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

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Abstract

We study the effects of a subsidy program designed to boost small and medium enterprises' export capabilities through 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 difference-in-differences design to compare the performance of firms that nearly got the subsidy with those that barely did not. Eligible firms experienced a large increase in revenues, return on equity, profits and value added per employee, 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 nearly 13%, mainly in full-time and permanent employees. Results of a survey conducted on TEM providers suggest that collaboration between beneficiary and providers persisted after the initial contract and that the availability of other services such as support for digitalization was associated with larger improvements in performance for the beneficiary firms.

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; Bloom and Van Reenen, 2010, 2011; Mion and Opromolla, 2014; Bender et al., 2018; Amador et al., 2018; Caliendo et al., 2020). However, we known much less about how firms can acquire such important assets. This is particularly relevant for small and medium sized enterprises (SMEs), where information frictions, financial frictions, and risk aversion might prevent them from searching and introducing the most effective practices. Indeed, policy-makers of all G20 countries have recently expressed concern about the lack of managerial skills, particularly among SMEs, and there is an ongoing debate on which tools may help firms to acquire such competencies.[1] One possibility would be to go "beyond the boundaries of the firm" and rely on external services such as consulting (Anderson and McKenzie, 2022). However, firms may be reluctant to use this option even when informed about the quality of the consultancy, because of concerns – among others – on the ability to finance the initial costs or uncertainty on the returns; this leaves room for public interventions to support firms in acquiring these inputs and improving their performance (Schivardi and Schmitz, 2020; Anderson and McKenzie, 2021). Despite the relevance of this issue, empirical evidence on how governments can incentivize firms to use consulting services and their effectiveness is scant.

This paper contributes to fill this gap in the literature by studying the effects of government-financed consulting services on SMEs performance using evidence from the *"Voucher for Internalization"*, a policy implemented in Italy in 2016 aimed at improving the exporting capacity of these firms. The program envisaged a 10,000-euro subsidy for the acquisition of consulting services that could be obtained from a list of providers, pre-selected by the Italian Ministry for Economic Development and with certified expertise in support for internationalization. Eligible firms could benefit from a subsidized consultancy through a Temporary Export Manager (TEM), a consultant

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

with expertise in internationalization, who provided support to the firm for at least six months. The subsidy was assigned at the end of 2015 and used during 2016.

To study the impact of TEMs, we collect data on the 4,146 firms that applied to the program. For each firm, we retrieve data on their application to the program from the Italian Ministry for Economic Development and on their balance sheets between 2013 to 2020 from AIDA, the Italian section of Bureau Van Dijk.[2] We then match applicants firms with their exports and imports at the country and product level by using data from the Italian Customs Agency. Finally, we obtain information on firms' workforce size and composition from the Italian Social Security database.

We identify the causal impact of the program by relying on the allocation process of the subsidy: firms applied online for the subsidy, and vouchers were awarded on a first-come first-served basis until the policy budget—nearly 20 million euros—was exhausted. The budget for the policy was much lower than the amounts requested by firms, and the high number of applicants led to the exhaustion of available funds within one minute since the opening of the application. Firms had no *a priori* indication on how many firms applied, and when. Therefore, we use the exact timing of the applications' submission to identify firms close winner and close loser, that is firms that nearly got and nearly missed the subsidy due to small differences in their application timing, and we compare them over time in a difference-in-differences setting. Throughout the paper we focus on firms that applied within a 30 seconds radius around the moment of exhaustion of resources. Our identifying assumption is that firms were allocated as-good-as-randomly on the two sides of the cutoff in this narrow time window, and that firms that applied with a slight delay to the program (comparison group) represent a good counterfactual for firms that applied a few seconds earlier with respect to the cutoff (treatment group). This procedure for TEMs' allocation rules out selection into treatment and allows us to estimate the causal effect of the program. We show that these two groups of firms are statistically indistinguishable in terms of their characteristics and outcomes right before the implementation of the program, and that they are on the same performance trend

[2]We obtain information for all but one of the firms applying to the program.

in the three years before it.

Our analysis delivers four main findings. First, TEMs improved firm imports and exports, particularly in markets outside the EU, which are characterized by higher barriers to entry. The effects materialize after three years since the use of the subsidy, when early applicants to the program increased their import by about 100,000 euros (+15% with respect to 2015) and their export by 200,000 euros (+13%) with respect to firms in the comparison group. We do not observe changes for trade within the EU market. Trade increases 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.

Second, although the policy was aimed at one specific dimension of the firms' activity, consulting had an impact on the overall firm performance as well. We find a positive effect on revenues, labor productivity and profitability, which builds up over time. By 2019, three years after the policy implementation, early applicants have about 600,000 euros more in revenues (+10.5% with respect to 2015), 5,000 euros more in value added per employee (+9.6%) and about 70,000 euros more in net profits (+55%) with respect to firms in the comparison group. In addition, they show more resilience during the first year of the Covid-19 pandemic, by further increasing their positive gap in performance with respect to late applicants, thanks to higher investments in intangible assets. This result is in line with Schivardi et al. (2021), who stress the importance of good management practices in facing the economic turmoil generated by the recent pandemic. We perform a number of checks to test the robustness of our findings by looking at different definitions of the neighborhood of the cutoff, empirical specifications and sample definitions; all these tests support the conclusions of our main analysis. In addition, the positive effects on performance are heterogeneous, with suggestive evidence pointing to greater benefits for smaller and less productive firms. We further observe that the effect of the policy is strongly differentiated across providers of consulting services. While some providers led to strong divergence in performance for treated firms with respect to the comparison group, others determined only minor deviations. It

should, however, be noted that firms were free in their choice of provider, which makes the matching between the firm and the consultancy provider endogenous. In this sense, this result should be considered suggestive rather than causal.

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

Finally, we conduct a qualitative survey with semi-structured interviews among TEM providers to shed light on the mechanism through which these effects unfold. We contacted 38 consulting firms that provided their services to 608 beneficiary firms, about 38% of the firms using the voucher. We find that the initial subsidy encouraged firms to establish a lasting relationship with providers, with 31 out of the 38 interviewed providers reporting that they had additional contracts with the beneficiary firms in addition to the one subsidized by the voucher. Moreover, our interviews unveil that consulting firms provided services beyond support for internationalization. In particular, the ability of consulting firms to provide services aimed at improving product commercialization and digitalization of their clients was associated with larger effects on the balance sheets of firms using the voucher.

Our research mainly contributes to two strands of the literature. First, it expands our knowledge on the role of management consulting services. Previous works, using RCTs, show that management consulting has large and positive effects on firm size,

productivity and profitability (Bloom et al., 2013; Bruhn et al., 2018). Iacovone et al. (2022) allocate randomly group-based and individual consulting and find that the former is more cost-effective. In the same spirit, Anderson and McKenzie (2022) compare the effectiveness of insourcing, outsourcing, consultancy and training of entrepreneurs for financial and marketing services. While insourcing, outsourcing, and consultancy appear to have similar effects and have a stronger impact than training, outsourcing and insourcing dominate in cost-benefit terms. In a historical perspective, Bianchi and Giorcelli (2022) and Giorcelli (2019) show that the effects of management training on firm outcomes are persistent and create positive spillovers in the supply chain.

Our contribution to this literature is fourfold. First, we exploit a policy that allowed firms to acquire consulting services with limited constraints on the type of services they could require. This provides useful guidance to policy-makers in helping them impart these services to firms and informs them on the effects for firms obtaining these services in a market environment. Second, unlike previous contributions, our study focuses on a high-income country, which shows substantial similarities to many other modern economies. Although consulting services are ubiquitous in such contexts and potentially much needed among SMEs, to date there is very limited evidence on how effective they are in advanced economies. Third, our analysis benefits from a larger sample with respect to previous studies, with about 1,600 firms receiving the treatment. Finally, our work focuses on a specific type of consultancy and its impact on trade, an aspect that has been neglected to date by the literature on consulting services. Only two recent studies, to the best of our knowledge, investigated the importance of management for trade: Bloom et al. (2021) assess how better management practices lead to a stronger performance in the export market in the US and China while Mion et al. (2017) show that management-specific market knowledge facilitates export to a certain location by exploiting managers' mobility. Neither of them, however, looks at the possible role of consultancy for firms to acquire specific skills and competencies that might promote their trade activity.

Secondly, our work relates to the literature on trade policies.[3] Srhoj et al. (2020) summarize the impact of export boosting policies across 26 countries, showing the strong heterogeneity in their structure and effectiveness. Previous studies analyze the impact of monetary support for exporting firms in terms of subsidies (Defever et al., 2020), credit guarantees (Felbermayr and Yalcin, 2013), and grant support (Görg et al., 2008). Munch and Schaur (2018), instead, find that export promotion leads to improvement in the outcome level of firms.

We contribute to this literature by showing the impact of consultancy services for trade. Our results entail important implications for the design of internationalization policies. Traditional trade policies have focused on tariffs or export subsidies, but financial frictions pose additional barriers to export by limiting the ability to defray fixed costs of entry in foreign markets. These constraints are particularly significant for intangible assets, which are considerably uncertain and feature information asymmetries and sunkenness (Haskel and Westlake, 2017). Our findings show that a small subsidy for the acquisition of a consultancy can boost firm internationalization both by enabling firms to get more inputs from abroad through imports and by increasing their revenues through exports.

Overall, our analysis demonstrates that consulting services can boost trade activities, and that their impact can spillover into many other dimensions of the firm. Importantly, we find that a relatively small in-kind incentive for SMEs can significantly stimulate export and firms' growth in multiple scopes. This is relevant for the debate on how to design effective 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.

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 favor of our identifying assumptions. Section 4 discusses the results

[3] Demidova and Rodríguez-Clare (2009) and Ding (2021) provide a useful review.

of our analysis and presents robustness tests for their validation. Section 5 concludes by discussing the quantitative implications of our findings, arguing that the policy may have induced firms to invest more in improving managerial skills to boost their growth.

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 their growth and employment capacity by subsidizing the acquisition of consulting services for trade.[4] These services could be acquired from a list of companies compiled by the Ministry of Economic Development (MISE). Firms offering consulting services need to have a consolidated experience in trade activities and knowledge of foreign languages. The program was targeted on SMEs with revenues above 500,000 euros in at least one of the three years before the application and innovative start-up, that is, firms which had been active for less than two years and with "production and commercialization of innovative goods or services with high technology content" as main activity (D.M. 15/05/2015 and d.l. n. 179 18/10/2012).

To receive the voucher, firms were required to apply through the MISE website and subsidies were assigned on a first-come, first-served basis after a preliminary assessment of the eligibility criteria carried out by the Ministry. Firms being awarded the voucher could hire a TEM for consulting services for a minimum of 6 months up to a maximum of 12 months. The primary role of this type of consultant was to assist the firm in studying target foreign markets and designing strategies to start or intensify export activities. With the support of TEMs, the policy aimed at providing firms with useful

[4]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.

managerial skills and expertise, e.g. knowledge of foreign markets, which have been proven to be a key asset for firm internationalization (Mion et al., 2017).

The first wave of the policy took place in 2015 and assigned a total budget of 19 million euros (38 million euros were allocated to the policy in its second wave in 2018). We restrict our analysis to the first edition of the voucher in which no information was available on how quickly resources would have been exhausted.[5] Participating firms received a subsidy amounting to 10,000 euros, with a minimum additional contribution from the firm of 3,000 euros. Thus, the total minimum value of the consultancy was 13,000 euros.

The 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, from 10:00 a.m. September 22^{nd} up to 5.00 p.m. 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 minute since the start of the application period. In addition, there were quotas for firms participating in special promotional events ("roadshows") and for those with legality ratings, i.e. a certificate indicating the firms' compliance with existing regulations and best practices.[6] Third, the Ministry checked the applications to verify their contents. Firms not complying with requirements were excluded, and some renounced the subsidy ex post. These firms were replaced with new ones 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 admitted to

[5] This also gives us a sufficiently long time horizon to study our outcomes of interest.
[6] These were issued by the Authority for Competition and Market after inspections of the firms.

the program. Then, 95 applications were excluded because they contained inconsistent information or did not comply with the conditions of the policy, while 32 applicants withdrew. Of the remaining 1,631 firms, 20 did not complete the procedures to receive the subsidy. In the end, 1,611 firms were assigned voucher. As far as the quota is concerned, about 260 firms participated in roadshows, and 110 provided a legality rating. Among them, a total of 226 were selected for the policy.[7]

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.[8] The duration of the contracts ranged between 6 and 12 months, with more than 50% of the firms establishing contracts of exactly 6 months. Preliminary information on the subject of consultancy[9] reveals that firms requested a variety of services from TEMs. Most were interested in attracting additional clients and contracts (46%) or in conducting market research activities (34%). The remaining firms requested other services ranging from legal consultancy on international markets (2.7%) to logistics and customs duty support (0.6%). In about 10% of the cases, the precise nature of the contract was not specified.

2.2 Data

We collected data from multiple administrative sources and built a unique employer-employee administrative dataset that covers the years between 2013 and 2019. We rely on four main data sources: data on the policy implementation, including a list of applicants and the assignment of vouchers from the MISE; firms balance sheets from AIDA Bureau Van Dijk; granular trade data at product-country-firm level provided by the Italian Customs; information on firms workforce from the National Social Security Institute (INPS) data.[10] Below, we provide additional information on each of these sources:

[7] In our analysis, we check for the robustness of our results excluding firms that received the vouchers because of the quotas.

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

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

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

- List of applicant firms (Ministry of Economic Development, MISE). We obtained detailed data on the administrative procedures related to the policy from MISE. The data include the complete list of firms that applied for the subsidy, as well as their administrative identifiers and exact time of application, which is crucial for our identification strategy. The data also report some firm characteristics, such as previous experience in international trade, participation in roadshows, the main sector of activity, and, if available, some information on the consultancy contract, such as the type of service received, the amount invested, and the identifier of the consultancy provider.[11] We used the unique firm administrative identifier to match this information with other data sources.

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

- Trade Data (Customs and Monopolies Agency). We match our set of firms with granular trade data at the country-product level provided by the Italian Customs and Monopolies Agency. Custom data represent an ideal source of information for analyzing firms trade performance since it allows observing each firms transactions both within and outside the European Union. Data are collected at the transaction level and are measured in both total value in euros and quantities in kilograms. Moreover, the data report, for each transaction, information on the type of goods 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

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

to a more detailed analysis of countries and products involved in the international transactions of our firms.

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

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

3 Empirical Strategy

To identify the impact of the TEM on firms' performance, we rely on the mechanism of assignment of the subsidy, which determines a quasi-random assignment of the voucher and allows us to define suitable treatment and comparison 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 second delay in applying. As described in Section 2.1, firms applied via an electronic procedure and applications were processed according to their submission time up to the exhaustion of available funds. Not all firms could access the subsidy, and firms did not know when the resources would run out, so eligibility for firms that applied in a certain time span around the time cutoff is as good as random.[12]

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

We identify the causal impact of the policy by comparing firms which nearly made the cutoff with firms that missed the cutoff by a few seconds in the spirit of Pinotti (2017). In practice, we consider firms around the cutoff time and run a differences-in-differences model in our main analysis.[13]

Our baseline model is the following:

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

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 comparison group in the periods right before and after the voucher assignment. This specification allows us to uncover the treatment effect of the policy after having netted out common time effects, and time-invariant firm characteristics. Since some firms were excluded from receiving the subsidy even if they applied before the cutoff time and other firms were deemed eligible even if they applied later in time, our treatment variable, $\mathbb{1}(\widetilde{t}_j < 0)$, identifies an Intention-to-Treatment effect (ITT).[14] To limit the influence of outliers, we winsorize our dependent variables at 1% in the main analysis.[15] We cluster standard error at the firm level.[16]

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

[13] We rely on a differences-in-differences 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 main strategy, while standard errors are larger when we estimate our treatment effect with a difference-in-discontinuity model. Estimates are discussed in Section 4.2.6.

