Regional policy evaluation in absence of an experimental design: is it possible?

Simona Comi[a] *and Laura Resmini*[a,b]

June 30th, 2017

First draft: not to quote without permission

Abstract

In a world of stringent public budget constraints, policy evaluation is extremely important, since it allows to understand whether a policy intervention is able to achieve its objective. However, rigorous analyses are difficult to carry out in absence of an experimental design. Experimental evaluation is based on the assumption that the assignment to the treatment, i.e. the participation in the policy intervention, is random. However, participation in public programs is intentionally non-random, therefore those receiving the treatment and those excluded from it may differ not only in their treatment status but also in other observed or unobserved characteristics. The challenge thus is obtaining a credible estimate of the counterfactual outcome while controlling for any possible source of selection bias. In this paper we discuss how this can be done. In so doing, we analyze in more depth the results obtained in assessing the impact of the export promotion policy implemented by Lombardy Region, one of the richest and most advanced regions in Italy, over the period 2010-2014. We start by applying a difference-in-difference fixed effect estimator and then perform different tests to verify that treated and control units do not systematically differ in their main observable characteristics. Eventually we report consistently positive and robust effects of export promotion activities implemented by Lombardy Region. We confirm that policy evaluation can be thoroughly carried out even in absence of an experimental design, provided that good quality administrative data are collected.

JEL code: F13, R50

Key-world: policy evaluation, export promotion policy.

[a] Dept. of Sociology and Social Research – University of Milano-Bicocca – via Bicocca degli Arcimboldi, 8 – 20126 Milano (Italy) (simona.comi@unimib.it)

[b] CERTeT, Università "L. Bocconi" – Via Roentghen 1 – 20136 Milano (Italy) (laura.resmini@unimib.it) – corresponding author.

Regional policy evaluation in absence of an experimental design: is it possible?

1. Introduction

Most governments consider exporting activity as an important tool for enhancing local development; therefore, they have taken many initiatives in encouraging firms to export. These initiatives include the provision of a wide range of services, such as information on business opportunities offered by foreign markets, technical counselling and training on export process, organization of promotional events, like business missions abroad, and promotion of participation in international trade fairs (Lederman et al., 2009). Despite the relevant amount of public resources invested in those kind of activities around the world, few empirical studies have analysed their effectiveness in increasing the volume of exported goods or improving firms' competitiveness abroad (Broocks, Van Biesebroeck, 2017; Volpe Martincus, Carballo, 2010; Gorg, Strobl, 2008). The main reason explaining this fact is not the lack of interest for the issue – whose importance is crucial in the present context of public budget constraints – but mainly the lack of appropriate data and the difficulties with a correct identification of the impact of the policy intervention in absence of an experimental design.

Experimental evaluation is based on the assumption that the assignment to the treatment, i.e. the participation in the policy intervention, is random. However, for many interventions, random assignment is not a feasible approach. First, random assignment is costly since it has to be developed before the implementation of the policy in co-operation with those who are in charge to manage the policy program. Secondly, participation in several public programs is intentionally non-random, and often is the results of an autonomous decision of participants who self-select themselves for participating in the public program. In such a situation, those receiving the treatment and those excluded from it may differ not only in their treatment status but also in other observed or unobserved characteristics that affect both participation and the outcome of interest. Therefore, the challenge is obtaining a credible estimate of the counterfactual while controlling for any possible source of selection bias.

In this paper we discuss how this can be done using data from an administrative archive. In describing the proposed evaluation strategy, we exploit the results obtained in analysing the impact of the export promotion policy implemented by Lombardy Region, one of the richest and most advanced regions in Italy, over the period 2010-2014 (Eupolis, 2016). In an attempt to make local firms more internationally competitive, Lombardy Region has implemented an extensive export support system consisting in financial grants to firms willing to increase their export performance through the acquisition of more competence on export processes and the participation in business missions abroad or international trade fairs. The impact of the export promotion program has been evaluated using a fixed-effect difference-in-difference estimator (FE-DID), which has the advantage of minimizing the distortions generated by the self-selection bias that usually affect *ex-post* policy evaluation. Despite its undeniable advantages, the estimates obtained with such estimator may still be inaccurate because of the effect of time-varying unobservables not properly controlled for or because a wrong specification of the evaluation exercise.

