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DISCUSSION PAPER SERIES

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ABSTRACT

The Effects of EU-Funded Enterprise Grants on Firms and Workers*

This paper investigates the effects of non-repayable enterprise grants financed from the European Union's Structural and Cohesion Funds on firm outcomes in Hungary using firm- and worker-level information on all rejected and successful grant applications between 2004-2014. In our model, after paying the fixed cost of applying, firms can purchase capital at a reduced marginal cost and they share the rent generated from the grant with their workers. In line with the model's predictions, larger than average, more productive and faster growing firms are more likely to apply for a grant. We combine panel regression methods with matching techniques to estimate the effect of grants by comparing successful and unsuccessful applicants' outcomes. Subsidized firms increase their employment, sales, capital-to-labor ratio and labor productivity, but not total factor productivity. The skill composition of workers is not affected by the grant but wages grow, especially for skilled workers. Firms winning multiple grans benefit more already from the first grant and successive grants have even larger effects. According to our simple calculations, each year's subsidy program created jobs in grant winning firms equivalent to 0.3-0.5 percent of total SME employment and contributed by 0.3-0.7 percentage points to aggregate SME productivity growth – with an annual cost often in excess of 1 percent of total SME value added. These results suggest that these grants promote firm growth, but do not lead firms to introduce new forms of production or upgrade technology.

JEL Classification: H25, D22, O16, J21

Keywords: enterprise grants, EU grants, worker effects, matched employer-employee data, Hungary

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1 Introduction

Many countries and regions allocate substantial sums for subsidies to achieve regional convergence and accelerate economic growth (Neumark & 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). These programs provide funding in the order of several percentage points of GDP each year for Central and Eastern European countries. While the majority of these funds were invested in public infrastructure, about 10 percent was used to provide direct grants to firms, making the program one of the largest enterprise subsidy schemes in the world.[1] A key motivation behind these policies is to raise the productivity and foster the growth of enterprises and, eventually, the SME sector as a whole. To assess the effects of the program, one should first estimate the effect of the grant on firm level growth and productivity. To gain additional insights into within-firm changes, worker composition and wage changes may also be computed. Finally, aggregate growth and productivity changes also depend on the type of firms that apply and win a grant, which fundamentally determines the policy's effect on reallocation.

To investigate these questions, we use a unique combination of administrative databases from Hungary, which provide information on all double-entry book keeping firms and all grant applications for the period of 2001-2014. For a subset of firms, employee level information is also available. A key advantage of the resulting dataset over most data of a similar focus is that it has information on rejected, rather than only on successful, applications. This unique feature has two advantages. First, we can study selection patterns into application and winning, a feature of grant subsidies that has received little attention so far despite its great importance for policy evaluation. Indeed, it is puzzling why, despite the obvious benefit of free capital, only a small fraction of entrepreneurs apply (15 percent of firms with employment between 5-250 in our data). Such an exercise may inform policy makers both about the perceived cost of applying and the allocation of funds across different firms. Furthermore, understanding the selection process helps us to estimate the aggregate effects of the program. Second, unsuccessful applicants constitute a more credible control group than all non-winning SMEs. Unsuccessful applicants are likely to be similar to winners since both were willing to and capable of investing in an application for the grant.[2] More-

[1] Subsidies to firms – repayable or in the form of subsidized loans – are not limited to the European Union. China is famous for heavily subsidizing its State-Owned Enterprises (e.g. Lim et al. 2018); in the United States the Small Business Administration allocates billions of dollars to small firms to ease their credit constraints (Brown & Earle 2017); subsidized loans are widespread in developing countries: Hassan & Sanchez (2009) report that in Latin America, the Middle East, North Africa and South Asia, 214 institutions deal with microfinance.

[2] This is important as Hurst & Pugsley (2011) document that most small businesses do not need financing as they do not want to grow.

over, the selection process for SMEs was based on a simple administrative checklist rather than on detailed project descriptions or sophisticated measures of firm attributes. Indeed, successful and unsuccessful applicants turn out to be very similar in our selection regressions.

To guide our thinking about grant effects, we consider several broad – and somewhat simplified – views, which are present in academic discussion and the popular press on how these grants work. According to the first, grants are *channeled out* from the firm and are used for the owner's private consumption, and part of it may even end up with the bureaucrat supervising the distribution of grants (Johnson 2017, ECA 2019, Mironov & Zhuravskaya 2016). This predicts an increase in reported capital but no effects on other inputs or the output of the firm. A second interpretation claims that grants constitute a cheap and easy way to finance firms, and so firms that are not able to secure market financing may become *addicted* to it and focus on rent seeking rather than improving productivity.[3] The third, most optimistic, view is that cheap capital promotes radical *technology upgrading* accompanied by strong productivity growth and the increase of the quality of the workforce.[4]

Our results, however, do not support any of these views. Therefore, we focus on a fourth possibility, namely, that when the main effect of to be a fall in the cost of capital is an *extension of the firm's current activities*. To fix ideas, we provide a model which originates from a standard framework of investment, similar to Criscuolo et al. (2019). A grant is a financing instrument with a specific cost structure: (i) it provides a subsidy for both the principal and interest, (ii) but has a fixed application cost. The model first predicts positive selection into applying. Regarding firm outcomes, it predicts positive capital, output and labor productivity growth and ambiguous employment effects. To study wage effects, we start from an imperfect labor market model following Kline et al. (2019) and argue that the rent generated by the low cost of capital will be shared with the workers, and the degree of sharing depends on skill.

The data support these predictions: firms which are initially larger, faster growing and more productive, are more likely to apply. The selection regressions also show that pre-application differences are much smaller between successful and unsuccessful applicants than between applicants and other firms. Thanks to the quasi-automatic decision process, we can interpret it as evidence that the success of the proposal depends partly on random factors, such as the exact timing of the application or random mistakes, which are not correlated with the quality of the project.

In our analysis of the outcomes of the program, we apply a combination of three econometric methods to handle any pre-application dissimilarities between rejected and successful firms. We

[3]This argument is often claimed in the popular press. See, for example, Portfolio (2016).

[4]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.

control for firm fixed-effects to eliminate any time-invariant differences between successful and unsuccessful applicants. We also include common event time dummies to the data around the application year, which removes any potential linear pre-application trends that affect the control and treated groups similarly.[5] Finally, we match successful and rejected firms on year of application, 2-digit industry and the quartile of output growth in the year before the application. Pre-trends disappear only when we perform all three methods simultaneously.

The effects of the subsidies on inputs, output and productivity are also consistent with the predictions of the investment model. We find large and statistically significant effects for capital stock, employment, capital intensity, sales and labor productivity, but only marginally significant effects for TFP. In our preferred estimates, capital stock increases by 26 percent and employment by 11 percent. In line with the lower effective price of capital, successful applicants rely more strongly on this input and so their capital-to-labor ratio increases by 16 percent. The value of sales increases by 17 percent. As a result, labor productivity also increases by 6.5 percent and TFP with 3 percent, but this coefficient is only marginally significant.

Regarding worker effects, we find that skill composition does not change, suggesting that no skill biased technological change takes place. Wages do react to the subsidy and they increase substantially for skilled workers (by 6-9 percent) and to a lesser extent for other, lower skilled employees (by 4 percent). One interpretation of this difference is that skilled workers are better at capturing the rents generated at the firm level, in line with Kline et al. (2019).[6]

We analyze the heterogeneity of the effect along several dimensions. First, we show that firms winning multiple grants grow faster than those that win only once. This evidence contradicts the addiction hypothesis and suggests that policy-makers should not discourage repeated applications. Second, we find that (very) small businesses (having lower levels of employment than the median firm of 15 employees) increase their capital stock more than larger firms, but employment, sales and productivity effects do not vary by size. We also test the variation of the effect along pre-application productivity and find that unproductive firms experience a larger scale effect but no productivity effect while productive firms grow less but have substantial productivity effects.

To summarize the effects of investment grants, we find that they foster firm growth along several dimensions (capital, employment, and output) and also raise labor productivity. These

[5] Note that this is only possible when the data include unsuccessful applications. It turns out that this is important, as the common event year dummies remove pre-trends nearly as much as matching does without the event time dummies.

[6] Another possible scenario is that employers replace part of their labor force with more qualified workers and so they get higher wages. Separations, however, do not increase and the newly hired workers' skills – measured by whether they come from a job or from non-employment and by their wages in the previous workplace – are not different from those of pre-subsidy hirings.

results do not support the views claiming that grants are channeled out from the firms or that uncompetitive firms become addicted to subsidized finance.[7] It is also hard, however, to claim that grants induce fundamental changes in the technology and efficiency of firms – we only find weak TFP effects, nor we have evidence on the skill upgrading of workers. The most likely scenario is, therefore, that firms grow by mostly expanding the activities they already performed.

Nevertheless, these firms belong to the most productive group of Hungarian SMEs and their expansion generates positive reallocation effects. According to our simple calculations, each year's subsidy program created jobs in grant winning firms equivalent to 0.3-0.5 percent of total SME employment. Similarly, they contributed by 0.3-0.7 percentage points to aggregate SME productivity growth. However, this contribution to aggregate SME employment and labour productivity growth does not seem to be very large compared to the cost of the program which was above 1 percent of aggregate SME value added in some years.[8]

Our paper contributes to the literature on the effects of financial support to firms. Closest to our work is Criscuolo et al. (2019), who study similar grants in the United Kingdom.[9] They find positive investment, employment and sales effects both at the firm and regional level, but no productivity effects. Bronzini & De Blasio (2006) and Bernini & Pellegrini (2011) investigate a similar investment subsidy in Southern Italy and find positive investment and size effects but negative productivity effects.[10] We contribute to this literature in several ways. We show that, similarly to most of the policies presented in the literature, EU-funded grants in Hungary had scale effects, but no TFP effects or skill upgrading. We also present a theoretical framework, based on the model of Criscuolo et al. (2019), suggesting mechanisms of the effects of this specific financial tool on various firm- and worker-level outcomes. Relevant to policy design, this model emphasizes

[7]The result that grants are not channelled out of firms resonates with the findings of Muraközy & Telegdy (2016), who study the distribution of EU grants in Hungary across townships with mayors from the ruling party and opposition and find, that political considerations do matter, but only for visible grants (like road construction or renovation of buildings). The paper does not find political distortions in the distributional process of direct enterprise subsidies.

[8]A key constraint to further pursue the welfare effects of the program is that we cannot quantify spillover effects on other firms or workers, which may be either positive or negative (for an analysis of the net employment effect of grants in the UK, see Criscuolo et al. (2019)). In addition so potential spillovers, such financing tools may also have general equilibrium effects by exerting pressure on bank loan supply.

[9]We found a number recent of evaluations of the effects of EU subsidies in the Central and East-European region (for Hungary, see KPMG 2017, Equinox 2016, Banai et al. 2020), and two analyses of small assistance programs financed by the USAID in the nineties in Macedonia (Bah et al. 2011) and Romania (Brown et al. 2005).

[10]De Mel et al. (2008) studies free cash subsidies for entrepreneurs in Sri Lanka and finds positive output effects. Another type of investment subsidy, FDI in underdeveloped regions, have been also studied by, i.e. Crozet et al. (2004), Devereux et al. (2007), Greenstone et al. (2010). A number of papers analyzed other types of financial support, such as subsidized loans in developed countries such as France (Bach 2013) and the United States (Brown & Earle 2017) and in states that belong to the developing world (e.g. Banerjee & Duflo 2014, Banerjee et al. 2015, in India). These studies rely on various estimation methods, including changing geographic eligibility (Criscuolo et al. 2019), instrumental variables (Bach 2013, Banerjee & Duflo 2014, Brown & Earle 2017) and randomized trials (Banerjee et al. 2015, De Mel et al. 2008, in Sri Lanka).

that fixed costs of application may play an important role in determining which firms benefit from the grants. Methodologically, we use unsuccessful applicants as a control group and demonstrate that these firms differ substantially from non-applicants, which are used as a control group in many evaluations.

The direct enterprise grants of the European Commission are part of the larger set of place-based development policies so our work can shed light on the effects of such policies (Neumark & Simpson 2015). In particular, Structural and Cohesion Funds are place-based policies supporting underdeveloped regions by combining infrastructure investment and business support. The regional effects of place-based policies have been studied both in the United States (Glaeser & Gottlieb 2008, Kline & Moretti 2013, 2014, Busso et al. 2013), and in the European Union (Becker et al. 2012, 2018).[11] These studies show positive effects of such policies, especially of infrastructure investment on growth, but find that somewhat smaller overall funding than provided may be optimal. We contribute to this literature by showing that, in one of the most heavily subsidized countries, development grants affect aggregate labour productivity of the SME sector both via within firm and reallocation effects, even if they do not contribute to within firm TFP growth. Interestingly, we find that the program does does not induce structural change since it does not affect industry shares.

Finally, our paper also contributes to the literature on rent sharing within firms (Christofides & Oswald 1992, Blanchflower et al. 1996, Kline et al. 2019, Abowd et al. 1999, Card et al. 2018, Carlsson et al. 2015). We show that employees, especially skilled workers, benefit from the rent generated by the cheap capital. Motivated by Kline et al. (2019), we argue that this is likely because higher skilled workers have more bargaining power resulting from the higher replacement cost associated with this employment category.

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.

[11]Note, however, that the large US programs analysed (e.g., the Tennessee Valley Authority and the Appalachian Regional Commission), included only infrastructure investment rather than firm grants.

2 Conceptual Framework

2.1 Model Setup

We consider grants as a financing option for investment with a particular cost structure. Compared to bank financing, grant-financed investments have a lower marginal cost of capital, because firms do not pay interest and depreciation on the part of their investment financed by the grant. Applying for a grant, however, involves a fixed cost, which is assumed to be higher than the fixed cost associated with bank financing. This cost includes the time cost of learning about grant opportunities, writing the proposal and the monetary cost of hiring a firm specialized in assisting the management with the application. We denote this fixed cost with F. Anecdotal evidence and the low share of applying firms (15 percent in our data) suggest that F may be substantial, especially for small firms.[12] For simplicity, we restrict our attention to a two-period case. We denote the capital stock of the firm with K_0 in the initial period. The firm makes a decision whether to apply in the initial period and how much to invest between periods 0 and 1.[13]

More explicitly, we incorporate the fixed cost of application in the model of Criscuolo et al. (2019), which relies on the Hall-Jorgenson cost of capital framework (Hall & Jorgenson 1967). In this framework, the optimal investment choice of the firm is given by the equality of the firm's marginal revenue product (MRPK) and the marginal cost of capital, ρ, which equals the sum of depreciation (δ) and interest (r):[14]

$$\rho = \delta + r$$

We denote the generosity of the grant with ϕ, which is the share of investment that is not refundable.[15] With the grant, the marginal cost of capital becomes ρ_{sub}:

$$\rho_{sub} = (1 - \phi)(\delta + r)$$

Figure 1 illustrates the decision problem of the firm. The horizontal axis marks the amount of capital.[16] The marginal revenue product of capital (MRPK) is downward sloping. The marginal

[12] This is analogous to the argument in Melitz (2003), namely, that the large share of non-exporting firms motivates the assumption that exporting is associated with high fixed costs.

