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Managerial Input and Firm Performance. Evidence from a Policy Experiment.

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

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14th April 2022

Abstract

We study the effects of a subsidy program designed to boost SMEs' export capabilities by means of a Temporary Export Manager (TEM), hired for at least 6 months to provide consulting on how to reach foreign markets. Firms applied online for the subsidy, and vouchers to hire TEMs were allocated on a first-come, first-served basis. We use a local difference-in-differences design to compare the performances of firms that nearly got the subsidy with those that barely did not. Eligible firms experienced a large increase in revenues, ROE, profits and value added per employee. This was accompanied by a significant growth in export in extra-EU markets four years after receiving the subsidy. The gains were larger for the least productive and smaller firms and effects were heterogeneous across TEM providers. TEMs were also effective in stimulating 'good' labor demand: besides intensifying exports, firms increased their workforce by about 13%, mainly driven by the increase of full time and permanent employees. Results of a survey conducted on TEM providers revealed that the voucher encouraged firms to use the consultancy to improve their export capabilities even after the initial subsidized service.

J.E.L. codes: L2, L38, 040, F14, H2, F2

Keywords: SMEs, export subsidy, labor demand, natural experiment, click-day.

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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 et al., 2007; Mion and Opromolla, 2014; Bender et al., 2018; Amador et al., 2018; Caliendo et al., 2020). Recent contributions have provided evidence on the causal impact of managerial practices on firm productivity (Bloom et al., 2013; Bruhn et al., 2018; Giorcelli, 2019), but much less is known about how firms can acquire such important assets. Indeed, policy-makers of all G20 countries have expressed concerns about the lack of managerial skills, particularly among Small and Medium Enterprises (SMEs), and there is an ongoing debate on which tools may boost such competencies.[1] Temporary consultancy may represent an effective solution for SMEs, but this option is costly and firms may encounter difficulties in accessing such services. Therefore, there is room for public interventions to support firms in acquiring these inputs and close the productivity gap (Schivardi and Schmitz, 2020). However, there is so far scant evidence on the effectiveness of this tool and on whether policies can successfully convey it.

In this paper, we address this gap in the literature by studying the impact of the *Vouchers for Internationalization*, a policy implemented in Italy in 2016 with the aim of improving SMEs export capacity, by providing consulting services through Temporary Export Managers (TEMs). We employ a wide set of administrative data on firms' balance sheets, trade and workforce composition, and we leverage on the allocation process of the subsidy to identify the effect of the policy. Firms applied online for the subsidy and vouchers were allocated on a first-come first-served basis until the policy budget—nearly 20 million euros—was exhausted. We exploit the exact timing of the applications' submission and compare over the years firms applying in a narrow window ($\pm$ 30 seconds) around the time of exhaustion of resources in a local difference-in-differences setting. This provides an as-good-as-random variation in TEMs' allocation, ruling out selection into treatment and allowing us to estimate the causal effect of the

[1] See, for instance, the OECD guidelines to address the Future of Work (Presidency, 2021).

program.

Our findings suggest that TEMs led to a strong increase in trade in terms of both exports and imports, even though these effects take time to build up and became large and significant after three years after the intervention. In particular, TEMs were effective in creating new market opportunities in extra-EU markets, i.e. markets with the highest entry barriers. The increase in trade seems to be attributable mostly to changes at the intensive margin with no changes in the number of trading partner countries, in the number of exported/imported products or in the firms' exporter status.

Another relevant finding of our study is that, although the policy's target was aimed at one specific dimension of the firms' activity, TEMs had an impact on firms' performance overall, in terms of revenues, productivity and profitability both in the short and long run. The effects appear to be heterogeneous, with suggestive evidence that smaller and less productive firms benefited the most. In addition, we provide suggestive evidence of large differences in the effectiveness of TEM providers. The endogeneity of the matching between firms and consulting firms prevents from a direct causal interpretation of these results. An important concern from a policy perspective is to understand whether public subsidies, beyond improving firms' performance, affect workers as well. For this reason, we also study the effect of the policy on the firms' labor demand. We find that one year after receiving the voucher there is an increase in the workforce of about one employee per firm; this trend steadily grows in magnitude and significance up to four employees over the following four years. 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 'good' contracts in terms of duration (permanent contracts) and working time (full-time). We validate our results with an array of robustness tests, which confirm our main estimates. Moreover, results of a survey conducted among TEM providers show that the initial subsidy encouraged firms to ask for consulting services that supported firm competitiveness also through improvements in their logistics, organization, and digitalization capabilities.

Our research relates to the literature on the role of managers and consulting services in improving firm performance. There is an increasing number of works studying the effect of management practices on productivity.[2] Bloom and Van Reenen (2011) establish a causal effect of management practices on productivity, which is in line with what Lazear and Shaw (2007) found from the perspective of personnel economics. Caliendo et al. (2020) study how productivity responds to firm reorganizations as measured by changes in the number of management layers, while Giorcelli (2019) examines the effects of a large training trip program organized for Italian managers during the Marshall Plan to learn about modern management practices in the US and finds that firms with trained managers showed higher survival rates and productivity. Bianchi and Giorcelli (2021) further show how training of managers led firms to adopt good managerial practices and had a positive impact on firm performance. More related to our setting, Bloom et al. (2021) assess how better management practices lead to a stronger performance on the export market. Our work most closely relates to Bloom et al. (2013) and Bruhn et al. (2018), who perform experiments in India and Mexico, respectively, to assess the effectiveness of consulting and mentoring for the growth of SMEs. Similar to our results, they find a strong impact of their treatment on firm size, productivity and profitability.

Our study contributes to this debate in several ways. First of all, we exploit a policy that involves firms that actively look for consulting services. This provides useful guidance to policy-makers in helping them impart these services to firms and informs them on what could be the effects for firms requiring these services in a market environment. Secondly, our results entail important implications for the design of internationalization policies. Traditional trade policies have focused on tariffs or export subsidies but financial frictions pose additional barriers to export by limiting the ability to defray fixed costs of entry in foreign markets (see, for instance, Demidova and Rodríguez-Clare (2009) or Ding (2021) for a review). These constraints are particularly significant for intangible assets, which are considerably uncertain and feature information asymmetries and sunkenness (Haskel and Westlake, 2017). Thirdly, unlike previous contributions,

[2] Bloom and Van Reenen (2011) and Hales (2019) provide nice overviews of it.

our study focuses on a high-income country, which is similar to many other developed economies. Although these 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 modern economies. A partial exception is Mion et al. (2017), who highlight the role of personal experience and managers' mobility in exporting to specific locations by exploiting managers mobility. In addition, our work focuses on a specific type of consultancy and its impact on trade, an aspect that has been neglected so far by the literature on consulting services. Our analysis not only demonstrates that such services can boost these activities, but also how their impact can spillover into many other firm dimensions.

Overall, we find that a relatively small in-kind incentive for SMEs can significantly stimulate export and firms' growth in general. This is relevant for the debate on how to design effective trade subsidies as it shows that moderate policy investments, with minimal interventions, can generate large returns. Specifically, TEMs' success highlights the importance of providing high-quality managerial inputs rather than generic financial incentives (Görg et al., 2008).

The remainder of the paper is as follows. Section 2 describes the policy setting of our study and the data used for the analysis. Section 3 describes the empirical strategy and provides evidence in favour of our identifying assumptions. Section 4 discusses the results of our analysis and provides robustness tests for them. Section 5 concludes by discussing the quantitative implications of our findings, arguing that the policy may have induced firms to invest more in managerial skills and export capabilities.

2 Institutional Setting and Data

2.1 The "Vouchers for internationalization" policy

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 both their growth and employment capacity.[3] The program targeted SMEs with revenues above 500,000 euros in at least one of the three years before the application or to SME start-ups. The goal of the government was to subsidize the acquisition of consulting services for trade from a list of companies compiled by the Ministry of Economic Development (MISE). Firms offering consulting services needed to have a consolidated experience in trade activities and knowledge of foreign languages. Additionally, the MISE assessed the presence of potential conflicts of interest and had the possibility to monitor a TEM's activity inside the firm to evaluate its truthfulness.

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 an assessment of the eligibility criteria carried out by the Ministry. Firms being awarded the voucher could use it to 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 is to assist a firm in studying targeted foreign target markets and designing strategies to start or intensify export activities. With the support from TEMs, 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 2016 and assigned a total budget of 19 million euros. This was raised to 38 million euros in the second wave in 2018. In order to have a sufficient time horizon and exclude potential selection due to strategic timing of firms' application, we restrict our analysis to the first edition of the voucher in which no information was available on the cutoff time to assign the voucher. Participating firms received a subsidy amounting to 10,000 euros, with a minimum additional contribution from the firm of 3,000 euros, for a total minimum value of the consultancy of 13,000 euros. Firms receiving this subsidy could not benefit from other government policies.

[3]The Vouchers were first introduced with Law n.133/2014 and later normative aspects were reported in the ministerial decree of the 15th of May 2015.

The mechanism of 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, specifically from 10:00 a.m. of September 22^{nd} up to October 2^{nd}, 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 two minutes from the start of the application period. In addition, there were quotas for firms participating in special promotional events ('roadshows') and for those that had obtained legality ratings, i.e. a certificate indicating the firms' compliance with existing regulations and best practices.[4] Third, the Ministry checked the applications to verify their contents. Firms not complying with the requirements of the policy were excluded, as well as those that renounced the subsidy ex post. These firms were replaced with new firms based on the time of their application. Lastly, eligible firms established contacts with TEMs by drawing from the list of consulting companies provided by the Ministry. After having arranged a formal consultancy contract, firms received the assigned grant within 60 days from their application.

During the first wave, 4,146 firms applied, of which 1,758 were initially selected. 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 of them did not complete the procedures to receive the subsidy. About 260 firms participated in roadshows and 110 provided a legality rating. Among them, a total of 226 were selected for the policy. In our analysis, we check for the robustness of our results excluding firms that received the vouchers because of the quotas.

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 of the service.[5] The

[4]These were issued by the Authority for Competition and Market after inspections of the firms.

[5]The distribution of the share of the service's price covered by the Voucher is reported in Appendix in the Figure A2.

duration of the contracts varied 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[6] reveals that firms requested a variety of services from TEMs. Most of them were interested in attracting additional clients and contracts (46%) or in conducting market research activities (34%). The remaining firms requested other kinds of 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

This paper benefits from a unique employer-employee administrative dataset, built by combining several data sources. Our data cover the years between 2013 and 2019, a period which includes the year of the first wave of the policy and the years close to it. 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 CERVED data; granular trade data at product-country-firm level provided by the Italian Customs; information on firms' workforce from the National Social Security Institute (INPS) data.[7]

We obtained detailed data on the administrative procedures related to the policy from the MISE. The data include the list of firms applying for the subsidy, together with their administrative identifiers and the time of application, which is crucial for our identification strategy. The data also report some firm characteristics, such as previous experience in trade, participation in roadshows, the main sector of activity, and, if available, information on the established contract, such as the type of received service, the amount invested, and the identifier of the consultancy provider.[8] We used a unique firm administrative identifier to match this information with other data sources.

[6]The main object of the contract was provided to the MISE at the time of the application.

[7]This was possible thanks to the VisitINPS initiative by the Italian Social Security.

[8]Road shows are events supported by the Ministry to illustrate policies aimed at helping firms enter or expand in international markets.

First, we match our set of firms with balance sheet data from the CERVED archives. 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 firm indicators and characteristics.

Then, we match our set of firms with granular data at the country-product level thanks to the information provided by the Italian Customs and Monopolies Agency. Custom data represent an ideal source of information for analyzing firms' trade performance because it allows observing each firm's transactions both within and outside the European Union. Data on trade transactions are collected quarterly and are measured in both total value in euros and quantities in kilograms. Moreover, the data report, for each transaction, information on the type of good traded based on the Combined Nomenclature (CN8) classification and on the country of origin or destination. We collapse our dataset at the firm-year level and build a panel for applicant firms. We start by looking at an aggregate trade dimension (total trade within and outside the European Union), and then we move to a more detailed analysis of countries and products involved in our firms' international transactions.

Finally, we merge firms participating in the application process with their workforce characteristics thanks to Italian Social Security data. We mostly rely on 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 their wages, part-time\full-time status, permanent or temporary contract, and broad occupation classification. We included a few demographic characteristics such as age and gender. We collapse our worker level data to the firm-month level.

Overall, we are able to match most of the firms (4,145) that applied for the policy with their related information.

3 Empirical Strategy

The main empirical challenge in identifying the effect of TEMs is how to deal with selection: firms obtaining consultancy from TEMs might differ from firms that do not. Failing to control for these differences would bias the estimates. To overcome this issue, we exploit the quasi-random assignment of the vouchers applying for the Voucher to identify suitable treatment and control groups.

The mechanism for the assignment of the subsidy offers 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 slight delay in submitting the application. 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.[9] Consequently, we are able to identify the causal impact of the policy by comparing firms who nearly made the cutoff with firms that missed the cutoff by a few seconds, following Pinotti (2017). In practice, we consider firms around the cutoff time and run a local differences-in-differences model. We focus on a local diff-in-diff rather than a difference-in-discontinuity (Grembi et al., 2016) because of the limited number of observations at the cutoff. This makes estimates of the discontinuity in the dependent variable imprecise at the cutoff. Reassuringly, we find that point estimates are similar to those obtained from our diff-in-diff while standard errors are larger when we estimate our treatment effect in a diff-in-disc model in Section 4.2.5.

Our baseline model is the following:

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

[9] Notice that in the first wave of the policy, firms had no information about the exhaustion time of the policy budget.

where Y_{jt} is the outcome of interest, $\widetilde{t}_j$ represents the time of application as a difference with respect to the time of exhaustion of the available funds, $Post_t$ is a dummy variable equal to 1 after 2015, θ_j is a firm fixed effect, η_t is a time fixed effect, and ε_{jt} is a random error term. Our parameter of interest is β_3, which identifies the treatment effect. This is obtained by comparing the treatment and the control group in the periods right before and after the assignment of the voucher. 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 $\mathbb{1}(\widetilde{t}_j < 0)$ identifies an Intention-to-Treatment effect (ITT).[10] To limit the influence of outliers, we winsorize our dependent variables at 1% in the main analysis.[11] We cluster standard error at firm level.[12]

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:

$$Y_{jt} = \alpha + \beta_1 \mathbb{1}(\widetilde{t}_j < 0) + \sum_{d \in (-2,4)/(-1)} \beta_2 \mathbb{1}(Year - 2016 = d) + \sum_{d \in (-2,4)/(-1)} \beta_3 \mathbb{1}(\widetilde{t}_j < 0) \mathbb{1}(Year - 2016 = d) + \theta_j + \eta_t + \varepsilon_{jt}. \quad (2)$$

On the one hand, interactions between the treatment dummy and years prior to the experiment allows us to investigate the presence of any pre-existing differential trend before the experiment between treated and control firms. On the other hand, 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.

[10] Since compliance with the time rule is very high, this will be very similar to the ATT. We further investigate this issue by estimating an IV model where we instrument the actual take-up by the policy with a dummy for having applied before the cutoff.

[11] Generally, results become more precise with this adjustment while point estimates are not substantially affected.

[12] Results are consistent also clustering at the second of the application arrival.

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 unlikely to be affected by the policy in the same period. The model also includes firm (θ_j) and year (η_t) fixed effects.