In order to overcome these shortcomings and obtain estimations as much precise as possible, we perform several robustness checks based on a different selection of treated vs control units, as well as controlling for the presence of a pre-treatment common trend. Eventually we report consistently positive effects of export promotion activities implemented by Lombardy Region.

The remainder of the paper is organized as follows. Section 2 reviews the main features of export promotion program implemented by Lombardy region while Section 3 describes the construction of the data set. Section 4 discusses the empirical strategy. Section 5 compares the main results, while Section 6 concludes.

2. Export promotion policies at regional level: vouchers for internationalization

The regional system to support the internationalization process of local firms includes a broad set of activities, encompassing most of the traditional policy interventions in this field, i.e. promotional activities, training and counselling.[1] The main objective is to increase the number of regularly exporting firms by improving firms' capacity to enter foreign markets. In order to achieve this objective, the Lombardy region has made a considerable financial effort, investing on average about €6 million per year in the considered period (2010-2014) and assisting more than 5,000 local firms, especially micro, small and medium-sized ones, which, generally speaking, lack the specific human and financial resources needed to face the challenges posed by internationalization. These figures reflect the growing commitment of Lombardy Region – in a context of severe constraints on public spending – in offering local firms a wide range of incentives for internationalization.[2]

The strengthening of SMEs' internationalization strategies is addressed by means of vouchers for internationalization, i.e. grants for firms that needs specific services, like market analyses or counselling on export procedures (Voucher A), or that wish to participate in institutionally-led business missions (Voucher B) or international trade fairs both individually (Voucher C1) or in group (Voucher C2). More specifically, each voucher represents a non-repayable financial support covering part of the expenses related to the provision of the services included in the voucher.

This financial support varies according to the typology of the voucher and the geographical area where business missions and trade fairs are held. Services covered by vouchers are not provided directly by Lombardy Region, but by accredited private institutions, which are in competition among them. Therefore, the vouchers have a secondary objective, i.e. developing a market for export promotion services: SMEs using the vouchers learn to

[1]Regional export support system does not include the provision of information services. However, firms interested in obtaining specific information on international markets, such as external prices, market regulations, trade barriers and so on, may address to the national Italian Trade Agency.

[2] Lombardy Region is not the only institution involved in export promotion activities. The regional program has been developed, implemented and financed in cooperation with other private or public institutions, like the Lombardy system of Chambers of Commerce. Italian Chambers of Commerce are independent public authorities performing functions of general interest for the business system, with a specific focus on local economic and business systems. Each Chamber of Commerce has its own articles of association, defines its own political program and is independent from a financial and management point of view.

appreciate the value of these services, while potential providers compete among them and are encouraged to provide high-quality export promotion services in order to be accredited by Lombardy Region.

All SMEs located or operating in Lombardy are eligible for export support; however, to access the voucher, they must intervene with a co-financing, which is usually larger than the public support. Firms may apply for multiple vouchers each year, but the total amount of the grant cannot exceed €15,000.[3]

This paper focuses on the effectiveness of Voucher C, the most successful ones among the different kinds of voucher offered by Lombardy Region. Voucher C absorbs more than the half of public resources devoted to export promotion incentives and it seems to be the most preferred by local firms, since about 58.5% of firms assisted by Lombardy Region through vouchers for internationalization have applied for (and received) Voucher C in the considered period (Eupolis, 2016).

3. The data set

The dataset we used to demonstrate how it is possible to perform sound policy evaluation exercises even in absence of an experimental design covers the period from 2010 to 2014 and was constructed by merging information taken from two different databases. In particular, we used the administrative data collected by Lombardy Region when firms apply for any voucher, supplemented with information taken from the ASIA dataset, built by the Italian National Statistical Office.

Administrative data include several qualitative and quantitative information on SMEs that have applied for the vouchers during the period from 2010 to 2014. For each application, the recorded information include the name of the firm, its VAT number, complete address, the founding year of each firm, sector of activity (ATECO 2007, 4 digits), total and export turnovers of the last three years, number of full-time equivalent employees of the last three years, as well as detailed information on the type and the number of the vouchers requested and obtained by each applicant.

