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An Impact Evaluation of the FONTAR Program in Argentina

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Knowledge Spillovers of Innovation Policy through Labor Mobility: An Impact Evaluation of the FONTAR Program in Argentina

Victoria Castillo[*] *Alessandro Maffioli*[†] *Sofía Rojo*[‡] *Rodolfo Stucchi*[§]

February 3, 2014

Abstract[**]

Although knowledge spillovers are at the core of the innovation policy's justification, they have never been properly measured by any impact evaluation. This paper fills this gap by estimating the spillover effects of the FONTAR program in Argentina. We use an employer-employee matched panel dataset with the entire population of firms and workers in Argentina for the period 2002-2010. This dataset allows us to track the mobility of qualified workers from FONTAR beneficiary firms to other firms and, therefore, to identify firms that indirectly benefit from the program through knowledge diffusion. We use a combination of fixed effect and matching to estimate the causal effect—direct and indirect—of the program on various measures of performance. Our findings are robust to a placebo test based on anticipatory effects and show that the program increased employment, wages, and the exporting probability of both direct and indirect beneficiaries. The analysis of the dynamic of these effects confirms that performance does not improve immediately after the treatment for neither direct nor indirect beneficiaries.

JEL Classification: D2, J23, L8, O31, O33

Keywords: Innovation, Labor mobility, Knowledge diffusion, Spillovers, Policy Evaluation

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1. Introduction

One of the main arguments in favor of innovation policy is that firms' investment in innovation activities is lower than the socially optimum value. The reason for this sub-optimal result is that innovators do not fully appropriate the benefits of their investment in innovation activities because other firms also benefit through knowledge diffusion.

The literature has identified several mechanisms through which knowledge can flow from one firm to another. Among these, several studies have identified the mobility of skilled workers as a crucial source of knowledge spillovers. In the innovation economics literature, these studies include works by Jaffe et al. (1993), Saxenian (1994), Almeida and Kogut (1999), Maskell and Malmberg (1999), Cooper (2001), Fosfuri et al. (2001), Almeida and Phene (2004), Fosfuri and Ronde (2004), Møen (2005 and 2007), Boschma, Eriksson and Lindgren (2009). The mobility of workers as source of spillovers has also been largely studied in the trade and foreign direct investment literature –see, for example, Aitken and Harrison (1999), Glass and Saggi (2002), Görg and Strobl (2005), Wei and Liu (2006), Buckley et al. (2007), Liu et al. (2009), Balsvik (2011), Stoyanov and Zubanov (2012).

Although knowledge spillovers are at the core of the innovation policy's justification, they have never been properly measured by any impact evaluation. Up to know, the increasing number of studies providing evidence on the positive effect of Technological Development Funds (TDF) on investment in innovation and firm's performance in Latin America (Binelli and Maffioli 2007, Chudnovsky et al. 2008, Hall and Maffioli 2008; Castillo et al. 2011; Crespi et al. 2011a; Crespi et al. 2011b) has focused on the effects on direct beneficiaries only, without considering spillover effects. This in fact requires assessing not only the programs' impact on their direct beneficiaries, but also the effects on those production units that did not receive any direct support, but may have somehow benefited from the interaction with direct beneficiaries (hereinafter referred to as "indirect beneficiaries").

The main contribution of this paper is to provide evidence on the effectiveness of the Argentinean Technological Development Fund, FONTAR, on direct and indirect beneficiaries. FONTAR is the main innovation support program in Argentina. The program started in 1995 and financed more than 1,000 firms between 1995 and 2006. Previous evaluations by Binelli and Maffioli (2007) and Chudnovsky et al. (2008) found that the program increased the investment in R&D of direct beneficiaries. However, these studies did not find clear evidence of the effect of the program on firm's performance and did not evaluate the effect of the program on indirect beneficiaries. In this paper, we estimate the medium long-run impact of FONTAR on a series of key performance indicators – including firms' growth in terms of employment, labor productivity through wages, exports, and survival – on both direct and indirect beneficiaries.

Although the program collected precise administrative records on direct beneficiaries, it did not collect the data needed for the evaluation of its long term effect. For this reason, in this study we use two sources of data: (i) the administrative records of the program, and (ii) an employer-employee dataset constructed by OEDE (Observatory of Employment and Entrepreneurial Dynamics). By merging these sources we are able to construct an employer-employee panel dataset that includes all the firms reporting formal employment in Argentina after 1996 and all the employees in those firms. The dataset includes firm level information about age, location, industry, employment, wages, and value of exports.

Our final dataset has several important features. First, it includes the information needed to compute various performance indicators. Second, it allows us to identify not only direct but also indirect beneficiaries of the program. Third, it includes a large number of firms, which increases the probability of finding good control groups. Finally, it has a panel structure which includes observations on both years before and after the program support. This allows us to implement a robust estimation strategy and identify the long run effects of the program.

The core of our identification strategy is based on a fixed-effect estimator. This estimator provides consistent estimates of the causal effect of the program if selection is based on non-observed time-invariant characteristics. To fulfill this condition we use a matching procedure to identify a sample of firms with similar pre-treatment characteristics, including the trend in outcome variables.

Our results show positive direct and indirect effects of the program on firm's growth measured by employment, wages, and the probability of exporting. Spillover effects are lower than the direct effects, but still quantitatively important. From a dynamic point of view, we find that neither direct nor spillover effects occurred immediately and that both increased overtime.

The rest of the paper is organized as follows. Section 2 describes the program. Section 3 describes the datasets and presents descriptive statistics. Section 4 discusses the identification strategy. Section 5 presents the empirical results. Finally, section 6 concludes.

2. The FONTAR program: rationale and expected effects

The Argentinean Technological Fund (*Fondo Tecnologico Argentino*, FONTAR) was created in 1995 and it has been one of the pillars of Argentina's innovation policy. Although the program has evolved and expanded its set of instruments, it has maintained its main focus on providing financial support to innovation projects through two main instruments: (i) reimbursable funding, though targeted credit for innovation, and (ii) non-reimbursable funding, through matching grants and tax credit. [1]

Nowadays, the program includes the following lines of financing: (i) Matching grants that target innovation projects with higher risk and less tangible assets. They finance up to 50% of eligible expenses, up to a maximum of AR$ 850,000. The firms that have applied to this mechanism are mainly SMEs. (ii) Credit that targets technological modernization projects with relatively lower risk and higher tangible assets. Credits finance up to 80% of eligible expenses up to a maximum of AR$ 2,000,000. Both large firms and SMEs have applied for credits. (iii) Tax credit: the CF targets both innovation and technological modernization projects. They finance up to 50% of eligible expenses, up to a maximum of AR$ 3,000,000. Both large and SMEs have applied for tax credits. (iv) Support for cluster and supplier development mechanisms have been recently introduced. This support targets both innovation and technological modernization projects. It finances up to 80% (or 50%) of eligible expenses, up to a maximum of AR$ 16,000,000.

The provision of public funding either in the form of grants or in the form of targeted credit responds to specific failures in the financial markets that severely constrain innovation and technology adoption projects (Hall and Lerner, 2010).

First, the estimation of the risk-adjusted return of innovation and technology adoption investments requires very specific technical expertise and a complete understanding of the market of reference (often not yet existing). This clearly implies asymmetries of information between potential investors and innovators that can be only partially remedied with high assessment costs by the investor. Programs such as FONTAR are designed to bear this assessment costs through the establishment and funding of review processes of the technical and commercial viability of the proposed investments. In this sense, the program not only operates as a sort of public venture capitalist, whose returns are the economic return of the investment, but also provides valuable signals to the financial markets on the technical and

[1] FONTAR tax credits are non-automatic and project based.

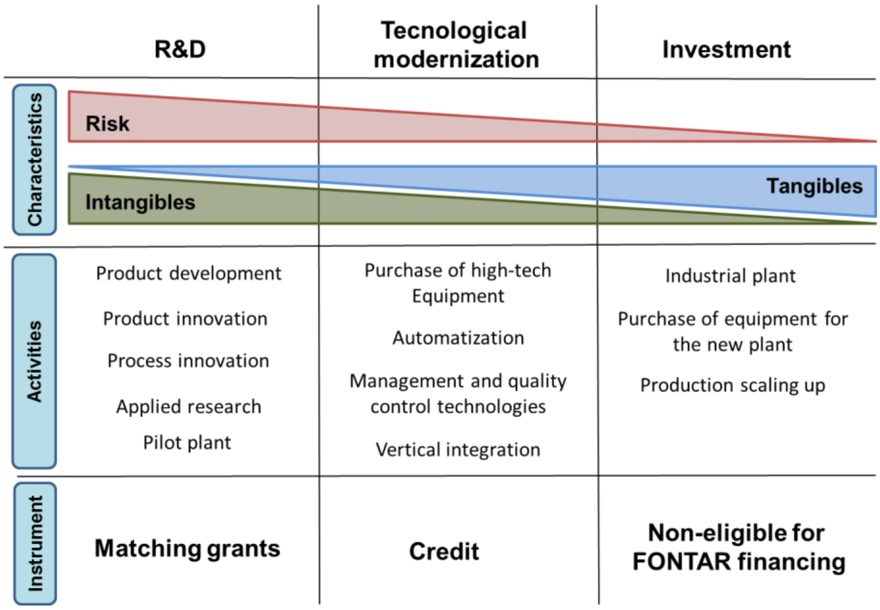
commercial sustainability of the investment.