The first step in our empirical analysis is to identify the cutoff time. We plot the share of firms that received the subsidy against the time of submission of their application. We group firms bins of one second and plot the share of successful applications by time of submission in Figure 1. The distribution of the acceptance rate clearly 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$^{\text{nd}}$ firms.[13]

Then, we assess whether firms were able to sort around the cutoff and examine the distribution of applications around the time funds ran out, as reported in Figure 2. Panel (a) plots the full distribution while Panel (b) focuses on the neighborhood of the cutoff which is 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. Our results are consistent if larger (40 seconds) or smaller (20 seconds) intervals are considered. Resources were exhausted within one minute from the opening of the application process, and the bulk of firms filed their request approximately in 30 seconds after the opening. The distribution does not show any 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' application and time their submission accordingly.

In order to correctly interpret the results, it is crucial to compare firms applying for the

[13] We test for the presence of other discontinuities by running Regression discontinuity regressions with a dummy for receiving the subsidy as the dependent variable and with the time of application as the running variable. We use a linear local polynomial, a triangular kernel, and bandwidth selected through the minimum squared error criterion. We perform this exercise using the `rdrobust` command developed by Calonico et al. (2017). We then use 16 second intervals and run a set of regressions at fake discontinuity points and at our cutoff. Finally, we plot the discontinuity coefficients together with the *z-statistic* for their significance in Figure A1 in the Appendix. The discontinuity at our cutoff (0) is clearly the largest and the only one which is significant at the 5% level. This provides comforting evidence concerning our choice. In a few cases after the time threshold, the equation could not be estimated since there was not enough variation in the dependent variables in the interval, i.e. there was not a sufficient number of firms receiving the subsidy.

policy to the general population of firms. This is because the empirical analysis involves a relatively small number of firms, which explicitly show their need for consulting services. We extract data from the universe of limited liability firms in Italy and compare our firms to the potential pool of applicants in 2015 (year of application for the policy), i.e. firms with revenues above 500,000 Euro in one of the three years before the policy. We report the related figures in Table A1 in the Appendix.[14] Panel (a) reports the baseline comparison between the firms in our sample and other limited liability firms. Applying firms are generally larger, have higher revenues, pay their employees more, and have a higher value added per employee but show a lower profitability according to the return on equity (ROE). Since applying firms are SMEs, we further restrict the sample of potential applicants to firms with less than 250 employees (size threshold for SMEs according to the Italian regulation) and replicate our analysis. Results, reported in Panel (b), highlight the previously mentioned differences even more and suggests that firms applying for the policy were positively selected. However, differences in some of these dimensions could be related to the larger size of the applying firms (employing 13 employees, which in some cases is even double in comparison with non-applying firms) or, possibly, to sectorial differences. To further delve into this issue, we net out these two components by using sector fixed effects and by controlling for employment in Table A2 in the Appendix. After having considered these dimensions, it appears that applying firms in general perform slightly worse than the overall population in terms of profitability and revenues, while their productivity levels are similar. The perception of this gap could lead firms to apply for public subsidies for additional support to their activities by means of consultancy. Therefore, it would seem that our results concern a group of firms that are larger than other firms but, at the same time, under perform in several dimensions once the size of their workforce is taken into account. Hence, they seem to have margin for improvement.

Finally, we check whether firms in our treatment and control groups are comparable in terms of observable characteristics. We consider several firm dimensions in the three

[14] We only consider firms with positive employment and with more than 1,000 Euro in employment costs.

years before the implementation of the policy (2013–2015) and in the year immediately before the application (2015) and compare firms on the two sides of the cutoff time. We report summary statistics for the treated and the control groups in Table 1.[15] Our results are encouraging: only in a few cases the differences between the two groups are statistically significant, and firms are extremely similar in many important dimensions, such as value added per employee and gross profits. In the year of the application, treated firms appear to be larger and more capital intensive. In addition, we also report normalized 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 to the former.

Differences in levels, anyway, would not be a cause of concern *per se* for our identification, since our difference-in-differences strategy exploits variation both over time and across firms differently exposed to the policy. Differentiating over time and within firms nets out any difference in levels between the two groups of firms. 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. Although we cannot explicitly test this, we can provide supporting evidence by considering the trends in the dependent variables before the introduction of the policy. We discuss this point in the following sections.

[15] 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.

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 of them already exported before applying to 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 a greater integration in the Global Value Chain (GVC). In addition, the knowledge provided by external consultants might be useful for exploring more distant markets with different regulations.

To investigate these margins, we first focus on aggregate measures of export at the firm and year level. We compute total exports and imports and we 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 to 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 firm by estimating Equation 2. By doing so, we analyze the difference between the two groups of firms in relation to the base year. Results are reported in Figure 3. We consider exports to countries outside the EU in Panel (a), and to countries inside the EU in Panel (b). Exports are relatively stable in the period before the policy, with minor deviations from the baseline period in both 2013 and 2014 (periods -2 and -1). Differences become slightly larger in the first period after the policy implementation, while a large difference,

statistically different from zero at the 5% level, emerges after three years. Treated firms display 200,000 euros more in export to countries outside the EU compared to the baseline year. 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 for markets inside the EU. 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 mostly performed in 2016 and it 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 the changes in imports, which follow the same pattern of exports. Imports from countries outside the EU increase after two years from the intervention, while there are no changes for imports from within the EU.

Finally, we consider two more aggregate outcomes: the total value of trade, i.e. the sum of export and import, and the net trade balance of the firm, i.e. the sum of total exports minus the sum of total imports. A positive effect would imply that the extra trade contributes to increase firm's profits. We report results for these two variables in Panel (e) and in Panel (f). Results are in line with previous evidence with total trade growing over time and an increasingly positive effect on the trade balance. However, these effects are less precisely estimated.

Results from a classical difference-in-differences model, are reported in Table 3. They confirm previous findings, but most of the coefficients are too imprecisely estimated. 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). The effects appear large compared to the small subsidy (10,000 euros) the firms received: by the fourth year after the application, firms that were awarded the voucher exported 200,000 euros more outside the EU in comparison to 2015 and imported an additional 100,000 euros.

Then, we decompose our trade outcome to investigate whether trade towards and from particular locations experienced stronger growth than others. We group countries based on their income group according to the World Bank 2020 classification and report results in Table A3 in the Appendix. Results show stronger export growth towards high and middle income countries (although not significant at conventional levels) and a significant increase in exports towards 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, the largest gains are from Europe and Central Asia.[16]

Additionally, we explore several other outcomes to assess how these additional trade flows occur. We report our results in Table A4 in the Appendix. First, we notice that there seems to be no effect at the extensive margin in terms of exports and imports. Indeed, coefficients for linear probability models with a dummy equal to one in the presence of a positive trade flow as dependent variable show a negligible magnitude and are not statistically significant. These results are reported in Columns (1) to (4). Then, we verify whether these additional trade flows also lead to an increase in the number of products or in the number of trading partner countries. Even in this case, we do not observe any changes in these dimensions after the implementation of the policy. Hence, it seems 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 would be a reasonable outcome, since the monetary value of the consultancy is limited in most cases (more than 80% of the contracts are below 14,000 euros in value). Figure A4, Figure A6, and Figure A5 in the Appendix report corresponding dynamic estimates.

[16]Ideally, the sum of all coefficients by trade flows should sum up to the aggregate effect. However, due to winsoring by outcome, this does not materialize and generates some discrepancies.

4.2 Firm outcomes

4.2.1 Main Findings

We now look at the impact of the subsidy on firms' balance sheet. We consider several firm dimensions by looking at costs, revenues, and profitability. We start with our simpler model (Equation 1) and then move on to its dynamic counterpart (Equation 2).

Table 4 reports results for our main variables of interest. The effects are generally positive and statistically significant: firms eligible to receive the subsidy spent more for their employees (in Section 4.3 we show that this is matched with an increase in the number of employees), have higher revenues, experience growth in value added per employee and increase their profitability (profits and return on equity, ROE). We do not detect changes in their capital/labor ratio. The effects are close to 7% of the average for the dependent variable for the control group in the period after the intervention. The Return on Equity is the only exception, with a 25.6% increase compared to the baseline. However, the increase in productivity (proxied by value added per employee) is not a strict target of the policy and might be coming from two different dynamics: 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 managers themselves might be providing counseling beyond the scope of their role, thus leading to a better performance of the firm overall. Since we do not have data on the activity of the consultants within the firms, it is not possible to empirically disentangle the two mechanisms.

Next, we 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 control firm, which supports our identification assumption. The positive effects of the policy build up over time and become more noticeable in the last period of the analysis (2019). In the year of the treatment and over the following two years, the treated firms enhance their performance modestly, while in the last period the improvements are substantial. For example, in 2019 profits increased almost twice the amount they had in the first

and the second year after the policy. Consistently with previous results, it would seem that the policy takes time to fully reveal its positive effects. Nevertheless, some earlier, albeit smaller, effects are detectable also in the short term.

Overall, these results show that the policy had a positive impact on firms' exports and general performance, with gains both in terms of size and profitability, which gradually increased over time and became particularly sizable after three years.

4.2.2 Heterogeneity

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 different types of firms are affected by these services. This also allows us to highlight 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 of the firms: geographic location, size, productivity (again measured as value added per worker), and previous exporter status, i.e. whether the firm was already exporting within or outside the European Union. All these 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 5. The equation is estimated using a log transformation to rescale the changes in the dependent variable across groups of firms.[17] 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,[18] 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[19], and the least productive firms. i.e. those in the bottom decile of the distribution, accrue larger gains. Triple interaction

[17] Again we resort to the inverse hyperbolic sine to accommodate for zeros in our estimation.

[18] This group consists of regions in the South of the country (Campania, Basilicata, Molise Abruzzo, Puglia and Calabria), as well as the Islands (Sicily and Sardinia).

[19] With 15 employees, which was also the threshold for differences in the stringency of the Employment Protection Legislation (EPL) regulation, with larger firms being subject to stricter rules.

terms are generally large, but for the most part imprecisely estimated. However, it should be noted that the sum of the two coefficients is generally different 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 ten 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 firms that were already exporting, although, also in this case, estimates are not precisely estimated.

These results provide additional important insight into the impact of the policy. Such a moderate intervention appears to have, in many cases, only small effects while the benefits seem to be extremely sizable for firms characterized by high levels of inefficiency. Therefore, it is possible that TEMs acted as a catalyst and impacted firms in ways beyond the mere support to export activities. We analyze this possibility in the next Section.

4.2.3 The heterogeneous effect of TEMs

While some TEMs may only provide firms with contacts of potential customers and marketing consultancy, others may also advise them firms to change their investments and structure, which, in turn, may generate larger benefits. This Section summarizes the main outcomes of several analyses on the role of the services supplied by TEMs. The obtained results should be interpreted cautiously, since they may reflect characteristics of the provider and of the beneficiary firm. This is because the matching between the specific firm and the provider is endogenous.

We start by studying the heterogeneity of the effects by TEM provider. Since many of them only deal with a limited number of firms, we restrict our attention to TEM providers that are involved in a sufficient number of contracts. We set this threshold to a minimum of 30 and collect all the others in a residual category. We report results in Table 6. The analysis hints at a strong heterogeneity, with one particular provider being associated with extremely large effects. This suggests that specific practices could generate much larger benefits for firms that acquire these services. This also implies

that the positive effects we find do not derive from the simple exposure to the policy, but rather are related to specific kinds of inputs and behaviors of the provider and the TEMs.

4.2.4 Qualitative evidence

To dig deeper into this possibility, we administered open-ended interviews between July and October 2021 to the consultancy firms that were accredited as TEM suppliers by the Ministry in 2015. Out of the 163 accredited firms, 43 participated in the interview (a response rate of 26.4%). These 43 consultancy firms provided their services to 682 firms that had been awarded the 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, in particular on the type of provided services, their usual type of customer, their evaluation of the voucher granted by the MISE, and whether the firm which used the voucher continued to use their services after the initial six-month 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 beneficiary 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 the estimated 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, subsidized through the voucher, was followed by subsequent consultancies paid for by the firm. This result is consistent with the possibility that the initial voucher encouraged firms to start paying for consulting services. Thus, the effects discussed in Section 4 may be the result of a longer consultancy period than the one initially supported by the Government.

We asked the TEM suppliers what type of services had been provided to the beneficiary firms. The open-ended answers were then categorized by the interviewer in four non-mutually exclusive groups:[20] 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 9 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 of support to the streamlining of production and the management of inputs and outputs.

Finally, we asked the TEM providers whether their support also concerned firms' digitalization. 15 out of the 35 firms that answered this question (43%) affirmed that they helped firms go digital.

To study whether the types of provided assistance are correlated with the positive effect of TEMs on firm performance, we include triple interactions with the various services provided in our models. As in Section 4.2.2, we re-scale the dependent variable using an inverse-hyperbolic sine transformation to allow comparability between results. Table 7 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 support for commercialization, and higher for those related to TEMs that provide assistance for digitalization. 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, these results may be affected by endogenous matching between providers and firms: further analysis would be needed to assess the causal interpretation of these parameters.[21]

[20] This grouping was later checked for consistency by the authors.

[21] 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

4.2.5 Robustness

We perform several robustness checks to validate our results and report them in Table A5. After presenting the main estimates in Panel (a) for comparison, we explore if results hold by using non-winsorized data (Panel b) or a logarithmic rather than a linear specification (Panel c).[22] Then, we investigate the magnitude of the ATT by exploiting an instrumental variable (IV) strategy, in which we instrument the actual payment of the subsidy with the timing of the application (Panel d). In addition, we restrict the sample to a balanced panel in which we require firms to have no missing data for the variable of interest throughout the observation period (Panel e) and to firms that are not part of any quota category for the subsidy assignment (legality rating or participation in roadshows, Panel f). Finally, we assess the robustness of our inference by clustering at the second of application rather than at the firm level (Panel g). Estimates are largely in line with our main specification, with some small variations. Results with the non-winsorized data and with the log transformation are consistent with the main estimates with some larger coefficients and much lower precision in the case of the non-winsorized data. Coefficients for the IV strategy are larger but reasonably close to the main ones, as it could be expected given the high compliance rate (about 80% of firms applying before the cutoff receive the subsidy). Restricting the sample to firms with non-missing observations for the variable of interest throughout the period of analysis leads to slightly smaller effects and, in some cases (employment cost, value added per employee, and ROE) to a loss of statistical significance at conventional levels. To provide more direct evidence on the consistency of these results with our main equation, we also show the time pattern of the effect in this restricted sample in Appendix Figure A7. The exclusion of firms that obtained the subsidy through the quota mechanism strengthens the results, while modifying the clustering levels only induces marginal inference changes. Furthermore, we assess the role played by our time window around the time of exhaustion of the funds allocated to the policy. More specifically, we use a 20 second-radius and a 40

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 margin to further research.

[22] We implement a inverse hyperbolic sine transformation.

second-radius radius around the cutoff. All of these results, reported in Appendix Figure A8 and A9, are remarkably similar to the main results.

Finally, the average effect across firms in a ±30 second-radius may be affected by possible confounding factors that are correlated with the time of application. It might be argued that firms applying before are still dissimilar from those appying 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. This compares outcome variables *exactly* at the cutoff in the years before and after the treatment took place. In doing so, we consider the following model:

$$Y_{jt} = \alpha + \beta_1 \mathbb{1}(\widetilde{t}_f < 0) + \beta_2 Post_t + \beta_3 \mathbb{1}(\widetilde{t}_f < 0) \times Post_t + \theta_j + \eta_t$$
$$\Big(\mathbb{1}(\widetilde{t}_f < 0) + Post_t + \mathbb{1}(\widetilde{t}_f < 0) \times Post_t\Big) f(\widetilde{t}) + \epsilon_{jt}$$

where $f(\widetilde{t})$ is a polynomial of the distance in milliseconds from the cutoff.