[14] Since compliance with the time rule is high, this is quite similar to the ATT. We provide direct evidence in this sense by estimating an IV model where we instrument the actual take-up of the policy with a dummy for having applied before the cutoff.

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

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

of our difference-in-differences model:

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

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

The first step in our empirical analysis is to identify the cutoff time. To this purpose, we plot the share of firms that received the subsidy against the time of submission of their application 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$^{\text{nd}}$ firm.[17]

Then, we test whether firms were able to sort around the cutoff by examining the distribution of applications around the time of exhaustion of the funds allocated to the policy. These tests are reported in Figure 2, in which Panel (a) plots the full distribution while Panel (b) focuses on the neighborhood of the cutoff used in our estimation.

[17] We test for the presence of other discontinuities by running Regression discontinuity models with a dummy equal to one for firms receiving the subsidy as the dependent variable and with the time of application as the running variable. We 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 10 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 the largest, and the only one that 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.

Throughout the analysis, we use a 30-second radius around the cutoff to focus only on firms that received the subsidy at the margin. Results, however, are consistent when we consider larger (40 seconds) or smaller (20 seconds) intervals. Resources were exhausted within one minute from the opening of the application process, and the bulk of firms filed their request approximately within 30 seconds after the opening. The distribution does not show any clear discontinuity at the cutoff, as proven by the McCrary test reported below in Panel (b). This is consistent with the fact that firms could not keep track of other firms' applications and time their submission accordingly.

Finally, we assess the soundness of our empirical framework. First, we check whether firms in our treatment and comparison group are comparable in terms of observable characteristics. We consider many firm dimensions in the three years before the implementation of the policy (2013-2015) and in the year immediately before the application (2015), comparing firms on the two sides of the cutoff time. We report summary statistics for the treated and the comparison groups in Table 1.[18] Results support our empirical exercise: in most cases, characteristics between treated and control firms are very close, and there are strong similarities in many important dimensions such as value added per employee and gross profits. Only in a few cases, the differences between the two groups are statistically significant. For instance, in the year of the application treated firms appear to be less profitable (measures in terms of Return of Equity, ROE) than firms in the comparison group. 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 as suggested by Imbens and Rubin (2015), which offers further reassurance about the reliability of our empirical analysis. To sum up, the available evidence shows relevant similarities between early and late applicants and supports our view that the latter represents a suitable counterfactual.

Second, we look at possible pre-trend in our outcomes of interest. Indeed, differences in

[18]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.

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

4 Results

4.1 Trade outcomes

We start by looking at firm internationalization, the primary outcome of the policy. Among the firms within the 30-second radius around the cutoff, many already exported before applying for the policy, with about 70% having positive exports in 2015. Export managers can help firms in different ways, e.g. by identifying new locations for their products or suggesting alternative customers within a country to which the firm was already exporting. However, the additional knowledge about foreign markets might also lead to changes in the inputs the firms choose for their production with greater integration in the global value chains. In addition, the knowledge provided by external consultants might be useful for exploring more distant markets with different regulations.

To investigate these margins, we first focus on measures of export by broad destination

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

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

Panel (c) and Panel (d) investigate the import changes, which follow the same pattern as exports. Imports from countries outside the EU increase after two years since 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. We report results for these two variables in Panel (e) and in Panel (f). Although less precise, results are in line with previous evidence, with total trade growing over time and an increasingly positive effect on the trade balance.

Results on trade performance from a classical difference-in-differences model are reported in Table 3. They confirm previous findings obtained through the event study estimates, even though in most of the coefficients the standard errors appear much larger. 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 with the small subsidy (10,000 euros) the firms received: by the fourth year after the application (2015), firms that were awarded the voucher had 200,000 euros in additional export revenues in countries outside the EU and increased their imports from this group of countries by 100,000 euros with respect to the comparison group.

Then, we decompose our trade outcome to investigate whether trade towards and from particular locations experienced stronger growth than others. We group countries according to their income according to the World Bank 2020 classification and their geographic location and report results in Appendix Table A1. 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, treated firms obtain the largest gains from Europe and Central Asia.[19]

Additionally, we explore several other outcomes to assess how these additional trade flows occur. We report our results in Table A2 in the Appendix. First, we notice that there seems to be no effect at the extensive margin in terms of exports and imports.

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

Indeed, coefficients for linear probability models for the presence of a positive trade flow show a negligible magnitude and are not statistically significant. We observe a small decline in export probability outside the European Union (-2 percentage point in probability and significant at 10%), but this negative discrepancy appears to be very short lived as shown in the event study version of this estimate, which we report below. 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 implementing 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, with more than 80% of the contracts below 14,000 euros in value. Figure A5, Figure A6, and Figure A7 in the Appendix report corresponding event study estimates. Panel (a) of Figure A5 sheds light on the export decline and shows that we register a negative deviation for treated firms only in the year of the policy implementation, while, in the longer run, the two groups remain at the level registered in the year before the policy assignment (-2 percentage point with respect to 2015). No long run impact on the export activity can be detected.

4.2 Balance sheet outcomes

4.2.1 Main Findings

We now turn to the impact of the subsidy on firms' performance measures, and look at costs, revenues, labor productivity and profitability. We start with our simpler difference-in-differences model (Equation 1) and then move on to its dynamic counterpart (Equation 2).

Table 4 reports the results for our main variables of interest. The effects are positive and statistically significant: firms eligible to receive the subsidy increase their total employees

costs (this is matched by an increase in the number of employees as shown in Section 4.3), revenues, value added per employee and profitability (net profits and Return On Equity, ROE). We do not detect changes in their capital/labor ratio. Compared to baseline, the effects range from a minimum increase of 5% for employment costs and revenues from sales, to 30/35% for the ROE indicator and net profits. The gain in labor productivity, proxied by value added per employee, is about 8% 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 consulting firm might be providing more general counseling through TEMs or additional services, thus leading to a better overall performance of the firm. Since we do not have explicit data on the activity of the consultants within the firms, it is not possible to investigate these two mechanisms directly.

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 comparison firms. This supports our identification assumption. The positive effects of the policy build up over time and become more noticeable in the last periods of the analysis (2019 and 2020). In the year of the treatment and over the following two years, the treated firms enhance their performance modestly, while in the last period the improvements appear substantial. Consistently with previous results, it would seem that the policy takes time to fully reveal its positive effects. Nevertheless, some, albeit smaller, effects are detectable also in the short term. Our findings on the build-up of the positive impacts over time are consistent with the long-lasting effects identified by previous research.

It is worth stressing that, during the first year of the pandemic (2020), treated firms further consolidated their better performance with respect to firms in the comparison group, as shown by revenues, labor productivity, net profits and ROE. Thus, treated firms, not only show improving performance but also higher resilience to shocks. More specifically, in the wake of the global pandemic, which limited personal interaction, early applicants to the policy increased more markedly their investment in immaterial assets

(+100,000 euro or +50% with respect to the pre-pandemic level in 2015) as shown in Figure A8 Panel (a),[20] in the Appendix. Investments in material assets, reported in Panel (b), also increased, but the effect is imprecise to lead to definitive conclusions. This further signals that the presence of consultants not only led to improvements in profitability and labor productivity but also in the ability of the firm to act flexibly and weather shocks more effectively.

Overall, these results show that the policy had a positive impact on firms' exports and general performance (revenues, labor productivity 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 these services affect different types of firms. This also highlights possible channels through which these services impact firms' performance.

For this purpose, we include triple interactions (and all relevant double interactions) in our models to test for differential effects across groups. We consider several dimensions of the firms: geographic location, size, labor 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.[21] The table reports the main difference-in-differences coefficient, the relevant

[20] The higher variance in the coefficient for 2020 can be explained by the higher variance of immaterial assets in 2020 with respect to previous years. The average for this outcome increases from 230,000 euro in 2019 to 450,000 euro in 2020 and the standard deviation register an even starker increase, from about 470,000 to 1,131,000 euro. Figures only marginally change if we exclude firms with no immaterial assets. This is not driven by a higher attrition of firms in the first year of the panemic. Indeed, we do not see a change in the attrition of firms in this year with 59 firms becoming inactive between 208 and 2019 and 66 becoming inactive between 2019 and 2020.

[21] We resort to the inverse hyperbolic sine to accommodate for zeros in our estimation.

triple interactions and, at the bottom, the p-value for the sum of the two interactions being equal to zero. Firms in the South,[22] seem to benefit less from the policy, although only in the case of ROE the difference between the two groups is significant at the 10% level. Interestingly, small firms, i.e. those below the median size in the sample (15 employees), and the least productive firms. i.e. those in the bottom half of the value added per employee distribution, accrue larger gains. Triple interaction coefficients are generally large, but for the most part imprecisely estimated. However, the impact for this group (obtained by summing up the main coefficient and the triple interaction) is often 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 as high as eight times larger for the least productive firms (effect on revenues, in Column 11) compared to other treated firms. Finally, it appears that the impact is smaller for already exporting firms.

These results provide additional suggestive evidence on the impact of the policy. Such a moderate intervention generates, in many cases, only small effects while the benefits seem to be extremely sizable for firms characterized by high levels of inefficiency.

4.2.3 The effect of heterogeneous TEMs

While some consultancy firms may only provide firms with contacts of potential foreign customers and general support to export, others may also advise firms on other aspects of theirs activities, which, in turn, may generate larger benefits. This section summarizes the main outcomes of several analyses on the role of the services supplied by consulting firms. These results should be interpreted cautiously since they may reflect the characteristics of the provider and of the beneficiary firm. Indeed, the matching between beneficiary firms and the provider of consulting services was left to the firm's choice according to the policy design, and it is endogenous.

We start by studying the heterogeneity in the effects across TEM providers. Since many

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

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 associated with very 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, with a response rate of nearly 27%. 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, the type of services provided by the consulting firms, 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 was followed by further consultancies paid for by the firm. This result is

consistent with the possibility that the initial voucher encouraged firms to start acquiring consulting services by themselves. Therefore, the effects discussed in Section 4 may be the result of a longer consultancy period than the one initially supported by the Government.

The additional interest in these services by firms benefiting from the voucher in the first wave of the policy can further be assessed from the participation in the second wave of the Voucher (2017). We obtained data from MISE on applicants to the second wave and matched them with firms applying in the first wave of the voucher. On average, quite a few firms in our application window (radius of 30 seconds around the cutoff) applied also to the second wave. However, firms that applied before the time of exhaustion of resources were more likely to apply again for the voucher (+7.5 percentage points out of a baseline of 27%), and firms which used the voucher showed an even starker difference in the application rate (+18 percentage points out of a baseline of 21 percentage points for firms not using the voucher in the first wave). It should be noted that this difference is not related to mechanical effects since being assigned the voucher in the first wave did not imply any advantage in the assignment for the following waves.[23]

Then, we asked the TEM suppliers what type of services had been provided to the beneficiary firms. These open-ended answers were then categorized into four non-mutually exclusive groups: commercialization (including assistance in identifying potential customers or suppliers abroad and marketing advice), production (including suggestions on how to restructure the productive process in support of internationalization), logistics, and regulatory advice (related to legal requirements and custom compliance). Figure 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

[23]Participation in the second wave could also mediate part of the effect of the policy we identified in our main estimates. To test for the contribution of the second wave of the voucher for internationalization, we augment our difference-in-differences model with a further term which interacts with a dummy for the period after the second wave (2018-2020) and a dummy for firms receiving the voucher in the second wave. Results for this model are reported in Table A3 in the Appendix and show that, although being a recipient of the voucher in the later wave is associated with even better performance, the impact of the first wave remains positive and highly significant.

(17 and 13 providers, respectively). These answers are interesting since they hinge on an important role in supporting the streamlining of production and the management of inputs and outputs.

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

To study whether the type of assistance available is correlated with the positive effect of the policy on firm performance, we include triple interactions with the various services provided in our models. Also in this case, we re-scale the dependent variable using an inverse-hyperbolic sine transformation to allow comparability between results. Table 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 production support, and higher for those related to TEMs that provide assistance for other services. Digitalization appears more consistently beneficial across outcomes, while commercialization and logistics seem beneficial especially in terms of profitability. However, by restricting the analysis to treated firms linked to interviewed TEMs, the sample size is cut by half and estimates generally lack precision. As discussed, 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.[24]

4.2.5 External validity: comparison with the general firm population

To correctly interpret our results, it is crucial to compare firms applying for the voucher to the general population of firms. This is because the empirical analysis involves a relatively small number of firms that explicitly show their need for consulting services.

We extract data from the universe of limited liability firms in Italy and compare our

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

firms to the potential pool of applicants in 2015 (the year of application for the policy), i.e. firms with revenues above 500,000 euros in one of the three years before the policy. We report the related figures in Appendix Table A4.[25] 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, and pay their employees sligthly more, but have a lower value added per employee and profitability according to the 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), magnify the previously mentioned differences (apart from profits, which are now higher among applying firms) and suggest that firms applying for the policy were significantly different from the average population of Italian firm in the same revenue and size category. However, differences in some of these dimensions could be related to the larger size of the applying firms (employing 24 employees with respect to 17 employees for non-applying firms, about 40% more) 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 Appendix Table A5. When considering these dimensions, it appears that applying firms are in general worse than the overall population: they pay less their employees, they have lower revenues, labor productivity, profits and ROE. Thus, once applying firms are compared to their peers in terms of size and sectors, they are lagging behind them. The perception of this gap could lead firms to apply for public subsidies for additional support to their activities through consultancy.

Therefore, it would seem that our results concern a group of firms that are larger than other firms in the economy but, at the same time, underperform in several dimensions once the size of their workforce is taken into account. Hence, they appear to have room for improvement and might benefit from external inputs promoting firm reorganization.

[25] We only consider firms with more than 1,000 euros in employment costs.

4.2.6 Robustness

We perform several robustness checks to validate our results and report them in Table A6. After presenting the main estimates in Panel (a) for comparison, we explore if the results hold by using non-winsorized data (Panel b), removing the zeroes for the periods after the policy in which the firm is no longer active (Panel c), or using a logarithmic rather than a linear specification (Panel d). Then, we investigate directly 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 e). In addition, we restrict the sample to firms active from 2013 to 2020 to deal with possible selective attrition (panel f),[26] and to firms that are not part of any quota category for the subsidy assignment due to legality rating or participation in roadshows (Panel g). We also consider possible differential trends between treated and comparison firms by first netting our a linear trend based on years before the policy intervention (Panel h) and then by including in our regression interactions between the levels of our variables in the baseline year (2015) and year fixed effects (Panel i). Finally, we assess the robustness of our inference by clustering at the second of application rather than at the firm level (Panel l). Estimates are largely in line with our main specification, with some small variations.

