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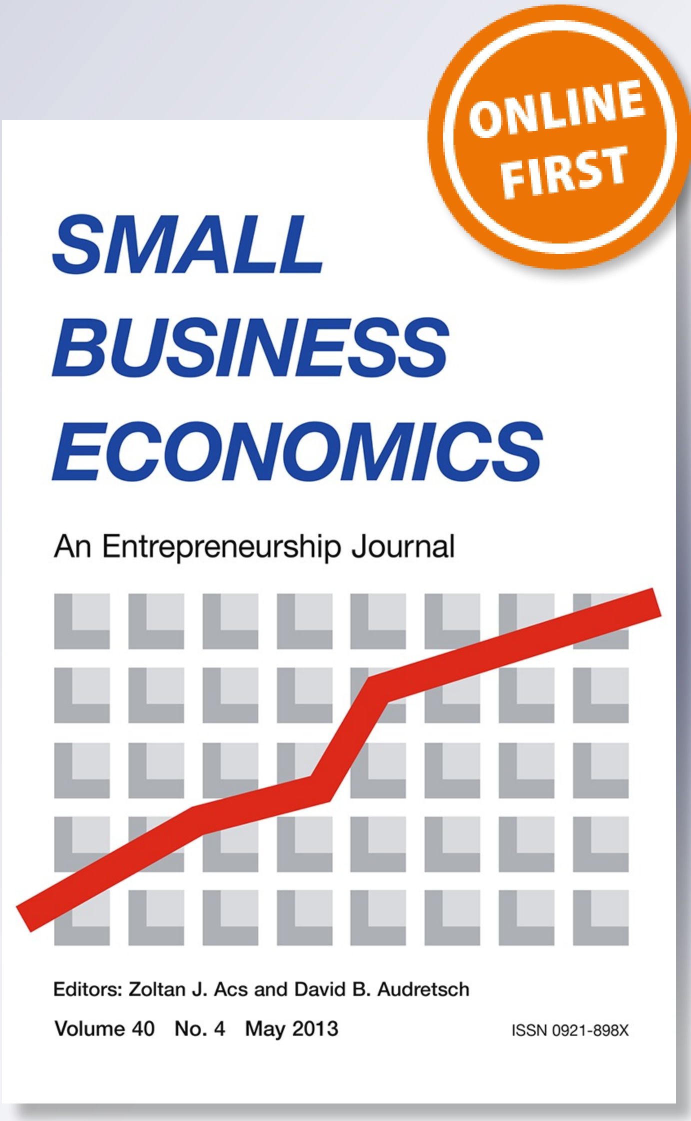

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The effect of innovation policy on SMEs' employment and wages in Argentina

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Abstract This article evaluates the effect of the Argentinean Support Program for Organizational Change on employment and wages. The program aimed at increasing small and medium-sized enterprises' competitiveness by co-financing technical assistance to support process and product innovation activities. Although employment is not usually the main objective of these types of programs, they are always implemented assuming that they create—or at least do not destroy—employment opportunities. We use a unique data set with information for the population of firms in Argentina from 1996 to 2008 to test this important assumption. Using a combination of fixed effects and matching, we find that both process and product innovation support increased employment and wages, with a higher impact on employment. In addition, we find that product innovation support had a larger effect on wages than process innovation support.

Keywords Innovation · Employment · Wages · Policy evaluation · SMEs · Argentina

JEL classifications D2 · J23 · L8 · O31 · O33 · L26

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1 Introduction

Most governments in both developed and developing countries pursue policies to foster the adoption of technologies and good practices by small and medium-sized enterprises (SMEs) under the assumption that these interventions contribute to economic growth in general and job creation in particular. Although rarely questioned, this approach is based on a premise that was long considered controversial: that technological change leads not only to higher efficiency, but also eventually to the creation of new and better jobs.

For a long time, theoretical models saw innovation as a potential threat to employment. The main

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argument was that technological change could create unemployment through the substitution of capital for labor. This discussion then evolved to take into account how different types of innovation under different market conditions may imply different effects on employment and employment composition. For instance, process innovation can induce a substitution of capital for labor, but can also result in higher productivity, lower prices, and higher demand, leading therefore to higher employment. Similarly, product innovation usually induces higher demand, but can also increase market power for innovators, inducing higher prices and lower demand.

Empirical studies have provided growing evidence of both overall positive effects of innovation on employment and rather heterogeneous effects depending on the types of innovation. Many studies, such as Pianta (2006), Hall et al. (2008), Harrison et al. (2008), have found that while process innovation tends to destroy jobs, product innovation creates jobs. The same seems also to be true in developing countries; Benavente and Lauterbach (2008), Álvarez et al. (2011), Aboal et al. (2011), Crespi and Tacsir (2011) and de Elejalde et al. (2013), using the analytical framework of Harrison et al. (2008), found a similar relationship between innovation and employment in Argentina, Chile, Costa Rica, and Uruguay.

Other studies, such as Berman et al. (1994), Bresnahan et al. (2002), Caroli and Van Reenen (2001), and Greenan (2003), focused on the relationship between technological change and the skill composition of the labor force. These studies show that because the development of new products and the introduction of new processes often require skills that are not available in a firm, innovation may induce relevant changes in the skill composition, and, consequently, in the real wage structure.

Despite this growing evidence of overall beneficial effects of technological change on employment, the question of the effectiveness of the innovation policy in creating new and better jobs remains wide open. This could be due in part to the fact that usually the main objective of innovation policy is increased firm productivity, though this objective is almost always viewed as not contrasting, or even complementary, to generating employment and improving the skills of the labor force.

So far, most of the literature addressing the effectiveness of subsidies or other innovation support instruments has focused on direct effects on firm innovation effort. Several studies show evidence of the absence of a "crowding-out" effect of public support on private investment in R&D. A non-exhaustive list of these papers includes Czarnitzki et al. 2007, Aerts and Schmidt 2008, Gonzalez and Pazo 2008, Czarnitzki and Lopes-Bento 2012, and Hottenrott and Lopes-Bento 2012. Similarly, other studies show evidence of the positive impact on the development of new production processes and/or products (Czarnitzki et al. 2007, 2011, Cappelen et al. 2011; Czarnitzki and Lopes-Bento 2011), an increase in R&D related jobs (Czarnitzki and Lopes-Bento 2012) and in R&D related wages (Wolff and Reinthaler 2008).

Much less explored are the effects of innovation policy on employment and employment composition. This is particularly true in developing countries and for programs aimed at fostering technological change in SMEs, which are often justified for their potential beneficial effects on employment. Among the few studies that addressed this topic, Hall and Maffioli (2008) discuss the results of impact evaluations of technology development funds in Latin America, showing relevant positive results on employment generation in Brazil. López-Acevedo and Tan (2010) show evidence of some impact on employment in Mexico and Colombia and positive effects on wages in Chile. Although these studies provide some evidence of the potential impact of innovation policy on employment, none of them specifically focused on this topic or on the differential effect of the types of innovation promoted by the policy on employment or wages.

This article aims to fill this knowledge gap by evaluating the impact of the Support Program for the Organizational Change (PRE for its Spanish name: Programa de Apoyo a la Reestructuración Empresarial) on employment and wages. This program aimed at strengthening Argentinian SMEs by co-financing up to 50 % of technical assistance plans to be implemented by private certified suppliers. For the purpose of our study, a key feature of this program is that because it was open to any type of technical assistance, most of the beneficiary firms eventually requested (and received) support for either product or process innovation activities. Because of this, the impact of different types of innovation support on employment and wages can be analyzed through the evaluation of this program.

We construct a unique data set that matches two different sources: administrative records from the program and a data set constructed by the *Observatory of Employment and Entrepreneurial Dynamics* (OEDE for its Spanish name) that contains social security and customs information for the population of firms and employees in Argentina over the period 1996–2008. Because the OEDE data include the entire population of Argentinean firms, the consolidation with the program administrative records was extremely efficient, leading to the identification of 98 % of program beneficiaries. Therefore, by merging our sources we end up with a data set with information on beneficiary and non-beneficiary firms since 1996, or 3 years before the first PRE beneficiary received support. The structure of the data allows us to use a fixed-effects model to control for unobservable factors that may affect the outcome variables in addition to treatment status. Furthermore, the availability of 3-year pre-treatment data—quite rare in this kind of study—allows us to use the ex-ante trends of the outcome variables to support the hypothesis that the treated and control firms would have behaved identically in the absence of treatment.

