The Effects of Subsidies on Firm Size and Productivity (Preliminary Draft)

Enia Bearzotti* Sašo Polanec† Tjaša Bartolj‡

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Abstract

This paper evaluates the impact of varying subsidy sizes and distinct program objectives on firm size and performance. The magnitude of treatment effects increases with subsidy size, although the marginal effects tend to decrease. We also find that treatment effects differ across subsidy programs due to their distinct objectives. Among these, labor-support measures are most effective at supporting employment, capital, and output while being most harmful to productivity. Contrary to theory, subsidies providing incentives for investments have no impact on capital or productivity. The treatment effects tend to decrease over time and are thus temporary. As recipient firms are more likely to receive additional support in the future, the effects of subsidies accumulate giving rise to permanent differences between subsidized and non-subsidized firms. However, the lack of productivity improvements in such firms questions the benefits of repeated supporting measures.

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*Institute for Economic Research, Kardeljeva pl. 17, 1000 Ljubljana, Slovenia; School of Business and Economics, University of Ljubljana, Kardeljeva pl. 17, 1000 Ljubljana, Slovenia; e-mail: enia.bearzotti@ier.si (corresponding author)

†School of Business and Economics, University of Ljubljana, Kardeljeva pl. 17, 1000 Ljubljana, Slovenia; e-mail: saso.polanec@ef.uni-lj.si

‡Institute for Economic Research, Kardeljeva pl. 17, 1000 Ljubljana, Slovenia; e-mail: bartoljt@ier.si

1 Introduction

Since the economic recession of 2008–09, government support to firms has made a comeback in the industrial policy in an increasing number of countries worldwide, including those that have historically advocated against protectionist measures.[1] Although these measures may have anti-competitive effects on international trade relations, governments increasingly justify the use of subsidies to alleviate the implications of various economic and non-economic shocks, such as the COVID-19 pandemic, climate change, fragile and disrupted global value chains, and digital transformation (OECD, 2022).

Many have questioned the benefits of subsidies.[2] Previous empirical studies often offered opposing conclusions, which strengthened the prevalent opinion of a poor understanding of the impact of this policy tool. Two issues hinder the evaluation of subsidy programs. First of all, data availability. Due to concerns regarding potential breaches of competition rules, governments are reluctant to disclose detailed information about the use of these policy measures. Secondly, it is methodologically difficult. The fact that firms may (and often do) receive subsidies multiple times of varying amounts and from programs with distinct objectives, either in the same year or in several consecutive years, directly translates into an identification problem. For these reasons, most studies have focused on individual subsidy programs, often leaving the question about potential additional support from other sources unanswered and being unable to adequately measure the effects.

This paper aims to investigate the effects of a wide variety of subsidies by adopting an identification strategy that can deal with repeated treatments. We explore two under-researched subsidy characteristics that may account for part of the heterogeneity of their effects: varying subsidy sizes and distinct program objectives. Previous studies ascribe differences in estimated effects to firm heterogeneity without addressing subsidy heterogeneity as a contributing factor due to limited information on subsidies. In contrast, we use detailed information about all programs directly subsidizing firms implemented in Slovenia from 2001 to 2017, including amounts, purpose of programs, and their objectives.

Our study is structured as follows. Section 2 extends the background of the study, outlining the most relevant research made, their conclusions, and the gaps our study is addressing. In addition, it presents a discussion of the predictions offered by the theoretical literature regarding the effects of subsidies. We describe our methodological approach in Section 3 and the sample of firms and treatments in Section 4. The results are discussed in Section 5, while the main takeaways of this study and policy implications are summarized in Section 6.

[1]For instance, the US Congress has recently passed two bills, providing \$52bn and \$400bn of incentives, respectively, supporting domestic industry to safeguard national security, increase job creation, and promote decarbonization (Economist, 2023).

[2]*Subsidy* is an umbrella term for a wide variety of public support schemes of which several discordant definitions exit. We refer to the definition of subsidies in use in the EU outlined in Article 107 of the Treaty on the Functioning of the European Union, prohibiting the use of State aid “granted by a Member State or through State resources in any form whatsoever which distorts or threatens to distort competition by favoring certain undertakings or the production of certain goods” unless specific conditions favoring the general economic development are met (Consolidated version of the Treaty on the Functioning of the European Union, 2012).

2 Literature Review and Scope of the Study

Empirical research has been mostly concerned with finding whether subsidies lead to additional jobs, investments, output, and productivity improvements relative to pre-intervention levels that would justify public spending. However, results are mixed and often contradicting.

Subsidies are found to incentivize job creation (e.g., Cerqua and Pellegrini, 2022; Batut, 2021; Criscuolo, Martin, Overman, and Reenen, 2019; Cahuc, Carcillo, and Barbanchon, 2019), but the extent of the positive impact gets reduced due to displacement effects (Einiö and Overman, 2020) and employee churning (Neumark and Grijalva, 2017). Investments are also found to increase (e.g., Galaasen and Irarrazabal, 2021; Decramer and Vanormelingen, 2016), although some studies find evidence of firms partly replacing their own resources with public funding.[3] Interestingly, one of the mechanisms through which subsidies are found to foster investments involves a certification/signaling effect of high firm quality, which in turn improves firms' access to long-term borrowing (e.g., Chiappini, Montmartin, Pommet, and Demaria, 2022; Mina, Minin, Martelli, Testa, and Santoleri, 2021; Mulier and Samarin, 2021; Meuleman and Maeseneire, 2012; Hyytinen and Toivanen, 2005). Czarnitzki and Lopes-Bento (2013) report that effects on R&D investment and employment are stable over time and do not decrease if firms receive additional support in the same year from other sources or consecutively over the years. Most studies reject productivity improvements (e.g., Mattsson, 2019; Criscuolo et al., 2019; Cooper, Meyer, and Schott, 2017; Koski and Pajarinen, 2015; Bernini and Pellegrini, 2011), which raises concerns over potential misallocation of resources since subsidies positively affect firm survival (e.g., Srhoj, Škrinjarić, and Radas, 2021; Galaasen and Irarrazabal, 2021; Cooper et al., 2017; Koski and Pajarinen, 2015; Schweiger, 2011). Bernini, Cerqua, and Pellegrini (2017) find that productivity losses are generally temporary, even showing evidence of positive effects arising after 3-4 years on account of technological change.

Mixed results are mostly attributed to firm heterogeneity. Subsidies are found to be most effective in smaller and younger firms due to tighter credit constraints (the more so the more innovative and technological their products and services) and usually at an early stage of their market penetration potential and learning curve (e.g., Chiappini et al., 2022; Vanino, Roper, and Becker, 2019; Criscuolo et al., 2019; Decramer and Vanormelingen, 2016; Bronzini and Iachini, 2014; Bia and Mattei, 2012). In contrast, Criscuolo et al. (2019) bring evidence that large and mature firms do not react to the incentive, while Decramer and Vanormelingen (2016) that these firms use subsidies to increase profits.

The question of whether heterogeneity of subsidies contributes to differential impacts has so far obtained limited attention in the literature likely due to data unavailability. As anticipated in the Introduction, we address two dimensions of subsidy heterogeneity: varying subsidy size and specificity of program objectives. There exist only a few papers dealing with the former (see, e.g., Vanino et al., 2019; Bondonio and Greenbaum, 2014; Bia and Mattei, 2012; Adorno, Bernini, and Pellegrini, 2007). These conclude in favor of a positive relationship between the grant size and the magnitude of treatment effects. Furthermore, Bia and Mattei (2012) and Adorno et al. (2007) advance the idea of an optimal subsidy size, beyond which marginal effects start decreasing, while Burger and Rojec (2018) conclude

[3] Surprisingly, Cerqua and Pellegrini (2014) and Bronzini and de Blasio (2006) come to opposite conclusions regarding the same subsidy scheme about the presence of an intertemporal substitution effect causing firms to reduce their investments in subsequent years.

that there exists a minimum threshold below which subsidies are ineffective (smaller subsidies are even found to negatively impact employment and revenues). However, these studies are limited in their scope, focusing solely on the short-term[4] impact on employment, turnover, and fixed assets, while not addressing potential effects on productivity. In contrast, Bronzini and Iachini (2014) reject the hypothesis that the intensity of the grant plays a role in determining the impact of investment subsidies on R&D investing, ascribing the effectiveness of subsidies for small firms to their difficulty in accessing credit markets. Another small group of studies compared the effects of subsidies across different programs. In particular, Hottenrott, Lopes-Bento, and Veugelers (2017) compare the direct and cross-scheme effects of research, development, and mixed R&D grants on research and development investments, finding that subsidies targeting research projects are more effective at supporting both outcomes. According to Koski and Pajarinen (2015), employment, R&D, and all other supporting measures fail to improve labor productivity while decreasing the likelihood of market exit for subsidized firms. Finally, Burger and Rojec (2018) find that different crisis-motivated subsidy programs primarily aimed at the rescuing and restructuring of troubled firms do not generally cause revenue growth, while positive employment effects are ascribed to employment subsidies only.

We add to the literature by investigating whether subsidy size and different objectives of subsidies can explain heterogeneity in treatment[5] effects on employment, capital, productivity, and value added over a five-year time frame. We apply a similar approach to Bondonio (2008) in allocating subsidies to five treatment groups based on the subsidy-to-value added ratio. In addition to estimating the effects of a pooled amount of all subsidies received in a given year, we perform estimations on four objective-specific subsidy groups: labor-support subsidies, productivity-enhancing subsidies, capital-deepening subsidies, and general subsidies.

Methodologically, we take a different approach from the aforementioned studies thanks to a comprehensive administrative dataset on all programs directly subsidizing firms implemented in Slovenia from 2001 to 2017. Unlike the large majority of studies, which focused on individual subsidy schemes, our data have the advantage of containing information on the history of received subsidies for both control and treated firms. In particular, we know when firms simultaneously benefited from multiple supporting measures in the same year or had received support shortly before and were still experiencing ongoing effects.[6] The possibility of multiple treatments directly translates into an identification problem.[7] Ideally, we would like to be able to estimate the effect originating from the use of one-time subsidies. Instead, firms may (and often do) obtain several grants over the years and from different subsidy schemes within the same year, causing the effects to build up and persist over time. In such a scenario, we would not

[4]Among the aforementioned papers, only Vanino et al. (2019) estimate effects distinguishing between short and medium term.

[5]A note on terminology. In the tradition of the policy evaluation literature, we will often refer to subsidies as treatments.

[6]Few studies addressed the issue of multiple grants in their methodologies. For instance, Hottenrott et al. (2017) made a separate analysis for firms that obtained multiple subsidies. Other studies, e.g., Burger and Rojec (2018) and Czarnitzki and Lopes-Bento (2013), use previous subsidy exposure as an additional confounder in the estimation procedure. In order to avoid lagged treatment effects, we used a sample restriction excluding all firms that were previously treated.

[7]Methodologies based on instrumental-variable estimators that are more suited to deal with an identification problem were infrequently applied in the context of subsidies because of the difficulty of finding valid instruments. An important exception is the study by Criscuolo et al. (2019), which took advantage of an exogenous source of variation (as determined by the EU) in the eligibility of firms to identify the causal effect of grants allocated via the Regional Selective Assistance program in the UK. They found an economically large and statistically significant program effect, limited to small firms, on employment and capital but no effect on productivity.

know the source of the estimated treatment effect, what portion is attributable to a subsidy received at a specific time. Hence, we adopt strict identification criteria in selecting subsidy treatments to be able to properly identify and measure the effect originating from individual grants. We then compare these well-identified treatment effects with those obtained by relaxing the strict identification criteria by allowing the receipt of additional public grants in the years subsequent to the receipt of the first subsidy.

2.1 Conceptual Framework

Our work relies on several strands of theoretical literature as subsidies are commonly provided to increase either the capital stock of firms (capital subsidies) or their labor (employment subsidies). However, in practice, subsidy schemes often pursue policy objectives that affect both inputs.

Most of the literature on capital subsidies concentrates specifically on the effects of R&D subsidies on investment. According to the standard neoclassical theory of investment behavior, the equilibrium R&D spending is determined by the firm profit-maximizing decision and lies at the point where the (increasing) marginal cost of capital (MCC) equates the (decreasing) marginal rate of return (MRR).[8] Capital subsidies are expected to expand firms' R&D investment by reducing the cost of capital facing the firm (the more generous the grant, the greater the reduction) and increasing the expected investment profitability (Criscuolo et al., 2019; Egger and Keuschnigg, 2015; Bronzini and Iachini, 2014). It follows that the extent to which capital increases depends on the specific slope of the marginal cost function of a recipient firm. For an equal subsidy amount, firms facing a steeply increasing MCC schedule, such as growth-oriented technology-intensive SMEs, will increase capital by a larger amount than firms that are less financially constrained.[9] The reduction in the cost of capital, however, masks a combination of effects. On the one side, the subsidy shifts the MCC to the right by allowing to undertake additional projects using capital with a lower marginal cost than before the subsidy. On the other, the receipt of a subsidy indirectly conveys information about the quality of the firm, reducing the informational asymmetries and further lowering the cost of internal and external funding. This implies both a downward shift in the MCC curve and a reduction in the slope in the upward-sloping part (Hyytinen and Toivanen, 2005). According to Egger and Keuschnigg (2015), an additional channel through which R&D subsidies stimulate late-stage investments involves the strengthening of internal funds, which get leveraged with additional external credit (either bank credit or venture capital for firms denoting lower and higher risk, respectively). Due to the monitoring and control exercised by venture capitalists, this form of financing, in turn, is predicted to bring about a certification effect for R&D intensive firms.

To be effective and trigger additional investments, the subsidy must be used to finance marginal investments, i.e., projects that would be unprofitable if privately financed only and would thus not be undertaken without (cost-free) public support. These are projects whose private rate of return is lower than the social rate of return due to, e.g., knowledge spillovers and environmental externalities, or that

[8]The marginal cost of capital represents the opportunity cost of investment and expansion. It is horizontal insofar as a firm has internal funds and becomes upward-sloping after a certain level of investment/expansion that requires relying also on external financing. In this part of the curve, factors such as asymmetric information, adverse selection, and moral hazard imply that the marginal cost of capital is an increasing function of the amount raised. The marginal rate of return ranks investment projects and expansion possibilities in descending order according to their expected return on the R&D investment (Hyytinen and Toivanen, 2005).

[9]In this regard, Figure 2 in Criscuolo et al. (2019) clearly depicts the difference in capital expansion between financially constrained and unconstrained firms.

involve a higher risk due to uncertainty about future product demand. If, instead, the subsidy is used to finance infra-marginal projects, i.e., investments that firms would have made even without the public incentive, the subsidy would merely represent a money transfer to firms and a deadweight loss from the societal perspective, without increasing the level of investments (Criscuolo et al., 2019; Bronzini and Iachini, 2014).

The extent to which capital subsidies affect firms' employment greatly depends on the degree of complementarity and substitution between labor and capital inputs (see, e.g., Cahuc, Carcillo, and Zylberberg, 2014, Ch. 2). When the reduction in the cost of capital relative to labor cost leads to a larger substitution effect than the scale/output effect (the increased production and higher local labor demand following the reduction in total costs and attraction of new investment to the area), employment is likely to increase (Criscuolo et al., 2019; Decramer and Vanormelingen, 2016; Bernini and Pellegrini, 2011).

