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The Impact of Credit Programs on the Post-Pandemic Reactivation of the Fabric of Production and Employment: The Case of Argentina

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Connectivity, Markets, and Finance Division

TECHNICAL NOTE
Nº IDB-TN-2728

September 2023

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Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library

The impact of credit programs on the post-pandemic reactivation of the fabric of production and employment: the case of Argentina / Alejandra Clemente, Martín Gonzales Rozada, Alejandro Támola, M. Carmen Fernandez Diez.
p. cm. — (IDB Technical Note ; 2728)
Includes bibliographical references.
1. Government aid to small business-Evaluation-Argentina. 2. Economic stabilization-Argentina. 3. Labor-Government policy-Argentina. 4. Coronavirus infections-Government policy-Argentina. 5. Coronavirus infections-Social aspects-Argentina. I. Clemente, Alejandra. II. González Rozada, Martín. III. Támola, Alejandro. IV. Fernández Diez, María Carmen. V. Inter-American Development Bank. Connectivity, Markets, and Finance Division. VI. Series
IDB-TN-2728

http://www.iadb.org

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THE IMPACT OF CREDIT PROGRAMS ON THE POST-PANDEMIC REACTIVATION OF THE FABRIC OF PRODUCTION AND EMPLOYMENT

The Case of Argentina

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Abstract*

This technical paper seeks to narrow the knowledge gap concerning the impacts of financing and provision of credit to accelerate recovery and adjustments in the wake of a crisis. Due to the lack of data in developing countries, the literature surrounding evaluation of this type of program in Latin American and Caribbean countries is relatively recent and scarcer still is the measurement of the impact of such programs in the context of the crisis caused by the COVID-19 pandemic. This work analyzes the impact of credit programs on the recovery productive structure and employment throughout the entire territory of Argentina, using key economic variables, and distinguishing between the different instruments utilized. The results suggest that the programs did indeed help to increase the number of workers employed and their real wages in beneficiary firms during the first three quarters of 2021; moreover, they impacted the sustainability of these Argentinian firms, increasing their chances of survival.

JEL codes: C21, D22, J21, J31
Keywords: impact evaluation, real wages, employment, credit, SME, firm survival

* The authors wish to acknowledge the excellent work of Sofía Rojo and Lara Lening who built the database for the evaluation of the Global Credit Program for the Reactivation of the Productive Sector (Programa Global de Crédito para la Reactivación del Sector Productivo) and processed all of the information required for the estimates presented in this paper. They are also grateful for the comments of Philip Keefer.

Executive Summary

The challenges of accessing credit for micro, small, and medium firms (MSMEs) intensify notably during a crisis. Specifically, these firms face greater barriers to access the credit they need to survive the immediate situation and, thereafter, recover in the long term. In response to the profound crisis generated by COVID-19, the Inter-American Development Bank (IDB) promptly implemented a financing plan to offer support to countries in the Latin America and Caribbean (LAC) region. As part of this plan, the Global Credit Program for the Reactivation of the Productive Sector was implemented in Argentina, and, according to the results of the impact evaluation presented in this technical paper, contributed to the sustainability of employment, salaries and, to a lesser degree, to the survival of beneficiary firms.

The purpose of the financial program was to support the sustainability of MSMEs as significant providers of employment in Argentina in the context of the crisis caused by the COVID-19 pandemic, establishing as specific objectives the support for short-term financial sustainability and promotion of economic recovery via access to productive financing. Medium-term credit lines backed by IDB resources were directed at MSMEs and sought to reestablish productive capacity, productive restructuring, and adaptations to the digital transformation process, as well as other emerging demands arising from the crisis.

The program made up of 14 lines, segmented in 3 groups: the first set of credit lines to respond to the economic emergency (REE lines), a second set oriented to providing working capital (WC lines) and a third set directed towards productive investment (PI lines).

This technical paper presents the results of the impact evaluation of the emergency financing program offered. A difference-in-differences methodology was used as the principal tool to detect impacts, during the first three quarters of 2021, on employment, real wages, and survival of the firms benefitting from the program's lines of credit. The following table shows the

TABLE A

MAIN EFFECTS ON EMPLOYMENT, REAL WAGES AND FIRM SURVIVAL OF THE GLOBAL CREDIT PROGRAM FOR THE REACTIVATION OF THE PRODUCTIVE SECTOR

	All firms	Firms supported with working capital	Firms supported with credit for productive investment
	(1)	(2)	(3)
Effect on employment	+	+	+
Effect on real wages	+	+	+
Effect on firm survival	+	+	+

Source: Authors' elaboration, based on results from the impact evaluation of the Global Credit Program for the Reactivation of the Productive Sector (AR-L1328).

main results of the program on employment and real wages.[1]

Column (1) indicates the impact of the program on employment and real wages in all the beneficiary firms; Column (2) presents the impact on the firms that accessed lines of credit for working capital, and Column (3) shows the impact on the firms that accessed lines of credit for productive investment. In all cases, it was found that the financing program had a positive (and statistically significant) effect, both on employment and on real wages, during the first three quarters of 2021. With regards the magnitude of the impacts, the results (detailed in Section 6.1), indicate an average increase of around 0.6 percent on employment in beneficiary firms. This positive impact is of a much greater magnitude for the firms that accessed lines of credit for working capital and productive investment. Regarding employee compensation in beneficiary firms, the results indicate that the program has had a positive effect, increasing real wages by around 3 percent, with the greatest increase among the workers whose firms received lines of credit for working capital (+6 percent). The results also suggest a positive and statistically significant effect of the program on firm survival, in particular for those that accessed lines of credit earmarked for working capital and productive investment.

Along with these general results for the beneficiary firms as a whole, the firms were also evaluated based on size (micro, small, and medium), leadership (women-led firms), export activity, and geographical location. When assessing the impact of the Global Credit Program for the Reactivation of the Productive Sector according to firm size, the greatest impact was seen on employment and the real wages of workers in small firms, compared with that observed in micro and medium enterprises. When it comes to the geographical location of the firms, the evidence presented indicates that the program had a positive impact on real wages in beneficiary firms located in the Autonomous City of Buenos Aires (CABA) and the rest of the country, whereas the impact was greater on employment than on real wages for firms located in the Province of Buenos Aires (PBA).

The program also had a positive effect on the real wages of workers in women-led firms,

[1] The original database was comprised of 11,136 beneficiary firms for any of the program's 14 lines of credit, and 350,615 non-beneficiary firms. After matching, the treatment group has a total of 10,933 firms, while the control group is made up of a total of 28,656 different non-beneficiary firms.

as compared to firms directed by women which did not benefit from the program. With regard to exporting firms, the evaluation suggests that the program contributed to an increase in the number of workers in these firms, compared with firms that did not export in the baseline. In conclusion, the results of the estimates suggest that the Global Credit Program for the Reactivation of the Productive Sector has helped the sustainability of employment, wages, and—to a lesser extent—the survival of the beneficiary firms, which demonstrates the value of the financial support provided by this type of program.

1

Introduction

Productive financing programs for micro, small, and medium firms (MSMEs) helps to increase growth by improving the ability to obtain financing and reducing credit restrictions. This is achieved by targeting segments where the aforesaid credit restrictions and market failures are most pronounced. Previous studies suggests that MSMEs, due to their nature, face problems that lead to credit restrictions (Jaffee and Russell, 1976; Stiglitz and Weiss, 1981) and are one-third more likely to face financial restrictions, compared with large firms (Beck, Demirgüç-Kunt, and Maksimovic, 2005).

Financing programs for small and medium firms (SMEs) have been effective in resolving some of these market failures. Public intervention can alleviate financial restrictions for firms with profitable projects, enabling them to improve the conditions that boost growth and productivity, such as low rates of capitalization and investment. In Colombia, for example, an expansion of economic activity in terms of SME sales was achieved, with a growth rate of 4 percent (Eslava, Maffioli, and Meléndez, 2014).

Difficulties in accessing credit for MSMEs get notably worse during a crisis (Cowling, Liu, and Ledger, 2012; Lee, Sameen, and Cowling, 2014). Due to COVID-19, there were disruptions to supply chains, consumer demand, and financial and labor markets. In this situation, MSMEs faced even greater barriers to accessing loans needed to survive the immediate situation and to recover in the long term (IDB, 2021). Public policies to support the fabric of production, synthetized in Herrera (2020), include monetary measures, such as flexible administrative charges and tax freezes, as well as financing to provide liquidity for these firms in the short term. The balance between reducing the number of firms going under along with the fiscal costs creates a need to target efforts and demonstrate their effectiveness and as well decide whether they are effective policy tools for maintaining investment, creating employment, and increasing productivity. The financial system can

use its position to facilitate the reallocation of factors (work, capital, and technology) by providing credit that helps accelerate the transition towards reestablishing equilibrium (Támola and Fernández Díez, 2020; Bebzuk, Fernández Díez, and Támola, 2021). This study helps narrow the knowledge gap on the impacts of financing aimed at accelerating recovery and adjustment in the wake of a crisis, where there is provision of credit to reach a state of equilibrium.

In October 2020, the Global Credit Program for the Reactivation of the Productive Sector was approved, and consisted of redirecting resources from previous loan contracts approved by the Republic of Argentina, totalling USD 500 million.[2] This modification took place in the context of the COVID-19 crisis and the need for funding, The general objective was to support the sustainability of MSMEs as providers of employment in the country during a global crisis, specifically to support financial sustainability in the short term and promote economic recovery by way of productive financing. The program consists of 14 lines of credit, detailed in Table 1.

[2] On 20 October 2020, the Loan Modification Agreement (Contrato Modificatorio de los Contratos de Préstamos) was signed by the Republic of Argentina and the IDB.

TABLE 1
LINES OF CREDIT OF THE PROGRAM

Lines of credit	Type of assistance	Beneficiaries	Current state
Capitalization of the Argentine Guarantee Fund (FOGAR) (Capitalización al Fondo de Garantías Argentino)	Guarantee		Executed
Credit line for simplified taxpayers	Response to the economic emergency (REE)	23,300	Executed
Credits for Reconversion of the Emergency Assistance Program for Work and Production (ATP) (Créditos para reconversión del Programa de Asistencia de Emergencia al Trabajo y la Producción)	Response to the economic emergency (REE)	Design stage	Executed
Credits for the tourism sector	Working capital (WC)	723	Currently being executed
Credits for the cultural sector	Working capital (WC)	112	Currently being executed
Credits for cooperatives	Working capital (WC)	59	—
SME plus	Working capital (WC)	4,123	Executed
Direct line of credits (COVID-19)	Productive investment (PI)	124	Executed
Federal development – working capital	Working capital (CT)	2,000	Currently being executed
Federal development – investments	Productive investment (PI)	600	Currently being executed
Productive investment and financial inclusion – direct credits	Productive investment (PI)	700	Currently being executed
Line of productive investment (LPI SMEs) with international financial institutions (IFIs)	Productive investment (PI)	4,000	Currently being executed

Source: SePyME. Execution status until December 2022.

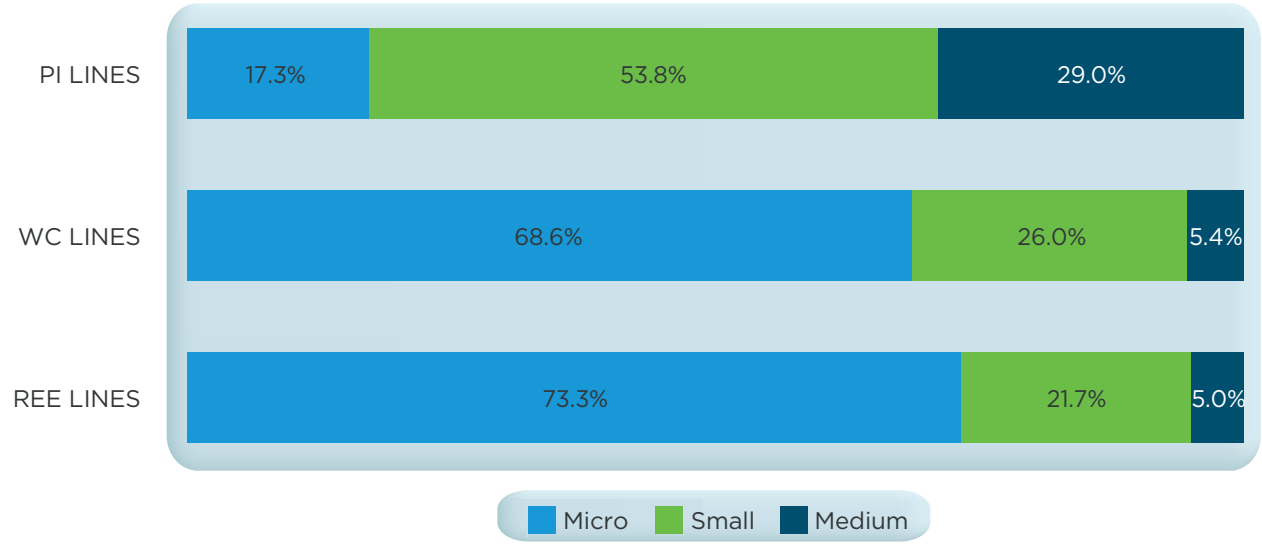
These 14 lines can be divided into three groups: a set of lines of credit responding to the economic emergency (REE lines), a group of lines of credit apportioning working capital (WC lines), and another set for productive investment (PI lines). The majority of the beneficiary firms received subsidies or credits at a 0 percent rate, or else at a subsidized rate; however, there are differences in the amounts received. The lines of credit for working capital and productive investment are which most resemble the support provided by traditional productive development programs, whereas the REE lines were a response to the emergency caused by the pandemic and were aimed at mitigating the economic impact of COVID-19. Thus, the assistance offered by the REE lines was substantially less than that of the first two lines.

