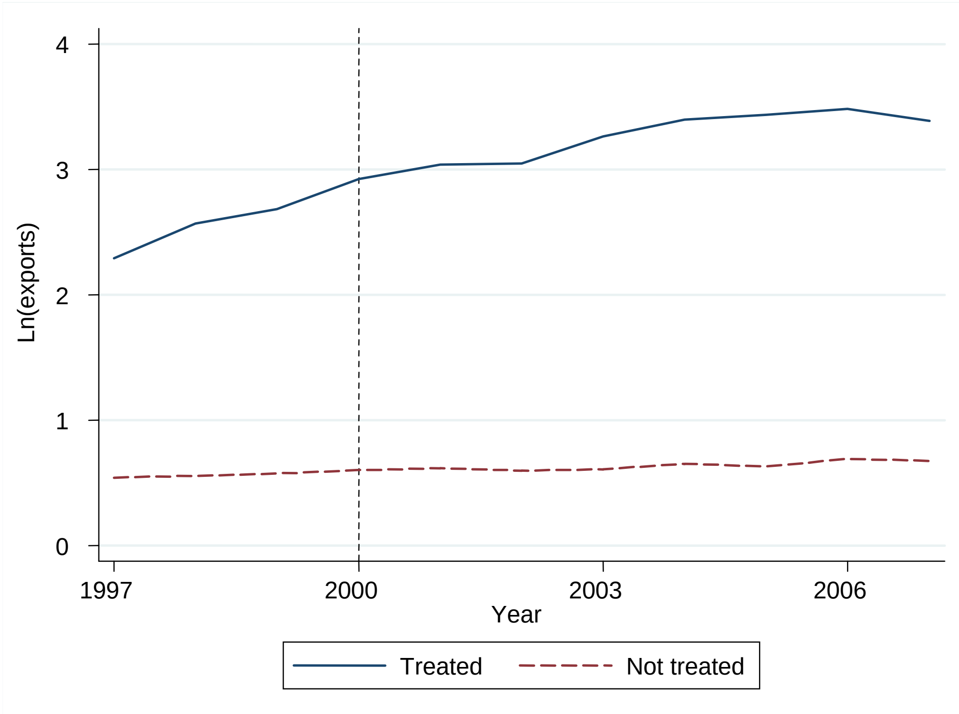


Figure 2. Employment in Logs (Before Matching)

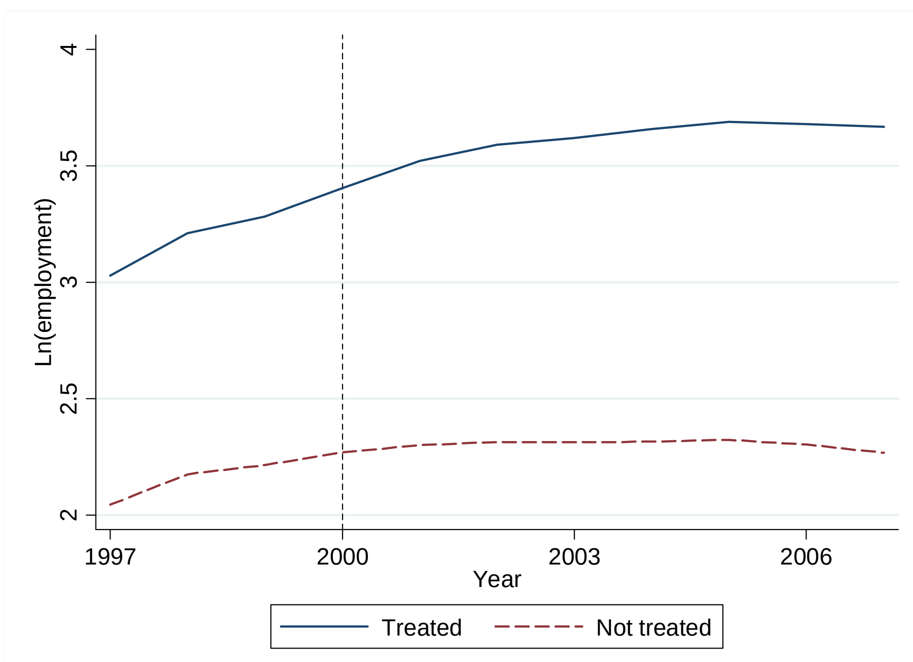
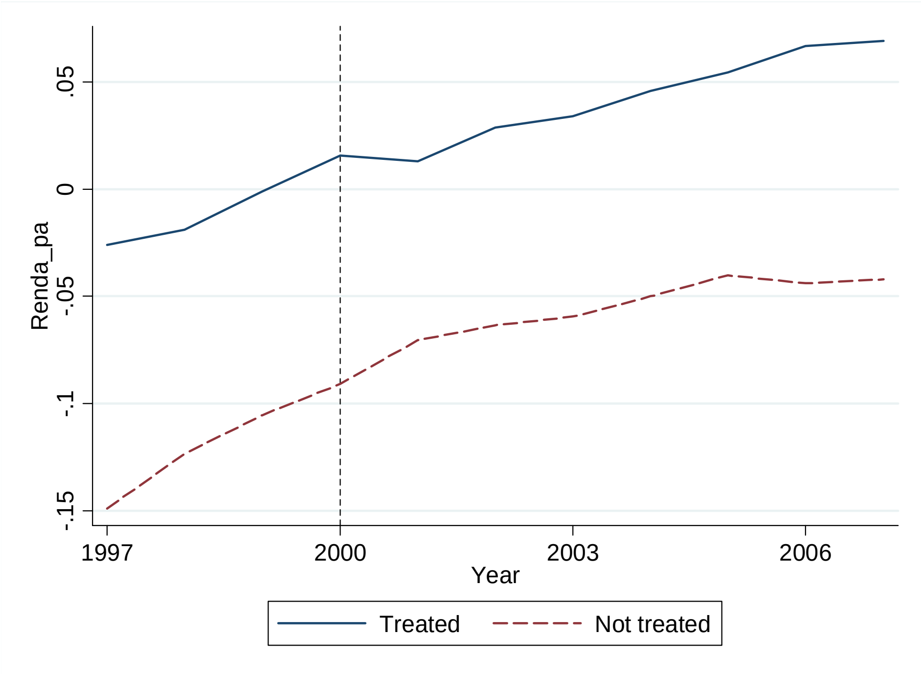


