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The effects of EU-funded enterprise grants on firms and workers[☆]

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ABSTRACT

This paper investigates the effects of non-repayable enterprise grants on Hungarian SMEs, financed from the European Union's Structural and Cohesion Funds between 2004–2014. Comparing firm- and worker level outcomes of successful and unsuccessful applicants, we find that subsidized firms increase their employment, sales, capital-to-labor ratio and labor productivity, but we find conflicting results for total factor productivity. The skill composition of workers is not affected by the grant, but wages increase, although only for skilled workers and especially managers. These results suggest that the program generates additional investment, but that investment leads to limited technology upgrading at best. According to our simple calculations, the cost of creating an additional job with this program was equivalent to 3 years average wage and each year's grants contributed to aggregate SME labor productivity growth by 0.2-0.6 percentage points — with an annual cost often in excess of 1 percent of total SME value added.

1. Introduction

Many countries and regions allocate substantial amounts for subsidies to achieve regional convergence and accelerate economic growth (Neumark and Simpson, 2015). Among the largest of such schemes are the European Union's (EU) Cohesion and Structural Funds, which spent EUR 347 billion between 2007 and 2013 assisting its less developed regions to achieve convergence (European Commission, 2007; Dvouletỳ et al., 2020).[1] These programs provide funding in the order of several percentage points of GDP each year for the less developed regions of the European Union, including practically all Central and Eastern European countries. While the majority of these funds were invested in public infrastructure, about 10 percent was used to provide direct grant financing to mainly small and medium-sized enterprises (SMEs) to promote firm growth, productivity upgrading, and entrepreneurship in general, making the program one of the largest enterprise subsidy schemes in the World. In this paper, we use unique linked

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[1] Subsidies to firms – non-repayable or in the form of subsidized loans – are not limited to the European Union. China is famous for subsidizing its state-owned enterprises (e.g. Claro, 2006; Girma et al., 2009; Lim et al., 2018); in the United States the Small Business Administration allocates small firm subsidies (Brown and Earle, 2017; Higgins et al., 2020); subsidized loans are also widespread in developing countries: (Hassan and Sanchez, 2009) report that in Latin America, the Middle East, North Africa and South Asia, 214 institutions deal with microfinance.

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employer–employee Hungarian data to quantify the effects of grants providing investment subsidies for SMEs on a wide array of firm and worker outcomes.

A key rationale for providing investment grants is to relax SMEs' financial constraints and help them make efficient investments that may improve their technology,[2] and to promote growth, leading to dynamism and reallocation. To investigate the extent to which this is the case, we provide a theoretical framework which considers several broad, and somewhat simplified, views about the effects of the grants, which are present in academic discussions and the popular press. According to the *replacement* view, these grants fully crowd out investment that would take place anyway.[3] According to the *channeling out* view, moral hazard problems dominate the use of grants, so they are typically used for the owners' private consumption, and part of them may end up with the bureaucrat supervising the distribution of grants (Johnson, 2017; Mironov and Zhuravskaya, 2016). The third view, which we call *more of the same*, states that grants lead to additional investment, but not to upgrading of technology. Finally, the *technology upgrading* view suggests that cheap capital promotes investment in new technologies accompanied by strong productivity growth and an increase in the quality of the workforce. The latter two views reflect the argument that such industrial policies can foster the expansion and modernization of capital stock (Rodrik, 2008; Aghion et al., 2011; McGillivray, 2018).

We present a framework to help us interpret our estimates qualitatively in light of these views. Based on the framework, one can distinguish between the views by jointly investigating a set of different outcomes. A substantial increase in capital stock suggests that there is no full replacement. If this capital growth is also accompanied by a significant increase in sales and employment, one can exclude channeling out. Finally, one can distinguish between 'more of the same' and 'technology upgrading' based on productivity and the increase in the share of skilled workers.

To distinguish between these views, we first estimate a selection equation for applying and winning. Then we estimate the effect of the grant on the outcomes mentioned above. When estimating the effect of the grants, we start from a differences-in-differences methodology combined with matching, which is standard in the literature (Dvouletỳ et al., 2020). We also exploit information on successful and unsuccessful applicants and run regressions on a matched sample of these firms, which are more similar to each other than grant winners and non-applicants. We also use an instrumental variable estimation as an alternative identification strategy, using winning rates in different years in the same type of grants as instruments. These aggregate winning rates mostly depend on the availability of funds and are exogenous from the perspective of firms.

The data support the predictions of the "more of the same" view: initially larger, faster growing, and more productive SMEs are more likely to apply. We find positive grant effects on capital, employment, sales, capital intensity, and labor productivity, but ambiguous effects on TFP. These joint effects are only in line with the 'more of the same' view, and are also consistent with the established results in the literature (e.g. Bronzini and De Blasio, 2006; Bernini and Pellegrini, 2011; Cerqua and Pellegrini, 2014; Pellegrini and Muccigrosso, 2017; Banai et al., 2020). Regarding worker effects, we find that skill composition does not change, which, together with the lack of a TFP effect, suggests that no skill-biased technological change takes place. The wages of all workers increase, but the increase is stronger for skilled workers, especially for managers, which is in line with the rent-sharing in our framework.

We also analyze the heterogeneity of the effect along two policy variables. First, we show that firms that win multiple grants grow faster than those that win only once. This finding suggests that policy makers should not discourage repeated applications. Second, large grants (relative to the firm's capital stock) have a greater impact on the firm's tangible assets but not on other attributes, suggesting that productivity effects do not depend on the size of the grant.[4]

Finally, even though we find limited evidence for within-firm productivity growth, the program did promote the growth of more productive firms, contributing to reallocation. We use simple calculations to quantify the effect of the grant on the winners' contribution to employment and productivity growth. This approach is similar to studies that calculate job creation (see e.g. Davis and Haltiwanger, 1992) and productivity growth contributions for different groups of firms (see e.g. Foster et al., 2008; Haltiwanger et al., 2013; Haltiwanger, 2015). Importantly, these calculations do not measure the causal effects of the program because they do not take into account the effect of grants on other firms. According to our calculations, each year's grant program created jobs in grant-winning firms equivalent to 0.2–0.8 percent of total SME employment. Similarly, the productivity growth in the winning firms was equivalent to 0.2–0.6 percentage points of the aggregate SME productivity growth. However, this contribution should be compared to the cost of the program, which was above 1 percent of the aggregate SME value added in some years.

The main contribution of this paper is to the literature of state-financed investment grants, in particular, EU-funded grants, which was recently summarized by Dvouletỳ et al. (2020). We contribute to this literature in a number of ways. We motivate our analysis with a framework which nests a number of views about the effect of these grants and derive predictions from each of the views. On the empirical side, we use a number of features in our data to improve the identification strategy typically used in the literature. First, we use rejected applications as a more credible control group, which is only available in few countries (similarly to Bronzini and De Blasio, 2006; Decramer and Vanormelingen, 2016). We are also able to follow firms for an extended period of time.[5] This allows us to test the parallel trend assumption rigorously and also to estimate long-term effects. Third, we present instrumental

[2] Note that this is one of the key aims specified in the policy documents of these programs. See, for example: https://www.palyazat.gov.hu/doc/3854.

[3] There is an extensive debate in the R&D literature on the degree to which public subsidies crowd out private investments (e.g. Szücs, 2020).

[4] Heterogeneity along this dimension has been studied by Srhoj et al. (2019), who finds that small grants have limited effects. We do not find evidence for heterogeneity by employment size, the skill level of the workforce, and macroeconomic conditions in the grants' effect on firm outcomes. Service firms had smaller input effects and somewhat higher productivity growth compared to industrial companies.

[5] In this respect, our analysis is similar to Pellegrini and Muccigrosso (2017), while the typical study only estimates effects for 2–3 years (Dvouletỳ et al., 2020).

variable estimates where the instruments are generated from time variation in the share of winners within subprograms. In terms of new questions, we analyze the selection of firms into application and subsequent winning, and two important variables (multiple grants and the size of the grant), which are important for policy design. We also quantify the direct benefits of the program, focusing on its effect on reallocation, a key potential benefit of entrepreneurship policies.[6]

By studying the effect of grants, we also contribute to the literature of SME support policies in general. State-financed investment grants represent only one of the policies that provide financing for SMEs (Cumming and Groh, 2018; Farag and Johan, 2021). Studying these policies adds to our understanding of the financial constraints SMEs face and the effects of SME support in general.[7] We contribute to this literature by studying a very specific financial instrument, a non-repayable investment grant.

Direct enterprise grants from the European Commission are also part of the larger set of place-based industrial policies (Neumark and Simpson, 2015). In particular, Structural and Cohesion Funds are place-based policies supporting underdeveloped regions by combining infrastructure investment and business support.[8] We contribute to this literature by showing that, in one of the most heavily subsidized countries, investment grants affect the aggregate productivity of the SME sector mainly via reallocation effects.

In what follows, Section 2 discusses the theoretical framework. Section 3 describes our data and the institutional features of grant distribution. Section 4 has our results on selection, Section 5 describes the econometric approach, while Section 6 presents the results. Finally, Section 7 concludes.

2. Conceptual framework

In this section, we briefly describe our conceptual framework for the different views. The main aim of the model is to discipline our results. We use this framework to interpret our reduced firm estimates qualitatively – answering which view is most in line with our findings –, but we do not attempt to estimate model parameters. Online Appendix C includes a more formal description of this framework.

We consider grants as a financing option for investment with a peculiar cost structure, available for firm i. Compared to bank financing, grant-financed investments have a lower marginal cost of capital, since firms do not pay interest and depreciation on the fraction financed by the grant. Applying for a grant, however, involves a fixed cost, which we assume to be higher than the fixed cost associated with bank financing. While most SMEs fill in a bank loan application without much assistance, most firms, when applying for grants, hire businesses specialized in writing grant proposals for the following reasons. First, filling out a bank application is much easier than a grant application, which requires filling out complex forms and submitting numerous official documents for a tight deadline, and firms often receive help from the bank when doing so. Second, the basic options when choosing a bank loan do not change frequently and firms can apply for a bank loan whenever they are ready, while keeping updated about the ever-changing grant opportunities and deadlines requires substantial resources. We also assume that the fixed cost of grant application is largely independent from firm size — the cost of collecting information and filling out the forms is similar for applications for smaller and larger grants.[9]

We model the various views with three parameters (see Table 1). First, g is simply the grant size available,[10] as a proportion of the firm's initial capital stock. If this is less than what the firm would invest from market sources (h_i), the grant *replaces* market financing. In this case, the firm finances some of h_i from the grant, but the grant will not affect the firm's overall investment. In the other three views $g \geq h_i$. Second, γ shows how much of the grant can be channeled out. This parameter reflects the extent to which grant distributors monitor grant recipients. In the "channeling out" view $\gamma = 1$, allowing managers to fully transform the grant to private consumption by, for example, buying a company car for private use, while in the remaining two views $\gamma < 1$. Third, ξ shows the extent to which additional investments lead to TFP growth. In the "more of the same" view $\xi = 0$, while in the "technology upgrading" view $\xi > 0$.