Various strands of the theory of labor markets have dealt with the effects of different employment support policies. According to the static neoclassical theory of labor demand (Cahuc et al., 2014), reducing the labor costs through subsidies leads to an immediate increase in labor demand via an adjustment in wages (potentially changing the employment composition in recipient firms, especially if the policy measure is targeted at specific groups of employees), where the size of the effect depends on the elasticities of both labor demand and labor supply. However, once the subsidy is no longer in place, the created jobs that are not profitable enough are eliminated, implying that employment effects are short-lived unless workers' productivity increases due to, e.g., on-the-job training.

In the dynamic theory of labor demand, the presence of adjustment costs[10] reduces firms' propensity to hire and fire workers to meet new economic conditions, therefore reducing fluctuations in the employment level. However, this aspect requires firms to adopt a forward-looking approach concerning their employment decisions. Hiring costs mean that, when hiring new workers in periods of strong labor demand, employers face a cost in addition to the actual wage.[11] A decrease in, or the covering of, hiring costs by means of hiring subsidies should therefore lead to a decrease in the overall labor cost and, consequently, to an increase in employment following an increased frequency in new hires and fewer separations in all firms that have a certain preference for the present or that do not fully anticipate the future evolution of labor demand. The positive impact on employment is expected to persist if firing costs are left in place (Bagliano and Bertola, 2004; Batut, 2021).

Mattsson (2019) provides a theoretical framework for understanding the effects of employment subsidies on productivity and profit. To maximize profits, the marginal revenue product of labor has to equal its wage. The marginal productivity of labor is assumed to be a decreasing function of the duration of unemployment for workers without schooling. Unemployed workers eligible for employment subsidies are, on average, expected to be less productive than workers employed without the subsidy and therefore,

[10]We only concentrate on the demand side of the labor market and thus on adjustment costs borne by firms when hiring and firing workers. In contrast, mobility costs are adjustment costs that are borne by employees and that thus pertain to the supply side of the labor market.

[11]In periods characterized by weak labor demand, hiring costs entail a lower "shadow wage" compared to the wage since retaining one additional unit of labor in the bad state implies saving the hiring cost of one additional worker in the strong state next period (Bagliano and Bertola, 2004). However, hires of new employees occur during strong labor demand states, which is why we limit to that case in the text.

without any compensation for firms, are more likely to remain unemployed in the long run. As it is not possible to exactly compensate firms for the difference in marginal productivity between each subsidized employee and regular workers due to imperfect information, Mattsson (2019) discusses various scenarios that could arise. Firstly, the subsidy could overcompensate for the lower marginal productivity of subsidized employees, therefore increasing profit per employee despite reducing TFP. Firms would thus have the incentive to replace regular workers with subsidized ones, leading to a questionable net effect on total employment. Secondly, the subsidy could not compensate enough for the loss in productivity of hiring eligible workers, leading to a reduction in profit per employee. This occurs when firms misjudge the productivity of new workers or too many resources are used to introduce them to the job and train them. Thirdly, if screening is successful, firms can hire eligible workers whose marginal productivity is equal to that of regular ones. The subsidy would thus represent compensation for the higher risk undertaken in hiring long-term unemployed workers with expected lower human capital. If productivity remains unaffected or increases and profit per employee increases, the subsidy would represent a waste of public resources. A final case involves the possibility that firms do not manage to maximize their profits, experiencing no reduction in productivity and yet lower profits (Mattsson, 2019).

Provided that subsidies lead to (i) increased investments (input additionality) by lowering the cost of capital and labor and (ii) increased innovations (output additionality), endogenous growth theory predicts that subsidies ultimately lead to more output and higher economic growth (e.g., Grossman and Helpman, 1991; Barro and Sala-i-Martin, 2004; Romer, 1990; Howitt and Aghion, 1998; Egger and Keuschnigg, 2015). These models advocate for the use of subsidies exactly because they are able to address market failures associated with underinvestment in R&D, in particular informational asymmetries in financial markets, knowledge spillovers, and monopoly pricing behavior relative to innovations, which cause a socially suboptimal rate of innovation and lower employment of highly qualified human capital. However, by indirectly affecting firm survival,[12] models with firm heterogeneity with respect to innovation abilities (e.g., Acemoglu, Akcigit, Alp, Bloom, and Keer, 2018; Galaasen and Irarrazabal, 2021) argue that subsidies aimed to foster R&D lead to an inefficient allocation of production inputs among surviving firms, ultimately slowing down aggregate productivity growth.[13] The literature on industrial protection (e.g., Acemoglu, Zilibotti, and Aghion, 2006; Melitz, 2005), instead, ascribes a positive effect on economic growth to temporary protectionist subsidies supporting domestic immature (or "infant") firms so long as assisted firms can benefit from dynamic learning effects.

3 Estimation of Treatment Effects Methodology

To estimate the treatment effects on firm size and performance, our outcome variables are computed as the cumulative forward growth rates of firms' employment, capital, value added, and total factor productivity (TFP). The latter is estimated at the NACE 2-digit industry level following the method by

[12] The international trade literature with monopolistically competitive firms (e.g., Melitz, 2003; Melitz and Ottaviano, 2008), despite not studying the impact of subsidies directly, provides a setting for understanding the effects of a decrease in production costs on firm productivity distribution and reallocation of market shares among surviving firms. By reducing fixed and variable costs, subsidies allow firms to achieve the minimum level of productivity needed to survive in the market.

[13] Acemoglu et al. (2018) conclude that the optimal policy best supporting aggregate productivity growth and welfare is not to subsidize the R&D of all incumbent firms but to free up resources used in the operations of low-type firms so that can be employed in R&D by high-type firms. This can be achieved by inducing a higher market exit rate of low-type firms, for instance, by taxing the operations of all firms.

Ackerberg, Caves, and Frazer (2015).[14] As the growth rates of the latter three are partly driven by price changes, we apply appropriate deflators to nominal variables. In particular, we deflate the nominal physical capital by investment-goods price index and industry-specific price indices (at NACE 2-digit level when available) for revenues, costs of intermediate goods and services, and value added. Our outcome variables are thus computed as follows:

$$\Delta y_{i,t+j} = y_{i,t+j} - y_{i,t-1} \tag{1}$$

for each firm i of the sample, $t = 2002, ..., 2016$, and $j = 1, ..., 5$ such that the sum $t + j$ goes up to 2021.[15]

In estimating the treatment effects of distinct subsidy sizes, our method follows the approach of Bondonio (2008). We assign a grant equal to zero to non-subsidized firms and "discretize" the space of treatments into different (in our case five) segments according to the received incentive level through a categorical treatment variable. Treatment sizes are based on the ratio between subsidy in time t and value added in time $t-1$. In this way, we ensure that the value added in the pre-treatment period has not been influenced by the subsequent grant.[16] We form five treatment intervals as defined in Table 1. These must be wide enough to have a sufficient number of observations for each treatment level, but small enough to provide meaningful information about the effects of increasing subsidy size.[17] For each treatment size level, n, we generate a treatment indicator, D^n_{it}, taking value 0 if the subsidy-to-value added ratio for firm i in period t is 0, and 1 if treatment level equals relative subsidy size in n, for each subject i, period t, and for $n = 1, ..., 5$.

Table 1: Treatment size levels and relative subsidy sizes

Treatment size	Relative subsidy size
1	(0, 0.05]
2	(0.05, 0.1]
3	(0.1, 0.15]
4	(0.15, 0.2]
5	(0.2, 0.25]

The identification of the causal impact of subsidies is impeded by features intrinsic to subsidy allocation and program evaluations. Firstly, identifying treatment effects requires estimating counterfactual outcomes.[18] Secondly, subsidized firms do not constitute a random sample of units from the universe of

[14] We use a revenue-based measure of TFP as suggested by Foster, Haltiwanger, and Syverson (2008).

[15] Cumulative forward growth rates are computed up to 2016, despite the availability of additional data beyond that year. As we do not know whether firms obtained any public support from 2018 onward, we ensure our results are not affected by the possibility of future treatments. It follows that the extension of the estimation sample depends on the outcome variable, further reducing the more forward-looking the latter.

[16] As robustness, we repeated the analysis using the absolute values of subsidies to ensure that relative subsidy sizes are not driven by heterogeneity in value added rather than subsidies.

[17] We considered having the interval open to the right at treatment size 5, thus including all subsidies greater than 20 percent of firms' value added. However, the presence of particularly large subsidies gave rise to unusually high estimates of treatment effects. Furthermore, closing the interval has the additional advantage of having 5 equidistant intervals comparable to each other.

[18] To effectively identify the causal effect of subsidies, we would ideally compare the actual outcomes of each recipient firm with one or more potential ones that would have occurred if the firm had not received the grant, or had received a different amount or type of subsidy. The "fundamental problem of causal inference" arises from the inability to observe the same unit in all potential states of the world at a given moment in time. Matching techniques are commonly used to overcome this challenge.

firms. Applying firms self-select into subsidy programs, while subsidy administrators select applicants based on given criteria. We neither know the reasons driving a firm to apply for a subsidy, nor the criteria used for the acceptance of applications. Failing to account for this gives rise to selection biases. Thirdly, not being able to condition estimations on all endogenous sources affecting outcome variables due to latent confounders implies not finding what portion of the variation observed in the outcome variable is caused by the subsidy. Finally, firms often obtain multiple supporting measures, which hinders the identification of the effects of one-time subsidies.

Previous studies adopted different strategies to overcome the aforementioned obstacles, employing procedures deemed fit to the particular subsidy scheme under study. The large majority of empirical studies adopted either a difference-in-differences (DID) estimation approach, matching, or regression discontinuity design. The econometric literature on policy evaluation has very recently advised against the use of DID-based methodologies expanded with a group- and time-fixed effects (known as two-way fixed effects regressions) in the presence of multiple treatments, arguing that such methods are likely to produce misleading estimates of the treatment effects on account of effect heterogeneity across time and subjects and has thus advanced various alternative estimators.[19] However, except for de Chaisemartin and D'Haultfœuille (2020), these new methods require a framework in which units, once they get treated, receive treatment in all subsequent periods ("irreversibility of treatment" in the words of Callaway and Sant'Anna, 2021), while none of them is suitable to deal with multiple treatments. These two limitations do not fit our subsidy data because firms may (or not) receive multiple treatments of varying size in an on-and-off mode.

In contrast, our approach involves a combination of matching and strict identification criteria to isolate the effect of individual subsidies from the receipt of additional supporting measures close in time. We employ propensity-score matching (Rosenbaum and Rubin, 1983) using the Abadie and Imbens (2011) clustered standard errors to deal with selection biases.[20] Provided that our propensity score model is well-specified, estimated propensity scores, ps, ensure that the conditional independence assumption (or unconfoundedness) holds, implying that treatment exposure is independent of the potential outcomes (Abadie and Imbens, 2016; Rosenbaum and Rubin, 1983). Put differently, matched pairs of treated and non-treated units are as similar to each other as possible in key observable aspects leading firms to apply for and obtain a grant.

$$ps_{it} = P[D_{it} = 1 | Z_{it}, X_{i,t-1}] \tag{2}$$

Following both the econometric literature and the established praxis, our propensity-score model is estimated by logit regressions and includes firm age, year and indicator variables for region, and industry as contemporaneous variables, Z_{it}. These variables control for unobserved heterogeneity associated with accumulated experience and maturity of firms, different sectors and regions, as well as capture macroeconomic shocks. The model also includes several pre-treatment variables, $X_{i,t-1}$: our variables of interest (logs of employment, capital, and TFP),[21] revenues (log of sales), and degree of indebtedness (debt-to-

[19]For instance, alternative estimators were proposed by Sun and Abraham (2021), Callaway and Sant'Anna (2021), Athey and Imbens (2022), and de Chaisemartin and D'Haultfœuille (2020).

[20]More specifically, we required 1:1 matches (with replacement) imposing a caliper of 0.2, which is deemed appropriate by the existing literature (e.g., Austin, 2011b) to improve the matching quality and ensure common support.

[21]We do not include log of value added to avoid multicolinearity with TFP and production factors.

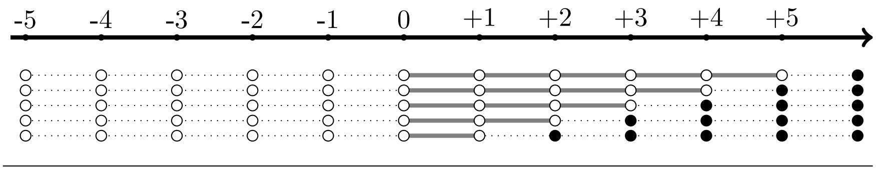
assets ratio).[22]

Following Rubin (1974), the causal impact of subsidies can then be obtained by computing the average of all individual-specific differences between matched outcomes over the number of treated units, an operation that results in the average treatment effects on the treated (ATET). In our case, ATETs are computed as:

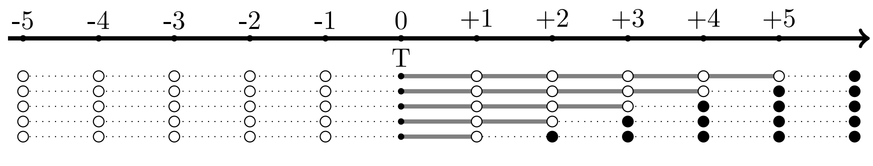
$$\delta_{t+j}^{ATET} = E(\Delta y_{i,t+j}^{1} | D_{it} = 1, ps_{it}) - E(\Delta y_{i,t+j}^{0} | D_{it} = 0, ps_{it}) \tag{3}$$

where the superscript denotes the treatment status of subject i, i.e., 1 for treated and 0 for non-treated units. The resulting ATETs are interpreted as estimates of the average excess cumulative growth that subsidized firms (in comparison to non-subsidized firms) experienced due to the receipt of subsidies.

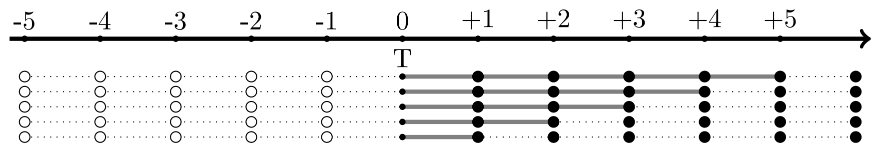
Figure 1: Identification of Treatments and Control Units



(a) Not-yet-treated controls



(b) Selected well-identified treatments



○No subsidy received

●Subsidy potentially received

• T = treatment at time 0

–Cumulative forward growth rates

(c) Selected treatments allowing for future treatments

Source: Own work.

