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Assessing the Impact of Cluster Policies: the Case of the Arranjos

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Assessing the Impact of Cluster Policies: the case of the *Arranjos Productivos Locais* in Brazil

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Abstract: This paper assesses the impact of a cluster development policy in Latin America. A Local Productive Arrangement (APL) is a cluster of firms within the same territory, operating around the same activity and maintaining ties of cooperation and learning among themselves and with other stakeholders. Using firm-level data comprising information on SMEs from Brazil for the years 2002 to 2009 we apply fixed effects, matching and reweighting methods to estimate both the direct and the indirect – i.e. spillovers – causal effect of participating in APLs on a series of SMEs' performance indicators, including level of employment, value of exports and likelihood of exporting. The cluster policy is found to generate a positive direct impact on the three outcomes of interest. Moreover, we find some evidence of positive spillovers on both export outcomes, which become more relevant in the medium and long term. The results reinforce the importance of correctly accounting for the timing and considering gestation periods when assessing the impact of clusters policies to allow both the direct and indirect effects of such policies to materialize.

Keywords: Cluster Policies; Brazil; Impact Evaluation; Panel Data; Fixed-effects method.

JEL Classification: C23, H43, L25, O12, O54, R10

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I. INTRODUCTION

The role of industrial clusters in the economic development of countries and regions has drawn the attention of both academics and policymakers. The idea of promoting the formation and development of clusters is based on the assumption that firm-level performance benefits from agglomeration. These so-called agglomeration economies were first introduced by Marshall (1920) and later rediscovered by Arrow and Romer. Agglomeration economies originate from a set of positive externalities that are simultaneously industry specific and location specific. These externalities are mainly due to knowledge or technology spillovers, input/output sharing and labor-market pooling. Because of Marshall's seminal work, this phenomenon is often referred to as *Marshallian externalities*.[1]

Porter (1998, 2000) has recently influenced the discussion of the relevance of industrial clusters arguing that while changes in technology and competition have diminished the relevance of location decisions, clusters are a striking feature of today's economies.[2] Indeed, as Krugman (1991) and, Ellison and Glaeser (1997) note, industries tend to be geographically concentrated. In this context, the main reason for interest towards industrial agglomeration, as originally pointed out by Rosenstein-Rodan (1943), is that firms may benefit from the proximity to each other due to the existence of the aforementioned externalities.

Several studies provide evidence in this direction. The empirical literature on agglomeration economies began in the 1970's with the contributions of Shefer (1973) and Sveikauskas (1975). However, the work by Ciccone and Hall (1996) was the first to clearly address endogeneity issues – i.e. reverse causality: firms benefit from agglomeration because of externalities and, at the same time, the best firms decide to locate close to other firms. Using data on gross state output in the United States, they find that a doubling of employment density increases average labor productivity by around 6 percent. Other papers presenting evidence of agglomeration economies include Ellison and Glaeser (1999), Hanson (2001), Dumais et al. (2002), Rosenthal and Strange (2003), Rodríguez-Clare (2005, 2007), Combes et al. (2008, 2010), Ellison et al. (2010) and Rizov et al. (2012).

This evidence on the benefits of agglomeration notwithstanding, it is also a well-known result in economic theory that in the presence of externalities the market often fails to assign resources optimally. This is particularly relevant when geographical proximity and industry complementarities cause agglomeration economies. In this context, cluster policies may foster the beneficial effects of agglomeration by creating a set of incentives to overcome the coordination failures that hamper the development of certain industries. A first stage of this type

[1] In more generic terms, the literature has also referred to the concept of industry-specific local externalities (ISLE). Henderson et al. (1995) refer to these types of industry-specific externalities that arise from regional agglomeration as "localization externalities", in particular when firms operate in related sectors and are closely located.

[2] The simplest definition of an industry cluster is derived from the work of Porter (1990), who defines clusters as "a geographic concentration of competing and cooperating companies, suppliers, service providers, and associated institutions".

of interventions is usually a process whereby linkages between the different cluster stakeholders (firms, support institutions, and public agencies) and externalities are built up through support to latent and potential clusters. This step includes the preparation and adoption of cluster development plans to map and facilitate cluster formation and encourage the interaction among private agents (private-private and public-private). In a second stage, consolidated clusters are usually supported by public investments to promote innovation and joint learning, and overcome technological, infrastructure and environmental constraints – e.g. investments in knowledge creation and technology adoption which are two of the major sources of industry and location specific externalities as well as joint-investments in public goods.

Several countries have implemented cluster-development programs. Unfortunately, few evaluations have been carried out to assess the impact of this type of public policies and, the evidence remains scarce and inconclusive.[3] Falck et al. (2009) evaluate a cluster program introduced in 1999 in Bavaria, Germany, aimed at fostering innovation and regional competitiveness by stimulating cooperation. They find that the cluster-oriented policy increased the likelihood of becoming an innovator in the targeted industry. Nishimura and Okamuro (2011) evaluate the Industrial Cluster Project (ICP)[4] in Japan. Their results suggest that beneficiaries expand the industry-university-government network. Interestingly, they find that while clusters participants that collaborate with national universities in the same region significantly improve the R&D productivity – without reducing patent quality – participation in the cluster project alone has no significant effect on the R&D productivity of firms. Finally, Martin et al. (2011a) analyze a public policy promoting industrial clusters in France for the period 1996-2004, and apply a difference-in-differences approach. They find that the program selected firms in relative decline and find no major effect on productivity. They also conclude the policy had no robust effects on employment and exports.

To our knowledge, there are no impact evaluations of cluster policies in Latin America. Although similar types of programs, such as agricultural (Cerdán-Infantes et al. 2008; López and Maffioli, 2008; González et al., 2009), innovation (Binelli and Maffioli, 2007; Hall and Maffioli, 2008; Crespi et al., 2011; Benavente et al., 2012), export promotion (Volpe and Carballo, 2008), SME support (Castillo et al., 2010; López Acevedo and Tan, 2010) and supplier development (Arráiz et al., 2011) have been analyzed,[5] none of these directly evaluate a cluster policy. The study by Maffioli (2005) is probably the closest piece of research to an impact evaluation of a

[3] As Anderson et al. (2004) point out, thorough evaluations of specific cluster initiatives and cluster actions are in fact few and have been developed only in few countries. Few solid attempts have been made to assess whether first-best results are obtained, go beyond efficiency in use of given resources to encompass economic results, or take into account interactions and synergies in the performance of different actors. Further, most evaluations of cluster policies pursued still focus on single tools, which fits poorly with the systemic notion of cluster policy.

[4] The ICP was initiated by the Japanese Ministry of Economy, Trade and Industry in 2001 and aims at developing regional industries and includes both direct R&D support and indirect networking/coordination support.

[5] Several of these evaluations were done by the Inter-American Development Bank.

cluster policy in this region. He presents a theoretical discussion of industrial networks and empirically analyzes the most important Chilean networking program, the PROFO program.[6]

In this paper, we aim at closing this knowledge gap and shedding light on the effectiveness of cluster policies by focusing on the case of Brazil's *Arranjos Produtivos Locais* (APL), a cluster development policy in place since 2004. More precisely, we analyze the impact of participating in APLs on a series of performance indicators for SMEs, including level of employment, value of exports and likelihood of exporting.[7]

We present evidence of a positive direct average effect of the APL program on the three outcomes of interest – level of employment, value of exports and the likelihood of exporting – we considered. Furthermore, those positive effects seem to be increasing over time. We also find evidence of positive spillovers on both export outcomes. These effects seem to become more pronounced in the medium and long term. Results reinforce the idea that finding direct and indirect effects of a cluster policy on firms' performance requires considering the fact that the realization of such impacts may require a period of gestation after the policy is implemented.

Our contributions to the empirical literature are twofold. First, we add to the general impact evaluation literature not only by studying the direct impact of a large cluster program, but also by testing for potential indirect (i.e. spillover) effects, and carefully considering the timing of the realization of impacts. Second, we provide what to our knowledge is the first evaluation of a specific cluster policy in Latin America, expanding the current literature which to this point has almost exclusively focused on developed countries.

The rest of the paper is organized as follows. Section II presents the APL program. Section III describes the relationships of interest, the ideal experiment, the estimation methods and the robustness checks used to assess the impact of the cluster policy. Section IV presents and summarizes the data used for the estimations and performs a preliminary analysis. Section V reports the estimation results and section VI concludes.

[6] The availability of relational data on a significant number of firm networks allows him to investigate in detail the relationship between network structure, public intervention and firm competitiveness. His econometric analysis confirms a strong correlation between PROFO firms' innovativeness and industrial cooperation, proving the existence of an interactive learning process among participant firms. Using sociometric data to refine the analysis of the impact of the program on the network multiplier he finds that participant firms increase their productivity and that this improvement is strongly correlated with firm centrality and network density, which are the two variables best representing the structure and function of the network multiplier and that are affected by PROFO.

[7] Given the confidentiality of the data, the estimations were conducted following the Instituto de Pesquisa Econômica Aplicada's microdata policy, which implies working in situ under the supervision of its staff and with blinded access to sensible information.

