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(Gazdaságfejlesztési célú európai uniós támogatások hatásvizsgálata a magyar kkv-szektorra)

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Contents

Abstract

Although EU funds play a pivotal role not only for Hungary but for the entire European Union as well, there is debate regarding their effectiveness in the literature. This paper investigates the impact of direct economic development subsidies extended in the context of the Cohesion Policy programmes 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 assess the effects of the beneficiaries' 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 impact on the number of employees, sales revenue, gross value added and in some cases, operating profit. However, the labour productivity of beneficiaries was not significantly affected by any of the support schemes. Furthermore, by explicitly comparing non-refundable subsidies (grants) and refundable assistance (financial instruments) extended under the Structural Funds and the Cohesion Fund, we find that there is no significant difference in their effectiveness.

JEL: D04, G38, H25, O22.

Keywords: programme evaluation, EU subsidies, firm-level effects, propensity score matching, fixed effects.

Összefoglaló

Habár a gazdaságfejlesztési célú európai uniós támogatások nemcsak Magyarország, de az Európai Unió számára is kiemelten fontosak, hatásosságuk a szakirodalomban vita tárgyát képezi. Tanulmányunkban a Strukturális Alapok és a Kohéziós Alap 2007–2013-as európai uniós költségvetési ciklushoz tartozó, közvetlen gazdaságfejlesztési célú támogatásainak hatását vizsgáljuk a magyar mikro-, kis- és középvállalati szektorra. Mikroadatbázisra épülő hatásvizsgálatunkban propensity score alapú párosítás és fixhatásos panelregresszió kombinációját alkalmazva kíséreltük meg a vállalatok első támogatásának hatását értékelni különböző vállalati teljesítménymutatókra. Eredményeink szerint a gazdaságfejlesztési célú források szignifikáns pozitív hatást gyakoroltak a foglalkoztatotti létszámra, az árbevételre, a bruttó hozzáadott értékre, és egyes esetekben az üzemi eredményre is. A vállalkozások munkatermelékenységét azonban egyik vizsgált támogatási program sem befolyásolta szignifikánsan. A Strukturális Alapok és a Kohéziós Alap vissza nem térítendő támogatásait és pénzügyi eszközeit expliciten összevetve emellett arra jutottunk, hogy a támogatástípusok hatékonysága között nincs jelentős különbség.

1 Introduction

Between 2007 and 2015, nearly HUF 1800 billion in direct economic development subsidies was distributed among 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 half per cent of GDP per year. Hence, in the absence of a developed capital market, EU funding represents one of the key funding instruments of the Hungarian SME sector – besides bank loans and guarantees. 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. Economic development funds play a pivotal role not only for Hungary, but for the European Union as well. A substantial part of the Community's budget is allocated to these purposes, as fostering the convergence of less developed regions is a central objective within the European Union. It is therefore important to evaluate how these funds are utilised.

Based on the literature, it is not at all clear whether economic development schemes indeed have a growth-stimulating effect. Among the studies investigating the impact of subsidies, Burnside and Dollar (2000) is regarded as a seminal paper, which evaluates the impact of economic development schemes on growth using a fairly large international (non-EU) panel. They found that subsidies have a clearly positive effect in countries with developed and well-functioning institutions, while there is no significant impact in countries with a weaker institutional system. These findings were later nuanced by Easterly et al. (2003). Based on estimates performed on a similar but expanded and adjusted sample, the authors found that a positive impact cannot be clearly verified. Bourguignon and Sundberg (2007) also emphasised this uncertainty, which confirms that no general claims can be made regarding the impact of economic development schemes on growth.

The set of EU programmes geared towards fostering the convergence of less developed regions is undoubtedly one of the most significant economic development subsidies, even by international standards. With the accession of Central and Eastern European countries in the 2000s, the scope of these schemes has further increased. Despite their significance in terms of economic history, the literature providing an ex post evaluation of the actual impact of the subsidies is quite limited. In this strand of literature, two typical approaches can be identified: the micro-level approach explicitly assessing the impact of the subsidies on the performance indicators of beneficiaries; and the macro-level analysis which treats the subsidy programme as an exogenous shock to the beneficiary sector(s), and measures the macroeconomic impact as its direct and spillover effect on the whole economy.

Before the 2000s, several studies employed the second 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 published numerous studies (particularly in the 2000s) 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 their impact on GDP. Regardless, the findings broadly confirm the positive impact of subsidies (e.g. Varga and in 't Veld, 2011; Monfort et al., 2016). Although using the above models allows a more accurate quantification of spillover effects, which may account for a significant part of the total economic impact, these types of models require numerous assumptions (related for instance to the corporate-level utilisation of funds) which lends uncertainty to the results.

Micro-level impact evaluations, explicitly examining the utilisation of funds at the corporate level, represent a different approach. Mouqué (2012) summarises these types of studies assessing European Union funds. The results vary by scheme and country, but the key findings can be summed up as follows. The subsidies had a fundamentally positive impact on output and employment (although the impact on the latter is smaller than indicated by monitoring data), 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 have proved surprisingly successful.

The impact evaluation of Hungarian programmes has primarily taken place in studies commissioned by the managing authorities, thus there are few publicly available analyses. One of the most important among them is the study by the HÉTFA Research Institute (Balás et al., 2015), which uses a macro-level approach: it estimates the impact of subsidies distributed during the 2007–2013 programming period employing a multi-sectoral, computable general equilibrium (CGE) framework. According to the findings, although developments provided a short-run, direct impulse to the Hungarian economy through investment demand and stimulated the construction industry to the greatest extent, they did not provide any long-term increase in capacity, or improvement in efficiency. At the end of the period, the level of GDP was nearly 2 per cent higher than it would have been without subsidies. Half of this effect was a one-off demand effect. Like many other studies, the authors stressed that a significant portion of subsidies were not geared towards directly stimulating growth nor is any short or medium-term impact expected of them, so the objectives are important to be taken into account when interpreting the results. A study published by the Budapest Institute (2013) used a different, micro-level approach, examining the impact of EU funds on enterprises similarly to the methodological framework presented in this paper. The analysis looked at the same programming period, however, data were only available up to 2011 at the time of publication. According to their findings, the subsidies had a positive impact on employment, but no significant growth was found in terms of the sales revenue.

As can be seen, giving an accurate estimate of the impact of European Union subsidies is not straightforward. Moreover, a significant portion of funding is not aimed at having a short-term positive economic effect, which makes the evaluation even more difficult. 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 term, 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 they attempt to measure an impact that was not an objective in the programmes. 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, we have not undertaken a general study of the impact of EU funds. This paper is limited to a narrow set of measures specifically targeting economic development and which are expected to have a positive impact on economic indicators.

Our method, which can be classified as a micro-level approach, can be broken down into two steps. As the first step, we estimate the probability of receiving a subsidy for every firm-year observation, and we match a non-subsidised (control) firm to each beneficiary enterprise based on this probability (propensity score matching). In the propensity score model, we mainly use explanatory variables describing the companies' performance before the subsidy, as well as 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 the difference-in-differences method, or more specifically, a fixed effects panel regression analogous to that in our case. Here, the reference time is the year before the first payment of the subsidy. Results obtained this way are interpreted as the causal impact of the subsidy. In terms of methodology, the study by the Budapest Institute (2013) is the most similar to the current paper among the Hungarian impact evaluations. However, our paper goes beyond this analysis in several aspects. We analysed the impact of EU subsidies on a longer sample, using time series that last until the end of 2015. Thus, we were able to examine the complete programming period. We lay special emphasis on the analysis of homogenous groups created based on various factors (such as size, sector, development objective). This may serve as useful input for models examining the macro-level impact of EU subsidies, as it provides an accurate overview about the utilisation of grants. Finally, we combine matching with fixed effects regression instead of treating them as separate results, which enhances the validity of our results.

Our findings are particularly relevant from a policy-making perspective. Our results confirm that the funds under review had a significant positive impact on the beneficiary corporations in terms of sales revenue, gross value added and number of employees. These impacts also proved to be persistent in the medium-term. However, the funds did not improve corporate efficiency (measured by labour productivity), although improving it would be essential from the perspective of long-term convergence, as this is one of the main underlying reasons of the income gap compared to more developed economies (Varga and in 't Veld, 2011). These findings apply to both refundable subsidies and non-refundable grants, moreover, we did not find any significant deviation between the effects of these two categories. This suggests that because refundable funds enable multiple utilisation, this form might be preferred in the case of objectives where both of them are applicable. Our analysis also sheds light on the significant heterogeneity among various objectives and sectors. The positive impacts were mainly perceptible in four sectors: trade and repair of motor vehicles, construction, the information and communication sector and the manufacture of plastic and metal products.

This study is structured as follows. Section 2 presents the evaluated measures 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 the scope of evaluation

2.1 SCOPE OF EVALUATION

The 2007–2013 programming period was the first entire period for Hungary since it joined the EU in 2004. This analysis attempts to evaluate the impact of subsidies *directly aiming economic development* of the Economic Development Operational Programme (EDOP) and the Regional Development Operational Programmes (RDOP) from the Structural Funds and the Cohesion Fund on the beneficiary corporations out of the total HUF 11 067 billion distributed in this programming period.[1] We have chosen to evaluate only direct economic development measures and corporate beneficiaries at the same time, because we assumed that we could only identify and verify the effects if final recipients of the development scheme are corporate beneficiaries, moreover the scheme itself explicitly aims the improvement of the performance of these beneficiaries.

Although the impact evaluation of operational programmes on various corporate performance indicators is the core issue of this paper, it is worth briefly reviewing the general and specific objectives of operational programmes to assess their fulfillment, moreover, to identify differences in the impacts of separate objectives. The comprehensive development plan for this period has 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 business environment and broadening the basis of developing economy. Although the objectives of the measures were often complex according to the strategic objectives of operational programmes, we identified the main goal (development objective) of each measure. The ones classified as having a measurable impact are shown in Table 1.

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

Development objective	Number of projects	Amount granted (HUF billion)
Employment enhancement	746	111.83
Support of research, development and innovation activity	3 810	296.10
Support of physical infrastructure for research, development and innovation activity	80	44.72
Environmental investment	124	8.24
Development of production plants, technology and capacity	21 491	432.29
Development of tourism	672	166.46
Development of corporate information and communication technology	2 948	18.83
Corporate consultancy	357	5.37
Total	30 228	1 083.87

Note: The table contains direct subsidies extended to micro, small and medium-sized enterprises, with the purpose of economic development.

While the above groups only include grants, *financial instruments* constitute a separate category (which cannot be broken down by development objective). Financial instruments include (1) micro financing, which replaces the function of small-amount

[1] Among these subsidy schemes amounting to HUF 1 789 billion during the programming period, we filtered out subsidies that do not have an economic development objective or if we deemed their impact not measurable. The latter refers to situations where the beneficiary did not receive the subsidy directly and therefore the end-beneficiary could not be observed, when the objective of the subsidy was to develop the environment of enterprises rather than the observed enterprises themselves, or when technological development had the objective of environmental protection. We classified 96.7 per cent of the non-refundable subsidies (HUF 1 250 billion) and 100 per cent of the refundable subsidies (HUF 379 billion) as having measurable impact.

loans that cannot be granted due to elevated costs or other borrowing constraints (these are essentially soft loans); (2) guarantees designed to decrease banks' lending risks; and (3) equity or venture capital instruments (Balás et al., 2015).[2] 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.

Where the size of our estimation database allows for reliable estimates, we also applied the methodology presented in the study to the individual development objectives. Our results and conclusions are presented in depth in section 4.

2.2 SUBSIDY DATA

Our data mainly come from two sources. On the one hand, we calculated the companies' 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).[3] 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.[4] The EMIR is a project-level database that shows the dates of payments (instead of the decision-making or contract-signing dates) with an annual frequency. From a methodological perspective, it is also important to note that although we have some information about submitted applications that were not granted any funding (non-winning applications), the date of the negative decision is not known.

Table 2 displays certain characteristics of the payment distribution for the set of the micro, small and medium-sized enterprises (SMEs) under review. Both subsidy categories exhibit distributions with heavy tails towards higher subsidy amounts due to a few exceptionally large subsidies. Because we are of the view that 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 the case of these subsidies, for the rest of this analysis, we will ignore about the top 1–2 per cent of subsidies (based on size).

Table 2
Descriptive statistics of subsidy amounts
(HUF million)

Indicator	Subsidy category	
	Non-refundable subsidies	Refundable subsidies
Mean	38.98	21.77
Standard deviation	203.83	73.56
1st percentile	1.18	0.80
5th percentile	2.25	1.53
10th percentile	2.70	3.07
25th percentile	4.68	4.88
Median	10.00	8.10
75th percentile	24.97	11.26
90th percentile	81.35	49.73
95th percentile	149.98	50.00
99th percentile	490.92	383.95

Note: The table contains subsidies extended to micro, small and medium-sized enterprises.

[2] 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.

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

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

Table 3 provides an overview of the time elapsed between the first and last payment of subsidies. It is visible that non-refundable subsidies are disbursed in an extended time period, contrary to refundable subsidies which are always fully paid out in the first year (and have thus not been included in the table). This stems from the nature of non-refundable subsidies, which are paid out upon presentation of a statement of fulfilment in the form of ex-post financing. However, as only a single treatment date can be handled by our methodology, we must unequivocally decide which of the payment dates is to be regarded as the treatment date in this impact evaluation. We chose the date of the first payment.[5] 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. The resulting biases are naturally the greater the longer the payment stretches over time. Based on the table, we can conjecture that the resulting bias is not that significant.