Given the level of detail of our data on subsidies, our identification strategy heavily relies on the selection of an adequate subsample of treatments and control units. Concerning the former, we apply

[22]To verify that the estimated ATETs are valid, we assess covariate balance of these variables in the matched sample as indicated in Austin (2011a). Satisfactory balance is achieved when confounding variables present a standardized mean difference between the treated and non-treated groups of at most 0.1. Some papers, e.g., Austin (2009), discuss the acceptance of thresholds as high as 0.25. However, we prefer adopting a stricter approach to ensure a higher degree of comparability between controls and treated firms. We follow Ho, Imai, King, and Stuart (2007) in ensuring that balance is achieved in covariates that strongly predict the outcome rather than in weak predictors. We do not assess covariate balance by means of *ttests*, which tested the hypothesis of equal means in control and treated units in the matched sample. This approach was often adopted in the past but is now rejected in many studies, including Austin (2009, 2011a) and Ho et al. (2007). According to these, *ttests* are influenced by sample sizes and are thus not reliable.

strict criteria to estimate the impact stemming from the receipt of one grant only (see panel (b) in Figure 1). More specifically, we want to ensure that firms receiving a subsidy in year $t = 0$ had not received any financial aid in the preceding five years, at least.[23] This condition relies on the assumption that firms do not exhibit any deviation in outcome variables five years after receiving a subsidy and allows us to include firms that received more than one supporting measure over the observed years. Because our outcome variables are forward-looking (grey solid line in the figure), we additionally ensure that selected treatments are not followed by other supporting measures within the period used in the calculation of growth rates. In other words, we measure the effects of a subsidy given at year $t = 0$ on the cumulative forward growth rates computed up to $t + j$, where $j = 1, ..., 5$, using a subsample of treatments that are isolated from the receipt of subsequent subsidies during the periods $t + 1$ to $t + j$. We do not impose conditions on additional treatments received further into the future since these do not influence the outcome variables used to estimate the effects of subsidies received at time t. In estimating the impact caused by objective-specific subsidies, we additionally ensure that firms did not receive any other type of grant in the same year.

The selection of suitable control units to be used in matching is also important. The econometric literature on policy evaluation typically considers two choices of control subjects: not-yet-treated units and never-treated units. Restricting the pool of controls to never-treated units prevents the estimated effects of subsidies from being influenced by the potentially persistent effects of control units' past treatments. Failing to ensure this could lead to matches, where both treated and control units receive treatment, albeit in different years. Opting for not-yet-treated units – in addition to the above – not only increases the set of available controls but also improves the similarity between treated and non-treated units. Control units that get treatment at a future year likely experience similar dynamics leading to future treatment to those experienced by already-treated units. As before, we take into account the forward-looking nature of our outcome variables. As shown in panel (a) of Figure 1, if treatment occurs at time $t = 0$ and the goal is to measure its effect on the cumulative forward growth rate computed up to time $t + j$ (grey solid line in the figure), we ensure that control units did not benefit from public support up to, and including, year $t + j$. We do not impose such condition in the year afterward: a firm could have obtained a subsidy in year $t + j + 1$ and still be an appealing control unit for forming a matched pair. Note that we define the condition of "no past treatment" as one in which firms did not receive any type of state funds, including those initially discarded due to subsidy type selection (including 2001 data). In this way, we ensure that controls had not benefited even from alternative sources of state aid as they would still affect our estimations of treatment effects.

The fact that firms may have obtained additional state grants over the years leads us to investigate how the effects of subsidies change in comparison to our benchmark analysis based on the strict identification criteria used to select well-identified treatments. Hence, we form another sample of treatments by selecting all those subsidies that, while being the first public support firms receive (or, better said, the first after at least five years of no state support), can be followed by one or more additional interventions within five years since their receipt (see panel (c) of Figure 1). Hence, the estimated effects based on this wider sample capture both direct effects of the first grant received in year t and indirect effects stemming from the receipt of additional subsidies sometime between $t + 1$ and $t + i$, where $i = 2, ..., 5$, that also

[23]This condition applies automatically upon market entrance, which allows us to retain firms' initial values.

influence the growth rate computed up to $t+i$.[24]

Because the literature has found that subsidies lower the likelihood of market exit in recipient firms (e.g., Srhoj et al., 2021, Koski and Pajarinen, 2015, and Schweiger, 2011), we also estimate the effects of subsidies limited to a restricted sample of firms which survive throughout the 5-year interval used to compute cumulative forward growth rates up to year $t+5$. By doing so, we avoid ATETs from being influenced (likely inflated) by firms that survive thanks to the grant. Although the samples are not quite comparable and ATETs cannot be nested, this exercise provides an attempt to decompose the effects of subsidies as it allows to extract the portion of the effect due to variables of interest while conditioning on survival that contributes to overall ATETs.

Finally, we outline our classification of subsidies based on their purposes and objectives. Unlike Burger and Rojec (2018), who also used Slovenian firm-level data on subsidies, we do not use the classification into categories applied in the original data because subsidies pursue a wide array of aims within each category, often sharing the same program objective while appearing in different categories. For instance, both "environmental subsidies" and "subsidies to SMEs" include measures intended to create new jobs as well as others supporting productivity improvements. Hence, following the original classification would not allow us to create distinct groups of subsidies, each aiming at a common target variable. Instead, we relied on all available information describing subsidy programs (i.e., "aims", "program name", and "category") to allocate subsidies to one of four groups: labor-support subsidies, capital-deepening subsidies, productivity-enhancing subsidies, and general subsidies. While the first three groups allow us to capture subsidies' "purest" (and potentially the largest) effects with the same main objectives, we gather in the last group all those subsidies whose objectives are rather general and could thus cause a combination of effects. This is the case, for example, of grants supporting the local economy (e.g., municipality) or the setup of new businesses.

4 Data

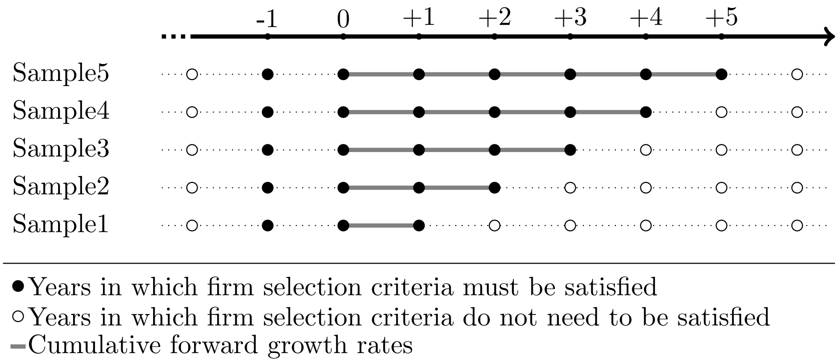
4.1 Data Sources

We rely on data attained from two main sources. Firm-level accounting data are provided by the Agency of the Republic of Slovenia for Public Legal Records and Related Services (AJPES) and comprise balance sheets and income statements for all Slovenian firms (with the exclusion of sole proprietorships and banks[25]) from 2002 to 2021. We additionally used AJPES data from 1994 onward to construct variable *firm age*. Firm-level subsidy data, available from 2001 to 2017, are provided by the Ministry of Finance of Slovenia, which keeps a record of all State aid payments ("State Aid dataset"). This dataset includes information about the payee of the subsidy, the subsidy category, the program scheme, the specific aim, the subsidy amount, and the instrument type. The two datasets are linked using unique firm registry num-

[24] We limit this exercise to the sole case of pooled subsidies as subsequent treatments might be of different types, which would affect the estimates of the impact of objective-specific subsidies. Furthermore, allowing the multiple-treatment context in ATET estimations using several size-based treatments implies that future subsidies are likely to be of different amounts, leading firms to fall into different treatment groups from the one first used in the estimation.

[25] AJPES has also balance sheets and income statements for sole proprietors. However, we do not use these as (i) reporting is not comparable (the sole proprietors report less detailed items) and (ii) capital and labor incomes are not separable, which prevents calculation of comparable measures of productivity.

Figure 2: Firm Selection Samples



Source: Own work.

bers. Table A.1 in Appendix A provides a list of the variables extracted from both data sources followed by a brief description.

4.2 Sample Construction

We apply sample selection criteria to both datasets. The sample of firms is restricted to economically active firms, which we define as having at least one employee and positive value-added. We also exclude firms operating in broad industries outside the scope of the present research like agriculture, utilities and public administration, and defense and social security.[26] Regarding subsidies, we only retain public financial support provided in the form of grants, representing over 80 percent of the aggregate value for all forms of financial aid in the original State Aid data. We exclude other types of instruments, such as affordable loans, loan write-offs, guarantees, tax reductions/delays, and equity investments, because they imply different temporal structures of benefits that may not be comparable to grants in the present value terms. Subsidies used for agriculture, fisheries, natural disasters, mining, culture, and the restructuring of firms are also dropped because they are not compliant with the scope of the present research. Likewise, we exclude employment subsidies targeting the disabled as these are motivated by social policy concerns rather than economic ones.

To use the largest sample possible in each estimation, we do not require firms to comply with selection criteria in all years, but only in those used in ATET estimations. As shown in Figure 2, we form five samples, each requiring firms to comply with firm selection criteria in the preceding year, given the inclusion of pre-treatment variables in our propensity score model, throughout all years up to the last used to compute the forward-looking outcome variable upon which the effect of subsidies is estimated. Estimations made on the restricted sample of firms continuously operating during five-year windows are computed for *Sample5* independently of the outcome variable.

Further details about the data cleaning procedure are provided in Table A.2, Appendix A. The original AJPES dataset consists of an unbalanced panel of firms either observed throughout the entire time interval or with different market entry/exit dates.[27] There are 1,134,363 observations uniquely

[26] Specifically, we exclude the following broad industries according to the 2-digit NACE Rev. 2 (2008) classification: agriculture, forestry, and fishing; mining and quarrying; water supply, sewerage, waste management, and remediation activities; public administration and defense, and compulsory social security; and electricity, gas, steam, and air conditioning supply.

[27] Despite the obligation to report to AJPES, there are cases of "year jumps" unrelated to firm exit.

identified by firm identifier and year, for a total of 125,264 firms observed between 2002 and 2021. Applying firm selection criteria[28] reduces the number of observations to 443,966 and the number of firms to 57,870, a reduction of approximately 60.9 percent and 53.8 percent, respectively. Limiting the sample of potential treatments to 2016 reduces observations and firms by 10.7 and 7.7 percentage points, respectively.

The State Aid dataset is made up of 284,795 observations (or treatments). These are not uniquely identified by the firm identifier and year (of subsidization) as firms often participated in multiple subsidy schemes in the same year. In total, 86,053 entities (including sole proprietorships) received state support at least once between 2001 and 2017. Applying subsidy selection criteria reduces the sample roughly by half to 145,765 treatments, corresponding to 68,851 subsidized entities. Summing the amounts of different subsidy schemes to firm-year level and additionally applying firm selection criteria, restricting the year of last treatment to 2016, yields 32,799 treatments that are uniquely identified by firm identifier and year, for a total of 13,775 subsidized firms. Hence, in the merged sample, 28.6 percent of firms have been subsidized at least once between 2002 and 2016. The subset of treatments is slightly smaller due to exclusion of relative subsidies above 25 percent of firm's value added, while larger reductions of sample are due to application of identification criteria selecting treatments according to the procedure outlined in Section 3. This provides a first indication of how frequent multiple treatments are.

4.3 Sample Description

The final sample of treatments is made up of subsidies from several categories foreseen by the legislator: employment subsidies (60 percent of total grants, with over 40 percent of available resources dedicated), subsidies to SMEs, subsidies incentivizing R&D and innovation, regional subsidies, subsidies for providing employees with training, environmental subsidies, and other minor categories (for more details see Table A.3 in Appendix A). Several subsidy schemes often share the same program objective while appearing in different categories. Hence, we dismembered these broad subsidy categories and allocated program schemes to one of four groups. Following our classification, labor-support subsidies include all hiring and wage subsidies; capital-deepening subsidies mostly include investments with environmental goals, such as energy savings and adaptation of production standards; productivity-enhancing subsidies mostly include incentives for R&D and training of labor; and finally, general subsidies include regional aid, support to SMEs, and environmental subsidies that incentivize the expansion of both production inputs (see Table A.4 in Appendix A for the complete list of subsidies that fall into each group following our classification, together with the associated aim, original category, frequency, and total value). The resulting sample of objective-specific subsidies (see Table 2, upper panel) is mostly composed of labor-support subsidies, over 60 percent of the total number of grants and 40 percent of all resources. The second most common type of support was productivity-enhancing subsidies, over 26 percent in terms of numbers but only 20 percent of resources. While less frequent, general subsidies were on average higher and thus totaled over 30 percent of all resources. Only a tiny fraction of the sample comprises capital-deepening subsidies. However, these feature a higher average grant than labor-support and productivity-enhancing subsidies (but not as high as general subsidies). In the well-identified defined treatment sample (see Table 2, lower panel), the labor-support subsidies are even more prominent (52 percent of all resources),

[28] Our data description refers to the widest sample, i.e. *Sample1*, as indicated in Figure 2.

Table 2: Summary Statistics of Objective-Specific and Pooled Subsidies

Subsidy Type	N		Mean (in EUR)	Std. Dev.	Value	
	Freq.	Percent			EUR	Percent
	Sample of treatments allowing for future treatments					
Labor-support	5,519	60.36	5,230	6,971	28,863,558	40.65
Capital-deepening	208	2.27	16,832	40,762	3,500,953	4.93
Productivity-enhancing	2,420	26.47	5,951	19,182	14,402,401	20.28
General	996	10.89	24,335	70,900	24,238,034	34.14
Pooled subsidies	8,787	-	7,920	28,306	69,588,728	-
Sum of obj-specific subsidies	9,143	100			71,004,946	100
	Sample of well-identified treatments					
Labor-support	4,112	65.64	5,117	6,652	21,042,264	52.03
Capital-deepening	143	2.28	13,134	15,748	1,878,187	4.64
Productivity-enhancing	1,497	23.90	3,578	11,202	5,356,870	13.25
General	512	8.17	23,754	77,413	12,162,293	30.08
Pooled subsidies	6,121	-	6,567	24,634	40,196,716	-
Sum of obj-specific subsidies	6,264	100			40,439,614	100

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The values of subsidies are reported in current EUR and the data are limited to *Sample1* (See Figure 2 for definitions). The total number of individual objective-specific subsidies is higher than the total number of pooled subsidies because some firms received more than one grant in the same year. The discrepancy in the total volume between pooled subsidies and the sum of objective-specific subsidies is due to the imposition of the higher bound at 25 percent of firm's value added. Pooling different subsidies by year implies that the relative size of the total subsidy amount exceeds the higher boundary more frequently, leading treatments to be discarded.

mostly at the expense of productivity-enhancing grants (13 percent of resources compared to 20 percent in the larger sample).

Table 3 describes the number and average size of pooled subsidies by treatment sizes and the two samples. Interestingly, the average absolute value of subsidies increases rather slowly from treatment size 2 onward (and decreases in one case), suggesting that the average size of firms decreases the larger the treatment size. More specifically, Table A.5 of Appendix A shows that, as treatment size increases, the average amounts of labor, capital, productivity, value added, sales, and, although slightly, firm age decrease, while the average debt-to-assets ratio steadily increases. The corresponding breakdown by treatment type reveals that the average size of firms is higher among recipients of capital-deepening subsidies while denoting a higher average indebtedness and lower average productivity. In contrast, recipient firms of labor-support subsidies are on average the smallest.

