Knowledge Spillovers of Innovation Policy through Labor Mobility: Evidence from the FONTAR Program in Argentina[*]

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

October, 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 2002-2013. 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 propensity score matching to estimate the causal effect—direct and indirect—of the program on various measures of firms' performance. We find that the program increased employment, the exporting probability, and value of exports 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. Our findings are robust to a placebo test based on anticipatory effects and different matched samples.

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

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

---

[*] We thank Gustavo Crespi, Ernesto Stein, and the participants to the 7th MEIDE and at seminars at ILADES and Inter-American Development Bank for useful comments. The usual disclaimers apply. The views expressed in this paper are those of the authors and do not represent those of the Inter-American Development Bank, the Argentinean Ministry of Labor, Employment, and Social Security, or the Economic Commission for Latin America and the Caribbean.

[†] Ministry of Labor, Employment, and Social Security. Buenos Aires, Argentina. E-mail: vcastillo@trabajo.gob.ar.
[‡] Office of Strategic Planning and Development Effectiveness, Inter-American Development Bank, Washington, DC, and Universidad de San Andrés, Buenos Aires, Argentina. E-mail: lfigal@iadb.org.
[§] Office of Strategic Planning and Development Effectiveness, Inter-American Development Bank, Washington, DC. E-mail: alessandrom@iadb.org.
[**] Economic Commission for Latin America and the Caribbean, Santiago, Chile. Email: ana.rojo@cepal.org
[††] Country Andean Department, , Inter-American Development Bank, La Paz, Bolivia. E-mail: rstucchi@iadb.org.

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 impact evaluations. 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 without considering spillover effects. Addressing spillover effects 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 started in 1995 and has been the main innovation support program in Argentina. 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 short and long-run impact of FONTAR on a series of key performance indicators–including firms' growth in terms of employment, labor productivity through wages, and exports–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 2002 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 for several years; our dataset covers the period 2002-2013. 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 commercial sustainability of the investment.

Second, the main and most valuable outcomes of innovation projects are intangible

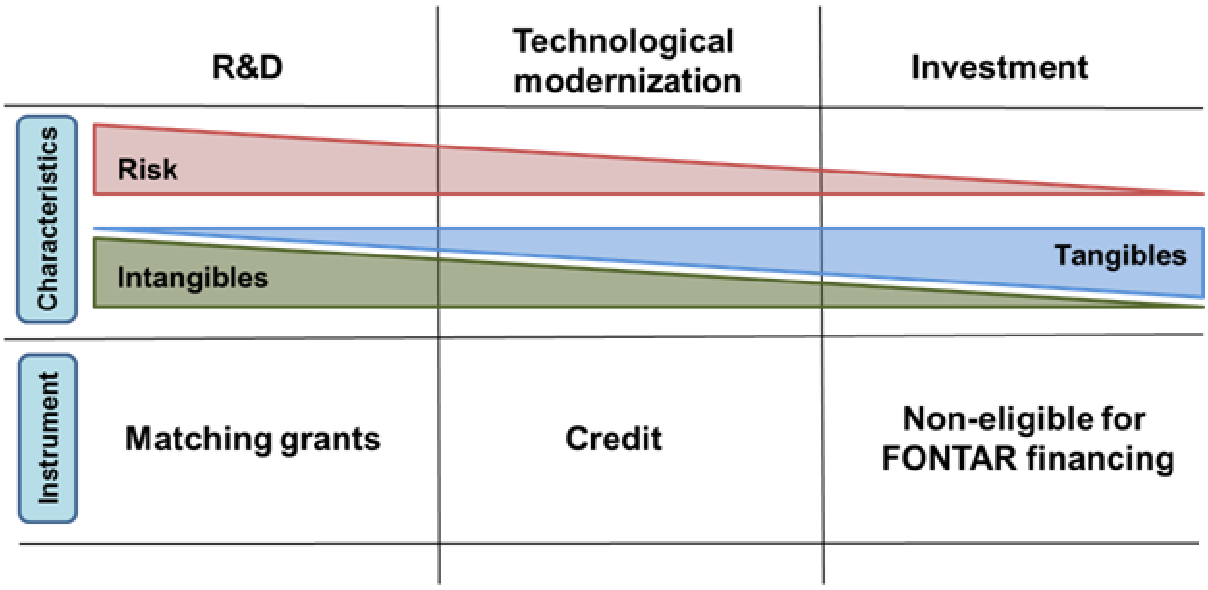
[1] FONTAR tax credits are non-automatic and project based.

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.

Third, 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, 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), 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 2002. 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 2013, 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. 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

[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.

gains.[3] 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

4.1 Identifying knowledge spillovers through labor mobility

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 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

[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).

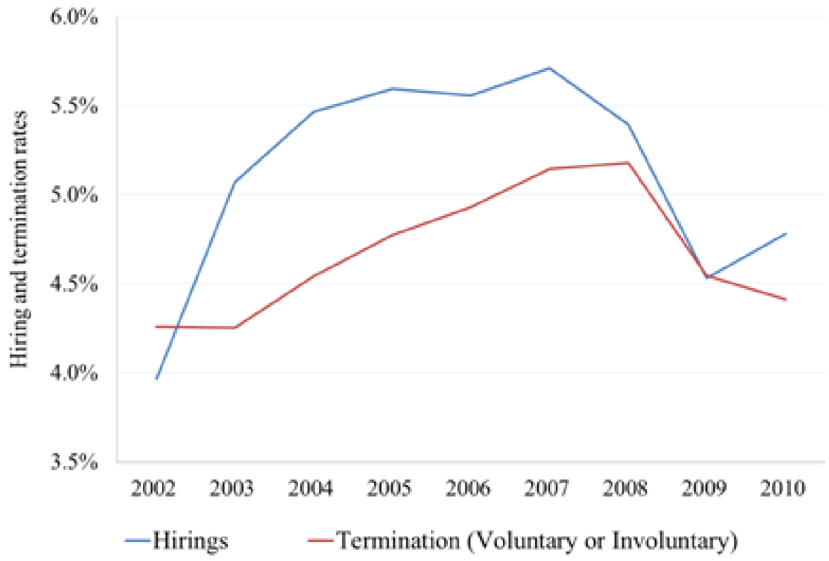
the firm and employee level. Here is where the employer-employee structure of our data becomes extremely valuable for our study. In fact, it allows us to define precise employment transition matrices and, consequently, to identify those firms that may have indirectly benefited from the program by hiring 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 using the unique tax identification code (CUIT) of each firm. This is a straightforward process which implies merging FONTAR administrative records with the OEDE dataset.

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 transitions we consider as relevant for potential knowledge spillovers.

To fully exploit the strengths of our identification strategy we focused our analysis on the cohorts of beneficiaries that received the program support between 2004 and 2006. 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 around five years after the treatment to have a proper assessment of long-run effects. Particularly 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 2007 to allow enough time to fully observe long-run indirect effects.

Second, the 2004/5/6 cohorts allow us to use pre-treatment data from a rather homogenous period. In fact, by focusing on these cohorts we can use a two-year post-devaluation period (2002-2003 for direct beneficiaries and 2004-2005 for indirect beneficiaries) 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.

