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Cluster development policy, SME's performance, and spillovers: evidence from Brazil

Lucas Figal Garone · Alessandro Maffioli · Joao Alberto de Negri · Cesar M. Rodriguez · Gonzalo Vázquez-Baré

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Abstract This paper studies the impact of the Brazilian *Arranjos Productivos Locais* (APL) policy, a cluster development policy, on small and medium enterprises' (SMEs) performance. Using firm-level data on SMEs for the years 2002–2009, this paper combines fixed effects with reweighting methods to estimate both the direct and the indirect causal effects of participating in the APL policy on employment growth, value of total exports, and likelihood of exporting. Our results show that APL policy generates a positive direct impact on the three outcomes of interest. They also show evidence of short-term negative spillovers effects on employment in the first year after the policy implementation and positive spillovers on export outcomes in the medium and long term. Thus, our findings highlight the importance of accounting for the timing and gestation periods of the effects on firm performances when assessing the impact of clusters policies.

Keywords Cluster policies · SMEs · Brazil · Impact evaluation · Spillovers · Panel data · Fixed-effects method

JEL Classifications C23 · D22 · H43 · L25 · L26 · O12 · O54 · R10

L. Figal Garone · A. Maffioli · G. Vázquez-Baré
Inter-American Development Bank, 1300 New York Ave, Washington, DC 20577, USA

L. Figal Garone
Universidad de San Andrés, Victoria, Argentina
e-mail: lfigal@iadb.org

A. Maffioli
e-mail: alessandrom@iadb.org

G. Vázquez-Baré
e-mail: gonzalovaz@iadb.org

J. A. de Negri
Instituto de Pesquisa Econômica Aplicada, Rio de Janeiro, Brazil
e-mail: joao.denegri@ipea.gov.br

C. M. Rodriguez (✉)
Department of Economics, Portland State University, 1721 SW Broadway, Portland, OR 97201, USA
e-mail: cesar.rodriguez@pdx.edu

1 Introduction

The role of industrial clusters in the economic development of countries and regions has often drawn the attention of both academics and policymakers. The idea of promoting the formation and development of clusters is based on the assumption and growing evidence that firm-level performance benefits from agglomeration. These agglomeration economies, which were first identified by Marshall (1920) and further studied by Arrow (1962) and Romer (1986), originate from industry- and location-specific externalities due to

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knowledge or technology spillovers, input–output sharing, and labor market pooling.[1]

In this paper, we analyze the effectiveness of cluster development policies (CDP) by focusing on the case of Brazil's *arranjos produtivos locais* (APL) policy in the states of Minas Gerais and Sao Paulo. We use firm-level administrative data on Brazilian small and medium enterprises (SMEs) from 2002 to 2009 to estimate the direct and the indirect—i.e., spillover—causal effects of participating in the APL policy on a series of SMEs' performance indicators.[2] These indicators comprise level of employment, value of exports, and likelihood of exporting. Our identification strategy adopts a combination of fixed effects and reweighting methods, which allows us to take full advantage of the length of the longitudinal dataset and deal with potential endogeneity and selectivity issues. In order to measure the indirect effects generated by the APL policy, we assume that geographical proximity within an industry is the main channel through which spillovers occur.

Our findings show that the APL policy increased the employment level of direct beneficiary firms by 17 percent when compared with the control group. We also find that the policy helped direct beneficiaries to increase the value of total exports by 90 percent and the likelihood of exporting by around eight percentage points. Furthermore, these positive effects seem to be constant or even increasing over time during the years after receiving the policy benefits. Regarding spillover effects, our results reveal a more complex dynamic: On the one hand, we find partial evidence of a negative indirect effect on employment in the first year after the implementation of the policy; on the other hand, we find positive spillover effects on the value of total exports and the likelihood of exporting. These latter effects become significantly relevant in the medium and long term, reaching the value of 15 percent and two percentage points in the sixth year after the treatment, respectively. Finally, the analysis of the heterogeneity of effects shows consistent patterns through different criteria of disaggregation of our sample (industry, location, and size) and confirms the positive conclusions based on the analysis on the entire sample.

The contribution of our findings to the existing literature is twofold. First, we add to the general empirical literature by studying the magnitude and timing of both long-term and spillover effects of a major CDP. Second, we produce what to our knowledge is the first rigorous impact evaluation of a CDP in Latin America, expanding the current literature that has exclusively focused on developed countries.

The rest of the paper is organized as follows. Section 2 discusses the rationale behind cluster policies. Section 3 presents Brazil's APL policy. Section 4 describes the research questions of interest and the estimation methods used to assess the impact of the cluster policy. Section 5 presents and summarizes the data used for the estimations. Section 6 reports the estimation results, and Sect. 7 presents the robustness checks to validate the empirical strategy. Finally, Sect. 8 concludes.

2 The rationale behind cluster policies

The discussion on the role of industrial clusters has been deeply influenced by Porter (1998, 2000) who argued that although changes in technology and competition may have diminished the relevance of location decisions, clusters are still a striking feature of today's economies.[3] Indeed, as Krugman (1991) and Ellison and Glaeser (1997) argued, industries still tend to be strongly geographically concentrated. This notion is also backed by abundant evidence on the importance of agglomeration economies. Since the 1970s, Sveikauskas (1975) demonstrated that productivity is generally higher in large cities, while Shefer (1973) showed that productivity is higher where

[1] Because of Marshall's seminal work, this phenomenon is often referred to as Marshallian externalities. 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] Given the confidentiality of the data, the estimations were conducted following the Instituto de Pesquisa Econômica Aplicada's (IPEA) microdata policy, which implies working in situ under the supervision of its staff and with blinded access to sensible information.

[3] 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".

industry size is larger. However, the work by Ciccone and Hall (1996) was the first to clearly address the endogeneity issues related to the relationship between agglomeration and productivity: On the one hand, firms' performance may benefit from agglomeration economies; on the other hand, firms with best performance may be the ones deciding to locate close to each other. Using data on gross state output in the USA, they found that doubling employment density increases average labor productivity by around six percent.

More recently, Ellison et al. (2010) used establishment-level data from the Census of Manufactures in USA for the period 1972–1997 to compute pairwise co-agglomeration measurements for manufacturing industries. They also constructed measures of the relevance of the three Marshall's mechanisms of industry agglomeration: (1) input–output tables for the importance of transport costs, (2) correlation across industries in terms of employment composition for the potential gains from labor market pooling, and (3) technology flows and patents citations for technological spillovers. The analysis of the relationship between co-agglomeration indices and these three measures eventually showed evidence that supports each of the mechanisms. These results suggest that input–output dependencies—i.e., firms locating near their customers and suppliers—are the most important factor, followed by labor pooling.[4]

If agglomeration is so beneficial for firms' performances, when and why is public intervention needed to promote industrial clusters? It is a well-known result in economic theory that in the presence of externalities, the market often fails to assign resources optimally, because of coordination failures. As originally pointed out by Rosenstein-Rodan (1943), investment decisions are interrelated and the investment of one firm can have a positive effect on the profitability of the investment of another firm. This is particularly important when geographical proximity and industry complementarities cause agglomeration economies.[5] For instance, without proper coordination, firms operating in the same industry and closely located may underinvest in labor training, knowing that none of them can individually fully appropriate of such investments because of labor mobility. Coordination failures, therefore, could lead a local economy or a cluster to low-investment equilibrium, because local agents, including the government, fail to coordinate investment decisions.[6]

In this context, CDP may foster the beneficial effects of agglomeration by creating a set of incentives to overcome coordination failures that hamper the development of certain industries. A CDP upgrade the cluster from the mere agglomeration to a local production system with benefits for the firms involved (European Commission 2002; Litzenberger and Sternberg 2005). For this reason, CDP devote attention and resources to strengthening coordination among cluster stakeholders. Typically, a CDP include a first phase when linkages between the different cluster stakeholders (firms, institutions, and public agencies) are built up through publically financed coordination activities. This step includes the preparation, negotiation, and adoption of development plans to map and facilitate linkages and encourage the interaction among private agents (between private firms but also in collaboration with the public sector). In a second phase, these development plans are executed usually through a combination of public and private investments aimed at promoting innovation and joint learning, and overcoming technological, infrastructure, and environmental constraints. Particular attention is usually devoted to investment in knowledge creation and technology adoption, which are two of the major sources of industry- and location-specific externalities.

On the basis of these justifications, several developed and developing countries have implemented CDP. In some cases, the implementation of CDP has been evaluated using case studies (e.g., Hartmann

[4] Other papers presenting evidence of agglomeration economies include Ellison and Glaeser (1999), Hanson (2001), Dumais et al. (2002), Rosenthal and Strange (2001, 2003), Rodriguez-Clare (2005, 2007), Combes et al. (2008, 2010), Rosenthal and Strange (2010), Li et al. (2012), and Rizov et al. (2012).

[5] Recent studies present evidence of the effect of clustering on the growth of new technology-based firms (Maine et al. 2010), the survival and performance of new firms (Wemberg and Lindqvist 2010), and how firm growth is influenced by the strength of the industrial cluster in which the firm is located (Beaudry and Swann 2009).