Our identification criteria for selecting treatments are justified by subsidized firms' high likelihood of receiving additional financial support in the years following the first grant. In this regard, Table A.6 in Appendix A provides an overview of how frequently firms obtained multiple subsidies. Specifically, panel (a) shows that, based on our subsidy classification, firms received only one type of public support in the same year in almost 88 percent of cases, while the remaining 12 percent obtained more types of support simultaneously. However, the degree of same-year participation in multiple subsidy schemes is higher when using disaggregated data at the individual level (see panel (b)). Panel (c) shows that participation in several subsidy programs over the observed years is also frequent. Only about half of the subsidized firms benefited from just one grant over the years; almost 20 percent of firms received financial support twice and another 11 percent three times. The maximum number of grants firms received over

Table 3: Subsidy Characteristics, by Size of Treatment

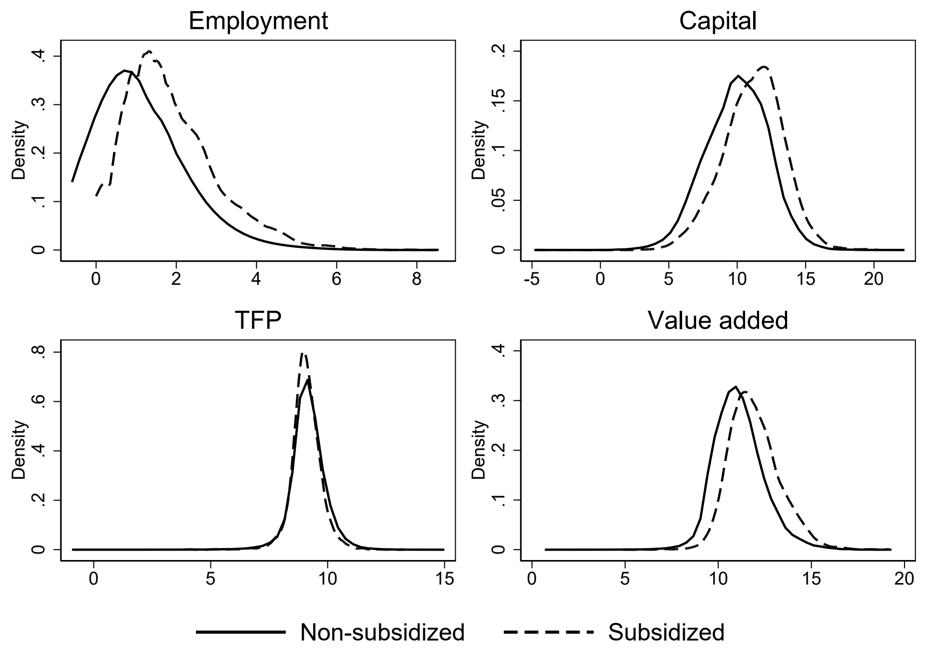
Treatment size	Sample of treatments allowing for future subsidies			Sample of well-identified treatments		
	Freq.	Mean		Freq.	Mean	
		Relative	Absolute (in EUR)		Relative	Absolute (in EUR)
1	5,467	0.017	5,082	3,594	0.018	4,112
2	1,678	0.072	11,856	1,240	0.072	9,796
3	826	0.123	12,280	647	0.123	9,404
4	502	0.173	13,808	397	0.174	10,253
5	314	0.224	15,399	243	0.223	12,831
Total	8,787	0.054		6,121	0.058	

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The values of subsidies are reported in current EUR and the data are restricted to *Sample1*. Treatment size is based on the relative subsidy size, computed as subsidy amount relative to (pre-treatment) value added, according to the intervals outlined in Table 1. Treatments allowing for future subsidies are selected according to the identification criteria shown in Figure 1, panel (c), while well-identified treatments are selected such as to isolate treatment effects stemming from one-time subsidies as shown in Figure 1, panel (b).

the years in the sample is 14, which only involves a tiny portion of firms (well below 1 percent of the total).

The subsidized and non-subsidized firms cannot be regarded as random samples from the population of firms due to selection into treatment. Figure 3 graphically depicts the underlying differences between the two groups of firms by showing firm-level distributions for (log of) employment, capital, value added, and productivity. The subsidized firms appear to have more employees and capital, and higher value added than their non-subsidized counterparts, whereas the two groups are similar in terms of productivity. Qualitatively similar differences are also obtained when using the whole sample of treatments.

Figure 3: Distributions of Size and Performance Measures: Subsidized vs. Non-subsidized Firms



Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The figure displays stochastic kernel density plots adjusted for the lower bounds, with Epanechnikov kernel and optimal bandwidth. The sample of subsidized firms consists of those that received public support in a given year and is restricted to units that may have received additional support in the years following the first grant (i.e., identified by the criteria outlined in Figure 1, panel c). Non-subsidized firms are firms that have not received a grant yet.

Next, Table 4 shows the composition of samples of firms in terms of size based on employment. Firms with 1-9, 10-49, 50-249, and 250 and more employees, are denoted as micro, small, medium, and large, respectively. The sample is mainly composed of micro and small firms, 86 percent and 11.6 percent of the total, respectively. These firms are also the recipients of most grants, over 70 percent micro firms and 23.8 percent small firms, while obtaining roughly 40 percent and 29.8 percent of the total available resources, respectively. As the average subsidy greatly increases with the size of firms, medium and large firms combined thus obtain approximately 30 percent of resources, despite receiving only 5.8 percent of the grants. Appendix A provides additional sample decompositions. According to Table A.7, firms of

Table 4: Sample Characteristics, by Firm Size

	Firms		Grants			
Firm Size	Total	Subsidized	Freq.	Mean	Std. Dev.	Value
Micro	32,935	5,932	6,184	4,500	6,643	27,825,440
Small	4,436	1,983	2,092	9,909	22,889	20,729,106
Medium	785	418	443	34,134	75,203	15,121,497
Large	108	66	68	86,951	187,749	5,912,687
Total	38,264	8,399	8,787			69,588,730

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to the sample used in estimations allowing for future treatments. The values of subsidies are reported in current EUR. "Firms" include both subsidized and not-yet-subsidized firms. Firm size is based on the number of employees. Firms with 1-9, 10-49, 50-249, and 250 and more employees, are denoted as micro, small, medium, and large, respectively. In the second and third columns, firms are classified based on their average employment over the years.

all ages are present in the sample, although the frequency naturally increases with firm age. Grants are concentrated in firms up to the age of 18 years of age, becoming less frequent in older firms. Table A.8 shows that industries with the highest number of firms (wholesale and retail trade, manufacturing, and professional, scientific and technical activities) received the greatest resources in terms of both number of grants and total value. However, manufacturing obtained by far the greatest amount of resources, 37 percent of the total, displaying a substantially higher average subsidy than the other industries. The second most subsidized industry is wholesale and retail trade, with 17 percent of the total resources allocated. A similar breakdown with respect to regions is found in Table A.9. 17,469 firms, over 45 percent of the total in our sample, are in the Central Slovenia Statistical Region (Osrednjeslovenska), but only 37 percent of the total subsidized firms that received about 37 percent of grants in terms of number and value are located there. The fact that firms located in other regions disproportionally benefited from grants compared to the Central Slovenia Statistical Region indicates the intention of the legislator to favor firms in rural/provincial areas.

Before performing matching, we compared the simple averages of the cumulative forward growth rates (up to year $t+1$, $t+3$, and $t+5$) between controls (treatment size 0 – the not-yet treated units) and the treated firms (treatment sizes 1 to 5) in the corresponding samples of reference (i.e., Sample1, Sample3, and Sample5). The resulting charts, depicted in Figure 4 in Appendix A, show that treatment size is positively related to both employment growth (particularly evident in the short run) and value added growth, whereas the relationship with the growth of capital is flat once firm receives the treatment. Interestingly, the average growth rates of productivity of the not-yet subsidized firms are higher than those of the recipient firms in the short term, while it is slightly lower over the longer horizon at higher treatment sizes. However, these statistics do not control for the observed differences between non-subsidized and subsidized firms, which is dealt with in the next section.

5 Results and Discussion

We begin the discussion of the results with the impact of pooled subsidies – a measure summing grants of all types – that firms received in a given year irrespective of the relative subsidy size under the presumption of homogeneity of effects. Then, we turn to the question of whether increasing subsidy size determines heterogeneous (and potentially increasing) ATETs. In the third and final part, we investigate how treatment effects change depending on the specific objectives of subsidy programs.

5.1 Treatment Effects of Subsidies Disregarding the Size of Subsidy

To investigate the impact of receiving a subsidy, we use a unique treatment indicator taking value 1 in case of receipt of grant and 0 otherwise (additional criteria are applied as described in Figure 1).[29]

Table 5: Effects of Pooled Subsidies – One-time support

Outcome variable	t+1	t+2	t+3	t+4	t+5
			Full Sample		
Employment	0.1907***	0.1411***	0.1202***	0.1114***	0.1012***
	(0.0084)	(0.0114)	(0.0139)	(0.0173)	(0.0212)
Capital	0.1754***	0.1585***	0.1446***	0.1771***	0.2133***
	(0.0199)	(0.0276)	(0.0352)	(0.0447)	(0.0536)
TFP	-0.1018***	-0.0786***	-0.0645***	-0.0732***	-0.0182
	(0.0111)	(0.0143)	(0.0164)	(0.0183)	(0.0240)
Value added	0.1076***	0.0817***	0.0602***	0.0690***	0.0627**
	(0.0124)	(0.0161)	(0.0199)	(0.0239)	(0.0300)
			Restricted Sample		
Employment	0.1480***	0.1137***	0.1098***	0.1194***	0.1012***
	(0.0134)	(0.0156)	(0.0184)	(0.0196)	(0.0212)
Capital	0.1222***	0.1132***	0.1450***	0.1710***	0.2227***
	(0.0352)	(0.0415)	(0.0461)	(0.0510)	(0.0551)
TFP	-0.1163***	-0.0964***	-0.0961***	-0.0993***	-0.0991***
	(0.0191)	(0.0210)	(0.0208)	(0.0220)	(0.0233)
Value added	0.0432**	0.0261	0.0305	0.0532**	0.0627**
	(0.0198)	(0.0223)	(0.0246)	(0.0268)	(0.0300)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below). The sample of treatments includes only well-identified treatments (see criteria outlined in panel (b) of Figure 1). "Restricted Sample" refers to a time-invariant sample of continuously surviving firms over a 5-year temporal window since the receipt of subsidy. Common support is ensured by caliper matching 0.2 and the balancing property is satisfied in all estimations. *, ** and *** denote statistical significance at 10%, 5% and 1% level, respectively.

Results reported in Table 5 reveal that receiving a subsidy causes a substantial increase in recipient firms' inputs and value added while bearing a productivity loss. Furthermore, there appears to be a transitory impact on employment and a more enduring effect on capital. Specifically, the increase in

[29] To make these results comparable with those that are obtained using size-based treatments, we apply the restriction of including treatments that are at most 25 percent of firm's value added.

employment and capital in the full sample is, on average, 19 and 17.5 percentage points (p.p. from here on) higher in the first year, respectively, than it would have been in the absence of financial aid. The effect on employment is decreasing over time, although still present after 5 years, whereas that on capital persists and further builds up in the long run. Productivity growth is on average over 10 p.p. lower in the first year than it would have been in the absence of the grant. The effect is diminishing over time, disappearing after 4 years. In the restricted sample, which features only surviving firms, the slowdown in productivity growth is more pronounced and persistent, as it equals on average about 11.6 p.p. in the first year, subsequently settling between 9.5 and 10 p.p. The increase in value added in the full sample is, on average, 10.8 p.p. higher in the first year than it would have been without the incentive.

To foster the comparison of calculated ATETs, we provide some back-of-the-envelope calculations, expressing the estimated effects in terms of increase in value added (in EUR) per EUR 1 of subsidy (see Appendix C for conversion formulas). Using initial labor productivity, the estimated effect of subsidies on employment after one year is equivalent to an increase in value added of about EUR 3.28 per EUR 1 subsidy. This is almost twice the size of the impact on value added – the estimated ATETs suggest EUR 1.85 of additional value added was produced per unit of subsidy. However, both the effect and the additional value added raised by the subsidy decrease over time, suggesting that subsidies represent a mere temporary boost for value added. Still, after five years, public support to firms gives rise, on average, to EUR 1.08 of additional value added per unit of subsidy spent. In the restricted sample, the effect in the first year is significantly lower, around 4.3 p.p. of higher added value, or EUR 0.79 of additional value added per unit of subsidy.

For comparison, Table 6 provides the estimates obtained by expanding the sample by allowing firms to receive additional support within five years after the first subsidy. This setting leads to substantially higher positive effects on inputs and value added, but the most remarkable aspect is the extent to which they continue to grow over time. For example, capital growth is on average higher by 22.4 p.p. in the first year than it would have been without at least one subsidy, an effect that steadily accrues to over 42 p.p. after five years, approximately double the effect obtained with strictly one grant. Similarly, the accumulated effects on employment after five years are more than double the size obtained in the one-time-support sample, whereas those on value added have almost tripled. Interestingly, the possibility of obtaining additional grants in subsequent years does not improve firms' productivity. On the contrary, the productivity loss is slightly greater than in the one-time-support sample.

Allowing for multiple treatment results in higher estimated effects due to the impact of grants received after t. This reveals another indirect channel through which subsidies affect our variables of interest: an increased likelihood of receiving additional support in the future after the initial grant. In Table 6, we report the estimated probability of receiving additional support anytime between time t and $t+i$, with $i = 1,...,5$. The probability of receiving another grant after one year is 23.5 p.p. higher for firms that already received the financial aid than those that did not. This probability further increases the longer is the time frame considered, totaling 44 p.p. after five years. We attribute higher likelihood of receiving subsidies of past winners to the following reasons. Firstly, firms that successfully applied for grants in the past learned how to write grant applications at lower costs and/or with greater likelihood of success (e.g. by identifying key ingredients of applications). Secondly, past success may lead

Table 6: Effects of Pooled Subsidies – Unrestricted support

Outcome variable	t+1	t+2	t+3	t+4	t+5
			Full Sample		
Future Treatment	0.2353***	0.3222***	0.3838***	0.4207***	0.4445***
	(0.0048)	(0.0054)	(0.0058)	(0.0061)	(0.0063)
Employment	0.2106***	0.2036***	0.2120***	0.2357***	0.2427***
	(0.0074)	(0.0090)	(0.0104)	(0.0118)	(0.0144)
Capital	0.2241***	0.2781***	0.3324***	0.3845***	0.4225***
	(0.0168)	(0.0211)	(0.0246)	(0.0289)	(0.0341)
TFP	-0.1047***	-0.0844***	-0.0908***	-0.0845***	-0.0902***
	(0.0094)	(0.0104)	(0.0115)	(0.0128)	(0.0142)
Value added	0.1285***	0.1539***	0.1644***	0.2085***	0.2129***
	(0.0103)	(0.0123)	(0.0140)	(0.0157)	(0.0188)
			Restricted Sample		
Future Treatment	0.2543***	0.3338***	0.3910***	0.4233***	0.4445***
	(0.0055)	(0.0060)	(0.0062)	(0.0062)	(0.0063)
Employment	0.1817***	0.1909***	0.2112***	0.2269***	0.2427***
	(0.0091)	(0.0109)	(0.0123)	(0.0133)	(0.0144)
Capital	0.2301***	0.2736***	0.3204***	0.3638***	0.4141***
	(0.0213)	(0.0249)	(0.0280)	(0.0307)	(0.0328)
TFP	-0.1012***	-0.0709***	-0.0774***	-0.0704***	-0.0785***
	(0.0116)	(0.0124)	(0.0134)	(0.0139)	(0.0145)
Value added	0.1143***	0.1351***	0.1677***	0.1904***	0.2129***
	(0.0127)	(0.0142)	(0.0163)	(0.0173)	(0.0188)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below). Treatment effects are identified according to criteria outlined in panel (c) of Figure 1. "Restricted Sample" refers to a time-invariant sample of continuously surviving firms over a 5-year temporal window since the receipt of subsidy. Common support is ensured by caliper matching 0.2 and the balancing property is satisfied in all estimations. *, ** and *** denote statistical significance at 10%, 5% and 1% level, respectively.

firms to attribute higher likelihood of success in subsidy tenders than those who never applied or applied unsuccessfully. Lastly, administrators might evaluate applications of past winners more favorably.