[13] A multi-period model, when one compares the present value of the one-time fixed cost with the flow of rents from every period resulting from the dynamic investment behavior, would make the model more complex without yielding additional insights.

[14] We model depreciation by assuming that the firm can sell its capital stock at δ times the original price or, alternatively, consider $\delta \to 1$.

[15] ϕ is typically 0.4-0.5 in the data, as shown in Figure 7.

[16] For tractability, we implicitly assume that the firm can finance investment into any type of capital from the

cost of capital is ρ when no grant is available while it is smaller and equal to $(1-\phi)\rho$ with the grant. The optimal capital level choice in the second period is K_1 if no grant is available and K_2 with the grant.[17]

In order to understand which firms apply, we analyze the investment decision rather than the optimal amount of capital, because the gain from the grant depends on the amount of investment and not the capital stock. Figure 2 shows the amount of investment between periods 0 and 1, with K_0 denoting the amount of capital in period 1. Gross investment (I) consists of 3 parts: (i) replacement of depreciated capital (δK_0), (ii) the new investment undertaken by the firm without the grant ($K_1 - K_0$); (iii) and the additional investment resulting from the grant ($K_2 - K_1$).

Period 1 benefit of the grant is the quasi-rent or profit, which equals the shaded area in Figure 2. The firm will apply for the grant if the fixed cost of applying, F, is smaller than the value of the quasi-rent discounted to initial period value, QR:

$$\text{QR} = \underbrace{\delta K_0[\phi(\delta + r)]}_{\text{Financing replacement}} + \underbrace{(K_1 - K_0)[\phi(\delta + r)]}_{\text{Investment on market rate}} + \underbrace{\int_{K_1}^{K_2} MRPK(k) - (1-\phi)(\delta + r)dk}_{\text{rent on generated investment}}$$

,

2.2 Selection Into Application

In the framework described above we can make connections between firm characteristics and the propensity to apply. The value of the quasi rent is increasing in total investment, which depends on (i) the capital stock of the firm and (ii) its growth potential. Figure 3 illustrates two firms with Firm 1 having a lower investment demand than Firm 2, either because because Firm 1 has less capital to replace or is less likely to grow. The large investment need of Firm 2 generates a high value of quasi rents and, therefore, the firm is more likely to apply for the grant.

Another possible dimension of heterogeneity in applications relates to financial constraints (Criscuolo et al. 2019). Constrained firms can only borrow at interest rate r up to a certain limit, and they face an upward-sloping credit supply afterwards, as shown in Figure 4. As a result, constrained firms operate with lower capital stock in the absence of a grant (K_1^{const}) than unconstrained firms ($K_1^{unconst}$), while both can increase their capital stock to K_2 when the grant is offered. This has two

grants, while in reality grants may be restricted to certain types of capital. Distinguishing between grant-eligible (e.g., machines) and other capital (e.g., motor vehicles) would yield similar predictions.

[17] In reality there is a maximum level of investment that can be financed through the grant and so the firm can only increase its capital stock accordingly. Figure 6 demonstrates, however, that this constraint is not binding for the majority of firms.

key implications. First, the investment effect of the grant is larger for constrained firms, because it reduces their cost of capital to a greater extent. Second, due to both the larger effect on investment and the larger fall in the cost of capital, constrained firms realize larger quasi-rents and so they are more likely to apply.

The fixed cost may also vary across firms. Most importantly, learning reduces F: after submitting an application, the fixed cost of submitting another will be much lower as employees already know how to write an application. An extra benefit from applying, thus, is the option to apply again for a lower fixed cost.

2.3 Size and Productivity

To understand how grants affect firm size and productivity, we need to impose more structure on the model. Let us assume that the production function is of constant returns to scale, capital and labor markets are perfectly competitive and firms face a downward-sloping product market demand curve. Under these assumptions one can rely on the Hicks-Marshall laws of derived demand. The change in capital depends on the generosity of the grant (ϕ) as follows:

$$\frac{\partial \ln K}{\partial \phi} = s_K \eta + (1 - s_K)\sigma$$

where $\eta > 0$ denotes the absolute price elasticity of demand, $\sigma > 0$ is the Hicks-Allen elasticity of substitution between labor and capital, and s_K is the capital share.[18] Under these conditions, the more generous the subsidy, the larger its effect on capital stock. The effect is also larger if product demand is elastic and if it is easy to substitute labor with capital.

The lower input cost resulting from the grant will increase sales in proportion to the elasticity of demand:

$$\frac{\partial \ln sales}{\partial \phi} = s_K \eta$$

The employment effect of the grant is given by the following equation:

$$\frac{\partial \ln L}{\partial \phi} = s_K(\eta - \sigma) \tag{1}$$

If the scale effect dominates ($\eta > \sigma$), an increase of the capital stock is associated with an increase in the number of employees. The more price-elastic the demand is and the more complementary capital and labor are, the larger the labor response. If, however, consumers are not price sensitive

[18] Note that $\partial \ln \rho = -\partial \phi$.

but it is easy to substitute labor with capital, employment will fall.

Since the grants cut the cost of capital relative to that of labor, the capital-labor ratio (or capital intensity) will increase. By definition, this increase is proportional to the elasticity of substitution:

$$\frac{\partial \frac{K}{L}}{\partial \frac{\rho}{w}} = \sigma > 0$$

Via increasing capital intensity, the grant will positively affect labor productivity, or value added per worker.

TFP is an alternative measure of productivity, showing how much added value is produced by the firm after controlling both for its labor and capital usage. As it controls for the change in capital intensity, it will not increase in our simple framework. If the firm ”overinvests” in capital stock by buying machines with a low marginal product, TFP may even decline. In contrast, if the grant helps technology upgrading or advanced methods of production control and therefore better work organization, TFP may increase.

2.4 Wages

The rents resulting from cheap capital may also affect the wages of workers under rent sharing in the firm. One way to model this is Kline et al. (2019), who assume that firms have wage-setting power and face a trade off between lower wages and higher turnover.

Wages are determined by the following rent-sharing equation:

$$w_j^t = (1-\theta)w_j^m + \theta\left(1 - \frac{1}{\eta}\right)\frac{\text{gross surplus}}{L} \tag{2}$$

where w_j^m is the market wage at which the firm can employ new workers, η is the elasticity of the product demand and L is the number of employees.

In this setting the wage paid for incumbent workers is the market wage plus a share of the gross surplus per worker. The key parameter is θ, the rent sharing parameter. In the Kline et al. (2019) model it depends – among other factors – on the specific investments the firm has to undertake after hiring a worker, such as on-the-job training or other fixed costs. Such investments improve the bargaining position of the worker as employers want them to work for the firm as long as possible to dissipate the fixed cost. This setup also implies that the types of workers associated with high levels of fixed costs are likely to have a higher θ, and therefore gain more from the firm-level gross surplus than those who have a low fixed cost of hiring. As on-the-job training and fixed costs associated with hiring are higher for high-skilled workers, such employees capture a larger value of the grants in this model.

One way to think about the gross surplus in our framework is the following. If the firm relies on market finance, it will have a capital stock K_1 (see Figure 1). Owners and workers can share the gross surplus per worker (or labor productivity), generated with capital K_1 minus the cost of capital per worker:

$$\frac{\text{gross surplus}}{L_1}^{\text{market}} = \frac{VA_1}{L_1} - \frac{\rho K_1}{L_1}$$

When investment is financed from a grant, both labor productivity and the capital cost change. Capital per worker will increase, resulting in higher labor productivity. The reduced capital cost will generate a direct rent which can be shared between owners and workers. In particular:

$$\frac{\text{gross surplus}}{L_2}^{\text{grant}} = \frac{VA_2}{L_2} - \frac{\rho K_2 - \phi\rho I + F}{L_2}$$

Here, $\phi\rho I$ is the capital cost saving resulting from the grant, and F is the (flow value of the) fixed cost of applying for the grant. The difference of these expressions is likely to be positive for two reasons. First, according to revealed preferences, firms only participate in the grant scheme if it increases the gross surplus, i.e. if the numerator of the difference is positive. Second, our empirical analysis shows that grants are associated with an increase in the number of workers $(L_2 > L_1)$, i.e. the denominator is larger under a grant.

Consequently, the rent – or gross surplus per worker – is likely to increase after receiving the grant both because of the lower cost of capital and the potential increase in labor productivity. This may increase the wage of all workers, but the effect is likely to be larger for highly skilled workers, whose turnover is more costly to the firm.

2.5 Predictions and Alternative Theories

This simple model provides a number of predictions. The first set concerns selection. Firms are more likely to apply if they are (i) larger, (ii) grow faster, (iii) face financial constraints and (iv) have information on the application procedure and so face lower fixed cost of application.

The second set of hypotheses concerns the effect of the grant on firm outcomes. We expect that treated firms (i) raise their capital stock, (ii) increase their capital intensity, (iii) grow faster but (iv) do not necessarily experience an increase in TFP, and (v) raise wages, especially for skilled workers. The employment effect is ambiguous and depends on the relative strength of the substitution effect between labor and capital and the scale effect.

It is instructive to compare these hypotheses with the alternative frameworks mentioned in the Introduction (Table 1). If grants are simply 'channeled out' by owners, i.e. they spend them on private consumption, one would expect an increase in the book value of capital, but not in sales, employment or productivity. The second scenario is that less successful firms apply for subsidized funding. This predicts negative selection into application. The final scenario is that of radical technology upgrading, which will increase TFP and, as long as it is skill biased, an increase in the share of skilled workers. Note that increased capital intensity, labor productivity and skilled workers' wages are predicted already when the cost of capital falls and so these results are not sufficient to detect technology upgrading.

3 Data and Institutional Features

3.1 Data

The empirical analysis of this paper relies on three databases. The first is an administrative panel of financial statements of all Hungarian firms with double-entry bookkeeping for the period between 2001 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. These data also include information on the type of grant the firms apply to (see the next subsection). Some sub-measure level variables are also available, such as the maximum amount of funds firms may apply for and the maximum share of cost covered by the grant. To bring the firm-level and application-level data to the same unit of time (year), we aggregate all applications that were filed in the same calendar year and had a similar purpose (e.g., purchase of equipment).

To study the effect of grants on the structure of employment and wages, we augment the data by a third administrative dataset, maintained by the National Pension Administration. We use a version received and maintained by the Data Bank of the CERS–IE, which has a 50 percent sample of the Hungarian population aged over 5 in 2002. The data span between 2003 and 2011 and it constitutes a panel of individuals with information on sex, age, 4-digit occupation ISIC code and wages for each month and job. Out of these data, the Data Bank created a version that has information for May each year and we use this in our analysis.

The three datasets are linked together with unique, anonymized firm identifiers. As the first grants were allocated in 2004, we keep balance sheet information starting with 2001 to have pre-treatment years for early subsidies. Firms that apply for grants are predominantly SMEs and we restrict the sample to firms which have an average number of workers between 5 and 250.[19] We drop agricultural enterprises, which are targeted by a different set of grants. We study the effect of subsidies on firm productivity and growth and so we keep only grants that finance capital used directly in the production process: buying machinery and equipment, expanding the enterprise's capacity by constructing new buildings or introducing new IT systems. Finally, we drop those cases where tangible assets, employment or sales are missing or are equal to zero (this affects about 2 percent of the firm-years). The final data include 63,480 enterprises and 520,097 firm-years whereby the average firm is observed for about 8 years. The worker data contain 10,769 firms (317,360 firm-years) and 2,737,811 worker-years. The descriptive statistics of the firm and worker samples are presented in Appendix Table A4 and they show that the average firm in the linked sample is slightly larger and more productive than in the full sample.

3.2 Institutional Setup

The stated purpose of the EU Structural and Cohesion Funds is to promote the convergence of poorer regions within the European Union. Although all member states contribute to these funds, they represent significant net transfers of resources from richer to poorer countries. The European Council negotiates, and the European Parliament approves the size and distribution of the funds for seven-year planning cycles and our data cover the 2000-2006 and 2007-2013 cycles. While the majority of these funds are spent on public projects (e.g. roads, schools), a substantial part is allocated as grants to firms.

EU funds are distributed by institutions set up by the central government of each member state. While they 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).[20] 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

[19] We drop very small businesses as their data are often unreliable and only a small proportion of them applied for and received grants.

[20] In particular, for each operational program the member state should designate three types of authorities. The *managing authority* manages the funds and, *inter alia*, ensures that funding is allocated in accordance with the criteria applicable to the operational program and that Community and national rules are not violated. The *certifying authority* has to certify expenditures before they are sent to the Commission while the *audit authority* is functionally independent from the managing authority and oversees the activities of the management and control systems.

and Sub-Measures, which we observe in the data. A Sub-Measure may include multiple calls for proposals. 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. These grants are typically not restricted to specific sectors of the economy; both industrial and service firms can apply.[21]

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).[22] For the calls included in our analysis, information on the number of applicants, the amount of funds eligible per application, the share of co-financing and the requirements regarding employment and sales growth are in Table A1.

The grants we study are often relatively small and typically a multitude of firms apply for each call. To streamline the process, decision making was usually automated in these programs, leaving little space for subjective elements in the decision making. Firms which 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 at a first-come, first-served basis.[23] Also, 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 pre-determined maximum (Muraközy & Telegdy 2016).

We use an example to illustrate how these programs work. In our sample the largest Sub-Measure is GOP 2.1.1 on technology upgrading ("Technológia-fejlesztés, beruházás"). This sub-measure is part of Measure GOP 2 on firm development ("Vállalkozásfejlesztés") and Operational Program GOP called Operative Program for Economic Development ("Gazdaságfejlesztési Operatív Program"). Altogether, 17 GOP 2.1.1 calls were announced between 2007 and 2012. A typical example of a call is GOP-2009-2.1.1/A, which was announced on 30.01.2009 and was open until 21.05.2009. Firms could apply for 1-50 million HUF, and the purpose of the grant was the purchase of new machines or IT upgrading. Only micro, small and medium sized firms with two years of existence were eligible.[24] Firms meeting these criteria and submitting a formally ac-

[21]The first row of Table 3 shows the proportion of industrial firms among never applied, rejected and winner businesses. Many applications arrived from both sectors, but industrial firms were more likely to apply and win than firms from the service sectors.

[22]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 into 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).

[23]In contrast, for calls for highway building, when a much larger amount of money was allocated to fewer firms, the evaluation process typically included more subjective criteria such as the quality of the business plan.

[24]The average Euro-HUF exchange rate was 280.

ceptable application were 'automatically' awarded the grant as long as the budget lasted. In total, out of 3,805 applying firms 2,936 won a grant. Between 2007 and 2012 three calls were announced yearly, each targeting projects in different size categories. Call A included grants between 1-20 million HUF; Call B included grants between 15-150 million HUF, while call C was for up to 500 million HUF. Each of the calls was open for at least one month and typically closed as the funding ran out. In some cases the calls were extended by providing extra funding.

In Hungary, 9,126 firms applied for the types of grants included in this study, which is only 15 percent of all firms from our sample (see Figure 5). In line with the lax conditions, 74 percent of applicants had at least one successful application during the period of study (sometimes this was not the first application). An important feature of this scheme is that a number of firms have applied multiple times. 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.

