

# Managerial Input and Firm Performance. Evidence from a Policy Experiment.\*

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## Abstract

This paper studies whether a small monetary subsidy to acquire consultancy services can boost firm performance. We examine an Italian subsidy program and identify the causal impact of the policy by comparing firms that nearly got the subsidy to firms that nearly missed it due to small differences in their time of application in a difference-in-differences framework. The subsidy was effective in increasing exports outside the EU, firm labor productivity, profitability, and size. Interviews with consultants reveal that the initial contracts led to enduring relationships with beneficiary firms, and providers offered additional services such as support for digitalization.

**J.E.L. codes:** L2, L38, O40, F14, H2, F2

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# 1 Introduction

In the last two decades, a growing and influential body of research has highlighted the role that the quality of management plays in shaping firms' performance (Bloom and Van Reenen, 2010, 2011; Bender et al., 2018; Caliendo et al., 2020; Roberts and Shaw, 2022). However, we know much less about how firms can acquire such important assets. This is particularly relevant for small and medium-sized enterprises (SMEs), where information frictions, financial frictions, and risk aversion might prevent them from searching for and introducing the most effective practices. Indeed, policy-makers of all G20 countries have recently expressed concern about the lack of managerial skills, particularly among SMEs, and there is an ongoing debate on which tools may help firms to acquire such competencies.<sup>1</sup> One possibility would be to go “beyond the boundaries of the firm” and rely on external services such as consulting (Anderson and McKenzie, 2022). However, firms may be reluctant to use this option even when informed about the quality of the consultancy because of concerns – among others – on the ability to finance the initial costs or uncertainty on the returns; this leaves room for public interventions to support firms in acquiring these inputs and improving their performance (Schivardi and Schmitz, 2020; Anderson and McKenzie, 2021). Despite the importance of this issue, there is limited empirical evidence on how governments can incentivize firms to use consulting services and on the efficacy of such efforts.

This paper contributes to filling this gap in the literature by studying the effects of government-financed consulting services on the performance of SMEs. In particular, we study the “*Voucher for Internationalization*”, a policy implemented in Italy in 2016 aimed at improving the exporting capacity of these firms. The program envisaged a 10,000-euro subsidy for the acquisition of consulting services that could be obtained from a list of providers pre-selected by the Italian Ministry for Economic Development. Eligible firms could benefit from a subsidized consultancy through a Temporary Export Manager (TEM), a consultant with expertise in internationalization who supported the firm for at least six months. The subsidy was assigned at the end of 2015 and used during 2016.

We collect data on the 4,146 firms that applied to the program. For each firm,

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<sup>1</sup>See, for instance, the OECD guidelines to address the Future of Work (OECD, 2021).

we retrieve data on their application to the program from the Italian Ministry for Economic Development and their balance sheets between 2013 and 2020 from AIDA, the Italian section of Orbis by Bureau Van Dijk.<sup>2</sup> We then match applicant firms with their exports and imports at the country and product level using data from the Italian Customs Agency. Finally, we obtain information on firms' workforce size and composition from the Italian Social Security database.

We identify the causal impact of the program by relying on the allocation process of the subsidy. Firms applied online, and vouchers were awarded on a first-come first-served basis until the policy budget—nearly 20 million euros—was exhausted. The resources supplied to the policy were much lower than the demand by firms and the high number of applicants led to the exhaustion of available funds within one minute since the opening of the application period. Firms had no *a priori* indication of how many firms applied and when. We use the exact timing of the applications' submission to identify close winners and losers, i.e. firms that nearly received or nearly missed the subsidy due to small differences in their application timing, and we compare them over time in a difference-in-differences setting.<sup>3</sup> Our identifying assumption is that firms were allocated as-good-as-randomly on the two sides of the cutoff in this narrow time window and that firms that applied with a slight delay to the program (comparison group) represent a good counterfactual for firms that applied a few seconds earlier with respect to the cutoff (treatment group). This procedure for TEMs' allocation rules out selection into treatment and allows us to estimate the causal effect of the program. We show that these two groups of firms are statistically indistinguishable in terms of their characteristics and outcomes right before the implementation of the program and that they are on the same performance trend in the three years before it.

Our analysis delivers four main findings. Firstly, TEMs improved firm imports and exports, particularly in markets outside the EU, which are characterized by higher barriers to entry. The effects materialize three years after using the subsidy, when early applicants to the program increased their annual exports by 200,000 euros (+13% with respect to 2015) and imports by about 100,000 euros (+15%), with respect to firms in the comparison group. We do not observe

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<sup>2</sup>We obtain information for all but one of the firms applying to the program.

<sup>3</sup>Throughout the paper, we focus on firms that applied within a 30 seconds radius around the moment of exhaustion of resources. Results do not change significantly when we consider smaller or larger time windows.

any changes in intra-EU trade. The increase in trade occurs at the intensive margin, with no changes in the number of trading partner countries, the number of exported/imported products, or the exporter status of the firms.

Secondly, even though the policy targeted a specific aspect of the firms' operations, consultancy services had a broader impact on overall firm performance. We find a positive effect on revenues, labor productivity, and profitability, which builds up over time. By 2019, three years after the policy implementation, early applicants have about 600,000 euros more in revenues (+10.5% with respect to 2015), 5,000 euros more in value added per employee (+9.6%) and about 70,000 euros more in net profits (+55%) with respect to firms in the comparison group. In addition, they showed more resilience during the first year of the Covid-19 pandemic by further increasing their positive gap in performance with respect to late applicants, thanks to higher investments in intangible assets. This result is in line with [Lamorgese et al. \(2021\)](#), who stress the importance of good management practices in facing the economic turmoil generated by the recent pandemic. We perform numerous checks to test the robustness of our findings by looking at different definitions of the neighborhood of the cutoff, empirical specifications, and sample definitions; all these tests support the conclusions of our main analysis. In addition, the positive effects on performance are heterogeneous, with evidence pointing to greater benefits for smaller and less productive firms. We further observe that the effect of the policy is different across consulting service providers. However, firms had the freedom to choose their own provider, potentially resulting in an endogenous matching between the firm and the consultancy service provider. In this sense, this result should be interpreted as suggestive rather than causal. Finally, we exploit the richness of the available balance-sheet data to identify proxies of financial constraints and find that the estimated effects are unlikely to be explained by the presence of these constraints.

Thirdly, we find that early applicants to the program increased their workforce size in the years after the policy implementation compared to the comparison group. Indeed, an important concern from a policy perspective is to understand whether public subsidies, besides improving firms' economic performance, affect workers as well. To answer this question, we study the effect of the policy also on the firms' labor demand: we find that one year after receiving the voucher, the workforce increases by about one employee per firm, and this trend grows

steadily in magnitude and significance up to four employees over the following four years (about +14% with respect to their employment level in 2015). The largest employment gains are accrued by male and more experienced employees, as well as by blue-collar workers. Most notably, we observe an increase in the number of high-quality contracts in terms of duration (permanent contracts) and working time (full-time).

Finally, we conduct a qualitative survey with semi-structured interviews among TEM providers to shed light on the mechanism through which these effects unfold. We interviewed representatives from 43 consulting firms that provided their services to 682 beneficiary firms, about 38% of the firms using the voucher. We find that the voucher encouraged firms to establish a lasting relationship with providers, with nearly 82% of interviewed providers reporting that they had additional contracts with the beneficiary firms beyond the one subsidized by the voucher. Moreover, our interviews unveil that consulting firms provided services beyond support for internationalization. In particular, the ability of consulting firms to provide services aimed at improving product commercialization and digitalization of their clients was associated with larger effects on the balance sheets of firms using the voucher.

Our research mainly contributes to two strands of the literature. First, it expands our knowledge of the role of management consulting services. Using RCTs, previous works show that management consulting has large and positive effects on firm size, productivity, and profitability (Bloom et al., 2013; Bruhn et al., 2018b). Iacovone et al. (2022) allocate randomly group-based and individual-level consulting and find that the former is more cost-effective. Anderson and McKenzie (2022) compare the effectiveness of insourcing, outsourcing, consultancy, and training of entrepreneurs for financial and marketing services. While insourcing, outsourcing, and consultancy appear to have similar effects and have a stronger impact than training, outsourcing and insourcing dominate in cost-benefit terms. From a historical perspective, Giorcelli (2019) and Bianchi and Giorcelli (2022) show that the effects of management training on firm outcomes are persistent and create positive spillovers in the supply chain. Our study provides four contributions to this literature. First, we exploit a policy that allowed firms to acquire consulting services with limited constraints on the type of services they could request. This provides useful guidance to policymakers on how to support the

acquisition of services to firms and informs them about the effects of obtaining these services in a market environment. Second, unlike previous contributions, our study focuses on a high-income country, which shows substantial similarities to many other modern economies. Although consulting services are ubiquitous in such contexts and potentially much needed among SMEs, to date there is very limited evidence on how effective they are in advanced economies. Third, our analysis benefits from a larger sample compared to previous studies, including approximately 1,600 firms that received the treatment. Additionally, we utilize a diverse set of firms. As a result, we are able to establish that managerial capital is lacking in a wider set of small businesses and has an impact on various dimensions of business outcomes. Finally, our work focuses on a specific type of consultancy and its impact on trade, which is an aspect that has been neglected to date by the literature on consulting services. Only two recent studies, to the best of our knowledge, have investigated the importance of management for trade: [Bloom et al. \(2021\)](#) assess how better management practices lead to a stronger performance in the export market in the US and China, while [Mion et al. \(2017\)](#) show that management-specific market knowledge facilitates export to a certain location by exploiting managers' mobility. However, neither of these studies examines the role of consultancy in enabling firms to acquire specialized skills and competencies that could potentially broaden their trade activities.

In addition, our work relates to the literature on trade policies.<sup>4</sup> [Srhoj et al. \(2020\)](#) summarize the impact of export-boosting policies across 26 countries, showing the strong heterogeneity in their structure and effectiveness. Previous studies analyze the impact of monetary support for exporting firms in terms of subsidies ([Defever et al., 2020](#)), credit guarantees ([Felbermayr and Yalcin, 2013](#)), and grant support ([Görg et al., 2008](#)). [Munch and Schaur \(2018\)](#), instead, find that export promotion leads to improvement in sales, value added, and value added per worker. We contribute to this literature by showing the impact of consultancy services for trade. Our results entail important implications for the design of internationalization policies. Traditional trade policies have focused on tariffs or export subsidies. However, acquiring intangible assets, such as managerial and organizational competencies, poses an additional significant barrier to internationalization. This is because the returns to these assets are

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<sup>4</sup>[Demidova and Rodríguez-Clare \(2009\)](#) and [Ding \(2021\)](#) provide a useful review.

uncertain and feature several structural characteristics, such as sunkness and economies of scale, that make them hard to accumulate for SMEs (Haskel and Westlake, 2017). Our findings demonstrate that a modest subsidy for obtaining consultancy services can enhance firm internationalization. This is achieved by enabling firms to access more foreign inputs through imports and by augmenting their revenues through exports.

The remainder of the paper is as follows. Section 2 describes the policy setting and the data. Section 3 describes the empirical strategy and provides evidence in favor of our identifying assumptions. Section 4 presents the results of our analysis and the robustness tests for their validation. Section 5 takes stock of the empirical findings and of the existing literature to discuss why firms do not use such consulting services given their high returns. Section 6 concludes.

## **2 Institutional Setting and Data**

### **2.1 Institutional Setting: The “Vouchers for internationalization”**

Following the Great Recession and the European Sovereign debt crisis, the Italian economy underwent a subdued recovery phase, with many of its SMEs facing difficulties in improving their performance. To support these firms, the Italian Government launched the ‘Vouchers for Internationalization’ policy in 2015 to stimulate their growth and employment capacity by subsidizing the acquisition of consulting services for trade.<sup>5</sup> These services could be acquired from a list of companies compiled by the Ministry of Economic Development (MISE). Firms offering consulting services needed to have consolidated experience in trade activities and knowledge of foreign languages. The program was targeted to SMEs with revenues above 500,000 euros in at least one of the three years before the application and innovative start-up, that is, firms that had been active for less than two years and with “production and commercialization of innovative goods or services with high technology content” as main activity (D.M. 15/05/2015 and d.l. n. 179 18/10/2012).

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<sup>5</sup>The Vouchers were first introduced with Law n.133/2014, and later normative aspects were reported in the ministerial decree of the 15<sup>th</sup> of May 2015.

To receive the voucher, firms were required to apply through the MISE website and subsidies were assigned on a first-come, first-served basis after a preliminary assessment of the eligibility criteria carried out by the Ministry. Firms being awarded the voucher could hire a TEM for consulting services for a minimum of 6 months up to a maximum of 12 months. The primary role of this type of consultant was to assist the firm in targeting foreign markets and designing strategies to start or intensify export activities. The policy aimed at providing firms with useful managerial skills and expertise, e.g. knowledge of foreign markets, which have been proven to be a key asset for firm internationalization (Mion et al., 2017).

The first wave of the policy took place in 2015 and assigned a total budget of 19 million euros.<sup>6</sup> We restrict our analysis to the first edition of the voucher in which no information was available on how quickly resources would have been exhausted.<sup>7</sup> Successful applicants received a subsidy amounting to 10,000 euros, with a minimum additional contribution from the firm of 3,000 euros. Thus, the total minimum value of the consultancy was 13,000 euros.

The policy involved four steps to be completed during the period between September and December 2015. First, firms were requested to send an expression of interest by filling out a registration form in early September. The second step took place over 11 days, from 10:00 a.m. September 22<sup>nd</sup> up to 5.00 p.m. October 2<sup>nd</sup>, during which firms could send their final applications. Since the Ministry adopted a first-come first-served eligibility criterion and firms were highly responsive in sending their applications, the allocation procedure resulted in a ‘click day’, and the total budget was exhausted within the first minute since the start of the application period. In addition, there were quotas for firms participating in special promotional events (“roadshows”) and for those with legality ratings, i.e. a certificate indicating the firms’ compliance with existing regulations and accounting best practices.<sup>8</sup> Third, the Ministry checked the applications to verify their content. Firms not complying with requirements were excluded, and some renounced the subsidy ex post. Lastly, eligible firms established contacts with TEMs by drawing from the list of consulting companies provided by the Ministry.

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<sup>6</sup>The budget for the policy increased to 38 million euros in 2018

<sup>7</sup>This also gives us a sufficiently long time horizon to study our outcomes of interest.

<sup>8</sup>These were issued by the Authority for Competition and Market after inspections of the firms.



After having arranged a formal consultancy contract, firms received the assigned grant within 60 days.

During the first wave, 4,146 firms applied, of which 1,758 were initially admitted to the program. Then, 95 applications were excluded because they contained inconsistent information or did not comply with the conditions of the policy, while 32 applicants withdrew. Of the remaining 1,631 firms, 20 did not complete the procedures to receive the subsidy. In the end, 1,611 firms were assigned vouchers. As far as the quota is concerned, about 260 firms participated in roadshows, and 110 provided a legality rating. Among them, a total of 226 obtained the subsidy.<sup>9</sup>

Regarding the characteristics of the contracts, about 80% had a value below 14,000 euros, with the voucher covering around 70% of the total cost.<sup>10</sup> The duration of the contracts ranged between 6 and 12 months, with more than 50% of the firms establishing contracts of exactly 6 months. Preliminary information on the subject of consultancy<sup>11</sup> reveals that firms requested a variety of services from TEMs. Most were interested in attracting additional clients and contracts (46%) or conducting market research (34%). The remaining firms requested other services ranging from legal consultancy on international markets (2.7%) to logistics and customs duty support (0.6%). In about 10% of the cases, the precise nature of the contract was not specified.

## 2.2 Data

We collected data from multiple administrative sources and built a unique employer-employee dataset that covers the years between 2013 and 2019. We rely on four main data sources: data on the policy implementation, including a list of applicants and the assignment of vouchers from the MISE; firms' balance sheets from AIDA Bureau Van Dijk; granular trade data at product-country-firm level provided by the Italian Customs; and information on firms' workforce from the National Social Security Institute (INPS) data.<sup>12</sup> Below, we provide additional information on each of these sources:

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<sup>9</sup>The exclusion of firms receiving the vouchers due to quotas does not alter our results.

<sup>10</sup>The distribution of the share of the service's price covered by the Voucher is reported in the Appendix in Figure A1.

<sup>11</sup>The main object of the contract was provided to the MISE at the time of the application.

<sup>12</sup>This was possible thanks to the VisitINPS initiative by the Italian Social Security.

*List of applicant firms* (Ministry of Economic Development, MISE). We obtained detailed data on the administrative procedures related to the policy from MISE. The data include the complete list of firms that applied for the subsidy, as well as their administrative identifiers and exact time of application, which is crucial for our identification strategy. The data also report some firm characteristics, such as previous experience in international trade, participation in roadshows, the main sector of activity, and, if available, some information on the consultancy contract, such as the type of service received, the cost, and the identifier of the consultancy provider.<sup>13</sup> We used the unique firm administrative identifier to match this information with other data sources.

*Firm Balance Sheets* (AIDA). We match our set of firms with their balance sheet data from the AIDA Bureau Van Dijk database. This dataset is constructed based on the Firm Registry of the regional Chambers of Commerce, and it covers all limited liability firms in the Italian economy. Balance sheet information is provided annually and contains information on revenues, value added, profits and other economic indicators.

*Trade Data* (Customs and Monopolies Agency). Then, we match our set of firms with granular trade data at the country-product level provided by the Italian Customs and Monopolies Agency. Custom data represent an ideal source of information for analyzing firms' trade performance since it allows observing each firm's transactions both within and outside the European Union. Data are collected at the transaction level and are measured in both total value in euros and quantities in kilograms. Moreover, for each transaction, the data report information on the type of goods traded based on the Combined Nomenclature (CN8) classification and the country of origin or destination. We collapse our dataset at the firm-year level and build a panel for applicant firms.

*Workforce Data* (Italian Social Security Institute, INPS). Finally, we merge firms participating in the application process with their workforce characteristics obtained from INPS. We mostly rely on the UNIEMENS archives, which contain information on firms' monthly mandatory statements for social security purposes. The dataset covers the universe of the private sector, non-agricultural employees in Italy, and provides information on job characteristics such as wage and type of

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<sup>13</sup>Roadshows are events supported by the Ministry to illustrate policies aimed at helping firms enter or expand in international markets.

contract (part-time\full-time status; permanent\temporary contract). We collapse our worker-level data at the firm-month level.