Eventually we end up with an unbalanced panel dataset including more than 5,000 SMEs operating in Lombardy that have applied for at least one voucher during the considered period. We assigned to the group of treated those firms which got a voucher C in 2012 (494) and to the control group firms that did not apply for voucher C in 2012 (2171). In so doing, we could observe each firm from 2010 to 2013, i.e. two years before the treatment, and two years after it.[4] We end up with 2465 firms and a total of more than 8 thousands observations. As it can be seen in Table A1, treated firms in 2011, i.e. before the treatment, were larger, slightly older and more productive than firms included in the control group. Moreover, they exported a higher percentage of their total turnover.

[3] As for example, in 2012 firms were allowed to apply for up to two Vouchers A and up to three vouchers B and C.
[4] In order to focus on the effect of the voucher C received in 2012, we excluded from the sample those firms that applied for a voucher C in 2013 too.

4. The Empirical Strategy

As discussed in the Introductory Section, the main two issues with counterfactual evaluation of policies are the choice of a suitable methodology able to provide reliable results in a quasi-experimental setting, and the choice of suitable control groups. The policy we are evaluating and the administrative longitudinal data we have allow us to use a fixed effect difference in difference (FE-DID) model of the following form:

$$Y_{it} = \alpha + \beta_1 \times TREATED_i \times Post_t + \tau_t + \mu_i + \theta X_{it} + \varepsilon_{it} \quad (1)$$

where "*it*" denotes the *i*-th firm at time *t*. *Y* is a measure of firm performance, *TREATED* is a dummy for the treated firms, $Post_t$ is a dummy for the years after the subsidies were granted, τ_t and μ_i are, respectively, time and firm fixed effects, *X* is a vector of time-varying firms' characteristics and ε is the error term.

Throughout the paper, we use fixed effect estimators to remove from equation (1) the firm fixed-effects, μ_i. Fixed effects estimators partly control for self-selection bias into treatment. In fact, it allows treatment to be correlated with time-constant heterogeneity, which is completely taken into account, but does not allow treatment in any time period to be correlated with idiosyncratic changes in the counterfactuals. Indeed, these estimates are consistent in this setting if the assignment of firms to the policy is strictly exogenous in year *t*, i.e. it is not correlated with the past, present of future error term, ε_{it}. Eventually, the bias is small and negligible whenever we can assume contemporaneous exogeneity ($Cov(TREATED_{it}; \varepsilon_{it})=0$), i.e. the assignment to the policy in year *t* does not depend on the unobservable time-varying characteristics of the firm in the same year (Imbens and Wooldridge, 2009). Of course, year fixed-effects control for any change overcoming all firms in any given year. In this specification, the parameter of interested is β_1.

When researchers use DID estimator, two assumptions must hold in order to get valid results. The first is the existence of a parallel trend between treated and control groups in each outcomes. In fact, DID estimates are valid only if one can provide evidence of the existence of a parallel trend regard to each outcomes for treated and controls in the absence of the treatment. We test for the presence of a pre-treatment parallel trend as in Muralidharana and Prakash (2013) using the following equation:

$$Y_{it} = \alpha + \gamma_1 \times TREATED_{it} \times TREND + \gamma_2 TREND_t + \theta X_{it} + \varepsilon_{it} \quad (2)$$

where the variable *TREND* is a linear trend that takes the value of one in 2008 and ends in 2011, the year before the introduction of the policy and the others variables are defined as in equation (1). A not statistically significant estimation of the coefficient of the interacted term, γ_1, will eventually confirm the existence of the parallel trend and validate the estimation of the effect of Voucher C on firm performance.

Secondly, a DID estimator is sensitive to every event that may happen at the same time of the treatment and affects treated and control groups differently. This is not a concern in our case, since the export promotion policy design allow us to find a control group extremely similar to the treated firms, exposed to the same policy because located in the same region and not statistically different with respect of the main observable characteristics.