Figure 6 demonstrates another important feature of this scheme: the maximum eligible size of the grant was not binding because of the co-payment requirements. Only about 12 percent of firms applied for, and received the maximum amount. At the same time nearly all applicants paid as little co-payment as was allowed (see Figure 7). Grant size varied considerably both in absolute terms and relative to firm size. Figure 8 shows the distribution of grant size relative to the firm's tangible capital in the year precedent to application. While many grants were relatively small, the majority of grants exceeded 10% of the firms tangible assets and another 10% of grants reached or exceeded the tangible assets of the company.

4 Which Firms Apply and Win?

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

Table 3 compares never applying firms with those which applied unsuccessfully and successfully.[25] Applying firms are larger – in terms of assets, employment and sales – than those that never applied while the difference between winners and losers is small. For example, the average number of employees is 15 in the group which never applied, 27 in the year before rejection and 29 before winning a grant. The capital intensity of applying firms is more than 100 log points larger than that

[25] The definitions of the variables are in Table 2. In the descriptive statistics we restrict the sample to the years after 2003 (as 2004 was the first year when Hungarian firms could apply for subsidy) and include all firm-years of never applying firms and the year precedent to application of the other two groups. Note that firms that filed multiple applications contribute with more than one year and they may be included both in the rejected and successful group with different firm-years.

of never applying firms. Applicants have higher productivity and grow faster than never applying businesses. Log labor productivity is 7.8 for firms that never applied and 8.3-8.4 for those which applied. (Normalized) TFP is slightly negative for the bulk of firms that never applied, 0.064 for the unsuccessful and 0.105 for successful applicants.[26] Real sales of never applying firms grow yearly by 1.6 percent only while the other two categories have a growth rate of 12-13 percent. Cash flow over the value of total assets (a measure of the ability of the firm to get a commercial loan as suggested by the theoretical work of Holmström & Tirole 1998) is only 2.3 percent in the first, and 16-17 percent in the second and third group, suggesting that financial constraints may not be the most important reason for applying.[27] Industrial firms are more likely to apply than service firms: only 20 percent of never applying firms are industrial companies compared to 29 percent for rejected applicants and 36 percent of winners. In terms of firm age, not applying and rejected firms are identical with over 10 years of existence on average, while winners are older by 1.5 years.[28]

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 activity. Never applying and rejected firms pay similar wages for their employees while winners pay higher wages by about 10 percent for both skilled and unskilled workers.

It is of particular interest whether the fixed cost associated with the application and, therefore, the decision to apply depends on previous experience. We proxy this with firm and worker-level variables. On the firm side, we construct dummy variables indicating whether they applied (successfully or unsuccessfully) in the past (see Figure 5). We also take advantage of the panel feature of the worker data to construct measures showing whether incumbent workers were employed previously at a company which applied for a grant when the worker was its employee. Similarly to the firm-level measures, we construct two such variables: one indicating that the previous employer applied, and the second that it won a grant. From these variables we construct a firm-level dummy variable indicating that the company has at least one such incumbent worker in a given year. We do this only for skilled workers as they are more likely to have taken part in the application procedure. As the bottom two lines of the table show, such workers are more likely to be in firms that applied. Workers who worked for companies that applied before, are present in 1.5 percent of the firm-years of never applying companies, in 6.2 percent in unsuccessfully applying companies and

[26]TFP is the residual of a production function estimated with the Levinsohn-Petrin method.

[27]These patterns do not result from industry composition or time effects. In simple regressions, where the dependent variable is a firm characteristic and the explanatory variables are application status (never applied, rejected, winner) and industry-year fixed effects, the differences between the three groups are very similar to the pattern emerging from the simple means presented in Table 3.

[28]Haltiwanger et al. (2013) show that older firms of different ages do not differ much, at least for employment growth. As most of our sample is older than 5 years, in the analysis we do not control for firm age. Firm age controls do not change the results.

in 9.4 percent of winners.

To shed more light on the selection of firms into application and winning, we run linear probability models. The estimated coefficients and the corresponding standard errors are presented in Table 4. In columns (1) and (2), the dependent variable is a dummy showing whether the firm applied and the sample consists of all firm-years of never applying firms and the year precedent to application for applying firms. In Columns (3) and (4) only the years preceding an application are used and the dependent variable is a dummy indicating winning.[29] The regressors include one measure of firm size (log tangible assets), growth (yearly sales growth), firm productivity (log labor productivity), a proxy for credit constraints (a dummy variable = 1 if the ratio of free cash flow relative to the value of assets is lower than its median value).[30] To study industry-level selection, we added the growth rate of sales at two-digit NACE industry and a variable measuring industry-level labor productivity in Hungary relative to the US.[31] In the worker-level data we also add the ratio of skilled workers and the log of wages. We also add proxies of the fixed costs of applying: whether the firm filed a grant application previously (successful or not), whether there are skilled workers in the firm who came from a company that applied for a grant (successfully or not) and the number of skilled workers. We cluster standard errors at the firm level.

In line with our theory on self-selection into paying the fixed cost of the application, larger, faster growing, and productive firms were more likely to apply. Based on the firm-level regression, a firm situated at the 75th percentile of the size distribution measured by the value of tangible assets is 2.5 percentage points more likely to apply than one at the 25th percentile. As the mean of the dependent variable is 0.043, the probability of a larger firm applying is 56 percent higher than that of a smaller one. We get positive effects, albeit smaller ones for sales and productivity: for sales growth, the difference is 9 percent and for labor productivity 11 percent. It is interesting that firms with low values of cash flow relative to the value of assets (our proxy for credit constraints) are less likely to apply. The associated coefficient is -0.016, suggesting that those firms with lower-than-median cash flow-asset ratio are 45 percent less likely to apply.

The proxies associated with previous experience show that prior application and winning are indeed positively associated with applying again and these effects are large. If the firm filed a grant application before, its chances of filing another one are larger by 4.8 percentage points or 104

[29]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.

[30]We chose the explanatory variables such that one is not constructed from the other. For example, we do not include both employment and labor productivity.

[31]This measure was obtained from the EU KLEMS database, available at http://www.euklems.net/. The exact calculation of this variable is available in Muraközy et al. (2019). If we replace the industry-level variables with a full set of two-digit industry-year interactions, the results remain practically identical.

percent while a successful application adds another 4.3 percentage points. The number of skilled employees (which proxies per-capita time cost of the application) has a positive effect on applying, and the effect is sizable: between the 25th and the 75th percentile the difference in the number of skilled workers is 3, so the variation in the effect is 0.006 percentage points or 12 percent. 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 (2.8 percentage points), but it does not matter whether the firm won or was rejected. This is plausible, because, regardless of the outcome, a person involved learns who to apply.

We include two industry-level variables to capture potential reallocation between industries – but we do not find evidence for such effect. Industry growth is not associated with the probability of applying: firms from declining and growing industries apply in the same proportions after controlling for their characteristics. Thus, previous firm growth and other characteristics capture selection better than industry growth. The coefficients associated with industry level labour productivity (measured by the distance of the Hungarian industry from the global frontier) are significant statistically but not economically.

To sum up, we find a strong positive selection into the application process, as size, productivity and growth are all positively associated with the propensity of application. Experience with the application process – which reduces the fixed cost of application – fosters further applications and winning an application also fosters further application activity. The self-selection process into applying is very much in line with the predictions of the model for investment grants and fixed costs, with the sole exception of financially constrained firms being more likely to apply; on the contrary, it seems that firms that are financially viable apply more frequently, suggesting that financial viability is also positively associated with the quality of investment opportunities.

We test for selection into winning in columns (3) and (4) of Table 4, where we drop firms that never applied and we compare rejected and successful applications only. In contrast to the application regressions, only few variables are statistically significant (mostly at the 5 percent level). The estimated coefficients are quite similar to those from the application regressions, but the dependent variable is 1 in 2/3 of the cases (compared to 4 percent in application regressions) and so the proportional differences induced by the regressors are small. These results underline that the decision-making process of SME applications is rather formal and does not take into account the performance of the firm – supporting the argument that unsuccessful applicants can make a sensible control group.

5 Econometric Approach

To study the effect of grants on firm outcomes, we rely on methods borrowed from the treatment effects literature. For lack of an experimental setting and suitable instrumental variables, we use a difference-in-differences strategy to estimate the treatment effects with successful applicants as the treated group and unsuccessful applicants as the control group.[32] 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 and the willingness to pay the fixed cost of application. In the context of our model, all firms that decided to file applications expected that the rents resulting from the grant would be larger than the fixed cost of application.

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

Even though 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.[33]

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.[34] 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. In order to capture the long-term effect with appropriate power, we extend ζ_3 to be one in all years $t \geq t_0^i + 3$ – practically we assume that the effect remains constant after 3 years.

Our benchmark specification is the following:

$$Y_{ikt} = \gamma^{win} \times win_{it} + \sum_{\tau=-3}^{+3} \zeta_\tau + \delta_{kt} + \nu_i + \varepsilon_{ikt}, \tag{3}$$

[32]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.

[33]In the matching procedure we make use of subsequent applications as well. In Section 6.4 we study the effects of the first and second application.

[34]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 the treatment while controlling for its growth in the control group.

To estimate the evolution of the treatment effect in 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 which received a grant.

$$Y_{ikt} = \sum_{\tau=-3}^{+3} \gamma_\tau \times win_{it}^{\tau} + \sum_{\tau=-3}^{+3} \zeta_\tau + \delta_{kt} + \nu_i + \varepsilon_{ikt}, \tag{4}$$

where the set of γ_τ coefficients shows how the outcome variable differs between 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.

We conduct the matching procedure in several steps. First, we trim the time series of firms symmetrically around application years (here we use all application years, not only the first as we did before). The reason for this is to drop firm-year observations which are too far away from an application and also to make the length of the time series of control and treated firms identical. Let us denote the date of application m of firm i with t_0^{im}. We only keep observations between [$t_0^{im} - 3$,...,$t_0^{im} + 3$], where t_0 denotes the application year. Note that this procedure transforms the data such that each application is an observation with a seven-year window, indexed by im.

Second, we classify these im windows into treated and control groups. We classify a window as treated if the firm applied successfully in t_0^{im} and as control if the application was rejected in t_0^{im}.[35] This procedure unambiguously classifies windows of firms with a single application into one group or the other, but for firms with multiple applications these windows can overlap and so some firm-years cannot be unambiguously classified into one of the groups. To resolve this issue, we discard windows when (i) the application in t_0^{im} was successful but there was another successful application in the $[t_0^{im} - 3, ..., t_0^{im} - 1]$ period or (ii) the application in t_0^{im} was unsuccessful but the

[35]Note that a firm can be in the treated group for one of its windows while in the control group for another.

firm applied successfully in the $[t_0^{im}-3,...,t_0^{im}+3]$ period.

Third, we do exact matching of these windows based on three variables: (i) the year of application, t_0^{im}; (ii) the 2-digit industry of firm (k); and (iii) the quartile of sales growth rate between $t_0^{im}-2$ and $t_0^{im}-1$. Therefore, the control group consists of firms applying in the same year, operating in the same industry and growing similarly to treated firms.

Fourth, to further guarantee that the control group is as similar to treated firms as possible, we conduct a propensity score matching based on pre-application characteristics within these year-industry-growth quartile cells. In particular, we run the following probit regression where the dependent variable equals one if the firm won an application in year t and zero otherwise and the right hand side consists of the inputs and the output of the firm:

$$Winner_{im} = f\left(X_{i,t_0^{im}-2}, X^2_{i,t_0^{im}-2}, X_{i,t_0^{im}-1}, X^2_{i,t_0^{im}-1}, industry_i, t_0^{im}, county_i\right) \tag{5}$$

where

$$X_{it} = \{\ln(emp_{it}), \ln(sales_{it}), \ln(wage\,bill_{it}), \ln(tangible\,assets_{it}), CashFlowLow_{it}\}$$

In these regressions, one observation is an application or a seven year window. We specify the function $f()$ the following way. Year, industry and county effects are controlled for, and the continuous variables are in log form. We include the levels and squared values of these variables in $t_0^{im}-1$ and $t_0^{im}-2$ to capture differences both in levels and growth rates. Note that these specifications include the lagged values of the performance measures, which will serve as dependent variables when estimating the effect of grants, capturing part or the unobserved heterogeneity across firms.

The predicted coefficients and standard errors of the probit regression are in Table A2. With the predicted propensity scores we perform a kernel matching with caliper = 0.05 to weight the control observations within the year-industry-growth quartile cells. We call this weighted sample the *matched sample*. This sample includes 1,635 treated and 1,039 control application windows.

The balancing tests comparing the control and treated groups in the year precedent to the treatment are in Appendix Table A3. The standardized difference of the key variables between the two sample means is never larger than 0.05.[36] To assess the external validity of the estimations performed on the matched sample, we compare the matched sample with the application sample. As Table A4 shows, 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

[36]As a rule of thumb, a standardized difference under 0.25 is acceptable (Imbens & Wooldridge 2009).

results are likely to be applicable to the whole sample.

As in any difference-in-differences specification, our identification assumption rests 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. The institutional analysis suggests that this is unlikely to be the case, given the automated selection process and simple criteria. The balancing tests sustain this as the levels and growth rates of the two groups are very similar and pre-trends are non-existent in this sample as we show shortly.

To summarize, the estimation method used in this paper differs from the conventional difference-in-differences estimation in three dimensions: (1) we use only applicant firms (which has the effect similar to matching, but on non-observable characteristics); (2) we add to the specifications a common trend around the first application for unsuccessful applications and the first successful application for winners (this is also contingent upon having information on the unsuccessful applications); (3) we also employ standard matching techniques to further control for differences between the control and treated groups. For such a multi step-method it is key to understand how the different steps affect the estimates and pre-trends. Table 5 presents the pre-trends for various estimation methods (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, 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 and without the common trends. The table reveals that switching to the application sample from the full sample decreases the pre-trends by about one-third, but they still remain large and statistically significant. The inclusion of common trends into the regression radically decreases the magnitude of the coefficients, from 0.25 – 0.50 to 0.05 – 0.08 and their level of statistical significance also weakens. The importance of the controls for common trends is further demonstrated by the fact that the matched data without these controls produce similar pre-trends as the applicant data with common trends. Finally, our preferred specification, the joint use of matching and common trends, completely eliminates pre-trends.[37]

These comparisons reveal that the key advantage of having information on unsuccessful applicants is that it allows capturing the common trend of all applicants before applying.

[37]Although our preferred specification cuts the time series of firms symmetrically around the application year, allowing for longer pre-trends does not result in larger pre-application coefficients (as we show in Figure A1).

6 Policy Effects

In this section we present the firm- and worker level outcomes of a grant, followed by a productivity decomposition and the heterogeneity of the effects.

6.1 The Effects of Grants on Firm Outcomes

Table 6 reports the main results from estimating Equation (3) for the key outcome variables. Panel A provides the estimated coefficients for the applicant sample. To start with the most direct effect of a grant, we find that successful applicants raise the value of their assets by 36 percent relative to unsuccessful ones.