Table 1 summarizes the predictions – our main hypotheses – of our model under the different views (the derivations are in Online Appendix C). First, we model the decision to apply for a grant by comparing the expected amount of quasi-rent generated by the grant with the fixed cost of the application. The quasi-rent results from four sources. First, the firm has to pay a lower amount for the investment it would undertake anyways from market sources. Second, it gets an extra investment at a lower cost. Third, the investment can lead to an increase in productivity (if $\xi > 0$). Finally, the owner may enjoy extra utility from channeling out the funds. Firms will apply whenever the expected quasi-rent is higher than the fixed cost of applying.

[6] Our data, however, do not allow us to estimate spillover effects.

[7] Close to the policy we study, De Mel et al. (2008) studies free cash subsidies for entrepreneurs in Sri Lanka and finds positive output effects. Lim et al. (2018) investigates a tax credit on investments in China and finds positive effects on both investment and productivity. A number of papers analyzed subsidized loans in developed countries (e.g. Bach, 2013; Brown and Earle, 2017; D'Ignazio and Menon, 2020; Higgins et al., 2020) and in the developing world (e.g. Banerjee and Duflo, 2014, in India). One of the most prevalent policies for providing funding for SMEs is targeted tax credits such as R&D credits (Agrawal et al., 2020), property tax reductions (Gobey and Matikonis, 2019) and payroll tax subsidies (Collischon et al., 2020). Other policies that provide public funding form SMES include innovation subsidies (Cecere et al., 2020), venture capital (Croce et al., 2019; Hennecke et al., 2019) credit guarantees (Martín-García and Santor, 2019) start-up subsidies (Caliendo and Künn, 2011).

[8] The regional effects of place-based policies have been studied both in the United States (Glaeser and Gottlieb, 2008; Kline and Moretti, 2013, 2014; Busso et al., 2013), in the European Union (Becker et al., 2012, 2018; Ku et al., 2020) and in emerging economies (Chaurey, 2017; Haschka et al., 2021; Ham et al., 2011; Lu et al., 2019). Note, however, that the large US programs analyzed (e.g., the Tennessee Valley Authority and the Appalachian Regional Commission), included only infrastructure investment rather than firm grants.

[9] Note that applying for grants and positive selection into application is very similar to export market participation, which is also modeling with a fixed cost of market entry in the (Melitz, 2003) framework.

[10] Here we consider the co-financing as part of g.

Table 1
Model parameters and predictions of various views.

Outcome\View	Replacement	Channeling out	More of the same	Technology upgrading
Parameters				
g	$< h$	$> h$	$> h$	$> h$
γ	n.a.	1	< 1	< 1
ξ	n.a.	n.a.	0	> 0
Selection				
Size	+	+	+	+
Productivity, past growth	0/+	0/+	+	+
Effect				
Investment	+	+	+	+
Revenue	0	0	+	+
Cap int.	0	+	+	+
Lab prod.	0	0	+	+
TFP	0	–	0	+
Skilled wages	0\+	0\+	+	+
Skilled share	0\+	0\+	0	+

This table compares the empirical predictions of various views for selection into application and the effects of the grants on firm outcomes. “Replacement” means that the grant replaces investment from market sources. “Channeling out” the money means using the grants for private consumption, therefore one does not expect an improvement in size or productivity. The “More of the same” view assumes that firms expand without upgrading their technology, while “technology upgrading” also assumes increases in TFP and potentially skill-biased technological change.

As larger firms tend to invest more, the quasi-rent realized on those investments increase in firm size in all views, but this increase is larger when the grant does not simply replace private investments. The quality of the firm’s investment opportunities, or the level of returns from the investment generated by the grant, also affects the level of rents. As much as productivity levels or past growth are correlated with the quality of investment opportunities, we can expect positive selection into applications along these variables, but only when the grant generates extra investment, i.e. in the “more of the same” and “technology upgrading” views.

Regarding the effects of grants on firm outcomes, we investigate each view separately in the model by comparing the firm’s outcomes when using the grant with the outcomes based on market financing. In the “replacement” view, the grant does not generate additional investment and, as a result, will not affect any of the other outcomes. In the “channeling out” view, the firm’s capital will increase in the books, but its productive capital will not change; we can distinguish this view from the others by observing an increase in capital but not in sales or employment. This artificial increase in capital will lead to an increase in the measured capital/labor ratio but to a fall in TFP. In the “more of the same“ view, there is an increase in productive capital, which is accompanied by an increase in other inputs and sales, as well as the capital-to-labor ratio, due to the lower cost of capital. The model, however, does not predict a change in TFP. Finally, in the “upgrading” view, we expect the same effects as in the previous view, but also an increase in TFP. Additionally, as much as technology upgrading leads to skill-biased technological change, this view may lead to an increase in the share of skilled workers.

We follow Kline et al. (2019), who use a rent-sharing approach to model wage changes when firms obtain patents. The grants generate rents because of both the lower capital cost and the potential increase in labor productivity. Therefore, we expect that the grant will have a positive effect on wages, especially for incumbent workers with good bargaining positions.

The aim of this illustrative model is to help us distinguish between some of the interpretations that have been offered for these grants. A few notes are in order. First, the model describes what happens at the firm level, and the different views are clearly defined at that level. However, different firms may have very different experiences. For example, depending on the value of investment under market conditions h_i, grants may replace investments in some firms while generating additional investment in others. Also, the extent to which additional investment leads to productivity growth (ξ) may differ between firms. Naturally, we can only estimate an average effect that reflects the typical experience. There can be much variation behind the average numbers.

Second, in the model, we define the views quite sharply, but the parameters are continuous. The main motivation for this is that we can link the views to the formal statistical tests in this way. For example, γ, the share of additional investment that can be channeled out, is continuous. Although the “channeling out” view is defined as $\gamma = 1$, the outcomes are likely to be similar if $\gamma = 0.9$ — most likely we would find an insignificant effect on sales and labor. The large effects on capital and the small effects on labor and sales are indicative of the high values of γ. The displacement view is also extreme in the sense that it requires complete replacement — but again, if replacement is close to complete, we expect small effects on capital. Similarly, it is unlikely that we can capture small positive values of ξ, the productivity growth. The statistical tests that we use have limited power.

Third, the two-period model in which sales and labor can be adjusted seamlessly does not describe the world perfectly. To remedy this, we present both short- and long-term estimates. When interpreting the results, we mostly focus on the long-term estimates, which are likely to allow us to capture more complete adjustments.

Fourth, our parameters γ and ξ capture a combination of supply- and demand-side effects. For example, ξ, the extent of productivity growth from additional investment, can capture demand-side factors, such as the set of investment opportunities available for the firm and also supply-side factors, such as the conditions for grants. While our results help us to narrow down the value of these parameters – mostly qualitatively – we do not aim at identifying the different determinants of these parameters, which would be hard to do with our data.

3. Data and institutional features

3.1. Data

The empirical analysis of this article is based on three databases. The first is an administrative panel of financial statements of all Hungarian firms with double-entry bookkeeping for the period between 1999 and 2014, collected by the National Tax and Customs Authority. It includes the balance sheets and income statements, as well as additional information such as the number of employees, the industry code of the firm, and the location of the company's headquarters.

Grant information also comes from administrative sources. The data were gathered by the Hungarian National Development Agency and incorporate all grant applications for the European Union's Structural and Cohesion Funds between 2004 (Hungary's EU accession) and 2014. The unit of observation in this database is a grant application, which can either be successful or unsuccessful. The data contain information on the time of the application, the time of decision, the total cost of the project, and the amount the firm received. The applied sum is also available after 2007, but only for a subset in the case of the unsuccessful applicants. The data also include the Sub-measure of the application, which provide information on the use of the grant (see the next subsection).

To study the effect of grants on the structure of employment and wages, we augment the data with a third administrative dataset, maintained by the National Pension Administration.[11] The version we use is a random sample of the population aged 5 or older in 2002. These 4.6 million individuals are followed between 2003 and 2011, with information available for May each year. In this analysis, we use only those individuals who have an employment contract in the given year, and we consider all the contracts of individuals who hold multiple jobs. As the sampling is based on individuals and not firms, the worker sample is not biased with respect to firm characteristics. The data have information on sex, age, 4-digit International Standard Classification of Occupations (ISCO) code and wages.

The three data sets are linked together with unique anonymized firm identifiers. As the first grants were allocated in 2004, we keep balance sheet information from 1999 to have pre-treatment years for early subsidies. We restrict the sample to firms that have an average number of workers between 5 and 250 during the period studied.[12] We drop agricultural enterprises, which are targeted by a different set of grants. Finally, we drop those cases where tangible assets, employment, material costs or sales are missing or are equal to zero (this affects about 2 percent of the firm-years).

The final data include 66,635 enterprises and 582,159 firm-years whereby the average firm is observed for about 8.7 years. Worker information is only available for a subset of these firms. These merged employer–employee data contain 54,157 firms (315,317 firm-years) and 2,654,635 worker-years. The comparison of the firm and the worker data in Appendix Table A7 in the Online Appendix shows that the firm and worker samples are rather similar in firm attributes.