[6] For a review on coordination problems in development, see Hoff (2000). On clusters and coordination failures, see also Rodriguez-Clare (2005).

2002; Aranguren et al. 2006; and manuals DTI 2004, and GTZ 2007). While informative, the main limitation of this approach is that case studies do not rely on causal inference and, therefore, their findings are hardly generalizable. In very few cases, rigorous evaluations have been carried out to assess the causal impact of CDP, and hence, the evidence on their effects remains scarce and inconclusive.[7] Falk et al. (2010) analyzed a cluster policy introduced in 1999 in Bavaria, Germany, aimed at fostering innovation and regional competitiveness by stimulating cooperation between science, business, and finance in the target industries. They found that the cluster-oriented policy increased the likelihood of firms innovating by 4.6–5.7 % points. In a similar fashion, Nishimura and Okamuro (2011) analyzed the industrial cluster project (ICP) in Japan.[8] Interestingly, they found that while clusters' participants that collaborated with national universities in the same region significantly improved their R&D productivity—without reducing patent quality—participation in the cluster project alone had no significant effect on the R&D productivity of firms. Finally, Martin et al. (2011) analyzed a CDP promoting industrial clusters in France for the period 1996–2004 applying a difference-in-differences approach. They found that the program selected firms in relative decline and find no major effect on productivity. They also concluded the policy had no robust effects on employment and exports.

To our knowledge, there are no impact evaluations of CDP in Latin America. Although similar types of programs, such as agricultural (González et al. 2010; Maffioli et al. 2012), innovation (Hall and Maffioli 2008; Crespi et al. 2011; Benavente et al. 2012), export promotion (Volpe and Carballo 2008), SME support (López Acevedo and Tan 2010; Castillo et al. 2014), and supplier development (Arráiz et al. 2013) have been evaluated, none of these directly evaluate a CDP. The study by Maffioli (2005) is probably the closest piece of research to an impact evaluation of a cluster policy in this region. The author presents a theoretical discussion of industrial networks and empirically analyzes the most important Chilean networking program, the PROFO program.[9]

Despite the scarce evidence on their impact, some scholars and policy makers argue that CDP might have two key implications for Latin America (Feser 2002): Firstly, they might generate innovation activity of sufficient scale, depth, and diversity to compete on an international level. Secondly, they might induce the public sector to utilize, promote, and exploit interdependencies between economic actors to more effectively foster innovation, productivity, and learning. To this extent, what would really matter in terms of policy implications for Latin America would be the mode of organization of interfirm linkages and the *governance* of value chains (Giuliani et al. 2005).

3 Brazil's *Arranjos Produtivos Locais* policy

During the last decade, the Brazilian public and private organizations that promote SMEs have increasingly focused on industrial clusters, known as *arranjos produtivos locais* (APL—local productive arrangements). The development of APL in many regions and states of Brazil eventually also drew the attention of Brazilian academics who over time produced a number of case studies that analyze a variety of aspects of the APL experience.[10]

[7] As Anderson et al. (2004) pointed 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.

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

[9] The econometric analysis presented in Maffioli (2005) 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 results show 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.

[10] For instance, Santos et al. (2002a, b), Cassiolato et al. (2003), Machado (2003), Hoffman (2004), La Rovere et al. (2004), Lastres and Cassiolato (2005), Mytelka and Farinelli (2005), La Rovere and Shibata (2007), and Souza Filho and Martins (2013).

According to the definition of the Brazilian Service to Support Micro and Small Enterprises (SEBRAE), APL are clusters of firms within the same administrative area (e.g., municipality) that share a particular economic specialization. Within each APL, firms operate in the same (or related) industry—for instance, manufacturing firms producing goods and services, suppliers of machinery and equipment, input providers, and human resource training firms—and maintain ties of cooperation and learning both among themselves and with other stakeholders such as government, business associations, financial and research institutions (Lastres and Cassiolato 2003; and, Lastres et al. 2003).

With the purpose of promoting local economic development and increase competitiveness,[11] in the early 2000s, the Brazilian federal and state governments acknowledged the need to introduce specific policy interventions to support the APL development. After a period of policy discussion and some pilot experiences, in 2004, the APL policy was officially launched with the main objective of creating jobs and increasing competiveness in both internal and international markets by fostering firms' efficiency and cooperation capacity.

Since its inception, APL promotion has been a key component of Brazil's industrial policy, being one of the pillars of Brazil's Industrial, Technological, and Foreign Trade Policy (PITCE) and at the core of the mandate of the Brazilian Industrial Development Agency (ABDI). In this context, in 2004, the federal government through the Ministry of Development, Industry, and Foreign Trade (MDIC) created a dedicated agency—the APL Permanent Working Group (GTP-APL)—which ever since has promoted coordination among the various federal and state agencies working with APL.

In practice, the design of the APL policy has followed the structure and tools of the typical CDP where the policy itself is system oriented and combines a variety of instruments (Rip 2003; Boekholt 2003). Although at the beginning it focused on a more limited set of interventions, over time, the APL policy has evolved and expanded its set of instruments to include several of the typical tools of CDP (see Table 1). As in a typical CDP, the APL policy intervention starts with the elaboration of strategic development plans by private and public sector agents. In this phase, which focuses on the development of strategies to foster cooperation within the APL, the role of policy agents is to facilitate interaction between the various agents and identify local leaders responsible for the execution of the plans. Trainings, workshops, and managerial technical assistance are key activities during this phase. In a second phase, once the plans are completed, the policy agents support the beneficiary firms through a variety of instruments aimed at increasing their performances. This second phase may include, among other things, export promotion activities, training and technology transfer activities, the creation of sector-specific technology centers, and other club-goods.

In terms of targeting, the main focus of the APL policy has always been on SMEs. For this reason, the policy has been mainly implemented by SEBRAE and designed to target territories with certain level of SMEs' concentration and specialization (often defined as existing or potential APL).[12] Although over time different proposals have been made (e.g., Lastres and Cassiolato 2003; and, Cassiolato 2012), the APL policy has never adopted very specific and objective criteria to identify the eligible territories for APL policy support. SEBRAE, in particular, has used relatively broad criteria that guided a case-by-case assessment process of the potential beneficiary territories (see Table 2).

Selecting the territories is only a first stage of the beneficiaries' selection process. Because most of the APL policy actions (trainings, technical assistance, seminars, trade missions, etc.) target individual firms or subgroup of firms within the APL, some criteria are also needed to guide the selection of the beneficiary firms *within* the targeted territory. For this task, SEBRAE uses very specific and objective criteria to define a SME combining revenue and employment thresholds (see Table 3).

According to SEBRAE and other Brazilian stakeholders responsible for the design and implementation of the APL policy, the rationale for using partial

[11] As defined in the *Termo de Referencia para Politica Nacional de Apoio ao Desenvolvimiento de arranjos productivos locais* (2004).

[12] SEBRAE's budget comes from contributions of 0.3–0.6 % of Brazilian corporations' payrolls. Resources are collected by the Brazilian Social Security Institute (INSS) and transferred to SEBRAE.

Table 1 Current instruments and actions for the development of APL

Stages	Characteristics	Instruments/actions
Institutional strengthening	Awareness, mobilization, and articulation actions to trigger the involvement and cooperation between the different local actors (firms, support institutions, government agencies)	Training to encourage the formation and/or consolidation of local leaders and coordinators
		Building and diffusion of supporting institutions networks
		Support to develop a common shared vision and local agenda
		Conferences, meetings, and workshops on business cooperation, and on environmental and social issues
Competiveness diagnostic assessment	Diagnostic assessments to characterize the dynamics of firms networks, their actual and potential connections with domestic and international markets, and business management	Data and information collection, surveys
		Market research and studies on production chains
		Support for pilot tests
		Promotion of the extension of business and consulting services
Design of APL's development plan	Development of a public–private model with strategic lines of actions and prioritization of activities, in order to boost competitiveness and employment of firms	Technical support for plan elaboration
		Promotion of technical visits
		Workshops to discuss opportunities and challenges for the APL
Implementation of APL's development plan	Organize and coordinate the implementation of eligible activities from the APL development plan	Financial support for the creation of common activities and club-goods (technology, business and distribution centers, and export platforms)
		Promotion of trade shows with potential (domestic and international) clients
		Organization of missions to incorporate new markets and technologies
Monitoring, evaluation, and dissemination of learned lessons	Monitoring and evaluation of goals and expected outcomes from the different activities implemented, and identification of learned lessons	Technical training to develop a monitoring and evaluation system
		Support for the dissemination of results and learned lessons

Source: SEBRAE–BNDES–FIESP–IDB

subjective criteria for the identification of the territories to be targeted aims at ensuring the ability to select situations in need of support, but also, with a credible development potential. In other words, with these criteria, authorities have been trying to strike the balance between supporting those agglomerations that were not so incipient enough to gain little benefit from the APL policy, but also not so developed enough to actually not need the policy support. This choice is consistent with a common approach adopted by CDP, which are usually designed to target on high-potential clusters (Cheshire 2003). In this framework, the primary objective of a CDP such as the APL policy is not cluster formation per se, but mostly the improvement of firms' performance through the solution of coordination and market failures specific to clustered firms (Buendia 2005; Schmiedeberg 2010).

