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Cluster development policies and firms' performance: evidence from an emerging economy in Latin America

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Abstract

Purpose – Impact evaluations of cluster programs at firm level are still scarce in the literature. The available evidence on the effectiveness of such programs based on rigorous quantitative impact evaluations is mixed. The purpose of this paper is to contribute to the body of literature the evaluates quantitatively the impact of cluster programs in emerging economies on firms' performance. In particular, the authors evaluate the impact of a cluster program in Uruguay on firms' sales and exports.

Design/methodology/approach – The authors use state-of-the-art impact evaluation methods to evaluate the impact of the program. In particular, difference in differences and matching methods

Findings – There is very strong evidence that the program had a positive impact on exports and the propensity to export of firms. However, the evidence of a positive impact on sales is weak. The evidence suggests that the maximum effect of the program can be found in the fourth or fifth year after the intervention.

Originality/value – The contribution of this paper to the literature is fourfold. First, this paper adds to the scarce body of literature evaluating the effects of cluster development programs with state-of-the-art impact evaluation methods. Second, it adds evidence for Latin America, a region that has implement a number of cluster policies (Maffioli *et al.*, 2016) and where, as far as the authors know, there is only one additional paper evaluating rigorously the impacts of them (Figal-Garone *et al.*, 2015). In addition, the authors provide evidence about the timing of the effects after the implementation of a cluster policy, an important issue that is mostly overlooked in the existent literature. Finally, the paper focus its attention on the impacts on exports and the propensity to export of firms, key elements for small open economies in Latin America that are heavily reliant on foreign currency inflows.

Keywords Cluster development policy, Firms' performance, Impact evaluation, Firm-level panel data, Latin America

Paper type Research paper

JEL classification – C23, D22, L25, O12, O54

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1. Introduction

The increasing interest in cluster programs as tools for promoting innovation, competitiveness and growth, particularly in developing countries, has been influenced partly by Michael Porter's work on clusters. Porter (1990) provided evidence that leading export firms belong to successful groups of rival firms within related industries. In his work, the geography had a fundamental role in favoring the processes of technological know-how, innovation and information creation. After three decades of this influential work, hundreds of cluster initiatives have been implemented worldwide (Martin *et al.*, 2011).

An important rationale for public intervention is based on the assumption that coordination failures emerge in the preliminary stages of the development of a cluster and that public support is needed to facilitate interaction and coordination among agents. This implies that the public support must be directed to solve the problems of coordination and strengthen networks and governance of the cluster. In the presence of externalities, the market allocates resources sub-optimally. Cluster development programs (CDPs) aim to promote the benefits of agglomeration economies by creating a set of incentives to mitigate the failures that prevent the development of certain industries in certain geographical areas. This implies, among other things, temporarily subsidizing the provision of public goods or goods that are sector specific (club goods).

Against CDPs, some scholars have argued that there is no market failure to be dealt with and that cluster policies inhibit factor movement toward more productive locations (Brakman and van Marrewijk, 2013). Also, it has been argued that policy-induced cluster creation may generate crowding-out of private initiatives (OECD, 2015).

The debate on the effectiveness of cluster policies is far from closed. Impact evaluations of cluster programs are very scarce in the literature and they are of critical importance to enrich this debate (Cantner *et al.*, 2019). Some characteristics of CDPs could explain the scarcity of rigorous impact evaluations, namely, high complexity, high dimensionality, the time lag of policy effects (Rothgang *et al.*, 2017), the intangibility of outputs and outcomes and the difficulty to isolate and demonstrate causal relations (including spillover effects) (Smith *et al.*, 2016).

We are aware of only a few papers that rigorously analyze the impact of cluster programs at firm level (Audretsch *et al.*, 2018; Li and Geng, 2012; Figal-Garone *et al.*, 2015; Martin, Mayer and Mayneris, 2011; Nishimura and Okamuro, 2011; Falck *et al.*, 2010). So far, the evidence on the effectiveness of such programs based on impact evaluations is mixed, and it seems that the devil is in the details, i.e. on the target clusters and the implementation details of the program.

This paper evaluates the impact of a cluster program in Uruguay on firms' performance, in particular on exports (the main goal of the program) and sales, using high quality firm level data and state-of-the-art impact evaluation methods. The Program for the Competitiveness of Clusters and Production Chains (PACC for its Spanish acronym) was created in 2005 with the aim to contribute to the development and the competitiveness of clusters and supply chains. As its inception the PACC has reached 21 clusters. A particular characteristic of this program is that the geographical dimension of clusters was not a core feature in the selection of clusters to be supported; in many cases the value chain was the target. This can be explained by the relative small size of the economy together with a highly concentrated industrial area.

The program was divided into three main components, namely, a strategic plan, matching grants for different projects and strengthening of the supporting institutions of the cluster. The program gave financial support for different initiatives, including research and development (R&D), quality management and commercialization, etc. However, most of them have the objective to increase exports.

Given its characteristics (Section 3), the program could have had positive impacts on cluster firms' outcomes through four main mechanisms as follows:

- coordination of private and public actors to improve exchange of information and networking;
- coordination of agents to generate strategic club assets;
- coordination of public agents to improve policy actions; and
- funding for projects within the strategic vision of the clusters.

The complexity and simultaneity of these mechanisms limits our capacity to disentangle the effects of each of these channels. Therefore, our empirical analysis will focus on the aggregate impact of the CDP.

The contribution of this paper to the literature is fourfold. First, this paper adds to the scarce body of literature evaluating the effects of CDPs with state-of-the-art impact evaluation methods. Second, it adds evidence for Latin America, a region that has implement a number of cluster policies (Maffioli *et al.*, 2016) and where, as far as we know, there is only one additional paper evaluating rigorously the impacts of them (Figal-Garone *et al.*, 2015). In addition, we provide evidence about the timing of the effects after the implementation of a cluster policy, an important issue that is mostly overlooked in the existent literature. Finally, the paper focus its attention on the impacts on exports and the propensity to export of firms, key elements for small open economies in Latin America that are heavily reliant on foreign currency inflows.

In what follows, in Section 2, we present a brief literature review. Section 3 discusses the main characteristics of the program and the implicit theory of change. Section 4 describes the data and the empirical methodology. Section 5 presents the results of the empirical analysis. Finally, Section 6 concludes.

2. Literature

Policies to promote the development of productive clusters are justified in the presence of economies of agglomeration and coordination failures. Agglomeration economies are the result of specific positive externalities of industry and business location (Arrow, 1962; Romer, 1986; Glaeser *et al.*, 1992). In this context, as noted by Rosenstein-Rodan (1943), investment decisions are interrelated and investment in a company can have a positive effect on the profitability of the investment in another company.

Schmitz (1995) defines the concept of collective efficiency to discuss the positive impacts of factors related to the competitiveness of enterprises in industrial concentrations. Collective efficiency is defined as the comparative advantage from external economies and local joint actions. The cluster presents opportunities for significant external economies. Hence, the analysis of industrial concentration is focused on the role of vertical and horizontal relationships that generate external economies and joint actions within clusters and improve performance. Therefore, a significant part of the gain in competitiveness of firms results from interactions between companies and between companies and cluster institutions (Humphrey and Schmitz, 2002).

The knowledge spillover theory justifies clusters (within the knowledge-based economies and industries) using similar arguments (Lehmann and Menter, 2017). The intangible and tacit knowledge flows through personal contacts – thus, geographic proximity and agglomeration matters (Acs *et al.*, 2013; Audretsch *et al.*, 2015).

Even though clusters policies have been usually linked to what Audretsch (2015) calls "the strategic management of places," they are not always restricted to location. In fact, as Cantner *et al.* (2019) points out, geographical proximity is not always a precondition for innovation outcomes, so cluster policies should not be overly restrictive with respect to the location of participants.

Papers evaluating clusters and cluster policies tended to fall into two camps, namely, qualitative case studies highlighting the relevance of contextual elements or quantitative evaluations seeking specific "hard" outcomes. The first approach fails to rely on causal inference and, therefore, their findings are hardly generalizable (Figal – Garone *et al.*, 2015). The second approach has also be subject to criticism and usually they have been difficult to implement in a rigorous way.

As a consecuence, rigorous impact evaluations of CDPs are scarce (OECD, 2015). Also, findings are usually not conclusive. The reason could be related to some extent to the fact that evaluation studies are performed before the actual economic impacts can be observed (a timing problem). In addition, the complexity of impact patterns poses a problem to identify policy effects ex post (Rothgang *et al.*, 2017).

To our knowledge only one rigorous quantitative analysis on the impacts of cluster policy has been applied to Latin America. Figal – Garone *et al.* (2015) study the impact of a Brazilian CDP on small and medium firms' exports and employment. They find evidence of a positive direct effect of the program on employment growth, the value of exports and the likelihood of exporting. They also find different effects in the short and medium and long term.

A difficulty that arises in evaluation of CDP is the possibility to separate direct effects of the program (on enterprises targeted) from indirect effects. Figal – Garone *et al.* (2015) try to separate both direct and indirect effects. While direct effects were positive as mentioned before, they find negative spillover effects on employment in the short term but none on the medium to long term and positive spillovers on export outcomes in the medium and long term. On the other hand, Audretsch *et al.* (2018) concentrates exclusively on the spillover effects of this cluster policy initiative on those firms and industries, which although are part of the cluser are not subsidized by it. They find a negative effect on those firms, suggesting a stealing market effect by part of the target firms. In conclusion the effect of cluster policy programs on enterprises finds conflicting evidence in previous empiric studies and additional research is needed to support any conclusion.

A few additional impact evaluation studies are available for European cluster programs. Martin, Mayer and Mayneris (2011) analyze the impact of a French CDP on firms' employment, exports and total factor productivity. Using a fixed effects regression and difference-in-differences with matching they conclude that the program did not have a robust impact on firms' performance variables. They associate these findings to the fact that for political reasons the program directed the funding to sectors or regions that were in decline.