Capital growth is accompanied by a 19 percent increase in employment relative to firms that applied unsuccessfully, suggesting that the scale effect on the product demand market was larger than the substitution of labor with capital (see Equation 1). Capital stock grew more than the number of employees, implying that capital intensity increased by 18 percent. The value of sales also increased by 25 percent and the stronger growth in sales and value added compared to that of labor implies that labor productivity also grew by 6 percent. There is no evidence for an increase in TFP.

These specifications, however, are contaminated with pre-trends, which has already been suggested by Table 5. Figure B1 in the Appendix investigates this issue further with the dynamic specification outlined in Equation (4), where year 0 denotes the year when the grant was awarded. The figure suggests the presence of pre-trends and no clear trend breaks for assets and output while there is a clear trend break for capital and capital intensity. We also find a positive pre-trend for productivity, followed by stagnation and a decline of TFP after the grant is awarded.[38]

This motivates to make the matched data with common event-time dummies our preferred specification.[39] The results are presented in Panel B of Table 6. Matching attenuates the magnitude of the estimated coefficients, but they remain economically meaningful and statistically significant. Relative to unsuccessful applicants, assets grew by 27 percent in the following three years, employment by 11 percent and so firms became 16 percent more capital intensive. Sales grew by 17 percent and labor productivity by 7 percent. The coefficient for TFP is 0.031 but it is significant only at the 10-percent level.

[38]To take the example of capital stock, prospective winners already had 15 percent more assets three years before the application than other applicant firms and this advantage increased to 18 percent in the year before the application. Afterwards, the capital stock of successful applicants increased to 35 percent higher levels than that of non-applicants.

[39]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.

Matching completely eliminates the pre-trends, as Figure 9 demonstrates. The estimated coefficients showing the difference in treated and untreated firms 1 and 2 years preceding the treatment are all small and insignificant.[40] Moreover, there is a clear trend break in the event time dummies around t_0, showing that successful applicants started growing faster and increased their labor productivity relative to the control group 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 and labor productivity in years 0 and 1 without further improvement, which we interpret as a signal for switching into more capital intensive production mode. Labour productivity has a fallback in year 3 which is caused by sales growth petering out with employment still on a growth trajectory. TFP is bouncing up and down, with some indication of an increasing post-grant trend.

These results are in line with the predictions of the illustrative 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 scenarios we outlined in the introduction. The increase in sales and employment suggest that the investment is productive and does not only represent channelling out the money from the firm while the flat TFP pattern does not indicate radical reorganization or technology upgrading.

These 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.

6.2 The Effects of Grants on Workers

This subsection investigates the effects of grants on workers. In particular, we are interested in the effect of a grant on the composition of workers and their wages. Composition is of key interest, because assessing whether skill share increases is the standard test for skill biased technological change (Caroli & Van Reenen 2001). Wages, however, can reflect both technological change and rent sharing.

We adapt to the worker-level database the identification strategy used so far and compare

[40] As a robustness check, we also ran the dynamic specification with 4 pre-event time dummies. As presented in Figure A1, the pre-event time dummies remain small and insignificant and the estimated effects of the grant are also unchanged (see Table A5).

worker outcomes in successful and unsuccessful applicants. A key 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 460 treated and 320 control firms.[41] We weight each worker-year observation with the inverse of the number of workers so each firm-year gets an equal weight in the regression to prevent large firms driving the results. We study two types of variables: the skill and wage of workers. When the dependent variable is the wage of the individual, we add to the controls a set of 2-digit occupational codes, and in some regressions we replace firm fixed-effects with worker fixed-effects.[42]

First we look at differences between skilled workers (managers, professionals and associate professionals) and unskilled employees (all the other occupational categories). As Table 7 shows, the proportion of skilled workers does not change in firms that received a grant. 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 interpretation that these grants are unlikely to fundamentally change the technology used by grant winning firms. Wages, however, do increase by 4.5 (8) percent in the specification with firm (worker) fixed effects, suggesting that employers share the rents originating from the cheap capital with their workforce. The table further demonstrates that the wage increase is not uniform across skill groups. Unskilled employees received a wage raise of 4-4.5 percent, while the skilled group enjoyed an increase of 6.5-9 percent, depending on whether firm or worker fixed-effects were controlled for in the regression. This is in line with the model and with the anecdotal evidence that skilled workers have higher bargaining power within the firm and so they are able to capture a higher share from rents associated with cheap capital.[43]

We further slice the data and look at the composition of employment and wage differentials in more detail: the skilled group is disaggregated into managers and other skilled workers and the unskilled group into medium and low skilled.[44] Appendix Table A6 shows the estimated effects for these groups. The proportion of managers declines by 1.8 percentage points, which is offset by a 1.4 percentage points increase in the numbers of other skilled workers (these coefficients are not measured precisely). As the roles of managers and high skilled workers are often interchangeable

[41]The balancing tests for this sample are presented in Appendix Table A3 and show that they are all smaller than 0.08. Table A4 compares the full sample with the matched sample and reveals that matched firms are somewhat larger, more productive but have the same ratio of skilled workers and pay similar wages.

[42]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.

[43]In the applicant sample we estimate a negative and significant, albeit small, coefficient for the effect of grants on the skilled share. The wage effects of the grant scheme are positive, but they are smaller and differ less across skill groups (see Appendix Table B1).

[44]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.

in small enterprises, we do not take this as evidence that these changes represent a real alteration in the used technology or work organization.

When firm fixed-effects are controlled for, the largest wage increase is measured for managers (9 percent) followed by other skilled workers (7 percent). The lower skilled groups's wages are imprecisely measured and are also smaller in magnitude. When worker fixed effects are accounted for in the regressions, the grant effect on managerial wages declines by 1 percent, for other skilled workers it increases by 9 percent and it does not change for the two lower skilled groups. Therefore, the high-skilled – both managers and professional workers – shared the rents of cheap capital with the owners.[45]

What do employers do with their workforce when the new capital reaches the company? Do they work with the same workers or do they tend to replace some? In the first two columns of Table 8 we look at hiring and separation rates in the same regression framework as before. In the first column, the dependent variable is a dummy showing whether the worker is a new hire, while in the second it is a dummy showing whether the worker leaves the firm in the following year. Again, one observation is a worker and the sample is the matched sample.[46] We find that the grant affects only hiring rates, but not separations, suggesting that employers keep their incumbent workers and they hire additional employees to keep up with the increased work load.

Finally, we attempt to measure the quality of newly hired workers to test whether the grant changes the unobserved characteristics of the firm's workforce. We apply two proxies for the quality of new hires: whether they came from a job as opposed from non-employment (as people holding jobs may have stronger skills relative to the non-employed) and their wage in their previous job. Column (3) of Table 8 reports the results from a regression where the sample consists of new hires, and the dependent variable is a dummy showing whether the worker arrived from another job or not. The estimated coefficient is small and insignificant, suggesting that the mix of newly hired workers does not change after the grant.[47] Column (4) of Table 8 reports regressions where the dependent variable is the wage of the newly hired person in his/her previous job (and the sample consists of those newly hired workers who did a job-to-job transition). The coefficient equals 0.044 but it is not different from zero in statistical terms. Again, we find little evidence for grant-receivers

[45] Appendix Table B2 presents the results for the application sample, which are very similar to those based on the matched sample.

[46] In the matched worker sample the firm level estimates are similar to those in the matched firm sample, with the exception of employment, where we estimate a coefficient of 0.02 only. The hiring and separation regressions are still useful to test whether churning increases in the firms after they receive the grant. Appendix Table B3 presents these results for the application sample. These are very similar to the matched sample with the exception of separations where we estimate a negative and significant coefficient of the magnitude of 0.045, further suggesting that grantees keep their incumbent workers with the firm.

[47] This result also shows that subsidized firms are not poaching workers from other businesses, but neither do they switch to the unemployed pool, which would increase the positive effects of the grant.

hiring higher skilled workers than other firms.

To sum up, we do not find evidence for skill upgrading in any of our different specifications suggesting there is no skill biased technological change. However, We do find evidence for increasing wages, which is in line with our rent sharing hypothesis.

6.3 Grants and Aggregate Productivity

The previous sections demonstrated that EU grants had a significant impact on firm-level growth and productivity. Given the overall magnitude of enterprise grants, it is interesting to establish the program's aggregate effects on the SME sector. In this subsection we present back-on-the-envelope calculations to estimate the overall employment effect 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 cohort and two years for the first cohort. 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.

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 for the employment effect of the grant, 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 8.6 percent in this counterfactual scenario. Therefore, grants contributed 12.1 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 firm equals 0.9 percent of total SME employment in this period. For the other two cohorts we estimate the contribution to be in the same range: 0.5 percent for the 2004-2005 cohort and 1.7 percent for the 2009-2011 cohort.

Panel A of Table 10 shows total productivity growth for the three 6-year long sub-periods. Let us consider the last period, 2008 to 2014. According to the column labeled "Total", labor productivity in the SME sector increased by 14.8 percent over 6 years. The column labeled "Grantee"

[48]We use our preferred specification from Panel B of Table 6.

shows that 3 percentage points of this growth was contributed by firms winning grants in the first half of this period (between 2009-2011) – 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 20 percent to overall productivity growth), while grant winners contributed similarly to the average firm between 2002 and 2008 (both the employment share and the contribution share was 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 employment and labour productivity growth effects (based on our preferred specification from Panel B of Table 6) of the grant from the actual growth rates at the firm level. We re-calculate 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 0.97 percentage points without the grant scheme instead of the observed 3.1 percentage points. Therefore grants increased the contribution of these firms by 2.14 percentage points.

The grant scheme can contribute to aggregate productivity growth via two channels. First, it increases the labour productivity of the grantee, generating a positive within-effect. Second, it contributes via reallocation because, according to our results, grantees are both more productive initially and grow faster. We decompose aggregate growth to these two channels following Foster et al. (2008), where aggregate productivity change is decomposed into three terms: a within-term (the contribution of within-firm productivity growth to aggregate productivity) a between term (the contribution of reallocation of labor between firms of different productivity levels), and the effect of net entry (we describe the decomposition in Appendix 2 in detail).

Panel B shows the within effect. The actual within contribution of the grant winning firms was only 0.2–0.6 percent in the three periods. However, productivity growth would have been slightly negative without the grant and so it had a positive effect on the within productivity contribution in the order of 0.4-1.3 percentage points.

The reallocation contribution is presented in Panel C. The initially more productive and strongly growing grantee firms indeed contributed substantially to aggregate SME labour productivity growth through reallocation, with close to 2 percentage points in the two most recent periods. It is worth pointing out that their contribution would have been substantial without the grant anyway and so the contribution of the grant was in the order of 0.15-0.8 percentage points. Nevertheless, we conclude that the grant scheme contributed to aggregate SME labor productivity growth both via the within and the reallocation channels.[49]

One should compare the benefits of the program to its cost. The cost of the grants in our data was annually in the order of 0.3 percent of 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 0.5-1.7 percent and the productivity contributions in the order of 0.5-2.1 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 the grant winning firms for the equivalent of 2.5 years 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 & Earle (2017), who show that subsidized SME loans created jobs at a cost around 9-10 months of average wage. Naturally, other benefits, such as spillovers and employment effects should also be included in a comprehensive cost-benefit analysis.

6.4 Variation of the Effect by the Number of Grants, Firm Size and Productivity

In this subsection, we add two dimensions to the analysis to investigate how the effect of grants varies across firms. We analyze one policy design – that of multiple grants –, and we also look at the variation of grant outcomes by pre-application size and productivity.

As we showed in Figure 5, 21 percent of firms won at least two grants, so it is relevant for policy makers to examine the effects of multiple grants, especially in the light of the addiction hypothesis: firms that are not able to secure their financing through the financial markets, may get addicted to cheap grants. This would indicate that firms with multiple grants utilize them less

[49] Note that our aim with the quantification of these contributions to macro-level employment and productivity is just to illustrate the economic significance and plausibility of our estimated effects and the two channels. This back-of-the-envelope methodology is clearly not suitable to estimate aggregate employment or productivity effects because it does ignore both spillover effects (either positive or negative) and general equilibrium effects.

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

efficiently than those businesses which receive only one grant. We investigate this question by replacing the variable of interest in the baseline regressions with the following three variables. The ‘one grant’ dummy indicates winning a single grant during the analyzed period. The ‘fist 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.[52] Therefore, 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 11 shows the results of this exercise. Despite the fact that the effect of the first grant on tangible assets is quite similar for single and multiple grants (0.19 and 0.24, respectively), the other outcomes are larger for those firms that apply for another grant later on. Both the employment and output effects are about twice as large, fostering a productivity effect of 11 percent, which is in sharp contrast with the single grant winners’ 4 percent. Not only is labor productivity growth larger for multiple winners, but here we measure a highly significant positive TFP effect of 6.4 percent. The second grant increases capital by 41 percent and also has large employment and output effects. The large scale effect, however, is not accompanied by any productivity effect for the second successful grant and TFP even declines after the second grant.

These results do not support the hypothesis of grant addiction as serial winners grow faster than single winners. They can rather be explained by experimenting: 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.

A relevant question of policy design is whether grants should be targeted at some types of firms, like those in a specific size category. Our simple theory furnishes ambiguous predictions about the relationship between firm size and the effects of the policy. On the one hand, large firms may have a larger set of potentially viable projects and so they may invest more when the cost of capital falls (Figure 2). On the other hand, large firms, investing more in each year, may be more able to spend the grant on projects which they would implement from market funding otherwise (for example, replacing obsolete machines in our model). Nevertheless, differences between smaller and larger firms have been shown to be relevant in similar schemes in other countries: for example, Criscuolo et al. (2019) show that the effect of subsidies on the employment of firms with fewer than 50 employees were 4 times larger than for larger firms. Brown & Earle (2017) study SME loans in the US and find that very small ($\leq$ 7 employees) firms have increased their employment to a slightly smaller extent than larger firms.

[52]Due to our matching procedure, which includes only three years after winning the grant, no firm wins more than 2 grants in this sample.

To investigate this source of heterogeneity empirically, we cut the sample at its median size in the year before application (15 employees), and run the regressions on the two subsamples. The estimated coefficients are presented in Panels A and B of Table 12. The capital effect is much larger for small firms (36 versus 20 percent), but the employment and output effects are very similar in magnitude. The productivity effects are also quite similar in the two samples.[53] It seems, thus, that large firms are more able to employ the fresh capital and obtain similar output and productivity effects with lower levels of investment.

An alternative measure for the set of potentially viable projects is the productivity of the firm. Panels C and D of Table 12 present the results when the sample is split at the median productivity level. The scale of the firm (both in input usage and output) increases by more than 10 percent faster for unproductive firms, but this leads to a productivity increase only for firms that were already productive before the grant was received: in their case, labor productivity improves by 10 percent, which is in sharp contrast with the marginally significant coefficient of 5 percent for unproductive firms. This heterogeneity is amplified by an 8 percent large increase in TFP, which we find only for productive firms.[54] The heterogeneous effects by employment and productivity suggest that small and unproductive firms use the cheap capital mainly for growing, but they cannot raise their productivity. Large or productive firms are better able to efficiently use the fresh capital and accompany the scale effect with a performance effect as well.