Figure 3. Labor Productivity (Before Matching)



5. Identification Strategy

We will use the group of non-participating firms to estimate the counterfactual outcome of the treated firms, i.e. to calculate what would have the outcome been for treated firms, had they not been treated. However, as the previous section suggests, the pool of untreated firms is not necessarily comparable to the group of beneficiaries, since the intervention is not randomly assigned and hence potential issues of self-selection and administrative selection bias arise which can seriously compromise the validity of the estimations.

Several techniques can be used to avoid these potential problems. We will use two methods to deal with selection bias, namely, standard fixed-effects regressions and a combination of fixed effects and propensity score matching.

First, if participation is determined by observable factors, these variables can be included as control variables in a regression framework. However, some of these relevant factors may be unobservable (for instance, entrepreneurial behavior of the firm, manager characteristics, etc), and thus cannot be accounted for. Nevertheless, the panel structure of our database allows us to eliminate all unobservable factors, as long as they do not vary with time, using a fixed-effects model.

More rigorously, we propose the following specification:

$$Y_{it} = \alpha_i + \mu_t + \beta T_{it} + \gamma X_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome of the firm i in year t, α_i captures all time-constant factors that affect the outcome and are firm-specific, μ_t represents yearly shocks that affect all firms, T_{it} is a binary variable that takes the value one since the year in which the firm i enters the program, X_{it} is a vector of time-varying control variables and ε_{it} is the usual error term assumed to be uncorrelated with T_{it}. The standard errors will be clustered at the firm level for the inference to be robust to within-firm correlation of the error terms. In absence of time-varying unobserved factors that affect both the outcome and the participation, the fixed-effects method leads to consistent estimator for β, the impact of the program.

The validity of the difference-in-differences (fixed-effects) estimator rests on the identification assumption that trends in the outcomes would have been equal in absence of treatment. However, this assumption may be difficult to accept when firms in the control group are very heterogeneous and very different from the participating firms, since firms that are very different are likely to follow different trends as well. In order to reinforce the results, we also run equation (1) on a matched sample, selecting among the firms in the comparison group those that are more similar to beneficiaries not only in terms of observed characteristics but also on their pre-treatment performance. We do this to ensure that we selecting from the control group only those which have pre-treatment trends that are similar to those in the treated group.

More precisely, we define the year previous to treatment as a baseline year and estimate the propensity score, i.e. the conditional probability of participation, using a probit model:

$$P\left(T_{it} = 1 | Z_{it}, Y_{it}^{l}\right) = \Phi\left(\theta Z_{it} + \lambda Y_{it}^{l}\right) \qquad (2)$$

for a fixed pre-treatment year t, where Z is a vector of covariates, Y^{l} is a vector of k lags of the outcome variable, $(Y_{it-1}, \dots Y_{it-k})$, and Φ is the standard Normal cumulative distribution function.

We use a different probit for each outcome. Since the main objective of the matching is to ensure that ex-ante trends are similar between groups, we argue that running separate probits for each outcome is a more flexible strategy to find appropriate matches for each treated firm; this is so because, for instance, a comparison firm may be a good match for a treated firm in terms of ex-ante trends in exports but may follow a different dynamic in employment. Therefore, running separate probits allows finding better matches for each outcome. The main disadvantage of this choice is that the resulting control groups are different for each outcome, which may complicate the comparison of the results across outcomes. However, considering the importance of the similarity of trends for the validity of the estimations, we believe that the advantages of this choice outweigh its costs.

Using the predicted probability of participation, we match each treated firm with the untreated firm with most similar propensity score; we then drop from the database all the control-group firms that are not matched to any treated firm and run equation (1) on this matched subsample.

6. Estimation Results

Results will be presented firstly for the full sample as a whole and then only focusing on the common support that will be explained and constructed below.

6.1 Full Sample Results

This section summarizes the results obtained by estimating equation (1) using the fixed effects estimator for the three outcomes of interest: employment (in logs), total exports (in logs) and labor productivity. The participation variable is a dummy that takes the value of 1 once the firm started participating in the public credit program.

Table 3 shows the impact of the program in employment. The dependent variable is the total number workers expressed in logarithms. Column 1 shows a strongly significant and positive effect of around 23%[9] when only controlling for time dummies. Column 2 shows that this effect is robust to the inclusion of control variables; the coefficient increases to 25%. Finally, column 3 includes industry-year interaction terms, which allow for differential time trends across industry sectors. The results are indistinguishable from the ones in column 2. Hence, although the effect of the program decreases as we control for observables, it does not differ significantly by adding control variables. When interpreting these impacts we need to take into account the trajectories of the control and the treated group throughout the period of analysis. At the baseline, the matched sample of treated firms exhibit on average 100 employees per firm, hence a 23% increase implies an increase of 23 employees for the treated firms with respect to the control group.

[9] More precisely, since the treatment variable is binary and the outcome is measured in logarithms, the correct way to interpret the coefficient is to calculate *exp*(b)-1. However, the "raw" coefficient is in most cases a very close approximation to the discrete impact, and hence we use what we consider the more straightforward way of interpreting the results.