Second, the main and most valuable outcomes of innovation projects are intangible and difficult to fully appropriate. These features make the market relationship between investors and innovators even more complicated. In fact, because most of the value of the investment is embedded in knowledge that may spill over to competitors, innovators may be reluctant to share critical information about the design and development of their projects with investors, worsening the asymmetric information problems. In addition, the intangible nature of the innovation outcomes makes it extremely difficult to use these outcomes as collateral, often leading to very high risk premium for investors.

Second, innovation projects are riskier than physical investment projects. For this reason, external investors systematically require higher risk premium for the financing of innovation activities than ordinary investment. Although per se this is not a market failure, public funding targeted to this kind of projects also aims at increasing their risk-adjusted return for both innovators and potential external investors.

Although these justifications generally apply to the entire program, because FONTAR’s lines of funding target different kinds of investments with different degree of risk and intangibleness (Figure 1), the justification of each line can be slightly differentiated. In fact, while the whole set of justifications clearly apply to the non-reimbursable instruments, which specifically target R&D projects with higher risks and intangible outcomes, the second and third justifications seem weaker in the case of the reimbursable instruments, which target projects aimed at the adoption of existing knowledge embedded in tangible assets and whose potential returns have already been demonstrated by earlier adopters. In this latter case, the policy intervention substantially solves a problem of asymmetry of information due to the degree of specificity that most likely goes beyond assessment capacity of the private financial sector.

Figure 1: The FONTAR Program



Source: FONTAR.

As discussed by Hall and Maffioli (2008) and Crespi et al. (2012), programs such as FONTAR are expected to produce a series of short, medium and long run effects, which reflect different stages of their intervention model. Based on this approach, a distinction can be made between innovation-input (short-term) outcomes, innovation-output (medium-term) outcomes, and economic-performance (long-term) outcomes. In this setting, programs such as FONTAR clearly aim at increasing firms' investment in innovation and R&D activities. Although the link between the provision of public funding and investment in innovation seems quite direct, effectiveness at this level still depends on the program's capacity to avoid crowding out effects – where public funding displace or substitute private spending – and to generate multiplier effects – where public funding leverages additional private resources. At this level, one can reasonably expect to observe some effects in the short run, almost contemporaneously to the provision of public funding.

The finding that investment increases as a consequence of the program support is a necessary, but not sufficient condition for a positive evaluation of these programs. Firms are in fact expected to translate this increased effort into outputs that reveal the successful realization of the innovation activities. For this purpose, various innovation-output indicators have been developed, including the number of patents and trademarks registered, the value of sales of new products, and dichotomous indicators on adoption of new process and products. Clearly, changes in these measures are not happening in the very short run. Therefore, depending on the complexity of the innovation activities, one to three years after receiving the public support are likely needed to observe any effect at this level.

Finally, not even the positive result of the overall innovation process can be assumed as a success if it does not translate into better economic performance for the program beneficiaries and, more in general, for the economy that provided the fiscal resources. Because the overarching objective of programs such as FONTAR is often related to the concepts of competitiveness and economic growth, measures of firm productivity, survival, and growth have been increasingly adopted to assess their effectiveness. However, the key challenge at this level is that this kind of results requires some time to mature. Again depending on the complexity of the innovation activities and on the production adjustments that these activities may require, between one to five years after receiving the public support seem to be needed before any impact can be observed at this level. This is even truer when indirect effects – such as spillover and general equilibrium effects – are considered. Additional time for the maturation of such effects is indeed required on top of the time needed for the direct effects.

The short run impact of FONTAR has already been evaluated. Binelli and Maffioli (2007) evaluate the short-run effect of the program and find significant multiplier effect of the program on private investment in R&D, but mainly as a consequence of the fiscal and targeted credit lines. The study by Chudnovsky et al. (2008) complemented and reinforced these findings by providing evidence that FONTAR matching-grant lines do not crowd out private investment in R&D (or, in another way, add on the existing private investment in R&D), but still have a limited multiplier effects. These findings, although generally positive, certainly require an assessment of the program’s medium and long-run effects to make sure that the public resources added on top of the private ones are actually producing significant returns in terms of economic performance.

To complement these previous findings, this paper focuses on the long-run and indirect effect of FONTAR program. This implies dealing with three fundamental challenges. First, the study needs to identify indirect beneficiary firms and control groups of non-beneficiary firms. Second the study requires specific information of firm-level economic performances for beneficiary, indirect beneficiary, and control non-beneficiary firms. Finally, this information must be available over a long period of time, at least five years after the program support is provided to the direct beneficiaries. While the next section will discuss how we addressed the two latter problems, the identification strategy section will discuss the former problem more in detail.

3. Data and descriptive statistics

Although the FONTAR executing unit has systematically produced high quality monitoring information, the collection of indicators for the evaluation of the long-term effect of the program was not included among its task until 2009. For this reason, any attempt to evaluate the impact of the program has to rely on the use of the secondary sources of information.

We use data from two different sources: (i) the administrative records of the program, and (ii) a dataset called BADE (Dataset for the Dynamic Analysis of Employment) that was constructed by OEDE (Observatory of Employment and Entrepreneurial Dynamics) at Ministry of Labor, Employment, and Social Security in Argentina. These sources were produced by different organizations, in different moments of time, and with different objectives. This heterogeneity demanded an important work of consolidation of the data.

The administrative records of the program provide detailed information about the main characteristics of the support provided to the firm –i.e. the year in which support was offered, the amount co-financed (ANR), the duration in months of the technical assistance, and the type of service received.

The OEDE dataset includes data from administrative records of two public entities: the National Administration of Social Security (ANSES), and the General Customs Bureau (DGA) of the Federal Administration of Taxes (AFIP). The dataset is a panel of firms that includes all the firms declaring employment in Argentina after 1996. It covers the manufacturing, services, and primary sectors and has firm level information about age, location, industry, number of employees, average wages, and value of exports. In 2010, the last year of our analysis, the dataset included around 6 million workers and 483 thousands firms.

We matched FONTAR and OEDE datasets using the unique tax identification code (CUIT) of each firm. We were eventually able to identify 97 percent of the beneficiaries of the program in OEDE dataset.

Our final dataset allows us to construct several measures of the outcomes of interest. In terms of measure of competitiveness, the data allow us to compute firms' growth in terms of number of employees, export volume and probability of exporting. Because increase in exports has often been related to productivity improvements,[2] one could argue that simultaneous positive effects on employment and exports signal productivity gains.[3] Because the OEDE data are based on up-to-date administrative record of all formal firms, in addition to firms' growth and exports we can also compute the survival probability of firms as an additional measure of competitiveness. Finally, we also compute the impact of the program on wages as a proxy of improved labor productivity.

Our dataset has other five fundamental features. First, because it allows us to track mobility of workers, it provides a unique framework to identify direct and indirect beneficiaries of the program. Second, it includes a large number of firms increasing the probability of finding non-beneficiary firms with the same characteristics of the beneficiary ones. Third, it has a panel structure, which allows controlling for time-invariant non-observables characteristics. Fourth, it includes observations on several years before treatment, allowing us to provide stronger evidence in support of our identification strategy. Finally, it includes observation on several years after treatment, which allows estimating the long run effect of the program.

4. Identification Strategy

The key challenge for our identification strategy is that we aim at measuring both the direct and spillover effects of the program. Therefore, we need to identify the impact of the program on direct beneficiaries–i.e. those firms that received the support of the program–and indirect beneficiaries–i.e. those firms that benefited from the program through their relation with direct beneficiaries.

Although the literature has considered various channels for spillover effects, in this paper we only focus on labor mobility. This particular channel seems to fit particularly well the case of a program such as FONTAR that focuses on fostering the creation of knowledge

[2] See Clerides et al. (1998), Bernard and Jensen (1999), Aw et al. (2000), Bernard et al. (2003) and Bernard and Jensen (2004). Furthermore, Melitz (2003)'s model shows how the exposure to trade induces only the more productive firms to export while simultaneously forcing the least productive firms to exit reallocating market shares (and profits) towards the more productive firms and contributing to an aggregate productivity increase.

[3] Furthermore, an increase in the probability of exporting would not only point to higher productivity, but also to the effectiveness of the FONTAR in covering part of the costs the investment in entering into new markets. In fact, because this investment mainly results in knowledge, the knowledge spillovers that may occur though labor mobility may lead to underinvestment and limit export opportunities in the absence of public support for the exporting pioneers. The cost of entering into new markets often consist of knowledge related to the assessment of the market demand, product standards, distribution channels, regulatory environment etc. (Melitz, 2003).

within the beneficiary firms. A good part of this knowledge would in fact be captured by the human resources operating in the beneficiary firm during the execution of the project. Therefore, spillovers may occur when one of these workers move to a non-beneficiary firm carrying with him part of the knowledge generated by beneficiary firms with the program support.