5.2 Treatment Effects of Subsidies by Relative Size of Subsidy

We use several treatment indicators (based on five intervals shown in Table 1) to investigate if the relative subsidy size matters. In what follows, we discuss the results pertaining to the full sample, presented in Table 7, while providing those limited to the restricted sample (only firms operating throughout the 5-year interval) in Table B.10 of Appendix B for robustness.

Our results show that the magnitude of treatment effects increase with relative size of subsidy. Although true for all outcome variables, this is particularly evident when considering the effects on employment. The increase in employment in the first year is, on average, roughly 11 p.p. higher than it would have been in the absence of the grant for firms in the first treatment group. The impact monotonically increases with the relative subsidy size: firms in the highest treatment group (i.e., 5) experienced a 45.8 p.p. higher employment growth than they would have had without the incentive. Similarly, the higher the financial aid received the lower is productivity growth. Specifically, the productivity growth in the first year is, on average, 6.6 p.p. and 19.9 p.p. lower for treatment levels 1 and 5, respectively, than it would have been in the absence of the grant. Hence, these results not only confirm that grants are detrimental to firms' productivity as observed in the most general case with one unique treatment indicator, but also suggest that the receipt of particularly large relative subsidies does nothing to foster productivity improvements or, at least, reduce the loss.

Overall, our results suggest that subsides between 20 percent and 25 percent of a firm's value added lead to particularly high absolute effects. Given that the average firm size is smaller in higher treatment groups (see Section 4.3), our results offer an alternative view on why several studies have found that subsidies are mostly effective when targeting small firms (e.g., Chiappini et al., 2022, Vanino et al., 2019, Criscuolo et al., 2019, Decramer and Vanormelingen, 2016, Bronzini and Iachini, 2014, and Bia and Mattei, 2012). This literature claims that subsidies help small firms more because they face more difficulties in accessing capital from banks and investors and thus public support relaxes their budget constraint. We argue that another or alternative explanation could be that these firms benefit from relatively larger subsidies than other firms.

However, from a policy perspective, several results stand against the use of particularly large subsidies relative to a firm's size. Firstly, the presence of decreasing marginal effects (in absolute values) in all four variables of interest. In treatment group 4, marginal effects turn negative for all outcome variables except employment (which still experiences a substantial slowdown by that group) denoting the presence of diminishing effects. For instance, the average short-term effect of subsidies equal to 20-25 percent of a firm's value added (treatment level 5) on capital is only slightly higher than that obtained with subsidies equal to 10-15 percent of a firm's value added (treatment level 3) – 0.248 vs. 0.243, respectively. Secondly, the ATETs of receiving the actual subsidy level compared to receiving a lower subsidy level (see Table B.12 of Appendix B) seem to support the conclusions offered by Bia and Mattei (2012) and Adorno et al. (2007) advancing the view of an optimal subsidy size, beyond which additional increases in the amount of support do not correspond to large enough increases in the desired effects. Methodologically, this approach brings the advantage of removing the selection biases stemming from firms' decision to apply and authorities' selection of applicants, since both treated and control units benefited from support (yet some selection bias remains with respect to the size of subsidy awarded). We find that increasing treatment sizes lead to increasing effects up to treatment level 3 at most and is only limited to employment and value added. Thirdly, estimated effects on values added in terms of increase in value added per unit of subsidy applying formula outlined in Appendix C show that EUR 1 of public support causes, on average, additional value added of roughly EUR 4 after one year in the first treatment group. It more than halves in the next group (EUR 1.88) and reduces even further with larger treatments (EUR 1.16, EUR 0.57, and EUR 0.96 in levels from 3 to 5, respectively). Similarly, the impact on employment is on average equivalent to EUR 6.18 of additional value added per unit of subsidy in the first treatment group. As before, the impact halves in the second group (approx. EUR 3) and further reduces as treatment size increases (EUR 2.39, EUR 1.74, and EUR 2.05 in groups 3 to 5, respectively). Finally, we find several instances of statistically insignificant effects corresponding to higher treatment sizes, an aspect that gets accentuated as the time since receiving the grant increases. The absence of long-term impacts at higher treatment sizes implies that effects caused by relatively small subsidies are more enduring.

Restricting the sample to continuously operating firms within the five-year interval after the inter-

Table 7: Effects of Pooled Subsidies, by Relative Size of Subsidy (Full Sample)

Treatment level	t+1	t+2	t+3	t+4	t+5
			Effects on Employment		
1	0.1096***	0.1084***	0.0877***	0.0574***	0.0776***
	(0.0104)	(0.0145)	(0.0179)	(0.0206)	(0.0263)
2	0.2196***	0.1679***	0.1251***	0.1096***	0.0601[b]
	(0.0191)	(0.0259)	(0.0307)	(0.0420)	(0.0522)
3	0.2928***	0.1687***	0.1656***[a]	0.1652***[a]	0.1294**[a]
	(0.0259)	(0.0356)	(0.0442)	(0.0527)	(0.0659)
4	0.3027***	0.2420***[a]	0.2470***[a]	0.0572	0.1329[b]
	(0.0323)	(0.0468)	(0.0569)	(0.0680)	(0.1015)
5	0.4585***[a]	0.2678***[a]	0.2303***[b]	0.1536*[a]	0.2210*[b]
	(0.0438)	(0.0577)	(0.0795)	(0.0882)	(0.1143)
			Effects on Capital		
1	0.1242***	0.1295***	0.1395***	0.1248**	0.2238***
	(0.0242)	(0.0326)	(0.0414)	(0.0509)	(0.0632)
2	0.2256***	0.1096*	0.2218***	0.1977*	-0.0954[a]
	(0.0470)	(0.0634)	(0.0752)	(0.1027)	(0.1398)
3	0.2430***	0.4199***[a]	0.2481**	0.2308[a]	0.2983[a]
	(0.0663)	(0.0972)	(0.1202)	(0.1471)	(0.2286)
4	0.1737*	0.1151[a]	0.2174[a]	0.2923[a]	0.3741[b]
	(0.1011)	(0.1403)	(0.1915)	(0.2223)	(0.2430)
5	0.2484*[a]	0.0313[a]	0.1483[a]	-0.2091	0.1830[b]
	(0.1281)	(0.1486)	(0.2051)	(0.3831)	(0.4486)
			Effects on TFP		
1	-0.0656***	-0.0629***	-0.0257	-0.0510**	-0.0375
	(0.0139)	(0.0164)	(0.0186)	(0.0227)	(0.0278)
2	-0.1622***	-0.1008***	-0.0581*	-0.0525	-0.1660***[a]
	(0.0268)	(0.0304)	(0.0340)	(0.0445)	(0.0566)
3	-0.1882***	-0.1172**[a]	-0.1726***	-0.1244*[a]	-0.0149[a]
	(0.0416)	(0.0492)	(0.0589)	(0.0685)	(0.0945)
4	-0.1338**	-0.1136*[a]	-0.1126[a]	-0.2116**[a]	-0.2587*[b]
	(0.0597)	(0.0680)	(0.0756)	(0.1025)	(0.1529)
5	-0.1985**[a]	-0.2119**[a]	-0.1490[a]	-0.3568**	0.0853[b]
	(0.0832)	(0.0859)	(0.1012)	(0.1640)	(0.1445)
			Effects on Value Added		
1	0.0707***	0.0550***	0.0489**	0.0473*	0.0527
	(0.0149)	(0.0193)	(0.0230)	(0.0274)	(0.0362)
2	0.1364***	0.0767**	0.0511	0.0833	-0.0068[b]
	(0.0284)	(0.0370)	(0.0410)	(0.0517)	(0.0693)
3	0.1419***	0.0552	0.0423[a]	0.0470[a]	0.0727[a]
	(0.0405)	(0.0557)	(0.0694)	(0.0752)	(0.1047)
4	0.0994*	0.1662**[a]	0.1612*[a]	-0.0977	0.1431[b]
	(0.0536)	(0.0751)	(0.0918)	(0.1036)	(0.1517)
5	0.2138***[a]	0.1219[a]	0.2108[b]	-0.1575	0.2792*[b]
	(0.0731)	(0.0956)	(0.1362)	(0.1805)	(0.1590)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below). The sample of treatments includes only well-identified treatments selected according to the identification criteria outlined in panel (b) of Figure 1. Common support is ensured by caliper matching 0.2. *, ** and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

vention (see Table B.10 in Appendix B) produces ATETs that are, in general, more modest than those observed in the more general case, but they nevertheless confirm the observations made above. This suggests that the highest individual treatment effects take place in firms that either exit the market sometime within five years after obtaining the public incentive or leave our sample of firms because they no longer comply with our minimum criteria defining “active firms”.

As in Section 5.1, the estimated effects are higher if multiple treatments are allowed (see Table B.11 of Appendix B and identification criteria in panel (c) of Figure 1) and the most interesting results concern long-term effects. For instance, after five years the cumulative growth of capital is, on average, between 37.5–62.2 p.p. higher than it would have been primarily due to receipt of one or more subsequent incentives, while the corresponding capital increase in the main analysis is, on average, only 22.4 p.p. for firms in the first treatment group and no impact is found for larger treatment levels. Hence, we argue that the possibility of obtaining multiple grants over the years translates into a permanent impact on recipient firms of all treatment groups. This applies to all outcome variables of interest, including productivity, which does not benefit from the possibility of multiple support measures within short time frame. Table 8 adds to the finding that the likelihood of obtaining another grant is higher for firms that received subsidies in time t by showing that this probability depends on the first treatment level. Specifically, it is the firms in the first treatment group (based on size of subsidy) that are more likely to resort to additional support (approximately 27.6 p.p. higher than it would have been, had they not already obtained support in the first place). As pointed out earlier, these firms are on average substantially bigger than those in larger treatment groups. The probability of obtaining additional support reduces drastically to 18 p.p. for those in the second treatment group and further decreases, though only slightly, for firms in larger treatment groups, as average firm size decreases along.[30] This finding suggests, in addition to the discussion of lower application costs and a learning effect associated with multiple subsidy applications pointed out in the previous Section, that firms’ employment structure could play an important role in the provision of subsidies. Larger firms have a greater number of employees, a fraction of which works on administrative tasks only. Having at disposal these workers allows firms to obtain public support more often (or more efficiently – a hypothesis we cannot verify due to the lack of information on rejected applications) as these workers can specialize in subsidy applications. In contrast, smaller firms usually do not have enough employees that would allow them to reach a comparable degree of specialization in subsidy application.

Yet, despite a higher probability of obtaining additional support within five years, firms in the first treatment group experience lower effects on the cumulative growth rates of any variable of interest compared to firms of other groups. Actually, the effects steadily increase together with treatment size. Hence, our results seem to corroborate the hypothesis that larger relative subsidies lead to bigger effects, even more so if followed by additional financial aid, which however occurs with a lower likelihood than if firms had first received a smaller treatment.

[30]While firm size decreases as treatment level increases, the drastic reduction in firm size occurs from treatment level 1 to treatment level 2. See Table A.5 for an overview of firms by treatment level.

Table 8: Likelihood of Obtaining Additional Support, by Relative Size of Subsidy

Treatment level	t+1	t+2	t+3	t+4	t+5
1	0.2759***	0.3669***	0.4298***	0.4691***	0.4975***
	(0.0063)	(0.0070)	(0.0073)	(0.0076)	(0.0078)
2	0.1811***	0.2655***	0.3280***	0.3577***	0.3713***
	(0.0097)	(0.0114)	(0.0125)	(0.0132)	(0.0135)
3	0.1578***	0.2336***	0.2808***	0.3118***	0.3276***
	(0.0130)	(0.0157)	(0.0174)	(0.0187)	(0.0194)
4	0.1397***	0.2005***	0.2454***	0.2712***	0.2827***
	(0.0162)	(0.0195)	(0.0222)	(0.0237)	(0.0249)
5	0.1507***	0.2113***	0.2713***	0.3057***	0.3128***
	(0.0210)	(0.0251)	(0.0284)	(0.0305)	(0.0321)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs, with corresponding standard errors (in parenthesis below), obtained in the full sample. Common support is ensured by caliper matching 0.2. *, **, and *** denote statistical significance at 10%, 5%, and 1% level, respectively. The sample of treatments is selected according to the identification criteria outlined in panel (c) of Figure 1.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

5.3 Treatment Effects of Objective-Specific Subsidies

Finally, we address the question of whether subsidies with distinct objectives give rise to heterogeneous treatment effects. As discussed above, we distinguish labor-support, capital-deepening, productivity-enhancing, and general subsidies. Due to a limited number of objective-specific treatments, we estimate ATETs by means of unique treatment indicators taking value 1 in case of receipt of the corresponding grant, irrespective of size, and 0 if firms have not yet received any support. We report the results pertaining to the full sample in Table 9 and those for the restricted sample in Table B.13 of Appendix B.

Labor-support subsidies are found to cause significant scale/output effects. By providing strong incentives for new hires by decreasing labor costs borne by employers through hiring and wage subsidies, firms increase their employment, on average, by 25.8 p.p. more than they would have without the incentive. These subsidies trigger also significant capital expansions (18.3 p.p.) due to either complementarity between the two inputs or a certification/signaling effect envisaged by the literature as a contributing factor to external financing. Greater availability of inputs at lower costs boosts value added (12.8 p.p.), while productivity significantly slows down (-15.8 p.p.). The effects are decreasing over time but are still significant after five years from the receipt of financial aid. Results are robust to a restriction of sample to firms continuously operating over the five-year window.

These results confirm the empirical observation that firms that grow in terms of employment tend to exhibit lower productivity (Nishida, Petrin, and Polanec, 2014; Foster, Haltiwanger, and Krizan, 2001). Firms might use employment subsidies to hire previously unemployed workers that tend to be less skilled due to a certain period of unemployment, inadequate schooling, or less experience in a particular job position. If so, the estimated long-term negative effect on productivity suggests that on-the-job training is ineffective at increasing the productivity of the newly hired. In such scenario, following Mattsson (2019), labor-support subsidies overcompensate firms for the lower productivity of new employees and incentivize them to substitute regular employees with subsidized ones to take advantage of the decrease in labor costs and the consequent increase in output. An alternative explanation of productivity reduction concerns potential changes in the employment structure after the receipt of an employment subsidy. For instance, newly hired employees might be tasked with administrative functions that lead to neither productivity improvements that would make production more efficient with a given quantity of inputs (technological change) nor increased sales through a scale impact change. Furthermore, firms might use subsidies to reduce prices, which could explain both the reduction in our measure of TFP and the observed boost in output (value added). This dynamic, however, would take place only if product demand is sufficiently elastic such that 1 percent decline in price results in an increase in quantity greater than 1 percent. Similarly, value added could increase if employment subsidies were to increase wages. Due to the unavailability of price and wage data, we can test neither of these hypotheses.