Our findings show both process and product innovation created more and better jobs in terms of real wages. Process and product innovation support increased employment by 22 and 19 %, respectively; this is approximately 5 workers per firm. The effect on wages was also quantitatively significant; while process innovation support increased real wages by 2 %, product innovation support increased it by 4 %.

The rest of the article is organized as follows. Section 2 describes the program. Section 3 describes the data set. Section 4 presents the identification strategy. Section 5 presents the empirical results and robustness check. Finally, Sect. 6 concludes.

1.1 The Support Program for the Organizational Change, PRE

The program was designed in the late 1990s in a particularly complex context for Argentine SMEs. The increase in competition resulting from the trade liberalization implemented at the beginning of the 1990s threatened low productivity firms and forced SMEs to be more competitive. In this context, the main objective of the program was to support SMEs in their effort to increase competitiveness through technical assistance services. The program therefore specifically targeted small-scale investment in intangible asset by SMEs.

The program was mainly justified on the basis of market failures related to asymmetric information and non-convexities in both the credit and business services markets (see IDB 1997[1]). The asymmetry of the information argument is often used to justify the provision of public subsidies to support private investment in intangible assets, such as the development of new product and processes (Hall and Lerner 2009). Investment in knowledge, especially when it is firm-specific, may in fact imply significant information asymmetries on its expected risk-adjusted value between the borrower and the lender. For this reason, this kind of investment is often severely discriminated against by the financial sector, especially in economies with relatively limited financial deepening. This problem is aggravated when the borrower is a SME (OECD 2006). The intangible nature of the investment outcome creates many concerns about possible collateral for financial support, a concern that is exacerbated when the other assets of the borrower are limited.

A similar argument applies to the business service market. In this case, providers usually have a much better idea of the value of the services they are providing and on the potential return for the client of these services (European Commission 2002). Once again, this problem is aggravated when the client is a SME with possibly limited internal capacity to conduct a proper cost-benefit analysis of the service to be purchased. This argument has been at the core of many technological extension programs aimed a demonstrating to wide groups of potential adopters the benefit of certain technological changes.

In addition to the problem of asymmetric information described above, indivisibilities and non-convexities could induce a situation where the supply of an input for SMEs simply does not exist (Ibarraran et al. 2010). In this context, SMEs may be constrained by the incapacity or unwillingness of crucial suppliers to scale down their services to meet the demand of smaller firms. In the credit market, for example, where part of the lending cost is not proportional to the size of

[1] Approximately 50 percent of the program was financed by a loan from the Inter-American Development Bank to the Argentine Government.

the loan—because it is mainly composed of the fixed cost of assessing borrower credit worthiness—and yields fall as the size of the loan shrinks, financial institutions might find that lending to SMEs is less profitable, and small loans may be undersupplied relative to the case of no transaction costs or entirely unavailable.

Non-convexities also play an important role in the lack of business services for SMEs, and the reason is similar to that described above for the credit market. Consulting firms find it less profitable to work with SMEs because small consultancy contracts have an important fixed cost of providing the service and, by definition, small revenue. Therefore, unless consulting firms can offer the same service to many SMEs, they would not offer services tailored to SMEs. Coordination failures present another argument for the justification of public intervention to address the problem of scale in SME access to key markets. SMEs could try to overcome the limitations imposed by indivisibilities and non-convexities on the supply side by coordinating their demand for goods and services (e.g., business services related to relatively standard businesses practices). However, because of the relatively high transaction costs and externalities implied by these efforts, coordination may fail to happen without public intervention, providing a clear justification for public programs aimed at promoting associativity, networking, and clustering. Although multiple arguments were used when the program was first implemented, the indivisibility and non-convexity problem in the business service supply seems to be the strongest justification of the PRE program.

The importance attributed to problems on the supply side of the business service market is also clear from the structure of the program. The program had three components. The first promoted the use of existing public and private support programs by offering information about the programs and their eligibility conditions. The second aimed at developing a market for tailored professional services for SMEs. Lastly, the third component offered direct support to SMEs by co-financing up to 50 % of the investment in professional services and technical assistance. With a budget of 154 million dollars, the program co-financed technical assistance services to 1,266 firms between 1999 and 2007.

During the 8 years in which the program was executed, the Argentine economy faced one of its most severe economic and institutional crisises.[2] In this context, the program faced two key reorganizations of its management that divide program implementation into three stages. In the first stage, 1999–2000, two private operators were in charge of most of the management of the program. The Argentine government, through the Program Executing Unit (PEU), was in charge of monitoring and evaluation activities. During this stage, 721 firms received support from the program with an average support of USD 18,677 (see Table 1). At the end of 2000, the government introduced significant fiscal restrictions as a consequence of the recession. The view of the role of the government in the execution of the program changed, the program was reformulated, and public-sector managers were replaced the private operators. During this stage, effort was concentrated on auditing instead of approving new projects. As a result, the program financed only 190 new projects. At the end of 2004, in a context of economic growth and fiscal surplus, support to SMEs became a priority and the program was reformulated again. This reformulation was the origin of the third stage of the program that lasted until the end of the program in 2007. During this stage, private institutions called "PREFI Windows" (entrepreneur associations, NGOs, or universities) and the public sector's PEU shared the management. Each PREFI was responsible for receiving, revising, and pre-evaluating the projects presented by SMEs, while PEU was responsible for the final evaluation and approval of the projects. From 2005 to 2007, 355 firms received support from the program. The average amount received by the beneficiaries in this stage was USD 3,497; this decrease is explained mainly by the devaluation in 2002, although the average amount in Argentinean pesos also fell to 12,463 pesos.

Although the reorganizations implied significant changes in the program's administrative processes, the main characteristics of the program did not change. The program was always demand-driven, and it worked as an open window in which potential beneficiaries postulated the support by presenting a development

[2] In December 2001, President De la Rua resigned, and between December 2001 and January 2002, five presidents were in charge of the government. During those months, Argentina changed its monetary regime and declared the default of its debt. In 2002, the economy contracted by 10.8 %. After that contraction, the economy grew at an average rate of 8 % between 2003 and 2008.

Table 1 Program information

	1999–2000	2001–2004	2005–2007	Total
Firms that received support	721	190	355	1,266
Firms that applied and did not received support	N.A.	73	1,265	1,338
Average amount received by beneficiary (in pesos)	18,677	16,895	12,239	16,604
Average amount received by beneficiary (in dollars)	18,677	5,632	3,497	12,463

Source: Administrative records of the program (SEPyME)

plan. Table 1 shows the number of firms that received the support and those that applied and did not receive the support.[3] Firms could request financing for a broad variety of eligible services, and they were allowed to choose the private provider of the technical assistance they need. Like other policies designed in the 1990s, the program was horizontal, i.e., it did not establish eligibility conditions by sector or geographical region. The few eligibility conditions that existed were related to size, age, and tax compliance: firms needed to be SMEs according to Argentine law, they needed to be least 2 years old, and they needed to be in good standing with both the fiscal and social security authorities. However, in practice, even though eligibility was only based on size, age, and good standing with authorities, firms that participated in the program were similar in other dimensions. More than half of them (54 %) belonged to the manufacturing sector, 19 % to the service sector, and 18 % to retail. Primary activities and the construction sector represented only 4 and 5 % of the beneficiaries, respectively. Around 48 % of the firms that received the program were created before 1990 and 52 % after that year. Ninety percent of the beneficiaries were corporations. In terms of location, 82 % of the firms were concentrated in the central region (Cordoba and Santa Fe) and in Buenos Aires. Table 2 compares beneficiaries and non-beneficiaries in terms of size, sector, age, and region. Beneficiaries were larger and older than non-beneficiaries. They were more concentrated in the manufacturing sector and in Buenos Aires and the central region.