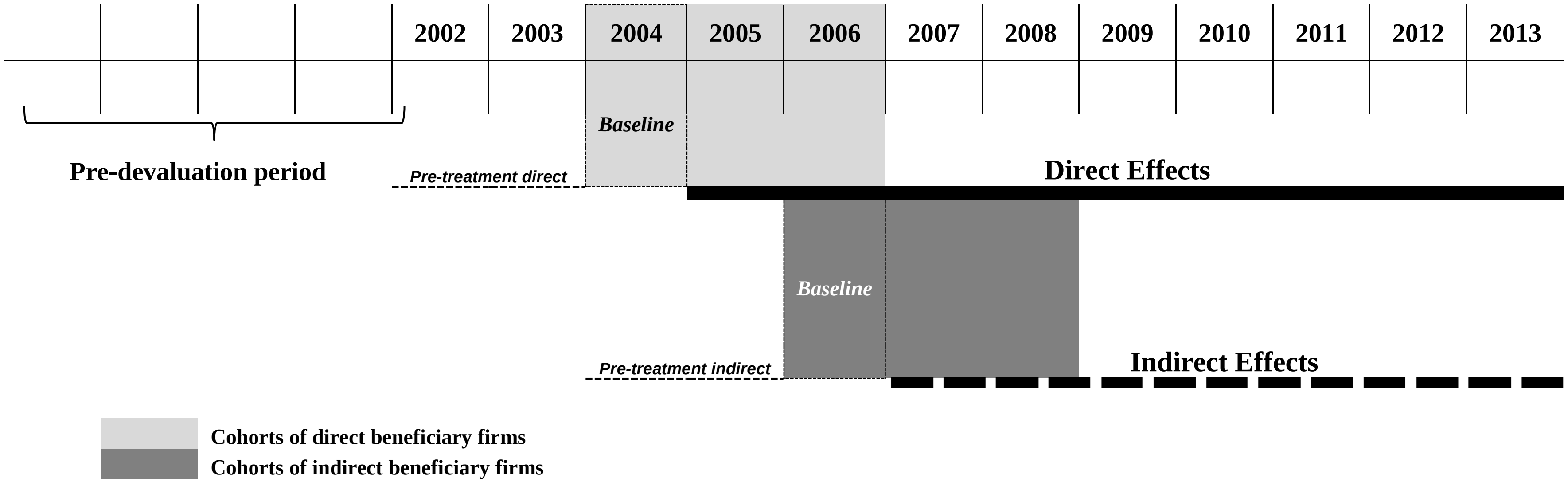
Finally, the analysis of these cohorts allow us to focus on a period when the source of indirect effect is potentially very important, given that during the recovery from the 2001 crisis the labor market was quite dynamic in the creation of new jobs and labor mobility was high. (Figure 2)

Table 1 summarizes the outflows of workers from the firms that received FONTAR support between 2004 and 2006. Around 100,000 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 43.6 percent of these workers eventually moved to a different firm. However, when we restrict the analysis considering a minimum duration of employment in a FONTAR beneficiary firm, the mobility drops considerably—13.8 percent of the workers that were in a FONTAR firm form more than two years moved to other firm.

Table 1: The mobility of workers in FONTAR beneficiary firms

	Years in a FONTAR beneficiary firm					**Total**
	< 1	**1 to 2**	**2 to 3**	**4 to 5**	**> 5**	
FONTAR firms 2004-2006						
Stay in the firm	14,495	6,196	3,947	7,267	24,382	56,287
Move to other firms	22,694	6,979	3,729	4,101	6,000	43,503
Total	37,189	13,175	7,676	11,368	30,382	99,790
Workers that create FONTAR spillovers	-	-	126	165	340	631
Indirect beneficiary firms	-	-	91	122	235	448

Figure 3: Cohorts used for the analysis



4.2. Identification strategy and estimation methods

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.[10]

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 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 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:

$$Y_{i,j,r,t} = \alpha_i + \alpha_t + \alpha_{j,r,t} + \beta . T_{i,t} + \gamma . X_{i,t} + \varepsilon_{i,j,r,t} \tag{1}$$

where $Y_{i,j,r,t}$ represents the set of outcomes to be considered for firm i, belonging to industry j, in region r and year t. Firm fixed effects α_i fully absorb any permanent heterogeneity at the

[10] 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.

firm, industry and region level, and α_t represents yearly shocks that affect all firms. Regarding the interaction terms, $\alpha_{j,r,t}$ are industry-region-year effects that fully absorb industry-year effects – i.e. time-specific shocks that affect the outcomes of all firms in industry j – and region-year effects such as the construction of a freeway, an airport, or implementation of new local policies.[11]

$T_{i,t}$ is a binary variable that takes the value of one the year in which the firm i enters the program and so thereafter. In the case of the comparison between indirect beneficiaries and the control group $T_{i,t}$ takes the value one since the year in which the indirect beneficiary firm started receiving the spillover as defined above. Therefore, β represents the parameter of interest which captures the causal effect of $T_{i,t}$ on the outcome under consideration. In other words, in absence of time-varying unobserved factors that affect both the outcome and the participation, β is the average impact of the FONTAR program on the direct or indirect beneficiary firms. Finally, X_{it} is a vector of time-varying control variables and $\varepsilon_{i,j,r,t}$ is the usual error term assumed to be uncorrelated with $T_{i,t}$. The standard errors will be clustered at the firm level for the inference to be robust to within-firm correlation of the error terms.

The set of year dummies (α_t) 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.

[11] A similar approach is followed by Moretti (2004) to measure human capital spillovers in manufacturing in the US.

Therefore, to strengthen the validity of our identification strategy, we combine the fixed effects methodology with propensity score matching, 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, a dummy variable that takes value one if the firm exported before the baseline, and export volume. 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.

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.[12]

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. Appendix I 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 direct beneficiaries were larger, older, paid higher wages, had higher probability of exporting than the rest of firms in Argentina and export more.[13] 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. In the matched sample beneficiary and control groups are more similar, confirming that matching was successful in identifying non-beneficiary firms with similar observable averages baseline characteristics of the direct beneficiaries. In few cases, where balancing is not perfect in levels—such as in the case of wages and wages of new employees— we expect the differences in those variables to be constant overtime, which is a sufficient condition to support the hypothesis of equality of trends in the absence of the treatment.

Table 3 shows analog results for indirect beneficiaries. Before indirect beneficiaries hired qualified workers previusly working in a FONTAR firm, they 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.

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 (see section 5.3).[14] Additional evidence of the validity of this assumption is also provided by the graphs in Figure 4 that shows the evolution of employment, wages, proportion of exporters and export volume for direct/indirect beneficiaries, and non-beneficiaries in the corresponding matched sample.

