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Waste of money or growth opportunity: The causal effect of EU subsidies on Hungarian SMEs

Ádám Banai[a,b,*], Péter Lang[a], Gábor Nagy[a], Martin Stancsics[a]

[a] Magyar Nemzeti Bank (the central bank of Hungary), Szabadság tér 9, Budapest, 1054, Hungary
[b] MNB Department, Corvinus University of Budapest, Fővám tér 8, Budapest, 1093, Hungary

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ABSTRACT

Although EU subsidies aiming at economic development play a pivotal role not only for Hungary but for the entire European Union as well, there is a debate regarding their effectiveness in the literature. This paper investigates the impact of direct economic development subsidies extended in the context of Structural Funds and the Cohesion Fund as part of the 2007–2013 programming period of the European Union on Hungarian micro, small and medium-sized enterprises. Based on a micro database, we evaluate the impact of corporates' first subsidies on various performance indicators, using a combination of propensity score matching and fixed effects panel regression. According to our results, economic development funds had a significant positive effect on the number of employees, sales revenue, gross value added and, in some cases, operating profit. However, the labour productivity of enterprises was not significantly affected by any of the support schemes. Furthermore, by explicitly comparing non-refundable subsidies (grants) and refundable assistance (financial instruments), we find that there is no significant difference in the effectiveness of the two types of subsidy.

1. Introduction

Over the funding period of 2007–2013, nearly HUF 1 800 billion (EUR 6.43 billion calculated at an average exchange rate of 280 EUR/HUF) in direct economic development subsidies was allocated to Hungarian enterprises in the context of the Economic Development Operational Programme and the Regional Development Operational Programmes. This means that on average, such EU subsidies amounted to more than one-half percent of GDP per year. Stock and bond markets are underdeveloped in Hungary, and are only a financing option for the largest companies. Therefore, besides bank loans and guarantees, EU funding represents a key external funding source for the Hungarian SME sector. A particularly important aspect is that two-thirds of the subsidies were absorbed by micro, small and medium-sized enterprises, which are most reliant on Hungarian financial intermediaries. Thus, EU funding can decrease this dependency and create an opportunity for diversifying firms' funding structures, thereby easing financing constraints. In this respect, such economic development subsidies play a pivotal role for Hungary, and also for countries struggling with similar problems. Besides, since easing financial constraints could facilitate growth, these subsidies are relevant for the European Union as a whole, because fostering the convergence of less developed regions is a central objective. It is therefore important to examine how these funds are utilised.

State subsidies, such as investment grants and various financial instruments, are usually justified by the existence of credit

* Corresponding author at: Magyar Nemzeti Bank (the central bank of Hungary), Szabadság tér 9, Budapest, 1054, Hungary.
E-mail addresses: banaia@mnb.hu (Á. Banai), langp@mnb.hu (P. Lang), nagy.gustav@gmail.com (G. Nagy), martin.stancsics@gmail.com (M. Stancsics).

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rationing as a result of credit market failures. However, theoretical models of such market failures are often not robust to certain modelling assumptions (Hillier and Ibrahimo (1993)). This could imply large differences in the effectiveness of various policies in separate models. For example, in the framework of Minelli (2009i), guarantees and interest rate subsidies appear to be optimal tools to mitigate the consequences of market failures, whereas investment grants are shown to be suboptimal. In contrast, in a model based on Holmstrom and Tirole (1997), Berlinger et al., 2017 show that the exact form of the state subsidy is irrelevant under certain conditions (most importantly, the existence of private opportunities to transform any subsidy into an incentive-compatible financing structure). Moreover, in the models of, e.g., Chaney and Thakor (1985) and Schertler (2000), certain types of subsidies distort firms' incentives and may thus even aggravate already existing market failures. Thus, as existing theoretical models do not provide unambiguous recommendations in this respect for policymakers, empirical analyses are indispensable.

Unfortunately, based on the empirical literature, it is not clear whether economic development schemes – in particular EU programmes – indeed have a growth-stimulating effect. In the academic literature providing an ex post evaluation of the impact of these considerable subsidy programmes, two typical approaches can be identified: macroeconomic modelling, which treats the subsidy programme as an exogenous shock to the beneficiary sector(s); and the empirical approach explicitly assessing the impact and utilisation of the subsidies on indicators of observational units (typically either recipient regions or corporate beneficiaries) of the sample.

Several studies employed the first approach, estimating the impact of these funds using macroeconomic models (e.g. Cappelen et al., 2003; Pereira and Gaspar, 1999). Alongside the positive economic effects, they emphasised that the impact may differ according to the type of subsidy. The European Commission has also published numerous studies to assess the impacts, examining the effect of various schemes and periods using a DSGE model (e.g. Varga and in 't Veld, 2011; Roeger et al., 2008; Monfort et al., 2016). Although they stress in several instances that the subsidies are not only targeted at GDP growth and that many schemes are expected to have a specifically long-term effect, the model primarily looks at the schemes' impact on GDP. Regardless, the findings broadly confirm the positive impact of subsidies. For Hungary, Balás et al. (2015) use a multi-sectoral, computable general equilibrium (CGE) framework to estimate the impact of subsidies distributed during the programming period of 2007–2013. According to the findings, subsidies provided a short-run, direct impulse to the Hungarian economy through investment demand. However, they did not provide any long-term increase in the firms' capacity or improvement in their efficiency. At the end of the period, the level of GDP was nearly 2 percent higher than it would have been without subsidies.

General equilibrium models allow for the quantification of spillover effects, which may account for a significant part of the total impact. However, such models require numerous assumptions (for instance related to the corporate-level utilisation of funds), which should be kept in mind when interpreting their results. Well-specified econometric models, on the other hand, are able to directly evaluate how EU subsidies have been utilised. One of the explicit goals of European development policies is to achieve greater economic and social cohesion and to reduce disparities between the level of development of the various *regions* (Rodríguez-Pose and Fratesi, 2004). Therefore, a regional-level analysis lends itself straightforwardly to assessing the effectiveness of EU subsidies in fulfilling their objective. Among regional-level analyses, most studies assessing the effect of European regional policy are based on an empirical version of a theoretical growth model (e.g. the neoclassical growth model) testing β -convergence along the lines of Sala-i-Martin (1996).[1] For example, Rodríguez-Pose and Fratesi (2004) use this framework to assess whether Structural Funds support achieved its goal of delivering cohesion. For the period 1989–1999, the authors find only a weakly positive and often insignificant impact of the Structural Funds on regional growth across Europe. They argue this is due to the inefficient allocation of funds among the main development axes. Trying to account for the reversal in the trend of convergence in regional income levels in the EU during the 1980s, Fagerberg and Verspagen (1996) also model the growth rate of regional GDP per capita over the period 1980–1990. While finding evidence for β -convergence, they also observe that even though EU investment loans contribute significantly to regional growth, transfers from the European Regional Development Fund seem to have a negative effect.

Instead of basing their empirical approach on theoretical underpinnings, some studies employing regional datasets apply techniques from the programme evaluation literature. A leading example among them is Becker et al. (2010), who exploit the fact that under the Objective 1 scheme, NUTS-2 regions with a per capita GDP level below 75 percent of the EU average qualify for Structural Funds transfers. The paper thus uses a regression discontinuity design (RDD) to estimate the impact of Objective 1 programme participation. According to its results, an impact of 1.6 percentage points of per capita GDP growth exists within the same programming period, but no significant effects on employment growth can be found. Complementing the previous study, Becker et al. (2013) investigate the heterogeneity in this regional average treatment effect. Using NUTS-2 data, they find that only about 30 and 21 percent of regions – those with sufficient levels of human capital and quality of government – show a significantly positive treatment effect on per capita income growth and per capita investment, respectively. Furthermore, Becker et al. (2012) focus on the estimation of the response of regional growth to changes in the *intensity* of regional transfers from the Structural Funds and the Cohesion Fund, using data at the NUTS-3 level. With generalised propensity score estimation, they arrive at the result that for 18 percent of the recipient regions a reduction of transfers would not reduce growth as they received more than their estimated maximum desirable intensity.