The distribution of these types of lines according to the size of the beneficiary firms reveals some different traits. In general, for the baseline used for the evaluation (fourth quarter of 2020, see below in this paper), it is noticeable that the average size of the firms that accessed lines of credit in response to the economic emergency is smaller than that observed in the other two categories, with an average of nearly 10.7 employees per firm. In the case of the lines intended for working capital, the average size of the firms was 12.5 employees. Finally, for the lines of credit oriented to productive investment, the average size per firm was 42.1 employees. The differences in the average size of the firms by type of line can be explained by the differences in the composition of the beneficiary firms that accessed the lines of credit according to their size (see Figure 1).

In the case of the REE lines, 73.3 percent of the beneficiaries were micro firms; 21.7 percent were small firms, while the remaining 5 percent were medium sized firms. In the case of the WC lines, 68.6 percent of beneficiaries were micro firms; 26 percent were small firms, while the remaining 5.4 percent were medium sized firms.

FIGURE 1

COMPOSITION OF THE BENEFICIARY FIRMS OF EACH LINE OF CREDIT ACCORDING TO SIZE



Source: Authors' elaboration, based on data from SePyME.
Note: PI (Productive Investment); WC (Working Capital); REE (Response to the Economic Emergency).

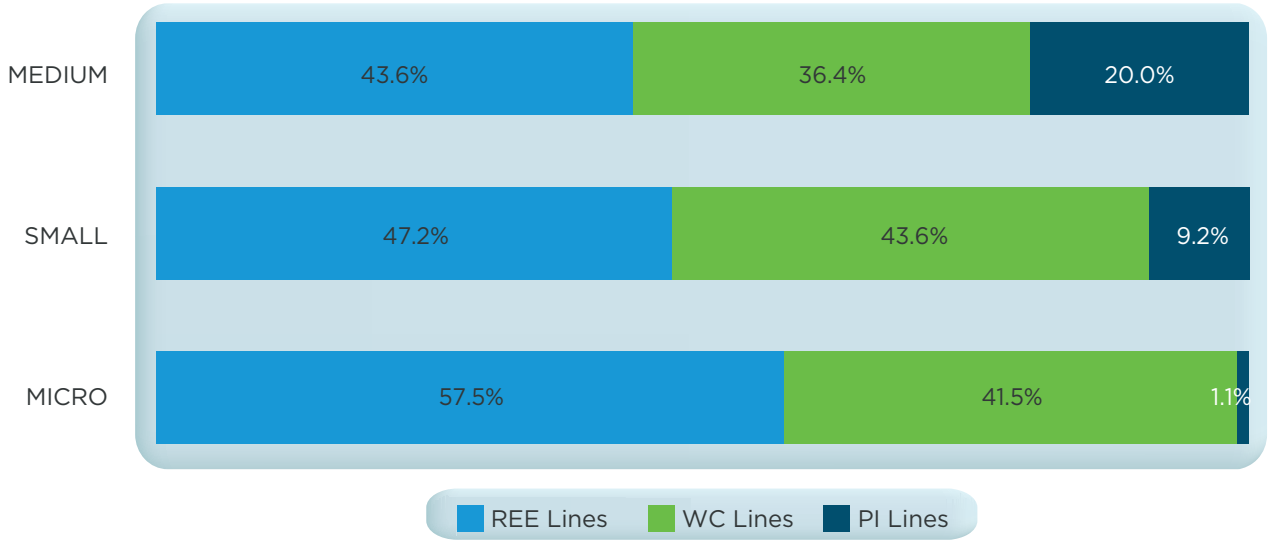
Finally, in the case of the productive investment lines, a greater share of small firms is seen, with 53.8 percent of the cases; in second place, 29 percent of the firms of this line are medium, while the remaining 17.3 percent are small firms (see Figure 1).

Figure 2 shows the distribution of the beneficiary firms by type of line of credit. As observed in the Figure, nearly 60 percent of credits accessed by micro firms were in response to the economic emergency. Exactly 20 percent of the lines of credit accessed by medium firms were for productive investment, compared to small firms, where less than 10 percent of credits included PI lines, while micro firms accessed only 1 percent.

The lines of response to the economic emergency include the Emergency Assistance Program for Work and Production (ATP) (Programa de Asistencia de Emergencia al Trabajo y la Producción) and credits for persons signed up to the Simplified Small Business Tax Code (Régimen Simplificado para Pequeños Contribuyentes (Monotributo)) and Small Business and Self-Employed Tax Code (PCA) (Pequeños Contribuyentes y Autónomos). These are lines of credit at a 0 percent rate for simplified taxpayers and the self-employed, and/or credits at a lower subsidized rate. The assistance received was a one-off loan of ARS 100,000. There are five lines of credit to finance working capital (or operating costs), two of which are designated for the tourism and culture sectors (Banco de la Nación Argentina [BNA] Turismo and BNA Cultura, respectively), and a third designed for cooperatives (BNA Cooperativas). These lines permitted access to credits where the maximum amount would be equivalent to 2.5 months of turnover for each firm, with a limit of up to ARS 10,000,000 for beneficiaries in the tourism sector, and ARS 7,000,000 for beneficiaries in the culture sector. In the case of cooperatives, the loans had an upper limit of ARS 2,000,000. Finally, within this category, federal

FIGURE 2

DISTRIBUTION OF THE BENEFICIARY MICRO, SMALL, AND MEDIUM FIRMS ACCORDING TO TYPE OF LINE OF CREDIT



Source: Authors' elaboration, based on data from SePyME.

development lines of credit were available for MSMEs in Chaco, Entre Ríos, La Rioja, Neuquén, San Juan, Santa Cruz, Río Negro or Salta, where it is expected that 20 percent of the credits will be granted to women-led SMEs. There are four lines of credit for productive investment in total and these permit access to an amount of up to ARS 2,000,000. Furthermore, direct credits for productive investment offered to micro and small firms from other sectors, such as industry, industrial services, auto parts, agroindustry, oil and gas, metallurgy, textile and footwear, provided access to between ARS 200,000 and ARS 3,500,000.

2

Review of the Literature

The present analysis is concerned with three generally differentiated themes: (i) in a general sense, literature related to productive development policies and, in particular, public financing programs (including public guarantee programs), designed to increase access and improve credit conditions for firms, fundamentally MSMEs; (ii) firm survival and financing, also of MSMEs, during periods of financial crisis and recessions; and (iii) recent literature targeted at analyzing interventions to support firms in response to the emergence of the COVID-19 crisis, considering, in particular, financing interventions.

2.1. Productive Development Interventions

When it comes to evaluating productive development policies and programs directed at innovation and knowledge in small firms, the majority are concentrated in developed countries. In general, due to the lack of data in developing countries, literature on evaluation of this type of program in Latin American countries is relatively recent. Within this group, a series of impact evaluations methodologically aligned with the evaluation of the Global Credit Program are reviewed below.[3]

In Mexico, Aparicio et al. (2021) evaluated the effectiveness of loans granted by the National Development Finance Agency (FND) (Financiera Nacional de Desarrollo) to rural producers and explored the potential difference in effects generated from credit for working capital and loans for fixed assets. The authors found that credit enhanced the probability of agricultural producers cultivating and selling their produce, that the use of improved inputs intensified, and that there was a switch from non-remunerated to remunerated labor. Most of these effects seem to be driven by loans for working capital, which suggests that lack of liquidity has been the most significant limitation for rural producers.

[3] This first section is not an exhaustive review; rather, it focuses on the reports related to impacts analyzed for the Global Credit Program for the Reactivation of the Productive Sector.

In Argentina, one of the elements already in place previous to the Global Credit Program was the technical assistance provided to the MSMEs co-financed with non-reimbursable contributions (NRC) from the Access to Credit and Competitiveness Program (PACC) (Programa de Acceso al Crédito y Competitividad), of the Undersecretariat for Small and Medium Enterprise and Regional Development (SePyME) (Subsecretaría de la Pequeña y Mediana Empresa y Desarrollo Regional), a department of the Ministry of Industry (Ministerio de Industria de la Nación Argentina). The main aim of the PACC is to finance, via non-reimbursable contributions (NRCs), technical assistance to firms to develop their business capacities and improve their competitiveness. Through technical assistance from the PACC, it was hoped to mitigate the effects of the diverse market and coordination failures, thereby enabling investment projects to emerge and ultimately have an impact on MSME performance. The first part of the program was developed between 2009 and 2015. In 2016, a new call for proposals was made, to encourage small and medium firms to sign up to the program.

Castillo et al. (2016), using a statistical matching technique alongside two panel data methods, evaluated the impact of the first part of the program and found that the PACC had a positive and significant impact on the growth of firms, measured in terms of employment, average wages paid by these firms, the probability of export, and the volume of exports. Moreover, the PACC beneficiary firms had better chances of surviving than firms in the control group. The panel data methods employed helped the authors to define a lower and an upper limit for the estimated impact. The results reported for the impact of the program on employment indicate an increase of between 5 and 18 percent with respect to firms in the control group. The program had an effect of between 1.4 and 2.5 percentage points on the probability of exporting and, furthermore, increased exports by between 6.1 and 9.3 percent for those firms that already exported. Finally, the authors reported positive and significant effects, both on the probability of firm survival where there was an increase of between 1.3 and 1.6 percentage points, and on average wages, of between 0.6 and 1.8 percent, compared with the control group.

Franco Churruarín and González-Rozada (2022) extend these results to the second stage of the program that began in 2016. The authors use the estimator of difference-in-differences (DD) of Card and Krueger (1994) with temporal variation in the treatment (the beneficiary firms join the program at different points in time, or staggered DD) and estimate the "heterogeneous treatment effects with staggered adoption", following the method proposed by Callaway and Sant'Anna (2021). The authors report that the program had a positive and statistically significant effect on employment, salaries, and the probability of survival of beneficiary firms, compared with the firms in the control group. The program boosted employment by around 9.5 percent in the beneficiary firms, compared with the control group firms. Furthermore, there is an increase of around 2 percentage points in the probability of survival and 3 percentage points in the average wages paid in the beneficiary firms. There is also an increase of 2.1 percentage points in the probability of exporting with significant effects in the volume of exports. The average impact of the PACC on employment is greater for firms in the goods production sector, compared with those operating in the service sector. The firms in goods production report an increase of nearly 10 percent in average number of employees, whereas for firms in the service sector, this increase is a little over 7 percent. The opposite is true when it comes to

chances of survival, where the program seems to have had an average impact of greater magnitude among firms in the service sector. The positive effect on the probability of export is explained mainly by the impact on firms in the service sector, whereas the average aggregate impact on salaries is concentrated in firms belonging in the goods production sector.

Using the same methodology, Franco Churruarín and González-Rozada (2022) analyze the impact of the program in one of the PACC components, a branch of support for productive conglomerates. These productive conglomerates are comprised of groups of micro, small, and medium firms belonging to the same sector and geographical region. The aim of the program is to contribute to productive development and encourage local production systems, value chains, and productive conglomerates, thereby improving the competitiveness of productive industrial conglomerates. To this end, this component of the program proposes financing for Initiatives for the Improvement of Conglomerates (Iniciativas de Mejora del Conglomerado), by granting non-reimbursable contributions (NRCs) for activities which can help improve competitiveness and boost the productivity of incipient productive conglomerates within a specific territorial area. The results presented by the authors suggest that the program has helped to increase employment and the probability of survival for firms belonging to the same geographical zone and productive sector. The impact of the program on the salaries of workers in these firms, compared to the firms of the control group, is positive but its statistical significance is less than that observed for employment. Furthermore, the program does not seem to have had a statistically significant impact on the probability of the beneficiary conglomerates exporting more, compared to the control group firms.

2.2. Firm Survival and Interventions in Response to the COVID-19 Crisis

The financial support program analyzed here arose in the context of, and in response to, the COVID-19 crisis. In particular, it sought to compensate, at least partially, for the financial problems generated by the crisis and act as one of the mechanisms for promoting firm survival. In this sense, it is worth reviewing the literature on firm survival in contexts of financial crises and also the literature covering COVID-19 interventions. With regards the first group, Tsoukas (2011) shows that financial development can have a significant impact on firm survival and in particular finds that the development of the stock market is associated positively with firm survival in emerging economies, while also signaling that bank intermediation can have a negative impact on firm survival, since banks tend to lend to firms which are already successful, and may be more reticent when it comes to lending to new firms, or those in difficulties. The relevance of the banking sector for firm survival during a crisis is analyzed by Abildgren et al. (2013). In their analysis, the authors find that the probability of firms experiencing an adverse event (default) is greater for those whose relationship with banks is less solid, thereby pointing to the importance of pre-existing financial relations as a determinant of the performance of the real economy when faced with situations of instability. A similar result is found by Meslier et al. (2022), after analyzing data from France at the regional level for the period 2005–2013. The authors find that long-term bank-firm relationships have a generally positive effect (including job creation), observed mainly in micro firms and among the small and medium firms that are less transparent, both in normal periods as well as in a crisis. This is consistent with the theory

that suggests that financial institutions which interact more closely with their clients and have fewer layers of management, with less hierarchical distance between loan officials and bank managers, are better equipped to collect and process non-financial information and therefore, better able to provide loans to smaller and less transparent firms.