---

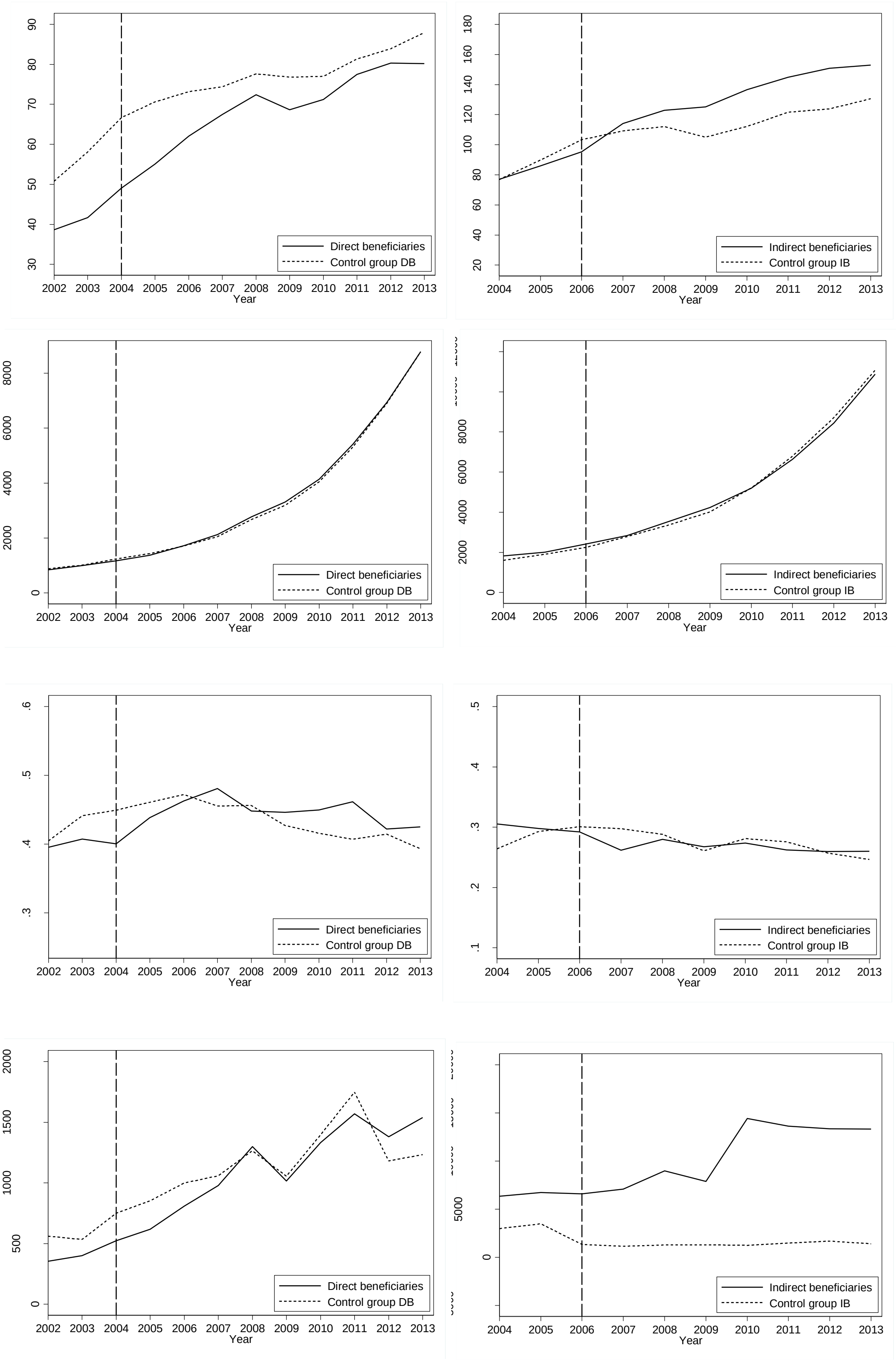
[12] The probability of having two firms with the same propensity score is also higher with the whole population of firms. Given that results could change if different firms are used as controls—i.e. there could be a sorting problem – the dataset needs to be sorted randomly before doing the matching.

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

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

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

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



5.1. The average direct and indirect effects of FONTAR program

Panel A in Table 4 shows the average impact of FONTAR on employment, wages, and 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 (Equation 1).

The average direct effect of the program on employment and exports is quantitatively and statistically significant. Specifically, we find that, relatively to the control group, the employment level, the probability of exporting, and the export volume increased by 30.4 percent, 3.87 percentage points, and 57.9 percent, respectively. The effect on wages, however, is not statistically significant when considering the matched sample.

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, probability of export and the level of exports was 30.2 percent, 2.97 percentage points, and 45.5 percent, respectively. In this case, the effect on the wages is also not statistically significant.

Table 4: Average effect of the program

	Number of employees (in logs)		**Average monthly wages (in logs)**		**Likelihood of exporting**		**Exports (in logs)**	
	Full sample	Matched sample	Full sample	Matched sample	Full sample	Matched sample	Full sample	Matched sample
A) Direct effect								
Average	0.412***	**0.304*****	0.0301*	**0.0323**	0.0319**	**0.0387****	0.783***	**0.579*****
	[0.0358]	**[0.0508]**	[0.0181]	**[0.0218]**	[0.0139]	**[0.0186]**	[0.168]	**[0.223]**
R-squared	0.04	**0.25**	0.83	**0.88**	0.00	**0.13**	0.000	**0.15**
# Observations	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**
# Firms	954,477	**636**	954,477	**636**	954,477	**636**	954,477	**636**
B) Spillover effect								
Average	0.274***	**0.302*****	-0.0267*	**0.00548**	-0.00340	**0.0297***	0.118	**0.455****
	[0.0409]	**[0.0497]**	[0.0162]	**[0.0196]**	[0.0130]	**[0.0155]**	[0.162]	**[0.186]**
R-squared	0.03	**0.21**	0.80	**0.90**	0.00	**0.11**	0.00	**0.11**
# Observations	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**
# Firms	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**

Notes: Results for the full and matched sample. Direct beneficiary is a dummy variable that takes value one for the direct beneficiaries of FONTAR after 2004, 2005 or 2006. Indirect beneficiary is a dummy variable that takes value one for the indirect beneficiaries of FONTAR after 2006, 2007 or 2008. All equations include firm level fixed-effects, industry and region trends, year dummies, and age and age squared. Robust standard errors in bracket, clustered by firm. ***, **, * significant at 1%, 5%, and 10%, respectively.

5.2 Dynamic, multi-treatment and dosage effects of FONTAR program

Previous results show the average direct and indirect effects of the program over whole period after treatment. Given that we observe firms each year 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.

To address these questions we estimate the following model:

$$Y_{i,j,r,t} = \alpha_i + \alpha_t + \alpha_{j,r,t} + \beta_0\, d_{short} + \beta_1 d_{medium} + \beta_2 d_{long} + \gamma . X_{i,t} + \varepsilon_{i,j,r,t} \qquad (2)$$

where d_{short} is a dummy variable that takes value one the first two years in which firm i receives the support, d_{medium} is a dummy variable that takes value one between the third and fourth year after treatment, and d_{long} is a dummy variable that takes value one after 5 years of treatment. Therefore, β_0 , β_1, and β_2 measures the short-term, medium-term, and the long-term effect respectively. All these effects are measured against the baseline – situation with no program – and therefore they are not the effect for that particular period but the cumulative effect until that period. 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. The estimates on the matched sample show that the effect of the program on employment increased over time, from a magnitude of 20.4 percent in the short-term up to 35.7 percent in the long-term. While the average effect on wages was not statistically significant, when considering the dynamics of the effects, we find that the effect on wages is statistically significant and increasing in the short and medium-term, but non-significant and decreasing in the long-term. The effect on the probability of exports is also increasing but significant in the medium and long-term. Lately, the effect on export volume increased over time, from a magnitude of 33.2 two years after entering the program up to 93.1 in the long-term.

Panel B in Table 5 shows analogous results for the dynamic indirect effect of FONTAR on employment and export volume. The effect appears in the short-term after firms hire skilled workers that were employed in a FONTAR firm and are increasing over time. The effect on the probability of exports appears only in the short-term. In this case, there is no effect on wages.