II. BRAZIL'S *ARRANJOS PRODUTIVOS LOCAIS*

In Brazil, during the last decade, the public and private organizations that promote small and medium-sized firms (SMEs)[8] have increasingly focused on the development of local productive clusters, known as *Arranjos Produtivos Locais* or APLs. According to the definition given by the Brazilian Service to Support Micro and Small Enterprises (SEBRAE), APLs – "local productive arrangements" or "local systems" – are clusters of firms within the same administrative area (e.g. municipality) that share a particular specialization. Firms within each cluster maintain ties of cooperation and learning both among themselves and with other stakeholders such as government, business associations, lenders, and teaching and research institutions. An APL is characterized by the existence of a group of firms operating in the same economic activity. For instance, manufacturing firms producing goods and services, suppliers of machinery and equipment, input providers, associations and cooperatives and human resource training firms.[9]

The importance of APLs in Brazil's industrial policy is illustrated by the fact that cluster support is recognized as one of the key pillars of Brazil's Industrial, Technological, and Foreign Trade Policy (PITCE), and of the Brazilian Industrial Development Agency (ABDI).[10] In the last decade most of the states who started implementing policies to develop and support clusters have been mainly guided by the federal government through the APL Permanent Working Group (GTP-APL) created in 2004 in the Ministry of Development, Industry and Foreign Trade (MDIC). This agency promotes coordination among the various federal and state agencies working with APLs.

The purpose of this integrated APL policy has been to stimulate local development through competitiveness and sustainability projects in territories where has been some kind of pre-existing agglomeration of SMEs. More specifically, the selection criteria for APLs in the states where we focus our evaluation (Sao Paulo and Minas Gerais) can be summarized in the following four points: (i) capability and possibilities of operating and collaborating with other organizations; (ii) form and degree of development of the APL – selection is guided by the number and maturity of participating institutions, the existence of a local governmental institution capable of coordinating collective actions, and the quality of linkages between firms

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[8] The Brazilian Institute of Geography and Statistics (IBGE) defines microenterprises and small businesses as firms employing up to 49 workers in the services sector and up to 99 in the industrial sector. For the National Bank for Economic and Social Development (BNDES) a microenterprise is a firm with sales of less than R$1.2 million, while a "small" firm has sales of between R$1.2 million and R$10.5 million, and a "medium-sized" one between R$10.5 million and R$60 million.

[9] See Lastres et al. (2002, 2003).

[10] This agency's main objectives are to coordinate and synchronize efforts made by various government institutions and levels to achieve consistency in productive-sector support policies, and to improve the country's technological base in areas displaying the greatest growth potential, including: (i) strengthening the industrial structure of industrial property; (ii) promoting innovative SME capacity; (iii) creating a favorable investment environment; and (iv) increasing research and development expenditure, in both the public and private sectors (see http://www.abdi.com.br/).

and other actors; (iii) socio-economic relevance of the main activity of the APL e.g. impact on GDP, exports and level of employment, and (iv) capability of generating new opportunities for social and economic development and innovation.[11]

Policy actions were designed following a set of guidelines that seek to maximize the impact on local development, increase social capital, and integrate participating agents.[12] Under this framework, participating ministries as well as governmental and non-governmental agencies designed and implemented several instruments to support APL development. More precisely, policy actions consist of two main components. First, private and public agents jointly elaborate strategic development plans. In this stage the main role of public agents is to facilitate interaction between the various agents involved and designate local leaders to be responsible for coordinating each plan's execution. The focus is on the development of methodologies for the organization and consolidation of the APLs. Secondly, – once each plan is completed – public agents support recently consolidated APLs through different instruments aimed at increasing competitiveness of the productive chains. This stage includes direct investment in infrastructure, equipment, specific training and technology transfer programs, implementation of sectorial technology centers, design offices, export promotion programs, and information systems for monitoring and evaluation.

The overall goals of this integrated policy are: (i) to promote economic development; (ii) to reduce social and regional inequalities; (iii) to foster technological innovation; (iv) to expand and modernize the productive base; (v) to foster employment and income; (vi) to reduce the failure rate of SMEs; (vii) to improve education and training; and (viii) to increase productivity, competitiveness and exports. This paper will be mainly focusing on the effectiveness of such policy in accomplishing goals (v) and (viii). The remaining goals, although key for policy formulation, will be addressed in future research. Moreover, due to problem of data availability we are not able to identify each instrument separately. Then, we evaluate the APL policy as a unique program compounding the different instruments just described.

III. ASSESSING THE IMPACT OF A CLUSTER POLICY

(a) The Causal Relationships of Interest

To measure the impact of a cluster policy we need to identify a *causal effect*. The causal effect of a policy is the difference between the value of the outcome variable after the policy took place and the value that the outcome variable would have been in absence of the policy. Suppose the

[11] This selection criteria is defined in *Politicas Estaduais para Arranjos Productivos Locais no Sul, Sudeste e Centro-Oeste do Brasil*, 2010 (www.bndes.gov.br).

[12] This is how the objectives are defined in the *Termo de Referencia para Politica Nacional de Apoio ao Desenvolvimiento de Arranjos Productivos Locais* (2004).

cluster policy variable takes the value one when firm *i* belongs to APL and zero otherwise – i.e. $C_i = \{0,1\}$. Suppose in addition that if firm *i* participates in APL – i.e. $C_i=1$ – its value of the outcome variable is Y_{i1} and if it does not participate – i.e. $Ci=0$ – its value of the outcome variable is Y_{i0}. In this setting, Y_{i1} and Y_{i0} are our *potential outcomes*.

Then, the observed outcome variable of firm *i* can be written as:

$$Y_i = \begin{cases} Y_{i0} & \text{if } C_i = 0 \\ Y_{i1} & \text{if } C_i = 1 \end{cases}$$

or equivalently,

$$Y_i = Y_{i0} + (Y_{i1}\text{-}Y_{i0}).C_i.$$

The difference between the potential outcomes $Y_{i1} – Y_{i0}$ is the *causal effect* of the cluster policy *C* on the outcome variable *Y* for firm *i*. The main problem in measuring this effect arises because the counterfactual outcome (Y_{i0} for treated firms, Y_{i1} for untreated firms) is never observed, and therefore we must estimate it.[13] Furthermore, because it is not possible to estimate the effect of *C* on *Y* for each individual *I* – except under very stringent and unrealistic assumptions – we often focus on estimating average treatment effects. For this, we need to identify a valid control group, in our case, a group of firms with the same characteristics than the group of beneficiaries of the cluster policy, differing from the formers only in that firms in the control group do not benefit from the policy.[14]

Before discussing how to ideally construct both the treated (beneficiaries) and control (non-beneficiaries) groups we also need to consider another key challenge in the evaluation of cluster policies: how to deal with externalities and spillovers. The first step in addressing this issue is to note that there are two types of beneficiaries (direct and indirect) and therefore two causal relationships of interest.

Direct beneficiaries are firms in a cluster that participate in the cluster policy program – i.e. they choose to actively participate in the cluster development plan. Indirect beneficiaries are those firms that do not participate in the program but have linkages with participants. For instance, firms that share the geographical location with participating firms may indirectly benefit from higher foreign direct investment in the region attracted by cluster firms (De Propris and Driffield,

[13] This is the Fundamental Problem of Causal Inference (see Holland, 1986).
[14] Basically, if assignment to treatment is randomized, the inference problem is straightforward because the treatment and control groups are from the same population. We can then estimate the Average Treatment Effect (ATE). With observational data the two groups are not drawn from the same population. In this case, we often want to estimate the Average Treatment Effect for the Treated (ATT).

2006).[15] We will base our definition of indirect beneficiaries on this geographical criterion; i.e., we assume that geographical proximity is the channel through which spillovers occur. Thus, an indirect beneficiary is a firm that does not participate in the plan but is located in a municipality where the APL policy was implemented. We will refer to the municipalities with a positive number of APL firms as "treated municipalities". Finally, non-beneficiaries are firms located in municipalities not treated by the program. However, some non-beneficiaries may be contiguous with a treated municipality and therefore benefit from spillovers. If this is the case then our estimates would be a lower bound of the impact of the program since these firms are also included in our control group.

(b) The Ideal Experiment

Like in other type of policies, the ideal experiment that would answer the causal effect questions is a randomized assignment of the cluster policy.[16] However, in this case, the randomization has to be done at two different levels – i.e. double randomization.[17] The need for this double randomization responds to the fact that both the locations where the policy will be implemented and the firms that will participate directly have to be selected. Then, we have three groups:

1. Direct Beneficiaries: APL firms

2. Indirect Beneficiaries: non-APL firms in a treated municipality

3. Non-beneficiaries (control or comparison group): non-APL firms in a non-treated municipality

Once the policy has been randomly assigned and in order to identify its effect, we are able to do two set of comparisons. The first comparison is between direct beneficiaries (APL firms) and similar non-beneficiaries. This comparison would provide the direct impact of the policy:

$$\mathbf{DATE = E[Y_i|D_i{=}1,C_i{=}1] - E[Y_i|D_i{=}0,C_i{=}0]}$$

where D_i takes value 1 if the municipality is a treated municipality and 0 otherwise and *DATE* is the Direct Average Treatment Effect i.e. it is the difference between the average value of the outcome variable for direct beneficiaries and non-beneficiaries.

The second comparison involves comparing indirect beneficiaries with similar non-beneficiaries. This comparison identifies the Indirect Average Treatment Effect i.e. spillover effects:

[15] Bronzini and Piselli (2009) consider geographical spillovers assuming that factors enhancing productivity in one region can also affect the productivity in the neighboring regions. Bottazzi and Peri (2003) use geographical proximity as a channel for R&D spillovers.
[16] With a large enough number of observations, the randomized assignment of the policy ensures that beneficiaries and non-beneficiaries have statistically equivalent averages not only for their observed characteristics but also for their unobserved characteristics before the policy is applied.
[17] See Angelucci and Di Maro (2010).

$$\mathbf{IATE = E[Y_i|D_i=1,C_i=0] – E[Y_i|D_i=0,C_i=0]}$$

In principle, we would like to randomly assign the treated municipalities and the cluster policy as described above (double randomization). However, in most cases cluster policies are not designed using randomized assignment. Hence, in the absence of a randomized control trial we use quasi-experimental methods that mimic an experiment under certain assumptions and therefore, we will focus on estimating the average effect of the treatment on the treated firms (ATT).