Results with the non-winsorized data are consistent with the main estimates with some larger coefficients and lower precision. Excluding zeroes leads to some smaller effects, which are, however, still significant at the classical level. The logarithmic transformation also provides results which are qualitatively consistent and sizable in magnitude, but coefficients for the baseline difference-in-differences tend to be less significant. Coefficients for the IV strategy are larger but reasonably close to the main ones, as could be expected considering the high compliance rate (about 80% of firms applying before the cutoff receive the subsidy). Restricting the sample to firms which are active from 2013 to 2020 leads to small variations in the coefficients. To provide

[26] We also assess the voucher's impact on firm's probability of survival of firms over the sample period. Figure A9 shows that treated firms do not differ in the probability of survival over our time horizon. We define a firm "active" in a specific year if it reports positive employment costs.

more direct evidence of 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 A10. The effects show strong similarities with the main sample, with highly statistically significant effects especially towards the end of the sample. The exclusion of firms that obtained the subsidy through the quota mechanism strengthens the results, while considering various trends has a minimal impact on the estimates but for the effect on capital/employment, which is negative and highly significant after netting out trends. Since trends do not seem to be very relevant for most of our results and trend estimates are not significantly different from zero for both treated and comparison firms, we keep our baseline estimation as our reference. Finally, changing the clustering level 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 test the robustness of our results by defining a 20-second radius and a 40-second radius around the cutoff and by running our main regressions with these alternative restrictions. All of these results, reported in Appendix Figures A11 and A12, are remarkably similar to the main estimates.

Finally, since the average effect across firms in a $\pm$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 applying later in some unobserved dimensions not captured by our previous tests. To focus more closely on the timing of the application and more directly exploit the change in the probability of receiving the voucher for a slightly earlier submission, we rely on a difference-in-discontinuities model. 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:

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

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

Also in this case, the results, reported in Table A7, are similar to the main ones but less precise as expected. Indeed, the coefficient β_3 is now estimated by exploiting a much smaller part of the variation in the data. Still, 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 without the voucher. To test this hypothesis, we assess whether the timing of the application is related to the impact of the policy. We split the treatment group based on firms' time of application in ten-second bins and then estimate our difference-in-differences model. We report coefficients in Figure A13 in the Appendix, 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 application is related to unobservable factors of the firms that may have increased their inherent potential for growth in the period after the policy implementation.

4.3 Employment and workforce composition

Firms appear to benefit from the policy both in the short term and, more distinctly, in the long term. These benefits are clear cut in several outcomes such as revenues,

labor productivity, profitability and trade. We next assess to what extent these gains translated into higher labor demand.

For this purpose, we exploit the more granular INPS data available monthly to shed further light on the timing of the effects. 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 a direct consequence of higher demand.

As in previous sections, we begin from our baseline difference-in-differences model. Table 8 presents these results for the overall number of workers and subgroups. Specifically, we first 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): 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). By doing so, we can understand which kinds of jobs are created and 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 or organizational activities with white-collar workers) and the quality of employment for workers.

We report the results in Table 8. Early applicants firms appear to increase their workforce by about 2.9 additional employees, although this effect is not precisely estimated. Then, we find that the TEM subsidy determined a strong increase in the number of stable jobs (Column 2) with an additional 1.2 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, 2.2 additional workers

with full-time contracts after the voucher assignment compared to the years before, as opposed to firms in the comparison 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 also this effect is imprecise. The effect on white-collar jobs (Column 5) is smaller (1.1 workers) but significant at a 10% level. Other kinds of jobs, such as managers and apprentices (Column 6 and Column 7), register only minimal changes. 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 2.1 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. To shed further light on this important issue, we run a more detailed specification of model 2, in which the time index t now represents months. We use September 2015 as the reference period. Following the same structure as Table 8, we graphically present 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 builds up 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) show the impact on permanent and full-time employees, respectively. For both categories, we find a significant growth in the number of employees with these types of contracts. By September 2019, early applicants increase the number of workers with permanent contracts by 2 and the number of workers with full-time contracts by 2.5, which corresponds to a large portion of the overall effect.

The decomposition of the effects across workers' qualifications 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 we

focus on narrower job categories. Also, these figures show small differences in employment before the policy, which are, however, never statistically significant at the 5% level. Nevertheless, we observe a steady increase in white-collar employees, which grows on average to a significant magnitude of one worker two years after the TEM assignment. We also observe steady but more modest growth in the number of apprentices, reaching a significant value of 0.25 additional workers after four years. The effect on blue-collar employees increases as well, but coefficients are never statistically significant at the 5% level. We do not find any impact on the number of managers.

We now move to 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 on 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 slightly biased in favor of men.

Finally, in Figure 8 we explore the effects on the age distribution of firms' workforce, assuming that age constitutes 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 (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 firms' internationalization through Temporary Export Managers. We identify the causal effect of the policy by exploiting the timing of application and the subsidy allocation based on a first-come, first-served rule. In a difference-in-differences framework we compare firms that barely received the subsidies with those that did not, due to very small differences in the time of application.

We find that the policy effectively boost firms' internationalization and in improving firms' performance under multiple dimensions. Our results confirm that managerial capabilities play a major role in positively affecting firms' ability to improve their performance. Importantly, we show that it takes time—up to four years since the application to the policy—for exports and imports to increase significantly. Before that, firms undertake changes that lead to an expansion of their workforce, a higher production level, and improvements in their revenues and labor productivity. More precisely, we find that eligible firms increase exports by an additional 200,000 euros to countries outside the European Union and imports by 100,000 euros by the fourth year after the assignment of the subsidy compared to applicants that did not receive the voucher. In addition, they experience revenue growth, value added per employee and net profits by, respectively, 6000,000 euros, 5,000 euros per employee and 70,000 euros. Finally, we observe an increase in the firm size of about 4 employees, corresponding to approximately 17% of the number of employees in the baseline year.

Treatment effects and cost-effectiveness vary across the characteristics of the applicant firms. Vouchers to less productive and smaller firms generate larger impacts. The timing of the effects on firms' outcome, workforce and trade is heterogeneous: firms first improve their performance and grow in size before increasing their exports and imports. This dynamic suggests that a larger workforce is essential 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 six-months consultancy (worth about 13,000 euros in most cases). 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 marked the beginning of a longer relationship with the consultants. Indeed, results from a survey we have administered to TEM providers suggest that most firms that benefited from the initial consultancy continued to invest in management skills and firm organization. The presence of additional services in particular related to commercialization and digitalization is associated with larger positive effects on firms' performance. Therefore, the policy mainly provided a *nudge* to undertake additional trust and investments in management capabilities.

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 TEM providers have very different impacts on firms. A better understanding of which elements contribute to making consultancy and TEMs successful in improving firm performance remains a critical area for future research.

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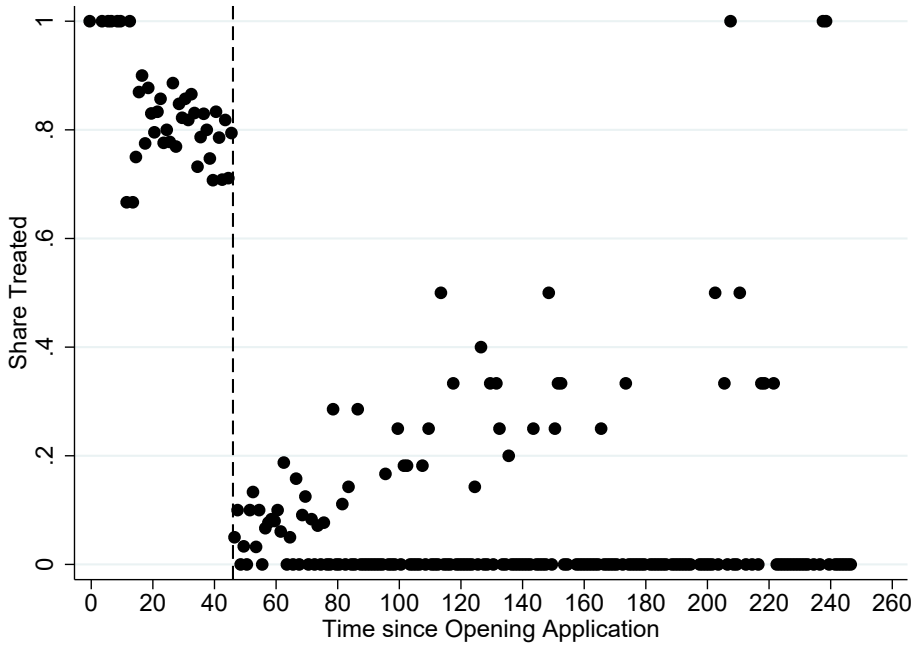
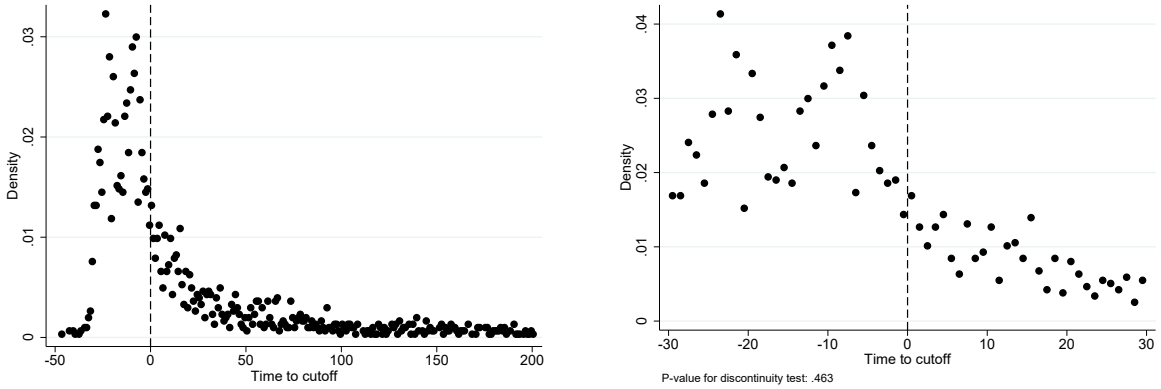
Figures

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

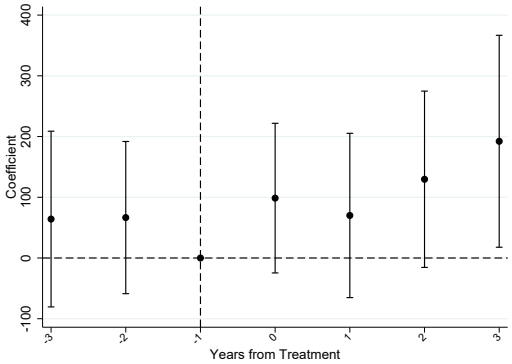


(a) Full distribution

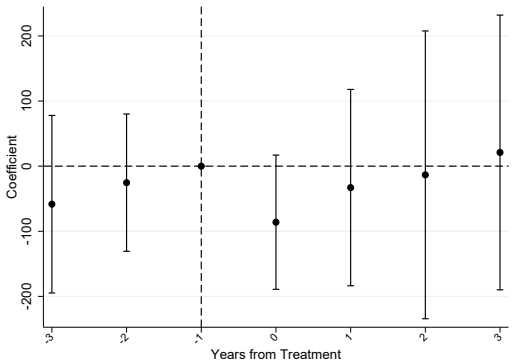
(b) 30 Seconds radius from cutoff

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.

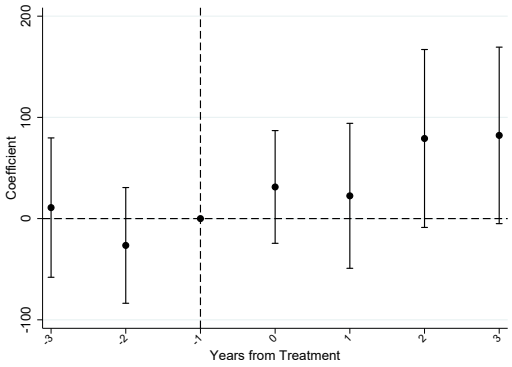
Figure 3: Effect of Subsidy Assignment on Firm Internationalization Over Time



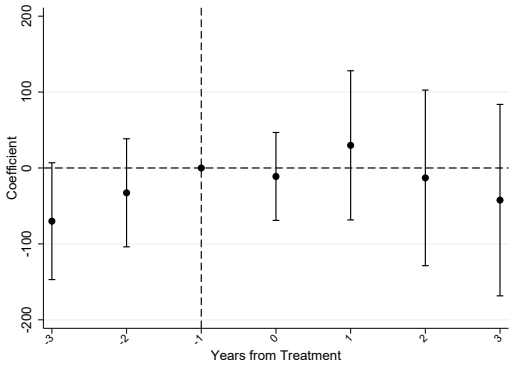
(a) Value of Export (Extra EU)



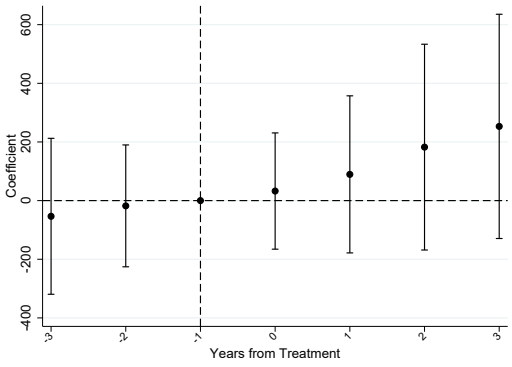
(b) Value of Export (Intra EU)



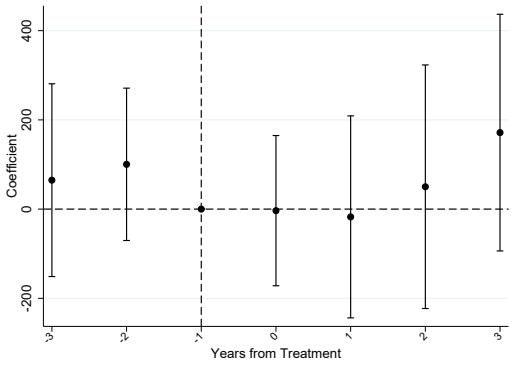
(c) Value of Import (Extra EU)



(d) Value of Import (Intra EU)



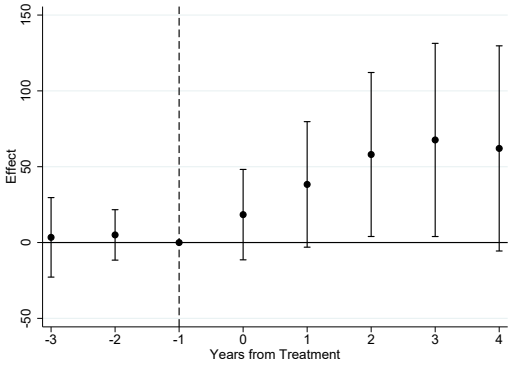
(e) Total Trade (IMP+EXP)



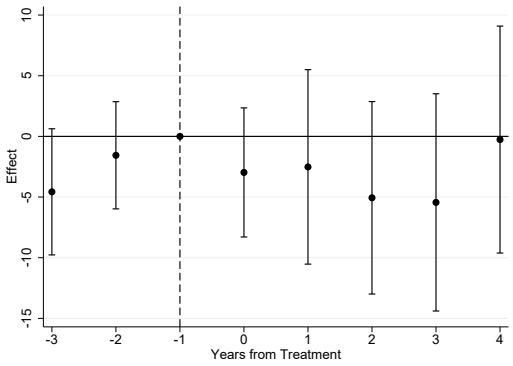
(f) Total Trade Balance (EXP-IMP)

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. All effects are reported in thousands of euros. Standard errors are clustered at the firm level.