Also in this case, results, reported in Table A6, are similar to the main ones, but are less precise, as the coefficient β_3 is now estimated exploiting a much smaller part of variation in the data. Yet, all point estimates remain close to our baseline results, further confirming the reliability of our difference-in-differences estimates.

In addition, 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 absent the policy. To test this hypothesis, we asses whether the timing of the application is actually related to the impact of the policy. We split the treatment group based on firms' time of applications in ten-second bins and then estimate our difference-in-differences model. We report coefficients in Appendix Figure A10, together with p-values for the equality of the coefficients across treatment groups. Effects appear to be fairly similar across bins, even though they tend to be larger for the bin closer to the cutoff. In no

case the p-value for the F-test hints at the possibility that the effect across groups is significantly different (in a statistical sense). Based on this result, it appears unlikely that the timing of the applications is related to unobservable factors of the firms that may have increase their inherent potential for growth in the period after the policy implementation.

4.3 Employment and workforce composition

Regarding internationalization and overall performance, firms appear to benefit from the presence of the TEM both in the short term and, more distinctly, in the long term. These benefits are reflected in several outcomes in terms of size, efficiency, profitability and trade. To conclude, we assess to what extent these gains translated into higher labor demand.

For this purpose, we exploit the more granular INPS data available on a monthly basis to shed further light on the timing of the effects. Indeed, this data in combination with the trade results obtained by using custom data and the quasi-experimental setting of the policy, allow us to consider the timing of the effect on firm size and trade and to verify which effect emerges first. This would rule out possible endogeneity due to the simultaneity of export-labor demand dynamics. Indeed, if we observe a higher labor demand before the increase in export, we might argue that the change in the labor force is a prerequisite for higher production and efficiency levels rather than being a direct consequence of higher demand.

As in previous sections, we begin from our baseline local difference-in-differences model. Table 8 presents these results for the overall number of workers and different subgroups. Specifically, first we estimate the effect on the total number of employees (Column 1); then we explore whether the TEM subsidy produces differentiated effects according to the worker's type of contract (Columns 2 and 3), and, in particular, we look at the number of workers on permanent contracts and in full time jobs. We also look at broad occupation groups (Columns 4 to 7) and, finally, at demographics in terms of gender

and age (Columns 8 to 12). In fact, it is important to understand which kind of jobs are created to assess the effects on the workforce structure. Additionally, this allows us to determine which kind of activities increase within the firms (production with blue-collar workers and other activities with white-collars workers) and what is the quality of employment for workers.

We report the results in Table 8. Out of 2.9 additional employees, we find that the TEM subsidy produced a strong impact on stable jobs (Column 2) with an additional 1.19 workers with a permanent contract (about 40% of the total effect). We also find that the largest growth is registered for full-time jobs, as reported in Column (3). Treated firms display, on average, 2.19 additional workers with full-time contracts after the assignment of the voucher compared to the years before, opposed to firms in the control group (about 75% of the total effects). In terms of the structure of jobs within the firm (Columns 4 to 7), the largest effect applies to blue-collar workers (about 60% of the effect), which testifies the impact of the policy on production, though this parameter is not precisely estimated. The effect on white-collar jobs (Column 5) is smaller (1.127 workers) but significant at a 10% level. Other kinds of jobs, such as managers and apprentices (Column 6 and Column 7), register small variations. Finally, in terms of demographics (Columns 8 to 12), we find larger effects for men (56% of the effect) and for middle-aged and older workers, with an overall 73% of the total effect (about 2.119 workers) coming from employees older than 30.

The dynamic of this effect confirms that an expansion of firms' activity and workforce is a prerequisite and not a consequence of the additional internationalization of firms receiving the voucher. Thus, we run a more detailed specification of model 2, in which the time index t now represents months. Following the same structure as Table 8, we graphically present these results in Figures 5–8. Panel (a) in Figure 5 shows the effect for total employees, where we observe a significant expansion in the workforce starting only eight months after the TEM assignment. Moreover, the impact on total employment increases both in the following months, amounting to nearly 3 additional employees per firm on average. This result is economically meaningful and supports the hypothesis

that the TEM produced long-lasting effects on firms' labor demand. Panel (b) and Panel (c) respectively show the impact on permanent and full-time employees. In both categories, we find a significant growth, with a distinct and large effect for both types of contracts, which corresponds to a large portion of the overall effect.

The decomposition of the effects by workers' qualification is shown in Figure 6. In this case, the evidence is less compelling compared to the results for the total workforce. The estimates are less precise, probably because of a loss in statistical power when the main sample is split into these categories. Also, they show a small difference in the employment before the policy, which is, however, never statistically significant at the 5% level. Nevertheless, we observe a steady increase of white-collar employees, which grows on average to a significant magnitude of 1-worker two years after the TEM assignment. We also observe a steady but more modest growth in the number of apprentices, reaching a significant value of 0.25 additional workers after four years. The trend for blue-collar employees increases as well, but the coefficients are never statistically significant at the 5% level. We do not find any impact on the number of managers.

We now move on to presenting the heterogeneous effects by gender. Since women have fewer opportunities in the labor market and less stable career perspectives (for example, due to absence from the working environment related to childbirth, as shown by Kleven et al. 2019), it is worthwhile to disentangle the labor demand effects separately for male and female employees. Figure 7 presents these results. By comparing the effects for females (Panel a) and males (Panel b), we observe that female employment grows less: after 24 months the increase in the workforce is balanced on an average of 1.5 additional employees, but in the long run the effect becomes stronger and more significant only for men. The magnitude for this group reaches 2 employees per firm at the end of our observation period. Therefore, the new jobs seem to be slightly biased in favor of men.

Finally, in Table 8 we explore the effects on the age distribution of the firms' workforce, where we consider age as a good proxy for work experience. The new work opportunities generated by the policy seem to be concentrated among older employees (above 45

years of age, reported in Panel c), while a smaller but statistically significant increase is observed for younger workers (younger than 30 years old, reported in Panel a). No significant effects are instead detected for middle aged workers (between 30 and 45, reported in Panel b). By the end of our period of analysis (end of 2019) firms had hired on average 1.8 senior employees and less than one junior employee.

5 Conclusions: Nudging Investments in Management Skills

This paper investigates the impact of a policy providing subsidies for consulting services to improve firm internationalization by means of Temporary Export Managers. We exploit the timing of application and the allocation of the subsidy based on a first-come, first-served basis and we compare firms that marginally received the subsidies with firms that marginally did not, due to small differences in the time of application.

We find that the policy was effective in boosting firm internationalization and in improving firm performance in general. Importantly, we show that exports and imports take time (up to four years) to increase significantly. Before that, firms undertake changes that lead to a higher level of production, an expansion of their workforce, and improvements in their profitability. More precisely, we find that eligible firms increase exports by an additional 200,000 euros towards countries outside the European Union and imports by 100,000 euros by the fourth year after the assignment of the subsidy compared to applicants that did not receive the voucher. In addition, they experience a growth in revenues, value added per employee and profits by, respectively, 1 million euros, 5,000 euros per employee and 400,000 euros. Finally, we observe an increase in firm size of about 4 employees, corresponding to about 17% of the number of employees in the baseline year.

Treatment effects and cost effectiveness vary across characteristics of the applicant firms. Vouchers to less productive and smaller firms generate larger treatment effects. The

timing of the effects on firms' outcomes, workforce and trade is heterogeneous: it would seem that firms first improve their performance and grow in size before increasing their exports and imports. This dynamic suggests that a larger workforce is an essential asset for expanding SMEs' trade performance.

The large effects we find may seem surprising at first, given that the amount of the subsidy was only 10,000 euros to each firm for a 6-months consultancy (worth 13,000 euros in total). However, we find that gains in trade are mostly at the intensive margin rather than at the extensive margin (exporter status, countries or products) and that the largest gains in percentage terms are experienced by the least productive firms, which might have had ample margins of improvements even with small interventions. This partly rationalizes the large effect of the policy. In addition, this initial consultancy might have marked the beginning of a longer relationship with the consultants. Indeed, results from a survey we administered to TEM providers suggest that most of the firms that benefited from the initial consultancy continued investing in management skills and firm organization. Thus, the policy has mainly provided a *nudge* to undertake additional investments in management capabilities.

Several factors may explain the need for such a nudge, despite the large positive impact on firm performance that we estimated. Credit market frictions for SMEs may play a role, together with entrepreneurs' aversion to risk or ambiguity in relation to the potential returns of hiring a consultant. This aversion could be perpetuated by lack of information on the returns to consulting advice (which consulting firms have difficulty credibly signaling), especially in very small firms and family businesses.

Our work also highlights several topics for future research. Indeed, there is still much to learn about how managerial inputs and expertise gained through consultancy affect firm performance. We find suggestive evidence that TEMs 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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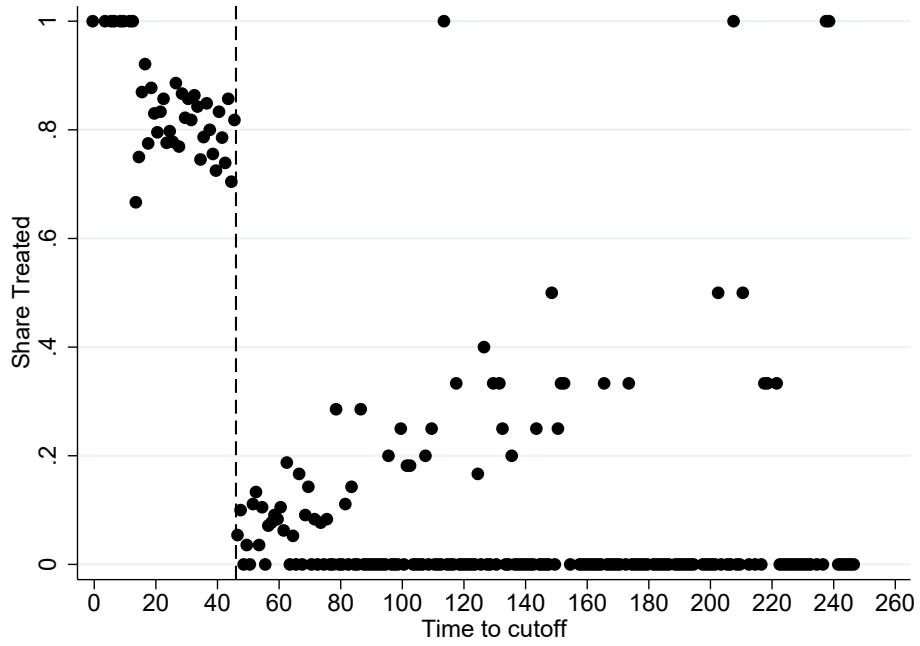
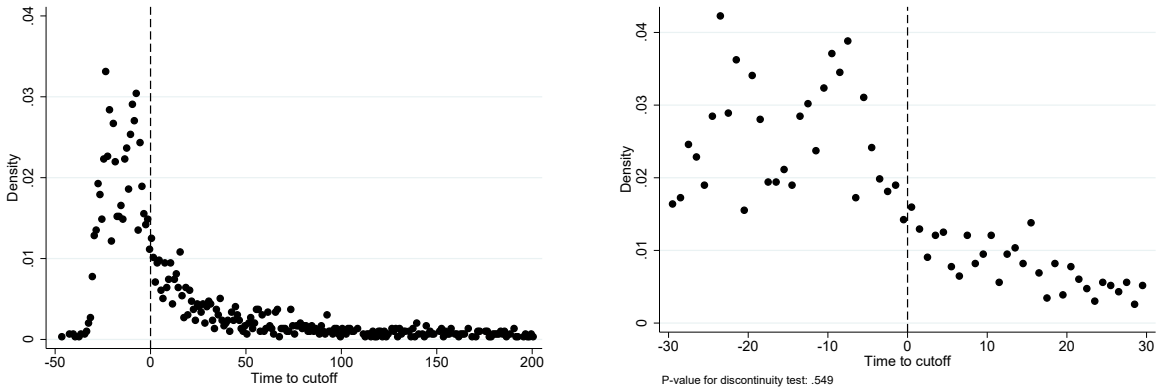
Figures

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

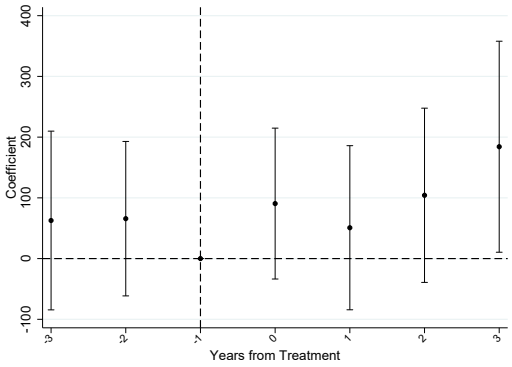


(a) Full distribution

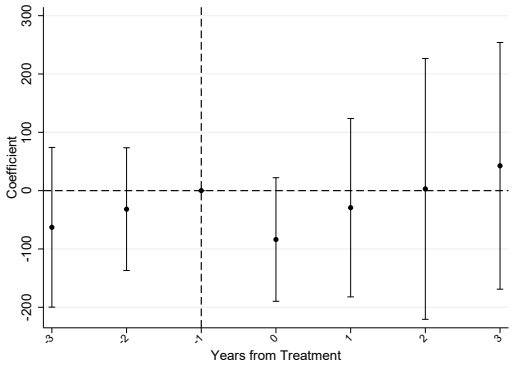
(b) 30 Seconds radius from cutoff

Note: Density of applications for the temporary export manager voucher by time of arrival of the completed application within the fist four minutes, and within 30 seconds with respect to the 2002$^{\text{nd}}$ application, which roughly corresponds to the theoretical exhaustion of resources.