3.2. Institutional setup

EU funds are allocated by institutions set up by the central government of each member state. While member states enjoy considerable autonomy in the details of their institutional structure, EU regulations prescribe a number of institutional guarantees regarding both the distribution of funds and the control of the process (Council Regulation No. 1083/2006). Eligible activities (and the associated calls) are classified in a hierarchic structure: the largest units are called Operational Programs (e.g. Operational Program for Economic Development). These are further divided into Measures and Sub-Measures, which we observe in the data. A Sub-Measure may include multiple calls for proposals.[13]

The rich information on the Sub-measures allows us to restrict our attention to grants which aim at investment promotion and technology upgrading. Therefore, we focus only on Sub-measures which (i) were aimed at firms (rather than, say, schools) and (ii) are investment grants (rather than, say, R&D, environmental or agricultural subsidies).[14] For the main calls included in our analysis, information on the number of applicants, the amount of funds eligible per application, the share of cofinancing, and the requirements regarding employment and sales growth are in Table A1 in the online Appedix as of 2010.[15]

These grants are typically not restricted to specific sectors of the economy; both industrial and service firms can apply. To streamline the process, decision-making was typically automated in these programs, leaving little space for subjective elements. Firms that satisfied a set of simple criteria (e.g. were at least 2 years old or had at least 5 employees) and submitted a formally complete application were awarded grants on a first-come, first-served basis. Furthermore, as part of these automated procedures, successful applicants typically received the full amount they applied for as long as it was not more than a predetermined maximum (Muraközy and Telegdy, 2016). We illustrate all these features for the largest Sub-Measure, GOP 2.1.1, in Online Appendix F.

In Hungary, 9,247 firms applied for the types of grants included in this study, which is only 14 percent of all firms in our sample (see Online Appendix Figure A1). In line with the lax conditions, 74 percent of applicants had at least one successful application

[11] These data are developed and maintained by the Data Bank of KRTK.

[12] There are very few large firms in the sample and we drop very small businesses as their data are often unreliable and only a small proportion of them applied for and received grants.

[13] These calls specify, inter alia, (i) the type of eligible activity, (ii) the eligibility criteria, (iii) the minimum and maximum grant size, (iv) the total amount available, (v) the minimum amount of co-financing, (vi) how proposals are evaluated and (vii) the deadline for application.

[14] For example, submeasure GOP 2.1.1, provides grant funding for upgrading firms' technologies in a broad sense. Other calls in line with these criteria, promote 'complex development' (GOP 2.1.2), investing in developing processes in line with quality certification requirements, such as ISO (GOP 2.2.2) or building e-Commerce (GOP 2.2.1) infrastructure or site development (ROP 1.1.1).

[15] The exact parameters and the names of subprograms changed across the years, but the types and structure of the programs remained mostly unchanged.

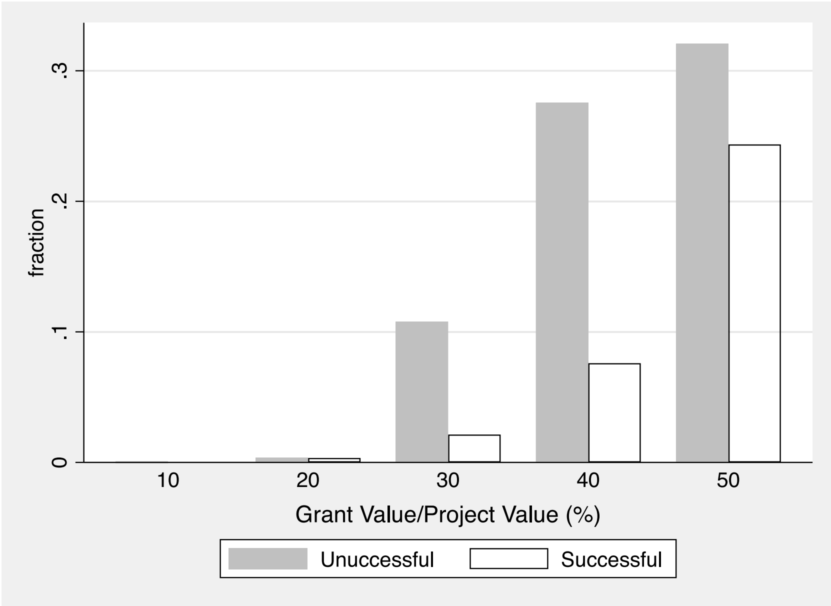

Fig. 1. Share of project cost covered by the grant. Notes: N = 9,371 grant applications. The figure presents the distribution of the value of the grant relative to the total financial cost of the project proposed by the firm.

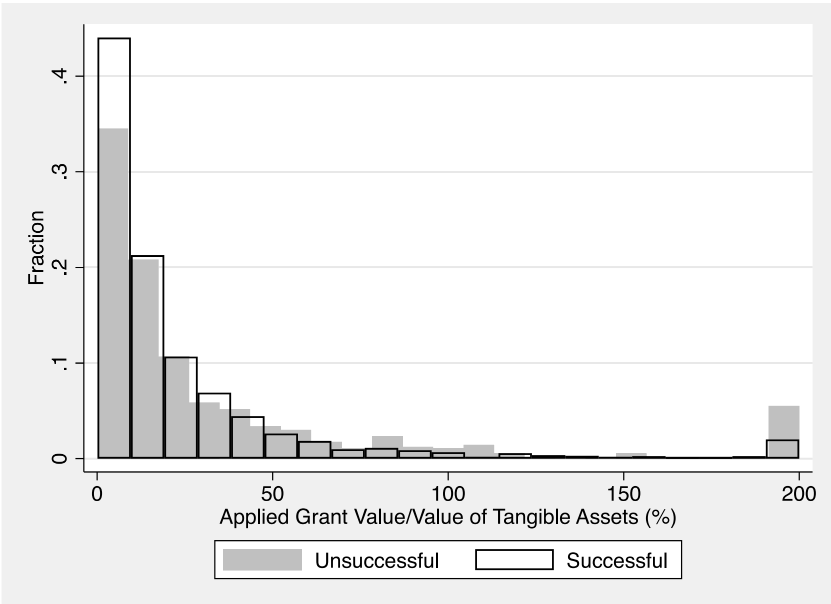

Fig. 2. Size of grant relative to firm size. Notes: N = 12,564 grant applications. The figure presents the size of the grant applied for relative to the firm's tangible assets. The variable is winsorized at the value of 2. The median size of the variable is 0.20 for unsuccessful applications and 0.14 for successful applications.

during the period of study (sometimes this was not the first application). Two-thirds of the successful firms won one grant, but some received more than one: 20 percent of them obtained two grants, 8 percent three grants and 6 percent received more than three grants. The time distribution of the successful and rejected applications is presented in Online Appendix Table A2. Until 2008, about 600 firms had won their first grant each year. Following the budget cycle of the European Commission, the number of subsidies increased to 1000 for the next two years and dropped continuously by the end of the period studied. The figure also shows that the number of rejected applications vary by year and diminishes in the last 4 years of the period studied. As there are relatively few applications towards the end of the studied period, we follow most of the treated firms for several years after the treatment.

Table 2
Descriptive statistics of successful applicants, unsuccessful applicants, and firms that never applied.

Variables	Never Appl.	Rejected	Winner	Winner - Rejected
FIRM-LEVEL				
Industry	0.21	0.30	0.36	0.059***
	(0.41)	(0.46)	(0.48)	(0.008)
Firm Age	10.36	10.40	11.83	1.43***
	(7.302)	(6.597)	(7.133)	(0.121)
Emp.	14.59	27.40	29.47	2.06***
	(22.71)	(33.55)	(34.75)	(0.597)
Assets (log)	9.24	10.93	11.21	0.281***
	(2.059)	(1.654)	(1.601)	(0.028)
Sales (log)	11.52	12.74	12.82	0.072***
	(1.426)	(1.292)	(1.271)	(0.022)
Investment	0.066	0.500	0.481	−0.019
	(1.193)	(1.009)	(1.001)	(0.019)
K-L ratio	7.068	8.099	8.290	0.191***
	(1.870)	(1.337)	(1.275)	(0.023)
L. Prod.	7.831	8.287	8.377	0.090***
	(0.949)	(0.785)	(0.734)	(0.013)
TFP	−0.011	0.062	0.103	0.041***
	(0.762)	(0.623)	(0.565)	(0.010)
WORKER-LEVEL				
Skilled	0.310	0.329	0.314	−0.015***
	(0.463)	(0.470)	(0.464)	(0.002)
Wage Skilled	111.2	112.3	126.0	13.669***
	(104.9)	(89.64)	(105.0)	(0.903)
Wage Unskilled	68.96	74.46	86.10	11.644***
	(38.37)	(38.83)	(45.08)	(0.256)
Person Appl. Before	0.015	0.065	0.106	0.041***
	(0.121)	(0.246)	(0.307)	(0.001)
Person Winner Before	0.008	0.043	0.074	0.031***
	(0.091)	(0.203)	(0.262)	(0.001)

Note: The table shows the means (standard deviations) of the characteristics for never applying firms (all firm-years) and unsuccessful and successful applicants (the year before application). The last column shows the difference between successful and unsuccessful applicants and the statistical significance of the corresponding t test. One observation is a firm-year (worker-year) for firm (worker)-level variables. 'Industry' refers to the share of firms, within the three groups, which operate in NACE rev. 2 sectors C, D and E. 'Skilled' refers to workers with ISCO code 1, 2, 3. Worker-level statistics are weighted with the inverse of the number of workers in the firm-year. Tangible assets, sales, capital-to-labor ratio and labor productivity are logged. TFP is the residual of a production function, estimated with the Levinsohn–Petrin method. N = 356,812/1,814,876 never applying, 4,829/57,037 rejected, 10,623/125,365 winner firm/worker-years with non-missing assets, employment, and sales figures (the number of observations for the other variables are in Appendix Table A7).

Online Appendix Figure A3 shows another important feature of this scheme: the maximum eligible size of the grant was typically not binding, probably due to copayment requirements. 10 percent of unsuccessful applicants and 22 percent of winners applied for more than 90 percent of the maximum amount. The amount co-financed varied between 30 and 50 percent for most applications (Fig. 1). The data also reveal that nearly all applicants paid as little copayment as was allowed: 77 percent of both rejected and successful applications requested at least 90 percent of the maximum share of the project cost covered by the grant. The size of the grant varied considerably both in absolute terms and relative to firm size. Fig. 2 shows the distribution of the grant size relative to the firm's tangible capital in the year prior to application. While many grants were relatively small, the majority of grants exceeded 10 percent of the firm's tangible assets and another 10 percent of grants reached or exceeded the tangible assets of the company.

