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

Abstract

Most governments seem to take a positive view on exporting activity, so that the more firms in the economy that export, the better. In this regard, it is not surprising that many governments at both national and regional level, have taken some initiatives in encouraging firms to export. However, there are few empirical studies that have investigated the impact of such policies. In this paper we discuss how regional policy can be thoroughly evaluated with a counterfactual approach even in absence of an experimental design. In so doing, we take advantage of the case of manufacturing and service firms in the Regione Lombardia, where an extensive and diverse export support system has been used in an attempt to make regional industries more internationally competitive. In this regard we had access to longitudinal plant-level information on all grants provided by local authorities as well as quantitative and qualitative information provided by the Amadeus data-set. We define as treated the population of firms located in Lombardia that applied for one the export promotion program in 2012.

The main two issues with counterfactual evaluation of policies are the choice of a suitable methodology able to provide reliable results in this 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 model (DD). Consider the following DD model:

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

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 allow 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_{ist}. Although, the eventual bias is small and negligible whenever we can assume contemporaneous exogeneity (Cov($TREATED_{it}$; ε_{it})=0), i.e. the assignment to the policy in year t does not depends on the unobservable time varying characteristics of the firm in the same year (Imbens and Wooldridge, 2009). Of course, the addiction of year fixed effects controls for any change overcoming all the firms in any given year. In this specification, the parameter of interested is β_1.

When researchers use DD, two assumptions need to hold in order to get valid results. The first one is the existence of a parallel trend between treated and control groups in each outcomes. In fact, DD estimations 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 parallel trend pre-treatment as in Muralidharana and Prakash (2013) using the following equation:

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

where the variable TREND is a linear trend that takes the value 1 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 interaction term, γ_1, will eventually confirm the existence of the parallel trend and validate the estimation of the effect of export grants on firm performance.

Secondly, DD is sensitive to every event that happens at the same time of the reforms in that particular region and impacts differently on treated and control groups. This is not a concern in our case, since the export promotion policy design allow us to find two control groups 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.

The second 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 to which the "treated" firms should be compared, we follow the literature on pre-treatment matching in panel fixed effect estimation and carefully select our control samples. Of course, we have to be able to create control samples as similar as possible to our treatment group on observables (balancing property of matching methods) aware that, even if we fail, differences in the means of the variables will be controlled for by our fixed effect estimations. Then, we can estimate our equations under the assumption of a similar distribution of unobservable characteristics between treated and control firms. In order not to have identification of our coefficients rely on residual unobserved heterogeneity, we restrict our samples to a common support and exclude from the analysis treated firms which are outside using a propensity score estimation. These firms will not be used in our analysis. We carefully selected control groups using Abadie et al.'s (2004) semiparametric ex-ante matching approach. We found control groups among the non-treated firms who applied for the same grant but failed in receiving it (*Control 1 or internal control*), and among the non-treated firms who did not apply for the grant in any year (*Control 2 or external control*). For each treated firm, we identified the closest five firms in each control group based on observables characteristic like industry, (log) assets, (log) sales and pre-treatment outcomes as (log) employees and (log) value added per employee 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, self-selected into the treatment as the treated firms. 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 estimation 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 unobservable. The estimation results obtained with this control group could probably act as an upper bound of the effect of the policy.

As outcomes, we consider a set of indicators of firm performance, capturing productivity (value added per employee) and profitability (ROA and ROE). When comparing treated firm to the internal control we are also able to exploit the richness of the data collected by Regione Lombardia about firms' performance on external markets internationalization and use export turnover on total turnover as a measure of export intensity.

Time-variant firms' characteristics, instead, include the size of firms and their experience on international markets. At this regard, we distinguished non-exporters, occasional exporters and exporters, defined as those firms who exported in all the considered years.

In order to better understand the true impact of the export promotion program, we used different measures, which range from a dummy variable, the amount of the grant obtained by each firm, the number of grants assigned to each firms, as well as the number of fairs attended because of the program.

We found that export promotion instruments generate a positive impact on firms' performance, higher when we use an external control group, confirming our idea that selection bias is higher when and external control group is used. This implies that policy evaluation can be thoroughly carried out even in absence of an experimental design, provided that good quality administrative data are collected.

References

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