Overall, we match all but one of the firms (4,145) that applied for the policy with their related information.

### 3 Empirical Strategy

To identify the impact of the subsidy on firms' performance, we rely on its assignment procedure, which determines a quasi-random allocation close to the time cutoff and allows us to define suitable treatment and comparison groups.

Indeed, the financial resources allocated to the voucher and the rules for its assignment offer an ideal setting: funds allocated to the policy were substantially lower than the amount requested, and the assignment process resulted in a click-day. As a consequence, many applicants were not granted the subsidy because of a small delay in applying. As described in Section 2.1, firms applied via an electronic procedure and applications were processed according to their submission time, up to the exhaustion of available funds. Not all firms could access the subsidy, and firms did not know when the resources would run out, so eligibility for firms that applied in a certain time span around the time cutoff is as good as random.<sup>14</sup>

We identify the causal impact of the policy by comparing firms that nearly made the cutoff with firms that missed the cutoff by a few seconds in the spirit of [Pinotti \(2017\)](#). In practice, we consider firms around the cutoff time and run a differences-in-differences model in our main analysis.<sup>15</sup> Our baseline model is the following:

$$Y_{jt} = \alpha + \beta_1 \mathbb{1}(\tilde{t}_j < 0) + \beta_2 Post_t + \beta_3 \mathbb{1}(\tilde{t}_j < 0) XPost_t + \theta_j + \eta_t + \varepsilon_{jt}, \quad (1)$$

where  $Y_{jt}$  is the outcome of interest,  $\tilde{t}_j$  represents the time of application as difference with respect to the time of exhaustion of the available funds,  $Post_t$  is

<sup>14</sup>Notice that in the first wave of the policy, firms had no information about the exhaustion time of the policy budget.

<sup>15</sup>Point estimates for a difference-in-discontinuity ([Grembi et al., 2016](#)) are similar, as discussed in Section 4.2.6, but noisier due to the small number of observations at the cutoff.

a dummy variable equal to 1 after 2015,  $\theta_j$  is a firm fixed effect,  $\eta_t$  is a time fixed effect, and  $\varepsilon_{jt}$  is a random error term. Our parameter of interest is  $\beta_3$ , which identifies the treatment effect. This is obtained by comparing outcome trends for the treatment and the comparison group in the periods right before and after the voucher assignment. This specification allows us to uncover the treatment effect of the policy after having netted out common time effects and time-invariant firm characteristics. Since some firms were excluded from receiving the subsidy even if they applied before the cutoff time, and other firms were deemed eligible even if they applied later in time, our treatment variable,  $\mathbb{1}(\tilde{t}_j < 0)$ , identifies an Intention-to-Treatment effect (ITT).<sup>16</sup> To limit the influence of outliers, we winsorize our dependent variables at 1% in the main analysis.<sup>17</sup> We cluster standard error at the firm level.<sup>18</sup>

In most cases, to provide visual evidence and to better describe both possible pre-trends and how the effect of the policy evolved over time, we estimate the event study version of our difference-in-differences model:

$$Y_{jt} = \alpha + \beta_1 \mathbb{1}(\tilde{t}_j < 0) + \sum_{d \in (-3,4)/(-1)} \beta_{2d} \mathbb{1}(Year - 2016 = d) + \sum_{d \in (-3,4)/(-1)} \beta_{3d} \mathbb{1}(\tilde{t}_j < 0) \mathbb{1}(Year - 2016 = d) + \theta_j + \eta_t + \varepsilon_{jt}. \quad (2)$$

Interactions between the treatment dummy and years before the experiment allow us to investigate the presence of any pre-existing differential trend before the policy between treated and control firms, while interactions with the following periods describe the dynamics of the treatment effect over time. We consider the year in which the voucher was assigned (2015) as our reference period. Since the application process took place at the end of 2015 and up to two additional months were needed to communicate the results to the beneficiaries, firms' outcomes were not affected by the policy in the year of the application.

The first step in our empirical analysis is to identify the cutoff time. To this purpose, we plot the share of firms that received the subsidy against the time of

<sup>16</sup>Since compliance is high, this is similar to the LATE, obtained by instrumenting the take-up with the eligibility determined by the timing of the application, as we show below.

<sup>17</sup>Generally, results become more precise with this adjustment while point estimates are not substantially affected.

<sup>18</sup>Results are consistent and also clustering at the second of the application arrival.

submission of their application in Figure 1. The distribution of the acceptance rate shows a discontinuity after 46 seconds from the opening of the online procedure. This corresponds to the arrival time of the application of the 2002<sup>nd</sup> firm.<sup>19</sup>

Then, we test whether firms were able to sort around the cutoff by examining the distribution of applications around the time of exhaustion of the funds allocated to the policy. These tests are reported in Figure 2: Panel (a) plots the full distribution while Panel (b) focuses on the neighborhood of the cutoff used in our estimation. Throughout the analysis, we use a 30-second radius around the cutoff to focus only on firms that received the subsidy at the margin.<sup>20</sup> Resources were exhausted within one minute since the opening of the application process, and the bulk of firms filed their request approximately within 30 seconds after the opening. The distribution shows no clear discontinuity at the cutoff, as proven by the McCrary test reported below Panel (b). This is consistent with the fact that firms could not keep track of other firms' applications and time their submission accordingly.

Finally, we assess the soundness of our empirical framework. First, we check whether firms in our treatment and comparison group are comparable in terms of observable characteristics. We report summary statistics for the treated and the comparison groups in the application year (2015) in Table 1.<sup>21</sup> Results support our empirical exercise: in most cases, characteristics between treated and control firms are very close, and there are strong similarities in many important dimensions such as value added per employee and gross profits. Moreover, areas where treated and control firms are located display similar broadband penetration, showing that differential internet speed is unlikely to drive the assignment to treatment around the cutoff. Only in a few cases, the differences between the two groups are statistically significant. For instance, in the year of the application, treated firms appear to be less profitable (measures in terms of Return of Equity, ROE) than firms in the comparison group.<sup>22</sup> In addition, we also report normalized

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<sup>19</sup>We test for the presence of other discontinuities by running RDD regressions at different points of the time of application distribution using as dependent variable a dummy equal to one for firms receiving the subsidy as the dependent variable and with the time of application as the running variable. We plot the discontinuity coefficients together with their *z-statistic* in Figure A2 in the Appendix. The discontinuity at our cutoff (0) clearly stands out and it is the only significant one.

<sup>20</sup>Main results are consistent if we consider larger (40 seconds) or smaller (20 seconds) intervals.

<sup>21</sup>We also present visually the average characteristics of firms in terms of trade and other dimensions by the time of application in Figure A3 in the Appendix.

<sup>22</sup>It should be noted that much stronger differences appear if we compare the whole set of

differences (Imbens and Wooldridge, 2009) in Table 2 to assess the relevance of the dissimilarities between the two groups. This measure is never above the critical threshold of 0.25 suggested by Imbens and Rubin (2015), which offers further reassurance about the reliability of our empirical analysis. To sum up, the available evidence shows relevant similarities between early and late applicants and supports our view that the latter represents a suitable counterfactual.

Second, we look at possible pre-trend in our outcomes of interest. Indeed, differences in levels would not be a cause of concern *per se* for our identification since our difference-in-differences strategy exploits variation over time and across firms differently exposed to the policy; differentiating over time and within firms nets out any level difference between the two groups of firms. Hence, the soundness of our empirical strategy relies on the identifying assumption that firms in the two groups would have moved on parallel trends without the policy. We provide supporting evidence for this by considering the trends in the dependent variables before introducing the policy. We explore this in Figure A4 in the Appendix. We use the year of the voucher assignment as the baseline year and then look at differences over time for treated and control firms with respect to the baseline period. In no case, we observe any evidence of differential pre-trends between the two groups of firms, and the coefficients for the years before the treatment are never statistically different from zero as further testified by the p-value of the F-test for the joint equality of the coefficients to zero. This strongly supports our identification strategy and provides evidence in favor of the causal interpretation of our empirical exercise.

## 4 Results

### 4.1 Trade outcomes

We start by looking at firm internationalization, the primary outcome of the policy. Among the firms within the 30-second radius around the cutoff, many

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early applicants with all late applicants as shows in Table A1: early applicants trade more, especially within the EU, are larger and spend more for their employees and they have higher profits. Counter intuitively, they are also in location with less access to fast connection, which might suggest that early applicants compensate this with a greater attention to the timing of the application. This difference is no longer present once the sample is restricted to the 30 second radius around the cutoff.

already exported before applying for the policy, with about 70% having positive exports in 2015. Export managers can help firms in different ways, e.g. by identifying new locations for their products or suggesting alternative customers within a country to which the firm was already exporting. However, the additional knowledge about foreign markets might also lead to changes in the inputs the firms choose for their production with greater integration in the global value chains. In addition, the knowledge provided by external consultants might be useful to explore more distant markets with different regulations.

To investigate these margins, we first focus on measures of export by broad destination at the firm and year level. We compute total exports and imports and aggregate them for two groups of countries: those belonging to the European Union and those outside the European Union countries. We assume the latter to be more difficult destinations to export since they are outside the Customs Union. Then we compare how exports evolve over time with respect to the year of application for the voucher (2015). Firms were awarded the voucher in 2016, and we expect the effects to materialize over time as firms adapt to the new opportunities for both inputs and outputs.

We compare the dynamics of the dependent variable between the two groups of firms by estimating Equation 2. Results are reported in Figure 3, in which we consider exports to countries outside the EU in Panel (a) and to countries inside the EU in Panel (b). The difference between treated and comparison firms remains stable in the periods before the policy, with minor deviations from the baseline period in 2013 and 2014 (periods -2 and -1). Though the magnitude of the effect slightly increases in the first period after the policy implementation, we observe a large difference, statistically different from zero at the 5% level, only after three years. In 2019, treated firms displayed 200,000 euros more in export to countries outside the EU compared to the baseline year with respect to firms in the comparison group. This dynamic is confined to markets outside the EU, for which it is likely that TEMs have a greater information advantage and capacity to favor the firm than in the case of the EU markets. Exports to EU countries are, indeed, extremely stable. The observed lag between the policy implementation and the detectable impact on exports seems reasonable given the necessity to adjust production and create market opportunities in more remote locations: as the consulting service was used in 2016 and lasted from 6 to 12

months, this corresponds to a 3-year lag for the effects to be fully appreciable.

Panel (c) and Panel (d) investigate changes in imports, which follow the same pattern as exports. Imports from countries outside the EU increased two years after the intervention, while there were no changes for imports from within the EU.

Also on aggregate (total value of trade and the sum of total exports minus the sum of total imports) trade increases although the impact is much less precise as shown in Panel (e) and in Panel (f).

Results on trade performance from a classical difference-in-differences model are reported in Table A2 in the Appendix. They confirm previous findings, but coefficients appear to be imprecisely estimated for the post period as a whole. The average gain for exports to extra-EU countries is about four times the gain to EU countries, and the effect on imports is similar in magnitude and significance (different from zero at the 5% level).

Then, we decompose our trade outcome to investigate whether trade towards and from particular locations experienced stronger growth than others. We group countries according to their income group in the World Bank 2020 classification and geographic location and report results in Appendix Table A3. Results show stronger export growth towards high and middle-income countries (although not significant at conventional levels) and a significant increase in exports toward Latin American, Middle Eastern, and North African countries. As for imports, we observe a statistically significant (at a 10% level) increase from high-income countries and a larger, but less precise, increase from middle-income countries. In terms of geographic location, treated firms register the largest increase from Europe and Central Asia.<sup>23</sup>

Additionally, we explore several other outcomes to assess how these additional trade flows occur. We report our results in Table A4 in the Appendix and corresponding event studies in Figure A5, Figure A6, and Figure A7 in the Appendix. There is no effect at the extensive margin in terms of exporter status, number of countries or number of products. The small decline in export probability outside the European Union (-2 percentage point in probability and significant at 10%)

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<sup>23</sup>The sum of all coefficients should match the aggregate effect. However, some discrepancies appear due to the winsoring for each specific outcome.



is short lived and is present only in the year of the policy implementation. Hence, it appears that the policy mostly acted at the intensive margin, allowing firms already involved in international trade to strengthen their position in existing markets with previously established products. This is a reasonable outcome in light of the small cost of the consultancy (more than 80% of the contracts are less than 14,000 euros in value).

## 4.2 Balance sheet outcomes

### 4.2.1 Main Findings

We now turn to the impact of the subsidy on firms' performance measures and look at costs, revenues, labor productivity, and profitability. We start with a graphical inspection of the effects in the event study difference-in-differences (Equation 2) and then move to estimating our baseline model (Equation 1).

We first explore the dynamics of the treatment effect and plot our results in Figure 4. In all cases, we do not detect any difference in trends between the treated and comparison firms. This supports our identification assumption. The positive effects of the policy build up over time and become more noticeable in the last periods of the analysis (2019 and 2020). In the year of the treatment and over the following two years, the treated firms enhance their performance modestly, while in the last periods, the improvements appear substantial. Consistently with previous results, the positive effects of the policy take time to materialize. Nevertheless, some, albeit smaller, effects are detectable also in the short term.<sup>24</sup>

Then, we report the results for the difference-in-differences estimates for our main variables of interest in Table 3. The effects are positive and statistically significant: firms eligible to receive the subsidy increase their total employees' costs (and the number of employees as shown in Section 4.3), revenues, value added per employee, and profitability (net profits and Return On Equity, ROE). We do not detect changes in their capital/labor ratio. Compared to the baseline

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<sup>24</sup>Note that results appear evident even when raw data are used in Figure A8 in the Appendix. In many cases, the two groups of firms have very similar values in terms of our variables of interest in the pre-period while they start diverging after the policy implementation. This figure also shows that, in some cases, the results derive from a decline, or more marked decline, in the performance of firms in the comparison group with respect to treated firms. This is particularly clear in the first year of the pandemic but also in other years for some variables such as net profits.

year, the effects range from a minimum increase of 5% for employment costs and revenues from sales to 30\35% for ROE and net profits. The gain in labor productivity, proxied by value added per employee is about 8% and might arise from two mechanisms: on the one hand, firms might be adjusting their production along the lines suggested by the consulting managers to increase exports; on the other hand, the consulting firm might be providing more general counselling through TEMs or additional services, thus leading to a better overall performance of the firm.

In addition, treated firms further consolidate their advantage over firms in the comparison group in the first year of the pandemic (2020) and show higher resilience to shocks, as it appears for several performance measures in Figure 4. Interestingly, in the wake of the global pandemic, which limited personal interaction, early applicants to the policy increased more markedly their investment in immaterial assets (+100,000 euro or +50% with respect to the pre-pandemic level in 2015) as shown in Figure A9 Panel (a),<sup>25</sup> in the Appendix. This, in turn, might have helped them to maintain their operations during the pandemic. Investments in material assets, reported in Panel (b), also increased, but this effect is too imprecise to lead to definitive conclusions.

These results further signal that the presence of consultants not only led to improvements in profitability and labor productivity but also in the ability of the firm to act flexibly and weather shocks more effectively.

#### **4.2.2 Heterogeneity by firm characteristics and TEM provider**

So far, we have only investigated the average effect of eligibility to receive the subsidy and acquire services from TEMs. In this section, we enrich the analysis by describing how these services affect different types of firms. This also highlights possible channels through which these services impact firms' performance.

For this purpose, we include triple interactions (and all relevant double interactions) in our models to test for differential effects across groups. We consider several dimensions: geographic location, size, labor productivity (measured as

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<sup>25</sup>The higher variance in the coefficient for 2020 can be explained by the higher variance of immaterial assets in 2020 with respect to previous years. Indeed the standard deviation increases from about 470,000 to 1,131,000 euro. We do not find higher attrition in the first year of the pandemic (59 firms become inactive between 2018 and 2019 and 66 between 2019 and 2020).

value added per worker), and previous exporter status, i.e., whether the firm was already exporting within or outside the European Union. All characteristics refer to the year of application before the TEM could have had any impact on firms' activity.

We report results for our firm-level variables in Table 4. The equation is estimated using a log transformation to rescale the changes in the dependent variable across groups of firms.<sup>26</sup> The table reports the main difference-in-differences coefficient, the relevant triple interactions, and, at the bottom, the p-value for the sum of the two interactions being equal to zero. Firms in the South,<sup>27</sup> seem to benefit less from the policy, although only in the case of ROE, the difference between the two groups is significant at the 10% level. Interestingly, small firms, i.e. those below the median size in the sample (15 employees), and the least productive firms, i.e. those in the bottom half of the value added per employee distribution accrue larger gains. Triple interaction coefficients are generally large, but for the most part, imprecisely estimated. However, the impact for this group (obtained by summing up the main coefficient and the triple interaction) often differs from zero at conventional significance levels, as reported in the bottom row, which implies detectable positive effects for these firms. In percentage terms, benefits can be as high as eight times larger for the least productive firms (effect on revenues, in Column 11) compared to other treated firms. Finally, it appears that the impact is smaller for already exporting firms. Such a moderate intervention generates, in many cases, only small effects, while the benefits seem extremely sizable for firms characterized by higher levels of inefficiency.

Since type of services and quality may differ substantially across providers we also explore whether the impact of the subsidy changes depending on the provider chosen by the beneficiary firm. To gain precision we focus on the seven providers with more than 30 clients and group all the others in a residual category. We then estimate our difference-in-differences model and report results in Table A5 in the Appendix. Results show substantial heterogeneity which suggests that the provider plays an important role in determining the success of the policy. Since, however, the matching between the provider and the beneficiary is endogenous

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<sup>26</sup>We resort to the inverse hyperbolic sine to accommodate for zeros in our estimation.

<sup>27</sup>This group consists of regions in the South of the country (Campania, Basilicata, Molise, Abruzzo, Puglia and Calabria), as well as in the Islands (Sicily and Sardinia).

and depends on the choice of the provider, these estimates should be taken with care and may not be causal.