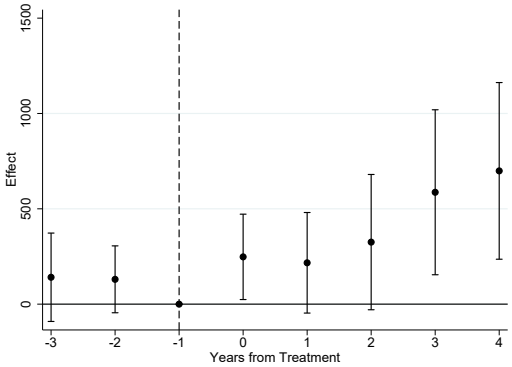
Figure 4: Effect of TEM Voucher Assignment on Firm Outcomes Over Time



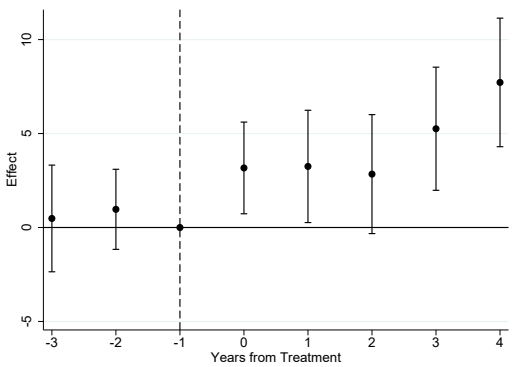
(a) Total Employment Costs



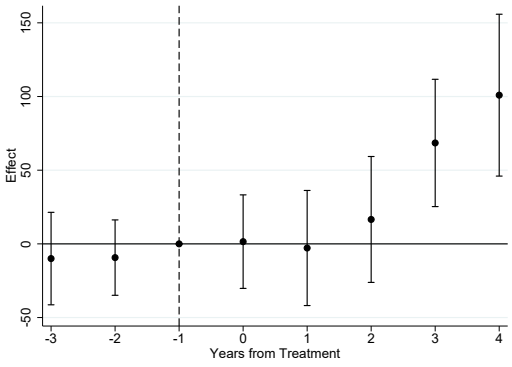
(b) Capital/labor Ratio



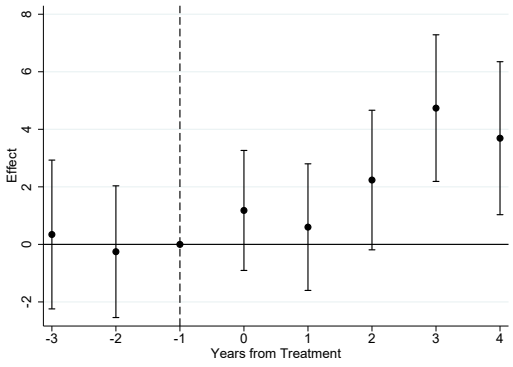
(c) Revenue from Sales



(d) Value Added per Employee



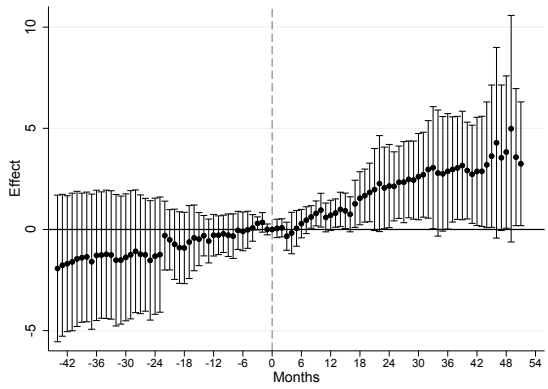
(e) Net Profits



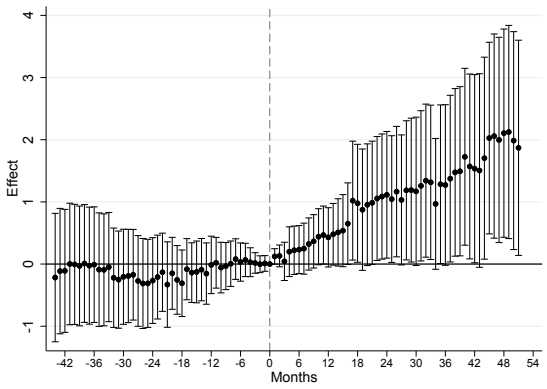
(f) Return on Equity

Notes: This figure reports the results of a difference-in-differences model estimated between 2013 and 2020. 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.All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points. 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.

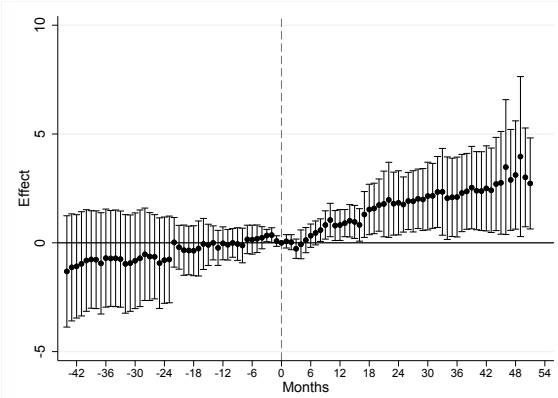
Figure 5: Effect of TEM Voucher Assignment on Firm labor Demand Over Time



(a) Total Employees



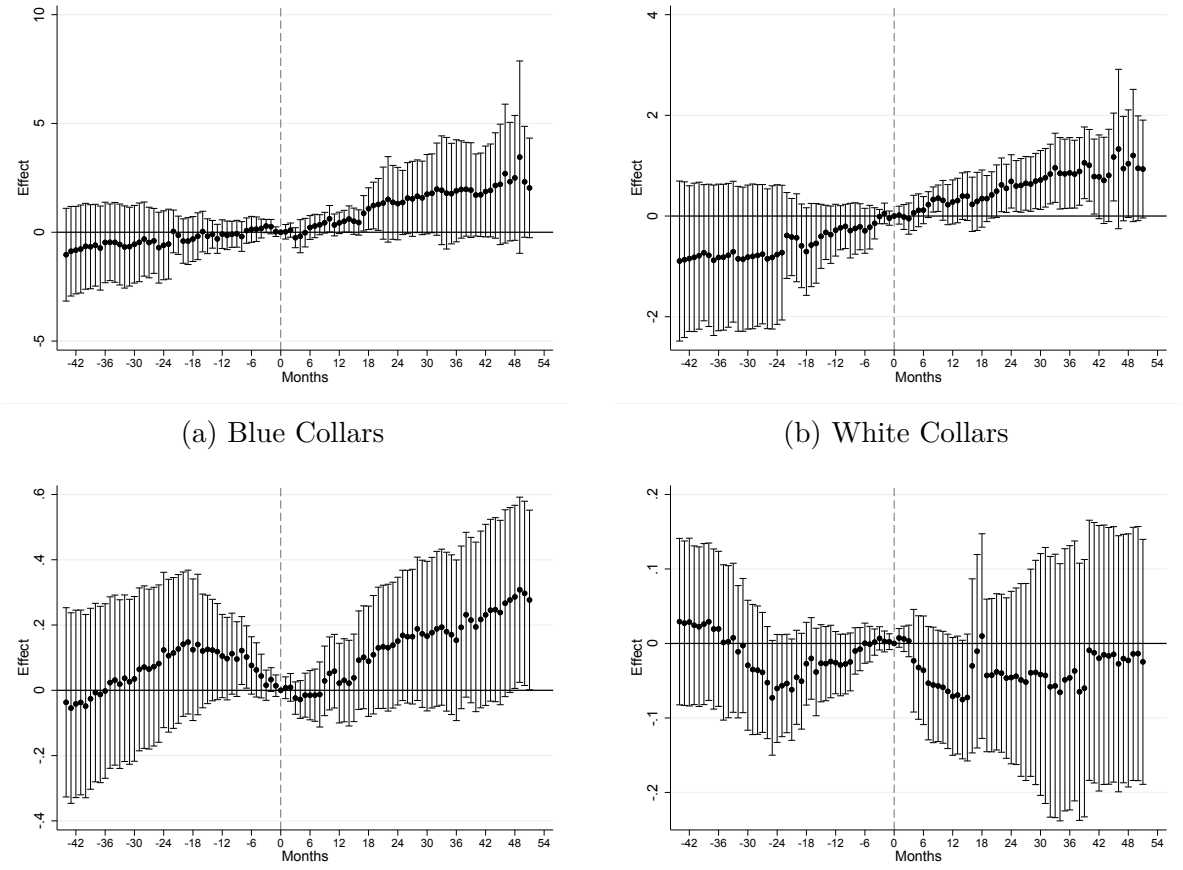
(b) Permanent Employees



(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

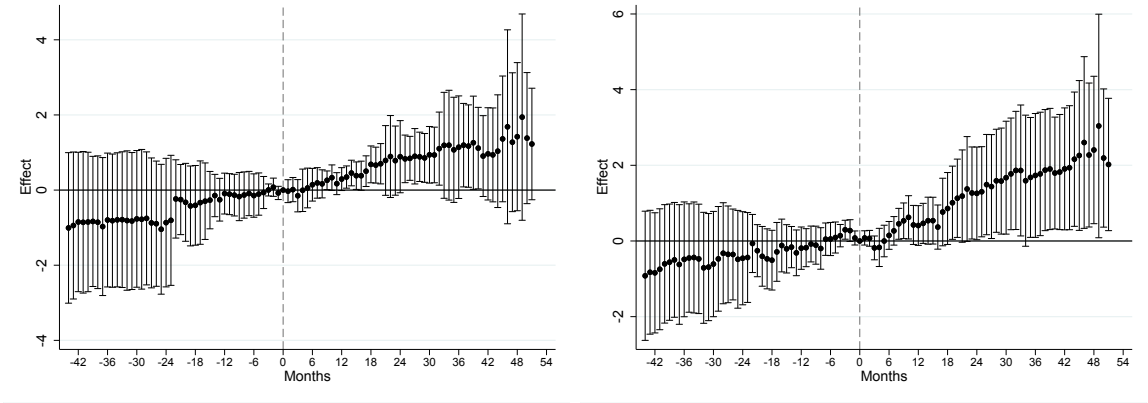
(b) White Collars

(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.

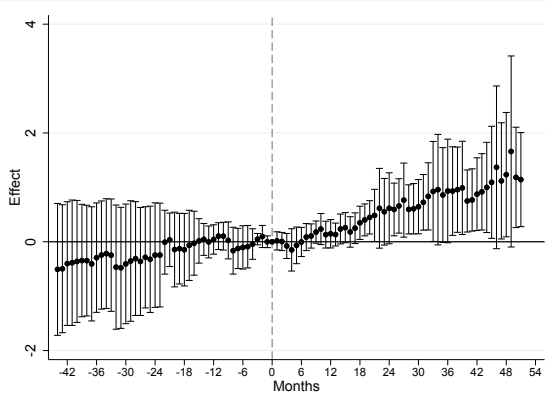
Figure 7: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Gender



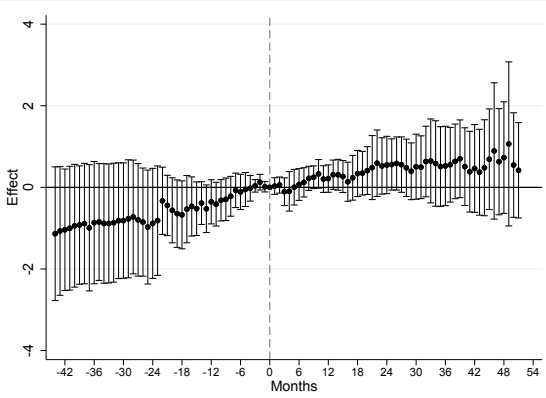
(a) Female Employees (b) Male Employees

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.

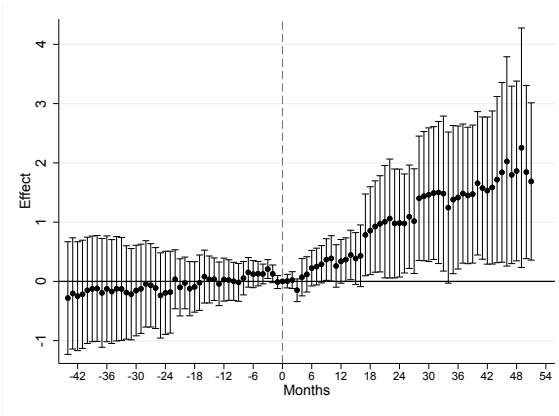
Figure 8: Effect of TEM Voucher Assignment on Firm labor Demand Over Time: Age Group