The number of firms that were beneficiaries at some point between 1999 and 2007 and were active in 1998 is 1,015.[4] The type of assistance co-financed by the program did not change significantly across reorganizations. A large majority of these firms requested support for process innovation; more precisely, 749 firms received support for process innovation, and only 167 firms received support for product innovation. Following Harrison et al. (2008), we assume that firms introducing a new product might need to change their production process. Therefore, if a firm asked for both product and process innovation support, we classify it as having asked for product innovation support. On the other hand, those firms that we classify as receiving process innovators asked for support in changing their production process without changing their products. Most of the firms that received support for innovation activities also asked for support of non-innovation activities. Only 99 firms received support only for non-innovation related activities. We aim at comparing process and product innovation support, and therefore we do not consider these firms in the evaluation. Another reason for not considering these firms is that the small number of beneficiaries in this category can affect the power of the test.

2 Description of the data set

We constructed a unique da taset by merging two different sources: (1) the administrative records of the program and (2) a data set called the Data Set for the Dynamic Analysis of Employment (BADE) that was constructed by the Observatory of Employment and Entrepreneurial Dynamics (OEDE) at the Ministry of Labor, Employment, and Social Security in Argentina. These sources were produced by different institutions, at different moments in time, and with different

[3] Most of the firms received support only once; only 19 firms received support more than once.

[4] We consider firms that were active in 1998 because our estimation strategy requires that all the firms are at the moment in which we estimate the propensity score, and we estimate the

Footnote 4 continued
propensity score the year before the beneficiaries receive support.

Table 2 Characteristics of beneficiary and non-beneficiary firms

	Beneficiaries (%)	Population of firms reporting employment in Argentina[a] (%)
(a) By size		
Micro	11	74
Small	39	19
Medium	36	5
Large	14	2
(b) By sector		
Primary	4	13
Manufacturing	54	11
Retail	18	25
Services	19	45
Construction	5	4
N.A.	1	3
(c) By the year in which the firm was created		
Before 1976	21	9
Between 1976 and 1991	27	14
Between 1992 and 2001	44	52
After 2001	7	24
N.A.	1	0
(d) By region		
Gran Buenos Aires (GBA)	44	40
Centro	42	38
Cuyo	5	6
Noreste Argentino (NEA)	1	4
Noroeste de Argentina (NOA)	5	5
Patagonia	2	5
N.A.	1	1

Source: Observatorio de Empleo y Dinámica Empresarial (OEDE) MTEySS and administrative records of the program (SEPyME)

[a] Average of the firms that were active between 1998 and 2008

objectives. This heterogeneity demanded a consistent way of identifying firms across data sources; we merged both data sets using the tax identifier code of each firm. We were able to identify 98 % of the beneficiaries in BADE, and the remaining 2 % did not declare employment.

The administrative records of the program provide detailed information on the main characteristics of the program's technical assistance—i.e., the year in which it was offered, the amount co-financed (ANR), the duration in months of the technical assistance, the type of firm that offered the services, and the type of service received by the firm.

The data set constructed at OEDE includes data from the administrative records of the National Administration of Social Security (ANSES) and the National Administration of Customs. The data set is an unbalanced panel of firms that includes all firms declaring employment in Argentina after 1996 regardless of sector. In this data set each entry and exit is recorded. In 2008, the last year of our analysis, the data set included around 570,000 firms.

In order to apply our chosen identification strategy, we restricted our analysis to the population of active firms between 1996 and 1998. Our final data set is an unbalanced panel with exit of firms after 1999 and no entry of new firms. Although this decision restricts the internal validity of our findings to those firms that were active from 1996 to 1998, we obtain a group of firms that is comparable to the beneficiaries, as will become clear in the following sections. Given that 80 % of the beneficiaries were active from 1996 to 1998, the price for restricting the sample is considerably lower than the benefit.

This data set provides us with information on firm size, location, industry, age, number of employees, average nominal wages in pesos (instead of deflating nominal variables by an aggregate price index, we use year dummies in the regressions to control for aggregate variations in the price level), employee experience, and the value of exports in dollars. Table 3 shows the definition of these variables.

The data set has three important advantages for our study. First, the large number of firms we are considering increases the probability of finding non-participant firms with the same characteristics as participants. Second, the panel structure of the data set allows us to control for time-invariant non-observables that may have determined the participation in each service of the program and the performance of firms. Finally, the availability of several years of information before the program initiation allows us to provide evidence in favor of the main assumptions of our identification strategy.

3 Identification strategy

The key challenge for our identification strategy is that we want to measure the effectiveness of the different kinds of technical assistance services provided by the program. Therefore, in addition to the usual problems related to the identification of counterfactual outcomes in a non-experimental setting, we also need to consider issues specifically related to the availability of multiple treatments.

As mentioned above, we have a large panel of firms that includes information on participants—before and after their participation in the program—as well as non-participants. Given that the interventions are not randomly assigned, the pool of non-participant firms is not necessarily comparable to the group of beneficiaries, and hence potential issues of self-selection and administrative selection bias arise.

In a simple regression framework, we could reduce the selection bias related to observable factors by simply including those factors as control variables in the regression. However, in our case some important differences between participant and non-participant firms may also be related to unobservable (or unobserved) factors, such as the entrepreneurial behavior or managerial skills of the owner. Our strategy is to take advantage of the panel structure of the data to control for unobservable sources of bias. The estimation of a fixed-effects model should allow us to control for all unobservable factors, as long as they do not vary with time. Unfortunately, the assumption that all unobservable sources of bias are constant over time is untestable. However, the credibility of this assumption is strongly related to how similarly participant and non-participant groups evolved in the pre-treatment period. For this reason, we will use matching techniques to identify a group of non-participant with similar characteristics, including evolution of outcome variables, before the beginning of the program.

The key identification condition in this case is that the expected value of the potential outcome in absence of the program is independent of treatment after conditioning on the unobserved fixed effects and the set of observable variables, i.e., $E\left(Y_{0,it}|\alpha_i, X_{it}, T_{i,t}\right) = E\left(Y_{0,it}|\alpha_i, X_{it}\right)$. The estimating equation is given by:

$$Y_{it} = \alpha_i + \lambda_t + \beta T_{it} + \gamma X_{it} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of firm i in year t, α_i captures all time-constant factors that affect the outcome and are firm-specific, λ_t represents yearly shocks that affect all firms, T_{it} is a binary variable that takes the value one starting in the year in which firm i enters the program, X_{it} is a vector of time-varying control

Table 3 Variable definitions

Variable	Definition
Number of employees	Average number of workers declared by the firm to the social security system during the year
Average wage	Nominal wage in Pesos obtained dividing the total amount of wages declared by the firm by the number of employees
Workers experience	Average number of years employees have been working at the firm
Proportion of women	Proportion of female employees
Value of exports	Value of exports in dollars declared by the firm. Available since 1998
Exporter	Dummy variable that takes value one if the firm exports during the year
Type of company	We classified firms according their legal constitution into: owned by one proprietary, incorporated company, and other types of companies
Size	We classified firms using the average employment of 2 consecutive years into microfirms (fewer than 4 employees), small firms (between 4 and 13 employees), medium-sized firms (between 14 and 50 employees), and large firms (more than 50 employees)
Age	Age of the firm obtained as the difference between each year and the year in which the firm was registered in the tax administration (AFIP)
Industry classification	It is the industry of the principal activity using ISIC rev. 3. The original information about industry comes from the records of AFIP and was improved by OEDE using information from the Survey of Labor Indicators (EIL) and other sources such as entrepreneur associations, regulation institutions, and other databases with records about firms
Location	Province where employers declare the largest number of employees. Therefore, it can differ from the fiscal address

variables, and ε_{it} is the usual error term assumed to be uncorrelated with T_{it}. The standard errors will be clustered at the firm level for the inference to be robust to within-firm correlation of the error terms. In the absence of time-varying unobserved factors that affect both the outcome and program participation, the fixed-effects model leads to a consistent estimator for β, the average impact of the program.

The validity of the fixed-effects estimator rests on the identification assumption that trends in the outcomes would have been equal in the absence of treatment. This assumption may be difficult to accept when firms in the control group are too heterogeneous and different from participating firms, i.e., firms that are very different are likely to follow different trends as well. Therefore, in order to reinforce the credibility of the method, we also run Eq. (1) on a matched sample, selecting firms in the comparison group that are the most similar to beneficiaries in terms of observed characteristics and pre-treatment performance. We do this to ensure that we are selecting from the control group only those firms that have pre-treatment trends that are similar to those in the treated group.