Our study focuses on the early implementation of the APL policy in the two most relevant and largest Brazilian states: Sao Paulo and Minas Gerais. Following the aforementioned criteria, SEBRAE identified territories in those states with a relatively high concentration of SMEs specialized in traditional sectors, such as the production of shoes, clothing, furniture, and construction material. Specifically, in Minas Gerais, the selection included territories specialized in the production of shoes (Nova Serrana and Regiao Metropolitana de Belo Horizonte), furniture (Ubá), clothing (Divinópolis), and electronics (Santa

Table 2 Selection criteria of APLs

a.	Capability and possibilities of operating and collaborating with other local organizations, such as universities and suppliers of machinery and equipment
b.	Form and degree of development of the APL: Selection is guided by the number and maturity of participating organizations, the existence of a local governmental institution capable of coordinating collective actions, and the quality of linkages between firms and other actors
c.	Socioeconomic relevance of the main activity of the APL, e.g., impact on GDP, exports, and level of employment
d.	Capability of generating new opportunities for social and economic development

Source: Campos et al. (2010)

Rita do Sapucaí). In the case of Sao Paulo, the selection included territories specialized in the production of shoes (Jaú, Birigui, and Franca), clothing (Americana e região, Ibitinga, Cerquilho-Tietê, Novo Horizonte, and Tabatinga), furniture and furnishing (ABC Itatiba, Mirassol, Porto Ferreira, and Votuporanga), and construction materials such as tiles (Itu, Tambaú, Tatuí, and Vargem Grande do Sul).

These APLs were mainly formed by small and medium family businesses that targeted primarily local markets. All the selected cases showed some preliminary form of collaboration among firms, other organizations, and local government authorities, but they were also clearly limited by common problems related to lack of the managerial skills required for a more effective cooperation, lack of technical skills needed to identify new markets and improve the products' quality and marketability, and lack of information and knowledge on new markets. For this reason, during the period considered in this study, SEBRAE's support for the development of theses APL focused mainly on the combination of three core interventions: (1) technical assistance—consultancy on business and competitiveness analysis including products' review; (2) training—courses, meetings, and workshops on management and business cooperation; and (3) trade promotion—fairs, business rounds, and exhibitions all over Brazil and foreign markets.

Since the participation in these activities was determined by the demand and restricted to the eligible SMEs (for SEBRAE support), firms that directly benefitted from the early implementation of the APL policy in Sao Paulo and Minas Gerais can be identified in SEBRAE's records of beneficiaries. Although these records allow us to accurately identify each of the firms treated with SEBRAE's instruments, in most cases, we cannot distinguish whether the firm participated in all activities or a subset of them, and the intensity of the participation. However, according to SEBRAE authorities, most of the firms listed in the APL beneficiary records participated in all the activities. This argument justifies an approach that look at the causal effect analysis of the APL policy as a unique program compounding all three core interventions originally implemented by SEBRAE. This approach is not necessarily a limitation since the APL policy was conceived to be and actually implemented as a systemic and comprising solution.

As we will see in the next sections, the challenge for this study is to isolate the additional effects due to the APL policy from other factors that may have affected the beneficiary firms (and the APL) performances. Because the first stage of the beneficiary firms' selection relied on partially subjective criteria to develop existing or potential APL, and the second stage of the selection was demand driven, our identification strategy will have to account for potential unobservable factors that may have affected selection and may also affect firms' performances.

Table 3 SEBRAE's classification of enterprises

Size	Sectors			
	Industry and building		Trade and services	
	Revenues (thousands US$)	Employees	Revenues (thousands US$)	Employment
Microbusiness	(0, 172]	[0, 19]	(0, 172]	[0, 9]
Small business	(172, 1,722]	[20, 99]	(172, 1,722]	[10, 49]
Medium business	≥1,722	[100, 499]	≥1,722	[50, 99]

Source: SEBRAE

4 Assessing the impact of a cluster policy

4.1 Research questions of interest

This study analyzes the impact of the APL policy on its two fundamental outcomes, namely employment and exports. More specifically, we use three firm-level measures: Number of formal employees, FOB value of exports, and probability of exporting.

We expect to find positive effects on all these three measures. In fact, the APL policy aims at creating jobs by fostering firm efficiency and increasing competiveness in both internal and international markets. Because increase in exports has often been related to productivity improvements,[13] one could argue that simultaneous positive effects on employment and exports signal productivity gains. Furthermore, an increase in the probability of exporting would not only point to higher productivity, but also to the effectiveness of the APL policy in addressing coordination failures that limit the investment in entering into new markets. In fact, because this investment mainly results in knowledge,[14] the knowledge spillovers that often occur in clusters may lead to underinvestment and limit export opportunities in the absence of proper coordination among potential exporters.[15]

In addition to estimate the direct impact of the APL policy, this study addresses the fundamental question of measuring its potential spillover effects. As pointed out by Giuliani et al. (2013), the estimation of spillover effects is a key issue for the evaluation of CDP. These effects, in fact, not only are very likely to occur and, for this reason, are at the very basis of the policy justification, but they are also explicitly pursued by the policy design. However, addressing this question requires additional steps than a standard impact evaluation, such as defining two types of beneficiaries and, therefore, two causal relationships of interest.

The first step is to define and identify two types of beneficiaries: Direct and indirect beneficiaries. The definition of direct beneficiaries (treated firms) is straightforward; these are firms that participate in the APL policy—i.e., they choose to actively participate in the activities included in the CDP. The indirect beneficiaries are those firms that do not participate in the policy, but because of the linkages they have with participants they may benefit from it.[16] Following the literature on spillover effects, we base our definition of indirect beneficiaries on a geographical criterion and on the similarity in the type of industry; i.e., we assume that geographical proximity within an industry is the main channel through which spillovers occur. Thus, an indirect beneficiary is a firm that does not participate in the APL policy but is located within a municipality (proxy variable for the APL area) where there are a positive number of direct beneficiaries in its same industry.[17]

To measure the direct and indirect impact of the cluster policy, we need to identify a valid control group for both the direct and indirect beneficiary firms. In this case, by following Holland's (1986) definition, that would be a group of firms with the same characteristics as the group of beneficiaries of the cluster policy, differing only from the formers in that firms in the control group do not benefit from the policy. In other words, non-beneficiaries are firms located in municipalities not treated by the CDP.[18] However, some non-beneficiaries may be contiguous to a treated industry-municipality, belong to the same

[13] 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 showed 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) toward the more productive firms and contributing to an aggregate productivity increase.

[14] The cost of entering into new markets often consists of knowledge related to the assessment of the market demand, product standards, distribution channels, regulatory environment etc. (Melitz 2003).

[15] On the role that public policy can play in fostering coordination among exporters see also Bernard and Jensen (2004).

[16] 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 2006). 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.

[17] Similar to the firm identifier, the municipality identifier was re-codified by IPEA to preserve the confidentiality of the data. Thus, it is not possible to link the APL (or firms) to real municipalities.

[18] We will refer to an industry with a positive number of treated firms within a municipality as a "treated industry-municipality" and to the municipalities with absence of treated firms as "non-treated municipalities".

industry, and therefore benefit from spillovers effects. If this is the case, then our estimates would be a lower bound of the impact of the policy since these firms are also included in our control group. In sum, we have three types of groups:

1. Direct Beneficiaries: treated firms (actively participate in the APL policy).
2. Indirect Beneficiaries: non-treated firms in a treated industry-municipality.
3. Non-beneficiaries (control group): non-treated firms in a non-treated municipality.

Because the APL policy was not randomly assigned, we rely on quasi-experimental methods that try to mimic the experimental setting under certain identification assumptions in order to have three sets of comparisons: (1) direct beneficiaries versus the control group, (2) indirect beneficiaries versus the control group, and (3) direct beneficiaries versus the indirect beneficiaries. The latter comparison will serve as a robustness check since we expect to interpret it as a mix of the former two effects.

4.2 Identification strategy and estimation methods

In the absence of a randomization, beneficiaries may differ from non-beneficiaries due to selection bias. In other words, in the case of CDP—as in other productive development policies—it is likely that beneficiaries are more productive than non-beneficiaries. Therefore, beneficiaries would show different outcomes than non-beneficiaries even in the absence of the CDP.