(a) Junior Employees



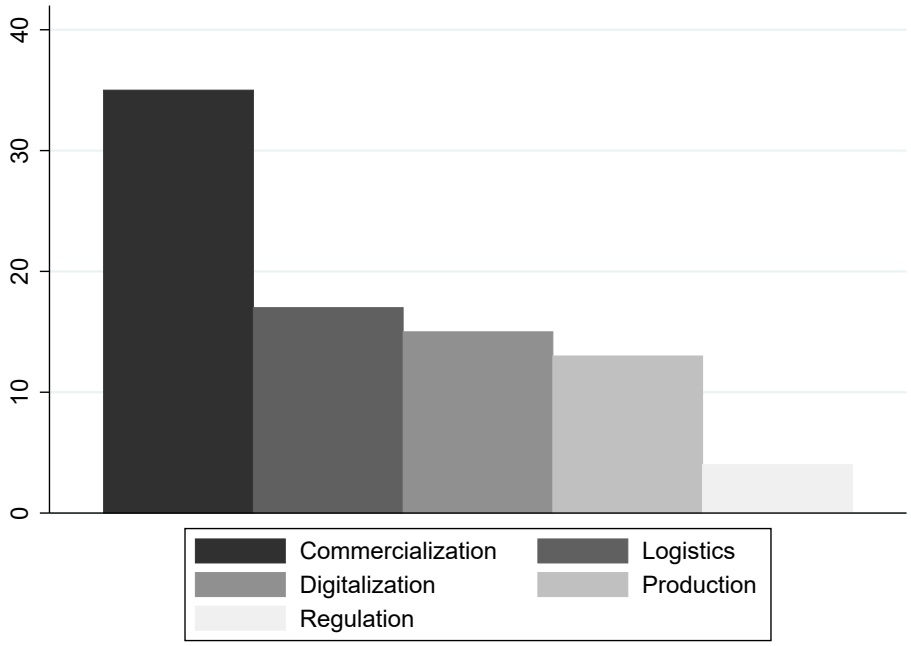
(b) Mid-Level Employees



(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 (1)-(2)	(4) Relative Difference (3)/(2)	(5) T-Stat
		Panel (a): Over three years before the policy (2013-2015)			
Export Extra EU	1381.097	1176.557	204.54	0.173	1.329
Export Intra EU	1911.445	1668.606	242.838	0.145	1.108
Import Extra EU	503.034	472.883	30.151	0.063	0.368
Import Intra EU	694.041	583.215	110.825	0.19	1.165
Total Trade	8078.436	7167.781	910.654	0.127	1.121
Trade Balance	3742.523	3357.416	385.107	0.114	0.691
Total Employment Cost	943.294	850.344	92.949	0.109	1.832
Capital Employment Ratio	75.811	67.341	8.47	0.125	1.705
Revenue from Sales	5491.51	5410.028	81.482	0.015	0.23
Value Added per Employee	54.615	55.524	-0.909	-0.016	-0.69
Net Profits	97.203	99.435	-2.232	-0.022	-0.154
ROE	6.526	8.293	-1.767	-0.213	-2.195
Broadband Conn. (% Buildings)	0.25	0.249	0.001	0.005	0.076
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	0	-0.001	-0.01
Tot. Employees	26.9	23.36	3.54	0.15	1.9
Permanent Employees	23.74	21.64	2.1	0.1	1.43
Full Time Employees	23.92	21.17	2.75	0.13	1.66
Blue Collars	15.07	12.9	2.17	0.17	1.81
White Collars	9.66	8.44	1.22	0.14	1.46
Managers	0.47	0.59	-0.12	-0.2	-0.65
Apprentices	1.34	1.15	0.19	0.17	1.43
Women	8.93	7.13	1.8	0.25	2.24
Men	17.97	16.23	1.74	0.11	1.4
Junior (age 16-29)	3.99	3.11	0.89	0.29	2.34
Mid-level (age 30-45)	12.88	11.27	1.61	0.14	1.64
Senior (age >45)	10.03	8.99	1.04	0.12	1.42
		Panel (b): Year of the policy assignment (2015)			
Export Extra EU	1409.093	1245.399	163.694	0.131	1.009
Export Intra EU	2040.698	1748.033	292.665	0.167	1.256
Import Extra EU	563.191	525.977	37.214	0.071	0.400
Import Intra EU	734.183	589.285	144.898	0.246	1.465
Total Trade	8234.942	7224.419	1010.523	0.140	1.219
Trade Balance	3737.930	3378.446	359.484	0.106	0.633
Total Employment	25.037	23.276	1.761	0.076	1.417
Total Employment Cost	968.518	879.266	89.252	0.102	1.712
Capital Employment Ratio	76.765	66.300	10.465	0.158	2.076
Revenue from Sales	5609.342	5616.144	-6.802	-0.001	-0.019
Value Added per Employee	54.497	55.710	-1.213	-0.022	-0.778
Net Profits	125.157	121.419	3.738	0.031	0.192
ROE	7.927	9.818	-1.891	-0.193	-1.811
Broadband Conn. (% Buildings)	0.251	0.250	0.001	0.005	0.077
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	0.000	-0.001	-0.011
Tot. Employees	28.26	23.66	4.6	0.19	1.8
Permanent Employees	24.11	21.64	2.47	0.11	1.75
Full Time Employees	24.63	21.15	3.48	0.16	1.7
Blue Collars	15.48	12.79	2.69	0.21	1.75
White Collars	10.6	8.85	1.74	0.2	1.55
Managers	0.52	0.59	-0.07	-0.12	-0.43
Apprentices	1.33	1.17	0.16	0.13	1.09
Women	9.57	7.23	2.34	0.32	1.81
Men	18.69	16.42	2.26	0.14	1.57
Junior (age 16-29)	3.97	2.88	1.09	0.38	1.59
Mid-level (age 30-45)	13.02	10.7	2.32	0.22	1.89
Senior (age >45)	11.27	10.08	1.19	0.12	1.39
Number firms	1782	587			

Notes: Summary statistics for treatment and comparison group. Column (3) reports the difference in the average between the two groups and Column (4) reports the 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. Variables for firm and trade outcomes are winsorized at 1%. All variables reported in thousands of euros but Roe, which is reported in percentage points.

Table 2: Comparison of Treated and Control Firms 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	1381.097	1176.557	0.061
Export Intra EU	1911.445	1668.606	0.052
Import Extra EU	503.034	472.883	0.017
Import Intra EU	694.041	583.215	0.053
Total Trade	8078.436	7167.781	0.069
Trade Balance	3742.523	3357.416	0.042
Total Employment Cost	943.294	850.344	0.085
Capital Employment Ratio	75.811	67.341	0.079
Revenue from Sales	5491.51	5410.028	0.011
Value Added per Employee	54.615	55.524	-0.029
Net Profits	97.203	99.435	-0.005
ROE	6.526	8.293	-0.078
Broadband Conn. (% Buildings)	0.25	0.249	0.003
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	0
	Panel (b): Year of the policy assignment (2015)		
Export Extra EU	1409.093	1245.399	0.047
Export Intra EU	2040.698	1748.033	0.059
Import Extra EU	563.191	525.977	0.019
Import Intra EU	734.183	589.285	0.067
Total Trade	8234.942	7224.419	0.075
Trade Balance	3737.930	3378.446	0.039
Total Employment Cost	968.518	879.266	0.079
Capital Employment Ratio	76.765	66.300	0.097
Revenue from Sales	5609.342	5616.144	-0.001
Value Added per Employee	54.497	55.710	-0.038
Net Profits	125.157	121.419	0.009
ROE	7.927	9.818	-0.085
Broadband Conn. (% Buildings)	0.251	0.250	0.004
Broadband Conn. >100 Mps (% Buildings)	0.193	0.193	-0.001

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

Table 3: Effect of TEM Voucher Assignment on Trade Flows

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

Note: Difference-in-differences regression for firm trade outcomes for years between 2013 and 2019. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 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. Variables are winsorized at 1%. All variables are reported in thousands of euros. Mean control is the average for firms which applied after the time threshold in the period after 2015. Standard errors are clustered at the firm level. Level of significance: *** 0.01, ** 0.05, * 0.1.

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

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

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *before cutoff* is a dummy indicating firms that applied before the 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. All variables are winsorized at 1%. Mean control is the average for firms which applied after the time threshold in the period after 2015. Standard errors are clustered at the firm level. All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points. 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	0.136	-0.030	0.036	0.322*	-0.041	-0.046	-0.006	-0.127	0.155	-0.027	0.033	0.368*
	(0.084)	(0.100)	(0.094)	(0.170)	(0.061)	(0.069)	(0.076)	(0.123)	(0.097)	(0.113)	(0.109)	(0.198)
Before Cutoff X Post X South	-0.099				-0.033				-0.091			
	(0.111)				(0.096)				(0.133)			
Before Cutoff X Post X Small Firm		0.309**				-0.023				0.331*		
		(0.151)				(0.115)				(0.177)		
Before Cutoff X Post X Low Productivity			0.174				-0.092				0.226	
			(0.153)				(0.114)				(0.179)	
Before Cutoff X Post X Exporter				-0.288				0.106				-0.323
				(0.188)				(0.138)				(0.219)
Observations	18,757	18,757	18,757	18,757	18,554	18,554	18,554	18,554	18,757	18,757	18,757	18,757
R-squared	0.733	0.733	0.733	0.733	0.726	0.726	0.726	0.726	0.697	0.697	0.697	0.697
P-value Sum	.707	**.014**	**.080**	.673	.414	.453	.253	.741	.609	**.026**	**.068**	.63
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	VA per employee (log)				Net Profits (log)				Roe (log)			
Before Cutoff X Post	0.178***	0.103	0.162*	0.290**	0.387**	0.342	0.270	0.432*	0.377***	0.268**	0.236**	0.505***
	(0.066)	(0.076)	(0.086)	(0.133)	(0.153)	(0.214)	(0.198)	(0.248)	(0.096)	(0.117)	(0.114)	(0.165)
Before Cutoff X Post X South	-0.007				-0.313				-0.275*			
	(0.109)				(0.264)				(0.165)			
Before Cutoff X Post X Small Firm		0.149				-0.001				0.149		
		(0.128)				(0.283)				(0.180)		
Before Cutoff X Post X Low Productivity			0.034				0.146				0.215	
			(0.127)				(0.283)				(0.180)	
Before Cutoff X Post X Exporter				-0.157				-0.123				-0.232
				(0.150)				(0.302)				(0.196)
Observations	18,514	18,514	18,514	18,514	18,748	18,748	18,748	18,748	18,550	18,550	18,550	18,550
R-squared	0.552	0.552	0.552	0.552	0.515	0.515	0.516	0.515	0.458	0.458	0.459	0.458
P-value Sum	.118	**.014**	**.035**	**.057**	.765	**.066**	**.039**	**.072**	.513	**.002**	**.001**	**.009**
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 half of the VA per employee distribution in 2015. Exporter is a dummy taking value one if the firm was already an exporter (within or outside the European Union) in 2015. The model also includes the interaction between the relevant dummy per column and the post dummy, year and firm fixed effects. P-value sum is the p-value for a F-test assessing whether the sum of the main coefficient (Before CutoffXPost) and of the appropriate triple interaction is different from zero. All variables are winsorized at 1%. Standard errors are clustered at the firm level. 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) Net Profits	(6) ROE
1st Provider X Post	-0.069	-0.054	0.027	0.054	0.435*	0.424***
	(0.133)	(0.105)	(0.152)	(0.114)	(0.251)	(0.147)
2nd Provider X Post	-0.214	-0.255	-0.334	-0.082	-0.131	0.145
	(0.276)	(0.223)	(0.330)	(0.192)	(0.382)	(0.252)
3rd Provider X Post	0.499***	0.359***	0.671***	0.588***	0.272	0.141
	(0.094)	(0.114)	(0.114)	(0.114)	(0.418)	(0.228)
4th Provider X Post	0.023	-0.185	0.085	-0.054	1.249**	0.800**
	(0.250)	(0.209)	(0.318)	(0.209)	(0.546)	(0.337)
5th Provider X Post	0.411*	0.242*	0.340	0.269	0.176	0.273
	(0.233)	(0.146)	(0.237)	(0.174)	(0.386)	(0.291)
6th Provider X Post	0.155	0.089	0.103	0.017	-0.530	0.312
	(0.255)	(0.178)	(0.304)	(0.266)	(0.685)	(0.341)
7th Provider X Post	0.140	0.109	0.345	0.491***	0.561	0.206
	(0.289)	(0.181)	(0.321)	(0.168)	(0.605)	(0.276)
Other Provider X Post	0.137*	-0.065	0.148	0.193***	0.349**	0.336***
	(0.079)	(0.059)	(0.092)	(0.065)	(0.147)	(0.093)
Observations	18,757	18,554	18,757	18,514	18,748	18,550
R-squared	0.733	0.726	0.697	0.552	0.516	0.458
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes by TEM provider between 2013 and 2020. 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) Net Profits	(6) Roe
Before Cutoff X Post	0.202	0.176	0.353	0.314*	-0.184	-0.177
	(0.288)	(0.187)	(0.261)	(0.176)	(0.491)	(0.350)
Before Cutoff X Post X Commercialization	-0.094	-0.260	-0.195	-0.153	0.642	0.587*
	(0.288)	(0.187)	(0.260)	(0.174)	(0.491)	(0.349)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.729	0.679	0.535	0.493	0.452
P-value Sum	.238	.236	.145	**.033**	**.007**	**.000**
Before Cutoff X Post	0.185	-0.035	0.216	0.237***	0.235	0.238*
	(0.117)	(0.090)	(0.137)	(0.089)	(0.210)	(0.141)
Before Cutoff X Post X Logistics	-0.118	-0.055	-0.077	-0.110	0.306	0.226
	(0.128)	(0.100)	(0.147)	(0.096)	(0.231)	(0.150)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	.524	.271	.257	.143	**.007**	**.000**
Before Cutoff X Post	0.102	-0.080	0.152	0.150**	0.409**	0.372***
	(0.094)	(0.072)	(0.109)	(0.076)	(0.173)	(0.108)
Before Cutoff X Post X Regulation	0.175	0.152	0.255	0.279**	0.146	0.025
	(0.173)	(0.142)	(0.214)	(0.138)	(0.359)	(0.293)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	.108	.61	**.058**	**.002**	.122	.177
Before Cutoff X Post	0.087	-0.064	0.153	0.154*	0.529***	0.407***
	(0.099)	(0.075)	(0.115)	(0.081)	(0.185)	(0.115)
Before Cutoff X Post X Production	0.102	-0.019	0.063	0.060	-0.413	-0.124
	(0.144)	(0.116)	(0.163)	(0.105)	(0.253)	(0.164)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	.175	.460	.173	**.038**	.635	**.077**
Before Cutoff X Post	0.060	-0.109	0.045	0.109	0.415*	0.294**
	(0.122)	(0.093)	(0.147)	(0.092)	(0.213)	(0.135)
Before Cutoff X Post X Digitalization	0.092	0.068	0.215	0.104	0.008	0.136
	(0.131)	(0.102)	(0.152)	(0.098)	(0.233)	(0.147)
Observations	9,579	9,498	9,579	9,484	9,575	9,478
R-squared	0.728	0.728	0.679	0.535	0.493	0.452
P-value Sum	.138	.618	**.026**	**.013**	**.033**	**.001**
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. Treated firms are the firms that applied for the voucher before the 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

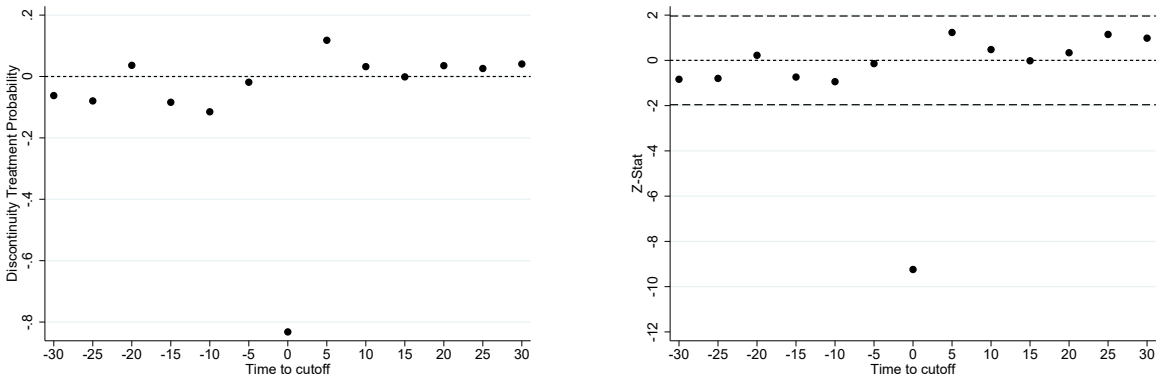
	Overall	Contract Type		Occupation				Demographics				
	Tot. Employees	Permanent	Full Time	Blue Collars	White Collars	Managers	Apprentices	Women	Men	Junior	Mid-Level	Senior
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Before Cutoff × Post	2.900	1.191**	2.192*	1.685	1.127*	-0.027	0.087	1.276	1.624*	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

Appendix Figures

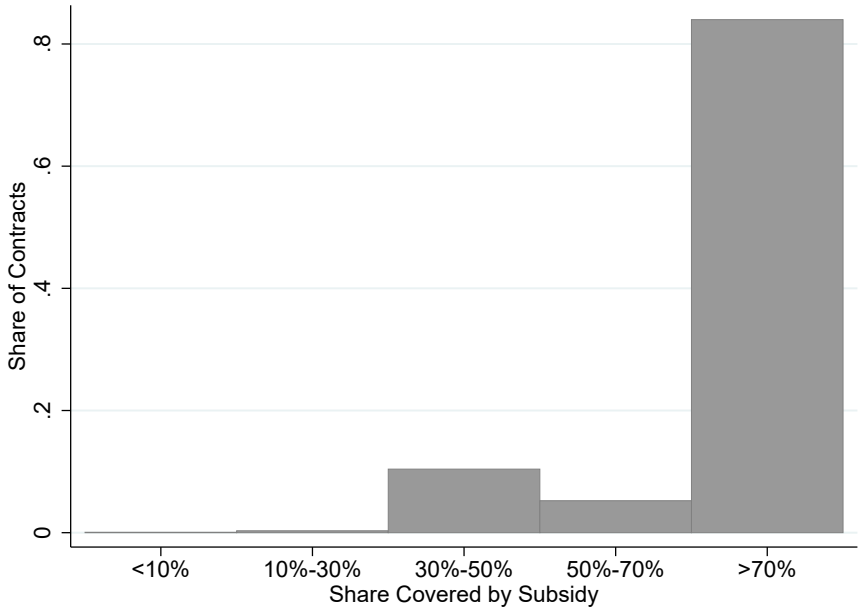
Figure A1: Test for Discontinuity in the Treatment Probability



(a) Coefficient (b) Z-Statistic

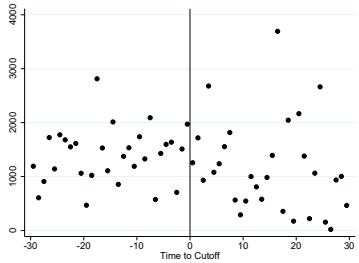
Note: Figure reports coefficients for RDD equations testing for the presence of a discontinuities in the share of firms benefiting from the subsidy by time of application. Panel (a) reports coefficients while Panel (b) reports corresponding z-statistics. Dotted line correspond to thresholds for 5% significance. 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

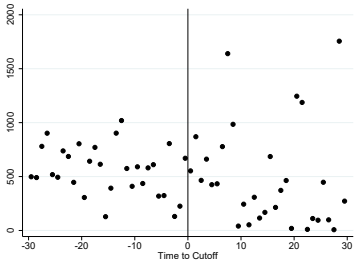


Note: Figure plots the share of contracts by the ratio between the amount of the subsidy (10,000 euros) and the total value in euros of the contract reported to the Ministry of Economic Development.