Table 3
Distribution of non-refundable subsidies according to the duration of their payment

Time passed between the first and last payment	Number of projects	Proportion of projects (per cent)
0 years	21 160	67.83
1 year	5 832	18.69
2 years	2 874	9.21
3 years	1 110	3.56
4 years	174	0.56
5 years	31	0.10
6 years	8	0.03
7 years	6	0.02
8 years	1	0.00

Note: The table contains subsidies extended to micro, small and medium-sized enterprises.

The distribution of the first payments of subsidies over time is shown in Table A1 of the Appendix. It can be seen that the payment of subsidies picked up speed during the second half of the cycle. (The table contains the payments of the subsidies, which might have taken place even after the budget cycle ended in 2013.)

Table 4 shows the distribution of subsidised enterprises by the number of funded projects. It is apparent that winning multiple subsidies is relatively frequent. However, extending multiple subsidies to a company creates an issue for the examination of the impact of subsidies. We handled this problem in the following manner. For one, as our other main data source, the NTCA database containing enterprises' financial indicators is available at the corporate level, we aggregated our project-level subsidy database to the corporate level in order to be able to link the data: we handled subsidies of the same type (refundable or non-refundable) and in the same year as a single subsidy. Moreover, although subsidies allocated in different years can theoretically be handled in a difference-in-differences framework using the lead and lagged values of the treatment indicator, it would pose problems for the matching procedure as earlier subsidies influence the values of our control variables. As a result, in our main analysis we only examined the impact of the first subsidy granted to companies. In our population featuring more elements (non-refundable subsidies) we also looked at the impact of the second subsidy.

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 the entities that were

[5] Using the last payment date is also an option, following 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 beneficiaries according to the number of funded projects

Number of funded projects	Subsidy category	
	Non-refundable subsidies	Refundable subsidies
1	14 259	11 832
2	3 014	1 366
3	1 204	218
4	624	74
5	330	19
6	187	10
7	116	6
8	57	1
9	35	5
10	15	0
11	11	1
12	9	2
13	3	0
14	2	0
15	0	1
16	0	1
20	0	1
25	0	1
Total	19 866	13 538

Note: The table contains micro, small and medium-sized subsidised enterprises.

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.[6,7] We classified businesses as micro, small and medium-sized enterprises if they were most often (mode value) categorised as such during the 2003-2015 period based on the threshold values for common European Union categories to avoid having to split up corporate histories due to changes in their size category over time when filtering companies by size. 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, so the database grew significantly.

We chose the dependent variables from the NTCA database: number of employees, real gross value added[8], real operating profit, real sales revenue, real tangible assets and labour productivity[9].[10] We also used real pre-tax profit[11], leverage (the ratio of liabilities to the balance sheet total), the ratio of export to sales revenue, foreign majority ownership, the current SME

[6] This analysis does not look at the impact of subsidies on large corporations. In terms of the presumable extent of impacts, see the Mouqué (2012) comprehensive study, which uses multiple impact evaluation analyses and finds that impacts on large corporations are not only smaller than in case of SMEs, but sometimes even insignificant. This may result from the fact that while the subsidies contribute to resolving or alleviating actual financial constraints for SMEs, large corporations typically do not face such constraints.

[7] The subsidies extended to the group of enterprises thus defined account for 90.95 per cent of non-refundable subsidies (HUF 1216 billion) and 83.11 per cent of refundable subsidies (HUF 345 billion).

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

[9] Real sales revenue per employee.

[10] We measured every financial indicator expressed as a value in real terms, at 2015 prices.

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

classification, the region of the head office and our own sectoral categorisation[12] as control variables in the propensity score model. Furthermore, to allow us to perform the matching not only based on *levels* but also based on *dynamics* before treatment, we also used the growth rate of some variables in the past three years, or more specifically, a version of it featuring better characteristics.[13]

Table A2 contains the descriptive statistics of our continuous corporate indicators while Table A3 contains the distribution of our categorical variables. 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.

[12] It differs from the NACE economic sectors in that in order to create categories of similar size, we broke down manufacturing into the manufacture of food products and beverages; the manufacture of textiles and wearing apparel; the manufacture of wood and paper products, furniture and printing; the manufacture of chemical and pharmaceutical products; the manufacture of plastic and metal products; the manufacture of electronic products; the manufacture of 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.

[13] 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 interpretable in this scenario as well 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.

3 Theoretical framework of the impact evaluation

Based on our theoretical considerations, the subsidies extended to enterprises may affect the subsidised entities in two ways: (1) they may provide additional funds for firms with restricted access to external funding,[14] and (2) they may serve as a cheaper source of funding than the currently available one for those companies not facing credit constraints. Because these subsidies can only be used to finance new investments, we expect the expansion of the stock of fixed assets in both cases, but in the case of firms with restricted access to funding, potentially to a greater extent than for the others. 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[15]), in the second case the stock of fixed assets will return to the previous level in the absence of preferential funding over time. It is important to note here that the latter case may also have a social benefit, provided that the subsidy facilitates the implementation of an investment which – although if the company was to assume all the costs of the investment, it would never recover those costs – is beneficial on the social level owing to positive externalities.

This additional investment will most likely lead to an increase in the production and the value added generated by the enterprise. The impact on the number of employees is not clear based on the above reasoning – the beneficiaries could, in principle, spend the subsidy to expand the capacities 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 declared to be employment expansion (and accordingly, commitments often included the hiring of new employees), 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 the its potential value at time t by $Y_{it}(1)$ if it 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 firm is among the supported entities 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):

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

if the time of the subsidy is t_0. 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. If such an effect nevertheless exists, our estimates may be biased in any direction: it is possible, for example, that in case the competitors of company *i* receive subsidies, it will negatively impact *i*, but if the partners of that same enterprise receive subsidies, it may affect it positively. Although spillover effects certainly exist, it is still required that we maintain the

[14] Financial frictions are not the only possible cause of credit constraints. When the project does not provide appropriate collateral – for example in the case of research, development and innovation investments – banks may not be willing to lend.

[15] This might occur if a firm which has not yet been granted bank funding raises a bank loan to pre-finance its subsidy. As the ex-post payment received in the case of a successful completion of the project functions essentially as collateral, the bank may be more willing to lend to this company. In turn, during the repayment of the loan the bank may acquire behavioural information about the company which may be of help when considering its further loan requests.

SUTVA assumption as we lack data about the network of relations among enterprises. However, the assumption we apply is not quite unrealistic because, on the one hand, a relatively small proportion of companies receive the kind of subsidy we are analysing (1 to 5 per cent depending on the programme) and, on the other hand, the subsidies are spread in time over the cycle.

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

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

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 which indicates whether the company receives a subsidy in the given year. It is important to note that this assumption is a lot more permissive than the usual conditional unconfoundedness assumed for cross-sectional data. The main difference is that the conditions of independence include also the past values of the dependent variable, and thus also the unobserved factors which are constant over time for each firm pertaining to the given variable.

Our last assumption, common support, is necessary to find a similar, non-subsidised company for each of the subsidised companies in terms of the variables that we condition on, that is, to avoid the need for extrapolation. In other words, in the case of a set of observations defined by the historical values of a given company's variables, the observations should not be unambiguously classifiable into the treated and non-treated categories. In the case of ATT, this is the following:

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

In our case this assumption arguably holds, given the large number of non-subsidised enterprises.

Of the above three assumptions, conditional unconfoundedness requires the most explanation. To shine more light on this issue, let us assume that the variable Y of a company is defined as follows:

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

where δ_t, η_i and ε_{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

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

This statement practically means that the current and future non-observable shocks do not influence the probability that the company receives subsidy. There are two types of selection that threaten the above assumption: self-selection by the enterprise 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) non-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 managing authority is also limited regarding the applicants. Moreover, in the case of smaller subsidies that we typically analyse, a number of the calls for proposals were such that the mere fact of meeting the conditions practically 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 in terms of the analysed variables. If this information also correlates with the dependent variable, our estimates may be biased. For example, if the enterprises that apply for subsidies are the ones that currently have good investment ideas, then the estimated impact will be biased upward. No fully reassuring answer can be given to this problem, but breaking it further down may facilitate its assessment.

It is important to emphasise that the subsidies could not only be used for innovative developments: approximately two-thirds of the allocated grants had capacity expansion as purpose. That a company (which, in its observed characteristics, behaves in a similar fashion than the subsidised companies) is unable to come up with an investment idea that would enable it to produce a larger amount with its current technology, is not too plausible. A more important problem occurs when the company is unable

to come up with the own contribution and pre-financing necessary for the subsidy as it faces credit constraints. Considering the fact that if the project is successfully implemented, the awarded sum represents tangible collateral for the bank, we assume that this is not a mass phenomenon either. And finally, a company does not submit an application if this was not beneficial for it, because the present value of the implemented project did not reach the present value of the costs entailed by the tendering process (administrative costs, own contribution and the cost of pre-financing), that is

$$PV_{\text{project}} \leq PV_{\text{admin}} + PV_{\text{fin}}.$$

The cost of funding (PV_{fin}) should not be too high according to an argument similar to the one for credit constraints. The administrative cost (PV_{admin}), which includes, for example, the uncertainty of fulfilling the commitments, the additional work and expenses entailed by the application process, and the expertise and motivation of the management, may obviously be different for each firm, but less so within one firm across time. 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 the administrative costs those applying for the first time. However, this can be eliminated for the most part by only analysing the first subsidies of each firm, 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*.[17] 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 years' growth rate (as defined in subsection 2.3) of these variables supplemented with the enterprise's current SME classification and the region of its head office. Besides, we require an exact match in terms of the sector.

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 difference remains in these variables between the two groups even after the matching. Therefore, we then apply the DiD method to filter out its time-constant part. Using panel data, DiD is the aggregate version of a panel regression with time and individual fixed effects in which the only further explanatory variable is an indicator which is one for the treated at the time of the treatment and zero otherwise (Angrist and Pischke, 2009). Therefore, we shall hereinafter regard this fixed effects panel regression as the starting point of our impact evaluation equation. The long time-dimension of the database is beneficial for several reasons. On the one hand, it allows us to obtain some indication about the correctness of the implicit assumption made in the DiD method whether the treated group changes in parallel with the control group in the lack of the treatment (the so-called parallel trend assumption). More specifically, we may check whether the parallel trend existed until the treatment. On the other hand, we may also identify the possible long-term effects of the treatment.

In view of the above, our impact evaluation procedure is comprised of the following steps. First, by applying a probit model, for every firm-year pair in our database, we estimate the probability of a company receiving subsidy during a given period, provided that it had not received any subsidy until then, based on various company characteristics prior to that date. These are the following in our model: 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 typical sector, and within sectors we match the non-treated firm with the propensity score closest to that of the treated company at the time of treatment (*nearest neighbour matching*).[18] We match the history of the control company to the history of the treated company for the years in which both are available.[19]

[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 enterprises were granted.

[17] Although the use of the regression discontinuity design may seem to be an obvious option by exploiting the application conditions, we do not apply this method for two reasons. On the one hand, conditions differ for each tender, and on the other hand, based on the information available to us, the assessing agency has requested the values of the characteristics featured in the conditions on a self-admission basis.

[18] According to Sekhon (2011), we perform the matching not based on the estimated participation probability, but based 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 effect, time fixed effect 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 constantly 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 the treated enterprises performed increasingly better than the others in most of the dependent variables during the periods prior to the treatment. This may be caused in our view 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 as 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" appearing in the relative periods. 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 enterprises that receive the subsidy in one given year (e.g. in 2010) and their pairs (and thus in this case the actual and the relative times coincide), the results resemble the results of the model estimated on the entire period by including the relative time dummies.

Despite the fact that the 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 the matching without taking into account the effect of the matching are invalid for two reasons: on the one hand, they disregard the number of parameters estimated in the first step, and on the other hand, 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 the sample size of $b = n/2$ (determined on a somewhat ad-hoc basis). As the last step, we adjust for the fact that in case of 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 similar 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 calculated 1000 subsample estimates and determined the confidence intervals based on these, using the above formula.

[19] As a result of this approach, we ignore a potential aspect of the impact of the subsidies: reducing companies' probability of bankruptcy. In addition, this is also a potential source of bias if the subsidies have a different impact on those treated companies which belong to a pair with an early exit.

[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 use entire company histories for the sampling. The sampling is done from the population prior to the matching.

4 Results

In this section we present our detailed results regarding the various populations, using the impact evaluation methodology presented in the previous section.

4.1 MAIN RESULTS

As we have described in Section 2, we performed estimations regarding the impact of subsidies directly aiming economic development under the EDOP and RDOPs. 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 financial instruments 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 which could potentially be used in the estimation (this is the first subsidy per beneficiary). 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 size of the panel database obtained after the matching, that is, the number of company-year observations used in the fixed effects estimation. The last column presents the average subsidy amount of the supported companies used in the given estimation.