Table 3: impact on Employment (Full Sample)

	(1)	(2)	(3)
BNDES	0.2307***	0.2531***	0.2528***
	(0.020)	(0.018)	(0.018)
lage		0.8756***	0.8762***
		(0.016)	(0.016)
lskill		0.1333***	0.1322***
		(0.008)	(0.008)
lwage		0.1011***	0.1016***
		(0.007)	(0.007)
patentes		0.0055	0.0060
		(0.004)	(0.004)
finep		0.2726***	0.2672***
		(0.066)	(0.066)
premio		0.1338***	0.1334***
		(0.008)	(0.008)
limp		0.0212***	0.0211***
		(0.001)	(0.001)
Constant	2.2865***	-0.8945***	-0.9151***
	(0.002)	(0.060)	(0.060)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	×	×	✓
R^2	0.02	0.071	0.073
Obs.	492480	492480	492480
No. of firms	49248	49248	49248

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4 shows the impact of the program in exports. The dependent variable is total exports expressed in logarithms. This variable is constructed in such a way that if a firm has no exports in a given year, a value of one is assigned. This procedure allows us to construct a logarithmic version of total exports with no missing values. Through this mechanism, the interpretation of the impact of the program in this variable is the same as the logarithm of employment. Column 1 reveals a strongly significant positive impact of 47% on exports when controlling for time dummies. The estimated impact decreases after the addition of control variables, but remains large and significant (39%). This effect is robust to the inclusion of industry-year interaction terms.

Table 4. Impact on Exports (Full Sample)

	(1)	(2)	(3)
PUCR	0.4765***	0.3880***	0.3896***
	(0.095)	(0.080)	(0.080)
lage		0.0449	0.0434
		(0.038)	(0.039)
lskill		0.0347***	0.0338***
		(0.012)	(0.012)
lwage		0.0395***	0.0399***
		(0.010)	(0.010)
patentes		-0.0082	-0.0075
		(0.017)	(0.017)
finep		1.0418***	1.0307***
		(0.368)	(0.368)
premio		5.8490***	5.8482***
		(0.064)	(0.064)
limp		0.0717***	0.0717***
		(0.004)	(0.004)
Constant	0.7106***	-0.1515	-0.1544
	(0.007)	(0.115)	(0.115)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	✗	✗	✓
R^2	0.02	0.297	0.30
Obs.	492480	492480	492480
No. of firms	49248	49248	49248

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5 shows the impact of the program in labor productivity. According to the first set of estimations, none of the specifications detect a significant impact. This lack of impact might seem counterintuitive. A word of caution is needed here. In this specific case, the lack of impact could be also related to way we are approximating labor productivity, i.e. through real wages, because real wages may change slower than real labor productivity.

Table 5. Impact on Labor Productivity (Full Sample)

	(1)	(2)	(3)
PUCR	-0.00002	0.0011	0.0016
	(0.012)	(0.009)	(0.009)
lage		-0.1056***	-0.1079***
		(0.013)	(0.013)
lskill		0.0004	0.0004
		(0.009)	(0.009)
lwage		0.6521***	0.6522***
		(0.020)	(0.020)
patentes		-0.0006	-0.0006
		(0.002)	(0.002)
finep		0.0402	0.0400
		(0.049)	(0.049)
premio		-0.0147**	-0.0147**
		(0.006)	(0.006)
limp		-0.0048***	-0.0048***
		(0.001)	(0.001)
Constant	-0.0403***	-4.4089***	-4.4116***
	(0.002)	(0.132)	(0.132)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	×	×	✓
R^2	0.007	0.255	0.255
Obs.	492460	492460	492460
No. of firms	49248	49248	49248

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

6.2 Construction of the Matched Sample

One possible concern with the previous estimations is that the control group is very heterogeneous and thus not necessarily comparable to the treated firms. To reinforce the validity of the results, we need to select from the control group a subgroup of firms that are much more similar to treated firms in terms of observable characteristics.

More precisely, we use matching techniques to pair each treated firm to the most similar untreated firm. We do this in two steps. We first estimate a probit model for the propensity score (i.e. the conditional probability of participation) for each firm using a vector of observed characteristics as predictors. We then match each beneficiary with the untreated firm with more similar propensity score, and we run the previous results on this new matched sample, dropping all the untreated firms that are never used as a comparison.

The probit model is run on the year previous to treatment to ensure that none of the predictors are affected by the intervention. In addition to standard control variables like age and industry sector, we also include several lags of the outcome variable to match not only on the values of observable characteristics but also to ensure that treated and control firms followed similar paths before treatment. As described in the methodological section this is a necessary condition for the difference in difference (fixed effects) estimator to be consistent. In particular, we run three different probit models to perform separate analyses for each outcome. In each one of these, we use four lags of the corresponding outcome variable to capture pre-treatment trends, plus a set of control variables as shown in tables 6 to 8.

The results of the probit models for 2001 are presented in table 6. The dependent variable is dichotomous and takes the value of one if the firm borrowed from either BNDES or FINEP in 2001.

Table 6. Participation Model

	Ln(employment)	Ln(exports)	Labor productivity
Yit-1	0.45***	0.025***	0.01
	(0.061)	(0.008)	(0.05)
Yit-2	-0.14*	0.005	-0.021
	(0.08)	(0.009)	(0.056)
Yit-3	0.043	0.01	-0.005
	(0.07)	(0.009)	(0.058)
Yit-4	-0.053	-0.017**	0.023
	(0.039)	(0.008)	(0.045)
lage	-0.173***	-0.109***	-0.103***
	(0.044)	(0.036)	(0.036)
lskill	0.258***	0.193***	0.185***
	(0.073)	(0.071)	(0.07)
lwage	0.134***	0.173***	0.177***
	(0.043)	(0.042)	(0.052)
multi	-0.553***	-0.63***	-0.578***
	(0.11)	(0.11)	(0.11)
patentes	0.039	0.059*	0.058*
	(0.035)	(0.034)	(0.034)
premio	0.154***	0.072	0.219***
	(0.059)	(0.067)	(0.06)
limp	0.02***	0.027***	0.031***
	(0.005)	(0.005)	(0.005)
clapo_0	0.117	-1.117***	-1.089***
	(0.173)	(0.145)	(0.078)
clapo_1	0.329**	-0.491***	-0.456***
	(0.148)	(0.136)	(0.062)
clapo_2	0.239*	-0.097	0.161
	(0.134)	(0.132)	(0.13)
Constant	-4.279***	-3.133***	-3.003***
	(0.34)	(0.324)	(0.344)
Pseudo R2	0.123	0.1035	0.1001
Obs.	49248	49248	49242