With only one treatment the procedure would define the year previous to treatment as a baseline year and estimate the propensity score, i.e., the conditional probability of participation, $P(T_{it} = 1|Z_{it}) = F(\theta Z_{it})$, for a fixed pre-treatment year t, where Z is a vector of covariates, and F is the standard normal or logistic cumulative distribution function. Using the predicted probability of participation, one would first match each treated firm with the untreated firm with the most similar propensity score and then drop from the database all the non-treated firms that are not matched to any treated firm. Finally, one would run Eq. (1) on this matched subsample.[5]

In this article, however, we are interested not in estimating the effectiveness of a unique treatment, but in comparing different types of support. For this reason, we have to extend our standard identification strategy to the case of multiple treatments. In practical terms, this implies that instead of having two status—treated and non-treated, treatment can take three values, i.e., $T = j$ with $j = (0, 1, 2)$. The treatment variable takes value zero when the firm receives no treatment. It takes value one when the firm receives support for product innovation. Finally, it takes value 2 when the firm receives support for process innovation. In this case, the conditional probability of participation—the propensity score—is given by

$$0 < \pi_j(x) \equiv P(T = j|x) < 1, \quad \forall j, x \tag{2}$$

Note that $\pi_0(x) = 1 - \sum_{j=1}^{J} \pi_j(x)$. To estimate π_j, we use a multinomial logit model using $j = 0$ as the omitted category. We estimate this model with the information before 1999 (the first year in which firms received support). By doing this, we do not include variables that can be affected by the program. The variables we include in x for the estimation of the propensity score are: employment in 1998, average wages in 1998, a dummy variable that takes value one if the firm exported in 1998, the proportion of women in 1998, the average growth in employment between 1996 and 1998, the average growth in wages between 1996 and 1998, the age of the firm, the experience of the workers measured by the number of years in the firm, industry dummies, type of society dummies, and region dummies.

When there is more than one treatment, there are many parameters of interest (Lee 2005). For example, it is possible to do pairwise comparisons or comparisons with the control. If we condition on two groups j and 0 (comparisons with the control group) and we define

$$\pi_{j|j_0} = \frac{\pi_j(x)}{\pi_j(x) + \pi_0(x)}, \tag{3}$$

then we have $T_j + T_0 = 1$ and we can use $\pi_{j|j0(x)}$ for propensity score matching. Therefore, after computing (3) for each treatment j, it is possible to apply matching to find non-participant firms with a similar probability of receiving treatment j and to define a matched sample, MS_j, for each treatment j. Then, the impact of treatment j can be obtained by the estimate of δ_j in the following equation

$$\begin{aligned} &Y_{it} = \alpha_{ji} + \lambda_{jt} + \delta_j T_{j,i,t-1} + \gamma_j X_{it} + \varepsilon_{jit} \\ &\quad i \epsilon \mathrm{MS}_j,\ j = (0, 1, 2) \end{aligned} \tag{4}$$

where Y_{it} is the outcome variable (log of the number of employees and log of the average wage). $T_{j,i,t}$ is a variable that takes value one after firm i receives support j; we consider lagged values of the treatment variable because we do not expect the policy to have

[5] This approach was used in several evaluations of productive development programs; see, for example, Arráiz et al. (2012).

contemporaneous effects. X_{it} is a set of control variables; we include age and age squared as control variables. λ_{jt} is a set of year dummies that captures all the time-varying non-observable factors that affect all the firms in the same way. α_{ij} captures time invariant non-observed firm characteristics that can affect the decision of firm i to participate in the program or its performance, for instance, managerial ability. To control for these time invariant firm characteristic, we estimate Eq. (4) by fixed effects. Finally, $\varepsilon_{j,it}$ is an error term that is not correlated with explanatory variables.

The set of year dummies plays an important role in our analysis. The period we consider showed important economic and institutional changes, and these variables aim at controlling for those changes. After a long recession that started in 1998, Argentina suffered a severe crisis in 2001. As a consequence of the crisis, there was a large devaluation of the Argentine peso, and the government declared the default of its debt. In 2002, the GDP contracted by 10.8 %. In 2003 a period of growth started and lasted until the end of the program in 2007. An inflationary process accompanied the recovery. In the context of our study, controlling for these factors is important because the recovery implied an increase in employment, and the inflationary process implied an increase in nominal wages. As far as these factors affect beneficiaries and non-beneficiaries, the set of dummy variables controls for their influence in employment and real wages.

4 Empirical results

We estimate Eq. (2) using a multinomial logit model for the categories: received support for process innovation, received support for product innovation, and not treated (omitted category). Like in the binary case, the coefficients are not the marginal effect. However, in this case, they do not even provide the sign of the marginal effect, and therefore they are difficult to interpret, and it is better to concentrate the attention on the propensity score obtained according to Eq. (3).[6]

We apply nearest neighbor matching with replacement to find, for each beneficiary, a non-beneficiary with similar value of the propensity score. Figure 1 compares the distribution of the propensity score for the matched sample. Panel (a) shows the comparison between firms that received support for process innovation and non-beneficiaries and panel (b) the comparison between firms that received support for product innovation and non-beneficiaries. In both cases, the distribution of the propensity score for beneficiaries is equal to the distribution for non-beneficiaries showing that the matching was successful in finding firms with similar propensity score.

The validity of the difference-in-differences (fixed-effects) estimator rests on the identification assumption that trends in the outcomes would have been equal in absence of treatment. However, this assumption may be difficult to accept when firms in the control group are very heterogeneous and very different from the participating firms, since firms that are very different are likely to follow different trends as well. In order to lend support to this assumption, we include in the estimation of the propensity score the trends of employment and wages between 1996 and 1998. By doing this, we want to ensure that the non-beneficiary firms in our matched sample are similar to beneficiaries not only in terms of observed characteristics, but also in terms of the pre-treatment dynamic of the outcomes. We can then test the effectiveness of our matching strategy by testing the equality of trends in the outcome variables over the period before the program implementation.

Table 4 shows the balance test for the treatment "Support for process innovation." As shown in the first three columns of the table, beneficiaries are different from non-beneficiaries prior to matching—the null hypothesis of equality of means is rejected for all the variables we observe. This reflects the fact that beneficiaries self-selected into the program. Beneficiaries were on average larger, older, and paid higher wages than non-beneficiaries. The proportion of incorporated companies and exporters was also larger for beneficiaries than non-beneficiaries. However, as shown in the last three columns of Table 4, after matching it is not possible to reject the hypothesis that treated and control firms had the same characteristics, including the trend in employment and wages before the beginning of the program. Only two variables are not balanced in the matched sample: the proportion of firms in the northwest region and the proportion of firms in the sector

[6] Table 8 in the "Appendix" shows the results of the estimation.

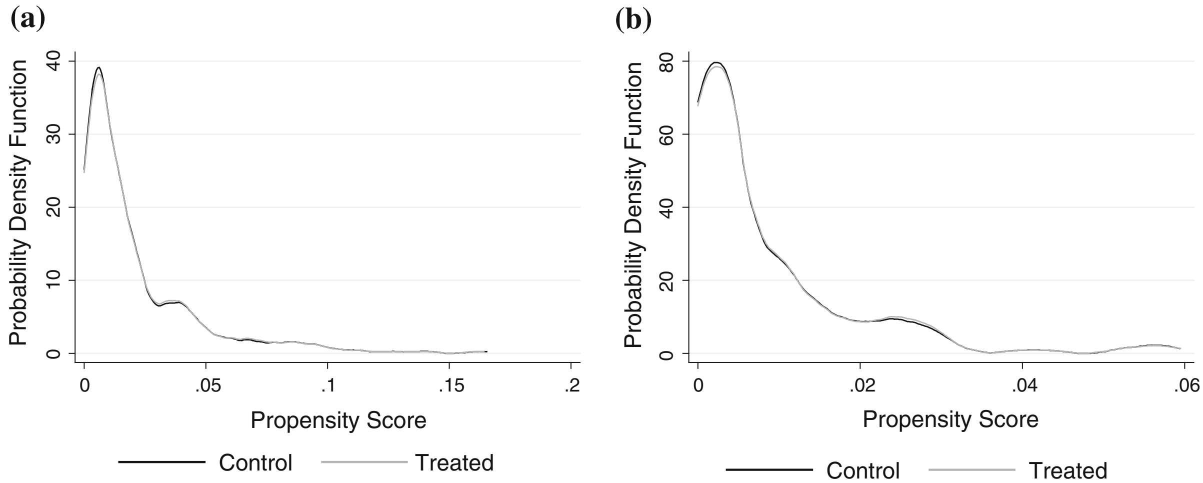

Fig. 1 Propensity score distributions. **a** Process innovation. **b** Product innovation. *Notes*: Firms in the matched sample

Hotels and Restaurants. However, even in these cases, the differences between treated and control group are quantitatively small: 5.8 versus 8.3 % in the first case and 1 versus 0.1 % in the second case. These slight differences in sector composition and firm location are clearly not a threat to our identification strategy, given that their influence on the outcomes of interest (if any) is likely to be constant over time and, therefore, captured by the fixed effects.