(c) Identification Strategy and Estimation Methods

The main idea to identify the impact of a cluster policy without random assignment of the policy is the same as in the case of random assignment – i.e. to compare direct and indirect beneficiaries with non-beneficiaries. However, in absence of a randomization, beneficiaries may be different from non-beneficiaries because of selection bias. In the case of cluster policies, as in other productive development policies, it is likely that beneficiaries are more or less productive than non-beneficiaries. Therefore, beneficiaries may show different outcomes than non-beneficiaries even in absence of the cluster policy. In terms of the notation described earlier, it is possible to expect both a positive – i.e. $E[Y_{i0}|D_i=1,C_i=1]$ or even $E[Y_{i0}|D_i=1,C_i=0]$ larger than $E[Y_{i0}|D_i=0, C_i=0]$ or negative – e.g. if the policy selected firms in sectors and regions in relative decline – *selection bias*. Then, the comparison of the average of the outcome variable between beneficiaries and non-beneficiaries will over or underestimate the effect of the policy.

As suggested before, the selection problem occurs because beneficiaries (direct and indirect) are different than non-beneficiaries even before the policy is implemented. Several techniques can be used to avoid these potential problems. First, if participation is determined by observable factors, these variables can be included as control variables in a regression framework. However, some of these relevant factors may be unobservable – e.g. entrepreneurial behavior of the firm, manager characteristics and leadership – and thus cannot be accounted for. Nevertheless, the panel structure of our database allows us to eliminate all confounding factors (both observable and unobservable), as long as they do not vary over time, using a fixed-effects model. Under this assumption we can identify the effect of the cluster policy on our outcomes of interest using the following fixed effect linear regression model:[18]

$$Y_{i,t} = \alpha_i + \mu_t + \beta . C_{i,t} + \gamma . X_{i,t} + \varepsilon_{i,t} \qquad (1)$$

where $Y_{i,t}$ is any of the outcomes under study of the firm *i* in year *t*, α_i captures all time-constant factors that are firm-specific, μ_t represents yearly shocks that affect all firms, β is the parameter of interest which captures the causal effect of $C_{i,t}$ (a binary variable that takes the value one

[18] See Bertrand et al. (2004) for a discussion on Differences-in-Differences estimates.

since the year in which the firm *i* enters the APL)[19] on the outcome under consideration, $X_{i,t}$ is a vector of time-varying control variables, and $\varepsilon_{i,t}$ is an error term which may be correlated within each cluster but is assumed to be independently and identically distributed across clusters. In the absence of time-varying unobserved factors that affect both the outcome and the participation in APL, the fixed-effects method leads to a consistent estimator for β, the average impact of the cluster policy on the treated firms.

The validity of the fixed-effects estimator rests on the identification assumption that trends in the outcomes would have been equal in absence of treatment. However, this non-testable assumption may be difficult to accept when firms in the control group are very heterogeneous and very different from the participating firms, since firms that are very different at the baseline are likely to follow different trends as well.

In order to reinforce the validity of our identification assumption, we run equation (1) on a matched sample, selecting among 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 use Nearest Neighbor Matching with one neighbor and replacement applying it to the year before the beginning of the cluster policy i.e. 2003. For each type of beneficiary we match on each outcome separately using its values for the period 2002-2003 and observed characteristics of the firm in 2003 i.e. region, industry, size, age and a Herfindahl index.[20] We do this to ensure that we select from the control group only those which have pre-treatment trends that are similar to direct and indirect beneficiaries.[21]

As mentioned above, the equality in the growth rate of the outcome variables in absence of the program is a non-testable assumption and the combination of these methods can make it more credible. It involves three steps: (i) estimating the propensity score before the treatment takes place, (ii) defining a matched sample of firms – we are left with the treated firms and their nearest neighbors –, and (iii) running a fixed effects model on this matched sample. Heinrich et al. (2010) provide guidelines for the application of this method. For a recent application of this procedure to evaluate a SME policy in Argentina see Castillo et al. (2010).

We reinforce our estimates by using entropy balancing, a multivariate reweighting method proposed by Hainmueller (2012). This method allows us to reweight our full sample such that the control group matches the covariate moments in the treatment group.[22] In this paper, we focus on

[19] In the case of indirect beneficiaries $C_{i,t}$ takes the value one since the year in which the municipality is a treated municipality.
[20] The Herfindahl index was created by sector-municipality-year using level of employment. For a full discussion on measures of concentration see Hay and Morris (1987).
[21] It is worth mentioning that in both matching and reweighting contexts we are left only with firms whose outcome was observed in both pre-treatment years i.e. 2002-2003.
[22] We use the *Stata* package called *ebalance* introduced by Hainmueller and Xu (2011). In order to create the weights we apply the entropy balancing to the year 2003 by type of beneficiary for each outcome separately using the value of the outcome in 2002 and 2003 and all the observed characteristics of the firm (region, industry, size, age

balancing the first moment (mean) of the covariates and pre-treatment values of the outcome. Both the matching and reweighting methods allow us to eliminate a potential source of bias because non-beneficiaries are now more similar to beneficiaries.[23]

In general, it takes time for the effects of cluster policy to manifest themselves in firm economic performance. The realization of the impact may require a period of gestation after the policy takes place. As a result, a proper consideration of the timing of the effects is crucial in an impact evaluation of a cluster program, and failures to account for this issue may lead to misleading conclusions and policy recommendations. To this end, we estimate the dynamic effects of the APL policy for the full, matched and reweighted sample. In terms of the previous notation:

$$Y_{i,t} = \alpha_i + \mu_t + \beta_1 . C_{1i,t} + \cdots + \beta_k . C_{ki,t} + \gamma . X_{i,t} + \varepsilon_{i,t} \qquad (2)$$

where $C_{ki,t}$ takes value one if the firm received the program *k* years ago and 0 otherwise.

(d) Robustness Checks

To further address the validity of the control group and therefore the robustness of our results we run a pre-treatment trends equality test which assesses whether the pre-intervention time trends for beneficiaries and non-beneficiaries are different.[24] We use only the observations of beneficiaries and non-beneficiaries in the pre-treatment period i.e. 2002-2003 and run the following regression:

$$Y_{i,t} = \alpha_i + \mu_t + \beta . Cs_i\_2003 + \gamma . X_{it} + \varepsilon_{it} \qquad (3)$$

where Cs_i_year is the interaction between the treatment status Cs_i and the respective pre-treatment year. This test allows us to validate our fixed-effects identification strategy.[25]

IV. THE DATA

Using the *Cadastro Nacional Pessoa Juridica* (CNPJ)[26] from firms participating in APLs and registered in SEBRAE, it was possible to match the information from the *Relacao Anual de*

and a Herfindahl index) for the direct impact while only these values of the outcome for the indirect impact in order the algorithm to converge.

[23] Heckman et al. (1997) and Heckman et al. (1998) point out this source of bias.

[24] See Galiani et al. (2005).

[25] See Heckman and Hotz (1989).

[26] The CNPJ is a unique number that identifies each firm in Brazil.

Informacoes Sociais (RAIS)[27] and *Secretaria de Comércio Exterior* (SECEX) of Brazil. Using the database of firms registered at SEBRAE, the spatial distribution of firms in Minas Gerais and Sao Paulo for which data are available is pictured in Figures 1 and 2.

Table 1 displays the composition of our sample over time. We do not consider firms with observations only before or only after their starting year of treatment (direct or indirect) in order not to contaminate our control and treated group respectively. After excluding large firms, firms with only one observation and sectors with too few firms participating in APLs, we are left with a sample of 580,083 observations from 110,601 SMEs for the period 2002-2009.[28] Table 2 presents the distribution of firms by starting year in APLs or treated municipality. The bulk of the cluster project started in 2004, and the number of firms participating in APLs before that year is negligible. Moreover, Table 3 and 4 show that the APL policy targeted firms mainly from the Leather industry and small firms.

The outcomes of interest are level of *employment*, the *log of exports* and a dummy (*dexport*) that takes the value 1 if the firm exports and value 0 otherwise.[29] Additional control variables include the log of firm's age and a Herfindahl index, which is a measure of concentration.

Table 5 and Figures 3-5 depict the evolution of the outcomes over time, which allows us to perform a preliminary analysis by comparing the performances of our three groups of interest. A salient feature highlighted by the figures is that, in all years under study, the treated group has better outcomes than the other two, and the indirect beneficiaries also perform better than the control group. For instance, in 2002, firms that will participate in the program later have on average 16 more workers than the indirect beneficiaries, which in turn have on average two more workers than the comparison group. A similar phenomenon is observed for the probability of exporting; in this case, in 2002 the cluster firms were three percentage points more likely to export than the indirect beneficiaries and five percentage points more likely than the control firms. This provides evidence of a selection bias where firms that participate in the program show a better performance even before treatment than the firms that do not participate, and thus would be expected to have higher outcomes in absence of the program.

[27] The RAIS is an annual survey including socio-economic information of firms in Brazil. It is an administrative record of the labor force profile which is mandatory in Brazil for all firms in all sectors.

[28] The following sectors: agriculture, livestock and related products, extraction of metallic minerals, motor vehicles, accommodation and food, ground transportation, financial intermediation, computers, public administration, defense and social security, education and associative activities presented only one observation in the 2007 RAIS and were excluded from the tables because of confidentiality issues. We also eliminate paper products, metal products, medical instruments and chemical products industries because they have a negligible number of APL firms.