Given that financial frictions and information problems which hamper firms' access to credit worsen in situations of crisis, the question arises as to the most effective way of deploying government funds to support the real economy during a crisis such as the COVID-19 pandemic which caused, among many other issues, severe problems of liquidity that particularly affected MSMEs. Among the battery of support measures implemented by governments during the crisis, was the channeling of resources by the banking system, an extremely important measure in the Latin America and Caribbean region, where the financial system is fundamentally based on these kinds of intermediaries. It is worth noting, however, that the recent literature suggests that pre-existing relationships can bias the allocation of credit even when the overall conditions vary significantly, which might mean that those firms most in need of liquidity are not adequately served.

The latter point has been explored by Core and De Marco (2021), for the case of public guarantees to small firms in Italy in the context of support measures for COVID-19. The analysis revealed that the supply factors (bank heterogeneity) had a more significant influence on loan conditions than the demand factors; the analysis also highlighted the importance of the network of bank branches, given that credit relationships and a local presence played a role in the allocation of credit. On the demand side, it was found that financially fragile firms, including small firms with limited cash reserves and higher leverage, and those classified as "zombie firms", reported higher rates of uptake of guaranteed loans.[4] A similar result was observed in Switzerland with regard to the probability of participating in a liquidity support program during the COVID-19 crisis, implemented via guaranteed loans. As Fuhrer, Ramelet, and Tenhofen (2021) report, analysis of the program revealed that the probability of participation responded to liquidity ratios (those with lower ratios had a higher probability of participating), whereas borrowing by firms and the presence of zombie firms did not significantly influence participation. Additionally, when it comes to the possible effectiveness of the program targeting, there was a larger relative presence of younger and smaller firms.

A study of interest that considers the characteristics of the beneficiary firms and the criterion of targeting is that by Huneeus et al. (2023), which looks at the public guarantees program, a part of the packet of support measures implemented in Chile during the COVID-19 crisis. The focus of analysis are the variations in aggregate risk, distinguishing between microeconomic factors and aggregate factors. Similarly, as in previous studies, the authors verified that the program effectively increased the volume of credit conceded, while keeping risks of delay and non-payment in check. The authors consider that containing the aggregate risk was affected, fundamentally, by factors of design and equilibrium behavior. Among the former, related to targeting

[4] These characterizations, for both the supply and the demand side, suggest a certain level of targeting efficiency, given that those firms in a delicate situation before the start of the crisis and with difficulties to access credit in that period would have less consideration for the supply side. Therefore, the financing would be mainly directed toward viable firms in the pre-crisis period, when they first began to notice liquidity problems. Although it might be argued that this is an imperfect process, it must be considered in the light of the increasing uncertainty that was generated, above all in the initial months of the pandemic.

criteria, the program imposed borrowing limits with regard to sales, interest rate ceilings, and a solid repayment history; with regard to the equilibrium conditions, the authors note that the bias towards the large and safe borrowers was preserved. In other words, the analysis shows that the targeting conditions were based on the basic guidelines for credit allocation already observed by the banks. This characterization is important because it implies that the characteristics of those who obtain resources from a program need not be so dissimilar from those that obtain them from outside the program.[5] The most direct consequence of this is that, in the event of fewer differences in the characteristics that determine access to the resources, it is reasonable to expect fewer differences with regard to the results. This is an important corollary that is relevant when it comes to providing a rationalization of the results, reported in Section 6.

A further study by Guerrero-Amezaga et al. (2022) analyzes, among other aspects, the process and the implicit selection bias in the design of credit support policies for Latin American firms during the COVID-19 crisis. Using survey data that covers around 35,000 small firms, the authors presented evidence that the smallest firms (with fewer than five full-time employees) and the informal firms were less aware of the existing programs, less inclined to request support, and had fewer probabilities of receiving it.[6] In a systematic analysis of economic policies in the region, they show that the great majority of economic policies for firms, stipulated formality as a basic criterion for eligibility. This, added to the fixed costs implicit in the request process, and the fact that informal firms often suffer from restricted access to banks and financial institutions, proved to be a limiting factor in terms of access to subsidized loans. Once more, this is an important finding that goes some way towards explaining the differences in results between firms that obtained support credits and those that did not.

A similar intervention to that studied in this document and fully analyzed, is the Paycheck Protection Program (PPP). Granja et al. (2022) analyze this intervention, focusing on targeting and on its effects on employment. The results of the analysis suggest that the funds were not effectively channeled to the regions that were most seriously affected by the pandemic but rather to those suffering a lesser impact, at least initially. On top of this, bank heterogeneity played a significant role in determining which firms received funds, and how rapidly their requests were processed. The banks with highest capacity to process loans and existing relationships with the Small Business Administration (SBA), and greater supervision, tended to better perform when it came to distribution of PPP loans. Despite the implementation of the PPP on a broad scale, the study reveals that it had only a limited impact on employment in the months following its launch.

The program and the determinants of effective resource distribution have also been analyzed by Li and Strahan (2021). In their analysis, the authors find evidence that preexisting relationships and experience in the local market were relevant factors in determining the distribution of resources. Regarding the effects of the program, they show that it had relatively minor effects on containing the rise in unemployment. The conclusions of these studies are similar to others who have examined the Paycheck Protection Program (Cherry et al., 2020; Barraza, Rossi, and Yeager, 2020).

[5] Reinforcing this characteristic, the authors show that there is no difference in the distribution of credit among firms in municipalities in lockdown and firms in adjacent municipalities with free circulation and that there was a generalized adoption of the credit program, irrespective of the impact of the pandemic.

[6] Moreover, the knowledge gaps, the requests for, and the acceptance of, support increased throughout the pandemic.

3

Objectives and Evaluation Methodology

The main objective of the evaluation has been to measure the potential impacts that may have occurred as a result of access to program credit resources by beneficiary firms during the crisis generated by the COVID-19 pandemic. The scope of this evaluation was provided by the set of MSME employers registered in the Small and Medium Enterprise and Entrepreneurs Secretariat (SePyME) (Secretaría de la Pequeña y Mediana Empresa y los Emprendedores) database. This is an administrative database and in order to maintain the confidentiality of its records, the work was conducted in conjunction with the agency itself.

3.1. Evaluation Methodology of the Program AR-L1328

The aim of evaluating the program's impact was to determine whether there was an improvement among participant firms in terms of certain variables of interest (denominated result variables), such as probability of survival, number of employees, and salaries paid.

The evaluation sought to answer the following question: in terms of the result variables, what do the firms' participation in the program gain, compared to not having participated? In general terms, this question could only be answered if it were possible to observe the situation of the beneficiaries of the program after the benefits had been granted if, instead of having participated in the program, they had not done so. That is, ideally speaking, the change in the result variables attributable to the program could be exactly determined if it were possible to observe a firm after having received the benefit of the program, and the same firm in the counterfactual state, that is, without having participated. However, it is obvious that the firm can only be observed in one of these

states. Therefore, if the firm participates in the program, it is impossible to observe it in a situation in which it has not participated, and vice versa.

There are different methods of evaluation that seek to resolve this problem, and which enable estimating changes in a firm's situation that are attributable exclusively to its participation as a program beneficiary. Methodologically, two potential states might be imagined for the same firm. The first would be a state in which it received the treatment, which reflects the situation of a firm after having participated as a program beneficiary. The second would be a state of not having received the treatment, which would reflect the situation of the same firm, at the same time following implementation of the Global Credit Program, of not having participated as a program beneficiary.

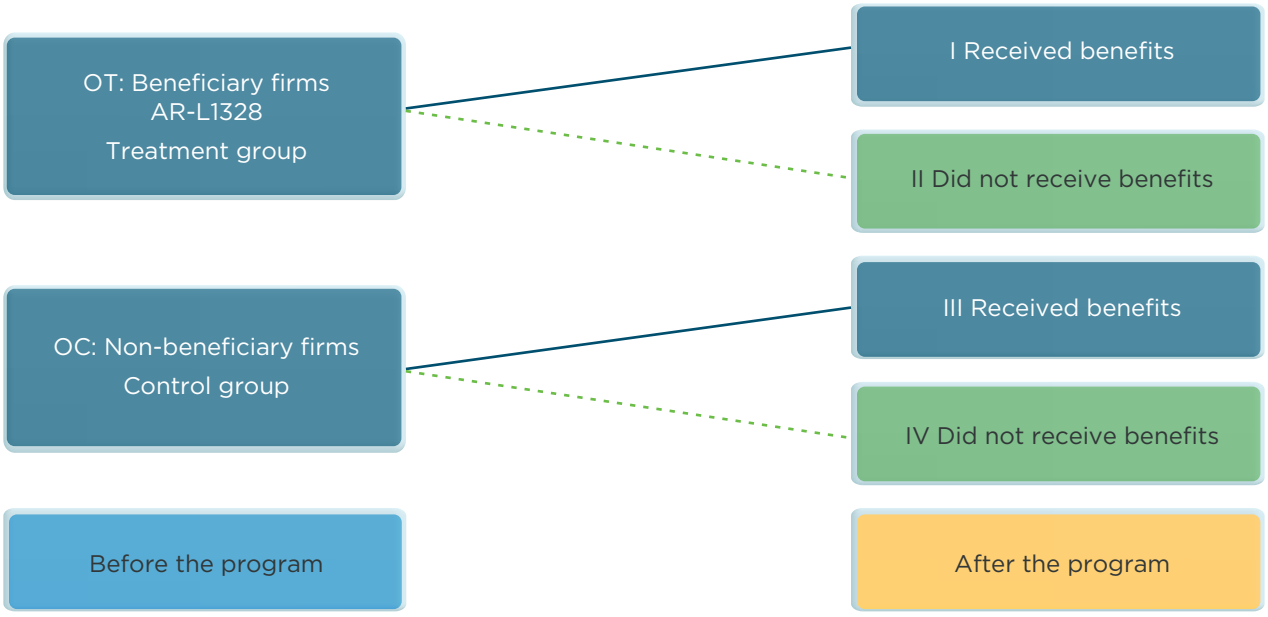
Given that it is impossible to observe the beneficiaries in the state of not receiving the treatment, that is, in the counterfactual situation of not having participated in the program, such a situation must therefore be estimated. A control group is used for this purpose, that is, firms of the same characteristics that have not participated in the program, thereby making it possible to estimate the situation of beneficiaries in the state of not receiving treatment.

The literature of impact evaluation identifies two ways of constructing the treatment and control groups. The first, known as randomized experiment, rests on designing the experiment in such a way that the assignment of the firms to the treatment group or the control group is carried out randomly. The second is based on identifying the causal effect via observational data, using quasi-experimental techniques. In general, in the social sciences it is not possible to use experimental design (in this case it would mean that the firms were randomly assigned to the program). In its place, quasi-experimental methods are used to estimate the causal effect of a policy or program. One of the advantages of experimental design is that random assignment guarantees that the firms of the treatment group are similar, in observable and non-observable characteristics, to the firms of the control group. When randomization is impossible, and there is a certain self-selection in the treatment, quasi-experimental methods may be used to define the counterfactual situation. In these cases, a relevant question in the evaluation is the degree to which firms in the control group are comparable to the state of not receiving treatment for the group of beneficiary firms.

Figure 3 exemplifies the situation described. The impact evaluation seeks to determine the difference in the result variables that arises from comparing Situations I and II, that is, how much better the firm is as a consequence of the program in the period following program execution. In other words, the result variable, for example, the number of employees in a firm after receiving the program benefit (Situation I of Figure 3) may be, compared to the number of employees that the same firm would have at the same point in time if it had not received the benefit of the program (Situation II of Figure 3).

The basic problem of the evaluation is that it is impossible to observe Situation II for the beneficiary firms of the program, which makes it necessary to estimate such a scenario based on a control group. Situation III corresponds to a firm which did not receive benefits from the program, observed in the period following program implementation. Insofar as the firms in Situation III are similar to those in Situation II, it would be valid to estimate the impact of the program on the number of employees of the beneficiary firms, as the difference in the number of employees in Situations I and III. This similarity of the firms in Situations II and III is what guarantees the experimental design.

FIGURE 3
IMPACT EVALUATION DIAGRAM



Note: The solid lines and rectangles in blue indicate the observable situations, while the grey broken lines and rectangles represent counterfactual situations (unobserved).

How can the control group be chosen to ensure that the firms in Situation III have very similar characteristics to the program beneficiaries in Situation II? The answer to this question is fundamental in all evaluation processes that use quasi-experimental methods.

The control group is generally made up of firms wishing to participate in the program, but which were either too late to sign up or could not do so for another reason, or else of firms which, though eligible for the program, did not participate. This is the case for the impact evaluation of the program AR-L1328.[7] The control group is comprised of the set of employer firms identified in the SePyME database that have similar characteristics to those that participated.