Table 4 also shows an overall test of balance. The pseudo *R*-square of a probit model for participation in support for process innovation activities vis-à-vis not participating in the program is 0.23 in the full sample. This means that the observables included in the model can explain the participation in support for process innovation activities. The likelihood ratio test rejects the null hypothesis that the model cannot explain participation with a *p* value of zero. When we estimate the same model in the sample of matched firms, the pseudo *R*-square is 0.01, and the test cannot reject the null hypothesis that the model does not explain participation. This finding allow us to conclude with confidence that treated and control firms in the matched samples were very similar before the inception of the program—even in terms of their growth rates in employment and wages—and that any difference we find between these samples during the period of the program's execution can be attributed to the participation in the program.

Table 5 shows the same balance tests for the treatment "Support for product innovation." As in the previous case, the matching is successful and identifies a sample of non-beneficiary firms that before the beginning of the program had the same observable characteristics—including their growth rates in employment and wages—as those firms that received support for product innovation. In this case, there are no unbalanced variables in the matched sample. It is interesting to note that the matching algorithm did not select any control firms from those industries where no beneficiary firms are operating—for example, fishing or mining.

The matched sample provides us with a group of treated and non-treated firms—for each type of support—that before the program had the same characteristics. Therefore, we can now focus on the effect of each type of support on employment and wages. Figure 2 shows the evolution of employment from 1996 to 2008 for firms that received support for process innovation and product innovation and for those firms that did not received support in both the unmatched and matched sample. This figure clearly shows that firms that received support—both for process and product—increased employment more than those firms that did not receive support; this is valid even if we only consider those firms in the matched sample, i.e., firms that before the implementation of the program had the same characteristics as treated firms. Figure 3 does the same for the average nominal wage paid by firms.

Table 4 Balance test, support for process innovation

Variable	Unmatched			Matched		
	Treated	Control	Diff.	Treated	Control	Diff.
Wages (in log)	6.281	5.948	0.333***	6.289	6.298	−0.009
Number of employees (in logs)	2.887	0.834	2.053***	3.002	3.020	−0.018
Number of employees squares (square of logs)	10.109	2.287	7.822***	10.485	10.491	−0.006
Age	14.232	8.014	6.218***	15.041	14.749	0.292
Age squared	407.970	175.270	232.7***	434.330	426.370	7.96
Workers experience	8.699	5.835	2.864***	9.040	8.700	0.34
Workers experience squared	145.520	93.653	51.867***	151.300	143.460	7.84
Proportion of women	0.214	0.256	−0.042***	0.212	0.205	0.007
Average growth in employment 1996–1998	0.222	0.112	0.11***	0.220	0.255	−0.035
Average growth of wages 1996–1998	0.016	−0.004	0.02***	0.016	0.018	−0.002
Dummy exported in 1998	0.254	0.012	0.242***	0.268	0.242	0.026
The owner of the firm is one person	0.093	0.572	−0.479***	0.084	0.087	−0.003
Incorporated company	0.856	0.226	0.63***	0.866	0.862	0.004
Other societies	0.043	0.112	−0.069***	0.041	0.044	−0.003
Located in Gran Buenos Aires	0.463	0.409	0.054***	0.463	0.442	0.021
Located in center	0.401	0.380	0.021	0.397	0.403	−0.006
Located in Cuyo	0.045	0.059	−0.014	0.047	0.046	0.001
Located in northeast	0.008	0.049	−0.041***	0.009	0.003	0.006
Located in northwest	0.059	0.054	0.005	0.058	0.083	−0.025*
Located in Patagonia	0.024	0.050	−0.026***	0.026	0.024	0.002
Agriculture, hunting and forestry	0.027	0.140	−0.113***	0.028	0.031	−0.003
Fishing	0.003	0.001	0.002**	0.003	0.003	0.000
Mining and quarrying	0.009	0.002	0.007***	0.010	0.011	−0.001
Manufacturing: Food and beverages	0.053	0.029	0.024***	0.057	0.058	−0.001
Manufacturing: textiles	0.035	0.018	0.017***	0.036	0.044	−0.008
Manufacturing: wood and paper	0.045	0.018	0.027***	0.047	0.058	−0.011
Manufacturing: chemical and plastic products	0.097	0.011	0.086***	0.095	0.090	0.005
Manufacturing: metals	0.107	0.022	0.085***	0.111	0.098	0.013
Manufacturing: machinery and equipment	0.069	0.006	0.063***	0.070	0.054	0.016
Manufacturing: electrical machinery and electronics	0.041	0.004	0.037***	0.041	0.036	0.005
Manufacturing: automobiles and motors	0.064	0.004	0.06***	0.067	0.060	0.007
Manufacturing: furniture	0.025	0.008	0.017***	0.027	0.030	−0.003
Electricity, gas, and water supply	0.004	0.001	0.003*	0.004	0.009	−0.005
Construction	0.053	0.041	0.012*	0.056	0.053	0.003
Wholesale and retail trade	0.187	0.260	−0.073***	0.179	0.201	−0.022
Hotels and restaurants	0.009	0.042	−0.033***	0.010	0.001	0.009**
Transport, storage and communications	0.035	0.093	−0.058***	0.030	0.040	−0.01
Financial intermediation	0.003	0.005	−0.002	0.003	0.004	−0.001
Real estate, renting and business activities	0.073	0.156	−0.083***	0.070	0.067	0.003
Education	0.004	0.015	−0.011**	0.004	0.001	0.003
Health and social work	0.040	0.042	−0.002	0.040	0.036	0.004
Other community, social and personal service activities	0.015	0.080	−0.065***	0.011	0.014	−0.003
Number of firms	749	354,455		749	651	

Table 4 continued

Variable	Unmatched			Matched		
	Treated	Control	Diff.	Treated	Control	Diff.
Pseudo R^2	0.230			0.013		
LR chi^2	2,333.89			25.64		
$p >$ chi^2	0.000			0.962		

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Panel A in Table 6 shows the estimates of Eq. (4) for the log of the number of employees; panel B does the same for the log of wages.[7] The first two columns show the effect of support for process innovation, and the last two columns show the effect of support for product innovation. The impact of the support for innovation—both process and product—on employment is quantitatively large and highly significant; while the support for process innovation increased employment by 22 %, support for product innovation increased employment by 19 %. The average number of employees of beneficiaries and non-beneficiaries in the matched sample in 1998 was close to 40 employees, and therefore 20 % represents approximately eight workers per firm. The employment distribution of firms is right-skewed, and therefore the median is lower than the average. The median level of employment is 25 employees, and therefore the effect on the median is 5 workers per firm.