Table 5
Summary statistics of the main estimates

	Number of treated companies	Number of potential control companies	Number of matched pairs	Sample size in the fixed effects model	Average subsidy size (HUF million)
Non-refundable subsidies	9 636	192 571	9 431	175 176	18.02
Refundable subsidies	2 587	192 571	2 585	44 654	13.42
Non-refundable subsidies – second treatment	2 938	4 472	1 822	36 786	17.96
Refundable subsidies compared to non-refundable subsidies	1 382	6 879	714	14 556	11.64/12.68 [a)]

[a)] *refundable subsidies/non-refundable subsidies*

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.[22] The size of the final sample used for the estimation proved to be sufficient for every model so that we could 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 by comparing the average subsidy amount and the estimated effect of the subsidy, 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 which received non-refundable subsidies from EDOP and/or RDOPs at some point in time during the period under review as treated. From these, we excluded those firms that also received a rural development subsidy at the same time or prior to the aforementioned subsidy. Moreover,

[22] The propensity score estimations for our two main specifications are shown in Appendix B.1.

because we only estimated the effect of the first subsidy, we excluded the company's history from the time of the second non-refundable subsidy. Our control group only consisted of firms which did not receive any kind of subsidy (including rural development subsidies of the EAFRD). Although we only considered the non-refundable subsidies as treatment, we did not exclude those firms from the treated group which were also granted financial instruments. The underlying assumption is that those enterprises that received both types of subsidies used the financial instrument as a product linked to the non-refundable subsidy (because in several schemes the subsidy included both a refundable and non-refundable part), therefore in their case we do not wish to separately evaluate the effect of the refundable part.

Figure 1 presents our results. The vertical line denotes the period *prior to* the time of the 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 the 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 – by the years elapsed since 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 somewhat 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 the 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 40–50 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.[23] 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. This suggests that without the subsidies, subsidised projects would predominantly not have taken place, or only after a certain period of time. In addition, 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 for 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, it is of importance to bear in mind that the decline of the stock of tangible assets 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. We can also note that the effect of the treatment is already apparent in the year prior to the treatment. This can be explained by the fact that because non-refundable subsidies are typically financed in an ex-post manner, the beneficiaries already start the investment prior to the first payment.

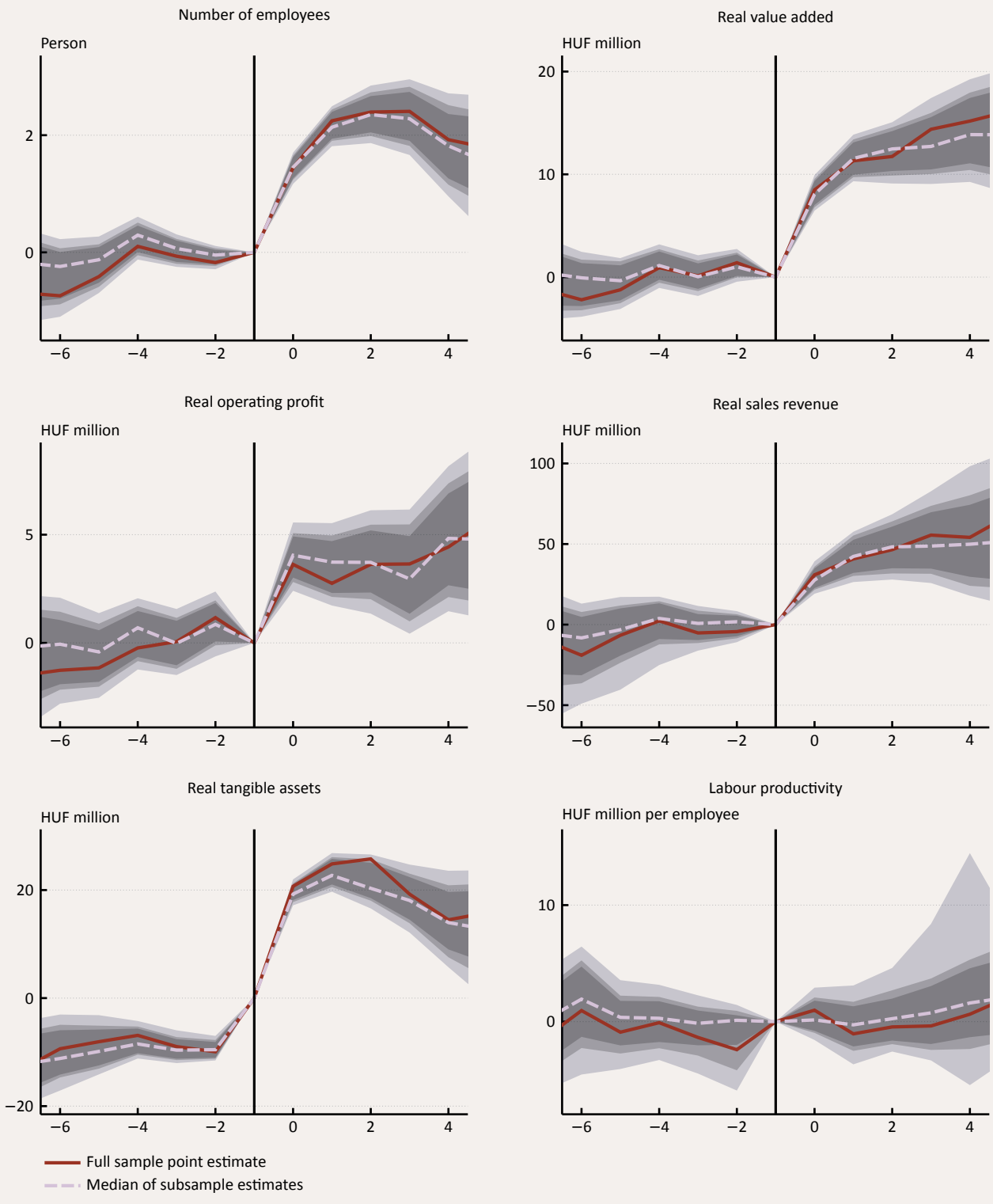
And finally, on the sixth subfigure we can see the subsidy's impact on labour productivity, that is, on per capita real sales revenue. According to our estimates, no significant impact is detectable. This suggests that, measured by labour productivity, the investments implemented from non-refundable EU subsidies can be regarded as expansion investments rather than efficiency-improving investments. This is reconcilable with the fact that one of the main objectives of the programmes was to expand employment. Furthermore, as typically the more productive firms receive support, the faster growth rate of such companies (compared to the less productive firms) may still result in the improvement of aggregate productivity.

4.1.2 REFUNDABLE SUBSIDIES

Considering that subsidies directly aiming economic development also include refundable ones, it is worthwhile to analyse the effect of refundable subsidies separately. Because certain calls for proposals exclusively contained the placement of financial

[23] It is important to note, however, that the change of the stock resulting from the treatment could naturally include the impact of other investments related to the subsidy as well.

Figure 1
Non-refundable subsidies



Note: relative time in years on the horizontal axis. Shaded areas represent 90%, 95% and 99% confidence intervals.

instruments, their effect can be analysed not only as a product linked to non-refundable subsidies, but also in itself. For this we considered as treated population the population of firms that were awarded a financial instrument as their first subsidy, considering only this first subsidy as treatment, while our control group included those enterprises which did not receive any subsidy from any EU funding programme for which we have data at our disposal.

When interpreting our results (Figure 2) it is important to bear in mind that our estimation database is a lot smaller here than in case of non-refundable subsidies, therefore the uncertainty of our estimates is also greater. Nevertheless, the treatment has a positive, and for 3 to 4 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 only from the period prior to the payment and not one year earlier. This coincides with our knowledge that similarly to loans, companies received their refundable subsidies through pre-financing.

4.2 OTHER RESULTS

In this subsection we present additional estimation results to provide a more complete picture of the effect of funding programmes. On the one hand, we demonstrate that within the group of non-refundable subsidies, the effect of the second subsidy does not necessarily correspond to the effect of the first subsidy, and on the other hand, we explicitly compare the effect of refundable subsidies to the effect of non-refundable subsidies. The results presented as robustness checks also illustrate that our most important result (the estimated effect of non-refundable subsidies) is not sensitive to our selected data cleaning and filtering techniques.

4.2.1 EFFECT OF THE SECOND SUBSIDY

In the previous part, for methodological reasons we only dealt with the first subsidy of enterprises. However, to gain a more complete picture we must also understand how additional subsidies influence the indicators of these firms, as the effect of the second (and subsequent) subsidies does not necessarily coincide with the effect of the first subsidies. On the one hand because the average amount awarded is considerably higher in this group (see Table A6 for non-refundable subsidies), and on the other hand selection may also differ (the assessing authority may have more information, while the administrative cost of application is lower for the firm).

The sample available to us in the case of non-refundable subsidies is large enough to estimate the effect of second grants explicitly. Our method differs from the method followed for the impact evaluation of first non-refundable subsidies in the following points. We set off from the population of firms which had at least one winning application during the programming period. We regard the second subsidy as the treatment. We disregard the company's history from the time of the third subsidy, if any. We complemented the matching with two additional criteria: we require an exact match in terms of the year and the size category[24] of the first subsidy of the treated and of the control companies.

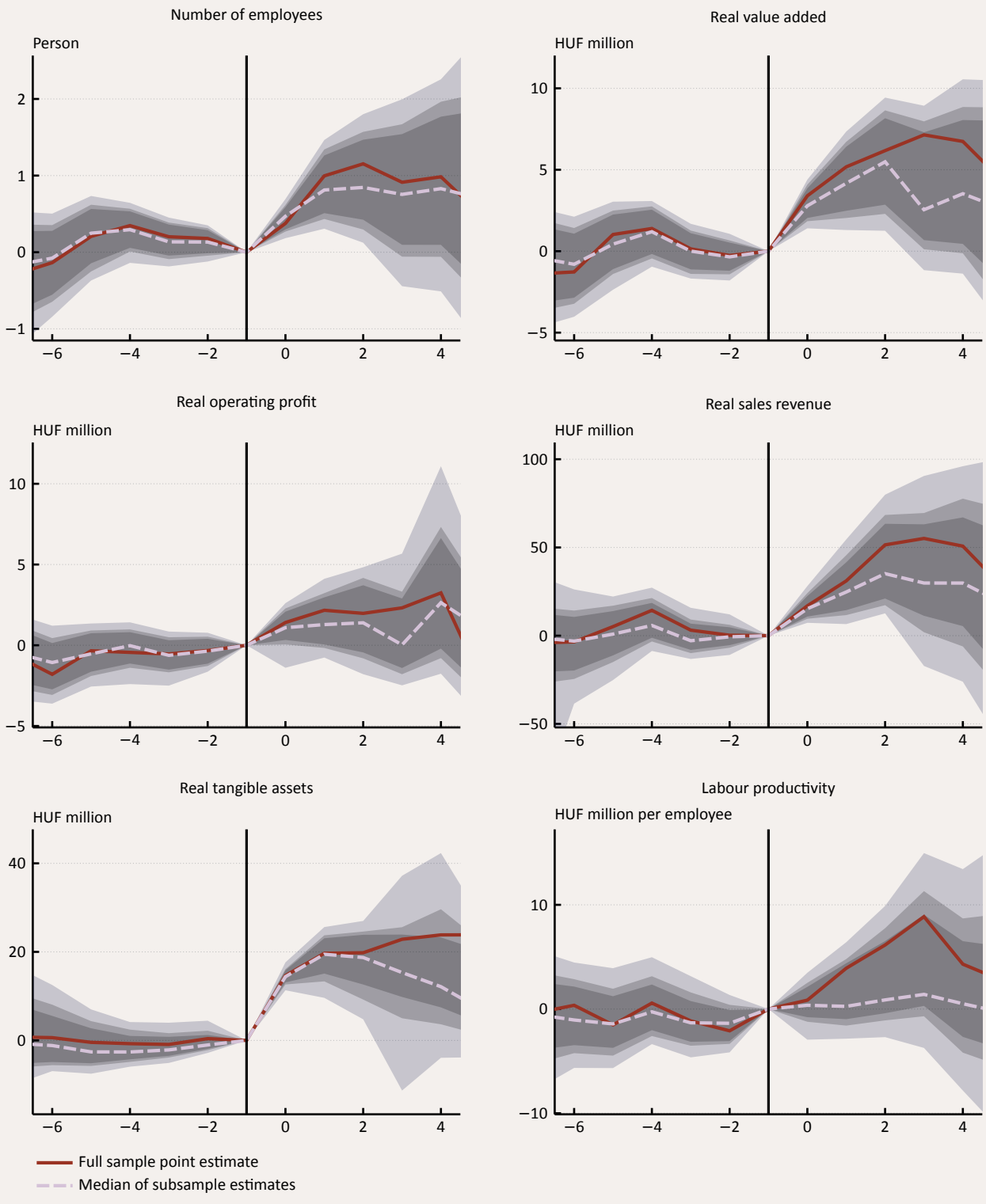
Based on our results (Figure 3), the effectiveness of the second subsidies is more moderate than that of the first subsidies. We see a similar increase in terms of the number of employees, but in the case of real value added and real operating profit, the effect wears off faster than the effect of the first subsidies. These results may seem peculiar at first glance, but they are less surprising if we consider the fact that due to lower administrative costs (e.g. familiarity with the application system) enterprises may embark on projects the return of which would be too low in the case of a first application.