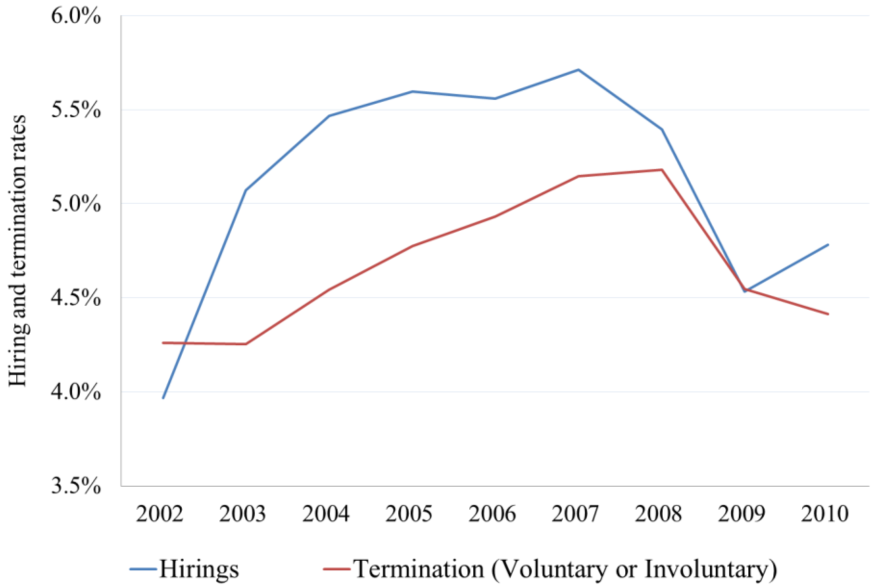
To identify knowledge spillovers through labor mobility, we need information at both the firm and employee level. Here is where the employer-employee structure of our data becomes extremely valuable for our study. In fact, it allows us to define precise employment transition matrices and, consequently, to identify those firms that may have indirectly benefited from the program by hiring specialized workers exposed to the knowledge created thanks to the program.

In practice, the identification of the indirect beneficiaries involves the following steps: (i) the identification of the direct beneficiaries; (ii) the definition of what is a firm-firm relationship that may involve spillover effects; (iii) the identification of the indirect beneficiaries on the basis of this rule. Therefore, first, we identified in our dataset the firms that directly benefited from the program. This is a straightforward process which implies merging FONTAR administrative records with the OEDE dataset. We identified in our data 905 firms that received support from the program between 1995 and 2006.

The definition of firm-firm relationships that involve spillover effects is more challenging. Having already restricted the nature of the relationship to transfers of labor force, we then needed to define if we wanted to consider all possible transitions of workers or if some restrictions were needed. In particular, because the FONTAR supports the generation of rather specific and complex knowledge, we could not simply assume all human resources in the beneficiary firms were exposed or able to absorb this knowledge.

Between 2002 and 2010 labor mobility was considerably high, involving approximately ten percent of total employment in Argentina every month. This implies that approximately five percent of employees left their current positions and five percent filled them (Figure 2). One of the main factors behind this high labor mobility is the short period of time new workers have stayed in the firm. In fact, close to 40 percent of new workers left the firm during the first quarter and close to 60 percent during the first year. During this period, approximately half of these terminations were voluntary and therefore associated to better job opportunities. Involuntary terminations were associated to fixed-term contracts (60 percent) or firings (40 percent).

Figure 2: Dynamics of private sector employment. Average of monthly rates, 2002-2010



Source: OEDE.

Because of the high labor mobility, we applied two restrictions for the identification of the workers who may cause knowledge diffusion and therefore spillovers. First, they need to have been exposed to the new knowledge generated in the beneficiary firm long enough to have learned something valuable. For this purpose, we restricted our analysis to the transfers of human resources who worked in a beneficiary firm for at least two years after the firm received FONTAR support. Second, these “knowledge carriers” need to be able to absorb relatively complex knowledge. Thus, we then restricted our analysis to the transfers of the most skilled labor force. Because the only measure of skill in our database is the real salary, we focus on the mobility of workers on the top quartile of the salary distribution of the firm of origin.

Summing up, we define indirect beneficiaries as those firms that: (i) never participated in FONTAR; (ii) hired skilled employees (top quartile in the firm wage distribution) that worked in a firm that received FONTAR for at least two years after the firms of origin received the FONTAR support.

These criteria allow us to significantly reduce the number of transfers we consider as relevant for potential knowledge spillovers. Table 1 summarizes the outflows of workers from the firms that received FONTAR support between 1995 and 2006. More than 120,000 workers had been somehow exposed to the FONTAR intervention during this period of time.

As we mentioned above, the overall mobility of this labor force is very high: around 52 percent of these workers eventually moved to a different firm. This would lead to around 117,000 job transitions, considering that workers may have move more than once after leaving the FONTAR beneficiary. However, when we restrict the analysis considering a minimum duration of employment in a FONTAR beneficiary firm, the mobility drops considerably.

Table 1: The mobility of workers in FONTAR beneficiary firms

	Years in a FONTAR beneficiary firm				Total
	< 1	**1 to 3**	**4 to 5**	**> 5**	
A) FONTAR 1995-2006					
Move to other firms	41,896	15,201	4,627	1,847	63,581
Stay in the firm	16,533	14,457	13,250	14,103	58,343
Total	58,429	29,658	17,877	15,950	121,924
B) FONTAR 2004					
Move to other firms	13,446	4,337	1,206	117	19,106
Stay in the firm	4,589	3,671	3,265	2,698	14,223
Total	18,035	8,008	4,471	2,815	33,329

Having identified both direct and indirect beneficiaries, we can define the identification strategy for the program impacts. Although the direct and spillover effects are clearly related, for the purpose of our estimates we analyze the direct participation in FONTAR and spillover effects as two separate treatments.[4]

Under certain identification assumptions, the structure of our data allows us to detect both direct and indirect effects by exploiting the variation across firms and over time. Because the FONTAR support is not randomly assigned, the pool of non-beneficiary firms is not necessarily comparable to the groups of beneficiaries and hence potential issues of administrative selection and self-selection may arise. This problem is also relevant for both the spillover effects. In fact, not only the direct beneficiary firms may self-select into the program because of characteristics that are also related to the outcome of interest, but also the indirect beneficiaries may be hiring skilled workers because of some characteristics also related to the outcome of interest. In both cases, a simple comparison between beneficiary (direct and indirect) and non-beneficiaries would lead to results biased by the selection in the

[4] Alternatively, the identification could have been approached as a multi-treatment problem. In theory, a multi-treatment approach could have been a better fit if firms that received direct support from the program had also hired human resources employed other beneficiary firms, i.e., if some beneficiary firms had received spillover effects from other beneficiaries. However, the available data do not include any such cases, and as a result we treat direct beneficiaries as a single group.

two treatments.

In a simple regression framework, we could reduce the selection bias related to observable factors by simply including those factors as control variables in the regression. However, in our case some important differences between participant and non-participant firms may also be related to unobservable (or unobserved) factors, such as the entrepreneurial behavior or managerial skills of the owner.

Our strategy is to take advantage of the panel structure of our data to control for potential unobservable sources of bias. In fact, assuming that the unobserved heterogeneity is constant over time we can eliminate these potential sources of bias using a fixed-effects model. More precisely, we propose the following specification:

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

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

The set of year dummies plays an important role in our analysis. After a long recession that started in 1998, Argentina suffered a severe crisis in 2001. As a consequence of the crisis, there was a large devaluation of the Argentine Peso and the government declared the default of its sovereign debt. Although in 2002 the GDP contracted by 10.8 percent, in 2003 started a period of growth for Argentina that lasted until the end of our sample period. Prices also changed during the recovery. In terms of our study controlling for these factors is important because the recovery also implied an increase in employment and nominal wages. As far as these factors affected beneficiaries and non-beneficiaries in the same way, the year dummy variables should properly control their influence on employment and real wages.

As mentioned before, the validity of our strategy rests on the identification assumption that the unobservable sources of bias are constant over time or, in other words, that trends in the outcome variables would have been equal in absence of the program. Unfortunately this assumption is not directly testable and it may be difficult to accept when firms in the control group are too heterogeneous and different from the participating firms – simply because firms that are very different are likely to follow different trends as well. Therefore, to reinforce our results, we also run equation (1) on a matched sample, selecting among the firms in the comparison group those that are more similar to beneficiaries not only in terms of observed characteristics but also on their pre-treatment performance. We do this to ensure that we select only those firms which have pre-treatment trends that are similar to those in the treated group.