The program also created better jobs measured by real wages. Even when our dependent variable is the average nominal wage, the effect of the support has to be interpreted in terms of real wages because we are including year dummies that control for the effect of all non-observables that vary over time and affect all the firms in the same way like the inflation rate. In addition, we are using a control group that is also affected by inflation. Although the impact of each type of support on real wages is lower than the effect on the quantity of employees, it is quantitatively significant. The support for process innovation increased real wages in 2 %, and the support for product innovation increased real wages in 4.4 %, more than two times larger than the effect of process innovation support.[8]

4.1 Robustness check

As we mentioned above, we control for the self-selection of firms into the program by assuming that willingness to participate is a function of unobserved variables that remain constant over time. Alternatively, firms may decide to participate in the program after observing the value of their previous outcome variables. In this case, the effect of the program can be identified by assuming that the expected value of the potential outcome in the absence of the program is independent of the treatment after conditioning on past outcome and other observable variables, i.e., $E\left(Y_{0,it}|Y_{i,t-1}, X_{it}, T_{i,t}\right) = E\left(Y_{0,it}|Y_{i,t-1}, X_{it}\right)$. In this case, the estimation equation is given by:

$$Y_{it} = \alpha_j + \lambda_{j,t} + \theta_j Y_{i,t-1} + \delta_j T_{j,i,t-1} + \gamma_j X_{it} + \varepsilon_{jit}, \quad j = (1,2) \tag{5}$$

where all the variables were defined after Eq. (4). Although it is not possible to know if the true model is

[7] The program was effective in increasing firm's competitiveness. We have two competitiveness measures available in our data set: firms' survival and exporting probability. Table 9 in the "Appendix" shows the impact of each type of support on these variables. To estimate these effects, we use linear probability models—i.e., we estimate Eq. (4) for each type of support considering two binary dependent variables: a dummy for survival and a dummy for exporters. These linear probability models allow us to control for unobserved time-invariant firm characteristics that can affect the decision of participating in the program and the performance in terms of survival and exports. Both process and product innovations support increased exporting and survival probabilities. However, the impact of product innovation support is higher. This result is consistent with the fact that firms with new or upgraded products can compete more efficiently in both the local and international market.

[8] This finding is similar to the findings for the survival and exporting probabilities in Table 9. These findings are also consistent with the high satisfaction of the beneficiaries with the program resulted from a satisfaction survey implemented by the program executing unit and with the fact that several firms that were first time users continued using professional services after their participation in the program.

Table 5 Balance test, support for product innovation

Variable	Unmatched			Matched		
	Treated	Control	Diff.	Treated	Control	Diff.
Wages (in log)	6.175	5.948	0.227***	6.190	6.265	−0.075
Number of employees (in logs)	2.813	0.834	1.979***	2.923	2.849	0.074
Number of employees squares (square of logs)	9.540	2.287	7.253***	9.855	9.455	0.4
Age	13.084	8.014	5.070***	13.711	13.038	0.673
Age squared	349.320	175.270	174.05***	366.740	372.970	−6.23
Workers experience	7.524	5.835	1.689***	7.855	7.349	0.506
Workers experience squared	107.340	93.653	13.687	112.060	108.450	3.61
Proportion of women	0.250	0.256	−0.006	0.246	0.246	0.000
Average growth in employment 1996–1998	0.190	0.112	0.078**	0.190	0.190	0.000
Average growth of wages 1996–1998	0.011	−0.004	0.015	0.011	0.024	−0.013
Dummy exported in 1998	0.293	0.012	0.281***	0.302	0.283	0.019
The owner of the firm is one person	0.108	0.572	−0.464***	0.094	0.075	0.019
Incorporated company	0.826	0.226	0.600	0.843	0.855	−0.012
Other societies	0.066	0.112	−0.046*	0.063	0.069	−0.006
Located in Gran Buenos Aires	0.473	0.409	0.064*	0.478	0.472	0.006
Located in center	0.401	0.380	0.021	0.390	0.358	0.032
Located in Cuyo	0.084	0.059	0.025	0.088	0.120	−0.032
Located in northeast	0.012	0.049	−0.037**	0.013	0.019	−0.006
Located in northwest	0.024	0.054	−0.03*	0.025	0.025	0.000
Located in Patagonia	0.006	0.050	−0.044***	0.006	0.006	0.000
Agriculture, hunting and forestry	0.042	0.140	−0.098***	0.044	0.044	0.000
Fishing	0.000	0.001	−0.001	0.000	0.000	0.000
Mining and quarrying	0.000	0.002	−0.002	0.000	0.000	0.000
Manufacturing: Food and beverages	0.090	0.029	0.061***	0.094	0.050	0.044
Manufacturing: Textiles	0.036	0.018	0.018*	0.038	0.019	0.019
Manufacturing: Wood and paper	0.048	0.018	0.030***	0.050	0.082	−0.032
Manufacturing: Chemical and plastic products	0.120	0.011	0.109***	0.120	0.113	0.007
Manufacturing: Metals	0.066	0.022	0.044***	0.069	0.082	−0.013
Manufacturing: Machinery and equipment	0.156	0.006	0.150***	0.151	0.145	0.006
Manufacturing: Electrical machinery and electronics	0.048	0.004	0.044***	0.050	0.069	−0.019
Manufacturing: Automobiles and motors	0.018	0.004	0.014***	0.019	0.031	−0.012
Manufacturing: Furniture	0.060	0.008	0.052***	0.063	0.082	−0.019
Electricity, gas, and water supply	0.000	0.001	−0.001	0.000	0.000	0.000
Construction	0.030	0.041	−0.011	0.025	0.019	0.006
Wholesale and retail trade	0.126	0.260	−0.134***	0.120	0.088	0.032
Hotels and restaurants	0.006	0.042	−0.036**	0.006	0.006	0.000
Transport, storage and communications	0.012	0.093	−0.081***	0.013	0.006	0.007
Financial intermediation	0.000	0.005	−0.005	0.000	0.000	0.000
Real estate, renting and business activities	0.108	0.156	−0.048*	0.107	0.107	0.000
Education	0.006	0.015	−0.009	0.000	0.000	0.000
Health and social work	0.018	0.042	−0.024	0.019	0.031	−0.012
Other community, social and personal service activities	0.012	0.080	−0.068***	0.013	0.025	−0.012
Number of firms	167	354,494		167	151	

Table 5 continued

Variable	Unmatched			Matched		
	Treated	Control	Diff.	Treated	Control	Diff.
Pseudo R^2	0.221			0.042		
LR chi^2	610.31			18.45		
$p > \text{chi}^2$	0.000			0.986		