In the opposite side a German quantitative evaluation study analyzed the impact of CDP on a cluster of high-tech industries (Falck *et al.*, 2010). The outcome variables were R&D spending, patents and innovation. Using a triple difference strategy they find weak positive effects of the program on the propensity to innovate, positive effects on the propensity of patenting and a negative effect on R&D spending.

The PACC can be placed between previously mentioned German and French programs in terms of the targeted industries. In fact, some of the clusters treated by PACC belonged to declining sectors (Footwear and Clothing) affected by growing foreign competition. Most of the other not only supported clusters belonged to dynamic sectors that were growing but also needed the foreign market to expand given the size of the internal market. So in this sense our search for effects is more closely related to the French study.

Most quantitative studies use official survey data, which is merged with administrative data from the program. As an exception, Nishimura and Okamuro (2011) use an original questionnaire sent to beneficiaries of an industrial cluster program for small and medium enterprises in Japan to evaluate the impact of the program. Given that the objective of the program was promoting local network for innovation on R&D productivity, they also use patent data [1].

Our empirical approach is different from all cited papers. Our control group comes from the internal revenue agency of Uruguay. This database includes the complete universe of formal enterprises.

3. The intervention and the causal mechanisms

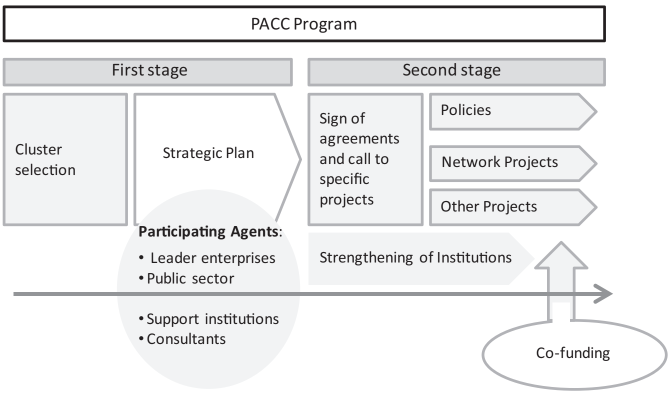
3.1 The intervention

CDPs generally have as a first step the generation of incentives for the development of strategic plans to solve coordination problems and increase competitiveness of the cluster. These plans allow for an improved business environment, organization of the supply of business support services and investments in basic common infrastructure (these actions can be associated with the emergence of a "perfect cluster" according to Smith *et al.*, 2016). In a second step a series of investments, including human capital, R&D and other innovation-related investments, are carried out to improve the productivity and competitiveness of firms in the cluster (the growth of a perfect cluster, in the cited work). Usually, the level of co-financing of these actions is negatively related to the private appropriability of the benefits of such actions or investments.

The PACC program in Uruguay had two main stages (Figure 1) as follows:

- cluster selection and preparation of competitiveness strengthening plans; and
- execution of projects and actions to strengthen public and private supporting institutions.

The process starts with a call for clusters, spread among interested agents through public agencies and communication channels. Following this call, firms gathered around a sectoral



Source: PACC (2009)

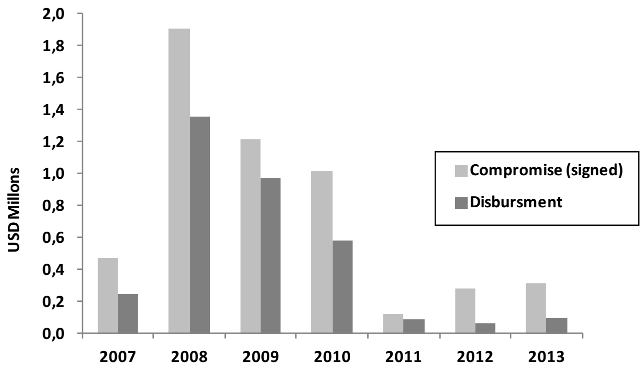
Figure 1. PACC's support model

chamber or association and together with a government agency (ministry or local government) submit applications. After a cluster is selected, its members should develop a strategic plan. The strategic plan contains the proposal of specific projects that are co-funded by the public sector according to the level of appropriability of the outcomes by individual firms vs the cluster. Those projects with high appropriability for only a limited number of firms in the cluster receive a lower percentage of public funds in comparison with those that have an impact on the entire cluster. Simultaneously, there are initiatives directed to strengthening public and private supporting institutions.

The PACC program started in 2005, but the first disbursement for projects was made in the year 2007. Even though the program ended in the year 2014; most of the disbursement was made in the period 2008-2010 as can be seen in Figure 2. These projects had a wide scope, namely, technical assistance, training, procurement of machinery and equipment for collective use, promotion of good manufacturing practices, environmental management, cleaner production, waste management, occupational health, actions directed to attraction of direct investment identified as critical in the strategic plan, development of collective trademarks, reorientation of training supply, facilitation of certification processes, market intelligence and access, development of distribution channels, technical assistance on quality-related topics, etc.

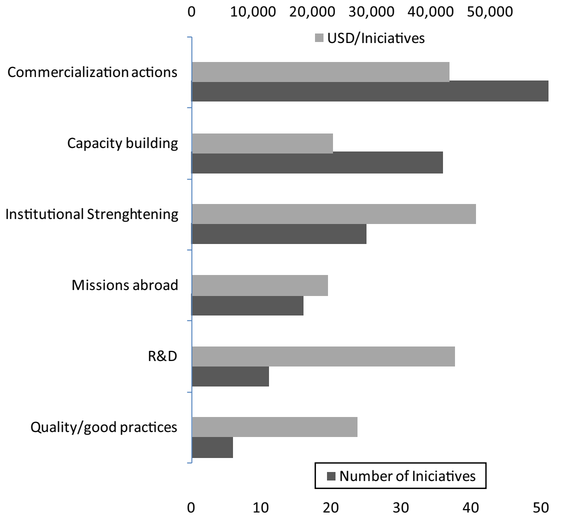
Figure 3 shows the projects grouped under six broad types of initiatives. As can be seen, commercialization actions were predominant, with almost 60 actions. These initiatives had a cost, on average, of $40,000 per initiative. Capacity building initiatives were second in terms of frequency, with nearly 40 initiatives up to June 2013. The average cost of each was $20,000. Institutional strengthening projects were third in terms of frequency, with more than 20 initiatives and having been on average the most budget demanding ones ($48,000 per initiative). Other projects included missions abroad, R&D and quality enhancement actions.

The program also invested resources on the strengthening of execution capabilities of business support institutions, including supervisory and monitoring actions and



Notes: We exclude the following clusters: tourism in Montevideo, tourism in Rocha, woods, automotive, oleaginous, editorial, music (all these cluster were in a very preliminary stage at the end of the program, and most of them had no project under execution). Last data available: June 2013
Source: PACC's administrative records

Figure 2.
PACC's co-financing compromises and disbursement



Source: PACC's administrative records

Figure 3.
Initiatives co-financed by PACC and average cost by type of initiative (in US dollars)

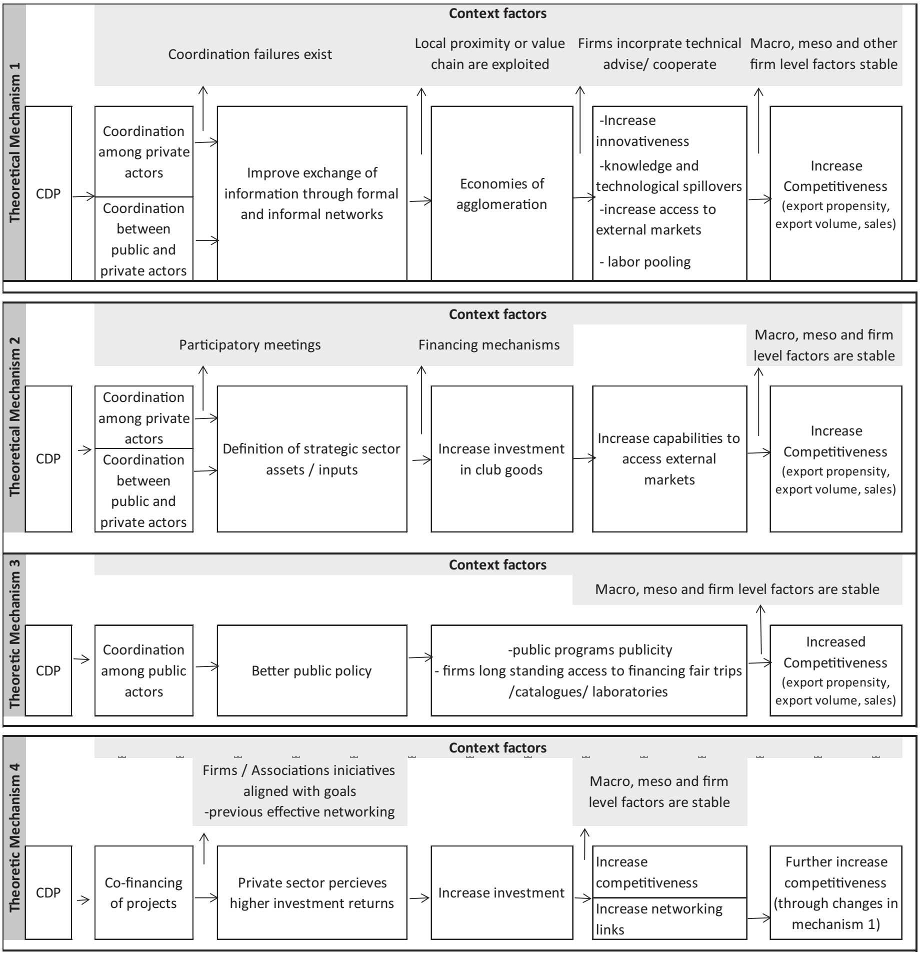
coordination workshops in which officials, consultants and businessmen discussed relevant topics related to the program's impacts. Also the program provided financing for training activities, consulting services and technical assistance for ministries and organizations aiming to improve their capacities to implement the support policies and to coordinate such activities with the PACC, among other objectives.