Regressions also control for geographical and industry dummies
* significant at 10%; ** significant at 5%; *** significant at 1%

From each probit model, we can conclude that the oldest firms with the most skilled workers and the highest wage expenditures have a higher probability of participating in the public credit program. Similarly, compared to the largest firms, smaller ones have more probability of participating in the program. This information is consistent with the summary statistics described above and gives evidence of a participation bias. In other words, we need to control for this selection bias to be able to attribute to the program the difference in outcomes between treated and non-treated firms. If we leave this issue unattained, the difference in outcomes may be given by the pre-treatment difference between treated and non-treated.

With the probit models estimates, we predict the probability of participation and match each beneficiary with the non-beneficiary with closest propensity score. We construct this control group using the one-nearest-neighbor algorithm. Finally, we drop from our sample all the control firms that are not matched to any treated firm. (See figures 4 to 6).

Figure 4. Employment (Matched Sample)

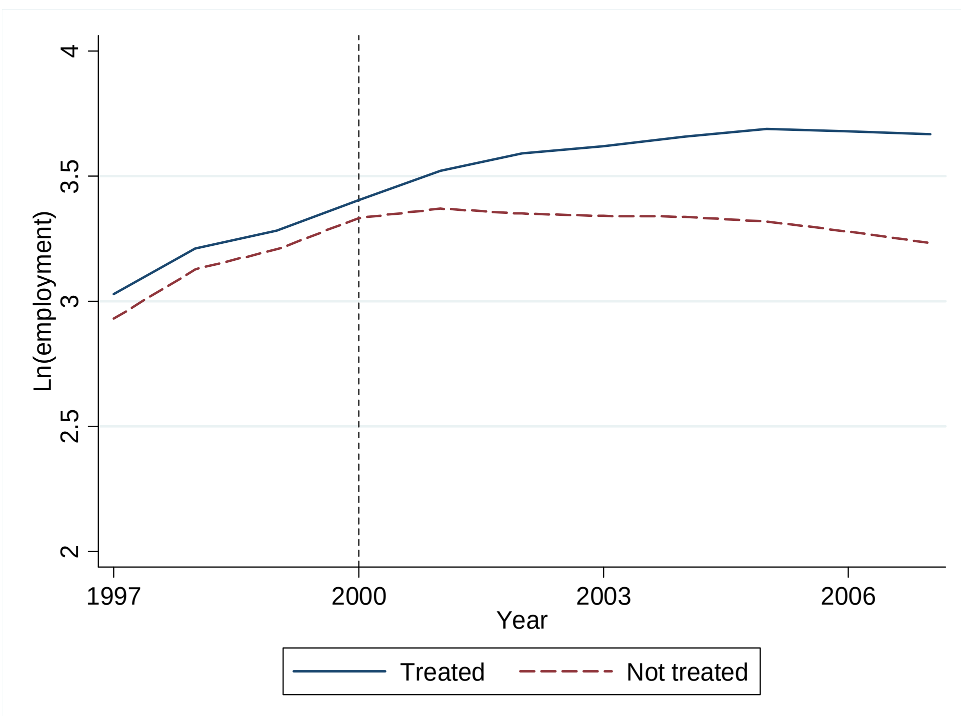


Figure 5. Exports (Matched Sample)