Another crucial choice in carrying out a policy evaluation even in absence of an experimental design is the choice of plausible counterfactual samples. Given that there is no natural control group the "treated" firms should be compared to, we start our analysis comparing firms that received the grant in 2012 to all the firms in our dataset that did not receive a voucher C in 2012. We then refine our analysis, and focus on those treated that received the grant only in 2012 (197) and compare them to those firms that never received a voucher C (1433). Besides this, we follow the literature on pre-treatment matching in panel fixed effect estimation and carefully reselect our control sample. Of course, we have to be able to create a control group as similar as possible to our treatment group on observables (balancing property of matching methods) aware that, even if we fail, fixed effects will control for differences in the means of the variables. Then, we can re-estimate our equation under the assumption of a similar distribution of unobservable characteristics between treated and control firms. In order not to have identification of our coefficients relying on residual unobserved heterogeneity, we restrict our sample to a common support and exclude from the analysis treated firms that are outside using a propensity score estimation. These firms will not be used in our analysis.

We carefully selected two control groups using a semiparametric ex-ante matching approach (Abadie et al., 2004). In particular, we identified suitable control groups among all the non-treated firms (Control 1) and among the non-treated firms who did not apply for Voucher C in any year (Control 2). For each treated firm in 2011, we have identified the closest five firms in each control group based on observable characteristics like industry, province, age of the firm, full time equivalent employees, and (log) labor productivity, turnover and pre-treatment outcome (export intensity) allowing for replacement. The advantage of having two controls group rely on the way they are defined. The firms in the first group, Control 1, probably at one point in time different from 2012, self-selected themselves into the treatment as the treated firms did in 2012. If they share the same self-selection process into the treatment with the treated firms, and have similar levels of unobservable characteristics upon with this process is based, the estimated results obtained with this control group should have a very small selection bias and could act as a lower bound to the effect of the policy. The firms in the second control group, Control 2, never applied for the grant and, even if balanced in term of means of observable characteristics, are more likely to violate the assumption of similar distribution of unobservables. Therefore, estimated results obtained with this control group can act as an upper bound of the effect of the policy.

As for the variables we used, we consider as outcome firms' performance on external markets, and use export turnover on total turnover as a measure of export intensity. Time-variant firms' characteristics, instead, include a second order polynomial in age, the size of each firm, measured as the number of full time equivalent employees, and the average labour productivity. We expect that, *ceteris paribus*, export performance of large, old and more efficient firms is better than that of small, young and less productive firms. Finally, the treatment variable is a dummy equal to one if firms received voucher C in 2012 and zero otherwise. Next section discusses main results. They confirm that policy evaluation can be thoroughly carried out even in absence of an experimental design, provided that good quality administrative data are collected.

5. Results

Table 1 compares Fixed-Effect DID estimates with those obtained with other two feasible estimators, i.e. random effect DID estimator and a more traditional OLS-DID. The estimate of 0.0146 in line and column one of Table 1 implies that Voucher C raises export intensity by 1.4 percentage point. It is significantly different from zero using robust standard errors and the magnitude, compared with less restrictive specifications, indicates that the risk of overestimating the impact of the policy is minimized, since, as expected, a FE-DID estimator allows to control for any possible bias due to time-invariant unobservables. For this reason, we consider estimates reported in column (1) of Table 1 as our benchmark and use these conservative estimates as a starting point for our robustness checks.

Table 1. FE-DID, RE-DID and OLS-DID estimates

VARIABLES	(1) FE-DID	(2) RE-DID	(3) OLS-DID
Voucher C	0.0146***	0.0225***	0.143***
	(0.00519)	(0.00512)	(0.0125)
Employees	0.00107***	0.00130***	0.00139***
	(0.000304)	(0.000180)	(0.000142)
Labour productivity (log)	0.0332***	0.0352***	0.0397***
	(0.00444)	(0.00349)	(0.00408)
Age	0.0215***	-0.000341	-0.00402***
	(0.00296)	(0.00169)	(0.00144)
Age^2	-0.000343***	-3.50e-05	0.000151**
	(0.000111)	(7.11e-05)	(6.19e-05)
Small firm (dummy)		0.0497***	0.0458***
		(0.0118)	(0.00814)
Medium-sized firm		0.0683***	0.0580***
		(0.0171)	(0.0125)
Time dummies	Yes	Yes	Yes
Sector dummies	No	Yes	Yes
Province dummies	No	Yes	Yes
Firm fixed effect	Yes		
Constant	-0.313***	-0.203***	-0.264***
	(0.0581)	(0.0510)	(0.0527)
Observations	8,356	8,344	8,344
R-squared	0.053		0.175
Number of id	3,553	3,546	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

As previously discussed, we need to demonstrate that also estimates obtained with a FE-DID estimator are reliable. This implies, first, that the specification we used is correct, and secondly, that the potential bias due to time-varying unobservables is negligible and does not affect the impact of the policy instrument.