[29] We replicate estimates using the log of employment as dependent variable and we find similar results to those presented in the paper in terms of the magnitude, sign and significance of the impact of the program on level of employment. For the sake of brevity we only present the results for level of employment for which the interpretation is more straightforward. For the outcome *log of exports* we assign the value of 0 when firms have 0 exports to avoid excluding non-exporting firms from our sample, which could bias the results by affecting the composition of the treatment and control groups (see Angrist and Pischke, 2008).

By looking at the changes in outcomes over time, we can find some evidence in favor of positive direct effects of the cluster policy. Employment of treated firms not only starts from a higher level but also increases more rapidly than for untreated firms, which after a slight decrease, nearly returns to its initial level from 2002. Indirect beneficiaries and control firms appear to move in tandem, which would suggest, at most, very modest indirect effects. As to the logarithm of exports, we also observe a much higher level for treated firms. The outcome for participating firms reveals a sharp increase around 2004 which stabilizes in an intermediate level after 2006, while non-participating firms show an almost constant level over the period under study, or, if anything, a slight rise or drop for indirect and non-beneficiaries respectively . Figure 5 suggests that most of the change in exports is driven by the probability of becoming an exporter. Importantly, these figures point to a time-varying impact of the cluster policy on export behavior of firms.

Some back-of-the-envelope calculations may give us a sense of the magnitudes of these effects. We can collapse the data in table 5 by averaging the periods before (2002-2003) and after (2004-2009) the treatment and compare the before-after changes for each group (see Tables 6-7). This simple diff-in-diff estimator yields positive direct effects on the three outcomes: we find an impact on employment of around 13 workers, a 64% increase in exports with a 5 percentage point increase in the probability of exporting. As to the indirect effects, these simple estimates are much more modest: a positive but small indirect effect of around 3 workers, a 11% increase in total exports and one percentage point increase in the probability of exporting.

In sum, our preliminary analysis points to positive direct effects with, at most, very modest indirect effects on our three outcomes. However, these naïve diff-in-diff estimators may be seriously affected by several types of biases. First, the figures reveal that the three groups present very different behaviors in the pre-treatment period; for instance, the log of exports show a sharp increase for treated firms between 2002 and 2003, while non-participating firms barely move. This dissimilarity between pre-treatment trends casts doubts on the validity of the diff-in-diff method, and is in fact a pervasive problem in the literature first described by Ashenfelter (1978). Furthermore, the figures suggest that by calculating an average effect over the whole period we may be masking some very complex dynamics and wasting relevant information on the interaction between the program and firm performance over time. With these initial estimates in mind, in the next section we will use the econometric methodology explained previously to account for these and other potential concerns such as differential trends by industry, and to carefully analyze the dynamic pattern of the effects.

V. ESTIMATION RESULTS

Employment

Using the full sample, our first set of results (Tables 8-9, Part A) show a positive and significant direct impact and a negative and significant indirect impact of the cluster program on employment;[30] more precisely, the program increases the employment level by around ten workers for direct beneficiaries. However, the negative effect on indirect beneficiaries is almost negligible: less than one worker. This is in line with the descriptive statistics presented in Table 5, which show that employment in APL firms grows on average faster than in non-beneficiaries while remains practically invariant in indirect beneficiaries.

A typical interpretation of the positive direct impact on employment is that agglomeration economies on the labor markets as well as knowledge and input's markets are direct externalities and are generally assumed to improve total factor productivity (TFP) and firm-level performance, and encourage the opening of new plants, the growing of existing plants or the attraction of other plants in the region.[31] APL policy through labor market externalities may foster the creation of pools of specialized workers allowing APL firms to demand more workers for different jobs but around the same activity and to propose specific trainings that could improve workers' efficiency. Moreover, knowing that they do not face ex post appropriation and that it is easier to recoup the cost of acquiring industry-specific human capital, workers join the cluster and invest in human capital (Rotemberg and Saloner, 2000). As Marshall stressed, workers learn skills quickly from each other in an industrial cluster. In this context, if APL workers have the risk of losing their jobs when there are shocks, they might be relocated more easily in other company inside the cluster moving to more productive firms.[32] On the other hands, workers in indirect beneficiaries may see the cluster as a more promising job alternative, with higher possibilities of professional growth. Then, the cluster might also absorb workers from firms outside but close to it. Indeed, the strong direct effect of the cluster policy on employment could be in part reflected by a slight relocation of workers from indirect beneficiaries to direct beneficiaries.

These estimates are robust to the inclusion of additional controls – log of firm's age and Herfindahl index – and industry-year interactions that allow different sectors to have different trends over time. Moreover, if we consider the dynamic effects (see Table 10, Part A) the direct effect appears to be increasing over time, starting from a magnitude of around six workers in the first year after treatment and up to around seventeen workers after six years of treatment.

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[30] It is worth mentioning that the fixed-effects method allows us to control for firm, municipality, state and industry non-observables that do not vary over time.

[31] See Rosenthal and Strange (2001) and Ellison et al. (2010). Li et al. (2012) find that industrial agglomeration has a positive and statistically significant causal impact on firm size for the case of manufacturing firms in China.

[32] Our results reinforce the idea that the benefits of labor pooling might prevail against the costs of labor poaching. This trade-off is addressed in Combes and Duranton (2006).

As mentioned above, a potential concern with this first set of results is that our database combines data from very heterogeneous firms, which could undermine the validity of our identification strategy. To account for this factor, we also run the previous regressions on a matched sample and on a reweighted sample as previously explained. The results are presented in Part B and C of Tables 8-9. Not only the magnitude of the estimated coefficients is similar, but also their statistical significance is maintained. These estimates are also robust to the inclusion of controls and industry-year interactions. Moreover, in Appendix I we show how the reweighting scheme allows making more similar the kernel density of the propensity score – estimated from a logit of the treatment variable on covariates – (Figure A) and balancing the covariates means (Figure B) between the direct beneficiaries and non-beneficiaries for the case of employment.

Finally, in Table 11 we assessed the validity of the "Equal trends assumption" comparing changes in employment for the treatment and comparison groups before the program is implemented. While the estimates of the direct impact based on the full sample clearly do not pass the pre-treatment trends equality test, after matching or reweighting trends in employment in the pre-treatment period are more similar (see Figures 6-7). This test supports the assumption that mean employment of the treatment and control groups would have continued to move in tandem in the post-intervention period in absence of the cluster program.

Exports and likelihood of exporting

Tables 12 and 13 show the results for the log of exports. We find large, positive and statistically significant average direct effects between 40% and 60% depending on the specification. If we correct the estimates to account for the fact that the treatment is a dummy variable, we get estimates ranging from around 50% to 80%.[33] Moreover, the dynamics of these effects for the full sample are consistent with what we described graphically in the previous section. The impact on exports is large and significant, but decreasing over time. However, as mentioned before, the control group does not seem to be a good comparison, since it exhibits a very different pre-treatment behavior than the treatment group. After correcting this issue, we get a different pattern of the effects. The reweighted sample reveals an increasing effect over time, although the increase in the impact is not monotonic.

As to the spillovers, table 13 reveals some modest but significant positive average indirect effects, which range from around 2% to 4%. According to table 14, these impacts seem to become relevant only after the second year, which is reasonable since spillover effects are expected to take time to materialize.[34] The estimates show a positive trend, reaching the value of around 10% in the sixth year.

[33] The correction is b*=(exp(b)-1)*100.
[34] See Jaffe et al. (1993).

Table 15 tests whether trends were similar before treatment. As suspected from the aforementioned figures, trends of APL firms and non-beneficiaries are statistically different for the full sample. Although our matching procedure reduces this difference, it does not manage to make it disappear. The reweighting procedure, on the other hand, successfully equalizes the trends, which suggests that the results based on this method would be the preferred ones (see Figure 8). With respect to indirect effects, we find no evidence of differential ex-ante trends (see Figure 9).

In sum, even if our data does not allow us to draw precise conclusions on the exact magnitude of the effects, all our results point to large but time-varying direct impacts and modest but time-increasing indirect effects on exports.

Two different factors could be driving these results. While the most straightforward one is that exporting firms could increase their level of exports because of the program, the cluster policy could also be an incentive for non-exporting firms to start exporting their products. In fact, our visual analysis in the previous section suggested that a higher proportion of exporting firms could be an important determinant of the change in average exports. To further explore this possibility, we analyze the effect of the program on the probability of exporting. The results can be seen in tables 16 and 17. Indeed, we find strongly significant increases in the likelihood of exporting of about 4 to 5 percentage points for direct beneficiaries. The ex-ante trends, which are different for both the full and matched samples, are equalized by reweighting procedure (see Table 19 and Figure 10). In this case, the effects are increasing over time, getting as high as seven percentage points (Table 18). As to indirect effects, the probability of exporting is increased for indirect beneficiaries by around one percent point, which is significant from a statistical point of view but modest from an economic perspective. Again, these effects become significant after the second year of the program and there is no evidence of differential ex-ante trends (Figure 11).

Thus, we find that the increase in exports is driven not only by the change in the level of exports but also by a higher proportion of exporting firms. These results are in line with the idea that APL firms are expected to have more advantages to compete in international markets and more likely to engage in export activities than non-beneficiaries that operate in isolated areas due to knowledge, skills, new technology and network creation through clustering. As mentioned before, these externalities may improve productivity and it has been pointed out in the literature (see Bernard et al., 2003, and Bernard and Jensen, 2004) that high productivity leads a firm to export. Even thought the results suggest that firms located outside the cluster but in the same municipality may also benefit from the cluster and its agglomeration externalities, they need more time to assimilate such benefits.

VI. FINAL REMARKS

In this paper, we present what is to our knowledge the first evaluation of the direct and spillover effects of a specific cluster policy in Latin America. Our first set of results on the Brazilian APL policy is positive and has interesting policy implications.