We take the year previous to treatment as a baseline year and estimate the propensity scores, i.e. the conditional probability of participation, $P(T_{it} = 1|Z_{it}) = \mathrm{F}(\theta Z_{it})$, for a fixed pre-treatment year *t*, where Z is a vector of covariates and F is the Logistic cumulative distribution function. Using the predicted probability of participation, one would first match each treated firm with the untreated firm with most similar propensity score and then drop from the database all the non-treated firms that are not matched to any treated firm. Finally, one would run equation (1) on this matched sample.

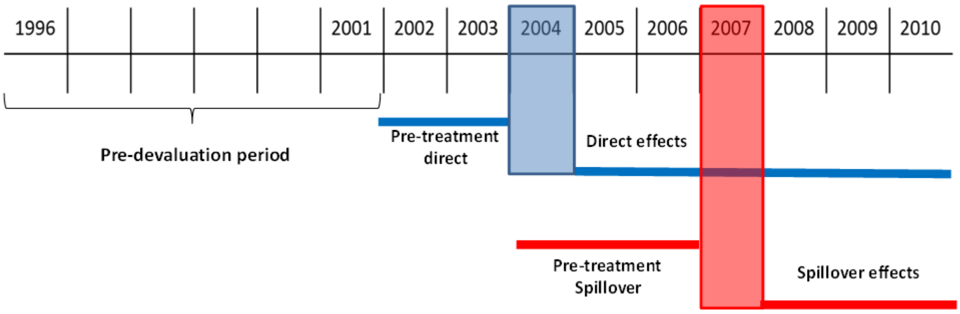
The variables we include in Z for the estimation of the propensity score are: employment, wages, and a dummy variable that takes value one if the firm exported before the baseline. It also includes the age of the firm, the experience of the workers measured by the number of years in the firm, industry dummies, type of society dummies, and region dummies.

Finally, to fully exploit the strengths of our identification strategy we focused our analysis on the cohort of beneficiaries that received the program support in 2004. As summarized by Figure 3, focusing on this cohort presents three key advantages. First, because we are looking for long run effects on firm performance, we want to have a relatively long series of post-treatment observations. Crespi et al. 2012 suggests considering between three to five years after the treatment to have a proper assessment of long-run effects. Because our panel ends in 2010, we therefore excluded the cohorts after 2006 from our analysis. In addition, because we define indirect beneficiaries as those firms that hired employees that worked in a FONTAR beneficiary firm for at least two years after that firm received the program support, we moved our selection back to the beneficiary cohorts before 2005 to allow enough time to fully observe long-run indirect effects.

Second, the 2004 cohort allows us to use pre-treatment data from a rather homogenous period. In fact, by focusing on this cohort we can use a two-year post-devaluation period (2002-2003) to identify beneficiary and non-beneficiary firms with similar trends in the outcome variable. This process and the entire analysis would certainly be more challenging including data from before and after the 2001 devaluation.

Finally, the analysis of the 2004 cohort allows us to focus on a period when the source of indirect effect is potentially very important, given that during the recovery from the 2001 crisis the labor market was quite dynamic in the creation of new jobs and labor mobility was high. (Figure 3)

Figure 3: Cohorts used for the analysis



5. Empirical results

As we mentioned above, the fixed-effects estimator provides us with a consistent estimate of the impact of the program if the selection into the program—and into the indirect treatment—depends on factors that do not vary in time and beneficiaries are not too different from non-beneficiaries in such a way that it is possible to assume that without the program they would had the same trend in the outcome variables.

Given that firms self-select into the program, we expect beneficiaries to be different from non-beneficiaries. In the case of indirect beneficiaries, it can also be the case that they self-select into hiring skilled workers that were employed in a FONTAR firm. Therefore, our strategy is to restrict the set of possible control firms to those with similar characteristics to the beneficiaries—including the evolution in the outcome variables. To do this, we use propensity score matching: we first estimate the probability of being beneficiary both direct and indirect using a logit model, then we define the propensity score as the probability of being beneficiary, and finally, we match firms using the propensity scores. We use nearest neighbor matching with one neighbor. Given that we observe the whole population of firms, the probability of finding good matches is considerably high.[5]

[5] The probability of having two firms with the same propensity score is also higher with the whole population of firms. Given that results could change if different firms are used as controls—i.e. there could be a sorting

Although our matching procedure guarantee that beneficiaries and non-beneficiaries have the same probability of being beneficiary, it does no guarantee however that non-beneficiaries in the matched sample have the same observable characteristics—on average—than beneficiaries. This balance needs to be tested. Table 2 shows the difference in mean test between direct beneficiaries and non-beneficiaries both for the full and matched samples. The analysis of the full sample reveals that before 2004 beneficiaries were larger, older, paid higher wages, and had higher probability of exporting than the rest of firms in Argentina.[6] These differences, which are expected given the FONTAR selection process, could bias upward the estimated impact of the program if the full sample were to be used. Conversely, in the matched sample beneficiary and control groups are balanced in every variable, confirming that matching was successful in identifying non-beneficiary firms with the same observable averages baseline characteristics of the direct beneficiaries.

Table 3 shows analog results for indirect beneficiaries. Indirect beneficiaries were also larger, older, and had higher exporting probability than non-beneficiaries. Thus, the unmatched sample could bias upward the estimates of the FONTAR impact. After defining the matched sample, indirect beneficiary and control groups are balanced in most observable characteristics. In few cases, where balancing is not perfect in levels—such as in the case of wages and wages of new employees—the differences in those variables are constant overtime, which is a sufficient condition to support the hypothesis of equality of trends in the absence of the treatment.

The matched sample has the purpose of making the assumption of equality of trends in absence of the treatment more credible by restricting the analysis to groups as comparable as possible both in terms of pretreatment levels and trends. Given that perfect balancing in all pre-treatment characteristics is always difficult to achieve, we also use a placebo test based on anticipatory effects to further validate our results.[7]

The results in Tables 2 and 3 make us confident about the identification strategy both for the direct and indirect effects in the matched samples.

problem—the dataset needs to be sorted randomly before doing the matching.

[6] We do not included indirect beneficiaries in the rest of firms.

[7] For a complete discussion on this kind of test, see section 5.2.1 of Angrist and Pischke (2008).