The control group must be comparable to the group of program beneficiary firms, not only in observable characteristics, but also in non-observable characteristics. For example, even after having selected a control group of firms with observable characteristics similar to the beneficiaries, it is possible that the eligible firms which did manage to participate in the program are much more enterprising or innovative than those which failed to do so. If this were true, the situation of the beneficiaries following the program, after not having received the program benefit (Situation II), would have achieved a better performance than the firms in the control group (Situation III). In this case, a direct

[7] The initials AR-L1328 or program will be used to make reference, accordingly, to the Global Credit Program for the Reactivation of the Productive Sector (AR-L1328) and to the Technical Regional *Cooperation Support to the Institutional Strengthening of Public Development Banks in the Use of Digital Tools* (RG-T3488), which supported the evaluation process.

comparison of the result variables in Situations I and III would not reflect differences attributable solely to the program, but also differences in characteristics —non-observable— between both groups.

In order to resolve the potential problem of selection bias in the fourth quarter of 2020, a matching procedure was used based on Mahalanobis distances, which corrects the Euclidean distance using the variances and covariances between observable characteristics of the firms (Abadie and Imbens, 2002). The matching consisted of assigning a respective control for each beneficiary firm of the program. The assignment was made on the basis of matching the program beneficiary firm with the firm in the control group that was the "most similar" possible, in order to generate treatment and control groups which were as similar as possible in Situations 0T and 0C. Once the matched pairs of beneficiaries and controls were established, the average impact of the program could be estimated as the difference in the result variable for each pair and this difference can thereafter be averaged over the number of program beneficiaries.

Once the treatment and control groups are comparable, the impact of the program can be estimated through comparison of the result variables between the two groups. There are two estimators that are widely used in the literature of evaluation to measure the impact of a program:

- The difference-in-differences estimator
- The cross-sectional estimator

Difference-in-differences (DD) estimators are the result of comparing the average values of the result variables of Situations 0T and 0C in Figure 3 with the averages of the result variables of Situations I and III for the firms of the treatment group and the control group, respectively (Ashenfelter, 1978). The impact of the program is thereby estimated by comparing the different averages corresponding to the different situations expressed as the following formula:

$$(Y_I - Y_{III}) - (Y_{0T} - Y_{0C}) = (Y_I - Y_{0T}) - (Y_{III} - Y_{0C}),$$

where Y_j is the average value of the result variable in Situation j. The assumption behind this estimator is that the change in the situation of the controls (III-0C) between the moment before and the moment after the program is a good approximation of the change that the firms of the treatment group would have experienced during that same period if they had not benefitted from the program (II-0T). In other words, the difference between the result variables before and after in the treatment group controls for potential differences in the observable characteristics, as compared to the same difference before and after in the group of non-beneficiary firms controls for non-observable characteristics that are constant over time.

The canonical DD estimator (Goodman-Bacon, 2021) is obtained from the estimate of a static panel data model with fixed cross-sectional effects and temporal effects. Denoting by y_{it} the result variable in the firm i (for example, number of employees) in the period t, and by T_{it} a binary variable that adopts the unitary value if the firm i is a beneficiary of the program in the period *t*. That is, the variable indicates that the firm i belongs to the treatment group. There are two time periods t=1, 2 that indicate the pre-treatment periods (the beneficiary firm has still not received the treatment), and post-treatment (the beneficiary firm receives the treatment), respectively. Therefore,

$$\gamma_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 I(t = 2) + \beta_3 T_{it} \times I(t = 2) + \varepsilon_{it} \quad (1)$$

Where I(•) is the indicating function. In this model, the coefficient β_3 is the parameter of DD. This equation can be extended by incorporating variables of control. Note that in order for the parameter of DD to correctly identify the Average Treatment Effect on Treated (ATT), it is necessary to fulfill the assumption of parallel trends. That is:

$$\begin{aligned}\beta_3 &= [E(\gamma_{it} | T_{it} = 1, I(t = 2) = 1) \\ &\quad - E(\gamma_{it} | T_{it} = 1, I(t = 2) = 0)] \\ &\quad - [E(\gamma_{it} | T_{it} = 0, I(t = 2) = 1) \\ &\quad - E(\gamma_{it} | T_{it} = 0, I(t = 2) = 0)] \\ &= [E(\gamma^1_{it} | T_{it} = 1, I(t = 2) = 1) \\ &\quad - E(\gamma^0_{it} | T_{it} = 1, I(t = 2) = 0)] \\ &\quad - [E(\gamma^0_{it} | T_{it} = 0, I(t = 2) = 1) \\ &\quad - E(\gamma^0_{it} | T_{it} = 0, I(t = 2) = 0)]\end{aligned}$$

Where γ^k_{it} (k=0, 1) is the potential result. In this equation, adding and subtracting $E(\gamma^0_{it} | T_{it} = 1, I(t = 2) = 1)$ on the right side of the equation, the following is obtained:

$$\begin{aligned}\beta_3 &= E(\gamma^1_{it} - \gamma^0_{it} | T_{it} = 1, I(t = 2) = 1) \\ &\quad + [E(\gamma^0_{it} | T_{it} = 1, I(t = 2) = 1) \\ &\quad - E(\gamma^0_{it} | T_{it} = 1, I(t = 2) = 0)] \\ &\quad - [E(\gamma^0_{it} | T_{it} = 0, I(t = 2) = 1) \\ &\quad - E(\gamma^0_{it} | T_{it} = 0, I(t = 2) = 0)] \\ &= ATT + bias\end{aligned}$$

For the bias to be zero in the previous equation it is necessary that the evolution of γ^0_{it} in the control group coincides with the evolution of γ^0_{it} in the treatment group. If the latter term is different from zero, there is bias for non-parallel trends. The evolution of γ^0_{it} in the treatment group is not observable, so that, if the coefficient of DD in the panel data model correctly identifies the ATT, it is necessary to assume that there is equality between this evolution and that observed in the control group. This assumption is known in the literature as the assumption of parallel trends.

In practice, the assumption of parallel trends is compared with a placebo test. For this test, a new baseline is taken previous to the true one and a new DD estimate is made using a "false" treatment group. That is, a treatment group that has not yet been benefited by the program (the treatment group in the true baseline). As the program has still not been implemented the DD coefficient panel data model should be equal to zero. If this occurs, new evidence is considered in favor of compliance with the parallel trends assumption.

However, not rejecting that outcomes in pre-treatment periods show parallel trends does not establish the validity of counterfactual parallel trends. Similarly, rejecting parallel trends in periods prior to policy implementation does not guarantee a breach of counterfactual parallel trends either.

Despite the above, it is usual in the literature to present the contrast of parallel trends in periods previous to implementation of the policy and, if the hypothesis of equality in the trends of the result variable between the treatment and control groups is rejected, to provide some correction of the bias provoked. One of these procedures described by Rambachan and Roth (2022), suggests assuming that the pre-existing difference in the trends persists and to simply extrapolate it. That is, if the null hypothesis of the placebo test is rejected, then in the equation prior to the coefficient β_3 the *bias* is subtracted to obtain the ATT ATT ($ATT = \beta_3 - bias$). The empirical evidence of compliance with the assumption of parallel trends and of the estimate of the effect using the method of extrapolation for the result variables of number of employees and real wages, is presented in greater detail in the Annex of this paper.[8]

[8] The contrasts for fulfillment of the assumption of parallel trends for all the results variables can be found in the final report of the program impact evaluation.

In the case of the program evaluation, the fourth quarter of 2020 was taken as a baseline. In that quarter, the firms of both groups were paired so as to be similar in observable characteristics. In contrast to the DD procedure described above, the first three quarters of 2021 are post-treatment periods. That is, there was a period before implementation of the policy and three periods following its execution. In this context, Equation (1) was generalized to consider all the post-treatment periods (Wooldridge, 2021). The temporal periods are indicated with t = 1, 2, 3, 4, where t = 1 corresponds to the fourth quarter of 2020 and t = 2, 3, 4, the first three quarters of 2021. Moreover, as above, T_{it} is a binary variable that adopts the unitary value if the firm i is beneficiary of the program in the period t. Finally, the denomination $d_t = 1$ (t=2, 3, 4) is given to an indicator variable of each quarter post-treatment and with DT = $d_2 + d_3 + d_4$. With these definitions, the generalization of Equation (1) is given by the following model of unobserved components:

$$\gamma_{it} = \alpha_i + \delta_t + \beta_2 T_{it} + \beta_3 T_{it} \times DT + \varepsilon_{it} \qquad (2)$$

Where α_i are fixed cross sectional effects and δ_t are fixed temporal effects. The fixed temporal effects capture the seasonality which might exist in the result variable. The coefficient β_3 continues to be the parameter which measures the impact of the program. To see this, consider the following conditional mathematical expectations:

$$E[\gamma_{it} \mid DT = 0, T_{it} = 0] = \alpha_i + \delta_1$$
$$E[\gamma_{it} \mid DT = 1, T_{it} = 0] = \alpha_i + \delta_2 + \delta_3 + \delta_4$$
$$E[\gamma_{it} \mid DT = 0, T_{it} = 1] = \alpha_i + \delta_1 + \beta_2$$
$$E[\gamma_{it} \mid DT = 1, T_{it} = 1] = \alpha_i + \delta_2 + \delta_3 + \delta_4 + \beta_2 + \beta_3$$

Then:

$$\tau^{DID} = \{E[\gamma_{it} \mid DT = 1, T_{it} = 1] - E[\gamma_{it} \mid DT = 0, T_{it} = 1]\}$$
$$\{E[\gamma_{it} \mid DT = 1, T_{it} = 0] - E[\gamma_{it} \mid DT = 0, T_{it} = 0]\}$$
$$= \{[\alpha_i + \delta_2 + \delta_3 + \delta_4 + \beta_2 + \beta_3] - [\alpha_i + \delta_1 + \beta_2]\}$$
$$- \{[\alpha_i + \delta_2 + \delta_3 + \delta_4] - [\alpha_i + \delta_1]\} = \beta_3$$

Where τ^{DID} is the average impact of the program on the beneficiary firms. Additionally, the average impact disaggregated by quarters was estimated considering the following model of unobserved components:

$$\gamma_{it} = \alpha_i + \delta_t + \beta_2 T_{it} + \sum_{j=2}^{4} \beta_{3j} T_{it} \times d_j + \varepsilon_{it} \qquad (3)$$

In this equation, β_{3j} (j = 2, 3, 4) measures the average impact of the program on the beneficiary firms in each of the quarters of 2021. Both Equation (2) and (3) were estimated, moreover, including control variables.

4

Brief Description of the Set of Firms

Table 2 presents a first comparison between the beneficiary firms and non-beneficiaries of the program. In general terms, it is observed that the non-beneficiary employer firms have, on average, a slightly higher number of employees and pay higher salaries and contributions. These global differences are statistically significant except in number of employees. There is a stratification of firms according to size. The micro firms are those with fewer than ten employees; the small, between ten and 50; the medium, between 50 and 200, and the large, over 200. The geographical location of the firms is also known.

The information available from the data used for this study allows for identifying the volume of

TABLE 2

NUMBER OF EMPLOYERS ACCORDING TO SIZE, BASED ON NUMBER OF EMPLOYEES, FOURTH QUARTER 2020

Size	Beneficiary employer	Non-beneficiary employer	General total
1. Micro	7,722	44,4401	452,132
2. Small	2,589	67,313	69,903
3. Medium	668	14,660	15,328
4. Large	157	4,241	4,398
General total	11,136	530,615	541,761

Source: Authors' elaboration based on the SePyME database.

TABLE 3

DESCRIPTION OF FIRMS ACCORDING TO STATUS AS EXPORTERS, PROGRAM BENEFICIARIES, AND NON-BENEFICIARIES, 2020

Status of the firm	Exporter 2020	Number of firms	Average of employees	Average wages
Beneficiary employer	0-No	10,600	13.15	44,881
	1-Yes	536	113.61	73,032
Non-beneficiary employer	0-No	522,999	16.84	50,022
	1-Yes	7,616	188.27	95,435

Source: Authors' elaboration based on data from SePyME.

firms that exported during 2020. In the group of program beneficiaries, it is observed that only 4.8 percent of the firms exported during 2020, whereas in the group of non-beneficiary firms this percentage barely reaches 1.4 percent (see Table 3).

Furthermore, it is also possible to identify which of these firms are directed by women. Table 4 presents the distribution of beneficiary firms by line of credit according to whether they are managed or owned by women. As a general feature, there is no observable presence of women in charge of beneficiary firms of the PCA line addressed to small taxpayers and self-employed, the line with the highest number of beneficiaries. In the aggregate total of all the lines of credit, the presence of women-led firms represents 16 percent of cases.

TABLE 4

COMPARISON OF NUMBER OF EMPLOYEES AND SALARY LEVELS AMONG BENEFICIARY FIRMS ACCORDING TO LINES OF CREDIT AND INCIDENCE OF WOMEN-LED FIRMS: FOURTH QUARTER OF 2020

Line of Products	Women-led firms	Number of firms	Average number of employees	Average wages
ATP	0-No	1,836	26.28	56,958
	1-Yes	565	33.61	59,252
BNA cooperatives	0-No	5	34.67	56,116
BNA culture	0-No	119	14.28	38,552
	1-Yes	36	18.55	47,610
BNA tourism	0-No	694	18.16	34,294
	1-Yes	221	24.67	38,477
Direct COVID-19 credits	0-No	52	126.28	91,192
	1-Yes	24	69.43	64,531
Direct reactivation credits	0-No	81	30.60	59,334
	1-Yes	43	26.50	53,032
FOGAR	0-No	323	25.33	52,280
	1-Yes	87	49.67	56,636
LIP SME	0-No	185	107.70	77,375
	1-Yes	83	94.16	71,181
PCA	0-No	3,469	3.04	35,575
SME PLUS	0-No	2,484	15.17	47,649
	1-Yes	712	13.68	48,257
Working capital reactivation	0-No	92	14.24	53,201
	1-Yes	19	18.39	60,141
Productive investment reactivation	0-No	5	122.27	51,786
	1-Yes	1	86.33	101,640

Source: Authors' elaboration based on data from SePyME.