A major advantage of using longitudinal firm-level datasets is that it allows us to account for unobservable factors—permanent and, to some extent, time varying—that may affect both the outcome of interest and the participation in the APL policy. Specifically, the effects of the APL policy will be estimated using the following fixed-effects linear regression model: [19]

$$Y_{i,j,m,s,t} = \alpha_i + \alpha_t + \alpha_{j,s,t} + \beta \cdot C_{i,t} + \gamma_1 \cdot X_{i,t} + \gamma_2 \cdot X_{m,t} + \gamma_3 \cdot X_{j,m,t} + \varepsilon_{m,t} + \varepsilon_{i,j,m,s,t} \quad (1)$$

where $Y_{i,j,m,s,t}$ represents the set of outcomes to be considered for firm i, belonging to industry j, in municipality m, in state s, and year t. Firm fixed effects α_i fully absorb any permanent heterogeneity at the firm, industry, municipality, and state level, and α_t represents yearly shocks that affect all firms. Regarding the interaction terms, $\alpha_{j,s,t}$ are industry-state-year effects that fully absorb industry-year effects—i.e., time-specific shocks that affect the outcomes of all firms in industry j—and state-year effects such as the construction of a freeway, an airport, or implementation of new local policies.[20] $C_{i,t}$ is a binary variable that takes the value of one the year in which the firm i enters the APL policy and so thereafter. In the case of the comparison between indirect beneficiaries and the control group, $C_{i,t}$ takes the value one since the year in which the industry within a municipality is a treated unit as defined above. Therefore, β represents the parameter of interest which captures the causal effect of $C_{i,t}$ on the outcome under consideration. In other words, β is the average impact of the cluster policy on the direct or indirect beneficiary firms.

The main source of heterogeneity not fully controlled in Eq. (1) is the time-specific shock that affects the outcomes of all firms in a municipality m, irrespective of the industry, and the time-varying firm-specific effects. To account for this heterogeneity, we included observable time-varying firm characteristics ($X_{i,t}$) such as the log of the firm's age, the log of the average years of schooling of workers, and the log of imports of capital goods (as a proxy for investment). Moreover, we also included the log of the number of firms by municipality-year as a proxy for municipality size ($X_{m,t}$) and a Herfindahl index ($X_{j,m,t}$).[21]

The validity of the fixed-effects estimator rests on the identification assumption that trends in the outcome variables would have been equal in the absence of treatment. However, this non-testable assumption may be problematic when firms in the control group can be very heterogeneous and dissimilar from the participating firms. More precisely, firms that are less similar at the baseline are likely to follow

[19] See Bertrand et al. (2004) for a formal discussion on differences-in-differences estimates.

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

[21] The Herfindahl index was created by industry-municipality-year using level of employment. For a full discussion on measures of concentration see Hay and Morris (1987).

different trends as well.[22] In this context, although we are accounting for many plausible sources of spurious correlation, we cannot completely rule out time-varying heterogeneity.

To strengthen the validity of our identification strategy, we combine the fixed-effects methodology with entropy balancing, a multivariate reweighting method proposed by Hainmueller (2012). This method helps eliminate a potential source of bias since *weighted* non-beneficiaries are expected to be more similar to beneficiaries.[23] The reweighting scheme assigns a scalar weight to each sample unit such that reweighted groups satisfy a set of balance constraints that are imposed on the sample moments of the covariate distributions. Entropy balancing allows us to obtain a high degree of covariate balance by construction while keeping the weights as close as possible to the base (unit) weights to prevent the loss of information.

In our particular case, we will reweight the control group to match the sample mean of the treatment group in the pre-treatment period in order to subsequently estimate Eq. (1) using the treatment group and the reweighted control group. As described by Hainmueller (2012), the weights ω_i are chosen by the following scheme:

$$\min_{\omega_i} H(\omega) = \sum_{\{i \backslash C_s = 0\}} h(\omega_i)$$

subject to balance and normalizing constraints

$$\sum_{\{i \backslash C_s = 0\}} \omega_i k_{ri}(X_i) = m_r \quad \text{with} \quad r \in 1, \ldots, \text{R} \quad \text{and}$$

$$\sum_{\{i \backslash C_s = 0\}} \omega_i = 1 \quad \text{and}$$

$$\omega_i \geq 0 \quad \text{for all } i \text{ such that } C_s = 0,$$

where C_s is the treatment status, $h(\cdot)$ is a Kullback (1959) entropy metric, and $k_{ri}(X_i) = m_r$ describes a set of R balance constraints imposed on the covariate mean of the reweighted control group in order to equal the covariate mean of the treatment group (Appendix 2).[24]

For each set of comparison—direct versus control, indirect versus control, and direct versus indirect—weights are created based on pre-treatment values of the selected outcomes (for 2002–2003) and on observed characteristics of the firm in 2003 such as industry, state, size, age, average years of schooling of workers, total imports of capital goods, and the Herfindahl index. The obtained weights from this process are then passed on to the fixed-effects model (1) through sampling weights that denote the inverse of the probability the observation is included as a result of the sampling design.

After controlling for all those sources of heterogeneity that affect both the set of outcomes and the participation in APL policy, the identifying assumption implies that the fixed-effects method applied to the reweighted sample leads to a consistent estimator for β. Finally, the standard errors will be clustered at the municipality level for the inference to be robust to correlation across firms.

We complete our evaluation by analyzing how the policy's effects vary over time and how they differ by location, sector, and firm size. The former exercise is a key complement to the assessment of the average effect of the policy over the entire period under consideration. In fact, interventions such as the APL policy usually imply some maturity time before having any significant effects on firms' performance, or in other words, the realization of such effects may require a period of gestation after the policy takes place. Therefore, a proper consideration of the timing of the effects is crucial and failure to account for this issue may lead to inaccurate conclusions and policy recommendations. For this purpose, we modify Eq. (1), modifying the treatment dummy for several dummies $C_{ki,t}$ indicating the number of years since entry to the program.

The assessment of the heterogeneity of the policy effects is also an important complement to the estimation of its average impact. In fact, due to the nature of the APL policy, the effects may vary according to certain characteristics of the beneficiaries. By restricting our analysis to the average impact on the whole sample of treated firms, we may overlook relevant findings on the policy effectiveness. In particular, the APL policy is likely to have differential effects depending on the beneficiaries' location, productive specialization, and size. For this reason, we expand our analysis to the evaluation of the

[22] For additional discussion regarding pre-treatment trends please refer to Dehejia and Wahba (1999), Blundell and Costas Dias (2000) and Imbens et al. (2001).

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

[24] We use the *Stata* package called *ebalance* introduced by Hainmueller and Xu (2011). For implementation issues see also Hainmueller (2012).

policy's effects by state, sectors, and size, and we estimate Eq. (1) substituting the treatment dummy with the interaction between the treatment variable and dummies capturing such characteristics.

5 The data

Our data were generated by combining different sources of information. In particular, we combine information from: (1) the *Relacao Anual de Informacoes Sociais* (RAIS),[25] (2) the *Secretaria de Comércio Exterior* (SECEX), and (3) the SEBRAE administrative records of the beneficiary firms. By merging these datasets, we are eventually able to identify direct beneficiary, indirect beneficiary, and non-beneficiary firms.

To prevent these groups from contamination, we trimmed the following firms from the final dataset: (1) firms with observations only before or after their starting year of (direct or indirect) treatment; (2) firms that change municipality or industry; and (3) firms located in a treated municipality that belong to a non-treated industry-municipality, as defined above. Moreover, we kept in the sample firms that are observed in both pre-treatment years, i.e., 2002–2003.[26] Hence, the sample totals 233,623 observations from 34,959 SMEs from Minas Gerais and Sao Paulo for the period 2002–2009.[27] Table 4 presents the distribution of firms by starting year in the APL policy and treated industry-municipality. In the APL considered in this study, the policy started in 2004. There are few cases of firms participating in some APL pilot projects receiving support before 2004, but neither those nor the municipalities where they are located will be considered in the analysis. Finally, Tables 5 and 6 show that the APL policy targeted firms mainly from the leather and furniture industries, relatively more concentrated in Minas Gerais, and for the great majority small.

Table 4 Number of firms by starting year in APL policy/treated industry-municipality

Starting year	DB	IB
2004	318	3,600
2005	52	517
2006	9	98
2007	0	0
2008	1	23
2009	25	6,405
Total	405	10,643

DB direct beneficiaries, *IB* indirect beneficiaries

Table 5 Number of firms by industry (state)

Industry	DB (MG)	DB (SP)	IB	CG	Total
Clothing	1	13	291	1,970	2,275
Leather	191	23	788	330	1,332
Non-metallic minerals	0	45	149	883	1,077
Machinery and equipment	9	1	38	1,937	1,985
Electronics and computer equipment	10	0	6	98	114
Furniture	70	30	560	1,275	1,935
Retail and wholesale	10	2	8,811	17,418	26,241
Total	291	114	10,643	23,911	34,959

DB direct beneficiaries, *IB* indirect beneficiaries, *CG* control group, *MG* Minas Gerais, *SP* Sao Paulo

The outcomes of interest are employment, value of total exports, and the likelihood of exporting—a dummy variable that takes the value 1 if the firm is an exporter and the value 0 otherwise.[28] Additional control variables include the firm's age, the average years of schooling of workers, the total imports of capital goods, the number of sampled firms by

[25] 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.

[26] Using the reweighting method will only keep firms who were observed in both pre-treatment years, i.e. 2002–2003.