Micro-level impact evaluations explicitly examining the utilisation of funds at the corporate level represent another empirical approach. Notable examples are Biagi et al. (2015), Trzciński (2011), Alecke et al., 2010 and Criscuolo et al. (2012), evaluating the corporate-level impact of European Union subsidies on Italian, Polish, German and UK data, respectively. They utilise various

[1] Hagen and Mohl (2011) provide an excellent review of the methodological approaches, related econometric issues and main empirical examples of analyses examining the impact of EU Structural Funds on regional economic growth or convergence.

standard methods from the programme evaluation literature: difference-in-differences, propensity score-based estimation, regression discontinuity design, as well as Heckman-type selection models; occasionally even a combination of these. Among others, the results of these are summarised by Mouqué (2012). They vary by scheme and country, but the key findings are the following. The subsidies had an essentially positive impact on output and employment, but did not have any notable impact on productivity. However, no significant results can be observed among large enterprises. In addition, there is room for further improvement in terms of cost effectiveness, as soft loans, subsidies of smaller amounts and even the provision of corporate consultancy proved to be surprisingly successful. Although not dealing with subsidies disbursed in the European Union, another notable analysis is that by López-Acevedo and Tan (2011), who apply a methodology very similar to ours to Latin American enterprise programmes. The four country-level studies detailed in the report found statistically significant impacts of all SME programmes on sales, while the significance of the estimated impacts on employment, export intensity, profitability and total factor productivity varied across countries. Among the analyses conducted on Hungarian data, Budapest Institute (2013) is the most similar to our approach. This study looked at the 2007–2013 programming period and used difference-in-differences estimation and propensity score matching. According to its findings, the subsidies had a positive impact on employment, but no significant growth was found in terms of the sales revenue.

We have not undertaken a general study of the impacts of European Union funds. Giving a comprehensive estimate of the total economic impact of these subsidies is problematic: EU funds are geared towards reinforcing both social and economic cohesion, so in many cases, no positive impact is expected in purely economic terms, or only in the very long run, spanning up to several decades. This is a fundamental issue in the macroeconomic models that investigate the impact of EU funds in the most comprehensive manner possible, as in some instances they attempt to measure effects that cannot be expected from the schemes. In addition, it is often very difficult to adequately factor in the impact of different schemes in a complex modelling framework (for example, Varga and in 't Veld, 2011 analyse in detail how they attempt to introduce the various subsidy types into their model as different shocks). For this reason, this paper is limited to a narrow group of EU subsidies that specifically target economic development and are expected to have a positive impact on economic indicators. From these, we examined refundable and non-refundable subsidies separately, as well as explicitly comparing their effectiveness. We estimated the impact of these subsidies on the performance indicators of micro, small and medium-sized enterprises.

Our method, a micro-level approach, can be broken down into two steps. As a first step, we estimate the probability of receiving a subsidy for every firm-year observation and match a non-subsidised (control) firm to each beneficiary enterprise based on this probability (propensity score matching). In the propensity score model, we use explanatory variables describing the companies' performance before the subsidy, as well as some of their other characteristics. Besides, we require an exact match in terms of the sector of economic activity. Secondly, to filter out the remaining differences between the matched enterprise pairs, we employ a fixed effects panel regression corresponding to a difference-in-differences approach. Results obtained this way are interpreted as the causal impact of the subsidy.

In terms of methodology, this study is similar to the micro-level evaluations mentioned above. However, our paper goes beyond them in several respects. From a policy perspective, our addition is that while a few earlier papers hinted at a comparison of refundable and non-refundable subsidies (for example, Biagi et al., 2015), we explicitly compare the two in order to get a more valid result. Our approach and long time series also enable us to inspect the dynamics of the impact. Equally importantly, from a methodological point of view, we lay great emphasis on ensuring the internal validity of our results. In particular, in contrast to most impact evaluations of EU programmes, we explicitly show the similar behaviour (parallel trend) of the treated and control groups before the subsidy. We also try to be more prudent about standard errors than earlier analyses.

This study is structured as follows. Section 2 presents the scope of evaluation and the available subsidy, balance sheet and profit and loss account data. Section 3 discusses the methodological considerations of our impact evaluation. In Section 4, we present our main findings, supplemented with robustness checks and details on additional estimations. Finally, Section 5 sums up the study and presents its main conclusions.

2. Data and scope of evaluation

2.1. Scope of evaluation

The 2007–2013 programming period was the first full EU development cycle since Hungary's accession in 2004. During this period, a grant of EUR 24.92 billion was allocated to Hungary from the Structural Funds and the Cohesion Fund of the European Union (Boldizsár et al. (2016i)). Supplemented with national own contribution, this amounted to contracted subsidies totalling HUF 9 754 billion (EUR 38.84 billion calculated at an average exchange rate of 280 EUR/HUF during the programming period). This analysis attempts to evaluate the impact of subsidies *directly aiming at economic development* on the beneficiary corporations out of this contracted amount. In Hungary, direct economic development subsidies were concentrated in the Economic Development Operational Programme (EDOP) and the Regional Development Operational Programmes (RDOP). Therefore, this study focuses on the direct economic development subsidies of the latter operational programmes. Among these subsidies, amounting to HUF 1 768 billion (EUR 6.31 billion) during the programming period, we filtered out subsidies if we deemed their impact not measurable.[2] We have

[2] This refers to the following types of situations. First, the firm benefiting from the subsidy occasionally differed from the entity receiving the payments and therefore the former could not be observed. Second, there were projects for which the objective of the subsidy was to develop the environment of enterprises rather than the observed enterprises themselves. Finally, in some instances, technological development subsidies had the

Table 1
Distribution of non-refundable subsidies according to development objective.

Development objective	Number of subsidies	Amount granted (HUF billion)
Employment enhancement	592	44.38
Support of research, development and innovation activity	3 665	233.40
Support of physical infrastructure for research, development and innovation activity	68	34.84
Environmental investment	83	7.20
Development of production plants, technology and capacity	13 900	356.92
Development of tourism	516	89.07
Development of corporate information and communication technology	2 925	18.66
Corporate consultancy	357	5.37
Total	22 106	789.84

Note: The table contains subsidies with the purpose of direct economic development, extended to micro, small and medium-sized enterprises excluding non-profit institutions serving households and the general government.

chosen to evaluate only the impact of these types of measures on corporate beneficiaries because we assumed that we could only identify and verify the effects if the final recipients of the development scheme are corporate beneficiaries. Moreover, the scheme itself explicitly aims at the improvement of the performance of these beneficiaries.

The impact evaluation of the above-mentioned subsidies on various corporate performance indicators is the core issue of this paper. Nevertheless, it is worth briefly reviewing the general and specific objectives of operational programmes to assess their fulfilment and to identify differences in the impacts by objectives. The comprehensive development plan for this period had the objective of increasing employment and creating the conditions for sustainable growth. The latter objective was broken down to three specific ones as competitiveness improvement, development of the business environment and fostering the growth of the whole economy. Although the objectives of the individual measures were often complex according to the strategic objectives of operational programmes, we identified the main goal (development objective) of each measure containing non-refundable subsidies (Table 1). Where the size of our estimation database allows for reliable estimates, we also applied the methodology presented in the study to these individual development objectives.

While the above groups only include non-refundable subsidies (grants), *financial instruments* constitute a separate category[3] (which cannot be broken down by development objective). Financial instruments include (1) micro financing, which replaces the function of small-amount loans that cannot be granted under market conditions due to elevated costs or other borrowing constraints (these are essentially subsidised loans); (2) guarantee instruments designed to decrease banks' lending risks; and (3) the development of the capital market in the form of providing funding to venture capital funds[4] (Balás et al., 2015).[5] Contrary to grants, which were allocated by a central agency, financial instruments are allocated by financial intermediaries (e.g. banks, venture capital funds and guarantee institutions), which are in turn selected via calls for proposals. Pricing and allocation decisions are then made by these financial intermediaries within certain regulatory limits.

2.2. Subsidy data

Our data mainly come from two sources. On the one hand, we calculated the enterprises' financial indicators based on the balance sheet and profit and loss account data submitted with their annual tax returns to the National Tax and Customs Administration (NTCA).[6] In addition, we obtained information on EDOP and RDOP subsidies from the Unified Monitoring Information System (EMIR) with the collaboration of the Prime Minister's Office.[7] From the EMIR we obtained a project-level database that shows the dates of payments (instead of the decision-making or contract-signing dates) at an annual frequency. We also have some information about submitted applications that were not granted any funding (non-winning applications); however, the dates of the negative decisions are not known.

Table 2 displays certain characteristics of the payment distribution for the set of the micro, small and medium-sized enterprises

(*footnote continued*)
objective of environmental protection. We classified 95.5 percent of the non-refundable and all the refundable subsidies as having a measurable impact.

[3] There have also been certain calls for proposals in which the funding of a project contained both a refundable and a non-refundable part. As we explicitly wanted to estimate the effect of refundable and non-refundable subsidies, we have not classified these combined instruments (or any of their parts) as either. Therefore, they are not featured in either the descriptive statistics or the impact evaluations.