Against our expectations, capital-deepening subsidies do not significantly impact capital growth, nor support productivity improvements. They, instead, lead to temporary increases in the labor force (16.5 p.p.) and delayed positive effects on value added (18.6 p.p. after two years). The lack of statistically significant impact on capital implies that subsidized and non-subsidized firms have comparable capital growth, despite the former obtaining an incentive to make investments. Given that the same result is observed if sample is limited to continuously operating firms, the finding could hint at the possibility that firms do not use this type of incentive to undertake marginal investment projects, but inframarginal ones, therefore substituting private for public resources to finance projects they would have undertaken anyway. Contrary to theoretical predictions arguing that the acquisition of modern capital (e.g., new machinery or investment in information and communication technology) should lead to a reorganization of production processes along more efficient lines, the investments undertaken with this type of subsidy did not bring about any productivity boost. Having observed that in our sample this category of subsidies mostly consists of subsidies with environmental protection goals, our results also deny the validity of Porter's Hypothesis stating that well-designed and stringent environmental regulations can stimulate innovations and, therefore, the productivity of firms or the product value for end users (Porter, 1991). Yet, the environmental focus of this class of subsidies could explain why our results are discordant to those of other studies finding evidence of a positive effect of investment subsidies on capital (e.g., Criscuolo et al., 2019).

Productivity-enhancing subsidies lead to similar effects as labor-support subsidies, although smaller and more temporary. Specifically, these subsidies support firms' expansion of both capital (10 p.p., which keeps increasing over time) and employment (6.9 p.p.). These subsidies have a strong focus on research and innovation, therefore involving both capital investments and more skilled workers, such as researchers and engineers, to be used in R&D projects. More inputs at lower costs positively affect value added (8.3 p.p.). This effect could potentially hide an increase in wages of high-skilled employees. Unlike labor-support subsidies, these measures do not cause productivity losses because they are used to hire (and train) more skilled labor, which may also lead to improved skill composition. Surprisingly, they do not affect it positively either, which could be due to the fact that productivity improvements leading to product or process innovations might take more than 5 years to realize.

Finally, general subsidies have short-term effects, generally dissipating within two years, mostly limited to supporting the expansion of capital (18.5 p.p.) and, to a lesser degree, employment (7.4 p.p.).

Table 9: Effects of Objective-Specific Subsidies (Full Sample)

Subsidy Type	t+1	t+2	t+3	t+4	t+5
	Effects on Employment				
Labor-support	0.2576***	0.1817***	0.1542***	0.1621***	0.1664***
	(0.0103)	(0.0144)	(0.0172)	(0.0222)	(0.0285)
Capital-deepening	0.1650***[a]	0.2228***	0.2528***[b]	0.2448*[b]	0.1025[b]
	(0.0612)	(0.0685)	(0.0900)	(0.1446)	(0.1678)
Productivity-enhancing	0.0688***	0.0647***	0.0729***	0.0092[a]	-0.0135
	(0.0172)	(0.0222)	(0.0278)	(0.0316)	(0.0405)
General	0.0737***	0.0870**	0.0140	0.0709[a]	0.0558[a]
	(0.0279)	(0.0370)	(0.0526)	(0.0581)	(0.0681)
	Effects on Capital				
Labor-support	0.1830***	0.1321***	0.1707***	0.1274**	0.1145
	(0.0254)	(0.0354)	(0.0455)	(0.0563)	(0.0739)
Capital-deepening	-0.0518[b]	0.1371[a]	0.1569	-0.0810[b]	-0.1566[b]
	(0.0734)	(0.1136)	(0.1788)	(0.2736)	(0.3110)
Productivity-enhancing	0.1002**	0.1729***	0.1180*	0.1764**	0.1867*
	(0.0427)	(0.0549)	(0.0678)	(0.0796)	(0.0958)
General	0.1848***	0.2503***	0.2886**[a]	0.4174***[a]	0.2011[b]
	(0.0640)	(0.0839)	(0.1121)	(0.1490)	(0.1780)
	Effects on TFP				
Labor-support	-0.1584***	-0.1119***	-0.1098***	-0.0972***	-0.1104***
	(0.0134)	(0.0174)	(0.0199)	(0.0248)	(0.0300)
Capital-deepening	-0.2420***[b]	-0.1189[a]	-0.1313	-0.1340[b]	-0.3402**[b]
	(0.0853)	(0.0849)	(0.0926)	(0.1235)	(0.1598)
Productivity-enhancing	-0.0145	0.0094	-0.0249	-0.0057	-0.0325
	(0.0237)	(0.0273)	(0.0298)	(0.0387)	(0.0447)
General	-0.0305	-0.0793**	-0.0530[a]	-0.0098[a]	0.0193[b]
	(0.0340)	(0.0380)	(0.0470)	(0.0629)	(0.0687)
	Effects on Value Added				
Labor-support	0.1276***	0.0832***	0.0715***	0.0862***	0.0852**
	(0.0154)	(0.0203)	(0.0236)	(0.0302)	(0.0387)
Capital-deepening	-0.0416[a]	0.1858*	0.1379[b]	-0.0177[b]	-0.2853[b]
	(0.1063)	(0.1051)	(0.1407)	(0.1906)	(0.2539)
Productivity-enhancing	0.0828***	0.0693**	0.1247***	0.0122[a]	0.0009
	(0.0248)	(0.0316)	(0.0390)	(0.0446)	(0.0544)
General	0.0544	0.1655***	-0.0324	0.0765[a]	0.1005[a]
	(0.0388)	(0.0519)	(0.0614)	(0.0733)	(0.0937)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below) found in the full sample. Treatment effects are well-identified (see identification criteria in panel (b) of Figure 1), additionally ensuring no same-year multiple treatments of different subsidy classes. Common support is ensured by caliper matching 0.2. *, **, and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

More ambiguous are the effects on productivity and value added since both realize with a two-year delay (-8 p.p. and 16.5 p.p., respectively). As pointed out in the description of the sample, these subsidies are characterized by rather general aims that leave firms ample discretion about how to use them. The results indicate that firms use these subsidies to expand inputs to take advantage of their decreased cost, while this does not immediately affect value added. However, our data does not allow us to further investigate this subject.

Table 10: Comparison of Treatment Effects in Value Terms, by Subsidy Type

	Outcome	
Treatment type	Employment	Value added
Labor-support	3.53 €	2.19 €
Capital-deepening	4.18 €	-
Productivity-enhancing	3.32 €	1.42 €
General	1.62 €	-

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the additional value added produced by spending EUR 1 in the corresponding objective-specific subsidy category. Exercise limited to converting statistically significant ATETs on the cumulative growth rates up to $t+1$. Formulae and interpretation are provided in Appendix C.

We conclude this chapter with cross-subsidy types comparisons of their impact on employment and value added by calculating the increase in value added per unit of subsidy spent (see Table 10). The greatest impact on employment was achieved by capital-deepening subsidies: EUR 1 of subsidies spent in this grant category causes value added to increase by EUR 4.18, evaluated at initial labor productivity. Labor-support subsidies follow with EUR 3.53 additional value added per unit of subsidy, only slightly greater than the impact of productivity-enhancing subsidies. The latter two are the only subsidy classes producing statistically significant effects on value added. Between the two, the highest impact was achieved by employment subsidies, causing EUR 2.19 additional value added per unit of subsidy.

6 Conclusions

This empirical study contributes to understanding of the causal impact of size of subsidy and specific aims of subsidies on several measures of firm size (employment, capital, and value added) and performance (total factor productivity).

We first demonstrate that the effects of subsidies vary significantly with their size relative to value added. Our data exhibit a positive relationship between the magnitude of effects and relative subsidy size, which is most evident in the first year after receipt of public support and employment as an outcome. The presence of decreasing marginal effects and even diminishing effects for the highest treatment sizes cast doubts on the suitability of particularly large relative subsidies since comparable effects can be obtained by smaller grants. Furthermore, the effects are more persistent when firms receive smaller treatments.

Secondly, we show that failure to distinguish between different objectives of subsidies in existing literature hampers the understanding of their estimated impact. Previous studies were more limited in scope as they concentrated on studying the effects of individual subsidy programs, most often either employment or R&D subsidies. In contrast, we have at our disposal a comprehensive dataset of all supporting measures given to Slovenian firms from 2001 to 2017 that include subsidies from a wide variety of categories and program objectives. By exploiting this richness of information, we find that the impact of subsidies depends on the underlying subsidy scope. Namely, productivity losses are mostly the result of subsidies intended for job creation. A plausible rationale could be that recent recruits generate lower additional value added in comparison to existing employees, which may also depend on the type of labor hired (e.g., occupational and skill level). Furthermore, the long-term negative effect on productivity casts doubt on the ability of firms to adequately train newly hired workers and augment their productivity.

Finally, our results suggest that subsidies generally have only temporary effects that tend to attenuate over time. However, allowing firms to obtain additional financial aid shortly after receiving the first subsidy substantially increases the impact of subsidies, which further builds up over time. The same holds for productivity losses. We believe that the use of multiple grants is particularly frequent because firms successfully applying for subsidies may apply more frequently, but also because past subsidy tender winners may enjoy learning-by-doing efficiency gains in attaining subsidies. It thus calls for a policy discussion about the benefits of having a fraction of firms frequently relying on public support, providing them the resources to keep and increase their inputs and, consequently, increase their output while not registering any rise in productivity.

Our results suffer from two methodological weaknesses. On the one side, our propensity-score matching approach might not have addressed all endogenous sources affecting both treatment and outcome variables due to unobservables. On the other, it does not rule out the presence of pre-trends that could contribute to explaining the results. In spite of richness of data, we cannot overcome these issues. Besides curtailing our limitations, future research could expand our findings by studying the mechanisms underlying the negative impact on productivity, compositional effects of hired labor, and the potential effects on competition.

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Appendix A Additional Data Description

Table A.1 provides an overview of the data extracted from our data sources.

Table A.1: Variable List

Variables	Description
	From AJPES
Firm identifier	Firm registration number.
Year	2002-2021
Employment	Number of employees as full-time equivalent
Tangible capital	
Value added	
Sales	
Material cost	
Total liabilities	
Industry	2-digit NACE Rev. 2 (2008) classification.
Region	Slovenia has 12 statistical regions.
	From the State Aid dataset
Firm identifier	Firm registration number.
Year	2001-2017
Subsidy category	Broad classification. E.g., employment subsidies, R&D subsidies, support to SMEs, environmental subsidies, regional support, etc.
Program name	Brief description of the policy aim.
Instrument	E.g., direct grants, tax delays, tax reductions, tax rebates, interest-free loans, reduced-interest loans, etc.
Purposes	Specific policy aim
Subsidy	Net amount

Source: Own work.

Table A.2 provides information about the data cleaning procedure applied to the original datasets. As outlined in Section 4.3, we impose specific firm and subsidy selection criteria. Firm selection criteria consist in retaining firms (excluding sole proprietorships and banks) with at least one employee and positive value added in the years used in the estimations and that, according to the 2-digit NACE Rev. 2 (2008) classification, are active in broad industries other than agriculture, forestry, and fishing; mining and quarrying; water supply, sewerage, waste management, and remediation activities; public administration and defense, and compulsory social security; and electricity, gas, steam, and air conditioning supply. Subsidy selection criteria involve selecting subsidies provided in the form of grants that were provided in contexts other than agriculture, fisheries, natural disasters, mining, culture, and for restructuring purposes.

Table A.2: Data Cleaning Procedure

Dataset		Original	Subsidy criteria	Firm selection up to				
				t+1	t+2	t+3	t+4	t+5
AJPES	Obs.	1,134,363	-	443,966	380,587	327,463	281,530	241,623
	Firms	125,264	-	57,870	49,751	43,774	38,519	33,884
				Restricting data to 2016				
	Obs.		-	322,569	296,766	275,801	257,466	241,623
	Firms		-	48,187	43,051	39,605	36,475	33,884
State Aid	Obs.	284,795[a]	145,765[a]	32,799	31,327	29,990	28,742	27,645
	Firms	86,053	68,851	13,775	12,958	12,279	11,641	11,110
				Excluding relative subsidies above 25 percent of firms' value added				
	Obs.			29,560	28,420	27,317	26,259	25,298
	Firms			12,635	12,012	11,435	10,876	10,399
				Selected treatments allowing for future treatments				
	Obs.			8,787	8,336	7,906	7,509	7,194
	Firms			8,399	7,964	7,541	7,159	6,855
				Selected well-identified treatments				
	Obs.			6,121	4,994	4,163	3,586	3,225
	Firms			5,918	4,832	4,026	3,465	3,113

Source: Authors' calculations based on AJPES and Ministry of Finance data.
[a]: Number of treatments not uniquely identified by firm identifier and year due to multiple grants in the same year.

Table A.3 provides an overview of the main broad categories of subsidies extended to firms. Each subsidy category includes several subsidy schemes each of which with a specific purpose.

Table A.3: Subsidy Characteristics, by Original Categories

Subsidy category	N		Mean (in EUR)	Std. Dev.	Value	
	Freq.	Percent			EUR	Percent
Subsidies to SMEs	1,985	21.23	3,394	15,358	6,736,108	9.68
R&D and innovation	291	3.11	25,808	38,759	7,510,262	10.79
Regional aid	246	2.63	65,286	123,108	16,060,313	23.08
Training	778	8.32	5,525	11,506	4,298,090	6.18
Environmental protection	403	4.31	14,935	37,682	6,018,953	8.65
Employment	5,622	60.14	4,994	3,836	28,078,638	40.35
Other	23	0.25			886,367	1.27
Total	9,348	100			69,588,730	100

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data comply with subsidy selection criteria and are restricted to the subsample of treatments allowing for future treatments (see selection criteria in panel (c) of Figure 1).

The following table provides the list of subsidies allocated to each specific objective-based group displaying information about the original aims and categorization. Objectives were primarily deduced from the information reported in the State Aid dataset in variables “aim”, “program name” (which provides a brief description of the program), and “category”.

Table A.4: List of Subsidies, by Aim and Category

Aim	Category	Value	Freq.
	Labor-support subsidies		
De minimis job creation in SMEs	Employment	1,653,305	611
De minimis creation of new jobs in enterprises in areas eligible for regional aid	Employment	9,785	6
De minimis aid for job creation	Employment	728,883	131
De minimis, job retention	Employment	52,158	12
Creating new jobs	Regional aid	584,392	3
Aid to compensate for the costs of assistance for disadvantaged workers	Employment	5,480	4
Employment support for disadvantaged workers in the form of wage subsidies	Employment	25,629,028	4,858
Total labor-support subsidies		28,663,029	5,625
	Capital-deepening subsidies		
Investment in early adaptation to new environmental standards	Environmental subsidies	609,890	20
Investment in energy saving	Environmental subsidies	179,219	13
Investments	Maritime transport	11,383	3
Investing in renewable energy	Environmental subsidies	381,094	5
Aid for the purchase of transport vehicles exceeding community standards for environmental protection	Environmental subsidies	2,311,235	160
Aid for investment in energy efficiency projects in buildings	Environmental subsidies	6,231	7
Total of capital-deepening subsidies		3,499,053	208
	Productivity-enhancing subsidies		
Experimental development	R&D and innovation subsidies	601,970	21
Industrial research	R&D and innovation subsidies	4,750,432	125
Pre-competitive R&D activities	R&D and innovation subsidies	881,381	21
Innovation aid for SMEs	R&D and innovation subsidies	62,277	8
Aid for young innovative enterprises	R&D and innovation subsidies	218,490	8
Aid for SME participation in fairs	Subsidies to SMEs	328,588	98
Aid for investment in research infrastructures	R&D and innovation subsidies	7,330	1
Aid for specific training	Training subsidies	2,483,339	381
Aid for lending highly qualified staff	R&D and innovation subsidies	89,155	4
Aid for general training	Training subsidies	1,814,501	396

Continued on next page

Table A.4: List of Subsidies, by Aim and Category

Aim	Category	Value	Freq.
Aid for the costs of industrial property rights for SMEs	R&D and innovation subsidies	580	1
Advisory assistance for the benefit of the SMEs	Subsidies to SMEs	1,896,624	1,347
Training aid	Training subsidies	250	1
Co-financing of advisory services in the field of renewable energy	Environmental subsidies	20,834	3
Basic research	R&D and innovation subsidies	839,529	95
Technical feasibility studies	R&D and innovation subsidies	59,118	7
Total of productivity-enhancing subsidies		14,054,399	2,517
	General subsidies		
Financial crisis	Remedying a serious disturbance in the economy	838,038	13
Investment and job creation from investment	Regional aid	15,464,036	220
Helping new and existing small businesses	Urban areas in decline	4,865	2
Reducing the company's current expenditure	Regional aid	599	15
Investment and employment aid	Subsidies to SMEs	4,508,058	537
Urban development aid	Regional aid	1,515	1
Operating aid	Regional aid	9,772	7
Operating aid (renewable energy)	Environmental subsidies	2,383,210	185
Operating aid (cogeneration)	Environmental subsidies	127,238	10
Services of general economic interest	Services of general economic interest	32,081	5
Start a small business (female entrepreneurs)	Subsidies to SMEs	2,837	3
Total of general subsidies		23,372,249	998

Source: Own classification based on Ministry of Finance data.