3.2 Impact mechanisms

Given the complexity of the PACC program, for analytical purposes, we separate the causal effect into four different mechanisms. Figure 4 shows the four different causal mechanisms operating as consequence of the program. However, it should be kept in mind that there are feedbacks from one to another and that it is not possible, empirically, to evaluate the impact of each of them separately with the available data.

A first type of intervention of the PACC was directed to increasing coordination among private agents, namely, generating cluster specific institutions. The underlying assumption was the existence of coordination failures along some value chains and in some regional clusters. This is the typical justification for cluster policies. In the presence of agglomeration economies (in regions or as it is more often in our case, in value chains) the facilitation of coordination and the spillover of information among firms in the cluster should help to internalize the external economies related to knowledge spillovers, labor pooling and other input/output externalities; this, in turn, should have an impact on the productivity of firms affected by the intervention, and therefore, on their "competitiveness" (Marshall, 1920), increasing exports and sales. Close to this idea, Li and Geng (2012) found evidence on the relevance of shared resources on cluster firm's performance.

A second type of initiative was directed to coordinate investment in club goods. The coordination among all relevant agents in a cluster with specific purposes can lead to investment in strategic assets for the club. For this causality to have a positive effect in the upcoming stages of this mechanism, the persistent participation of a critical mass of



Source: Own elaboration

Figure 4.
Theoretical mechanisms operating under PACC

interested agents (firms, public institutions, R&D centers) is needed. In fact, one way in which cluster policies can lead to important effects on the development of clusters and cluster organizations in the short- to medium term is through the development of a cooperation structure between cluster actors and to jointly define specific patterns of increase in R&D activities (Engel *et al.*, 2019; Töpfer *et al.*, 2017).

The relevance of firms' self-perception of being part of a cluster and of being capable of accessing the club goods or more generally, shared resources within it, have been documented to have positive effects on cluster performance at firm level (Li and Geng, 2012). Therefore, we expect that this second channel will impact also on sales and exports.

In PACC, the coordination of private actors and between these and public actors was facilitated through the definition of a cluster strategic plan. The result of this process was a sector validated document containing the strategic lines of actions for the cluster. The consensual definition of strategic lines for the cluster, at least at a theoretic level should help to build consciousness on the benefits of cluster-level investments, even in those cases where the appropriability of the action is very low at the individual level (i.e. it should ease the creation of club goods). Given that most common club goods generated by the program were directed to the objective of facilitating access to external markets (e.g. participation in fair trips), we expect this channel to have worked mostly through the impact on easing the access to external markets, and therefore increasing exporting opportunities.

The third mechanism is coordination between public actors. According to the program's records, 4% of the financial resources of the program were allocated to upgrade the coordination among public agents and to generate better public policies to promote the development of clusters. If coordination of public institutions is achieved and this is conducive to better public policies, we expect this to have a positive effect on cluster's competitiveness, and therefore, on sales and exports of firms. It is probable that this mechanism did not work as expected. Pietrobelli (2019) points out, after a revision of some CDPs in Latin America, that this type of coordination is the most difficult of three types (private-private, private-public and public-public). In his view, differences over mandates, bureaucratic processes, strategic view and short-term political considerations among public agents trumped the collaboration opportunities.

Co-financing is the fourth and final theoretical mechanism identified in the case of PACC. The largest share of PACC resources was directed to co-finance projects that resulted from the strategic plans (approximately 80% of funds). These funds could be used to purchase machinery and equipment, for the installation of technology centers, capacity building, traveling or any other type of investment identified as a priority for the cluster. This funding is directed not only to generate club goods but also in some cases private goods. In theory, given that this funding was subsidized by the public sector it should increase private investment's returns on both private and club goods and also lift some credit restrictions, where both channels lead to increased total investment. This, in turn, should lead to an increase in productivity and competitiveness and should be reflected in higher sales and exports. Given that most of these investments were on club goods it also served as a way of strengthening and increase network links and this could have an additional impact on the competitiveness of the cluster through the first theoretical mechanism.

A key element of all these mechanisms is the presence of spillovers, i.e. firms that do not participate in the program but because of the linkages they have with direct beneficiaries, may indirectly benefit from the program. It is likely that the firms that do not participate in the cluster program but belong to the cluster sector, have geographical proximity or have linkages with the value chain to which the cluster belongs, are indirect beneficiaries of the main CDP mechanisms.

Addressing the spillovers issue pose a challenge in evaluating a CDP. As noted in Maffioli, Pietrobelli and Stucchi (2016) addressing this question requires additional steps beyond a standard impact evaluation. It is necessary to define and identify direct and indirect beneficiary firms. Comparing of both with a pure control group allows identification of two causal relationships of interest. First, by comparing direct beneficiaries and similar non-beneficiaries from a pure control group provides the direct impact of the program. A second comparison, between the indirect beneficiaries and similar non-beneficiaries, would identify the indirect impact of the program (i.e. the impact due to externalities).

Unfortunately, the available data does not allow a clear identification of these groups of direct and indirect beneficiaries and does not allow identifying the specific impact of each individual mechanism described above.

4. Data and empirical strategy

4.1 Data

We have three sources of information as follows:

- administrative program information containing a list of participating companies and clusters and the number and the starting date of projects in which each firm participated;
- firm level information on annual operating income (sales) for the period 2005-2012 from the internal revenue agency of Uruguay [Dirección General Impositiva (DGI), for its Spanish acronym); and
- firm level information on annual exports of goods for the period 2004-2014, obtained from an exports database made available by Uruguay XXI Institute.

The database that we will use in this paper was constructed for the specific purpose of evaluating the program. The list of participant firms refers to companies that belong to any of the 14 participating clusters that at some point up to 2012 were involved in some program's activities (e.g. participation in co-financing investment projects) and could be identified from records of such activities. These firms, which we have identified as direct beneficiaries of the program, constitute the *treatment group*.

The main limitation of the available information relates to unidentified participating companies. That is, firms that are not registered in the program database (e.g. because administrative records were incomplete) and firms that are registered, but without taxpayer identifier number and that, therefore, cannot be identified in the export and sales databases. This primarily affects the representativeness of the treatment group and secondly it can potentially contaminate the control group. The type of bias that this could cause is unknown. We will discard some clusters to limit this problem.

From a total of 725 firms that could be identified as treated firms, it was possible to assign taxpayer identifier number to 43% of them. The problem of information is different for different clusters. As shown in Table 1, the lack of information at firm level is fairly widespread in the two largest clusters (in terms of number of participating firms), namely, Apiculture and Tourism in Colonia. Both clusters were excluded from our sample. After excluding these two sectors, 71% of the treated firms could be identified by their taxpayer identifier number.

The sales database has information on annual sales turnover from 2005 to 2012 for all Uruguayan firms belonging to the sectors of the analyzed clusters. These sectors were defined based on the ISIC (Revision 4) and comprise the typical cluster activities [2]. Sale turnover is the total value (in Uruguayan pesos at current prices) of product or services sold (value added tax included) over a year and generated from daily operation of the firm.

In our preferred estimations we restrict the sample to those companies (participants and non-participants) with positive sales for every year between 2005 and 2012. There are a number of firms with zero sales in some years of this period but it is not possible to identify the causes of such records, therefore we prefer to exclude these firms from our sample. With this selection criterion we have a total of 111 participants (treated) and 2,256 non-participant (control) firms in the sales database (Table 2). However, we will also show results for the full sample including firms with zero sales in at least one year in the period (yielding a sample of 244 participants and 8,736 non-participants).

The exports database contains yearly information at firm level (identified by taxpayer identifier number, in US dollars at current prices) of exports by products (at six digit of the Mercosur Common Nomenclature, NCM) for all Uruguayan firms for the period 2004-2014.

Cluster	Firms identified as participants	Participants with taxpayer identifier number	Percentage with taxpayer identifier number
Life sciences	8	8	100
Software	25	25	100
Naval	11	10	91
Clothing	30	27	90
Gemstones	9	8	89
Design	53	45	85
Food	29	24	83
Blueberries	42	26	62
Audiovisual	63	37	59
Footware and leather goods	57	32	56
Olives	9	5	56
Viticulture	31	12	39
Apiculture	220	48	22
Tourism in Colonia	138	3	2
Total	725	310	43

Source: Based on information provided by the PACC

Table 1. Number of firms according to PACC's records and percentage of taxpayer identifier numbers identified by cluster

Cluster	All firms			Restricted sample (positive sales in all years)		
	Treated	Control	Total	Treated	Control	Total
Food	24	3,464	3,488	23	1,091	1,114
Blueberries	24	13	37	9	2	11
Audiovisual	35	1,084	1,119	8	246	254
Footwear and leather goods	28	125	153	17	50	67
Life sciences	7	29	36	6	5	11
Design	41	171	212	10	25	35
Naval	10	285	295	4	104	108
Olives	6	2	8	0	0	0
Gemstones	7	82	89	2	16	18
Software	24	1,707	1,731	8	234	242
Clothing	26	1,537	1,563	14	365	379
Viticulture	12	237	249	10	118	128
Total	244	8,736	8,980	111	2,256	2,367

Source: Based on information provided by DGI

Table 2. Number of firms in the sample for the assessment of impact on sales

This database has information on exports of goods, and therefore, those clusters that are services providers were excluded from the analysis (i.e. software, audiovisual and design). The Naval cluster was excluded because none of the participating companies exported in the period under analysis. After excluding these clusters we are left with 142 participating firms in this database. Of these, 104 exported in at least one year in the period 2004-2014 (38 never exported) and 70 exported in the year previous to the intervention.