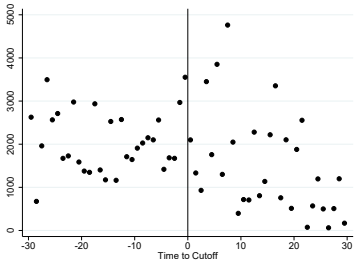
Figure A3: Observable Characteristics for Trade and Firm variables in 2015 by Time of Application



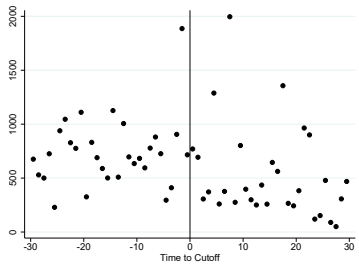
(a) Value Export (Extra EU)



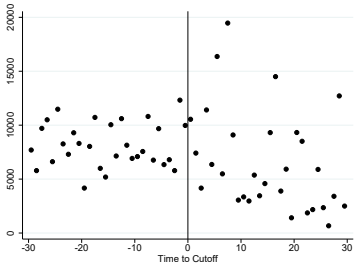
(b) Value Import (Extra EU)



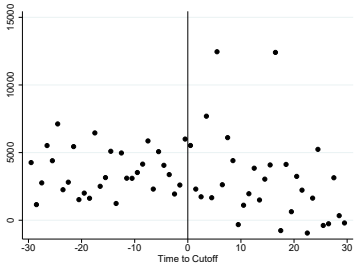
(c) Value Export (Intra EU)



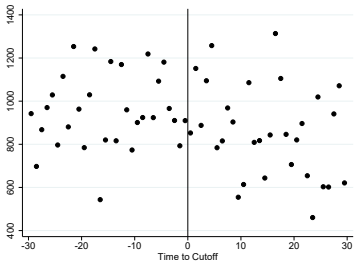
(d) Value Import (Intra EU)



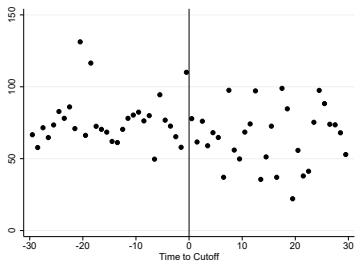
(e) Total Trade



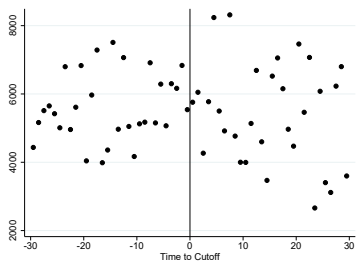
(f) Trade Balance



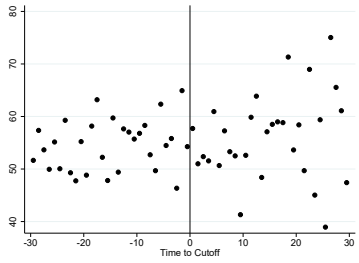
(g) Total Employment Costs



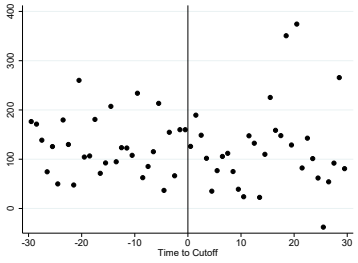
(h) Capital/labor Ratio



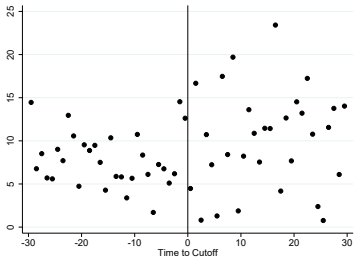
(i) Revenue from Sales



(j) Value Added per Employee



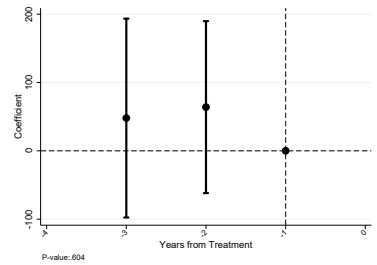
(k) Net Profits



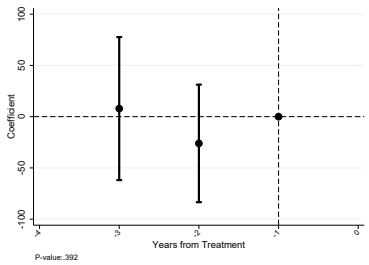
(l) Return on Equity

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

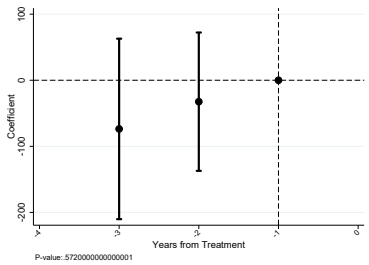
Figure A4: Differences in Trends for Main Variables for Treated and Control Firms in the Periods before the Policy Voucher Assignment.



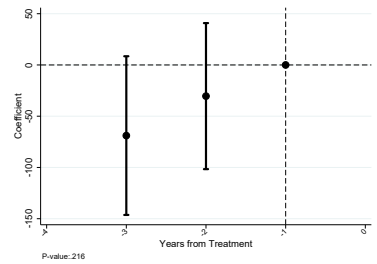
(a) Value Export (Extra EU)



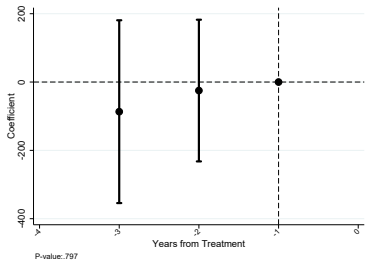
(b) Value Import (Extra EU)



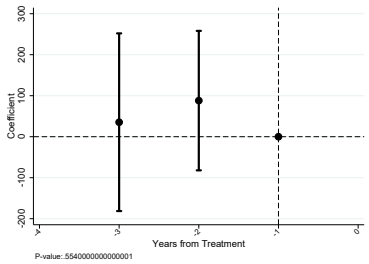
(c) Value Export (Intra EU)



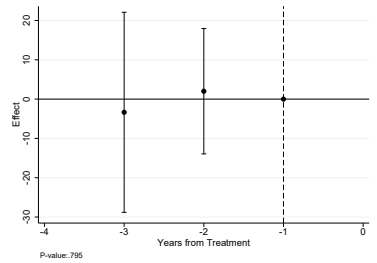
(d) Value Import (Intra EU)



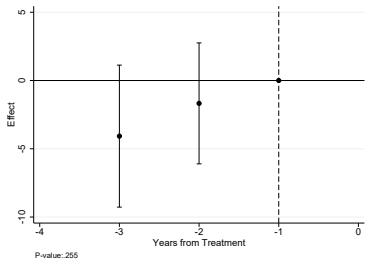
(e) Total Trade



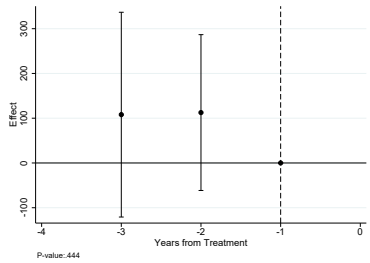
(f) Trade Balance



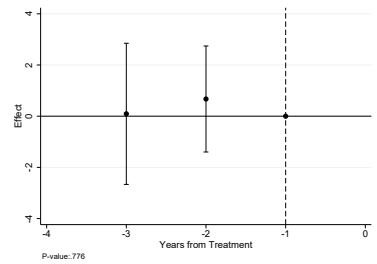
(g) Total Employment Costs



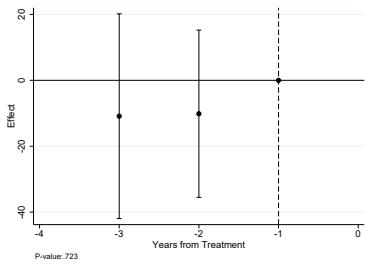
(h) Capital/labor Ratio



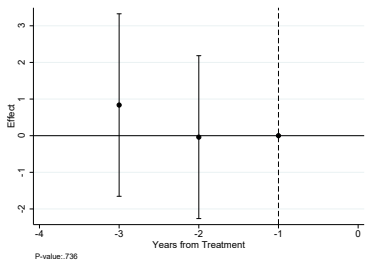
(i) Revenue from Sales



(j) Value Added per Employee



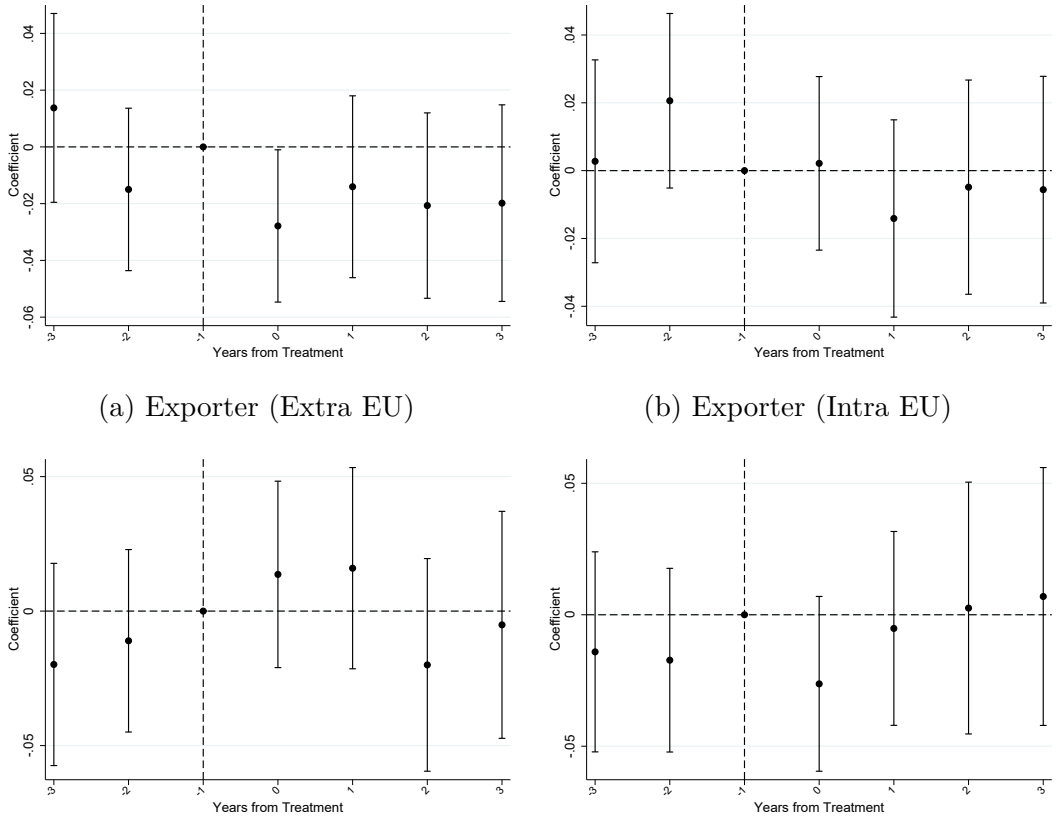
(k) Net Profits



(l) Return on Equity

Notes: This figure reports results from a difference-in-differences model for the periods before the voucher assignment (2013-2015). All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 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. All reported are reported in thousand of euros but for Roe for which the effect is reported in percentage points. Standard errors are clustered at the firm level.

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



(a) Exporter (Extra EU)

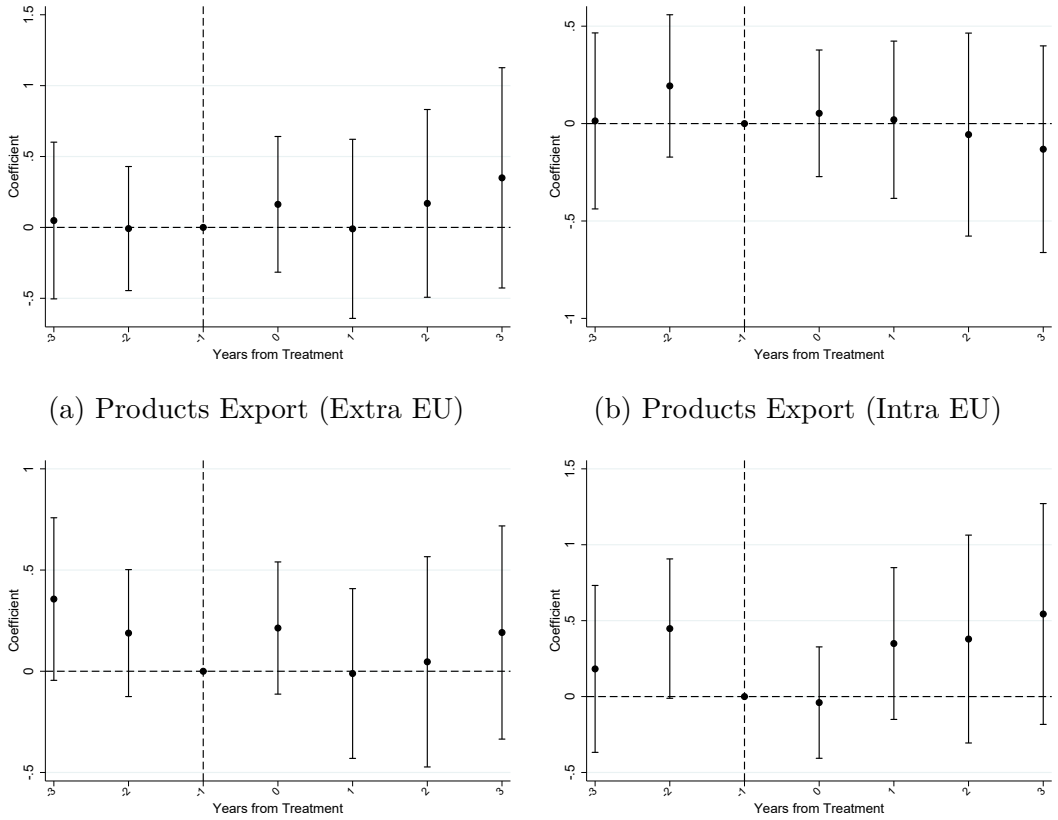
(b) Exporter (Intra EU)