Table 2: Balance test, direct beneficiaries

	Full sample (776,825 firms)					Matched sample (374 firms)			
	Treated	Non-treated	t-stat	p-value		Treated	Non-treated	t-stat	p-value
Large firms	0.158	0.012	18.450	0.000	***	0.158	0.153	0.140	0.888
Medium-size firms	0.400	0.053	21.360	0.000	***	0.400	0.395	0.100	0.917
Small firms	0.274	0.247	0.850	0.393		0.274	0.274	0.000	1.000
Micro firms	0.168	0.688	-15.470	0.000	***	0.168	0.179	-0.270	0.787
Age	15.689	12.699	3.500	0.000	***	15.689	15.416	0.180	0.858
Age squared	460.290	300.100	3.050	0.002	***	460.290	468.270	-0.090	0.931
Number of employees 02	32.703	6.441	14.990	0.000	***	32.703	31.839	0.180	0.860
Number of employees 03	39.264	6.985	17.940	0.000	***	39.264	37.800	0.250	0.802
Number of employees 04	47.321	7.578	20.790	0.000	***	47.321	46.632	0.100	0.922
Average monthly wages 02	785.050	484.470	7.730	0.000	***	785.050	774.810	0.160	0.871
Average monthly wages 03	913.880	596.460	6.940	0.000	***	913.880	908.220	0.090	0.929
Average monthly wages 04	1098.900	736.190	7.590	0.000	***	1098.900	1089.600	0.130	0.895
Wage of new workers 02	486.050	196.800	4.480	0.000	***	486.050	476.880	0.150	0.883
Wage of new workers 03	588.710	237.690	9.300	0.000	***	588.710	574.440	0.270	0.789
Wage of new workers 04	808.940	291.380	6.000	0.000	***	808.940	787.070	0.250	0.805
Union workers (%) 02	87.390	98.159	-14.350	0.000	***	87.390	87.963	-0.230	0.816
Union workers (%) 03	87.291	98.137	-14.520	0.000	***	87.291	88.256	-0.390	0.694
Union workers (%) 04	87.959	98.121	-13.610	0.000	***	87.959	88.658	-0.310	0.760
Prop. of exporters_02	0.374	0.026	30.100	0.000	***	0.374	0.337	0.750	0.454
Prop. of exporters_03	0.421	0.029	32.110	0.000	***	0.421	0.405	0.310	0.755
Prop. of exporters_04	0.400	0.025	33.930	0.000	***	0.400	0.382	0.380	0.707
Value of exports_02	200000	18451	1.440	0.150		200000	230000	-0.310	0.759
Value of exports_03	300000	20828	1.780	0.074	*	300000	240000	0.520	0.602
Value of exports_04	390000	23340	2.390	0.017	**	390000	450000	-0.340	0.732
Prop of multinationals	0.016	0.002	4.090	0.000	***	0.016	0.011	0.450	0.654
SRL	0.874	0.276	18.400	0.000	***	0.874	0.889	-0.480	0.635
Other commercial society	0.037	0.109	-3.200	0.001	***	0.037	0.032	0.280	0.778
Other association form	0.016	0.083	-3.370	0.001	***	0.016	0.011	0.450	0.654
Buenos Aires	0.458	0.341	3.390	0.001	***	0.458	0.468	-0.210	0.838
Center	0.342	0.432	-2.490	0.013	**	0.342	0.311	0.660	0.513
NEA	0.016	0.052	-2.270	0.023	**	0.016	0.032	-1.010	0.313
NOA	0.053	0.060	-0.450	0.655		0.053	0.058	-0.220	0.823
Cuyo	0.116	0.065	2.860	0.004	***	0.116	0.111	0.160	0.872
Patagonia	0.016	0.050	-2.150	0.032	**	0.016	0.021	-0.380	0.704
Agriculture	0.074	0.202	-4.410	0.000	***	0.074	0.111	-1.240	0.215
Forestry	0.021	0.003	4.710	0.000	***	0.021	0.047	-1.410	0.159
Fishing	-	-	-	-		-	-	-	-
Metallic mineral extraction	0.005	0.000	8.180	0.000	***	0.005	0.000	1.000	0.318
Oil and gas extraction									
Other mining	-	-	-	-		-	-	-	-
Food and beverages	0.058	0.035	1.750	0.080	*	0.058	0.074	-0.620	0.536
Textiles	0.005	0.008	-0.390	0.698		0.005	0.005	0.000	1.000
Apparels	0.005	0.008	-0.420	0.671		0.005	0.000	1.000	0.318
Leather products	-	-	-	-		-	-	-	-
Wood products	-	-	-	-		-	-	-	-
Paper products	0.021	0.003	4.920	0.000	***	0.021	0.005	1.350	0.178
Editing products	0.011	0.011	-0.120	0.904		0.011	0.021	-0.820	0.412
Oil products	-	-	-	-		-	-	-	-
Chemical products	0.084	0.007	13.130	0.000	***	0.084	0.068	0.580	0.563
Rubber products	0.042	0.008	5.050	0.000	***	0.042	0.042	0.000	1.000
Non-metallic minerals	0.016	0.005	2.110	0.035	**	0.016	0.037	-1.280	0.201
Common metallic products	0.016	0.003	3.100	0.002	***	0.016	0.011	0.450	0.654
Other metallic products	0.053	0.020	3.140	0.002	***	0.053	0.042	0.480	0.630
Machinery and equipment	0.111	0.008	15.830	0.000	***	0.111	0.058	1.850	0.065
Electric products	0.047	0.003	11.700	0.000	***	0.047	0.026	1.090	0.277
Radio and television	0.011	0.000	8.060	0.000	***	0.011	0.021	-0.820	0.412
Medical instruments	0.016	0.001	5.540	0.000	***	0.016	0.011	0.450	0.654
Automotive and transportation	-	-	-	-		-	-	-	-
Furniture	0.005	0.008	-0.470	0.641		0.005	0.005	0.000	1.000
Recycling	0.005	0.000	4.190	0.000	***	0.005	0.011	-0.580	0.563
Construction	0.042	0.035	0.500	0.618		0.042	0.047	-0.250	0.805
Car sales and car repair	0.005	0.051	-2.860	0.004	***	0.005	0.005	0.000	1.000
Wholesale	0.058	0.074	-0.840	0.398		0.058	0.058	0.000	1.000
Retail	0.026	0.191	-5.760	0.000	***	0.026	0.016	0.710	0.476
Automotive transportation	-	-	-	-		-	-	-	-
Sea and river transportation	0.005	0.001	2.440	0.015	**	0.005	0.011	-0.580	0.563
Load and storage	0.005	0.016	-1.140	0.252		0.005	0.000	1.000	0.318
Mail and telecommunications	-	-	-	-			-	-	-
Financial intermediation	-	-	-	-			-	-	-
Insurance	-	-	-	-			-	-	-
Financial interm. aux. serv.	-	-	-	-			-	-	-
Computer services	0.142	0.005	25.380	0.000	***	0.142	0.174	-0.840	0.400
Research and development	0.011	0.001	4.920	0.000	***	0.011	0.021	-0.820	0.412
Law and accounting services	0.047	0.111	-2.790	0.005	***	0.047	0.037	0.510	0.611
Social services	0.032	0.064	-1.840	0.066	*	0.032	0.021	0.640	0.523

Notes: Treated firms only include direct beneficiaries. Control firms are those firms that did not received the support of the program (directly or indirectly through labor mobility). Sectors with no direct beneficiaries were removed from the sample.

Table 3: Balance test, indirect beneficiaries vs non-beneficiaries

	Full sample (773,980 firms)					Matched sample (444 firms)				
	Treated	Non-treated	t-stat	p-value		Treated	Non-treated	t-stat	p-value	