4.2.2 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

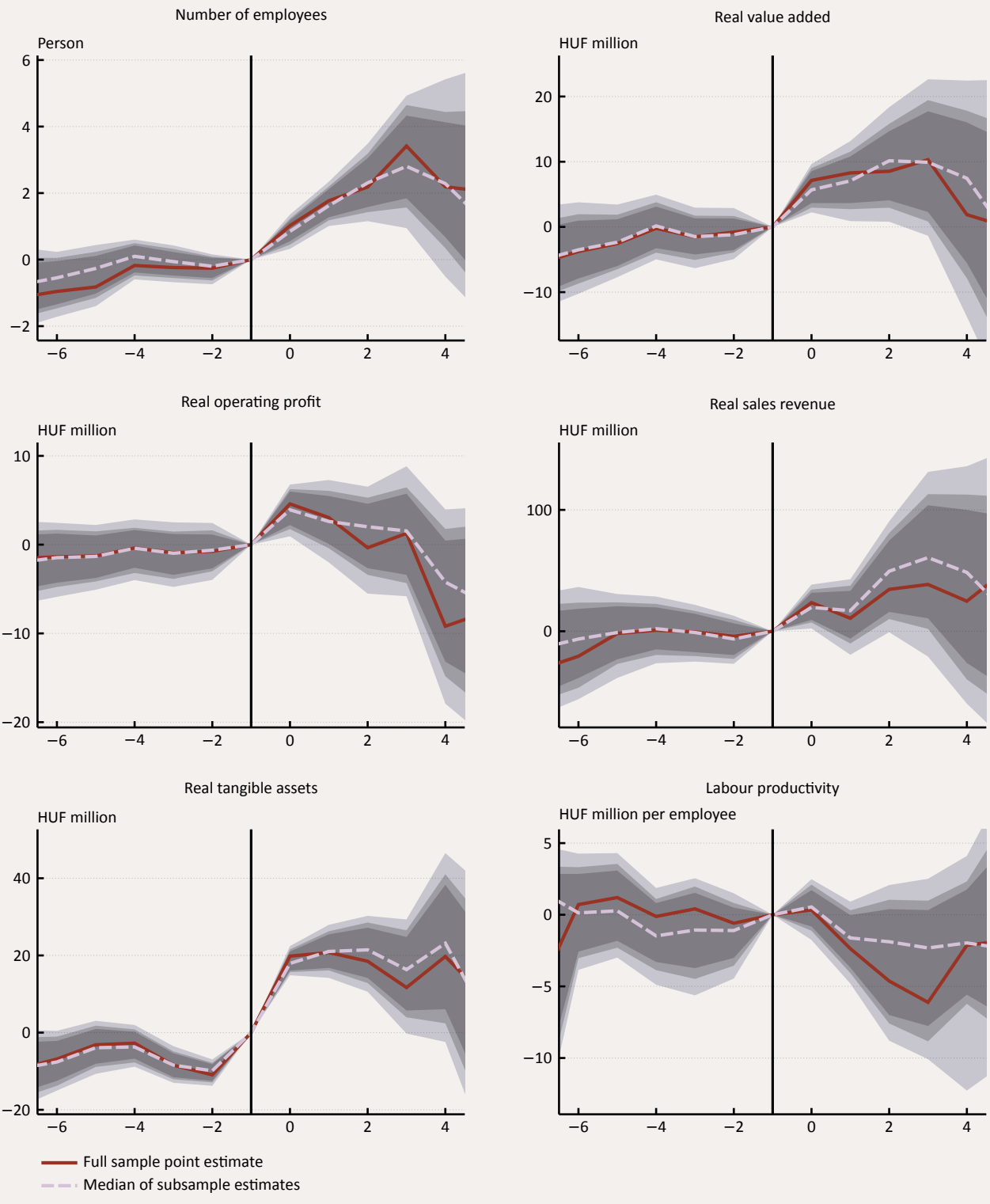
[24] Based on the amount of subsidy, we generated the following left-open intervals: 0–5 million HUF, 5–10 million HUF, 10–15 million HUF, 15–20 million HUF, 20–30 million HUF, 30–50 million HUF, 50–75 million HUF, 75–100 million HUF. Funding of over 100 million HUF was quite rare, therefore we capped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

Figure 2
Refundable subsidies



Note: relative time in years on the horizontal axis. Shaded areas represent 90%, 95% and 99% confidence intervals.

Figure 3
Non-refundable subsidies – second treatment



Note: relative time in years on the horizontal axis. Shaded areas represent 90%, 95% and 99% confidence intervals.

more beneficial in view of their "reusability", having the same impact with lower expenses. However, when comparing the results of various estimations with different treated populations and treatment definitions, it is important to bear in mind that 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 currently 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 certain at all that if the companies that received non-refundable subsidy had received refundable assistance, whether its effect would have been comparable with the estimated effect of refundable subsidies. Thus, our results can only be compared with proper precaution.

Therefore, we made an attempt to analyse this issue by slightly modifying our main methodology. We set off from the population of those firms which received only one kind of subsidy (refundable or non-refundable) during the programming period. We analysed the first subsidy of these firms. We estimated the matching model exclusively based on the periods prior to the subsidy, and the two possible outcomes represented the type of the awarded subsidy. When generating the pairs we required exact matching in terms of the sector, the year of subsidy and the subsidy's size category[25], moreover, for a given refundable subsidy beneficiary we again looked for the nearest neighbour based on the propensity score from the companies with similar characteristics that obtained non-refundable subsidies.

Based on our results (Figure 4), there is no considerable difference between the effectiveness of 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 financial instruments. However, we should not forget either that these results are only valid for those firms that are able to implement their committed investments from financial instruments, too. Therefore, despite our estimates, financial instruments 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 are nevertheless socially desirable.

4.2.3 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. We repeated the estimations pertaining to the non-refundable subsidies so that (1) we did not exclude the firms that obtained large subsidies (top 1–2 per cent) and (2) in the case of an observation considered as outlier or erroneous, we only excluded that observation and not the entire history of the company. As evident from Figure A4, these estimates are practically the same as the original ones. 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 grant sometime during the period.[26] As visible in the figure, the results obtained this way are also almost identical to our main estimates.

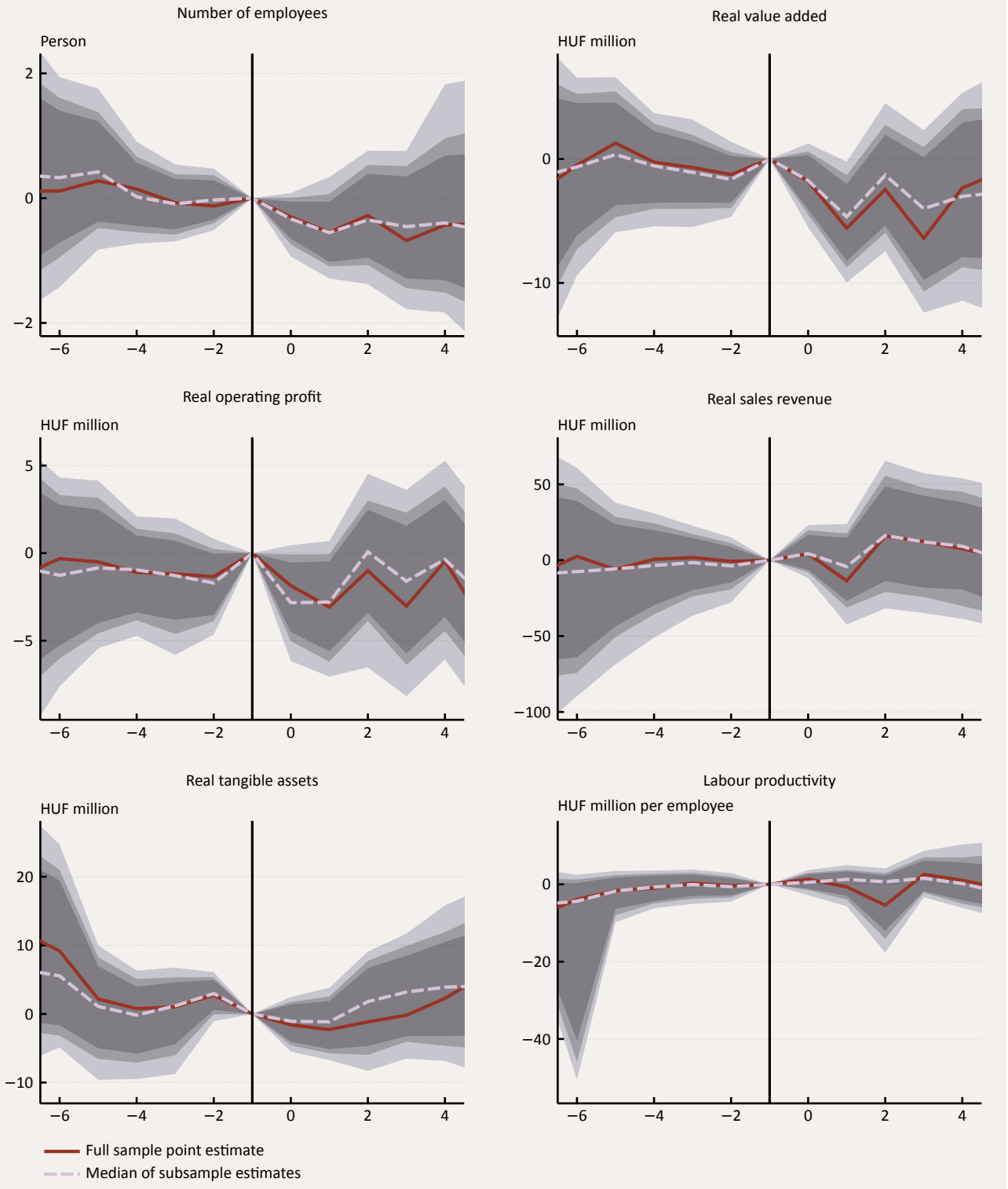
In addition, we also performed an estimation in which we only considered those enterprises that received one non-refundable subsidy during the period (as opposed to our main estimation where we measured the impact of the first subsidy of companies which could obtain an arbitrary number of subsidies.) This should not be considered as a robustness check similar to the previous ones, since the firms 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 the subsidy had a smaller impact on these companies. Although the received average subsidy amount is slightly lower in this population than in the originally analysed one (Table A6), the difference in effectiveness most likely has some other reasons, too. If, for example, at the allocation decision, the authority assessing the applications also took into account the success of the subsidies previously won by the company, this is exactly the result we expect.

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 from its second subsidy, as we wanted to estimate the effect of a single subsidy as opposed to

[25] Similarly to the estimation for the second subsidy, we generated the following left-open intervals: 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 HUF.

[26] However, we do not have information on the time of the negative decision in case of non-subsidised applicants. 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.

Figure 4
Relative effectiveness of refundable subsidies compared to non-refundable subsidies



Note: relative time in years on the horizontal axis. Shaded areas represent 90%, 95% and 99% confidence intervals.

the effect of the whole program. However, this procedure introduces endogenous selection to the estimation: if, for example, those firms get further subsidies on which the first subsidy had a greater effect, as a result of restricting their histories our estimates will be biased downward. Hence, to give an upper estimate for the effect of the subsidies, we also performed our main estimations without dropping the observations from the second treatment on. As displayed on Figure A4, from the first subsidy on there is a dynamic growth in the estimated effect 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 in effect still not significant.

As we have argued at the introduction of our empirical strategy that each step of our estimation procedure is necessary to obtain reliable estimates, we consider it important to show the consequences of omitting its certain elements. For this purpose we estimated a model for our main specification (1) without utilising matching and relative time dummies (i.e. comparing the treated population with the total potential control population in a standard fixed effects panel regression with time and firm fixed effects) and (2) with matching, but omitting relative time dummies. Besides these, as a robustness check of the 1-to-1 matching, we also performed an estimation using 1-to-5 matching[27]. Our results for the six dependent variables under review are shown in Tables A7–A12. Our baseline estimates with 1-to-1 matching are virtually identical to the estimates with 1-to-5 matching. However, the presence of the relative time dummies proves to be crucial: in both sets of estimates without relative time fixed effects, the parallel trend assumption is not supported by the pre-treatment dynamics in the case of most variables: subsidised firms performed substantially differently (grew faster or decreased more slowly) before the treatment than control companies.

We have estimated the effect of the subsidies on the *levels* of the variables of interest. In our opinion, besides making the interpretation of the results easier, this approach also reflects our expectation that a subsidy of a certain size has an absolute contribution to the analysed variables, regardless of their magnitude. However, if the effect of the subsidies is proportional to the size of the beneficiaries (i.e. multiplicative), using the logarithms of these variables might be a more appropriate specification. In addition, as the results can approximately be interpreted as percentage changes in this case, it also gives an idea about the economic significance of the subsidies for the beneficiaries. This estimation could only be performed on the number of employees and real sales revenue[28], as real operating profit and real value added has a considerable number of negative observations, while the stock of tangible assets is zero in approximately the third of the observations (mostly micro enterprises), which would lead to implausibly high percentage change estimates. The results (Figure A5), obtained for our main specification, are in their dynamics similar to the estimates in levels. Furthermore, the effect of the subsidies (roughly 20% increase in number of employees and more than 50% increase in sales revenue) can be considered positively significant for the subsidised companies.