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



(a) Products Export (Extra EU)

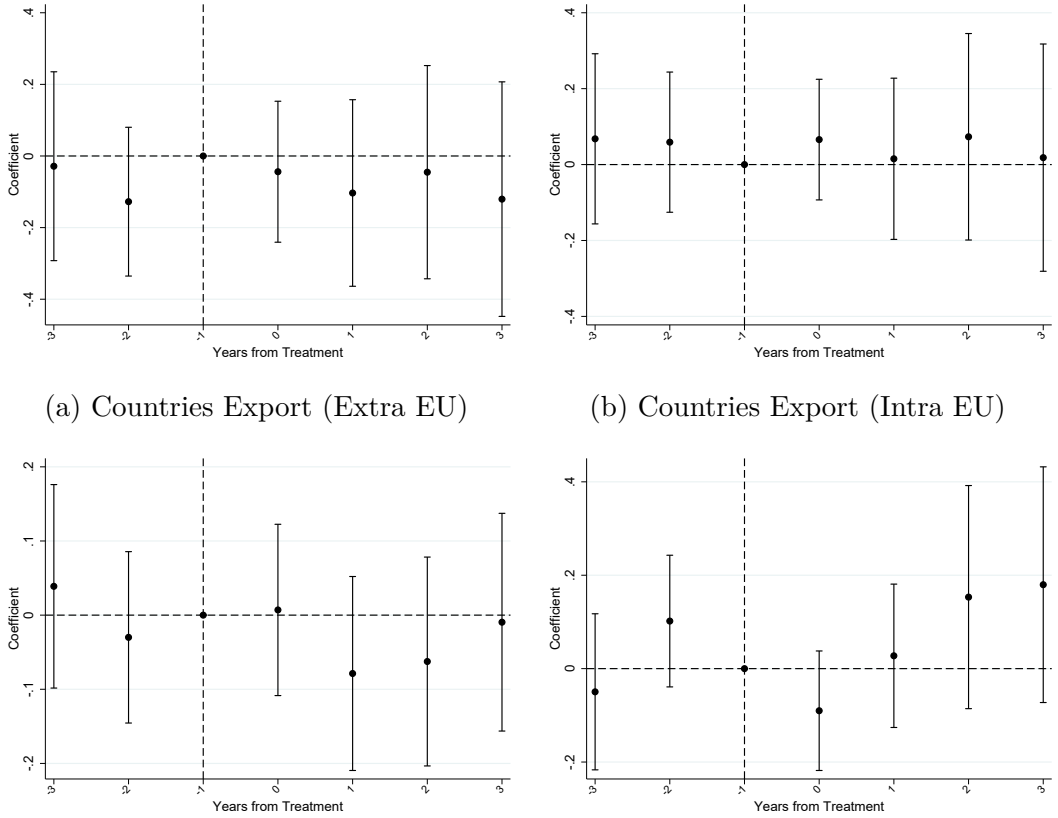
(b) Products Export (Intra EU)

(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 Number of Countries for Export and Import by Broad Destination



(a) Countries Export (Extra EU)

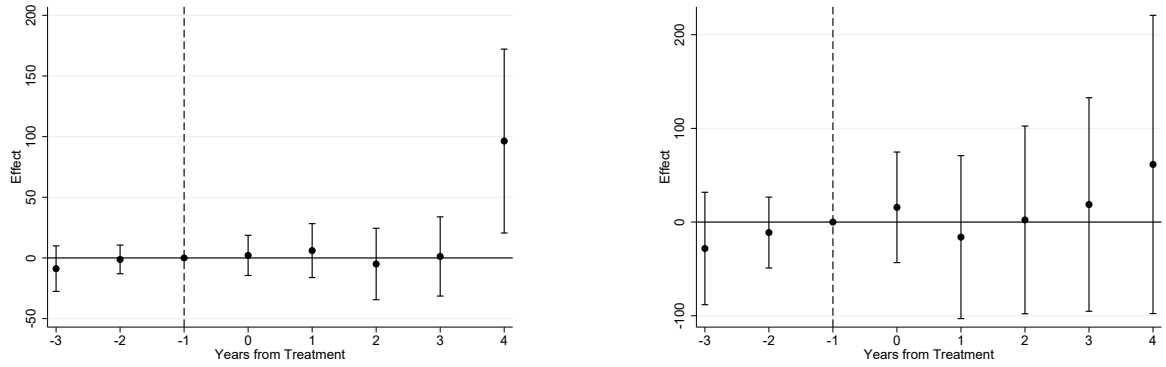
(b) Countries Export (Intra EU)

(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.

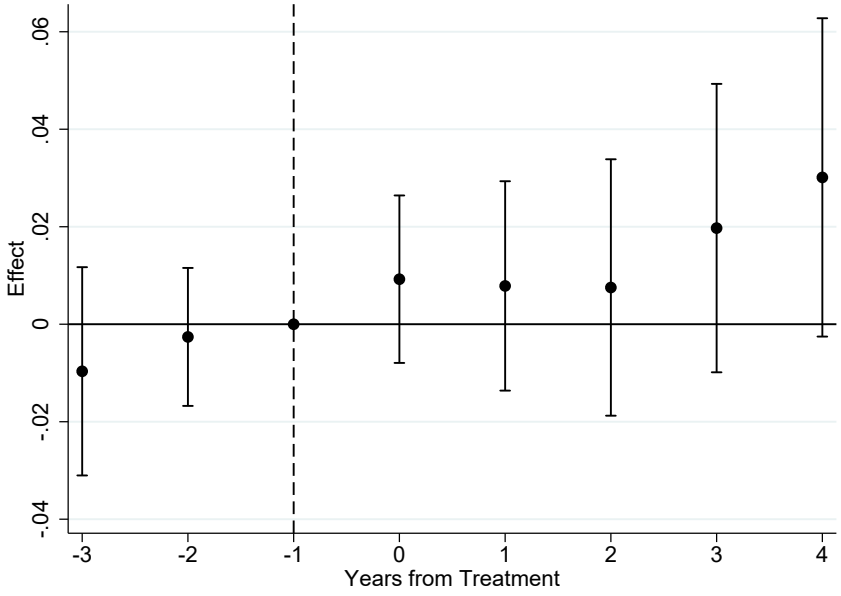
Figure A8: Effect of TEM Voucher Assignment on firms' Assets



(a) Immaterial Assets (b) Material Assets

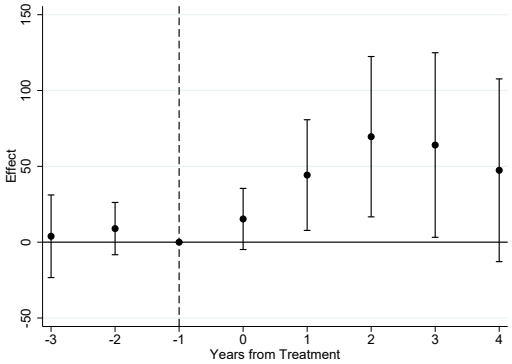
Notes: This figure reports results from a difference-in-differences model estimated between 2013 and 2019. All variables are winsorized at 1%. Treated firms are the firms that applied for the voucher before the 2002[nd] application. Firms were included in the analysis if they applied within a radius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

Figure A9: Effect of TEM Voucher Assignment on firms' Survival probability

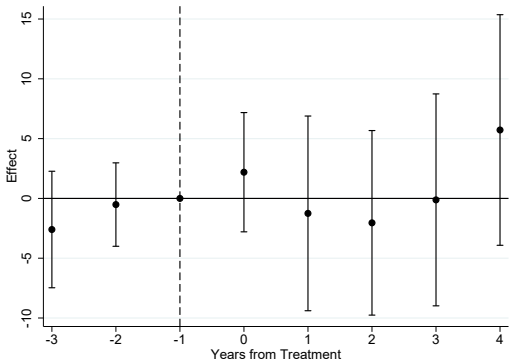


Notes: This figure reports results from a difference-in-differences model estimated between 2013 and 2019. Treated firms are the firms that applied for the voucher before the 2002[nd] application. Firms were included in the analysis if they applied within a radius of 30 seconds of the threshold. Regression 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 one if the firm is active in year t. A firm is considered to be active if it has positive employment expenditure in year t. Standard errors are clustered at the firm level.

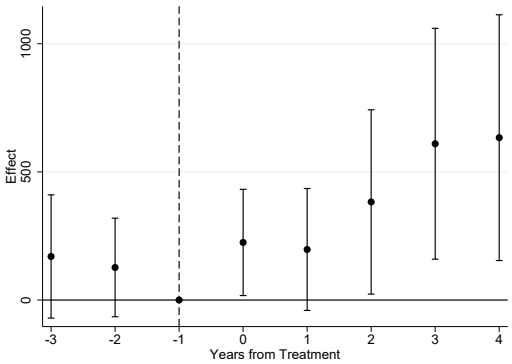
Figure A10: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Active from 2013 to 2020



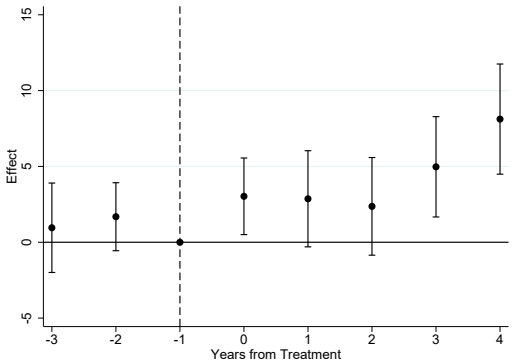
(a) Total Employment Costs



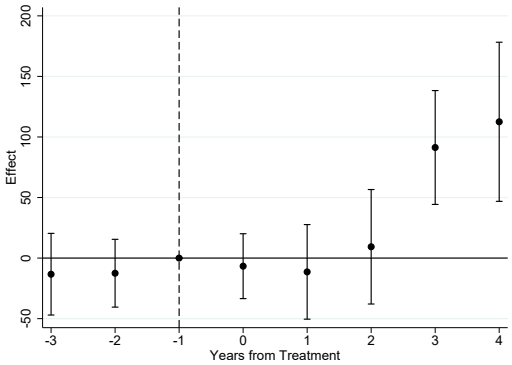
(b) Capital/labor Ratio



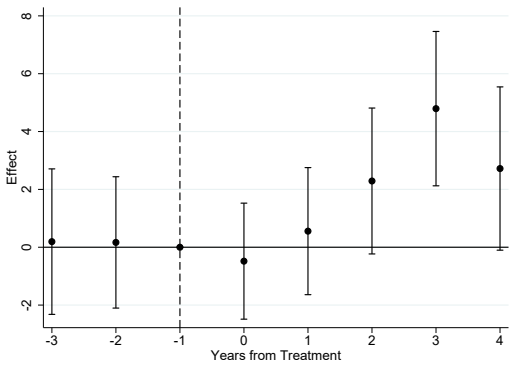
(c) Revenue from Sales



(d) Value Added per Employee



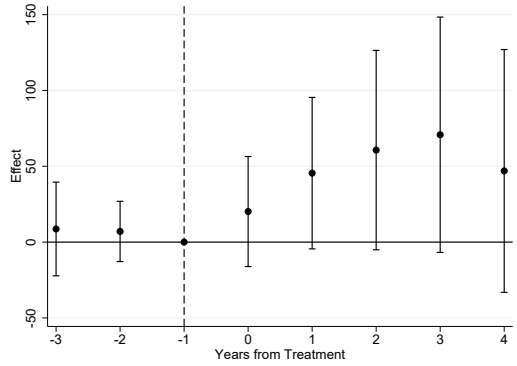
(e) Net profits



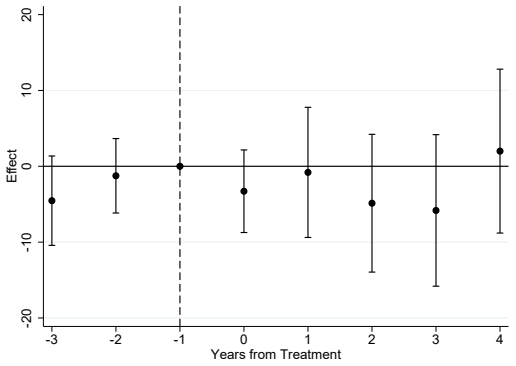
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2020. Capital/labor ratio 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 being active from 2013 up to 2020. Regression includes firm and year fixed effects. Coefficients of the difference with respect to the base year reported together with their 95% confidence interval. All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points. Standard errors are clustered at the firm level.

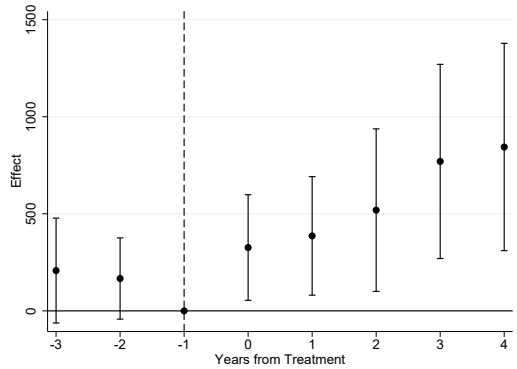
Figure A11: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 20 Seconds



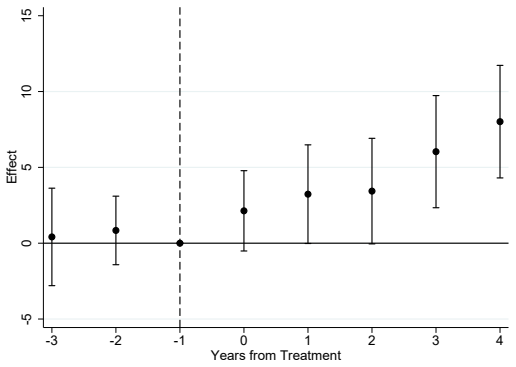
(a) Total Employment Costs



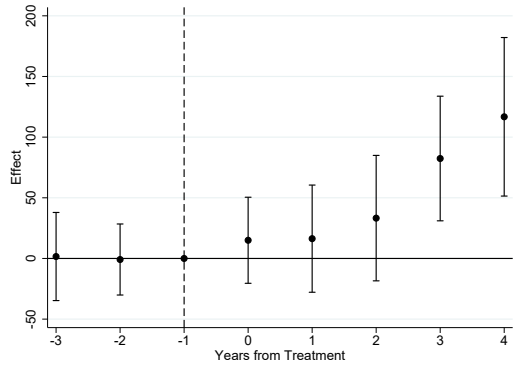
(b) Capital/labor Ratio



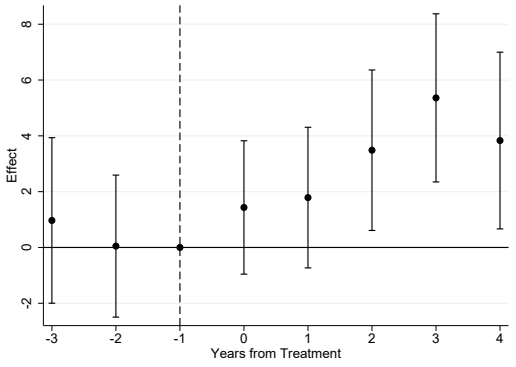
(c) Revenue from Sales



(d) Value Added per Employee



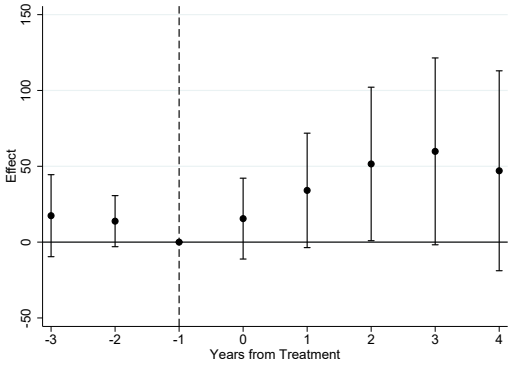
(e) Net profits



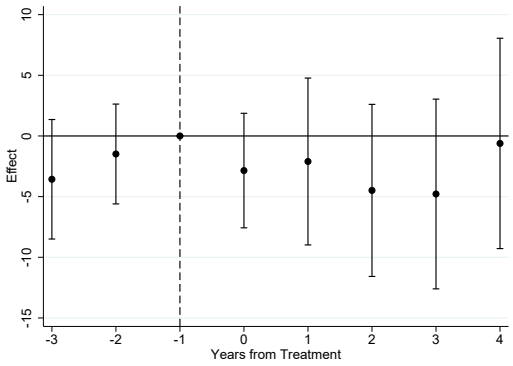
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2020. 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. All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points. Standard errors are clustered at the firm level.