Large firms	0.517	0.045	34.900	0.000	***	0.517	0.504	0.280	0.783	
Medium-sized firms	0.309	0.169	5.730	0.000	***	0.309	0.343	-0.780	0.433	
Small firms	0.153	0.469	-9.730	0.000	***	0.153	0.131	0.660	0.511	
Micro firms	0.021	0.317	-9.770	0.000	***	0.021	0.021	0.000	1.000	
Age	19.792	11.681	9.890	0.000	***	19.792	17.614	1.320	0.188	
Age squared	736.520	294.510	8.710	0.000	***	736.520	608.110	1.020	0.310	
Number of employees 05	392.530	16.248	84.000	0.000	***	392.530	310.240	1.030	0.305	
Number of employees 06	442.610	18.527	87.960	0.000	***	442.610	388.080	0.590	0.552	
Number of employees 07	493.630	19.885	91.210	0.000	***	493.630	508.280	-0.130	0.896	
Average monthly wages 05	1969.700	896.890	21.160	0.000	***	1969.700	1712.600	1.660	0.097	*
Average monthly wages 06	2334.200	1097.800	21.760	0.000	***	2334.200	2036.000	1.820	0.069	*
Average monthly wages 07	2819.400	1358.800	19.070	0.000	***	2819.400	2485.700	1.810	0.070	*
Wages of new employees 05	1614.900	798.200	12.250	0.000	***	1614.900	1237.700	2.180	0.029	**
Wages of new employees 06	1829.600	953.070	10.880	0.000	***	1829.600	1500.400	2.470	0.014	**
Wages of new employees 07	2303.600	1176.600	15.650	0.000	***	2303.600	1986.500	1.680	0.093	*
Union workers (%) 05	78.780	97.188	-24.240	0.000	***	78.780	79.061	-0.100	0.918	
Union workers (%) 06	78.927	97.165	-24.610	0.000	***	78.927	79.228	-0.110	0.913	
Union workers (%) 07	58.938	73.513	-5.240	0.000	***	58.938	57.988	0.250	0.806	
Wages Q1/_Wages Q4 05	0.288	0.306	-1.240	0.216		0.288	0.286	0.110	0.913	
Wages Q1/_Wages Q4 06	0.283	0.320	-2.670	0.008	***	0.283	0.284	-0.080	0.934	
Wages Q1/_Wages Q4 07	0.286	0.323	-2.850	0.004	***	0.286	0.289	-0.250	0.800	
Prop. of exporters 05	0.339	0.053	19.440	0.000	***	0.339	0.335	0.100	0.923	
Prop. of exporters 06	0.331	0.057	18.090	0.000	***	0.331	0.322	0.200	0.845	
Prop. of exporters 07	0.343	0.056	19.190	0.000	***	0.343	0.347	-0.100	0.923	
Value of exports 05	14000000	63003	34.350	0.000	***	14000000	1800000	1.860	0.064	*
Value of exports 06	17000000	72740	38.360	0.000	***	17000000	1800000	2.100	0.037	**
Value of exports 07	23000000	83696	39.490	0.000	***	23000000	1700000	2.110	0.035	**
SRL	0.860	0.468	12.060	0.000	***	0.860	0.881	-0.680	0.494	
Other commercial society	0.025	0.067	-2.530	0.011	**	0.025	0.017	0.640	0.524	
Other association form	0.038	0.043	-0.400	0.692		0.038	0.038	0.000	1.000	
Multinational	0.182	0.005	36.940	0.000	***	0.182	0.148	0.990	0.323	
CBA	0.572	0.413	4.970	0.000	***	0.572	0.521	1.110	0.268	
Center	0.242	0.359	-3.750	0.000	***	0.242	0.347	-2.540	0.012	**
NEA	0.021	0.040	-1.470	0.143		0.021	0.025	-0.300	0.761	
NOA	0.047	0.055	-0.560	0.575		0.047	0.030	0.960	0.337	
Cuyo	0.064	0.067	-0.210	0.831		0.064	0.038	1.260	0.210	
Patagonia	0.055	0.067	-0.730	0.467		0.055	0.038	0.870	0.384	
Agriculture	0.030	0.140	-4.890	0.000	***	0.030	0.051	-1.170	0.243	
Forestry and wood	-	-	-	-		-	-	-	-	
Fishing	0.008	0.002	2.350	0.019	**	0.008	0.008	0.000	1.000	
Metallic mineral extraction	-	-	-	-		-	-	-	-	
Oil and gas extraction	0.017	0.001	6.510	0.000	***	0.017	0.004	1.350	0.178	
Other mining	0.008	0.003	1.650	0.099	*	0.008	0.000	1.420	0.157	
Food and beverages	0.047	0.040	0.530	0.600		0.047	0.051	-0.210	0.831	
Textiles	0.008	0.011	-0.350	0.727		0.008	0.013	-0.450	0.654	
Apparels	0.004	0.013	-1.200	0.230		0.004	0.000	1.000	0.318	
Leather products	0.004	0.006	-0.360	0.717		0.004	0.000	1.000	0.318	
Wood products	0.004	0.009	-0.760	0.450		0.004	0.008	-0.580	0.563	
Paper products	0.013	0.003	2.440	0.015	**	0.013	0.021	-0.710	0.477	
Editing products	0.008	0.010	-0.200	0.844		0.008	0.008	0.000	1.000	
Oil products	0.008	0.000	6.490	0.000	***	0.008	0.000	1.420	0.157	
Chemical products	0.038	0.008	5.070	0.000	***	0.038	0.038	0.000	1.000	
Rubber products	0.017	0.009	1.190	0.233		0.017	0.013	0.380	0.704	
Non-metallic minerals	0.013	0.006	1.470	0.142		0.013	0.008	0.450	0.654	
Common metallic products	0.013	0.004	2.110	0.035	**	0.013	0.004	1.000	0.316	
Other metallic products	0.021	0.024	-0.330	0.743		0.021	0.000	2.260	0.025	**
Machinery and equipment	0.038	0.012	3.540	0.000	***	0.038	0.038	0.000	1.000	
Electric products	0.008	0.004	1.140	0.256		0.008	0.013	-0.450	0.654	
Radio and television	0.008	0.000	5.710	0.000	***	0.008	0.004	0.580	0.563	
Medical instruments	-	-	-	-		-	-	-	-	
Automotive and transportation	0.047	0.006	8.200	0.000	***	0.047	0.042	0.220	0.824	
Furniture	-	-	-	-		-	-	-	-	
Recycling	-	-	-	-		-	-	-	-	
Construction	0.148	0.082	3.700	0.000	***	0.148	0.148	0.000	1.000	
Car sales and car repair	0.021	0.043	-1.660	0.097	*	0.021	0.025	-0.300	0.761	
Wholesale	0.089	0.086	0.170	0.864		0.089	0.119	-1.060	0.292	
Retail	0.034	0.177	-5.750	0.000	***	0.034	0.021	0.840	0.400	
Automotive transportation	0.034	0.087	-2.870	0.004	***	0.034	0.030	0.260	0.794	
Sea and river transportation	0.008	0.001	3.370	0.001	***	0.008	0.013	-0.450	0.654	
Load and storage	0.025	0.020	0.610	0.545		0.025	0.013	1.010	0.314	
Mail and telecommunications	0.017	0.009	1.340	0.180		0.017	0.008	0.820	0.412	
Financial intermediation	0.025	0.005	4.450	0.000	***	0.025	0.004	1.910	0.057	*
Insurance	0.013	0.002	4.050	0.000	***	0.013	0.013	0.000	1.000	
Financial interm. aux. serv.	0.004	0.004	0.100	0.922		0.004	0.000	1.000	0.318	
Computer services	0.089	0.010	12.040	0.000	***	0.089	0.089	0.000	1.000	
Research and development	0.004	0.001	1.840	0.065	**	0.004	0.004	0.000	1.000	
Law and accounting services	0.089	0.091	-0.120	0.903		0.089	0.157	-2.250	0.025	**
Social services	0.025	0.036	-0.890	0.371		0.025	0.025	0.000	1.000	