As for the specification of the model, Table 2 is quite informative. First, the impact of Voucher C is consistently positive across different specification, and its magnitude does not change by adding further controls. As for the latter, we found that the share of export turnover on total turnover increases with the size of the firms, measured by the number of full-time equivalent employees, the age of the firm and its productivity. These results are not surprising since they are consistent with the main theories on firms' internationalization, according to which only larger, more efficient and more experienced firms are able to face challenges posed by internationalization (Melitz and Redding, 2014, Bernard and Jensen, 1999).

Table 2. FE-DID: different specifications of the model

VARIABLES	(1)	(2)	(3)	(4)
Voucher C	0.0142***	0.0145***	0.0141***	0.0146***
	(0.00522)	(0.00522)	(0.00519)	(0.00519)
Employees		0.000535*	0.00111***	0.00107***
		(0.000297)	(0.000304)	(0.000304)
Labour productivity (log)			0.0341***	0.0332***
			(0.00443)	(0.00444)
Age				0.0215***
				(0.00296)
Age^2				-0.000343***
				(0.000111)
Time dummies	Yes	Yes	Yes	Yes
Constant	0.282***	0.270***	-0.159***	-0.313***
	(0.00194)	(0.00698)	(0.0562)	(0.0581)
Observations	8,356	8,356	8,356	8,356
R-squared	0.038	0.039	0.051	0.053
Number of id	3,553	3,553	3,553	3,553

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Needless to say, the causal interpretation of this estimate relies on the assumption that selection into treatment is random once we condition on a sufficiently large set of observables and time-invariant unobservables, i.e. treated and untreated firms are sufficiently similar. In order to be sure that this assumption holds in the present context, we estimate the pre-treatment trend in the outcome of both treated and untreated firms. We found that the outcome pre-treatment trend for supported firms is not statistically different from the trend of non-treated firms, as Table 3 shows. Therefore, we can conclude that selection into the treatment is not due to systematic differences between treated and control firms not captured by the FE-DID estimators, i.e. by the presence of time-varying unobservables.

Since the selection of the control group is crucial in ex-post policy evaluation, we further explore the marginality of the selection-bias in two different ways. First, we apply an *ex-ante* matching technique test as described above and compare the treated firms to our Control 1 group. As it can be seen in Table A2, after the matching procedure, those two groups are pretty similar and do not statistically differ along many relevant observables characteristics in 2011.

Table 3 Parallel Test Trend -FE-DID

VARIABLES	(1)
Trend_treated	0.00202
	(0.00335)
Trend	0.0102***
	(0.00181)
Labour productivity	1.16e-05
	(7.94e-06)
Emplyees	0.000599
	(0.000365)
Constant	0.225***
	(0.0108)
Observations	6,883
Number of id	3,287
R-squared	0.016

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Given this strong result, we expected to find again a positive impact of Voucher C on firm performance, Table 4 confirm our prior, though the estimated coefficient associated to the voucher C in 2012 is slightly lower (Column 1). We also re-run the parallel trend test (Column 2 of Table 4) and conclude that again we have control and treated firms with a parallel trend in the export intensity before the treatment.

Table 4: FE-DID after ex-ante matching (1) and parallel Trend test (2)

VARIABLES	(1)	(2)
Voucher C	0.0122**	
	(0.00489)	
Trend*treated		-0.00176
		(0.00181)
Trend		0.0144***
		(0.000922)
Employees	0.00124***	0.00139***
	(0.000293)	(0.000241)
Labour productivity (log)	0.0475***	0.0382***
	(0.00430)	(0.00391)
Age	0.0142***	
	(0.00304)	
Age2	-6.75e-06	
	(0.000110)	
Time dummies	Yes	NO
Constant	-0.379***	-0.165***
	(0.0576)	(0.0501)
Observations	8,166	8,765
R-squared	0.063	0.057
Number of id	1,750	1,750

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Treated: all treated firms. Controls: all non-treated firms matched to treated ones.