[4] As these capital instruments differ from the other two types of financial instruments in several respects (for example in the form of repayment or the extent of control the financing agency obtains over the subsidised firm), we excluded these subsidies from our analysis.

[5] The main difference between grants and financial instruments lies in the fact that the latter have to be repaid in some form or another, whereas the former do not. Therefore, to emphasise this contrast, we will refer to grants as non-refundable subsidies and financial instruments as refundable subsidies in this paper.

[6] We supplemented corporate data obtained from the NTCA with data from the Hungarian Central Statistical Office's Business Register (BR).

[7] We also have data on the subsidies of the European Agricultural Fund for Rural Development (EAFRD), but we do not attempt to estimate their effect. We only use this data to define the treated and control groups more precisely.

Table 2
Descriptive statistics of the subsidy amounts (HUF million).

	Subsidy category	
	Non-refundable subsidies	Refundable subsidies
Mean	35.73	22.88
Standard deviation	85.45	35.59
1st percentile	0.78	1.50
5th percentile	1.37	3.00
10th percentile	2.49	3.50
25th percentile	5.31	6.00
Median	14.79	10.00
75th percentile	29.89	30.00
90th percentile	81.83	50.00
95th percentile	131.71	50.00
99th percentile	401.89	162.40

Note: The table contains subsidies with the purpose of direct economic development, extended to micro, small and medium-sized enterprises excluding non-profit institutions serving households and the general government.

Table 3
Distribution of subsidies according to the duration of their payment.

Time passed between the first and last payment	Subsidy category	
	Non-refundable subsidies	Refundable subsidies
0 years	13 231	7 776
1 year	5 353	0
2 years	2 443	0
3 years	909	0
4 years	140	0
5 years	17	0
6 years	7	0
7 years	5	0
8 years	1	0
Total	22 106	7 776

Note: The table contains subsidies with the purpose of direct economic development, extended to micro, small and medium-sized enterprises excluding non-profit institutions serving households and the general government.

(SMEs) under review. Both subsidy categories exhibit distributions with heavy tails towards higher subsidy amounts due to a few exceptionally large subsidies. In our view, the impact of the highest subsidies can be better captured with individual analysis instead of statistical methods, as selection based on unobservables may be particularly strong in their case. Therefore, in the impact evaluation we will ignore about the top 1–2 percent of non-refundable subsidies (based on size).

Table 3 provides an overview of the time that elapsed between the first and last payment of subsidies. Non-refundable subsidies are disbursed over an extended time period due to the form of ex post financing, contrary to refundable subsidies that are always fully paid out in the first year. However, as only a single treatment date can be handled by our methodology, we must decide which of the payment dates is to be regarded as the time of treatment. We chose the date of the first payment.[8] With this decision, we underestimate the impact of the subsidy during the years when payment was still ongoing if the subsidy only exerts its full effect after the last payment. We opted for this approach as we feel that this bias is less severe than the one that would have stemmed from choosing the time of the final payment as the treatment date.

Table 4 shows the distribution of subsidised enterprises by the number of funded projects. It is apparent that winning multiple subsidies is relatively frequent, mainly among the beneficiaries of non-refundable grants. However, extending multiple subsidies to an enterprise creates an issue for the examination of the impact, as earlier subsidies influence the values of our control variables. To handle this, during aggregation of the subsidy data to the enterprise level, we treat multiple subsidies of the same type and extended in the same year as one subsidy, and after aggregation we only analyse the first subsidy of a given company.

[8] Using the last payment date is also an option, according to the argument that an investment is completed following the submission of the last statement of fulfilment, and the new capital good resulting from the investment is only incorporated into production and exerts an effect once the investment is completed. However, if we regard the last payment date as the treatment date and there are numerous grants in which the subsidy has already started making an impact on the company before the last payment, when applying matching to data immediately preceding treatment, the matching would at least partially be based on observations affected by the treatment. This bias is definitely smaller if we identify the first payment date as the treatment date.

Table 4
Distribution of subsidised enterprises according to the number of funded projects.

Number of funded projects from the given subsidy category	Subsidy category	
	Subsidised firms from non-refundable subsidies	Subsidised firms from refundable subsidies
1	7 831	5 185
2	2 325	801
3	1 034	144
4	544	60
5	288	12
6	177	10
7	106	5
8	56	1
9	32	5
10	12	0
11	12	1
12	4	2
13	3	0
14	2	0
15	0	1
16	0	1
20	0	1
25	0	1
Total	12 426	6 230

Note: The table contains micro, small and medium-sized subsidised enterprises excluding non-profit institutions serving households and the general government.

2.3. The financial indicators of the enterprises

The NTCA database contains data for every enterprise subject to taxation and conducting double-entry bookkeeping. As we also want to investigate the effect of the subsidy on the earnings of the beneficiaries, we filtered out those entities that were classified as non-profit institutions serving households or into the sector of general government at any point in their history. The corporate set thus obtained was further narrowed to micro, small and medium-sized enterprises.[9] To avoid having to split up corporate histories due to changes in their size category when filtering enterprises by size, we classified businesses as micro, small and medium-sized enterprises if they were most often (mode value) categorised as such during the analysed period based on the threshold values for common European Union categories. In our analysis, we used balance sheet and profit and loss account data of corporates from 2004, as double-entry bookkeeping became mandatory for a larger share of SMEs at the time of Hungary's accession to the EU and so the database grew significantly.

We chose the dependent variables from the NTCA database: number of employees, real gross value added[10], real operating profit, real sales revenue, real tangible assets and labour productivity[11] .[12] We also used real pre-tax profit[13], leverage (the ratio of liabilities to the balance sheet total), the ratio of export to sales revenue, foreign majority ownership, the current SME classification, the region of the head office and our own sectoral categorisation[14] as control variables in the propensity score model. Furthermore, to allow us to perform the matching not only based on *levels* but also on *dynamics* before treatment, we used the growth rate of some variables in the past three years, or more specifically, a version of it featuring better characteristics.[15]

Tables A.2 and A.1 in the Online Appendix contain the descriptive statistics of our continuous corporate indicators and the

[9] Out of the total disbursed amount of HUF 1 768 billion, the subsidies under review extended to the group of enterprises thus defined account for 1 036.26 HUF billion in non-refundable subsidies and 178.34 HUF billion in refundable subsidies.

[10] Defined by subtracting material expenses from the sum of the net sales revenue and the capitalised value of own performance.

[11] Real value added per employee.

[12] We measured every financial indicator expressed in real terms at 2015 prices.

[13] To ensure that the cost of finance is factored in when matching treated and control enterprises, we used real pre-tax profit instead of real operating profit in the propensity score model.

[14] It differs from the NACE level 1 codes in that in order to create categories of similar size, we broke down manufacturing into the manufacture of food products and beverages; textiles and wearing apparel; wood and paper products, furniture and printing; chemical and pharmaceutical products; plastic and metal products; electronic products; machinery and transport equipment; and other manufacturing. In addition, we combined financial and insurance activities with the sector of real estate activities; and the sector of public administration and defence, compulsory social security with education, and human health and social work activities.

[15] Some of these variables often take on a value of zero, in which case the natural growth rate (apportioning the difference between periods $t-1$ and $t-4$ to the value for the period $t-4$) cannot be calculated. We therefore used an indicator as a variant of the growth rate that is also interpretable in this scenario and behaves better for the purpose of matching. This indicator can be expressed using the following formula: $\frac{X_{t-1}-X_{t-4}}{2\max\{|X_{t-1}|;|X_{t-4}|\}}$.Although the indicator is less intuitive and therefore more difficult to interpret, it is better suited to our objectives as we only use it for matching.

distribution of our categorical variables, respectively. Unfortunately, our database contains observations of the variables under review that are either unrealistically high or low or violate fundamental accounting relations. We classified these observations as erroneous and removed all corporate histories from our database that included such observations. As we show among our robustness checks, our results are robust to this relatively strict data cleaning procedure, which implies that we removed enterprises that are not systematically different from the perspective of the impact evaluation.

3. Theoretical framework of the impact evaluation

Based on theoretical considerations, the subsidies extended to enterprises may affect these entities in two ways: (1) they may provide additional funds for firms with restricted access to external funding, and (2) they may serve as a cheaper source of funding than what is currently available for companies not facing credit constraints (see, e.g., Criscuolo et al., 2012). Because these subsidies can only be used to finance new investments, we expect the expansion of the stock of fixed assets in both cases, potentially to a greater extent in the case of companies with restricted access to funding. A difference is that according to our expectations, in the first case, part of the newly developed capacity can be sustained even with market funding (provided that the subsidy helps the beneficiary alleviate its financing constraints, e.g. by establishing a banking relationship). In the second case the stock of fixed assets will return to the previous level in the absence of preferential funding over time. However, the latter case may also have a social benefit, provided that the subsidy facilitates the implementation of an investment which is beneficial on the social level owing to positive externalities.