Notes: Treated firms only include indirect beneficiaries. Control firms are those firms that did not received the support of the program (directly or indirectly through labor mobility). Sectors with no indirect beneficiaries were removed from the sample.

Panel A in Table 4 shows the average impact of FONTAR on employment, wages, and the probability of exports for direct beneficiaries. For each variable we estimated the same equations on two samples—full sample and matched sample. We estimated all regressions using the fixed-effects (within-group) estimator with robust standard errors.

The direct effect of the program on employment, real wages, and probability of exports is quantitatively and statistically significant. The average direct effect of the program on employment, real wages, the probability of exports, and the survival probability is 17.2 percent, 6.1 percent, 6.2 percent, and 3.7 percent, respectively.

Panel B in Table 4 shows analogous results for the spillover effects. The spillover effects are qualitatively similar to the direct effects although quantitatively smaller. The spillover effect on employment, real wages, and probability of exports was 14.9 percent, 3.6 percent, and 4.8 percent, respectively. In this case, the effect on the survival probability is not statistically significant.

Figure 4 shows the evolution of employment, wages, and the proportion of exporter for direct beneficiaries, indirect beneficiaries, and non-beneficiaries in the corresponding matched sample.

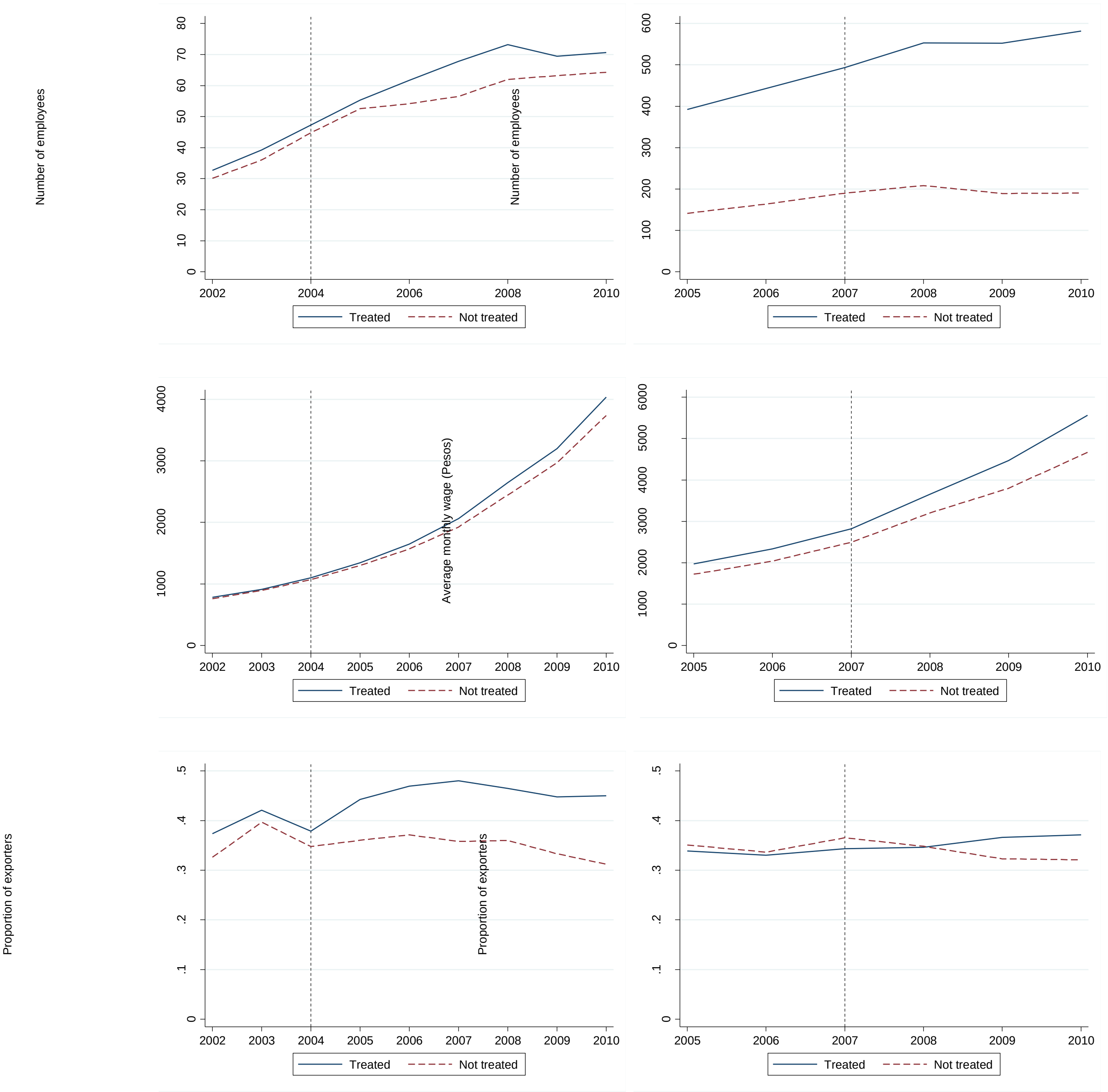
Table 4: Average effect of the program

	Dep. Var.: Number of employees (in logs)		Dep. Var.: Average monthly wages (in logs)		Dep. Var.: 1 if exporter		Dep. Var.: 1 if survive	
	Full sample	Matched	Full sample	Matched	Full sample	Matched	Full sample	Matched
A) Direct effect								
Direct beneficiary	0.388***	0.172**	0.0285	0.0615**	0.0628***	0.0623**	0.0649***	0.0371***
	[0.0461]	[0.0716]	[0.0209]	[0.0309]	[0.0185]	[0.0242]	[0.00766]	[0.0128]
R-squared	0.12	0.20	0.79	0.87	0.00	0.02	0.06	0.04
Number of observations	3,773,123	3,119	3,773,123	3,119	3,773,123	3,119	3,773,123	3,119
Number of firms	776,825	374	776,825	374	776,825	374	776,825	374
B) Spillover effect								
Indirect beneficiary	0.204***	0.149**	0.0174	0.0357*	0.0322***	0.0482**	0.0269***	-0.0069
	[0.0420]	[0.0586]	[0.0142]	[0.0209]	[0.0121]	[0.0188]	[0.00767]	[0.0105]
R-squared	0.08	0.10	0.68	0.82	0.00	0.01	0.079	0.031
Observations	3,002,880	2,591	3,002,880	2,591	3,002,880	2,591	3,002,880	2,591
Number of firms	773,980	444	773,980	444	773,980	444	773,980	444

Notes: Direct beneficiary is a dummy variable that takes value one for the direct beneficiaries of FONTAR after 2004. Indirect beneficiary is a dummy variable that takes value one for the indirect beneficiaries of FONTAR after 2007. All equations include firm level fixed-effects, year dummies, and age and age squared. Robust standard errors in bracket, clustered by firm. ***, **, * significant at 1%, 5%, and 10%, respectively.

Figure 4: Evolution of employment, wages, and proportion of exporters. Matched sample

A. Direct effect **B. Spillover effect**



Previous results show the average direct and indirect effects of the program over the period we observe after treatment. Given that we observe firms several years after they receive support, we can estimate the way in which the effect takes place in time. We can answer questions like how long it takes to see the effect of the program or whether the effect lasts several years after the firm receives support.

The dynamic effect of the program also provides us with an important robustness check. As we mentioned above, we aim at estimating the causal effect of the program both on direct and indirect beneficiaries. To check that what we are estimating is the effect of the program, the increase in outcome variables should not appear before beneficiaries received the program.

To address these questions we estimate the following model:

$$(2) \qquad Y_{it} = \alpha_i + \mu_t + \beta_{-1}\, d_{i-1} + \beta_0\, d_{i0} + \beta_1 d_{i1} + \cdots + \beta_k d_{ik} + \gamma X_{it} + \varepsilon_{it},$$

where d_{-1}.is a dummy variable that takes value one one year before the firms receive the support from the program, d_0 is a dummy variable that takes value one the year in which firm i receives the support, d_1 is a dummy variable that takes value one one year after firm i receives the support, and so on.

The coefficient β_{-1} measures the effect of the program one year before the firm received the support. We use this coefficient as a placebo test: to confirm the causal interpretation of our results this coefficient has to be zero. Similarly, β_0 measures the impact of the program the firm receives the support, β_1 measures the effect one year later, and β_k measures the effect after k years. All these effects are measured against the baseline – situation with no program—and therefore they are not the effect for that particular year but the cumulative effect until that year. Like in previous case, we estimate equation (2) for direct and indirect beneficiaries separately.

Panel A in Table 5 shows the dynamic direct effect of the program. In the case of the number of employees, β_{-1} is significant when we consider the firms in the full sample and non-significant when we consider firms in the matched sample. This finding shows that the estimates in the full sample have no causal interpretation and might reflect the fact that larger firms applied for the program. They also provide additional evidence in favor of our identification strategy. In fact, only the estimates in the matched sample satisfy the condition that the consequence cannot appear before the cause. The estimates on the matched sample show that the effect of the program on employment appears three years after the firm received the support of the program. The effect is increasing and continues growing even six years after receiving the benefit.

The effect on real wages is increasing, but statistically non-significant. It becomes significant at ten percent only when we do not control for industry trends. The effect on the probability of exports is similar to the effect on employment. Although quantitatively smaller, it starts after the third year, it is increasing, and it continues growing even after six years. The effect on survival probability starts one year after entering the program and is significant even after six years.

Panel B in Table 5 shows analogous results for the dynamic indirect effect of FONTAR. In the case of employment, the effect appears one year after firms hire skilled workers that were employed in a FONTAR firm. The effect on real wages appears three years after the firm hires the workers that were employed in a FONTAR firm and the effect is significant only at ten percent. The effect on the probability of exports appears two year after the hiring of FONTAR workers. In this case, there is no effect on the survival probability even three years after the hiring of FONTAR workers.

Figure 5 shows the evolution of the direct and indirect effects with 95 percent confidence intervals. The direct effect on wages needs special attention. Although the effects are statistically not significant, they show an increasing trend and after six years the lower limit of interval is close to zero. This trend shows that it is likely that the effect would be significant in a longer period. The same occurs with the indirect effect on wages. The effect is only significant at ten percent after three years—and therefore the 95 percent confidence interval includes zero—but the trend is positive.

Table 5: The dynamic effect of the program

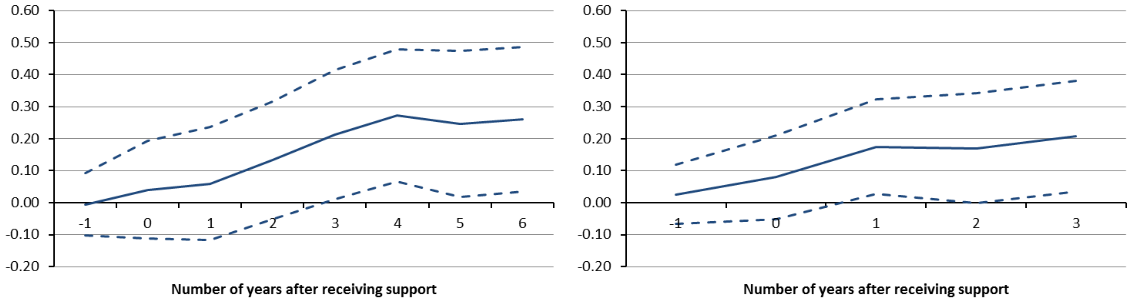
	Dep. Var.: Number of employees (in		Dep. Var.: Average monthly wages		Dep. Var.: 1 if exporter		Dep. Var.: 1 if survive	
	Full sample	Matched sample	Full sample	Matched sample	Full sample	Matched sample	Full sample	Matched sample
A) Direct effect								
d_1	0.138***	-0.00198	-0.00901	-0.0264	0.021	-0.0233	0.0565***	0.0000
	[0.0433]	[0.0564]	[0.0171]	[0.0250]	[0.0240]	[0.0334]	[0.00291]	[0.000267]
d0	0.327***	0.0452	-0.00478	-0.0117	0.0418	-0.0192	0.0527***	0.0179
	[0.0533]	[0.0788]	[0.0240]	[0.0328]	[0.0261]	[0.0331]	[0.0126]	[0.0215]
d1	0.479***	0.0698	0.0244	0.0659	0.0543*	0.0196	0.0812***	0.0304*
	[0.0546]	[0.0904]	[0.0282]	[0.0444]	[0.0277]	[0.0400]	[0.00875]	[0.0155]
d2	0.517***	0.137	0.0141	0.0324	0.0864***	0.0479	0.0952***	0.0414***
	[0.0578]	[0.0941]	[0.0281]	[0.0417]	[0.0268]	[0.0376]	[0.00925]	[0.0153]
d3	0.515***	0.220**	0.0375	0.0434	0.107***	0.0845**	0.0979***	0.0456*
	[0.0626]	[0.102]	[0.0280]	[0.0425]	[0.0274]	[0.0402]	[0.0135]	[0.0255]
d4	0.490***	0.282***	0.032	0.0585	0.0878***	0.0753*	0.123***	0.0446**
	[0.0657]	[0.106]	[0.0303]	[0.0459]	[0.0289]	[0.0432]	[0.0122]	[0.0191]
d5	0.470***	0.250**	0.0538*	0.0787	0.0714**	0.0750*	0.132***	0.0560***
	[0.0733]	[0.116]	[0.0320]	[0.0487]	[0.0302]	[0.0444]	[0.0126]	[0.0207]
d6	0.448***	0.265**	0.0159	0.0940*	0.0765**	0.104**	0.0925***	0.0312***
	[0.0713]	[0.116]	[0.0500]	[0.0518]	[0.0307]	[0.0440]	[0.00751]	[0.0103]
R-squared	0.12	0.21	0.79	0.87	0.00	0.02	0.06	0.05
Number of observations	3,773,123	3,119	3,773,123	3,119	3,773,123	3,119	3,773,123	3,119
Number of firms	776,825	374	776,825	374	776,825	374	776,825	374
B) Spillover effect								
d_1	0.071	0.0267	0.000565	-0.00311	-0.0136	0.0062	0.0687***	-0.000464
	[0.0493]	[0.0465]	[0.0192]	[0.0256]	[0.0154]	[0.0204]	[0.00359]	[0.000376]
d0	0.317***	0.0788	-0.00441	-0.00833	-0.0026	-0.00969	0.0907***	0.02
	[0.0520]	[0.0672]	[0.0220]	[0.0293]	[0.0164]	[0.0237]	[0.00762]	[0.0145]
d1	0.372***	0.172**	0.0355	0.0092	0.0177	0.0152	0.0916***	-0.00392
	[0.0541]	[0.0750]	[0.0221]	[0.0298]	[0.0200]	[0.0274]	[0.0112]	[0.0170]
d2	0.360***	0.172**	0.0081	0.0261	0.0303	0.0597**	0.0937***	0.0013
	[0.0614]	[0.0874]	[0.0241]	[0.0327]	[0.0187]	[0.0280]	[0.0125]	[0.0168]
d3	0.307***	0.210**	0.00229	0.0627*	0.0331*	0.0688**	0.0651***	0.00179
	[0.0623]	[0.0882]	[0.0246]	[0.0380]	[0.0198]	[0.0304]	[0.00623]	[0.00737]
R-squared	0.08	0.10	0.68	0.82	0.00	0.01	0.08	0.03
Number of observations	3,002,880	2,591	3,002,880	2,591	3,002,880	2,591	3,002,880	2,591
Number of firms	773,980	444	773,980	444	773,980	444	773,980	444