Table 5. The dynamic effects of the program

	Number of employees (in logs)		Average monthly wages (in logs)		Likelihood of exporting		Exports (in logs)	
	Full Sample	Matched sample	Full sample	Matched sample	Full Sample	Matched sample	Full sample	Matched sample
A) Direct effect								
Short_term	0.403***	**0.204*** **	0.0296**	**0.0397** **	0.0522***	**0.0246**	0.854***	**0.332***
	[0.0306]	**[0.0435]**	[0.0140]	**[0.0176]**	[0.0137]	**[0.0173]**	[0.147]	**[0.192]**
Medium_term	0.414***	**0.289*** **	0.0547**	**0.0701** **	0.0430***	**0.0419***	0.932***	**0.600** **
	[0.0392]	**[0.0611]**	[0.0230]	**[0.0283]**	[0.0158]	**[0.0218]**	[0.190]	**[0.266]**
Long_term	0.393***	**0.357*** **	0.0228	**0.0318**	0.0137	**0.0605** **	0.676***	**0.931*** **
	[0.0481]	**[0.0720]**	[0.0230]	**[0.0300]**	[0.0175]	**[0.0260]**	[0.221]	**[0.323]**
R-squared	0.04	**0.25**	0.827	**0.88**	0.001	**0.13**	0.001	**0.15**
# Observations	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**
# Firms	954,477	**636**	954,477	**636**	954,477	**636**	954,477	**636**
B) Spillover effect								
Short_term	0.337***	**0.288*** **	-0.00767	**-0.00130**	0.0149	**0.0324** **	0.264	**0.419** **
	[0.0357]	**[0.0416]**	[0.0142]	**[0.0163]**	[0.0136]	**[0.0151]**	[0.163]	**[0.180]**
Medium_term	0.239***	**0.296*** **	-0.0374**	**0.00953**	-0.0150	**0.0273**	0.0262	**0.485** **
	[0.0479]	**[0.0634]**	[0.0182]	**[0.0233]**	[0.0141]	**[0.0186]**	[0.177]	**[0.224]**
Long_term	0.167**	**0.323*** **	-0.0715**	**-0.00324**	-0.0147	**0.0304**	0.0218	**0.559** **
	[0.0758]	**[0.0760]**	[0.0292]	**[0.0305]**	[0.0212]	**[0.0235]**	[0.254]	**[0.274]**
R-squared	0.03	**0.211**	0.80	**0.90**	0.00	**0.11**	0.00	**0.11**
# Observations	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**
# Firms	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**

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.

As Figure 1 shows, different instruments were used to attend firm's needs. It is important to understand if there is a more effective instrument and if the combination of the two is desirable. For this reason, we estimate equation (1) substituting the treatment dummy with three dummies that identify whether the firm receive a matching grant line only, credit line only or both.

Table 6 shows the effect by type of instrument. In the case of the direct effect we observe that the positive effect of the program is mainly explained by matching grants or the combination of matching grants and credit. The only variable that is affected by credit only, in addition to matching-grants and the combination of both, is employment. Conversely, the indirect effect was driven mainly by credit.

Table 6. Heterogeneity of impacts by type of instrument

	Number of employees (in logs)		Average monthly wages (in logs)		Likelihood of exporting		Exports (in logs)	
	Full	Matched	Full	Matched	Full	Matched	Full	Matched

	Sample	sample	sample	sample	Sample	sample	sample	sample
A) Direct effect								
Matching grant	0.438***	**0.320*** **	0.0362*	**0.0478***	0.0376**	**0.0523*** **	0.708***	**0.648*** **
	[0.0461]	**[0.0602]**	[0.0210]	**[0.0250]**	[0.0164]	**[0.0210]**	[0.191]	**[0.244]**
Credit	0.287***	**0.215*** **	0.000915	**-0.0148**	0.00594	**0.00112**	0.688*	**0.238**
	[0.0609]	**[0.0748]**	[0.0416]	**[0.0407]**	[0.0295]	**[0.0326]**	[0.383]	**[0.418]**
Both	0.570***	**0.463*** **	0.0673	**0.0566**	0.0624	**0.0483**	1.717***	**1.134*** **
	[0.102]	**[0.107]**	[0.0597]	**[0.0714]**	[0.0552]	**[0.0496]**	[0.637]	**[0.568]**
R-squared	0.041	**0.252**	0.827	**0.881**	0.001	**0.132**	0.000	**0.149**
# Observations	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**
# Firms	954,477	**636**	954,477	**636**	954,477	**636**	954,477	**636**
B) Spillover effect								
Matching grant	0.314***	**0.342*** **	-0.0545**	**-0.0123**	-0.0116	**0.0259**	-0.0532	**0.351**
	[0.0653]	**[0.0709]**	[0.0257]	**[0.0274]**	[0.0166]	**[0.0200]**	[0.191]	**[0.240]**
Credit	0.165***	**0.186*** **	0.00130	**0.0222**	0.0146	**0.0430***	0.374	**0.647*** **
	[0.0517]	**[0.0659]**	[0.0229]	**[0.0265]**	[0.0205]	**[0.0224]**	[0.283]	**[0.287]**
Both	0.603***	**0.632*** **	-0.0196	**0.0173**	-0.0499	**-0.0119**	-0.256	**0.108**
	[0.144]	**[0.164]**	[0.0394]	**[0.0436]**	[0.0561]	**[0.0587]**	[0.545]	**[0.561]**
R-squared	0.033	**0.218**	0.798	**0.898**	0.001	**0.115**	0.000	**0.115**
# Observations	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**
# Firms	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**

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

An additional source of identification of the effect of the program is the fact that not every firm received the same amount of co-financing by FONTAR. This fact allows us to explore how the effects vary according to the treatment intensity. This is also valid for the estimation of the indirect effect because the share of FONTAR workers to total workers varies across indirect beneficiary firms.

Table 7 shows the estimation results. Results prove to be robust to different definitions of the treatment. In addition, this Table allow us to estimate the average change in performance variables to changes in the treatment amounts. In fact, one percent increase in the amount received though FONTAR co-financing leads to an increase in 0.036 percent in employment, in 0.005 percent in the probability of export, and 0.07 percent in the value exported. In the case of indirect beneficiaries, a one percent increase in the share of FONTAR workers to total workers drives an increase of 0.056 percent increase in employment, 0.006 percent in the probability of export, and in 0.087 percent in the value exported. …

Table 7. Treatment intensity

	Number of employees (in logs)		**Average monthly wages (in logs)**		**Likelihood of Exporting**		**Exports (in logs)**	
	Full	Matched	Full	Matched	Full	Matched	Full	Matched

	sample	sample	Sample	sample	Sample	sample	sample	sample
A) Direct effect								
Intensity (log)	0.048***	**0.037*** **	0.003	**0.003**	0.004**	**0.005** **	0.09***	**0.07*** **
	[0.00419]	**[0.00583]**	[0.00215]	**[0.00257]**	[0.00163]	**[0.00213]**	[0.0194]	**[0.0252]**
R-squared	0.04	**0.25**	0.83	**0.88**	0.00	**0.13**	0.00	**0.15**
# Observations	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**	5,463,765	**6,811**
# Firms	954,477	**636**	954,477	**636**	954,477	**636**	954,477	**636**
B) Spillover effect								
Intensity (log)	0.049***	**0.056*** **	-0.004	**0.002**	0.001	**0.006** **	0.035	**0.087*** **
	[0.00724]	**[0.00878]**	[0.00312]	**[0.00372]**	[0.00228]	**[0.00271]**	[0.0284]	**[0.0327]**
R-squared	0.03	**0.21**	0.80	**0.90**	0.00	**0.11**	0.00	**0.11**
# Observations	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**	5,324,696	**5,653**
# Firms	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**	1,007,261	**627**

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

Overall our findings show that the FONTAR program has been effective in fostering the growth of firms and their exports—both the probability of becoming an exporter and the value of exports for firms that were already exporting. In the case of direct beneficiaries, these results were driven by the innovation projects they co-financed with the program. In the case of indirect beneficiaries, the results were driven by the knowledge acquired by hiring qualified workers that were exposed to the innovation project while they were in a FONTAR firm. In both cases the results reflect an implicit increase in firms' productivity. In fact, the strong simultaneous effect on both employment and exports, is hardly achievable without a significant increase in firms' productivity. Although weaker, the increase in wages observed when considering the dynamic effect reinforces the hypothesis of an increase in productivity. The weak increase in wages after the treatment is somewhat expected because a large proportion of wages is set through unions agreements. The unionization rate in direct and indirect beneficiary firms is on average 86 percent and 79 percent, respectively.[15] We have therefore collected evidence that supports the hypothesis that the innovation projects put in place by the FONTAR program during this period, mainly matching grants were actually effective.