5

Identification of the Control Group for Program Evaluation

The baseline of the evaluation was established in the fourth quarter of 2020, since at this time there was a relaxing of the restrictions of Decrees N°297/2020, establishing Preventative and Mandatory Social Isolation (ASPO) (Aislamiento Social, Preventivo y Obligatorio), and N°520/2020, enforcing Preventative and Mandatory Social Distancing (DISPO) (Distanciamiento Social, Preventivo and Obligatorio) owing to the COVID-19 pandemic that had seriously affected the Argentinian economy in the second quarter of 2020 and also, to a lesser degree, in the third quarter of that year. Although some of the pandemic-related measures were still in place in the fourth quarter of 2020, these continued to be applied during 2021, which means that the economic conditions are similar.

In the fourth quarter of 2020, the database contained a total of 11,136 beneficiary firms that received funds from the program during 2021, and 530,615 non-beneficiary firms. According to the terms of reference of the evaluation, the analysis for estimating the effects of the program is focused on the set of micro, small, and medium firms, that is, on the set of firms with up to 200 employees. Therefore, the final treatment group in the baseline has 10,933 firms, while the group of non-beneficiaries relevant for the evaluation comprises 523,403 firms.[9]

As mentioned in the methodology section, in order to identify the impact of the program on the result variables, the groups of beneficiary firms of the program and the group of non-beneficiaries must be as similar as possible. To build two groups of firms, beneficiaries and non-beneficiaries, which are similar in observable characteristics, a

[9] Certain cases with null or empty values for the salary variables were excluded, as well as those non-beneficiary firms whose salary level exceeds the maximum registered in the treatment group.

subgroup of firms was selected from the set of non-beneficiary firms. This subgroup of firms with characteristics similar to the beneficiary firms was denominated the control group.

The control group was selected from the pool or set of all the non-beneficiary firms of the program on the basis of their observable characteristics. A procedure based on Mahalanobis distances was used for this purpose. For each observation of the treatment group in the baseline, that is, for each beneficiary firm of the program in the fourth quarter of 2020, the three firms most similar or comparable were selected from the pool of possible controls. This selection was made independently for each firm of the control group, such that the same matching could be associated with more than one beneficiary firm.

The vector x of observable variables considered for the selection of the control group are: number of employees, salary, identifier variables of location such as the Autonomous City of Buenos Aires (CABA) or Greater Buenos Aires (GBA), identifier variables of the main branches of activity (industry, commerce, hotels and accommodation, and catering services), type of firm according to size (micro, small, and medium enterprise), and indicator variable of exporting firm during 2020.

For each pair of observations, a score or distance measurement of the type $d'Vd$ was calculated, where $d = (x_i - x_j)$ is the vector of differences between two evaluated observations i and j for each firm i of the treatment group with respect to each firm j from the pool of possible controls, and V is the inverse of the matrix of covariances of the vector of observable variables x considered for the matching. This matrix (inverse) of covariances V is calculated on the basis of the total number of beneficiary firms of the program that comprise the treatment group.

As a result of this procedure, a control group of similar characteristics to the group of beneficiary firms for the fourth quarter of 2020 was obtained. The treatment group (beneficiary firms) contains a total of 10,933 firms and the control group comprises a total of 28,656 different non-beneficiary firms. According to the power calculations made, the sizes of both groups are sufficient to estimate the impact of the program on the variables of interest.

The control group has, on average, observable characteristics similar to those of the group of beneficiary firms, as presented in Table 5. The first column of the table shows the name of the characteristic selected; Columns (2) and (3) show the average of each characteristic in the groups of non-beneficiary firms and beneficiaries respectively. Column (4) shows the statistic of contrast. In all columns the numbers in parenthesis show the standard errors. In the final column, the contrasts of differences of means for each observable characteristic are statistically insignificant, which suggests that the groups are comparable in these characteristics.

TABLE 5

TEST OF DIFFERENCES OF MEANS, BENEFICIARY FIRMS AND CONTROL GROUP

Variables (1)	Non-beneficiary firms (2)	Beneficiary firms (3)	Difference (4)
Number of employees	12.98	13.00	-0.02
	(0.1361)	(0.2361)	(0.2725)
Salary	45.730	45.744	-13.8
	(152.52)	(264.92)	(305.18)
Turnover 2019	3.91E+07	4.06E+07	-1.471.942
	(815.675)	(1.701.443)	(1.886.858)
Turnover 2020	1.85E+07	1.97E+07	-1.20E+06
	(692.786)	(1.728.381)	(1.862.057)
Percentage of firms in CABA	0.184	0.184	0.000
	(0.0021)	(0.0037)	(0.0043)
Percentage of firms in GBA	0.302	0.302	0.000
	(0.0025)	(0.0044)	(0.0051)
Percentage of firms Branch C	0.49	0.49	0.000
	(0.0028)	(0.0048)	(0.0055)
Percentage of firms Branch G	0.119	0.119	0.000
	(0.0018)	(0.0031)	(0.0036)
Percentage of firms Branch I	0.087	0.087	0.000
	(0.0016)	(0.0027)	(0.0031)
Percentage of micro firms	0.704	0.703	0.001
	(0.0025)	(0.0044)	(0.0050)
Percentage of small firms	0.236	0.236	0.000
	(0.0023)	(0.0041)	(0.0047)
Percentage of medium firms	0.061	0.061	0.000
	(0.0013)	(0.0023)	(0.0026)
Percentage of exporting firms 2020	0.038	0.041	-0.003
	(0.0011)	(0.0019)	(0.0022)

Source: Authors' elaboration based on the SePyME database.
Note: Numbers in parenthesis are the standard errors calculated with the degrees of freedom defined by Welch. Statistical significance: * 10 percent, ** 5 percent and *** 1 percent.

6

Effects of the Global Credit Program for the Reactivation of the Productive Sector on Employment, Salary and Firm Survival

This section presents the impact that the program had on employment, salary, and firm survival. This impact was estimated for the total of firms and afterwards broken down according to size of firm, the effect of the program on firms led by women, and firms classified as exporters.

6.1. Impact of the Program on Employment

Table 6 presents the results of the estimate of the effect of the program on number of employees, using Equation (2) of the methodology. The estimates presented in the table use those firms that are present in the four quarters of the analysis (balanced panel of firms).[10] The first line of the table shows the estimate of the coefficient of DD ($\hat{\beta}_3$ in Equation (2)) that measures the impact of the program on the logarithm of employment. Columns (1) and (2) of the table show the estimate for all firms. Columns (3) and (4) use only those firms that accessed lines of working capital,

[10] Estimates of the program impact using an unbalanced panel can be found in the final report of the program evaluation.

TABLE 6
AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0055** (0.0026)	0.0055** (0.0026)	0.0146*** (0.0046)	0.0149*** (0.0046)	0.0763*** (0.0088)	0.0746*** (0.0090)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Average number of employees 4T20	14	14	13	13	42	42
Number of observations	14,1481	14,1481	60,388	60,388	6,316	6,316

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

and Columns (5) and (6) show the impact of the program on employment for those firms that accessed lines of credit for productive investment. The columns with odd numeration do not use control variables, whereas those with even numeration include the logarithm of the real average wages of each firm as a control variable. The table shows as a reference the average number of employees in the firms of the control group in the baseline of the evaluation, namely, the fourth quarter of 2020.

As observed in the table, the impact of the program on employment is positive and statistically significant in all the specifications. The estimate of the coefficient of average impact of the treatment on the total of beneficiary firms in the two specifications of the balanced panel is very similar and suggests an increase of around 0.6 percent in employment in the beneficiary firms of the program compared to those of the control group. The magnitude of the impact is slightly under that estimated for other productive development programs, such as the PACC, mentioned in Section 2.

When considering the firms broken down by specific lines, (Columns (3) to (6)), the positive impact on employment is much greater than for the aggregate of the total of lines of the program. In the lines of credit dedicated to working capital, the table suggests that the Global Credit Program increased employment by around 1.5 percent, whereas for the lines of credit designated to productive investment the impact was an average increase of employment in the beneficiary firms of around 7.5 percent. These latter results are similar in magnitude to those found by Castillo et al. (2016), and by Franco Churruarín and González-Rozada (2022), in the evaluation of the first and the second part of the PACC respectively.

Table 7 shows the results of the estimate of Equation (3) of the methodology. The structure is similar to that of Table 6, except that the average impact of the program is disaggregated for each quarter of 2021. The first file of the table shows the estimate of the average impact of the program on employment in the first quarter of 2021 ($\hat{\beta}_{32}$ in Equation (3)). The fourth file shows the estimate of the average impact of the program in the second quarter of 2021 ($\hat{\beta}_{33}$ in Equation (3)). The seventh file shows the estimate of the average impact of the program in the third quarter of 2021 ($\hat{\beta}_{34}$ in Equation (3)). The estimate of Equation (3) is the generalization of the canonical DD model with a pre-treatment period and

TABLE 7

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT, PER QUARTER

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment T121	0.0062***	0.0062***	0.0167***	0.0170***	0.0458***	0.0454***
	(0.0023)	(0.0023)	(0.0043)	(0.0043)	(0.0070)	(0.0072)
Treatment T221	0.0031	0.0031	0.0089*	0.0091*	0.0797***	0.0780***
	(0.0030)	(0.0030)	(0.0054)	(0.0054)	(0.0108)	(0.0111)
Treatment T321	0.0072**	0.0072**	0.0182***	0.0186***	0.1034***	0.1006***
	(0.0035)	(0.0035)	(0.0062)	(0.0062)	(0.0124)	(0.0125)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Si	Yes	Yes
Average number of employees 4T20	14	14	13	13	42	42
Number of observations	14,1481	14,1481	60,388	60,388	6,316	6,316

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

a post-treatment period. As in Table 6, Columns (1) and (2) show the impact on employment in all the firms; Columns (3) and (4), the impact on employment in those firms that accessed lines of credit for working capital, and Columns (5) and (6), for those firms that accessed lines of credit for productive investment.

The results presented in Table 7 suggest that the positive impact of the program on average employment in the first three quarters of 2021 arises from the impact in the first and third quarter. In these quarters the coefficient of the average effect of the program on employment is positive, of a similar magnitude, and statistically significant. In the first quarter of 2021, the program had increased average employment in the beneficiary firms by around 0.6 percent compared to the average number of employees in the non-beneficiary firms, where, during the third quarter, this increase was around 0.7 percent. The statistical significance is somewhat greater for the estimates of the first quarter than for those in the third, which suggests that the impact of the program was more pronounced at the beginning of 2021.

Table 7 also shows an estimate of the average impact of the program on employment during the second quarter of 2021. The effect is positive, half as great in magnitude, compared with the results of the first and third quarter and is not statistically significant, which indicates that the program did not have an effect on employment during this second quarter.

The final four columns of Table 7 break down the impact on employment considering lines of credit dedicated to working capital and productive investment most similar to traditional lines of productive development programs, as mentioned in the introduction of this technical paper. It may be seen that, in the case of the lines dedicated to working capital, although the impact is positive in the three quarters post-treatment, it is greater in magnitude in the first and third quarter and slightly smaller in the second quarter of 2021. The results presented in the table suggest that the program increased employment in the beneficiary

firms by around 1.7 percent in the first and third quarters of 2021. For the lines of credit dedicated to productive investment, the impact on employment is positive and growing throughout the three post-treatment quarters analyzed. Table 7 shows that the program had induced an increase of employment in the beneficiary firms of 4.5 percent in the first quarter of 2021; of almost 8 percent in the second quarter, and around 10 percent in the third quarter of 2021.

Table 8 shows the impact of the program on employment for three sizes of firms: micro firms, small firms, and medium firms. Columns (1) and (2) show the estimate of the effect of the program on the number of employees in micro firms using Equation (2) of the methodology. Columns (3) and (4) show the effect in small firms, and Columns (5) and (6), the impact on employment in the medium firms. The table has the same structure as Table 6. The first line measures the impact of the program on employment. As observed in the first two columns, the program seems not to have affected employment in the micro beneficiary firms during the first three quarters of 2021 compared to the micro firms of the control group.

Columns (3) and (4) show a positive and statistically significant impact of the program on the number of employees in the two specifications considered. The coefficient that measures this impact suggests that the program had increased employment in the small beneficiary firms in terms of average employment in the small firms of the control group by around 0.6 percent.

The final two columns of the table show the estimate of the impact of the program on employment in medium firms where, in contrast to what occurred in small firms, the results suggest that the program did not have a significant impact on employment.