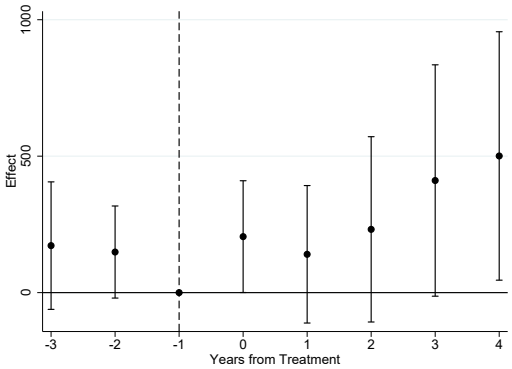
Figure A12: Effect of TEM Voucher Assignment on Firm Outcomes Over Time: Radius 40 Seconds



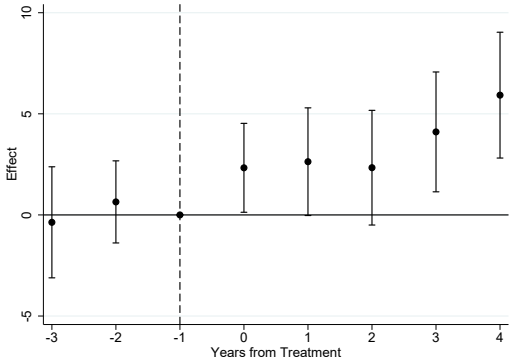
(a) Total Employment Costs



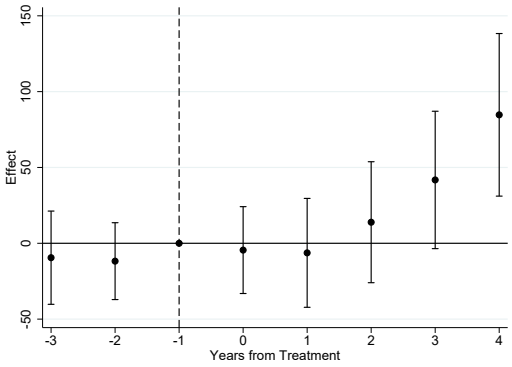
(b) Capital/labor Ratio



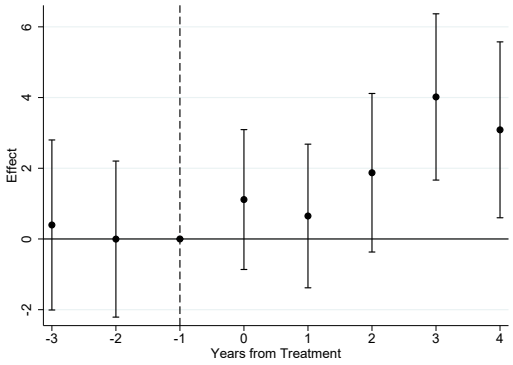
(c) Revenue from Sales



(d) Value Added per Employee



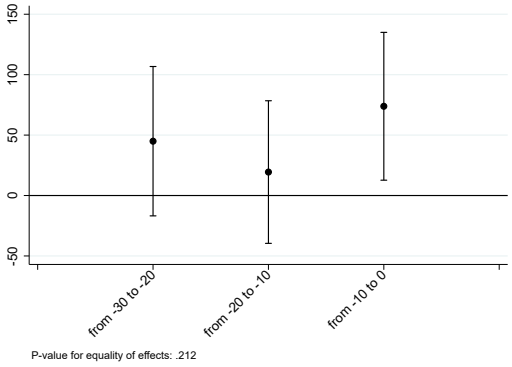
(e) Net profits



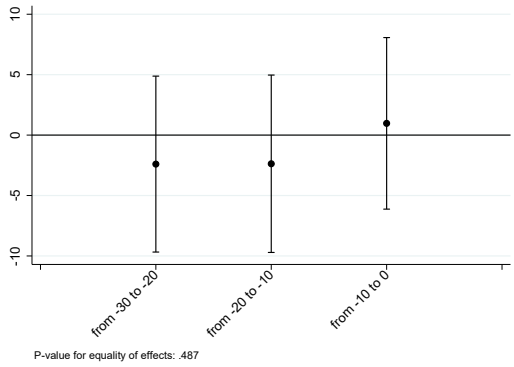
(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2020. 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. All effects are reported in thousand of euros but for Roe for which the effect is reported in percentage points. Standard errors are clustered at the firm level.

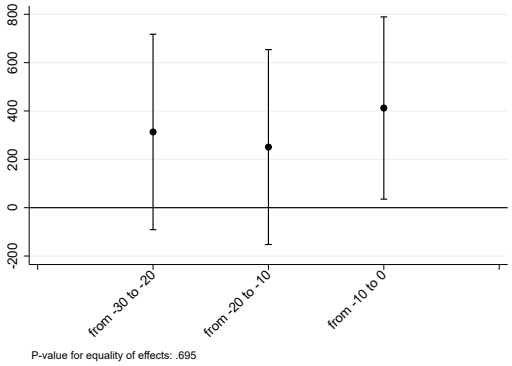
Figure A13: Effect of TEM Voucher by Time of Application Bin (10 Seconds)



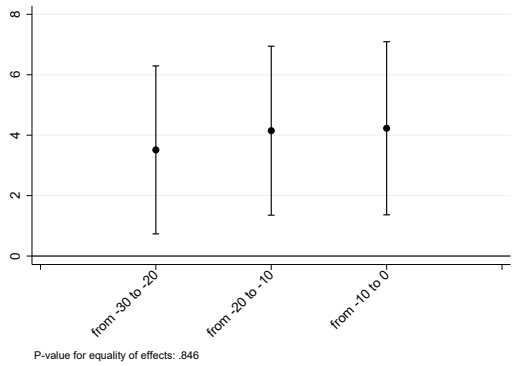
(a) Total Employment Costs



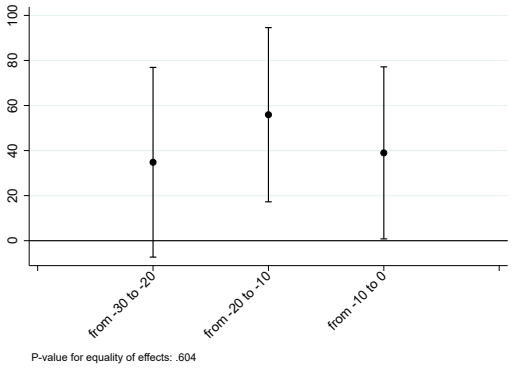
(b) Capital/labor Ratio



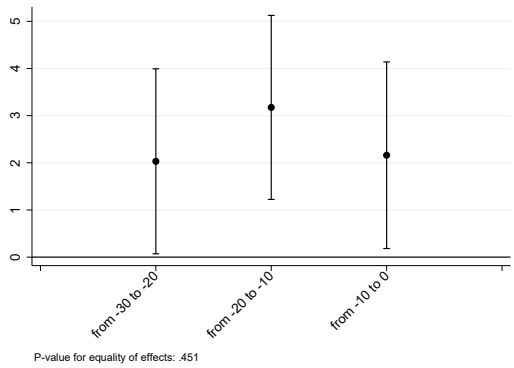
(c) Revenue from Sales



(d) Value Added per Employee



(e) Net profits



(f) Return on Equity

Notes: Results of a difference-in-differences model estimated between 2013 and 2020. Firms applying before the cutoff are divided 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. All effects are reported in thousand of euros but for Roe which is reported in percentage points. Standard errors are clustered at the firm level.

Appendix Tables

Table A1: Effect of TEM Voucher Assignment on Trade by Group of Countries

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

Note: Difference-in-differences regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 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 A2: 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	-0.020*	-0.013	0.011	0.005
	(0.012)	(0.012)	(0.012)	(0.015)
Observations	16,583	16,583	16,583	16,583
R-squared	0.783	0.804	0.700	0.646
Mean Control	.63	.61	.44	.43
	Panel (b): Products			
Variables	Prod. Extra EU EXP	Prod. Intra EU EXP	Prod. Extra EU Imp	Prod. Intra EU Imp
Before Cutoff X Post	0.155	-0.098	-0.069	0.100
	(0.277)	(0.210)	(0.190)	(0.294)
Observations	16,390	16,390	16,390	16,390
R-squared	0.899	0.899	0.892	0.835
Mean Control	8.4	4.76	4.26	4.56
	Panel (c): Countries			
Variables	Count. Extra EU EXP	Count. Intra EU EXP	Count. Extra EU Imp	Count. Intra EU Imp
Before Cutoff X Post	-0.026	0.001	-0.039	0.050
	(0.130)	(0.112)	(0.051)	(0.081)
Observations	16,390	16,390	16,390	16,390
R-squared	0.951	0.937	0.866	0.832
Mean Control	5.62	5.01	1.58	1.97
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: Difference-in-differences regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 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. All variables are measured in thousand of euros. Level of significance: *** 0.01, ** 0.05, * 0.1.

Table A3: Firm-level Outcomes: Additional Impact of Second Wave

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

Notes: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. *Post 2015* is the period after 2015, the year of the voucher assignment, while *before cutoff* is a dummy indicating firms that applied before the 2002nd firm, which corresponds to the theoretical exhaustion of available funds. *Post 2017* is the period after 2017, the year of the voucher assignment for the second wave, while *Recipient Second Wave* is a dummy indicating firms that received the voucher during the second wave of the policy. Firms are included in the sample if they applied within a radius of 30 seconds with respect to the theoretical exhaustion time of funds. C/E Ratio is computed as the total value of material and immaterial assets over the number of employees. All variables are winsorized at 1%. Standard errors are clustered at the firm level. All variables are measured in thousand of euros but for Roe which is measured in percentage points. Level of significance: *** 0.01, ** 0.05, * 0.1.

Table A4: Comparison of Applicants and General Firm Population

Variable Name	(1) Average Applicants	(2) Average Non-Applicants	(3) Difference	(4) T-Stat
		Panel (a): All		
Total Employment	24.464	22.736	1.728	3.66
Cost per employee	35.467	35.022	0.444	1.84
Capital Employment Ratio	88.917	123.085	-34.166	-8.75
Revenues from Sales	5662.412	5612.964	49.445	0.349
VA per Employee	54.362	56.837	-2.473	-4.35
Net Profits	110.718	120.009	-9.291	-1.141
Roe	7.691	10.753	-3.061	-7.909
Observations	4,145	274,110		
		Panel (b): below 250 Employees		
Total Employment	24.179	17.11	7.354	15.84
Cost per employee	35.472	34.875	0.592	2.45
Capital Employment Ratio	88.998	123.168	-34.25	-8.77
Revenues from Sales	5646.77	4421.524	1240.886	8.859
VA per Employee	54.376	56.633	-2.27	-3.99
Net Profits	110.64	93.3	17.416	2.15
Roe	7.692	10.88	-3.187	-8.239
Observations	4,096	266,977		

Note: Comparison of firms applying for the policy and the general firm population in Italy. All variables are winsorized at 1%. We exclude firms with no employees in 2015 and firms with less than 1,000 euros in Costs for personnel. Panel (a) includes all firms with at least 500,000 euros in revenues in one of the three years before the policy, 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. All variables are reported in thousand of euros but for Roe, which is reported in percentage points. Robust standard errors are used to compute the t-statistic.

Table A5: 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
Total Employment	4.407	9.439		
Cost per employee	-1.384	-5.719	-1.621	-6.81
Capital Employment Ratio	-0.312	-0.079	3.543	0.939
Revenues from Sales	253.001	1.779	-735.637	-6.949
VA per Employee	-4.364	-7.679	-4.403	-7.739
Net Profits	-28.111	-3.44	-52.015	-6.55
Roe	-3.227	-8.279	-3.204	-8.21

Note: Comparison of firms applying for the policy and the general firm population in Italy. Table reports coefficient of a regression having the variable in the first column as dependent variable and a dummy for applicants for the policy as independent variable. All variables are winsorized at 1%. Firms included in the analysis if they have more than 500,000 euros in revenues in one the three years preceding the policy implementation and less than 250 employees in 2015 as in Panel (b) of Table A4. 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. All variables are reported in thousand of euros but for Roe, which is reported in percentage points. Robust standard errors are used to compute the t-statistic.

Table A6: Effect of TEM Voucher Assignment on Balance Sheet Outcomes: Robustness

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

Note: Difference-in-differences regression for the effect of being assigned the TEM voucher on firm balance sheet outcomes. "Post" is the period after 2015, 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 A6 for the sake of comparison. Panel (b) reports the results for estimates of the same equation with the dependent variable not winsorized. Panel (c) uses winsorized variables at 1% but excluding zeroes when the firm is not active in the periods after the policy implementation. Panel (d) reports the effect for the variables in logs (we use an inverse hyperbolic sign transformation). Panel (e) displays results for an instrumental variable strategy where the fact that the firm used the voucher to hire a TEM is instrumented with the fact that it applied before the exhaustion time of funds. F-statistic for the relevance of the instrument reported at the bottom of the panel. Panel (f) reports results from a specification equivalent to Panel (a) but restricting the sample to firms active from 2013 to 2020. We define a firm active if it has positive employment cost during the year. Panel (g) excludes firms that were included in the program due to quotas, that is firms that participated in Roadshows or that had a legality rating at the time of the application. Panel (h) nets out a linear trend, different for treated and comparison firms, based on years before the intervention. Panel (i) includes interactions between year fixed effects and levels of our variables in 2015. Finally, Panel (l) replicates estimates from Panel (a) but standard errors are clustered at the second of application level. Standard errors are clustered at firm level. All effects are reported in thousand of Euro but for Roe for which the effect is reported in percentage points. Level of significance: *** 0.01, ** 0.05, * 0.1.

Table A7: Firm-level Outcomes: Difference-in -Discontinuity Strategy

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

Note: Difference-in-discontinuity regression for firm trade outcomes. *Post* is the period after 2015, the year of the voucher assignment, while *Before Cutoff* is a dummy indicating firms that applied before the 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. Mean control is the average for the comparison group in the periods after 2015. All variables are winsorized at 1%. Standard errors are clustered at firm level. All effects are reported in thousand of Euro but for Roe for which the effect is reported in percentage points. 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?