First, we show that firm-level performance benefits from the cluster policy. We find positive direct average effects of the program on employment, level of exports and likelihood of exporting with an increasing pattern over time. These findings compare favorably with the results obtained for the industrial clusters in France (Martin et al., 2011a) where the policy had no robust effect on employment or exports, and are in line with the intuition that small firms benefit more and generate more agglomeration economies as pointed out by Henderson (2003), Rosenthal and Strange (2003, 2010) and Martin et al. (2011b). Second, we also find the presence of positive spillovers through geographical proximity on both export outcomes which gain relevance in the medium and long-term. Third, our dynamic analysis reinforces the importance of correctly accounting for the timing and considering gestation periods when assessing the impact of clusters policies to allow both the direct and indirect effects of such policies to materialize.

This study is only the first step towards a better understanding of the impacts of cluster policies in developing countries. Further research is required to rationalize the impacts of clusters policies and to explore its mechanisms in depth. Many of the limitations of our study are related to data availability, and a lot of interesting insights on these issues could be obtained from a richer set of data. A more comprehensive approach would benefit from controlling for other characteristics of the firms, industries and regions.

In the same direction, future extensions of this study could focus on the heterogeneity of impact for different types of firms and industries, and also how different kind of industries can interact to generate different types of externalities. Moreover, there are several mechanisms that create spillovers and therefore indirect beneficiaries. For example, indirect beneficiaries could also be defined as non-treated firms that hired workers that were working in a treated firm i.e. labor mobility is the channel for spillovers.[35] Worker-level data would allow identifying indirect effects through this channel and for instance, to determine whether workers are moving inside the cluster from outside. Finally, it would be interesting to extend the analysis of spillover effects using geospatial data on firm location, which can help not only to use more precise definitions of clustering but also to explore how indirect effects vary with distance to the cluster. Only a significantly expanded set of information will allow these research extensions.

[35] See Maliranta et al. (2009).

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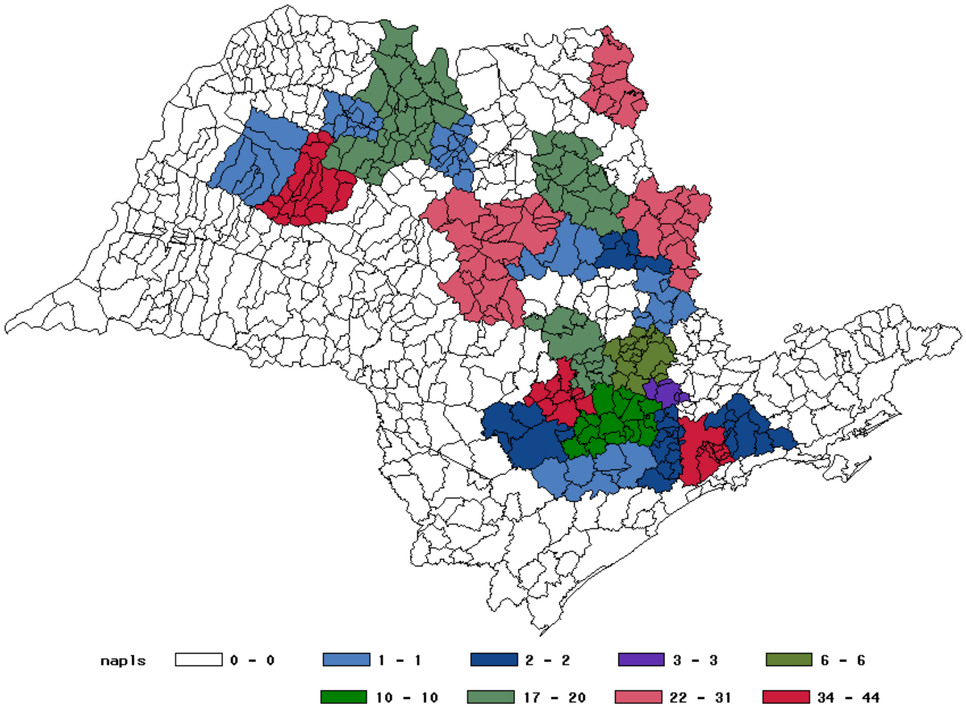
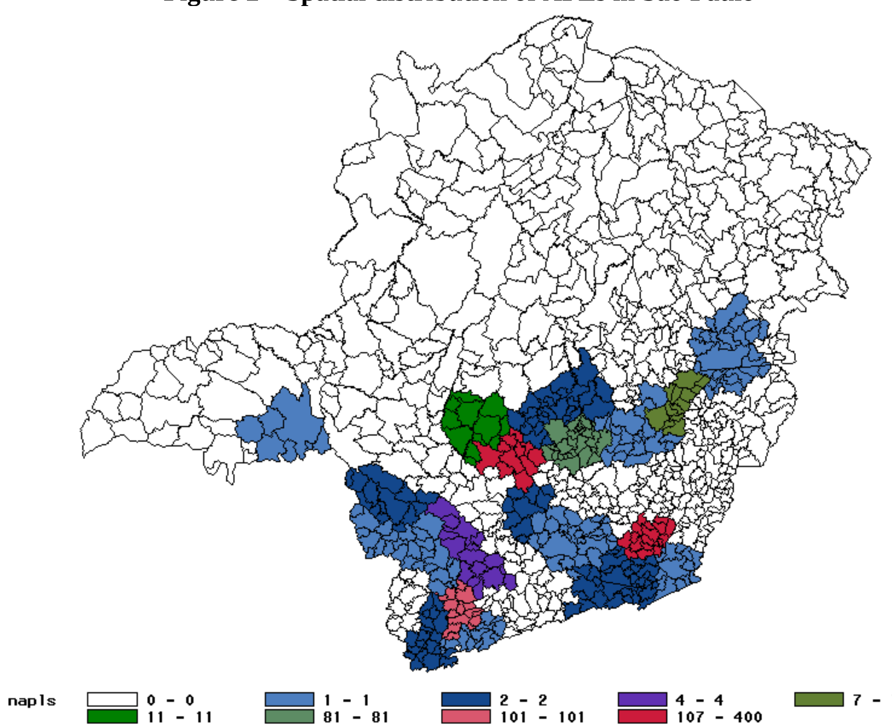
Figure 1 – Spatial distribution of APLs in Minas Gerais



Source: Instituto de Pesquisa Econômica Aplicada

Figure 2 – Spatial distribution of APLs in São Paulo



Source: Instituto de Pesquisa Econômica Aplicada

Table 1. Temporal composition of the sample

Year	Observations	%	Observations: APL firms	Indirect Beneficiaries	Non-beneficiaries
2002	61,665	10.63	455	29,937	31,273
2003	71,354	12.30	499	34,643	36,212
2004	74,425	12.83	508	35,014	38,903
2005	75,468	13.01	515	33,061	41,892
2006	73,433	12.66	494	29,912	43,027
2007	75,133	12.95	505	28,853	45,775
2008	76,967	13.27	511	28,060	48,396
2009	71,638	12.35	496	26,614	44,528
Total	580,083	100.00	**3,983**	**246,094**	**330,006**

** The first column (APL firms) displays the observed number of firms in each year that will be eventually treated between 2004 and 2009. The second column displays the observed number of firms in each year that will become indirect beneficiaries between 2004 and 2009. The third column represents the observed number of firms in each year that are non-beneficiaries for the whole period.*

Table 2. Number of firms by starting year in APLs or treated municipality

Starting year	APL firms	IB
2004	396	29,621
2005	66	3,827
2006	14	387
2007	3	430
2008	9	97
2009	60	6,206
Total	**548**	**40,568**

** "IB" is indirect beneficiaries.*

Table 3. Number of firms by industry

Industry	APL firms	IB	NB	Total
Clothing	25	4,053	5,207	9,285
Leather	285	1,562	956	2,803
Non-metallic minerals	48	256	1,524	1,828
Machinery & Equipment	19	4,028	5,364	9,411
Electronics & Computer equipment	21	398	285	704
Furniture	130	1,678	2,968	4,776
Retail & Wholesale	20	28,593	53,181	81,794
Total	**548**	**40,568**	**69,485**	**110,601**

** "IB" is indirect beneficiaries and "NB" is non-beneficiaries.*

Table 4. Number of firms by size

Firm Size	APL firms	IB	NB	Total
Small	517	39,764	68,655	**108,936**
Medium	31	804	830	**1,665**
Total	**548**	**40,568**	**69,485**	**110,601**

** "IB" is indirect beneficiaries and "NB" is non-beneficiaries.*

Table 5. Evolution of average outcomes

	Direct Beneficiaries			**Indirect Beneficiaries**			**Non-Beneficiaries**		
Year / Outcome	Em	Le	De	Em	Le	De	Em	Le	De
2002	36.618	0.772	0.075	20.043	0.477	0.045	18.348	0.268	0.024
2003	38.268	1.572	0.146	19.379	0.451	0.041	17.837	0.265	0.023
2004	43.115	1.995	0.181	20.393	0.485	0.044	17.829	0.268	0.023
2005	46.375	1.966	0.175	21.632	0.473	0.042	17.968	0.232	0.020
2006	48.714	1.732	0.154	22.520	0.534	0.050	17.609	0.222	0.020
2007	52.429	1.620	0.143	23.548	0.554	0.052	17.969	0.213	0.019
2008	55.057	1.645	0.145	24.629	0.557	0.052	18.344	0.196	0.017
2009	54.421	1.670	0.147	24.677	0.557	0.052	19.074	0.202	0.018