4.3 SUBGROUP ESTIMATES

In this subsection, we present our estimates for the subgroups of the population divided based on various characteristics. Although our primary objective is to provide a complete picture of the impact of subsidies and shed light on the differences in the impact of subsidies between various subsidy and enterprise categories, in the case of some groups the number of companies receiving subsidies is too low. For this reason, only those results are included where sufficient observations are available for a reliable estimation.[29] When interpreting the results it is important to bear in mind that they are not directly comparable in the sense that we cannot draw conclusions from them 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 the subsidy amount (Table A5), objective and even in other characteristics of the scheme.

We performed separate estimations for non-refundable subsidies according to size category. We defined size category as the mode of the SME classifications observed on 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 we obtained the expected results in all cases (Figure A1). Larger firms receive on average larger subsidies, and accordingly, the effect also increases with

[27] 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.

[28] Due to the zero values in sales revenue, in this case we opted for the $x \mapsto \log(1 + x)$ transformation instead of simply taking natural logarithms. In case of non-zero values, this is almost identical to the latter.

[29] In practice, this means that we present those groups where at least 400 pairs are included in the fixed effects estimation.

the size categories. For most variables, micro and small enterprises coincide also in terms of the dynamics of the effect (in the case of medium-sized enterprises the size of the sample is unfortunately not large enough to accurately estimate the long-term impact of their subsidies).

For the breakdown of the subsidies by development objective, we had sufficient observations available for a reliable estimation for the following topics: support of research, development and innovation activity; development of production plants, technology and capacity; development of corporate information and communication technology. When interpreting these results (Figure A2) it is important to bear in mind that the average subsidy amounts differ significantly. The winners of R&D subsidies receive on average three times as high of an amount as the winners of subsidies aiming the development of production plants, technology and capacity and more than six times than the beneficiaries of corporate ICT development subsidies. In the light of this, it is rather surprising that the absolute effect of these subsidies is not considerably higher than that of the other two – moreover, their effect wears off fairly quickly. 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 results obtained for subsidies for the development of production plants, technology and capacity, we should highlight that 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, therefore their pairs in the control group catch up with them over time. None of the subsidy types have any effect on labour productivity.

The number of non-refundable subsidies also enabled us to estimate the impacts in a sector-based breakdown. Here (Figure A3) mainly the subsidies granted for companies active in the manufacturing of plastic and metal products stand out; we estimated the biggest impact on this sector in terms of most examined variables (the average subsidy amount was the highest in this sector as well, but the estimated coefficients can be considered high even so). In terms of certain dependent variables, we also measured higher than average effects for companies active in construction, in the sector of manufacture of wood and paper products, furniture, printing, and finally in trade and repair of motor vehicles. In the remaining three sectors for which we could perform an estimation (information and communication; financial and insurance activities, real estate activities; professional, scientific and technical activities) the effects were relatively low and wore off quickly. We did not perceive any change in labour productivity in any of the examined sectors.

5 Conclusion

The effectiveness of subsidies aiming 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 of the subsidies directly targeting economic development 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 given to the final beneficiaries, 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, on sales revenue, on gross value added and in certain cases also on operating profit of the beneficiaries. In the case of non-refundable subsidies this effect is of substantial magnitude, for financial instruments 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 such 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 financial instruments. We also analysed the effect of companies' second non-refundable subsidies which, based on our results, is similar to the effect of the first subsidy, but it is more moderate.

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 premises, technology and capacity also perform well, their impact proved to be persistent. Finally, in the sector-based splitting, the manufacturing of plastic and metal products stands out, but we also estimated significant effects for companies active in construction, the information and communication sector, and 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 may 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 in how large portions should the available funds be allocated to achieve optimal effect. Another direction could be to explore the relative efficiency of various funding forms, in particular, the relationship between the various subsidies and market loans. 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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Appendix A Additional descriptive statistics

Table A1
Distribution of subsidies in time

Year of first payment	Subsidy category	
	Non-refundable subsidies	Refundable subsidies
2007	8	0
2008	1 138	554
2009	3 672	918
2010	3 444	1 009
2011	2 791	1 822
2012	5 298	4 367
2013	8 256	4 625
2014	4 725	1 583
2015	1 708	793
2016	156	202

Note: The table contains subsidies extended to micro, small and medium-sized enterprises.

Table A2
Descriptive statistics of continuous corporate indicators

Variable name	Mean	Standard deviation	1st percentile	5th percentile	10th percentile	25th percentile	Median	75th percentile	90th percentile	95th percentile	99th percentile
Number of employees	5.31	14.03	1.00	1.00	1.00	1.00	2.00	4.00	10.00	19.00	63.00
Real gross value added (HUF million)	16.37	90.32	−7.45	−0.98	−0.22	0.00	1.68	8.13	29.29	66.37	303.98
Real operating profit (HUF million)	3.29	103.26	−26.21	−5.37	−2.37	−0.32	0.07	1.61	8.28	20.76	99.68
Real pre-tax profit (HUF million)	4.50	790.90	−31.44	−5.33	−2.24	−0.25	0.10	1.51	7.75	19.83	101.32
Real sales revenue (HUF million)	88.02	679.89	0.00	0.00	0.00	0.55	6.91	34.45	145.08	333.68	1437.82
Real tangible assets (HUF million)	31.53	311.66	0.00	0.00	0.00	0.00	0.51	6.24	36.33	95.91	541.73
Labour productivity (HUF million / no. of people in emp.)	20.56	157.01	0.00	0.00	0.59	2.62	6.54	16.08	38.25	65.37	213.18
Leverage (per cent)	53.89	37.05	0.00	0.00	1.61	17.48	55.12	94.17	100.00	100.00	100.00
Export ratio (per cent)	3.09	14.68	0.00	0.00	0.00	0.00	0.00	0.00	0.00	12.01	98.06

Note: The table contains observations of micro, small and medium-sized enterprises not categorised as faulty. In case of observations with negative equity, leverage has been maximised at 100 per cent.

Table A3
Distributions of the categorical variables

	Number of observations	Relative to the population in question (per cent)
Current corporate size		
Microenterprise	4 115 587	91.62
Small enterprise	316 721	7.05
Medium-sized enterprise	46 904	1.04
Large enterprise	696	0.02
Government or local government-owned enterprise (above a 25 per cent ownership ratio)	12 309	0.27
Ownership		
Domestic ownership of minimum 50 per cent	4 075 844	90.73
Foreign ownership over 50 per cent	292 629	6.51
Sectoral category		
Agriculture, forestry and fishing	139 600	3.11
Mining and quarrying	5 839	0.13
Manufacture of food products and beverages	56 394	1.26
Manufacture of textiles and wearing apparel	38 665	0.86
Manufacture of wood and paper products, furniture; printing	94 510	2.10
Manufacture of chemical and pharmaceutical products	7 964	0.18
Manufacture of plastic and metal products	115 335	2.57
Manufacture of electronic products	30 791	0.69
Manufacture of machinery and transport equipment	55 648	1.24
Other manufacturing	16 428	0.37
Electricity, gas, steam and air conditioning supply	7 506	0.17
Water supply, sewerage, waste management and remediation activities	16 036	0.36
Construction	425 315	9.47
Wholesale and retail trade; repair of motor vehicles and motorcycles	1 111 436	24.74
Transportation and storage	153 116	3.41
Accomodation and food service activities	206 153	4.59
Information and communication	225 558	5.02
Financial and insurance activities; real estate activities	439 055	9.77
Professional, scientific and technical activities	602 735	13.42
Administrative and support service activities	235 039	5.23
Public administration, defence; education; human health and social work activities	262 529	5.84
Arts, entertainment and recreation	106 871	2.38
Other activities	139 580	3.11

Note: The table contains observations of micro, small and medium-sized enterprises not categorised as erroneous.

Table A3
Distributions of the categorical variables (continued)

	Number of observations	Relative to the population in question (per cent)
Regional category		
Southern Great Plain	411 806	9.17
Southern Transdanubia	316 958	7.06
Northern Great Plain	416 226	9.27
Northern Hungary	317 378	7.07
Tax and Customs Directorate for Priority Cases	1 363	0.03
Central Transdanubia	389 570	8.67
Central Hungary	2 275 186	50.65
Western Transdanubia	361 873	8.06

Note: The table contains observations of micro, small and medium-sized enterprises not categorised as erroneous.

Appendix B Results of additional estimations

B.1 PROPENSITY SCORE MODELS

In order to give an idea about which explanatory variables determine the receipt of a subsidy significantly and thus influence matching substantially, for our main specifications we also display the results of the first step of the estimation, i.e. the propensity score model (Table A4). However, we would like to refrain from interpreting the results in a detailed manner since we consider this step of the estimation as a technical result rather than a causal model of the selection mechanism. Some reasons for this are that (1) the propensity score equation does not distinguish between allocation selection and self-selection; moreover, (2) it only indirectly controls for companies' other financing options (or, more generally, further potential sources of endogeneity may be identified in the model). Nevertheless, the sign of our variables bearing a statistically significant coefficient is in accordance with our expectations. In addition, balancing tests defined as normalised differences do not exceed the one quarter value usually employed as a rule of thumb, which means that the means of the samples of treated firms and controls chosen with this matching do not differ substantially from one another in any of the variables under review.

Table A4
Propensity score models

	Non-refundable subsidies	Refundable subsidies
Number of employees	3.53×10^{-03} ***	4.12×10^{-03} ***
	[3.99×10^{-04}]	[8.73×10^{-04}]
Real value added	1.05×10^{-06} ***	-1.98×10^{-07}
	[6.28×10^{-08}]	[1.85×10^{-07}]
Real operating profit	2.17×10^{-07} ***	-1.21×10^{-08}
	[4.73×10^{-08}]	[1.57×10^{-07}]
Real sales revenue	2.53×10^{-08} ***	3.98×10^{-08} ***
	[5.91×10^{-09}]	[1.48×10^{-08}]
Tangible assets	1.35×10^{-07} ***	6.91×10^{-08} **
	[1.45×10^{-08}]	[3.39×10^{-08}]
3-year change in number of employees	0.21 ***	0.04
	[0.03]	[0.04]
3-year change in real value added	−0.10 ***	−0.02
	[0.02]	[0.03]
3-year change in real operating profit	0.11 ***	0.09 ***
	[0.01]	[0.02]
3-year change in real sales revenue	0.17 ***	0.21 ***
	[0.03]	[0.04]
3-year change in tangible assets	0.93 ***	0.32 ***
	[0.02]	[0.03]
Majority foreign ownership	−0.44 ***	−0.43 ***
	[0.03]	[0.06]
Share of export in sales revenue	0.05 **	−0.05
	[0.03]	[0.05]
Leverage	−0.03 *	−0.06 ***
	[0.01]	[0.02]
Constant	−8.46	−8.17
	[103.53]	[429.61]
Sector dummies	yes	yes
Region dummies	yes	yes
Size category dummies	yes	yes
Year dummies	yes	yes

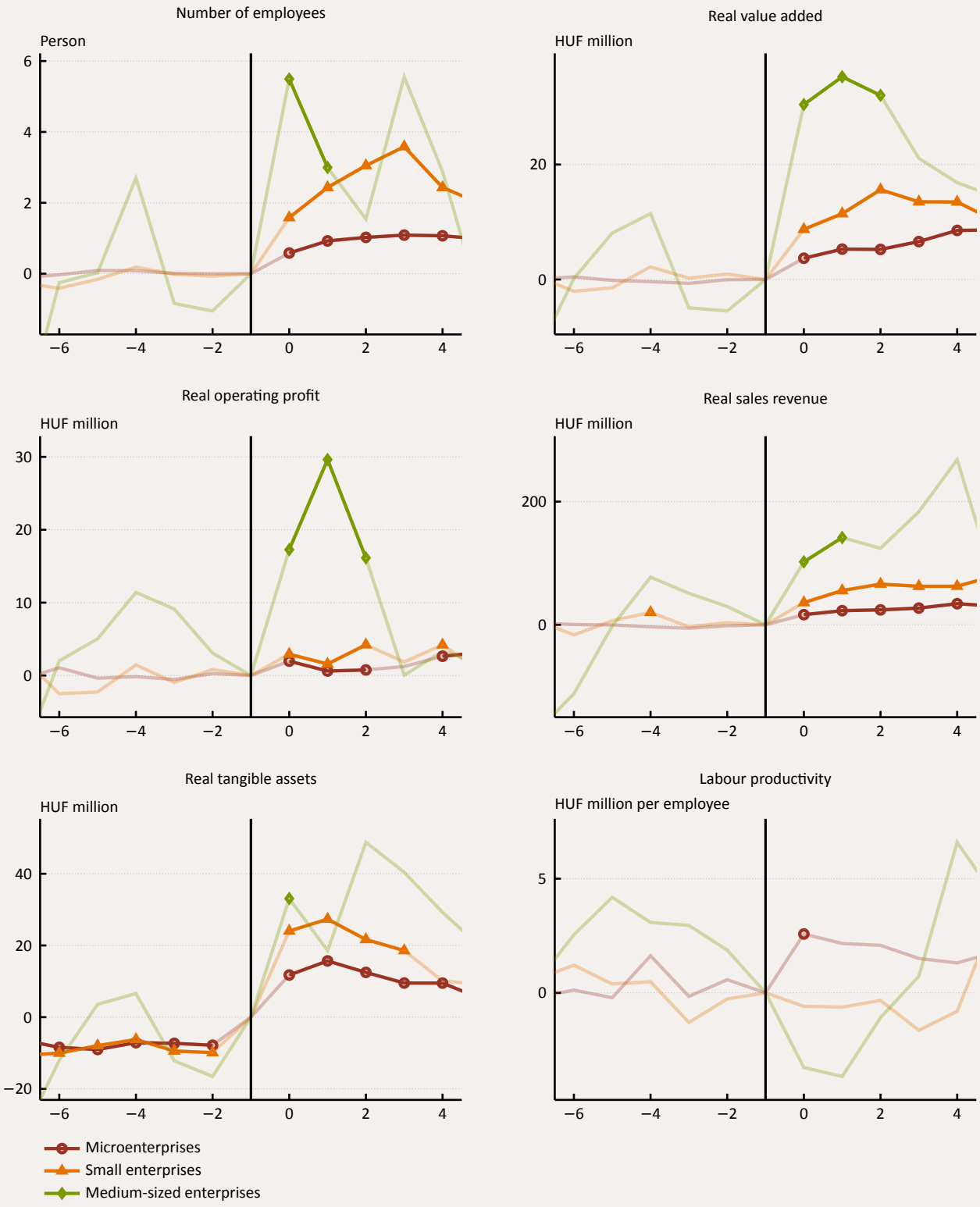
** Significant at the 10% level.*
*** Significant at the 5% level.*
**** Significant at the 1% level.*
Note: standard errors in brackets.