When the effect on employment per quarter (Columns (1) and (2) of Table 9) for micro firms is disaggregated, a similar panorama is observed. In general, there is no statistically significant impact on employment in any of the quarters. In the small firms, Columns (3) and (4) of Table 9, a similar pattern is observed to that described previously for the aggregate effect, that is, a positive and statistically significant effect during the first and third quarter of 2021, and an impact that is not statistically significant in the second quarter of that year. The magnitude of the effect of the program on employment for small firms in the specifications of the balanced panel suggests that the effect is somewhat greater in the third quarter of 2021, at

TABLE 8

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN MICRO, SMALL AND MEDIUM FIRMS

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0014 (0.0031)	0.0013 (0.0031)	0.0055* (0.0033)	0.0059* (0.0032)	0.0013 (0.0050)	0.0015 (0.0050)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Average number of employees 4T20	3	3	22	22	95	95
Number of observations	89,555	89,555	30,868	30,868	7,684	7,684

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 9

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN MICRO, SMALL AND MEDIUM FIRMS

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment T121	0.0009 (0.0026)	0.0009 (0.0026)	0.0053** (0.0029)	0.0056** (0.0029)	0.0047 (0.0043)	0.0049 (0.0043)
Treatment T221	0.0007 (0.0036)	0.0006 (0.0036)	0.0036 (0.0036)	0.0039 (0.0036)	-0.0018 (0.0056)	-0.0016 (0.0056)
Treatment T321	0.0025 (0.0042)	0.0025 (0.0042)	0.0077* (0.0044)	0.0081* (0.0044)	0.0009 (0.0069)	0.0013 (0.0069)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Average number of employees 4T20	3	3	22	22	95	95
Number of observations	89,555	89,555	30,868	30,868	7,684	7,684

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

around 0.8 percent, compared to the impact in the first quarter of 2021, of between 0.5 percent and 0.6 percent. Finally, for the medium firms the program seems not to have had an effect on employment (Columns (5) and (6)).

The results presented so far suggest that the impact of the program on employment can be explained by the effect on small firms.

6.2. Impact of the Program on Real Wages

Table 10 shows the average impact of the program on real wages. The structure of this table is the same as that of Table 6. The result variable is the natural logarithm of real average wages, and the specifications that include controls, use

TABLE 10

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0316*** (0.0100)	0.0318*** (0.0100)	0.0619*** (0.0198)	0.0623*** (0.0198)	0.0319** (0.0136)	0.0295** (0.0120)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.2	10.2	10.2	10.2	10.6	10.6
Number of observations	14,1481	14,1481	60,388	60,388	6,316	6,316

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the real average wages in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

the number of employees as an explanatory variable. As with employment, the program had a positive and statistically significant impact on the real wages of workers in the beneficiary firms. The coefficient that measures this impact has a magnitude of around 0.03 in any of the specifications of the table. This value suggests an average impact on the real wages of workers in beneficiary firms of around 3 percent with respect to average real wages (measured in logarithms) of the non-beneficiary firms. To express these results in magnitudes, the real average wages of the non-beneficiary firms in the fourth quarter of 2020 was around ARS 27.000, and the program had an impact that indicates an increase of around ARS 9,300 on real average wages during the first three quarters of 2021. The magnitude of the impact on salaries is very similar to that found by Franco Churruarín and González-Rozada (2022) in the evaluation of the second part of the PACC mentioned above and is greater than that found by Castillo et al. (2016) in the evaluation of the first stage of this same program.

The effect of the program on real wages of the firms with lines of credit addressed to working capital (Columns (3) and (4)) is positive and statistically significant. The coefficient that estimates the impact suggests that the program had increased the real wages of beneficiary firms of these lines by around 6 percent. This result arises irrespective of whether or not the model used includes controls.

The estimate of the impact of the program on the real wages of workers of firms that received lines of credit for productive investment is shown in Columns (5) and (6). The impact is positive and statistically significant in all the specifications. The estimate indicates that the program would have increased real wages in these firms by around 3 percent.

When this average impact per quarter is broken down it is observed that, unlike the situation with employment, the effect of the program on the real wages of workers in beneficiary firms is positive and statistically significant in all the specifications estimated for each of the quarters of 2021. Table 11 shows these results, repeating

TABLE 11

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment T121	0.0289*** (0.0098)	0.0293*** (0.0098)	0.0691*** (0.0198)	0.070*** (0.0198)	0.0086 (0.0107)	0.0072 (0.0107)
Treatment T221	0.0264** (0.0103)	0.0265*** (0.0103)	0.0344* (0.0205)	0.0344* (0.0205)	0.0332* (0.0174)	0.0311** (0.0154)
Treatment T321	0.0394*** (0.0117)	0.0396*** (0.0117)	0.0821*** (0.0225)	0.0824*** (0.0225)	0.0539*** (0.0182)	0.051*** (0.0157)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.2	10.2	10.2	10.2	10.6	10.6
Number of observations	14,1481	14,1481	60,388	60,388	6,316	6,316

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the real average wages in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

the structure shown in Table 7. As seen in the table, the estimate of the coefficient of the average effect of the program on real wages has a magnitude of between 0.0098 and 0.029 in the first quarter of 2021; of between 0.023 and 0.027 in the second quarter, and around 0.040 in the third quarter of 2021. These values indicate that the program has had a stable effect on real wages during the first two quarters of 2021 of between 2.2 percent and 2.9 percent with respect to the average wages of the non-beneficiary firms of the program. In the third quarter of 2021 the impact is of a higher magnitude, reaching an increase of around 4 percent above real average wages in the firms of the control group.

For the beneficiary firms of the working capital lines of credit, Columns (3) and (4) suggest a positive impact and of greater magnitude in the first and third quarters of 2021, and a lower impact in the second quarter. The magnitude of the effect in the first and third quarters implies that the program would have increased average real wages in the beneficiary firms of these lines between 7 percent and 8 percent.

For the beneficiary firms of productive investment lines of credit, Columns (5) and (6) of Table 11 show the impact of the program on the real wages of workers. The impact is positive but is not statistically significant in the first quarter of 2021. Thereafter, it grows in magnitude and becomes statistically significant in the second and third quarters of that year. The impact estimate suggests an increase of around 3 percent in the real wages of workers in beneficiary firms in the second quarter of 2021, and of 5 percent in the third quarter.

The following two tables show the results of the evaluation of the program, disaggregating the beneficiary firms into micro, small, and medium groups. Columns (1) and (2) of Table 12 show the impact of the program on real wages of the workers in micro beneficiary firms. The specifications corresponding to the balanced panel show a positive and statistically significant effect on real wages in the aggregate. The coefficient that measures this impact has a magnitude of around 0.02. This value suggests an average impact on the real wages of workers in beneficiary firms of around 2 percent with respect to the real average wages (measured in logarithms) of the non-beneficiary firms. With respect to the real average wages of the control group firms in the fourth quarter of 2020, which were around ARS 24,300, the estimated impact suggests an

TABLE 12

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN MICRO, SMALL AND MEDIUM FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0222* (0.0122)	0.0222* (0.0122)	0.0538** (0.0242)	0.0546** (0.0242)	0.0527 (0.0383)	0.0531 (0.0383)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.1	10.1	10.4	10.4	10.6	10.6
Number of observations	89,555	89,555	30,868	30,868	7,684	7,684

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

increase average of almost ARS 5,500 during the first three quarters of 2021.

Columns (3) and (4) show the impact of the program on the real wages of workers in small firms. These columns suggest an aggregate effect on the first three quarters of 2021 that is both positive and statistically significant. The coefficient that measures this impact is around 0.05, which implies an increase of 5 percent for average real wages for small firms in the control group. As the real average wages in these firms was around ARS 33,000 pesos in the fourth quarter of 2020, this signifies an increase in average wages in beneficiary firms of around ARS 22,300 pesos in the first three quarters of 2021.

In the aggregate of the first three quarters of 2021, Columns (5) and (6) show the coefficient that measures the impact of the program on the real wages of workers in medium firms. The coefficient is positive, but is not statistically significant, suggesting there was no impact on the real wages of workers in medium firms.

The results of Table 13 suggest that the positive impact the program had on the real wages of workers in micro firms was produced during the first and third quarters of 2021. In both quarters the coefficient that measures this impact is positive and statistically significant, which suggests an effect of between 2.2 percent and 2.7 percent on real average wages of the firms of the control group.

With regard to small firms, Columns (3) and (4) of the table show that there is an increase in real wages which is more robust in the first and third quarter of 2021 but, contrasting what occurred with the micro firms, the effect during the second quarter of 2021 continues to be positive and statistically significant. The results shown in the table suggest an increase of between 4 percent and 5 percent in the real wages of workers in small beneficiary firms in the first two quarters of 2021, and an increase of 7 percent in the third quarter. The final two columns of the table show that the impact of the program on the real wages of workers of medium firms was null.

TABLE 13

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN MICRO, SMALL AND MEDIUM FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment T121	0.0221* (0.0122)	0.0221* (0.0122)	0.0491** (0.0237)	0.0500** (0.0237)	0.0453 (0.0351)	0.0458 (0.0351)
Treatment T221	0.0179 (0.0127)	0.0179 (0.0127)	0.0426* (0.0242)	0.043* (0.0242)	0.0454 (0.0391)	0.0456 (0.0391)
Treatment T321	0.0265* (0.0146)	0.0265* (0.0146)	0.0697** (0.0274)	0.0708*** (0.0275)	0.0675 (0.0417)	0.0679 (0.0417)
Controls	No	Yes	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.1	10.1	10.4	10.4	10.6	10.6
Number of observations	89,555	89,555	30,868	30,868	7,684	7,684

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. All the estimates use the balanced panel of firms. The fixed effects correspond to each beneficiary firm and its controls.

To summarize, when the impact of the program is explored by dividing the firms into micro, small, and medium categories, the results would indicate that the impact of the program on real wages has been greater in small firms than in micro firms but does not seem to have had a significant effect on medium firms.

This evidence suggests that the aggregate effect of the program found in Table 11 for the total of firms would be mainly explained by the effect of the program on small firms.

6.3. Impact of the Program on Survival

Table 14 shows the preliminary results in relation to the survival of beneficiary firms in the program, compared with the non-beneficiaries. For this purpose, three indicator variables were defined that take the value 1 if the firms considered in the baseline (fourth quarter of 2020) record information in the first quarter of 2021 and/or in the second quarter of 2021 and/or in the third quarter of 2021. This variable represents an empirical indicator of survival, given that it is assumed that if the firm is reporting information, then it must still be active. Thereafter, the equation (1) of the methodology that represents the canonical DD model is estimated with a pre-treatment period and a post-treatment period. Column (1) of the table shows the estimate, taking the first quarter of 2021 as the post-treatment period; Column (2) establishes the second quarter of 2021 as the post-treatment period and finally Column (3) takes the third quarter of 2021 as the period of post-treatment. In all the specifications the pre-treatment period is the fourth quarter of 2020. Given this definition of the result variable, a positive value for the coefficient of DD indicates a greater percentage of survival in the group of beneficiary firms in the program.

The results of these estimates suggest a positive and statistically significant effect of the program on firm survival with respect to the firms of the control group. However, the magnitude of the impact is relatively small when compared with that found in the evaluation of the PACC, mentioned in Section 3 of this paper. Column (1) shows an average impact of 0.31 percent and 0.47 percent in survival of the beneficiary firms in the first and second quarter of 2021, respectively. Column (3) shows the impact on survival in the third quarter of 2021. This impact of 0.41 percent is not statistically significant.

Disaggregating the impact of the program on firms with different lines of credit, working capital (WC), and productive investment (PI), reveals that the impact on survival is significantly greater. Columns (1) to (3) and Columns (4) to (6) in Table 15 reproduce the results of Table 14 for the beneficiary firms of lines of

TABLE 14

AVERAGE IMPACT OF THE PROGRAM ON FIRM SURVIVAL

Dependent variable: Survival	(1)	(2)	(3)
Treatment	0.0031* (0.0016)	0.0047** (0.0022)	0.0041 (0.0027)
Controls	No	No	No
Fixed effects	Yes	Yes	Yes
Number of observations	39,605	39,605	39,605

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 15

AVERAGE IMPACT OF THE PROGRAM ON THE SURVIVAL OF THE BENEFICIARY FIRMS OF WORKING CAPITAL AND PRODUCTIVE INVESTMENT LINES OF CREDIT

	Beneficiary firms of WC lines			Beneficiary firms of PI lines		
Dependent variable: Survival	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Treatment	0.006** (0.0025)	0.0116*** (0.0034)	0.0111*** (0.0041)	0.0042 (0.0033)	0.0124** (0.0050)	0.0126** (0.0064)
Controls	No	No	No	No	No	No
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	16,858	16,858	16,858	1,638	1,638	1,638

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The fixed effects correspond to each beneficiary firm and its controls.

credit for working capital and productive investment, respectively. In both cases Columns (2), (3), (5) and (6) show that the impact of the program on the survival of the beneficiary firms following WC and PI lines of credit is more than a percentage point around two and three quarters following the program's implementation.

These results of the program on the probability of survival in the lines of credit most similar to support provided by traditional productive development programs, are similar to those found, for example, in the evaluation by Castillo et al. (2016) of the PACC and mentioned in Section 3.

6.4. Impact of the Program on Women-Led Firms

This section considers the impact of the program on employment and real wages in women-led firms. Tables 16 and 17 show the estimates of the effect of the program on employment. In both tables not only are all the firms of the panel used (unbalanced panel), but also only those firms that are present in the four quarters of the analysis (balanced panel). The coefficient that measures the aggregate impact in the first three quarters of 2021 is positive, but not statistically significant. These

TABLE 16

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN WOMEN-LED FIRMS

Dependent variable: Log (No. of employees)	**(1)**	**(2)**	**(3)**	**(4)**
Treatment ($\hat{\beta}_3$)	0.007 0.0060	0.0062 (0.0060)	0.009 (0.0058)	0.0087 (0.0059)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	19	19	18	18
Balanced panel	No	No	No	No
Number of observations	22,999	22,999	22,328	22,328

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

results suggest a neutral impact of the program on employment in beneficiary women-led firms.