### **4.2.3 Heterogeneity by proxies of financial constraints**

Financial constraints may play a role in explaining why firms need the support of the voucher to start using the services provided by the TEM.<sup>28</sup> If this is the case, we may expect that ex-ante non-financially constrained firms that did not get the voucher would nonetheless pay for the consulting services, given the high returns of these activities. The estimated average effect of the policy would, thus, stem mostly from financially constrained firms that would not otherwise be able or willing to afford these expenditures. To test this hypothesis, we consider four proxies of the firm's financial conditions. These proxies are the pledgeability of its assets (as measured by the share of tangible over total fixed assets), the financial leverage, the [Whited and Wu \(2006\)](#) Index (that signals the likelihood that the firm faces financial constraints), and the ratio of cash-holdings to assets. We define dummies equal to 1 if the firm in 2015 had above-median values for these variables, and we test for heterogeneity of our results with respect to these dummies following the methodology implemented in Section [4.2.2](#).

Results provided in Appendix Table [A6](#) do not support the hypothesis that financial constraints play a role in explaining the effects. The triple interactions are never statistically significant and point estimates show that firms that have ex-ante more pledgeable assets or more cash-holdings over assets show stronger effects in terms of total employment costs and revenues from sales. Leverage is generally uncorrelated with the strength of the results, and the Whited-Wu Index is - if anything - negatively correlated with the impact of the policy on Roe. This evidence does not support the hypothesis that financial constraints play a role in explaining the estimated effects of the voucher on firms' performance.

### **4.2.4 Qualitative evidence**

To dig deeper into this possibility, we administered open-ended interviews to the consultancy firms that were accredited as TEM suppliers by the Ministry in 2015 between July and October 2021. Out of the 163 accredited firms that

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<sup>28</sup> Alternative explanations, such as imperfect information and risk or ambiguity aversion, are discussed in Section [5](#).

provided consultancy for the policy, 43 participated in the interview (response rate close to 27%). These 43 consultancy firms provided their services to 682 firms that had been awarded vouchers in 2016. Each interview lasted from 20 to 40 minutes. The interviewer asked questions about the consultancy provided to the beneficiary firms of the 2016 vouchers, the type of services provided by the consulting firms, their evaluation of the voucher granted by the MISE, and whether the beneficiary firms continued to use their services after the initial contract. The open-ended questions were later discretized (Appendix A provides the list of questions administered during the interview).

We use these interviews to assess (i) whether client firms received consultancy from the TEM provider after the initial subsidized contract; (ii) whether the provided services included consultancy on other activities besides export (iii) whether this broader consultancy is linked to larger positive effects on firm performance.

Out of the 38 TEM providers that reported information on further collaboration between the beneficiary of the subsidy and the consulting firm, 31 (81.6%), corresponding to over 92% of beneficiary firms linked to interviewed providers, confirmed that the initial consultancy was followed by further consultancies paid for by the firm. This result is consistent with the possibility that the initial voucher encouraged firms to start acquiring consulting services by themselves. Therefore, the effects discussed in Section 4 may be the result of a longer consultancy period than the one initially supported by the Government.

The additional interest in these services by firms using the voucher in the first wave of the policy can also be assessed from the participation in the second wave of the Voucher (2017). We obtained data from MISE on applicants to the second wave and matched them with firms applying in the first wave of the voucher. Firms that applied before the time of exhaustion of resources were more likely to apply again for the voucher (+7.5 percentage points out of a baseline of 27%), and firms that used the voucher showed an even starker difference in the application rate (+18 percentage points out of a baseline of 21%). It should be noted that this difference is not related to mechanical effects since being assigned the voucher in the first wave did not imply any advantage in the assignment for the following

waves.<sup>29</sup>

Then, we asked the TEM suppliers what type of services had been provided to the beneficiary firms. These open-ended answers were then categorized into four non-mutually exclusive groups: commercialization (including assistance in identifying potential customers or suppliers abroad and marketing advice), production (including suggestions on how to restructure the productive process in support of internationalization), logistics, and regulatory advice (related to legal requirements and custom compliance). Figure A10 in the Appendix shows the distribution of answers provided by the 40 providers that answered this question. While almost all TEMs providers asserted that they gave commercialization advice, a relevant share of them declared that they also provided logistics and production support (17 and 13 providers, respectively). These answers are interesting since they hinge on an important role in supporting the streamlining of production and the management of inputs and outputs. Finally, we asked the TEM providers whether their support also concerned firms' digitalization and 15 out of the 35 firms that answered this question reported that they helped firms go digital.

To study whether the type of assistance available is correlated with the positive effect of the policy on firm performance, we include triple interactions with the various services provided in our models. Also in this case, we re-scale the dependent variable using an inverse-hyperbolic sine transformation to allow comparability between results. Table 5 reports the main difference-in-differences terms, the triple interactions, and the p-value for the sum of the two interactions being equal to zero. The results show that the effects are generally lower for firms linked to TEMs that provide production support, and higher for those related to TEMs that provide assistance for other services. Digitalization appears more consistently beneficial across outcomes, while commercialization and logistics seem beneficial, especially in terms of profitability. However, by restricting the analysis to treated firms linked to interviewed TEMs, the sample size is cut by half and estimates generally lack precision. As discussed above, also these results may arise from the endogenous matching between providers and firms: further

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<sup>29</sup>We also test if participation in the second wave mediated part of the positive effects of the first wave. Results for a diff-in-diff augmented with the second wave, reported in Table A7 in the Appendix, show that, although also the voucher in the second wave has a positive impact, the impact of the first wave remains positive and highly significant.

analysis would be needed to assess the causal interpretation of these parameters.<sup>30</sup>

#### **4.2.5 External validity: comparison with the general firm population**

In principle, one may argue that firms applying for the voucher are the ones who can benefit, and those who do not apply would not benefit. Naturally, we do not observe what the impact on those who did not apply. However, given the large increase in productivity and long-term employment on the treated, it is important to characterize firms applying to the policy to understand which firms apply for this subsidy and whether it might be reasonable to argue that the voucher would be as effective with the average firm eligible to apply for it. To do so, we compare firms applying for the voucher to the general population of firms.

We extract data from the universe of limited liability firms in Italy and compare our firms to the potential pool of applicants in 2015 (the year of application for the policy), i.e. firms with revenues above 500,000 euros in one of the three years before the policy and with less than 250 employees (SMEs). We report the related figures in Appendix Table A8.<sup>31</sup> Results show that applicant firms are larger in terms of the number of employees, revenues and profits but have a lower capital to employment-ratio, labor productivity and Roe with respect to Italian firms in the same revenue and size category. The negative differences with respect to other firms become even more marked in Table A9 in the Appendix where we control for sector and number of employees). Applying firms are in general worse than the overall population: they pay less for their employees, and they have lower revenues, labor productivity, profits, and ROE. Thus, once these firms are compared to their peers in terms of size and sectors, they are lagging behind them. The perception of this gap could lead firms to apply for public subsidies for additional support to their activities through consultancy.

These patterns can be rationalized in several ways: on the one hand, more productive firms might be less eager to acquire these services since they may feel that there is less room for them to improve and the services would provide minimal benefits; on the other hand, smaller firms might feel that they lack the resources

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<sup>30</sup>For example, a comparison between firms that acquire consultancy from the provider and firms that would be willing to buy services from the same provider but do not as a consequence of being excluded from the subsidy, would allow us to uncover the causal effect of each provider. Since this information is not available in the data, we leave this to further research.

<sup>31</sup>We only consider firms with more than 1,000 euros in employment costs.

to profitably employ these services and, in addition, they might face even more severe liquidity constraints. Whether this kind of intervention may be beneficial for these groups of firms in developed economies remains an open question.

#### 4.2.6 Robustness

We perform several robustness checks to validate our results.

Firstly, we experiment with some variations in our estimated equation in terms of the definition of the sample and estimated equation and report results in Table [A10](#). After presenting the main estimates in Panel (a) for comparison, we explore if the results hold by using non-winsorized data (Panel b), removing the zeroes for the periods after the policy in which the firm is no longer active (Panel c), or using a logarithmic rather than a linear specification (Panel d). Then, we investigate the magnitude of the LATE directly by exploiting an instrumental variable (IV) strategy, in which we instrument the actual payment of the subsidy with the timing of the application (Panel e). In addition, we restrict the sample to firms active from 2013 to 2020 to deal with possible selective attrition (panel f),<sup>32</sup> and to firms that are not part of any quota category for the subsidy assignment due to legality rating or participation in roadshows (Panel g). We also consider possible differential trends between treated and comparison firms by first netting our linear trend based on years before the policy intervention (Panel h) and then by including in our regression interactions between the levels of our variables in the baseline year (2015) and year fixed effects (Panel i). Finally, we assess the robustness of our inference by clustering at the second application rather than at the firm level (Panel l). Estimates are largely in line with our main specification.

Secondly, we show that changing the radius around the cutoff to a 20-second radius or a 40-second radius around the cutoff has little impact on the size and significance of the estimates. Results for these two different samples, reported in Figure [A13](#) and Figure [A14](#) in the Appendix, are very close to those of the baseline sample.

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<sup>32</sup>We also assess the voucher's impact on the firm's probability of survival of firms over the sample period. Figure [A11](#) shows that treated firms do not differ in the probability of survival over our time horizon. We define a firm "active" in a specific year if it reports positive employment costs. In addition we show the time pattern of the effect for this subsample of firms in Figure [A12](#) in the Appendix.



Thirdly, it might be argued that earlier applicants within the 30-second radius are still dissimilar from those applying later in some unobserved dimensions not captured by our previous tests. To focus more closely on the timing of the application and more directly exploit the change in the probability of receiving the voucher for a slightly earlier submission, we rely on a difference-in-discontinuities model (Grembi et al., 2016). This compares outcome variables *exactly* at the cutoff in the years before and after the treatment took place. The results reported in Table A11 in the Appendix, are in line with our main estimates but less precise since this estimation strategy exploits a much more local variation.

Finally, it is also possible that firms that applied earlier are driven by a special interest in the policy and would have performed better than late applicants even without the voucher. To test this hypothesis, we assess whether the timing of the application is related to the impact of the policy. We split the treatment group based on firms' time of application in ten-second bins and then estimate our difference-in-differences model. We report coefficients in Figure A15 in the Appendix, together with p-values for the equality of the coefficients across treatment groups. Effects appear to be fairly similar across bins and, in no case, the p-value hints at the possibility that the effects are different across bins. Hence, it appears unlikely that the timing of the application is related to unobservable factors of the firms that may have increased their inherent potential for growth in the period after the policy implementation.

### **4.3 Employment and workforce composition**

Firms appear to benefit from the policy both in the short term and, more distinctly, in the long term. These benefits are clear-cut in several outcomes such as revenues, labor productivity, profitability, and trade. We next assess to what extent these gains translated into higher labor demand.

For this purpose, we exploit the more granular INPS data available monthly to shed further light on the timing of the effects. These data, in combination with the trade results obtained by using custom data, and the quasi-experimental setting of the policy, also allow us to consider the timing of the effect on firm size and trade, and to verify which effect emerges first.

We present our main results in the form of an event study at the month level in

Figure 2. September 2015 is used as the reference period. Panel (a) shows that treated firms increase their number of employees in the four years following the policy assignment by about 4 workers with respect to firms in the comparison group. Treated firms also increase the number of workers with good contracts as shown in Panel (b) for workers with permanent contracts and Panel (c) for workers with full time contracts, which increase by 2 and by 2.5 units respectively. A small impact on employment emerges as soon as 8 months after the policy assignment and this shows that the increase in workforce predates the improvements for the performance of the firm.

Figure A16, Figure A17, and Figure A18 report the impact for other subcategories in the Appendix. We find that the largest increase is accrued by blue collar workers while white collar register a minor increase and no changes appear in terms of apprentices or managers. The increase in employment is slightly stronger for men than for women and, in terms of age, workers with more than 45 years of age increase the most with respect to younger workers. Firms expand their workforce mostly recruiting experienced production workers.

Table A12 in the Appendix, finally, report baseline difference-in-differences estimates, which are, however, often noisy.

## 5 Discussion

The results on balance-sheets variables show that investing in TEM consultancy is highly profitable over the medium term for firms. By the fourth year since the use of the voucher, profits increased by around 100 thousand euros, while return on equity increased by more than 30%. These results are consistent with those of Bruhn et al. (2018a), who also find that paying for consultancy services would be highly profitable for firms.

Given this large positive impact, questions naturally emerge: why are firms not using such services even without the voucher? Why do firms in the control group not (on average) pay 13 thousand euros out of pocket to purchase a service that provides such a significant benefit?

Our setting does not directly answer this question. Nevertheless, we can use the information generated by the policy and additional information gathered in the

literature, together with qualitative evidence from interviews with consulting companies to examine various potential explanations.

The management literature has focused on three main reasons why firms do not invest to improve their managerial practices (e.g., [Gibbons and Henderson 2012](#)). The first is the motivation problem (people know what would improve performance but lack the incentives). The second and third concern information frictions: the perception problem (i.e., the firm's decision-makers do not realize they are inefficient) and the inspiration problem (i.e., the decision-makers know they are inefficient but do not know how to fix this). In our case, the motivation and perception problems may be relevant: firms may underestimate the gains from the use of TEMs and thus would need the additional 10 thousand euros of the voucher to use these services. Conversely, the inspiration problem is unlikely to be relevant, as firms in the control group are aware of the existence of these services. [Bruhn et al. \(2018a\)](#) point out that entrepreneurs may be risk averse concerning the potential returns from hiring a consultant. This aversion could be perpetuated by a lack of information in the market on the returns to consulting advice, which consulting firms have difficulty credibly signalling. Since both treated and comparison group firms applied to get the subsidy, we can rule out that information frictions also encompassed a lack of information about the *existence* of these services.

Other information frictions may be key constraints preventing more firms from using these services. Firms may not know the returns to these consultancy activities, finding it difficult to judge the skills and quality of outside providers, and not being confident that they can trust an outside firm. A natural follow-up question is then to ask whether there is scope for policy interventions aimed at alleviating such policy and information frictions, besides direct subsidies. However, existing evidence from randomized policy trials show that providing information and quality ratings alone does not seem to be sufficient to encourage more firms to use professional business services ([Anderson and McKenzie \(2021\)](#)).

Financial constraints may, in principle, be the source of the problem. Indeed, in the short run, we do not observe higher profits but rather increased productivity. Financial constraints may hinder firms' ability to improve their management using external consultants since the market cost of consulting, as an intangible investment, would be difficult to collateralize. Additionally, we do not observe

consulting firms accepting delayed payments or working with financial services firms to provide credit to cover their services. Yet, this conjecture is not supported by the empirical results: using various proxies of financial constraints, we fail to find a differential impact on firms that were ex-ante more likely to be constrained.

We, thus, conclude that uncertainty about the benefits and risk aversion are the most likely explanations for the lack of market transactions in consulting services.

## **6 Conclusions: Nudging Investments in Management Skills**

This paper investigates the impact of a policy providing subsidies for consulting services to improve firms' internationalization through Temporary Export Managers. We identify the causal effect of the policy by exploiting the timing of application and the subsidy allocation based on a first-come, first-served rule. In a difference-in-differences framework, we compare firms that barely received the subsidies with those that did not, due to very small differences in the time of application.

We find that the policy effectively boosts firms' internationalization and performance across multiple dimensions. Importantly, we show that it takes time—up to four years since the application of the policy—for exports and imports to increase significantly. Before that, firms undertake changes that lead to an expansion of their workforce, a higher production level, and improvements in their revenues and labor productivity. More precisely, we find that eligible firms increase exports by an additional 200,000 euros to countries outside the European Union and imports by 100,000 euros by the fourth year after the assignment of the subsidy compared to applicants who did not receive the voucher. Trade increases mostly at the intensive margin. In addition, they increase revenues by 600,000 euros, value added per employee by 5,000 euros, and profits by 70,000 euros. Finally, we observe an increase in the firm size of about 4 employees (about 17% with respect to 2015 employment).

Treatment effects and cost-effectiveness vary across the characteristics of the applicant firms. Vouchers to less productive and smaller firms generate larger impacts. Indeed these firms might have had ample margins of improvements

even with small interventions. This partly rationalizes the large effect of the policy. The timing of the effects for the various outcomes suggests that a larger workforce is essential for expanding SMEs' trade performance.

These large effects may seem surprising at first, given that the amount of the subsidy was only 10,000 euros (and total expenditure was mostly below 14,000 euros). However, the magnitude of the results is comparable or smaller to other findings in the literature. For instance, [Bloom et al. \(2013\)](#) document a 9% increase in sales and a 17% increase in output TFP one year after offering management consulting to large Indian firms. Our results are smaller than those of [Bruhn et al. \(2018a\)](#), which estimate a 26% increase in TFP within one year for small Mexican firms receiving managerial consultancy, a 70% growth in sales and a 44% increase in employment within five years.

In addition, this initial consultancy marked the beginning of a long relationship with the consultants. Indeed, results from a survey we have administered to TEM providers suggest that most firms that benefited from the initial consultancy continued to invest in management skills and firm organization. The presence of additional services in particular related to commercialization and digitalization is associated with larger positive effects on firms' performance.

Certainly, we do not want to advocate for free consulting, given its extremely high cost. However, the policy we analysed provided a *nudge* to undertake additional trust and investments in management capabilities with a limited budget expenditure.

Our work also highlights several topics for future research. One interesting extension involves examining in more detail the spillover of better management practices across firms within the same industry or region and the complementarity of different bundles of management practices. More generally, there is still much to learn about how managerial inputs and expertise gained through consultancy affect firm performance. We find suggestive evidence that TEM providers have very different impacts on firms. A better understanding of which elements contribute to making consultancy and TEMs successful in improving firm performance remains a critical area for future research.