Secondly, we re-estimate eq. (1) on a sub-sample of treated and control firms. In particular, we considered as treated those firms who received Voucher C in 2012 and as control those firms who have never received Voucher C in the considered period. These two group are probably very similar and they only differ with respect to the self-selection process in 2012. Thus, even if balanced in term of means of observable characteristics, in this case we are more likely to violate the assumption of similar distribution of unobservables, and we expect to have an upper bias, if any.

Results, reported in Table 5 confirm the positive impact of the treatment. Firms benefitting from Voucher C still enjoy an export intensity of 1.4 percentage point higher than the control group. Therefore, adding firm-fixed effects in the regression yields to more conservative estimates than alternative estimators, though self-selection on a time-varying unobservables is still possible. However, it does not represent a big concern, as indicated by the results of the common-trend test, shown in Table 6, according to which the two groups differ only for the treatment status.

Table 5 FE-DID, RE-DID and OLS-DID: restricted sample

VARIABLES	(1) FE-DID	(2) rE-DID	(3) OLS-DID
Voucher C	0.0146*	0.0222***	0.115***
	(0.00813)	(0.00798)	(0.0176)
Employees	0.00122***	0.00151***	0.00169***
	(0.000420)	(0.000233)	(0.000185)
Labour productivity (log)	0.0359***	0.0325***	0.0298***
	(0.00512)	(0.00389)	(0.00417)
Age	0.0236***	2.13e-05	-0.00286*
	(0.00357)	(0.00198)	(0.00173)
Age^2	-0.000460***	-0.000133	1.73e-05
	(0.000137)	(8.83e-05)	(7.92e-05)
Small firm (dummy)		0.0364***	0.0363***
		(0.0126)	(0.00878)
Medium-sized firm (dummy)		0.0524***	0.0433***
		(0.0196)	(0.0143)
Time dummies	Yes	Yes	Yes
Sector dummies	No	Yes	Yes
Province dummies	No	Yes	Yes
Firm fixed effects	Yes		
Constant	-0.404***	-0.171***	-0.137**
	(0.0672)	(0.0574)	(0.0546)
Observations	6,000	5,989	5,989
R-squared	0.050		0.159
Number of id	2,652	2,646	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6 Common trend: restricted sample

VARIABLES	(1) PARALLEL TREND TEST-
trend_treated	0.00561
	(0.00486)
Trend	0.00563***
	(0.00210)
Labour productivity (log)	0.0209***
	(0.00539)
Employees	0.00118**
	(0.000471)
Constant	-0.0755
	(0.0672)
Observations	4,826
Number of id	2,427
R-squared	0.016

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Finally, we apply the *ex-ante* matching technique described above to this sample, and compare the treated firms (those who received the grant only in 2012) to our Control 2 group. As it can be seen in Table A3, after the matching procedure, those two groups are quite similar and do not statistically differ along many relevant observables characteristics in 2011. We can thus assume that we have a similar distribution of unobservables in the two samples, even if we are aware that due to some unobservables shock in 2012, the firms in the treated group may have self-seleceted themselves into the grant. As expected, the effect of the voucher is now slightly higher (Table 7) and represents an upper bound to the true effect of the voucher C on export intensity. We again run the parallel trend test and definitively conclude that, before the treatment, the export intensity trends of control and treated firms did not systematically differ one from the other.

6. Concluding remarks

This paper dealt with *ex-post* policy evaluation and shows that it is possible to provide robust and reliable estimates even in absence of a random assignment to the treatment. In order to demonstrate this, we provided evidence on the benefit of export promotion program implemented by Lombardy region. We found that Vouchers for internationalization and, more precisely, Voucher supporting the participation of local SMEs to international trade fairs (Voucher C), help firms raising the share of export turnover on total turnover of about 1.4 percentage points.