[27] Several industries presented only one observation in the 2007 RAIS and were therefore excluded due to confidentiality issues. Other industries such as paper products, metal products, medical instruments and chemical products industries were also excluded since they had a negligible number of APL participating firms.

[28] Both for *employment* and for *total exports*, the series will be expressed in natural logarithms. For the outcome *log of exports* we assign the value of 0 when firms have 0 exports to avoid excluding non-exporting firms from the sample, which could bias the results by affecting the composition of the treatment and control groups (see Angrist and Pischke, 2008).

Table 6 Number of firms by size

Firm size	DB	IB	CG	Total
Small	392	10,486	23,636	34,514
Medium	13	157	275	445
Total	405	10,643	23,911	34,959

DB direct beneficiaries, *IB* indirect beneficiaries, *CG* control group

municipality-year, and the Herfindahl index—a measure of agglomeration. All these variables but the Herfindahl index will be expressed in natural logarithms.

Table 7 depicts the evolution of the outcomes over time and offers a preliminary analysis by comparing the performances of our three groups of interest (Appendix 1). A salient feature highlighted by the figures is that, in all years under study, the treated group has been outperforming the other two groups, while the indirect beneficiaries also had a better performance than the control group. This provides evidence of a potential selection bias since direct and indirect beneficiaries of the APL policy show a better performance—than the firms that do not participate—before starting the treatment. Thus, it would be expected to have higher outcomes for those groups in the absence of the policy. The next section will use the econometric methodology previously explained to correct for several types of biases in order to estimate the impact of the APL policy and carefully analyze the dynamic pattern of the effects.

6 Results

The estimates on employment show the expected positive direct effect and a more surprising negative spillover effect (Table 8). Specifically, we find that, relatively to the control group, the employment level of the direct and indirect beneficiary firms increased by 17 percent and decreased by 2.5 percent, respectively. These estimates are in line with the result of the comparison between direct and indirect beneficiary firms, which is around 22 percent. When considering the dynamic effects and spillovers, we find that the direct effect on employment increased over time, from a magnitude of 12 percent in the first year after treatment up to 26 percent after six years of treatment. For indirect beneficiaries, we find that the decrease in employment is actually significant only in the first year after treatment with a value of approximately four percent.

The estimates on exports show evidence of large, time-varying, direct impacts and some medium- and long-term spillover effects both in terms of export volume and probability of exporting (Tables 9, 10). More specifically, we find that relatively to the control group, beneficiary firms increased their value of total exports by 90 percent and their likelihood of exporting in about eight percentage points,[29] while the indirect beneficiaries experienced only modest and not statistically significant increases. However, when we analyze the dynamic trend of the spillover effects, we find that the latter effects become significant after the fourth year of treatment showing an almost constant trend and reaching values of around 15 percent for total exports and two percentage points for the likelihood of exporting. The aforementioned effects seem to be consistent with Jaffe et al. (1993) in that spillover effects may take time to materialize. The positive spillover effects on exports measures are also evidenced by the lower coefficient we estimated when comparing direct beneficiary firms with indirect beneficiaries rather than with the control group.

Overall, our findings show that the APL policy in Minas Gerais and Sao Paulo has been effective in fostering job creation through efficiency gains and spillovers. In fact, as expected, we find rather strong simultaneous effects on both employment and export measures, which will be hardly achievable without a significant increase in firms' productivity. We have therefore collected evidence that supports the hypothesis that the efficiency enhancing activities put in place by the APL policy during this period, namely technical assistance, training, and trade promotion were actually effective.

In this context, the specific components of these activities aimed at promoting coordination among firms—and inducing direct beneficiary to invest more in labor training, innovation, and internationalization—may have been particularly important. Their

[29] The large direct effect on exports could be partially due to the fact that we are not excluding non-exporting firms and therefore the average of exports before the program was implemented is relatively low (U$S 21,744) compared with the one that only considers exporting firms (US$ 914,738).

Table 7 Evolution of average outcomes

Year/outcome	Direct beneficiaries			Indirect beneficiaries			Control group		
	Lem	Lex	Dex	Lem	Lex	Dex	Lem	Lex	Dex
2002	3.186	0.756	0.072	2.579	0.424	0.039	2.482	0.244	0.022
2003	3.298	1.807	0.165	2.633	0.430	0.039	2.516	0.255	0.023
2004	3.413	2.291	0.207	2.681	0.498	0.045	2.591	0.304	0.026
2005	3.471	2.287	0.204	2.729	0.482	0.042	2.645	0.293	0.025
2006	3.484	1.951	0.171	2.765	0.565	0.054	2.683	0.352	0.031
2007	3.547	1.863	0.163	2.794	0.582	0.054	2.725	0.357	0.031
2008	3.571	1.934	0.170	2.815	0.578	0.054	2.770	0.367	0.031
2009	3.541	1.942	0.171	2.789	0.568	0.053	2.778	0.372	0.032

Lem log of employment, *Lex* log of total exports, *Dex* dexport

Table 8 Effects on *log of employment*

Variables	Direct versus control	Indirect versus control	Direct versus indirect
Average (C)	0.1656***	−0.0248***	0.2218***
	(0.049)	(0.009)	(0.051)
C_1	0.1175***	−0.0397***	0.1070***
	(0.045)	(0.007)	(0.020)
C_2	0.1931***	−0.0114	0.1852***
	(0.047)	(0.011)	(0.033)
C_3	0.2308***	0.0023	0.2118***
	(0.075)	(0.020)	(0.042)
C_4	0.2378***	0.0081	0.3421***
	(0.083)	(0.024)	(0.126)
C_5	0.2497**	−0.0030	0.3394***
	(0.118)	(0.023)	(0.108)
C_6	0.2644**	−0.0014	0.3480***
	(0.105)	(0.018)	(0.084)
Observations	155,145	230,437	81,664
Number of firms	24,316	34,554	11,048
Number of municipalities	1,017	1,021	68
Firm fixed effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Industry-state-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "C" is the treatment variable; (c) "C_k" indicates if the firm or industry-municipality received the program k years ago; (d) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9 Effects on *log of exports*

Variables	Direct versus control	Indirect versus control	Direct versus indirect
Average (C)	0.9067***	0.0236	0.4570**
	(0.221)	(0.030)	(0.185)
C_1	0.8914***	−0.0267	0.4230**
	(0.293)	(0.040)	(0.191)
C_2	1.0332***	0.0235	0.7052***
	(0.311)	(0.046)	(0.148)
C_3	0.7801***	0.0791	0.4462**
	(0.283)	(0.080)	(0.200)
C_4	0.8828***	0.1617**	0.3313
	(0.255)	(0.074)	(0.277)
C_5	0.8753***	0.1500**	0.3640
	(0.279)	(0.070)	(0.224)
C_6	0.9028***	0.1540**	0.3649
	(0.287)	(0.071)	(0.240)
Observations	155,145	230,437	81,664
Number of firms	24,316	34,554	11,048
Number of municipalities	1,017	1,021	68
Firm fixed effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Industry-state-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "C" is the treatment variable; (c) "C_k" indicates if the firm or industry-municipality received the program k years ago; (d) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 10 Effects on *likelihood of exporting*

Variables	Direct versus control	Indirect versus control	Direct versus indirect
Average (C)	0.0762***	0.0025	0.0392**
	(0.021)	(0.003)	(0.017)
C_1	0.0770***	−0.0034	0.0394**
	(0.029)	(0.004)	(0.016)
C_2	0.0873***	0.0025	0.0689***
	(0.031)	(0.004)	(0.014)
C_3	0.0594**	0.0082	0.0300
	(0.026)	(0.007)	(0.022)
C_4	0.0726***	0.0181***	0.0226
	(0.023)	(0.006)	(0.029)
C_5	0.0735***	0.0177***	0.0281
	(0.026)	(0.007)	(0.022)
C_6	0.0812***	0.0192***	0.0321
	(0.027)	(0.007)	(0.024)
Observations	155,145	230,437	81,664
Number of firms	24,316	34,554	11,048
Number of municipalities	1,017	1,021	68
Firm fixed effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Industry-state-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "C" is the treatment variable; (c) "C_k" indicates if the firm or industry-municipality received the program k years ago; (d) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

importance is indeed well reflected in our strong positive results on both measures of exports. In this case, APL policy may have contributed not only by fostering firm-level productivity—given that more productive firms self-select into export markets—and directly facilitating the access to foreign market, but also by allowing beneficiary firms to effectively overcome coordination failures that, in a context of high entry costs and knowledge spillovers, often discouraged investment in the exploration of new international markets. The relevance of these coordination failures is reinforced by the medium-term–long-term spillover effects, which suggest that the knowledge on external markets generated by the beneficiaries eventually also benefitted the cluster firms that did not actively participate in the APL policy.

The short-term negative spillover effect on employment could be due to the difference in timing of the effects between direct and indirect beneficiaries. Because direct beneficiary firms received the benefits of the APL policy before and more intensively than the indirect beneficiaries, in the very short run the former probably tended to absorb and attract employees from the pool of specialized workers of the clusters, at the expenses of the other firms in the APL. Over time, this effect faded away, most likely because the benefit of the APL policy started to spread to indirect beneficiaries and because additional employment started to be relocated from outside the APL.