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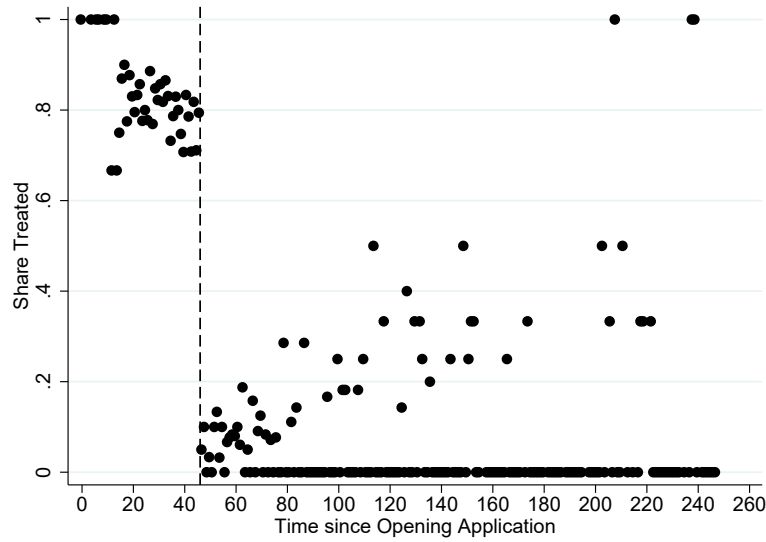
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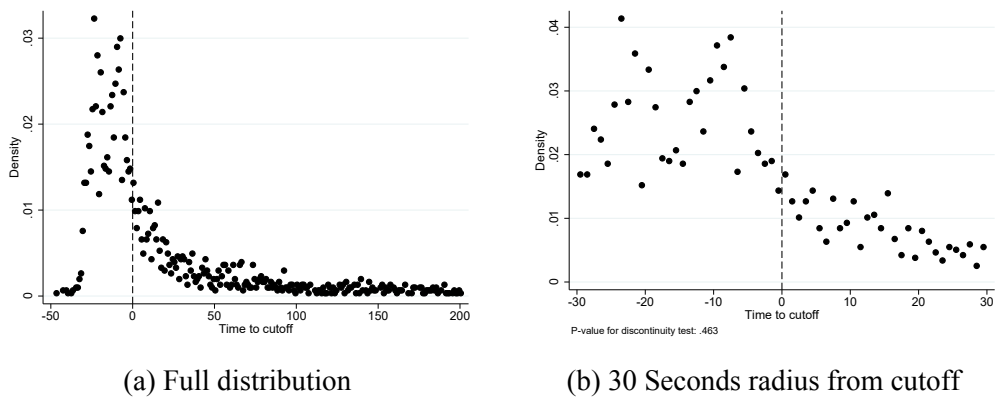
# Figures

Figure 1: Share of firms who were assigned the subsidy by application time



Note: Share of firms receiving the temporary export manager voucher by time of application.

Figure 2: Density Discontinuity

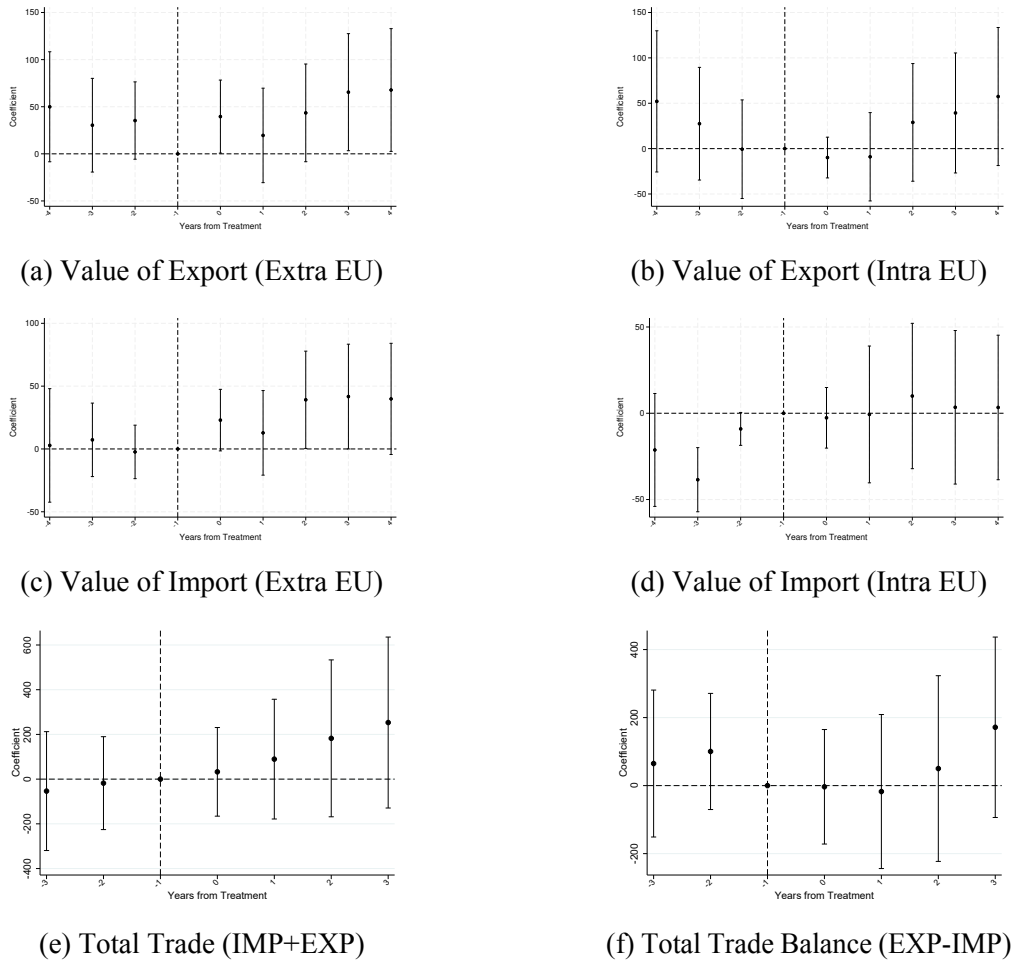


(a) Full distribution

(b) 30 Seconds radius from cutoff

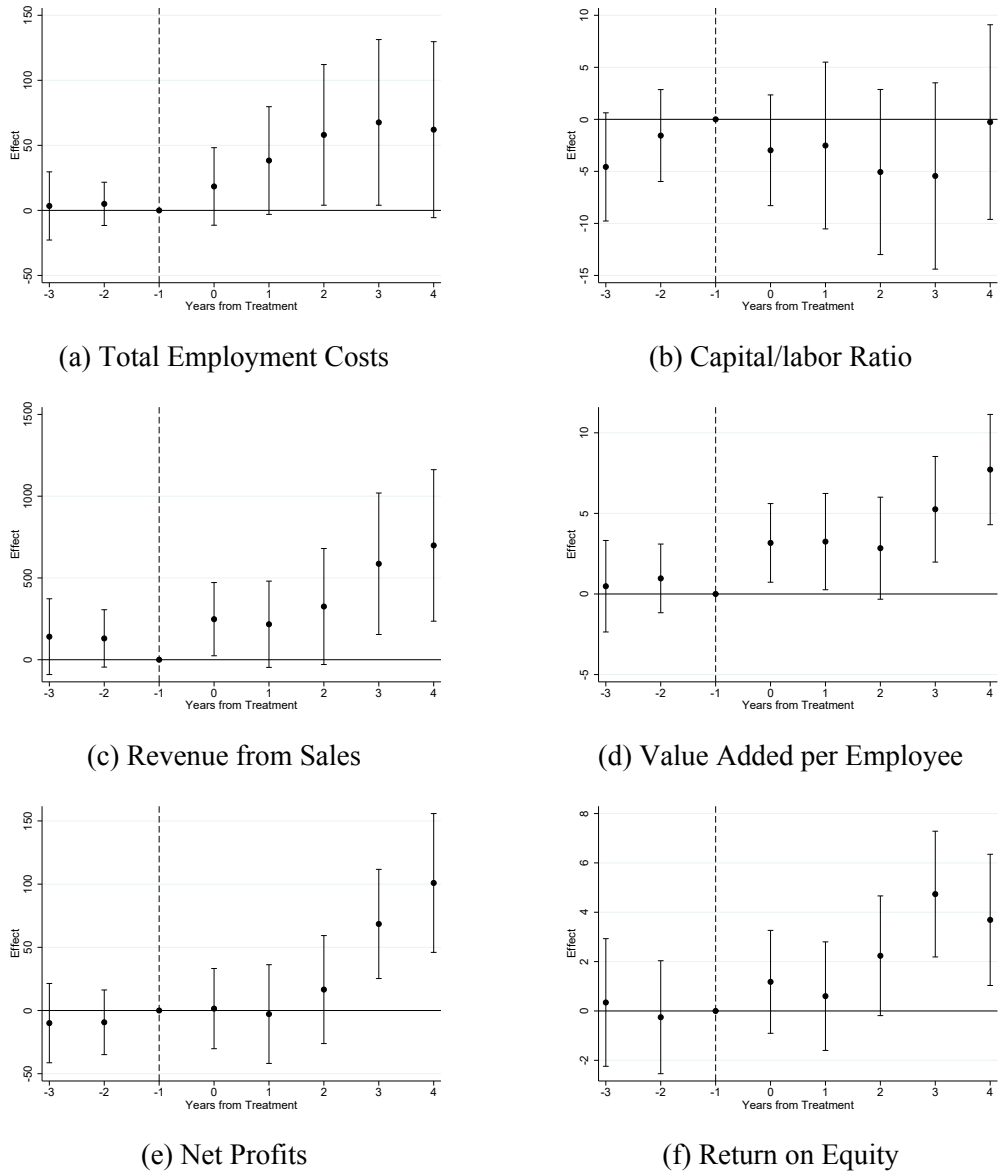
Note: Density of applications for the temporary export manager voucher by the time of arrival of the completed application within the first four minutes, and within 30 seconds with respect to the 2002<sup>nd</sup> application, which roughly corresponds to the theoretical exhaustion of resources.

Figure 3: Effect of Subsidy Assignment on Firm Internationalization Over Time



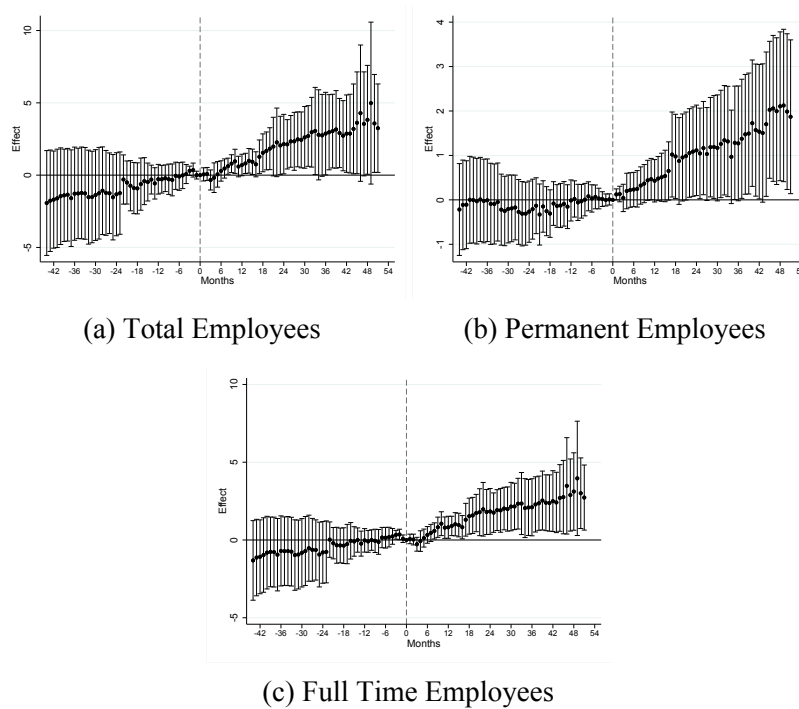
*Notes:* This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year reported together with their 95% confidence interval. All effects are reported in thousands of euros. Standard errors are clustered at the firm level.

Figure 4: Effect of TEM Voucher Assignment on Firm Outcomes Over Time



Notes: This figure reports the results of a difference-in-differences model estimated between 2013 and 2020. Capital/labor ratio is computed as the ratio between total assets (material+immaterial) and the number of employees. The base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. All effects are reported in thousands of euros but for RoE which is reported in percentage points. Coefficients of the difference with respect to the base year were reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

Figure 5: Effect of TEM Voucher Assignment on Firm labor Demand Over Time



*Notes:* This figure reports the results of a difference-in-differences model based on monthly data between 2012 and 2019. The base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and month fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

## Tables

Table 1: Comparison of Treated and Control Firms In the Year of the Application to the Policy (2015)

Outcome variable	(1) Average Treated	(2) Average Controls	(3) Difference (1)-(2)	(4) T-Stat
Export Extra EU	1409.093	1245.399	163.694	1.009
Export Intra EU	2040.698	1748.033	292.665	1.256
Import Extra EU	563.191	525.977	37.214	0.400
Import Intra EU	734.183	589.285	144.898	1.465
Total Trade	8234.942	7224.419	1010.523	1.219
Trade Balance	3737.930	3378.446	359.484	0.633
Total Employment	25.037	23.276	1.761	1.417
Total Employment Cost	968.518	879.266	89.252	1.712
Capital Employment Ratio	76.765	66.300	10.465	2.076
Revenue from Sales	5609.342	5616.144	-6.802	-0.019
Value Added per Employee	54.497	55.710	-1.213	-0.778
Net Profits	125.157	121.419	3.738	0.192
ROE	7.927	9.818	-1.891	-1.811
Broadband Conn. (% Buildings)	0.251	0.250	0.001	0.077
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	0.000	-0.011
Tot. Employees	28.26	23.66	4.6	1.8
Permanent Employees	24.11	21.64	2.47	1.75
Full Time Employees	24.63	21.15	3.48	1.7
Blue Collars	15.48	12.79	2.69	1.75
White Collars	10.6	8.85	1.74	1.55
Managers	0.52	0.59	-0.07	-0.43
Apprentices	1.33	1.17	0.16	1.09
Women	9.57	7.23	2.34	1.81
Men	18.69	16.42	2.26	1.57
Junior (age 16-29)	3.97	2.88	1.09	1.59
Mid-level (age 30-45)	13.02	10.7	2.32	1.89
Senior (age >45)	11.27	10.08	1.19	1.39
Number firms	1782	587		

*Notes:* Summary statistics for treatment and comparison group. Column (3) reports the difference in the average between the two groups and Column (4) reports the t-statistic for the difference between the two groups obtained from a OLS regression on the variable on a dummy for having applied before the time cutoff. Standard errors are clustered at firm level. Variables for firm and trade outcomes are winsorized at 1%. All variables reported in thousands of euros but Roe, which is reported in percentage points.

Table 2: Comparison of Treated and Control Firms In the Year of the Application to the Policy (2015): Normalized Differences

Outcome variable	(1) Average Treated	(2) Average Controls	(3) Normalized Differences
Export Extra EU	1409.093	1245.399	0.047
Export Intra EU	2040.698	1748.033	0.059
Import Extra EU	563.191	525.977	0.019
Import Intra EU	734.183	589.285	0.067
Total Trade	8234.942	7224.419	0.075
Trade Balance	3737.930	3378.446	0.039
Total Employment Cost	968.518	879.266	0.079
Capital Employment Ratio	76.765	66.300	0.097
Revenue from Sales	5609.342	5616.144	-0.001
Value Added per Employee	54.497	55.710	-0.038
Net Profits	125.157	121.419	0.009
ROE	7.927	9.818	-0.085
Broadband Conn. (% Buildings)	0.251	0.250	0.004
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	-0.001

Notes: Normalized differences for the comparison of the treatment and comparison group (Imbens and Wooldridge, 2009). Normalized differences are computed according to the following formula:  $\Delta = \frac{\bar{X}_T - \bar{X}_C}{\left(\frac{s_T^2 + s_C^2}{2}\right)^{\frac{1}{2}}}$  Variables for firm and trade outcomes are winsorized at 1% and reported in thousands of euros but for Roe, reported in percentage points.

Table 3: Effect of TEM Voucher Assignment on Balance Sheet Outcomes

Variables	(1) Employment Cost	(2) C/E Ratio	(3) Revenue from Sales	(4) VA per Employee	(5) Net Profits	(6) Roe
Before Cutoff X Post	46.105 (24.974)	-1.256 (3.149)	325.720 (165.366)	3.968 (1.194)	43.297 (15.488)	2.461 (0.817)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
R-squared	0.917	0.795	0.917	0.652	0.624	0.382
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *before cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm, which corresponds to the theoretical exhaustion of available funds. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. All variables are winsorized at 1%. Mean control is the average for firms which applied after the time threshold in the period after 2015. Standard errors are clustered at the firm level. All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points.

Table 4: Heterogeneous Effects by Sub-Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Employment Cost (log)				Capital/Employment Ratio (log)				Revenues from Sales (log)			
Before Cutoff X Post	0.136 (0.084)	-0.030 (0.100)	0.036 (0.094)	0.322 (0.170)	-0.041 (0.061)	-0.046 (0.069)	-0.006 (0.076)	-0.127 (0.123)	0.155 (0.097)	-0.027 (0.113)	0.033 (0.109)	0.368 (0.198)
Before Cutoff X Post X South	-0.099 (0.111)				-0.033 (0.096)				-0.091 (0.133)			
Before Cutoff X Post X Small Firm		0.309 (0.151)				-0.023 (0.115)				0.331 (0.177)		
Before Cutoff X Post X Low Productivity			0.174 (0.153)				-0.092 (0.114)				0.226 (0.179)	
Before Cutoff X Post X Exporter				-0.288 (0.188)				0.106 (0.138)				-0.323 (0.219)
Observations	18,757	18,757	18,757	18,757	18,554	18,554	18,554	18,554	18,757	18,757	18,757	18,757
R-squared	0.733	0.733	0.733	0.733	0.726	0.726	0.726	0.726	0.697	0.697	0.697	0.697
P-value Sum	0.707	<b>0.014</b>	<b>0.080</b>	0.673	0.414	0.453	0.253	0.741	0.609	<b>0.026</b>	<b>0.068</b>	0.63
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	VA per employee (log)				Net Profits (log)				Roc (log)			
Before Cutoff X Post	0.178 (0.066)	0.103 (0.076)	0.162 (0.086)	0.290 (0.133)	0.387 (0.153)	0.342 (0.214)	0.270 (0.198)	0.432 (0.248)	0.377 (0.096)	0.268 (0.117)	0.236 (0.114)	0.505 (0.165)
Before Cutoff X Post X South	-0.007 (0.109)				-0.313 (0.264)				-0.275 (0.165)			
Before Cutoff X Post X Small Firm		0.149 (0.128)				-0.001 (0.283)				0.149 (0.180)		
Before Cutoff X Post X Low Productivity			0.034 (0.127)				0.146 (0.283)				0.215 (0.180)	
Before Cutoff X Post X Exporter				-0.157 (0.150)				-0.123 (0.302)				-0.232 (0.196)
Observations	18,514	18,514	18,514	18,514	18,748	18,748	18,748	18,748	18,550	18,550	18,550	18,550
R-squared	0.552	0.552	0.552	0.552	0.515	0.515	0.516	0.515	0.458	0.458	0.459	0.458
P-value Sum	0.118	<b>0.014</b>	<b>0.035</b>	<b>0.057</b>	0.765	<b>0.066</b>	<b>0.039</b>	<b>0.072</b>	0.513	<b>0.002</b>	<b>0.001</b>	<b>0.009</b>
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes by firm characteristics. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. South is a dummy taking value one if the applying firm is located in the South or in the Islands (Sicily and Sardinia). Small firm is a dummy taking value one if the firm employs less than 15 employees in 2015 (this also corresponds to the median size of applying firms). Low Productivity refers to firms in the bottom half of the VA per employee distribution in 2015. Exporter is a dummy taking value one if the firm was already an exporter (within or outside the European Union) in 2015. The model also includes the interaction between the relevant dummy per column and the post dummy, year and firm fixed effects. P-value sum is the p-value for a F-test assessing whether the sum of the main coefficient (Before CutoffXPost) and of the appropriate triple interaction is different from zero. All variables are winsorized at 1%. Standard errors are clustered at the firm level.