This additional investment resulting from the subsidy will most likely lead to an increase in production and value added generated by the company. The impact on the number of employees is not clear based on the above reasoning – the beneficiaries could, in principle, spend the subsidy on a capacity expansion using current technologies or to introduce labour-saving technologies. Taking into account, however, that one of the main objectives of the programming period under review was employment expansion, we also expect an increase in the number of employees. The above line of thought does not, however, allow us to have clear expectations regarding the impact on productivity.

3.1. Identification

In this subsection we specify the impact to be estimated and our identification assumptions using the Neyman-Rubin causal model (see, e.g., Imbens and Wooldridge, 2009). Let Y_{it} be the variable of interest, and denote its potential value at time t by $Y_{it}(1)$ if firm i takes part in the programme, and by $Y_{it}(0)$ if it does not take part. Furthermore, let G_i be an indicator variable with a value of 1 if the analysed company is supported and 0 otherwise. The impact that we would like to estimate is the effect of the subsidy on the subsidised entities τ periods after the subsidy (*average treatment effect for the treated*, ATT):

$$ATT_\tau = E\left[Y_{t_0+\tau}(1) - Y_{t_0+\tau}(0) \mid G_i = 1\right]$$

where t_0 is the time of the subsidy. The fundamental issue is the fact that we can observe only one of the outcomes. We thus need additional assumptions to be able to estimate the ATT.

First, we assume that the potential outcomes of a given company are not influenced by the way the subsidies are allocated among the other companies (*stable unit treatment value assumption*, SUTVA). This assumption practically excludes spillover effects. Although spillover effects certainly exist, it is still required that we maintain the SUTVA assumption as we lack data about the network of relations among companies. However, this assumption is not quite unrealistic because, on the one hand, a relatively small proportion of companies (fewer than 5 percent) receive the kind of subsidy we analyse. On the other hand, the subsidies are spread in time over the cycle.

Our most important identification assumption is *conditional unconfoundedness*. It asserts that conditional on certain factors the allocation of subsidies is independent from the potential outcomes – it can be regarded as random. We assume this in the following form:

$$(Y_{it+\tau}(0),\ Y_{it+\tau}(1)) \perp W_{it} \mid X_{it-1},\ ..., X_{it-s},\ Y_{it-1},\ ...,\ Y_{it-s}\ \ \forall\ \tau \geq 0,\ s > 0,$$

where Y_{it} continues to be the variable of interest, X_{it} contains explanatory variables considered to be relevant, and W_{it} is a dummy variable indicating whether the company receives a subsidy in the given year. This assumption is a lot more permissive than the usual conditional unconfoundedness for cross-sectional data. The main difference is that the conditions of independence also include the past values of the dependent variable, and thus also the unobserved factors that are constant over time for each company pertaining to the given variable.

Our last assumption is *common support*, which arguably holds, given the large number of control companies. It implies that any given company's observation is not unambiguously classifiable into the treated and non-treated categories, given the historical values of the company's variables:

$$P(W_{it}) < 1 \mid X_{it-1},\ ..., X_{it-s},\ Y_{it-1},\ ...,\ Y_{it-s}.$$

Of the above three assumptions, conditional unconfoundedness requires the most explanation. To shine more light on this issue, let us assume that the variables X and Y are determined as follows:

$$Y_{it} = f\left(W_{it}, ..., W_{it-s}, Y_{it-1}, ..., Y_{it-s}, X_{it-1}, ..., X_{it-s}, \delta_t, \eta_i, \varepsilon_{it}\right),$$

$$X_{it} = g(W_{it}, ..., W_{it-s}, Y_{it-1}, ..., Y_{it-s}, X_{it-1}, ..., X_{it-s}, \gamma_t, \zeta_i, \mu_{it}),$$

where δ_t, η_i, ε_{it}, γ_t, ζ_i and μ_{it} are non-observable. Because we can control for the individual and time fixed effects, in order for the conditional unconfoundedness assumption to hold, we need that

$$\varepsilon_{it}, ..., \varepsilon_{it+\tau}, \mu_{it}, ..., \mu_{it+\tau} \perp W_{it} \mid Y_{it}, ..., Y_{it-s}, X_{it-1}, ..., X_{it-s} \ \forall \ \tau \geq 0, \ s > 0.$$

This statement practically means that the current and future non-observable shocks (pertaining to both the dependent variable and the explanatory variables) do not influence the probability that the company is subsidised. There are two types of selection that threaten the above assumption: self-selection by the company and allocation selection by the agency assessing the proposal.

Allocation selection is the less serious problem. Although it is possible that a feature (not constant in time) not observed by us, but observed by the evaluating party, influences both the allocation and the potential outcomes (for example the business plan), the information available for the evaluating agency is also limited regarding the applicant companies. Moreover, in the case of smaller subsidies that we typically analyse, a number of calls for proposals were such that the mere fact of meeting the conditions automatically entailed a positive decision (Hungarian Government (2011), §24).

Self-selection is a more severe problem since the company certainly does have some information that we cannot observe, but which influences its decisions. If this information also correlates with the dependent variable, our estimates may be biased. For example, if the companies that apply for subsidies are the ones that currently have good investment ideas, better financing opportunities or a more efficient organisational structure, then the estimated impact will be biased upward. No fully reassuring answer can be given to this problem, but the following points should be considered. First, the subsidies could be used not only for innovative developments – a major part of the allocated grants had capacity expansion as a purpose. Second, there should be no major difference in the current financing options of the companies from banks, as in case of a successful implementation of the project the awarded sum represents tangible collateral for the bank. Third, the (perceived) administrative costs of submitting a proposal consist of elements that might vary across firms, but much less so for a given firm through time (quality of management, expenses entailed by the application process). The main concern in relation to this could be that the administrative costs of companies who already submitted applications in the past would most likely be lower than those of companies applying for the first time. However, this can be eliminated for the most part by only analysing each firm's first subsidy. Therefore, neither the subsidised nor the non-subsidised companies have experience in the implementation of such projects.[16]

3.2. Empirical strategy

To measure the impact of the subsidy, we essentially use a *difference-in-differences* (DiD) approach in this analysis, supplemented by the selection of the control group by *propensity score matching*. For the matching we primarily use the lagged value of the dependent variable, the lagged value of some arguably relevant explanatory variables and the lagged value of the three year growth rate (as defined in subsection 2.3) of some of these variables supplemented with the company's current SME classification and the region of its head office. Besides, we require an exact match in terms of the sector.[17]

If we were able to find perfect matches based on every variable (especially the dependent variable), we could consistently estimate the impact of the subsidy by comparing the averages of the variables of interest of the treated companies and the control companies matched to them. In reality, some differences remain in these variables between the two groups even after the matching. Therefore, to filter out its time-constant part, we then apply a fixed effects panel regression that can be considered a disaggregated version of the traditional DiD (Angrist and Pischke, 2009, p. 170). The long time dimension of the database is beneficial for several reasons. On the one hand, it allows us to check whether a parallel trend existed between the treated and control firms until the treatment. On the other hand, we may also identify the possible longer-term effects of the treatment.

Our procedure comprises the following steps. First, by applying a probit model, for every company-time pair in our database, we estimate the probability of a company receiving a subsidy during that period – provided that it had not received any subsidy until then – based on various company characteristics prior to that date. In our model, these are the following: number of employees, real sales revenue, real stock of tangible assets, real pre-tax profit, real gross value added, the three year growth rate of these, in addition, leverage, the ratio of export to sales revenue, the indicator variable of foreign majority ownership, region, and current SME classification. After that we match a company that was never treated to each beneficiary. We require an *exact match* in terms of the main activity's sector and a binary variable stating whether the company operates with negative equity.[18] Within these categories we match the control company with the propensity score closest to that of the treated company at the time of treatment (*nearest neighbour matching*).[19] We match the history of the control company to the history of the treated company for the years in which both are available.

[16] More specifically, in the implementation of projects executed between 2007 and 2015, falling within our analysed category. Unfortunately, we do not have information on what other subsidies these companies were granted.

[17] The quality of the matching is investigated in Online Appendix B.

[18] We expect firms with negative equity to behave differently from other enterprises, as officially, if a firm operates with negative equity for two consecutive years and does not take any action against this situation, it may face legal sanctions.

[19] Following Sekhon (2011), we perform the matching not based on the estimated participation probability but on the expected value of the latent equation of the probit model to avoid any crowding next to 0 and 1. Moreover, we define a fairly narrow maximum distance (caliper, 0.02) to ensure the similarity of the treated and control companies.