B.2 SUBGROUP ESTIMATES

Table A5
Summary statistics of estimates for non-refundable subsidies by groups

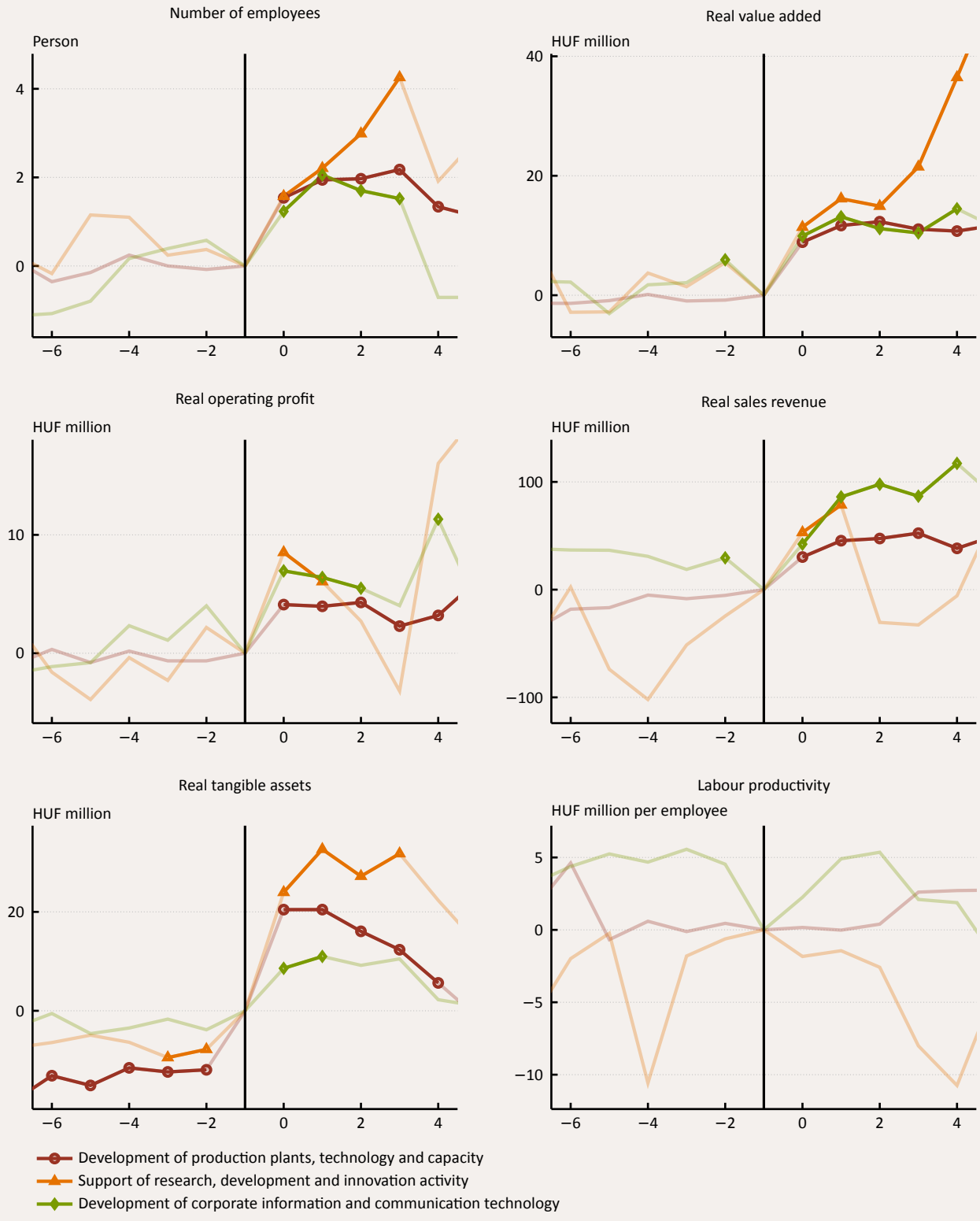
	Number of treated companies	Number of potential control companies	Number of matched pairs	Sample size in the fixed effects model	Average subsidy size (HUF million)
Corporate size					
Microenterprises	4 943	177 042	4 880	90 824	11.74
Small enterprises	3 967	14 089	3 819	71 590	22.94
Medium-sized enterprises	726	1 440	524	9 456	31.60
Development objective					
Development of production plants, technology and capacity	7 426	192 571	7 304	134 842	14.04
Support of research, development and innovation activity	792	192 571	764	13 994	45.36
Development of corporate information and communication technology	1 428	192 571	1 402	26 706	6.17
Sectoral category					
Construction	1 033	19 657	1 001	18 574	17.47
Manufacture of wood and paper products, furniture; printing	499	4 675	478	8 496	18.62
Wholesale and retail trade; repair of motor vehicles and motorcycles	2 529	51 990	2 476	47 08	13.04
Information and communication	476	8989	454	8 064	24.03
Manufacture of plastic and metal products	930	5 607	869	15 856	23.33
Financial and insurance activities; real estate activities	451	14 827	434	7 828	23.19
Professional, scientific and technical activities	893	25 928	864	15 512	15.84

Figure A1
Non-refundable subsidies by size category



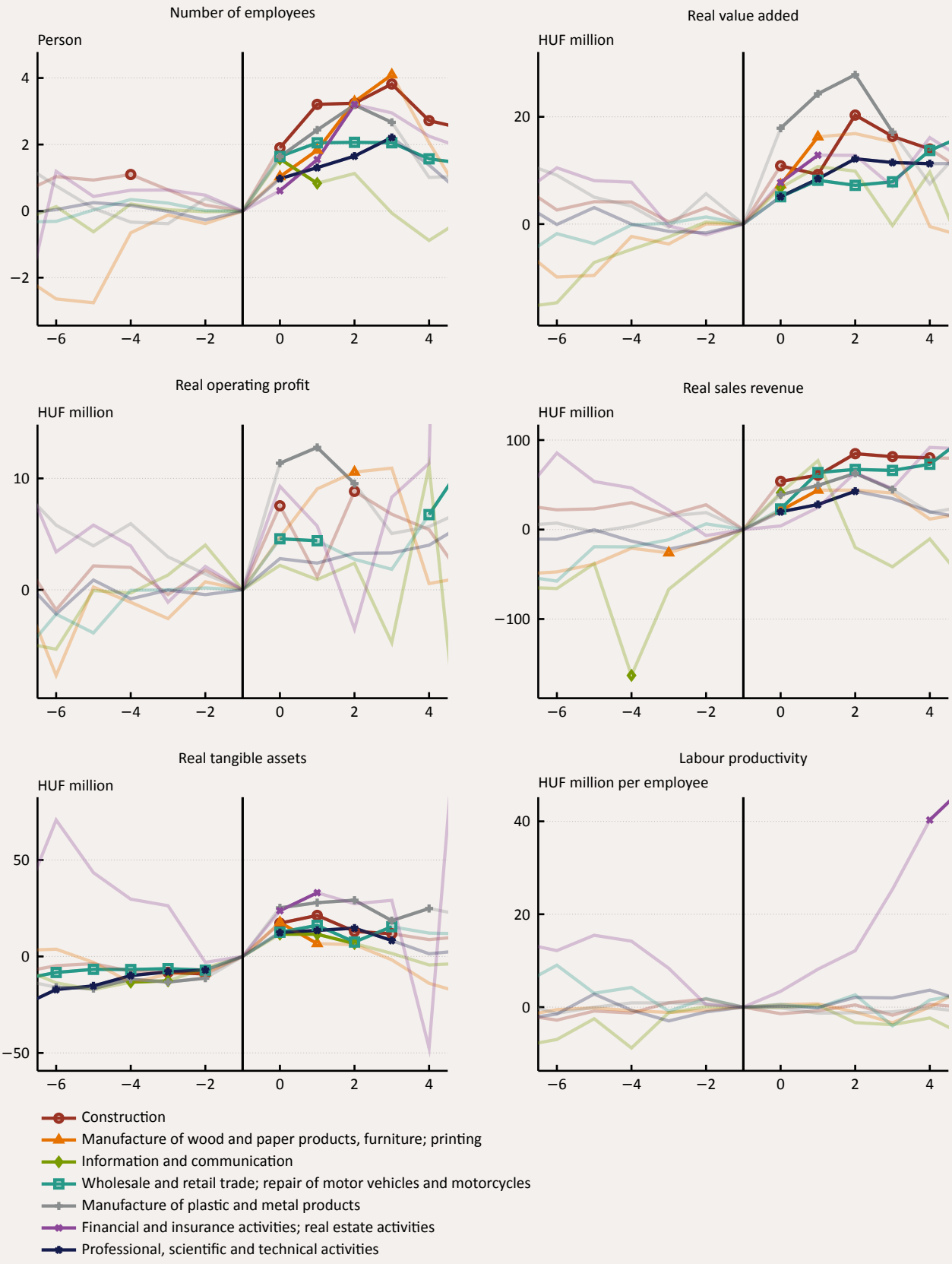
Note: markers represent significance on the 95% level.

Figure A2
Non-refundable subsidies by development objective



Note: markers represent significance on the 95% level.

Figure A3
Non-refundable subsidies by sectoral category



Note: markers represent significance on the 95% level.

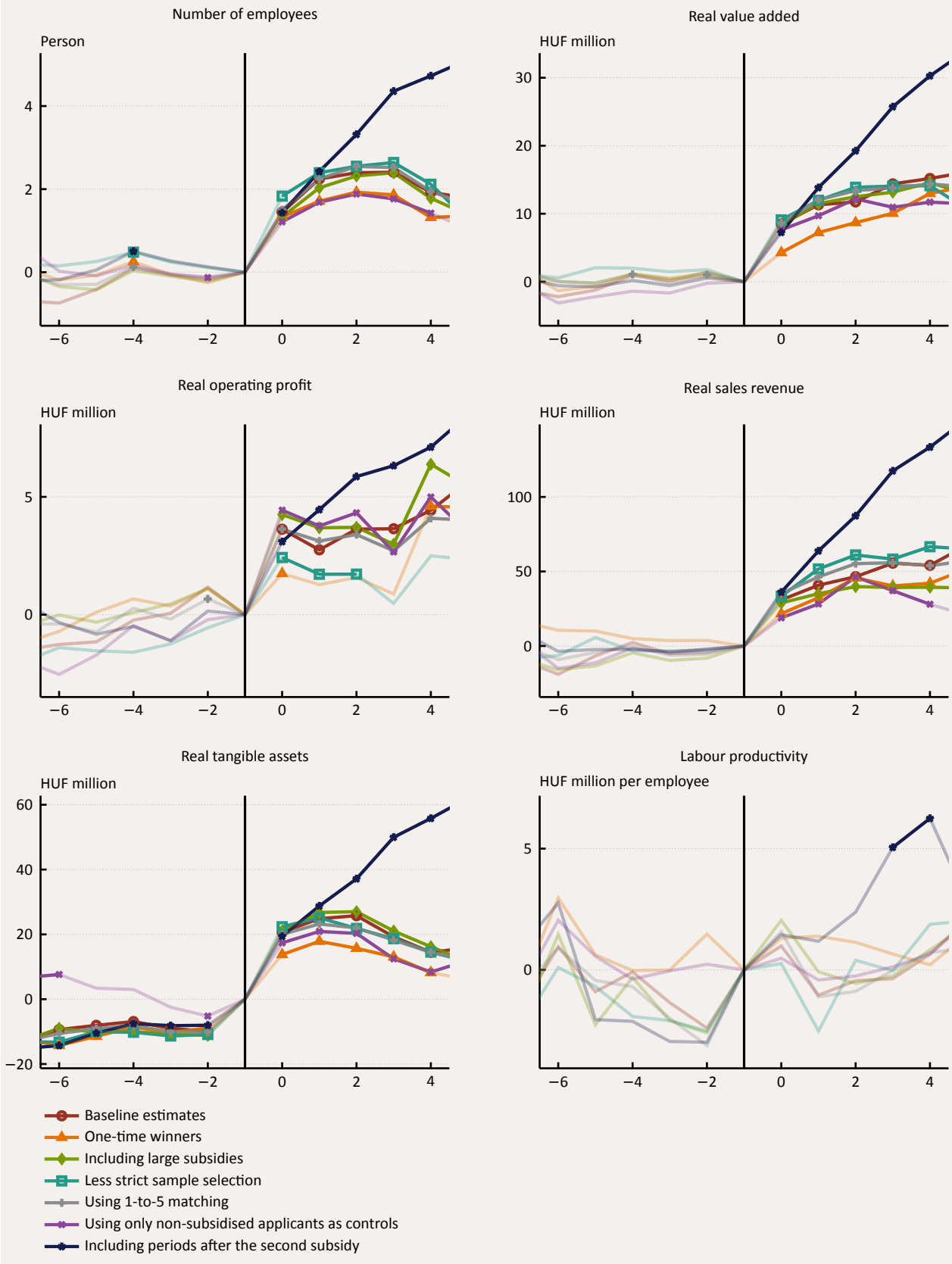
B.3 ROBUSTNESS CHECKS

Table A6
Summary statistics of robustness checks

	Number of treated companies	Number of potential control companies	Number of matched pairs	Sample size in the fixed effects model	Average subsidy size (HUF million)
One-time winners	6 389	192 562	6 326	128 496	15.36
Including large subsidies	9 654	192 562	9 448	175 734	18.90
Less strict sample selection	11 394	260 518	11 277	199 018	19.22
Using 1:5 matching	9 636	192 562	45 808	852 486	18.02
Using only non-subsidised applicants as controls	9 636	5 270	9 073	170 132	17.87
Including periods after the second subsidy	9 472	192 570	9 314	191 660	17.54 [a]

[a] *First subsidy*

Figure A4
Robustness checks



Note: markers represent significance on the 95% level.