Table 5: Heterogeneous Effects by Services Provided

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment Cost	K/L Ratio	Revenues	VA/Employee	Net Profits	Roe
Before Cutoff X Post	0.202 (0.288)	0.176 (0.187)	0.353 (0.261)	0.314 (0.176)	-0.184 (0.491)	-0.177 (0.350)
Before Cutoff X Post X Commercialization	-0.094 (0.288)	-0.260 (0.187)	-0.195 (0.260)	-0.153 (0.174)	0.642 (0.491)	0.587 (0.349)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.729	0.679	0.535	0.493	0.452
P-value Sum	0.238	0.236	0.145	<b>0.033</b>	<b>0.007</b>	<b>0.000</b>
Before Cutoff X Post	0.185 (0.117)	-0.035 (0.090)	0.216 (0.137)	0.237 (0.089)	0.235 (0.210)	0.238 (0.141)
Before Cutoff X Post X Logistics	-0.118 (0.128)	-0.055 (0.100)	-0.077 (0.147)	-0.110 (0.096)	0.306 (0.231)	0.226 (0.150)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	0.524	0.271	0.257	0.143	<b>0.007</b>	<b>.000</b>
Before Cutoff X Post	0.102 (0.094)	-0.080 (0.072)	0.152 (0.109)	0.150 (0.076)	0.409 (0.173)	0.372 (0.108)
Before Cutoff X Post X Regulation	0.175 (0.173)	0.152 (0.142)	0.255 (0.214)	0.279 (0.138)	0.146 (0.359)	0.025 (0.293)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	0.108	0.61	<b>0.058</b>	<b>0.002</b>	0.122	0.177
Before Cutoff X Post	0.087 (0.099)	-0.064 (0.075)	0.153 (0.115)	0.154 (0.081)	0.529 (0.185)	0.407 (0.115)
Before Cutoff X Post X Production	0.102 (0.144)	-0.019 (0.116)	0.063 (0.163)	0.060 (0.105)	-0.413 (0.253)	-0.124 (0.164)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	0.175	0.460	0.173	<b>0.038</b>	0.635	<b>0.077</b>
Before Cutoff X Post	0.060 (0.122)	-0.109 (0.093)	0.045 (0.147)	0.109 (0.092)	0.415 (0.213)	0.294 (0.135)
Before Cutoff X Post X Digitalization	0.092 (0.131)	0.068 (0.102)	0.215 (0.152)	0.104 (0.098)	0.008 (0.233)	0.136 (0.147)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	0.138	0.618	<b>0.026</b>	<b>0.013</b>	<b>0.033</b>	<b>0.001</b>
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

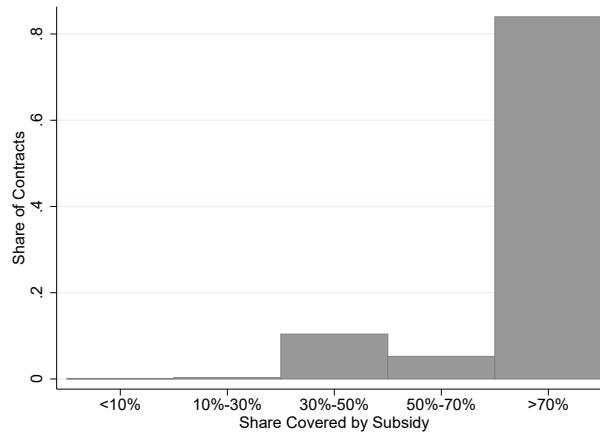
*Notes:* Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Treated firms were included in the analysis only if we could interview their TEM provider (the number of treated firms declines from 1,779 to 557). Commercialization, Logistics, Regulations, Production, and Digitalization are dummies equal 1 if the TEM linked to the treated firm asserts it provides these services. The model also includes the interaction between the relevant dummy per column and the post dummy, year and firm fixed effects. P-value sum is the p-value for an F-test assessing whether the sum of the main coefficient (Before CutoffXPost) and of the appropriate triple interaction is different from zero. All variables are winsorized at 1%. Standard errors are clustered at the firm level.



# For Online Publication: Appendix

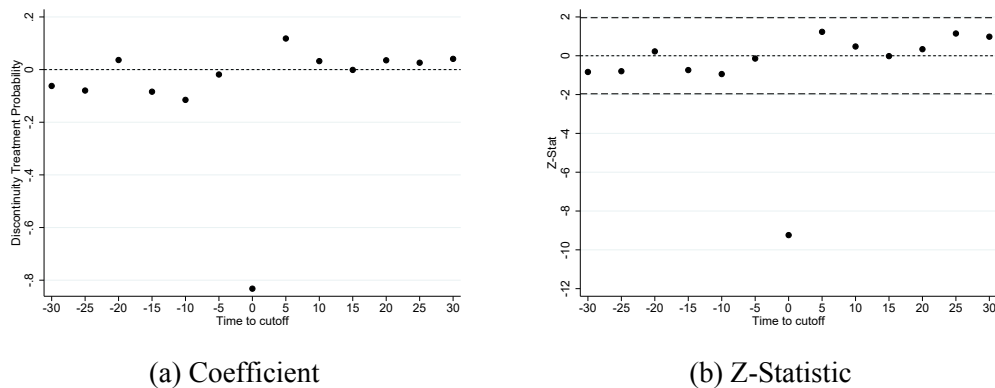
## Appendix Figures

Figure A1: Share of Contracts by Amount Covered by the Policy



Note: Figure plots the share of contracts by the ratio between the amount of the subsidy (10,000 euros) and the total value in euros of the contract reported to the Ministry of Economic Development.

Figure A2: Test for Discontinuity in the Treatment Probability

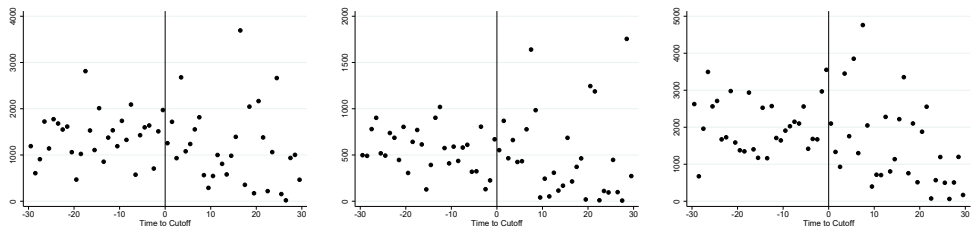


(a) Coefficient

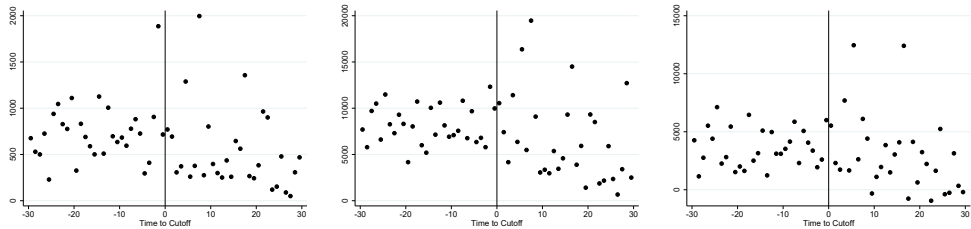
(b) Z-Statistic

Note: Figure reports coefficients for RDD equations testing for the presence of a discontinuities in the share of firms benefiting from the subsidy by time of application. Panel (a) reports coefficients while Panel (b) reports corresponding z-statistics. Dotted line correspond to thresholds for 5% significance. The equation is estimated with the `rdrubst` command by [Calonico et al. \(2017\)](#) and it uses a linear local polynomial, a triangular kernel, and bandwidth selected through the minimum squared error criterion.

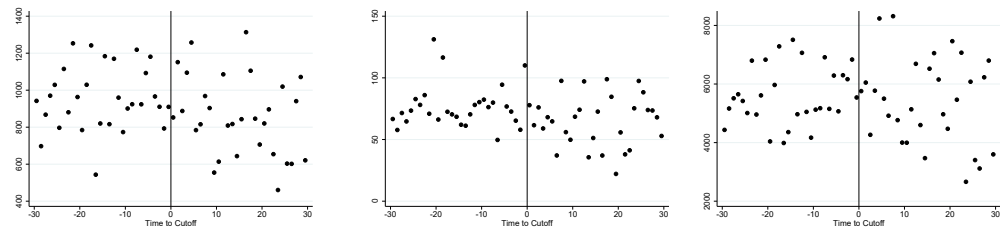
Figure A3: Observable Characteristics for Trade and Firm variables in 2015 by Time of Application



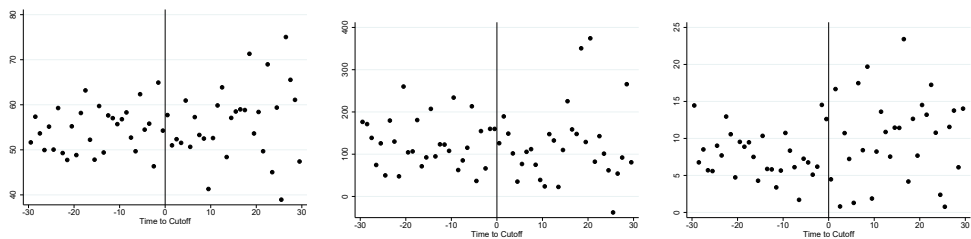
(a) Value Export (Extra EU) (b) Value Import (Extra EU) (c) Value Export (Intra EU)



(d) Value Import (Intra EU) (e) Total Trade (f) Trade Balance



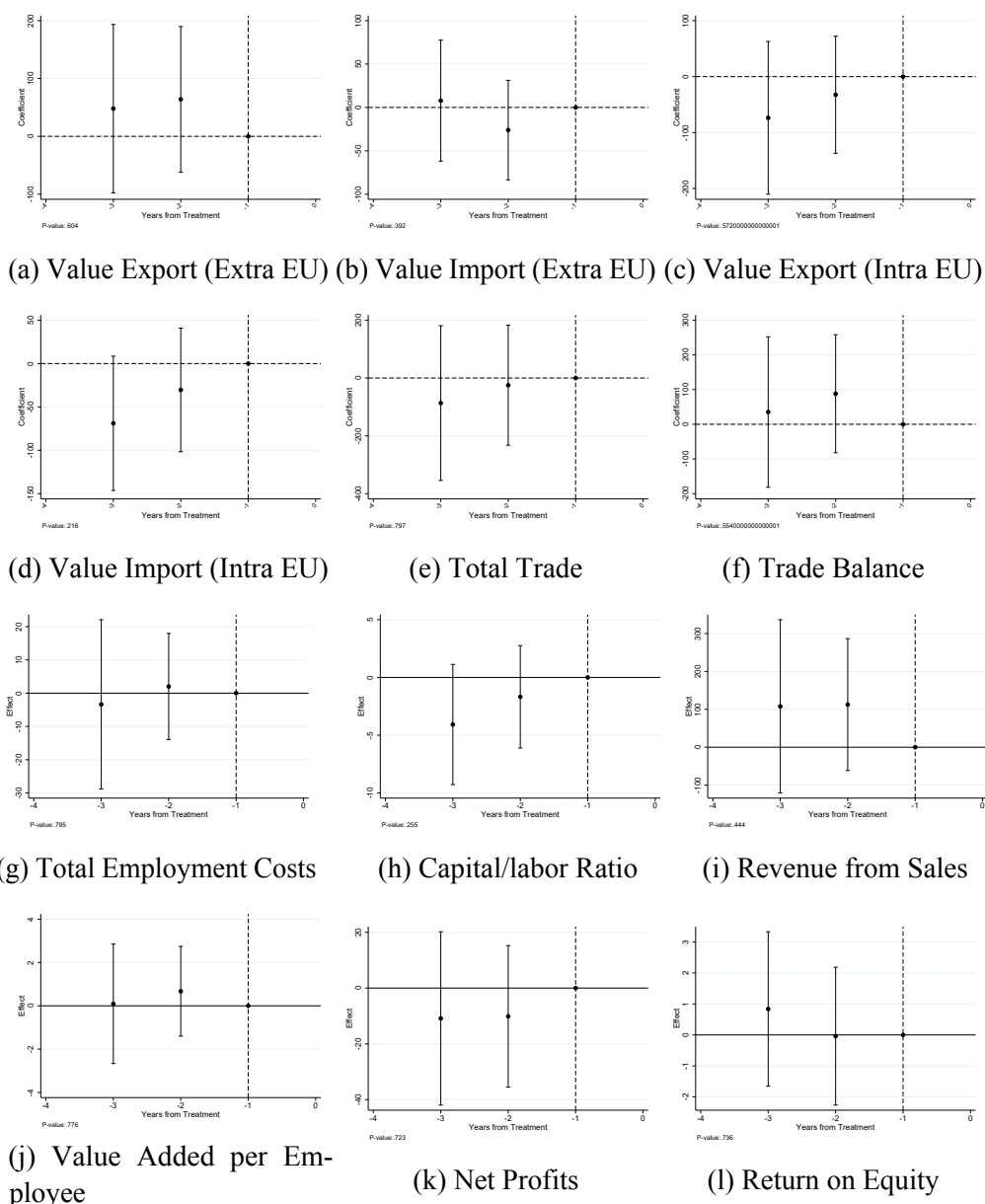
(g) Total Employment Costs (h) Capital/labor Ratio (i) Revenue from Sales



(j) Value Added per Employee (k) Net Profits (l) Return on Equity

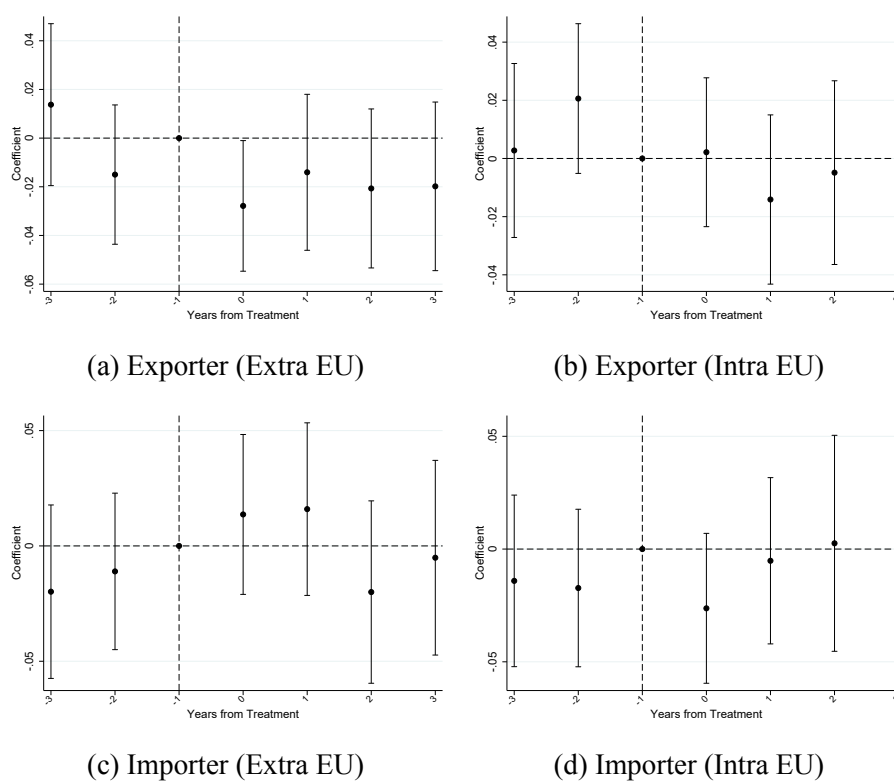
*Notes:* Average trade and firm characteristics for firms applying for the subsidy by time of application. Sample restricted to firms applying within a 30 second radius from exhaustion of funds. All variables are winsorized at 1%.

Figure A4: Differences in Trends for Main Variables for Treated and Control Firms in the Periods before the Policy Voucher Assignment.