Comparing successful and unsuccessful applicants reveals some differences between the two groups. First, Fig. 2 shows that while the distribution of grant size compared to capital stock is quite similar in the two groups, unsuccessful applications tend to be somewhat larger. This results from the lower success rate in subprograms with larger maximum grant size.[16]

4. Which firms apply and win?

We start our empirical analysis by studying the selection of firms into application for grants and into winning.

[16] This is also implied by Online Appendix Figure A3, showing that the applied sum compared to the maximum was actually lower for unsuccessful applicants. Although the share of co-financing was similar for successful and unsuccessful applicants, the latter group typically applied for larger co-financing (Fig. 2). This is a consequence of the fact that programs with larger maximum grant size typically also required less co-financing. We will investigate whether the differences in the applied sum between the two groups matter for our results in Panel C of Table 5 by matching also for the applied sum. This methodological change does not affect the results, suggesting that our strategy controls for the relevant differences between successful and unsuccessful applicants.

Table 2 compares firms that never applied with the pre-application characteristics of firms which applied unsuccessfully and successfully.[17] Applying firms are more likely to belong to industrial sectors than those that never apply: the share of industrial firms is 21 percent among never applying firms, 30 percent among unsuccessful and 36 percent among successful applicants.[18] They are also larger – in terms of assets, employment, and sales – than those that never applied, while the difference between successful and unsuccessful applicants is rather small. For example, the average number of employees is 14.6 in the group that never applied, 27.4 in the year before rejection for unscuccessful applicants, and 29.5 before winning a grant. Firm growth, measured by the change of log tangible assets for 3 year periods, is 7 percent for non-applying firms and around 50 percent for applicants, regardless of their success. Applicants also have higher productivity than companies that never apply. (Normalized) TFP is slightly negative for the bulk of firms that never applied, 0.062 for unsuccessful and 0.103 for successful applicants.[19]

The bottom part of the table presents the means and standard deviations for worker-level variables. About one-third of the workforce is skilled, regardless of application status.[20] Never applying and rejected firms pay similar wages to their employees, while winners pay higher wages by about 10 percent for both skilled and unskilled workers.

We construct two types of variables to investigate the role of experience in applying. On the firm side, we construct dummy variables that indicate whether they had applied (either successfully or unsuccessfully) in the past. Based on worker data, we also construct two firm-level dummy variables indicating that the company has at least one skilled employee in a given year who was employed by another firm that applied/won in the past.[21] As the bottom two lines of the table show, firms with more workers who have such an experience are indeed more likely to apply. Although the differences between successful and unsuccessful applicants are not large, especially compared to the difference between never applying and applying firms, they are nevertheless statistically significant, as the last column of Table 2 demonstrates.

To provide a clearer picture of the selection mechanisms of application and subsequent winning, we run probit models to obtain the partial effects of variables on the probability of applying and winning. The estimated marginal effects and the corresponding standard errors are presented in Table 3 and reinforce the results suggested by the unconditional statistics. In columns (1) and (2), the dependent variable is a dummy that shows whether the firm applies next year.[22] In line with our theory on self-selection into paying the fixed cost of the application, larger, faster-growing, and more productive firms were more likely to apply. Based on the firm-level regression presented in column (1) of the table, a firm situated at the 75th percentile of the size distribution measured by employment is 3.6 percentage points more likely to apply than one at the 25th percentile (the other explanatory variables are kept at their mean value). As the mean of the dependent variable is 0.044, the probability of a larger firm applying is 82 percent higher than that of a smaller one. We get positive effects, albeit smaller ones for sales and productivity: for investment, this difference between the 25th and 75th percentiles is 16 percent and for TFP 11 percent. The proxies associated with previous experience show that prior application and winning are indeed positively associated with applying again, and these effects are large.[23]

We investigate the probability of winning in columns (3) to (5) of Table 3, where the dependent variable indicates whether the firm applied successfully and we drop firms that never applied. As the dependent variable is in the range of 0.7–0.8 (compared to 0.04 in application regressions), the proportional differences induced by the regressors are small. For example, the difference between a firm with its TFP at the 75th percentile of the distribution compared to another at the 25th percentile is a mere 3 percent. The applied sum (in column (5)) has a negative coefficient, showing that firms which apply for large grants (75th percentile of the grant size distribution) are 20 percent less likely to win compared to firms that apply to small grants (at the 25th percentile). This relatively small effect is likely to arise from the lower success rate in subprograms that offer larger grants.

These results highlight that, despite the positive self-selection of firms to apply, the decision-making process of SME applications is rather formal and does not depend much on the performance of the firm. This fact supports the argument that unsuccessful applicants can form a sensible control group.

17 The definitions of the variables are in Online Appendix Table A2. We restrict the sample to the years after 2004 (the first year when firm subsidies were available in Hungary) and include all firm-years of never applying firms and the year prior to application of the other two groups. Note that firms that filed multiple applications contribute more than one year and they may be included both in the rejected and successful groups with different firm-years.

18 In Online Appendix Table A3 we provide a detailed sectoral distribution by application status, which reveals that applying firms are especially likely to be in heavy industry while they have lower proportions in all the service sectors, relative to never applying firms.

19 Our preferred measure for TFP is the one estimated with the Levinsohn–Petrin method (Levinsohn and Petrin, 2003) using a value-added production function, but the results are similar if we use other popular estimators including those proposed by Wooldridge (2009) and Ackerberg et al. (2015).

20 As we lack data on the education of workers, we define skilled workers as those whose occupation belongs to the three highest ISCO categories (managers, professionals,technicians and associate professionals). All other occupations are classified as unskilled.

21 We focus on skilled workers as they are more likely to work with applications.

22 For ease of comparison across samples and to prevent large firms driving the results, we weight the worker-level sample with the inverse of the number of workers observed in a firm-year. Regressors include a measure of firm size (log employment), growth (the growth of tangible assets in the two years preceding the application), and firm productivity (TFP). In the worker-level data, we also add the log of wages. We add two proxies of the fixed costs of applying: whether the firm filed a grant application previously (successful or not) and whether there are skilled workers in the firm who came from a company that applied for a grant (successfully or not). In column (5) we also include the log of the applied sum for firms when this variable is available. We cluster the standard errors at the firm level. As a robustness check, we defined the dependent variables such that they equal one in the years 1, 2 and 3 years before the application. The results are robust to this change.

23 If the firm filed a grant application before, its chances of filing another one are larger by 6.3 percentage points or 143 percent while a successful application adds another 2.6 percentage points (59 percent). To turn to worker-level variables (presented in column 2 of the table), the presence of at least one skilled person in the company who was employed previously in a firm that applied also has a large effect on applying (17 percent), but it does not matter whether the firm won or was rejected. This is plausible because, regardless of the outcome, a person involved learns to apply.

Table 3
Selection of firms into application and winning.

	(1) Applied	(2) Applied	(3) Winner	(4) Winner	(5) Winner
Employment	0.021***	0.017***	0.003	0.002	0.025***
	(0.000)	(0.001)	(0.004)	(0.005)	(0.006)
Investment	0.010***	0.007***	0.006	−0.001	0.014***
	(0.000)	(0.000)	(0.004)	(0.003)	(0.005)
TFP	0.008***	0.009***	0.036***	0.040***	0.046***
	(0.000)	(0.001)	(0.007)	(0.008)	(0.009)
Appl. Before	0.039***	0.038***	0.037**	0.038**	0.041**
	(0.002)	(0.002)	(0.015)	(0.016)	(0.016)
Winner Before	0.021***	0.022***	0.042***	0.042**	0.047***
	(0.002)	(0.002)	(0.016)	(0.018)	(0.018)
Wage (log)		0.000		−0.003	
		(0.001)		(0.005)	
Person Appl. Before		0.008**		−0.003	
		(0.004)		(0.027)	
Person Winner Before		−0.007		0.006	
		(0.004)		(0.032)	
Grant Size					−0.045***
					(0.004)
Sample	Firm	LEED	Firm	LEED	Firm
Method	Probit	Probit	Probit	Probit	Probit
Observations	303,005	2,122,641	13,223	166,832	9,199
Mean Depvar	0.044	0.048	0.699	0.687	0.782

Note: Firm-year observations in columns (1), (3), and (5), worker-year observations in columns (2) and (4). The table reports marginal effects based on probit models. Dependent variable: the firm applied the following year (columns (1), (2)) or won a grant the following year (columns (3)-(5)). When the dependent variable indicates applying, the sample consists of all firm-years for never applying firms and the year precedent to application for applying firms. When the dependent variable indicates winning, the sample consists of the year precedent to application. Wage, Person Applied/Winner before are individual-level variables. Firms that applied multiple times are included with all years precedent to application. Year dummies are included in all specifications. Standard errors clustered at the firm-level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

5. Econometric approach

To study the effect of grants on firm outcomes, we rely on methods borrowed from the treatment effects literature. Our main approach is a difference-in-differences strategy to estimate treatment effects with successful applicants as the treated group and unsuccessful applicants as the control group.[24] Using this control group rather than all non-winning firms clearly addresses two interconnected biases arising from the self-selection of firms into application: applying companies' needs for finance, resulting from their quicker growth and good investment ideas, and the willingness to pay the fixed cost of application.

More precisely, we restrict the sample to firms that applied for grants in our sample period. We call this sample *applicant sample*.

Although a number of firms applied multiple times, in our main specification, we use a single treatment approach and focus on the first application of always unsuccessful applicants and the first successful application of firms which applied successfully at least once. We denote the time of this *relevant application* by t_0^i for firm i.[25]

Firm i in year t is considered treated ($win_{it} = 1$) if the firm is a successful applicant and $t \geq t_0^i$. If the firm never applied successfully or only in a later year, the firm-year is untreated. Based on this definition, we define event time dummies (ζ_τ) for every firm in the application sample to capture trends around the relevant application year.[26] ζ_τ takes the value of one if $t - t_0^i = \tau$. For example, ζ_{-3} equals one 3 years before the relevant application. To capture the long-term effect with appropriate power, we extend ζ_4 to be one in all years $t \geq t_0^i + 4$ — practically we assume that the effect remains constant after 4 years.