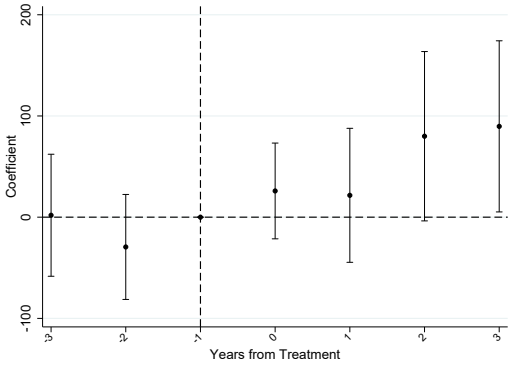
Figure 3: Effect of Subsidy Assignment on Firm Internationalization Over Time



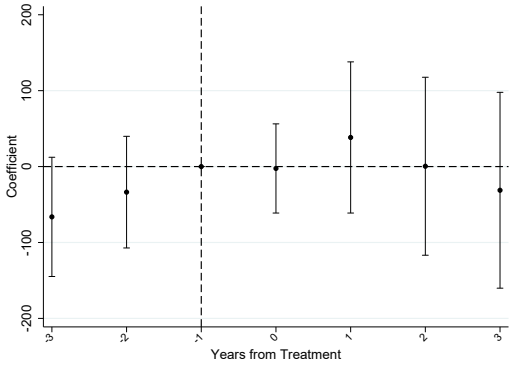
(a) Value of Export (Extra EU)



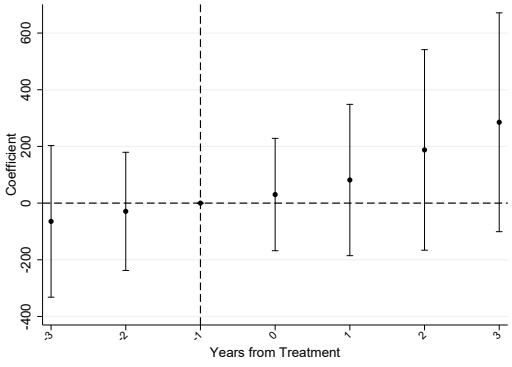
(b) Value of Export (Intra EU)



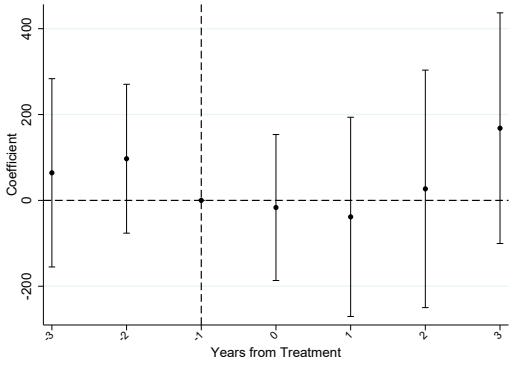
(c) Value of Import (Extra EU)



(d) Value of Import (Intra EU)



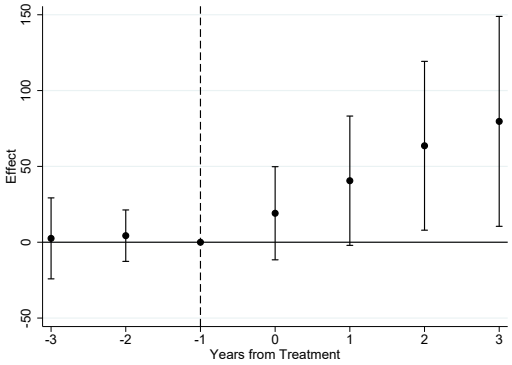
(e) Total Trade (IMP+EXP)



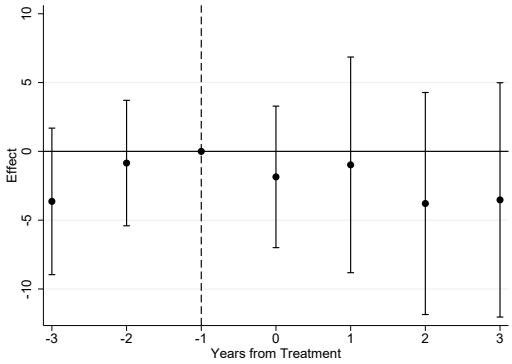
(f) Total Trade Balance (EXP-IMP)

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 2002nd 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. Effects are reported in thousands of euros. Standard errors are clustered at the firm level.

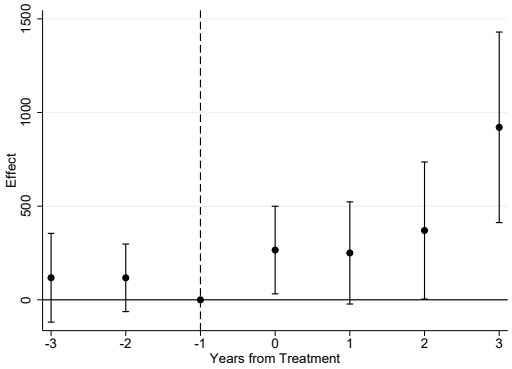
Figure 4: Effect of TEM Voucher Assignment on Firm Outcomes Over Time



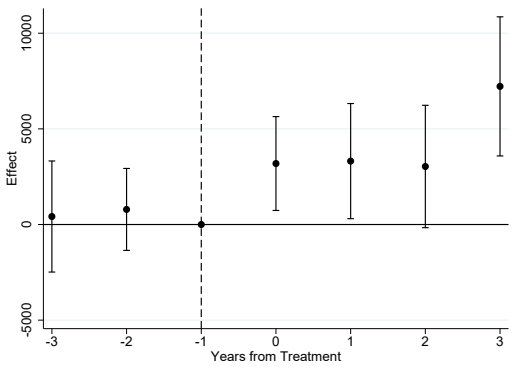
(a) Total Employment Costs



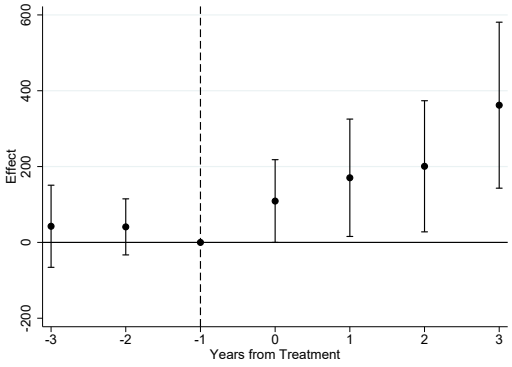
(b) Capital/labor Ratio



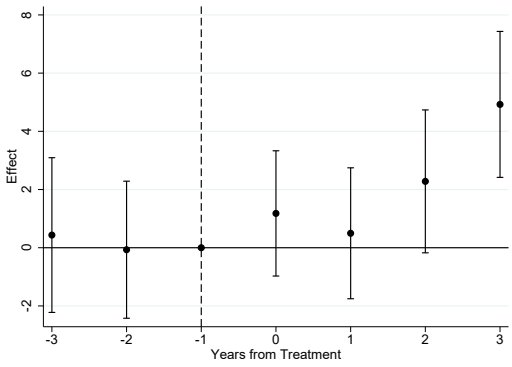
(c) Revenue from Sales



(d) Value Added per Employee



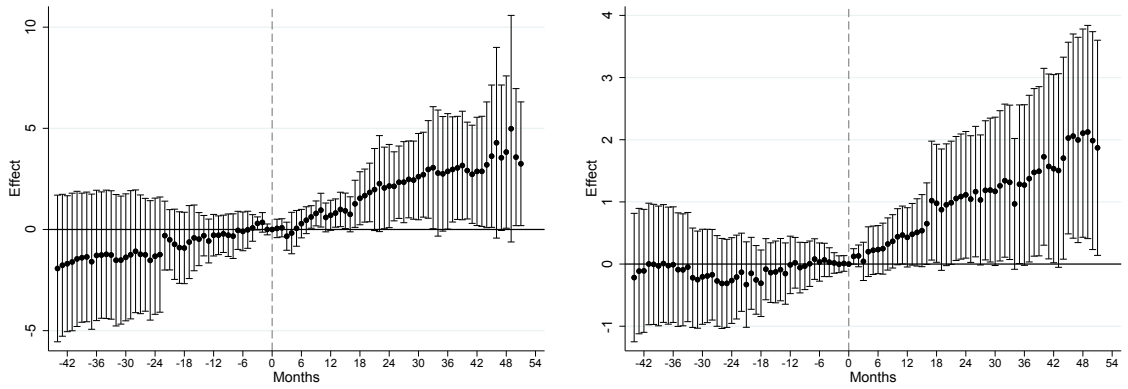
(e) Gross Profits



(f) Return on Equity

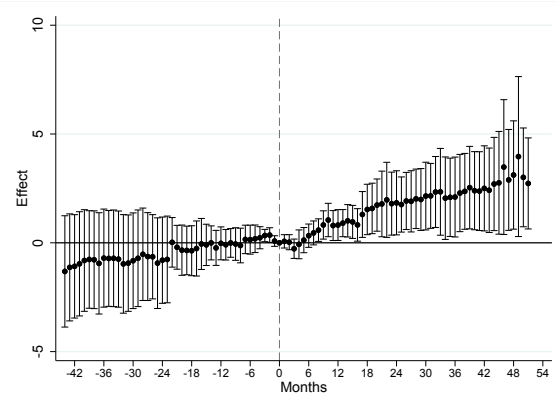
Notes: This figure reports the results of a difference-in-differences model estimated between 2013 and 2019. Capital/labor ratio computed as the ratio between total assets (material+immaterial) and number of employees. Base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd 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. Standard errors are clustered at the firm level.

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



(a) Total Employees

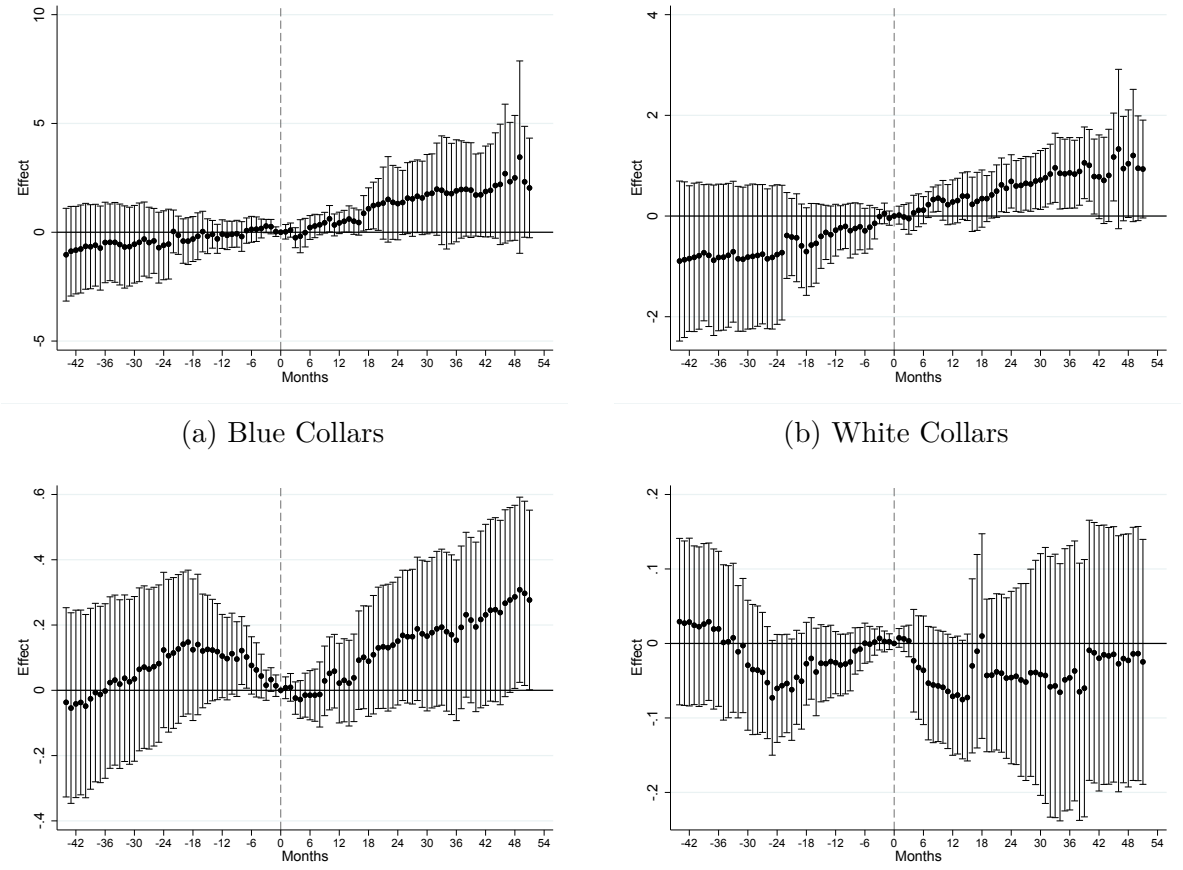
(b) Permanent Employees



(c) Full Time Employees

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

Figure 6: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Worker Qualification



(a) Blue Collars

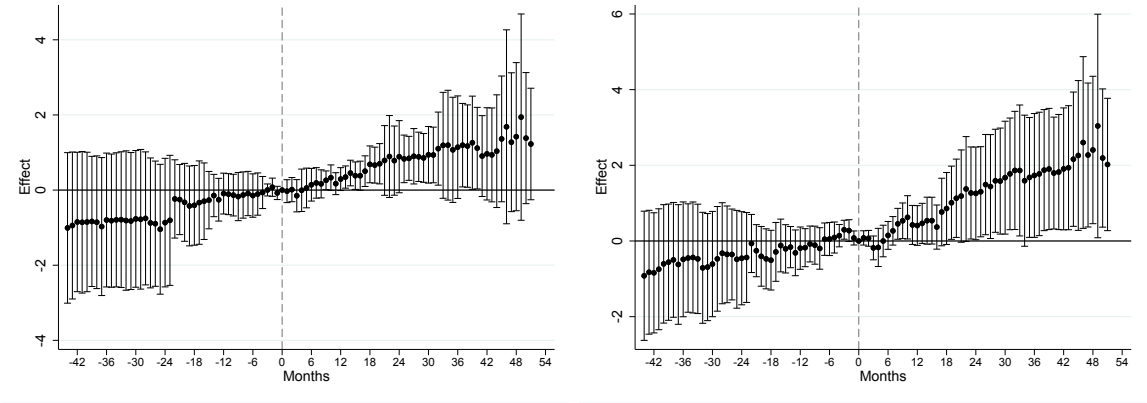
(b) White Collars

(c) Apprentices

(d) Managers

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

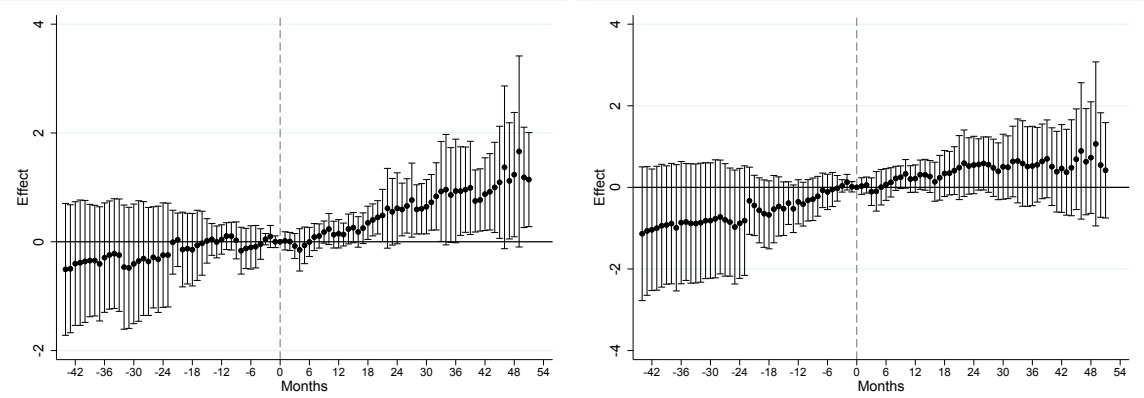
Figure 7: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Gender



(a) Female Employees (b) Male Employees

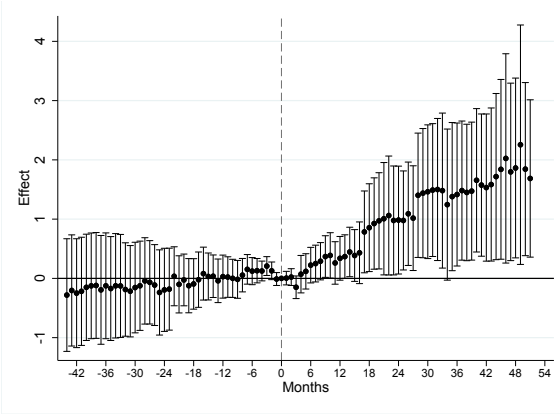
Notes: This figure reports results of a difference-in-differences model based on monthly data between 2012 and 2019. Base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd application. Firms were included in the analysis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