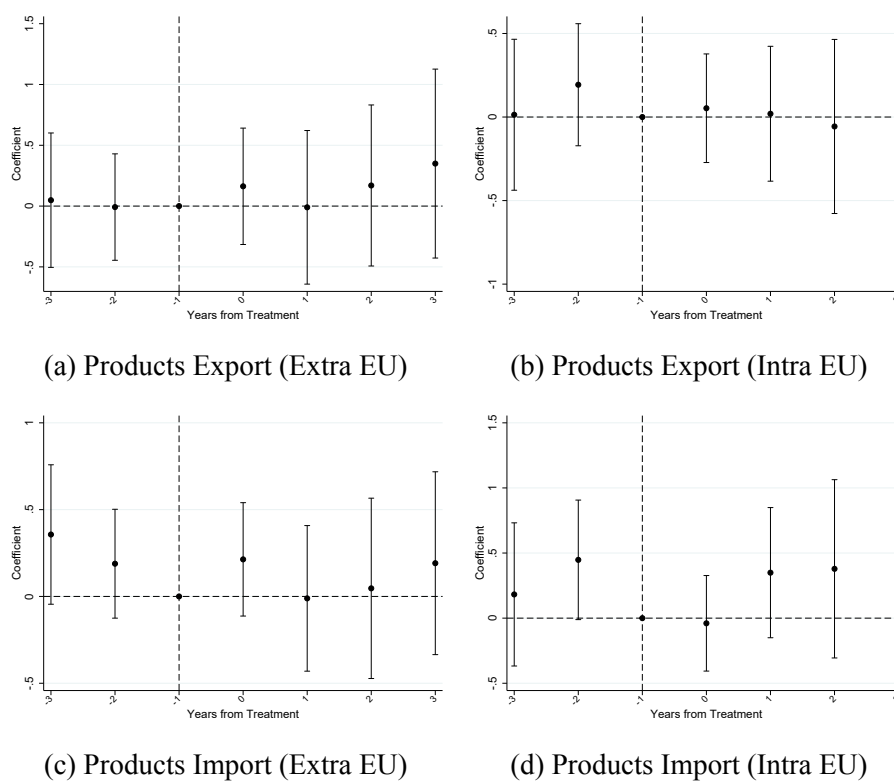
*Notes:* This figure reports results from a difference-in-differences model for the periods before the voucher assignment (2013-2015). All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. All effects are reported in thousands of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

Figure A5: Effect of TEM Voucher Assignment on Exporting and Importing by Broad Destination



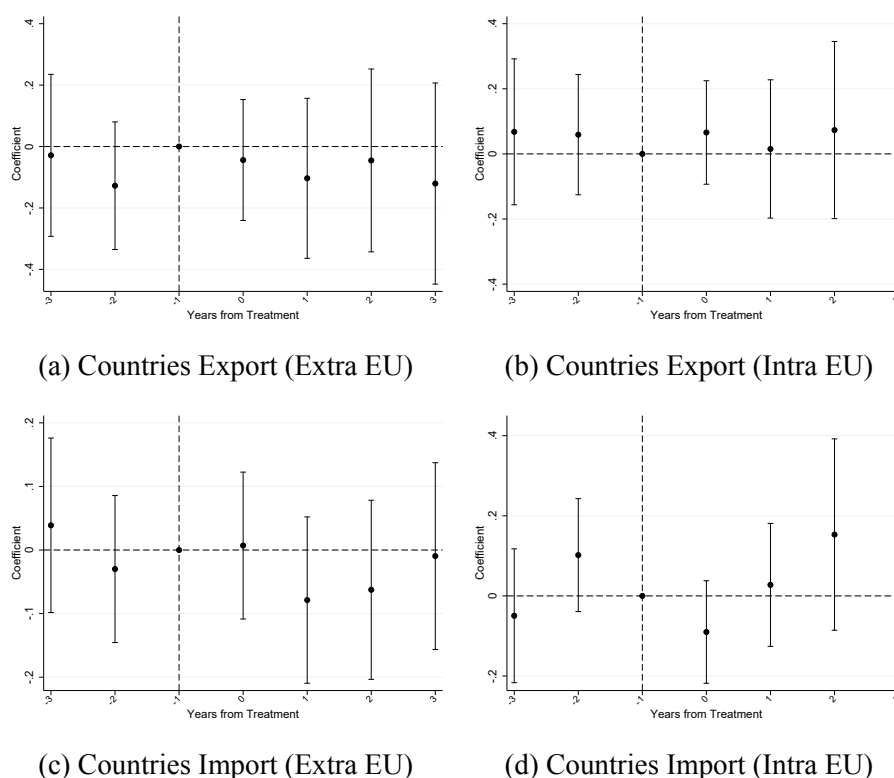
*Notes:* This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Estimates are based on linear probability models with the dependent variable equal to one if the firm exports/imports to/from the specified group of countries and zero otherwise. Standard errors are clustered at the firm level.

Figure A6: Effect of TEM Voucher Assignment on Number of Products for Export and Import by Broad Destination



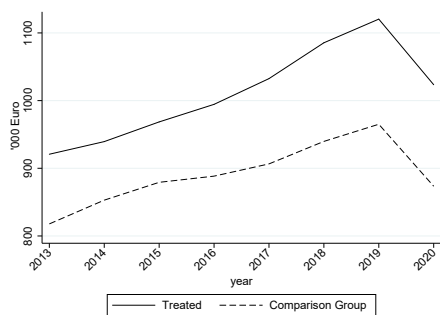
*Notes:* This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Estimates are based on OLS models with the dependent variable equal to the number of products that the firm exports/imports to/from the specified group of countries. Standard errors are clustered at the firm level.

Figure A7: Effect of TEM Voucher Assignment on Number of Countries for Export and Import by Broad Destination

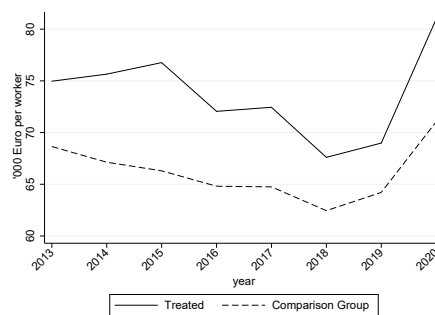


*Notes:* This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Estimates are based on OLS models with the dependent variable equal to the number of countries to which the firm exports/imports from the specified group of countries. Standard errors are clustered at the firm level.

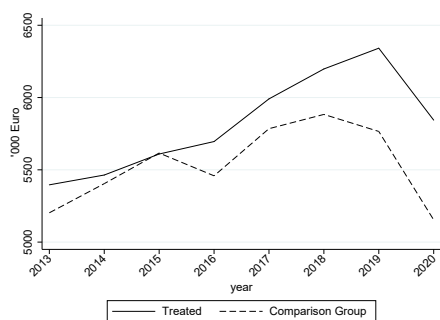
Figure A8: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: raw data



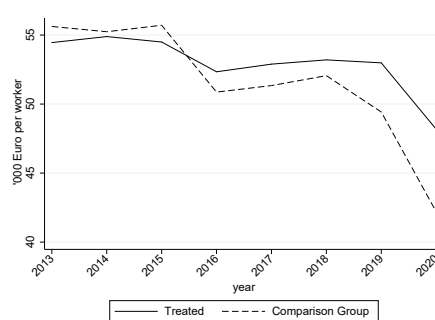
(a) Total Employment Costs



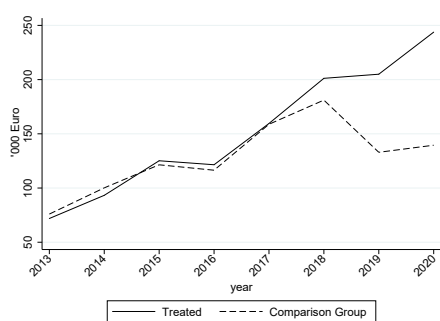
(b) Capital/labor Ratio



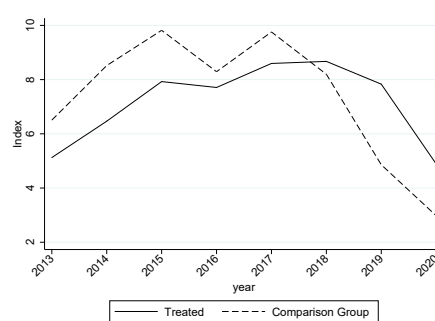
(c) Revenue from Sales



(d) Value Added per Employee



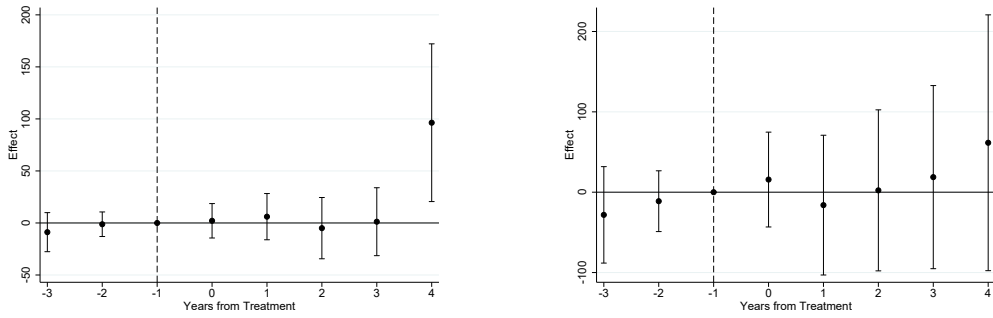
(e) Net Profits



(f) Return on Equity

Notes: This figure reports averages for early applicants (Treated) and late applicants (Comparison Group) for the years between 2013 and 2020. Variables winsorized at 1%.

Figure A9: Effect of TEM Voucher Assignment on firms' Assets

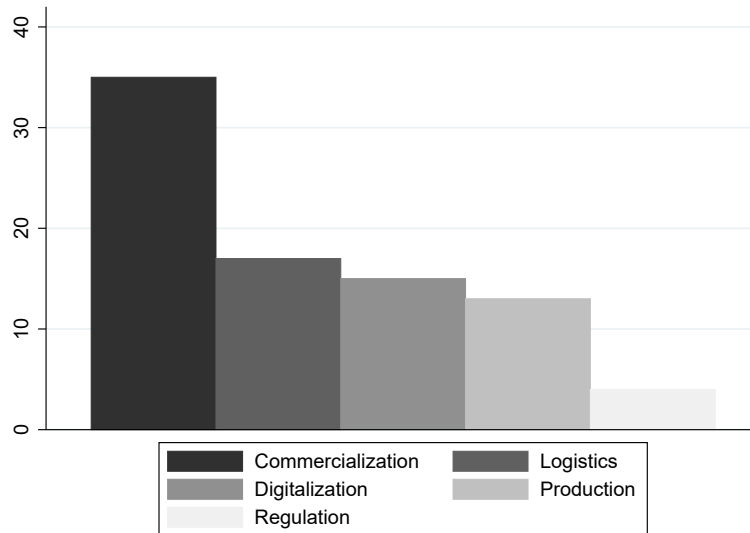


(a) Immaterial Assets

(b) Material Assets

Notes: This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. All effects are reported in thousands of euros. Standard errors are clustered at the firm level.

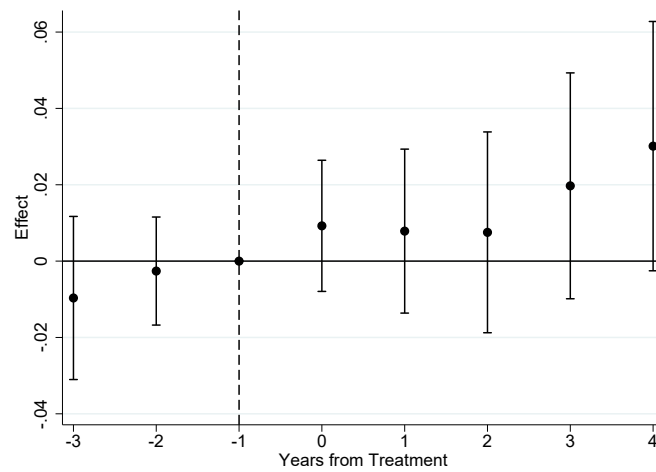
Figure A10: Services provided by TEMs to firms that benefited from the voucher



Note: Services provided by TEMs according to an open-ended answer provided by 40 TEM consultancy firms interviewed during the period June-October 2021.

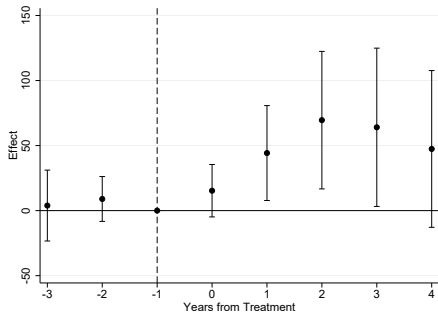


Figure A11: Effect of TEM Voucher Assignment on firms' Survival probability

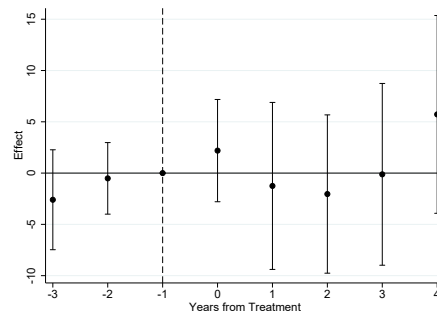


*Notes:* This figure reports results from a difference-in-differences model estimated between 2013 and 2019. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Estimates are based on OLS models with the dependent variable equal to one if the firm is active in year  $t$ . A firm is considered to be active if it has positive employment expenditure in year  $t$ . Standard errors are clustered at the firm level.

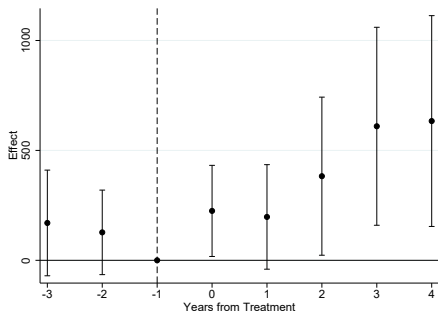
Figure A12: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Active from 2013 to 2020



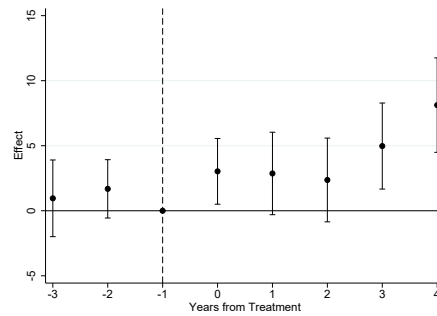
(a) Total Employment Costs



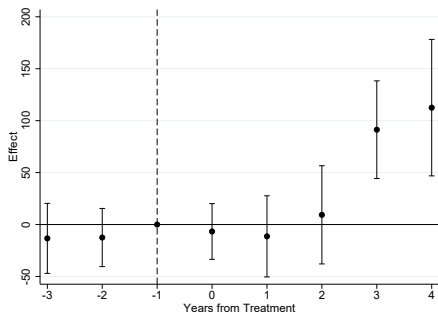
(b) Capital/labor Ratio



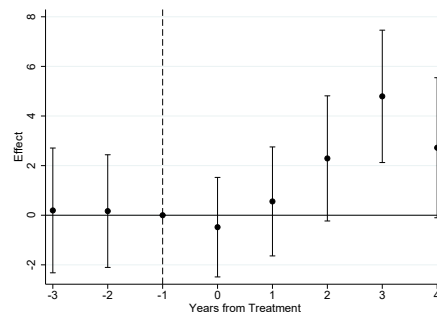
(c) Revenue from Sales



(d) Value Added per Employee



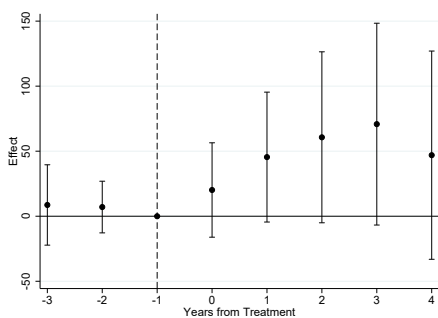
(e) Net profits



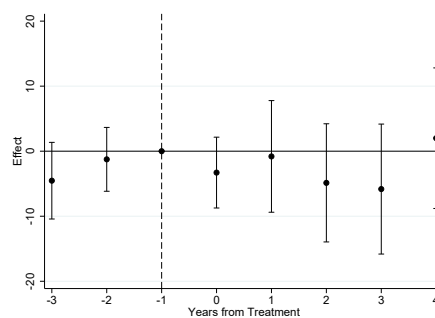
(f) Return on Equity

*Notes:* Results of a difference-in-differences model estimated between 2013 and 2020. Capital/labor ratio is computed as the ratio between total assets (material+immaterial) and the number of employees. The base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The sample was restricted to firms being active from 2013 up to 2020. The regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. All effects are reported in thousands of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

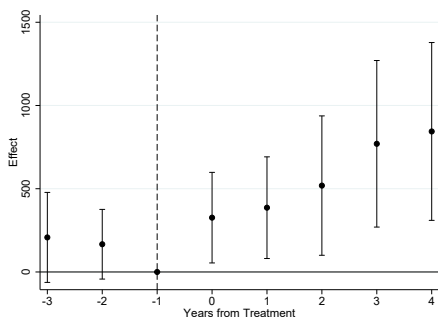
Figure A13: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 20 Seconds



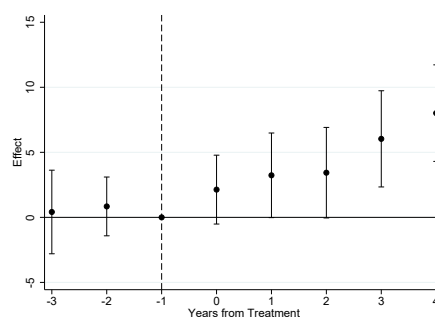
(a) Total Employment Costs



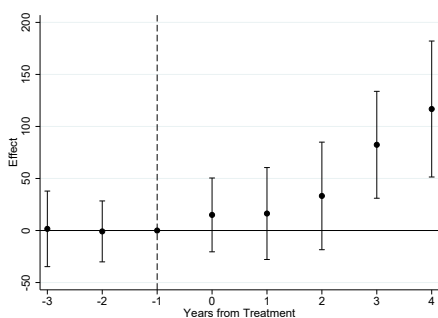
(b) Capital/labor Ratio



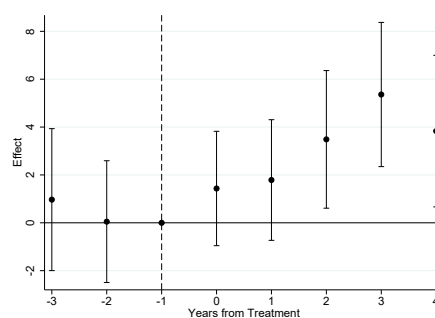
(c) Revenue from Sales



(d) Value Added per Employee



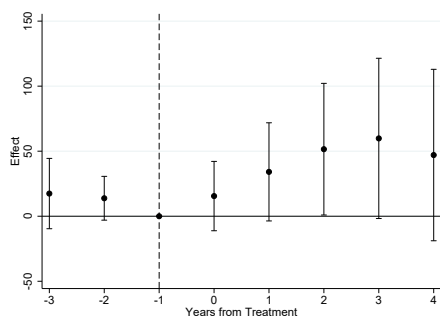
(e) Net profits



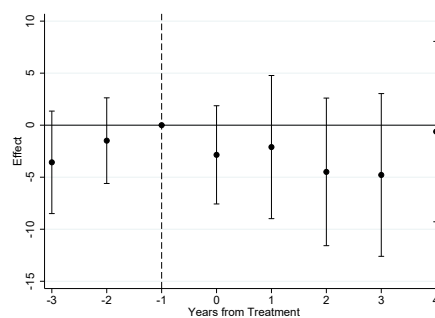
(f) Return on Equity

*Notes:* Results of a difference-in-differences model estimated between 2013 and 2020. The Capital/labor ratio is computed as the ratio between total assets (material+immaterial) and the number of employees. The base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 20 seconds of the threshold. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. All effects are reported in thousands of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