Our benchmark specification is the following:

$$Y_{ikt} = \gamma^{win} \times win_{it} + \sum_{\tau=-4}^{+4} \zeta_\tau + \delta_{kt} + \nu_i + \varepsilon_{ikt}, \tag{1}$$

[24] We experimented with several settings for a regression discontinuity (RD) design. We used Central Hungary's borders, since that region is more developed than the other regions and therefore receives smaller amounts from the Structural and Cohesion Funds. These regressions, however, did not have enough power, as Hungarian government programs 'mirror' the EU financed grants in Central Hungary. At the other end of the distribution, grant conditions are less demanding in the most underdeveloped regions. But, again, the RD design did not have enough power because very few firms applied from these areas.

[25] As we demonstrated above, the likelihood of winning is correlated with previously successful application and so later successful applications reinforce the treatment. In the matching procedure, we make direct use of subsequent applications as well. In Section 6.3 we study the heterogeneity between the first and second applications.

[26] Note that without information on unsuccessful applications, controlling for common trends is not possible. At the end of this section, we show how pre-treatment trends vary by the sample (application or matched) and controls for common trends.

where i, k and t index firms, industries, and calendar years, respectively. Y_{ikt} is the dependent variable. To control for common sectoral or price shocks, we add to the equation a set of 2-digit industry-year dummies (δ_{kt}). We also add the common event time dummies for winners and losers centered around the relevant application to capture common growth patterns. Finally, we control for firm fixed-effects ν_i to partial out any differences between control and treated units that are fixed over the observation period. Our variable of interest is win_{it}. The coefficient associated with it (γ^{win}), measures the growth of the outcome variable relative to the period before treatment while controlling for its growth in the control group.

To estimate the evolution of the treatment effect over time, we extend the baseline specification by replacing win_{it} with event study variables (win_{it}^{τ}), defined similarly to the ζ_τ event study dummies, but take the value of one only for firms that received a grant:

$$Y_{ikt} = \sum_{\tau=-4}^{+4} \gamma_\tau \times win_{it}^{\tau} + \sum_{\tau=-4}^{+4} \zeta_\tau + \delta_{kt} + \nu_i + \varepsilon_{ikt}, \tag{2}$$

where the set of γ_τ coefficients shows how the outcome variable differs between the treated and control firms τ years before/after the application.

In this difference-in-differences setting, successful and unsuccessful applicants may still differ in their unobserved time-varying characteristics. To alleviate this problem, we apply a matching strategy to make treated and control groups as similar as possible in the pre-application period. In particular, after trimming the data for a 9-year window around the application to generate "application windows" to make the time series of the control and treated firms more similar, we perform exact matching of these windows based on three variables: (i) the year of application, t_0^{im}; (ii) the 2-digit industry of the firm (k); and (iii) the quartile of investment (measured as the change in ln tangible assets) between $t_0^{im}-4$ and $t_0^{im}-1$.[27] Finally, we conduct a propensity score kernel matching based on pre-application characteristics within these year-industry-growth quartile cells. The details of our matching procedure are included in Online Appendix D.[28]

The matched sample has 1,528 treated and 943 control application windows.[29] The standardized difference of the key variables between the treated and control sample means is never larger than 0.04, with the exception of the sum applied for, where it is 0.3 (see Online Appendix Table A6).[30] To assess the external validity of the estimations performed on the matched sample, we compare the matched sample with the application sample in Online Appendix Table A7. The means of the variables are very similar in the two samples, suggesting that the matched sample is not very different from the population of applications, and so the matched results are likely to be applicable to the whole sample.

5.1. Identification

As in any difference-in-differences specification, our identification assumption is based on a version of the parallel trends assumption. The estimated coefficients are unbiased only if the growth and performance dynamics of successful applicants in the matched sample would have been similar to unsuccessful applicants, had they not won a grant. This assumption is violated if firms with stronger growth plans are more likely to win grants, conditional on applying. In this section, we present the methods that we use to attenuate the potential bias in our regressions.

The treated and control groups may differ in unobservables. In order to investigate this, we check for pre-trends in a number of variables, which were not used in the matching procedure. First, we were concerned that if more successful applicants had been more profitable before applying, they could have secured more financing for investments. We proxy for this with the return on assets (ROA). Second, exporting firms may follow different growth strategies than firms that only serve the domestic market. We investigate pre-trends in export status, which is often considered a good proxy for firm capabilities and is also correlated with worker quality (see e.g. Verhoogen, 2008) and innovation (Golovko and Valentini, 2011). Finally, firms with different intensity of knowledge or intangible assets may have different investment and growth trajectories. We proxy for this with the share of intangible assets among assets (Arrighetti et al., 2014; Cucculelli and Bettinelli, 2015). We do not find evidence for pre-trends in any of these variables (Online Appendix Table A8).

We also investigate the robustness of our results by applying an instrumental variables strategy. We exploit the variation in the share of successful applicants between years within different subprograms.[31] In particular, for each subprogram-year, we calculate the share of successful applications and subtract the mean share of successful applications for that subprogram across the different years and use this variable as an instrument for the firm-level win variable. This variable is clearly correlated with our variable of interest (winning a grant), and it is unlikely to be correlated with unobserved firm characteristics. First and most important, it is defined at the sub-program level and so it is external to each applying firm (by normalizing at the sub-program level, we also handle

[27] Using quartiles of other variables (e.g., output) and growth measured at shorter period (2 years) yields essentially to the same results.

[28] Our approach combines exact matching as a first step with propensity score matching as a second step. Exact matching is often considered the "ideal" method, as it generates a balance of covariates automatically (Imai et al., 2014), while propensity score matching can, in some cases, exacerbate imbalance (King and Nielsen, 2019). The combination of the two methods often used in the literature (see e.g. Stuart, 2010).

[29] Online Appendix Table A5 shows how the size of the control and treatment group changes by the main steps of data manipulation, as well as the pre-treatment values of size and productivity.

[30] As a rule of thumb, a standardized difference under 0.25 is acceptable (Imbens and Wooldridge, 2009). Note that the sum applied for pertains to different samples in the control and treated groups. Restricting the sample to the years when this variable is available leads to a standardized difference of 0.14.

[31] Note that there are more than 1,000 subprograms in our database, each of them representing quite heterogeneous calls.

the possibility that firms may self-select themselves into different subprograms). Second, based our analysis of the institutions, a key determinant of this share is the amount of funding available, which is driven by the EU budget cycle and pre-agreed priorities and not firm-level developments. Finding similar results with this instrument to our main specification (Table 5) provides further evidence that our results are unlikely to be driven by endogenous winning.

6. Policy effects

In this section, we present our empirical results. We start with the firm-level outcomes, followed by the effects of the grant on worker composition and wages. Next, we examine the heterogeneity of the effects. Finally, we discuss the macroeconomic effects of the grant on aggregate employment and productivity.

6.1. The effects of grants on firm outcomes

Table 5 reports the main results of the estimation Eq. (1) for the key outcome variables. Panel A provides the estimated coefficients for the applicant sample, while Panel B shows the results for the matched sample. The effects are similar, but the magnitude of the estimates is slightly smaller on the matched data. Using the latter, more conservative estimates, we find that relative to unsuccessful applicants, assets grew by 28 percent in the following three years, employment by 13 percent and, as a result, firms became 14 percent more capital intensive.[32] Sales grew by 17 percent and labor productivity by 6 percent. The coefficient for TFP is 0.023 and it is not significant even at the 10 percent level.[33] The instrumental variable estimation (Panel D) yields very similar estimates to our main specification.[34]

A potential concern with these results is that successful applicants may apply for different sums compared to unsuccessful ones, which affects their counterfactual growth. To check for this view, Panel C shows the estimated effects from a matched sample where we also match on the applied sum.[35] These results are very similar to the preferred specification, but the point estimates are somewhat larger (except for the effect on labor productivity, which is smaller and loses its statistical significance).

Fig. 3 shows the coefficients of the dynamic specification outlined in Eq. (2). In this specification, pre-trends completely disappear.[36] Moreover, there is a clear trend break in the event time dummies around t_0, showing that successful applicants started to grow faster relative to the control group right after they received the grant. The figure also shows that capital, employment, and output continuously grew during the post-grant period. There is an increase in capital intensity in years 0 and 1 without further improvement, which we interpret as a signal for switching into a more capital intensive production mode. Labor productivity increases by 7 percent following the grant, but it has a fallback in year 3 which is caused by sales growth petering out with employment still on a growth trajectory. TFP decreases slightly before the grant followed by a non-significant increase, giving little evidence for substantial improvement.

From a methodological angle, it is of interest to compare the pre-trends of the different estimators. Table 4 presents the pre-trends for various estimation methods from the benchmark specification (we use the log of tangible assets as the dependent variable, but the results are similar to the other dependent variables). We start with the full sample (including non-applicants), where we cannot add common trends. Next, we switch to the applicant sample and perform the regressions with and without the common trends. Finally, we use the matched sample with the common trends. The table reveals that switching to the applicant sample from the full sample reduces the pre-trends by about one-third, but they still remain large and statistically significant. The inclusion of common trends into the regression on the applicant sample mostly eliminates the pre-trend, while our preferred specification, the joint use of matching and common trends, completely eliminates them. These comparisons reveal that the key advantage of having information on unsuccessful applicants is that it allows one to capture the common trend of all applicants before applying, that is, their higher growth and investment levels.[37]

These results are in line with the predictions of the ''more of the same'' view from our model, in which the fall of the marginal cost of capital leads to increased investment, capital intensity, and labor productivity. Importantly, the results do not support the alternative views. The large and persistent effect on capital shows that the grant does not only crowd out market financing. The increase in sales and employment suggest that the investment is productive and does not only represent channeling out the money from the firm, while the flat TFP pattern does not indicate radical reorganization or technology upgrading.[38]

32 From now on, we present only the results based on the matched sample, and we discuss the results from the applicant sample in footnotes only. The estimated coefficients of the applicant sample are in Appendix B.

33 The estimates on the applicant sample are: 36 percent increase in capital, 19 percent increase in employment, 16 percent increase in capital intensity, 6 percent increase in labor productivity and no significant effect on TFP.

34 The coefficient (standard error) of the instrument in the first-stage regression equals 0.312 (0.005) and the F-statistic equals 2976.98. None of the coefficients differ significantly from our main specification.

35 As we discussed in Section 3.1, this variable is only available for a selected subset of firms and so this sample is smaller than our preferred matched sample.