To apply a first filter (i.e. a first matching criterion) on non-participating firms we identified groups of typical exportable goods for each cluster and then the control group was defined as those companies that in any year of the period 2004-2014 exported any of these products. Table A1 in the Appendix shows the typical export goods of the clusters, based on their NCM codes. From a total of 7,373 non-participating firms we selected 1,668 firms that had positive exports in at least one of these characteristics goods in at least one year in the period of analysis.

Table 3 shows the number of participating (treated) and non-participating (control) firms that are included in the impact analysis on exports. We will perform the impact evaluation using three alternative samples. The first sample includes all participating and non-participating companies, including the 38 participant firms that never exported in period. The second sample only includes companies that have exported at least in one year in the period (i.e. we will be excluding the 38 participant firms that never exported). Finally, the third sample will only include participants and non-participants that exported in the year before the intervention.

The treatment status of a participating firm is defined by the treatment status of its cluster in the program. It was assumed that the start of treatment for a cluster (and all participating firms) is the year when the first project in the cluster began. Table 4 shows the status of the treatment for each cluster by year. We also indicate if the cluster was included in the impact evaluation or not (as explained above some clusters were excluded for a number of reasons). From the 14 participating clusters that we have information on (Table 1), 12 were included in the analysis of sales (2 were excluded because of lack of identifier codes for a high proportion of firms) and 8 were included in the analysis of goods exports (3 are services clusters and 1 did not export in the entire period). The number of pre and post intervention years varies by industry and also according to the database analyzed.

4.2 Empirical strategy

The identification of the impact of PACC on the performance of firms will be based on the assumption that participation in the program depends on both observable characteristics of firms and persistent unobserved factors over time. Under these assumptions the average effect of the program can be identified by a difference-in-differences (DID) regression, i.e. estimating the following fixed-effect equation for the outcome variable Y_{it}: [3]

Table 3. Number of participating and non-participating firms with taxpayer identifier number in the export database

Cluster	All firms		Firms that exported in at least one year btw. 2004-2014		Firms that exported the year before the intervention	
	Treated	Control	Treated	Control	Treated	Control
Blueberries	26	25	17	25	6	8
Life sciences	8	320	8	320	5	135
Olives	5	12	4	12	1	5
Gemstones	8	81	4	81	2	28
Clothing	27	455	17	455	14	311
Footwear and leather Goods	32		20		12	
Food	24	775	22	775	18	232
Viticulture	12		12		12	
Total	142	1,668	104	1,668	70	719

Source: Based on information provided by the PACC and Uruguay XXI Institute

	Time period covered by DGI database (sales)											Included in the analysis of:	
Cluster	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	Sales	Export
Food	0	0	0	0	0	1	1	1	1	1	1	Yes	Yes
Blueberries	0	0	0	1	1	1	1	1	1	1	1	Yes	Yes
Audiovisual	0	0	0	0	1	1	1	1	1	1	1	Yes	No
Foothware and l eather goods	0	0	0	1	1	1	1	1	1	1	1	Yes	Yes
Life sciences	0	0	0	0	0	0	1	1	1	1	1	Yes	Yes
Design	0	0	0	0	0	1	1	1	1	1	1	Yes	No
Naval	0	0	0	0	0	0	1	1	1	1	1	Yes	No
Olives	0	0	0	0	0	0	0	0	0	1	1	Yes*	Yes
Gemstones	0	0	0	1	1	1	1	1	1	1	1	Yes	Yes
Software	0	0	0	0	1	1	1	1	1	1	1	Yes	No
Clothing	0	0	0	1	1	1	1	1	1	1	1	Yes	Yes
Viticulture	0	0	0	0	0	1	1	1	1	1	1	Yes	Yes
	Time period covered by export database												

Notes: *All firms are excluded if we restrict the sample to those with positive sales every year between 2005 and 2012. The number 0 or 1 in the table indicates the status of the treatment for each cluster: = 1 on and after the year when at least 1 project is executed under the PACC and = 0 otherwise

Table 4. Clusters included in the impact analysis of sales and export, time period covered and treatment status by cluster

$$Y_{it} = \beta D_{it} + \gamma X_{it} + \delta_t + u_i + e_{it} \tag{1}$$

where D_{it} is 1 when the firm is a beneficiary of the program and 0 otherwise, X_{it} is a vector of control variables not affected by the program, δ_t is a time effect that affects all companies equally, u_i is the unobserved heterogeneity correlated with the other observed regressors (particularly D_{it}) and e_{it} is an error independent of the remaining regressors. Note that this specification allows for the inclusion of specific time trends by sector (dummies resulting from the interaction of time dummies and sectoral dummies).

In the DID, the key assumption for β to be a consistent estimator of the average treatment effect is that the trend in the outcome variable in the absence of treatment is the same for firms in the treatment group (participants) and the control group (non-participants). While it is not possible to test the validity of this assumption, it is possible to test the existence of parallel trends before treatment in outcome variables.

The finding of different trends before treatment, which is equivalent to the significance of a placebo experiment, invalidates the application of the DID method, at least on the full sample. An alternative in this case is to restrict (or re-weight) the control group, matching treatments and controls based on observable pre-treatment variables.

To reinforce the validity of our identification assumption, we estimate equation (1) on a matched sample, selecting among firms in the control group that are more similar to participants in terms of pre-treatment variables. In particular, we apply the Nearest Neighbor Matching algorithm based on the Propensity Score within each sector. That is, for every individual in the treatment group a matching individual sharing similar observables characteristics (i.e. covariates) is found from among the non-treatment group. These covariates are variables not affected by the treatment, in particular we used the lagged level and growth of the outcome variable [4]. We perform estimates with one and five nearest neighbors with replacement [5].

We also test the robustness of our estimates by using entropy balancing, a multivariate reweighting method proposed by Hainmueller (2012), to estimate the impact of the program. This method allows us to reweight our full sample in such a way that the control group matches the covariate moments of the treatment group. The estimations presented below are based on balancing the mean of the pre-treatment variables within each sector, this mean that the treatment and reweighted control group match exactly on mean of these variables.

We use the same variables in the matching (i.e. the propensity score) and in the reweighting method. In general, these variables are transformations of the outcome variables in the pre-intervention period [6]. For sales we use the following two variables:

(1) log of total sales in the year before treatment; and

(2) average growth of total sales before treatment.

Meanwhile, when analyzing export data we use the following ones:

- log of total export in the year before treatment;
- log of total exports to Mercosur (a set of countries close to Uruguay) in the year before treatment;
- log of exports of the typical cluster good in the year before treatment; and
- average growth of total exports before treatment [7].

Our treatment group consists of those firms that participated in any of the CDP activities (in general, those that participated in investment projects within the strategic definitions of the cluster). Therefore, our treatment group is what we have identified as direct beneficiaries, i.e. firms that choose to actively participate in the activities included in the CDP.

With the objective of building a credible counterfactual, the control group includes those firms that in the administrative records of sales and export are identified as belonging to the same sector or exporting the same products as the direct beneficiaries but have not been identified as beneficiaries in PACC's administrative records. Therefore, it is likely that within this group we have firms that may be indirectly affected by the program and others that could theoretically be categorized in the pure control group.

The information available makes possible to identify the average global effect (the joint effect of all possible impact channels) of the program. However, it is not possible to disentangle the individual effect of each of the impact mechanisms described in Section 3. Addressing this issue is a challenge for this type of programs given the interrelation and feedback between these causal mechanisms.

Given our empirical strategy and in particular the definition of the treatment and control groups, it is expected that the estimated effect captures mostly the impact of the investment in co-finance projects carried out within the framework of the program and that have greater appropriability by participating firms. This is so, as this type of effect is surely present in the treatment group (the participants in these projects) and not in the control group. On the contrary, the impact mechanisms that can affect in a generalized way all the firms of the sector (both direct participants and non-participants) are more likely to be neutralized or attenuated when comparing treatment and control groups. For example, the impact of the better public policies resulting from the greater coordination of public agencies (the third mechanism), is likely to be underestimated given our empirical strategy.

To analyze the timing of the effects we use the following specification:

$$Y_{it} = \beta_1 D_{1it} + \beta_2 D_{2it} + . + \beta_k D_{kit} + \gamma X_{it} + \delta_t + u_i + e_{it} \quad (2)$$

where D_k takes the value 1 if the firm received the intervention k years ago (from the past year in the database), 0 otherwise. Therefore, β_i is the accumulated effect of the program i years after the intervention.

Finally, to address the validity of the control group, and therefore, the robustness of our estimates we assesses whether the pre-intervention time trends for participants and non-participant are different using the following equation:

$$Y_{it} = \varphi_j D^j_{it} + \beta D_{it} + \gamma X_{it} + \delta_t + u_i + e_{it} \quad (3)$$

where D^j take the value 1 for treated firms during the j-years before the intervention and 0 otherwise. Our data allow us to identify more than one trend break before the intervention. In the estimates presented in the Appendix we assesses whether the outcome variable present different trends one and two years before the intervention (i.e. $m = 1$ and m = 2). Under the null hypothesis of common trends all the coefficients φ must be statistically equal to zero. This is the condition that must be verified to validate our fixed-effects identification strategy.

We also perform mean tests on the matching variables for intervention and control groups before and after the matching (or reweighting) to give some evidence of the quality of the matching (or reweighting) (they are also shown in the Appendix).