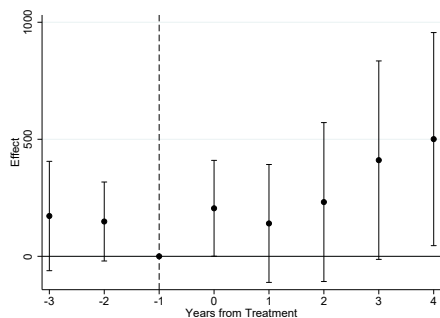
Figure A14: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 40 Seconds



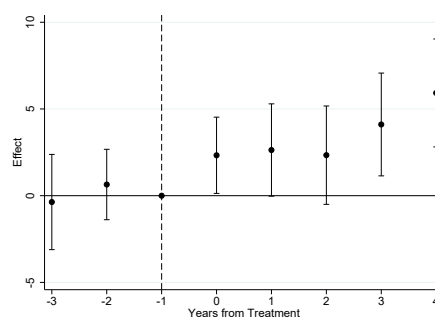
(a) Total Employment Costs



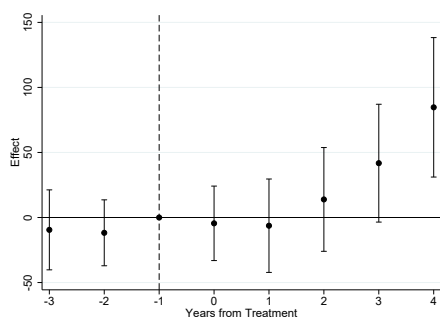
(b) Capital/labor Ratio



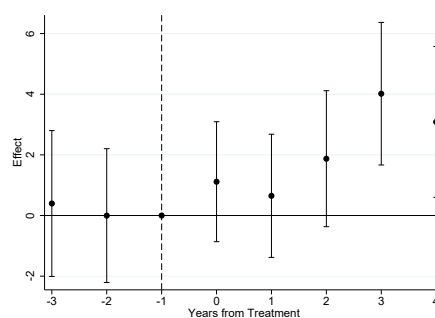
(c) Revenue from Sales



(d) Value Added per Employee



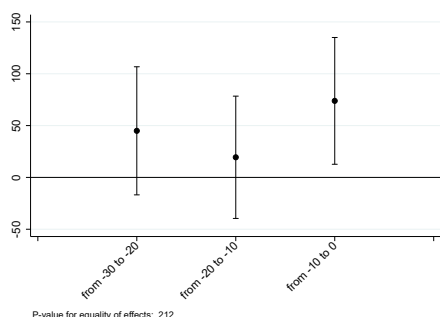
(e) Net profits



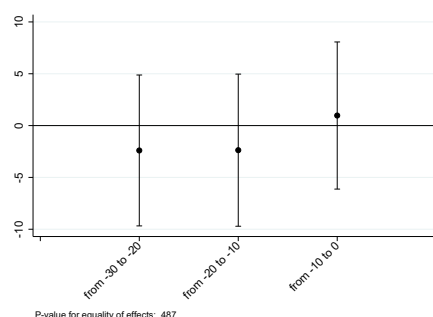
(f) Return on Equity

*Notes:* Results of a difference-in-differences model estimated between 2013 and 2020. The Capital/labor ratio is computed as the ratio between total assets (material+immaterial) and the number of employees. The base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 40 seconds of the threshold. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. All effects are reported in thousands of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

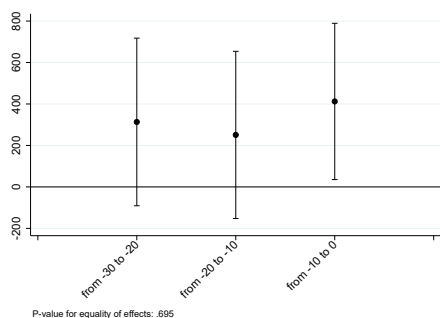
Figure A15: Effect of TEM Voucher by Time of Application Bin (10 Seconds)



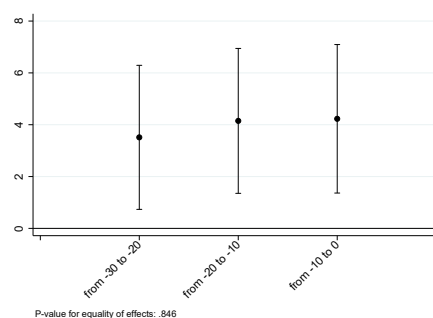
(a) Total Employment Costs



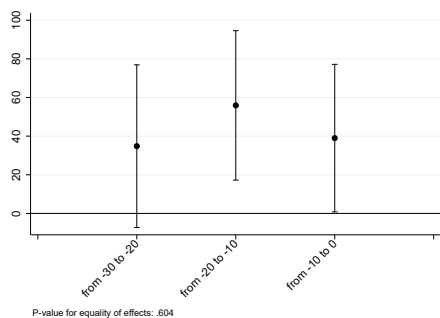
(b) Capital/labor Ratio



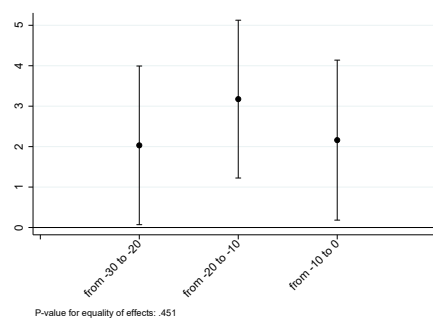
(c) Revenue from Sales



(d) Value Added per Employee



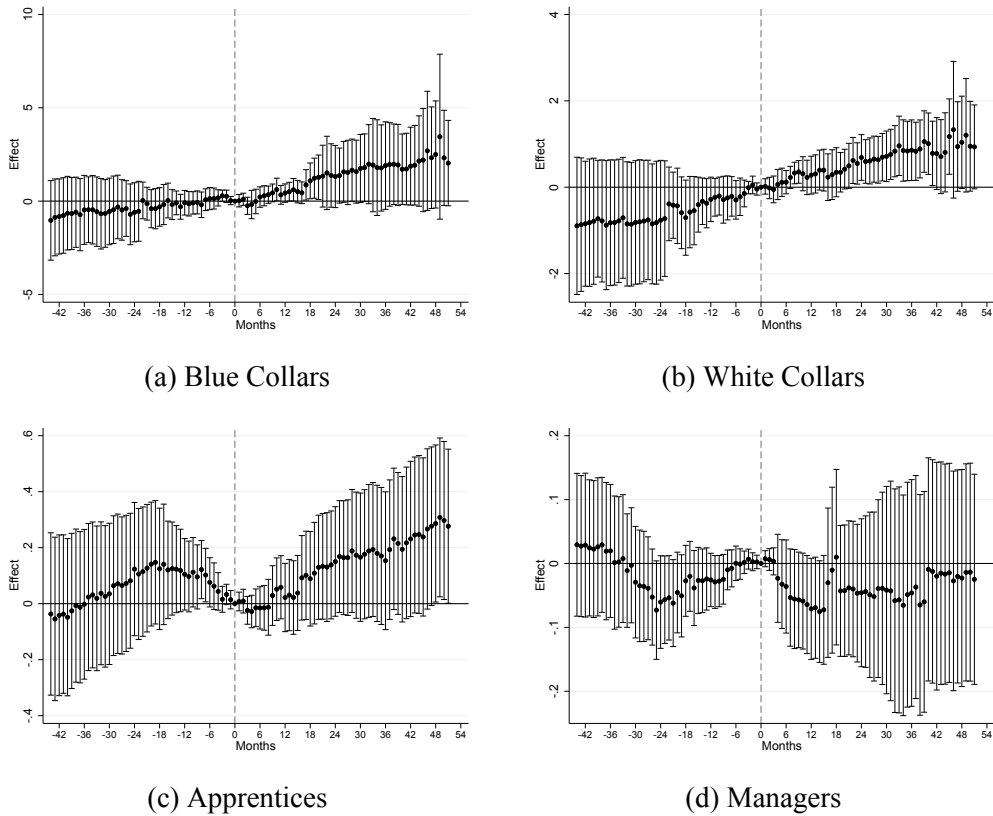
(e) Net profits



(f) Return on Equity

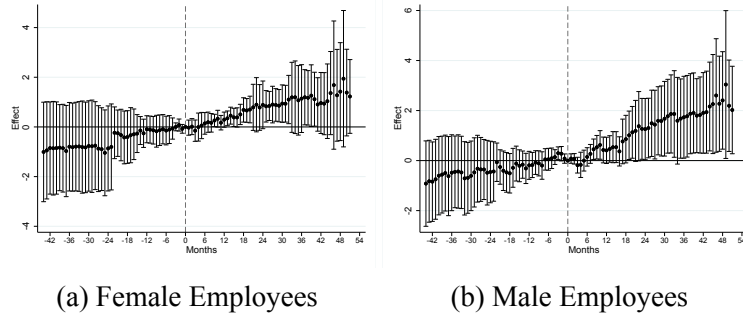
*Notes:* Results of a difference-in-differences model estimated between 2013 and 2020. Firms applying before the cutoff are divided into groups based on the time of application. We report treatment effects together with p-values for a F-test for the equality of the effects. The capital/labor ratio is computed as the ratio between total assets (material and immaterial) and the number of employees. The base year is 2015. All variables are winsorized at 1%. Treated firms are firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. All effects are reported in thousands of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

Figure A16: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Worker Qualification



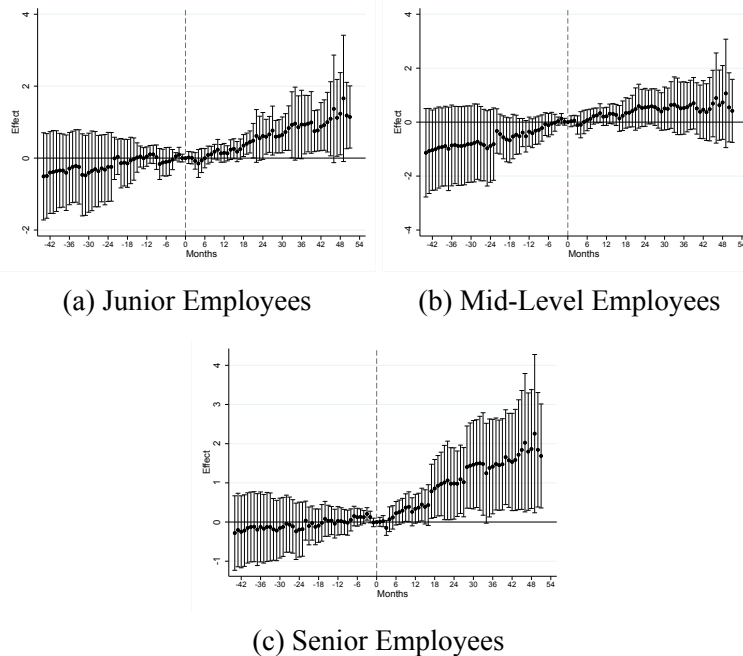
*Notes:* This figure reports the results of a difference-in-differences model based on monthly data between 2012 and 2019. The base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and month fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

Figure A17: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Gender



Notes: This figure reports the results of a difference-in-differences model based on monthly data between 2012 and 2019. The base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The regression includes firm and month fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

Figure A18: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Age Group



Notes: This figure reports the results of a difference-in-differences model based on monthly data between 2012 and 2019. The base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Young Employees (Panel a) are workers below 29, Mid-Level Employees (Panel b) are workers between 30 and 45; Senior Employees (Panel c) are workers above 45 years of age. The regression includes firm and month fixed effects. Coefficients of the difference with respect to the base year are reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

## Appendix Tables

Table A1: Comparison of Treated and Control Firms In the Year of the Application to the Policy (2015): all applicants included

Outcome variable	(1) Average Treated	(2) Average Controls	(3) Difference (1)-(2)	(4) T-Stat
Export Extra EU	1422.331	1275.604	146.726	1.307
Export Intra EU	2016.619	1544.353	472.266	3.195
Import Extra EU	572.484	497.704	74.78	1.172
Import Intra EU	707.277	552.038	155.239	2.382
Total Trade	8248.06	7052.112	1195.947	2.2
Trade Balance	3750.393	3348.381	402.011	1.077
Total Employment	25.052	23.176	1.876	2.224
Total Employment Cost	973.017	882.24	90.777	2.473
Capital Employment Ratio	78.053	76.461	1.591	0.393
Revenue from Sales	5616.874	5407.7	209.173	0.864
Value Added per Employee	54.442	53.086	1.356	1.325
Net Profits	120.395	85.103	35.291	2.444
ROE	7.968	7.431	0.537	0.707
Broadband Conn. (% Buildings)	0.252	0.285	-0.032	-3.054
Broadband Conn. >100 Mps (% Buildings)	0.194	0.223	-0.029	-3.377
Number firms	1834	2310	-476	

*Notes:* Summary statistics for treatment and comparison group when we use all early and late applicants. Column (3) reports the difference in the average between the two groups and Column (4) reports the t-statistic for the difference between the two groups obtained from an OLS regression on the variable on a dummy for having applied before the time cutoff. Variables for firm and trade outcomes are winsorized at 1%. All variables are reported in thousands of euros but Roe which is reported in percentage points.

Table A2: Effect of TEM Voucher Assignment on Trade Flows

Variables	(1) Export extra EU	(2) Export Intra EU	(3) Import Extra EU	(4) Import Intra EU	(5) Total trade	(6) Trade Balance
Before Cutoff X Post	79.558 (57.834)	-0.343 (85.976)	59.065 (31.061)	24.552 (46.047)	162.832 (146.274)	-4.401 (108.717)
Observations	16,390	16,390	16,390	16,390	16,390	16,390
R-squared	0.894	0.929	0.900	0.899	0.934	0.925
Mean Control	1242.93	1968.47	469.5	598.57	4279.47	2143.33
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

*Note:* Difference-in-differences regression for firm trade outcomes for years between 2013 and 2019. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm, which corresponds to the exhaustion of available funds. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. Total Trade is computed as the sum of imports and exports from countries within and outside the European Union, while Trade balance is the sum of all exports minus all imports. Variables are winsorized at 1%. All variables are reported in thousands of euros. Mean control is the average for firms that applied after the time threshold in the period after 2015. Standard errors are clustered at the firm level.



Table A3: Effect of TEM Voucher Assignment on Trade by Group of Countries

Panel (a): Export											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Income Group				Geographic Area						
Variables	High-Income	Middle-Income	Low-Income	Other	East Asia-Pacific	Europe-Central Asia	Latin America-Caribbean	Middle East-North Africa	North America	Sub-Saharan Africa	Other
Before Cutoff X Post	55.736 (35.092)	40.244 (38.764)	-0.157 (0.563)	0.850 (1.679)	-14.342 (16.077)	20.375 (24.228)	22.289 (10.791)	27.289 (14.952)	-1.333 (2.717)	1.625 (4.270)	16.978 (21.350)
Observations	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390
R-squared	0.856	0.867	0.632	0.725	0.822	0.839	0.778	0.837	0.734	0.737	0.808
Mean Control	624.01	571.11	2.72	13.59	247.08	318.24	88.78	203.29	24	26.45	254.51
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Panel (b): Import											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Income Group				Geographic Area						
Variables	High-Income	Middle-Income	Low-Income	Other	East Asia-Pacific	Europe-Central Asia	Latin America-Caribbean	Middle East-North Africa	North America	Sub-Saharan Africa	Other
Before Cutoff X Post	14.295 (8.493)	24.511 (30.669)		0.219 (0.690)	9.264 (17.001)	18.085 (7.578)	-0.132 (1.663)	2.346 (3.506)	-0.004 (0.039)	-0.474 (0.363)	0.167 (8.077)
Observations	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390	16,390
R-squared	0.793	0.909		0.775	0.906	0.764	0.806	0.800	0.502	0.753	0.833
Mean Control	75.16	366.49	0	3.67	242.23	63.96	8.24	12.55	.15	.84	76.4
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Difference-in-differences regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. Panel (a) reports results for exports while Panel (b) reports results for imports. Columns from (1) to (4) report the impact on trade with respect to country income group and Columns from (5) to (11) report results for trade with respect to the geographic area of the trading partner. Countries are allocated to categories based on the World Bank classification (2020). The result in Column (3) of Panel (b) is missing due to insufficient variation in the data. All variables are winsorized at 1%. Standard errors are clustered at the firm level.

Table A4: Effect of TEM Voucher Assignment on Other Trade Outcomes

Variables	Panel (a): Extensive Margin			
	(1) Export Extra EU	(2) Export Intra EU	(3) Import Extra EU	(4) Import Intra EU
Before Cutoff X Post	-0.020 (0.012)	-0.013 (0.012)	0.011 (0.012)	0.005 (0.015)
Observations	16,583	16,583	16,583	16,583
R-squared	0.783	0.804	0.700	0.646
Mean Control	0.63	0.61	0.44	0.43
Variables	Panel (b): Products			
	Prod. Extra EU EXP	Prod. Intra EU EXP	Prod. Extra EU Imp	Prod. Intra EU Imp
Before Cutoff X Post	0.155 (0.277)	-0.098 (0.210)	-0.069 (0.190)	0.100 (0.294)
Observations	16,390	16,390	16,390	16,390
R-squared	0.899	0.899	0.892	0.835
Mean Control	8.4	4.76	4.26	4.56
Variables	Panel (c): Countries			
	Count. Extra EU EXP	Count. Intra EU EXP	Count. Extra EU Imp	Count. Intra EU Imp
Before Cutoff X Post	-0.026 (0.130)	0.001 (0.112)	-0.039 (0.051)	0.050 (0.081)
Observations	16,390	16,390	16,390	16,390
R-squared	0.951	0.937	0.866	0.832
Mean Control	5.62	5.01	1.58	1.97
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

*Note:* Difference-in-differences regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. Columns from (1) to (4) in Panel (a) are linear probability models with the dependent value equal to one if the firm has a positive trade value in terms of exports (columns (1) and (2)) or imports (columns (3) and (4)) with countries outside the EU or inside the EU. Panel (b) looks at the number of products while Panel (c) looks at the number of countries involved in trade with the firm inside or outside the EU. All variables are winsorized at 1%. Standard errors are clustered at the firm level. All variables are measured in thousands of euros.