As the next step, we estimate the following model:

$$Y_{i\tau} = \sum_{\tau=T_0}^{-2} \beta_\tau D_{i\tau} + \sum_{\tau=0}^{T_1} \beta_\tau D_{i\tau} + \mu_\tau + \delta_t + \eta_i + \varepsilon_{i\tau},$$

where τ is the time relative to the subsidy, while t is the actual (calendar) time, η_i, δ_t and $\varepsilon_{i\tau}$ denote the individual fixed effects, time fixed effects and the idiosyncratic error term, respectively. μ_τ is the time fixed effect of the year relative to the subsidy[20], which is of key significance in our model. Finally, $D_{i\tau}$ is a variable which is the relative time for treated enterprises and constant 0 for control enterprises. If our assumptions are correct, the corresponding coefficient (β_τ) will equal the quantity of interest, ATT_τ.

We can justify the inclusion of the relative time fixed effects as follows: based on our observation, violating the parallel trend assumption, the group of treated companies performed increasingly better than the other companies in terms of most of the dependent variables during the periods prior to treatment. This may be caused by the fact that at the allocation decision, the performance of recent periods is positively taken into account (while control companies have been selected so they resemble the supported companies in that respect). By including the relative time dummies in the equation, we filter out this subsidy-time-dependent "effect". This is analogous to comparing the outcomes of the two groups based on the relative time (i.e., shifting the subsidies to one date) in the case of a traditional DiD approach. Our argument is also supported by the fact that if we narrow down our sample to the companies receiving the subsidy in a given year (e.g. in 2010) and their pairs (and thus in this case the actual and relative times coincide), the results resemble the results of the model estimated on the entire period including the relative time dummies. The necessity of including these dummies, and therefore the necessity of the matching, is illustrated in Subsection 4.2 and in the corresponding Figures C.1–C.3 in the Online Appendix.

Despite the fact that matching-based methods enjoy great popularity in the programme evaluation literature, there are few results about the asymptotic distribution of the resulting estimators. It is certain that the tests performed on the sample obtained after matching without taking into account the effect of the matching are invalid for two reasons: they disregard the number of parameters estimated in the first step, and the resulting sample cannot be regarded as random. According to Abadie and Imbens (2008), the standard errors obtained from the traditional bootstrap methods are not applicable either. Based on their suggestion, we calculate our confidence intervals based on the procedure of Politis and Romano (1994). Their main idea is that, in contrast to the traditional bootstrap, we select a sample with $b < n$ observations (where n denotes the total number of observations) *without replacement*.[21] In our case, we perform sampling with a sample size of $b = n/2$ (determined on a somewhat ad hoc basis). As the last step, we adjust for the fact that on a smaller sample, the estimation is less accurate. As shown by Abadie and Imbens (2002), the one-to-one matching based on one continuous variable and arbitrarily many discrete variables is $\sqrt{n}$ -consistent. Moreover, the fixed effects estimator is also $\sqrt{n}$ -consistent. Based on this we assume that our complete, two-step estimator is $\sqrt{n}$ -consistent, too. We generate our confidence intervals similarly to the *percentile bootstrap* procedure, adjusting for the sample size:

$$CI_\alpha = \left[\hat{\theta} + \frac{\sqrt{b}}{\sqrt{n}}\left(\hat{\theta}_{\frac{\alpha}{2}} - \hat{\theta}\right), \hat{\theta} + \frac{\sqrt{b}}{\sqrt{n}}\left(\hat{\theta}_{1-\frac{\alpha}{2}} - \hat{\theta}\right)\right],$$

where $\hat{\theta}$ is the point estimate performed on the entire sample pertaining to parameter θ, and $\hat{\theta}_q$ is the q -quantile of the subsample estimates. For each of our estimates, we calculate 1000 subsample estimates and determine the confidence intervals based on these, using the above formula.

4. Results

This section starts with the presentation of our results pertaining to refundable and non-refundable subsidies, as well as their relative effectiveness. In addition, in the case of non-refundable subsidies, we also report various subsample estimates.

4.1. Main estimates

In the estimates presented in this subsection, we attempt to differentiate the effects of the non-refundable and refundable subsidies. We had the presumption that using refundable subsidies requires a completely different motivation and performance from the beneficiary, which may also be reflected in the evolution of the dependent variables. The most important characteristics of the populations used in the estimations are displayed in Table 5. The first column shows the number of treatments that could potentially be used in the estimation (a detailed overview of the number of subsidies excluded at each step of the data cleaning procedure, as well as the various sample restrictions, is presented in Table A.3 in the Online Appendix). The third column shows the number of firms to which we could successfully match an acceptably similar control firm from the companies shown in the second column. The fourth column includes the number of firm-year observations obtained after the matching. The last column presents the average subsidy amount of the supported companies used in the given estimation.

Thanks to the size of our database we managed to find a pair that operates in the same sector and is sufficiently similar in terms of the estimated propensity score for most of the supported enterprises. The size of the final sample used for the estimation proved to be

[20] In the case of a control company, the relative time is defined as the relative time of the treated company matched with it.

[21] Because we have a panel database, we sample entire company histories. The sampling is done prior to the matching.

Table 5
Summary statistics of the main estimates.

	Number of treated firms	Number of potential control firms	Number of matched pairs	Sample size in the fixed effects model	Average subsidy size (HUF million)
Non-refundable subsidies	7 421	192 503	7 243	134 684	20.26
Refundable subsidies	1 480	192 503	1 475	27 920	18.15
Relative effectiveness	1 480	7 421	787	14 788	14.03/12.61[a]

[a] refundable/non-refundable subsidies.

sufficient in the case of every model so that we can reasonably precisely measure the effect of the subsidies. Finally, showing the average subsidy amounts makes it possible to draw stylised conclusions regarding the efficiency (per unit effect) of the subsidies, even though the subsidy amounts are not explicitly included in the estimations.

4.1.1. Non-refundable subsidies

We specified the impact evaluation of non-refundable subsidies as follows. We regarded firms that received non-refundable subsidies (as defined in Subsection 2.1) at some point in time during the period under review as treated. From these, we excluded those firms that also received any rural development subsidies or venture capital instruments at any time during the period. Moreover, because we only estimated the effect of the first subsidy, we excluded the company's history from the time of the second subsidy regardless of type. Our control group only consisted of firms that did not receive any subsidy from any EU funding programme for which we have data at our disposal.

Fig. 1 presents our results. The vertical line denotes the period *prior to* the time of treatment (that is, prior to the year of the first payment related to the given subsidy). We consider the effect of the treatment from this point on, and not from the time of treatment, because the payment of subsidies took place sometime during the given year, but our dependent variable stems from the tax return showing the end-of-year status, so the effect of the subsidy can already appear in the year of the grant. The periods located to the left from this date are useful to check the parallel trend assumption, while to the right the effect of the subsidy can be observed – provided that the identification assumptions are valid – in the years after the first payment.

The parallel trend assumption cannot be rejected based on the periods prior to the subsidy, therefore it is credible that we compare sufficiently similar firms in terms of the impact evaluation. The number of employees remains consistently higher throughout the analysed time horizon *as a result of* the treatment, by approximately two. The growth of real value added is significant throughout the entire time horizon; an annual growth in real value added of more than HUF 10 million can be observed. The treatment has a more moderate, but similarly significant positive effect on real operating profit for all five years examined. The growth of real sales revenue as a result of the treatment also remains significantly positive for at least five years, with an average magnitude of around HUF 50 million.

Examining the change in the stock of real tangible assets allows for inferences about the extent to which projects funded by subsidies are additional investments compared to projects that companies would have carried out anyway. Based on our results, the stock of real tangible assets increases due to the treatment, the extent of which is comparable to the average project size approximated by the average subsidy amount plus average own contribution. A potential implication of this could be that without the subsidies, subsidised projects would predominantly not have taken place, or only later. The gradual, slow decline of the treatment effect can be explained by various factors. For one, it is consistent with the hypothesis that the subsidies were granted to firms not facing credit constraints, or that they were not successful in alleviating these constraints in the long term. It is also possible, however, that as a result of the subsidies the beneficiaries just advanced some of their planned investments, and the decline we see is the control group catching up with them to a certain extent as they get to make those investments. Nevertheless, the decline of the treatment effect over time essentially reflects the accounting treatment of depreciation, which does not necessarily coincide with the actual usability of the asset. Therefore, the renewal of these assets might be due outside of the time horizon under review. The effect of the treatment is already apparent in the year prior to treatment. This can be explained by the fact that because non-refundable subsidies are typically financed in an ex post manner, the beneficiaries may already start the investment in the year prior to the first payment.