5. Results

5.1 Sales

Table 5 presents an informal test to validate the identification assumption. We show that the hypothesis of parallel trends of sales turnover between treatment and control group before the intervention cannot be rejected. This support the assumption that the trend of the outcome variable in the absence of treatment it would be quite similar for participants and non-participants firms (Figure A1). As some of the estimates are made in matched samples of firms or, alternatively, reweighting the firms of the control group, Table A2 of the

	Sample 1: Firms with positive sales in all periods			
		Matched sample (nearest neighbor)		
	Full sample (1)	1 neighbor (2)	5 neighbors (3)	Reweighted sample (4)
Treatment since one year before the PACC	0.024 (0.049)	0.027 (0.036)	0.002 (0.033)	0.048 (0.033)
Treatment since two years before the PACC	0.147** (0.065)	−0.038 (0.144)	0.044 (0.060)	0.025 (0.080)
Observations	18,936	1,504	3,064	18,936
R^2	0.315	0.302	0.268	0.278
Number of id	2,367	188	383	2,367
Standard error	0.382	0.444	0.434	0.44
Fixed effects	YES	YES	YES	YES
Time fixed effects	YES	YES	YES	YES
Industry trends	YES	YES	YES	YES

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 5. Pre-treatment trends equality test on (log of) sales

Appendix reports the test of equality of mean between treatment and control group in each of these samples.

Table 6 reports the estimations of the CDP impact on sales turnover using the sample of firms that have positive sales in the entire period after intervention (both in the treatment and control groups). We do not find any impact of the program on sales in the unrestricted sample (Columns 1 and 2) as well as in matched (Columns 3 to 6) and reweighted samples (Columns 7 and 8).

Now let's look at the evidence of impacts for different time horizons. Rows β_1 to β_6 of the Table 7 show the cumulative effect of the program for years one to six after the intervention [Equation (2)]. We do not find any significant affect at any time length for any of the models. In other words, this lack of effect seems to be very robust with this sample of firms.

When we use the full sample, without excluding firms that have zero sales in at least one of the years of the period, the picture gets blurred (Table A3 in the Appendix). With the exception of the fixed effects models in Columns 1 and 2, in all the other cases we find a significant positive effect on sales at 5% confidence level. There is some evidence that the full effect of the program takes time to materialize, taking the results of the matched and reweighted samples, it could take between two and five years (Table A4). However, in these cases the hypothesis of parallel trends is rejected, and therefore, these results must be taken with grain of salt (Table A5).

Taking together the evidence presented in this subsection, the conclusion should be that there is a very weak and, not consistent across samples, evidence of a positive effect on sales.

5.2 Exports

The picture when we analyze the impact of the program on exports is very different to the one commented upon in the previous subsection. Almost without exception in the many different exercises that we performed, we find a positive effect. In Table 8, we show the results including all the treated firms, in particular, those that never exported either before the intervention or after it. We expect the average treatment effect on treated (β) to be downward biased in these estimations because the control group comes from an exports database, and therefore, by definition, we do not have firms that never exported in the control group. Note that with the exception of the fixed effects regression where we are not controlling for industry trends, in all the other cases the β is significantly different from zero. Moreover the estimations imply a very high impact in exports, from 55% (Column 7) to 12 times higher (Column 4).[8]

In Tables 10, perform the same exercises as in Table 9 but when we only keep in the control and intervention groups those firms that are at least exporting in the year before the intervention. As expected, in general, this increases the effect of the program. The results are very consistent and seem to show a very important impact of the program on exports (Tables 10-11).

Tables A7 and A8 in the appendix reports the pre-treatment trends equality test. We show that the hypothesis of parallel trends of export between treatment and control group before the intervention cannot be rejected (Figure A2). Table A6 reports the test of equality of mean between treatment and control group in each of these samples used to estimate the impact of the program

In the next two tables we replicate the same exercises as in the previous two tables but now we try to find some patterns related to the time of exposure to the treatment. Even though there is some heterogeneity across samples and specifications, in general, we can see

Table 6. Estimation of the average treatment effects on (log of) sales

	Sample 1: Firms with positive sales in all years							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	−0.049 (0.085)	−0.053 (0.109)	0.015 (0.115)	0.018 (0.152)	0.02 (0.085)	−0.009 (0.114)	−0.028 (0.095)	−0.01 (0.097)
Observations	18,936	18,936	1,504	1,504	3,064	3,064	18,936	18,936
R^2	0.292	0.314	0.178	0.302	0.182	0.267	0.193	0.322
Number of id	2,367	2,367	188	188	383	383	2,367	2,367
Standard error	0.388	0.382	0.471	0.444	0.453	0.434	0.474	0.435
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 7.
Estimation of the dynamic average treatment effects on (log of) sales

	Sample 1: Firms with positive sales in all years							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	0.012 (0.056)	−0.018 (0.055)	0.039 (0.089)	−0.053 (0.100)	0.048 (0.067)	−0.031 (0.074)	0.041 (0.068)	−0.027 (0.060)
β_2	−0.047 (0.091)	−0.078 (0.106)	−0.01 (0.123)	−0.098 (0.140)	0.005 (0.091)	−0.079 (0.110)	−0.006 (0.097)	−0.068 (0.099)
β_3	−0.001 (0.094)	−0.033 (0.112)	0.045 (0.128)	−0.023 (0.152)	0.067 (0.096)	−0.018 (0.119)	0.041 (0.079)	−0.004 (0.085)
β_4	−0.029 (0.098)	−0.027 (0.123)	0.012 (0.158)	0.042 (0.166)	0.05 (0.113)	0.035 (0.130)	0.023 (0.111)	0.045 (0.106)
β_5	−0.216 (0.131)	−0.174 (0.169)	−0.182 (0.196)	0.024 (0.213)	−0.143 (0.122)	−0.046 (0.154)	−0.194 (0.161)	−0.011 (0.163)
β_6	−0.349* (0.184)	−0.273 (0.249)	−0.288 (0.236)	0.033 (0.253)	−0.239 (0.171)	−0.063 (0.215)	−0.281 (0.234)	0.003 (0.234)
β_7	18,936	18,936	1,504	1,504	3,07	3,072	18,936	18,936
	0.293	0.311	0.187	0.278	0.188	0.254	0.187	0.254
β_8	2,367	2,367	188	188	384	384	2,367	2,367
	0.388	0.383	0.469	0.443	0.452	0.433	0.466	0.446
Observations	18,936	18,936	1,504	1,504	3,072	3,072	18,936	18,936
R^2	0.293	0.311	0.187	0.278	0.188	0.254	0.187	0.254
Number of id	2,367	2,367	188	188	384	384	2,367	2,367
Standard error	0.388	0.383	0.469	0.443	0.452	0.433	0.466	0.446
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.051$; $^{*}p < 0.1$

Table 8. Estimation of the average treatment effects on (log of) export

	Sample 1: All firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	0.973 (0.506)	0.946*** (0.215)	1.830*** (0.435)	2.563** (0.641)	2.000*** (0.482)	2.298*** (0.519)	0.437* (0.183)	0.750** (0.254)
Observations	19,888	19,888	2,343	2,343	4,697	4,697	19,888	19,888
R^2	0.007	0.033	0.06	0.16	0.055	0.15	0.029	0.098
Number of id	1,808	1,808	213	213	427	427	1,808	1,808
Standard error	3.777	3.737	3.336	3.215	3.525	3.375	3.52	3.402
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 9. Estimation of the average treatment effects on (log of) export

	Sample 2: Firms that export at least in the year before PACC							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	1.720*** (0.130)	2.423*** (0.310)	1.091 (0.656)	3.046** (0.866)	1.465*** (0.235)	2.026*** (0.361)	1.384*** (0.177)	2.037*** (0.288)
Observations	8,679	8,679	1,386	1,386	3,080	3,080	8,679	8,679
R^2	0.088	0.189	0.076	0.218	0.079	0.184	0.08	0.211
Number of id	789	789	126	126	280	280	789	789
Standard error	3.674	3.483	3.272	3.113	3.633	3.47	3.289	3.063
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 10. Estimation of the dynamic average treatment effects on (log of) export

	Sample 1: All firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	1.136** (0.363)	1.126** (0.286)	1.261* (0.577)	1.458 (0.889)	1.223** (0.472)	1.040** (0.396)	0.707 (0.475)	0.772** (0.278)
β_2	1.289*** (0.259)	1.575*** (0.134)	1.355* (0.528)	1.566** (0.585)	1.561* (0.656)	1.774*** (0.439)	0.673* (0.326)	1.185*** (0.186)
β_3	1.410** (0.514)	1.394*** (0.177)	2.185*** (0.381)	2.921** (0.755)	2.149*** (0.459)	2.316** (0.672)	0.652** (0.186)	1.198** (0.366)
β_4	1.654** (0.543)	1.337*** (0.094)	2.644*** (0.443)	2.937*** (0.692)	2.743*** (0.586)	2.577** (0.655)	0.947*** (0.153)	1.119*** (0.219)
β_5	1.077 (0.818)	0.855*** (0.205)	2.624*** (0.626)	3.570** (1.145)	2.583** (0.736)	2.945** (0.757)	0.269 (0.180)	0.780* (0.334)
β_6	0.572 (0.630)	0.601 (0.340)	2.393*** (0.546)	2.491 (1.337)	2.436*** (0.491)	2.858*** (0.663)	0.066 (0.346)	0.634 (0.423)
β_7	−0.225 (0.664)	0.026 (0.341)	2.231** (0.713)	4.867** (1.830)	1.995** (0.740)	3.330** (0.949)	−0.872* (0.407)	−0.211 (0.690)
β_8	−0.411 (0.963)	−0.509 (0.332)	1.178 (0.882)	1.977** (0.655)	1.677* (0.770)	2.570*** (0.499)	−1.212* (0.479)	−0.490** (0.164)
Observations	19,888	19,888	2,343	2,343	4,697	4,697	19,888	19,888
R^2	0.008	0.033	0.068	0.166	0.059	0.153	0.037	0.1
Number of id	1,808	1,808	213	213	427	427	1,808	1,808
Standard error	3.775	3.736	3.327	3.208	3.521	3.372	3.507	3.398
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 11. Estimation of the average treatment effects on (log of) export