Figure A5
Non-refundable subsidies – estimates in logarithms

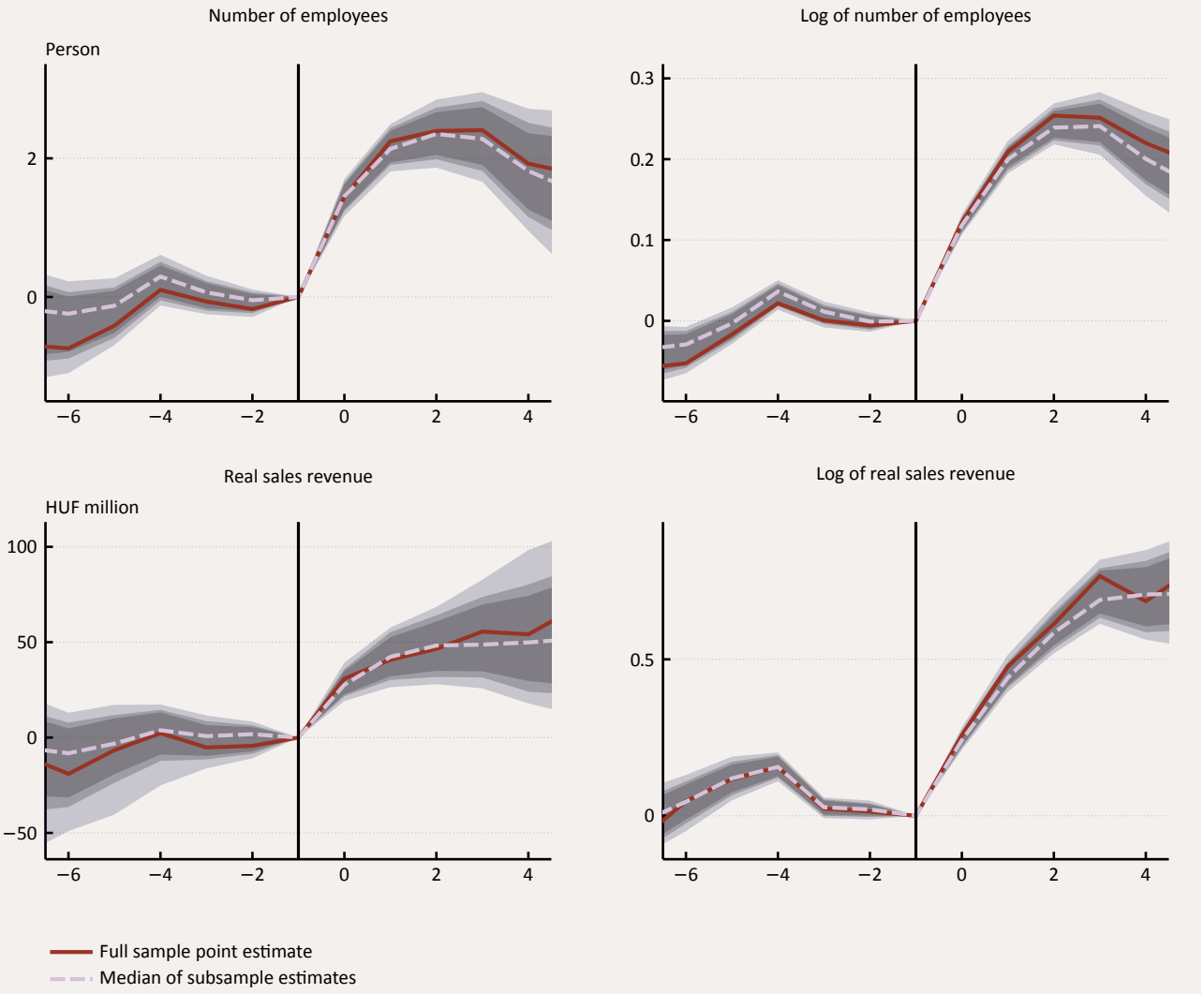


Table A7
Comparison of different estimation methods for number of employees

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-5.99 * [-7.03, -4.94]	-5.53 * [-7.49, -1.59]	-2.39 [-6.54, 1.30]	-3.03 [-5.86, 0.18]
-10	-4.03 * [-4.52, -3.55]	-2.47 * [-3.30, -1.03]	-0.36 [-2.81, 0.75]	-0.52 [-1.99, 0.27]
-9	-2.91 * [-3.22, -2.60]	-1.52 * [-2.02, -0.51]	-0.71 [-1.26, 0.59]	0.07 [-0.70, 0.57]
-8	-2.67 * [-2.93, -2.41]	-1.68 * [-1.90, -0.82]	-0.76 [-1.06, 0.37]	-0.01 [-0.62, 0.40]
-7	-2.57 * [-2.80, -2.34]	-1.75 * [-1.99, -1.10]	-0.69 [-0.95, 0.27]	-0.03 [-0.52, 0.27]
-6	-2.60 * [-2.81, -2.40]	-2.01 * [-2.13, -1.39]	-0.74 [-0.89, 0.07]	-0.30 [-0.56, 0.08]
-5	-2.50 * [-2.68, -2.31]	-1.99 * [-2.03, -1.49]	-0.41 [-0.59, 0.14]	-0.29 [-0.41, 0.09]
-4	-2.01 * [-2.18, -1.83]	-1.59 * [-1.63, -1.20]	0.10 [-0.05, 0.51]	0.12 * [0.05, 0.43]
-3	-1.43 * [-1.60, -1.26]	-1.11 * [-1.14, -0.83]	-0.07 [-0.20, 0.23]	-0.06 [-0.11, 0.18]
-2	-0.70 * [-0.87, -0.53]	-0.62 * [-0.64, -0.46]	-0.17 [-0.23, 0.07]	-0.12 [-0.16, 0.05]
0	0.93 * [0.77, 1.09]	1.04 * [0.84, 1.14]	1.44 * [1.24, 1.65]	1.50 * [1.30, 1.64]
1	1.10 * [0.93, 1.27]	1.19 * [0.98, 1.31]	2.25 * [1.90, 2.44]	2.25 * [1.97, 2.34]
2	0.97 * [0.79, 1.16]	0.92 * [0.66, 1.14]	2.39 * [1.99, 2.73]	2.55 * [2.16, 2.65]
3	0.54 * [0.32, 0.77]	0.54 * [0.12, 0.76]	2.41 * [1.82, 2.83]	2.52 * [2.00, 2.68]
4	-0.25 [-0.52, 0.02]	-0.32 * [-0.94, -0.06]	1.92 * [1.15, 2.51]	1.95 * [1.37, 2.27]
5	-0.71 * [-1.03, -0.40]	-0.91 * [-1.65, -0.55]	1.78 * [0.79, 2.38]	1.63 * [0.97, 2.06]
6	-1.07 * [-1.48, -0.66]	-1.49 * [-2.35, -0.86]	1.57 * [0.29, 2.52]	1.55 * [0.59, 2.06]
7	-1.11 * [-1.89, -0.32]	-1.15 * [-2.52, -0.18]	1.81 [-0.10, 3.91]	2.14 * [0.74, 3.04]
8	2.41 [-4.23, 9.06]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.837	0.877	0.878	0.875
R^2 (demeaned model)	0.002	0.006	0.004	0.004
N	2 285 569	172 036	172 036	837 357
Degrees of freedom	1 854 182	154 268	154 250	797 083
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

Table A8
Comparison of different estimation methods for real value added

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-40.64 * [-44.47, -36.81]	-25.54 * [-32.88, -10.34]	-25.26 [-31.73, 4.88]	-8.35 [-21.55, 3.43]
-10	-29.54 * [-31.31, -27.78]	-13.72 * [-17.12, -6.36]	-7.06 [-10.87, 5.38]	0.75 [-5.47, 5.16]
-9	-24.07 * [-25.20, -22.93]	-11.32 * [-12.86, -5.13]	-3.35 [-5.82, 3.92]	0.80 [-2.47, 3.73]
-8	-22.91 * [-23.84, -21.97]	-11.05 * [-12.27, -6.46]	-1.14 [-3.91, 3.55]	1.80 [-1.47, 3.49]
-7	-19.53 * [-20.38, -18.69]	-11.32 * [-12.82, -7.77]	-1.19 [-3.30, 2.87]	1.60 [-1.12, 3.20]
-6	-18.39 * [-19.14, -17.64]	-12.50 * [-13.35, -9.31]	-2.21 [-3.23, 1.70]	0.06 [-1.65, 1.88]
-5	-18.58 * [-19.25, -17.90]	-12.67 * [-13.14, -9.89]	-1.23 [-2.54, 1.54]	-0.27 [-1.68, 1.37]
-4	-15.24 * [-15.88, -14.59]	-9.76 * [-10.21, -7.59]	0.92 [-0.55, 2.71]	1.08 * [0.03, 2.39]
-3	-10.46 * [-11.09, -9.83]	-6.79 * [-7.25, -5.12]	0.12 [-1.36, 1.62]	-0.17 [-1.02, 1.15]
-2	-3.67 * [-4.28, -3.05]	-2.52 * [-2.96, -1.48]	1.40 [-0.02, 2.38]	1.01 * [0.18, 1.99]
0	2.22 * [1.63, 2.82]	3.42 * [2.55, 4.16]	8.48 * [6.79, 9.55]	8.64 * [7.33, 9.20]
1	2.74 * [2.11, 3.36]	3.76 * [2.48, 4.81]	11.32 * [9.72, 13.39]	12.04 * [10.37, 13.03]
2	2.27 * [1.58, 2.95]	1.87 * [0.41, 3.62]	11.74 * [9.88, 14.57]	13.39 * [11.19, 14.37]
3	0.66 [-0.17, 1.50]	0.40 [-2.06, 2.36]	14.39 * [10.02, 15.98]	13.81 * [10.90, 15.11]
4	0.59 [-0.42, 1.61]	-0.86 [-4.21, 1.62]	15.18 * [10.42, 17.95]	14.35 * [11.22, 16.54]
5	0.89 [-0.28, 2.06]	-1.25 [-6.35, 1.36]	16.14 * [9.62, 19.01]	13.99 * [10.44, 17.12]
6	1.26 [-0.26, 2.78]	-3.16 [-8.66, 0.96]	17.11 * [8.68, 21.72]	15.42 * [10.65, 18.67]
7	1.09 [-1.82, 4.00]	-3.51 [-12.04, 2.36]	16.92 * [3.23, 26.70]	16.14 * [7.52, 22.55]
8	-0.25 [-25.39, 24.88]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.817	0.840	0.840	0.836
R^2 (demeaned model)	0.005	0.004	0.003	0.003
N	3 185 852	175 176	175 176	852 486
Degrees of freedom	2 626 511	157 408	157 390	812 212
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