** "Em" is employment, "Le" is log of exports and "De" is dexport.*

Table 6. Before-After comparisons

	Direct Beneficiaries			**Indirect Beneficiaries**			**Non-Beneficiaries**		
	Em	Le	De	Em	Le	De	Em	Le	De
Before	37.443	1.172	0.111	19.711	0.464	0.043	18.092	0.267	0.024
After	50.018	1.771	0.157	22.900	0.527	0.049	18.132	0.222	0.019
Difference	12.575	0.599	0.047	3.189	0.063	0.006	0.040	-0.045	-0.004

** "Em" is employment, "Le" is log of exports and "De" is dexport.*

Table 7. Naïve diff-in-diff estimates

	Employment	Log(exports)	Pr(export)
Direct	12.54	0.64	0.05
Indirect	3.15	0.11	0.01

Figure 3. Evolution of average employment

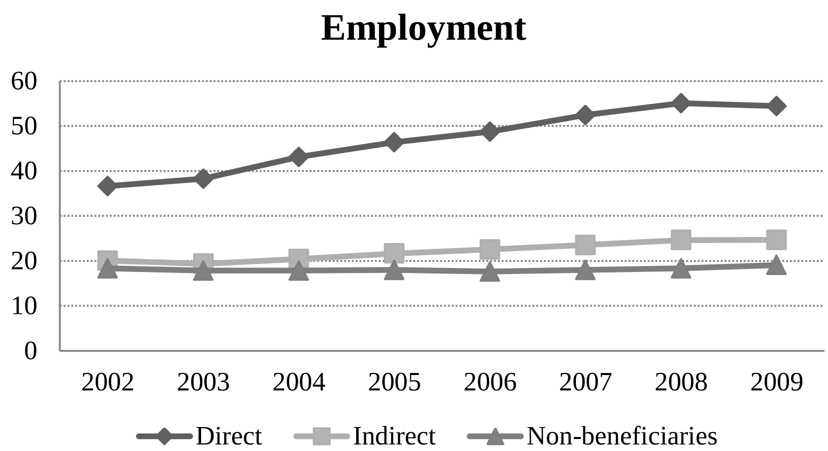


Figure 4. Evolution of average log of exports

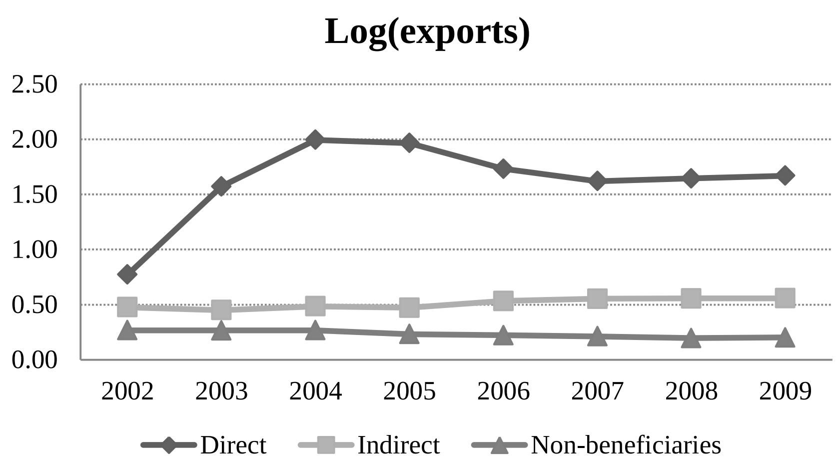
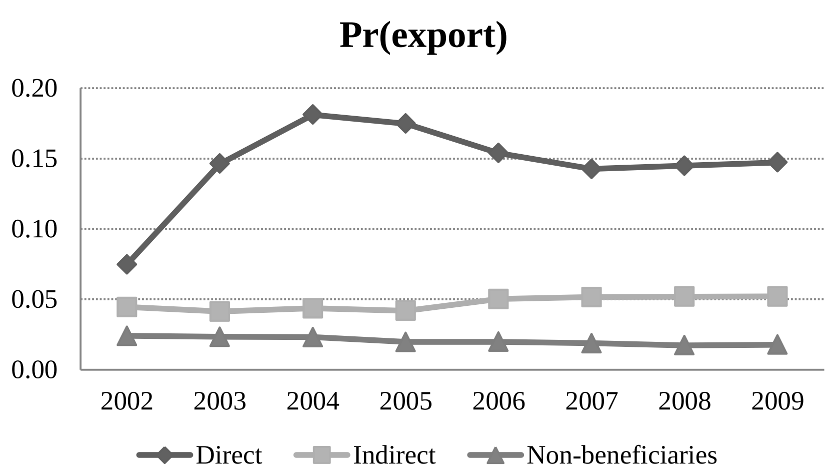


Figure 5. Evolution of average likelihood of exporting



Impact on Employment

Direct impact on *employment*

Figure 6. Mean *employment* for each group over time (DB vs. NB)

a- Full Sample | **b- Matched Sample** | **c- Reweighted Sample**

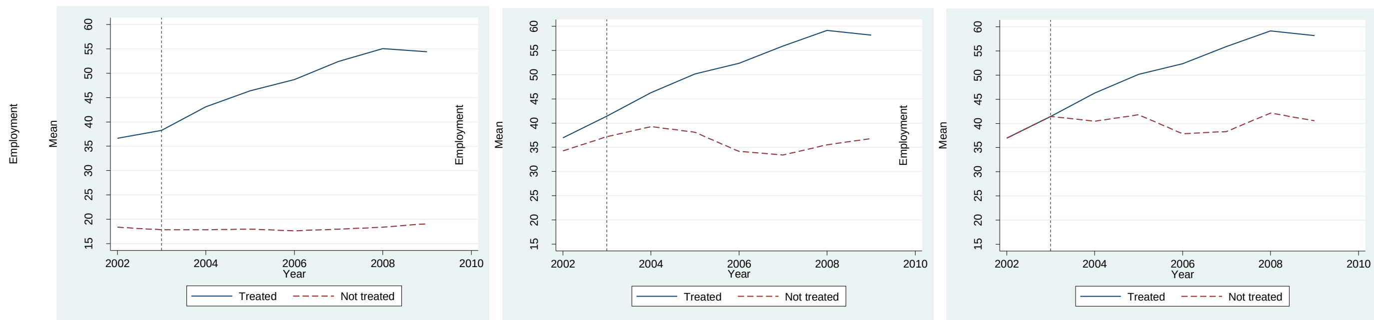


Table 8 - Direct effect on *employment*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	9.6953***	9.6123***	9.4563***	7.3737***	6.7961***	6.2989***	9.2933***	8.4325***	8.9041***
	(1.545)	(1.540)	(1.578)	(1.806)	(1.801)	(2.057)	(1.926)	(1.782)	(1.899)
Constant	15.1206***	-14.8586***	-14.9929***	35.9960***	8.6899	8.1755	37.0556***	1.6381	-4.3763
	(0.099)	(1.531)	(1.525)	(1.152)	(28.827)	(31.368)	(0.976)	(27.409)	(29.318)
Observations	333,989	333,989	333,989	5,001	5,001	5,001	186,586	186,586	186,586
R-squared	0.034	0.045	0.050	0.052	0.057	0.091	0.044	0.056	0.078
Number of firms	70,033	70,033	70,033	694	694	694	29,951	29,951	29,951
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*
b) "C" is the treatment variable.

Indirect impact on *employment*

Figure 7. Mean *employment* for each group over time (IB vs. NB)

a- Full Sample **b- Matched Sample** **c- Reweighted Sample**

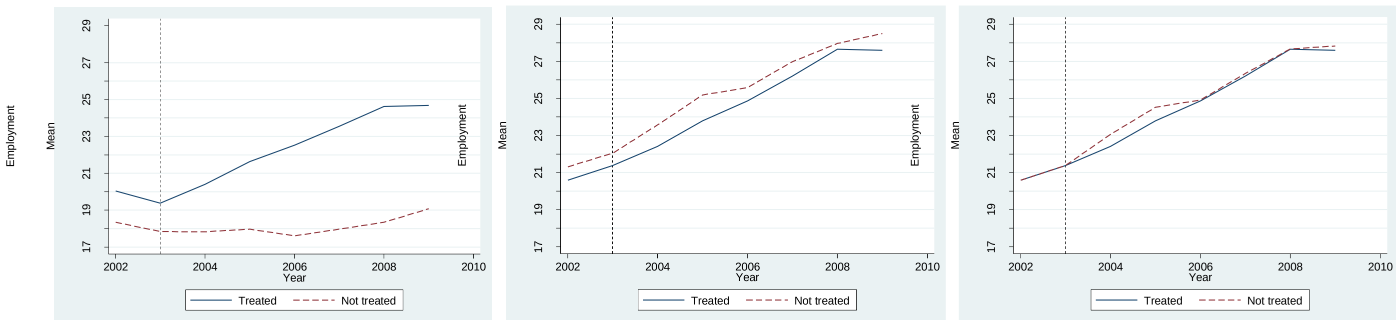


Table 9 - Indirect effect on *employment*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	-0.3227***	-0.5050***	-0.5086***	-0.8934***	-1.0757***	-0.9645***	-0.6911***	-0.8490***	-0.8179***
	(0.110)	(0.110)	(0.109)	(0.169)	(0.171)	(0.169)	(0.144)	(0.146)	(0.146)
Constant	16.7326***	-14.2319***	-15.0885***	21.4787***	-6.6594**	-8.7506***	21.3203***	-3.5949	-5.3704**
	(0.067)	(1.226)	(1.224)	(0.078)	(2.838)	(2.840)	(0.068)	(2.329)	(2.327)
Observations	576,100	576,100	576,100	255,026	255,026	255,026	372,552	372,552	372,552
R-squared	0.030	0.039	0.044	0.023	0.027	0.032	0.024	0.027	0.032
Number of firms	110,053	110,053	110,053	38,659	38,659	38,659	57,702	57,702	57,702
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*
b) "C" is the treatment variable.