	Sample 2: Firms that exported at least on the year before PACC							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	1.428*** (0.353)	1.449** (0.440)	1.121** (0.339)	2.254** (0.578)	1.219** (0.335)	1.007 (0.545)	1.343** (0.345)	1.195** (0.410)
β_2	1.263* (0.526)	2.037** (0.519)	0.911** (0.348)	3.044** (1.162)	1.053* (0.484)	1.323* (0.577)	0.989 (0.509)	1.321 (0.802)
β_3	1.934** (0.514)	2.795*** (0.364)	1.285 (0.771)	3.283** (0.885)	1.637** (0.460)	2.529*** (0.478)	1.727*** (0.405)	2.256** (0.663)
β_4	2.504*** (0.283)	2.696*** (0.577)	1.786* (0.797)	3.283* (1.307)	2.199*** (0.258)	2.092*** (0.462)	2.066*** (0.251)	1.971** (0.617)
β_5	2.323*** (0.317)	2.889*** (0.621)	1.368 (1.141)	4.132** (1.078)	1.974*** (0.316)	2.726*** (0.639)	1.780*** (0.403)	2.707** (0.719)
β_6	2.214*** (0.233)	3.196*** (0.370)	1.088 (1.257)	3.517** (1.186)	2.047** (0.523)	2.829*** (0.225)	1.696** (0.441)	3.121*** (0.256)
β_7	0.468 (0.747)	2.687*** (0.476)	−0.529 (1.374)	2.32 (2.174)	0.191 (0.448)	2.250*** (0.496)	−0.266 (0.606)	2.311*** (0.535)
β_8	−0.536 (0.741)	1.312 (0.756)	−2.211 (1.724)	0.764 (2.347)	−0.759 (0.722)	1.466 (0.764)	−1.383 (1.011)	1.530 (1.049)
Observations	8,679	8,679	1,386	1,386	3,080	3,080	8,679	8,679
R^2	0.091	0.19	0.097	0.223	0.085	0.187	0.098	0.214
Number of id	789	789	126	126	280	280	789	789
Standard error	3.671	3.482	3.243	3.112	3.624	3.469	3.259	3.057
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

that the accumulated impact of the program increases until the fourth or fifth year after the intervention. The results for the year even and eight must be taken with great care because only half of the intervention group have experienced these number of years after the intervention and these firms are concentrated in a couple of sectors (clothes and shoes and leather). In any case, the evidence shows the importance of taking into account the time when analyzing the impact of this kind of programs (similar evidence was found by Figal – Garone *et al.*, 2015).

5.3 Likelihood of exporting

In this subsection we show the results for the propensity to export. In other words, instead of the value of exports as dependent variable we now have a dummy variable indicating if the firm is exporting or not. We perform exactly the same exercises as in the previous subsection.

The results are qualitatively similar. There is a robust positive effect of the intervention on the probability of exporting. The estimates, presented in Table 12, show an increase in the propensity of exporting that ranges from 4.5% to 25% (for the same reason as in previous subsection, we expect this estimation to be downward biased).

When we only keep in the control and intervention groups those firms that are at least exporting in the year before the intervention the effects are in general larger (Table 13). The effect of the intervention on the propensity to export seems to increase until the fourth or fifth year after the intervention (Tables 14 and 15).

Tables A9 and A10 in the Appendix reports the pre-treatment trends equality test on propensity to export that validates the identification assumption implicit in the previous estimates.

6. Conclusions

Policies to promote the development of clusters are widespread in the world and particularly, in Latin America. The complexity of CDPs introduces several obstacles to quantitative impact analyzes in comparison to other more directed industry policies such as innovation policies or export promotion policies. For this reason, rigorous impact evaluations of cluster programs at firm level are extremely scarce in the literature. The objective of this paper is to contribute to this body of literature by evaluating the impact of a CDP in Uruguay using standard impact evaluation methods. In addition, the paper contributes to understand the policy effect's lags and the relevance of CDPs to increase exports and the likelihood of exporting, two key variables for small open Latin American economies.

The evidence shows that the program in Uruguay had a very strong and significant effect on exports and on the propensity of exporting. This effect is very robust across samples and econometric specifications. However, the evidence of a positive impact on sales is weak and in some cases with alternative samples, null.

This evidence is consistent with one of the main objectives of the cluster program in Uruguay, increasing exports. Surveys carried out in Uruguay previous to the PACC initiative showed that firms' main obstacle to engage in innovation and export activities was the limited market size in Uruguay. This is typical for most Latin American economies. It seems that the PACC program helped lifting some of the constraints that firms faced to export. Therefore, from a policy perspective, CDPs seem to be an instrument that could work in the region to incentivize exporting activities.

Commercial fairs and trademarks were in general the most common club goods built under PACC, according to administrative files. According to Porter (2000) the selection of

Table 12. Estimation of the average treatment effects on propensity to export

	Sample 1: All firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	0.084* (0.034)	0.085*** (0.015)	0.165*** (0.038)	0.224*** (0.045)	0.176*** (0.043)	0.195*** (0.038)	0.044** (0.016)	0.068* (0.027)
Observations	19,888	19,888	2,343	2,343	4,697	4,697	19,888	19,888
R^2	0.006	0.032	0.056	0.145	0.054	0.146	0.021	0.085
Number of id	1,808	1,808	213	213	427	427	1,808	1,808
Standard error	0.358	0.354	0.303	0.294	0.321	0.308	0.317	0.307
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 13. Estimation of the average treatment effects on propensity to export

	Sample 2: Firms that export the year before PACC							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	0.152*** (0.017)	0.221*** (0.025)	0.085 (0.045)	0.246*** (0.060)	0.121*** (0.017)	0.167*** (0.027)	0.119*** (0.015)	0.185*** (0.023)
Observations	8,679	8,679	1,386	1,386	3,080	3,080	8,679	8,679
R^2	0.098	0.204	0.064	0.201	0.077	0.179	0.075	0.195
Number of id	789	789	126	126	280	280	789	789
Standard error	0.334	0.316	0.291	0.278	0.321	0.307	0.289	0.271
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 14. Estimation of the dynamic average treatment effects on propensity to export

	Sample 1: All firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	0.098** (0.036)	0.105*** (0.025)	0.113* (0.055)	0.147 (0.078)	0.108* (0.045)	0.094* (0.037)	0.069 (0.048)	0.072** (0.028)
β_2	0.116*** (0.025)	0.147*** (0.011)	0.129** (0.047)	0.148** (0.055)	0.143* (0.057)	0.161*** (0.029)	0.069* (0.030)	0.114*** (0.016)
β_3	0.125** (0.040)	0.126*** (0.018)	0.194*** (0.029)	0.273*** (0.058)	0.190*** (0.041)	0.202** (0.057)	0.063** (0.019)	0.109** (0.041)
β_4	0.142** (0.042)	0.118*** (0.010)	0.228*** (0.039)	0.236*** (0.046)	0.236*** (0.058)	0.209** (0.056)	0.085*** (0.020)	0.101*** (0.023)
β_5	0.076 (0.063)	0.057* (0.023)	0.219** (0.054)	0.288** (0.091)	0.209** (0.067)	0.233*** (0.058)	0.01 (0.017)	0.05 (0.036)
β_6	0.05 (0.044)	0.049 (0.029)	0.218*** (0.041)	0.204* (0.089)	0.214*** (0.043)	0.238*** (0.049)	0.014 (0.030)	0.054 (0.045)
β_7	−0.008 (0.049)	0.023 (0.039)	0.220** (0.065)	0.433** (0.162)	0.183** (0.068)	0.291** (0.082)	−0.055 (0.043)	0.001 (0.075)
β_8	−0.023 (0.061)	−0.036 (0.018)	0.131 (0.068)	0.178** (0.046)	0.161** (0.056)	0.219*** (0.031)	−0.085** (0.024)	−0.039 (0.024)
Observations	19,888	19,888	2,343	2,343	4,697	4,697	19,888	19,888
R^2	0.007	0.032	0.062	0.15	0.057	0.148	0.027	0.087
Number of id	1,808	1,808	213	213	427	427	1,808	1,808
Standard error	0.358	0.354	0.302	0.293	0.321	0.308	0.316	0.307
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 15. Estimation of the average treatment effects on propensity to export

	Sample 2: Firms that exported at least in the year before PACC							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	0.112** (0.032)	0.130** (0.034)	0.083** (0.031)	0.180** (0.058)	0.090** (0.032)	0.083* (0.041)	0.110*** (0.027)	0.112** (0.033)
β_2	0.109* (0.048)	0.197*** (0.039)	0.076** (0.027)	0.266** (0.098)	0.090* (0.040)	0.121* (0.048)	0.086* (0.042)	0.134* (0.063)
β_3	0.170** (0.059)	0.262*** (0.032)	0.101 (0.066)	0.287*** (0.070)	0.135** (0.047)	0.225*** (0.048)	0.152** (0.044)	0.217** (0.060)
β_4	0.216*** (0.037)	0.243*** (0.051)	0.139** (0.049)	0.246* (0.101)	0.179*** (0.018)	0.165*** (0.030)	0.172*** (0.023)	0.180** (0.047)
β_5	0.187*** (0.034)	0.234*** (0.054)	0.086 (0.084)	0.295** (0.077)	0.144*** (0.028)	0.194** (0.054)	0.133** (0.034)	0.217** (0.062)
β_6	0.209*** (0.018)	0.288*** (0.033)	0.095 (0.091)	0.292** (0.089)	0.182*** (0.044)	0.238*** (0.020)	0.161** (0.041)	0.277*** (0.018)
β_7	0.084 (0.067)	0.263*** (0.037)	−0.019 (0.094)	0.216 (0.151)	0.047 (0.040)	0.193*** (0.033)	0.014 (0.043)	0.211*** (0.037)
β_8	0.006 (0.049)	0.145** (0.046)	−0.158 (0.117)	0.071 (0.160)	−0.031 (0.047)	0.123* (0.056)	−0.07 (0.069)	0.151* (0.070)
Observations	8,679	8,679	1,386	1,386	3,080	3,080	8,679	8,679
R^2	0.099	0.205	0.078	0.204	0.082	0.181	0.087	0.198
Number of id	789	789	126	126	280	280	789	789
Standard error	0.334	0.316	0.29	0.279	0.32	0.307	0.288	0.271
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

club goods such as joint marketing actions (trade fairs, trade magazines or marketing delegations), enhance the reputation of a location in a particular field and might reduce perceived buying risks by the potential clients by offering the possibility to multisource or switch vendors, which then might have positive externalities on all members of the cluster. This could potentially explain the results found in the paper and may be an important takeaway for policymakers.