Figure 8: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Age Group



(a) Junior Employees

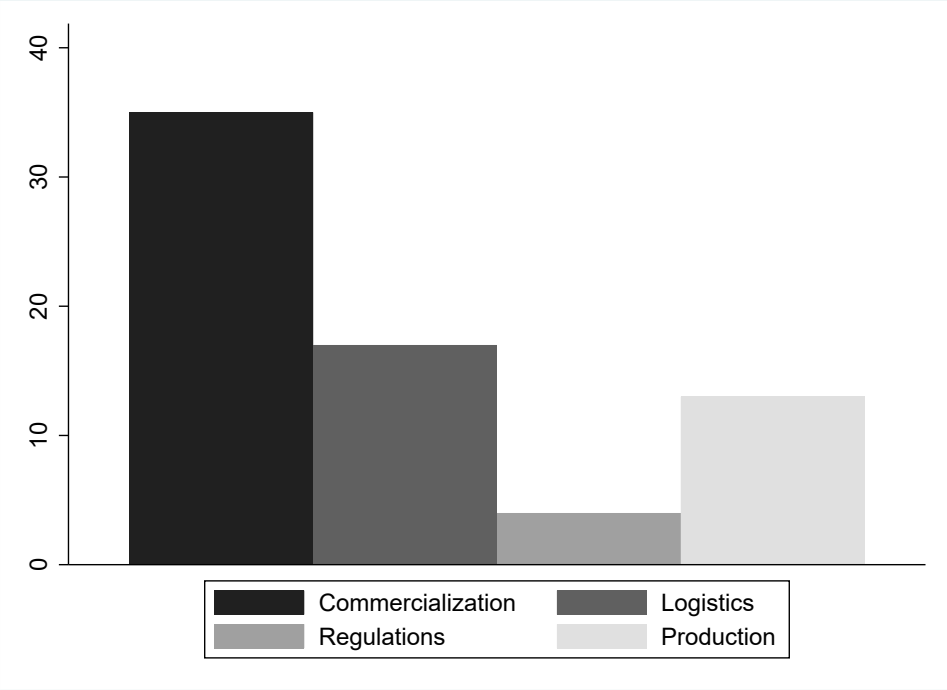
(b) Mid-Level Employees



(c) Senior Employees

Notes: This figure reports the results of a difference-in-differences model based on monthly data between 2012 and 2019. Base month is September 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd 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. Regression includes firm and month fixed effects. Coefficients of the difference with respect to the base year reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

Figure 9: 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.

Tables

Table 1: Comparison of Treated and Control Firms Before the Policy Implementation

Outcome variable	(1) Average Treated	(2) Average Controls	(3) Difference (2)-(1)	(4) Relative Difference (3)/(2)	(5) T-Stat
		Panel (a): Over three years before the policy (2013-2015)			
Export Extra EU	1379.573	1111.383	268.19	0.241	1.804
Export Intra EU	1911.656	1632.973	278.682	0.17	1.286
Import Extra EU	497.252	442.187	55.065	0.124	0.698
Import Intra EU	695.507	574.828	120.679	0.209	1.241
Total Trade	8085.394	6922.944	1162.449	0.167	1.485
Trade Balance	3740.023	3246.314	493.708	0.152	0.893
Total Employment	24.562	22.708	1.853	0.081	1.513
Total Employment Cost	943.32	847.034	96.286	0.113	1.891
Capital Employment Ratio	75.692	66.73	8.961	0.134	1.741
Revenue from Sales	5489.102	5332.903	156.198	0.029	0.442
Value Added per Employee	54567.945	55061.406	-493.46	-0.008	-0.374
Gross Profits	2806.205	2653.913	152.291	0.057	0.967
ROE	6.524	8.187	-1.663	-0.203	-2.013
Broadband Conn. (% Buildings)	0.247	0.25	-0.002	-0.008	-0.138
Broadband Conn. >100 Mps (% Buildings)	0.19	0.193	-0.002	-0.01	-0.204
Tot. Employees	23,36	26,90	3,54	0,15	1,90
Permanent Employees	21,64	23,74	2,10	0,10	1,43
Full Time Employees	21,17	23,92	2,75	0,13	1,66
Blue Collars	12,90	15,07	2,17	0,17	1,81
White Collars	8,44	9,66	1,22	0,14	1,46
Managers	0,59	0,47	-0,12	-0,20	-0,65
Apprentices	1,15	1,34	0,19	0,17	1,43
Women	7,13	8,93	1,80	0,25	2,24
Men	16,23	17,97	1,74	0,11	1,40
Junior (age 16-29)	3,11	3,99	0,89	0,29	2,34
Mid-level (age 30-45)	11,27	12,88	1,61	0,14	1,64
Senior (age >45)	8,99	10,03	1,04	0,12	1,42
		Panel (b): Year of the policy assignment (2015)			
Export Extra EU	1405.75	1178.646	227.103	0.192	1.44
Export Intra EU	2036.977	1705.732	331.244	0.194	1.442
Import Extra EU	548.814	483.246	65.568	0.135	0.753
Import Intra EU	735.398	581.762	153.635	0.264	1.514
Total Trade	8249.494	6982.746	1266.748	0.181	1.572
Trade Balance	3739.456	3288.95	450.505	0.136	0.794
Total Employment	25.035	23.259	1.776	0.076	1.417
Total Employment Cost	967.659	875.184	92.475	0.105	1.769
Capital Employment Ratio	76.093	65.571	10.521	0.16	2.025
Revenue from Sales	5608.846	5532.416	76.43	0.013	0.207
Value Added per Employee	54366.054	55093.988	-727.933	-0.013	-0.469
Gross Profits	2871.625	2748.569	123.055	0.044	0.748
ROE	7.914	9.803	-1.888	-0.192	-1.751
Broadband Conn. (% Buildings)	0.248	0.25	-0.001	-0.007	-0.156
Broadband Conn. >100 Mps (% Buildings)	0.19	0.194	-0.003	-0.015	-0.222
Tot. Employees	23,66	28,26	4,60	0,19	1,80
Permanent Employees	21,64	24,11	2,47	0,11	1,75
Full Time Employees	21,15	24,63	3,48	0,16	1,70
Blue Collars	12,79	15,48	2,69	0,21	1,75
White Collars	8,85	10,60	1,74	0,20	1,55
Managers	0,59	0,52	-0,07	-0,12	-0,43
Apprentices	1,17	1,33	0,16	0,13	1,09
Women	7,23	9,57	2,34	0,32	1,81
Men	16,42	18,69	2,26	0,14	1,57
Junior (age 16-29)	2,88	3,97	1,09	0,38	1,59
Mid-level (age 30-45)	10,70	13,02	2,32	0,22	1,89
Senior (age >45)	10,08	11,27	1,19	0,12	1,39
Number firms	1765	553	1212	2.191	

Notes: Summary statistics for treatment and control group. Column (3) reports the difference in the average between the two groups and Column (4) reports the ratio between Column (3) and Column (1). Column (5) 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. The regression includes year fixed effects and standard errors are clustered at firm level. All variables for firm and trade outcomes are winsorized at 1% and reported in thousands of euros.

Table 2: Comparison of Treated and Control Firms Before the Policy Implementation: Normalized Differences

Outcome variable	(1) Average Treated	(2) Average Controls	(3) Normalized Differences
	Panel (a): Over three years before the policy (2013-2015)		
Export Extra EU	1379.573	1111.383	0.082
Export Intra EU	1911.656	1632.973	0.061
Import Extra EU	497.252	442.187	0.033
Import Intra EU	695.507	574.828	0.057
Total Trade	8085.394	6922.944	0.092
Trade Balance	3740.023	3246.314	0.055
Total Employment	24.562	22.708	0.071
Total Employment Cost	943.32	847.034	0.089
Capital Employment Ratio	75.692	66.73	0.083
Revenue from Sales	5489.102	5332.903	0.021
Value Added per Employee	54567.945	55061.406	-0.015
Gross Profits	2806.205	2653.913	0.046
ROE	6.524	8.187	-0.073
Broadband Conn. (% Buildings)	0.247	0.25	-0.006
Broadband Conn. >100 Mps (% Buildings)	0.19	0.193	-0.007
	Panel (b): Year of the policy assignment (2015)		
Export Extra EU	1405.75	1178.646	0.068
Export Intra EU	2036.977	1705.732	0.068
Import Extra EU	548.814	483.246	0.036
Import Intra EU	735.398	581.762	0.07
Total Trade	8249.494	6982.746	0.097
Trade Balance	3739.456	3288.95	0.049
Total Employment	25.035	23.259	0.067
Total Employment Cost	967.659	875.184	0.083
Capital Employment Ratio	76.093	65.571	0.098
Revenue from Sales	5608.846	5532.416	0.01
Value Added per Employee	54366.054	55093.988	-0.023
Gross Profits	2871.625	2748.569	0.035
ROE	7.914	9.803	-0.084
Broadband Conn. (% Buildings)	0.248	0.25	-0.005
Broadband Conn. >100 Mps (% Buildings)	0.19	0.194	-0.011

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

Table 3: 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	65.125	14.172	63.494**	34.069	176.861	-18.265
	(56.685)	(86.524)	(31.307)	(46.535)	(147.862)	(111.161)
Observations	16,156	16,156	16,156	16,156	16,156	16,156
R-squared	0.893	0.929	0.900	0.899	0.933	0.924
Mean Control	1193.86	1911.33	438.85	580.54	4124.59	2085.81
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. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 2002nd 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 form countries within and outside the European Union, while Trade balance is the sum of all exports minus all imports. All variables are winsorized at 1%. Effects are reported in thousands of euros. Standard errors are clustered at the firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table 4: Effect of TEM Voucher Assignment on Firm Balance Sheet Outcomes

Variables	(1) Employment Cost	(2) C/E Ratio	(3) Revenue from Sales	(4) VA per Employee	(5) Gross Profits	(6) Roe
Before Cutoff X Post	55.267**	-0.858	408.477**	3,637.226***	195.813**	2.010**
	(25.211)	(3.367)	(163.288)	(1,269.859)	(83.905)	(0.866)
Observations	15,315	14,913	15,315	15,087	15,195	15,125
R-squared	0.918	0.821	0.918	0.660	0.907	0.415
Mean Control	887.77	59.8	5430.7	49006.91	2665.13	7.8
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 2002nd 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. Effects are reported in thousands of euros. All variables are winsorized at 1%. Standard errors are clustered at the firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table 5: 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	0.176**	0.017	0.136*	0.289**	0.047	0.018	0.010	-0.016	0.203**	0.025	0.160*	0.300*
	(0.089)	(0.104)	(0.073)	(0.138)	(0.061)	(0.064)	(0.053)	(0.095)	(0.103)	(0.118)	(0.087)	(0.160)
Before Cutoff X Post X South	-0.035				-0.062				-0.012			
	(0.171)				(0.134)				(0.216)			
Before Cutoff X Post X Small Firm		0.325**				0.028				0.366**		
		(0.153)				(0.109)				(0.181)		
Before Cutoff X Post X Low Productivity			0.641				0.411				0.857*	
			(0.434)				(0.283)				(0.509)	
Before Cutoff X Post X Exporter				-0.179				0.087				-0.130
				(0.163)				(0.115)				(0.192)
Observations	16,156	16,156	16,156	16,066	15,737	15,737	15,737	15,659	16,156	16,156	16,156	16,066
R-squared	0.696	0.696	0.698	0.693	0.759	0.759	0.759	0.758	0.647	0.647	0.650	0.642
P-value Sum	.332	.002	.069	.21	.894	.607	.13	.268	.315	.004	.042	.106
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	VA per employee (log)				Gross Profits (log)				Roe (log)			
Before Cutoff X Post	0.450**	0.253	0.431***	0.647**	0.187*	0.106	0.186**	0.278*	0.383***	0.220*	0.285***	0.282*
	(0.175)	(0.192)	(0.157)	(0.285)	(0.107)	(0.131)	(0.093)	(0.168)	(0.105)	(0.122)	(0.093)	(0.152)
Before Cutoff X Post X South	0.119				0.155				-0.349			
	(0.419)				(0.241)				(0.217)			
Before Cutoff X Post X Small Firm		0.472				0.234				0.203		
		(0.324)				(0.192)				(0.185)		
Before Cutoff X Post X Low Productivity			0.923				0.758				0.386	
			(0.869)				(0.520)				(0.463)	
Before Cutoff X Post X Exporter				-0.249				-0.071				0.056
				(0.341)				(0.203)				(0.191)
Observations	15,916	15,916	15,916	15,830	16,031	16,031	16,031	15,943	15,957	15,957	15,957	15,868
R-squared	0.492	0.492	0.493	0.486	0.621	0.621	0.624	0.616	0.494	0.494	0.494	0.494
P-value Sum	.134	.005	.113	.034	.113	.015	.065	.068	.857	.002	.139	.003
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 2002nd 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 correspond to the median size of applying firms). Low Productivity are firms in the bottom decile 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. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table 6: Heterogeneous Effects by Provider

Variables	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Gross Profits	(6) ROE
1st Provider X Post	0.058	0.098	0.182	0.314	0.148	0.380**
	(0.127)	(0.089)	(0.147)	(0.278)	(0.154)	(0.150)
2nd Provider X Post	-0.209	-0.070	-0.292	-0.092	-0.217	0.160
	(0.272)	(0.195)	(0.322)	(0.452)	(0.326)	(0.241)
3rd Provider X Post	0.614***	0.370***	0.820***	1.721***	0.873***	0.134
	(0.089)	(0.110)	(0.109)	(0.276)	(0.113)	(0.234)
4th Provider X Post	0.297	-0.047	0.379	-0.054	0.456*	0.914***
	(0.218)	(0.202)	(0.276)	(0.556)	(0.274)	(0.319)
5th Provider X Post	0.363	0.195	0.319	0.604	0.351	0.325
	(0.240)	(0.136)	(0.245)	(0.447)	(0.297)	(0.267)
6th Provider X Post	-0.128	-0.108	-0.239	-0.015	-0.327	0.234
	(0.256)	(0.164)	(0.313)	(0.635)	(0.369)	(0.307)
7th Provider X Post	0.176	0.259***	0.415	1.332***	0.408	0.316
	(0.295)	(0.085)	(0.328)	(0.487)	(0.306)	(0.283)
Other Provider X Post	0.187**	0.019	0.209**	0.498***	0.230**	0.309***
	(0.080)	(0.056)	(0.094)	(0.166)	(0.099)	(0.096)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
R-squared	0.697	0.759	0.648	0.493	0.622	0.494
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 TEM provider. Treated firms are the firms that applied for the voucher before the 2002nd application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Effect of the policy is decomposed by provider of the temporary export manager. We group together 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. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table 7: Heterogeneous Effects by Services Provided