A note of caution is needed for the interpretation of the results on heterogeneous effects, which are estimated by interacting the treatment variable with a set of firms' characteristics (Tables 11, 12). Although the richness of our database allows us to compute these interactions, their estimation comes at some costs. The most obvious one is that the coefficients corresponding to each interaction are likely to be more imprecise than those of the average effects, since only a smaller group of firms is captured by each interaction dummy variable. This issue becomes even more challenging when we expand the analysis to the estimation of the dynamic trend of these heterogeneous effects, given that each of the coefficients of the heterogeneous effect for each period is estimated through the interaction among the treatment variable, the dummies for firm characteristics, and the time dummies. This could be particularly problematic in the estimation of the indirect effects, which tend to appear only after a certain period of time.[30]

With this caveat in mind, the analysis of the heterogeneous effects confirms the robustness of our previous results. In particular, for all the interactions, we find positive direct effects on the three outcome of interest. The only exception is the non-metallic product sector that does not show significant average

[30] Since the assessment of heterogeneous effects inevitably implies statistical power problems—i.e. the sub-sample of beneficiaries for each interaction term could be rather small—we follow the standard rule-of-thumb of considering interactions for which at least twenty beneficiaries are available. We make an exception in the case of the heterogeneity by size, because the sample can only be divided in small and medium firms.

Table 11 Heterogeneity of direct effects

Characteristic * Treatment / Characteristic * Dynamic Treatment									
	Effects by Sector			Effects by States			Effects by Size		
	lemployment	lexports	dexport	lemployment	lexports	dexport	lemployment	lexports	dexport
	Leather			Minas Gerais			Small		
Average	0.1420*	1.0342***	0.0810***	0.1463*	0.9532***	0.0807***	0.1687***	0.8712***	0.0742***
	(0.082)	(0.266)	(0.025)	(0.075)	(0.259)	(0.028)	(0.051)	(0.255)	(0.024)
C_1	0.1348*	1.2968***	0.1027***	0.1414*	1.3818***	0.1211***	0.1216***	0.8827***	0.0761**
	(0.076)	(0.344)	(0.036)	(0.073)	(0.393)	(0.043)	(0.047)	(0.322)	(0.032)
C_2	0.2350***	1.0548**	0.0799*	0.1728***	1.0209**	0.0857**	0.2014***	1.0184***	0.0863**
	(0.066)	(0.434)	(0.043)	(0.065)	(0.396)	(0.042)	(0.049)	(0.340)	(0.034)
C_3	0.2424**	0.8549*	0.0583	0.1710*	0.5310**	0.0375	0.2385***	0.7315**	0.0561**
	(0.109)	(0.439)	(0.043)	(0.091)	(0.244)	(0.028)	(0.079)	(0.300)	(0.028)
C_4	0.2156**	0.9094***	0.0712**	0.2056**	0.8118***	0.0670***	0.2432***	0.8160***	0.0673***
	(0.104)	(0.304)	(0.029)	(0.104)	(0.217)	(0.020)	(0.087)	(0.269)	(0.025)
C_5	0.2139	0.9063***	0.0700**	0.2216	0.8872***	0.0754***	0.2542**	0.8149***	0.0684**
	(0.161)	(0.317)	(0.031)	(0.155)	(0.263)	(0.024)	(0.125)	(0.296)	(0.028)
C_6	0.1692*	0.8661***	0.0708**	0.2532**	0.9288***	0.0816***	0.2676**	0.8530***	0.0768***
	(0.096)	(0.317)	(0.032)	(0.121)	(0.258)	(0.024)	(0.111)	(0.302)	(0.029)
	Non-metallic minerals			Sao Paulo			Medium		
Average	0.1325	0.1964	0.0366	0.2206***	0.9618*	0.0785**	0.2733***	3.8299***	0.2758**
	(0.107)	(0.267)	(0.029)	(0.054)	(0.505)	(0.038)	(0.090)	(1.376)	(0.120)
C_1	0.0818	-0.1735	-0.0020	0.1058***	0.3756	0.0295	0.1484**	3.1009***	0.2668***
	(0.070)	(0.169)	(0.018)	(0.031)	(0.354)	(0.026)	(0.060)	(0.858)	(0.089)
C_2	0.2301**	0.3051	0.0492*	0.2950***	1.4609**	0.1241**	0.2362***	3.5857**	0.2705*
	(0.115)	(0.249)	(0.026)	(0.065)	(0.623)	(0.053)	(0.086)	(1.693)	(0.156)
C_3	0.3132**	0.6778**	0.0802**	0.4378***	1.7881**	0.1457**	0.3532***	3.8497**	0.2642**
	(0.156)	(0.333)	(0.033)	(0.130)	(0.747)	(0.057)	(0.116)	(1.555)	(0.131)
C_4	0.3341*	0.7945**	0.0968**	0.3946***	1.5213**	0.1224**	0.3239**	4.0980**	0.2741**
	(0.182)	(0.389)	(0.039)	(0.107)	(0.608)	(0.047)	(0.140)	(1.605)	(0.133)
C_5	0.3171	0.8546*	0.1094**	0.4011***	1.2918**	0.1033**	0.4501***	4.0405**	0.2717**
	(0.202)	(0.437)	(0.045)	(0.111)	(0.618)	(0.048)	(0.156)	(1.618)	(0.135)
C_6	0.3194	0.9882*	0.1341**	0.3743***	1.2678*	0.1135*	0.4902***	4.2772**	0.2949**
	(0.226)	(0.523)	(0.053)	(0.119)	(0.717)	(0.058)	(0.172)	(1.668)	(0.141)
	Furniture								
Average	0.2489***	1.0336**	0.0917**						
	(0.062)	(0.458)	(0.045)						
C_1	0.1422***	0.7981	0.0822						
	(0.037)	(0.636)	(0.067)						
C_2	0.1804***	1.5633**	0.1409*						
	(0.068)	(0.724)	(0.075)						
C_3	0.3241***	0.7500	0.0589*						
	(0.119)	(0.456)	(0.034)						
C_4	0.4232***	1.0581**	0.0769**						
	(0.122)	(0.448)	(0.037)						
C_5	0.5374***	1.0170**	0.0765*						
	(0.197)	(0.507)	(0.046)						
C_6	0.7104***	1.2400***	0.1048**						
	(0.211)	(0.475)	(0.041)						

Observations	18,692
Firms	2,847
Municipalities	548
Firm-fixed effects	Yes
Time effects	Yes
Industry-state-year fixed effects	Yes
Controls	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "Characteristic*treatment" or "Characteristic × dynamic treatment" are dummy variables that result from the interaction between the treatment variable or dynamic treatment variables and the respective characteristic; "lemployment" is the log of employment, "lexports" is the log of total exports, and "dexport" is a dummy variable that takes the value 1 if the firm exports and value 0 otherwise; (c) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 12 Heterogeneity of indirect effects