In addition, we found that the timing is important when assessing the impact of this kind of programs. The evidence suggests that the maximum effect of the program can be found in the fourth or fifth year after the first intervention (depending on the sample and econometric specifications). The fact that the effects are stronger after a few years could be because of the usual reason of slow diffusion (considering that a large share of the co-financing was directed first to strengthening cluster institutions).

This also has important policy implications. First, it seems important to sustain policy efforts to be able to see them make a difference on key performance variables at firm level. Second, initiatives to accelerate the diffusion of knowledge and other club goods generated by the cluster policy, may be relevant to shorten the time needed for a relevant impact on firms' performance. Moreover, this shorten span could make the difference between surviving or not for some firms. Finally, the evidence suggests that the evaluation of the effectiveness of cluster programs should not be done in a very short time window.

It is worth noting that our empirical strategy does not allow identifying separately the impact of each mechanism through which the CDP could have affected firm's performance. This is a challenge for this type of programs, given the interrelation and feedback between causal mechanisms. In this paper we were only able to estimate the joint effect of all possible impact channels. Future research must try to identify the relative importance of alternative causal mechanisms. From a policy perspective, it is important to design evaluation strategies from the onset and they should be an integral part of programs. Only in this way future research will be able to have enough data and clean strategies to unravel the most relevant causal paths.

It should also be noted that in the presence of spillover effects in non-participating companies, our empirical strategy results in under- or over-estimation of the direct impact of CDP on the treatment group. This drawback is a consequence of the fact that the control group in all estimations may include firms that could be indirect beneficiaries of the program, due to the externalities generated in the whole sector or value chain in which the cluster is developed. Future research needs to address the estimation of spillover effects and disentangle the direct and indirect effects of cluster policies. One possible way forward to capture sector or region level impacts is to apply the novel approach of synthetic controls proposed by Abadie *et al.* (2010).

Notes

1. They find that the participation in the program alone does not have an effect on R&D productivity. Only firms that participate in the program and collaborate with partners outside the cluster (e.g. universities) show higher R&D productivity (higher number of patents).
2. It should be noted that, once the match between the list of participating companies (provided by us) and the sales database was made by DGI, the taxpayer identifier number of the firm was removed to maintain the confidentiality of information at the firm level. Therefore, it is not possible to match any additional firm-level information that is not contained in this database.
3. An exposition of the methods available for evaluating quantitatively the impacts of cluster policies can be found in Maffioli, Pietrobelli and Stucchi (2016).

4. An alternative to conditioning in a multidimensional vector of covariates, is conditioning in a function of the relevant observed covariates. One possible function proposed by Rosenbaum and Rubin (1983) is the propensity score, i.e. the probability of participating in the program given observed covariates. Matching procedures based on this balancing score are known as propensity score matching.
5. To be more precise, we use the option "ties" in the stata package psmatch2. Therefore, if there are more than one firm identified as a match (i.e. with identical characteristics) all of them will be included.
6. Note that we are also implicitly using sector as matching/reweighting variable as we are implementing both methods within each sector.
7. The growth is approximated by the log difference. The number of differences averaged (i.e. the number of pretreatment period) varies between 1 and 6 depending on the sector and the variables.
8. The increase in exports is computed in the following way: e^{β}-1.

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Further reading

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Appendix

	NCM codes
Food	04XXXX, 18XXXX, 19XXXX, 21XXXX, 22XXXX
Blueberries	081040
Footware and leather work	64XXXX, 41XXXX- 43XXXX
Life sciences	30XXXX, 9018XX-9027XX
Olives	1509XX
Gemstones	7103XX
Clothing	41XXXX-43XXXX, 5XXXXX
Viticulture	2204XX

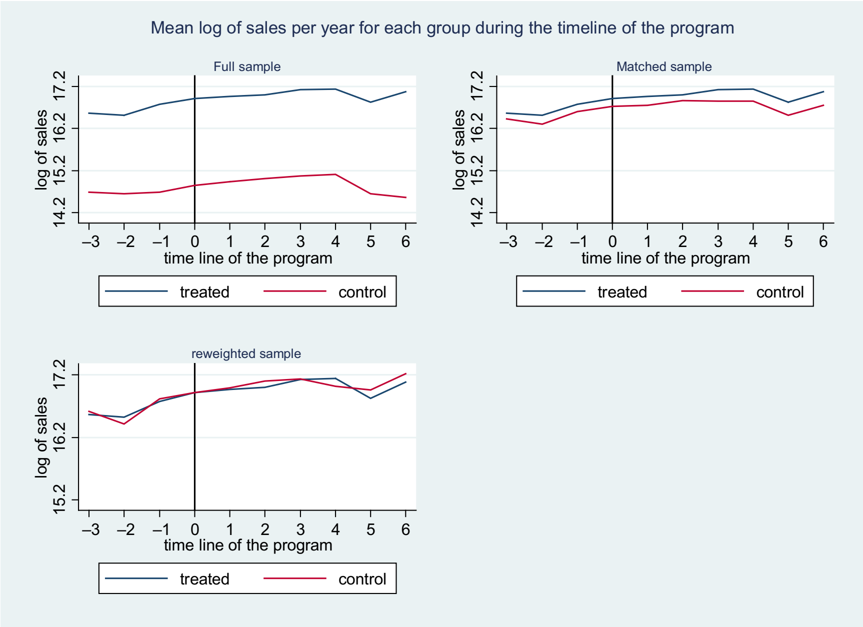
Note: For NCM codes see www.mercosur.int/politica-comercial/ncm/

Table A1. Identification of typical export goods by cluster, based on the aggregation of goods from the Mercosur common nomenclature (NCM, 2012, 6 digits)

Cluster	Pre-treatment Variable	Treated	Full sample	Control Matched sample (nearest neighbor) 1 neigbhor	5 neigbhor	Reweighted sample
Food	Sales	18.13	15.25***	18.06	17.90	18.13
	Sales growth	0.20	0.19	0.16	0.18	0.20
Bluberries	Sales	17.66	17.57	17.57	17.57	17.66
	Sales growth	−0.07	−0.32	−0.32	−0.32	−0.29
Audiovisual	Sales	16.27	13.84***	16.22	16.17	16.27
	Sales growth	0.32	0.19	0.41	0.22	0.32
Footwear and leather goods	Sales	16.15	14.08***	15.97	15.73	16.14
	Sales growth	0.11	0.12	0.15	0.18	0.11
Life sciences	Sales	17.15	15.27	17.56	15.27	16.24
	Sales growth	0.35	0.17	0.20	0.17	0.35
Design	Sales	14.82	13.61***	14.09	13.89	14.82
	Sales growth	0.28	0.22	0.36	0.27	0.28
Naval	Sales	17.86	15.45***	16.34	18.49	15.90
	Sales growth	0.52	0.14***	0.61	0.33	0.52
Gemstones	Sales	14.22	15.30	13.07	13.66	14.23
	Sales growth	−0.37	0.37	0.06	−0.04	−0.37
Software	Sales	15.84	14.84	15.68	15.87	15.84
	Sales growth	0.19	0.24	0.10	0.24	0.19
Clothing	Sales	17.70	14.19***	17.43	17.13	17.69
	Sales growth	0.05	0.23	0.07	0.15	0.05
Viticulture	Sales	17.18	15.08***	16.95	16.70	17.17
	Sales growth	0.11	0.08	0.18	0.12	0.11
Total	Sales	16.92	14.84***	16.73	16.75	16.80
	Sales growth	0.16	0.20	0.18	0.18	0.14

Notes: Sales = log of total sales in the year before the treatment; sales growth = average growth of total sales before the treatment (approximated by the average of the log difference. The number of differences averaged varies between 1 and 5 depending on the cluster). Reject the null of equal mean between treated and control firms at $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A2.
Mean of pre-treatment variable used in the matching (sales)



Note: The lines in each graph show the annual mean of (log) sales of treated (blue) and control (red) firms. For the third graph the average of the control firms is a weighted average based on multivariate reweighting method proposed by Hainmueller (2012). The horizontal axis indicates the years of exposure to the program which is specific for each sector, where 0 is the year of the start of PACC and the negative numbers (in absolute terms) indicate the number of years before the program

Figure A1.
Sales trends before and after the intervention

Table A3. Estimation of the average treatment effects on (log of) sales

	Sample 2: all firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	0.781^{*} (0.369)	0.498 (0.436)	2.209^{***} (0.461)	2.850^{***} (0.623)	1.864^{***} (0.354)	2.014^{***} (0.398)	1.138^{**} (0.386)	1.515^{***} (0.442)
Observations	71,840	71,840	3,000	3,000	6,120	6,120	71,840	71,840
R^2	0.071	0.082	0.111	0.18	0.089	0.161	0.103	0.183
Number of firms	8,980	8,980	375	375	765	765	8,980	8,980
Standard error	4.692	4.665	3.775	3.668	3.862	3.729	3.983	3.804
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A4. Estimation of the dynamic average treatment effects on (log of) sales