	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Gross Profits	(6) Roe
Before Cutoff X Post	0.464**	0.254*	0.622***	0.964***	0.502**	-0.161
	(0.199)	(0.141)	(0.178)	(0.356)	(0.220)	(0.261)
Before Cutoff X Post X Commercialization	-0.281	-0.227	-0.374**	-0.508	-0.230	0.559**
	(0.197)	(0.139)	(0.173)	(0.348)	(0.215)	(0.259)
Observations	8,488	8,299	8,488	8,392	8,430	8,395
R-squared	0.697	0.768	0.631	0.474	0.611	0.487
P-value Sum	.043	.670	.021	.015	.014	0.000
Before Cutoff X Post	0.248**	0.068	0.297**	0.591***	0.319**	0.217*
	(0.109)	(0.074)	(0.126)	(0.212)	(0.130)	(0.132)
Before Cutoff X Post X Logistics	-0.077	-0.041	-0.035	-0.167	-0.050	0.241*
	(0.116)	(0.079)	(0.132)	(0.225)	(0.134)	(0.140)
Observations	8,488	8,299	8,488	8,392	8,430	8,395
R-squared	0.697	0.768	0.631	0.474	0.611	0.487
P-value Sum	.094	.708	.030	.049	.033	0.000
Before Cutoff X Post	0.193**	0.040	0.261**	0.459**	0.268**	0.358***
	(0.090)	(0.063)	(0.106)	(0.186)	(0.111)	(0.108)
Before Cutoff X Post X Regulations	0.178	0.064	0.232	0.548	0.316	-0.063
	(0.169)	(0.135)	(0.210)	(0.338)	(0.196)	(0.295)
Observations	8,488	8,299	8,488	8,392	8,430	8,395
R-squared	0.697	0.768	0.631	0.474	0.611	0.486
P-value Sum	.031	.446	.022	.004	.004	.322
Before Cutoff X Post	0.221**	0.077	0.311***	0.496**	0.302***	0.392***
	(0.093)	(0.065)	(0.109)	(0.196)	(0.115)	(0.114)
Before Cutoff X Post X Production	-0.068	-0.132	-0.143	-0.002	-0.050	-0.155
	(0.141)	(0.099)	(0.160)	(0.252)	(0.159)	(0.157)
Observations	8,488	8,299	8,488	8,392	8,430	8,395
R-squared	0.697	0.768	0.631	0.474	0.611	0.486
P-value Sum	.279	.582	.300	.054	.124	.129
Before Cutoff X Post	0.138	0.058	0.132	0.332	0.184	0.286**
	(0.121)	(0.079)	(0.145)	(0.230)	(0.146)	(0.134)
Before Cutoff X Post X Digitalization	0.078	-0.033	0.209	0.263	0.164	0.118
	(0.126)	(0.085)	(0.147)	(0.240)	(0.147)	(0.144)
Observations	8,156	7,979	8,156	8,064	8,102	8,063
R-squared	0.692	0.762	0.629	0.470	0.609	0.485
P-value Sum	.032	.736	.003	.005	.004	.001

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 2002nd 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 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. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table 8: Effect of TEM Voucher Assignment on Firm Labor Demand

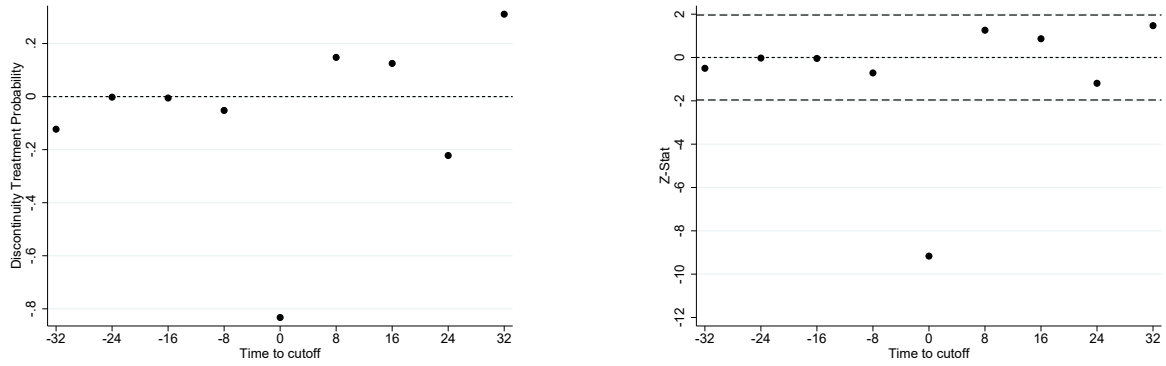
	Overall	Contract Type		Occupation				Demographics				
	Tot. Employees	Permanent	Full Time	Blue Collars	White Collars	Managers	Apprentices	Women	Men	Junior	Mid-Level	Senior
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Before Cutoff × Post	2.900	1.191**	2.192*	1.685	1.127*	-0.0268	0.0867	1.276	1.624*	0.780	0.977	1.142*
	(1.774)	(0.580)	(1.182)	(1.213)	(0.581)	(0.0718)	(0.113)	(0.919)	(0.898)	(0.547)	(0.699)	(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 2002nd 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 in number of employees. All variables are winsorized at 1%. Standard errors are clustered at the firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Appendix

Figures

Figure A1: Test for Discontinuity in the Treatment Probability

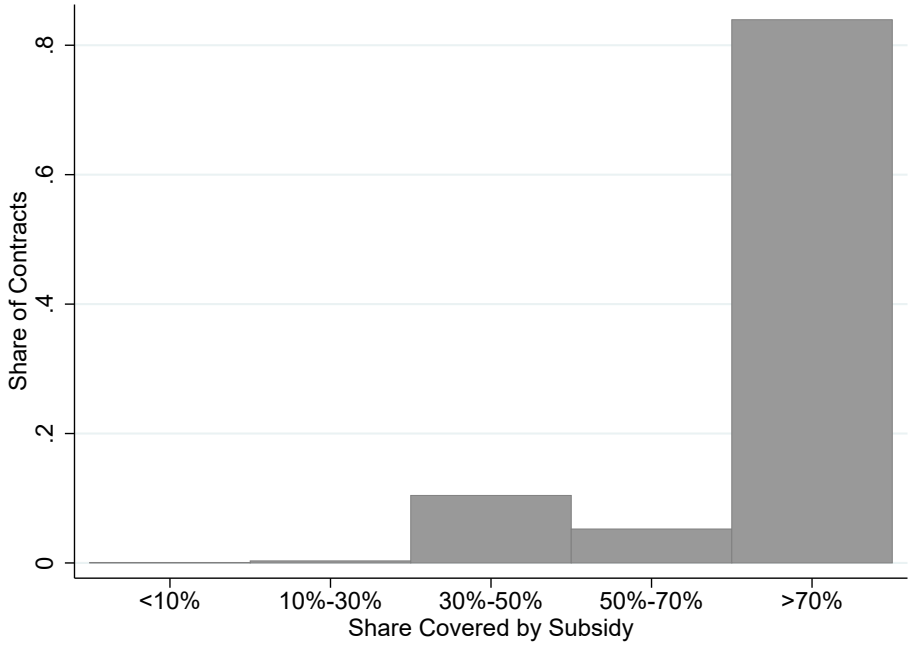


(a) Coefficient

(b) Z-Statistic

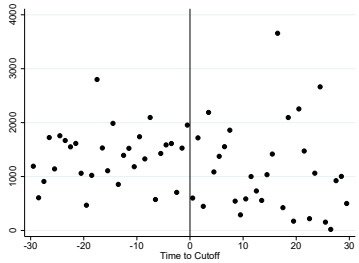
Note: Figure reports coefficients for RDD equations testing for the presence of a discontinuities in the share of firms benefiting from the subsidy by time of application. Panel (a) reports coefficients while Panel (b) reports corresponding z-statistics. Dotted line correspond to thresholds for 5% significance. Equation estimate with the rdrobust command by Calonico et al. (2017) with optimal bandwith selection.

Figure A2: Share of Contracts by Amount Covered by the Policy

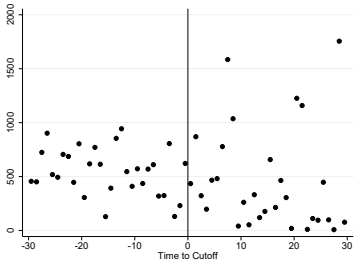


Note: Figure plots the share of contracts by the ratio between the amount of the subsidy (10,000 Euro) and the total value in Euro of the contract reported to the Ministry of Economic Development.

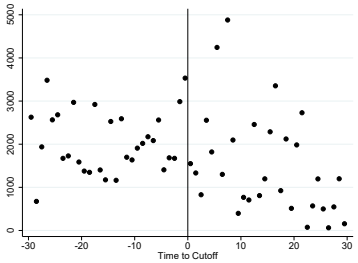
Figure A3: Observable Characteristics for Trade and Firm variables in 2015 by Time of Application



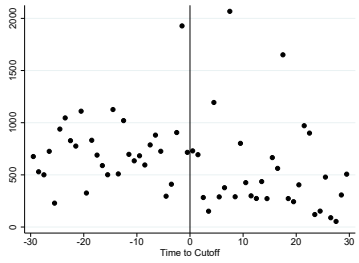
(a) Value Export (Extra EU)



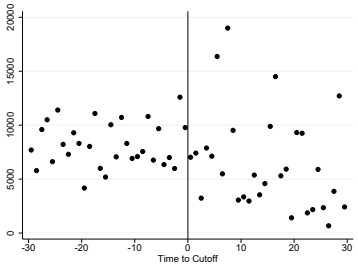
(b) Value Import (Extra EU)



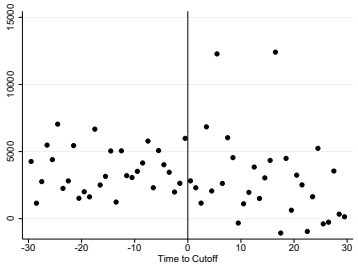
(c) Value Export (Intra EU)



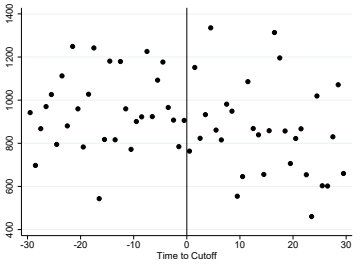
(d) Value Import (Intra EU)



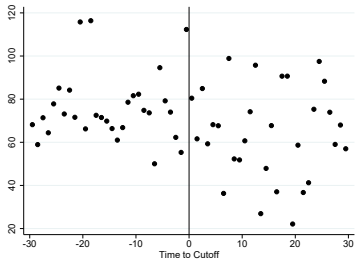
(e) Total Trade



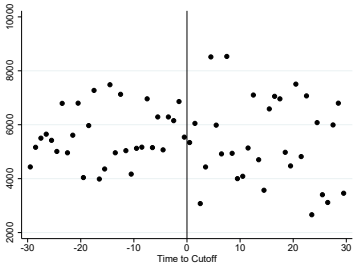
(f) Trade Balance



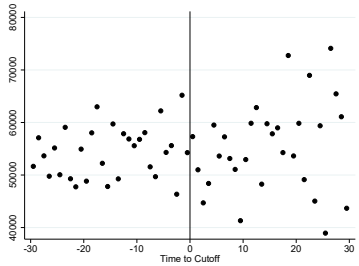
(g) Total Employment Costs



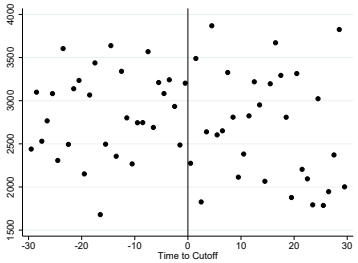
(h) Capital/labor Ratio



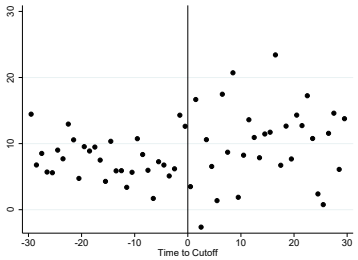
(i) Revenue from Sales



(j) Value Added per Employee



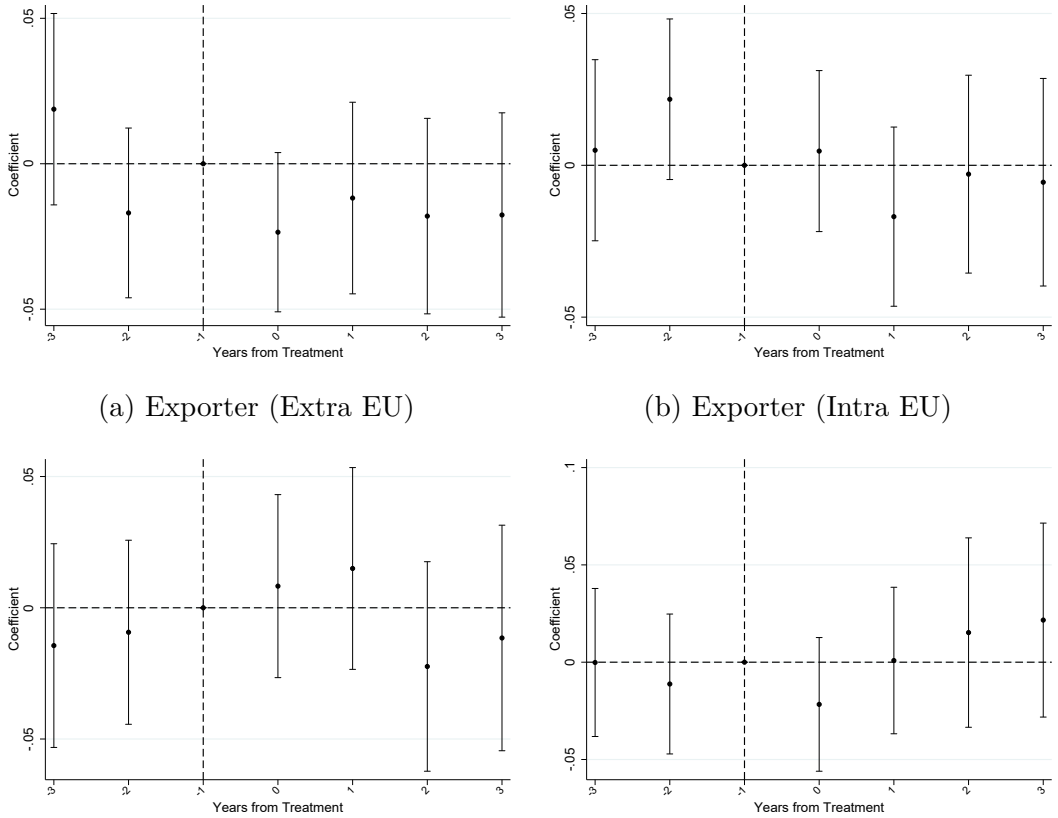
(k) Gross Profits



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



(a) Exporter (Extra EU)