5.3. Robustness checks

The validity of our results rests on the assumption that in the absence of the treatment—direct

[15] In the firms that did not participated in FONTAR the percent of unionized workers is 99 percent. See Appendix I, Table A.

or indirect—beneficiaries would have the same trend in the performance variables. Altough we cannot test this counterfactual assumption, we can test if this assumption was valid before the treatment took place. Therefore we run a pre-treatment trends equality test which assesses whether the pre-intervention time trends of beneficiaries and non-beneficiaries are different.[16] We run the following regression using the observations of beneficiaries and non-beneficiaries in the pre-treatment period only:

$$Y_{i,j,r,t} = \alpha_i + \alpha_t + \alpha_{j,r,t} + \beta_0 . Placebo\_0_{i,t} + \beta_1 . Placebo\_1_{i,t} + \gamma . X_{i,t} + \varepsilon_{i,j,r,t} \quad (3)$$

where $Placebo\_0_{i,t}$ and $Placebo\_1_{i,t}$ are dummy variables for future treatment.[17] The lack of significance of the coefficient of this lead would provide clear evidence of the similarity of pre-treatment trends in the outcome variable and strongly support the validity of the assumption of equal trend in absence of the treatment.[18] In fact, since the program cannot have an effect on the outcome before participation, the significance of this variable would suggest that the treatment dummies are capturing differences between beneficiary and non-beneficiary firms other than participation that are not being accounted for.

Table 8 shows that there are no significant differences in the pre-treatment trends of the outcome variables among the groups in the matched sample. These results support the assumption that the average outcomes of the beneficiary firms and the matched control groups would have followed a similar pattern – moving in tandem – in the post-intervention period in the absence of treatment.

Table 8. Pre-treatment trends equality test

	Number of employees (in logs)		**Average monthly wages (in logs)**		**Likelihood of exporting**		**Exports (in logs)**	
	Full sample	Matched sample	Full sample	Matched sample	Full Sample	Matched sample	Full sample	Matched sample
A) Direct effect								
Placebo_0	0.369***	**-0.000959**	0.00274	**0.0116**	0.0286	**-0.0148**	0.540***	**-0.278**
	[0.0400]	**[0.0630]**	[0.0182]	**[0.0279]**	[0.0197]	**[0.0312]**	[0.205]	**[0.325]**
Placebo_1	0.214***	**-0.0494**	0.0116	**0.0358***	0.0269	**-0.00369**	0.417**	**-0.0572**
	[0.0296]	**[0.0475]**	[0.0137]	**[0.0212]**	[0.0187]	**[0.0271]**	[0.183]	**[0.261]**
R-squared	0.093	**0.361**	0.524	**0.598**	0.000	**0.096**	0.000	**0.118**
# Observations	1,045,059	**1,760**	1,045,059	**1,760**	1,045,059	**1,760**	1,045,059	**1,760**
# Firms	424,129	**606**	424,129	**606**	424,129	**606**	424,129	**606**
B) Spillover effect								
Placebo_0	0.309***	**0.0753**	-0.0290	**-0.0131**	0.0346*	**0.00293**	0.445**	**0.0958**

[16] See Galiani et al. (2005), Castillo et al. (2014), Arráiz et al. (2013), and Arráiz et al. (2014).
[17] $Placebo\_2_{i,t}$ is omitted in equation (3) because of perfect collinearity.
[18] See Heckman and Hotz (1989).

	[0.0420]	**[0.0534]**	[0.0183]	**[0.0247]**	[0.0193]	**[0.0271]**	[0.196]	**[0.273]**
Placebo_1	0.163***	**-0.0177**	-0.000596	**0.00168**	0.0150	**-0.00966**	0.214	**-0.0265**
	[0.0303]	**[0.0411]**	[0.0137]	**[0.0215]**	[0.0199]	**[0.0250]**	[0.190]	**[0.239]**
R-squared	0.123	**0.353**	0.516	**0.556**	0.000	**0.102**	0.000	**0.125**
# Observations	1,346,772	**1,634**	1,346,772	**1,634**	1,346,772	**1,634**	1,346,772	**1,634**
# Firms	537,146	**572**	537,146	**572**	537,146	**572**	537,146	**572**

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

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. The panel structure of the data, also allowed us to 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 30.2 and 30.4 percent employment growth as a consequence of the program. The program also strengthened the ability of direct and indirect beneficiaries to compete internaitonally. The program increased the probability of exporting and the value of exports of direct beneficiaries by 3.87 percentage points and 57.9 percent, respectively. In the case of indirect beneficiaries these numbers were 2.97 percentage points and 45.5 percent, respectively. The effects of the program direct and indirect were increasing with the intensity of the treatment. 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.

References

Aitken, B. and A. Harrison.1999. "Do Domestic Firms Benefit from Direct Foreign Investment? Evidence from Venezuela," *American Economic Review* 89 (3), 605-618.

Almeida, P. and B. Kogut. 1999. "Localization of Knowledge and the Mobility of Engineers in Regional Networks," *Management Science* 45 (7), 905-917.

Almeida, P. and A. Phene. 2004. "Subsidiaries and knowledge creation: the influence of the MNC and host country on innovation," *Strategic Management Journal* 25 (8-9), 847-864.

Angrist, J. and J. Pischke. 2008. "*Mostly harmless econometrics. An empiricist's companion*," Princeton University Press.

Arráiz, I, M. Melendez, and R. Stucchi. 2014. "Partial credit guarantees and firm performance: Evidence from the Colombian National Guarantee Fund," *Small Business Economics*, Forthcoming.

Arráiz, I., F. Henriquez, and R. Stucchi. 2013. "Supplier development programs and firm performance: Evidence from Chile," *Small Business Economics* 41(1), 277-293, 2013.

Aw, B.Y., Chung, S., Roberts, M.J., 2000. "Productivity and turnover in the export market: micro-level evidence from the Republic of Korea and Taiwan (China)," *World Bank Economic Review* 14 (1), 65-90.

Balsvik, R. 2011. "Is labor mobility a channel for spillovers from multinationals? Evidence from Norwegian manufacturing," *Review of Economics and Statistics* 93 (1), 285–97

Binelli, C. and A. Maffioli. 2007. "A Micro-econometric Analysis of Public Support to Private R&D in Argentina," *International Review of Applied Economics* 21(3), 339-359.

Bernard, A. B., Jensen, J.B., 1999. "Exceptional exporter performance: cause, effect, or both?" *Journal of International Economics* 47 (1), 1-25.

Bernard, A.B., Eaton, J., Jensen, J.B., Kortum, S., 2003. "Plants and productivity in international trade," *American Economic Review* 93 (4), 1268-1290.

Bernard, A.B., Jensen, J.B., 2004. "Why some firms export?," Review of Economics and Statistics 86 (4), 561-569.

Boschma, R., R. Eriksson and U. Lindgren. 2009. "How does labour mobility affect the performance of plants? The importance of relatedness and geographical proximity," *Journal of Economic Geography* 9 (2), 169-190.

Buckley, P., J. Clegg, P. Zheng, P. Siler, G. Giorgioni. 2007. “The impact of foreign direct investment on the productivity of China’s automotive industry,” *Management International Review* 47 (5), 707-724.

Castillo, V., A. Maffioli, S. Rojo, and R. Stucchi. 2013. “The effect of innovation policy on SMEs’ employment and wages in Argentina,” *Small Business Economics*, Forthcoming.

Chudnovsky, D., A. López, M. Rossi, and D. Ubfal. 2006. “Evaluating a program of public funding of private innovation activities. An econometric study of FONTAR in Argentina,” OVE Working Papers Nro. 1606, Inter-American Development Bank.

Clerides, S., Lack, S., Tybout, J.R., 1998. “Is learing by exporting important? Micro-dynamic evidence from Colombia, Mexico and Morocco,” *The Quarterly Journal of Economics* 113 (3), 903-947.