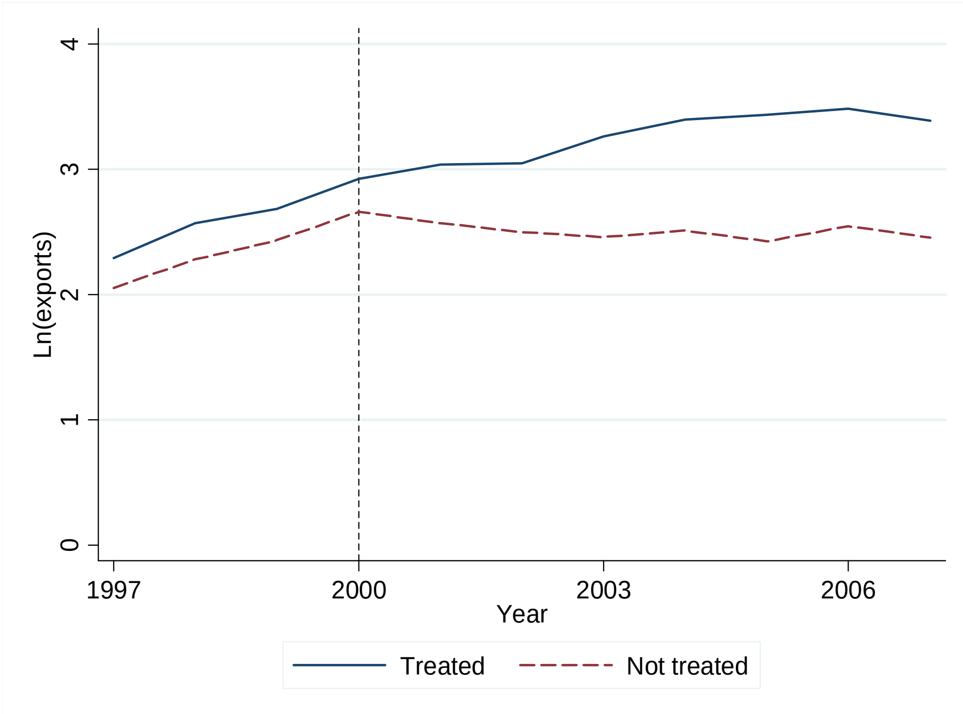
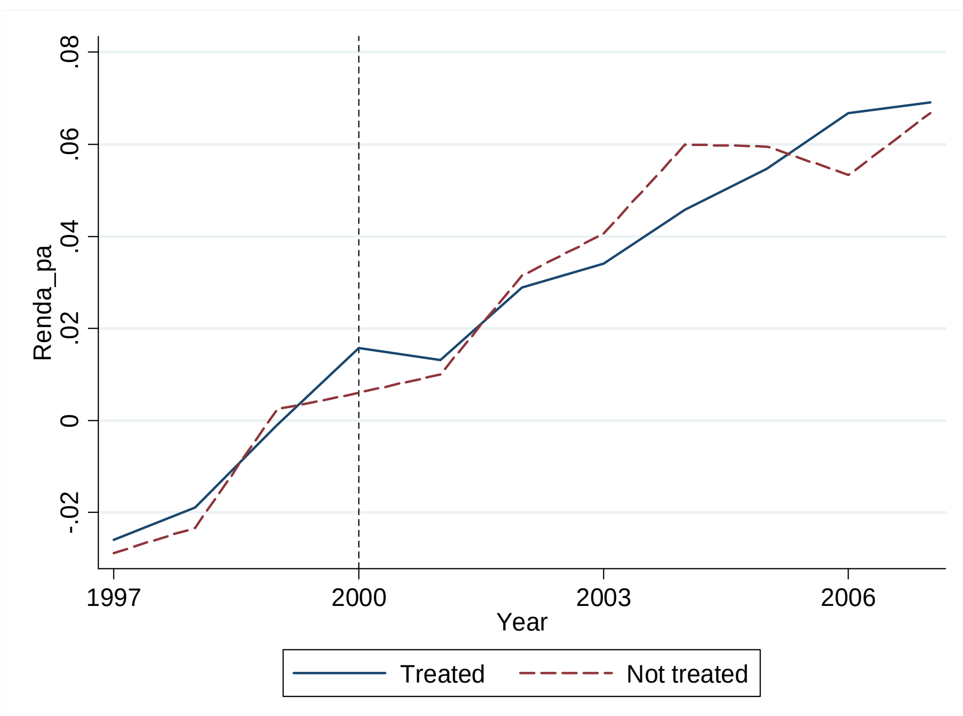


Figure 6. Labor Productivity (Matched Sample)



Tables A1 to A3 (see Appendix II) show the balancing test for the covariates included in each participation equation considering a control group defined by the matching procedure. In fact, after the matching, the hypothesis of equality of means of observable characteristics for both treated and untreated firms cannot be rejected. In sum, both the graphical evidence and the statistical tests suggest that the matching is successful in constructing a control group that is very similar to the treated group. Once these characteristics (including the pre-treatment trends) between participating and non-participating firms are balanced, the common support defined is free from selection bias and we can attribute the difference to the program participation. Thus, we can now proceed to run the previous regressions in this new matched sample.

6.3 Matched Sample Results

Tables 7 to 10 present the results of the estimation over the common support. In general, the results for the matched sample are very similar to the ones for the full sample. The estimated impact on employment is again around 24% and around 40% for exports, while we find no significant impact on average standardized wages.

Table 7. Impact on Employment (Matched Sample)

	(1)	(2)	(3)
PUCR	0.2462***	0.2404***	0.2395***
	(0.029)	(0.026)	(0.026)
lage		1.1563***	1.1577***
		(0.090)	(0.091)
lskill		0.0186	0.0201
		(0.075)	(0.075)
lwage		-0.0197	-0.0229
		(0.088)	(0.087)
patentes		0.0049	0.0061
		(0.010)	(0.010)
finep		0.3183***	0.3253***
		(0.079)	(0.079)
premio		0.1085***	0.1076***
		(0.027)	(0.027)
limp		0.0262***	0.0262***
		(0.004)	(0.004)
Constant	3.1698***	0.1734	0.1944
	(0.014)	(0.611)	(0.604)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	✗	✗	✓
R^2	0.09	0.171	0.176
Obs.	15700	15700	15700
No. of firms	1570	1570	1570

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. Impact on Exports (Matched Sample)

	(1)	(2)	(3)
PUCR	0.5358***	0.4060***	0.3979***
	(0.119)	(0.102)	(0.102)
lage		0.0073	-0.0014
		(0.309)	(0.312)
lskill		-0.1072	-0.1191
		(0.184)	(0.184)
lwage		0.2976*	0.2987*
		(0.159)	(0.160)
patentes		0.0931	0.0975
		(0.076)	(0.075)
finep		1.9314	1.9102
		(1.353)	(1.349)
premio		4.4198***	4.4214***
		(0.213)	(0.212)
limp		0.0913***	0.0912***
		(0.015)	(0.014)
Constant	2.4279***	-0.1550	-0.0874
	(0.060)	(1.374)	(1.373)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	×	×	✓
R^2	0.01	0.208	0.21
Obs.	15800	15800	15800
No. of firms	1580	1580	1580

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Impact on Labor Productivity (Matched Sample)