In the sixth subfigure, we can see the subsidy's impact on labour productivity. According to our estimates, no significant impact is detectable. Together with our previous results concerning the rise in value added and number of employees, this implies that these two variables grow at a similar rate as a result of the subsidies. This insignificant impact suggests that the investments implemented from non-refundable EU subsidies can be regarded as capacity-expanding rather than efficiency-improving (measured by labour productivity). This is reconcilable with the fact that one of the main objectives of the programmes was to foster employment. Besides, as typically the more productive firms receive support, the faster growth rate of such companies may still result in an improvement of aggregate productivity.

4.1.2. Refundable subsidies

For the estimation of the impact of refundable subsidies, we considered as treated the population of firms that were awarded a financial instrument as their first subsidy. We again considered only the first subsidy as treatment. Our control group was the same as in the case of non-refundable subsidies.

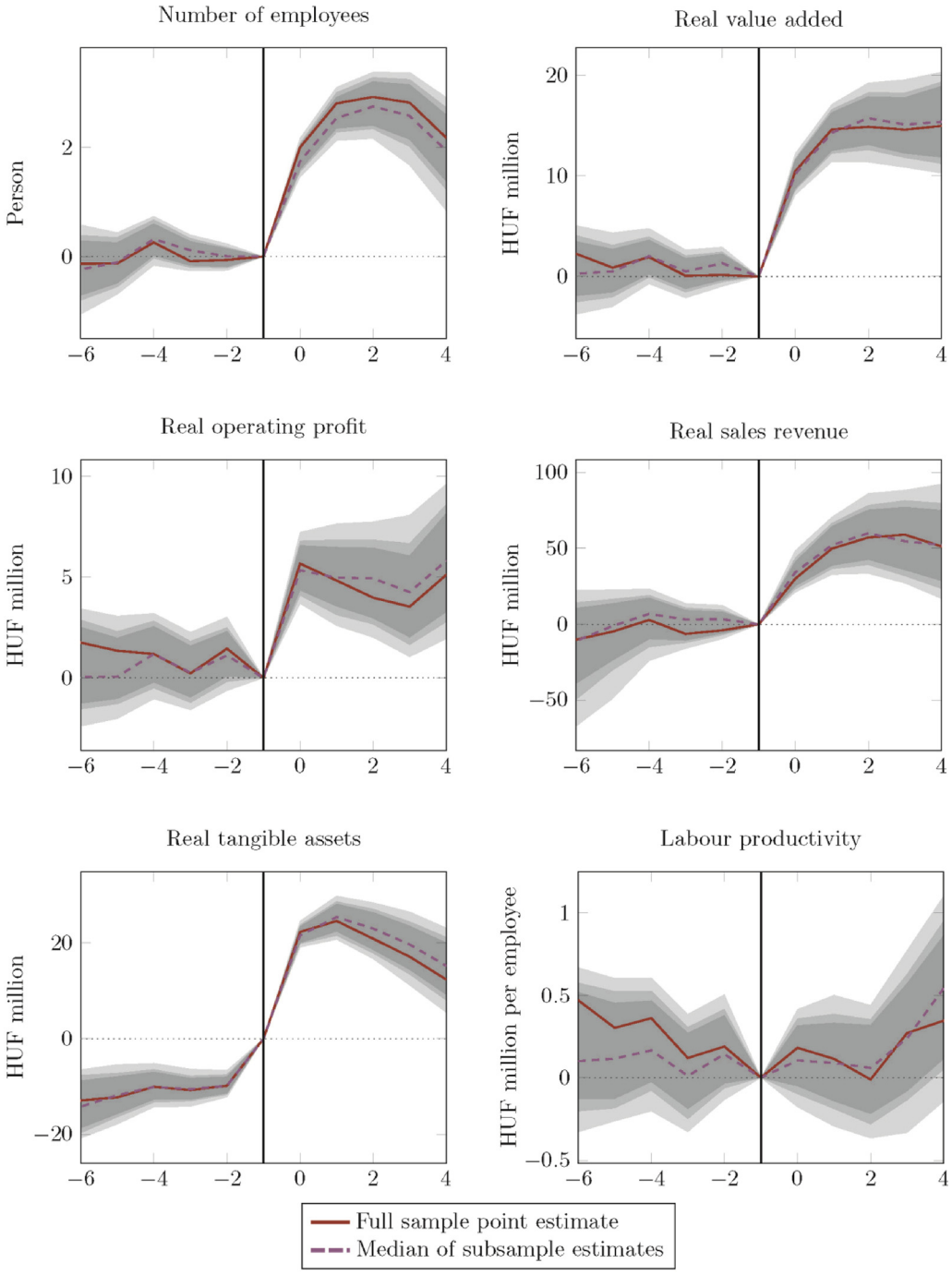

Fig. 1. Non-refundable subsidies.
Notes: Relative time in years on the horizontal axis. Shaded areas represent 90 %, 95 % and 99 % confidence intervals.

In this case, our estimation database is much smaller than that of non-refundable subsidies, therefore the uncertainty of our estimates is also greater. Nevertheless, as shown in Fig. 2, the treatment has a positive, and for three to four years significant, effect in terms of the number of employees, real value added and real sales revenue, while there is no significant effect on labour productivity, similarly to non-refundable subsidies. The estimates also show an insignificant effect on real operating profit. It is worth highlighting that in case of the stock of tangible assets, contrary to non-refundable subsidies, the effect of the treatment starts from the period prior to the payment and not one year earlier. This coincides with the fact that similarly to loans, companies received their refundable subsidies through pre-financing.

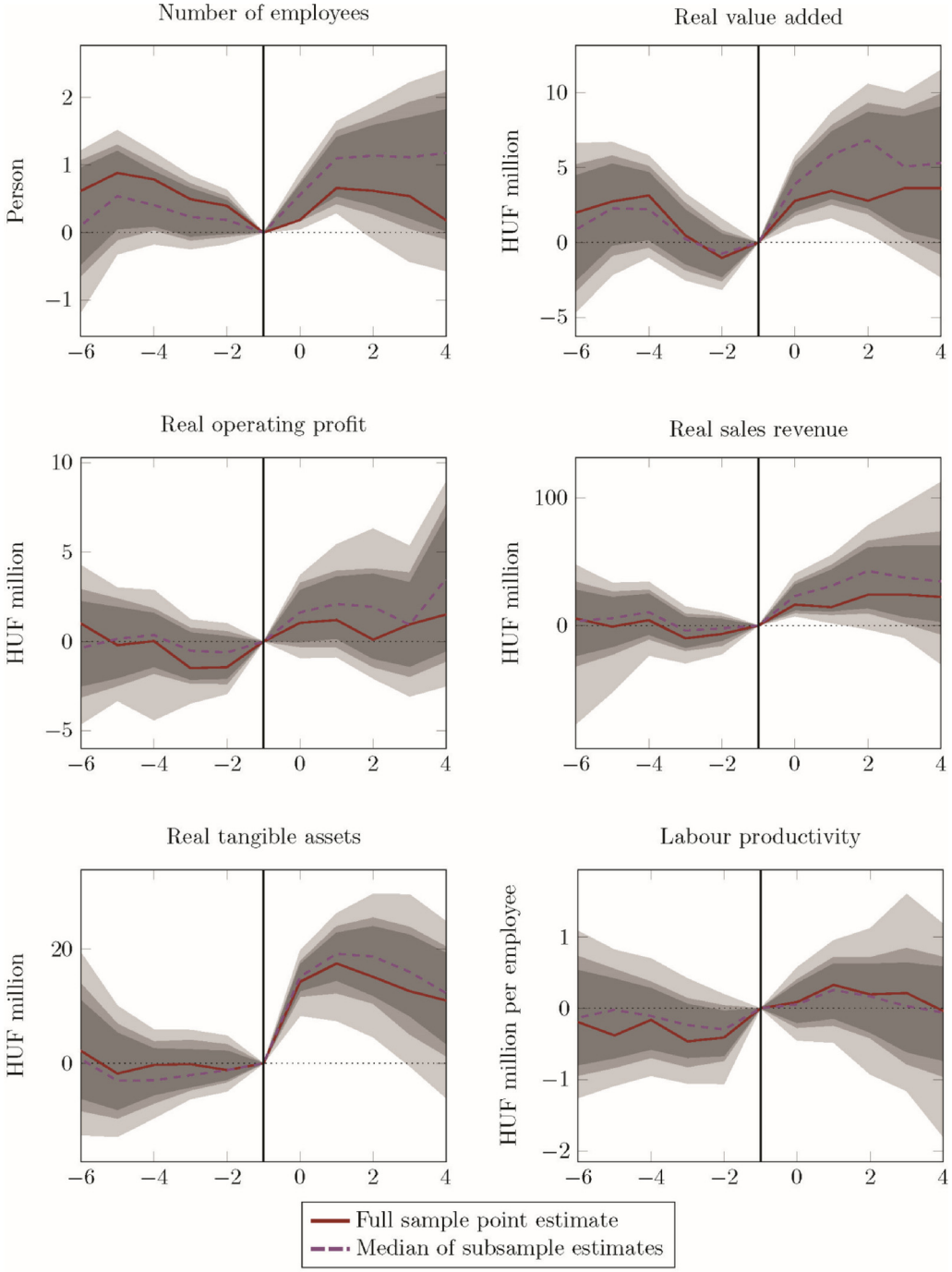

Fig. 2. Refundable subsidies.
Notes: Relative time in years on the horizontal axis. Shaded areas represent 90 %, 95 % and 99 % confidence intervals.