36 Online Appendix Figure B1 shows the coefficients from the dynamic specification on the applicant sample. The figure suggests slight pre-trends at most, showing that focusing on applicant and the event study specification eliminates most of the pre-trends present in the original data.

37 Note that, according to a comparison of Panels A and B, even the slight pre-trend in the applicant sample affect the point estimates substantially indicating that the matching step matters regarding the point estimates.

38 The statistical tests formally reject the hypotheses that there is complete replacement ($g < h$) or complete channeling out ($\gamma = 0$). While it is easily possible that some replacement and channeling out takes place, the magnitudes of the estimates suggest that these are far from complete. The large capital effect shows that there is indeed substantial additional investment. The findings that sales and employment growth are of the same magnitude as capital growth suggest that channeling out is also far from complete.

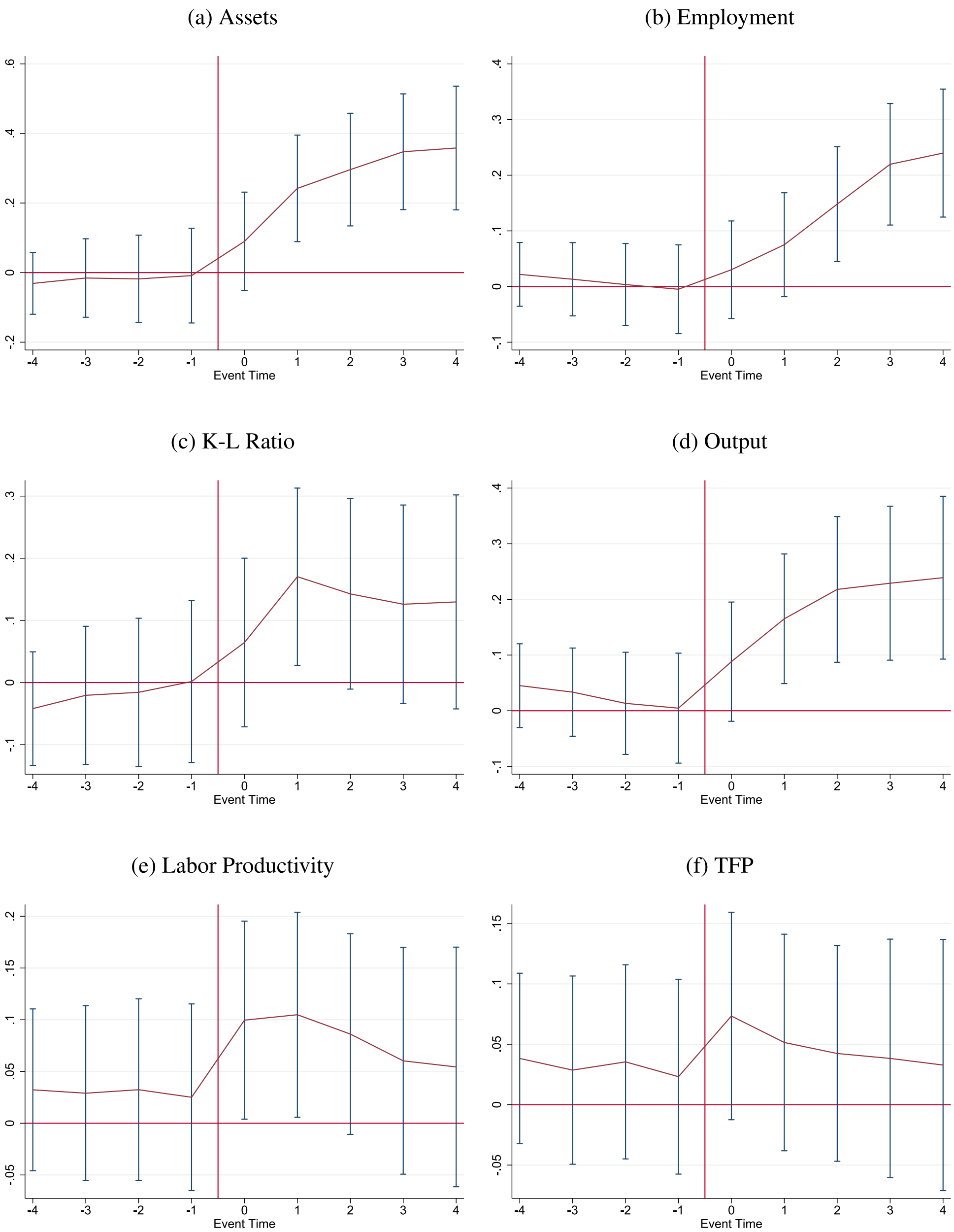

Fig. 3. Event-Time regressions: Dynamic effects of the subsidy. Notes: N = 24,088 firm-years (23,636 in the LP and TFP regression). The figure presents the estimated coefficients and the 99-percent confidence intervals of the event study regressions of Eq. (2). The main explanatory variables are event study dummies around winning the grant. The regressions are performed on the matched sample. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm fixed effects. Standard errors are clustered at the firm level.

Our results are quite similar to those of other authors studying similar schemes. Criscuolo et al. (2019) study subsidies in the UK and find positive employment and investment effects, but no effect on TFP. Their preferred IV estimate for within-firm employment growth is 4.6 percent per 10 percentage point increase in maximum subsidy rate, and they find slightly smaller effects for turnover. In comparison, in our case, we find an 11 percent increase of employment for grants with a typical investment intensity of 50 percent.

Table 4
Comparison of pre-treatment trends in log assets with various samples and methods of estimation.

Year	Full sample	Applicant s.	Applicant s.	Matched sample
−4	0.249***	0.085***	0.030	−0.031
	(0.014)	(0.015)	(0.029)	(0.034)
−3	0.334***	0.122***	0.050	−0.015
	(0.016)	(0.018)	(0.032)	(0.044)
−2	0.440***	0.179***	0.055	−0.018
	(0.017)	(0.021)	(0.035)	(0.049)
−1	0.600***	0.294***	0.073**	−0.009
	(0.018)	(0.023)	(0.037)	(0.053)
Common Trends	No	No	Yes	Yes
N	578,532	119,189	119,189	24,088
R-squared	0.815	0.779	0.780	0.844

Notes: Firm-year observations. Dependent variable: log tangible assets. This table reports the estimated coefficients (standard errors) associated with 4, 3, 2 and 1 years before receiving the subsidy. All regressions include yearly treatment dummies, industry-year controls and firm fixed-effects. Post-treatment dummies are also included in the regression but not reported here. Full sample = all firms from the sample; Applicant sample = firms that applied during the observation period; Matched sample = successful and unsuccessful applicants matched with exact and propensity score matching methods. Common trends = Event year dummy variables added as controls around the application year for the grant. Standard errors clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

6.2. The effects of grants on workers

We adapt the identification strategy used so far to the worker-level database and compare the worker outcomes in successful and unsuccessful applicants.[39] We run these regressions at the worker, rather than the firm, level to account for possible changes in the composition of the workforce. To keep these results comparable to those based on firm-level regressions, we weight each worker-year observation with the inverse of the number of workers in the firm. This way each firm-year gets an equal weight in the regression, and we prevent large firms driving the results. We study two types of variables: the proportion of workers in various skill groups and the corresponding wages. When the dependent variable is the individual's wage, we add to the controls a set of 2-digit occupational codes, and in the wage regressions we replace firm fixed-effects with worker fixed-effects.[40]

First, we look at the shares of various types of workers: skilled workers (further divided into managers and professionals) and unskilled workers (divided into medium and low-skilled).[41] As Panel A of Table 6 shows, the proportion of different types of workers does not change significantly, with the share of skilled workers remaining exactly the same after receiving the grant.[42] Given that technology upgrading is likely to be skill biased in emerging markets including Hungary (Lindner et al., 2019), this result is in line with the "more of the same" view.

Wages, however, do increase according to Panel B of the Table. The wage of low- and medium-skilled workers increases 4.8 and 2.6 percent but the point estimates are statistically insignificant, while the wage of skilled workers increases by 10.3 percent. There appears to be a difference within the skilled group, with managers receiving a higher raise (15 percent) compared to the 10.5 percent increase for professionals. This is in line with the model and with skilled workers, especially managers, having higher bargaining power within the firm, and so they are able to capture a higher share from rents associated with cheap capital.

6.3. Heterogeneity of the effect: Policy parameters, firm characteristics and macroeconomic conditions

In this subsection, we add two policy parameters to the analysis to investigate how the effect of grants varies across firms: what is the effect of multiple grants and how does the effect depend on the size of the grant (relative to the firm's capital stock). We also study the heterogeneity of the effect by firm size, industry, and the skill level of its workforce. Finally, we test whether

[39] One difference in data coverage is that we can only link workers to a subset of firms, as we described in Section 3. This reduces the number of matches in the matched sample to 280 treated firms and 189 control firms. The propensity score estimation is in Online Appendix Table A4, the balancing tests in Online Appendix Table A6. The standardized difference of variables between the control and treated firms pre-treatment are all smaller than 0.1 with the exception of the sum requested where this statistic equals 0.19. Online Appendix Table A7 compares the full sample with the matched sample and reveals that the matched firms are somewhat larger, more productive, but have the same ratio of skilled workers and pay similar wages.

[40] We do not control for worker fixed-effects when the dependent variable is the skill of the worker because the effect of the grant on the skill composition of the firm would be identified only from workers who were in the firm already before the grant was obtained and switched occupations during their stay with the firm.

[41] The 2-digit ISIC code occupations between 40 and 70 are classified as medium skilled and the codes equal to 80 and 90 are the low skilled.

[42] We also estimated the effect of the grant on hiring and separation rates, and did not find any effect.

Table 5
The effects of the grant on firm outcomes.