7 Conclusions

This paper has investigated the effects of a large firm-level grant scheme from Hungary with the help of a simple theoretical framework and empirical analysis. In the theoretical framework grants are interpreted as a type of financial instrument which lowers the marginal cost of capital but comes with a fixed cost. The theory predicts positive selection into applications and growth of the firms output, inputs and labor productivity.

In the empirical analysis we contrast unsuccessful with successful grant applicants to get rid of potential biases arising from the selection of better firms into application. In addition, we combine panel data and matching methods to further decrease the pre-application differences between the control and treated groups. Our empirical results are largely in line with this framework, showing

[53]Note that the large difference in the coefficient in the capital regression may come from the cap on the absolute value of the grant. Indeed, smaller firm received 21 percent of their tangible assets in grant value while the larger half of firm distribution received only 8.5 percent (measured at the median value of the ratio).

[54]Productivity and size are not correlated and so the two heterogeneity measures divide firms into dissimilar categories. Firms that are large/small and have high/low labor productivity at the same time make up 48 percent of the sample.

positive selection into grant applications, increasing the size of the treated firms (both measured by inputs and output), labor productivity and capital intensity. We do not find, however, changes in the skill intensity of the workforce or increase in TFP. Our worker level results show that all workers benefit from cheap capital in terms of higher wages but high-skilled workers enjoy higher benefits. The heterogeneity analysis reveals that multiple grants have larger impacts on firm outcomes than single ones. Small and unproductive firms enjoy larger scale effects but they cannot increase their productivity while large and productive firms can.

Besides supporting the view that these grants provide important investment incentives to firms with good investment opportunities, our study contradicts other prevalent views on the effects of EU grants. First, 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 its owners. Second, 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 does not support the view that grants would trigger radical technology upgrading.

In terms of policy, the fact that only a relatively small subset of firms applied for these grants, providing ‘free capital’, underlines that the fixed cost of applications can be substantial in such programs, which leads to positive self-selection. This needs to be taken into account when designing such programs and also when evaluating them. Furthermore, our results suggest that these grants can contribute to positive reallocation but we found no evidence for technology upgrading within the firm. While our approach is not suitable for evaluating the welfare effects of the grants, a simple decomposition exercise showed that the grant scheme is likely to positively contribute to the labor productivity growth of the SME sector both via its positive within-firm and reallocation effects. However, this aggregate productivity contribution is of a similar magnitude as the cost of the program.

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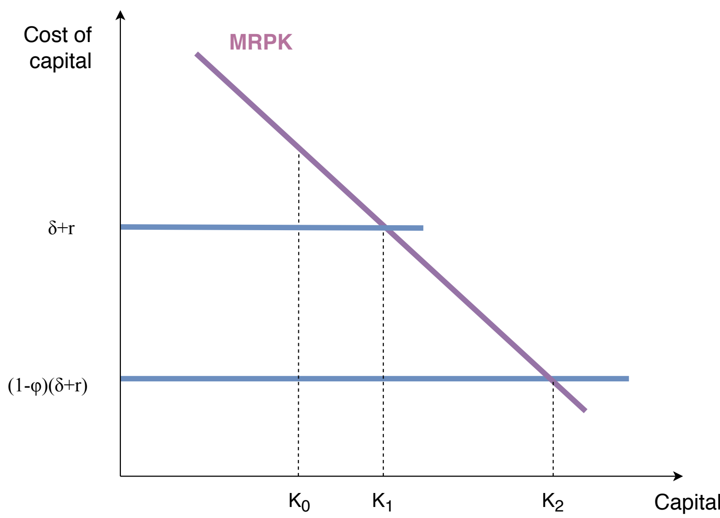
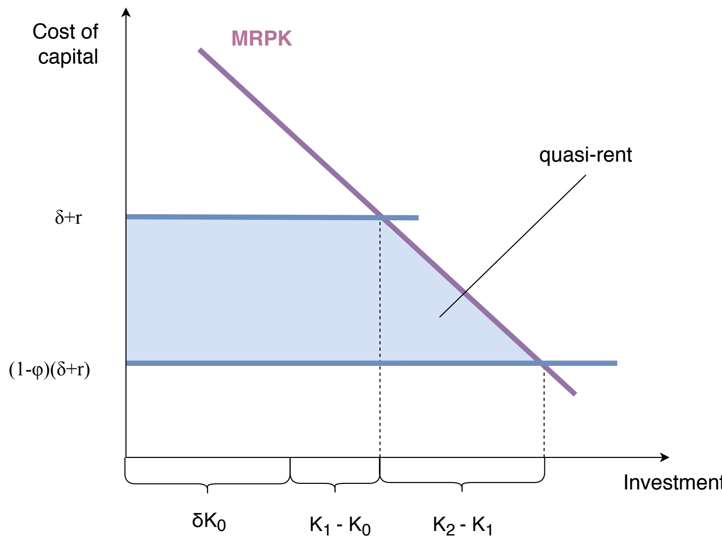
Figures and Tables

Figure 1: Optimal Capital Choice



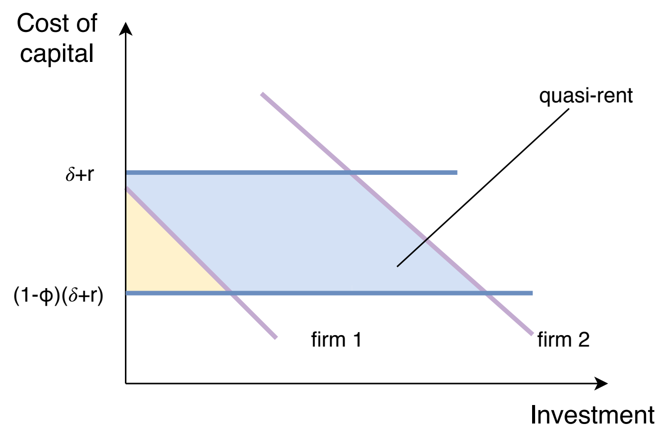
Notes: The figure shows the optimal capital choice without the grant (K_1) and in the presence of the grant (K_2).

Figure 2: Optimal Investment Choice and the Grant-Generated Quasi-Rent



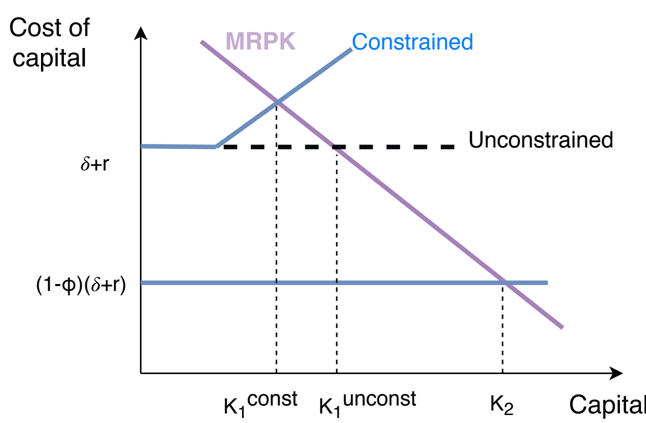
Notes: Notes: The figure shows the amount of quasi-rent generated by the reduced cost of gross investment in the presence of the grant. The firm will apply for the subsidy if the quasi-grant is larger than the fixed cost of applying.

Figure 3: Optimal Investment Choice for Small and Large Firms



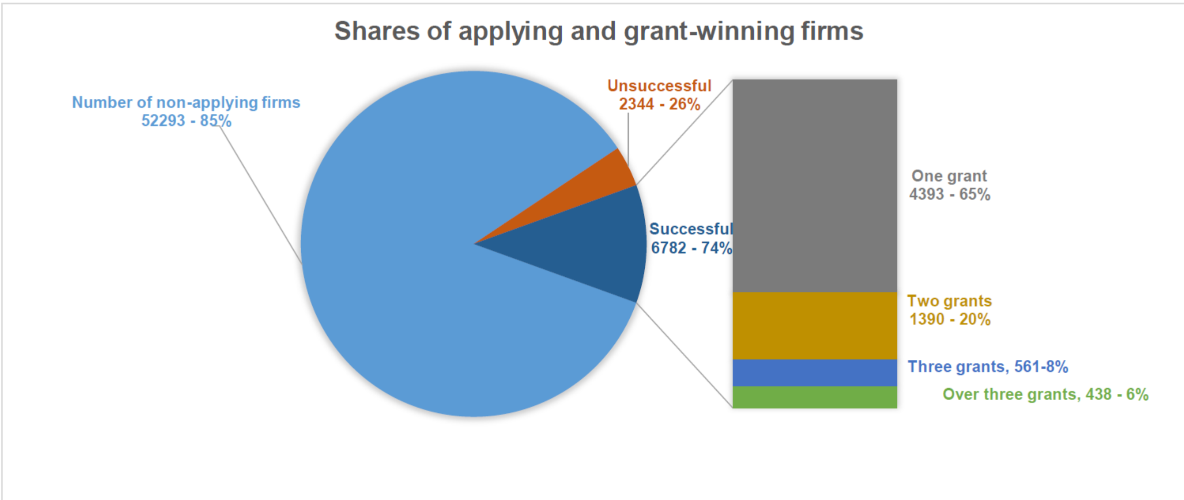
Notes: The figure shows that the amount of quasi-rent is larger for firms which have larger gross investment levels. Therefore, large firms are more likely to apply for the grant.

Figure 4: Capital Choice for Credit Constrained Firms



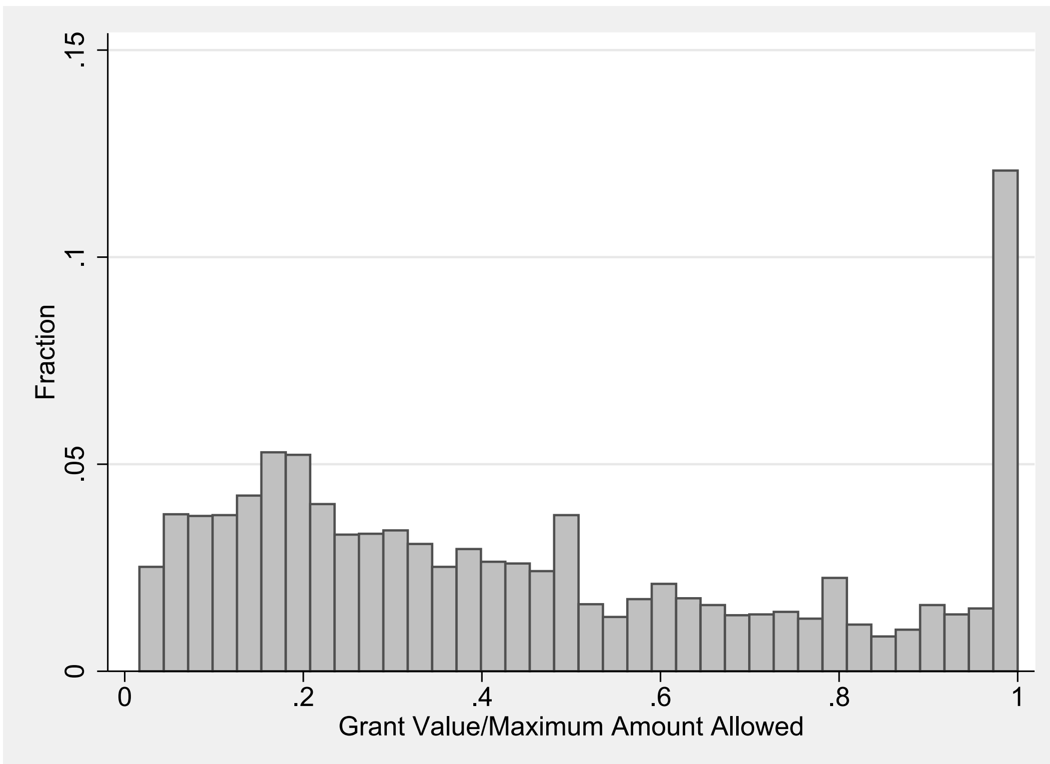
Notes: The figure shows the initial and final capital stock of firms facing financial constraints.

Figure 5: Number of Firms by Application Status



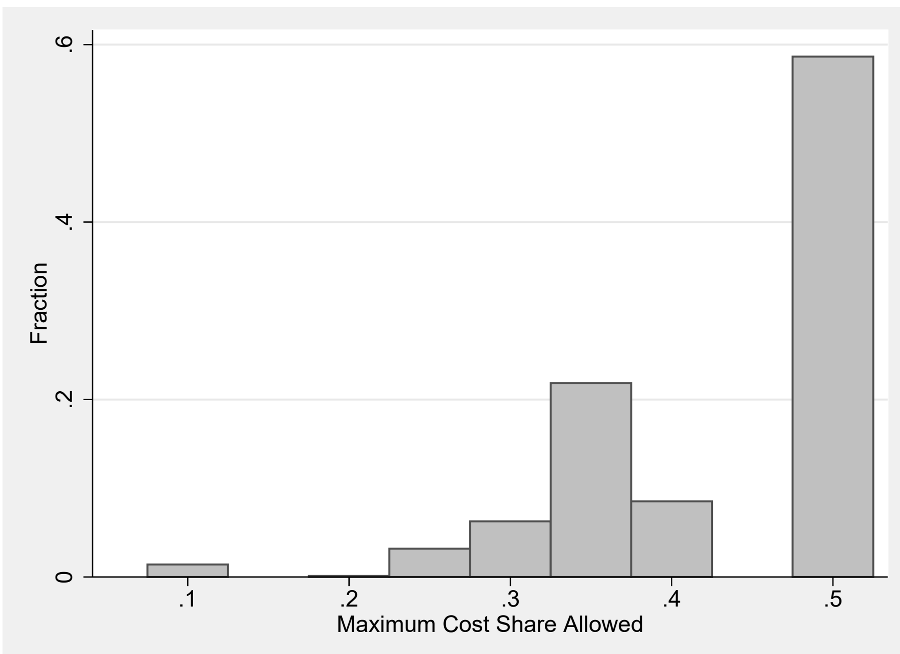
Notes: N = 63,480 firms. Sample: firms with average employment size of 5-250 between 2001-2014. The figure presents the number and share of firms that did not apply, applied but were rejected and had successful grant applications for capacity building and technology upgrading. Successful firms are further divided by the number of successful grant applications. Grant applications within one year are merged together.