Table 10. Dynamics effects on *employment*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
C_1	5.6711***	-0.1593*	3.4767**	-0.5364***	6.1735***	-0.4961***
	(0.886)	(0.088)	(1.581)	(0.130)	(1.361)	(0.111)
C_2	7.4725***	-0.7357***	5.2488**	-1.1537***	7.8666***	-0.9645***
	(1.362)	(0.124)	(2.218)	(0.193)	(1.831)	(0.157)
C_3	8.8454***	-0.8715***	6.8189**	-1.3250***	8.4316***	-1.0536***
	(1.864)	(0.156)	(2.674)	(0.245)	(2.220)	(0.199)
C_4	11.3108***	-0.9400***	8.9975***	-1.5260***	10.3994***	-1.2131***
	(2.378)	(0.177)	(3.122)	(0.282)	(2.864)	(0.226)
C_5	14.1919***	-0.5926***	12.3965***	-1.1975***	13.2967***	-0.9294***
	(2.924)	(0.203)	(3.902)	(0.327)	(3.616)	(0.263)
C_6	16.7360***	-0.2154	15.2751***	-0.9164**	17.3583***	-0.5601*
	(3.530)	(0.231)	(4.663)	(0.382)	(4.479)	(0.300)
Constant	-15.2314***	-15.3454***	10.2686	-8.8231***	-2.3577	-5.3750**
	(1.519)	(1.247)	(31.516)	(2.842)	(29.287)	(2.328)
Observations	333,989	576,100	5,001	255,026	186,586	372,552
R-squared	0.051	0.044	0.096	0.032	0.083	0.033
Number of firms	70,033	110,053	694	38,659	29,951	57,702

a) Fixed-effects estimates with controls and industry trends.

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "C_k" indicates if the firm received the program k years ago.

Table 11. Pre-treatment trends equality test on *employment*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
Cs_2003	4.3367***	0.0724	0.7947	-0.0901	-0.1244	-0.0065
	(0.684)	(0.078)	(1.538)	(0.117)	(1.324)	(0.091)
Constant	-35.8139***	-40.0245***	-22.6056	-46.6899***	-2.2046	-40.2645***
	(3.406)	(2.371)	(41.683)	(3.097)	(26.882)	(2.673)
Observations	68,439	132,065	1,388	77,318	59,902	115,404
R-squared	0.025	0.027	0.079	0.030	0.083	0.025
Number of firms	38,488	74,363	694	38,659	29,951	57,702

a) Fixed-effects estimates with controls and industry trends.

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "Cs_year" is the interaction between the treatment variable and the respective pre-treatment year.

Impact on level of exports

Direct impact on *log of exports*

Figure 8. Mean *log of exports* for each group over time (DB vs. NB)

a- Full Sample **b- Matched Sample** **c- Reweighted Sample**

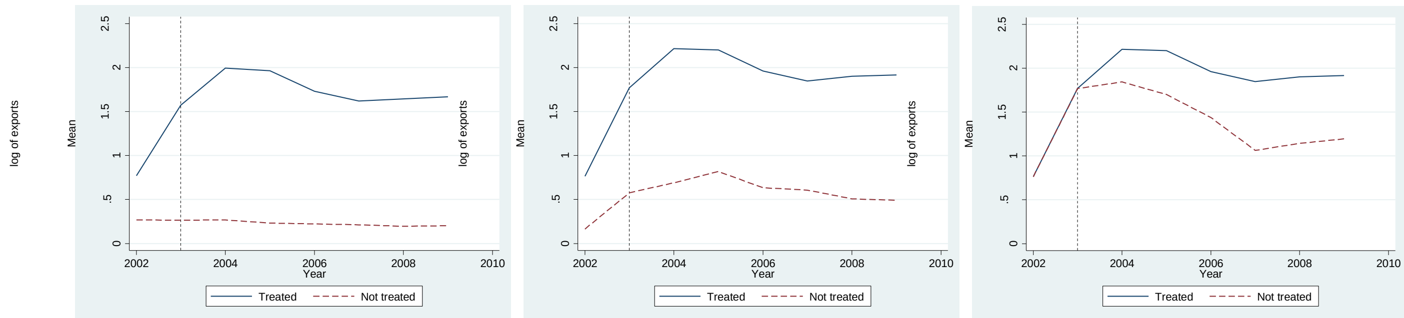


Table 12 - Direct effect on *log of exports*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	0.5790***	0.5787***	0.5104***	0.4781***	0.4543***	0.3983***	0.6485***	0.6180***	0.5935***
	(0.118)	(0.118)	(0.118)	(0.141)	(0.141)	(0.141)	(0.160)	(0.156)	(0.154)
Constant	0.1858***	0.0444	0.0468	0.6203***	3.3350*	3.4565**	0.8109***	0.3169	0.1221
	(0.006)	(0.060)	(0.060)	(0.103)	(1.707)	(1.741)	(0.107)	(2.297)	(2.166)
Observations	333,989	333,989	333,989	4,955	4,955	4,955	186,586	186,586	186,586
R-squared	0.003	0.003	0.008	0.035	0.038	0.067	0.042	0.044	0.066
Number of firms	70,033	70,033	70,033	697	697	697	29,951	29,951	29,951
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*
b) "C" is the treatment variable.

Indirect impact on *log of exports*

Figure 9. Mean *log of exports* for each group over time (IB vs. NB)

a- Full Sample **b- Matched Sample** **c- Reweighted Sample**

Table 13 - Indirect effect on *log of exports*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	0.0234***	0.0226***	0.0215***	0.0158	0.0136	0.0246*	0.0437***	0.0424***	0.0446***
	(0.008)	(0.008)	(0.008)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)
Constant	0.2843***	0.1983***	0.1577**	0.5079***	0.4160**	0.3178*	0.4923***	0.3837***	0.2895**
	(0.005)	(0.062)	(0.061)	(0.007)	(0.185)	(0.184)	(0.006)	(0.143)	(0.143)
Observations	576,100	576,100	576,100	237,224	237,224	237,224	372,552	372,552	372,552
R-squared	0.002	0.002	0.005	0.002	0.002	0.005	0.002	0.002	0.004
Number of firms	110,053	110,053	110,053	35,834	35,834	35,834	57,702	57,702	57,702
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

b) "C" is the treatment variable.

Table 14. Dynamics effects on *log of exports*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
C_1	0.6238***	-0.0003	0.5149***	0.0040	0.4805***	0.0096
	(0.113)	(0.007)	(0.140)	(0.012)	(0.160)	(0.010)
C_2	0.6385***	-0.0123	0.4440**	-0.0044	0.5810***	0.0085
	(0.145)	(0.010)	(0.192)	(0.017)	(0.217)	(0.014)
C_3	0.4004***	0.0472***	0.2382	0.0475**	0.5064**	0.0697***
	(0.152)	(0.012)	(0.218)	(0.020)	(0.219)	(0.016)
C_4	0.4097***	0.0615***	0.3091	0.0814***	0.8180***	0.0959***
	(0.157)	(0.013)	(0.197)	(0.022)	(0.242)	(0.017)
C_5	0.3882**	0.0705***	0.2694	0.0957***	0.6946***	0.1012***
	(0.159)	(0.014)	(0.205)	(0.024)	(0.235)	(0.018)
C_6	0.4156**	0.0778***	0.3454	0.1051***	0.7561***	0.1074***
	(0.171)	(0.015)	(0.236)	(0.027)	(0.261)	(0.019)
Constant	0.0514	0.1102*	3.4168*	0.3386*	0.1750	0.2833**
	(0.060)	(0.062)	(1.742)	(0.185)	(2.143)	(0.143)
Observations	333,989	576,100	4,955	237,224	186,586	372,552
R-squared	0.009	0.005	0.068	0.006	0.067	0.005
Number of firms	70,033	110,053	697	35,834	29,951	57,702

a) Fixed-effects estimates with controls and industry trends.

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "C_k" indicates if the firm received the program k years ago.

Table 15. Pre-treatment trends equality test on *log of exports*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
Cs_2003	0.7865***	-0.0039	0.4841**	0.0155	-0.0018	-0.0055
	(0.152)	(0.011)	(0.207)	(0.018)	(0.280)	(0.012)
Constant	0.0668	-0.0271	12.8835**	0.0333	3.9742	-0.0112
	(0.292)	(0.238)	(5.793)	(0.367)	(9.294)	(0.273)
Observations	68,439	132,065	1,394	71,668	59,902	115,404
R-squared	0.015	0.001	0.100	0.001	0.103	0.001
Number of firms	38,488	74,363	697	35,834	29,951	57,702

a) Fixed-effects estimates with controls and industry trends.

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "Cs_year" is the interaction between the treatment variable and the respective pre-treatment year.