Panel A: Applicant Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.360***	0.193***	0.162***	0.249***	0.060***	0.013
	(0.028)	(0.018)	(0.025)	(0.023)	(0.014)	(0.012)
Observations	119,189	119,189	118,019	119,189	116,071	116,071
R^2	0.780	0.743	0.720	0.780	0.581	0.447

Panel B: Matched Sample

Dependent:	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.277***	0.132***	0.142***	0.166***	0.058***	0.023
	(0.040)	(0.025)	(0.036)	(0.030)	(0.022)	(0.019)
Observations	24,088	24,088	23,988	24,088	23,636	23,636
R^2	0.844	0.837	0.779	0.872	0.655	0.523

Panel C: Matched Sample (Matching with Applied Sum)

Dependent:	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.317***	0.195***	0.128**	0.207***	0.042	0.030
	(0.061)	(0.038)	(0.054)	(0.049)	(0.032)	(0.031)
Observations	8,105	8,105	8,082	8,105	7,919	7,919
R^2	0.853	0.849	0.790	0.880	0.701	0.595

Panel D: Instrumental Variable Estimation

	(1)	(2)	(3)	(4)	(5)	(6)
	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.429***	0.237***	0.192***	0.266***	0.034	–0.015
	(0.042)	(0.028)	(0.036)	(0.033)	(0.022)	(0.019)
Observations	119,189	119,189	118,019	119,189	116,071	116,071
R^2	0.041	0.023	0.015	0.026	0.005	0.002

Notes: Firm-year observations. This table reports the estimated coefficients (standard errors) associated with the dummy variable = 1 in the year and after the firm won its first grant (see Equation (1)) for the different dependent variables. Regressions in Panel A and D are based on the applicant sample, in Panel B on the main matched sample, in Panel C on an alternative matched sample, when the sum applied for is included in the matching. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm fixed-effects. Instrument in Panel D: the share of winners in operative subprograms (Coefficient (se) on the IV in first stage = 0.312 (0.005); F-statistic of first stage = 2976.98). Standard errors clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

macroeconomic conditions alter the effect by estimating separate effects for firms that applied before or during the 2008/2009 Great Recession.

As we showed in Online Appendix Figure A1, one third of the winners won at least two grants. To study how winning multiple grants affects performance, we replace the variable of interest in the baseline regressions (Eq. (1)) with the following three variables. The 'one grant' dummy indicates winning a single grant during the analyzed period. The 'first grant' dummy indicates the first grant of firms that win multiple grants, while the 'second grant' dummy switches to one when these firms win their second grant. Comparing the coefficient of the 'one grant' dummy with that of the 'first grant' dummy shows whether the effect of a single grant is larger than that of the effect of the first grant in a firm with multiple grants.

Table 7 shows the results of this exercise. Despite the fact that the effect of the first grant on tangible assets is practically identical for firms that apply once or multiple times (21 percent), the other outcomes are much larger for those firms that apply for another grant later on: the employment effect is 1.5 times, and the output effect about twice as large, resulting in a productivity effect of 8 percent. This is in sharp contrast with the single grant winners' 4 percent productivity result. Not only is labor productivity growth larger for multiple winners, but here we measure a positive TFP effect of 4.6 percent. The second grant increases capital by 37 percent and also has large employment and output effects. However, the large-scale effect is not accompanied by any positive productivity effect for the second successful grant.

One potential reason for this finding is experimentation: if the first grant leads to great positive changes in firm outcomes, the owners of the firm are more likely to apply for another grant. This suggests that restricting firms from filing multiple applications would not improve the effectiveness of these policies.

Panel A of Table 8 investigates heterogeneity by grant size and firm size. We capture grant size with a dummy showing whether the ratio of the applied grant and the tangible capital stock of the firm is larger than 10 percent and firm size with another dummy showing whether the firm has more than 25 employees. We augment Eq. (1) with an interaction term between win_{it} and these

Table 6
The effects of the grant on employment composition and wages.

Dependent:	Skilled	Manager	Professional	Med. Sk.	Low Sk.
Panel A: Employment					
Winner	0.001	−0.025	0.026*	0.012	−0.014
	(0.017)	(0.017)	(0.014)	(0.017)	(0.013)
Observations	55346	55346	55346	55346	55346
R-squared	0.287	0.157	0.305	0.302	0.317
Panel B: Wage					
Winner	0.103**	0.151**	0.105**	0.026	0.048
	(0.042)	(0.063)	(0.048)	(0.026)	(0.033)
Observations	14,581	5,543	8,876	26,145	8,404
R-squared	0.861	0.863	0.872	0.838	0.874

Notes: Worker-year observations. This table reports the estimated coefficients (standard errors) associated with the dummy variable = 1 in the year and after the firm won its first grant and the dependent variable is the type of occupation of the worker (Panel A) and the log wage (Panel B). Regressions are based on the matched sample and they are weighted by the inverse of the number of workers in a firm-year. The regressions in Panel A are run on all the workers in the sample and the regression in Panel B are restricted to workers with the different occupation types. All regressions include industry-year controls and event year dummy variables around the application year. In addition, regressions in Panel A include firm fixed effects and 2-digit occupational effects and in Panel B worker fixed effects. 'Skilled' refers to workers with ISCO code 1, 2, 3. Standard errors clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table 7
Single and multiple winners.

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
One grant	0.210***	0.082***	0.127***	0.097***	0.039	0.015
	(0.044)	(0.027)	(0.040)	(0.033)	(0.024)	(0.022)
First grant	0.206***	0.132***	0.068*	0.198***	0.082***	0.046**
	(0.044)	(0.028)	(0.040)	(0.033)	(0.024)	(0.021)
Second grant	0.369***	0.146***	0.221***	0.130***	−0.000	−0.030
	(0.035)	(0.021)	(0.035)	(0.025)	(0.022)	(0.019)
R-squared	0.845	0.838	0.779	0.873	0.655	0.523

Notes: N = 24,088 (23,626 when Labor Productivity and TFP are the dependent variables). Firm-year observations. The regressions are based on the matched sample. The table presents the estimated coefficients (standard errors) from Eq. (1) where the grant variable was replaced by three dummy to show the variation of the grant effects by single-multiple grant winning (Panel A) and the size of the grant relative to the firm's tangible assets (Panel B). "One grant" = 1 in the year and after the firm won a grant for firms that received one grant; "First grant" = 1 in the year and after the firm won its first grant for firms that received multiple grants; "Second grant" = 1 in the year and after the firm won its second grant. Intensity of treatment is represented by three categories showing whether the grant was below 5 percent, between 5–20 percent or above 20 percent of the firm's pre-subsidy assets, interacted with the treatment variable. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm fixed effects. Regressions in Panel B include interaction terms (intensity variables with post-treatment trends, log employment with treatment and event time trends). Standard errors clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

dummies to investigate whether the effects are heterogeneous along these dimensions and we also control for separate common trends after winning large grants and large firms separately.

As expected, larger grants initiate larger investment: the coefficient on the main effect equals 0.15 (marginally significant), while for the interaction term with large grants it is 0.34 (highly significant). We did not find evidence of heterogeneous effects in terms of any of the other outcome variables. From a policy point of view, this implies that smaller grants may be more efficiently absorbed by the economy.[43] We do not find any evidence of different effects between smaller and larger firms when we control for grant intensity.

Panels B and C show the estimated heterogeneity of the grant effect along two variables. In these specifications we also control for the large grant dummy, which proved to be important in Panel A. Service firms have smaller grant-induced growth rates of assets and employment, showing that the grant is more likely to replace market financing for service firms.[44] Firms applying in the

[43] A regression where the treatment dummy is interacted with the value of grant intensity leads to similar conclusions (see Online Appendix Table A9).

[44] Service firms also experience larger productivity effects, even though the productivity effects estimated for service firms are not significantly different from zero (Panel B). For services, the point estimate of the labor productivity effect is 0.43 and that of the TFP effect is 0.058, with standard errors between 0.07–0.08.

Table 8
Heterogeneity of grant effects: grant and firm attributes and macroeconomic conditions.

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Panel A: Grant Size, Firm Size						
Winner	0.151*	0.203***	−0.043	0.273***	0.008	0.024
	(0.092)	(0.069)	(0.082)	(0.079)	(0.060)	(0.060)
Winner x Large Grant	0.341***	−0.018	0.359***	−0.122	0.033	−0.017
	(0.118)	(0.079)	(0.107)	(0.096)	(0.068)	(0.065)
Winner x Emp > 25	−0.043	0.016	−0.074	0.006	0.062	0.059
	(0.132)	(0.089)	(0.135)	(0.103)	(0.064)	(0.060)
R-squared	0.855	0.851	0.793	0.880	0.702	0.596
Panel B: Economic Sector						
Winner	0.424**	0.381***	0.044	0.294***	−0.076	−0.072
	(0.180)	(0.095)	(0.183)	(0.107)	(0.085)	(0.077)
Winner x Service	−0.314*	−0.187**	−0.124	−0.019	0.119	0.130*
	(0.182)	(0.093)	(0.183)	(0.103)	(0.081)	(0.072)
R-squared	0.855	0.850	0.793	0.880	0.702	0.596
Panel C: Crisis						
Winner	0.130	0.197**	−0.063	0.280***	0.073	0.087
	(0.120)	(0.077)	(0.107)	(0.085)	(0.066)	(0.066)
Winner x Appl. in Crisis	0.013	0.023	−0.010	−0.008	−0.075	−0.073
	(0.126)	(0.079)	(0.114)	(0.096)	(0.069)	(0.066)
R-squared	0.855	0.850	0.793	0.880	0.702	0.596
Panel D: Skilled workforce						
Winner	0.163	0.245***	−0.078	0.281***	−0.006	0.021
	(0.109)	(0.069)	(0.111)	(0.083)	(0.062)	(0.062)
Winner x Skilled Workforce	−0.020	−0.031	0.013	−0.012	0.015	0.008
	(0.129)	(0.078)	(0.120)	(0.101)	(0.068)	(0.064)
R-squared	0.852	0.849	0.789	0.882	0.689	0.589

Notes: Firm-year observations. This table reports the estimated coefficients (standard errors) associated with the dummy variable = 1 in the year and after the firm won its first grant (see Equation (1)) for different dependent variables. The treatment variable is interacted with dummies indicating grants larger than 10 percent of tangible assets, firms with employment > 25 in the year before the application, the service sector, years after 2008 and a dummy indicating that the proportion of skilled labor is higher than the median before the application. All regressions include industry-year controls, event year dummy variables around the application year for the grant, firm fixed-effects, a separate post-trend of high-intensity grants and separate post-trends for the dummy variables interacted with the treatment. Standard errors clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table 9
The contribution of grantees to SME sector employment growth.