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

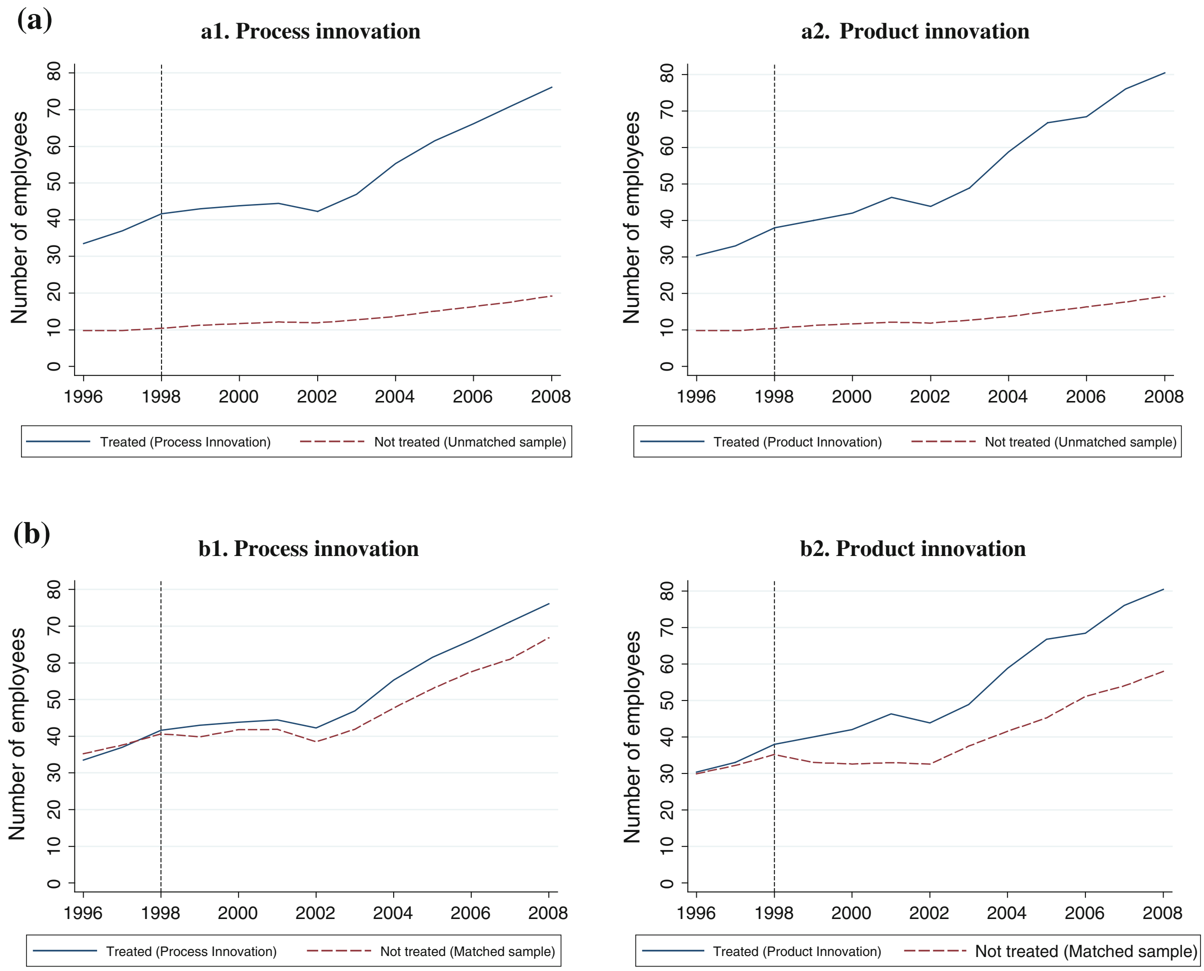

Fig. 2 The evolution of employment. **a** Unmatched sample. **b** Matched sample

given by Eq. (4) or by Eq. (5), the relationship between these equations is extremely useful. In fact, if the true model is given by Eq. (4), as we think it is, the estimate of δ_j from Eq. (5) will underestimate the true parameter and provide an inferior limit. On the other hand, if the true model is given by Eq. (5), the estimates of Eq. (4) overestimate the true parameter and provide an upper limit (Angrist and Pischke 2009, chap. 5). Table 7 shows the results of the estimation of Eq. (5). As expected, the values of the coefficients are lower than the one estimated in Table 6. However, they are all positive and statistically significant; this

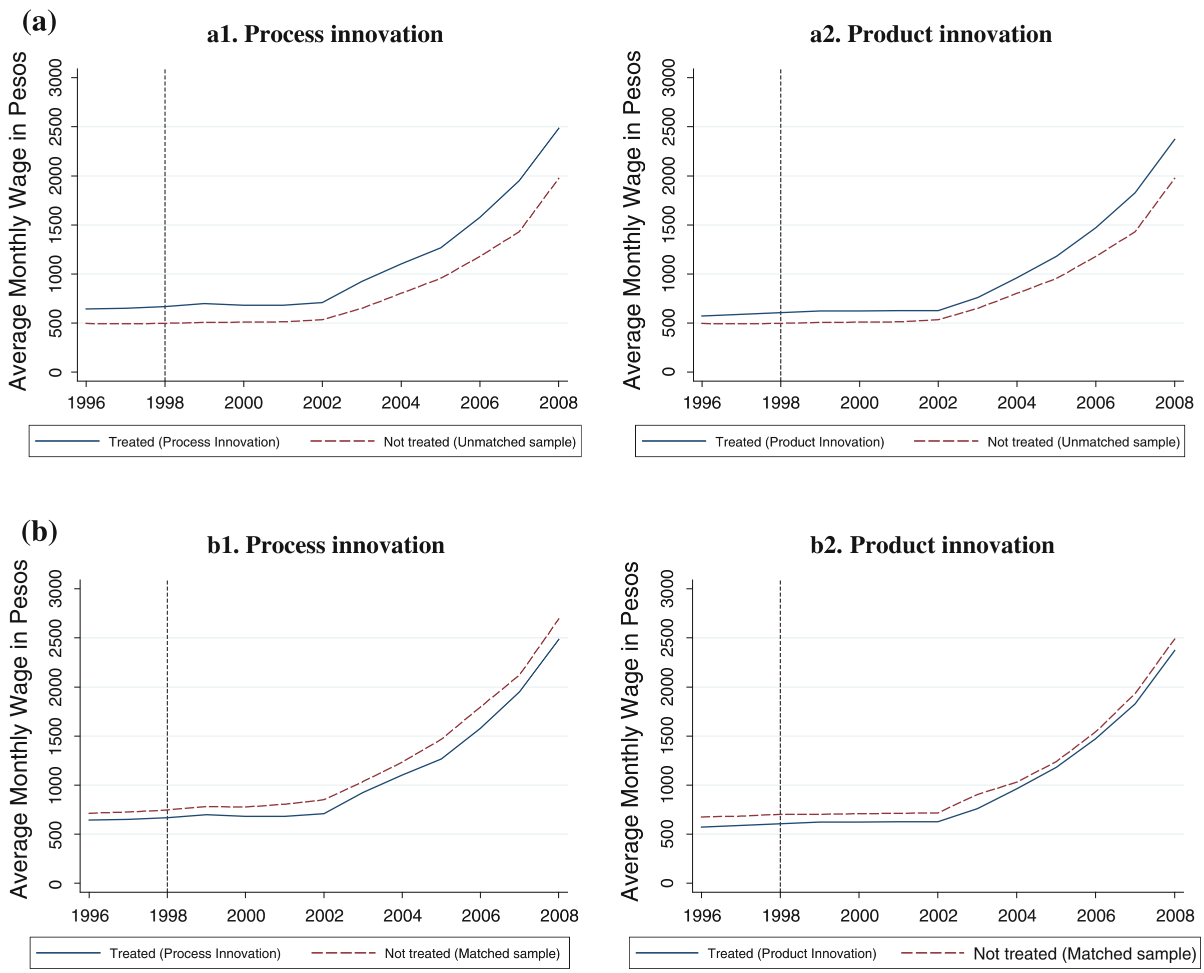

Fig. 3 The evolution of wages. **a** Unmatched sample. **b** Matched sample

means that the program was effective even if we consider the lower limit. The main conclusions remain valid; the effect on employment was considerably larger than the effect on wages, and both process and product innovation support increased employment in 11 %. The only result that does not prove robust is the higher impact of product innovation support on real wages; in this case, both product and process innovations support increased real wages in 2 %.

5 Conclusions

For many years, governments of both developed and developing countries have implemented programs aimed at promoting the adoption of new technologies and business practices by SMEs. This type of program is usually designed and implemented with the main objective of increasing firm-level efficiency and competitiveness under the assumption that by achieving these objectives more and better jobs will eventually be created. Our findings confirm that positive effects for both employment and wages can be achieved by implementing programs that support innovation and technical change. In addition, they also show that both product and process innovation support can result in increased employment and higher wages.

Using a quasi-experimental identification strategy, we find large effects on employment attributable to the program's support of both process and product innovation, with increases of 22 and 19 %, respectively. This means that the median firm employing 25 workers before participation was able to create five

Table 6 The impact of the program on employment and wages

	Support for process innovation		Support for product innovation	
	Unmatched sample [1a]	Matched sample [2a]	Unmatched sample [1b]	Matched sample [2b]
(A) Dependent variable: log(number of employees)				
PRE $t-1$	0.239***	0.223***	0.123***	0.192***
	[0.0143]	[0.0176]	[0.0318]	[0.0398]
Firm level fixed effects	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Age and age squared	Yes	Yes	Yes	Yes
R-squared	0.03	0.08	0.03	0.07
(B) Dependent variable: log(wages)				
PRE $t-1$	0.0425***	0.0195***	0.0459***	0.0437***
	[0.00572]	[0.00723]	[0.0128]	[0.0157]
Firm level fixed effects	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Age and age squared	Yes	Yes	Yes	Yes
R-squared	0.78	0.83	0.78	0.83
Number of observations	2,535,028	12,966	2,529,388	2,871
Number of firms	355,204	1,400	354,661	318

PRE is a dummy variable that takes value one after the firm receives the support. PRE $t-1$ is the lag of PRE. Robust standard errors in brackets

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

additional jobs per firm with the program support. The impact of the program on real wages, although lower than that on employment, is also quantitatively important. The program support for process innovation increased real wages by 2 %, while the support for product innovation increased real wages by 4 %.

These findings not only allow us to conclude that the PRE program created more and better jobs, but that it was also effective in fostering efficiency and competitiveness. In fact, if we couple our findings on employment and wages with the evidence of the program's positive effect on both firm survival and export, we can reasonably conclude that the program induced a relevant increase in labor productivity.