Notes: All equations include firm level fixed-effects, year dummies, and age and age squared. Robust standard errors in bracket, clustered by firm. ***, **, * significant at 1%, 5%, and 10%, respectively.

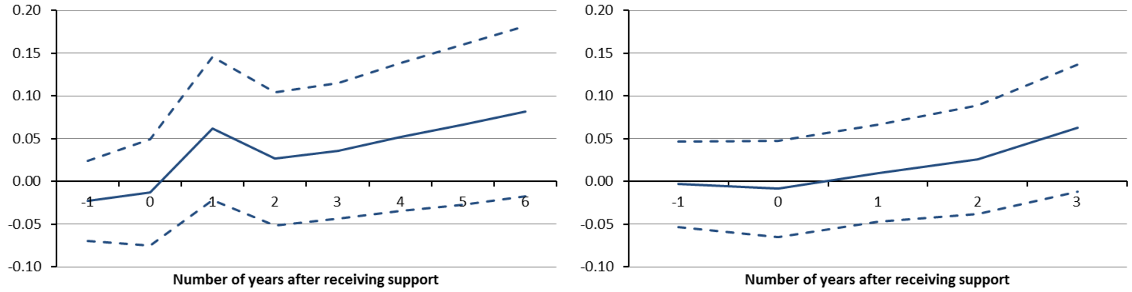
Figure 5: Direct and indirect impact of the program

A. Direct **B. Indirect**

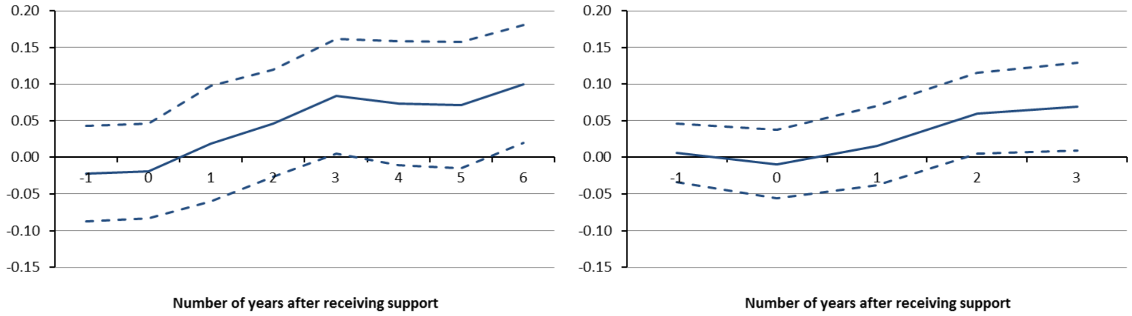
a. Employment



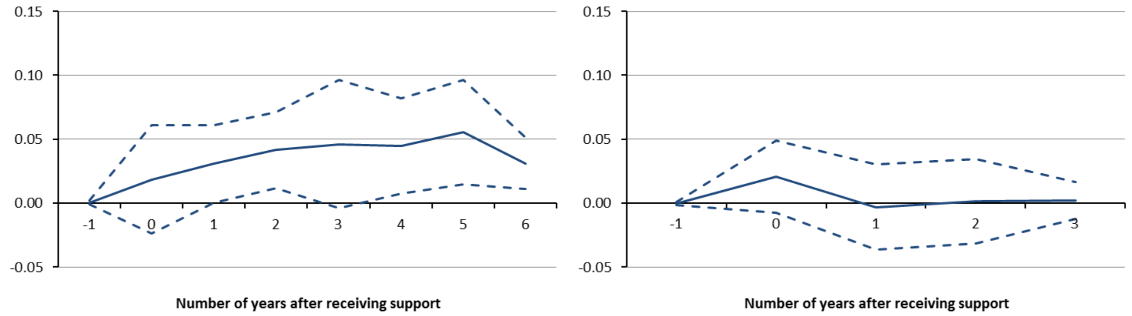
b. Wages



c. Probability of export



d. Probability of survive



6. Conclusions

In this paper we estimated the long run direct and spillover effects of the FONTAR program on several measure of firms' performance. To estimate the spillover effects we considered the diffusion of knowledge through the mobility of qualified workers from the firms that received the FONTAR support to firms that did not receive any direct support. Our empirical strategy takes advantage of a large employer-employee panel dataset that allows us to control the selection bias using fixed-effects. Thanks to the panel structure of the data, we could also check the robustness of our identification strategy with a placebo test based on anticipatory effects.

In line with the theory of change that justifies the program, we found not only positive direct and spillover effects of the program on firms' performance, but also increasingly significant and positive effects over time. Direct and indirect beneficiaries experienced respectively 17.2 and 14.9 percent employment growth as a consequence of the program. The program also strengthened the ability of direct beneficiaries to compete, increasing their survival probability by 3.7 percent. Positive effects on firms efficiency and skills are also signaled by the increased probability of exporting (6.2 and 4.8 percent for direct and indirect beneficiaries) and increased real wages (6.1 and 3.6 percent for direct and indirect beneficiaries). None of these effects occurred immediately: the direct effect on employment and the probability of exports occurred three years after the firms received the support from FONTAR, while the effect on wages appeared only after six years. A similar dynamic, in shorter terms, occurred with the spillover effects: indirect effects on employment, real wages, and probability appeared one, three, and two years after the firm hired skilled FONTAR workers.

These findings shed light on two fundamental aspects of programs that provide public funding to private innovation project. First, they confirm that if these programs affect firms' innovation investment in the short run—as previous evaluations have shown is the case with the FONTAR—they will have also a positive effect on the firms' competitive performance in the medium-long run. Second, they provide evidence on the validity of one key theoretical justification for these programs—i.e. the lack of full appropriation of benefits of innovation investments by the investors. In fact, because private firms have no reason to include knowledge spillover benefits in the maximization function of their investment in innovation, they will end up investing below the social optimum without proper support by agents maximizing social returns.

These findings have clear implication for policy design, in particular with reference to the dimensioning of programs such as FONTAR. In fact, because many times externalities and dynamic effects are not fully (or properly) considered in ex-ante cost-benefit analysis, the decision on the size of these interventions could be quite biased and lead to design programs that are out of proportion to their potential social return; most likely undersized and underfunded programs.

In addition, these findings points to the need of planning longer-term impact evaluations to be able to detect effects on most relevant outcomes of interest. This does not necessarily mean that final impact evaluations should be carried out five years after the project's execution. Evaluations could focus instead on the first cohorts of treated firms, so that by the end of a program some results on performance could also be assessed. However, in some cases data collections data several years after the programs' initial implementation may be needed. This could make the political-economy of evaluations quite challenging, given that the time-frame they cover may overcome the tenure of the authorities responsible of their planning, budgeting, and implementation. A way to mitigate this problem could be to link these evaluations to data sources which are collected independently from the program—as those used in this study—with the shortcoming that data may not be perfectly tailored to the objectives of the program.

Future research should focus on closing some gaps that for data limitation this study could not address. First, although the assumption that high wage workers are qualified workers is certainly reasonable, study including precise information on workers qualifications could add to the understanding of the specific mechanism through which the spillovers occur. Second, although this study provides evidence on the program impact on firms' efficiency, its finding could be complemented by future research that focuses on direct measures of productivity, such as labor productivity and TFP.

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