Period	Cohort	Initial share	Actual growth	Counterfactual growth	Contribution of grant (relative to initial SME emp.)
2002–2008	2004–2005	3.9%	35.6%	18.8%	0.7%
2005–2011	2006–2008	7.6%	20.8%	5.8%	1.1%
2008–2014	2009–2011	14.2%	16.3%	1.9%	2.0%

Notes: This table decomposes shows contribution of subsidized firms to employment growth. The rows show the different (overlapping) periods. The “initial share” column shows the initial labor share of grantees in the given cohort. “Actual growth” shows the realized growth of this set of firms. “Counterfactual growth” shows the employment growth of these firms if they had not received the grants based on our preferred estimates in 5, Panel B. The last column shows the contribution of the grant to total SME employment growth.

crisis years, captured by an indicator showing that the grant was received after 2008, do not demonstrate larger growth rates than those which applied in pre-crisis years (Panel C). This may be because, even though financing constraints became more substantial in this period, investment opportunities also declined. Finally, in Panel D we estimate heterogeneous effects by the skill level of the workforce. We define a skilled workforce by the average number of skilled workers in the company prior to application. The dummy equals one if this variable is larger than the median in all firms (0.22).[45] We find again that the estimated effect of the subsidy does not differ between these two types of companies.

6.4. Grants, job creation and productivity

The previous sections demonstrated that EU grants had a significant impact on firm-level growth and labor productivity. In this subsection, we quantify how many jobs were created as a result of the grant in grant-winning firms and how much these firms

[45] As we control for industry effects, the skill dummy is measured relative to the average skill level within the industry.

Table 10
Aggregate labor productivity growth decomposition of the SME sector.

Period	Cohort	Total	Grantee	Counterfactual	Difference
2002–2008	2004–2005	15.11%	0.56%	0.11%	0.45%
2005–2011	2006–2008	10.29%	1.95%	0.91%	1.04%
2008–2014	2009–2011	14.79%	3.11%	1.33%	1.79%

Notes: This table decomposes the labor productivity growth of the Hungarian SME sector based on the method in Foster et al. (2008). We describe our decomposition methodology in detail in Online Appendix E. The rows show the different (overlapping) periods. Total is total real labor productivity growth in the SME sector in that period. Grantee contribution shows the contribution to this productivity growth of firms winning grants in the first half of the period (in the years shown in the cohort column). Counterfactual shows the contribution of these firms if they had not received the grants based on our preferred estimates in 5, Panel B. The difference column shows the difference between the actual and counterfactual contributions. For example, between 2005 and 2011 productivity growth was 10.29 percent in the SME sector, from which 1.95 percentage points were contributed by firms which won a grant in 2006, 2007 or 2008. In the absence of grants, their contribution would have been 0.91 percentage points. The difference between these two numbers is 1.04 percentage points.

contributed to SME productivity growth as a result of the grant. The purpose of this exercise is to illustrate the economic significance of grants and not to quantify the macroeconomic effects of the grant, which would require us to estimate the externalities these firms generate.[46] We present back-on-the-envelope calculations to estimate the overall employment effect over Hungarian SMEs and we also perform a simple decomposition exercise to quantify the aggregate productivity effects of the grant scheme.

In these calculations, we pool grant winners into three cohorts by the year of successful application (2004–2005, 2006–2008 and 2009–2011). We pool winners from the 3-year periods to report the results in a more perspicuous way and also to increase the stability of our decompositions. We follow these cohorts for 6 years, starting one year before the first grants were distributed to the second and third cohorts and two years for the first cohort.[47]

Table 9 quantifies the contribution of these firms to aggregate SME employment. Let us start with the 2006–2008 cohort. In the base period (2005), these firms made up 7.6 percent of total SME employment. Between 2005 and 2011, employment in these firms increased by 20.8 percent. By subtracting the estimate of the effect of the grant on employment, equal to the product of the employment figure in the base period and the estimated effect of the grant on employment, we can assess the counterfactual employment growth of these firms in the absence of a grant.[48] According to our estimation, grant winning firms would have grown by 5.8 percent in this counterfactual view. Therefore, grants contributed 15 percentage points to the growth of these firms. By multiplying this effect with the initial employment share of these firms, we find that the employment effect of grants on these firms equals 1.1 percent of total SME employment in this period. For the other two cohorts, we estimate the contribution to be in the same range: 0.7 percent for the 2004–2005 cohort and 2 percent for the 2009–2011 cohort.

Table 10 shows total productivity growth for the three 6-year-long subperiods based on the productivity decomposition described in Online Appendix E.[49] Let us consider the last cohort, receiving grants between 2009-11. According to the column labeled "Total", labor productivity in the SME sector increased by 14.8 percent over 6 years. The column labeled "Grantee" shows that 3.1 percentage points of this growth were contributed by firms winning grants in the 6-year period betwene 2008-2014 — in other words, these firms contributed by 21 percent to total SME productivity growth. The employment share of this group was 14.2 percent in 2008, the base year of this period (see Table 9). Therefore, grant winning firms contributed a 50 percent higher share to total productivity growth between 2008 and 2014 than a random group of firms with a similar employment share. The numbers are even larger for the 2005–2011 period (grantees with a 7.6 percent employment share contributed by nearly 20 percent to overall productivity growth), while grant winners contributed slightly above the average firm between 2002 and 2008 (contributing with 5 percent with and employment share of 3.9 percent).

Needless to say, the contribution of subsidized firms does not represent the productivity contribution of the grant scheme. Indeed, as we have shown, there is positive selection into applying for grants, and it is likely that these firms would have contributed substantially to productivity growth even if they had not received a grant. To assess the macro effects of the grant scheme, we calculate a counterfactual contribution showing how much the same group of firms would have contributed without the grant. To do so, we subtract the estimated effects of employment and labor productivity (based on our preferred specification from Panel B of Table 5) of the grant from the actual growth rates at the firm level. We recalculate the productivity contribution of these firms based on the modified growth rates. These results are shown in the columns labeled "Counterfactual" of Table 10. For example, in the third period, the grant winning firms would have contributed only 1.33 percentage points without the grant scheme instead of

[46] Our concept of job creation refers to the literature starting with the work of Davis and Haltiwanger (1992). The concept of the contribution of grant winning firms follows the literature that quantifies the contribution of groups of firms to aggregate productivity growth (see e.g. Foster et al., 2008; Haltiwanger et al., 2013; Haltiwanger, 2015).

[47] This period of 3–4 years after winning a grant matches the timeline of our preferred regressions, which estimate the long term effect of 3–4 years of the receival of the grant.

[48] We use our preferred specification from Panel B of Table 5.

[49] This decomposition exercise focuses on within-firm productivity improvement and reallocation but ignores other costs and benefits, such as spillovers arising from poaching employment and customers from other firms and the firm itself being the buyer of other firms' products should also be included in a comprehensive cost–benefit analysis.

the observed 3.1 percentage points. Therefore, grants increased the contribution of these firms by 1.8 percentage points. According to these calculations, the contribution of grant winners is between 0.45-1.8 precentage point for the 3-year cohorts, or 0.2-0.6 percentage points for each year's grants.

One should compare the benefits of the program with its cost. The cost of grants in our data was annually on the order of 0.3 percent of the aggregate SME value added between 2004 and 2007, and increased to 1.3–1.7 percent between 2008–2014.[50] Therefore, the total cost of subsidizing each of our 3-year cohorts was in the order of 1–4.5 percent of SME value added for our different periods. Compared to this cost, the employment contributions of 0.7–2 percent and the productivity contributions in the order of 0.5–1.8 percentage of aggregate SME value added do not seem especially large. As for employment effects, a natural measure is the cost per job. The program created one more job in grant-winner firms for the equivalent of 2.5 years of average wage in the first two cohorts and 3.5 years of average wage in the final cohort[51] This can be compared to the results of Brown and Earle (2017), which shows that subsidized SME loans created jobs at a cost around 9–10 months of average wage.

7. Conclusions

This article has investigated the effects of a large firm-level grant scheme in Hungary with the help of a simple theoretical framework and empirical analysis. Our empirical results show positive selection into grant applications, and winning a grant increases the inputs, labor productivity, and capital intensity of the treated firms. We do not find, however, changes in the skill intensity of the workforce or a substantial increase in TFP. Our worker level results show that low-skilled workers benefit only marginally from cheap capital in terms of higher wages but high-skilled workers enjoy substantial benefits.

Our study supports the view that these grants provide important investment incentives to firms with good investment opportunities, and they use these funds to expand their production. The finding that grants generate additional investment suggests that subsidized investment does not completely crowd out market-based investment. Second, the positive employment and sales growth effects contrast with the view that these funds are channeled out from the firm and are spent on the personal consumption of its owners or grant distributors. Third, the results on positive selection and outstanding outcomes for firms with multiple grants contradict the idea that grants are used to support uncompetitive firms. Finally, the lack of TFP and skill composition effects contradicts the view that grants trigger radical technology upgrading. Consequently, we interpret this as evidence for "more of the same" view in our theoretical framework, showing that grants generate productive investment but do not lead to technology upgrading.

Methodologically, our paper demonstrates that having information on unsuccessful applicants goes a long way towards providing credible estimates for the effects of a program. This not only eliminates selection based on time-invariant features but, crucially, it allows one to control for the time trend in growth and investment around the application date, common to both groups of firms. We also show that the results are very similar when we rely on an instrumental variable identification strategy.

In terms of policy, the fact that only a relatively small subset of firms applied for these grants, providing "free capital", suggests that the fixed cost of applications can be substantial in such programs, leading to positive self-selection. Regarding policy design, we find that allowing firms to apply multiple times improves the efficiency of the program, but larger grants do not lead to larger output and productivity effects.

To quantify the economic significance of the program, we calculate that each year's subsidy program created jobs in grant winning firms equivalent to 0.2–0.7 percent of total SME employment and contributed to aggregate SME productivity growth by 0.2–0.6 percentage points — with an annual cost often in excess of 1 percent of total SME value added.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jce.2022.09.001.

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[50] We use SME value added as a comparison both because it is strongly related to labor cost and productivity and also because it also related to GDP.

[51] Using the average wage for full time workers in the mid-year of the cohort, based on data from the Central Statistical Office, https://www.ksh.hu/docs/hun/xstadat/xstadat_hosszu/h_qli001.html

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