4.1.3. Relative effectiveness of refundable subsidies compared to non-refundable subsidies

From a policy perspective, it is especially important to know whether there is any difference in effectiveness between refundable and non-refundable subsidies. If there is no difference or if the difference is negligible, refundable funds can be considered as more beneficial in view of their reusability, having the same impact with lower expenses. However, in our main estimations we determined the average impact of the treatment for the treated entities (ATT). This is, in general, different from the average impact of the treatment for the entire corporate population (*average treatment effect*, ATE) because the characteristics of the entities supported in the given scheme may substantially differ from the entities not receiving any subsidy or the ones that obtained subsidy in other schemes. For this reason, it is not at all certain that, if the companies that received non-refundable subsidies had received refundable subsidies,

its effect would have been comparable with the estimated effect of refundable subsidies. So our results of various estimations with different treated populations and treatment definitions can only be compared with proper precautions.

Therefore, we made an attempt to analyse this issue by slightly modifying our main methodology. We set off from the population of those firms that received some kind of EDOP/RDOP subsidy, but did not win any grants from the EAFRD or any capital instruments during the programming period. We analysed the first subsidy of these firms. We estimated the propensity score model based on the periods prior to the subsidy, and the two possible outcomes represented the type of the awarded subsidy.[22] When generating the pairs we required an exact match in terms of sector, negative equity dummy, year of subsidy and the subsidy's size category.[23] Moreover, for a given refundable subsidy beneficiary we again looked for the nearest neighbour based on the propensity score from companies with similar characteristics that obtained non-refundable subsidies.

Based on our results (Fig. 3), there is no considerable difference between the effectiveness of refundable and non-refundable subsidies: our estimates are insignificant in most cases. This is an encouraging result looking ahead since in subsequent programming periods it is expected that an increasing proportion of funds will be allocated as refundable subsidies. However, these results are only valid for those firms that are able to implement their committed investments from refundable subsidies, too. Therefore, despite our estimates, refundable subsidies are presumably not perfect substitutes for non-refundable subsidies, because non-refundable subsidies enable the implementation of projects that are not profitable in the economic sense, but that are nevertheless socially desirable.

4.2. Robustness checks

To ascertain that our main results do not depend considerably on the applied filtering and restriction of the sample, we performed additional sets of estimations for our three main specifications: non-refundable subsidies, refundable subsidies and their relative effectiveness. We repeated the estimations so that (1) we did not exclude the firms that obtained large subsidies (top 1–2 percent) and (2) in the case of an observation considered as an outlier or erroneous, we only excluded that observation and not the entire history of the company. As evident from Figures C.1–C.3 in the Online Appendix, these estimates are similar to the original ones.

As a way to ascertain the necessity of the first step in our empirical strategy and to test the robustness of the 1-to-1 matching, we performed an additional set of estimations: (1) discarding matching and relative time dummies completely, (2) utilising matching but omitting relative time dummies, and (3) using 1-to-5 matching[24] (Figures C.1–C.3). The omission of the relative time dummies led to the pre-treatment parallel trend ceasing to hold in the case of the first two specifications (non-refundable and refundable subsidies). This indicates that controlling for the subsidy-time-dependent "effect" (for which matching is fundamental) is essential. Furthermore, the fact that the results obtained using 1-to-5 matching are virtually identical to our baseline results suggests that the estimates are robust to the choice of the matching method.

It can also be argued that, despite matching firms on pre-treatment observed characteristics, our controls might still be substantially different in terms of unobserved factors not constant in time (e.g. project ideas, growth intention) as they have been chosen from the set of all non-subsidised companies. To mitigate this issue of potential self-selection, we performed an estimation in which the potential controls were the set of non-subsidised firms that applied for a non-refundable subsidy sometime during the period.[25] As is visible in Figure C.1, the results obtained this way are also almost identical to our main estimates.

In addition, we also performed estimations in which we only considered those companies that received one subsidy during the cycle (as opposed to our main estimation, in which we measured the impact of the first subsidy of companies that could obtain arbitrarily many subsidies). This should not be considered as a typical robustness check since companies subsidised once or several times may be fundamentally different in terms of their observable and non-observable characteristics and also in terms of the awarded amount. Based on our results for the effectiveness of refundable and non-refundable subsidies, the impact is in general smaller than in our main specification. Although the received average subsidy amount is slightly lower in this population (Table C.1 in the Online Appendix), the difference in effectiveness most likely has other reasons, too. If, for example, the authority assessing the applications also takes into account the outcomes of the subsidies previously won by the company, this is exactly the result we expect. In case of the estimates of the relative effectiveness of refundable subsidies, the results are similar to the main estimates. As a further robustness check for the relative effectiveness, we estimated a model in which those firms were included that obtained only one type of subsidy during the programming period. The difference in effectiveness is still insignificant.

In our main estimations the history of a treated company (and the history of the control company matched to it) was dropped from the sample beginning with its second subsidy, as we wanted to estimate the effect of a single subsidy as opposed to the effect of the whole programme. However, this procedure introduces endogenous selection to the estimation: if, for example, those firms on which the first subsidy had a greater effect get further subsidies, our estimates will be biased downward as a result of restricting their histories. Hence, to give an upper estimate of the effect of the subsidies, we also performed our main estimations without dropping

[22] Again, we did not categorise combined instruments as either non-refundable or refundable subsidies.

[23] Based on the amount of subsidy, we generated the following left-open intervals: HUF 0–5 million, HUF 5–10 million, HUF 10–15 million, HUF 15–20 million, HUF 20–30 million, HUF 30–50 million, HUF 50–75 million, HUF 75–100 million. In the case of refundable subsidies, funding of over HUF 100 million was quite rare and therefore we capped the analysed subsidy amounts at that value.

[24] We matched the five closest control firms in terms of the estimated propensity score in the neighbourhood of the treated company defined by the caliper. If there were fewer than five control firms in this range, we matched each of them.

[25] We only have data on non-subsidised applicants in the case of non-refundable subsidies. However, we do not have information on the time of negative decisions. Therefore, we are not able to ensure that both matched firms were applying at the same time. This means that potential self-selection cannot be fully eliminated.