	(1)	(2)	(3)
PUCR	-0.0054	-0.0063	-0.0067
	(0.015)	(0.012)	(0.012)
lage		-0.1051*	-0.1082*
		(0.056)	(0.056)
lskill		0.1374***	0.1367***
		(0.051)	(0.051)
lwage		0.8182***	0.8186***
		(0.143)	(0.143)
patentes		-0.0089	-0.0084
		(0.009)	(0.009)
finep		-0.0902	-0.0908
		(0.133)	(0.133)
premio		-0.0275	-0.0276
		(0.019)	(0.019)
limp		-0.0057**	-0.0058**
		(0.003)	(0.003)
Constant	0.0708***	-5.3923***	-5.3580***
	(0.014)	(0.838)	(0.838)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	×	×	✓
R^2	0.009	0.323	0.324
Obs.	15790	15790	15790
No. of firms	1579	1579	1579

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

One potential concern with the results for exports is that the estimators are mixing two distinct possible effects: on the one hand, the program may increase export volumes, but also change the pool of exporting firms by inducing firms to start exporting. To address this issue, we perform two separate analyses. First, we study the impact of the program on the probability of a firm being an exporter using as the outcome of interest a binary variable that takes the value one if the firm has non-zero exports. To estimate this specification we use a linear probability model.

Such model has some limitations with respect to its close-related probit or logit, mainly the fact that marginal effects are constant[10]. Nevertheless, it has the advantage of straightforwardly controlling for fixed effects. The results of these estimations are presented in table 10.

Table 10. Impact on Probability of Exporting (Matched Sample)

	(1)	(2)	(3)
PUCR	0.0197*	0.0108	0.0112
	(0.011)	(0.009)	(0.009)
lage		-0.0007	-0.0015
		(0.027)	(0.027)
lskill		-0.0112	-0.0120
		(0.017)	(0.017)
lwage		0.0144	0.0147
		(0.011)	(0.011)
patentes		0.0038	0.0045
		(0.004)	(0.004)
finep		0.0886	0.0908
		(0.086)	(0.084)
premio		0.4506***	0.4510***
		(0.019)	(0.019)
limp		0.0068***	0.0068***
		(0.001)	(0.001)
Constant	0.1984***	0.0690	0.0702
	(0.006)	(0.114)	(0.114)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	✗	✗	✓
R^2	0.005	0.224	0.227
Obs.	15830	15830	15830
No. of firms	1583	1583	1583

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

[10] Another drawback of the linear probability model is that it does not guarantee that the predicted probability to be between zero and one, although this is irrelevant in this case where the estimates are not used for prediction

In principle, it can be argued that firms that move from non-exporter to exporter are those that were able to overcome credit constraint and access international markets. In other words, those firms that were affected by the program and increase their productivity by moving to international markets are the most productive ones. As the table shows, we find no significant impact on the probability of exporting. This finding suggests that the positive impact found in the previous estimations must be mainly driven by the increase in export volumes among firms that were already exporting.

To further test this hypothesis, we now study the effect of the program on export volumes by restricting the sample to firms that were already exporting in the two years previous to treatment. These results are presented in table 11.

Table 11. Impact on Quantity Exported (Matched Sample)

	(1)	(2)	(3)
PUCR	1.1504***	1.0086***	1.0073***
	(0.374)	(0.338)	(0.338)
lage		-1.1934	-1.0868
		(1.449)	(1.458)
lskill		0.3043	0.4070
		(0.995)	(0.991)
lwage		0.9161**	0.8149*
		(0.434)	(0.424)
patentes		-0.0376	-0.0430
		(0.094)	(0.090)
finep		1.5790***	1.5522***
		(0.442)	(0.458)
premio		2.5402***	2.5346***
		(0.221)	(0.219)
limp		0.1336***	0.1358***
		(0.032)	(0.032)
Constant	11.0240***	2.8974	2.8140
	(0.207)	(5.797)	(5.818)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	✗	✗	✓
R^2	0.057	0.190	0.200
Obs.	3140	3140	3140
No. of firms	314	314	314

Cluster-robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

The findings reveal very large and significant impacts, supporting the hypothesis that the effect on exports is almost entirely driven by the increase in export volumes among exporting firms, while not affecting the probability of becoming an exporter.

In sum, the results for the matched sample confirm the previous findings; we estimate positive and large impacts on employment and exports, but not for the average standardized wages. Moreover, the impact on exports is driven by the increase in export volumes among exporting firms.

6.4 Dynamic Effects of the Program

While the previous results estimate the average impact for the whole post-treatment period, we now set the attention on analyzing the dynamic pattern of these effects. In other words, the interest is to disentangle the effect of the program to understand if those effects are constant or vary over time.

We modify our econometric specification by replacing the treatment variable with a dummy variable D_{it} that takes the value one in the first year of treatment and zero otherwise. Also we will use several lags of this variable, D_{it-k}, each one of those indicating the impact of the intervention in the k-th year of treatment. Table 12 shows the results for the three outcomes of interest.

Table 12. Dynamic Effects (Matched Sample)