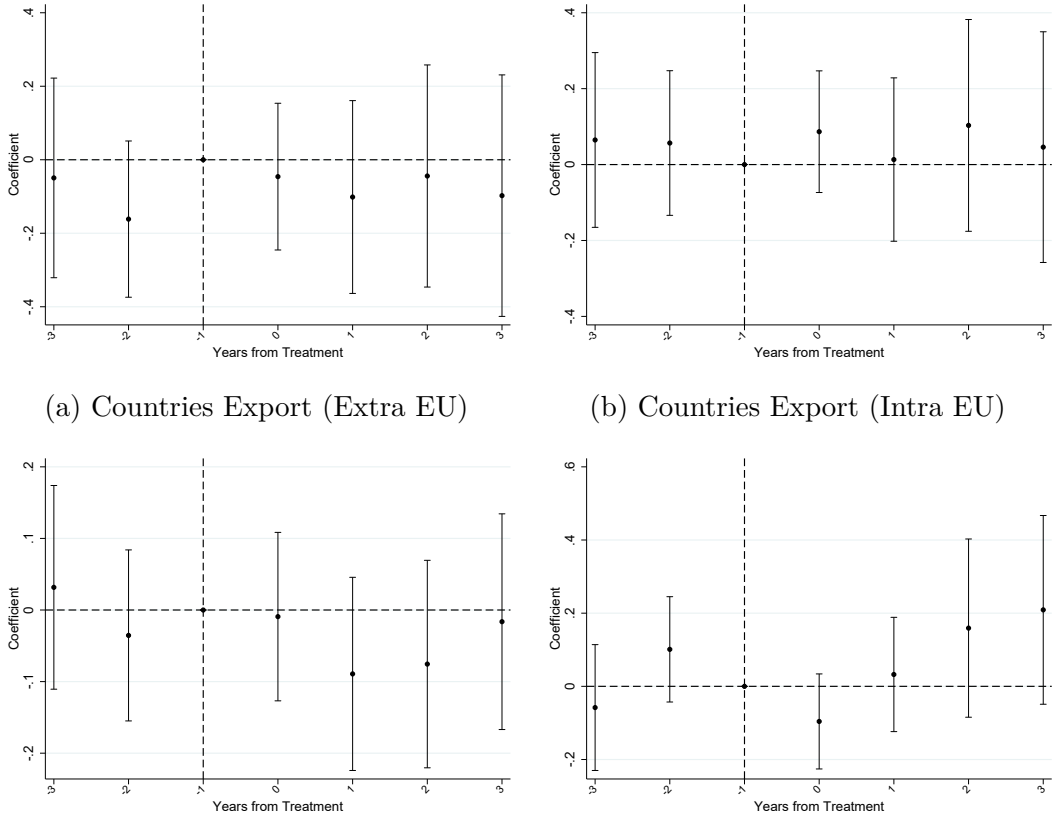
(b) Exporter (Intra EU)

(c) Importer (Extra EU)

(d) Importer (Intra EU)

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 2002nd 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. Estimates are based on linear probability models with 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 A5: Effect of TEM Voucher Assignment on Number of Countries for Export and Import by Broad Destination



(a) Countries Export (Extra EU)

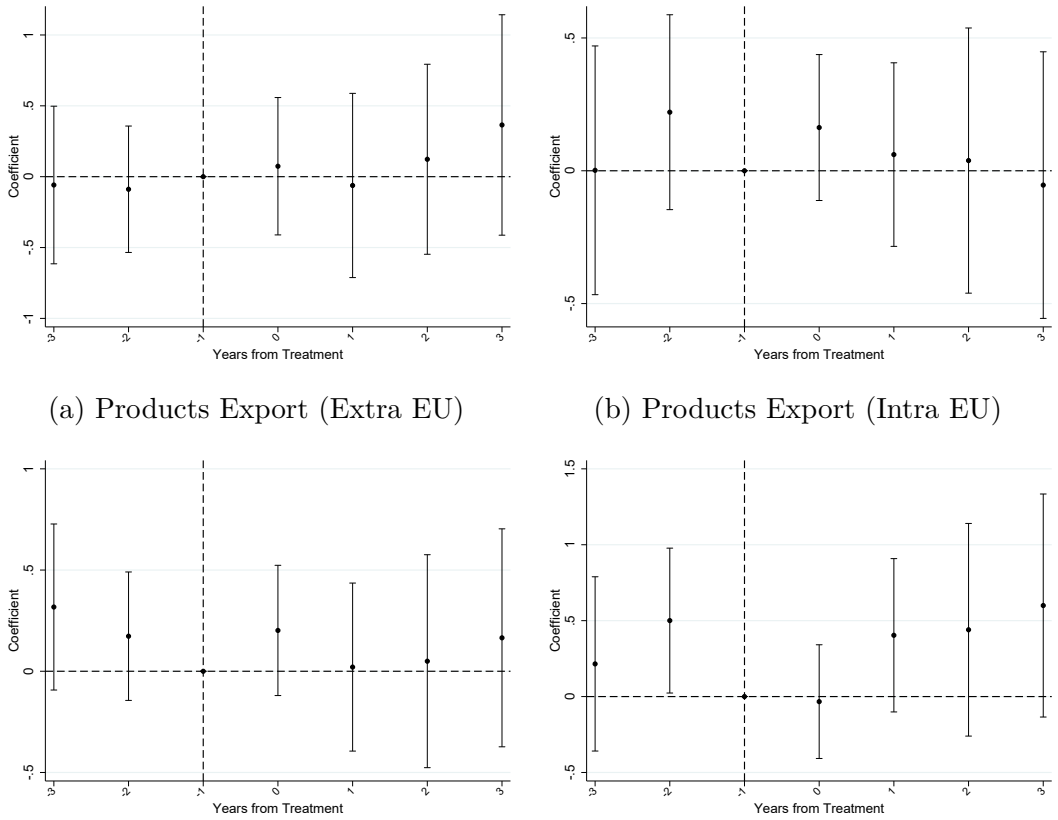
(b) Countries Export (Intra EU)

(c) Countries Import (Extra EU)

(d) Countries Import (Intra EU)

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 2002nd 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. Estimates are based on OLS models with 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 A6: Effect of TEM Voucher Assignment on Number of Products for Export and Import by Broad Destination



(a) Products Export (Extra EU)

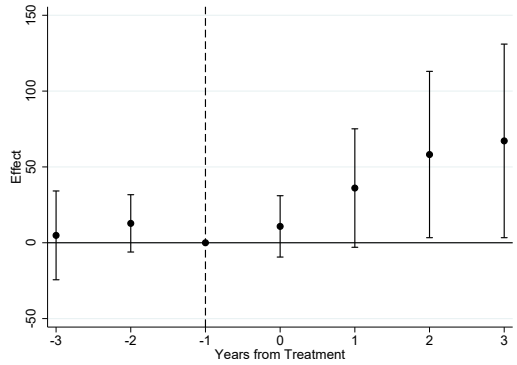
(b) Products Export (Intra EU)

(c) Products Import (Extra EU)

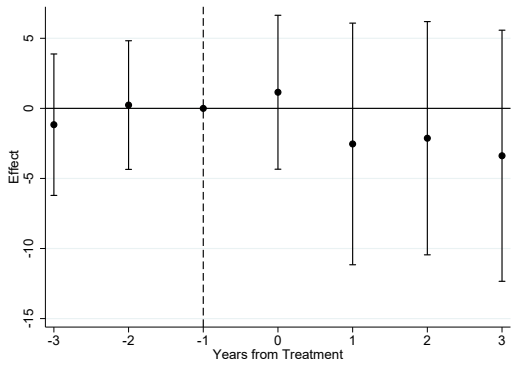
(d) Products Import (Intra EU)

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 2002nd 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. Estimates are based on OLS models with dependent variable equal to the number of products which the firm exports/imports to/from the specified group of countries. Standard errors are clustered at the firm level.

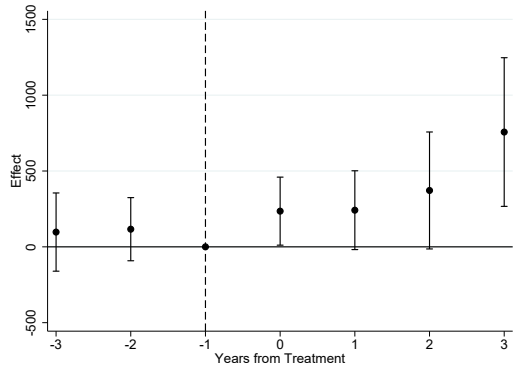
Figure A7: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Balanced Panel



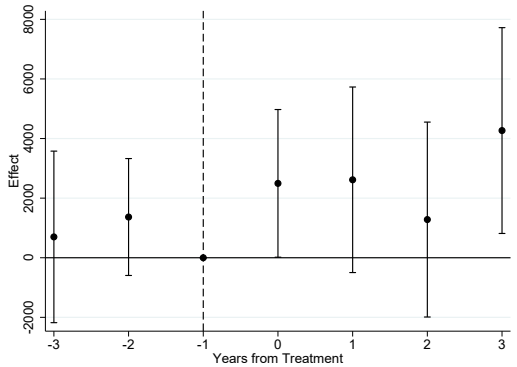
(a) Total Employment Costs



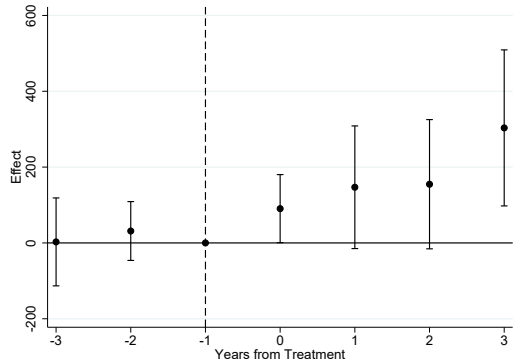
(b) Capital/labor Ratio



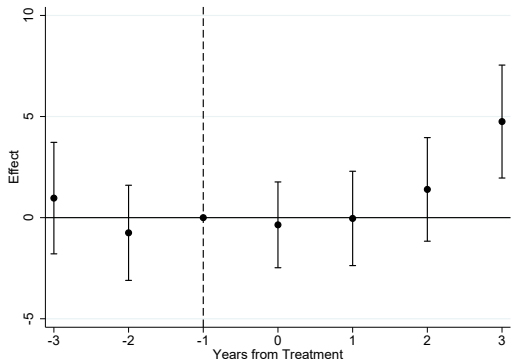
(c) Revenue from Sales



(d) Value Added per Employee



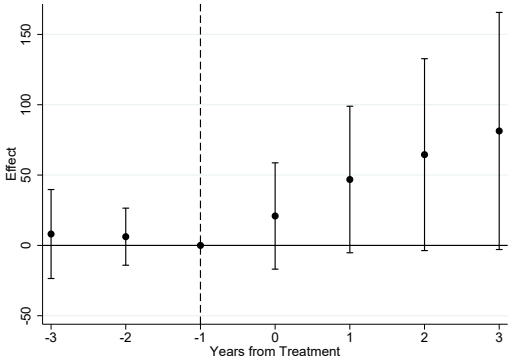
(e) Gross profits



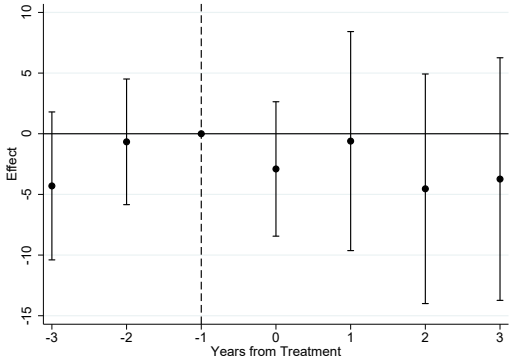
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2019. Capital/labor ratio computed as the ratio between total assets (material+immaterial) and number of employees. Base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Sample restricted to firms having positive employment over the whole period of analysis (2012–2019). Regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

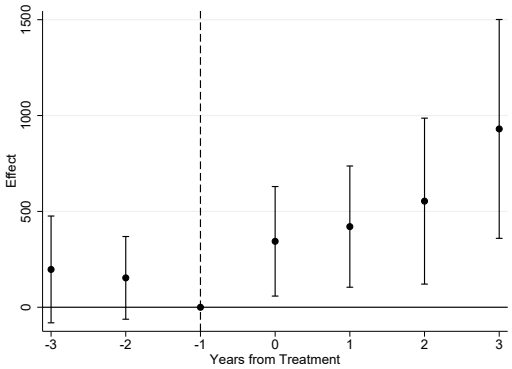
Figure A8: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 20 Seconds



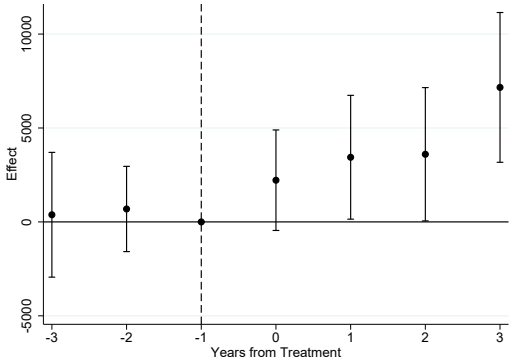
(a) Total Employment Costs



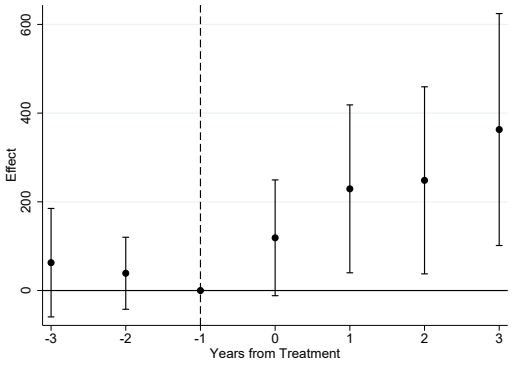
(b) Capital/labor Ratio



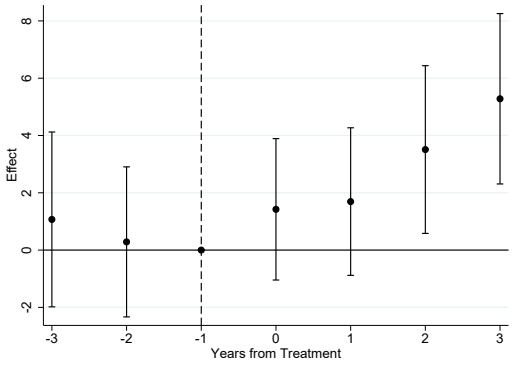
(c) Revenue from Sales



(d) Value Added per Employee



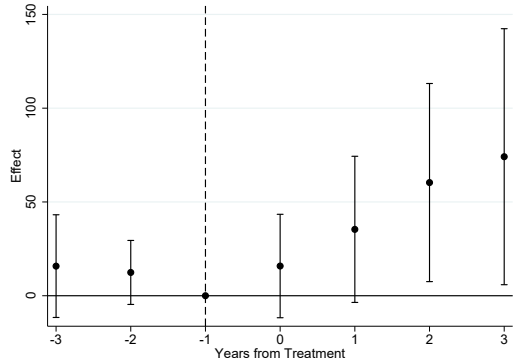
(e) Gross profits



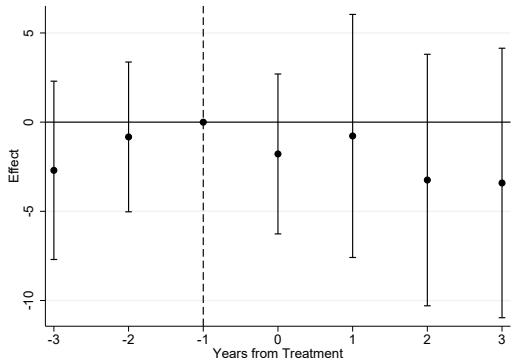
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2019. Capital/labor ratio computed as the ratio between total assets (material+immaterial) and number of employees. Base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd 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 reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

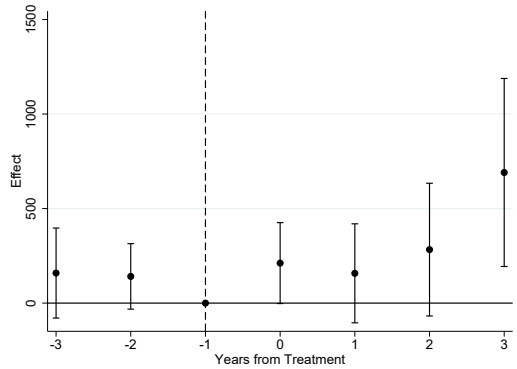
Figure A9: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 40 Seconds