Impact on the likelihood of exporting

Direct impact on *dexport*

Figure 10. Mean *dexport* for each group over time (DB vs. NB)

a- Full Sample **b- Matched Sample** **c- Reweighted Sample**

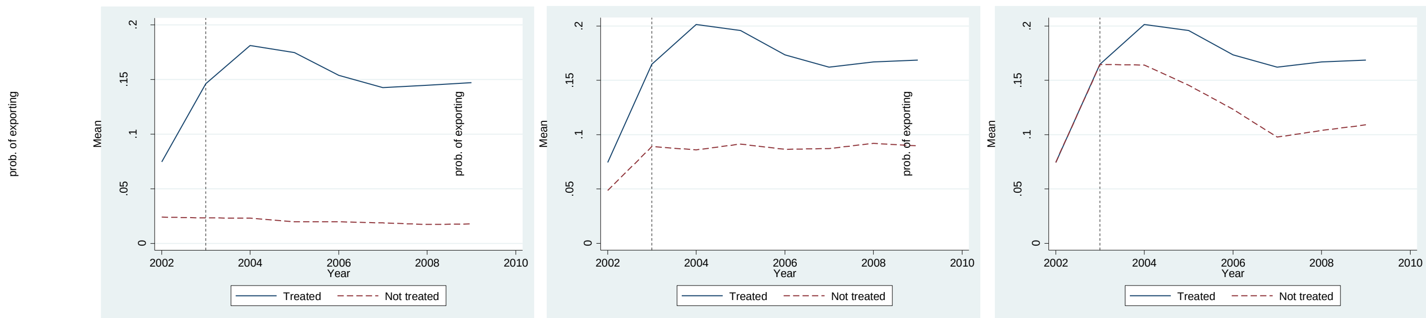


Table 16 - Direct effect on the *likelihood of exporting*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	0.0454***	0.0454***	0.0388***	0.0490***	0.0491***	0.0414***	0.0581***	0.0559***	0.0536***
	(0.011)	(0.011)	(0.011)	(0.014)	(0.013)	(0.014)	(0.015)	(0.014)	(0.014)
Constant	0.0171***	-0.0012	-0.0004	0.0731***	0.3194*	0.2984*	0.0792***	0.0275	0.0100
	(0.001)	(0.006)	(0.006)	(0.009)	(0.165)	(0.172)	(0.009)	(0.178)	(0.176)
Observations	333,989	333,989	333,989	4,994	4,994	4,994	186,586	186,586	186,586
R-squared	0.002	0.002	0.006	0.027	0.030	0.049	0.036	0.037	0.053
Number of firms	70,033	70,033	70,033	690	690	690	29,951	29,951	29,951
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

b) "C" is the treatment variable.

Indirect impact on *dexport*

Figure 11. Mean *dexport* for each group over time (IB vs. NB)

a- Full Sample **b- Matched Sample** **c- Reweighted Sample**

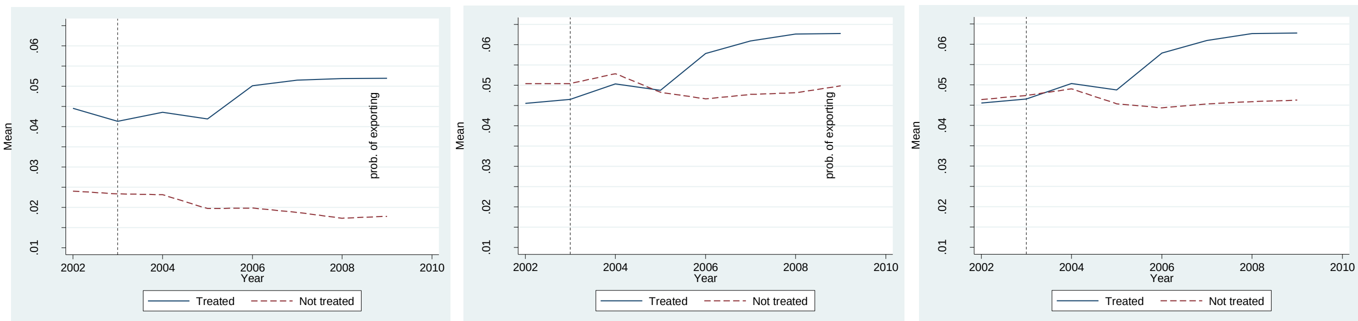


Table 17 - Indirect effect on *the likelihood of exporting*

	A - Full sample			B - Matched sample			C - Reweighted sample		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C	0.0026***	0.0025***	0.0023***	0.0032**	0.0031**	0.0037***	0.0062***	0.0062***	0.0061***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.0264***	0.0133**	0.0106*	0.0473***	0.0269	0.0220	0.0464***	0.0195	0.0155
	(0.001)	(0.006)	(0.006)	(0.001)	(0.018)	(0.018)	(0.001)	(0.014)	(0.014)
Observations	576,100	576,100	576,100	237,582	237,582	237,582	372,552	372,552	372,552
R-squared	0.002	0.002	0.004	0.002	0.002	0.004	0.001	0.001	0.003
Number of firms	110,053	110,053	110,053	35,888	35,888	35,888	57,702	57,702	57,702
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry trends	No	No	Yes	No	No	Yes	No	No	Yes
Reweighting scheme	No	No	No	No	No	No	Yes	Yes	Yes

*a) Fixed-effects estimates reported with Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

b) "C" is the treatment variable.

Table 18. Dynamics effects on *likelihood of exporting*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
C_1	0.0516***	-0.0001	0.0443***	0.0011	0.0446***	0.0021*
	(0.011)	(0.001)	(0.013)	(0.001)	(0.015)	(0.001)
C_2	0.0516***	-0.0017*	0.0450**	0.0005	0.0559***	0.0019
	(0.014)	(0.001)	(0.018)	(0.002)	(0.021)	(0.001)
C_3	0.0280**	0.0057***	0.0352*	0.0074***	0.0470**	0.0094***
	(0.014)	(0.001)	(0.021)	(0.002)	(0.021)	(0.002)
C_4	0.0272*	0.0068***	0.0413**	0.0099***	0.0675***	0.0119***
	(0.014)	(0.001)	(0.020)	(0.002)	(0.021)	(0.002)
C_5	0.0255*	0.0077***	0.0345*	0.0110***	0.0605***	0.0125***
	(0.015)	(0.001)	(0.020)	(0.002)	(0.021)	(0.002)
C_6	0.0286*	0.0086***	0.0461*	0.0124***	0.0669***	0.0135***
	(0.016)	(0.001)	(0.024)	(0.003)	(0.023)	(0.002)
Constant	0.0001	0.0054	0.2975*	0.0241	0.0134	0.0148
	(0.006)	(0.006)	(0.172)	(0.018)	(0.174)	(0.014)
Observations	333,989	576,100	4,994	237,582	186,586	372,552
R-squared	0.006	0.004	0.049	0.005	0.053	0.004
Number of firms	70,033	110,053	690	35,888	29,951	57,702

a) Fixed-effects estimates with controls and industry trends

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "C_k" indicates if the firm received the program k years ago.

Table 19. Pre-treatment trends equality test on *likelihood of exporting*

	A- Full sample		B - Matched sample		C- Reweighted sample	
	Direct	**Indirect**	**Direct**	**Indirect**	**Direct**	**Indirect**
Cs_2003	0.0709***	-0.0007	0.0374*	0.0007	-0.0001	-0.0005
	(0.015)	(0.001)	(0.020)	(0.002)	(0.025)	(0.001)
Constant	0.0039	-0.0111	1.1862**	-0.0119	0.6272	-0.0114
	(0.029)	(0.024)	(0.586)	(0.037)	(0.701)	(0.028)
Observations	68,439	132,065	1,380	71,776	59,902	115,404
R-squared	0.012	0.001	0.090	0.001	0.090	0.001
Number of firms	38,488	74,363	690	35,888	29,951	57,702

a) Fixed-effects estimates with controls and industry trends.

*b) Cluster-robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.*

c) "Cs_year" is the interaction between the treatment variable and the respective pre-treatment year.

Appendix I. Example of outputs of the reweighting scheme. Direct impact on *employment*

Figure A. Kernel density of propensity score before and after reweighting

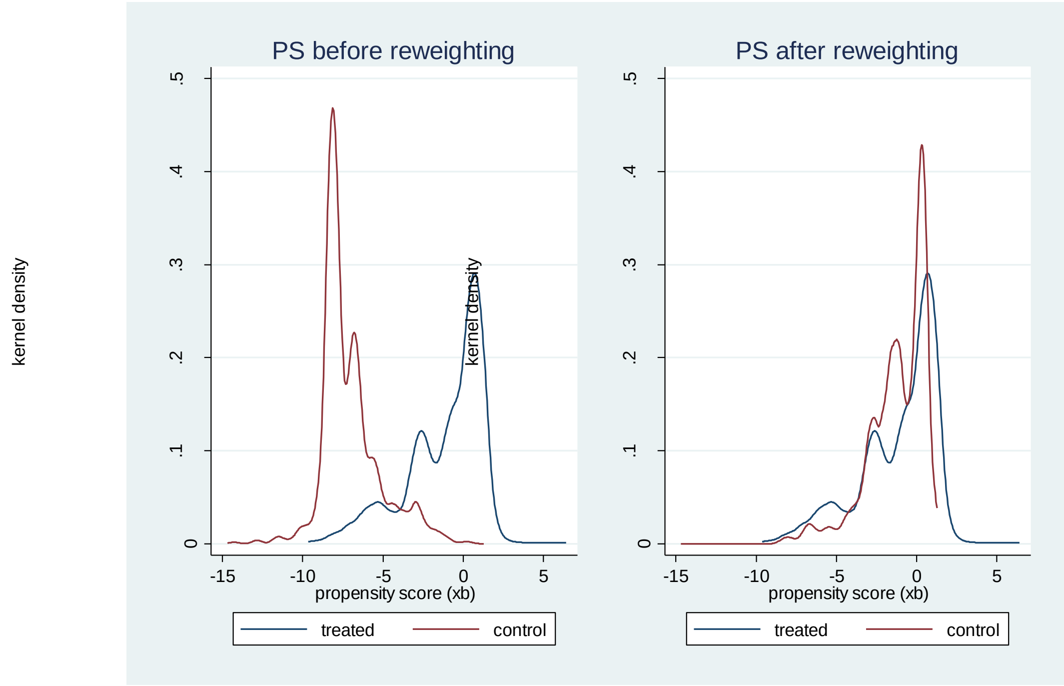


Figure B. Standardized difference between Treated and Control group