Figure 6: Actual Relative to Maximum Grant Size



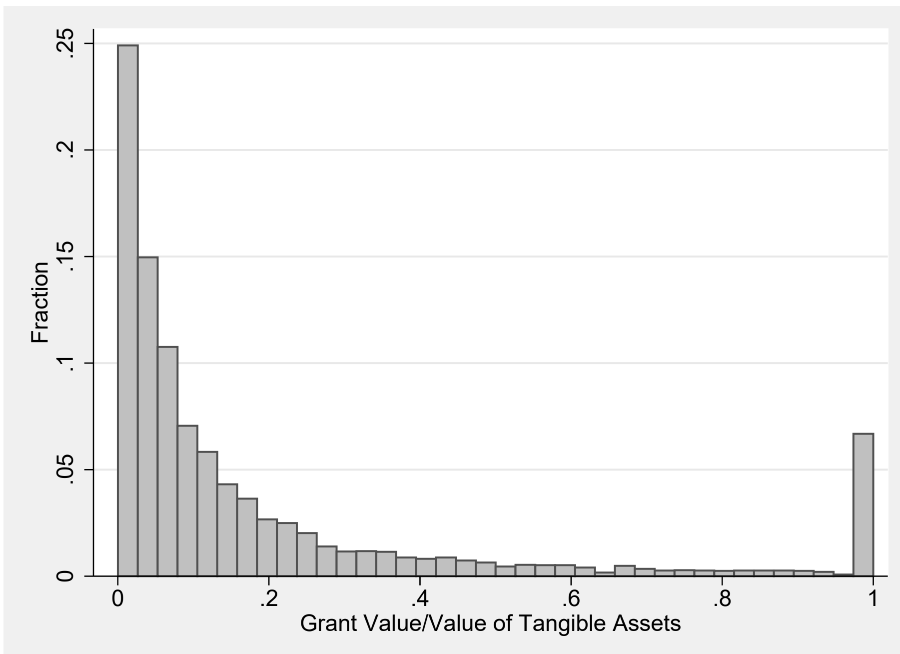
Notes: N = 4,906 grant applications. The figure presents the distribution of the ratio of the actual grant value and the maximum grant value specified in the Call for Application.

Figure 7: Share of Project Cost Covered by the Grant



Notes: N = 5,128 successful 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.

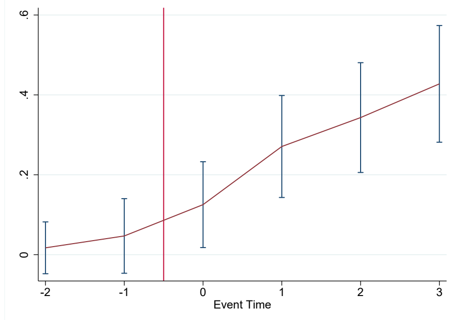
Figure 8: Size of Grant Relative to Firm Size



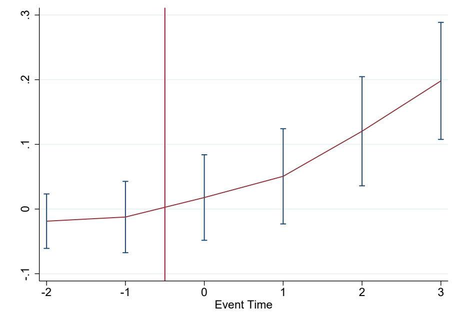
Notes: N = 6,377 successful grant applications. The figure presents the size of the grant relative to the firm's average value of tangible assets in the two precedent years of receiving the first grant. The variable is winsorized at the value of 1.

Figure 9: Dynamic Effects of the Subsidy

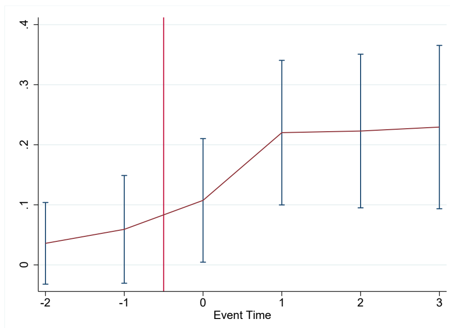
(a) Assets



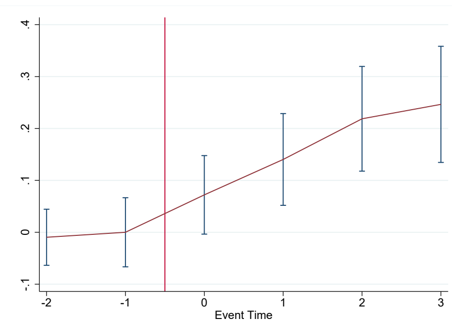
(b) Employment



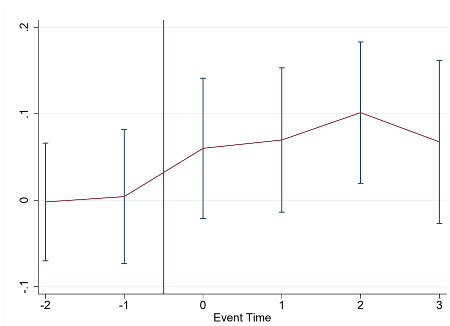
(c) K-L Ratio



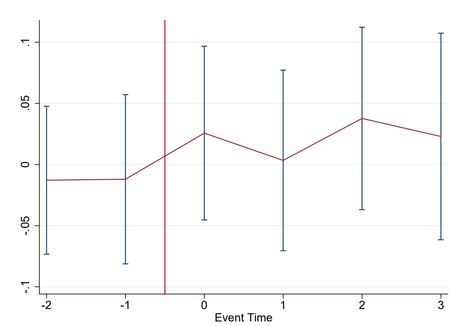
(d) Output



(e) Labor Productivity



(f) TFP



Notes: N = 18,631 firm-years (18,276 in the LP and TFP regression). The figure presents the estimated coefficients and the 99-percent confidence intervals of the event study regressions of Equation (4). 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.

Table 1: Predictions of Various Scenarios

Outcome\scenario	**Channeling out**	**Addiction**	**Investment subsidy**	**Technology upgrading**
Selection	?	-	+	+
Effect on:	+	?	+	+
Investment	+	?	+	+
Revenue	0	?	+	+
Cap int.	+	?	+	+
Lab prod.	0	?	+	+
TFP	-	?	0	+
Skilled wages	0	?	+	+
Skilled share	0	?	0	+

This table compares the empirical predictions of various scenarios for selection into application and the effects of the grants on firm outcomes. Channelling out the money means using the grants for private consumption, therefore one does not expect an improvement in size or productivity. In the addiction scenario uncompetitive firms apply for the grants, therefore one can expect negative selection into application. The investment subsidy scenario assumes that firms expand without upgrading their technology, while technology upgrading also assumes increases in TFP and potentially skill-biased technological change.

Table 2: Definition of Variables

Variable Name	Variable Definition
Application process	
Applied	=1 in the year and after the first grant application.
Winner	=1 if in the year and after the first successful grant.
One Grant Winner	=1 if in the year and after the successful grant for firms that won one grant during the observation period.
Multiple Grant Winner	=1 if in the year and after the first successful grant for firms that won multiple grants during the observation period.
Second Grant	=1 if in the year and after the second successful grant.
Inputs	
Capital	The book value of the firm's tangible assets, deflated by 2-digit price deflators.
Employment	Average yearly employment over months.
Output	
Value of Sales	Value of sales, deflated by 2-digit price deflators.
Productivity	
Labor Productivity	Value added (sales - material cost) over employment.
Total Factor Productivity	Residual from a production function, estimated with the Levonson-Petrin method.
Capital Structure	
Capital-to-Labor Ratio	Value of tangible assets over employment.
Cash Flow to Equity	= (After tax profits + depreciation)/Assets.
Worker-level variables	
Skilled Worker	=1 if the 2-digit ISIC occupation code = 1, 2 or 3.
Wage	Annual gross wage of the worker, deflated by the CPI.
Person Applied Before	= 1 if the person is skilled and worked for a firm which applied for subsidy.
Person Winner Before	= 1 if the person is skilled and worked for a firm which won a subsidy.

Note: This table contains the definition of the variables used in the paper.

Table 3: Descriptive Statistics of Successful Applicants, Unsuccessful Applicants, and Firms Never Applied

	Never Applied	Unsuccessful	Successful
Industry	.203	.294	.356
Firm Age	10.3	10.4	11.8
	(7.2)	(6.6)	(7.1)
Employment	14.6	27.2	29.4
	(22.4)	(33.3)	(34.7)
Assets	9.24	10.92	11.20
	(2.05)	(1.65)	(1.60)
Sales	11.54	12.74	12.81
	(1.41)	(1.29)	(1.27)
Sales Growth	0.016	0.135	0.122
	(0.668)	(0.513)	(0.440)
K-L ratio	6.99	8.09	8.29
	(1.93)	(1.34)	(1.29)
Labor Prod.	7.83	8.29	8.38
	(0.90)	(0.61)	(0.54)
TFP	−0.012	0.064	0.105
	(0.759)	(0.622)	(0.566)
Cash Flow/Equity	0.023	0.158	0.169
	(11.82)	(0.153)	(0.147)
Skilled	0.309	0.330	0.316
	(0.462)	(0.470)	(0.465)
Wage Skilled	111.7	111.8	121.2
	(1053)	(89.42)	(98.28)
Wage Unskilled	69.0	73.9	82.9
	(38.4)	(38.4)	(43.4)
Person Applied Before	0.015	0.062	0.094
	(0.121)	(0.241)	(0.291)
Person Winner Before	0.008	0.040	0.065
	(0.091)	(0.196)	(0.246)

Note: The table shows the means (standard deviations for continuous variables) of the characteristics for never applying firms (all firm-years) and unsuccessful and successful applicants (the year before application). One observation is a firm-year for firm-level variables, a worker-year for worker-level variables. 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 = 355,842/1,865,096 never applying, 4,868/56,602 rejected, 10,673/109,258 winner firm/worker-years with non-missing assets, employment, and sales figures (the number of observations for the other variables are in Appendix Table A4).

Table 4: Selection of Firms Into Application and Winning

	(1) Applied	(2) Applied	(3) Winner	(4) Winner
Assets	0.011^{***}	0.012^{***}	0.006^{**}	0.004
	(0.000)	(0.000)	(0.003)	(0.003)
Sales Growth	0.011^{***}	0.014^{***}	-0.005	-0.018^{*}
	(0.001)	(0.001)	(0.009)	(0.011)
L. Prod.	0.005^{***}	0.006^{***}	0.010^{*}	0.011
	(0.000)	(0.001)	(0.006)	(0.007)
Cash Flow Low	-0.016^{***}	-0.019^{***}	-0.060^{***}	-0.065^{***}
	(0.001)	(0.001)	(0.008)	(0.010)
Appl. before	0.048^{***}	0.056^{***}	0.035^{**}	0.046^{***}
	(0.003)	(0.004)	(0.014)	(0.017)
Winner before	0.043^{***}	0.061^{***}	0.023	0.023
	(0.004)	(0.006)	(0.015)	(0.018)
Industry Sales Growth	0.007	0.005	-0.031	-0.012
	(0.006)	(0.006)	(0.051)	(0.056)
EU_KLEMS	-0.001^{***}	-0.002^{***}	0.005^{**}	0.005^{*}
	(0.000)	(0.000)	(0.002)	(0.003)
Wage		-0.004^{***}		0.001
		(0.001)		(0.005)
No. Skilled		0.002^{***}		-0.001
		(0.000)		(0.001)
Person Appl. Before		0.028^{***}		-0.001
		(0.008)		(0.028)
Person Winner Before		-0.004		0.025
		(0.011)		(0.032)
Observations	342012	2064864	14581	156045
R^2	0.046	0.049	0.078	0.047
Mean depvar	0.043	0.048	0.692	0.651

Note: Firm-year observations in columns (1) and (3), worker-year observations in columns (2) and (4). Columns (1) and (3) are based on the firm sample, columns (2) and (4) on the worker sample. The table reports linear probability models with the dependent variable indicating that the firm applied the following year (columns (1), (2)) or won a grant the following year (columns (3), (4)). 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. Firms that applied multiple times are included with all years precedent to application. Assets and labor productivity are logged. 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.

Table 5: Comparison of Pre-Treatment Trends with Various Samples and Methods of Estimation

	Sample				
	Full	Applicant	Applicant	Matched	Matched
3 years before	0.244***	0.141***	0.053*	NA.	NA.
	(0.014)	(0.015)	(0.029)		
2 years before	0.348***	0.206***	0.059*	0.044***	0.005
	(0.016)	(0.018)	(0.033)	(0.016)	(0.023)
1 year before	0.508***	0.331***	0.080**	0.081***	0.001
	(0.017)	(0.020)	(0.035)	(0.024)	(0.034)
Winner	0.971***	0.695***	0.403***	0.316***	0.237***
	(0.019)	(0.024)	(0.038)	(0.036)	(0.043)
Common trends	No	No	Yes	No	Yes
R-squared	0.833	0.795	0.796	0.873	0.874
Observations	516802	109938	109938	18631	18631

Notes: Firm-year observations. Dependent variable: log tangible assets. This table reports the estimated coefficients (standard errors) associated with 3, 2 and 1 years before receiving the subsidy. All regressions include industry-year controls and firm fixed-effects. Full sample = all firms from the sample; Application 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. NA = not applicable. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table 6: The Effects of the Grant on Firm Outcomes

Panel A: Applicant Sample

	(1) Assets	(2) Emp.	(3) K-L ratio	(4) Sales	(5) L. Prod.	(6) TFP
Winner	0.364^{***}	0.187^{***}	0.177^{***}	0.244^{***}	0.058^{***}	0.011
	(0.027)	(0.018)	(0.024)	(0.022)	(0.014)	(0.012)
Observations	109938	109938	109938	109938	107011	107011
R^2	0.796	0.760	0.740	0.794	0.594	0.464

Panel B: Matched Sample

	(1) Assets	(2) Emp.	(3) K-L ratio	(4) Sales	(5) L. Prod.	(6) TFP
Winner	0.269^{***}	0.106^{***}	0.163^{***}	0.172^{***}	0.074^{***}	0.031^{*}
	(0.037)	(0.021)	(0.034)	(0.025)	(0.020)	(0.018)
Observations	18631	18631	18631	18631	18276	18276
R^2	0.874	0.871	0.813	0.901	0.679	0.560

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 (3). Regressions in Panel A are based on the applicant sample while in Panel B are based 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 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: The Effect of the Grant on the Composition of the Workforce and Wages

	Skilled	W. Sk.	W. Usk.	W. Sk.	W. Usk.
Winner	-0.003	0.063^{**}	0.039^{**}	0.093^{***}	0.039^{**}
	(0.014)	(0.028)	(0.017)	(0.033)	(0.019)
Fixed effects	Firm	Firm	Firm	Worker	Worker
Observations	72782	22060	50171	20138	44455
R^2	0.312	0.626	0.579	0.842	0.840

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. When the dependent variable is the wage of skilled (unskilled) workers, the sample is restricted to skilled (unskilled) workers. Regressions are based on the matched sample and they are weighted by the inverse of the number of workers in a firm-year. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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 8: Hiring, Separations and the Quality of New Hires

	New hire	Separation	Job-to-job	Prev. Wage
Winner	0.024^{*}	-0.004	-0.025	0.044
	(0.014)	(0.012)	(0.030)	(0.062)
Observations	71942	70523	16911	10033
R^2	0.091	0.086	0.168	0.334

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. The dependent variables are the following: (1) a dummy = 1 if the worker was hired in the precedent year; (2) a dummy = 1 if the worker will be separated the subsequent year; (3) a dummy = 1 if the worker moved to the firm from another job; (4) the log of the workers' previous wage. Columns (1)-(2) show regressions for all workers, while the sample in columns (3)-(4) is restricted to new hires. Regressions are based on the matched sample and they are weighted by the inverse of the number of workers in a firm-year. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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

Period	Cohort	Initial share	Actual growth	Counterfactual growth	Contribution of grant (relative to initial SME emp.)
2002-2008	2004-2005	3.9%	35.6%	21.9%	0.5%
2005-2011	2006-2008	7.6%	20.8%	8.6%	0.9%
2008-2014	2009-2011	14.2%	16.3%	4.6%	1.7%

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 6, Panel B. The last column shows the contribution of the grant to total SME employment growth.