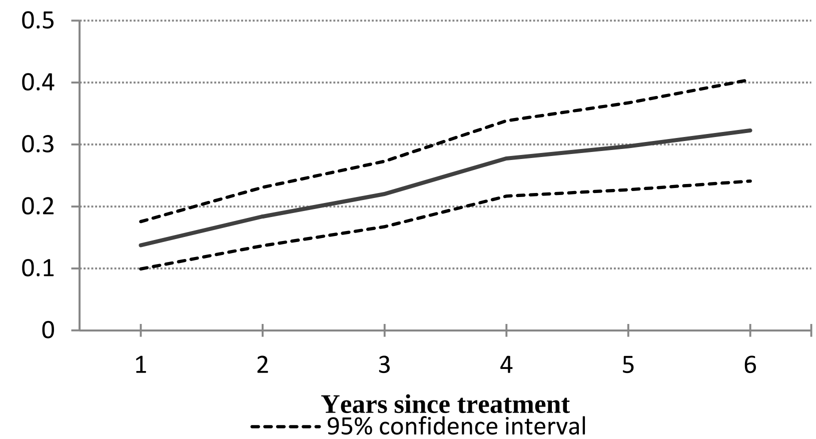
	Ln(employment)	Ln(exports)	Profits per worker
D_t	0.1373***	0.1191	0.0016
	(0.019)	(0.117)	(0.014)
D_{t-1}	0.1838***	0.3923***	-0.0089
	(0.024)	(0.119)	(0.013)
D_{t-2}	0.2201***	0.4076***	-0.0123
	(0.027)	(0.127)	(0.013)
D_{t-3}	0.2774***	0.5646***	-0.0095
	(0.031)	(0.140)	(0.015)
D_{t-4}	0.2971***	0.5262***	0.0084
	(0.036)	(0.140)	(0.018)
D_{t-5}	0.3227***	0.3787***	-0.0198
	(0.042)	(0.140)	(0.026)
lage	1.1604***	0.0015	-0.1083*
	(0.091)	(0.312)	(0.056)
lskill	0.0175	-0.1267	0.1370***
	(0.075)	(0.184)	(0.051)
lwage	-0.0256	0.2955*	0.8188***
	(0.087)	(0.159)	(0.143)
patentes	0.0063	0.0991	-0.0085
	(0.009)	(0.074)	(0.009)
finep	0.3144***	1.8690	-0.0931
	(0.080)	(1.349)	(0.133)
premio	0.1079***	4.4214***	-0.0275
	(0.027)	(0.212)	(0.019)
limp	0.0261***	0.0914***	-0.0058**
	(0.004)	(0.014)	(0.003)
Constant	0.2139	-0.0555	-5.3758***
	(0.605)	(1.370)	(0.839)
Fixed effects	✓	✓	✓
Time dummies	✓	✓	✓
Industry-year interactions	✓	✓	✓
R^2	0.18	0.211	0.324
Obs.	15700	15800	15790
No. of firms	1570	1580	1579

Cluster-robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

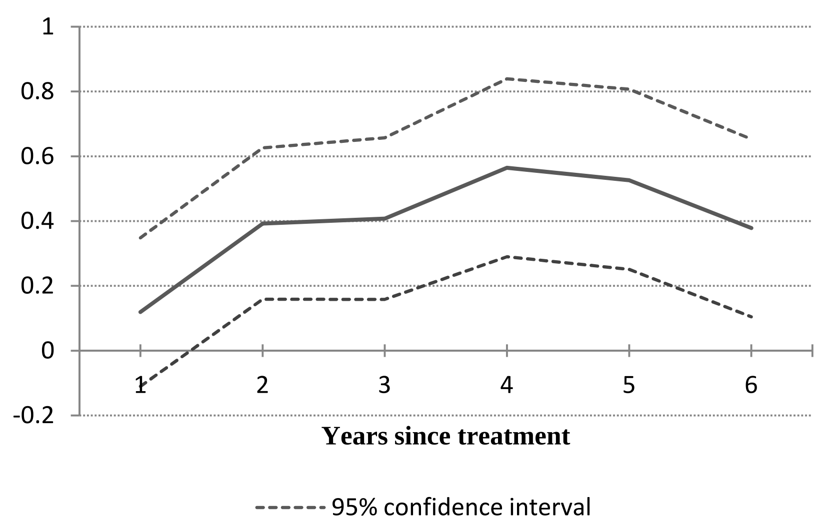
The impact on employment is always significantly positive and increases with time. In figure 7 these effects are depicted with its corresponding standard error. It is clear the strong and significant positive trend that the treatment has.

Figure 7. Dynamic Impact on Employment



In the case of exports, the impact of the program seems to take longer to appear in the regressions, since the coefficient for the first dummy is insignificant. The dynamic impact exhibits an inverted U-shape, increasing during most of the years but slightly decreasing in the last one. This pattern is displayed in figure 8 and although this evidence is preliminary it suggests the existence of an optimal duration of the treatment.

Figure 8. Dynamic Impact on Exports



7. Conclusions

The main objective of this paper was to provide evidence on the effectiveness of public credit line in Brazil. We find that access to public credit lines has a significant and robust positive impact on employment and exports, while we do not find evidence of a significant effect on our measure of productivity. Interestingly enough, our findings show that impact on exports is mainly driven by the increase in export volumes among exporting firms, while we do not significant effect on the probability of becoming an exporter.

These results suggest that the second-tier public credit system effectively foster firms' growth and, more specifically, it helps exporters to maintain and increase their operations, while they do not provide conclusive evidence of productivity gains. Some caution is probably needed when interpreting this lack of effect on productivity: in this case, the result may be more related to the specific indicator we are using rather than to a real lack of impact. In fact, one would expect a simultaneous increase of export and employment to be accompanied by improvements in productivity. Unfortunately, due to data limitation, we could not compute our preferred measure of productivity, TFP, and, therefore, we have to acknowledge that our results remain inconclusive in this particular aspect.

Because of the relevance and size of the state-owned Banks in Brazil, our findings offer a valuable contribution to the debate on which policy instruments should be used to support the development of a competitive productive system in emerging countries. Sound and wide access to credit has always been considered a key ingredient of any private sector development strategy. Our results show that the provision of credit through second-tier development banks in Brazil play a significant role in making credit available for firms and effectively improve firms competitiveness, in particular when measured in terms of volume of exports.