When this effect is broken down by quarter (Table 17), a positive and statistically significant impact is observed on employment in women-led firms in the first quarter of 2021. In that first quarter, the coefficient that measures the impact of the program suggests an increase of nearly 9 percent in employment in beneficiary firms. In the second and third quarters of 2021, the estimates of this coefficient continue to be positive, but are no longer statistically significant, which suggests that the effect of the first quarter disappears over time.

Tables 18 and 19 show the estimate of the impact of the program on real wages for

TABLE 17

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN WOMEN-LED FIRMS

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment T121	0.0072 (0.0361)	0.0065 (0.0361)	0.0088* (0.0351)	0.0085* (0.0351)
Treatment T221	0.0064 (0.0067)	0.0057 (0.0067)	0.0077 (0.0066)	0.0074 (0.0066)
Treatment T321	0.0074 0.0083)	0.0064 (0.0083)	0.0105 (0.0081)	0.0101 (0.0081)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	19	19	18	18
Balanced panel	No	No	Yes	Yes
Number of observations	22,999	22,999	22,328	22,328

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 18

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN WOMEN-LED FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.0575** (0.0262)	0.0577** (0.0261)	0.0495* (0.0261)	0.0500* (0.0261)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.3	10.3	10.3	10.3
Balanced panel	No	No	Yes	Yes
Number of observations	22,999	22,999	22,328	22,328

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 19

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN WOMEN-LED FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment T121	0.0502** (0.0245)	0.0505** (0.0245)	0.0438* (0.0245)	0.0446* (0.0245)
Treatment T221	0.0493* (0.0268)	0.0494* (0.0268)	0.0379 (0.0266)	0.0382 (0.0266)
Treatment T321	0.0733** (0.0326)	0.0734** (0.0326)	0.0667** (0.0326)	0.0671** (0.0326)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.3	10.3	10.3	10.3
Balanced panel	No	No	Yes	Yes
Number of observations	22,999	22,999	22,328	22,328

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

workers in women-led firms. The coefficient that measures the impact of the program on real wages in the aggregate of the three quarters of 2021 under consideration in this evaluation, is positive and statistically significant in all the specifications considered. The magnitude of the coefficient suggests an average impact of around 5 percent on real average salaries of workers in firms of the control group. This signifies that in the first three quarters of 2021, workers in women-led firms that participated in the program achieved an increase, on average, of around ARS 19,300 in real wages, compared with the real average salaries in non-beneficiary women-led firms in the baseline.

Table 19 shows the impact of the program on real wages per quarter. As observed in the table, the coefficient that measures this impact is positive and statistically significant in the first and third quarter of 2021 and continues to be positive, though it stops being statistically significant during the second quarter of that year. The results suggest that the effect on real wages is more marked in the third quarter of 2021 than in the first. The average effect of the program in the first quarter of 2021 is between 4 percent and 5 percent, whereas in the third quarter the impact is between 6.5 percent and 7 percent on real average wages of workers in non-beneficiary women-led firms.

To summarize, the results of this section indicate that the program had a greater effect on employment and real wages in women-led firms, compared with the aggregate effect found in Sections 6.1. and 6.2.

6.5. Impact of the Program on Exporting Firms

This section analyzes the impact of the program on employment and real wages in firms that exported in the fourth quarter of 2020 (hereinafter, exporting firms). Table 20 estimates the effect of the program on employment in these

TABLE 20

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN EXPORTING FIRMS

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.0170* (0.0094)	0.0167* (0.0093)	0.0180* (0.0094)	0.0175* (0.0093)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	51	51	49	49
Balanced panel	No	No	Yes	Yes
Number of observations	5,917	5,917	5,788	5,788

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

firms in the first three quarters of 2021. The coefficient that measures the impact of the program is positive and statistically significant in all the specifications considered. The magnitude of the coefficient suggests that the program would, on average, have increased employment in the exporting firms by between 1.7 percent and 1.8 percent with respect to employment in firms that did not export in the fourth quarter of 2020.

Table 21 shows the disaggregated effect per quarter. As observed in the table, the coefficient

TABLE 21

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN EXPORTING FIRMS

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment T121	0.014 (0.0090)	0.0135 (0.0089)	0.0129 (0.0089)	0.0122 (0.0087)
Treatment T221	0.0150 (0.0107)	0.0145 (0.0105)	0.0165 (0.0108)	0.0157 (0.0106)
Treatment T321	0.0220* (0.0124)	0.0221* (0.0123)	0.0245** (0.0124)	0.0245** (0.0124)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	51	51	49	49
Balanced panel	No	No	Yes	Yes
Number of observations	5,917	5,917	5,788	5,788

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 22

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN EXPORTING FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	-0.0067 (0.0141)	-0.003 (0.0140)	-0.0092 (0.0142)	-0.0051 (0.0140)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.6	10.6	10.7	10.7
Balanced panel	No	No	Yes	Yes
Number of observations	5,917	5,917	5,788	5,788

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

that measures the effect of the program on employment is positive and statistically significant only in the third quarter of 2021. The coefficient grows in magnitude throughout the quarters analyzed, which would suggest perhaps that if an evaluation of a longer period were made, the aggregate effect would be statistically significant. The estimated increase on employment in the third quarter of 2021 is around 2.5 percent.

Tables 22 and 23 show the impact of the program on the real wages of workers in exporting firms in the baseline. In this case, at both the aggregate level of the three quarters of 2021 and in each one of the quarters measured individually,

TABLE 23

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN EXPORTING FIRMS

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment T121	-0.0108 (0.0148)	-0.0079 (0.0147)	-0.0131 (0.0149)	-0.01 (0.0147)
Treatment T221	-0.0112 (0.0151)	-0.0074 (0.0149)	-0.0146 (0.0149)	-0.0104 (0.0147)
Treatment T321	0.0020 (0.0160)	0.0063 (0.0158)	0.0002 (0.0160)	0.0051 (0.0158)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.6	10.6	10.7	10.7
Balanced panel	No	No	Yes	Yes
Number of observations	5,917	5,917	5,788	5,788

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

the program does not show an effect on real wages compared with the real average salaries of the firms in the control group.

The results presented in this section suggest that the program would have had a positive impact on employment in the exporting firms, but not on the salaries paid by those firms.

6.6. Impact of the Program on Firms Classified by their Geographical Location

This section analyzes the impact of the program on employment and real wages in firms classified according to their geographical location. In order to gather sufficient observations in each locality, the firms were divided into three groups: beneficiary firms located in CABA, firms located in the province of Buenos Aires (PBA), and firms located in the rest of the country. Tables 24 and 25 show the impact of the program on employment of the CABA firms. As in the previous descriptions, Table 23 shows the aggregate impact in the first three quarters of the 2021, and Table 25 breaks down the effect for each quarter. The results presented in both tables suggest that the program had no impact on employment in the CABA firms.

Tables 26 and 27 show the impact of the program on the real wages of workers in the CABA firms. In this case, and in contrast to what happens with employment, the coefficient that measures the impact of the program is positive and statistically significant, both for the aggregate of the three quarters of 2021 and for each quarter separately. Table 26 shows that, on average, real wages increased by around ARS 3,500 in the program's beneficiary firms in the first three quarters of 2021.

Tables 28 to 31 show the impact of the program on employment and real wages in the beneficiary firms located in the PBA. In contrast to the situation found in the CABA firms, these tables show a positive and statistically significant impact on employment, but not on the real wages of workers in beneficiary firms located in the PBA. Table 28 shows that the coefficient that measures this impact has a magnitude of around 0.007 in any of the specifications utilized, which implies an increase of around 0.7 percent in the number of workers in the beneficiary firms in comparison

TABLE 24

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN FIRMS LOCATED IN THE AUTONOMOUS CITY OF BUENOS AIRES

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.0008 (0.0065)	0.0003 (0.0065)	0.0027 (0.0063)	0.0027 (0.0064)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	27	27	27	27
Balanced panel	No	No	Yes	Yes
Number of observations	28,777	28,777	26,639	26,639

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 25

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN FIRMS LOCATED IN THE AUTONOMOUS CITY OF BUENOS AIRES

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment T121	-0.0012 (0.0054)	-0.0017 (0.0054)	0.0018 (0.0049)	0.0017 (0.0049)
Treatment T221	-0.0001 (0.0075)	-0.0006 (0.0075)	0.0001 (0.0074)	0.0001 (0.0074)
Treatment T321	0.0038 (0.0090)	0.0033 (0.0091)	0.0063 (0.0088)	0.0062 (0.0089)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	27	27	27	27
Balanced panel	No	No	Yes	Yes
Number of observations	28,777	28,777	26,639	26,639

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 26

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN FIRMS LOCATED IN THE AUTONOMOUS CITY OF BUENOS AIRES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.1077*** (0.0323)	0.1077*** (0.0323)	0.1127*** (0.0327)	0.1126*** (0.0327)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.3	10.3	10.3	10.3
Balanced panel	No	No	Yes	Yes
Number of observations	28,777	28,777	26,639	26,639

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

with the control group firms. Table 28 suggests that this impact on employment took place, principally, during the first quarter of 2021.

In Tables 30 and 31 the coefficient that measures the impact of the program on the real wages of the workers in firms located in the PBA is not statistically significant in any of the specifications analyzed. This would be evidence that the program has had no effect on the salaries of firms located in the PBA.

Finally, Tables 32 to 35 show the estimate of the impact of the program on employment

TABLE 27

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN FIRMS LOCATED IN THE AUTONOMOUS CITY OF BUENOS AIRES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment T121	0.1062*** (0.0316)	0.1062*** (0.0316)	0.1131*** (0.0316)	0.113*** (0.0316)
Treatment T221	0.1032*** (0.0349)	0.1031*** (0.0349)	0.105*** (0.0347)	0.1049*** (0.0347)
Treatment T321	0.1139*** (0.0360)	0.1139*** (0.0360)	0.1199*** (0.0368)	0.1198*** (0.0368)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.3	10.3	10.3	10.3
Balanced panel	No	No	Yes	Yes
Number of observations	28,777	28,777	26,639	26,639

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 28

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN FIRMS LOCATED IN THE PROVINCE OF BUENOS AIRES

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.0043 (0.0044)	0.0043 (0.0044)	0.0074* (0.0044)	0.0074* (0.0044)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	24	24	24	24
Balanced panel	No	No	Yes	Yes
Number of observations	46,433	46,433	42,928	42,928

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

and real wages in firms in the rest of Argentina. Tables 32 and 33 show the program had a neutral impact on the number of employees in these beneficiary firms in the aggregate of the three quarters of 2021, and also when this effect is disaggregated by quarter.

Table 34 shows that the estimate of the coefficient that measures the impact of the program on real wages of workers in beneficiary firms operating in the interior of Argentina, in the aggregate post-treatment period, is positive and statistically significant in the two specifications

TABLE 29

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN FIRMS LOCATED IN THE PROVINCE OF BUENOS AIRES

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment T121	0.0081** (0.0039)	0.0080** (0.0039)	0.0091** (0.0038)	0.0091** (0.0038)
Treatment T221	0.0024 (0.0052)	0.0025 (0.0052)	0.0057 (0.0051)	0.0057 (0.0051)
Treatment T321	0.0022 (0.0060)	0.0021 (0.0060)	0.0074 (0.0060)	0.0074 (0.0060)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	24	24	24	24
Balanced panel	No	No	Yes	Yes
Number of observations	46,433	46,433	42,928	42,928

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 30

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN FIRMS LOCATED IN THE PROVINCE OF BUENOS AIRES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	0.0105 (0.0150)	0.0106 (0.0150)	-0.007 (0.0134)	-0.0067 (0.0134)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.2	10.2	10.2	10.2
Balanced panel	No	No	Yes	Yes
Number of observations	46,433	46,433	42,928	42,928

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent. The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

that use the balanced panel. When this effect is broken down by quarter (Table 35), it is observed that the impact is positive in all quarters of 2021, and its magnitude is greater in the third quarter of that year. The magnitude of the coefficient estimated suggests that the program increased the real wages of the firms located in the PBA by around 2.7 percent. This effect grows over time and reaches 3.3 percent in the third quarter of 2021.