Cooper, D. 2001. “Innovation and reciprocal externalities: information transmission via job mobility,” *Journal of Economic Behavior and Organization* 45 (4), 403–425.

Crespi, G., A. Maffioli, and M. Melendez. 2011a. “Public Support to Innovation: The Colombian COLCIENCIAS’ Experience,” IDB Publications 38498, Inter-American Development Bank.

Crespi, G., Solis, and E. Tacsir. 2011b. “Evaluación del impacto de corto plazo de SENACYT en la innovación de las empresas panameñas,” IDB Publications 38218, Inter-American Development Bank.

Fosfuri, A. and T. Rønde. 2004. “High-tech clusters, technology spillovers, and trade secret laws,” *International Journal of Industrial Organization* 22, 45–65.

Fosfuri, A., M. Motta, and T. Rønde. 2001. “Foreign direct investments and spillovers through workers’ mobility,” *Journal of International Economics* 53, 205–222.

Glass, A. and K. Saggi .2002. “Multinational firms and technology transfer,” *The Scandinavian Journal of Economics* 104 (4), 495–513.

Görg, H., and E. Strobl. 2005. “Spillovers from foreign firms through worker mobility: An empirical investigation,” *Scandinavian Journal of Economics* 107 (4), 693–709.

Hall, B. and A. Maffioli. 2008. “Evaluating the impact of technology development funds in emerging economies: evidence from Latin America,” *European Journal of Development Research*, 20(2), 172-198.

Jaffe, A., M. Trajtenberg, and R. Henderson. 1993. “Geographic Localization of Knowledge Spillovers as Evidenced by Patent Citations.” *The Quarterly Journal of Economics* 108 (3), 577–98.

Liu, X. J. Lu, I. Filatotchev, T. Buck, and M. Wright. 2010. “Returnee entrepreneurs, knowledge spillovers and innovation in high-tech firms in emerging economies,” *Journal of International Business Studies* 41, 1183–1197.

Liu, X. and Buck, T. 2007. “Innovation performance and channels for international technology spillovers: Evidence from Chinese high-tech industries,” *Research Policy* 36(3), 355-366.

Maskell, P. and A. Malmberg. 1999. “Localised learning and industrial competitiveness,” *Cambridge Journal of Economics* 23 (2), 167-185.

Melitz, M., 2003. “The impact of trade on intra-industry reallocations and aggregate industry productivity,” *Econometrica* 71 (6), 1695-1725.

Møen, J. 2005. “Is mobility of technical personnel a source of R&D spillovers?” *Journal of Labor Economics* 23, 81–114.

Møen, J. 2007. “R&D spillovers from subsidized firms that fail: Tracing knowledge by following employees across firms,” *Research Policy* 36(9), 1143-1464.

Saxenian, A. 1994. “Regional advantage: culture and competition in Silicon Valley and Route 128,” *Harvard University Press*, Cambridge, Massanchusetts.

Stoyanov, A. and N. Zubanov. 2012. “Productivity spillovers across firms through worker mobility,” *American Economic Journal: Applied Economics* 4, 168-198.

Wei, Y. and X. Liu. 2006. “Productivity spillovers from R&D, exports and FDI in China's manufacturing sector,” *Journal of International Business Studies* 37, 544–557.

Appendix I.

A- Balance test, direct beneficiaries (2004 baseline)

	Full sample				Matched sample			
	Treated	**Control**	**t-stat**	**p-value**	**Treated**	**Control**	**t-stat**	**p-value**
Number of employees 2004	49.08	7.04	39.37	0.00	49.08	66.67	-2.79	0.01
Number of employees 2003	41.70	6.82	22.18	0.00	41.70	58.11	-2.78	0.01
Number of employees 2002	38.89	6.94	18.02	0.00	38.89	50.77	-2.12	0.03
Average monthly wages 2004	1174	727	12.17	0.00	1174	1237	-1.01	0.31
Average monthly wages 2003	1001	609	10.46	0.00	1001	1012	-0.19	0.85
Average monthly wages 2002	841	513	9.59	0.00	841	884	-0.76	0.45
Proportion of exporters 2004	0.40	0.03	44.80	0.00	0.40	0.45	-1.19	0.23
Proportion of exporters 2003	0.41	0.03	44.15	0.00	0.41	0.44	-0.82	0.41
Proportion of exporters 2002	0.40	0.03	40.50	0.00	0.40	0.40	-0.16	0.87
Log of exports 2004	4.85	0.27	50.19	0.00	4.85	5.50	-1.27	0.21
Log of exports 2003	4.84	0.27	49.23	0.00	4.84	5.14	-0.61	0.54
Log of exports 2002	4.67	0.28	44.63	0.00	4.67	4.59	0.17	0.87
Large firms	0.19	0.01	30.29	0.00	0.19	0.25	-1.78	0.08
Medium-sized firms	0.36	0.06	25.08	0.00	0.36	0.46	-2.55	0.01
Small firms	0.31	0.25	2.32	0.02	0.31	0.23	2.09	0.04
Micro firms	0.14	0.68	-21.63	0.00	0.14	0.06	3.39	0.00
Age	15.88	12.57	4.95	0.00	15.88	18.24	-2.00	0.05
Multinational	0.02	0.00	8.20	0.00	0.02	0.01	1.36	0.18
Av. wage of new workers	6.74	6.24	16.58	0.00	6.74	6.73	0.16	0.88
Union workers (%)	0.86	0.99	-30.95	0.00	0.86	0.86	0.07	0.95
Hiring-Termination rate	2.02	1.54	4.36	0.00	2.02	1.87	0.95	0.34
Personas fisicas	0.07	0.50	-16.29	0.00	0.07	0.03	2.13	0.03
SA	0.66	0.14	28.77	0.00	0.66	0.76	-2.73	0.01
SRL	0.23	0.12	5.83	0.00	0.23	0.19	0.96	0.34
Other commercial society	0.03	0.09	-3.94	0.00	0.03	0.01	1.67	0.10
Other association form	0.01	0.15	-7.08	0.00	0.01	0.00	1.21	0.23
Cuyo region	0.10	0.06	3.42	0.00	0.10	0.10	0.15	0.88
Noreste region	0.02	0.05	-2.41	0.02	0.02	0.03	-0.70	0.48
Noroeste region	0.05	0.05	-0.29	0.77	0.05	0.06	-0.34	0.74
Pampeana region	0.82	0.79	1.06	0.29	0.82	0.81	0.29	0.77
Patagonia region	0.01	0.05	-3.06	0.00	0.01	0.01	0.20	0.84
Agriculture and Livestock	0.06	0.15	-4.82	0.00	0.06	0.03	1.49	0.14
Forestry	0.01	0.00	2.17	0.03	0.01	0.00	0.65	0.52
Fishing	0.00	0.00	1.11	0.27	0.00	0.00	0.83	0.41
Oil and gas extraction	0.00	0.00	1.62	0.11	0.00	0.00	-0.26	0.79
Other mining	0.00	0.00	0.58	0.56	0.00	0.00	0.83	0.41
Food and beverages	0.07	0.03	4.96	0.00	0.07	0.11	-1.40	0.16
Textile products	0.01	0.01	-0.21	0.83	0.01	0.00	0.27	0.79
Apparels	0.00	0.01	-1.07	0.28	0.00	0.00	-0.26	0.79
Wood products	0.01	0.01	-0.36	0.72	0.01	0.00	1.18	0.24
Paper products	0.02	0.00	7.07	0.00	0.02	0.02	-0.39	0.70
Editing products	0.01	0.01	-0.10	0.92	0.01	0.02	-0.88	0.38
Oil products	0.01	0.00	7.80	0.00	0.01	0.00	1.18	0.24
Chemical products	0.07	0.01	15.57	0.00	0.07	0.06	0.52	0.60
Rubber products	0.03	0.01	6.00	0.00	0.03	0.04	-0.69	0.49
Non-metallic minerals	0.02	0.00	3.79	0.00	0.02	0.00	1.44	0.15
Common metallic products	0.02	0.00	7.29	0.00	0.02	0.02	0.54	0.59
Other metallic products	0.04	0.02	4.00	0.00	0.04	0.05	-0.22	0.83
Machinery and equipment	0.10	0.01	21.79	0.00	0.10	0.13	-0.98	0.33
Office machines	0.00	0.00	2.75	0.00	0.00	0.00	0.83	0.41
Electric products	0.04	0.00	13.29	0.00	0.04	0.04	-0.00	1.00
Radio and television	0.01	0.00	5.71	0.00	0.01	0.00	1.18	0.24
Medical instruments	0.04	0.00	19.16	0.00	0.04	0.02	1.48	0.14
Automotive and transportation	0.02	0.00	4.11	0.00	0.02	0.01	0.46	0.64
Other transportation equipment	0.00	0.00	0.98	0.33	0.00	0.00	-0.26	0.79
Furniture	0.01	0.01	0.31	0.76	0.01	0.03	-2.17	0.03
Recycling	0.00	0.00	2.51	0.01	0.00	0.01	-0.91	0.36
Construction	0.03	0.04	-0.84	0.40	0.03	0.03	0.18	0.86
Car sales and car repair	0.01	0.04	-3.41	0.00	0.01	0.02	-1.29	0.20
Wholesale	0.05	0.07	-1.20	0.23	0.05	0.06	-0.55	0.58
Retail	0.01	0.16	-7.60	0.00	0.01	0.01	0.20	0.84
Sea and river transportation	0.00	0.00	1.77	0.08	0.00	0.00	-0.26	0.79
Load and storage	0.01	0.01	-1.41	0.16	0.01	0.01	-0.37	0.71
Mail and telecommunications	0.01	0.01	-0.53	0.60	0.01	0.00	0.27	0.79
Financial intermediation	0.00	0.00	-0.32	0.75	0.00	0.00	0.83	0.41
Real estate services	0.01	0.12	-6.52	0.00	0.01	0.00	1.44	0.15
Computer services	0.19	0.01	45.17	0.00	0.19	0.18	0.26	0.80
Research and development	0.01	0.00	10.32	0.00	0.01	0.00	1.21	0.23
Law and accounting services	0.03	0.10	-4.23	0.00	0.03	0.03	0.08	0.93
Education	0.01	0.02	-1.91	0.06	0.01	0.01	-0.87	0.38
Social services	0.02	0.05	-2.44	0.02	0.02	0.02	-0.39	0.70
Enterprises services	0.01	0.04	-3.43	0.00	0.01	0.00	0.27	0.79
Other services	0.01	0.05	-3.57	0.00	0.01	0.00	0.65	0.52