Table A5: Heterogeneous Effects by Provider

Variables	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Net Profits	(6) ROE
1st Provider X Post	-0.069 (0.133)	-0.054 (0.105)	0.027 (0.152)	0.054 (0.114)	0.435 (0.251)	0.424 (0.147)
2nd Provider X Post	-0.214 (0.276)	-0.255 (0.223)	-0.334 (0.330)	-0.082 (0.192)	-0.131 (0.382)	0.145 (0.252)
3rd Provider X Post	0.499 (0.094)	0.359 (0.114)	0.671 (0.114)	0.588 (0.114)	0.272 (0.418)	0.141 (0.228)
4th Provider X Post	0.023 (0.250)	-0.185 (0.209)	0.085 (0.318)	-0.054 (0.209)	1.249 (0.546)	0.800 (0.337)
5th Provider X Post	0.411 (0.233)	0.242 (0.146)	0.340 (0.237)	0.269 (0.174)	0.176 (0.386)	0.273 (0.291)
6th Provider X Post	0.155 (0.255)	0.089 (0.178)	0.103 (0.304)	0.017 (0.266)	-0.530 (0.685)	0.312 (0.341)
7th Provider X Post	0.140 (0.289)	0.109 (0.181)	0.345 (0.321)	0.491 (0.168)	0.561 (0.605)	0.206 (0.276)
Other Provider X Post	0.137 (0.079)	-0.065 (0.059)	0.148 (0.092)	0.193 (0.065)	0.349 (0.147)	0.336 (0.093)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
R-squared	0.733	0.726	0.697	0.552	0.516	0.458
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

*Notes:* Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes by the TEM provider between 2013 and 2020. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. The effect of the policy is decomposed by the provider of the temporary export manager. We group all providers with less than 30 contracts from firms applying for the policy. All variables are winsorized at 1%. Standard errors are clustered at the firm level.

Table A6: Heterogeneity by proxies of financial constraints

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Total Employment Cost (log)				Capital/Employment Ratio (log)				Revenues from Sales (log)			
Before Cutoff X Post	0.080 (0.121)	0.039 (0.083)	0.128 (0.107)	0.037 (0.116)	-0.072 (0.087)	-0.105 (0.071)	-0.038 (0.082)	-0.086 (0.088)	0.072 (0.143)	0.098 (0.103)	0.194 (0.127)	0.088 (0.138)
Before Cutoff X Post X High Tangibility	0.085 (0.151)				0.056 (0.114)				0.154 (0.177)			
Before Cutoff X Post X High Leverage		0.147 (0.152)				0.108 (0.114)				0.066 (0.178)		
Before Cutoff X Post X High Whited-Wu			-0.023 (0.150)				-0.028 (0.113)				-0.125 (0.176)	
Before Cutoff X Post X High Cash-Holdings				0.189 (0.149)				0.090 (0.112)				0.130 (0.175)
Observations	18,757	18,757	18,757	18,757	18,554	18,554	18,554	18,554	18,757	18,757	18,757	18,757
R-squared	0.733	0.734	0.733	0.736	0.727	0.726	0.726	0.728	0.698	0.698	0.697	0.700
P-value Sum	<b>.064</b>	.144	.32	<b>0.014</b>	0.824	0.969	0.389	0.955	<b>0.029</b>	0.259	0.574	<b>0.044</b>
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	VA per employee (log)				Net Profits (log)				Roe (log)			
Before Cutoff X Post	0.164 (0.101)	0.125 (0.080)	0.174 (0.091)	0.168 (0.100)	0.360 (0.197)	0.375 (0.200)	0.290 (0.195)	0.199 (0.214)	0.347 (0.130)	0.298 (0.110)	0.336 (0.120)	0.258 (0.135)
Before Cutoff X Post X High Tangibility	0.042 (0.126)				-0.022 (0.283)				-0.011 (0.179)			
Before Cutoff X Post X High Leverage		0.103 (0.126)				-0.035 (0.283)				0.101 (0.180)		
Before Cutoff X Post X High Whited-Wu			0.009 (0.125)				0.122 (0.284)				0.006 (0.180)	
Before Cutoff X Post X High Cash-Holdings				0.040 (0.124)				0.283 (0.282)				0.154 (0.177)
Observations	18,514	18,514	18,514	18,514	18,748	18,748	18,748	18,748	18,550	18,550	18,550	18,550
R-squared	0.553	0.552	0.552	0.555	0.515	0.516	0.515	0.516	0.458	0.459	0.458	0.459
P-value Sum	<b>.006</b>	<b>.02</b>	<b>.034</b>	<b>0.004</b>	<b>0.096</b>	<b>0.089</b>	<b>0.046</b>	<b>0.008</b>	<b>0.005</b>	<b>0.005</b>	<b>0.011</b>	<b>0.000</b>
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes by firm-level proxies of financial constraints. Treated firms are the firms that applied for the voucher before the 2002<sup>nd</sup> application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. High Tangibility is a dummy taking value one if the applying firm had in 2015 a ratio of tangible over total fixed assets above the median (defined over the set of treated and control firms). High Leverage is a dummy taking value one if the firm had in 2015 leverage above the median (defined over the set of treated and control firms). Leverage is equal to  $(1 - NetWorth/Assets)$ . High Whited-Wu Index is a dummy taking value one if the firm reported in 2015 an index of financial constraints above the median (defined over the set of treated and control firms). The Whited and Wu (2006) index for firm  $i$  active in industry  $j$  is defined as:

$$WW_{i,j} = -0.091CashFlow_i - 0.062 * 1(Dividends > 0)_i + 0.021 \frac{LongTermDebt}{Assets} - 0.044 * log(Assets) + 0.102SalesGrowth_j - 0.035SalesGrowth_i$$

High Cash-Holdings is a dummy taking value one if the firm reported in 2015 above-median cash-holdings over assets (defined over the set of treated and control firms). The model also includes the interaction between the relevant dummy per column and the post dummy, year and firm fixed effects. P-value sum is the p-value for a F-test assessing whether the sum of the main coefficient (Before CutoffXPost) and the appropriate triple interaction is different from zero. All variables are winsorized at 1%. Standard errors are clustered at the firm level.

Table A7: Firm-level Outcomes: Additional Impact of Second Wave

Variables	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Net Profits	(6) ROE
Before Cutoff X Post 2015	45.716 (24.942)	-1.270 (3.149)	323.872 (165.159)	3.956 (1.194)	43.370 (15.501)	2.458 (0.816)
Recipient Second Wave X Post 2017	86.761 (29.831)	3.695 (3.807)	412.296 (206.900)	3.060 (1.465)	-16.537 (23.516)	0.685 (1.014)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
R-squared	0.917	0.795	0.918	0.652	0.624	0.382
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. *Post 2015* is the period after 2015, the year of the voucher assignment, while *before cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm, which corresponds to the theoretical exhaustion of available funds. *Post 2017* is the period after 2017, the year of the voucher assignment for the second wave. *Recipient Second Wave* is a dummy indicating firms that received the voucher during the second wave of the policy. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. All variables are winsorized at 1%. Standard errors are clustered at the firm level. All variables are measured in thousands of euros but for Roe which is measured in percentage points.

Table A8: Comparison of Applicants and General Firm Population

Variable Name	(1) Average Applicants	(2) Average Non-Applicants	(3) Difference	(4) T-Stat
Total Employment	24.179	17.11	7.354	15.84
Cost per employee	35.472	34.875	0.592	2.45
Capital Employment Ratio	88.998	123.168	-34.25	-8.77
Revenues from Sales	5646.77	4421.524	1240.886	8.859
VA per Employee	54.376	56.633	-2.27	-3.99
Net Profits	110.64	93.3	17.416	2.15
Roe	7.692	10.88	-3.187	-8.239
Observations	4,096	266,977		

Note: Comparison of firms applying for the policy and the general firm population in Italy. All variables are winsorized at 1%. We exclude firms with no employees in 2015 and firms with less than 1,000 euros in Costs for personnel. The sample includes all firms with at least 500,000 euros in revenues in one of the three years before the policy and firms with less than 250 employees. T-stat obtained from a regression on the variable reported in the first column and a dummy for being an applicant. All variables are reported in thousands of euros but for Roe, which is reported in percentage points. Robust standard errors are used to compute the t-statistic.

Table A9: Comparison of Applicants and General Firm Population: Accounting for Sector and Employment

Variable	(1)	(2)	(3)	(4)
	Sector FE		Sector FE and Employment	
	Difference	T-Stat	Difference	T-Stat
Total Employment	4.407	9.439		
Cost per employee	-1.384	-5.719	-1.621	-6.81
Capital Employment Ratio	-0.312	-0.079	3.543	0.939
Revenues from Sales	253.001	1.779	-735.637	-6.949
VA per Employee	-4.364	-7.679	-4.403	-7.739
Net Profits	-28.111	-3.44	-52.015	-6.55
Roe	-3.227	-8.279	-3.204	-8.21

*Note:* Comparison of firms applying for the policy and the general firm population in Italy. Table reports coefficient of a regression having the variable in the first column as dependent variable and a dummy for applicants for the policy as independent variable. All variables are winsorized at 1%. Firms included in the analysis if they have more than 500,000 euros in revenues in one the three years preceding the policy implementation and less than 250 employees in 2015 as in Panel (b) of Table A8. Regression for Column (1) and Column (2) also includes sector fixed effects (two digits ATECO). Regression for Columns (3) and (4) includes sector fixed effects and the level of employment in 2015. All variables are reported in thousands of euros but for Roe, which is reported in percentage points. Robust standard errors are used to compute the t-statistic.

Table A10: Effect of TEM Voucher Assignment on Balance Sheet Outcomes: Robustness

Variables	(1) Employment Cost	(2) C/E Ratio	(3) Revenue from Sales	(4) VA per Employee	(5) Net Profits	(6) Roe
Panel (a): Baseline (winsored 1%)						
Before Cutoff X Post	46.105 (24.974)	-1.256 (3.149)	325.720 (165.366)	3.968 (1.194)	43.297 (15.488)	2.461 (0.817)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Panel (b): No Winsoring						
Before Cutoff X Post	111.889 (53.387)	-10.870 (9.342)	352.792 (214.154)	4.550 (1.497)	64.323 (31.881)	2.532 (0.885)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
Mean Control	921.79	73.66	5923.05	49.62	132.98	6.7
Panel (c): No Zeroes						
Before Cutoff X Post	38.840 (21.259)	0.728 (3.327)	262.622 (155.627)	3.068 (1.016)	44.386 (16.770)	2.193 (0.860)
Observations	17,965	17,359	17,965	17,184	17,953	17,166
Mean Control	991.71	74.14	6088.05	56	157.9	7.82
Panel (d): Logs						
Before Cutoff X Post	0.117 (0.076)	-0.050 (0.057)	0.139 (0.089)	0.179 (0.063)	0.345 (0.142)	0.340 (0.089)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
Panel (e): Instrumental Variable (IV) estimate						
Receiving Subsidy X Post	61.337 (33.140)	-1.664 (4.174)	433.326 (219.681)	5.242 (1.579)	57.621 (20.618)	3.248 (1.078)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
F-test	3042.39	2996.62	3042.39	3015.73	3032.91	3103.68
Panel (f): Balanced Panel						
Before Cutoff X Post	43.841 (22.666)	1.937 (3.337)	310.592 (160.977)	3.394 (1.177)	47.623 (17.841)	1.856 (0.848)
Observations	15,271	15,260	15,271	15,235	15,268	15,171
Mean Control	1067.64	73.25	6505.16	57.06	192.19	8.41
Panel (g): No Quota						
Before Cutoff X Post	52.220 (25.766)	0.101 (3.367)	333.618 (173.303)	3.804 (1.255)	42.356 (16.392)	2.380 (0.852)
Observations	17,527	17,339	17,527	17,300	17,519	17,333
Mean Control	879.27	65.29	5489.3	49.62	145.08	6.9
Panel (h): Detrended Variables						
Before Cutoff X Post	73.121 (24.973)	-9.506 (3.149)	723.460 (165.356)	4.088 (1.194)	27.498 (15.488)	3.470 (0.817)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Panel (i): Trends in Baseline Variables						
Before Cutoff X Post	48.422 (24.376)	1.347 (2.900)	380.513 (155.734)	3.613 (1.121)	39.648 (15.062)	1.501 (0.734)
Observations	17,619	17,538	17,619	17,529	17,616	17,527
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Panel (l): Cluster at Second of Application						
Before Cutoff X Post	46.105 (26.974)	-1.256 (2.586)	325.720 (153.235)	3.968 (1.275)	43.297 (14.128)	2.461 (0.790)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Note: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. "Post" is the period after 2015, the year of the voucher assignment, while "Before Cutoff" is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm, corresponding to the theoretical exhaustion of available funds. Firms included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. Panel (a) reports baseline results from A10 for the sake of comparison. Panel (b) reports the results for estimates of the same equation with the dependent variable not winsored. Panel (c) uses winsored variables at 1% but excludes zeroes when the firm is not active in the periods after the policy implementation. Panel (d) reports the effect of the variables in logs (we use an inverse hyperbolic sign transformation). Panel (e) displays results for an instrumental variable strategy where the fact that the firm used the voucher to hire a TEM is instrumented with the fact that it applied before the exhaustion time of funds. The F-statistic for the relevance of the instrument is reported at the bottom of the panel. Panel (f) reports results from a specification equivalent to Panel (a) but restricts the sample to firms active from 2013 to 2020. We define a firm as active if it has a positive employment cost during the year. Panel (g) excludes firms that were included in the program due to quotas, that is firms that participated in Roadshows or that had a legality rating at the time of the application. Panel (h) nets out a linear trend, different for treated and comparison firms, based on years before the intervention. Panel (i) includes interactions between year fixed effects and levels of our variables in 2015. Finally, Panel (l) replicates estimates from Panel (a) but standard errors are clustered at the second of application level. Standard errors are clustered at the firm level. All effects are reported in thousands of Euro but for Roe which is reported in percentage points.

Table A11: Firm-level Outcomes: Difference-in-Discontinuity Strategy

Variables	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Net Profits	(6) ROE
Before Cutoff X Post	58.094 (46.967)	4.937 (5.134)	529.465* (290.508)	3.731* (2.144)	51.504* (29.491)	3.166** (1.538)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
R-squared	0.917	0.795	0.917	0.652	0.624	0.382
Mean Control	914.65	65.45	5609.34	49.18	145.76	6.82
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

*Note:* Difference-in-discontinuity regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm, which corresponds to the exhaustion of available funds. The equation also includes also a linear polynomial in time allowing for different slopes on the two sides of the time cutoff and in the period before and after the policy. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. The C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. Mean control is the average for the comparison group in the periods after 2015. All variables are winsorized at 1%. Standard errors are clustered at the firm level. All effects are reported in thousands of Euro, but for Roe which is reported in percentage points. Level of significance: \*\*\* 0.01, \*\* 0.05, \* 0.1.



Table A12: Effect of TEM Voucher Assignment on Firm Labor Demand

	Overall	Contract Type		Occupation				Demographics				
	Tot. Employees (1)	Permanent (2)	Full Time (3)	Blue Collars (4)	White Collars (5)	Managers (6)	Apprentices (7)	Women (8)	Men (9)	Junior (10)	Mid-Level (11)	Senior (12)
Before Cutoff × Post	2.900 (1.774)	1.191 (0.580)	2.192 (1.182)	1.685 (1.213)	1.127 (0.581)	-0.027 (0.0718)	0.087 (0.113)	1.276 (0.919)	1.624 (0.898)	0.780 (0.547)	0.977 (0.699)	1.142 (0.614)
Observations	195,574	195,574	195,574	195,574	195,574	195,574	195,574	195,574	195,574	195,574	195,574	195,574
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression at the month-firm level for the effect of being assigned the TEM voucher on firm workforce size and composition. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002<sup>nd</sup> firm. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the exhaustion time of funds. Columns from (10) to (12) investigate the impact on workers by age groups: Young are workers below 29, Mid-Level are workers between 30 and 45; Senior are workers above 45 years of age. Effects are reported as number of employees. All variables are winsorized at 1%. Standard errors are clustered at the firm level.

## **A Questionnaire administered to TEM providers**

The interviews were administered between July and October 2021. The TEM providers were asked to participate in this interview to support an economics research conducted by economists active in academia and international organizations.

The questionnaire was intended as an outline for an open-ended interview. Two research assistants were trained to perform the interview. The interviews lasted between 20 and 45 minutes.

### **Section 1: What do the Temporary Export Manager do?**

- How long have you been offering Temporary Export Manager services?
- What kind of services were you providing in 2015 to your clients interested in an Export Manager?
- Which type of firms were you mainly serving in 2015 in terms of size, industry, and destination markets?
- Was consulting limited to providing contacts for new customers or suppliers, or did it extend to organizing and managing the production process?
- Did you also support firms in their digital transformation?
- Did the services only target exports or imports as well?
- For which type of firms you consider your support to be most effective?
- What is the average number of firms a TEM manages? What was the average number back in 2015?
- In addition to the agreed fixed fee, did you also benefit from a variable component linked to foreign turnover?
- How did your customer base evolve and what has been the role of vouchers in this regard?
- Did firms increase their employment as a result of the internationalization induced by your services?

## **Section 2: Experience with the vouchers**

- In how many waves of the Vouchers for Internationalization have you participated as a potential TEM provider?
  - If they stopped after the first one: why did you stop participating?
- Compared to the service provided by TEM and market price, do you feel that the value of the voucher in the first edition was: adequate, insufficient, or more than sufficient?
- Did you acquire new customers thanks to the voucher policy?
- Did the customers acquired with the voucher continue to use your services afterwards or did your relationship end with the first contract?