Notes: Data restricted to *Sample1* (see definitions in Figure 2) and to the sample of treatment allowing for future subsidies (see selection criteria in panel (c) of Figure 1).

Table A.5: Summary Statistics of Firms, by Treatment Size and Type

	Firm Age		Employment		Capital		Log of TFP		Value Added		Debt-to-Assets		Sales	
Treat. Size	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
0	10.58	5.42	6.46	25.97	188,185	3,367,663	9.18	0.73	238,096	1,748,172	1.06	51.50*	1,167,624	12,989,842
1	11.14	5.53	24.57	124.76	1,217,958	14,613,563	9.16	0.63	871,722	5,873,826	0.61	0.33	3,943,008	39,573,420
2	10.60	6.30	7.86	24.67	243,624	1,692,200	9.03	0.62	193,890	703,360	0.65	0.37	789,104	3,331,918
3	10.58	6.38	4.94	9.99	126,368	535,808	8.96	0.54	118,381	287,584	0.67	0.42	462,350	1,125,654
4	9.94	6.33	4.63	8.50	115,364	560,608	8.82	0.59	96,759	216,216	0.68	0.44	352,755	794,513
5	10.44	6.53	3.92	5.35	118,515	477,244	8.88	0.70	92,738	185,949	0.72	0.80	353,034	675,352
Treat. Type														
Labor-support	11.09	6.19	11.82	65.86	311,663	3,212,265	9.04	0.62	309,986	1,682,344	0.63	0.40	1,406,486	10,445,076
Capital-deepening	10.89	5.95	28.71	77.05	3,394,637	22,718,940	8.97	0.58	1,303,025	7,187,848	0.69	0.25	16,585,250	187,400,768
Productivity-enhancing	10.17	5.02	27.57	158.48	1,415,721	18,537,966	9.17	0.65	1,085,728	8,184,923	0.62	0.37	3,923,516	20,471,142
General	11.39	5.78	22.86	58.14	1,687,828	14,555,682	9.15	0.64	843,069	2,723,616	0.60	0.32	3,750,000	15,744,658

Source: Own calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to *Sample1* (see definitions in Figure 2) and to the sample of treatment allowing for future subsidies (see selection criteria in panel (c) of Figure 1). Treatment size 0 denotes not-yet-treated firms, whereas treatment sizes 1 to 5 are based on the relative subsidy size with respect to pre-treatment value added following our classification reported in Table 1.
*: This particularly high number in the debt-to-assets ratio is due to the fact that until 2016 firms were not required to recapitalize if they had negative equity, and could continue to operate. This peculiarity is confined to an extremely small part of the firm sample as merely 2.42 percent of firms denote a debt-to-assets ratio higher than 2.

The following tables show the frequency with which firms obtained several grants within the same year (panels (a) and (b)) and over the years (panel (c)). We distinguish between multiple treatments using our level of subsidy aggregation into objective-specific groups (panel (a)) and the disaggregated data of the original dataset (panel (b)).

Table A.6: Multiple Treatments

No. of Grants	Freq.	Percent
1	28,785	87.76
2	3,614	11.02
3	388	1.18
4	12	0.04
Total	32,799	100

(a) Same-year multiple treatments: aggregated data into objective-specific subsidy groups

No. of Grants	Freq.	Percent
1	26,168	79.78
2	4,428	13.50
3	1,311	4.00
4	498	1.52
5	211	0.64
6	97	0.30
7	42	0.13
8	28	0.09
9	14	0.04
10	2	0.01
Total	32,799	100

(b) Same-year multiple treatments: disaggregated data

No. of Grants	Freq.	Percent
1	6,926	50.28
2	2,721	19.75
3	1,513	10.98
4	840	6.10
5	579	4.20
6	389	2.82
7	241	1.75
8	183	1.33
9	120	0.87
10	83	0.60
11	36	0.26
12	65	0.47
13	40	0.29
14	39	0.28
Total	13,775	100

(c) Over-the-Years Multiple Treatments

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to "Sample1" (see definitions in Figure 2) and selected according to firm and subsidy selection criteria outlined in Section 4.3, but without additionally imposing identification criteria in order to show the multiple treatment problem.

The following tables (A.7, A.8 and A.9) decompose the sample of subsidized and not-yet subsidized used in estimations allowing for future treatments by various confounding variables.

Table A.7: Sample Characteristics, by Firm Age

	Firms		Grants			
Firm age	Total	Subsidized	Freq.	Mean	Std. Dev.	Value
1-3	288	9	1,075	5,873	12,545	6,313,571
4-6	2,324	125	1,555	7,258	20,023	11,285,527
7-9	4,996	450	1,067	6,749	15,006	7,201,309
10-12	5,389	741	1,772	6,176	22,986	10,943,144
13-15	5,190	1,003	1,043	9,930	39,196	10,356,681
16-18	4,301	1,022	1,088	10,714	34,395	11,656,448
19-21	3,378	919	914	10,983	48,481	10,038,343
22-24	2,841	825	273	6,570	4,390	1,793,708
25-28	9,557	3,305	-	-	-	-
Total	38,264	8,399	8,787			69,588,730

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to "Sample1" (see definitions in Figure 2). Sample of subsidized and not-yet subsidized firms used in estimations allowing for future treatments (see selection criteria in Figure 1). The values of subsidies are reported in current EUR. In the second and third columns, firms are classified based on their maximum age achieved in the sample.

Table A.8: Sample Characteristics, by Broad Industries

	Firms		Grants			
Industry	Total	Subsidized	Freq.	Mean	Std. Dev.	Value
Manufacturing	5,283	1,625	1,702	15,150	54,452	25,785,850
Construction	5,692	892	922	5,814	14,445	5,360,769
Wholesale and Retail Trade	9,728	2,116	2,222	5,469	15,448	12,151,419
Transportation and Storage	2,314	562	583	8,687	21,281	5,064,264
Accommodation and Food Service Activities	1,977	471	491	6,464	25,451	3,173,920
Information and Communication	1,832	418	440	9,204	23,485	4,049,911
Financial and Insurance Activities	531	126	128	8,499	27,807	1,087,826
Real Estate Activities	778	135	143	7,151	11,268	1,022,652
Professional, Scientific and Technical Activities	7,019	1,388	1,459	5,464	10,696	7,971,559
Administrative and Support Service Activities	1,195	272	284	6,804	11,481	1,932,214
Education	377	99	105	5,009	5,308	525,967
Human Health and Social Work Activities	693	120	124	4,889	3,024	606,201
Arts, Entertainment and Recreation	304	62	65	5,464	5,402	355,182
Other Service Activities	541	113	119	4,210	4,809	500,999
Total	38,264	8,399	8,787			69,588,731

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to "Sample1" (see definitions in Figure 2). Sample of subsidized and not-yet subsidized firms used in estimations allowing for future treatments (see selection criteria in Figure 1). The values of subsidies are reported in current EUR. Industries reported according to the NACE 1-digit classification.

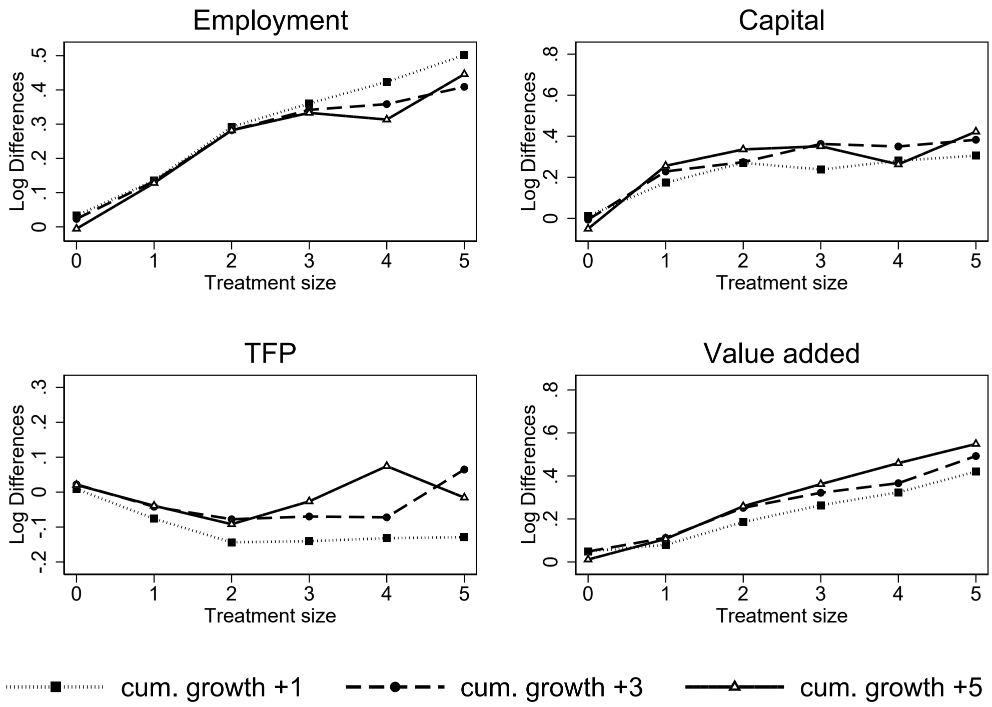
Table A.9: Sample Characteristics, by Region

	Firms		Grants			
Region	Total	Subsidized	Freq.	Mean	Std. Dev.	Value
Pomurska	833	256	269	8,849	17,498	2,380,431
Podravska	4,666	1,192	1,255	7,626	26,577	9,570,907
Koroška	750	240	256	10,122	31,516	2,591,227
Savinjska	3,403	935	967	8,838	44,120	8,545,923
Zasavska	413	131	138	6,897	11,338	951,845
Posavska	660	222	232	6,000	16,255	1,391,946
Jugovzhodna Slovenija	1,600	449	465	10,023	40,798	4,660,527
Osrednjeslovenska	17,469	3,109	3,248	7,965	23,777	25,869,334
Gorenjska	3,518	810	849	7,799	31,339	6,621,411
Primorsko-notranjska	634	190	200	5,845	12,354	1,168,959
Goriška	1,766	438	466	5,253	15,908	2,447,924
Obalno-kraška	2,552	427	442	7,666	24,328	3,388,297
Total	38,264	8,399	8,787			69,588,729

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to "Sample1" (see definitions in Figure 2). Sample of subsidized and not-yet subsidized firms used in estimations allowing for future treatments (see selection criteria in Figure 1). The values of subsidies are reported in current EUR.

The following graphs provide simple comparisons of outcome variable averages (cumulative growth up to year $t+1$, $t+3$, and $t+5$) between subsidized and non-subsidized firms before performing matching. These averages are computed by pooling all firm-year observations for each treatment group, ranging from 0 (not-yet subsidized firms) to the highest treatment 5 (subsidy in the range 20-25 percent of nominal value added).

Figure 4: Average Outcomes, by Treatment Size



Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: Data restricted to *Sample1* (see definitions in Figure 2) and to the sample containing not-yet subsidized and subsidized firms, the latter of which selected allowing for future treatments (see selection criteria in Figure 1).

Appendix B Additional Results

Table B.10: Effects of Pooled Subsidies, by Relative Size of Subsidy (Restricted Sample)

Treatment level	t+1	t+2	t+3	t+4	t+5
	Effects on Employment				
1	0.0871***	0.0822***	0.0780***	0.0861***	0.0776***
	(0.0158)	(0.0190)	(0.0227)	(0.0246)	(0.0263)
2	0.1765***[b]	0.1013**[b]	0.0699[b]	0.0494[b]	0.0601[b]
	(0.0367)	(0.0420)	(0.0473)	(0.0489)	(0.0522)
3	0.2573***[a]	0.1768***[a]	0.1370**[a]	0.1277**[a]	0.1294**[a]
	(0.0461)	(0.0557)	(0.0592)	(0.0637)	(0.0659)
4	0.2790***[b]	0.2001***[b]	0.2195**[b]	0.1815*[b]	0.1329[b]
	(0.0705)	(0.0751)	(0.0875)	(0.1019)	(0.1015)
5	0.3277***[b]	0.2555**[b]	0.2570**[b]	0.2527**[b]	0.2210*[b]
	(0.0948)	(0.1168)	(0.1167)	(0.1177)	(0.1143)
	Effects on Capital				
1	0.0781*	0.0868*	0.0876	0.1086*	0.1136*
	(0.0409)	(0.0482)	(0.0544)	(0.0577)	(0.0639)
2	0.2232**[a]	0.1335[a]	0.1062[a]	0.1301[a]	0.2540*[a]
	(0.0902)	(0.1043)	(0.1230)	(0.1370)	(0.1485)
3	0.2809*[b]	0.2867*[b]	0.3333*[b]	0.4073**[b]	0.4567**[b]
	(0.1513)	(0.1684)	(0.1827)	(0.1881)	(0.2108)
4	0.1598[b]	-0.1742[b]	-0.0685[b]	-0.1086[b]	0.0041[b]
	(0.1412)	(0.2080)	(0.2460)	(0.2433)	(0.2745)
5	0.3401[b]	0.3746[b]	0.1548[b]	-0.1779[b]	-0.0692[b]
	(0.3184)	(0.4105)	(0.4401)	(0.4332)	(0.4714)
	Effects on TFP				
1	-0.0773***	-0.0564**	-0.0343	-0.0555**	-0.0523**
	(0.0216)	(0.0223)	(0.0248)	(0.0252)	(0.0263)
2	-0.1898***[a]	-0.1268***[a]	-0.0631[a]	-0.0010[a]	-0.0723[a]
	(0.0420)	(0.0451)	(0.0491)	(0.0612)	(0.0533)
3	-0.2118***[b]	-0.1598**[b]	-0.1541**[b]	-0.1897**[b]	-0.2160***[b]
	(0.0697)	(0.0730)	(0.0684)	(0.0786)	(0.0799)
4	-0.0400[b]	-0.1166[b]	-0.2497**[b]	-0.0807[b]	-0.0691[b]
	(0.0817)	(0.0982)	(0.1164)	(0.1149)	(0.0933)
5	-0.0978[b]	0.0493[b]	-0.0431[b]	-0.1043[b]	0.0682[b]
	(0.1028)	(0.1323)	(0.1321)	(0.1496)	(0.1211)
	Effects on Value Added				
1	0.0139	0.0244	0.0504*	0.0517	0.0527
	(0.0243)	(0.0259)	(0.0300)	(0.0319)	(0.0362)
2	0.0422[b]	-0.0139[b]	0.0614[b]	-0.0305[b]	-0.0068[b]
	(0.0540)	(0.0569)	(0.0720)	(0.0648)	(0.0693)
3	0.1174*[a]	-0.0083[a]	-0.0419[a]	-0.0189[a]	0.0727[a]
	(0.0683)	(0.0898)	(0.0979)	(0.1139)	(0.1047)
4	0.1998**[b]	0.0663[b]	0.0520[b]	0.1580[b]	0.1431[b]
	(0.0896)	(0.1113)	(0.1231)	(0.1345)	(0.1517)
5	0.0894[b]	0.0800[b]	-0.0299[b]	0.1646[b]	0.2792*[b]
	(0.1362)	(0.1352)	(0.2281)	(0.1697)	(0.1590)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below). Treatment effects are well-identified (see identification criteria in panel (b) of Figure 1). "Restricted Sample" refers to a time-invariant sample of continuously surviving firms over a 5-year temporal window since the receipt of subsidy. Common support is ensured by caliper matching 0.2. *, ** and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