B- Balance test, indirect beneficiaries (2006 baseline)

	Full sample				Matched sample			
	Treated	**Control**	**t-stat**	**p-value**	**Treated**	**Control**	**t-stat**	**p-value**
Number of employees 2004	95.24	7.58	70.81	0.00	95.24	103.32	-0.83	0.41
Number of employees 2003	86.17	7.35	62.31	0.00	86.17	89.83	-0.41	0.68
Number of employees 2002	77.28	7.22	53.92	0.00	77.28	76.85	0.05	0.96
Average monthly wages 2004	2420	1048	25.69	0.00	2420	2246	0.71	0.48
Average monthly wages 2003	2013	867	25.49	0.00	2013	1905	0.51	0.61
Average monthly wages 2002	1830	736	26.73	0.00	1830	1605	1.10	0.27
Proportion of exporters 2004	0.29	0.02	33.14	0.00	0.29	0.30	-0.22	0.83
Proportion of exporters 2003	0.30	0.02	31.90	0.00	0.30	0.29	0.15	0.88
Proportion of exporters 2002	0.31	0.02	30.50	0.00	0.31	0.26	1.06	0.29
Log of exports 2004	3.62	0.24	37.27	0.00	3.62	3.68	-0.13	0.90
Log of exports 2003	3.70	0.24	36.35	0.00	3.70	3.56	0.29	0.77
Log of exports 2002	3.77	0.25	34.86	0.00	3.77	3.27	0.98	0.33
Large firms	0.38	0.01	55.12	0.00	0.38	0.40	-0.51	0.61
Medium-sized firms	0.30	0.06	18.21	0.00	0.30	0.35	-1.38	0.17
Small firms	0.23	0.26	-1.15	0.25	0.23	0.24	-0.25	0.80
Micro firms	0.09	0.67	-22.6	0.00	0.09	0.01	4.40	0.00
Age	17.20	11.85	7.76	0.00	17.20	18.93	-1.14	0.25
Multinational	0.14	0.00	54.62	0.00	0.14	0.11	1.07	0.29
Av. wage of new workers	7.18	6.61	16.53	0.00	7.18	7.16	0.34	0.73
Union workers (%)	0.79	0.99	-47.79	0.00	0.79	0.82	-1.15	0.25
Hiring-Termination rate	2.02	1.44	6.03	0.00	2.02	1.60	1.56	0.12
Personas fisicas	0.10	0.54	-16.05	0.00	0.10	0.09	0.24	0.81
SA	0.60	0.13	25.24	0.00	0.60	0.60	-0.04	0.97
SRL	0.21	0.13	4.32	0.00	0.21	0.24	-0.78	0.44
Other commercial society	0.05	0.08	-2.07	0.04	0.05	0.03	1.25	0.21
Other association form	0.04	0.12	-4.24	0.00	0.04	0.04	0.14	0.89
Cuyo region	0.13	0.06	5.71	0.00	0.13	0.13	0.22	0.83
Noreste region	0.03	0.04	-1.2	0.23	0.03	0.03	0.16	0.87
Noroeste region	0.07	0.05	1.67	0.10	0.07	0.07	0.22	0.83
Pampeana region	0.73	0.79	-2.85	0.00	0.73	0.71	0.47	0.64
Patagonia region	0.03	0.05	-1.41	0.16	0.03	0.07	-1.74	0.08
Agriculture and Livestock	0.08	0.13	-2.71	0.01	0.08	0.08	-0.06	0.96
Forestry	0.00	0.00	0.28	0.78	0.00	0.00	0.87	0.39
Fishing	0.00	0.00	1.46	0.14	0.00	0.00	0.87	0.39
Oil and gas extraction	0.04	0.00	27.17	0.00	0.04	0.02	1.48	0.14
Metallic mineral extraction	0.00	0.00	6.82	0.00	0.00	0.01	-0.83	0.41
Other mining	0.01	0.00	2.22	0.03	0.01	0.00	0.34	0.73
Food and beverages	0.05	0.03	2.43	0.02	0.05	0.06	-0.79	0.43
Textile products	0.01	0.01	0.77	0.44	0.01	0.00	0.73	0.47
Apparels	0.00	0.01	-0.97	0.33	0.00	0.00	0.87	0.39
Wood products	0.00	0.01	-0.81	0.42	0.00	0.00	0.87	0.39
Paper products	0.02	0.00	5.62	0.00	0.02	0.03	-1.08	0.28
Editing products	0.02	0.01	1.49	0.14	0.02	0.02	-0.08	0.93
Oil products	0.00	0.00	4.55	0.00	0.00	0.01	-0.83	0.41
Chemical products	0.03	0.00	7.55	0.00	0.03	0.03	0.36	0.72
Rubber products	0.02	0.01	4.24	0.00	0.02	0.02	0.34	0.73
Non-metallic minerals	0.00	0.00	-0.18	0.85	0.00	0.00	-0.20	0.84
Common metallic products	0.00	0.00	0.25	0.80	0.00	0.00	-0.20	0.84
Other metallic products	0.04	0.02	2.96	0.00	0.04	0.02	0.85	0.40
Machinery and equipment	0.05	0.01	9.03	0.00	0.05	0.07	-0.99	0.33
Electric products	0.02	0.00	6.53	0.00	0.02	0.04	-1.59	0.11
Radio and television	0.00	0.00	3.13	0.00	0.00	0.00	0.87	0.39
Automotive and transportation	0.02	0.00	3.83	0.00	0.02	0.01	0.32	0.75
Furniture	0.01	0.01	-0.13	0.90	0.01	0.00	1.23	0.22
Electricity, gas and water	0.01	0.00	2.22	0.03	0.01	0.01	-0.28	0.78
Construction	0.11	0.04	6.00	0.00	0.11	0.13	-0.70	0.48
Car sales and car repair	0.02	0.04	-1.6	0.11	0.02	0.02	0.10	0.92
Wholesale	0.07	0.06	0.76	0.45	0.07	0.05	0.87	0.38
Retail	0.02	0.17	-7.19	0.00	0.02	0.01	0.59	0.55
Hotel and restaurants	0.00	0.04	-3.57	0.00	0.00	0.01	-0.83	0.41
Automotive and train transportation	0.03	0.07	-2.79	0.01	0.03	0.03	-0.34	0.74
Sea and river transportation	0.01	0.00	4.72	0.00	0.01	0.01	-0.77	0.44
Load and storage	0.01	0.01	-0.76	0.45	0.01	0.01	0.14	0.89
Mail and telecommunications	0.02	0.01	2.70	0.01	0.02	0.02	0.20	0.84
Financial intermediation	0.01	0.00	2.81	0.01	0.01	0.01	0.48	0.63
Insurance	0.01	0.00	4.32	0.00	0.01	0.02	-0.75	0.45
Real estate services	0.00	0.10	-5.8	0.00	0.00	0.00	-0.20	0.84
Transportation equipment rental	0.00	0.00	0.27	0.79	0.00	0.00	-0.20	0.84
Computer services	0.10	0.01	21.48	0.00	0.10	0.10	-0.00	1.00
Research and development	0.00	0.00	1.94	0.05	0.00	0.00	0.87	0.39
Law and accounting services	0.09	0.09	-0.19	0.85	0.09	0.08	0.51	0.61
Temporal employment agencies	0.01	0.00	12.33	0.00	0.01	0.00	1.23	0.22
Education	0.02	0.02	1.17	0.24	0.02	0.03	-0.28	0.78
Social services	0.02	0.04	-2.35	0.02	0.02	0.01	0.32	0.75
Waste disposal	0.00	0.00	1.20	0.23	0.00	0.01	-1.29	0.20
Enterprises services	0.01	0.04	-2.53	0.01	0.01	0.00	1.51	0.13
Cinema, radio and television	0.01	0.02	-0.9	0.37	0.01	0.01	0.14	0.89