Characteristic * Treatment / Characteristic * Dynamic Treatment									
	Effects by Sector			Effects by States			Effects by Size		
	lemployment	lexports	dexport	lemployment	lexports	dexport	lemployment	lexports	dexport
	Leather			Minas Gerais			Small		
Average	-0.0176	0.1616	0.0089	-0.0172	0.0113	0.0024	-0.0100	0.0661	0.0073
	(0.050)	(0.157)	(0.014)	(0.064)	(0.175)	(0.019)	(0.029)	(0.118)	(0.010)
C_1	-0.0324	-0.0402	-0.0111	0.0263	-0.0354	-0.0142	-0.0232	-0.0512	-0.0048
	(0.030)	(0.235)	(0.023)	(0.048)	(0.335)	(0.038)	(0.019)	(0.147)	(0.014)
C_2	-0.0127	0.0503	-0.0022	0.0217	-0.1946	-0.0212	-0.0061	0.0488	0.0047
	(0.046)	(0.218)	(0.019)	(0.053)	(0.315)	(0.033)	(0.030)	(0.156)	(0.013)
C_3	0.0160	0.2353	0.0127	0.0147	-0.1660	-0.0146	0.0177	0.1550	0.0134
	(0.083)	(0.318)	(0.027)	(0.085)	(0.296)	(0.035)	(0.058)	(0.247)	(0.020)
C_4	0.0145	0.5613***	0.0510***	-0.0668	0.3473	0.0468*	0.0246	0.3792*	0.0409**
	(0.102)	(0.214)	(0.017)	(0.088)	(0.264)	(0.026)	(0.071)	(0.202)	(0.016)
C_5	-0.0204	0.5439***	0.0508***	-0.1050	0.4365	0.0573*	-0.0095	0.3505*	0.0396**
	(0.094)	(0.187)	(0.019)	(0.126)	(0.299)	(0.029)	(0.067)	(0.183)	(0.017)
C_6	-0.0409	0.7634***	0.0742**	-0.0783	0.5156	0.0631*	-0.0118	0.4462**	0.0508**
	(0.074)	(0.284)	(0.030)	(0.068)	(0.345)	(0.035)	(0.053)	(0.224)	(0.022)
	Non-metallic minerals			Sao Paulo			Medium		
Average	-0.0198	-0.0776	-0.0001	-0.0043	0.0845	0.0082	0.1062**	0.0981	-0.0158
	(0.043)	(0.111)	(0.012)	(0.036)	(0.144)	(0.012)	(0.046)	(0.258)	(0.023)
C_1	-0.0495	-0.0608	-0.0033	-0.0293	-0.0526	-0.0030	0.0941**	-0.1363	-0.0225
	(0.030)	(0.101)	(0.011)	(0.018)	(0.164)	(0.015)	(0.041)	(0.344)	(0.032)
C_2	-0.0449	-0.0707	0.0005	-0.0126	0.1625	0.0150	0.0870*	0.1170	-0.0239
	(0.042)	(0.121)	(0.014)	(0.037)	(0.173)	(0.013)	(0.045)	(0.375)	(0.033)
C_3	0.0008	-0.0369	0.0086	0.0291	0.3031	0.0244	0.1563*	0.4126	0.0029
	(0.052)	(0.137)	(0.013)	(0.072)	(0.317)	(0.025)	(0.080)	(0.323)	(0.023)
C_4	0.0240	-0.0507	0.0069	0.0698	0.4157*	0.0382**	0.1040	0.4517	0.0094
	(0.064)	(0.162)	(0.016)	(0.085)	(0.239)	(0.018)	(0.096)	(0.320)	(0.020)
C_5	0.0324	-0.0545	0.0058	0.0511	0.3145*	0.0287**	0.1175	0.4186	0.0084
	(0.069)	(0.173)	(0.017)	(0.067)	(0.170)	(0.013)	(0.095)	(0.308)	(0.021)
C_6	0.0214	-0.1998	-0.0110	0.0771	0.3382	0.0302*	0.2586*	0.7437**	0.0124
	(0.066)	(0.225)	(0.021)	(0.059)	(0.213)	(0.017)	(0.134)	(0.330)	(0.024)
	Furniture								
Average	0.0165	-0.0534	0.0055						
	(0.036)	(0.145)	(0.012)						
C_1	0.0085	-0.0791	0.0027						
	(0.034)	(0.125)	(0.011)						
C_2	0.0623	0.2152	0.0293*						
	(0.044)	(0.163)	(0.015)						
C_3	0.0236	-0.0848	0.0006						
	(0.083)	(0.315)	(0.028)						
C_4	0.0636	-0.1401	-0.0010						
	(0.072)	(0.272)	(0.023)						
C_5	-0.0480	-0.1329	0.0014						
	(0.116)	(0.308)	(0.027)						
C_6	0.0873	-0.1594	0.0031						
	(0.102)	(0.350)	(0.030)						

Observations	26,401
Firms	3,985
Municipalities	550
Firm-fixed effects	Yes
Time effects	Yes
Industry-state-year fixed effects	Yes
Controls	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "Characteristic*treatment" or "Characteristic*dynamic treatment" are dummy variables that result from the interaction between the treatment variable or dynamic treatment variables and the respective characteristic; "lemployment" is the log of employment, "lexports" is the log of total exports, and "dexport" is a dummy variable that takes the value 1 if the firm exports and value 0 otherwise; (c) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

effects most likely due to a sample size problem. In terms of sectors, both the leather products and furniture show almost identical effects in terms of export outcomes. The average effect on employment is, however, a much higher for the furniture sector, a difference that seems to be driven by stronger long-run effects. In terms of states, the results for export outcomes are again almost identical, while those on employment differ and show stronger effects in Sao Paulo. Since the effects in employment are the only ones to show some significant variation by sectors, the differential effect by state most likely reflect a slightly different sector composition of the beneficiaries in the two states. Finally, the results by size clearly show stronger direct effects for medium firms, both on average and overtime. Although the interpretation of the magnitude of these effects requires extreme caution given the small sample of medium firms, stronger direct effects on relatively larger firms seems to be consistent with a policy aimed at fostering the firms' competitiveness on more complex market.

The analysis of the heterogeneity of the indirect effects results in a relatively less conclusive picture, but still quite consistent with our general findings. Positive spillover effects are in fact mainly concentrated in export outcomes and tend to appear 3 years after the implementation of the policy. The stronger externalities are observed in the leather product sector. Although it is impossible to disentangle how much this differential is determined by differences in samples, these strong indirect effects for the shoes industries are clearly consistent with the hypothesis that the support provided to direct beneficiaries to overcome the costs of exploring new markets and adapting the production to the demand of foreign markets has eventually also benefitted other cluster producers who did not actively participate in the APL policy.

7 Robustness checks

To properly validate the credibility of our identification assumption and, therefore, the robustness of our results, we run a pre-treatment trends equality test which assesses whether the pre-intervention time trends of beneficiaries and non-beneficiaries are different (e.g., Galiani et al. 2005; Castillo et al. 2014; and, Arráiz et al. 2014). Only using the observations of beneficiaries and non-beneficiaries in the pre-treatment period, i.e., 2002–2003, the following regression is run:

$$Y_{i,j,m,s,t} = \alpha_i + \alpha_t + \alpha_{j,s,t} + \beta \cdot Cs_{i2003} + \gamma_1 \cdot X_{i,t} + \gamma_2 \cdot X_{m,t} + \gamma_3 \cdot X_{j,m,t} + \varepsilon_{m,t} + \varepsilon_{i,j,m,s,t} \quad (2)$$

where Cs_{i2003} is a dummy variable for future participation in APL policy, taking the value of one if the firm entry the program after 2003.[31] As discussed by Heckman and Hotz (1989), 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 validity of the assumption of equal trend in the absence of the treatment. 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.

The validity of our previous findings is confirmed by these robustness checks. Table 13 shows that there are no significant differences in the pre-treatment trends of the outcome variables among the groups of firms compared in the analysis.[32]

These results support the assumption that the average outcomes of the beneficiary firms and the reweighted control groups would have followed a similar pattern—moving in tandem—in the post-intervention period in the absence of the APL policy.

8 Final remarks

This paper presents what to our knowledge is the first evaluation of the direct and spillover effects of a CDP in Latin America. Using firm-level administrative data from 2002 to 2009, we provide evidence on the impact of Brazil's APL policy on SME's employment and exports for the states of Minas Gerais and Sao Paulo.

[31] Cs_i_2002 is omitted in Eq. (2) because of perfect collinearity.

[32] Additional evidence of the validity of this assumption is also provided by the graphs and tables in appendix 1 and 2, which show that treated and the reweighted comparison groups are very similar both in levels and trends of observed characteristics in the pre-treatment period.

Table 13 Pre-treatment trends equality test

	Direct versus control	Indirect versus control	Direct versus indirect
Lemployment			
Cs_2003	0.0050	0.0008	0.0155
	(0.027)	(0.004)	(0.014)
Lexports			
Cs_2003	0.0639	−0.0427	−0.0070
	(0.566)	(0.060)	(0.212)
Dexport			
Cs_2003	0.0098	−0.0046	−0.0004
	(0.053)	(0.006)	(0.020)
Observations	48,632	69,108	22,096
Number of firms	24,316	34,554	11,048
Number of municipalities	1,017	1,021	68
Firm fixed effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
Industry-state-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

(a) Fixed-effects estimates on reweighted sample; (b) "Cs_year" is the interaction between the treatment variable and the respective pre-treatment year; "lemployment" is the log of employment, "lexports" is the log of total exports, and "dexport" is a dummy variable that takes the value 1 if the firm exports and value 0 otherwise; (c) robust standard errors clustered at the municipality level in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The general assessment of the APL policy seems to be positive and with relevant policy implications.

First, the paper shows that direct beneficiaries clearly benefited from the APL policy. We find positive direct average effects of the policy on employment, value of total exports, and the likelihood of exporting with a constant or increasing pattern over time. Given the small and only temporary negative effect on employment of indirect beneficiaries, these findings also point to an overall positive effect on local economic development. Second, the paper presents evidence of positive spillovers through geographical proximity within an industry on both export outcomes—total exports and the probability of exporting—in the medium and long term. Third, the analysis of the heterogeneity of effects shows clear evidence of consistent patterns through different criteria of disaggregation of our sample (industry, location, and size) and confirms the positive conclusions based on the analysis on the entire sample. Overall, these findings point to the relevance and effectiveness of CDP activities aimed at fostering firms' efficiency, promoting coordination among firms, and supporting them in increasing their presence in more competitive international markets. They also further highlight the importance of correctly accounting the timing and gestation periods of CDP to allow for their direct and indirect effects to materialize.

This study is a first step toward a better understanding of the impacts of CDP on developing countries. Further research is required to fully understand and rationalize the impacts of CDP and to explore its mechanisms in depth. The main limitations of this study are related to data availability. For instance, indirect beneficiaries could alternatively be defined as non-treated firms that hired workers employed in treated firms, i.e., labor mobility would be the channel for spillovers, as in Maliranta et al. (2009). Finally, the analysis of spillover effects could be extended by using geospatial data on firm location. These kind of data would not only allow a more precise definition of agglomeration and spillover effects but also make possible the analysis of how indirect effects vary with the distance to direct beneficiaries.