Table 10: SME Sector Aggregate Labor Productivity Growth Decomposition

Panel A: Total Contribution

Period	Cohort	Total	Grantee	Counterfactual	Difference
2002-2008	2004-2005	15.11%	0.56%	0.00%	0.56%
2005-2011	2006-2008	10.29%	1.95%	0.72%	1.24%
2008-2014	2009-2011	14.79%	3.11%	0.97%	2.14%

Panel B: Within Contribution

2002-2008	2004-2005	14.06%	0.21%	-0.19%	0.40%
2005-2011	2006-2008	-0.15%	0.35%	-0.43%	0.78%
2008-2014	2009-2011	5.68%	0.60%	-0.72%	1.32%

Panel C: Reallocation Contribution

2002-2008	2004-2005	1.05%	0.34%	0.19%	0.15%
2005-2011	2006-2008	10.44%	1.61%	1.15%	0.46%
2008-2014	2009-2011	9.11%	2.51%	1.69%	0.82%

Notes: This table decomposes the labour productivity growth of the Hungarian SME sector based on the method in Foster et al. (2008). We describe our decomposition methodology in detail in Appendix 2. Panel A shows the decomposition of total productivity growth. The rows show the different (overlapping) periods. Total is total real labour 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 6, 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% 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 0.72 percentage points. The difference between these two numbers is 1.24 percentage points. Panel B does the same exercise for the within contribution and Panel C for the reallocation contribution (including the between, cross, entry and exit components.

Table 11: Single vs. Multiple Grants

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
One grant	0.187^{***}	0.053^{**}	0.134^{***}	0.094^{***}	0.043^{**}	0.015
	(0.041)	(0.023)	(0.038)	(0.027)	(0.022)	(0.020)
First grant	0.239^{***}	0.122^{***}	0.118^{***}	0.213^{***}	0.111^{***}	0.064^{***}
	(0.040)	(0.023)	(0.037)	(0.028)	(0.022)	(0.020)
Second grant	0.416^{***}	0.166^{***}	0.249^{***}	0.188^{***}	0.009	-0.035^{*}
	(0.034)	(0.019)	(0.034)	(0.025)	(0.022)	(0.020)

Notes: Firm-year observations. The table presents the estimated coefficients (standard errors) from Equation 3 where the grant variable was replaced by three dummy variables: (i) ”One grant” = 1 in the year and after the firm won a grant for firms that received one grant; (ii) ”First grant” = 1 in the year and after the firm won its first grant for firms that received multiple grants; (iii) ”Second grant” = 1 in the year and after the firm won its second grant. The regressions are based 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 clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table 12: Heterogeneity of the Effect by Firm Size and Productivity

Panel A: Smaller than Median

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.360^{***}	0.108^{***}	0.252^{***}	0.185^{***}	0.081^{***}	0.018
	(0.056)	(0.032)	(0.051)	(0.038)	(0.028)	(0.026)
Observations	9038	9038	9038	9038	8831	8831
R^2	0.804	0.572	0.795	0.827	0.670	0.550

Panel B: Larger than Median

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.199^{***}	0.113^{***}	0.086^{*}	0.171^{***}	0.074^{***}	0.048^{*}
	(0.048)	(0.026)	(0.045)	(0.032)	(0.028)	(0.026)
Observations	9567	9567	9567	9567	9421	9421
R^2	0.886	0.846	0.841	0.906	0.702	0.583

Panel C: Less Productive than Median

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.347^{***}	0.122^{***}	0.225^{***}	0.208^{***}	0.048^{*}	-0.015
	(0.056)	(0.031)	(0.053)	(0.034)	(0.028)	(0.025)
Observations	9073	9073	9073	9073	8936	8936
R^2	0.863	0.869	0.783	0.890	0.449	0.457

Panel D: More Productive than Median

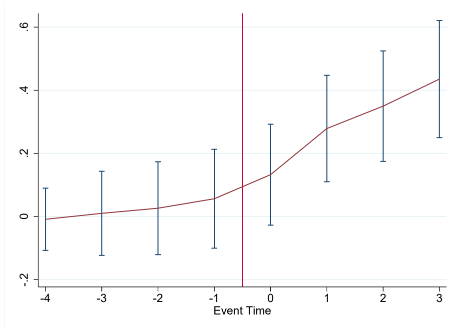
	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.183^{***}	0.089^{***}	0.093^{**}	0.134^{***}	0.103^{***}	0.081^{***}
	(0.045)	(0.027)	(0.042)	(0.035)	(0.027)	(0.025)
Observations	9530	9530	9530	9530	9317	9317
R^2	0.873	0.878	0.805	0.898	0.641	0.588

Note: 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 (3), when the sample is split by pre-application firm size and productivity. Firm size is measured by employment, productivity by the ratio of value added and employment in the year precedent to application. The sample is split into 'low' and 'high' groups based on pre-application employment/labor productivity being lower/higher than its median value. The regressions are based 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 clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

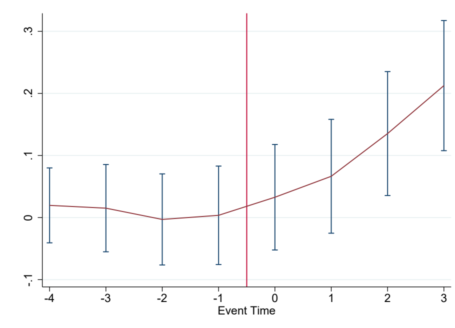
Appendix A: Additional Figures and Tables

Figure A1: Dynamic Effects of the Subsidy with Long Pre-Trends

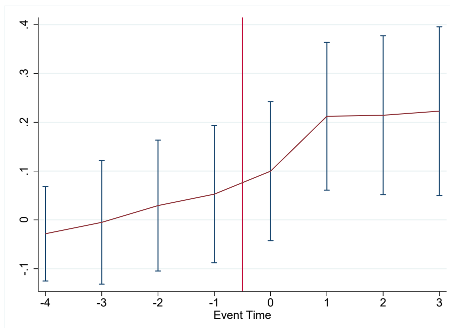
(a) Assets



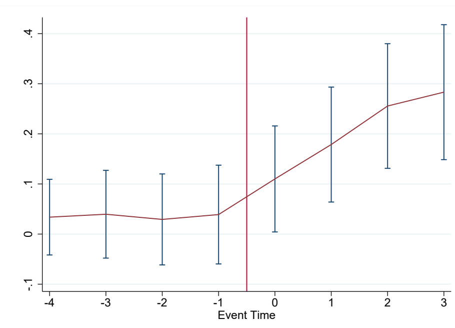
(b) Employment



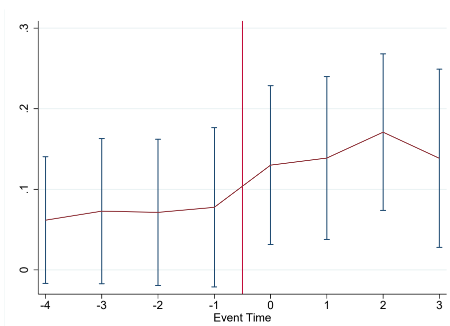
(c) K-L Ratio



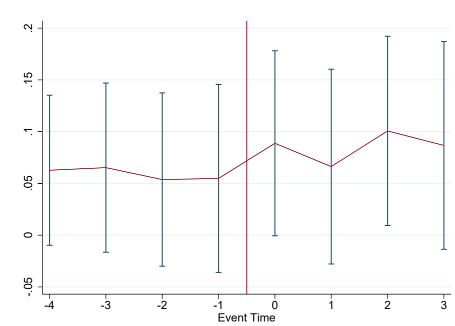
(d) Output



(e) Labor Productivity



(f) TFP



Notes: N = 23,133 firm-years (22,686 in the LP and TFP regression). The figure presents the estimated coefficients and the 99-percent confidence intervals of the event study regressions of Equation (4). 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.

Table A1: Description of Grant Types

Aim	Code	Min (HUF mn)	Max (HUF mn)	Max Share investment financed from grant	Size	Commitment for the two years after project ending
Development of firm's technology	GOP 2.1.1	1	150	50	micro, small and medium	
A	GOP 2.1.1	1	20	50	micro and small	Number of employees and net sales won't decline compared to base year, for medium companies sales should grow by 4-6%
B	GOP 2.1.1	20	150	50	micro, small and medium	Number of employees and net sales won't decline compared to base year, for medium companies sales should grow by 4-6%, for big companies by 5%
M	GOP 2.1.1	1	4	50	micro	Personelle expenditures should account for at least 50% of the grant
Modern management systems and techniques	GOP 2.1.2	0	1,4	50	micro, small and medium	Net sales should grow by 4-6 % and all sales should grow by a share of 0.8-1
Process management and e-commerce	GOP 2.2.1	1	25	50	micro, small and medium	Operational activities should grow by 25-50% of the grant amount OR sales from ecommerce should reach 120-300% of the grant amount
Introduction of quality, environment and other management systems	GOP 2.2.2	0	1,5	50	micro, small and medium	-
Site development	ROP 1.1.1	10	100	50	micro, small and medium	-

Note: This table describes the main features of the EU Structural and Cohesion Funds' subprograms aiming capacity building, purchase of new machinery and software.

Table A2: Results of Propensity Score Estimation

Firm	Employment	Sales	Tangible Assets	Average Wage	Low Free Cash
Log(var)	-0.001	0.052	0.140*	-0.166	-0.133
	(0.072)	(0.139)	(0.067)	(0.168)	(0.015)
Log(var) Squared	0.025	0.005	-0.008*	0.001	
	(0.015)	(0.006)	(0.004)	(0.009)	
Log(var) Lagged	0.015	0.155	-0.092	0.334*	
	(0.065)	(0.126)	(0.062)	(0.157)	
Log(var) Lagged Squared	-0.028*	-0.01	0.005	-0.01	
	(0.014)	(0.005)	(0.003)	(0.008)	
LEED					
Log(var)	-0.145	0.130	0.024	0.488	-0.168**
	(0.159)	(0.300)	(0.131)	(0.506)	(0.024)
Log(var) Squared	0.047	0.000	-0.001	-0.029	
	(0.029)	(0.012)	(0.007)	(0.025)	
Log(var) Lagged	0.178	-0.163	0.119	-0.063	
	(0.149)	(0.284)	(0.128)	(0.455)	
Log(var) Lagged Squared	-0.060*	0.002	-0.005	0.012	
	(0.029)	(0.012)	(0.007)	(0.023)	

Notes: Firm-level observations. N = 69,442/26,515 in the firm/worker sample. This table presents the estimated coefficients (standard errors) form a probit regression used to obtain the propensity score for matching. The dependent variable = 1 in the year precedent to winning a grant. The sample consists of all years precedent to an application. The regression includes industry, year and county controls. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table A3: Balance of Covariates in the Matched Sample

	Firm Data			Worker Data		
	Control	Treated	Std. Diff.	Control	Treated	Std. Diff.
Tangible Assets	10.817	10.937	0.056	11.044	11.131	0.043
	(1.565)	(1.469)		(1.456)	(1.374)	
Sales	12.794	12.82	0.015	13.077	13.058	−0.011
	(1.233)	(1.208)		(1.267)	(1.135)	
Employment	2.773	2.803	0.025	2.867	2.955	0.073
	(0.868)	(0.865)		(0.853)	(0.854)	
Wage	7.69	7.688	−0.004	7.774	7.742	−0.051
	(0.419)	(0.412)		(0.444)	(0.446)	
Sales Growth	0.035	0.048	0.026	0.098	0.085	−0.033
	(0.378)	(0.33)		(0.283)	(0.285)	
Labor Productivity	10.021	10.017	−0.003	10.21	10.103	−0.083
	(0.901)	(0.898)		(0.964)	(0.856)	
TFP	0.089	0.103	0.017	0.136	0.123	−0.017
	(0.583)	(0.517)		(0.551)	(0.478)	
Credit Constrained	0.415	0.418	0.005	0.055	0.045	−0.03
	(0.493)	(0.493)		(0.227)	(0.208)	
Ratio Skilled				0.332	0.342	0.016
				(0.471)	(0.475)	
Wage Skilled				11.487	11.421	−0.082
				(0.531)	(0.603)	
Wage Unskilled				11.109	11.089	−0.030
				(0.447)	(0.469)	

Notes: This table presents the average values (standard deviations) of firm and worker characteristics in the control and treated groups one year before application and the corresponding difference in average values between treated and control firms, scaled by the square root of the sum of variances. Tangible Assets, Sales, Wages and Labor Productivity are in logarithm.

Table A4: Comparison of the Application and Matched Samples

	Application Sample			Matched Sample		
Variable	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Firm Data						
Employment	2.718	0.985	109956	2.779	0.901	18697
Tangible Assets	10.789	1.685	109956	10.965	1.540	18697
Sales	12.498	1.367	109956	12.745	1.277	18697
Wage	7.635	0.518	109826	7.682	0.432	18694
Sales Growth	0.062	0.506	100254	0.018	0.417	18532
Labor Pr.	8.229	0.809	107037	8.246	0.713	18342
TFP	0.043	0.638	107037	0.053	0.559	18697
Free Cash Low	0.426	0.494	99451	0.480	0.500	18418
LEED						
Employment	2.786	0.911	872715	2.899	0.878	74686
Tangible Assets	10.839	1.613	872715	11.144	1.444	74686
Sales	12.586	1.280	872715	12.979	1.256	74686
Wage	7.663	0.467	872671	7.748	0.440	74686
Sales Growth	0.068	0.483	859780	0.007	0.364	74399
Labor Pr.	8.239	0.770	857649	8.344	0.718	73532
TFP	0.058	0.605	857649	0.091	0.549	73317
Free Cash Low	0.414	0.493	852764	0.073	0.260	74020
Proportion Skilled	0.325	0.468	850382	0.346	0.476	72783
Wage Skilled	11.513	0.671	230881	11.549	0.647	22093
Wage Unskilled	11.141	0.540	611886	11.188	0.508	50182

Notes: This table presents the average values (standard deviations) of firm and worker characteristics in the full and matched samples. Tangible Assets, Sales, Wages and Labor Productivity are in logarithm.

Table A5: The Effects of the Grant on Firm Outcomes with Long Pre-Trends

	(1) Assets	(2) Emp.	(3) K-L ratio	(4) Sales	(5) L. Prod.	(6) TFP
Winner	0.280***	0.104***	0.176***	0.176***	0.084***	0.036*
	(0.040)	(0.023)	(0.036)	(0.028)	(0.020)	(0.018)
Observations	23133	23133	23133	23133	22686	22686
R^2	0.846	0.846	0.777	0.876	0.651	0.519

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 (3). Regressions in Panel A are based on the applicant sample while in Panel B are based 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 clustered at the firm level. *** = significant at the 1-percent level; ** = significant at the 5-percent level; * = significant at the 10-percent level.