Vouchers for internationalization are targeted to SMEs, therefore, self-selection into export promotion support is likely to be a serious concern. We tackled this issue in several ways. First, we conditioned on a set of observable characteristics of firms and invoked the usual selection-on-observable assumption. Then, we

Table 7. FE-DID after ex-ante matching (1) and parallel Trend test (2) Restricted sample.

VARIABLES	(1)	(2)
Voucher C	0.0156*	
	(0.00840)	
Trend*treated		0.00454
		(0.00355)
Trend		0.00960***
		(0.00161)
Employees	0.00283***	0.00203***
	(0.000663)	(0.000454)
Labour productivity (log)	0.0498***	0.0124**
	(0.00734)	(0.00612)
Age	0.0165***	
	(0.00498)	
Age2	-0.000206	
	(0.000183)	
Time dummies	Yes	NO
Constant	-0.503***	0.0791
	(0.0982)	(0.0793)
Observations	3,005	2,983
R-squared	0.059	0.036
Number of id	825	825

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Treated: firms that received voucher C only in 2012. Controls: firms that never received voucher C matched to treated ones.

demonstrated that the results are robust to using different econometric techniques (ex-ante matching) and different sub-samples of treated and control firms. Finally, we found that our findings also survive to a common pre-treatment trend test. Therefore, we can conclude that export promotion grants offered by Lombardy region have a positive impact on local firms' export intensity whose magnitude reasonably varies between 1.2 and 1.6 percentage points.

This paper has interesting policy implication, not because it demonstrates the effectiveness of export promotion programs, but mainly because it suggests that sound policy evaluation exercises can be run also in absence of an experimental design, which is the most common situation in public programs supporting SMEs.

References

Abadie A., Drukker D., Herr L., Imbens G. (2004), Implementing matching estimators for average treatment effects in Stata, The Stata Journal, 4 (3), pp. 290–311

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

Broocks A., Van Biesebroeck J. (2017), The impact of export promotion on export market entry, Journal of International Economics, 107, pp. 19-33

Éupolis Lombardia (2016), Le misure regionali per l'internazionalizzazione delle imprese. Esperienze e risultati, Policy paper GEN15010.

Görg, H., Henry, M. & Strobl, E. (2008), Grant support and exporting activity, Review of Economics and Statistics 90(1), 168-174.

Imbens GW, Wooldridge JM (2009), Recent developments in the econometrics of program evaluation. Journal of Economic Literature 47, (1), pp. 5-86.

Lederman D., Olarreaga M., Pyton L. (2009), "Export Promotion Agencies Revisited", World Bank working paper 5125.

Melitz, M. J. & Redding, S.J. (2014), Heterogeneous firms and trade, in Gopinath, G., Helpman, E. & Rogoff, K. (eds.), Handbook of International Economics vol. 4, North Holland, Amsterdam, 1-54.

Muralidharana and Prakash (2013), Cycling to School: Increasing Secondary School Enrollment for Girls in India, NBER n. 19305

Volpe Martincus C., Carballo J. (2010), Export promotion: Bundled Services work better, The World Economy, 33(12), 1718-1756.

Appendix

Table A1 – Descriptive statistics, treated vs. untreated (all sample)

Variables	Obs	Mean	Std. Dev.	Min	Max
Untreated:					
Outcome	2171	.2602529	.2927375	0	1
Turnover	2171	5181912	6902524	13000	3.90e+07
Full time equivalent employees	2171	21.3576	27.95218	.13	233
Labour productivity (log)	2171	12.24495	.8771598	9.21034	15.42337
Age	2171	10.58038	5.450266	0	39
Treated:					
Outcome	494	.4331061	.302107	0	1
Turnover	494	6279822	7036597	24700	3.54e+07
Full time equivalent employees	494	27.26026	31.62805	.5	235
Labour productivity (log)	494	12.3203	.7595136	9.421412	15.28733
Age	494	11.46154	5.475146	0	51