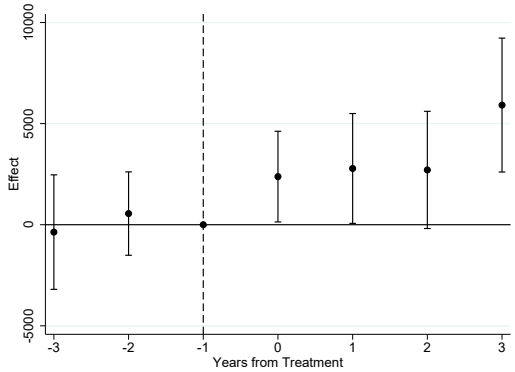
(a) Total Employment Costs



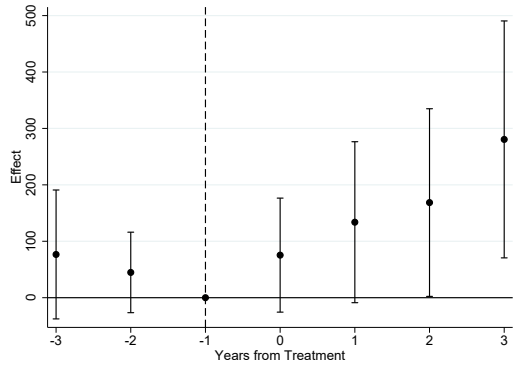
(b) Capital/labor Ratio



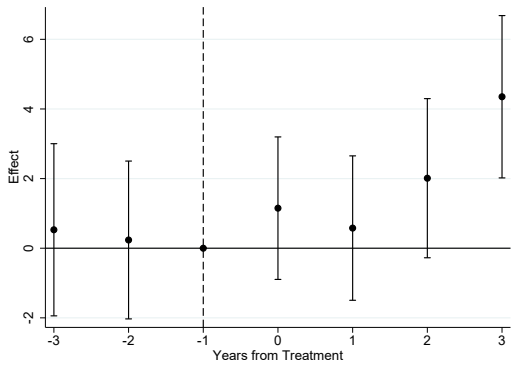
(c) Revenue from Sales



(d) Value Added per Employee



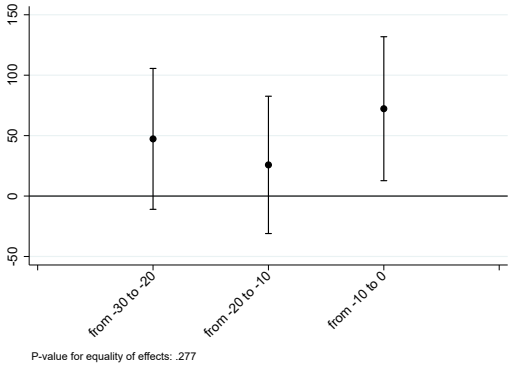
(e) Gross profits



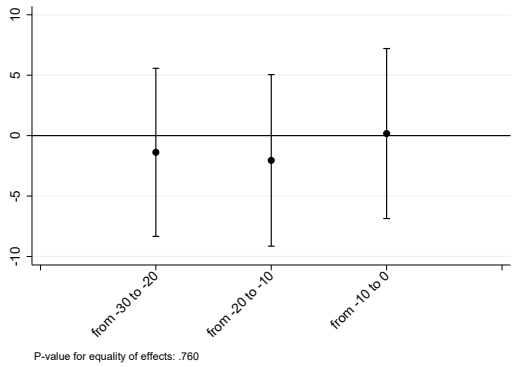
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2019. Capital/labor ratio computed as the ratio between total assets (material+immaterial) and number of employees. Base year is 2015. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002nd 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 reported together with their 95% confidence interval. Standard errors are clustered at the firm level.

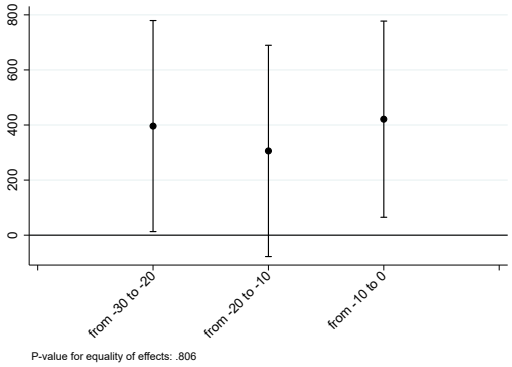
Figure A10: Effect of TEM Voucher by Time of Application Bin (10 Seconds)



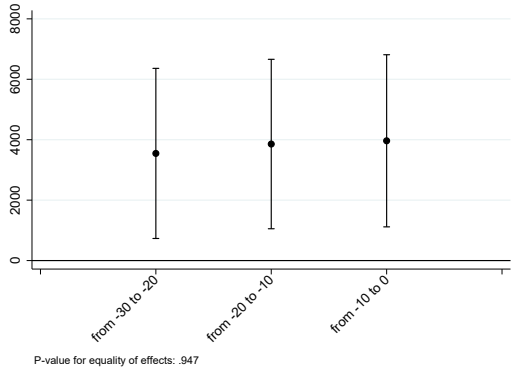
(a) Total Employment Costs



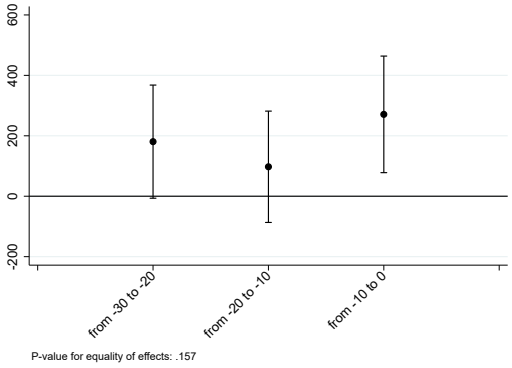
(b) Capital/labor Ratio



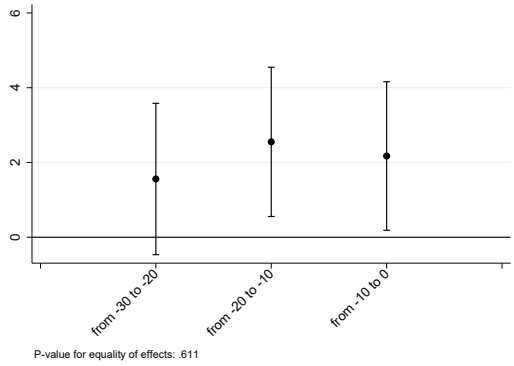
(c) Revenue from Sales



(d) Value Added per Employee



(e) Gross profits



(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2019. Firms applying before the cutoff are divided in groups based on time of application. We report treatment effects together with p-values for a F-test for the equality of the effects. Capital/labor ratio computed as the ratio between total assets (material and immaterial) and number of employees. Base year is 2015. All variables are winsorized at 1%. Treated firms are firms that applied for the voucher before the 2002nd application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Standard errors are clustered at the firm level.

Tables

Table A1: Comparison of Applicants and General Firm Population

Variable Name	(1) Average Applicants	(2) Average Non-Applicants	(3) Difference	(4) T-Stat
		Panel (a): All		
Roe	7.879	11.484	-3.607	-9.359
Cost per employee	35.501	28.375	7.126	31.17
Total Employment	25.158	13.845	11.312	24.45
Revenues from Sales	5,777.379	3,096.379	2,680.999	19.979
VA per Employee	54.665	49.005	5.658	9.05
Observations	3,967	451,638		
		Panel (b): below 250 Employees		
Roe	7.879	11.593	-3.713	-9.64
Cost per employee	35.506	28.228	7.275	31.819
Total Employment	24.944	11.682	13.475	29.209
Revenues from Sales	5,761.33	2,613.008	3,164.371	23.629
VA per Employee	54.679	48.726	5.939	9.5
Observations	3,963	404,665		

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 Euro in Costs for personnel. Panel (a) includes all firms with at least 500,000 Euro in revenues in one of the three years before the policy (and hence eligible for it), and Panel (b) restricts the sample to firms with less than 250 employees and more than 500,000 in revenues in one of the years before the policy. T-stat obtained from a regression on the variable reported in the first column and a dummy for being an applicant. Robust standard errors are used to compute the t-statistic.

Table A2: Comparison of Applicants and General Firm Population: Accounting for Sector and Employment

	(1)	(2)	(3)	(4)
	Sector FE		Sector FE and Employment	
Variable	Difference	T-Stat	Difference	T-Stat
Roe	-3.832	-9.84	-3.792	-9.729
Cost per employee	3.493	15.319	2.244	10.039
Total Employment	9.175	19.819		
Revenues from Sales	1,707.417	12.71	-191.529	-1.96
VA per Employee	1.761	2.829	0.439	0.709

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 Euro in revenues in one the three years preceding the policy implementation and less than 250 employees in 2015 as in Panel (b) of Table A1. Regression for Column (1) and Column (2) also includes sector fixed effects (two digits ateco). Regression for Column (3) and (4) includes sector fixed effects and the level of employment in 2015. Robust standard errors are used to compute the t-statistic.

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	56.022	27.540	-0.287	0.747	-17.223	18.528	21.512**	25.127*	-2.729	1.621	19.839
	(35.371)	(36.964)	(0.571)	(1.634)	(16.648)	(24.592)	(10.427)	(15.047)	(2.761)	(3.973)	(21.057)
Observations	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156
Mean Control	603.32	542.42	2.79	11.90	245.48	315.57	78.2	193.37	23.73	24.09	227.75
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	13.506	40.349		-0.004	14.198	17.502**	-0.606	2.589	-0.007	-0.496	-0.937
	(8.259)	(27.245)		(0.655)	(17.106)	(7.393)	(1.716)	(3.542)	(0.041)	(0.380)	(7.563)
Observations	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156	16,156
Mean Control	68.8	341.67	0	3.32	225.28	55.49	8.54	12.25	.16	.89	71.91
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 2002nd 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 Column 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). Results in Column (3) of Panel (b) was not possible due to insufficient variation in the data. All variables are winsorized at 1%. Standard errors are clustered at the firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

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

	Panel (a): Extensive Margin			
	(1)	(2)	(3)	(4)
Variables	Export Extra EU	Export Intra EU	Import Extra EU	Import Intra EU
Before Cutoff X Post	-0.018	-0.014	0.005	0.008
	(0.012)	(0.012)	(0.012)	(0.015)
Observations	16,156	16,156	16,156	16,156
Mean Control	.63	.61	.44	.43
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
	Panel (b): Products			
Variables	Prod. Extra EU EXP	Prod. Intra EU EXP	Prod. Extra EU Imp	Prod. Intra EU Imp
Before Cutoff X Post	0.174	-0.022	-0.052	0.116
	(0.280)	(0.198)	(0.192)	(0.305)
Observations	16,156	16,156	16,156	16,156
Mean Control	8.26	4.62	4.18	4.48
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
	Panel (c): Countries			
Variables	Count. Extra EU EXP	Count. Intra EU EXP	Count. Extra EU Imp	Count. Intra EU Imp
Before Cutoff X Post	-0.002	0.022	-0.046	0.061
	(0.131)	(0.114)	(0.052)	(0.083)
Observations	16,156	16,156	16,156	16,156
Mean Control	5.47	4.96	1.57	1.95
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 2002nd 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 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 which are 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. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table A5: Effect of TEM Voucher Assignment on Firm Balance Sheet Outcomes: Robustness

Variables	(1) Employment Cost	(2) C/E Ratio	(3) Revenue from Sales	(4) VA per Employee	(5) Gross Profits	(6) Roe
			Panel (a): Baseline (winsored 1%)			
Before Cutoff X Post	48.467**	-1.085	374.251**	3,791.061***	183.187**	2.101**
	(24.452)	(3.115)	(158.058)	(1,220.213)	(80.587)	(0.841)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
			Panel (b): No Winsoring			
Before Cutoff X Post	113.876**	-11.011	345.554	4,321.084***	207.434	2.132**
	(52.177)	(9.934)	(220.098)	(1,548.396)	(139.302)	(0.913)
Mean Control	899.89	68.2	5807.16	49148.11	2804.41	7.64
			Panel (c): Logs			
Before Cutoff X Post	0.171**	0.038	0.207**	0.488***	0.224**	0.319***
	(0.077)	(0.054)	(0.091)	(0.160)	(0.096)	(0.092)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
			Panel (d): Instrumental Variable			
Receiving Subsidy X Post	59.707**	-1.325	461.044**	4,632.937***	225.878**	2.579**
	(30.064)	(3.806)	(194.437)	(1,494.906)	(99.297)	(1.033)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
F-test	3,720.64	3,671.56	3,720.64	3,743.45	3,672.31	3,742.86
			Panel (e): Balanced Panel			
Before Cutoff X Post	37.169	-0.364	330.198**	1,209.145	160.294*	1.171
	(23.425)	(3.490)	(161.070)	(1,053.907)	(82.649)	(0.847)
Observations	12,656	12,054	12,656	11,893	12,019	11,480
			Panel (f): No Quota			
Before Cutoff X Post	55.091**	-0.113	393.267**	3,335.975***	182.818**	2.103**
	(25.757)	(3.341)	(169.012)	(1,290.680)	(84.625)	(0.888)
Observations	14,679	14,298	14,679	14,465	14,561	14,501
			Panel (g): Cluster at Second of Application			
Before Cutoff X Post	48.467*	-1.085	374.251**	3,791.061***	183.187**	2.101**
	(26.341)	(2.438)	(160.748)	(1,205.807)	(78.532)	(0.795)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
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, year of the voucher assignment, while "Before Cutoff" is a dummy indicating firms that applied before the 2002nd firm, which corresponds 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 A5 for the sake of comparison. Panel (b) reports the results for estimates of the same equation with the dependent variable in logs (we use an inverse hyperbolic sign transformation). Panel (c) 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. Panel (d) reports results from a specification equivalent to Panel (a) but restricting the sample to firms with positive revenues throughout the observation period (from 2013 up to 2019). C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. Panel (e) replicates estimates from Panel (a) but standard errors are clustered at the second of application level. Effects are reported in thousand of Euro. Standard errors are clustered at firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

Table A6: Firm-level Outcomes: Difference-in -Discontinuity Strategy

Variables	(1) Employment Cost	(2) K/L Ratio	(3) Revenues	(4) VA/Employee	(5) Gross Profits	(6) ROE
Before Cutoff X Post	78.182*	1.799	524.751*	2,979.677	314.786**	2.945*
	(46.263)	(5.141)	(274.618)	(2,187.003)	(151.988)	(1.564)
Observations	16,156	15,737	16,156	15,916	16,031	15,957
Mean Control	895.44	59.41	5440.91	48699.78	2675.03	7.76
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 2002nd 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. C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. Effects are reported in thousand of Euro. All variables are winsorized at 1%. Standard errors are clustered at firm level. Level of Significance: *** 0.01, ** 0.05, * 0.1.

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 economic 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 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, 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 the their digital transformation?
- Did the services only target exports or also imports?
- 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 evolved 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, more than sufficient.
- Did you acquire new customers thanks to the voucher policy?
- Did the customers acquired with the voucher continued to use your services afterwards or did your relationship ended with the first contract?