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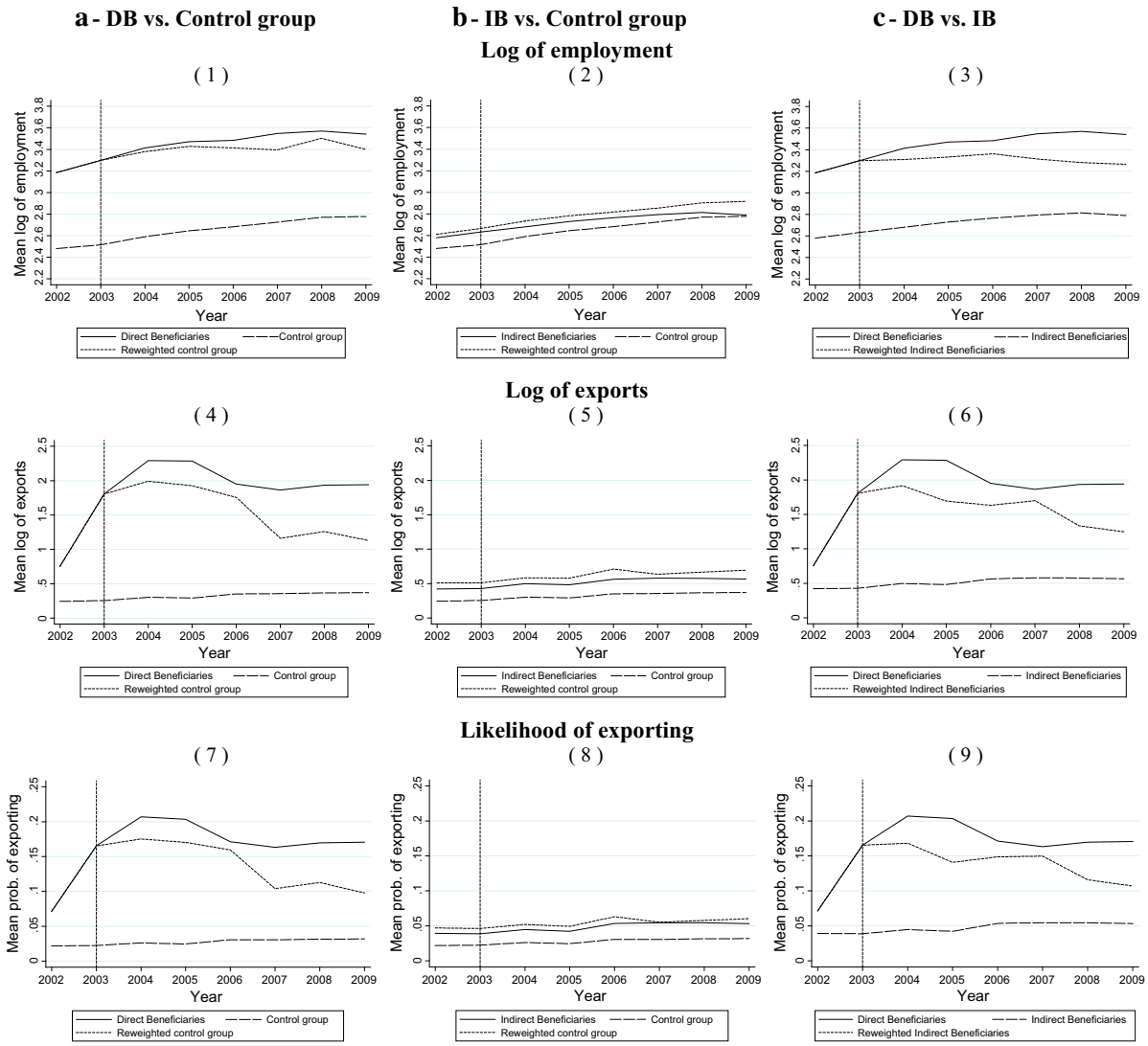
Appendix 1: Mean outcomes over time



** "DB" is direct beneficiaries and "IB" is indirect beneficiaries*

Appendix 2: Mean comparison tests (before and after reweighting)—2003

See Tables 14, 15, 16.

Table 14 Direct versus control group

Variable	Mean of DB	Mean of control group	
	Sample	Sample	Reweighted sample
Log of employment	3.30	2.52***	3.30
Log of employment 2002	3.19	2.48***	3.19
Log of exports	1.81	0.26***	1.81
Log of exports 2002	0.76	0.24***	0.76
Likelihood of exporting	0.17	0.02***	0.17
Likelihood of exporting 2002	0.07	0.02***	0.07
Clothing	0.03	0.08*	0.03
Leather	0.53	0.01***	0.53
Non-metallic minerals	0.11	0.04	0.11
Machinery and equipment	0.02	0.08***	0.02
Electronics and computer equipment	0.02	0.00	0.02
Furniture	0.25	0.05	0.25
Retail and wholesale	0.03	0.73***	0.03
Small firm	0.91	0.98***	0.91
Medium firm	0.09	0.02***	0.09
Minas Gerais	0.72	0.31***	0.72
Sao Paulo	0.28	0.69***	0.28
Log of age	2.57	2.51	2.57
Log of average years of schooling	2.02	2.24***	2.02
Log of imports of capital goods	0.07	0.10	0.07
Herfindahl index	0.09	0.13	0.09
Number of firms	405	23,911	23,911
Number of municipalities	58	959	959

(a) Mean values reported; (b) "DB" is direct beneficiaries, and "IB" is indirect beneficiaries; (c) robust standard errors clustered at the municipality level; ***, **, and * indicate that the mean difference between the treated and control group is significantly different to zero at a level of 1, 5 or 10 %, respectively

Table 15 Indirect versus control group

Variable	Mean of IB	Mean of control group	
	Sample	Sample	Reweighted sample
Log of employment	2.63	2.52***	2.67
Log of employment 2002	2.58	2.48**	2.61
Log of exports	0.43	0.26	0.51
Log of exports 2002	0.42	0.24	0.51
Likelihood of exporting	0.04	0.02	0.05
Likelihood of exporting 2002	0.04	0.02*	0.05
Clothing	0.03	0.08**	0.03
Leather	0.07	0.01	0.09

Table 15 continued

Variable	Mean of IB	Mean of control group	
	Sample	Sample	Reweighted sample
Non-metallic minerals	0.01	0.04**	0.02
Machinery and equipment	0.00	0.08***	0.01***
Electronics and computer equipment	0.00	0.00***	0.00
Furniture	0.05	0.05	0.06
Retail and wholesale	0.83	0.73	0.78
Small firm	0.97	0.98	0.97
Medium firm	0.03	0.02	0.03
Minas Gerais	0.32	0.31	0.32
Sao Paulo	0.68	0.69	0.68
Log of age	2.56	2.51	2.56
Log of average years of schooling	2.25	2.24***	2.25
Log of imports of capital goods	0.45	0.10**	0.49
Herfindahl index	0.01	0.13***	0.05***
Number of firms	10,643	23,911	23,911
Number of municipalities	62	959	959

(a) Mean values reported; (b) "IB" is indirect beneficiaries; (c) robust standard errors clustered at the municipality level; ***, **, and * indicate that the mean difference between the treated and control group is significantly different to zero at a level of 1, 5 or 10 %, respectively

Table 16 Direct versus indirect

Variable	Mean of DB	Mean of IB	
	Sample	Sample	Reweighted sample
Log of employment	3.30	2.63***	3.30
Log of employment 2002	3.19	2.58***	3.19
Log of exports	1.81	0.43***	1.81
Log of exports 2002	0.76	0.42*	0.76
Likelihood of exporting	0.17	0.04***	0.17
Likelihood of exporting 2002	0.07	0.04*	0.07
Clothing	0.03	0.03	0.03
Leather	0.53	0.07**	0.53
Non-metallic minerals	0.11	0.01	0.11
Machinery and equipment	0.02	0.00*	0.02
Electronics and computer equipment	0.02	0.00	0.02
Furniture	0.25	0.05	0.25
Retail and wholesale	0.03	0.83***	0.03
Small firm	0.91	0.97***	0.91
Medium firm	0.09	0.03***	0.09
Minas Gerais	0.72	0.32	0.72
Sao Paulo	0.28	0.68	0.28
Log of age	2.57	2.56	2.57
Log of average years of schooling	2.02	2.25***	2.02
Log of imports of capital goods	0.07	0.45**	0.07
Herfindahl index	0.09	0.01**	0.09
Number of firms	405	10,643	10,643
Number of municipalities	58	62	62

(a) Mean values reported; (b) "DB" is direct beneficiaries, and "IB" is indirect beneficiaries; (c) robust standard errors clustered at the municipality level; ***, **, and * indicate that the mean difference between the treated and control group is significantly different to zero at a level of 1, 5 or 10 %, respectively

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