Table A2 – Average characteristics of treatment and control 1 group

VARIABLES	(1) Treated (SD)	(2) Control (SD)	(3) Difference (2)-(1) (SE)
Export intensity	.43 (.30)	.40 (.29)	-.03** (.014)
Turnover (milion)	6.28 (7.04)	5.76 (6.52)	-.52 (.32)
Employees	27.3 (31.65)	25.08 (28.02)	-2.20 (1.41)
Log labour productivity	12.32 (.76)	12.30 (.73)	-.022 (.034)
Age	11.38 (5.18)	11.46 (5.04)	.08 (.24)
Equity (million)	0.484 (1.222)	.399 (1.07)	-.085 (.054)
Province			
BG	.12 (.32)	.11 (.32)	-.007 (.015)
BS	.14 (.35)	.15 (.36)	.007 (.01)
CO	.06 (.24)	.05 (.01)	.004 (.012)
CR	.04 (.20)	.02 (.15)	-.17** (.008)
LC	.05 (.22)	.05 (.22)	-.000** (.01)
LO	.01 (.08)	.02 (.13)	.012* (.006)
MB	.11 (.32)	.13(.33)	.013(.016)
MI	.15 (.36)	.18(.38)	.02(.02)
MN	.05(.21)	.03(.18)	-.01(.01)
PV	.02 (.15)	.03(.16)	.002 (.007)
SO	.01 (.10)	.006 (.082)	-.003(.004)
VA	.09 (.29)	.06 (.004)	-.036*** (.012)
Sector			
1	.05(.23)	.02 (.13)	-.04*** (.007)
2	.23 (.42)	.25 (.43)	.02(.02)
3	.22 (.42)	.29 (.46)	.07*** (.02)
4	.29 (.46)	.24 (.43)	-.06*** (.02)
5	.004 (.06)	.02 (.13)	.01** (.01)
6	.12 (.32)	.11 (.32)	-.01 (.02)
7	.06 (.23)	.06 (.24)	.01 (.01)
8	.01 (.10)	.01 (.09)	-.002 (.004)
Observations	8,353	8,341	8,341
R-squared	0.053		0.173
Number of id	3,553	3,546	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Control 1 group: Firms that do not received Voucher C in 2012 matched to treated ones

Table A3 – Average characteristics of treatment and control 2 group

VARIABLES	(1) Treated (SD)	(2) Control (SD)	(3) Difference (2)-(1) (SE)
Export intensity	.37 (.31)	.32 (.29)	-.04* (.02)
Turnover (milion)	6.21 (7.67)	5.55 (6.77)	-.66 (.56)
Employees	25.0 (29.8)	22.61 (25.51)	-2.39 (2.13)
Log labour productivity	12.26 (.81)	12.25 (.73)	-.014 (.060)
Age	11.04 (5.39)	11.29 (5.16)	.25 (.42)
Equity (million)	.456 (0.99)	.359 (.85)	-.096 (.071)
Province			
BG	.12 (.33)	.13 (.33)	.004 (.027)
BS	.12 (.33)	.11 (.32)	-.008 (.03)
CO	.07 (.26)	.05 (.23)	-.016 (.019)
CR	.05 (.22)	.02 (.15)	-.03* (.013)
LC	.03(.18)	.05 (.23)	.02 (.017)
LO	.01 (.07)	.01 (.12)	.008 (.009)
MB	.09 (.29)	.12 (.32)	.02 (.03)
MI	.14 (.35)	.19 (.39)	.05 (.03)
MN	.05 (.22)	.03 (.18)	-.02 (.01)
PV	.04 (.19)	.03 (.17)	-.009 (.014)
SO	.005 (.07)	.008 (.09)	.003 (.007)
VA	.07 (.25)	.06 (.23)	-.010 (.019)
Sector			
1	.05 (.23)	.03 (.17)	-.03* (.014)
2	.23 (.42)	.24 (.42)	.01 (.03)
3	.26 (.44)	.28 (.45)	.02 (.04)
4	.27 (.45)	.26 (.44)	-.013 (.04)
5	.01 (.10)	.03 (.16)	.02 (.01)
6	.11 (.31)	.10 (.30)	-.01 (.02)
7	.05 (.23)	.06 (.23)	.001 (.02)
8	.005 (.07)	.008 (.09)	.003 (.007)
Observations	8,353	8,341	8,341
R-squared	0.053		0.173
Number of id	3,553	3,546	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Treated: firms that received voucher C only in 2012. Control 2 group: firms that never received Voucher C matched to treated ones