Appendix II. Average effect of the program – Matched Samples

Nearest Neighbors		**Direct effects**				**Spillover effects**			
		lemp	lw	dexp	lexp	lemp	lw	dexp	lexp
	FONTAR	0.302***	0.00577	0.0422***	0.710***	0.317***	-0.0314*	0.0160	0.274*
256		[0.0368]	[0.0181]	[0.0140]	[0.167]	[0.0403]	[0.0162]	[0.0131]	[0.162]
	Number of observations	202,088	202,088	202,088	202,088	240,852	240,852	240,852	240,852
	Number of firms	18,893	18,893	18,893	18,893	26,266	26,266	26,266	26,266
	FONTAR	0.294***	0.0112	0.0465***	0.735***	0.312***	-0.0281*	0.0188	0.299*
128		[0.0368]	[0.0182]	[0.0141]	[0.167]	[0.0404]	[0.0162]	[0.0131]	[0.162]
	Number of observations	137,094	137,094	137,094	137,094	155,194	155,194	155,194	155,194
	Number of firms	12,736	12,736	12,736	12,736	16,861	16,861	16,861	16,861
	FONTAR	0.286***	0.0135	0.0530***	0.781***	0.308***	-0.0238	0.0239*	0.352**
64		[0.0372]	[0.0183]	[0.0142]	[0.168]	[0.0405]	[0.0162]	[0.0131]	[0.161]
	Number of observations	91,738	91,738	91,738	91,738	95,245	95,245	95,245	95,245
	Number of firms	8,465	8,465	8,465	8,465	10303	10,303	10,303	10,303
	FONTAR	0.279***	0.0138	0.0555***	0.799***	0.303***	-0.0182	0.0261**	0.387**
32		[0.0376]	[0.0183]	[0.0142]	[0.169]	[0.0411]	[0.0163]	[0.0131]	[0.162]
	Number of observations	60,137	60,137	60,137	60,137	57,550	57,550	57,550	57,550
	Number of firms	5,526	5,526	5,526	5,526	6,217	6,217	6,217	6,217
	FONTAR	0.280***	0.0155	0.0582***	0.831***	0.304***	-0.0183	0.0303**	0.426***
16		[0.0380]	[0.0186]	[0.0143]	[0.170]	[0.0415]	[0.0165]	[0.0133]	[0.163]
	Number of observations	37,382	37,382	37,382	37,382	34,198	34,198	34,198	34,198
	Number of firms	3,430	3,430	3,430	3,430	3,698	3,698	3,698	3,698
	FONTAR	0.287***	0.0158	0.0592***	0.844***	0.314***	-0.0135	0.0326**	0.467***
8		[0.0390]	[0.0185]	[0.0145]	[0.172]	[0.0424]	[0.0164]	[0.0136]	[0.166]
	Number of observations	22,632	22,632	22,632	22,632	19,928	19,928	19,928	19,928
	Number of firms	2,074	2,074	2,074	2,074	2,163	2,163	2,163	2,163
	FONTAR	0.262***	0.0280	0.0562***	0.817***	0.316***	-0.0100	0.0322**	0.476***
4		[0.0398]	[0.0186]	[0.0152]	[0.179]	[0.0444]	[0.0168]	[0.0142]	[0.172]
	Number of observations	14,222	14,222	14,222	14,222	12,098	12,098	12,098	12,098
	Number of firms	1,314	1,314	1,314	1,314	1,314	1,314	1,314	1,314
	FONTAR	0.266***	0.0307	0.0590***	0.842***	0.330***	-0.00839	0.0392***	0.543***
3		[0.0411]	[0.0190]	[0.0156]	[0.185]	[0.0459]	[0.0170]	[0.0146]	[0.177]
	Number of observations	11,859	11,859	11,859	11,859	9,988	9,988	9,988	9,988
	Number of firms	1,100	1,100	1,100	1,100	1,093	1,093	1,093	1,093
	FONTAR	0.267***	0.0230	0.0566***	0.798***	0.317***	-0.00181	0.0447***	0.612***
2		[0.0443]	[0.0198]	[0.0166]	[0.197]	[0.0476]	[0.0176]	[0.0153]	[0.185]
	Number of observations	9,434	9,434	9,434	9,434	7,860	7,860	7,860	7,860
	Number of firms	873	873	873	873	865	865	865	865
	FONTAR	0.304***	0.0323	0.0387**	0.579***	0.302***	0.00548	0.0297*	0.455**
1		[0.0508]	[0.0218]	[0.0186]	[0.223]	[0.0497]	[0.0196]	[0.0155]	[0.186]
	Number of observations	6,811	6,811	6,811	6,811	5,653	5,653	5,653	5,653
	Number of firms	636	636	636	636	627	627	627	627

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