	Sample 2: All firms							
	Full sample		Matched sample (nearest neighbor)				Reweighted sample	
			1 neighbor		5 neighbors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1	0.936** (0.373)	1.113*** (0.351)	1.496*** (0.359)	1.393** (0.507)	1.145*** (0.352)	0.963** (0.344)	1.069*** (0.343)	1.305*** (0.314)
β_2	1.187** (0.398)	1.008* (0.501)	0.351*** (0.542)	2.782*** (0.809)	1.914*** (0.440)	2.171*** (0.548)	1.406** (0.471)	1.763*** (0.479)
β_3	1.100** (0.388)	0.519 (0.638)	2.913*** (0.605)	3.173*** (0.663)	2.333*** (0.404)	2.382*** (0.459)	1.404** (0.481)	1.682** (0.581)
β_4	0.549 (0.371)	−0.133 (0.648)	2.995*** (0.690)	3.510*** (0.718)	2.269*** (0.433)	2.432*** (0.335)	0.948* (0.513)	1.406** (0.573)
β_5	−0.083 (0.378)	−0.304 (0.353)	2.891*** (0.855)	3.926*** (0.886)	2.042*** (0.497)	2.422*** (0.475)	0.431 (0.740)	1.439** (0.464)
β_6	−0.467 (0.688)	−0.62 (0.642)	2.974** (1.130)	3.591*** (0.726)	2.128** (0.685)	1.871* (0.862)	−0.031 (0.883)	1.203 (0.847)
Observations	71,840	71,840	3,000	3,000	6,120	6,120	71,840	71,840
R^2	0.071	0.082	0.116	0.186	0.091	0.163	0.105	0.183
Number of firms	8,980	8,980	375	375	765	765	8,980	8,980
Standard error	4.692	4.665	3.767	3.658	3.86	3.725	3.978	3.803
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A5.
Pre-treatment trends equality test on (log of) sales

	Sample 2: All firms			
		Matched sample (nearest neighbor)		
	Full sample (1)	1 neighbor (2)	5 neighbors (3)	Reweighted sample (4)
Treatment since one year before the PACC	1.006*** (0.263)	0.345 (0.501)	0.285 (0.361)	0.159 (0.222)
Treatment since two years before the PACC	−0.095 (0.298)	−1.265** (0.513)	−1.697*** (0.535)	−0.680** (0.288)
Observations	71,840	3,000	6,120	71,840
R^2	0.082	0.182	0.164	0.183
Number of firms	8,980	375	765	8,980
Standard error	4.665	3.666	3.723	3.803
Fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

	Treated		Control Matched sample (nearest neighbor)		
	Pre-treatment variable	Full sample	1 neigbhor	5 neigbhors	Reweighted sample
Bluberries					
Total export	2.90	3.88	7.93*	6.47*	2.90
Export to MCS	1.07	1.48	2.24	2.47	1.07
Specific goods exports	0.76	2.49	5.56***	4.15**	1.84
Export growth	0.35	0.43	1.21	0.71	0.35
Life sciences					
Total export	8.37	4.70	8.83	9.68	8.37
Export to MCS	6.25	3.11	3.92	7.33	6.25
Specific goods exports	6.84	2.65*	6.54	6.20	4.34
Export growth	0.85	0.10**	1.35	1.00	0.85
Gemstones					
Total export	2.38	4.75	4.59	3.65	2.38
Export to MCS	0.00	2.21	0.00	0.00	0.01
Specific goods exports	0.00	3.33	4.59	3.65	2.38
Export growth	0.30	0.32	0.57	0.46	0.30
Olives					
Total export	2.73	4.01	6.33	5.29	2.74
Export to MCS	1.46	1.08	3.46	0.94	1.46
Specific goods exports	2.73	3.43	6.33	5.19	2.61
Export growth	0.57	−0.03	1.18	2.10	0.57
Food and viticulture					
Total export	10.17	5.77***	10.63	10.37	10.16
Export to MCS	7.17	2.57***	6.55	6.42	7.16
Specific goods exports	9.19	6.53***	11.88	11.47	9.18
Export growth	0.66	0.50*	0.82	0.86	0.66
Clothing footwear and leather goods					
Total export	5.88	4.68	10.74***	10.59***	5.87
Export to MCS	4.13	2.62**	7.41**	7.76***	4.13
Specific goods exports	3.86	4.49	10.47***	10.19***	3.87
Export growth	0.38	0.21	1.19	0.55	0.38

Notes: Total export = log of total export in the year before the treatment; export to MCS = log of total export to Mercosur in the year before the treatment; specific goods exports = log of export of typical "cluster" good in the year before the treatment; export growth = average growth of total export before the treatment (approximated by the average of the log difference. The number of differences averaged varies between 1 and 6 depending on the cluster)

Table A6.
Mean of pre-treatment variable used in the matching

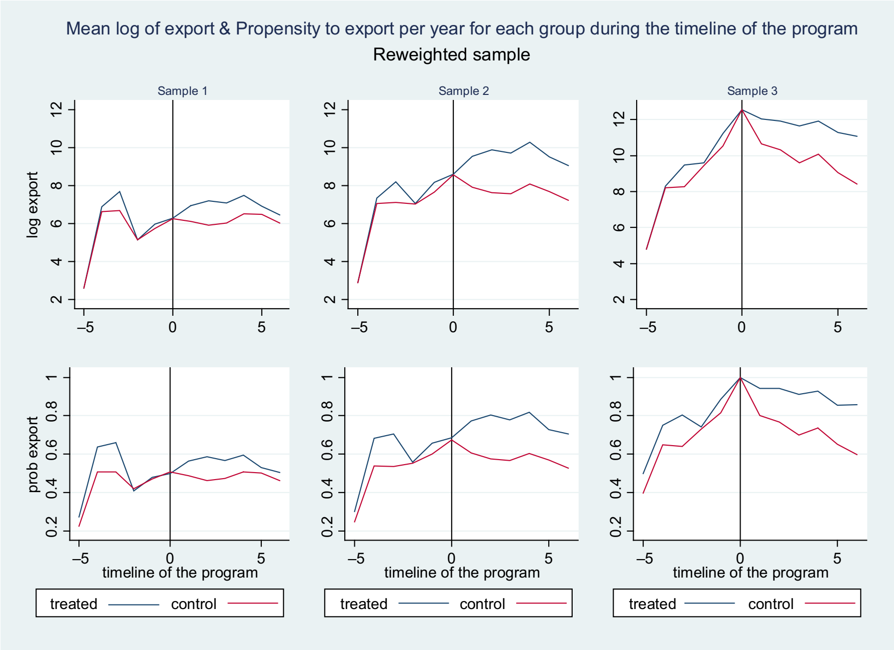


Figure A2.
Exports and propensity to export trends before and after the intervention

Notes: (1) The lines in each graph show the annual mean of log exports (or export propensity) of treated (blue) and control (red) firms. In the case of the control firms is a weighted average based on multivariate reweighting method proposed by Hainmueller (2012). The horizontal axis indicates the years of exposure to the program which is specific for each sector, where 0 is the year of the start of PACC and the negative numbers (in absolute terms) indicate the number of years before the program. (2) each of the three columns of graph correspond to the following samples: Sample 1: all firms; Sample 2: firms that exported in at least one year between 2004-2014; Sample 3: firms that exported at least in the year before the start of the program

Table A7. Pre-treatment trends equality test on (log of) export

	Sample 1: All firms			
		Matched sample (nearest neighbor)		
	Full sample	1 neighbor	5 neighbors	Reweighted sample
	(1)	(2)	(3)	(4)
Treatment since one year before the PACC	0.345 (0.267)	−0.776 (0.645)	−0.865** (0.306)	−0.168 (0.247)
Treatment since two years before the PACC	0.475 (0.492)	0.251 (0.498)	0.257 (0.361)	0.345 (0.452)
Observations	19,888	2,343	4,697	19,888
R^2	0.033	0.161	0.151	0.098
Number of firms	1,808	213	427	1,808
Standard error	3.737	3.216	3.375	3.402
Fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A8. Pre-treatment trends equality test on (log of) export

	Sample 2: Firms that exported at least in the year before de PACC			
		Matched sample (nearest neighbor)		
	Full sample	1 neighbor	5 neighbors	Reweighted sample
	(1)	(2)	(3)	(4)
Treatment since one year before the PACC	0.676 (0.406)	−0.333 (0.754)	−0.816 (0.559)	−0.544 (0.379)
Treatment since two years before the PACC	0.181 (0.666)	0.557 (0.693)	0.769 (0.702)	0.884 (0.701)
Observations	8,679	1,386	3,080	8,679
R^2	0.189	0.218	0.185	0.211
Number of firms	789	126	280	789
Standard error	3.483	3.115	3.47	3.062
Fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A9. Pre-treatment trends equality test on propensity to export

	Sample 1: All firms			
		Matched sample (nearest neighbor)		
	Full sample	1 neighbor	5 neighbors	Reweighted sample
	(1)	(2)	(3)	(4)
Treatment since one year before the PACC	0.045 (0.025)	−0.05 (0.058)	-0.069^{*} (0.028)	−0.001 (0.024)
Treatment since two years before the PACC	0.016 (0.047)	−0.001 (0.041)	0.014 (0.036)	0.019 (0.049)
Observations	19,888	2,343	4,697	19,888
R^2	0.032	0.145	0.146	0.085
Number of firms	1,808	213	427	1,808
Standard error	0.354	0.294	0.308	0.307
Fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A10. Pre-treatment trends equality test on propensity to export

	Sample 2: Firms that exported at least the year before de PACC			
		Matched sample (nearest neighbor)		
	Full sample	1 neighbor	5 neighbors	Reweighted sample
	(1)	(2)	(3)	(4)
Treatment since one year before the PACC	−0.067 (0.040)	−0.006 (0.062)	−0.049 (0.050)	−0.029 (0.024)
Treatment since two years before the PACC	−0.008 (0.059)	−0.001 (0.078)	0.043 (0.068)	0.057 (0.062)
Observations	8,679	1,386	3,080	8,679
R^2	0.204	0.201	0.179	0.195
Number of firms	789	126	280	789
Standard error	0.316	0.279	0.307	0.271
Fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes

Notes: Cluster-robust standard errors in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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