This study significantly contributes to the growing literature on the effectiveness of innovation policy for SMEs, by filling a key knowledge gap on its impact on employment. Very few previous studies have addressed this topic, and none has done so while distinguishing between support for product and process innovation. Although still rather heterogeneous, the limited comparable existing evidence is generally consistent with our findings. Among the studies surveyed by Hall and Maffioli (2008), those in Argentina, Chile, and Brazil included the effects on employment. Only the latter study detected a considerably positive and statistically significant effect (with an increase of 79 %) on employment. In the other cases, the effects were still positive, but lower and statistically non-significant. A more recent study on the Chilean case (Benavente et al. 2012) found positive and significant effects on employment of the FONTEC program (with an increase of 4.6 %). Finally, also reported are the positive and significant effect innovation programs on employment in Mexico (between 6 and 15 % depending on the specification) and positive and significant effects on wages in Chile (with increases between 8.5 and 5 % depending on the program), Colombia (1 %), and Mexico (5 %).

This study also significantly contributes to the policymaking debate in emerging economies. Competitiveness and employment are often considered contrasting or even opposing objectives, at least in the short run. Our article shows that policy instruments aimed at fostering innovation and competitiveness in emerging countries are likely to produce significant

Table 7 Robustness check

	Support for process innovation	Support for product innovation
Dependent variable: log(number of employees)		
log(number of employees, $t-1$)	0.908***	0.907***
	[0.0006]	[0.0006]
PRE $t-1$	0.113***	0.107***
	[0.0086]	[0.0197]
Firm-level random effects	Yes	Yes
Year dummies	Yes	Yes
Age and age squared	Yes	Yes
R-squared	0.91	0.90
Dependent variable: log(wage)		
log(wage, $t-1$)	0.868***	0.868***
	[0.0008]	[0.0008]
PRE $t-1$	0.027***	0.022***
	[0.00248]	[0.00697]
Firm-level random effects	Yes	Yes
Year dummies	Yes	Yes
Age and age squared	Yes	Yes
R-squared	0.90	0.90
Number of observations	2,179,824	2,174,727
Number of firms	317,278	316,735

PRE is a dummy variable that takes value one after the firm receives the support. PRE $t-1$ is the lag of PRE. Robust standard errors in brackets

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

positive effects in terms of employment even in a relatively short timeframe.

This work also opens new challenges for future research. First, our analysis is limited to the estimation of the program's effectiveness. The cost-effectiveness analysis would require a detailed estimation of the program's costs and is beyond the scope of this study because of data availability. In addition, programs such as the PRE that focus on the creation and diffusion of knowledge are subject to potential spillover effects. If these spillovers occurred in the present context, our estimates could underestimate the effect of the program, as some of the firms we are considering as controls could have benefitted from this positive of spillovers. The study of spillovers would require identifying the mechanism through which the spillovers take place and is also out of the scope of this paper. For instance, spillovers may have occurred through labor mobility: employees of beneficiaries firms may have first acquired valuable knowledge on the new product or process introduced with the program support and then moved to other non-beneficiary firms taking this knowledge with them. Finally, the PRE program also explicitly aimed at strengthening the supply side of the market of business services for SMEs. The estimation of the program impact on these providers could be another question to be answered by studies on this or similar programs.

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Appendix

See Tables 8 and 9.

Table 8 The probability of participating in each activity

	Support for product innovation		Support for process innovation	
	Coef.	SE	Coef.	SE
Wages (in logs)	−0.132	0.179	0.118	0.084
Number of employees (in logs)	1.959	0.285***	1.675	0.126***
Number of employees (square of logs)	−0.236	0.045***	−0.191	0.019***
Age	0.038	0.027	0.024	0.014*
Age squared	0.000	0.000	0.000	0.000
Workers experience	−0.029	0.057	−0.014	0.026
Workers experience squared	−0.002	0.002	−0.001	0.001
Proportion of women	0.827	0.363**	−0.081	0.195
Average growth in employment 1996–1998	−0.080	0.224	0.165	0.095*
Average growth in wages 1996	0.347	0.631	0.550	0.275**
Dummy exported in 1998	0.664	0.232***	0.706	0.120***
The owner of the firm is one person	16.965	1.677***	0.719	0.458
Incorporated company	18.297	1.703***	2.137	0.442***
Other societies	17.572	1.704***	0.977	0.476**
Located in Gran Buenos Aires	1.265	1.010	0.003	0.247
Located in center	1.896	1.011*	0.669	0.248***
Located in Cuyo	1.802	1.038*	0.052	0.297
Located in northeast	0.802	1.229	−0.904	0.475*
Located in northwest	1.108	1.121	0.653	0.287**
Agriculture, hunting and forestry	0.217	0.816	−0.126	0.434
Fishing	−30.322	9,633,255.000[a]	0.784	0.806
Mining and quarrying	−29.800	5,637,620.000[a]	1.485	0.535***
Manufacturing: food and beverages	1.325	0.763*	0.946	0.403**
Manufacturing: textiles	0.579	0.823	0.883	0.421**
Manufacturing: wood and paper	1.373	0.801*	1.334	0.410***
Manufacturing: chemical and plastic products	1.944	0.763**	1.702	0.396***
Manufacturing: metals	1.375	0.787*	1.779	0.392***
Manufacturing: machinery and equipment	2.805	0.763***	1.938	0.406***
Manufacturing: electrical machinery and electronics	2.394	0.812***	2.157	0.422***
Manufacturing: automobiles and motors	1.230	0.933	2.320	0.406***
Manufacturing: furniture	2.342	0.789***	1.565	0.439***
Electricity, gas, and water supply	−28.405	5,433,679.000[a]	1.519	0.691**
Construction	−0.158	0.885	0.710	0.408*
Wholesale and retail trade	0.220	0.748	0.773	0.378**
Hotels and restaurants	−1.151	1.226	−0.435	0.527
Transport, storage and communications	−0.640	1.008	0.164	0.429
Financial intermediation	−29.628	3,374,209.000[a]	−0.292	0.800
Real estate, renting and business activities	1.130	0.750	0.893	0.391**
Education	−30.063	1,947,634.000[a]	−0.876	0.685
Health and social work	−0.386	0.925	0.898	0.418**
Constant	−29.791	.	−12.048	0.781***
Number of observations				355,363
LR chi^2 (*df*) (*p* value)				2,928.99 (82) (0.0000)

Table 8 continued

	Support for product innovation		Support for process innovation	
	Coef.	SE	Coef.	SE
Log likelihood				−4,992.99
Pseudo R^2				0.23

Multinomial logistic regression

[a] There are no beneficiaries in these industries

Table 9 The impact of the program on the exporting and survival probabilities

	Support for process innovation		Support for product innovation	
	Unmatched sample	Matched sample	Unmatched sample	Matched sample
Dependent variable: dummy exports in period *t*				
PRE $t-1$	0.0482***	0.0293***	0.0739***	0.0582***
	[0.00714]	[0.00843]	[0.0165]	[0.0202]
Age	0.00114***	0.00613***	0.00112***	0.00674*
	[4.31e−05]	[0.00157]	[4.28e−05]	[0.00374]
Age squared	−2.07e−06**	−4.95e−05**	−2.12e−06**	−0.000122**
	[9.59e−07]	[2.51e−05]	[9.53e−07]	[6.00e−05]
R-squared	0.00	0.02	0.00	0.02
Dependent variable: survives in period *t*				
PRE $t-1$	0.0288***	0.00659*	0.0326***	0.0218**
	[0.00322]	[0.00393]	[0.00701]	[0.00995]
Age	−0.0144***	−0.00744***	−0.0144***	−0.00884***
	[5.34e−05]	[0.000640]	[5.35e−05]	[0.00139]
Age squared	0.000113***	5.30e−05***	0.000113***	6.33e−05***
	[1.01e−06]	[9.97e−06]	[1.02e−06]	[1.98e−05]
R-squared	0.06	0.03	0.06	0.04
Number of obs.	2,535,028	12,966	2,529,388	2,871
Number of firms	355,204	1,400	354,661	318

All equations include firm level fixed-effects. Robust standard errors in brackets

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

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