However, as in the case of most empirical studies, a good dose of caution is needed when impact evaluation findings are used in a debate on the alternative use of public resources. First, although they are consistently robust under our specification, one should carefully consider the external validity of our results. We limit our analysis to a set of credit line managed by two key state-owned development Banks (BNDES and FINEP). Therefore, our results only reflect the effectiveness of these institutions. Expand their significance to other sources of public credit in Brazil or other countries would require a set of well define interpretative assumptions. Second, because of methodological reasons we have focus on a particular cohort of credit recipients. The level effectiveness of public program could potentially depend on external factors that may vary other time. The period we considered may have been particularly problematic for private lenders and, therefore, we cannot exclude that the positive effects of the public credit lines we observe may depend on the that particular conjuncture. Finally, we are not able to complement our impact analysis with a similarly robust assessment of the efficiency of the credit lines we consider. Providing credit at certain condition could be quite costly for the public budgets. These costs should be more than compensated by the value of the benefits we observed in order to concluded that this specific use of public resources as a valuable return for the Brazilian society.

This paper also contributes to the methodological debate on how to evaluate the effectiveness of programs aimed at supporting firm-level performances. In particular, we show how to take advantage of a data setting that not only allow us to reduce selection bias by controlling for firm level fixed effects, but also to further improve the credibility of the difference-in-difference assumption by matching treated and comparison groups on the pre-treatment trends of the outcomes variables. Because administrative dataset with similar characteristics to the one we used are becoming more and more available, our estimation strategy may be replicated to evaluate similar programs in other emerging countries.

These contributions notwithstanding, further research is certainly still need in this area. First, as explained before, our preferred measure of productivity, TFP, could not be computed with the data available for this study. Microdata on TFP are potentially available in Brazil, though only for manufacturing firms. A first extension of this study will consider this measure. Second, future research should expand the analysis beyond average treatment effects. With access to more detailed information about the characteristics of the credit lines, we could analyze the heterogeneous effects that access to public credit line may have depending on loan terms, targeted firms populations and other specific requirements of the credit lines. Third, future research should also focus on better understanding the relationship between credit conditions and performances. For this purpose, one should be able to not only control for firm-level pre-treatment economic performances (which under reasonable assumptions could be consider a good proxy of a firm's financial health), but also for the firm-level financial characteristics. This kind of data are more complicated to construct, but they are potentially available in financial systems with a certain level of supervision and they could provide a key contribution to a better understanding of mechanism through which public credit lines affect firm-level performances.

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Appendix I. Construction of Variables

PUCR: dichotomous variable that takes the value of one if the firm borrowed from either BNDES or FINEP in 2001.

Employment: firm's total employment is constructed by counting the number of records in the PIS RAIS, weighting these counts by the number of months the employee was hired at the firm. For instance, an employee remaining employed throughout the year counts as being equal to 1, while if it remained employed for six months in the year it counts as 0.5. Thus this variable actually reflects the number of jobs provided by the firm during the year.

Exports: total value in U.S. dollars (U.S. $ FOB) of export transactions per firm in each year. This information was obtained through the sum of all operations into a single total exports per firm per year.

Labor Productivity: it is obtained through the difference between income and average income of the employee in the sector (CNAE4), in the unity of the federation (UF) and the class of personnel actions of the firms hired, according to the expression

$$Wpad_{ijlkm} = \frac{W_{ijlkm} - \bar{W}_{jlkm}}{STD(\bar{W}_{ijlkm})}$$

where W_{ijlk} represents the wage of the *i*-th employee in the *j*-th firm in the *l*-th location in the *k*-th sector of economic activity within the *m*-th size category. After the standardization of the average income of the employee, the averages are calculated for each firm, according to the expression:

$$\bar{W}pad_{jlkm} = \sum_{i=1}^{PO} \frac{Wpad_{ijlkm}}{PO_{jlkm}}$$

The above information represents a measure of labor productivity at the firm level.

Appendix 2. Balancing Tests

Table A1. Balancing Tests (Employment)

	Treated	Control	Difference
Y_{it-1}	3.404	3.368	0.036
Y_{it-2}	3.284	3.243	0.041
Y_{it-3}	3.210	3.165	0.045
Y_{it-4}	3.029	2.971	0.058
lage	2.758	2.743	0.015
lskill	2.127	2.122	0.005
lwage	6.719	6.722	-0.003
multi	0.387	0.299	0.087
patentes	0.056	0.075	-0.019
premio	0.151	0.133	0.017
limp	3.188	3.056	0.132
clapo_0	0.062	0.363	-0.300
clapo_1	0.753	0.778	-0.025
clapo_2	0.146	0.145	0.001

* significant at 10%;
** significant at 5%;
*** significant at 1%

Table A2. Balancing Tests (Exports)

	Treated	Control	Difference
Y_{it-1}	2.922	2.788	0.134
Y_{it-2}	2.685	2.557	0.128
Y_{it-3}	2.570	2.372	0.198
Y_{it-4}	2.293	2.112	0.181
lage	2.758	2.734	0.024
lskill	2.127	2.122	0.005
lwage	6.719	6.717	0.002
multi	0.039	0.026	0.012
patentes	0.056	0.040	0.016
premio	0.151	0.148	0.002
limp	3.188	2.899	0.289
clapo_0	0.070	0.067	0.003
clapo_1	0.753	0.767	-0.014
clapo_2	0.146	0.148	-0.002

* significant at 10%;
** significant at 5%;
***; significant at 1%

Table A3. Balancing Tests (Labor Productivity)

	Treated	Control	Difference
Y_{it-1}	0.016	0.007	0.009
Y_{it-2}	-0.0011	-0.0006	-0.0005
Y_{it-3}	-0.019	-0.025	0.006
Y_{it-4}	-0.026	-0.030	0.005
lage	2.758	2.746	0.012
lskill	2.127	2.122	0.005
lwage	6.719	6.693	0.025
multi	0.039	0.039	0.000
patentes	0.056	0.039	0.017
premio	0.151	0.141	0.010
limp	3.188	3.114	0.074
clapo_0	0.070	0.061	0.009
clapo_1	0.753	0.774	-0.021
clapo_2	0.146	0.142	0.004

* significant at 10%;
** significant at 5%;
*** significant at 1%