Table A6: The Effect of the Subsidy on the Composition of the Workforce and Wages of Occupational Groups

Panel A: Employment

	Manager	Skilled	Med. Sk.	Low Sk.
Winner	-0.018	0.014	0.002	0.003
	(0.011)	(0.011)	(0.014)	(0.009)
Observations	72782	72782	74685	74685
R^2	0.154	0.346	0.321	0.356

Panel B: Wages, Firm FE

	W. Man.	W. Sk	W. Med. Sk.	W. Low Sk.
Winner	0.090^{**}	0.067^{**}	0.030	0.049^{*}
	(0.039)	(0.031)	(0.019)	(0.027)
Observations	7701	14292	35752	14362
R^2	0.758	0.622	0.577	0.678

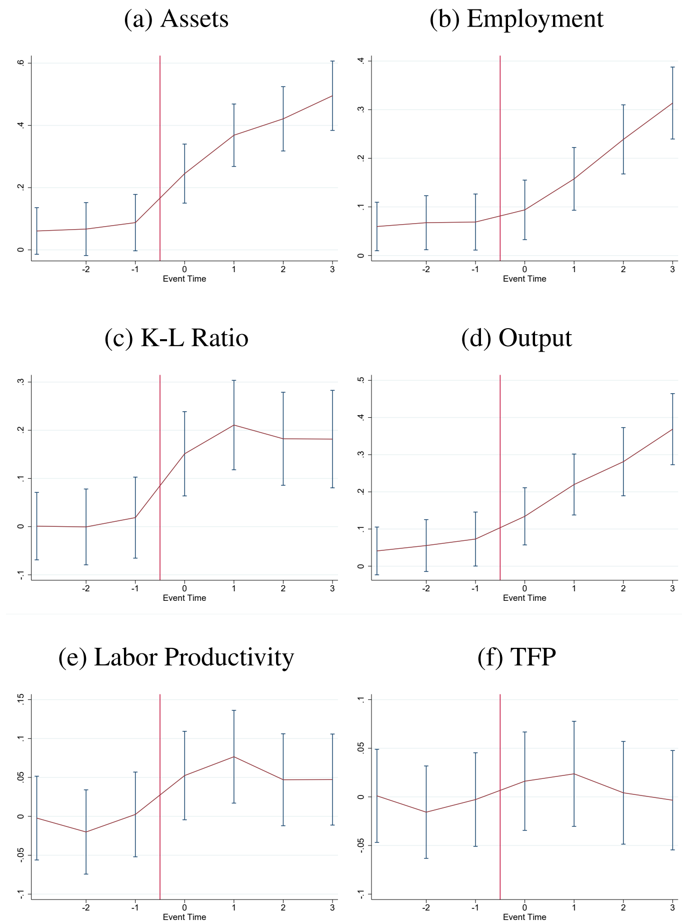
Panel C: Wages, Person FE

	W. Man.	W. Sk	W. Med. Sk.	W. Low Sk.
Winner	0.080^{*}	0.092^{**}	0.027	0.056^{*}
	(0.045)	(0.040)	(0.023)	(0.030)
Observations	7268	12683	31813	12206
R^2	0.869	0.832	0.823	0.892

Note: Worker-year observations. This table repeats the analysis of Table 7 but splits skilled workers into managers and other skilled workers and splits unskilled workers into medium skilled occupations and low skilled occupations. In panel A, the dependent variable is a dummy = 1 if the worker belongs to the given skill group, while in Panel B and C the dependent variable is log wage. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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.

Appendix B: Effects of Grants Based on the Applicant Sample

Figure B1: Dynamic Effects of the Subsidy (Applicant Sample)



Notes: N = 109,938 firm years (107,011 in LP and TFP regression). The figure presents the estimated coefficients and the 99-percent confidence intervals of the event study regressions of Equation (4). The main explanatory variables are event study dummies around winning the grant. The regressions are performed on the applicant 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.

Table B1: The Effect of Subsidy on the Composition of the Workforce and Wages (Applicant Sample)

	Skilled	W. Sk.	W. Usk.	W. Sk.	W. Usk.
Winner	-0.011^{**}	0.034^{***}	0.024^{***}	0.038^{***}	0.024^{***}
	(0.005)	(0.012)	(0.007)	(0.012)	(0.006)
Fixed effects	Firm	Firm	Firm	Worker	Worker
Observations	850355	230491	611719	210536	544717
R^2	0.352	0.650	0.590	0.859	0.834

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 subsidy. When the dependent variable is the wage of skilled (unskilled) workers, the sample is restricted to skilled (unskilled) workers. Regressions are based on the matched sample and they are weighted by the inverse of the number of workers in a firm-year. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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 B2: The Effect of the Subsidy on the Composition of the Workforce and Wages of Occupational Groups (Applicant Sample)

Panel A: Employment

	Manager	Skilled	Med. Sk.	Low Sk.
Winner	-0.016^{***}	0.005	0.006	0.006
	(0.004)	(0.004)	(0.005)	(0.004)
Observations	850355	850355	872686	872686
R^2	0.184	0.378	0.336	0.398

Panel B: Wages, firm FE

	W. Man.	W. Sk	W. Med. Sk.	W. Low Sk.
Winner	0.049^{***}	0.029^{**}	0.022^{***}	0.023^{**}
	(0.017)	(0.015)	(0.008)	(0.010)
Observations	75045	154567	397035	213993
R^2	0.758	0.655	0.603	0.641

Panel C: Wages, person FE

	W. Man.	W. Sk	W. Med. Sk.	W. Low Sk.
Winner	0.047^{**}	0.035^{**}	0.025^{***}	0.019^{*}
	(0.019)	(0.014)	(0.008)	(0.011)
Observations	70515	137333	352886	180872
R^2	0.863	0.868	0.828	0.857

Note: worker-year observations. This table repeats the analysis of Table 7 but splits skilled workers into managers and other skilled workers and splits unskilled workers into medium skilled occupations and low skilled occupations. In panel A, the dependent variable is a dummy = 1 if the worker belongs to the given skill group, while in Panel B and C the dependent variable is the the log wage. The observations are worker-years. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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 B3: Hiring, Separations and the Quality of New Hires (Applicant Sample)

	New hire	Separation	Job-to-job	Prev. Wage
Winner	0.026***	-0.045***	0.004	-0.019
	(0.007)	(0.006)	(0.012)	(0.019)
Observations	802679	761792	215941	130828
R^2	0.133	0.102	0.186	0.369

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. The dependent variables are the following: (1) a dummy = 1 if the worker was hired in the precedent year; (2) a dummy = 1 if the worker will be separated the subsequent year; (3) a dummy = 1 if the worker moved to the firm from another job; (4) the log of the workers' previous wage. Columns (1)-(2) show regressions for all workers, while the sample in columns (3)-(4) is restricted to new hires. Regressions are based on the applicant sample and they are weighted by the inverse of the number of workers in a firm-year. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm or worker fixed effects. The wage regressions also include 2-digit occupational codes. 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 B4: Single vs. Multiple Grants (Applicant Sample)

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
One grant	0.208^{***}	0.096^{***}	0.112^{***}	0.122^{***}	0.026^{*}	0.001
	(0.028)	(0.019)	(0.026)	(0.023)	(0.015)	(0.013)
First grant	0.467^{***}	0.261^{***}	0.206^{***}	0.368^{***}	0.111^{***}	0.040^{***}
	(0.031)	(0.020)	(0.027)	(0.024)	(0.016)	(0.014)
Second grant	0.345^{***}	0.172^{***}	0.173^{***}	0.168^{***}	-0.003	-0.032^{***}
	(0.018)	(0.011)	(0.017)	(0.014)	(0.011)	(0.009)

Notes: Firm-year observations. The table presents the estimated coefficients (standard errors) from Equation 3 where the grant variable was replaced by three dummy variables: (i) "One grant" = 1 in the year and after the firm won a grant for firms that received one grant; (ii) "First grant" = 1 in the year and after the firm won its first grant for firms that received multiple grants; (iii) "Second grant" = 1 in the year and after the firm won its second grant. The regressions are based on the applicant sample. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm fixed effects. 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 B5: Heterogeneity of the Effect by Firm Size and Productivity (Applicant Sample)

Panel A: Smaller than Median, Applicant sample

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.361^{***}	0.194^{***}	0.166^{***}	0.238^{***}	0.044^{**}	0.005
	(0.036)	(0.023)	(0.034)	(0.030)	(0.020)	(0.016)
Observations	53616	53616	53616	53616	53616	52245
R^2	0.789	0.764	0.731	0.764	0.710	0.440

Panel B: Larger than Median, Applicant Sample

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.273^{***}	0.222^{***}	0.051	0.279^{***}	0.055^{***}	0.029^{*}
	(0.036)	(0.024)	(0.033)	(0.030)	(0.019)	(0.017)
Observations	54702	54702	54702	54702	53461	53461

Panel C: Less Productive than Median, Applicant Sample

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.371^{***}	0.196^{***}	0.175^{***}	0.254^{***}	0.057^{***}	0.010
	(0.037)	(0.024)	(0.034)	(0.030)	(0.019)	(0.017)
Observations	52242	52242	52242	52242	50995	50995

Panel D: More Productive than Median, Applicant Sample

	Assets	Emp.	K-L ratio	Sales	L. Prod.	TFP
Winner	0.270^{***}	0.112^{***}	0.159^{***}	0.165^{***}	0.053^{**}	-0.001
	(0.044)	(0.026)	(0.042)	(0.027)	(0.022)	(0.023)
Observations	9202	9202	9202	9202	9202	9043
R^2	0.902	0.907	0.849	0.936	0.908	0.657

Note: 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 (3), when the sample is split by pre-application firm size and productivity. Firm size is measured by employment, productivity by the ratio of value added and employment in the year precedent to application. The sample is split into 'low' and 'high' groups based on pre-application employment/labor productivity being lower/higher than its median value. The regressions are based on the applicant sample. All regressions include industry-year controls, event year dummy variables around the application year for the grant and firm fixed effects. 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.

Appendix C: Productivity Decomposition

We follow Foster et al. (2008), who decompose aggregate productivity growth into within, between, cross and net entry terms. As all of these terms are sums of firm-level variables, one can quantify the contribution of a subset of firms – grant winners in our case – to each of them.

The original decomposition starts with the 6-year change between $t-6$ and t in aggregate productivity ($\Delta PROD_t$):

$$\Delta PROD_t = \underbrace{\sum_{i\in C} \theta_{i,t-6} \Delta prod_{i,t}}_{within} + + \underbrace{\sum_{i\in C} (prod_{i,t-6} - PROD_{t-6}) \Delta\theta_{i,t}}_{\text{between}} + + \underbrace{\sum_{i\in C} \Delta prod_{i,t} \Delta\theta_{i,t}}_{\text{cross}} +$$
$$+ \underbrace{\sum_{i\in N} \theta_{i,t}(prod_{i,t} - PROD_{t-6}) + \sum_{i\in X} \theta_{i,t-6}(prod_{i,t-6} - PROD_{t-6})}_{\text{net entry}}$$

where $\theta_{i,t}$ is the employment share of firm i at year t, $prod_{i,t}$ and $PROD_t$ are productivity measures at the firm and aggregate level, respectively. Δ always denotes change between $t-6$ and t. C denotes continuing firms, N new entrants and X exiting firms.

The interpretation of these firms is the following. The *within* term is the sum of firm-level productivity changes for continuing firms, weighted with their initial employment share. This term is large if firms, especially large firms, increased their productivity quickly. The *between* term captures the main channel of reallocation by quantifying the extent to which initially more productive firms grew faster. The *cross* term captures whether firms increasing their employment share were also able to improve their productivity. The *net entry* term is positive if new entrants were more productive relative to exiting firms.

As all these terms are sums of firm-level moments, we can further decompose each term to the contribution of grant winning and other continuing firms (disregarding exit and entry for grant winners):

$$\Delta PROD_t = \underbrace{\sum_{i \in grantee} \theta_{i,t-6} \Delta prod_{i,t} + \sum_{i \in other} \theta_{i,t-6} \Delta prod_{i,t}}_{\text{within}} +$$

$$+ \underbrace{\sum_{i \in grantee} (prod_{i,t-6} - PROD_{t-6}) \Delta\theta_{i,t} + \sum_{i \in other} (prod_{i,t-6} - PROD_{t-6}) \Delta\theta_{i,t}}_{\text{between}} +$$

$$+ \underbrace{\sum_{i \in grantee} \Delta prod_{i,t} \Delta\theta_{i,t} + \sum_{i \in other} \Delta prod_{i,t} \Delta\theta_{i,t}}_{cross} +$$

$$+ \underbrace{\sum_{i \in N} \theta_{i,t} (prod_{i,t} - PROD_{t-6}) + \sum_{i \in X} \theta_{i,t-6} (prod_{i,t-6} - PROD_{t-6})}_{\text{net entry}}$$

The total contribution of grant winning firms will be the sum if the three grant-winner terms, i.e. $\sum_{i \in grantee} \theta_{i,t-6} \Delta prod_{i,t} + \sum_{i \in grantee} (prod_{i,t-6} - PROD_{t-6}) \Delta\theta_{i,t} + \sum_{i \in grantee} \Delta prod_{i,t} \Delta\theta_{i,t}$.

We apply this methodology in the following way:

1. First, we take the sample of SMEs.
2. We calculate the aggregate labor productivity in this sample in each year. We weight with employment shares, therefore $\theta_{ij} = \frac{emp_{i,t}}{\sum_{all} emp_{i,t}}$.
3. We set up three cohorts of grant winners: (i) those that won between 2004-2005, (ii) between 2006-2008, and (iii) between 2009-2011.
4. For each cohort, we set up a 6-year window, which start in the year before the firms started to win (so the initial productivity level is not affected by the grants), and end three years after they won. The windows for the three cohorts are: (i) 2002-2008, (ii) 2005-2011, and (iii) 2008-2014.[55] This timing allows us to have a sufficient number of winner firms in each window and follow them for a period which is in line with our empirical setup.
5. We conduct the decomposition of labour productivity described above for all three windows, calculating each of the terms separately for grant winners and other firms. Note that this only requires information in the first and last year of the period for each window.

We extend this decomposition with a counterfactural exercise. In particular, we are interested what would have been the contribution of these firms, had they not received the grant. We calculated this counterfactual in the following way:

[55] We attempted to make windows of similar length which do not start or end in the crisis year of 2009.

1. For this exercise we took firms' initial productivity and size as given. We have modified the productivity and employment growth rates of each firms by subtracting our preferred estimates for the employment and labour productivity effects. The growth effect we use is 10.6 percent and the productivity effect is 7.4 from Table 6, Panel B.[56]

2. We calculate the "counterfactual" contribution of the grant winning firms with these counterfactual growth rates.

3. Comparing these counterfactual contributions with the observed ones helps us in understanding the effect of the grant.

[56]Therefore we use the modified $\Delta\bar{\theta}_{i,t} = (\Delta\theta_{i,t})(1-\beta_{emp})$ instead of $\Delta\theta_{i,t}$ in the grantee between and cross terms and the modified $\Delta p\bar{r}od_{i,t} = \Delta prod_{i,t} - \beta_{prod}$ instead of $\Delta prod_{i,t}$ in the grantee within and cross terms.