Table B.11: Effects of Pooled Subsidies Allowing for Future Treatments, by Relative Size of Subsidy (Full Sample)

Treatment level	t+1	t+2	t+3	t+4	t+5
	Effects on Employment				
1	0.1536***	0.1652***	0.1558***	0.1963***	0.2162***
	(0.0086)	(0.0109)	(0.0123)	(0.0142)	(0.0165)
2	0.2505***	0.2407***	0.2557***	0.2890***	0.2771***[a]
	(0.0168)	(0.0215)	(0.0241)	(0.0299)	(0.0360)
3	0.3090***	0.3141***	0.3294***	0.3061***	0.3479***
	(0.0234)	(0.0303)	(0.0368)	(0.0440)	(0.0481)
4	0.3999***[a]	0.2991***	0.2991***[a]	0.3558***[a]	0.2786***[a]
	(0.0299)	(0.0422)	(0.0515)	(0.0566)	(0.0785)
5	0.4419***	0.4408***[a]	0.3441***	0.4049***[a]	0.3917***[a]
	(0.0390)	(0.0537)	(0.0671)	(0.0748)	(0.1088)
	Effects on Capital				
1	0.1972***	0.2370***	0.2880***	0.3388***	0.4391***
	(0.0193)	(0.0246)	(0.0287)	(0.0325)	(0.0374)
2	0.3015***	0.3188***	0.3334***	0.4369***	0.4037***
	(0.0407)	(0.0509)	(0.0644)	(0.0734)	(0.0912)
3	0.2875***	0.3237***	0.5563***[a]	0.4872***	0.3750**
	(0.0643)	(0.0842)	(0.1003)	(0.1116)	(0.1554)
4	0.3264***	0.2972***[a]	0.2588*	0.1615[b]	0.5736***[a]
	(0.0810)	(0.1131)	(0.1396)	(0.1646)	(0.1972)
5	0.3578***	0.3680***[a]	0.3386**[a]	0.2134[a]	0.6217***[a]
	(0.1178)	(0.1428)	(0.1635)	(0.2083)	(0.2299)
	Effects on TFP				
1	-0.0612***	-0.0608***	-0.0344***	-0.0856***	-0.0718***
	(0.0110)	(0.0118)	(0.0133)	(0.0140)	(0.0154)
2	-0.1532***	-0.1364***	-0.1690***	-0.1171***	-0.1402***
	(0.0232)	(0.0251)	(0.0295)	(0.0323)	(0.0371)
3	-0.2304***	-0.1341***	-0.1796***[a]	-0.1728***	-0.1672***
	(0.0338)	(0.0374)	(0.0434)	(0.0508)	(0.0631)
4	-0.1711***	-0.1696***[a]	-0.1561**	-0.0881[b]	-0.1308[a]
	(0.0420)	(0.0541)	(0.0639)	(0.0642)	(0.0825)
5	-0.2144***	-0.0530[a]	-0.0481[a]	-0.1218[a]	-0.0891[a]
	(0.0596)	(0.0649)	(0.0727)	(0.0938)	(0.1137)
	Effects on Value Added				
1	0.1097***	0.1363***	0.1396***	0.1748***	0.1918***
	(0.0120)	(0.0142)	(0.0162)	(0.0183)	(0.0211)
2	0.1390***	0.1760***	0.1463***	0.2415***	0.2049***[a]
	(0.0239)	(0.0284)	(0.0336)	(0.0376)	(0.0446)
3	0.1895***	0.2224***	0.2328***	0.2303***	0.2553***
	(0.0371)	(0.0471)	(0.0535)	(0.0605)	(0.0680)
4	0.2349***[a]	0.2200***	0.2049***[a]	0.2940***[a]	0.1339[a]
	(0.0483)	(0.0663)	(0.0725)	(0.0873)	(0.1153)
5	0.2311***	0.4147***[a]	0.3365***	0.3617***[a]	0.3268***[a]
	(0.0654)	(0.0861)	(0.0951)	(0.1316)	(0.1215)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below) obtained in the full sample. Treatment effects are identified according to the criteria outlined in panel (c) of Figure 1. Common support is ensured by caliper matching 0.2. *, ** and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

Table B.12: Marginal Effects of Increasing Relative Subsidy Sizes (Full Sample)

Treated vs. Control	t+1	t+2	t+3	t+4	t+5
	Effects on Employment				
2 vs. 1	0.0973***	0.0627*[a]	0.0501	0.0982*[a]	0.0702[a]
	(0.0319)	(0.0379)	(0.0474)	(0.0573)	(0.0700)
3 vs. 2	0.1010***	0.1019*[a]	0.0997[a]	-0.0026[a]	0.0193[a]
	(0.0380)	(0.0555)	(0.0637)	(0.0740)	(0.0968)
4 vs. 3	0.0453	0.0681[b]	0.1400*[b]	0.0892[b]	0.0752[b]
	(0.0412)	(0.0646)	(0.0808)	(0.1184)	(0.1583)
5 vs. 4	0.0807[a]	0.1111[a]	0.0964[a]	0.0608[a]	0.0301[a]
	(0.0586)	(0.0903)	(0.1033)	(0.1393)	(0.1820)
	Effects on Capital				
2 vs. 1	-0.0208[a]	0.1084	-0.0898[a]	-0.0628[a]	0.2212[a]
	(0.0789)	(0.0960)	(0.1157)	(0.1396)	(0.1853)
3 vs. 2	-0.0525[a]	0.1375[a]	-0.0138	0.2403[a]	0.1028[b]
	(0.0990)	(0.1285)	(0.1677)	(0.2000)	(0.2538)
4 vs. 3	0.0840[a]	-0.1483	0.0108[a]	-0.0462[b]	0.0780[a]
	(0.1183)	(0.1933)	(0.2287)	(0.2778)	(0.3364)
5 vs. 4	-0.0782[a]	0.3162[a]	0.0771[a]	-0.2548[a]	-0.1798[b]
	(0.1701)	(0.2257)	(0.2670)	(0.3942)	(0.4197)
	Effects on TFP				
2 vs. 1	-0.0925**[a]	-0.0219	-0.0343[a]	-0.0612[a]	0.0241[a]
	(0.0374)	(0.0501)	(0.0481)	(0.0504)	(0.0734)
3 vs. 2	-0.0360[a]	-0.0894[a]	-0.0421	-0.0737[a]	-0.0317[b]
	(0.0467)	(0.0558)	(0.0749)	(0.0735)	(0.1081)
4 vs. 3	-0.0224[a]	-0.0361	0.1033[a]	0.0471[b]	-0.1764[a]
	(0.0557)	(0.0746)	(0.0875)	(0.0996)	(0.1358)
5 vs. 4	0.0511[a]	-0.0902[a]	0.0436[a]	-0.2113[a]	-0.0833[b]
	(0.0772)	(0.0933)	(0.1060)	(0.1928)	(0.2167)
	Effects on Value Added				
2 vs. 1	0.0285	0.0704[a]	0.0371	0.0189[a]	0.0811[a]
	(0.0402)	(0.0547)	(0.0589)	(0.0673)	(0.0910)
3 vs. 2	0.1130**	-0.0319[a]	0.0281[a]	-0.1160[a]	0.0105[a]
	(0.0506)	(0.0664)	(0.0801)	(0.0978)	(0.1272)
4 vs. 3	-0.0247	0.1591[b]	0.1062[b]	0.0991[b]	0.0941[b]
	(0.0627)	(0.1008)	(0.1140)	(0.1496)	(0.2403)
5 vs. 4	0.1390[a]	0.1540[a]	0.2557*[a]	0.1745[a]	0.1858[a]
	(0.0898)	(0.1279)	(0.1553)	(0.1980)	(0.2850)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with corresponding standard errors (in parenthesis below) of obtaining one-higher treatment level as opposed to the treatment received. Treatment effects are well-identified (see criteria outlined in panel (b) of Figure 1). Common support is ensured by caliper matching 0.2. *, **, and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

Table B.13: Effects of Objective-Specific Subsidies (Restricted Sample)

Subsidy Type	t+1	t+2	t+3	t+4	t+5
	Effects on Employment				
Labor-support	0.2226***	0.1839***	0.1717***	0.1714***	0.1664***
	(0.0185)	(0.0221)	(0.0249)	(0.0265)	(0.0285)
Capital-deepening	0.1167[b]	0.1244[b]	0.1057[b]	0.1131[b]	0.1025[b]
	(0.0957)	(0.1125)	(0.1207)	(0.1437)	(0.1678)
Productivity-enhancing	0.0123	0.0066	0.0141	-0.0135	-0.0135
	(0.0254)	(0.0301)	(0.0336)	(0.0368)	(0.0405)
General	0.0399[a]	0.0520[a]	0.0348[a]	0.0715[a]	0.0558[a]
	(0.0403)	(0.0499)	(0.0612)	(0.0669)	(0.0681)
	Effects on Capital				
Labor-support	0.1160***	0.0629	0.0938	0.1207*	0.1718**
	(0.0446)	(0.0528)	(0.0608)	(0.0646)	(0.0718)
Capital-deepening	0.0457[a]	0.2055[a]	0.1568[a]	-0.2913[a]	-0.1089[a]
	(0.1532)	(0.2066)	(0.2904)	(0.3461)	(0.3007)
Productivity-enhancing	0.0379	0.0392	0.1332*	0.1241	0.1717*
	(0.0636)	(0.0711)	(0.0808)	(0.0876)	(0.0918)
General	0.1951*[a]	0.2163[a]	0.2354[a]	0.2156[a]	0.2658[a]
	(0.1141)	(0.1360)	(0.1466)	(0.1571)	(0.1650)
	Effects on TFP				
Labor-support	-0.1673***	-0.1092***	-0.1117***	-0.1094***	-0.1113***
	(0.0242)	(0.0257)	(0.0288)	(0.0298)	(0.0304)
Capital-deepening	0.0679[a]	-0.1587[a]	-0.0357[a]	-0.0226[a]	-0.1200[a]
	(0.1488)	(0.1348)	(0.1316)	(0.1365)	(0.1670)
Productivity-enhancing	-0.0518	-0.0754**	-0.0840**	-0.0959**	-0.0830*
	(0.0354)	(0.0377)	(0.0413)	(0.0440)	(0.0448)
General	-0.1562***[a]	-0.0926[a]	-0.0781[a]	-0.1442**[a]	-0.1470**[a]
	(0.0488)	(0.0574)	(0.0593)	(0.0636)	(0.0713)
	Effects on Value Added				
Labor-support	0.0454*	0.0511*	0.0527	0.0905**	0.0852**
	(0.0260)	(0.0308)	(0.0339)	(0.0353)	(0.0387)
Capital-deepening	-0.0511[b]	-0.1533[b]	-0.0779[b]	-0.1337[b]	-0.2853[b]
	(0.1561)	(0.1788)	(0.1857)	(0.2165)	(0.2539)
Productivity-enhancing	0.0327	0.0348	0.0356	-0.0156	0.0009
	(0.0367)	(0.0395)	(0.0438)	(0.0489)	(0.0544)
General	-0.0179[a]	0.0406[a]	0.0213[a]	0.0798[a]	0.1005[a]
	(0.0531)	(0.0577)	(0.0667)	(0.0751)	(0.0937)

Source: Authors' calculations based on AJPES and Ministry of Finance data.
Notes: The table reports the ATETs with the corresponding standard error (in parenthesis below). Treatment effects are well-identified (see identification criteria in panel (b) of Figure 1), additionally ensuring no same-year multiple treatments of different subsidy classes. "Restricted Sample" refers to a time-invariant sample of continuously surviving firms over a 5-year temporal window since the receipt of subsidy. Common support is ensured by caliper matching 0.2. *, ** and *** denote statistical significance at 10%, 5%, and 1% level, respectively.
[a]: Weak covariate imbalances below 0.2 standardized difference.
[b]: More severe covariate imbalances above 0.2 standardized difference.

Appendix C Conversion Formulas

In what follows, we provide a proxy for the effect of subsidies on the outcomes of interest by finding the amount of additional value added produced for every unit of subsidy spent.

We start from the effect of subsidies on value added. Given the formula for the growth of value added, γ_{t+i}^{VA}, and for the relative measure of subsidy, S_t:

$$\gamma_{t+i}^{VA} = \frac{VA_{t+i} - VA_{t-1}}{VA_{t-1}} \tag{C.1}$$

$$S_t = \frac{\sigma_t}{VA_{t-1}} \tag{C.2}$$

where σ_t is the amount of subsidy in monetary terms, we can take the ratio of the two to obtain the change in value added per unit of subsidy:

$$\frac{VA_{t+i} - VA_{t-1}}{VA_{t-1}} \times \frac{VA_{t-1}}{\sigma_t} = \frac{VA_{t+i} - VA_{t-1}}{\sigma_t} \tag{C.3}$$

The formula above allows finding the additional value added increase per unit of subsidy spent.

We can obtain a similar conversion into value added terms of the impact of subsidies on employment. Following the same steps above outlined, we obtain the ratio:

$$\frac{EMP_{t+i} - EMP_{t-1}}{EMP_{t-1}} \times \frac{VA_{t-1}}{\sigma_t} = \frac{EMP_{t+i} - EMP_{t-1}}{\sigma_t} \times \frac{VA_{t-1}}{EMP_{t-1}} \tag{C.4}$$

This formula provides a proxy of the effect of subsidies on employment evaluated at initial labor productivity (the second factor in the multiplication). The latter term works as a conversion factor from employment into units of value added.

Provided that value added is similar for treated and control units in matched pairs, which is assumed to be the case given that the balancing property holds, the quantities above found can in practice be approximated by the ratio between the corresponding ATET and the average relative subsidy size of the respective treatment group. The two averages in both the numerator and denominator simply cancel out.