TABLE 31

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN FIRMS LOCATED IN THE PROVINCE OF BUENOS AIRES

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment T121	0.0162 (0.0164)	0.0163 (0.0164)	-0.0077 (0.0137)	-0.0073 (0.0137)
Treatment T221	-0.0037 (0.0152)	-0.0036 (0.0152)	-0.0172 (0.0132)	-0.0171 (0.0132)
Treatment T321	0.0189 (0.0178)	0.0190 (0.0178)	0.0040 (0.0171)	0.0042 (0.0171)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.2	10.2	10.2	10.2
Balanced panel	No	No	Yes	Yes
Number of observations	46,433	46,433	42,928	42,928

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 32

AVERAGE IMPACT OF THE PROGRAM ON EMPLOYMENT IN FIRMS LOCATED IN THE INTERIOR OF THE COUNTRY

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	-0.0009 (0.0038)	-0.0011 (0.0038)	0.0051 (0.0037)	0.0052 (0.0037)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	25	25	25	25
Balanced panel	No	No	Yes	Yes
Number of observations	77,618	77,618	77,618	77,618

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 33

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON EMPLOYMENT IN FIRMS LOCATED IN THE INTERIOR OF THE COUNTRY

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment T121	0.002 (0.0035)	0.002 (0.0035)	0.0060* (0.0034)	0.0061* (0.0034)
Treatment T221	-0.0041 (0.0044)	-0.0042 (0.0044)	0.0025 (0.0043)	0.0025 (0.0043)
Treatment T321	-0.0008 (0.0050)	-0.0010 (0.0050)	0.0069 (0.0049)	0.0069 (0.0049)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	25	25	25	25
Balanced panel	No	No	Yes	Yes
Number of observations	77,618	77,618	77,618	77,618

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 34

AVERAGE IMPACT OF THE PROGRAM ON REAL WAGES IN FIRMS LOCATED IN THE INTERIOR OF THE COUNTRY

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment ($\hat{\beta}_3$)	-0.0142 (0.0139)	-0.014 (0.0139)	0.0265** (0.0132)	0.0267** (0.0132)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	25	25	25	25
Balanced panel	No	No	Yes	Yes
Number of observations	77,618	77,618	77,618	77,618

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

TABLE 35

AVERAGE IMPACT OF THE PROGRAM PER QUARTER ON REAL WAGES IN FIRMS LOCATED IN THE INTERIOR OF THE COUNTRY

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment T121	0.0073 (0.0145)	0.007 (0.0145)	0.0215* (0.0129)	0.0218* (0.0129)
Treatment T221	0.0106 (0.0149)	0.0106 (0.0149)	0.0253* (0.0136)	0.0254* (0.0136)
Treatment T321	0.0253 (0.0154)	0.0251 (0.0154)	0.0327** (0.0154)	0.033** (0.0154)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (real wages) average 4T20	10.1	10.1	10.2	10.2
Balanced panel	No	No	Yes	Yes
Number of observations	77,618	77,618	71,658	71,658

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2020 corresponds to the firms of the control group. The fixed effects correspond to each beneficiary firm and its controls.

7

Discussion and Final Considerations

The COVID-19 crisis brought about a change in economic, financial, and social conditions unprecedented in recent history. The abrupt change in the fundamental conditions of economic relations led to the accelerated and generalized implementation of compensatory measures that sought to contain the extension and duration of the inevitable negative effects affecting the economy. On top of fiscal, monetary and regulatory measures, financial support measures were also introduced, which includes the Global Credit Program for the Reactivation of the Productive Sector, the effects of which are analyzed in this work.

Analyzing the effects of the support measures helps measure the validity and effectiveness of these instruments as effective policy tools to guarantee MSME survival, the generation of revenues, and employment creation, of which there is scant evidence in developing countries. However, the nature and circumstances of the intervention differed significantly with respect to the usual, almost routine, interventions of public support for financial access or the policy responses to recessions or financial crises. A priori, it is reasonable to suppose that the returns of the intervention may be different from those verified in similar operations implemented in pre-pandemic conditions. Restrictions on the use of productive factors, variable regulations according to each sector and sanitary conditions, the differential contraction of demand, the generalized increase of uncertainty, and the changes in relative prices, are some of the reasons why the impacts of this type of intervention can differ from other comparable cases.

Other countries also implemented similar programs. Some of them, in particular those known as PPPs in the United States, have been evaluated with regard to their impacts, with similar methodologies to those used in this work. As previously explained in the literature review section, such analysis yielded some results of interest that offered a framework of reference for a discussion of the results obtained herein. Among them, the following are

worth highlighting: (i) the impacts on employment, although positive, were, in general, less than expected; (ii) the scale of the impacts was affected by complementary measures; (iii) targeting of the resources was dominated by conditions of supply rather than demand. In particular, the use of the banking systems meant that pre-existing relationships became much more significant in determining how the resources were allocated, and (iv) the size of firms and the condition of formality were also presented as factors of importance with regard to the distribution of financing resources.

In the case of Argentina, in addition to the elements indicated above, a series of idiosyncratic conditions existed which made it difficult to anticipate the impacts of the financial support program. The first significant characteristic in the case of Argentina is the scant financial depth and the relatively low dependence of firms on bank financing.[11] A further relevant characteristic that may potentially affect the materialization and distribution of the impacts, is the duality between the informal and formal segments of the economy, particularly when it comes to the labor dimension. On the one hand, in the formalized segments there is strong labor regulation accompanied by extensive and high-volume trade union membership, which introduces rigidities both in wages and in employment. On the other, there is a broad informal economy, in which the said regulations have a significantly lower prevalence. One important characteristic of informality in Argentina is the prevalence of partial informality. That is, situations in which firms maintain part of their labor force and sales "off the books". Therefore, it is relatively common that within a firm, part of the labor force is registered while another is not, and also that a part of the activities of the same employee is not reported. This type of partial informality is particularly relevant to this study, given that it might be affecting measurement of the variables of interest. Finally, a further relevant institutional characteristic is the federal nature of the organization of government that also introduced variation into the distribution of support at the regional level.

In this context, this work bridges a significant knowledge gap by providing an impact analysis of the entire credit program, considering the whole national territory, on key economic variables, and distinguishing between the instruments used. The results of this evaluation suggest that, in general, the program helped to increase the number of workers employed and their real wages in the beneficiary firms during the first three quarters of 2021. Moreover, the program influenced the sustainability of these firms by enhancing their probability of survival.

Within these general effects, it is worth noting that there are significant heterogeneities according to size of firm, type of instrument, and geographical region (among others). An initial element of heterogeneity is associated with the type of credit conceded, whether working capital or investment capital credit. Considering employment, the firms that received investment credits saw, on average, a markedly greater impact than those that used working capital. In contrast, when the effect on salary is considered, it is observed that those firms that received working capital increased salaries proportionally more than those that received investment credit. When it comes to firm survival, the size of the effects on working capital and investment is similar.

In the second place, considering the size of the firms, it is noticeable that there was no significant impact on employment in the micro

[11] This characteristic is, to a large extent, the corollary of a history of extreme financial volatility, which results in firms limiting their dependence on external financing.

or medium firms, but there was an impact on small firms.[12] With regard to salaries, the greatest positive effects were seen in the small firms and thereafter in the micro firms, insofar as the medium firms did not report statistically significant differences.

In the third place, regarding the effects segmented by geographical distribution, it was observed that the beneficiary firms operating in CABA did not report, on average, significant increases in employment, but on the other hand did report increases in real wages. In contrast with these results, the beneficiary firms located in the PBA did not experience significant salary rises, whereas employment did rise. With regard to the impacts in beneficiary firms located in the rest of the country, the results are similar to those of CABA, in that they showed no significant differences in employment, but there were differences in salaries. These results are consistent with those reported by Franco Churruarín and González-Rozada (2022) for the conglomerates component of the PACC, which means it is probable that the existence of these conglomerates is the reason for the effects attributed to the program when the geographical location of the firms is analyzed.[13]

In conclusion, these results point to the presence, in general, of positive impacts in the dimensions considered by the program. However, there is substantial heterogeneity in the distribution of the magnitudes in each dimension. Which factors determine the heterogeneity seen in size, type of credit, and geographical location? Unfortunately, the available information does not permit alternative granular hypotheses to be evaluated. As previously indicated, based on the results of other similar studies, it is possible to speculate that the supply factors may have influenced in such a way that the firms of the control group received equivalent financing from outside of the program. It is also possible that the idiosyncratic characteristics (low financial depth, dual economy with high informality, alongside strong regulation and trade union membership, and the presence of supplementary support plans at different levels of government), might have also been determinant in the distribution of the impacts. Given the importance of having adequate diagnostics for possible future interventions, it may well be worth pursuing further research to analyze the determinants of the computed impacts in greater detail.

[12] With respect to the determinants of this differential on the impact, it is only possible to speculate, given that the available information does not permit analysis in greater depth. The possible arguments are discussed below, after presenting the salary and regional results.

[13] The program also had a positive effect on real wages for workers in women-led firms in comparison with women-led firms that did not receive the benefit. With regard to exporting firms, the evaluation suggests that the program contributed to an increase in the number of workers in these firms, compared with firms that did not export in the baseline.

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Annex.
Empirical Evidence for the Assumption of Parallel Trends

This annex presents empirical evidence regarding the assumption of parallel trends for the main results. The assumption is that the untreated units provide the appropriate counterfactual of the trend that the treated units would have followed had they not been treated, that is, that the two groups would have had parallel trends. In practice, the literature contrasts this assumption with a test of parallel trends in periods prior to the treatment. That is, the equation of DD is estimated by using a "false" treatment group, or a group that has not been affected by the program. In this case, the false treatment group is comprised of the set of beneficiary firms of the program in the fourth quarter of 2020, or before receiving the benefit, and the baseline is the fourth quarter of 2019, when the program had yet to be implemented. In this estimate, not rejecting that the DD coefficient is statistically equal to zero is interpreted as evidence in favor of compliance with the assumption of parallel trends. There is no doubt that this contrast in common previous trends is important in the validation of the assumption of parallel trends that underpins a DD analysis. However, not rejecting that the results in periods previous to the treatment show parallel trends, should not be confused with establishing the validity of the counterfactual parallel trends. Equally, rejecting parallel trends in periods prior to application of the policy must also not be confused with the non-fulfillment of counterfactual parallel trends. However, it is common in the literature to present the contrast of parallel trends in periods before policy implementation and, if the hypothesis of equality in the trends of the

outcome variable between the treatment and control groups is rejected, to provide some bias correction caused by this breach. Rambachan and Roth (2022) suggest assuming that the pre-existent difference in the trends persists, and to simply extrapolate it. In the context of the methodology section of this work, the procedure is seen below. Consider the equation of the DD coefficient in the equation (1):

$$\begin{aligned}\beta_3 &= E(\gamma_{it}^1 - \gamma_{it}^0 | T_{it} = 1, I(t = 2) = 1) \\ &+ [E(\gamma_{it}^0 | T_{it} = 1, I(t = 2) = 1) \\ &- E(\gamma_{it}^0 | T_{it} = 1, I(t = 2) = 0)] \\ &- [E(\gamma_{it}^0 | T_{it} = 0, I(t = 2) = 1) \\ &- E(\gamma_{it}^0 | T_{it} = 0, I(t = 2) = 0)] \\ &= ATT + bias\end{aligned}$$

In this equation, the *bias* becomes zero if the assumption of parallel trends is fulfilled. Otherwise, the assumption is therefore:

$$ATT = \beta_3 - bias$$

And the *bias* is estimated with the coefficient of the contrast of common trends in periods previous to the treatment.

Table A1 shows a placebo test that seeks to check the assumption of parallel trends, necessary to enable the DD coefficient to correctly measure the impact of the program, in the case of the effect on employment.

Columns (1) and (3) show the estimate of the equation (1) of the methodology without including controls, while Columns (2) and (4) do include them. Columns (1) and (2) show the estimates using the unbalanced panel, which is the totality of the firms, whereas Columns (3) and (4) restrict these estimates to the balanced panel. In all the specifications, the coefficient that measures the placebo effect is negative and statistically significant. Following the approach of Rambachan and Roth described above, this result suggests that the estimates presented in the main text of this report would constitute a limit that is below the true effect of the program.

Table A2 repeats the exercise, but for the natural logarithm of salary. The structure of the table is the same as in Table A1. The results are similar. In principle, the DD coefficient is negative and statistically significant, wherein the assumption is rejected that the trends in the salary logarithm in the periods previous to program implementation are equal. As mentioned above, this does not mean that the assumption of counterfactual trends is necessarily invalid. However, it is usual in the literature to present the impact, assuming

TABLE A1.

CONTRAST OF THE ASSUMPTION OF PARALLEL TRENDS (NUMBER OF EMPLOYEES)

Dependent variable: Log (No. of employees)	(1)	(2)	(3)	(4)
Treatment	-0.0122***	-0.0115***	-0.0108***	-0.0106***
	(0.0039)	(0.0039)	(0.0038)	(0.0038)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Average number of employees 4T20	14	14	14	14
Number of observations	77,292	77,292	70,868	70,868

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The average number of employees in the fourth quarter of 2019 corresponds to the firms of the control group.

TABLE A2.

CONTRAST OF THE ASSUMPTION OF PARALLEL TRENDS (REAL WAGES)

Dependent variable: Log (real wages)	(1)	(2)	(3)	(4)
Treatment	-0.0550** (0.0124)	-0.0550** (0.0124)	-0.0570** (0.0125)	-0.0570** (0.0125)
Controls	No	Yes	No	Yes
Fixed effects	Yes	Yes	Yes	Yes
Temporal effects	Yes	Yes	Yes	Yes
Log (Real Wages) average 4T19	10	10	10	10
Number of observations	77,292	77,292	70,732	70,732

Note: The values in parenthesis are the standard robust errors. Statistical significance: * 10 percent, ** 5 percent, *** 1 percent.
The natural logarithm of the average real wages in the fourth quarter of 2019 corresponds to the firms of the control group.

that the assumption is not fulfilled. Taking the estimate of the DD coefficient of Table 24, of approximately 0.030 and extrapolating the result of around -0.056 from Table A2, an average impact of 0.086 is obtained from the program on the salaries of workers in beneficiary firms. This suggests an average increase in real wages in beneficiary firms of 8.6 percent. The other possible interpretation of the results detailed in the main text is that the average estimated impact is a limit that is below the true impact of the program.

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