Table A9
Comparison of different estimation methods for real operating profit

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-12.94 * [-16.69, -9.19]	-10.86 * [-17.91, -4.71]	-13.17 [-21.42, 4.21]	-4.07 [-14.35, 4.54]
-10	-7.46 * [-9.19, -5.74]	-5.94 * [-8.05, -1.25]	-3.97 [-5.21, 5.05]	2.29 [-1.81, 5.37]
-9	-7.71 * [-8.83, -6.60]	-7.13 * [-7.99, -3.71]	-3.76 [-4.18, 2.03]	-0.64 [-2.09, 1.95]
-8	-7.58 * [-8.50, -6.67]	-6.51 * [-7.48, -3.99]	-1.15 [-3.05, 2.06]	0.30 [-1.58, 1.65]
-7	-6.81 * [-7.63, -5.98]	-6.21 * [-7.17, -4.03]	-1.51 [-3.03, 1.62]	-0.39 [-1.66, 1.45]
-6	-6.04 * [-6.78, -5.30]	-5.99 * [-6.67, -4.01]	-1.27 [-2.16, 1.44]	-0.39 [-1.27, 1.09]
-5	-6.53 * [-7.19, -5.86]	-5.38 * [-5.99, -3.85]	-1.15 [-2.02, 0.89]	-0.74 [-1.48, 0.59]
-4	-5.08 * [-5.71, -4.45]	-3.51 * [-4.05, -2.29]	-0.23 [-0.84, 1.69]	0.27 [-0.35, 1.45]
-3	-3.61 * [-4.23, -2.99]	-2.17 * [-2.92, -1.30]	0.06 [-1.20, 1.16]	-0.19 [-0.97, 0.86]
-2	-1.04 * [-1.65, -0.44]	-0.36 [-0.86, 0.31]	1.17 [-0.11, 1.96]	0.67 * [0.03, 1.59]
0	0.41 [-0.17, 0.99]	0.75 * [0.25, 1.40]	3.63 * [2.81, 5.07]	3.63 * [3.13, 4.75]
1	-0.89 * [-1.51, -0.28]	-0.40 [-1.32, 0.40]	2.75 * [2.12, 4.96]	3.12 * [2.63, 4.64]
2	-1.48 * [-2.15, -0.81]	-1.31 * [-2.36, -0.18]	3.63 * [2.02, 5.46]	3.40 * [2.56, 4.94]
3	-2.77 * [-3.59, -1.95]	-2.09 * [-3.67, -0.85]	3.65 * [1.00, 5.47]	2.71 * [1.35, 4.38]
4	-1.53 * [-2.52, -0.53]	-1.10 [-2.95, 0.80]	4.43 * [2.12, 7.37]	4.09 * [2.88, 6.29]
5	-0.17 [-1.31, 0.98]	0.08 [-3.00, 1.74]	5.65 * [1.81, 8.45]	4.02 * [2.42, 6.75]
6	2.09 * [0.61, 3.57]	2.44 [-1.58, 4.13]	7.31 * [3.48, 17.51]	6.70 * [4.46, 13.15]
7	3.10 * [0.26, 5.95]	1.88 [-3.47, 5.61]	4.90 [-2.08, 13.98]	7.01 * [2.03, 11.63]
8	-4.12 [-28.70, 20.47]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.938	0.502	0.503	0.530
R^2 (demeaned model)	0.000	0.001	0.001	0.001
N	3 185 852	175 176	175 176	852 486
Degrees of freedom	2 626 511	157 408	157 390	812 212
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

Table A10
Comparison of different estimation methods for real sales revenue

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-162.59 * [-194.23, -130.96]	-88.53 * [-150.58, -34.39]	-38.50 [-145.95, 68.06]	-21.07 [-102.86, 35.72]
-10	-48.65 * [-63.23, -34.07]	44.94 [-63.67, 161.99]	86.14 [-26.31, 204.94]	66.40 [-13.69, 155.29]
-9	-92.66 * [-102.03, -83.28]	-55.34 * [-89.53, -24.31]	-7.40 [-48.34, 24.04]	-11.42 [-37.20, 10.91]
-8	-84.17 * [-91.90, -76.44]	-42.84 * [-74.96, -20.94]	0.61 [-42.53, 23.28]	1.67 [-25.28, 15.48]
-7	-77.44 * [-84.40, -70.49]	-52.17 * [-77.05, -33.12]	-9.17 [-38.99, 14.67]	0.39 [-22.11, 11.27]
-6	-75.00 * [-81.21, -68.78]	-57.99 * [-77.06, -40.03]	-19.03 [-36.41, 7.84]	-9.16 [-23.68, 6.05]
-5	-70.32 * [-75.91, -64.73]	-55.78 * [-68.47, -38.25]	-6.63 [-23.71, 11.83]	-4.25 [-18.79, 8.84]
-4	-57.69 * [-63.01, -52.36]	-40.78 * [-50.23, -28.27]	2.31 [-12.29, 14.49]	-0.30 [-11.20, 11.54]
-3	-38.74 * [-43.97, -33.52]	-24.91 * [-31.37, -15.82]	-5.18 [-11.43, 8.65]	-6.25 [-9.78, 6.02]
-2	-13.58 * [-18.67, -8.50]	-9.32 * [-13.31, -4.40]	-4.35 [-8.52, 6.42]	-5.58 [-6.49, 5.13]
0	7.24 * [2.33, 12.15]	10.94 * [5.89, 14.92]	30.87 * [21.65, 35.99]	35.27 * [24.45, 35.20]
1	9.58 * [4.41, 14.75]	14.68 * [4.72, 19.50]	40.77 * [30.22, 55.26]	46.34 * [35.58, 51.64]
2	7.81 * [2.18, 13.44]	5.34 [-5.90, 15.00]	46.49 * [31.74, 64.07]	55.23 * [40.55, 60.64]
3	0.13 [-6.77, 7.02]	-6.70 [-19.43, 7.74]	55.54 * [31.54, 73.70]	55.83 * [37.54, 65.87]
4	-4.04 [-12.43, 4.36]	-16.33 [-33.72, 5.23]	54.12 * [24.07, 80.25]	53.85 * [33.02, 69.29]
5	-1.34 [-11.01, 8.33]	-13.71 [-41.82, 4.48]	67.59 * [22.49, 88.80]	57.21 * [31.01, 75.08]
6	-11.90 [-24.41, 0.61]	-41.81 * [-66.49, -10.49]	50.37 [-3.08, 90.80]	45.98 * [12.31, 70.15]
7	7.66 [-16.35, 31.67]	-25.70 [-61.69, 11.41]	68.11 [-7.16, 125.66]	70.22 * [14.66, 102.60]
8	-77.37 [-284.79, 130.04]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.771	0.823	0.824	0.826
R^2 (demeaned model)	0.001	0.001	0.001	0.001
N	3 185 852	175 176	175 176	852 486
Degrees of freedom	2 626 511	157 408	157 390	812 212
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

Table A11
Comparison of different estimation methods for real tangible assets

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-85.67 * [-100.91, -70.43]	-33.78 * [-60.89, -20.07]	-47.91 * [-72.72, -1.16]	-41.92 * [-62.23, -18.63]
-10	-59.89 * [-66.91, -52.86]	-22.76 * [-34.22, -11.60]	-11.72 [-33.50, 4.83]	-16.42 * [-28.84, -6.32]
-9	-43.55 * [-48.07, -39.03]	-13.19 * [-19.05, -5.67]	-16.90 * [-22.15, -3.62]	-10.97 * [-17.63, -4.64]
-8	-40.87 * [-44.59, -37.14]	-15.74 * [-20.75, -9.61]	-14.32 * [-20.31, -5.12]	-13.54 * [-17.87, -7.25]
-7	-38.06 * [-41.41, -34.71]	-18.42 * [-21.64, -12.68]	-13.26 * [-18.21, -6.39]	-13.07 * [-16.24, -8.12]
-6	-36.29 * [-39.28, -33.29]	-19.03 * [-21.57, -14.39]	-9.38 * [-14.69, -4.94]	-10.58 * [-13.99, -7.42]
-5	-34.95 * [-37.64, -32.26]	-20.47 * [-22.00, -16.24]	-8.07 * [-13.06, -5.10]	-9.09 * [-12.27, -6.59]
-4	-30.76 * [-33.32, -28.20]	-18.89 * [-19.89, -15.62]	-6.90 * [-10.48, -5.26]	-8.17 * [-10.56, -6.22]
-3	-24.53 * [-27.05, -22.02]	-16.78 * [-17.35, -14.26]	-8.98 * [-11.47, -7.22]	-10.02 * [-11.44, -8.04]
-2	-16.53 * [-18.98, -14.08]	-13.17 * [-13.61, -11.69]	-9.94 * [-11.23, -7.75]	-10.10 * [-10.86, -8.09]
0	18.69 * [16.32, 21.06]	18.69 * [16.99, 19.25]	20.70 * [17.78, 21.25]	20.00 * [18.14, 20.74]
1	20.17 * [17.68, 22.66]	19.50 * [16.89, 20.80]	24.84 * [20.55, 26.10]	23.21 * [20.48, 24.84]
2	18.21 * [15.50, 20.93]	13.59 * [10.74, 16.18]	25.77 * [18.04, 25.70]	21.99 * [17.66, 23.41]
3	14.89 * [11.56, 18.21]	9.19 * [5.47, 12.30]	19.24 * [13.85, 23.04]	18.23 * [14.53, 21.05]
4	10.70 * [6.65, 14.74]	3.87 [-1.57, 7.85]	14.49 * [7.55, 20.88]	14.45 * [9.29, 18.24]
5	9.03 * [4.37, 13.69]	1.71 [-5.58, 6.15]	15.83 * [3.67, 21.19]	11.89 * [6.30, 17.57]
6	6.61 * [0.58, 12.63]	-3.70 [-11.53, 3.55]	17.30 [-4.19, 27.62]	17.26 * [3.52, 23.26]
7	3.11 [-8.46, 14.68]	-11.09 * [-19.65, -0.94]	6.66 [-9.63, 25.78]	13.26 * [1.01, 19.18]
8	11.00 [-88.93, 110.93]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.824	0.890	0.890	0.893
R^2 (demeaned model)	0.002	0.010	0.006	0.007
N	3 185 852	175 176	175 176	852 486
Degrees of freedom	2 626 511	157 408	157 390	812 212
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

Table A12
Comparison of different estimation methods for labour productivity

	(1) Fixed effects w/o matching	(2) Fixed effects w/ 1:1 matching	(3) Fixed effects w/ 1:1 matching (main specification)	(4) Fixed effects w/ 1:5 matching
relative time × treated				
-11	-13.55 * [-26.29, -0.80]	-4.83 [-26.53, 0.35]	-5.38 [-26.41, 5.22]	-5.95 [-23.64, 0.56]
-10	-5.05 [-10.99, 0.89]	-4.01 [-9.00, 1.46]	-4.87 [-10.60, 3.95]	-1.44 [-6.89, 2.85]
-9	-5.15 * [-8.97, -1.34]	-3.74 * [-6.99, -0.07]	-2.68 [-6.15, 2.44]	-2.34 [-4.38, 1.24]
-8	-2.34 [-5.47, 0.79]	-2.01 [-5.27, 0.32]	-0.43 [-3.83, 2.89]	-1.18 [-2.88, 1.48]
-7	-1.86 [-4.67, 0.95]	-1.35 [-4.41, 0.60]	-1.53 [-4.48, 2.67]	-0.89 [-3.87, 1.76]
-6	0.26 [-2.24, 2.76]	0.34 [-2.35, 3.43]	0.95 [-2.26, 5.26]	0.91 [-0.97, 4.45]
-5	-1.93 [-4.18, 0.32]	-1.20 [-2.97, 0.19]	-0.91 [-2.73, 2.23]	-0.44 [-1.48, 1.73]
-4	-1.96 [-4.10, 0.18]	-1.31 * [-2.77, -0.23]	-0.09 [-2.28, 2.12]	-0.70 [-1.51, 1.40]
-3	-0.85 [-2.95, 1.25]	-0.38 [-1.34, 0.51]	-1.36 [-2.91, 1.29]	-2.00 [-1.85, 0.43]
-2	0.15 [-1.90, 2.20]	0.15 [-0.56, 0.84]	-2.40 [-4.17, 0.93]	-3.13 [-1.77, 0.18]
0	-0.75 [-2.72, 1.23]	-0.52 [-1.32, 0.07]	0.98 [-0.96, 2.08]	1.57 [-0.33, 1.64]
1	-1.32 [-3.40, 0.75]	-1.01 * [-2.24, -0.50]	-1.05 [-2.50, 1.70]	-1.11 [-1.72, 0.77]
2	-1.08 [-3.34, 1.17]	-0.77 [-2.36, 0.06]	-0.45 [-1.93, 2.69]	-0.89 [-1.37, 1.28]
3	-0.93 [-3.67, 1.82]	-0.83 [-2.52, 0.32]	-0.37 [-2.43, 3.70]	-0.02 [-0.99, 2.28]
4	-0.36 [-3.68, 2.97]	-0.07 [-2.19, 1.73]	0.64 [-2.35, 5.32]	0.72 [-0.41, 3.34]
5	-0.18 [-4.02, 3.66]	1.32 [-1.74, 3.18]	2.10 [-1.56, 6.63]	0.87 [-0.32, 4.30]
6	0.18 [-4.82, 5.17]	2.11 [-0.75, 4.28]	1.96 [-2.86, 8.94]	1.86 [-0.03, 5.75]
7	-0.46 [-10.06, 9.14]	1.87 [-2.56, 4.88]	3.21 [-10.01, 10.76]	2.49 [-5.30, 7.35]
8	-9.56 [-90.65, 71.54]	-	-	-
Year fixed effects	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes
Relative time fixed effects	no	no	yes	yes
R^2 (full model)	0.640	0.717	0.717	0.728
R^2 (demeaned model)	0.000	0.000	0.000	0.000
N	2 285 569	172 036	172 036	837 357
Degrees of freedom	1 854 182	154 268	154 250	797 083
Average subsidy amount (HUF million)	18.11	18.02	18.02	18.02

**Significant at the 5% level*
95% Confidence intervals in brackets

MNB Working Papers 8

Impact evaluation of EU subsidies for economic development on the Hungarian SME sector

Budapest, July 2017