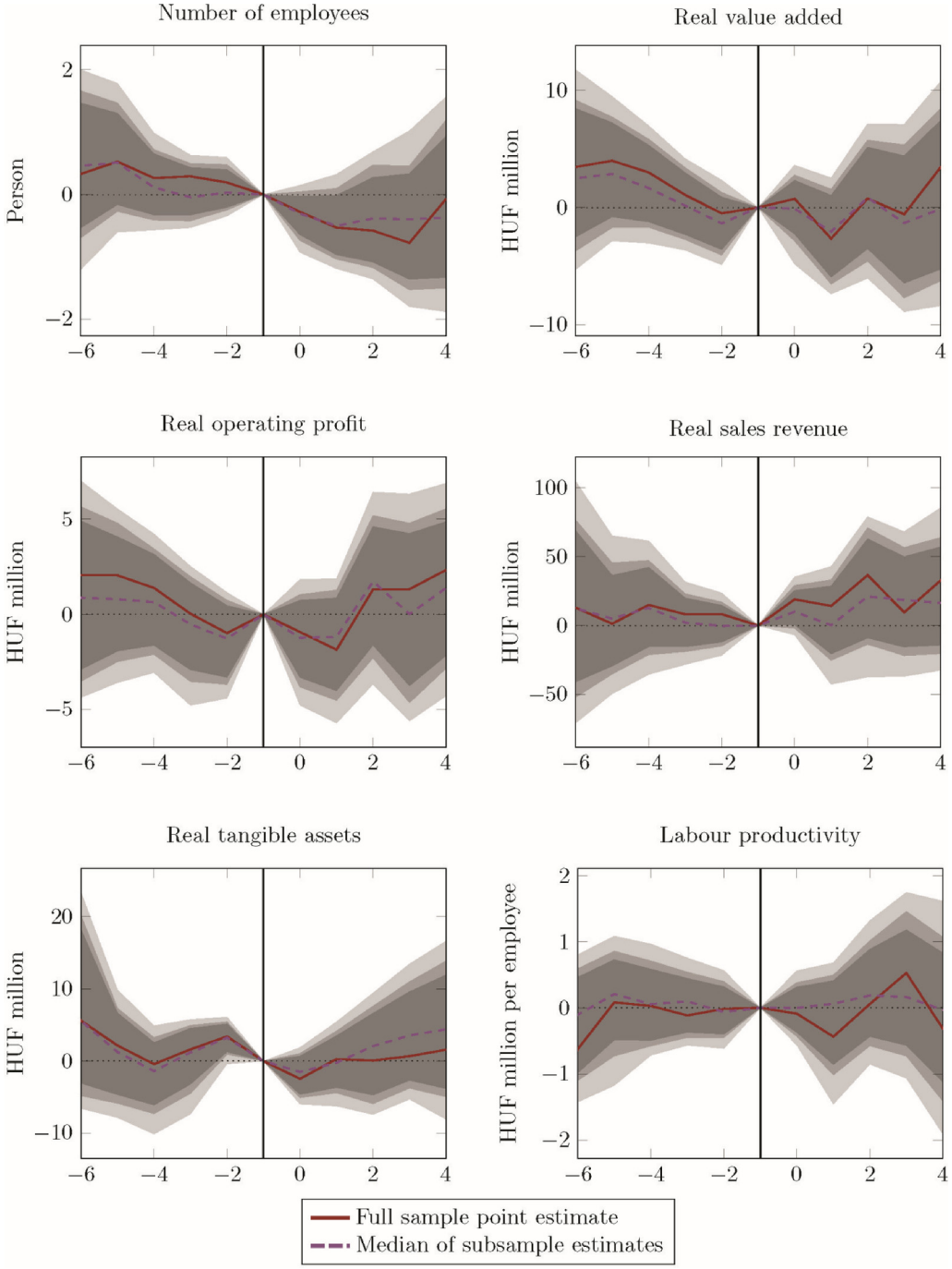

Fig. 3. Relative effectiveness of refundable subsidies compared to non-refundable subsidies.

the observations from the second treatment on. As displayed in Figures C.1 and C.2, from the first subsidy on there is a dynamic growth in the estimated effect after winning a refundable or non-refundable subsidy for every variable under review except for labour productivity. We consider this mainly as an effect of further subsidies. The impact on labour productivity is still not significant. When comparing the two types of subsidies using this specification, refundable subsidies have a significantly lower impact than non-refundable subsidies on the number of employees and real value added. However, these estimates also contain the effect of further subsidies and therefore we do not consider it as evidence that non-refundable subsidies are more effective.

4.3. Subgroup estimates

In this subsection, we present our estimates for the subgroups of the beneficiary population of non-refundable subsidies divided based on various characteristics. Although our primary objective is to provide a complete picture of the impacts and shed light on the differences among them by various subsidy and company categories, in the case of some groups the number of companies receiving subsidies is too low. For this reason, only those results where a sufficient number of observations is available for a reliable estimation are included.[26] These results are not directly comparable in the sense that we cannot draw conclusions regarding the relative effectiveness of the various subsidy types, or that of the subsidies granted to various groups. The reason for this is that the subsidies in the various categories may differ in terms of subsidy amount (Table D.1 in the Online Appendix), objective, other characteristics of the scheme, and also in the treated companies.

We performed separate estimations for the non-refundable subsidies according to size category (the mode of the SME classifications observed in the analysed time horizon). We estimated the impact for all three categories featured in our analysis (micro-enterprises, small enterprises and medium-sized companies) and obtained the expected results in all cases (Figure D.1 in the Online Appendix). Larger companies receive on average larger subsidies, and accordingly, the effect also increases with the size categories.

For the breakdown of the subsidies by development objective, we had sufficient observations available for a reliable estimation for the following categories: support of research, development and innovation activity; development of production plants, technology and capacity; development of corporate information and communication technology. The average subsidy amounts differ significantly: the winners of R&D subsidies receive on average three times as high an amount as the winners of subsidies aimed at the development of production plants, technology and capacity and more than seven times that of the beneficiaries of corporate ICT development subsidies. In light of this, it is rather surprising that the absolute effect of the former subsidies is not considerably higher than that of the other two (Figure D.2 in the Online Appendix). It is of course also possible that the return period for research and development is considerably longer than the interval we analysed. The efficiency of ICT subsidies is exceptionally high with respect to practically every analysed variable, only the impact on the number of employees diminishes quickly. In terms of the subsidies for the development of production plants, technology and capacity, the effect seems to be persistent for most of the variables. The stock of real tangible assets is an exception, which either suggests that the higher level may not be sustainable with market funding or that the supported entities only brought some investments forward and therefore their pairs in the control group catch up with them over time. None of the subsidy types have a significant effect on labour productivity.

The number of non-refundable subsidies also enabled us to estimate the impacts in a sector-based breakdown. Here (Figure D.3 in the Online Appendix), in terms of certain dependent variables, we measured higher than average effects for companies active in construction, in the manufacture of wood and paper products, plastic and metal products, and in trade and repair of motor vehicles. In the remaining two sectors for which we could perform an estimation (professional, scientific and technical activities and information and communication) the effects were somewhat lower. We did not perceive any change in labour productivity in any of the examined sectors.

5. Conclusion

The effectiveness of subsidies aiming at economic development is subject to debate within the literature. In our analysis, we attempted to answer this question through the combination of propensity score matching and fixed effects estimation in respect to the direct economic development subsidies allocated to Hungary under the 2007–2013 programming period, granted to SMEs. More specifically, from the subsidies available under the Economic Development Operational Programme and the Regional Development Operational Programmes considered to be directly aimed at economic development, we estimated the effect of the first subsidy for each firm with respect to the number of employees, real gross value added, real operating profit, real sales revenue, real tangible assets and labour productivity.

According to our results, these programmes had a significant positive effect on the number of employees, the sales revenue, the gross value added and in certain cases also the operating profit of the beneficiaries. In the case of non-refundable subsidies this effect is of substantial magnitude, for refundable subsidies it is more modest but still significant. However, labour productivity did not change significantly as a result of any of the subsidies. Therefore, our results are consistent with the hypothesis that firms used EU funds primarily to expand capacity and not to enhance efficiency. This is reconcilable with the fact that one of the main objectives of the funding programmes was to expand employment. Furthermore, because typically the more productive firms receive support, the faster growth rate of these firms (compared with the less productive ones) may still result in an increase in aggregate productivity.

A finding that is particularly relevant from a decision-making perspective is that there is no significant difference between the effectiveness of projects funded by refundable and non-refundable subsidies. This is an encouraging result looking ahead, since in subsequent programming periods it is expected that an increasing proportion of funds will be allocated as various financial instruments.

We also performed estimations for subgroups of the population defined by various characteristics. By size category, we obtained the expected result: larger companies receive larger subsidies and accordingly the effect is stronger. When breaking down subsidies by development objective, the efficiency of ICT subsidies stands out with respect to practically every analysed variable. Subsidies for the development of production plants, technology and capacity also perform well and their impact proved to be persistent. Finally, in

[26] In practice, this means that we present groups where at least 300 pairs are included in the fixed effects estimation.

the sector-based breakdown, in terms of certain dependent variables, we measured higher than average effects for companies active in construction, in the manufacture of wood and paper products, plastic and metal products, and in trade and repair of motor vehicles. In terms of labour productivity, none of the subsidies have any significant effect in any of the subsample-based estimations.

Analysing the macroeconomic effects (e.g. unemployment, GDP) of EU subsidies could be the subject of further research. We believe that our results may be useful for calibrating the shocks symbolizing the various programmes in macroeconomic models used for such purposes. From a policy standpoint, it may also be relevant to analyse the relationship between the amount of subsidy and the achieved effect, and to get an idea about its functional form. This latter may provide a reference point to decide on the size of the portions in which the available funds should be allocated to achieve the optimal effect. It would also be interesting to investigate the effect of subsidies from a firm entry and exit perspective, in particular whether subsidised firms are more likely to stay in the market. Finally, it would be useful to model the selection mechanism in more detail and to understand on what basis companies choose from among the various available funding sources.

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Declaration of Competing Interest

None.

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Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.ecosys.2020.100742.

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